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Abstract

In this paper, we investigate the effect of exposure to homicides on the educational performance and human capital investments of students in Brazil. We combine extremely granular information on the location and timing of homicides with a number of very large administrative educational datasets, to estimate the effect of exposure to homicides around schools, students' residence, and on their way to school on these outcomes. We show that violence has a detrimental effect on school attendance, on standardised test scores in math and Portuguese language and increases dropout rates of students substantially. The effects are particularly pronounced for boys, indicating important heterogeneous effects of violence. We use exceptionally rich information from student- and parent-background questionnaires to investigate the effect of violence on the aspirations and attitudes towards education. In line with the effects on dropout and the longer-term human capital accumulation of students, we find that boys systematically report lower educational aspiration towards education. Making use of the very rich information from the homicides and education data, we explore a number of underlying transmission channels, including mechanisms related to school supply, bereavement and incentives for human capital investments.

JEL Classification: I25, K42, O12

Keywords: homicides, human capital investments, education, Brazil.

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1 Introduction

After a decade of declining rates of crime and homicides, more recently, Brazil (and other countries in Latin America) has observed a steep increase in violent crime. According to statistics from the World Bank, Brazil has one of the highest homicide rates in the world. In 2016, the intentional homicide rate in Brazil was more than 29 per 100,000 people, which is approximately 6 times the US rate and 29 times the UK rate. According to national security statistics, in 2016, 61,283 homicides were registered in the country[1]. The Brazilian Institute of Applied Economic Research (Ipea) estimated that the cost of violence corresponds to more than 5% of the country's gross domestic product (GDP), not including many intangible costs that are difficult to quantify (Cerqueira et al. (2007)). The pain, suffering, and trauma caused by direct victimisation and exposure to violence in the local neighbourhood may negatively affect a variety of societal outcomes, among those educational production. Violence may affect school supply and the behaviour of students, parents, teachers, and principals. In this paper, we estimate the effect that exposure to violence has on the performance of students in Brazil, using a unique novel dataset containing georeferenced information on all homicides occurring in the public way and combining this with very detailed information on student performance.

Several qualitative studies by psychologists, psychiatrists, and sociologists have found a range of adverse consequences in the behaviour of children after exposure to community violence: depression, anxiety, hyper-vigilance, avoidance, aggressive behaviour, delinquency, and deterioration of cognitive performance (Cooley-Quille et al. (1995) Smith and Tolan (1998), Fowler et al. (2009), Farrell et al. (2010), Sharkey et al. (2014)). Community violence can also affect attendance at school. When a crime occurs in the neighbourhood or in the proximity of the schools, parents may feel uneasy about sending their children to school. According to the 2012 edition of the Brazilian National Survey of School Health[2], almost 9% of the 9th grade students that answered the survey declared they had stopped going to school at least once in the 30 days preceding the survey due to not feeling safe on the way from their residence to school. Low attendance caused by fear can potentially damage the learning process of the students. They fail to attend classes that form part of their curriculum, and they are also deprived of regular contact with their classmates. This will eventually lead to low scores on their exams and can potentially affect a number of measures

[1] www.forumseguranca.org.br.

[2] *Pesquisa Nacional de Saúde do Escolar*, in Portuguese. Available from www.ibge.gov.br.

of school failure, including repetition and dropout rates. The exposure to homicides in the local neighbourhood may also reveal information to students and parents about likely victimisation and affect the expected returns on education and hence the optimal schooling decision.

Because of the potential for such negative externalities, the cost of violence may go well beyond the cost of direct victimisation. Poor neighbourhoods with lower socio-economic status often register higher rates of violence, and if this also has a negative effect on human capital accumulation, this could be a relevant channel leading to the perpetuation of poverty. The correlation between socio-economic conditions and crime rates nevertheless makes the estimation of the causal effect of exposure to violence on schooling outcomes difficult, as one needs to disentangle (unobserved) neighbourhood characteristics, which may be related to high levels of violence and worse schooling outcomes, from the underlying causal relationship.

This paper estimates the causal effect of day-to-day violence on schooling performance, using a unique set of Brazilian microdata. We have information on the exact date and precise location of each homicide, the schools and residences of students. We exploit the variation of homicides across space and over time to estimate the effect of exposure to homicides on a number of educational outcomes including test scores, repetition, dropout rates, school transition, and attendance, while controlling for school and time fixed effects, and in the most satiated specifications, school-specific time trends. Given the prevalence of high crime rates and economic deprivation in many countries in Latin America and elsewhere, the findings from this analysis may be relevant for the understanding of the perpetuation of poverty in these countries.

There are few studies estimating the relationship between exposure to day-to-day violence and school performance (e.g. Grogger (1997), and Aizer (2008)), which given the cross-sectional nature of their data generally cannot deal with the endogeneity problem arising from the fact that violence might be correlated with other sources of socio-economic disadvantages and school outcomes. There is also a related literature focusing on violent conflict, making use of variation in conflict across space and over time to estimate the effect of conflict exposure on educational outcomes. Brück et al. (2019) study the effect of the Israeli-Palestinian conflict on various education outcomes for Palestinian high school students by exploiting within-school variation in the number of conflict-related Palestinian fatalities during the academic year. Brown and Velásquez (2017) use the secular change in violence induced by the Mexican war on drugs to estimate the effect on human capital accumulation using municipality-level changes in drug-related violence. They argue that the negative effects of violence on education are driven by the economic consequences and financial hardship to households in relation to the violent conflict. Monteiro and Rocha (2017) estimate the effect of gunfights between drug gangs in Rio de Janeiro's favelas (slums) on student achievements using panel data for the city of Rio de Janeiro. They examined the effect of conflicts in favelas on students who study in schools located in favelas and in schools located within a 250 m radius from a favela border and found that student test scores in math were lower in the years in which they were exposed to gun battles.

Violence in Brazil is nevertheless a more widespread phenomenon that differs from armed conflict between drug gangs occurring in favelas, in terms of the intensity and the concentration of occurrence, both over time and across space. The measure of violence we use, homicides, captures the widespread nature of violence in Brazil and allows us to estimate the effect of day-to-day violence on student achievements in a much more general context, likely to be much more representative of the violence Brazilians face on a daily basis. The extremely granular information on the location of homicides allows us to investigate the exposure in the very close proximity to schools and the residences of students.[3] This makes the results presented in the paper relevant for the understanding of the externalities of day-to-day violence present in Brazil and in many other countries.

We focus our analysis on the city of São Paulo, which is the largest city in the Americas with a population of 12 million people. São Paulo provides an ideal setting for our study, because of the extremely detailed schooling outcomes we have available for São Paulo, and because of the sheer size of the data; we have information on 9 million primary and secondary school students located in more than 1,500 schools that we observe over the period from 2007-2013. The city provides also an interesting case study for understanding day-to-day violence in countries with more moderate crime levels, as is ranks close to the US in terms of the homicide rate.[4]

There are three main contributions of the paper. First, we provide the first causal estimates of the effects of day-to-day violence on schooling outcomes. For that purpose, we combine extraordinarily rich set of microdata on student outcomes with a measure of violence that is consistent across space and time: homicides. These allows us to focus on variation in day-to-day violence over time and across space, that is comparable and minimises measurement error. For these homicides, we have extremely granular address information, which we geocode and match with information on

[3]The context also likely affects the reporting of violence, which is why we focus our analysis on violence measures based on official death records, minimising the risk for selective reporting.

[4]The homicide rate in São Paulo has dropped dramatically over the past 20 years, from highs around 53 to 6 per 100,000 population in 2018, roughly equivalent to the level of the homicide rate in the US.

the addresses of the schools and residences of students attending these schools. This allows us to investigate the effect of exposure to violence around the schools and residences of students. We find that violence around the schools leads to a substantial deterioration in the educational performance of schoolchildren, as measured by standardised test scores in math and Portuguese language. We find that one additional homicide in a 25 m radius around schools reduces test scores in math and language by about 5% of a standard deviation in test scores. Furthermore, we find that homicides increase dropout rates and have a negative effect on attendance. We use very rich information on the student background and find that the effects are particularly pronounced among students from relatively poorer families, indicating that income may work as a buffer against the negative effects of crime. We also show that the estimated effects are particularly strong for boys, both for test scores and attendance. The results are robust to the inclusion of school-specific time trends and to a battery of checks for selection, spatial correlation, and different specifications.

Second, we are the first to investigate exposure to violence on the way to school for each student. We use Google APIs and design an algorithm to build corridors along the path from the residences to schools and investigate the effect of exposure to homicides in these corridors. As corridors vary in length for students attending the same school, different corridors mechanically may have different propensities to experience homicides. To deal with this, we provide within-corridor estimates and find a substantial and economically meaningful increase in dropout rates as result of exposure to violence on the school path. An additional homicide leads to an increase in the probability of a student dropping out of school of 3%, an increase of about 20% compared to the baseline. The results are robust to a number of different specifications; for example, we vary corridor width and constructed three distinct corridors - walking, driving, and public transport - to check for sensitivity of the results by examining potential alternative routes.

Third, we make use of the extremely rich information we have available on students, teachers and parents to investigate the underlying transmission channels. We show that different from the armed conflict settings in Monteiro and Rocha (2017) and Brück et al. (2019), day-to-day violence does not affect the supply of schooling, either through teacher absenteeism, teacher or head teacher turnover. We also show that the effects are not driven by bereavement for the death of a peer, a friend or a teacher. Different from Monteiro and Rocha (2017), we find that exposure to homicides has longer-term effects on the human capital accumulation of children and the effects are not driven by short-lived effects before the test date or by reductions in school attendance only. We use the extremely rich information we have available on student and parent reported educational aspirations and attitudes and find that boys' aspiration suffers as a consequence to homicide exposure. This is consistent with a transmission channel that operates through a differential effect on the incentives to invest in human capital for boys, who make up the vast majority of victims in homicides in Brazil.

The remainder of the paper is organised as follows. Section 2 explains the institutional background. Section 3 details the datasets used in the analysis. Section 4 presents the identification strategy applied to estimate the causal effect of violence on educational outcomes. Sections 5, 6, and 7 explain the results, and Section 8 presents the final remarks.

2 Institutional Background

The Brazilian educational system is predominantly regulated by the federal government, which is also responsible for distributing resources to states and municipalities. These secondary layers of government not only manage the funds received but are also allowed to implement state- or municipality-specific programmes and policies. The educational system is composed by two main levels: 1) *Educação Fundamental* (basic education), which comprises *Educação Infantil* (nursery), *Ensino Fundamental* (primary school), and *Ensino Médio* (secondary education) and 2) *Educação Superior* (higher education).

Public primary education is offered at no cost for all, irrespective of age, and it is mandatory for children between 6 and 14 years of age. It lasts nine years,[5] and it is divided into two stages: the first cycle, which comprises 1st to 5th grade, and the second cycle, which includes 6th to 9th grade. Public secondary school is also offered at no cost and lasts 3 years. It is not compulsory, but recent regulation pushes towards gradually making secondary education compulsory as well. To be able to enrol in secondary school, students must conclude primary school.

A school year contains at least 800 hours spread over at least 200 school days. The precise starting and ending days of the school year vary across schools and over the years. Figure A1 in the annex exemplifies the school calendar in São Paulo for 2010. Every year, the São Paulo State Secretariat of Education formally announces, by releasing a document called *Resolução*, the desirable starting day of the school year. In general, the first semester finishes on the last working day of June. The second semester starts on the first working day of August and finishes on the last

[5] Previously, primary school began at age 7 and lasted eight years. In 2006, the government passed a law that expanded primary school from eight to nine years and mandatory enrolment at 6 years old. States and municipalities had until 2010 to implement the new law.

working day before Christmas. Each semester is composed of two bimesters, with roughly 50 days each. The precise ending dates of each bimester are school specific. This setup leads to semesters that are defined state-wide, and bimesters that are school specific. Students may be retained in a grade at the end of the year if they do not achieve adequate school performance and/or they do not meet the minimum level of attendance required by law, which is at least 75% of the school days in primary schools and 85% in secondary schools.

Considering the nature of funding and administration of schools, they can be classified into four types: federal, state, municipal, and private schools. The first three are essentially public schools, maintained by the respective administrative units. In general, private schools are of better quality; however, only a relatively small share of the population can afford the substantial school fees charged by these schools. At least 87% of the students go to public schools in Brazil. In São Paulo, this number is slightly smaller at 80%. Schools may offer all or only specific levels of basic education, and there are schools that offer only primary education, some only secondary education, and some offer both.

Public school students are not bound to a specific school. They are able to enrol in any school with vacancies. In most cases, students attend schools located within walking distance of their residences. When this is not possible, they may qualify for school transport.

3 Data

We build a novel dataset by combining administrative data from three institutions: the Brazilian Ministry of Health, Brazilian Ministry of Education, and São Paulo State Secretariat of Education, and link these datasets using school, class, and individual identifiers and geographic information from the addresses.

3.1 Educational data

We have access to unique microdata, based on the Brazilian School Census and collected by the Brazilian Ministry of Education, on the universe of students in primary and secondary school. The Brazilian School Census comes from individual records of students and teachers, and rich background information on the physical characteristics of their schools, including information on the addresses of the schools. For all students attending municipal schools, we have information on the address of residence of the students. Unfortunately, this information is not available for students in state or private schools. Unique student and teacher identifiers allow us to follow students over time and across schools, which enables us to construct some of the outcomes we use in the analysis: grade repetition, dropout, transition from primary to secondary school and school transfers. Characteristics of students and teachers include date of birth, sex, race, student grade, and teacher educational background (among others).

We focus the analysis on the city of São Paulo over the period from 2007 to 2013. For consistency, we do not consider nursery schools[6] or any kind of special education, which is offered to students with special needs. The final dataset contains an average of 1.2 million students per year spread over more than 1,500 schools.

Of the schools in the sample, about a third are run by the state, and about 17% are run by the municipality. The large fraction of private schools (almost 50%) reveals that, given that only about 20% of students are enrolled in these, private schools are on average much smaller compared to state and municipal schools. Close to 60% of public schools offer free school meals, indication the lower socio-economic background of students attending these schools.

The majority of observations cover students in primary school (84%).[7] Measures of school efficiency, such as repetition and dropout rates, reveal substantial problems in the Brazilian educational system. More than 6% of schoolchildren repeat any given grade, and almost 10% drop out of a given grade.[8] In terms of transition from primary to secondary education, around 75% of students carry on beyond compulsory education and enrol in secondary school.

We combine the records from the Brazilian School Census with data from standardised test scores from SARESP,[9] provided by the São Paulo State Secretariat of Education, using unique student identifiers. The exam is carried out every year and evaluates the performance of students in Portuguese and math in the 5th, 7th, and 9th grades of primary and in the 3rd grade of secondary school.[10] To be able to compare the results to national standardised exams, we focus on test scores for the 5th and 9th grades of primary school and the 3rd grade of secondary school. These coincide with the end of each of the educational cycles described above. SARESP also collects

[6]Pre-primary education has gone through a period of very rapid expansion over the last years and comprises a number of different levels across ages, which makes it difficult to come up with a consistent definition of pre-school type.

[7]This number reflects both, the longer period of primary education, 9 years versus 3 years of secondary education, and the non-compulsory nature of secondary education.

[8]We define repetition as a student being enrolled into the same grade in the following year. The variable dropout includes the temporary dropout rate, where students leave school for one or more years but enrol at school again at a later point. The variable also includes students who do not enrol in secondary school after leaving the school system after primary school.

[9]*Sistema de Avaliação de Rendimento Escolar do Estado de São Paulo*, namely the System of Evaluation of Educational Performance of the State of São Paulo.

[10]SARESP is mandatory for schools in the state system, but some municipal and private schools opt-in and participate in the tests.

additional information through very detailed student and parent background questionnaires. These are completed after the exam by the students and are taken home and completed by the parents or legal guardian. For this paper, we are particularly interested in the childrens self-assessment of their performance, their educational aspirations and involvement in school, the parental assessment of their childrens performance and involvement, the parental self-assessment of their involvement, and the assessment of parental involvement by their children. For instance, for the self-assessment of their involvement in school, children are asked whether they do their homework, whether they plan to go to university, or whether they perceive themselves as good students. Parents answer questions on their perception of their childrens engagement in school, for example, whether they think their child is interested in school, is doing well in school, or studies at home. They also answer a number of questions on their involvement with their childrens education, for example, whether they help their child with homework, whether they ask about school, or whether they participate in parent evenings. Similarly, their children answer on whether their parents help with homework or ask about homework. In addition, parents are also asked to assess the safety of their children in school.

We also have access to individual attendance records of students in all state schools, which are collected by the São Paulo State Secretariat of Education. We again link the attendance records with the school census data using unique student identifiers. The dataset provides the individual attendance records of all students at state schools in São Paulo at a bimonthly frequency. The data contain information on the number of school days missed with some basic information about the reason for non-attendance (if any).

3.2 Violence data

We use microdata of official death records published by the Brazilian Ministry of Health. This dataset comes from the Mortality Information System, which compiles information from death certificates on all natural and non-natural deaths in Brazil. We use information from the ICD-10 coding of cause of non-natural deaths to identify victims of intentional homicides. In addition to cause of death, the death certificates contain characteristics of the deceased, such as date of birth, sex, race, occupation, and the location of occurrence of the homicide.

We have information on the precise location available only for homicides that occur in the public way.[11] We believe these homicides are particularly salient for our analysis for two reasons.

[11] In Figure A3, we present a comparison between homicides in the public way and the remaining homicide cases, which may

First, these homicides garner considerable attention and are particularly visible to the population. Second, these homicides form a more homogeneous group of homicides (and largely exclude domestic homicides). We geocode homicide addresses using the Google Maps API and restrict homicides geocoded at the street level, which correspond to 95% of all homicides in the public way.

Table 1 displays summary statistics of the victims of homicides for which the death occurs in the public way, and the description of the characteristics of homicides. Approximately 70% of the homicides are a result of assault by gun discharge, and about 10% each by assault using a sharp or blunt object. The majority of victims are in the age group between 19 and 50 years old, but a substantial number (8.4%) of relatively young victims of homicide are between the ages of 11 and 18. The vast majority of victims are male, and individuals from a lower socio-economic background are over-represented as victims of homicides, as indicated by the very low levels of completed education. Figure A3 in the annex shows the distribution over time and space of the homicides in the public way in São Paulo. Darker shades of red represent areas more affected by homicides. In the paper, we use the variation of homicides over time and space depicted in the maps, allowing us to disentangle the effect of violence from other correlates of socio-economic variables and thus establish causality between violence and education, as described in the next section.

4 Identification Strategy

Disentangling the effect of violence on education from confounding factors is not straightforward. In our case, poor neighbourhoods may register higher homicide rates, and students from disadvantaged backgrounds may be more likely to attain unsatisfactory results at school. Hence, it is necessary to deal with confounding factors that may lead to a positive association between levels of violence and poor educational performance. If, for example, areas with low socio-economic status also exhibit high crime rates, and if pupils from relatively poorer households in these areas also perform worse at school, this would lead to a positive relationship in these variables even in the absence of any causal effect of violence on education.

To deal with potential confounders, we use variation in homicides across space and time, where we are able to pinpoint the precise location of these homicides to the exact street address, while applying school fixed effects, effectively dealing with unobservable characteristics of the school and neighbourhood. We also include time fixed effects to account for time trends in outcomes. The most

occur at a hospital or residence, for example.

satiated specification includes school and time fixed effects as well as school-specific time trends allowing for outcomes in each school to follow its specific trend. Using the variation in homicides in the vicinity of schools (or the residence of students), we estimate the effect of exposure to violence on educational outcomes using the following estimation equation:

$$y_{ist} = \beta_0 + \beta_1 homicides_{st} + X_{it}\beta_2 + Z_{st}\beta_3 + d_s + d_t + d_{st} + u_{ist}, \tag{1}$$

where y_{ist} is a range of different measures for the educational outcomes of $student_i$, $homicides_{st}$ is the number of homicides that lie in the close periphery of schools, X_{it} is a vector of individual characteristics, Z_{st} are school and classroom time-varying characteristics, d_s and d_t are school and time fixed effects, respectively, d_{st} is a school-specific time trend, and u_{ist} is an error term.

We present an example of the variation we use in the maps in Figure A4 in the annex.[12] Each individual map shows schools and homicides in the public way in a neighbourhood in São Paulo in a semester. The information on very precise school addresses and the addresses of occurrence of homicides allows us to construct extremely granular exposure points, and we focus, for most of the analysis, on homicides occurring in a 25 m radius around schools. We chose a small radius around schools to make the best use of the very precise geocoding at the rooftop level of school addresses and the location of homicides and to avoid potential measurement error from choosing a larger radius that may lead to measures of homicide exposure that overlap different schools.[13]

For identification, we assume that, conditional on time and school fixed effects, the variation in the number of homicides in a very small geographic area around schools (and the residence of students) in a given period is random. We also include a very rich set of individual, teacher, classroom, and school characteristics to reduce sampling variability. The inclusion of these controls should not affect the estimates in a meaningful way, given our identification strategy, as we are in practice holding the socio-economic composition of students (and school inputs) constant. Although our identification strategy does not rely on baseline characteristics being balanced across schools that are exposed and not exposed, given the very localised measure of exposure to homicides, we can nevertheless directly test for this. For this purpose, we define schools that, over the period of interest, are ever exposed as 'ever exposed' schools and all others as 'never exposed' schools.

[12] A dynamic homicide map for all of Brazil and the period from 2003 to 2016 is available on www.cost-of-crime.com/homicide-map.

[13] For our preferred radius of 25 m, we have a minimal overlap, and 97% of homicides are unique to one school. This overlap quickly increases with larger radii: for 100 m, we have only 8% unique homicide-school combinations, and the number drops to less than 7% for a radius of 500 m.

Results in Table A1 show that student characteristics are balanced across a very large number of socio-economic background variables. We find that school characteristics are very similar, and for a very large number of variables, only three differences are statistically significant, in line with expectations.[14]

We use Equation 1 to separately estimate the effect of exposure around the school and around the residence of students on their educational outcomes. In addition, we are interested in testing whether exposure on the path from the residence to school has an effect on educational outcomes. For this purpose, we created corridors around the shortest distance path from the home address of students to their schools using Google APIs. In effect, we built polygons of different orthogonal distances from the path and count the homicides occurring within these corridors. As these corridors are different for students attending the same school, but living at different addresses, we estimate a variant of Equation 1:

$$y_{ict} = \beta_0 + \beta_1 homicides_{ct} + X_{it}\beta_2 + Z_{st}\beta_3 + d_c + d_t + u_{ict}, \tag{2}$$

where y_{ict} denotes the educational outcomes for students in the same corridor,[15] $homicides_{ct}$ is the number of homicides occurring in a corridor during a school year, and d_c is corridor fixed effects. Because corridors vary in length for students attending the same school, different corridors mechanically have a different propensity to be exposed to homicides. Corridor fixed effects hold this propensity constant over time, effectively eliminating the mechanical difference for exposure. Because different corridors lead to the same schools, the model including corridor fixed effects effectively also holds school time-invariant characteristics constant. Alternatively, we also estimated models including time and school fixed effects while controlling for distance.[16] The estimates of these models are very similar to the estimates of the corridor fixed effects (results available in Table A16 in the annex). We calculated three alternative corridors: walking, driving, and public transport. Rather than seeing these strictly as the walking, driving, and public transport path, we consider these to be simple alternative corridors useful to determine the sensitivity of a particular path. We built corridors for the three alternatives based on 50 and 100 m widths centred on the

[14] Alternatively, we also regressed 'treatment status' on the full set of individual and school characteristics using a school panel setup, and we cannot reject the null hypothesis of no joint significance in an F-test.

[15] This means students attending the same school and living at the same address. There are multiple observations from following the same corridor over time and from the fact that students live at the same address and attend the same school.

[16] We use the natural log of the calculated path distance from the Google Directions API. We restrict the maximum corridor length to 3,000 m to limit the number of API calls and to exclude cases of mistaken address information from geocoding. Overall, we exclude about 1.5% of the cases.

path. Figure A5 shows a fictional example for a walking path corridor including different widths and exposures to homicides on the path.

5 Results - Exposure around Schools

In this section, we present the results of the effect of exposure to violence around schools using the granular 25 m radius exposure measure of homicides. We start with estimating the effect of homicides on measures of academic achievement in Subsection 5.1 and provide a battery of robustness checks in Subsection 5.1.1. We then investigate heterogeneous effects in Subsection 5.1.2. Finally, we estimate the effect of homicide exposure on a number of additional outcomes, including school attendance, self-reported measures of aspiration, attitudes, and perception of students and their parents, and measures of student progress through the education system.

5.1 Effect of homicides on academic achievement

First, we estimate the effect of exposure to violence on academic achievement using the standardised test scores in math and Portuguese from SARESP for students in state schools. Both test scores are normalised with a mean of 250 and a standard deviation of 50. As the explanatory variable *Homicides*, we use the count of homicides in a 25 m radius around the school. We present robust standard errors clustered at the school level in parentheses. To account for possible spatial dependence between schools and for serial correlation, we also compute the Conley standard errors[17] Conley (1999), presented in brackets. Table 2 presents the regression results of the effect of violence on math and language test scores for students in the 5th and 9th grades of primary school and the 3rd grade of secondary school.

In the first column, we estimate the effect of homicides on standardised math test scores, including school and time (year) fixed effects without further individual or school controls. In the second column, we include the rich set of student, teacher, classroom, and school characteristics as controls.[18] In the third column, in addition to the full set of controls, we include as a control the interaction between school and time, allowing for school-specific time trends.

Across specifications, we find a negative effect of homicides on math test scores. Adding the full set of controls does not significantly change the estimate, lending further credibility to the

[17] We compute Conley standard errors using a 25 m cut-off distance in accordance with the definition of measure for exposure. Results remain the same if we use 50 or 100 m.

[18] Please see table notes for a detailed description of the full set of controls.

identification strategy. The inclusion of the controls nevertheless reduces noise and hence improves precision of the estimate. Using our preferred specification in column (2) including the full set of controls, we find that an additional homicide in the surroundings of schools during the year decreases math test scores by about 2.3 points, an effect equal to roughly 5% of a standard deviation of test scores.

The inclusion of school-specific time trends increases the results slightly, to an effect size equal to 6.6% of a standard deviation, significant at the 5% level.[19] Applying Conley standard errors to address potential spatial and serial correlation reduces the standard errors and improves precision further, suggesting that spatial and/or serial correlation of homicides is not relevant in our context.

In columns (4) to (6), we repeat the exercise for Portuguese language scores. Across specifications, we find that exposure to homicides around schools has a negative effect on test scores of slightly smaller but overall roughly similar magnitudes.[20] The coefficient for our preferred specification in column (5) is 2.1 percentage points, equivalent to about 4% of a standard deviation in test scores.

The estimated effects on math and Portuguese test scores are sizeable and economically important. To put our estimates in context, we suggest comparing the effect of exposure to one additional homicide with the effect of educational inputs, for example teacher quality. Our estimates show that exposure to a homicide in the school vicinity has approximately the same effect as a reduction in teacher quality[21] by half a standard deviation on nationally standardised distributions of achievement, demonstrating the economic relevance of the effects. With violence being a widespread phenomenon in Brazil and homicides reaching an all-time high in recent years, this suggests that exposure to violence may contribute significantly to low achievement of students, particularly in areas more prone to violence. While São Paulo offers an ideal setting to study the effect of violence on homicides because of the outstanding educational data, it is indeed the state with the lowest homicide rate in Brazil,[22] making our estimates potentially even more relevant for states with higher homicide rates and a higher chance to be exposed.

This nevertheless also raises the question regarding how the effect of exposure to a homicide varies by general crime levels. More frequent exposure to crime may either lead to a stronger or

[19] When comparing the coefficients pairwise across our specifications, none of the pairwise differences are statistically significant.

[20] This is consistent with the findings of Monteiro and Rocha (2017), who found that the coefficients for language are generally smaller compared to the effects for math test scores. Our results differ though from their estimates on language test scores, as their estimates for the Portuguese language are a magnitude smaller and not significant at conventional levels.

[21] As estimated by Rockoff (2004).

[22] www.forumseguranca.org.br

weaker reaction to homicide exposure. To test for how our effects vary by crime levels, we estimate our preferred specification from columns (2) and (5) in Table 2 separately for children in schools in *high-crime* and *low-crime* areas.[23] We present the results in Table 3. We find that the effects are much more pronounced in *low-crime* areas, both for math and Portuguese test scores. We find no effect in *high-crime* areas. This is consistent with the hypothesis that the effects of violence are relatively less pronounced when violence is endemic, a result also documented in the context of birth outcomes of mothers affected by homicide exposure in Brazil according to Foureaux Koppensteiner and Manacorda (2016).

To understand whether the main effects are driven by shifts in the lower or upper part of the test score distribution, we also create indicator variables identifying students performing at different levels of proficiency. For both math and Portuguese performance, we create variables indicating *very low* (10th percentile), *low* (25th percentile), *median*, *high* (75th percentile), and *very high* (90th percentile). The variables *high* and *low* correspond to what the State Secretariat defines as the 'advanced' level and 'below the basic level' of proficiency. Table 4 presents the results. We find that exposed students are more likely to be classified as performing at *very low* and *low* levels of proficiency, both in the math and Portuguese language tests. We also find that students are less likely to be classified as performing at *high* and *very high* levels, indicating that students over the entire test score distribution are affected by homicide exposure. The shift in the distribution is nevertheless more pronounced for below the median in the test score distribution.

5.1.1 Robustness checks

5.1.1.1 Spatial distribution of homicide exposure

Because schools are often located close to each other in the high-density urban setting of São Paulo, we focus on a 25 m radius around schools as measure of exposure, using the very granular geographic information we have on the addresses of schools and the occurrence of homicides. This minimises potential measurement error from avoiding exposure to the same homicides overlapping across different schools.[24] To test the robustness of the 25 m measure, we also create exposure

[23]For this purpose, we consider the homicide count in rings around schools (between 500 and 25 m radii around schools) over the entire period (2007-2013) to proxy for the homicide proneness of the school surroundings. We classify school surroundings as *low* homicide, where the homicide count is less than or equal to the median and as *high* where the homicide count is above the median.

[24]To address potential spatial and serial correlation in our data, we compute Conley (Conley (1999)) standard errors using a weighted average of spatial covariances with a cut point of 25 m. We also computed these standard errors at a 100 m cut-off; the results are unchanged. We find that the standard errors are generally smaller when using the Conley correction, and hence, we are not worried that spatial or serial correlation affects the precision favourably to finding significant estimates when not

measures including homicides that are farther away from schools. We expect that the coefficient reduces in size when including homicides that are farther away. This happens for two reasons. First, if we believe that homicides farther away from schools have a weaker effect on students because of the less direct exposure of students, including these homicides will dilute the overall coefficient. Putting it differently, homicides that occur in the very close vicinity of the school likely are much more visible and can be observed by the largest possible fraction of students, whereas homicides farther away are less salient.[25]

Second, once we increase the radius around schools, we find that exposure areas start to overlap more frequently, reducing the signal of the measure. As a robustness check, we therefore estimate regressions in Table 2 using exposure to homicides for larger radii of 100 and 500 m from school. We present the estimates in Table A2. As expected, we find that the coefficients for homicides in a 100 m perimeter are substantially reduced for math and Portuguese language scores. The coefficients are roughly 61% and 58% of the original coefficients, respectively. We lose any effect for exposure at 500 m for math and Portuguese, and the coefficients are close to zero and not statistically significant. Alternatively, we estimate the effect for annuli or 'rings' of different width corresponding to the radii estimated above. As these will not be a weighted average of the original and additional homicides, we expect that the coefficients will drop at a quicker rate when considering homicides in the ring measure. The estimates are presented in Table A3. Indeed, we find that, while the estimates for the annuli of 25-100 m are still negative, the coefficient is reduced at a quicker rate when compared to the 100 m radius measure.

5.1.1.2 Timing of homicide exposure

Although test scores are only available annually, we can still use the high frequency nature of the homicide data to learn about the role of the timing of exposure. First, we use the information on the timing of homicides and the precise test date to learn about whether the results on the performance of students in standardised tests are short lived. To do so, we exclude homicides closer to the test dates from our homicide measure. We present the results in Table 5. To start, we excluded all homicides in the two-week window prior to the test dates. In fact, no homicides occur just prior to the test dates, indicating that the main estimates provided in Table 2 are not caused

addressing potential spatial correlation. We report these standard errors in brackets for all presented specifications. In general, spatial standard errors are similar to regular clustered standard errors, confirming that spatial correlation likely plays no major role in our context.

[25] When increasing the radius, homicides that previously were captured in the 25 m radius now define exposure for additional schools but are on average farther away from schools, hence diluting the effect of exposure of the original estimates.

by short-run effects. This is confirmed by the identical coefficient in columns (1) and (2), and in (6) and (7) for math and Portuguese test scores. In columns (3) and (8), we exclude homicides one month prior to the test. We find very consistent effects compared to the benchmark coefficient; the coefficients for math and Portuguese are even slightly more pronounced. This is also true when excluding all homicides occurring in the second school term. Columns (4) and (9) reveal even more pronounced effects, both for math and Portuguese language performance for homicides occurring more than six months prior to the test date.

This exercise shows that the overall effect is not driven by short-run effects of exposure to homicides just prior to the test date, as would be consistent with effects driven by the short-run stress and a short-run effect on mental well-being of students exposed to homicides.[26] We can also rule out that the effects on test scores are caused by a short-run disruption in the organisation of the tests by homicide exposure around schools or the compositional change of students induced by any short-run effect on the mental well-being of students. The strengthening of the effects for homicides occurring temporally further away from the test date, indicates that any underlying mechanism behind the effects is likely of a longer-term nature. We discuss this in more detail in Subsection 7.

The information on the timing of homicides also allows us to engage in a falsification exercise. Mechanically, homicides occurring after the test dates should not affect the test performance of children. To test for this, we create leads of our explanatory variable. A significant effect of the lead homicide measure may indicate a violation of the identification assumptions. In columns (5) and (10) of Table 5, we report the coefficients for the lead exposure variable. We find no effect of homicide leads on either math or Portuguese language test scores. The coefficients are much smaller and not statistically significant, lending extra credibility to our estimation strategy.

5.1.1.3 Characteristics of victims

We also use information on the victims and create homicide counts specific to victim characteristics.[27] We use information on the age and sex of the victim and on the cause of death that allows us to categorise homicides by means involving firearms or any other means.[28] We report the effects

[26]Our results contrast in this respect with the findings by Brück et al. (2019) who found that students in schools exposed to conflict-related fatalities during the Second Intifada in the West Bank led to the deterioration in school outcomes in the short-run, which they attribute to the short-term worsening in the students' psychological well-being.

[27]Due to the origin of the data from public health records, namely death records, the information on the characteristics on the crime are relatively limited. For example, we do not have information on the perpetrator in the data or information on the circumstances of the crime, which is sometimes available in crime surveys or police incidence data.

[28]When creating categories of homicide victim characteristics, we are somewhat restrained by relatively small numbers in some categories, which is why we focus on creating relatively coarse main categories. For example, male victims account for roughly 92% of all homicide victims, and homicides by means of gun discharge for roughly 69%. We report descriptive statistics

for these victim characteristics in Panel B of Table 5. Compared to the baseline coefficient for math and Portuguese language reported in columns (1) and (5), we find no pronounced differences by homicide category. In columns (2) and (6) , we report the coefficients for victims older than 18. The coefficients are slightly bigger, indicating that the main effects are not driven by homicide victims of similar age to the students in our sample. Next, we report the coefficients only using homicides involving male victims (Columns (3) and (7)). The effects are again slightly bigger, both for math and Portuguese test scores. Finally, we estimate the effect only using victims that were killed by firearm discharge. Again, the coefficients are larger compared to the benchmark, including all homicides, both for math and Portuguese. This finding is consistent with homicides involving the discharge of a firearm being more perceptible by victims or generally being perceived as more serious. Estimating the effects for the largest groups of victims and finding effect sizes in line with the overall effects both for math and language scores reassures us that the effects are not driven by a small number of very specific cases of victims that have an especially large effect. To the contrary, we find evidence that the effects for most generic types of victims might even have slightly more pronounced effects on student achievement in SARESP.

5.1.1.4 Testing for selection in attendance at tests

For a low-stakes test, attendance rates at the SARESP test are high with approximately 87% of students sitting the test. Because of the low-stakes nature of the tests, schools have generally little incentive to manipulate attendance of students at the test, and the scope for selection based on incentives to schools is likely negligible. Despite the high attendance rates, we would like to rule out that attending students are self-selected and that this process is correlated with exposure to homicides. If homicides in the school surroundings affect students' decisions to participate in the test and the propensity to attend differs systematically by student types, this could bias our results.

To test whether students taking SARESP are selected, we start by testing whether violence in the school surroundings affects attendance of students at the math and language tests. For this purpose, we estimate the effect of exposure to homicides in the school surroundings with an indicator on whether a student attended the test, separately for math and for Portuguese. Columns (1) and (8) of Table A4 report the effect on attendance for math and Portuguese, respectively. Both estimated coefficients are small (1.4% and 1.3% compared to the mean) and are not statistically

for all available homicide characteristics in Table 1.

significant. We further test whether the composition of students attending the test differs in any other way. We do this by estimating the effect of homicide exposure on the fraction of boys and girls, white and non-white students, and students from low versus high-income backgrounds. All of the coefficients are small and not significant, and we are therefore confident that self-selection of students into the test does not bias our estimates.[29]

Although within-year transfers across schools are rare, these might lead us to miss selection using the above measures for attendance. We therefore test separately whether homicide exposure has an effect on within-year transfers of students. We create an indicator variable taking a value of 1 for students that attended a school at the end of the school year different from the school they were initially registered in at the beginning of the school year. In Table A5, we report the estimates. The coefficients are very small (close to zero) and not statistically different from zero. Taking these results together, we are confident that the estimates are not biased through selected attendance at the SARESP tests.

5.1.2 Heterogeneous Effects

5.1.2.1 Analysis by cohort

One additional advantage of the data and identification strategy in this paper is that we can investigate the effect of exposure to homicides for standard outcomes for different age groups. In Table A6, we present the results of the effect of exposure to violence on math and language test scores for each of the three cohorts in our sample: the 5th and 9th grades of primary school and the 3rd grade of secondary school. All specifications include time and school fixed effects and the full set of controls. The coefficients for math are most pronounced for students in the 5th and 9th grades of primary school, for whom an additional homicide in the surroundings of the school during the year implies a reduction of 4.8% and 4% of a standard deviation of math proficiency, respectively. The effect is much smaller and not significant for the 3rd grade of secondary school. We find a very similar pattern for the Portuguese language test scores, with the most pronounced effects for 5th graders and smaller effects for 9th graders and for the final year in secondary school. Splitting the sample by grade nevertheless reduces the precision of the estimates so that, apart from the effects for 9th-grade math test scores, none of the coefficients are separately significant.

[29]This is consistent with the fact that the coefficients in Table 2 do not vary across specifications when adding a very large set of socio-economic controls.

5.1.2.2 Analysis by gender

In Table A7, we present the results of the effect of violence in the school surroundings on math and language standardised test scores separately for boys and girls. All specifications include time and school fixed effects and the full set of controls. We find negative effects of homicide exposure for both boys and girls, but the effects on boys are more pronounced than for girls. For each additional homicide around the school in the year, boys' math proficiency decreases by about 5.9% of a standard deviation and their Portuguese language proficiency decreases by 5.6%. The effect on girls is about half this size for math at 3.5% of a standard deviation in math, and only significant at the 10% significance level when considering Conley standard errors. Girls' language coefficient is not significant at the conventional levels.[30]

Strikingly, while we find more pronounced effects on educational outcomes for boys compared to girls, we find that parents evaluate the safety of their children at schools differently. We present these estimates in Table A9. Asked about whether parents think their *child is safe at school* or *feels safe at school* and about their rating of the security at school, parents perception of the safety of their children is reduced throughout all of these categories for boys and girls. The effects are nevertheless much more pronounced for girls, suggesting that the subjective evaluation of school safety by parents suffers more for girls than boys. This suggests that the stark difference we document in the effects on math and Portuguese test scores are not driven by the relative shift in the perception of safety (by parents).

These findings are consistent with gender differences in psychological resilience in dealing with stressors leading to girls being less affected regarding their educational outcomes than boys.[31]

5.1.2.3 Analysis by socio-economic status

Next, we use information on parental income and educational background to examine heterogeneous effects by socio-economic status. First, we split the sample by income per capita and classify parents whose family income per capita is less than the median income in each year of the analysis as *low income* and classify others as *high income*. Second, we separately analyse students whose

[30] This finding contrasts with the findings by Monteiro and Rocha (2017), who found stronger effects of exposure to gun battles in Rio de Janeiro for boys than girls.

[31] Evidence from psychology presents very mixed results on systematic gender differences in stress resilience, but point to an important distinction between the perception of stressors and coping mechanisms for dealing with stress, leading to an ambiguous effect of stress on objective outcome measures. Arla and Holly (2003) show that female high school students rated the perceived stressfulness of five hypothetical scenarios higher than male students but were also more likely to seek support. Matud (2004) shows stark differences in the perception of stress, with females subjectively being more stressed than male participants in the study.

parents have completed, at most, primary school, denoted as *less educated* and students for whom at least one parent has completed at least secondary school, denoted as *more educated*.

In Table A8, we present the results of the effect of violence around schools on test scores separately for each of these categories. All specifications include time and school fixed effects and the full set of controls. Columns (1) and (2) compare math test scores of children in low- and high-income families. We find a much more pronounced and statistically significant negative effect for low-income students, while the effect for high-income students is very close to zero and not statistically significant. We find the same pattern for language proficiency, revealing a similarly stronger effect for students from lower compared to higher income families, as shown in columns (5) and (6).

In columns (3) and (4), we compare the math proficiency of students by the educational background of their parents. Although not significant at conventional significance levels, the results suggest that students whose parents are more educated are more affected in math by exposure to homicides. We observe a similar pattern for Portuguese test scores, but the differences are less pronounced compared to the socio-economic background. We should emphasise that all estimates in Table A8 include the full set of individual controls (i.e. in columns (1), (2), (5), and (6), we control for the educational background of the parents, and in columns (3), (4), (7), and (8), we control for income).[32]

These results suggest that socio-economic background may have a mediating role. High income seems to provide a buffering mechanism against the harmful effect of exposure to violence. Parents of higher socio-economic status may be better able to shield their children from the negative effects of exposure to violence, possibly through additional safety measures or by providing a sense of security by dropping and picking up their children by car. This is also consistent with the body of literature documenting how parents' socio-economic status may influence children's educational performance through their behaviour and beliefs. In particular, parents of a higher socio-economic status are generally more likely to actively engage in their children's educational process. They are more engaged with teachers, spend more time with their children, and provide more assistance and support for learning at home (Flouri and Buchanan (2004), Davis-Kean (2005), Dearing et al. (2006) Guryan et al. (2008), Houtenville and Conway (2008), De Fraja et al. (2010), Gelber (2013), Mora and Escardíbul (2018)).

[32] We experimented with alternative definitions of high versus low education for parents. Generally, because two individuals are involved, it is much more difficult to define low versus high education households, compared to using income. Alternative classifications (i.e. for high education where both parents have beyond primary education) deliver very similar results.

The contrary effects by education are somewhat unexpected. As we simultaneously also control for parental income, these results possibly point to a different mechanism at work, and we can only speculate on the mechanism. More highly educated parents, with everything else equal, possibly may have a better perception of the risks involved when exposed to violence, and in the event of a homicide, they might be more cautious in sending their children to school, hence affecting their children's performance.

5.2 Student attendance

Attendance is an important input factor in educational production. Lower school attendance as a consequence of exposure to homicides may at least partially explain lower test performance. Aucejo and Romano (2016) found that a reduction in absences at school leads to an increase in both math and reading test scores. We are therefore interested in first understanding whether exposure to homicides around schools affects attendance of students at school, and we use unique individual attendance records of students to whom we have access. Attendance records in São Paulo are available at the bimester. As the ending dates of the bimesters are school specific and these dates are not available centrally, we group the first two and last two bimesters into two semesters.[33] We then calculate the attendance rate of each student for the entire year and in the first and second semesters to use the higher frequency nature of the data. We use the same routine to calculate the explanatory variables. *Homicides (year)* corresponds to the number of homicides within a 25 m radius from school in the entire year. *Homicides (1st semester)* and *Homicides (2nd semester)* are the numbers of homicides within a 25 m radius from school in the first and second semesters. In Table 6, we present the regression results of the effect of violence on attendance. In the first column, we present the results for annual attendance records, and in columns (2) and (3), the results for the first and second semesters, respectively.

We find that one additional homicide in the year reduces attendance by approximately 1%. These results are largely confirmed when examining attendance separately by semester. Each additional homicide around the school in the first semester also reduces attendance in the respective semester by 1%. The coefficients for the second semester exceed the magnitude of the coefficients of the first semester. In the second semester, one additional homicide in the surroundings of the school reduces attendance by about 2% for our preferred specification.[34]

[33] We used the official starting and ending dates of each semester provided by the São Paulo State Secretariat of Education.

[34] This small difference in the outcomes for these semesters could be explained by the dynamic incentives for students to attend over the year. As students can be retained if they fall below a 75% attendance threshold, students may be more prudent

We are also interested in understanding potential heterogeneous effects in line with the previous section. In Table A10, we report the effects on attendance by cohort. We find that attendance at primary school is affected by homicide exposure, whereas we do not find an effect on attendance in secondary school. Table A11 presents the effect of exposure to violence in the school surroundings on attendance in the year and in each semester for boys and girls. We confirm the general pattern across the semesters, with stronger effects in the second semester for both boys and girls. Overall, we find that the effect of homicide exposure on attendance is much more pronounced for boys than for girls, confirming the more pronounced effects for boys in math and language achievement. Finally, we also examine how the effects vary by the socio-economic background of the parents. The results in Table A12 by family income are consistent with the patterns we find for test scores. High income seems to mediate the negative effect of homicides on attendance, and the estimates on absenteeism are much more pronounced for low-income families. When splitting the sample by parental education, we do not find a clear pattern for the effects by family income.

The effects of exposure to homicides on absenteeism are concerning, as low attendance may also hurt achievement. Being an important input factor in educational production, it may also constitute a relevant channel through which violence affects performance on math and Portuguese tests. Alternatively, the effects on attendance and achievement may reflect a general shift away from human capital investments and may therefore be jointly determined.

To determine how much of the results on test scores can be explained by absenteeism alone, we estimate specifications in columns (2) and (5) in Table 2, including student attendance as a control. The results in Table A13 show a decrease of about 17% in the math coefficient and an 11% decrease in the language coefficient. Although the inclusion of an endogenous variable on the right-hand side poses its own concerns, this exercise may explain the role of attendance as an underlying channel explaining the negative effect on achievement. Interestingly, the inclusion of student attendance in either math or Portuguese reduces the coefficient on test scores only minimally. We interpret this as evidence that the reduction in attendance is unlikely to be the main driver of the negative

regarding their attendance earlier in the school year. Later in the year, when students have more control over their overall yearly attendance, they may be less prudent. We find some evidence for that when comparing the mean attendance rates. In the first semester, attendance is close to 2% higher compared to the second semester. In addition, the law regulating student attendance in São Paulo states that, if a student has accumulated excessive absences, the school must intervene and inform parents, so that they can take measures to remedy the problem. If parents are unsuccessful and the problem persists, the school must notify *Conselho Tutelar*, which is a local legal institution responsible for ensuring the well-being of children and adolescents. This is to attempt to take measures during the year to avoid student repetition due to absences. If students accumulate excessive absences in the first semester, the schools intervene and try to remedy the situation. As a result of the efforts of parents and the schools, the effect in the first semester may decrease. In the second semester, closer to the end of the year, in the event of any negative shock that may affect student attendance, the school may not have time to intervene before the end of the year. Moreover, since it is the end of the year, students may find it harder to catch up with missed classes and potentially miss even more school days.

effects on student achievement.

5.3 Student and parental aspirations and attitudes towards education

In addition to the objective educational outcomes (test scores and attendance), we have a unique set of self-reported measures available regarding student aspirations, attitudes, and their general perception towards education and school. We can mirror these student-reported variables with information collected from their parents. These outcomes collected in the socio-economic background questionnaire of SARESP put us in a unique position to understand better how exposure to violence affects student aspirations, perceptions about their performance at school, and general attitudes towards school. A similar set of questions answered by their parents allows us to validate the results from a parental perspective.[35] We start by analysing the answers from student-reported aspirations, attitudes, and perception. In addition to their aspiration for post-compulsory education, we are particularly interested in students' general attitudes towards education, their perception of their own performance, and their self-documented home effort towards education (i.e. homework). We use the answers to the binary questions (where agreement with a statement takes a value equal to one, and zero otherwise) as dependent variables and estimate the effect of exposure to homicides using the same specification with the full set of controls as in column (5) of Table 2. We report the estimates separately for boys and girls in Table 7.

We start with the aspiration to continue with post-compulsory education. The question is framed as 'I intend to go to university'. Roughly half of students agree with this statement. We find that exposure to homicides in the school surroundings decreases agreement with this statement for boys by about 3.4%, an 8% reduction in the fraction of boys agreeing with this statement compared to the mean. In contrast, we find the opposite effect for girls. Girls are 4% more likely to agree with the statement when exposed to homicides, but the effect is not significant at conventional levels.

Next, we investigate the effect on self-assessed performance in school. We find that boys are significantly less likely to agree with the statement 'I am a good student'. We find that homicide exposure reduces the propensity to agree with this statement by 14% (compared to a mean of 0.39). We find again, in contrast to boys, the opposite effect on girls. We cannot distinguish whether this reduction for boys is the outcome of reduced effort and willingness to invest in their education, or

[35]We focus on answers provided by 9th grade students for two reasons. First, the 9th grade socio-economic questionnaire contains the most complete set of answers consistently collected across several waves of SARESP. Second, 9th grade students are at the end of compulsory schooling; hence, their answers regarding their aspirations for post-compulsory education are the most relevant in understanding dropout rates and school transition to secondary education.

a change in their perception about their likely performance.

In the next columns, we find that boys were 12% less likely to report that they are *interested in school activities*, while we find no effect on girls. We find a similar pattern for the effects on student effort, measured by their attitude towards homework. We find that boys are less likely report that they *do their homework in time* and are more likely report that they do not do homework at all, while we find the opposite effect for girls.[36]

We also look at additional outcomes related to student attitudes towards school. Boys and girls both less frequently agree that their *school is a nice place*, with a slightly larger coefficient for girls. Boys also less frequently report that they like being at schools, compared to girls as a response to violence exposure, but none of these estimates are statistically significant.

These results provide an intriguing angle on how exposure to homicides changes the aspirations and attitudes towards education differently for boys and girls. When exposed to homicides around school, boys change their attitude towards education and generally display less interest in further education, have a lower perception about their performance at school, and demonstrate lower effort directed towards school, whereas there is no negative effect for girls.

These results from the students are confirmed by the answers from the parent questionnaire. We report these outcomes in Table 8. Parents of boys report that their child is, on average, less interested in school when exposed to violence.[37] They report less frequently that their child likes school (not statistically significant), and less frequently report that their child *is doing well in school* (a reduction by 8% compared to the mean, significant at the 5% level), whereas we do not find any such negative effect of violence on girls reported by the parents. The estimates for girls are either very small or even of the opposite sign, but are not significantly different from zero.[38] Parents of boys also less frequently report that their child *studies at home*, confirming the reduction in the student self-reported engagement with homework. The estimated effect corresponds to a 24% reduction, significant at the 10% and 5% levels, for standard and Conley standard errors, respectively. There is no effect for girls. Parents also less frequently report that their child *does their homework in time* and more frequently that their child does *homework while watching TV* for boys. The coefficient for girls is effectively zero for *doing homework in time* and positive and of similar magnitude for *doing homework while watching TV*.

[36]We find that girls are more likely to report doing their homework while watching TV. These estimates are not conditional on doing homework. Indeed, about 9% of boys report not engaging in homework, compared to 5.5% for girls.

[37]This is on a scale from 0 (very negative) to 10 (very positive). The estimated effect for boys corresponds to a reduction by 5% of a standard deviation.

[38]We find no effect on good behaviour at school for boys, but a positive effect for girls; significant at the 10% level.

These results confirm the results based on the self-assessment of students. Exposure to violence systematically changes the aspirations and attitudes related to education for boys but does not negatively affect girls. The effects are particularly pronounced for variables more directly measuring current investments and input into education.

To test whether the findings presented in Table 8 on the attitudes of students observed by their parents are simply reflecting a change in the behaviour of their children or reflect a change in the attitude of the parents in response to homicides that differs by sex, we also investigate measures of parental involvement in the schooling of the children. We report the results in Table A14. Across a variety of variables measuring parental involvement, including *helping with their studies at home*, *participation in parent evening*, *talking about school*, and *following the child's homework* we do not find any significant effects of exposure to homicides.[39]

Lastly, we investigate how children report on how involved their parents are with their education. The estimates, reported in columns (9) to (12), on *parents helping with homework* and *asking about homework* show a significant difference between boys and girls. While there is a small and insignificant negative effect for boys on parental help with their homework, the effect is quite pronounced and significant for parents showing an interest by asking about their homework.[40] A caveat of these self-reported measures is that they may reflect both an objective change in parental involvement and a change in the perception of students of their parents' involvement.

Overall, these estimates reveal how the aspirations and attitudes of students assessed by themselves and their parents change differentially for boys and girls in response to homicide exposure. The differences between boys and girls along a number of measures of student attitudes and behaviour related to education are striking and consistent with the differences we report in terms of standardised test scores and attendance, in particular. In Section 7, we will investigate the potential underlying transmission channels of the main effects in light of these outcomes further.

5.4 Student progression

In addition to test score results and attendance, we are interested in student progression as additional educational outcomes and measures of human capital accumulation. We have these measures for a longer period, 2007 to 2013, and for all cohorts. Because we have information on the addresses of students in municipal schools and can investigate the effect of exposure to homicides

[39] There is some tendency for parents to be less involved in boy's education, for example in *helping children with their studies at home*, for which the difference between boys and girls is quite pronounced.

[40] Boys are 12% less likely to report that their parents ask about homework compared to the mean across all students.

not only around schools, but also around their residence, we focus the analysis for these outcomes on students from municipal schools. Linking individual school census records over time enables us to follow students as they progress through their educational careers. We are particularly interested in grade repetition, dropout rates, and the transition from primary to (non-compulsory) secondary education. Despite efforts to reduce grade repetition and dropout rates, for example through the introduction of automatic grade promotion policies, grade repetition and dropout rates in Brazil remain high. In our sample, 6% of students repeat any given grade, and about 10% of students drop out of school.[41]

Table 9 presents regression results of the effect of violence on these outcomes for all students in primary and secondary school, by place of exposure. *Panel A* and *Panel B* present the results for exposure around schools and around the residences of students, respectively.[42] *Repetition* is a dummy variable that indicates whether the student attends the same grade in the subsequent year. *Dropout* is a dummy variable that captures whether a student drops out of school during or at the end of the school year. We are also interested in the transition from primary to secondary school. The variable *school transition* indicates whether students progress to secondary school after completing compulsory education. Roughly 75% of students in our sample continue to secondary school.

The estimates for exposure to homicides around schools on repetition and dropout, presented in Panel A, are of the expected sign but are not statistically significant. The effect on school transition is very close to zero. Next, we investigate the effect of exposure to homicides around the residence of students.[43] While we find very small and insignificant effects for repetition and school transition in Panel B, we find a large positive effect on dropout, significant at the 10% level. Exposure to homicides increases the propensity to drop out of school by roughly 7 percentage points, an almost 50% increase compared to the mean. To improve precision of these estimates, we combine exposure around schools and residences in Panel C. Overall, we find a precisely estimated and sizeable increase in the dropout rate of about 42% compared to the mean. In Table A15 we also estimate

[41] This includes students that drop out of school temporarily and re-enrol at a later stage.

[42] For this purpose, we geocoded the addresses linked to the full eight-digit Brazilian postcode (*Código de Endereçamento Postal*). For confidentiality reasons, we are limited to the postcode information of student addresses, different from school addresses and the address of the occurrence of homicides, for which we have the full address details including full street addresses and postcodes. In the urban context of Brazil, these postcodes relate to a relatively small geographic area containing a block of houses. Geocoding these areas returns the centroid of these areas. Because of the measurement error that we introduce by the less precise geocoding, the results are likely subject to attenuation bias, and hence are possibly biased towards zero.

[43] We can estimate the effect of exposure to homicides around schools only for the school census outcomes because we do not have address information available for the SARESP sample, providing us with test score data and school attendance.

the effects separately for boys and girls, and confirm the differential effect of homicide exposure documented for test scores and attendance. The effect for boys is slightly more pronounced across the different specifications.

The results on dropout point to the long-lasting consequences of crime exposure on the human capital accumulation of children in Brazil. Given the stark consequences of dropout, these findings are in line with the negative effect on self-reported aspirations to continue to post-compulsory education, indicating a substantial shift away from further human capital investments as a consequence of homicide exposure.

6 Results - Exposure on the Residence-School Path

Our measures for exposure so far are focused on homicides around schools and the residences of students. Having established an effect of homicide exposure around the school and residence of students on measures of student progression, in particular dropout rates, we would like to investigate further how exposure on the way from the residence to school affects these outcomes. Exposure on the path from the residence to school may be particularly salient, as students would very likely observe the presence of police and emergency services after an occurrence of violent crime, such as a homicide where the victim is assaulted in the public way.[44] To do so, we built corridors as outlined in Section 4 using Google APIs to construct the path based on the shortest distance between the school and residence for each student. Along the walking path line, we construct polygons of 50 m width (25 m to each side of the walking path), which we refer to as *corridors*. We then create a count of the number of homicides occurring within each corridor in a given year. In addition to the walking path, we create alternative corridors based on the shortest driving path and the shortest path using public transport. We focus on educational outcomes presented in the previous section, for which we can build the corridor data.

The results are presented in Table 10. Panel A presents the outcomes for the walking path. Consistent with the estimates for exposure around schools and the residence, we find small and insignificant effects on repetition and school transition. We also find a sizeable and positive effect on dropout rates, which is insignificant at conventional levels. To boost precision, we widen the corridors. As we ultimately do not know which way students actually take, this will more likely capture any exposure to homicides on the path from the residence to school (and vice versa). We

[44] Unfortunately, our violence data, which are based on death certificates, do not contain information on the time of occurrence, which we would have liked to use to concentrate on homicides occurring during likely school commuting periods.

illustrate this in Figure A5. Doing so may dilute the effect in line with the dilution documented for the school radius. As expected, the effect sizes reduce slightly, but we gain by having more precise estimates. We find that an additional homicide in the 100 m width corridor, leads to an increase in the propensity to drop out of 17%, compared to the baseline.

In Panels B and C, we investigate the effect for alternative definitions of the residence-school path for driving and public transport. While the Google Maps API uses the respective algorithm to identify the driving and public transport path, we not necessarily assume that these would more likely affect students actually driving to school or using public transport, but see these as alternative paths to school.[45] Using these alternative corridors, we find a very consistent positive effect of very similar magnitude on the effects for the walking path on student dropout rates. For the 50 m width corridor, we find that one additional homicide leads to an increase in the dropout rate of about 4%, or 26% compared to the mean. Widening the corridors reduces the effect size slightly but helps with precision. This constitutes a substantial and economically meaningful increase in dropout rates as result of exposure to violence on the school path and confirms the findings for exposure around schools and residences.

We repeat the exercise focussing on the 50 m width corridors separately for boys and girls in Table A17. We again find much more pronounced effects on dropout for boys than girls, consistent with the above findings on dropout and the findings on other schooling outcomes and students' aspirations and attitudes.

Across the different corridor definitions, we find throughout a negative effect of exposure to homicides in the different corridors (and different widths) on school transition. As we have a substantially smaller sample based on final-year students, for which we can estimate the effect on transitioning to secondary school, the estimates are noisier and not statistically significant. These are nevertheless of economically meaningful magnitude, with an additional homicide leading to a reduction of students enrolling in secondary school between half and just over 1 percentage point, a decrease of between 8% and 18% of students compared to the mean transition rate. Increasing the width of the corridor again reduces the magnitude of the estimates in line with expectations.

[45]The driving path may, for example, constitute the safest path to go to school, avoiding shorter but possibly less safe walking paths to school; hence, students may actually walk on this route to school and back.

7 Transmission Channels

Exposure to violence may affect educational outcomes through a number of potential channels, where the relative importance of each of these channels likely differs depending on the context. In the case of violence related to conflict, as in Brück et al. (2019) or conflict-like scenarios as in Monteiro and Rocha (2017), the disruption of school supply is likely to affect the quality of the learning environment and hence educational outcomes. The context in our paper differs considerably from the conflict background in Brück et al. (2019) and Monteiro and Rocha (2017) by focussing on day-to-day violence. We therefore first want to test the relevance of a transmission channel related to school supply. Furthermore, we also want to rule out that the effects we document in this paper are driven by a bereavement effect, where students are affected from the direct loss of a relative or friend. Although these effects may be relevant in itself, the very specific transmission mechanism would make the results difficult to generalise to other contexts. Lastly, a potential transmission channel that has received much less attention is related to the theoretical connection between crime and human capital investments (Soares (2010)). This channel may work through reduced expected life-span similar to Jayachandran and Lleras-Muney (2009) and Oster et al. (2013) or more generally through increased uncertainty about the future. We investigate in this section whether our findings are compatible with this human capital investment channel.

7.1 Teacher attendance and school supply

In the paper by Monteiro and Rocha (2017), the authors argue that their negative effects of exposure to gang battles on test scores of students in schools in the proximity to favelas in Rio de Janeiro are driven by effects on the supply of schooling, including higher teacher absenteeism, principal turnover, and temporary shutdowns. In line with these findings, Brück et al. (2019) also provided evidence that conflict affects school supply through the deterioration of the school infrastructure. Although our context is very different from that of Monteiro and Rocha (2017) and Brück et al. (2019), which focus on conflict situations, where our focus is on day-to-day violence, we investigate this potential transmission channel. Having documented that exposure to violence reduces attendance of students at school, as teachers are also exposed to the violence around the school, we test whether exposure to violence affects teacher attendance. We create the teacher attendance rate based on the daily attendance records of teachers that we have available. We report the coefficient in Table 11, column (3). We find no evidence that exposure around schools reduces teacher attendance. The coefficient is extremely small and not statistically significant. Alternatively, we can test how much teacher attendance affects the coefficients on test scores estimated in Table 2. We include teacher attendance as an additional control in specifications in columns (3) and (6) of Table 2. The difference in the coefficients when including teacher attendance is minimal (results available upon request).

We also investigate whether homicide exposure may lead to other forms of disruption in the school routine, for example through higher teacher or principal turnover. We find no effect on either. These results are contrary to those of Monteiro and Rocha (2017) who stated that the effect of exposure to drug battles on educational outcomes is partially caused by teacher absenteeism and turnover but is not unexpected, as their definition of violence exposure is closer to the conflict scenario of the Palestine conflict in the study by Brück et al. (2019). Given the evidence presented in this section, we believe we can rule out that the effects on achievement are caused by lower teacher attendance or disruption in the provision of schooling caused by teacher or principal turnover.

7.2 Bereavement effect

To check whether the effect we find is driven by grief due to the death of a peer student, a sibling or a friend (who may live in the same neighbourhood, but may not attend the same school) we drop from the explanatory variable all the victims who are 18 years old or younger. We present the results in Panel B of Table 5; the specification for all entries follows the most satiated specification of columns (2) and (5) of Table 2. Column (1) shows the effect of homicides around the school including all the victims. In column (2), we exclude all 18-year-old or younger victims. Column (3) considers only male victims in the explanatory variable and column (4) only gunshot victims. Results do not differ in any meaningful way indicating that the negative effects on test scores are not caused by grief for a peer, friend or sibling.[46]

In addition, we can investigate further whether any of the victims, either a student at the same school or a teacher, are among any of the homicide victims in our dataset by using information from attendance records from the São Paulo State Secretariat of Education. These data contain information about deceased individuals. For each deceased student or teacher, we identified the cause of death by linking these data with information on death records from *Datasus*. From the student data, we identified 501 deceased students in the period of 2010 to 2013. To be able to

[46]As we constrain the sample of homicide victims to the ages 19 and older, we assume that any peer student, sibling or friend would not be older than 18 for this exercise.

identify the cause of death, we had to drop 10 observations with the same year of death, sex, and date of birth. From the 491 left, we could successfully identify the cause of death of 347 cases. From those, 38 cases were victims of homicides, but only four of them happened in the public way. None of these four cases occurred in the proximity of schools and hence were not included in our explanatory variable. We repeated the same exercise for the teachers. From 2010 to 2013, we identified 131 deceased teachers, and we could identify the cause of death of 43 of the cases. From those cases, only one of them was a homicide victim; however, the homicide did not happen in the public way. Taken together, we are hence confident that the effects are not due to grief or bereavement for deceased peers or teachers of the students in our dataset.

7.3 Human capital investments

An extensive literature has documented the role of life expectancy for the human capital investment decisions of individuals from a theoretical and empirical perspective (Becker (1964), Ben-Porath (1967), Oster et al. (2013)). There also exists a small literature on how changes to life expectancy that differ by sex, such as health and violence shocks, affect investments in education by sex. Jayachandran and Lleras-Muney (2009) use the rapid reduction in maternal mortality linked to the introduction of sulfa drugs in Sri Lanka to document the effect of life expectancy on human capital investments for girls. Gerardino (2015) showed that, when male-biased violence is high, as measured by homicides rates in Colombian municipalities, boys are less likely to enrol in secondary school relative to girls, possibly due to a reduction in the returns to education.

In Brazil, homicide is a leading cause of death for boys up to their mid-twenties and the vast majority of homicide victims are male.[47] Exposure to homicides may therefore change the perception of safety of males and females differentially, and hence possibly diminish the perceived returns to education for boys more than for girls. Alternatively, the effect may work more generally through an increase in uncertainty linked to homicide exposure that is more pronounced for boys than for girls. In Section 5, we find that the effects on test scores, attendance, and dropout are substantially larger for boys, suggesting that boys react more strongly to the homicide exposure in the school surroundings as suggested by the human capital transmission channel.

Any effect that works through investment decisions should extend beyond a short-term effect on test scores. In Table 5 we showed that the effects on test scores were not driven by homicides close to the test date, and are hence unlikely driven by the short-term stress or short-term effect

[47] Indeed, more than 90% of victims are male, as shown in Table 1.

on the well-being of students. Indeed, we found that the effects were even more pronounced when considering only homicides in the first term, at least 6 month prior to the SARESP test date. Such a longer-term effect is consistent with an underlying channel related to human capital investments, where exposed students alter their investments in their human capital. Different from Monteiro and Rocha (2017), who find no evidence for the persistence of the effects on test scores, we also document fundamental changes in human capital accumulation that go beyond short-run effects on test scores. Particularly, the effects on dropout indicate fundamental changes in the human capital accumulation of school children with the potential long-term consequences in the labour market.

A transmission channel based on the changes in the incentives for boys and girls to invest in human capital is also consistent with the differential effects on aspiration and attitudes for boys and girls presented in Subsection 5.3. The finding on boys much less likely to report intending to go to university is the most manifest indication that homicide exposure affect the long-term investments in human capital for boys, but not to the same extent for girls. While it is difficult to test this channel more directly against other channels, taken together, the pronounced effects for boys and the persistence of the effects on achievement, attendance, dropout and the educational aspiration and attitude presented in this paper are an indication that exposure to homicides may affect the incentives to invest in human capital, and boys being disproportionally affected by this.

8 Final Remarks

This paper uses georeferenced data on homicides for Brazil and links these data with measures of school performance to estimate the causal effect of exposure to violence on schooling outcomes and human capital accumulation. We find that students exposed to violence perform worse in math and Portuguese language tests. We find that one additional homicide during the school year leads to a 4.6% of a standard deviation reduction in math and a 5.5% of a standard deviation reduction in language test scores. The results are robust to the inclusion of school-specific time trends and to various checks for selection, spatial correlation, and different specifications. We create indicator variables that identify that, over the entire test score distribution, students are affected by homicide exposure. The shift in the distribution is nevertheless more pronounced for below the median in the test score distribution. We use very rich information on the student background and find that the effects are particularly pronounced among students from relatively poorer families, possibly suggesting that income works as a buffer against the negative effect of crime. We also show that the estimated effects are particularly strong for boys, both for test scores and attendance.

Violence around school also affects the attendance of the students at school. Our estimates show that one additional homicide in the year increases absences by around 1%. We nevertheless find that absenteeism can only explain a fraction of the negative effects on the performance measured by standardised tests.

In addition, we examine exposure to violence in the school path from the residence to school. We use Google APIs and design an algorithm to build corridors along the path line from residences to school and examine exposure to homicides in these corridors. Within-corridor estimates show that dropout rates increase after exposure to homicides in the school path. The results are robust to different specifications, such as corridor width and distinct corridors - walking, driving, and public transport.

We use extremely rich information on student educational aspirations and attitudes to investigate a number of potential underlying mechanisms. We provide suggestive evidence that exposure to homicides may deteriorate incentives to invest in human capital for boys, who are most likely to be victimised in homicides. We show that the results are not driven by changes in the supply of schooling induced by homicides, for example, by changes in the attendance and turnover of teachers and principals. We also show that the effects are not driven by bereavement for the death of a friend or a teacher.

These results are important to quantify some of the costs of day-to-day violence that go beyond the cost of direct victimisation and have so far being neglected in cost estimates. Improved cost estimates are important for the design of optimal policies targeting crime and violence, including on the prevention of crime. The negative effects we find on measures of school performance, in particular dropout, suggest that violence affects human capital accumulation, possibly leading to long-lasting consequences for the affected children. Since poor neighbourhoods are often more violent, violence is potentially one additional contributor for the socio-economic gradient we observe in many low- and middle-income countries plagued with high crime rates. Because the effects are more concentrated among boys, exposure to violence may also be a contributing factor to the reversed gender gap in education observed in Brazil and other Latin American countries.

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Table 1: Homicides characteristics

	Homicide victims characteristics	
	Mean	Std.Dev.
Age		
02-10	0.003	0.053
11-15	0.021	0.143
16-18	0.076	0.265
19-25	0.264	0.441
26-30	0.191	0.394
31-40	0.254	0.435
41-50	0.131	0.337
50+	0.060	0.238
Demographics		
Male	0.924	0.265
White	0.420	0.494
Black	0.103	0.304
Mixed	0.453	0.498
Single	0.639	0.480
Married	0.125	0.330
Separated	0.026	0.158
Education		
None	0.013	0.113
01-03 years	0.092	0.290
04-07 years	0.386	0.487
08-11 years	0.270	0.444
12+ years	0.033	0.179
	Homicide characteristics	
	Number	Percent
Assault by gun discharge	1,709	69.190
Assault by sharp object	273	11.053
Assault by blunt object	256	10.364
Assault by bodily force	137	5.547
Assault by other means	95	3.846
Total	2,470	100.000

Notes: The table includes all homicides for which the death occurs in the public way in São Paulo over the period of 2007 to 2013, which were geocoded at the street level.

Table 2: Effect of exposure to violence around the school on academic achievement

	Math proficiency			*Language proficiency*		
	(1)	(2)	(3)	(4)	(5)	(6)
Homicides	−2.745	−2.289	−3.307	−2.644	−2.138	−2.739
	(2.512)	(1.105)**	(1.350)**	(2.858)	(1.085)**	(1.413)*
	[1.773]	[0.850]***	[0.985]***	[1.869]	[0.872]**	[0.991]***
Observations	676,082	676,082	676,082	675,733	675,733	675,733
Controls	No	Yes	Yes	No	Yes	Yes
School / time	Yes	Yes	Yes	Yes	Yes	Yes
School x time	No	No	Yes	No	No	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables *Math proficiency* and *Language proficiency* are math and Portuguese standardised test scores normalised at a (250,50) scale. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. **Individual controls** are age, sex and race fixed effects, grade, dummies indicating whether at home the student has access to daily newspaper, magazines, dictionary, novels, poetry and short stories books and encyclopaedias; commuting time from residence to school; age, race, education and employment status of the father and the mother; income and number of people in the house; if parents own the house or rent it; if the house has supply of energy, water, gas, sewage and garbage collection; number of tv's, radios, bathrooms, cars, maids, vacuum cleaners, washing machines, dvd's, fridges, freezers, telephone, computers, cable tv, microwave and internet. **Teacher controls** are sex, age and race of the Portuguese and math teachers. **School controls** are number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, sports court, teachers' room, principal's room, internet connection and if the school offers school meals. **Classroom controls** are share of black students, share of girls and share of students above the appropriate age.

Table 3: Effect of exposure to violence around the school on academic achievement - by neighbourhood crime level

	Math proficiency			*Language proficiency*		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	*Low*	*High*	*All*	*Low*	*High*
Homicides	−2.289	−3.555	−0.183	−2.138	−2.940	0.134
	(1.105)**	(1.794)**	(1.509)	(1.085)**	(2.061)	(1.253)
	[0.850]***	[1.407]**	[1.229]	[0.872]**	[1.390]**	[1.142]
Observations	676,082	426,653	249,429	675,733	426,709	249,024
School / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. *Low* and *High* refer to crime levels in the neighbourhoods surrounding the schools. We consider a 500 m radius from school and identify schools ever exposed to homicides during the period of 2007 to 2013; then we subtract exposure in the 25 m radius during the period of analysis (2010 to 2013), and classify as *Low* level when the count of homicides is less than or equal to the median and *High* level when it is higher than the median. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables *Math proficiency* and *Language proficiency* are math and Portuguese standardised test scores normalised at a (250,50) scale. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table 4: Effect of exposure to violence around the school on academic achievement - levels of proficiency

	Math proficiency					*Language proficiency*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Very low	*Low*	*Median*	*High*	*Very high*	*Very low*	*Low*	*Median*	*High*	*Very high*
Homicides	0.010	0.014	−0.022	−0.012	−0.006	0.010	0.019	−0.014	−0.008	−0.005
	(0.010)	(0.011)	(0.010)**	(0.009)	(0.005)	(0.008)	(0.010)*	(0.010)	(0.008)	(0.005)
	[0.007]	[0.007]**	[0.008]***	[0.008]	[0.005]	[0.006]*	[0.007]***	[0.008]*	[0.007]	[0.004]
Observations	676,082	676,082	676,082	676,082	676,082	675,733	675,733	675,733	675,733	675,733
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. *Very low* and Low are students in the 10 and 25 percentiles of test scores distribution; and *High* and *Very high* students in the 75 and 90 percentiles. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table 5: Effect of exposure to violence around the school on academic achievement - timing and groups of victims

Panel A: Homicide timing

	Math proficiency					*Language proficiency*				
	(1) *All homicides*	(2) *Excluding last two weeks*	(3) *Excluding last month*	(4) *Excluding 2nd semester*	(5) *Homicides lead*	(6) *All homicides*	(7) *Excluding last two weeks*	(8) *Excluding last month*	(9) *Excluding 2nd semester*	(10) *Homicides lead*
Homicides	−2.289	−2.289	−2.515	−3.073	−0.924	−2.138	−2.138	−2.372	−2.926	0.556
	(1.105)**	(1.105)**	(1.138)**	(1.179)***	(1.565)	(1.085)**	(1.085)**	(1.188)**	(1.264)**	(1.111)
	[0.850]***	[0.850]***	[0.952]***	[0.973]***	[1.365]	[0.872]**	[0.872]**	[0.939]**	[0.990]***	[1.045]
Observations	676,082	676,082	676,082	676,082	534,837	675,733	675,733	675,733	675,733	534,573
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Groups of victims

	Math proficiency				*Language proficiency*			
	(1) *All victims*	(2) *18+ yr old victims*	(3) *Male victims*	(4) *Gunshot victims*	(5) *All victims*	(6) *18+ yr old victims*	(7) *Male victims*	(8) *Gunshot victims*
Homicides	−2.289	−2.322	−2.808	−3.000	−2.138	−2.304	−2.477	−2.724
	(1.105)**	(1.167)**	(1.106)**	(1.396)**	(1.085)**	(1.136)**	(1.124)**	(1.399)*
	[0.850]***	[0.888]***	[0.865]***	[1.053]***	[0.872]**	[0.915]**	[0.839]***	[1.009]***
Observations	676,082	676,082	676,082	676,082	675,733	675,733	675,733	675,733
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable Homicides corresponds to the number of homicides within a 25 m radius from school. **In Panel A**, in columns (2) and (7) we exclude from the explanatory variable homicides in the two-week window prior to the test dates; in columns (3) and (8) we exclude from the explanatory variable homicides one month prior to the test; in columns (4) and (9) we exclude from the explanatory variable all homicides occurring in the second school term; in columns (5) and (10) we used homicides lead as explanatory variable. **In Panel B**, in columns (2) and (6) we exclude from the explanatory variable homicides victims younger than 18 years old; in columns (3) and (7) we exclude from the explanatory variable female victims; in columns (4) and (8) we include in the explanatory variable only gunshot victims. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table 6: Effect of exposure to violence around the school on attendance

	Attendance (year)	*Attendance (1st semester)*	*Attendance (2nd semester)*
	(1)	(2)	(3)
Homicides (year)	−0.010 (0.005)** [0.004]**		
Homicides (1st semester)		−0.010 (0.004)** [0.004]**	
Homicides (2nd semester)			−0.021 (0.005)*** [0.007]***
Mean	0.879	0.888	0.870
Observations	709,386	709,386	709,386
School / time	Yes	Yes	Yes
Controls	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Dependent variables are the attendance rates in the year and in each semester. Explanatory variables *Homicides (year)* corresponds to the number of homicides within a 25 m radius from school in the entire year; *Homicides (1st semester)* and *Homicides (2nd semester)* are the number of homicides within a 25 m radius from school in the first and second semesters. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table 7: Effect of exposure to violence around school on **student reported** outcomes

	I intend to go to university		*I am a good student*		*I like school activities*		*I do my homework on time*		*I do my homework watching TV*		*I do not do homework*		*My school is a nice place*		*I like being at school*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	−0.034	0.024	−0.054	0.020	−0.035	−0.013	−0.007	0.030	−0.004	0.010	0.003	−0.006	−0.005	−0.018	−0.003	−0.002
	(0.022)	(0.016)	(0.029)*	(0.018)	(0.017)**	(0.026)	(0.017)	(0.017)*	(0.020)	(0.016)	(0.009)	(0.011)	(0.027)	(0.043)	(0.019)	(0.023)
	[0.017]**	[0.014]	[0.021]***	[0.013]	[0.014]**	[0.018]	[0.014]	[0.013]**	[0.016]	[0.014]	[0.009]	[0.008]	[0.020]	[0.029]	[0.014]	[0.020]
Mean	0.417	0.618	0.393	0.459	0.288	0.240	0.270	0.299	0.216	0.232	0.088	0.055	0.228	0.160	0.268	0.256
Observations	97,700	104,882	99,250	106,153	98,781	105,892	96,970	104,414	96,838	104,069	97,037	104,305	99,837	106,434	98,167	105,194
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 9th grade of primary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables are students' answers to a socio-economic questionnaire collected by the school. *I am planning to go to university* is a dummy equal to one if the student answers she wants to keep studying, graduate from high school and go to university; and zero otherwise. *I am a good student*, *I like school activities*, *My school is a nice place* and *I like being at school* are a dummies equal to one if students completely agree with the statements and zero otherwise. *I do my homework on time*, *I do my homework watching TV* and *I do not do my homework* are dummies equal to one if the student answers she always does that and zero otherwise. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table 8: Effect of exposure to violence around school on **parent reported** outcomes

	Child's interest in school		*My child likes school*		*My child is doing well in school*		*My child behaves at school*		*My child studies at home*		*My child does homework on time*		*My child does homework watching TV*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	−0.124	0.011	0.008	0.023	−0.032	0.012	−0.020	0.004	−0.036	−0.005	−0.024	0.002	0.022	0.020
	(0.084)	(0.079)	(0.020)	(0.021)	(0.017)*	(0.016)	(0.015)	(0.019)	(0.015)**	(0.019)	(0.026)	(0.019)	(0.020)	(0.018)
	[0.065]*	[0.071]	[0.017]	[0.017]	[0.014]**	[0.016]	[0.013]	[0.016]	[0.011]***	[0.015]	[0.019]	[0.015]	[0.015]	[0.016]
Mean	6.880	7.482	0.403	0.369	0.390	0.473	0.461	0.567	0.148	0.214	0.287	0.313	0.355	0.410
Observations	97,219	104,126	93,003	100,922	94,510	102,346	92,355	100,574	98,733	105,632	88,648	98,032	89,638	99,878
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 9th grade of primary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables are parents' answers to a socio-economic questionnaire collected by the school. *My child is interested in school* is a rating of child's interest in school by the parents, ranging from 0 -very negative- to 10 -very positive. *My child likes school*, *My child is doing well in school*, *My child behaves at school*, *My child studies at home*, *My child does homework on time* and *My child does homework watching TV* are dummies equal to one if parents completely agree with the statements and zero otherwise. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table 9: Effect of exposure to violence on student progression

Panel A: Around the school

	Repetition	*Dropout*	*School transition*
	(1)	(2)	(3)
Homicides	0.007	0.034	0.005
	(0.012)	(0.022)	(0.046)
Mean	0.047	0.152	0.739
Observations	2,088,720	2,467,920	287,304
R^2	0.054	0.074	0.086
School / time	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Panel B: Around the residence

	Repetition	*Dropout*	*School transition*
	(1)	(2)	(3)
Homicides	−0.004	0.073*	−0.002
	(0.004)	(0.044)	(0.015)
Mean	0.047	0.150	0.723
Observations	1,981,436	2,334,286	244,302
R^2	0.059	0.082	0.145
School/neighb/time	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Panel C: Around the residence and the school

	Repetition	*Dropout*	*School transition*
	(1)	(2)	(3)
Homicides	−0.000	0.063**	0.012
	(0.005)	(0.031)	(0.018)
Mean	0.047	0.150	0.723
Observations	1,981,436	2,334,286	244,302
R^2	0.059	0.082	0.145
School/neighb/time	Yes	Yes	Yes
Controls	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered (at the school level in Panel A and at the neighbourhood level in Panels B and C) in parentheses.

Notes: The analysis includes students from the 1st grade of primary school to the 3rd grade of secondary school, over the period of 2007 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school in Panel A, from residence in Panel B and from school and residence in Panel C. Dependent variable *Repetition* is a dummy variable which indicates whether the student has to repeat the same grade as the current year in the coming year. *Dropout* is a dummy variable which captures if the student drops out school in the successive year. *School progression* indicates if the students in the last grade of primary school progress to secondary school in the subsequent school year, for that reason, regressions for this outcome include only students at the final grade of primary school. All regressions include time fixed effects. Controls include individual characteristics, school characteristics and classroom composition. **Individual controls** are age, sex and race fixed effects. For a detailed list of school and classroom controls, refer to Table 2 notes.

Table 10: Effect of exposure to violence on the residence-school path on student progression

Panel A: Walking

	Repetition		*Dropout*		*School transition*	
	(1)	(2)	(3)	(4)	(5)	(6)
	50m width	*100m width*	*50m width*	*100m width*	*50m width*	*100m width*
Homicides	0.003	−0.000	0.032	0.025*	−0.006	−0.003
	(0.004)	(0.003)	(0.024)	(0.014)	(0.017)	(0.012)
Mean	0.047	0.047	0.150	0.150	0.724	0.724
Observations	1,876,928	1,876,928	2,210,087	2,210,087	231,184	231,184
R^2	0.060	0.060	0.082	0.082	0.140	0.140
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Driving

	Repetition		*Dropout*		*School transition*	
	(1)	(2)	(3)	(4)	(5)	(6)
	50m width	*100m width*	*50m width*	*100m width*	*50m width*	*100m width*
Homicides	0.005	0.000	0.039*	0.033**	−0.014	−0.007
	(0.003)	(0.002)	(0.021)	(0.013)	(0.015)	(0.011)
Mean	0.047	0.047	0.150	0.150	0.724	0.724
Observations	1,876,928	1,876,928	2,210,087	2,210,087	231,184	231,184
R^2	0.060	0.060	0.082	0.082	0.140	0.140
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Public transport

	Repetition		*Dropout*		*School transition*	
	(1)	(2)	(3)	(4)	(5)	(6)
	50m width	*100m width*	*50m width*	*100m width*	*50m width*	*100m width*
Homicides	0.004	−0.001	0.039*	0.032**	−0.005	−0.001
	(0.003)	(0.003)	(0.023)	(0.014)	(0.016)	(0.012)
Mean	0.047	0.047	0.150	0.150	0.724	0.724
Observations	1,876,928	1,876,928	2,210,087	2,210,087	231,184	231,184
R^2	0.060	0.060	0.082	0.082	0.140	0.140
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the corridor level in parentheses.

Notes: The analysis includes students from the 1st grade of primary school to the 3rd grade of secondary school over the period of 2007 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within corridors of 50 m and 100 m width. Dependent variable *Repetition* is a dummy variable which indicates whether the student has to repeat the same grade as the current year in the coming year. *Dropout* is a dummy variable which captures if the student drops out school in the successive year. *School progression* indicates if the students in the last grade of primary school progress to secondary school in the subsequent school year, for that reason, regressions for this outcome include only students at the final grade of primary school. All regressions include time and corridor fixed effects. Controls include individual characteristics, school characteristics and classroom composition. **Individual controls** are age, sex and race fixed effects. For a detailed list of school and classroom controls, refer to Table 2 notes.

Table 11: Effect of exposure to violence around school on school supply

	Teacher turnover	*Principal turnover*	*Teacher attendance*
Homicides	0.042	0.001	−0.001
	(0.048)	(0.001)	(0.001)
	[0.037]	[0.002]	[0.001]
Observations	92,873	2,385	124,715
Mean	0.285	0.015	0.951
Controls	Yes	Yes	Yes
School / time	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes teachers over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables *Teacher turnover* and *Principal turnover* measure if teachers/principals do not appear in the school system in the following year. *Teacher attendance* is teacher's attendance rate in the school year. All regressions include time and school fixed effects. Controls for regressions on teacher turnover and attendance include individual characteristics and school characteristics. Controls for regressions on principal turnover are school characteristics. **Individual controls** are age, sex and race fixed effects. For a detailed list of school controls, refer to Table 2 notes.

Annex

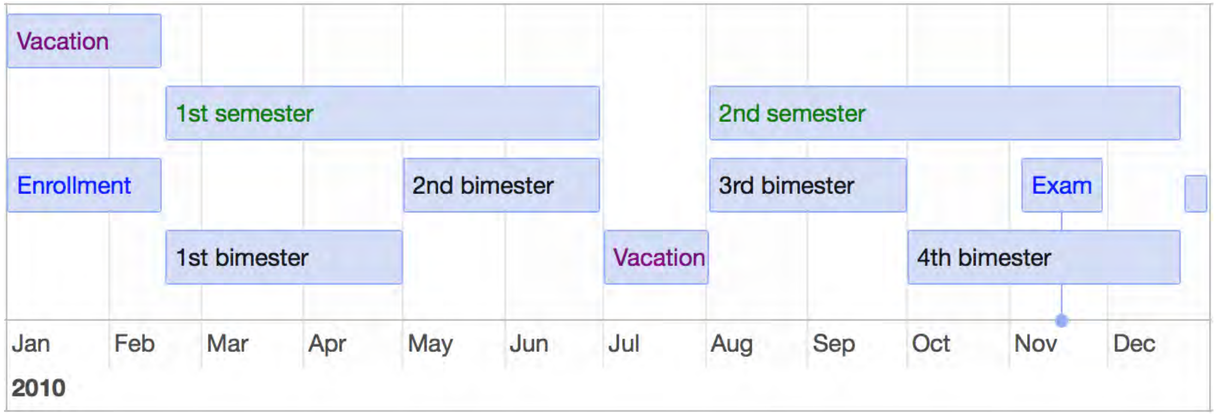


Figure A1: School Calendar in São Paulo

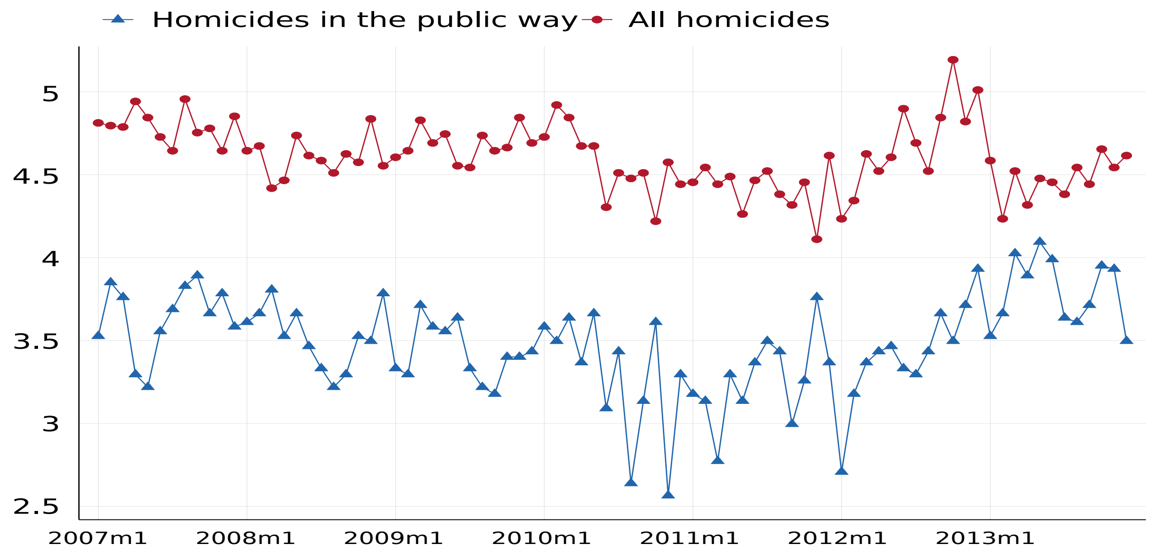
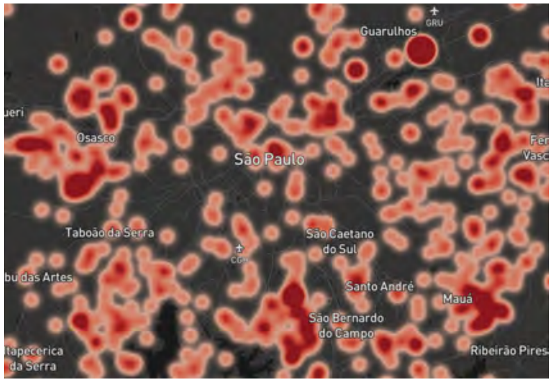
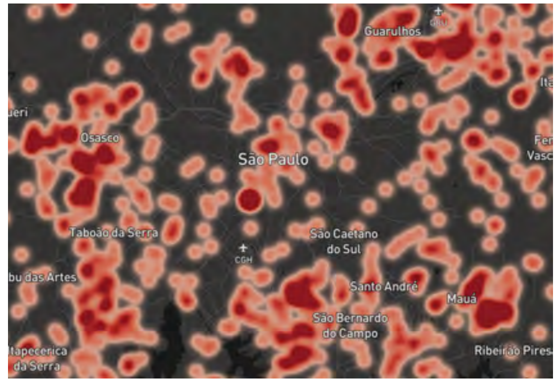


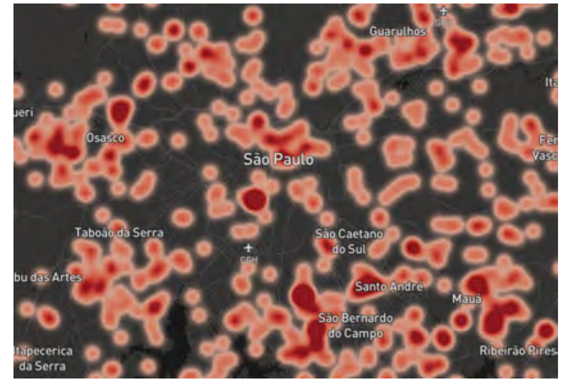
Figure A2: Homicide rates



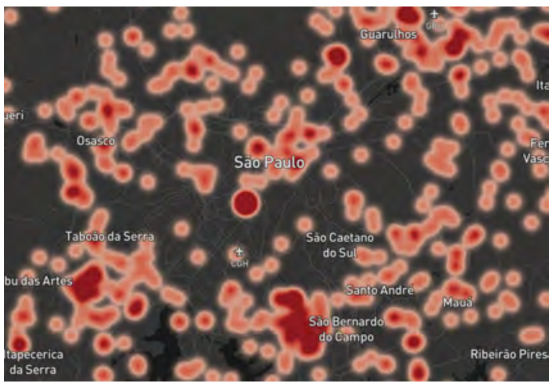
(a) 2007



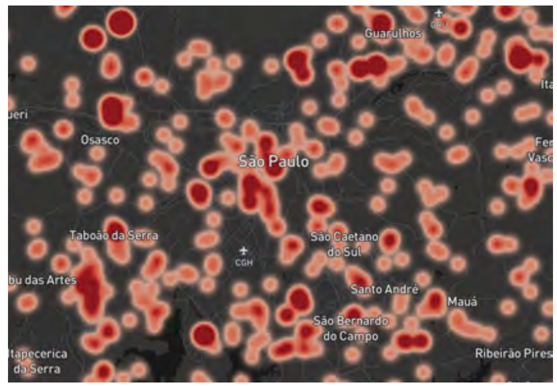
(b) 2008



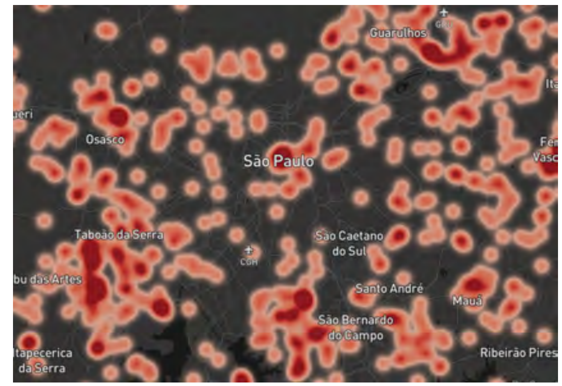
(c) 2009



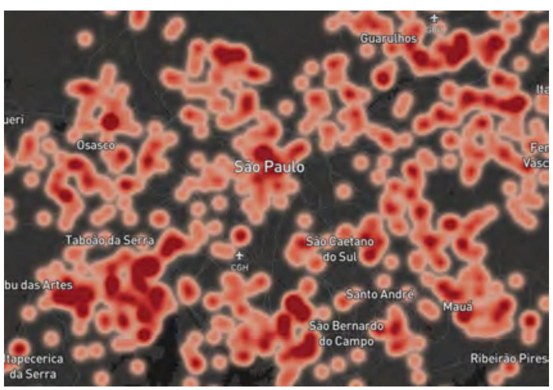
(d) 2010



(e) 2011

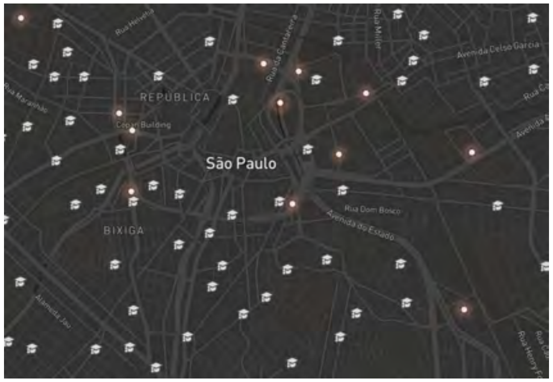


(f) 2012

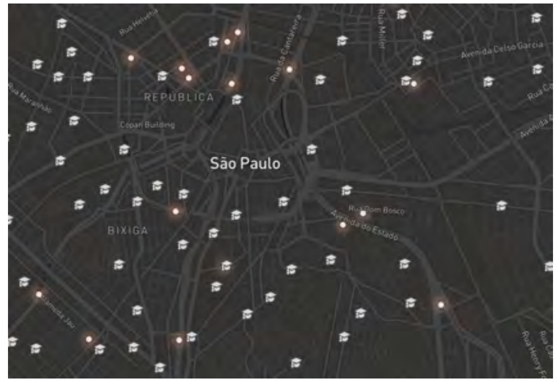


(g) 2013

Figure A3: Homicides in the public way in São Paulo



(a) 2007



(b) 2008

(c) 2009

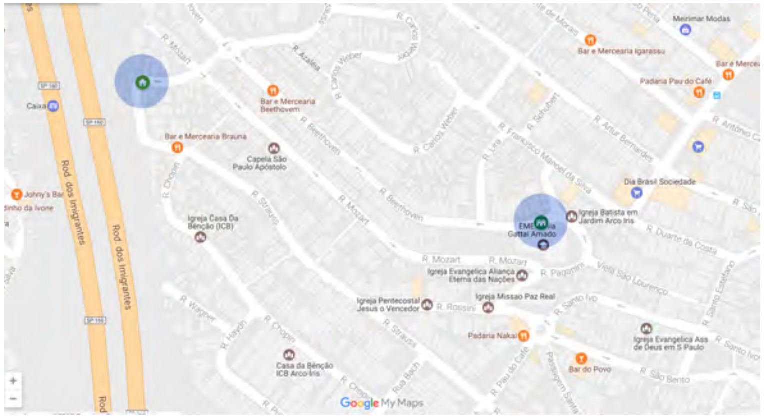
(d) 2010

(e) 2011

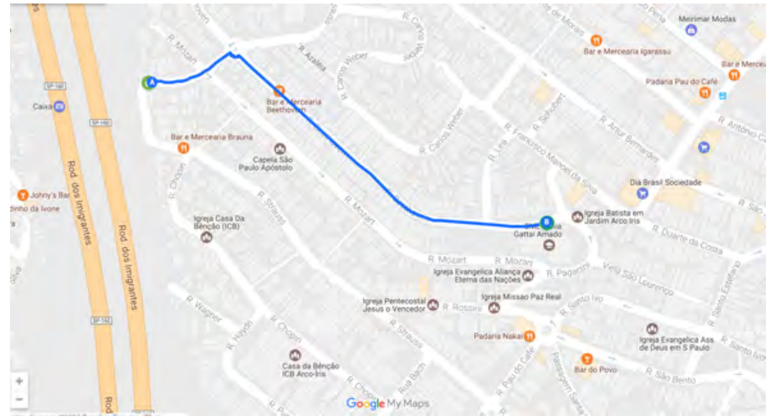
(f) 2012

(g) 2013

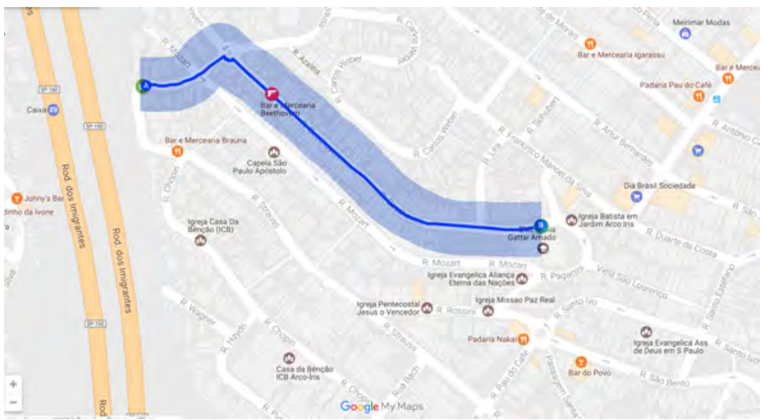
Figure A4: Homicides and schools in a São Paulo neighbourhood



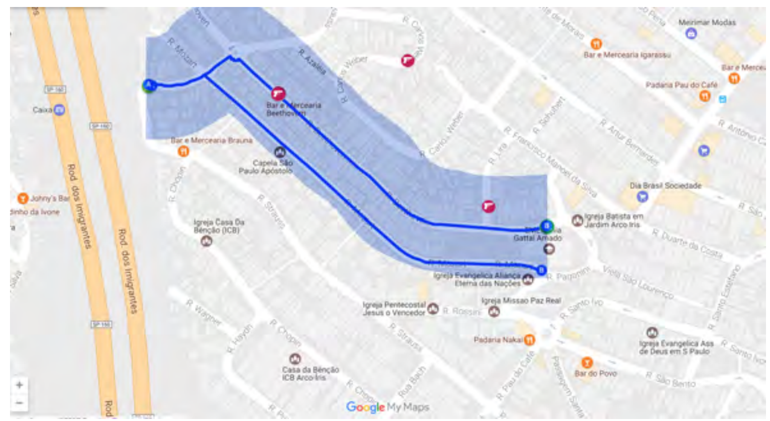
(a) School and residence radius



(b) Shortest walking distance from residence to school



(c) Corridor 1



(d) Corridor 2

Figure A5: Walking path from residence to school - Corridors

Table A1: Balancing tests

	Ever exposed	Never exposed	Diff.	Std. Error
Students characteristics				
Age	13.8946	13.3430	-0.5516	0.5070
Female	0.5148	0.5064	-0.0084	0.0544
White	0.5328	0.6133	0.0805	0.0555
Black	0.0604	0.0497	-0.0107	0.0210
Mixed	0.3975	0.3249	-0.0726	0.0522
Income per capita	388.4261	442.9101	54.4841	49.6501
Own home	0.4969	0.4309	-0.0661	0.0627
Rent home	0.5031	0.5691	0.0661	0.0627
Father's education: low	0.6455	0.5584	-0.0871	0.0643
Father's education: mid	0.2513	0.3034	0.0522	0.0599
Father's education: high	0.0404	0.0766	0.0361	0.0420
Mother's education: low	0.5884	0.5243	-0.0640	0.0646
Mother's education: mid	0.3369	0.3516	0.0146	0.0598
Mother's education: high	0.0484	0.0950	0.0465	0.0486
Father's employment: has a job	0.4192	0.3619	-0.0574	0.0543
Father's employment: has a temp. job	0.1510	0.1260	-0.0250	0.0348
Father's employment: has no job	0.0336	0.0256	-0.0080	0.0117
Mother's employment: has a job	0.3578	0.3104	-0.0473	0.0524
Mother's employment: has a temp. job	0.1226	0.0987	-0.0240	0.0307
Mother's employment: has no job	0.1225	0.0938	-0.0287	0.0273
Travel time from home to school (in min.)	34.5827	34.7566	0.1739	1.9143
Number of people in the house	4.4689	4.4240	-0.0449	0.1999
Has at home: newspapers	0.2163	0.2328	0.0164	0.0532
Has at home: magazines	0.3309	0.3485	0.0175	0.0590
Has at home: dictionary	0.8762	0.8545	-0.0217	0.0437
Has at home: books	0.8284	0.8126	-0.0158	0.0450
Has at home: scientific books	0.7632	0.7490	-0.0142	0.0533
Has at home: water supply	0.9725	0.9685	-0.0040	0.0223
Has at home: sewage supply	0.8639	0.8831	0.0192	0.0406
Has at home: electricity supply	0.9638	0.9729	0.0090	0.0185
Has at home: gas supply	0.2099	0.2721	0.0622	0.0603
Has at home: waste collection	0.9217	0.9307	0.0090	0.0266
Has at home: television	0.9646	0.9604	-0.0042	0.0244
Has at home: radio	0.8045	0.8122	0.0077	0.0465
Has at home: bathroom	0.9092	0.9153	0.0061	0.0311
Has at home: car	0.4479	0.5042	0.0562	0.0641
Has at home: maid	0.0749	0.1029	0.0280	0.0425
Has at home: vacuum cleaner	0.3344	0.3802	0.0459	0.0633
Has at home: washing machine	0.8548	0.8648	0.0100	0.0395
Has at home: DVD player	0.8807	0.8819	0.0012	0.0377
Has at home: refrigerator	0.9276	0.9286	0.0009	0.0295
Has at home: freezer	0.4956	0.4960	0.0004	0.0626
Has at home: telephone	0.6769	0.6621	-0.0148	0.0592
Has at home: computer	0.7394	0.7492	0.0098	0.0516
Has at home: cable TV	0.4797	0.5537	0.0739	0.0622
Has at home: microwave	0.7670	0.7691	0.0020	0.0497
Schools characteristics				
Computer lab	0.9250	0.9169	-0.0081	0.0549
Science lab	0.4125	0.3839	-0.0286	0.1037
Library	0.1000	0.2061	0.1061	0.0815
Teachers' room	0.9500	0.9764	0.0264	0.0300
Principal's room	1.0000	0.9650	-0.0350	0.0373
Sports court	0.8500	0.9401	0.0901*	0.0500
Internet	0.9875	0.9806	-0.0069	0.0214
School meals	1.0000	0.7983	-0.2017**	0.0893
Staff members	89.3000	72.8267	-16.4733**	7.7087
Number of school rooms in use	15.5250	16.6263	1.1013	1.8146

Notes: Levels of education are coded as low for parents with up to 8 years of education; mid for parents with secondary school or incomplete high education; and high for parents with complete high education. Employment situation is coded as 'has a job' if parents either have a job, or own a business, or are retired; 'temp. job' if they work independently doing some services, or only do temporary jobs; and 'no job' if they are unemployed.

Table A2: Effect of exposure to violence around the school on academic achievement - 25 m, 100 m and 500 m radii

	Math proficiency			*Language proficiency*		
	(1)	(2)	(3)	(4)	(5)	(6)
	25 meters	*100 meters*	*500 meters*	*25 meters*	*100 meters*	*500 meters*
Homicides	−2.289	−1.403	0.029	−2.138	−1.233	−0.149
	(1.105)**	(0.763)*	(0.165)	(1.085)**	(0.631)*	(0.158)
	[0.850]***	[0.602]**	[0.143]	[0.872]**	[0.557]**	[0.140]
Observations	676,082	676,082	676,082	675,733	675,733	675,733
School / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m, 100 m and 500 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school in columns (1) and (4), 100 m in columns (2) and (5) and 500 m in columns (3) and (6). Dependent variables *Math proficiency* and *Language proficiency* are math and Portuguese standardised test scores normalised at a (250,50) scale. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A3: Effect of exposure to violence around the school on academic achievement - 'Rings'

	Math proficiency			*Language proficiency*		
	(1)	(2)	(3)	(4)	(5)	(6)
	25 meters	*25-100 meters*	*100-500 meters*	*25 meters*	*25-100 meters*	*100-500 meters*
Homicides	−2.289	−0.978	0.099	−2.138	−0.805	−0.100
	(1.105)**	(1.035)	(0.169)	(1.085)**	(0.815)	(0.163)
	[0.850]***	[0.818]	[0.144]	[0.872]**	[0.721]	[0.146]
Observations	676,082	676,082	676,082	675,733	675,733	675,733
School / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m, 100 m and 500 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. In columns (1) and (4) explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school; in columns (2) and (5) explanatory variable *Homicides* corresponds to the number of homicides within a 100 m radius from school minus homicides within a 25 m radius from school; in columns (3) and (6) explanatory variable *Homicides* corresponds to the number of homicides within a 500 m radius from school minus homicides within a 100 meter radius from school. Dependent variables *Math proficiency* and *Language proficiency* are math and Portuguese standardised test scores normalised at a (250,50) scale. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A4: Attendance at Math and Language tests

	Attendance at Math test							*Attendance at Language test*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	All	*Boys*	*Girls*	*White*	*Non-white*	*Low income*	*High income*	*All*	*Boys*	*Girls*	*White*	*Non-white*	*Low income*	*High income*
Homicides	−0.012	−0.018	−0.006	−0.007	−0.011	0.006	−0.004	−0.011	−0.015	−0.006	−0.007	−0.010	0.003	−0.002
	(0.011)	(0.015)	(0.010)	(0.012)	(0.010)	(0.008)	(0.008)	(0.011)	(0.014)	(0.009)	(0.012)	(0.009)	(0.008)	(0.007)
	[0.008]	[0.010]*	[0.008]	[0.009]	[0.009]	[0.007]	[0.007]	[0.008]	[0.010]	[0.008]	[0.009]	[0.009]	[0.007]	[0.007]
Observations	777,371	388,428	388,943	271,385	207,396	191,549	220,244	777,371	388,428	388,943	271,385	207,396	191,549	220,244
Mean	0.870	0.863	0.877	0.892	0.877	0.943	0.948	0.869	0.862	0.877	0.892	0.876	0.943	0.948
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables *Attendance at Math test* and *Attendance at Language test* indicate whether the student attended the respective exam or not. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A5: Student Mobility

	Within year transfer	
	(1)	(2)
Homicides	−0.001	0.001
	(0.002)	(0.003)
	[0.003]	[0.003]
Mean	0.016	0.016
Observations	777,371	777,371
School / time	Yes	Yes
Controls	No	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variable *In year transfer* indicates if the student changes school within the year. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A6: Effect of exposure to violence around the school on academic achievement - heterogeneous effects by cohort

	Math 5th grade (primary school)	*Math 9th grade (primary school)*	*Math 3rd grade (secondary school)*	*Language 5th grade (primary school)*	*Language 9th grade (primary school)*	*Language 3rd grade (secondary school)*
	(1)	(2)	(3)	(4)	(5)	(6)
Homicides	−2.407	−2.032	−0.890	−3.435	−1.733	1.072
	(1.649)	(0.934)**	(2.753)	(2.637)	(1.493)	(2.434)
	[1.805]	[0.965]**	[2.156]	[2.212]	[1.260]	[2.095]
Observations	266,683	298,353	111,046	266,334	298,353	111,046
School / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 meter radius from school. Dependent variables *Math proficiency* and *Language proficiency* are Math and Portuguese standardised test scores normalised at a (250,50) scale. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A7: Effect of exposure to violence around the school on academic achievement - heterogeneous effects by gender

	Math proficiency		*Language proficiency*	
	(1)	(2)	(3)	(4)
	Boys	*Girls*	*Boys*	*Girls*
Homicides	−2.971	−1.767	−2.813	−1.406
	(1.554)*	(1.075)	(1.326)**	(1.441)
	[1.165]**	[0.885]**	[1.048]***	[1.167]
Observations	335,038	341,044	334,702	341,031
School / time	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables *Math proficiency* and *Language proficiency* are math and Portuguese standardised test scores normalised at a (250,50) scale. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A8: Effect of exposure to violence around the school on academic achievement - heterogeneous effects by socio-economic status

	Math proficiency				*Language proficiency*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low income	*High income*	*Less educated*	*More educated*	*Low income*	*High income*	*Less educated*	*More educated*
Homicides	−2.730	0.472	−0.092	−1.732	−3.711	0.336	−0.253	−0.660
	(1.811)	(1.528)	(1.725)	(1.322)	(1.406)***	(1.676)	(1.523)	(1.532)
	[1.333]**	[1.454]	[1.275]	[1.082]	[1.246]***	[1.412]	[1.288]	[1.229]
Observations	180,719	208,828	207,915	229,331	180,627	208,709	207,757	229,311
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables *Math proficiency* and *Language proficiency* are Math and Portuguese standardised test scores normalised at a (250,50) scale. We coded as *Low income* parents whose income per capita is below the median income in each year and *High income* otherwise. *Less educated* include only cases in which both parents have only primary school and *More educated* cases in which at least one of the parents have more than primary school. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A9: Effect of exposure to violence around the school on assessment of school security by parents

	My child is safe at school		*My child feels safe at school*		*My child's school security*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	−0.001	−0.015	0.004	−0.025	−0.053	−0.099
	(0.024)	(0.016)	(0.019)	(0.013)**	(0.132)	(0.147)
	[0.018]	[0.012]	[0.015]	[0.013]*	[0.093]	[0.115]
Mean	0.289	0.222	0.326	0.258	5.171	4.981
Observations	90,091	98,212	90,842	99,032	98,206	105,150
School / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 9th grade of primary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables are parents' answers to a socio-economic questionnaire collected by the school. *My child is safe at school* and *My child feels safe at school* are a dummies equal to one if parents completely agree with the statements and zero otherwise. *My child's school security* is a rating of school security by the parents, raging from 0 -very negative- to 10 -very positive. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A10: Effect of exposure to violence around the school on attendance - heterogeneous effects by cohort

	Attendance 5th grade (primary school)	*Attendance 9th grade (primary school)*	*Attendance 3rd grade (secondary school)*
	(1)	(2)	(3)
Homicides	−0.012	−0.015	0.003
	(0.006)*	(0.006)**	(0.013)
	[0.006]**	[0.006]**	[0.009]
Mean	0.915	0.854	0.866
Observations	270,865	315,760	122,761
School / time	Yes	Yes	Yes
Controls	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Dependent variables are the attendance rates in the year. Explanatory variable *Homicides* correspond to the number of homicides within a 25 m radius from school in the year. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A11: Effect of exposure to violence around the school on attendance - heterogeneous effects by gender

	Attendance (year)		*Attendance (1st semester)*		*Attendance (2nd semester)*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	−0.015	−0.007				
(year)	(0.006)**	(0.004)*				
	[0.005]***	[0.004]*				
Homicides			−0.017	−0.008		
(1st semester)			(0.007)**	(0.004)**		
			[0.006]***	[0.004]**		
Homicides					−0.031	−0.012
(2nd semester)					(0.004)***	(0.005)**
					[0.008]***	[0.006]*
Mean	0.875	0.883	0.884	0.891	0.866	0.875
Observations	353,778	355,608	353,778	355,608	353,778	355,608
School / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Dependent variables are the attendance rates in the year and in each semester. Explanatory variables *Homicides (year)*, *Homicides (1st semester)* and *Homicides (2nd semester)* correspond to the number of homicides within a 25 m radius from school in the entire year, in the first and in the second semester. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A12: Effect of exposure to violence around the school on attendance - heterogeneous effects by socio-economic status

	Attendance (year)				*Attendance (1st semester)*				*Attendance (2nd semester)*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Low income	*High income*	*Less educated*	*More educated*	*Low income*	*High income*	*Less educated*	*More educated*	*Low income*	*High income*	*Less educated*	*More educated*
Homicides	−0.005	0.001	−0.002	−0.005								
(year)	(0.003)	(0.003)	(0.004)	(0.003)								
	[0.003]	[0.003]	[0.003]	[0.003]*								
Homicides					−0.005	−0.002	−0.003	−0.005				
(1st semester)					(0.003)**	(0.003)	(0.003)	(0.004)				
					[0.003]**	[0.002]	[0.003]	[0.003]*				
Homicides									−0.018	−0.009	−0.027	−0.008
(2nd semester)									(0.004)***	(0.006)	(0.008)***	(0.005)
									[0.005]***	[0.006]	[0.008]***	[0.005]
Mean	0.902	0.908	0.902	0.909	0.904	0.910	0.905	0.910	0.901	0.906	0.900	0.907
Observations	182,633	209,722	210,993	229,400	182,633	209,722	210,993	229,400	182,633	209,722	210,993	229,400
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Dependent variables are the attendance rates in each semester and in the year. Explanatory variables *Homicides (year)*, *Homicides (1st semester)* and *Homicides (2nd semester)* correspond to the number of homicides within a 25 m radius from school in the entire year, in the first and in the second semester. We coded as *Low income* parents whose income per capita is below the median income in each year and *High income* otherwise. *Less educated* include only cases in which both parents have only primary school and *More educated* cases in which at least one of the parents have more than primary school. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A13: Effect of exposure to violence around the school on academic achievement: the role of students attendance

	Math proficiency		*Language proficiency*	
	(1)	(2)	(3)	(4)
Homicides	−2.128	−1.776	−2.204	−1.958
	(1.194)*	(1.072)*	(1.091)**	(0.999)*
	[0.894]**	[0.852]**	[0.897]**	[0.876]**
Observations	641,530	641,530	641,208	641,208
School / time	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Student attendance	No	Yes	No	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 5th and 9th of primary school and the 3rd grade of secondary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables *Math proficiency* and *Language proficiency* are math and Portuguese standardised test scores normalised at a (250,50) scale. All regressions include time and school fixed effects. Controls include individual characteristics, teachers characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A14: Effect of exposure to violence around the school on parental involvement with education

	I help my child studying at home		*I participate in my child's parent evening*		*I talk to my child about school*		*I follow my child's homework*		*My parents help me with homework*		*My parents ask about my homework*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	−0.034	0.026	0.015	−0.008	−0.020	−0.035	−0.028	−0.019	−0.002	0.025	−0.052	−0.014
	(0.018)*	(0.020)	(0.015)	(0.014)	(0.022)	(0.017)**	(0.016)*	(0.015)	(0.015)	(0.014)*	(0.017)***	(0.022)
	[0.020]*	[0.017]	[0.012]	[0.011]	[0.018]	[0.015]**	[0.014]*	[0.013]	[0.015]	[0.011]**	[0.016]***	[0.017]
Mean	0.453	0.427	0.435	0.491	0.221	0.257	0.104	0.125	0.224	0.188	0.471	0.404
Observations	98,242	105,062	176,457	167,542	175,150	166,450	174,526	165,891	96,291	103,561	96,687	103,935
School / time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses. Conley standard errors computed at the 25 m cutoff distance in brackets.

Notes: The analysis includes students in the 9th grade of primary school, over the period of 2010 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school. Dependent variables are parents and student's answers to a socio-economic questionnaire collected by the school. *I help my child studying at home*, *I participate in my child's parent evening*, *I talk to my child about school*, *I follow my child's homework* are dummies equal to one if parents completely agree with the statements and zero otherwise. *My parents help me with homework* and *My parents ask about my homework* are dummies equal to one if the student answers those situations always happen and zero otherwise. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. For a detailed list of controls refer to Table 2 notes.

Table A15: Effect of exposure to violence on student progression - heterogeneous effects by gender

Panel A: Around the school

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	0.008	0.005	0.035	0.033*	0.009	0.001
	(0.012)	(0.012)	(0.025)	(0.019)	(0.063)	(0.039)
Mean	0.056	0.037	0.162	0.143	0.713	0.765
Observations	1,043,413	1,045,307	1,246,576	1,221,344	143,759	143,545
R^2	0.064	0.044	0.081	0.069	0.086	0.088
School / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Around the residence

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	−0.009*	0.001	0.077*	0.068	−0.005	0.002
	(0.005)	(0.004)	(0.044)	(0.044)	(0.014)	(0.024)
Mean	0.056	0.037	0.160	0.140	0.694	0.752
Observations	989,203	990,912	1,178,794	1,153,902	122,697	121,343
R^2	0.070	0.049	0.088	0.076	0.144	0.151
School/neighb/time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Around the residence and the school

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	−0.002	0.002	0.067**	0.059*	0.011	0.013
	(0.006)	(0.005)	(0.032)	(0.031)	(0.024)	(0.018)
Mean	0.056	0.037	0.160	0.140	0.694	0.752
Observations	989,203	990,912	1,178,794	1,153,902	122,697	121,343
R^2	0.070	0.049	0.088	0.076	0.144	0.151
School/neighb/time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered (at the school level in Panel A and at the neighbourhood level in Panels B and C) in parentheses.

Notes: The analysis includes students from the 1st grade of primary school to the 3rd grade of secondary school, over the period of 2007 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25 m radius from school in Panel A, from residence in Panel B and from school and residence in Panel C. Dependent variable *Repetition* is a dummy variable which indicates whether the student has to repeat the same grade as the current year in the coming year. *Dropout* is a dummy variable which captures if the student drops out school in the successive year. *School progression* indicates if the students in the last grade of primary school progress to secondary school in the subsequent school year, for that reason, regressions for this outcome include only students at the final grade of primary school. All regressions include time fixed effects. Controls include individual characteristics, school characteristics and classroom composition. **Individual controls** are age, sex and race fixed effects. For a detailed list of school and classroom controls, refer to Table 2 notes.

Table A16: Effect of exposure to violence on the residence-school path on student progression - school fixed effects controlling for distance

Panel A: Walking

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	50m width	*100m width*	*50m width*	*100m width*	*50m width*	*100m width*
Homicides	0.000	−0.002	0.027	0.022**	0.006	−0.000
	(0.003)	(0.002)	(0.017)	(0.010)	(0.017)	(0.012)
Mean	0.047	0.047	0.150	0.150	0.724	0.724
Observations	1,862,503	1,862,503	2,192,720	2,192,720	230,129	230,129
R^2	0.057	0.057	0.079	0.079	0.134	0.134
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Driving

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	50m width	*100m width*	*50m width*	*100m width*	*50m width*	*100m width*
Homicides	0.004	−0.000	0.032**	0.029***	−0.007	−0.008
	(0.003)	(0.002)	(0.016)	(0.011)	(0.014)	(0.010)
Mean	0.047	0.047	0.150	0.150	0.724	0.724
Observations	1,862,502	1,862,502	2,192,719	2,192,719	230,129	230,129
R^2	0.057	0.057	0.079	0.079	0.134	0.134
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Public transport

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	50m width	*100m width*	*50m width*	*100m width*	*50m width*	*100m width*
Homicides	0.001	−0.002	0.032*	0.029***	0.004	−0.001
	(0.003)	(0.002)	(0.017)	(0.011)	(0.017)	(0.012)
Mean	0.047	0.047	0.150	0.150	0.724	0.724
Observations	1,862,503	1,862,503	2,192,720	2,192,720	230,129	230,129
R^2	0.057	0.057	0.079	0.079	0.134	0.134
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the school level in parentheses.

Notes: The analysis includes students from the 1st grade of primary school to the 3rd grade of secondary school over the period of 2007 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within corridors of 50 m and 100 m width. Dependent variable *Repetition* is a dummy variable which indicates whether the student has to repeat the same grade as the current year in the coming year. *Dropout* is a dummy variable which captures if the student drops out school in the successive year. *School progression* indicates if the students in the last grade of primary school progress to secondary school in the subsequent school year, for that reason, regressions for this outcome include only students at the final grade of primary school. All regressions include time and school fixed effects. Controls include natural log of the calculated path distance, individual characteristics, school characteristics and classroom composition. **Individual controls** are age, sex and race fixed effects. For a detailed list of school and classroom controls, refer to Table 2 notes.

Table A17: Effect of exposure to violence on the residence-school path on student progression - heterogeneous effects by gender

Panel A: Walking

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	0.002	0.003	0.039	0.025	−0.005	−0.007
	(0.004)	(0.004)	(0.024)	(0.023)	(0.019)	(0.024)
Mean	0.056	0.037	0.159	0.140	0.697	0.753
Observations	936,389	938,463	1,115,440	1,092,341	116,023	114,890
R^2	0.071	0.050	0.089	0.077	0.137	0.144
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Driving

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	0.005	0.005	0.046**	0.031	−0.018	−0.011
	(0.004)	(0.003)	(0.021)	(0.021)	(0.018)	(0.020)
Mean	0.056	0.037	0.159	0.140	0.697	0.753
Observations	936,389	938,463	1,115,440	1,092,341	116,023	114,890
R^2	0.071	0.050	0.089	0.077	0.137	0.144
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Public transport

	Repetition		*Dropout*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	0.004	0.004	0.046**	0.032	−0.002	−0.009
	(0.004)	(0.003)	(0.023)	(0.023)	(0.018)	(0.022)
Mean	0.056	0.037	0.159	0.140	0.697	0.753
Observations	936,389	938,463	1,115,440	1,092,341	116,023	114,890
R^2	0.071	0.050	0.089	0.077	0.137	0.144
Corridor / time	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the corridor level in parentheses.

Notes: The analysis includes students from the 1st grade of primary school to the 3rd grade of secondary school over the period of 2007 to 2013. Explanatory variable *Homicides* corresponds to the number of homicides within corridors of 50 m and 100 m width. Dependent variable *Repetition* is a dummy variable which indicates whether the student has to repeat the same grade as the current year in the coming year. *Dropout* is a dummy variable which captures if the student drops out school in the successive year. *School progression* indicates if the students in the last grade of primary school progress to secondary school in the subsequent school year, for that reason, regressions for this outcome include only students at the final grade of primary school. All regressions include time and corridor fixed effects. Controls include individual characteristics, school characteristics and classroom composition. **Individual controls** are age, sex and race fixed effects. For a detailed list of school and classroom controls, refer to Table 2 notes.

Geographic Coordinates and School-residence Corridors

To define the measures of exposure to violence, it was necessary to geocode the addresses of the schools, residences, and homicides. For the schools, we have the precise address, including street and house number. For the residences, the street and house number are confidential information and cannot be accessed. However, we were granted access to the postcodes and neighbourhoods. In São Paulo, postcodes are quite small units and, in some cases, even more precise than the street names, as streets are typically broken up into several postcodes. For the homicides, we also have the precise location for each case.

We used the Google Maps API to geocode the addresses. There are five possible geocoding outcomes, which vary depending on the amount of information used in the process: street, neighbourhood, municipality, state, and not found. If the address is geocoded at the street level, it means that the returned result is a precise geocode, for which Google has information down to street address precision. When street-level information is not available, the returned geocoded addresses are approximations, either interpolated between two precise points or the geometric centre of a result, such as a polyline (for example, a street) or polygon (region).

In our analysis, we use only returned addresses geocoded at the street level. Hence, even though we have different levels of information on the addresses of schools, residences, and homicides, the geocoding accuracy level for all these three units is the street level. From the addresses that we geocoded, 96% of the schools and 97% of the residences were geocoded at the street level, and 95% of the homicides in public were geocoded at the street level.

We also used Google to calculate the corridors from residence to school. We used the Google Directions API and calculated path polylines of walking transport mode for each school/residence pair, which we call the *homicide exposure point (HEP)*. For each pair, we went through all the homicide points and calculated the nearest distance between a homicide and that particular polyline. We also calculated walking and straight distances from the residence to school and from the residence to the *HEP*.

To make those calculations feasible and limit the time necessary to run them, we defined some filter rules as follows:

- Define the threshold distance between the homicide points and path polylines to 500 m;
- Ignore walking mode if the straight-line distance is greater than 15 km;

- Define $double - distance = max(straight - linedistance * 2, 500 * 2)$: If $double - distance$ is greater than 100 km, ignore the homicide point outside the circle with a radius of $double - distance/2$ and centre as the middle of the straight line between the school and residence; if $double - distance$ is less than or equal to 100 km, ignore the homicide point if the straight-line distance between the homicide point and either the school location or residence location is greater than double the distance.

To avoid billions of unnecessary API requests, the straight-line distance calculations, distances along the path of walking distance transport mode polylines, and nearest distance between the homicide points to polylines were all calculated with Google's code without invoking Google APIs. Overall, we used approximately two billion API requests to geocode our data and to generate the corridors for our analysis.