Essays on the Economics of Crime

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A thesis submitted for the degree of Doctor of Philosophy

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University of Leicester

2018

Abstract

This thesis studies some of the many costs associated with exposure to crime. Chapter 2 focuses on indirect exposure to crime, and investigates how homicides affect students' performance. A number of large administrative Brazilian datasets is used to estimate the causal effect of exposure to homicides in the public way on schooling outcomes. Within-school estimates show that violence in the surroundings of schools, at the residence of students, and on the walking path from residence to school has a negative effect on a number of measures of school achievement such as test scores, repetition, dropout and school progression. Results also show that school attendance suffers following a homicide in the school surroundings. Exceptionally rich data allow the investigation of heterogeneous effects and of the channels underlying these effects.

Chapter 3 examines the effect of individual criminal victimisation in robbery and theft on birth outcomes using a unique dataset from Brazil combining information on the universe of victims of crime with vital statistics data. Results show that victimisation in robbery during the first trimester reduces birthweight substantially, by about 60 grams – 10 percent of a standard deviation in birthweight – and increases the likelihood for low birthweight by about 40 percent compared to the baseline. The results are robust to the inclusion of place of residence, hospital and time fixed effects and to the inclusion of a very large array of mother and pregnancy characteristics. Results also show that victimisation leads to a substantial increase in fetal deaths and a positive selection of live births, hence likely providing a lower bound of the estimated effects on birthweight. The very rich information from crime and birth records allow the investigation of the mechanisms underlying the estimated relationship.

Chapter 4 studies the effect of criminal victimisation on labour market performance. A number of very rich Brazilian administrative datasets is combined to estimate the effect of exposure to day-to-day crime events of robbery and theft on monthly attendance and turnover of public servants. Using individual and workplace fixed effects, estimates show that after becoming a victim of robbery or theft, monthly attendance of public servants in the workplace is reduced. Individuals who were victims of crime are also more likely to change their workplace or to leave their job subsequently.

Acknowledgements

First, I would like to express my gratitude to my supervisor Martin Foureaux Koppensteiner for accepting nothing less than excellence from me, for all the incentives to produce good research, for helping me to deal with my lack of confidence and for preparing me not only for the completion of my PhD, but for the academic life ahead of me. I will be forever grateful to you. I also would like to thank my examiners, Olivier Marie and James Rockey for their insightful comments.

I would like to thank the Department of Economics for funding my PhD. I also thank all members of the Department for their support and effort on providing PhD students with such a friendly environment, with plenty of opportunities for learning. I also would like to acknowledge the help of all the nice staff members of the ministries and secretaries in Brazil with accessing some of the datasets I use in this thesis.

I must thank my friends, especially Emma, Laura, Sebastian, Mengxing and all the others who shared my good and bad moments. Special thanks goes to Douglas for all the support and words of encouragement in some difficult moments and for his help on collecting some of the datasets.

A very special thanks goes to my family for understanding my absence in such crucial moments. I am also grateful to my sister Aline for incentivising me to pursue a PhD, for the priceless help when I first arrived in the UK and for constantly checking on me. And last, but by no means least, I thank God, who reminds me every day that "I can do everything through Him who strengthens me".

I would like to dedicate this thesis to the lovely memory of my grandmother, Anair Colares (1931-2014), whose life was devoted to the well-being of her family and whose last words to me were: "you don't need to worry, everything is going to be all right" – a day before my APG review. Unfortunately, I could not come back to her and say she was right, so I hope she can hear me now: sim, Dona Nair, deu tudo certo!

Declaration

Chapters 2 and 3 are joint work with my PhD supervisor, Dr. Martin Foureaux Koppensteiner. I was responsible for the data analysis and the writing of the chapters was shared.

Chapter 2 has benefited from very useful comments and suggestions from Esteban Aucejo, Fernanda Brollo, Paolo Buonanno, Antonio Ciccone, David Figlio, Jeffrey Grogger, Mark Hoekstra, Michael Lovenheim, Philip Oreopoulos, Ian Walker and seminar participants in Lancaster, Leicester, Reading and Surrey, the 2017 IZA Workshop on the Economics of Education, and the 2018 Workshop on Human Capital at the Bank of Italy.

Chapter 2 has been presented under the title "Afraid to go to school? Estimating the Effect of Exposure to Violence on Schooling Outcomes" in seminars in Lancaster, Leicester, Reading and Surrey; in the 2017 IZA Workshop on the Economics of Education, Bonn, Germany; in the 2018 Workshop on Human Capital at the Bank of Italy, Rome, Italy; in the 2018 Royal Economic Society Annual Conference, Brighton, UK; in the Annual Conference of the European Society for Population Economics, Antwerp, Belgium; in the XXVII AEDE Meeting, Barcelona, Spain. It is also scheduled to be presented in:

- August 2018: EEA-ESEM Cologne 2018, Germany.
- September 2018: 30th EALE Conference, Lyon, France.
- January 2019: The 2019 North American Winter Meeting of the Econometric Society, Atlanta, US.

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Chapter 1

Introduction

Crime and violence have important consequences on welfare, ranging from material losses, the quantity and quality of life – through reductions in lifespan and increased morbidity –, to avoidance behaviour as response to crime. Measuring the causal effect of crime on these outcomes is not straightforward. Crime and violence in an area are correlated with the underlying socioeconomic conditions; some of these are unobservable to the researcher. This leads to an endogeneity problem when estimating the effect of crime on the above outcomes. In addition, many of the costs are not straightforward to quantify and require extraordinarily rich data. Because crime is, – even in the context of high crime environments – a relatively rare event, making it difficult to detect some of these effects on a number of outcomes when using survey data. Newly available administrative data at the population level that contain information on crime and a number of outcomes are now accessible to researchers to address some of these issues.

In this thesis, I combine very rich Brazilian administrative data to estimate the effect of exposure to crime on a number of outcomes. I start, in Chapter 2, by investigating how exposure to crime affects students' performance at school. As a measure of exposure to crime, I use georreferenced data of homicides for Brazil. I find that students exposed to violence have a lower performance in Math. Violence around school also affects school attendance of students. The results I present suggest that violence affects human capital accumulation of children. Since poor neighbourhoods are often also more violent, violence is potentially one additional contributor for the socioeconomic gradient we observe in many low and middle income countries plagued with high crime rates.

In the following chapters, I estimate some of the costs of individual criminal victimisation. In Chapter 3, I estimate the effect of being a victim in robbery and theft on birth outcomes using a unique dataset from Brazil combining information on the universe of victims of crime with vital statistics data. I find that victimisation in robberies during the first trimester significantly reduces birthweight and indicators for poor health at birth, such as low birthweight. These results contribute to a growing literature on the effects of maternal stress induced by violent events on birth outcomes and to the understanding of the societal cost of crime.

In Chapter 4, I investigate how criminal victimisation affects labour market performance of public servants. I find that after becoming a victim of robbery or theft, monthly attendance of public servants in the workplace is reduced. Individuals who were victims of crime are also more likely to change their workplace or to leave their job subsequently. Absenteeism and turnover in the public sector are very disruptive not only for the workers, but also for the individuals who benefit from their services. On the public servant side, while job transfer and job departure may be part of coping strategies, these outcomes may nevertheless affect their careers and, in the Brazilian case, also affect their prospective to retire at their desired time. On the side of the beneficiaries of public services, these events may disturb the efficient delivery of services, possibly generating negative externalities for students in Brazil. Hence, this chapter also shows how violence and crime may contribute to the underlying problems with providing high quality primary and secondary education in Brazil.

Chapter 2

Afraid to go to School? Estimating the Effect of Violence on Schooling Outcomes

2.1 Introduction

After a decade of declining rates of crime and homicides, Brazil (and other countries in Latin America) has observed a steep increase in violent crime. Today, Brazil has one of the highest homicide rates in the world, according to statistics from the World Bank. 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[1], in 2016, 61,283 violent deaths were registered in the country. The Brazilian Institute of Applied Economic Research (Ipea) estimated that the cost of violence corresponds to more than 5 percent of the country's GDP, not including yet many intangible costs which are difficult to quantify (Cerqueira et al. (2007)). The pain, suffering and trauma caused by direct

[1] Available from `http://www.forumseguranca.org.br`

victimisation and exposure to violence in the local neighbourhood may negatively impact a variety of societal outcomes, among those educational production. Violence may affect school supply as well as the behaviour of students, parents, teachers and principals. In this paper, we want to estimate the effect exposure to violence has on the performance of students in Brazil making use of unique novel dataset containing georeferenced information on all homicides occurring in the public way and combining this with very detailed information of student performance.

A number of qualitative studies by psychologists, psychiatrists and sociologists has found a range of adverse consequences in the behaviour of children after exposure to community violence: depression, anxiety, hyper vigilance, avoidance as well as 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 students' attendance at school. When a crime occurs in their neighbourhood of residence or in the proximity of their schools, parents may feel uneasy of sending their children to school. According to the 2012 edition of the Brazilian National Survey of School Health, almost 9 percent 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, for not feeling safe on the way from 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 from the regular contact with their classmates. This will eventually lead to low scores in their exams and potentially impact a number of measures of school failure, including repetition and dropout. The exposure to homicides in the local neighbourhood may also reveal information to students and parents about likely victimisation and affect the expected returns to 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 socioeconomic status often register higher rates of violence and if this has also a negative effect on human capital accumulation, this could be a relevant channel leading to the perpetuation of poverty. The correlation between socioeconomic 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 both high levels of violence and to worse schooling outcomes, from the underlying causal relationship.

This paper estimates the causal effect of violence on schooling performance, using a unique set of Brazilian microdata. We make use of access to information on the exact timing and the precise location of each homicide, and information on the location of the schools students attend, and their residence. We exploit 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, school progression and attendance, while controlling for school and time fixed effects. Given the prevalence of high crime rates 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 is a small number of studies estimating the relationship between exposure to violence and school performance (for example Grogger (1997), and Aizer (2008)) which generally, given the cross-sectional nature of the data used, cannot deal with the endogeneity problem arising from the fact that violence might be correlated with other sources of socioeconomic disadvantages and school outcomes. A notable exception is Monteiro and Rocha (2017) who estimate the causal effect of gunfights between drug gangs in Rio de Janeiro's favelas (slums) on students' achievements using panel data for the city of Rio de Janeiro. They look at the effect of conflicts in favelas on students who study in schools located in favelas and in schools located within a 250 meter radius from a favela border and find that students' test scores in Math are lower in years in which they are exposed to drug battles. This paper sheds light on how these conflicts affect children's development in the context of conflicts associated with drug battles in poor neighbourhoods or their close proximity. The conflicts in and around favelas are often context specific, and for example related to battles between rival drug gangs, but violence in Brazil is a more widespread phenomenon. The measure of violence we use, homicides, will be able to capture the widespread nature of violence and allows us to estimate the effect of violence on students' achievements in a much more general setup likely more representative for violence in Brazil.

In this paper, we introduce a unique set of microdata, which provides us with a measure of violence that is consistent across space and time: homicides in the public way. This is important as this allows to use variation over time and across space, including across vast areas and different administrative units for which consistent crime data, that includes information on violence, is rarely available. For these homicides we have available extremely granular address information which we geocode and combine with the georeferenced information on the addresses of schools and the address of residence of students attending these schools. This allows us to not only investigate the effect of violence in the surroundings of schools or the residence of students, but also to investigate in detail exposure to violence on the way to school for a period of 7 years. We focus most of our analysis on the city of São Paulo, which is the largest city in the Americas with a population of 12 million people. Within-school estimates show that violence at the surroundings of school and residence and at the walking path from residence to school has a negative effect on attendance and on a number of measures of educational achievements such as test scores, repetition, dropout and school progression.

We find that homicides in the surroundings of schools lead to a substantial deterioration of educational performance of schoolchildren, as measured by standardised test scores in Math and Portuguese Language. We find that one additional homicide in a 25 meter radius around schools reduces test scores in Math by about 5 percent of a standard deviation in test scores. Furthermore, we find that homicides also increase repetition and dropout rates and negatively impact school attendance. Using rich information on the student background, we 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 furthermore show that the effect cannot be explained by lower attendance rates alone and 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. To our best knowledge, this is the first paper to provide credible causal estimates on the effect of exposure to homicides on schooling outcomes that uses a generalisable measure of violence.

The remainder of the paper is organised as follows. Section 2.2 explains the institutional background. Section 2.3 details the datasets used in the analysis. Section 2.4 presents the identification strategy applied to estimate the causal effect of violence on educational outcomes. Sections 2.5, 2.6, 2.7 and 2.8 explain the results and Section 2.9 presents the final remarks.

2.2 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 programs and policies. The educational system is composed by two main levels: *Educação Fundamental* (basic education) - which comprises *Educação Infantil* (nursery), *Ensino Fundamental* (primary school), *Ensino Médio* (secondary education) - and *Educação Superior* (higher education).

Public primary education is offered at no cost for all, irrespective of the age, and it is mandatory for children between 6 and 14 years of age. It lasts 9 years[2] and it is divided in 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 day of the school year varies across schools and over the years. Figure 2.1 in the Annex exemplifies the school calendar in São Paulo for 2010. Every year São Paulo State's 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; second semester starts on the first working day of August and finishes on the last working day before Christmas. Each semester is composed by two bimesters, with roughly 50 days each, the precise ending dates of each bimester is 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 in case 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 percent of the school days in primary schools and 85 percent in secondary schools.

[2]Previously, primary school began at age 7 and lasted eight years. In 2006, the government passed a law that expands primary school from 8 to 9 years and mandatory enrolment at 6 years old. States and municipalities had until 2010 to implement the new law.

Considering the nature of funding and administration of schools, they can be classified in 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 in these schools. At least 87 percent of the students go to public schools in Brazil, in São Paulo this number is slightly smaller, 80 percent. Schools may offer all or only specific levels of basic education, and there are schools which offer only primary, some only secondary and some offer both primary and secondary education.

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.

2.3 Data

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

2.3.1 Educational data

We have access to unique microdata of all students in primary and secondary school, collected by the Brazilian Ministry of Education that contains information on the addresses of students and their schools. From 2007, this dataset contains information from individual records on students, schools and teachers and their characteristics. In addition, it is possible to follow students over time and across schools through a unique student identifier, which allows us to construct some of the outcomes we use in the analysis: repetition, dropout, school progression and school transfers. Characteristics of students and teachers include date of birth, sex, race, grade for the students, and educational background for the teachers (among other).

Table 2.1 presents summary statistics of students and school characteristics for São Paulo over the period from 2007 to 2012. For consistency, we do not consider nursery schools[3] and any kind of special education, which is offered to students with special needs. The final dataset contains on average 1.8 million students a year spread over more than 3,000 schools. The majority of observations covers students in primary school (77 percent) and given universal primary school enrolment and the longer duration of primary school, demographic characteristics are roughly representative for the population at large. Measures of school efficiency, such as repetition and dropout reveal substantial problems in the Brazilian educational system. Close to 8 percent of schoolchildren repeat any given grade and almost 13 percent drop out of a given grade. There is a substantial number of students that change school after the school year. Only 73 percent of students carry on beyond compulsory education and enrol in secondary school. A small fraction (0.1 percent) of students changes school during the school year.

Of the schools in the sample, about a third is run by the state (mainly secondary schools) and about 17 percent are run by the municipality. The large fraction of private schools reveals that, given that only about 20 percent of students are enrolled in these, private schools are on average much smaller compared to state and municipal schools. Close to 60 percent of schools offer free school meals, an indication for students from poor households.

[3]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.

We use standardised test scores from SARESP[4], provided by São Paulo State's Secretariat of Education. The exam is carried out every year and evaluates the performance of students in Portuguese and Math at 5th, 7th and 9th grades of primary and at 3rd grade of secondary school. In order to be able to compare the results to national standardised exams, we focus on test scores for 5th and 9th grades of primary school and 3rd grade of secondary school. These coincide with the end of each of the educational cycles described above.

Attendance data is also provided by São Paulo State's Secretariat of Education. The dataset contains attendance record of all students at state schools in São Paulo at a bimonthly frequency. The data contains information on the number of school days missed with some basic information about the reason of non-attendance. The attendance data can be merged to individual school records from the school census by the unique student identifier.

2.3.2 Violence data

We use microdata of official death records published by the Brazilian Ministry of Health. This dataset comes from the Mortality Information System[5], 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. We believe these homicides are particularly salient for our analysis for two reasons: first, these homicides cause a lot of attention and are particularly

[4] 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.

[5] *Sistema de Informações sobre Mortalidade (SIM)*, in Portuguese.

visible to the population. Second, these homicides form a more homogeneous group of homicides (and for example largely exclude domestic homicides). We geocode homicides addresses using Google maps API's and restrict homicides geocoded at the street level, which correspond to 95 percent of all homicides in the public way.

Table 2.2 displays summary statistics of the victims of homicides for which the death occurs in the public way, as well as the description of characteristics of homicides. Approximately 70 percent of the homicides are a result of assault by gun discharge, and about 10 percent each by assault using sharp or blunt object. The majority of victims is in the age group between 19 to 50 years old, but there is substantial number (8.4 percent) of relatively young victims of homicide between the ages of 11 and 18. The vast majority of victims is male, and individuals from lower socioeconomic background are over proportionally represented as victims of homicides, as indicated by very low levels of completed education. Figure 2.2 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 make use of the variation of homicides over time and space depicted in the maps allowing us to disentangle the effect of violence from other correlates of socioeconomic variables and thus establishing causality between violence and education, as described in the next section.

2.4 Identification Strategy

Disentangling the effect of violence on education from confounding factors is not straightforward. In our case, poor neighbourhoods often register higher homicide rates and students from disadvantaged background are 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 socioeconomic characteristics 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.

In order to deal with these 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 variation of homicides in each area associated with the measures of schooling performance of pupils whose neighbourhoods or schools were exposed to violence allow the estimation of the following model:

$$y_{ist} = \beta_0 + \beta_1 homicides_{st} + X_{ist}\beta_2 + Z_{st}\beta_3 + d_s + d_t + u_{ist} \tag{2.1}$$

y_{ist} is a range of different measures for schooling outcomes; $homicides_{st}$ is the number of homicides that lie in the close periphery of schools; X_{ist} are vectors of individual characteristics; Z_{st} are school and classroom time varying characteristics; d_s and d_t are school and time fixed effects, respectively; and u_{ist} is the error term.

We present an example of the variation we use in the maps in Figure 2.3 in the Annex. The maps show the variation in homicides over time and across space, each individual map shows schools (white dots) and homicides in the public way (green circles) in a neighbourhood in São Paulo in a semester. The very precise information on school addresses and the address of occurrence of homicides allow us to construct very granular exposure points, and we focus on homicides occurring in a 25 meter radius[6] around schools. The very granular geographic information helps

[6] As a robustness check, we estimated regressions using different radius measures and results are similar.

us to minimise measurement error and to avoid that the measure of homicides of two schools in close proximity overlap.

As identifying assumption we assume that conditional on time and school fixed effects characteristics, the variation in the number of homicides in a very small geographic area is exogenous. In addition, we include a very rich set of individual, teacher, classroom and school characteristics to reduce sampling variability. We test for balancing of a large set of school and students characteristics by schools exposed and not exposed to violence, results in Table 2.14 show that the differences are very small and, from the very large set of characteristics, only three are statistically different.

To test how violence at different places of exposure impacts educational outcomes separately, we estimate the effect using measures for exposure at the school, the residence of students and on the way from the residence to school.

2.5 Results

In this section we present the results of the effect of exposure to violence on schooling outcomes. First, we investigate how violence affects test scores and attendance of children in Subsection 2.5.1 and Subsection 2.5.2. We then look at some broader measures of school performance in Subsection 2.5.3.

2.5.1 Effect of violence in the school surroundings on standardised test scores

First, we are interested in whether exposure to violence affects educational achievement of students. We use standardised test scores in Math and Portuguese Language as measures of achievement. Table 2.3 presents the regression results of the effect of violence on Math and Language standardised test scores. All test scores are normalised at a (250,50) scale. The analysis includes students in 5th and 9th grades of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. The explanatory variable *Homicides* corresponds to the count of homicides within a 25m radius from school. We present robust standard errors clustered at the school level in parentheses. In order to account for possible spatial dependence between schools and for serial correlation, we compute Conley standard errors[7] Conley (1999), presented in brackets.

In the first column, we estimate the effect of homicides on standardised Math test scores without further individual controls (only school and time fixed effects); in the second column we include the rich set of student controls. In columns three, four and five we include, respectively, teacher characteristics, school characteristics and the classroom composition as controls.

Across specification we find a negative effect of homicides on Math test scores. Adding individual, teacher, school and classroom controls does not change the coefficients in any meaningful way, lending extra credibility to the identification strategy, but improve precision of the estimate. We find that an additional homicide in the surroundings of school during the year decreases Math test scores by 4.7 percent of a standard deviation.

In columns six to ten we repeat the exercise for Portuguese language scores; all test scores are normalised at a (250,50) scale. Across specifications, we find that exposure to homicides around schools has a negative effect on test scores. This is consistent with the findings of Monteiro and Rocha (2017), who find that the coefficients for Language, although negative, are generally smaller compared to the effects for Math test scores and not significant.

We also create indicator variables identifying high and low performers in these grades to investigate whether the effects are particularly driven by shifts in the lower

[7]We compute Conley standard errors using a 25m cut-off distance. Results remain the same if we use 50m or 100m.

or upper part of the test score distribution, Table 2.17 in the Annex presents the results. The variables *Math high level* and *Language high level* indicate whether students reach the 'advanced' level in these subjects. Similarly, *Math low level* and *Language low level* show if the student's test scores are considered in the 'below the basic' level in these subjects. These variables provide an easy way to identify how students may be impacted differently at different parts of the skills distribution. We find that students are more likely to be classified as low level and less likely to be classified as high level in Math when they are exposed to violence around the school during the year. The coefficient for *Math low level* is higher compared *Math high level*, suggesting that low achieving students are more affected.

2.5.2 Effect of violence in the school surroundings on attendance

Next, we investigate whether homicides in the surroundings of the school affects students' attendance. In Table 2.4 we present the regression results of the effect of violence on attendance. We have access to the number of absences of each student for each bimester. As the ending dates of the bimesters are school specific and are not available from the data, we group the four bimesters in two semesters.[8] We then calculate the percentage of absences of each student in the entire year, in the first and in the second semester. We use the same routine to calculate the explanatory variables: *Homicides (year)* corresponds to the number of homicides within a 25m radius from school in the entire year; *Homicides (1st semester)* and *Homicides (2nd semester)* are the number of homicides within a 25m radius from school in the first and second semesters.

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

We find that one additional homicide in the year increases the number of absences by 1.4 percent. Each additional homicide around the school in the first semester increases the number of absences in the respective semester by 1.5 percent. In the second semester, one additional homicide in the surroundings of the school increases absences by 3.2 percent.

The coefficients for the second semester exceed the magnitude of the coefficients of the first semester. These results possibly 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 percent attendance threshold, earlier in the school year students may be more prudent regarding their attendance. Later in the year, when students have more control over their overall yearly attendance, they may be less prudent. In addition, the law regulating student attendance in São Paulo states that if a student has accumulated an excess of 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 make an effort and take measures during the year to avoid students' repetition due to absences. If students accumulate an excess of absences in the first semester, the school intervene and tries to remedy the situation. As a result of parents and school's effort, 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 impact 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 missed classes and potentially miss even more school days.

Attendance may be one possible mechanism through which violence affects school performance. Aucejo and Romano (2016) found that a reduction in absences at school leads to an increase in both Math and reading test scores. Given the increase in absences and the large negative effect we find on test scores, attendance is possibly one of the reasons why violence affects test scores, therefore signalling the need for extra measures to deal with this problem.

2.5.3 Effect of violence in the school and residence surroundings on additional schooling outcomes

In addition to test score results, we are interested in additional educational outcomes as broad measures of educational achievement. We have these measures for a longer period, 2007 to 2013, and for all cohorts. Table 2.5 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 in the school and residence surroundings, respectively, which are defined as the number of homicides that lie in a 25m perimeter from school (residence). We also look at the effect of exposure to violence in the walking path from residence to school for a fraction of students, for whom we have access to their postcode of residence. For that, we use Google maps API to calculate the shortest walking distance between school and residence geocoordinates. Along the walking path line, we build polygons of 50m width (25m to each side), which we call corridor, we show an example in Figure 2.4. Then, we calculate the closest orthogonal distance of each homicide, within the corridor, to the walking path line and estimate within corridor regressions, presented in *Panel C*.

Repetition is a dummy variable, which indicates whether the student has repeated the same grade as the current year in the coming year. We find positive coefficients for all types of exposure, but significant at the ten percent significance level in *Panel A*; this suggests an effect size, compared to the mean repetition rate, of 11.5 percent.

Dropout is a dummy variable, which captures whether a student drops out of school at the end of the school year (or indeed during the year). We find consistent positive coefficients, but significant and larger in magnitude for exposure in the school path. The variable *School progression* indicates whether students in the last grade of primary school progress to secondary school at the end of primary school. Although negative, as expected, the coefficients for this variable are not significant at conventional levels of significance.

We also investigate the effect of violence on school mobility. The variables *Between year transfer* and *Private school transfer* capture students who transfer to a different school in the following year and the latter specifies whether students transfer from a public to a private school. Results suggest that students are more likely to transfer schools between years in the event of a homicide near the school, and that parents are more likely to enrol their children in private schools; each additional homicide in the year rises transfers to private schools in the following year by 12.4 percent. The coefficients for the variable *In year transfer* indicate that transfers within the year are not affected by violence, which shows that violence does not change school composition within the year, reassuring the results on test scores found in the previous section.

2.6 Robustness Checks

In this section we present a number of robustness checks. First, we look at spatial correlation in Subsection 2.6.1. We then look at selection and teachers' attendance in Subsections 2.6.2 and 2.6.3.

2.6.1 Spatial correlation

Using spatial variation for identification has been identified as potentially problematic (Conley (1999)). In our context, dependent variables and our explanatory variable possibly are spatially correlated. In order to address this concern, we compute Conley standard errors using a weighted average of spatial covariances, using a cut point of 25 meters.[9] We report these standard errors in brackets for all specifications presented. In general, spatial standard errors are similar to regular clustered standard errors, confirming that spatial correlation likely plays no role in our context.

As schools are distributed very close to each other given the high-density urban setting of São Paulo, we use the very granular geographic measures provided by our data and consider homicides within a 25 meters radius from school as measure of exposure avoiding that exposure to the same homicides overlaps across different schools. As a robustness check, we estimated regressions in Table 2.3 using homicides within a radius of 100 meters from school, we present the estimates in Table 2.16. The coefficients on Math test scores are consistently negative across specifications and of similar magnitude, but roughly 30 percent smaller. This reduction is likely driven by dilution of the original effect. When increasing the radius, homicides that previously were captured in the 25 meters radius now define exposure for additional schools, but are on average further away from schools and hence diluting the effect of exposure of the original estimates. We find a similar reduction in the effects on Portuguese test scores.

2.6.2 Selection

Given that we only consider students who attend the exams in our analysis, this may lead to a selection problem potentially introducing a bias to results we present in Table 2.3. If homicides in the school surroundings affect students' decisions to take the test and the propensity to attend differs systematically by student types, this could bias our results accordingly. Because this is low-stakes test and schools have generally little incentive to manipulate attendance of students at the test, the scope

[9] We also computed these standard errors at 100m cut-off, the results are unchanged.

for selection is relatively small. Overall, approximately 87 percent of students attend the test.

In order to rule out the possibility that the students taking SARESP are selected, we test if violence in the school surroundings affect attendance at Math and Language tests. For this purpose, we estimate the effect of exposure to homicides in the school surroundings on an attendance indicator separately for Math and for Portuguese. The coefficients in Table 2.15 are small and not statistically different from zero, which reassures that the results in Table 2.3 are not biased by selection.

2.6.3 Teachers' attendance

As teachers are also exposed to the violence around the school, we test whether the effects are driven by teacher absenteeism rather than by a direct effect on students by including teacher attendance as a control in specifications in columns (5) and (10) in Table 2.3. We present the results in Table 2.13, the difference in the coefficients when including teacher attendance is minimal. This is contrary to Monteiro and Rocha (2017) who state that the effect of exposure to drug battles on educational outcomes they find is at partially caused by teacher absenteeism and turnover.

2.7 Heterogeneous Effects

We are also interested in understanding how specific groups of students are affected differently by violence around the school. We investigate heterogeneity in the effect of violence on attendance and test scores by splitting the sample by cohort, by gender and by socioeconomic status.

2.7.1 Analysis by cohort

In Table 2.6 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: 5th and 9th grades of primary school and 3rd grade of secondary school. All specifications include time and school fixed effects and the full set of controls. The coefficients for Math are significant and sizeable in magnitude, particularly for students in 5th grade of primary school, for whom an additional homicide in the surroundings of the school during the year implies in a reduction of 7.9 percent of a standard deviation of Math proficiency. The effect is slightly smaller for 3rd grade of secondary school, 6 percent of a standard deviation. Students in 9th grade are the least affected, compared to the other mentioned cohorts, but exhibit still a considerable effect, with a 3 percent of a standard deviation reduction in Math test scores. The coefficients for Language are also larger for 5th graders compared to the other cohorts, but not significant. This pattern is consistent with two different mechanisms for the different age groups being at work: a negative effect induced by fear for the younger students and a negative effect due to the disincentives to invest in human capital for the older students.

We also estimate how the attendance of each cohort is affected by violence, the results are in Table 2.7. Each additional homicide in the year increases absences of 9th grade students by 1.9 percent. The effect is slightly smaller for 5th graders 1.3 percent. The coefficients for secondary school are not significant at the conventional levels of significance.

2.7.2 Analysis by gender

In Table 2.8 we present 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 that boys are more affected than girls, for each additional homicide around the school in the year, boy's Math proficiency decreases by 6.7 percent of a standard deviation. The effect on girls is about half this size, 3.1 percent of a standard deviation, and only significant at the 10 percent significance level when considering Conley standard errors.

Table 2.9 presents the effect of exposure to violence in the school surroundings on boys and girls' attendance in the year and in each semester. All specifications include time and school fixed effects and the full set of controls. Violence in the school surroundings affects both boys and girls, however, the coefficients for boys are larger, confirming a similar pattern documented for test scores.

2.7.3 Analysis by socioeconomic status

We use information on parental income and educational background to look at heterogeneous effects by socioeconomic status. First, we split the sample by income per capita and classify as *Low income* parents whose family income per capita is less than the median income in each year of the analysis and *High income* otherwise. Second, we analyse separately students whose both parents' level of education is at most primary school, defined as *Less educated* and students whose both parents have more than secondary school, defined as *More educated*.

In Table 2.10 we present the results of the effect of violence around the school on test scores for each of the defined categories. All specifications include time and school fixed effects and the full set of controls. Columns (1) and (2) compare Math test scores of low and high income children. We find a much more pronounced and statistically significant negative effect for low income students. We find the same pattern for language proficiency which reveals a stronger effect for lower compared to higher income students, as shown in columns (5) and (6). In columns (3) and (4) we compare Math proficiency of students by educational background of their parents. Although not significant at usual significance levels, results suggest that students whose parents are more educated are more affected and the same is true for the language test scores. We should emphasize, that all estimates in Table 2.10 include the full set of individual controls, i.e. in columns (1), (2), (5) and (6) we control for educational background of the parents, in columns (3), (4), (7) and (8) we control for income.

We also look at absences of students considering the same categorisation. Results in Table 2.11 are consistent with the patterns we find for test scores. We first present absences in the year, then in each semester. All specifications include time and school fixed effects and the full set of controls. In columns (1) and (2), we compare absences of low and high income students in the year. Although not statistically significant, results suggest that low income students are slightly more likely to be absent when there is a homicide in the surroundings of school. This difference is driven by differences in attendance in the first semester; results for the second semester are relatively balanced. When comparing absences by levels of education, we find that students whose parents are more educated are more likely to be absent in the event of a homicide in the school surroundings.

These results suggest that socioeconomic background plays an important role. Income seems to act as a buffer against the harmful effect of exposure to violence. Parents of higher SES may be better able to shield their children from the negative effect of the exposure to violence, for example through additional safety measures or by giving a sense of security by dropping and picking-up their children by car. This is also consistent with the large body of literature which has documented that parents' socioeconomic status may influence children's educational performance through their behaviour and beliefs. In particular, parents of a higher socioeconomic status are in general more likely to actively engage in their children's educational process, they 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 and Isen (2013), Mora and Escardíbul (2018)).

The contrary effects by education seem at first surprising. As we simultaneously also control for parental income, these results possibly point to a different mechanism at work. For the results on attendance, more educated parents possibly may have a better perception of the risks involved, and in the event of a homicide, they might be more cautious in sending their children to school. They may also have better means in compensating for missed days at school by substituting educational inputs at school with their own input. Without further evidence, these results though call for a cautious interpretation.

2.8 Mechanisms

In this section we investigate potential underlying mechanisms through which violence affects students' performance at school.

2.8.1 Bereavement effect

In order to check whether the effect we find is driven by grief due to the death of a peer or a teacher at the same school, we use information on deceased students and teachers from São Paulo State's Secretariat of Education. We identified the cause of death by linking these data with information on death records from *Datasus*. From the students' data, we identified 510 deceased students in the period of 2010 to 2013. In order to be able to identify the cause of death, we had to drop 10 observations with the same year of death, sex and date of birth. From the 500 left, we could successfully identify the cause of death of 353 cases. From those, 41 cases were victims of homicides, but only 4 of them happened in the public way. None of these 4 cases nevertheless occurred in 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 220 deceased teachers and we could identify the cause of death of 128 of the cases. From those cases, none of them were homicide victims. We are hence confident, that the effects are not due to grief of bereavement of peers or teachers of the students in our dataset.

To rule out the possibility that the variable *Homicides* is also capturing grief for the loss of 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 for Math proficiency in Table 2.20; the specification for all entries follow the most satiated specification of columns (5) of Table 2.3. 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. We hence can rule out a channel based on grief for the loss of an individual related to the students, either teacher, classmates or friends of the same age or younger.

2.8.2 Human capital accumulation

A substantial literature has documented the role of life expectancy for the human capital investment decisions of individuals (Becker (1964), Ben-Porath (1967), Jayachandran and Lleras-Muney (2009), Oster et al. (2013)). The gender specific results presented here are consistent with differences in the disincentives to invest in education for boys and girls linked to the pronounced differences in the probability of direct victimisation in a homicide by sex. This relates to a literature that has analysed how shocks to life expectancy that differ by sex, such as health and violence shocks, affect investments in education. Gerardino (2015) shows that when male-biased violence is high, boys are less likely to enrol in secondary school relative to girls. The author proposes two channels which might be responsible for this result: an increase in the opportunity cost of attending school and a reduction in the returns to education.

In the previous section, we find that the effect on boys is profoundly larger on both test scores and attendance. Boys seem to react much more strongly to the homicide exposure in the school surroundings. Simultaneously, recall that the vast majority of homicide victims are male; indeed more than 90 percent are male, as shown in Table 2.2. In Brazil, homicide is a leading cause of death for boys up to their mid-twenties. The difference in victimisation rates in homicides by sex might affect the perception of safety of males and females differently. The underlying mechanism behind our results may include a component related to the perceived returns to education that may be affected by directly experiencing homicides in the neighbourhood. Essentially, a non-negligible risk to die as homicide victim may impact the decision to invest in education.

When investigating the effects by grade, in Table 2.6 we found particularly pronounced effects for students towards the end of secondary school[10] and at the end of the first cycle of primary school[11]. The number of children victimised in homicides at the age of 11 is very small, but substantially larger at the age of 18, when possibly at least some of the children (mostly boys) may have had some exposure to criminal activities. One might therefore expect that differences by sex may be pronounced for the older cohort of students in secondary school if a human capital mechanism is at work.

To investigate this further and to separate a human capital effect from a mechanism that arises from the general fear of going to school, we break down the results by cohorts further by sex. We present the results in Table 2.18. Indeed, we find that results for Math in secondary school are much more pronounced for boys, whereas we find no effect for girls. In fact the estimate for girls is positive and close to zero. While

[10]Students in the final grade of secondary school would largely be around 18 years of age.
[11]Target age for students in 5th grade is 11 years.

there also exists a difference in the effects for the younger cohorts, the difference is much less pronounced and even inverted for 9th graders. For Portuguese language we find a pattern that is almost the inverse of the results for math, possibly indicating that the production of language and Math skills are fundamentally different. We also look at the effect on absenteeism by cohort and sex in Table 2.19. While we confirm that the effect on absenteeism is not present for the secondary school students, we find that the effects do not differ strongly by sex, with slightly more pronounced effects for boys in both, 5th and 9th grade.

2.8.3 Students' attendance

In section 2.5.2 we show that attendance is - apart from an outcome in its own right - also one of the possible mechanisms through which violence in the surroundings of school affects students' performance. Although we believe that attendance and test scores may be affected negatively by the change in incentives for boys to invest in education, we still would like to understand the role of absenteeism caused by exposure to homicides possibly facilitating the negative effects on test scores. In order to tease out how much of the results on test scores can be explained by absenteeism alone, we estimate specifications in columns (5) and (10) in Table 2.3 including students attendance as a control. Results in Table 2.12 show a decrease of about 17 percent of the Math coefficient and 20 percent decrease of Language coefficient. Although one needs to be careful when including an endogenous variable on the right hand side, this exercise may shed some light on the potential mechanisms. Interestingly, the inclusion of student attendance in either Math or Portuguese reduces the coefficient on test scores only very mildly. We interpret this as evidence, that attendance is only partially responsible for the negative effect on test scores suggesting possibly an underlying human capital mechanism driving the results on test scores (and possibly simultaneously of attendance).

2.9 Final Remarks

This paper uses georeferenced data on homicides for Brazil, and links these data with measures of schooling performance to estimate the causal effect of exposure to violence on schooling outcomes.

We find that students exposed to violence have lower performance in Math with larger effects for students in 5th grade of primary school and the last grade in secondary school. The coefficients for Language proficiency are negative and similar in magnitude to the coefficients in Math, but not significant at conventional levels of significance. We create indicator variables which identify high and low performance students and find that in the event of a homicide in the year, low achievers in Math are more affected compared to high achievers.

Violence around school also affects attendance of the students at school, especially in the second semester. Estimations show that one additional homicide in the year increases absences by 1.4 percent. These findings point to the potential role of attendance as a mechanism through which violence affects students' performance, calling attention to public policies aimed at improving safety conditions around the school.

We find heterogeneous effects of violence on test scores and attendance of boys and girls. In both cases, the effect is larger on boys, calling attention for another possible mechanism through which violence affects student's performance at school: a shift in the incentives to invest in human capital. We also provide evidence for parental income serving as a buffer against the negative effect of exposure to crime.

In addition, we look at the effect of violence in broader measures of school achievement for all the cohorts in primary and secondary schools. We find that one additional homicide in the school surroundings during the year increases the repetition rate by 11.5 percent. Dropout rates increase after exposure to violence in the school path from residence to school. We also find an effect on school transfers. Homicides around the school during the year increase transfers to alternative schools in the following year, and students are more likely to transfer to private schools, revealing a potential behavioural channel of parents as reaction to homicides in the school surroundings.

These results are important to quantify some of the costs of violence that go beyond the cost of direct victimisation. Even though we only measure the short term impact of violence, the negative effect we find on measures of school performance suggest that violence also affects human capital accumulation. Since poor neighbourhoods are often more violent, violence is potentially one additional contributor for the socioeconomic gradient we observe in many low and middle income countries plagued with high crime rates.

2.10 Tables and Figures

Table 2.1: Education summary statistics

	Students characteristics		
	Mean	Std.Dev.	Obs
Age			
06-10	0.315	0.464	10,400,046
11-15	0.462	0.499	10,400,046
16-18	0.201	0.400	10,400,046
18+	0.022	0.148	10,400,046
Demographics			
White	0.366	0.482	10,400,046
Black	0.031	0.173	10,400,046
Mixed	0.189	0.392	10,400,046
Male	0.503	0.500	10,400,046
School performance			
Repetition	0.075	0.263	8,873,385
Dropout	0.126	0.332	10,400,046
School progression	0.730	0.444	1,052,547
Private school transfer	0.005	0.072	10,400,046
Between year transfer	0.161	0.368	8,873,385
In year transfer	0.001	0.030	10,400,046
	School characteristics		
	Mean	Std.Dev.	Obs
General characteristics			
Federal	0.001	0.025	3,164
State	0.340	0.474	3,164
Municipal	0.165	0.371	3,164
Private	0.495	0.500	3,164
Has principal's office	0.889	0.262	3,164
Has teachers' office	0.943	0.190	3,164
Has computer lab	0.773	0.362	3,164
Has science lab	0.345	0.425	3,164
Has library	0.421	0.353	3,164
Number of school rooms in use	14.914	8.670	3,164
Has internet access	0.967	0.113	3,161
Number of staff members	60.777	45.636	3,164
School meals	0.583	0.456	3,164

Note: The table includes students from 1st grade of primary school to 3rd grade of secondary school over the period between 2007 and 2012. *Dropout* is a dummy variable which captures if the student drops out school in the successive year. *Private school transfer* captures students who transfer from a public to a private school in the following year. *Between year transfer* and *In year transfer* indicate whether the student transfers to a different school between or within the school year, respectively. *School progression* indicates if students in the last grade of primary school progress to secondary school in the subsequent school year, for that reason, this variable is calculated only for students at the final grade of primary school.

Table 2.2: Homicides characteristics

	Homicide victims characteristics	
	Mean	Std.Dev.
Age		
02-10	0.002	0.048
11-15	0.018	0.131
16-18	0.066	0.248
19-25	0.225	0.418
26-50	0.495	0.500
50+	0.194	0.395
Demographics		
Male	0.925	0.263
White	0.420	0.494
Black	0.100	0.300
Mixed	0.456	0.498
Single	0.642	0.480
Married	0.122	0.328
Separated	0.027	0.161
Education		
None	0.013	0.115
01-03 years	0.094	0.292
04-07 years	0.391	0.488
08-11 years	0.266	0.442
12+ years	0.032	0.176
	Homicide characteristics	
	Number	Percent
Assault by gun discharge	1,813	69.093
Assault by sharp object	282	10.747
Assault by blunt object	275	10.480
Assault by bodily force	152	5.793
Assault by other means	102	3.887
Total	2,624	100.000

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

Table 2.3: Effect of exposure to violence in the school surroundings on standardised test scores

	Math proficiency					*Language proficiency*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicides	−2.556	−2.281	−2.399	−2.428	−2.349	−2.111	−1.271	−1.266	−1.299	−1.188
	(2.553)	(1.036)**	(0.933)**	(0.945)**	(0.967)**	(2.969)	(1.015)	(1.006)	(1.027)	(0.977)
	[1.823]	[0.846]***	[0.783]***	[0.788]***	[0.808]***	[2.003]	[0.991]	[0.955]	[0.947]	[0.930]
Observations	666,718	666,718	666,718	666,718	666,718	666,453	666,453	666,453	666,453	666,453
School/time fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Teacher controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
School controls	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Classroom controls	No	No	No	No	Yes	No	No	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m 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, teacher 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; travel 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, 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 2.4: Effect of exposure to violence in the school surroundings on attendance

	Absences year		*Absences 1st semester*		*Absences 2nd semester*	
	(1)	(2)	(3)	(4)	(5)	(6)
Homicides (year)	0.016 (0.005)*** [0.003]***					
Homicides (year)		0.014 (0.004)*** [0.004]***				
Homicides (1st sem.)			0.017 (0.006)*** [0.004]***			
Homicides (1st sem.)				0.015 (0.004)*** [0.004]***		
Homicides (2nd sem.)					0.036 (0.004)*** [0.005]***	
Homicides (2nd sem.)						0.032 (0.005)*** [0.006]***
Observations	726,215	726,215	726,215	726,215	726,215	726,215
School/time fe	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Dependent variables are the percentage of absences in the year and in each semester. Explanatory variables *Homicides (year)* corresponds to the number of homicides within a 25m radius from school in the entire year; *Homicides (1st semester)* and *Homicides (2nd semester)* are the number of homicides within a 25m 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. **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; travel 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. **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, 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 2.5: Effect of exposure to violence on additional schooling outcomes

Panel A: Exposure in the school surroundings

	Repetition		*Dropout*		*Private school transfer*		*Between year transfer*		*In year transfer*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Homicides	0.011**	0.009*	0.011	0.009	0.001**	0.001**	0.042*	0.043*	0.000	0.000	−0.031*	−0.026
	(0.005)	(0.005)	(0.010)	(0.010)	(0.001)	(0.001)	(0.022)	(0.022)	(0.000)	(0.000)	(0.018)	(0.017)
Observations	7,698,069	7,698,069	8,580,404	8,580,404	6,237,778	6,237,778	6,897,926	6,897,926	8,944,932	8,944,932	1,047,110	1,047,110
R^2	0.050	0.124	0.022	0.082	0.010	0.012	0.065	0.248	0.002	0.002	0.052	0.122
School/time fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Panel B: Exposure in the residence surroundings

	Repetition		*Dropout*		*Private school transfer*		*Between year transfer*		*In year transfer*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Homicides	0.010*	0.002	0.034	0.041	0.000	0.000	0.069	0.003	−0.000	−0.000	0.018	0.007
	(0.005)	(0.005)	(0.023)	(0.030)	(0.001)	(0.001)	(0.044)	(0.020)	(0.000)	(0.000)	(0.018)	(0.017)
Observations	1,646,763	1,646,763	1,848,197	1,848,197	1,643,719	1,643,719	1,357,756	1,357,756	1,850,684	1,850,684	259,129	259,129
R^2	0.020	0.059	0.035	0.108	0.021	0.022	0.067	0.644	0.014	0.014	0.051	0.106
Neighb./time fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Panel C: Exposure in the school path

	Repetition		*Dropout*		*Private school transfer*		*Between year transfer*		*In year transfer*		*School progression*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Homicides	0.006	0.004	0.048*	0.050**	0.000	0.000	0.014	0.002	−0.001	−0.001	0.003	−0.004
	(0.004)	(0.004)	(0.024)	(0.024)	(0.001)	(0.001)	(0.024)	(0.011)	(0.000)	(0.000)	(0.025)	(0.020)
Observations	1,558,943	1,558,943	1,747,582	1,747,582	1,556,877	1,556,877	1,286,191	1,286,191	1,749,409	1,749,409	245,698	245,698
R^2	0.024	0.063	0.036	0.108	0.017	0.018	0.091	0.672	0.103	0.103	0.053	0.107
Corridor/time fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

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

Note: The analysis includes students from 1st grade of primary school to 3rd grade of secondary school over the period between 2007 and 2012. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m radius from school. 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. *Private school transfer* captures students who transfer from a public to a private school in the following year. *Between year transfer* and *In year transfer* indicate whether the student transfers to a different school between or within the school year, respectively. *School progression* indicates if 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 individual characteristics, school characteristics and classroom composition. **Individual controls** are: age, sex and race fixed effects; **Classroom controls** are: share of black students, share of girls and share of students above the appropriate age. **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, internet connection and if the school offers school meals.

Table 2.6: Effect of exposure to violence in the school surroundings on standardised test scores - 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	−3.970	−1.492	−3.039	−2.518	−0.185	−1.907
	(1.904)**	(0.757)**	(1.837)*	(2.866)	(1.660)	(2.576)
	[1.529]***	[0.917]	[1.468]**	[2.265]	[1.370]	[2.253]
Observations	237,000	308,311	121,407	236,735	308,311	121,407
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m 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, teacher 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; travel 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, 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 2.7: Effect of exposure to violence in the school surroundings on attendance - heterogeneous effects by cohort

	Absences 5th grade (primary school)	*Absences 9th grade (primary school)*	*Absences 3rd grade (secondary school)*
	(1)	(2)	(3)
Homicides	0.013	0.019	−0.004
	(0.006)**	(0.004)***	(0.006)
	[0.005]**	[0.006]***	[0.006]
Observations	247,122	337,888	141,205
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Dependent variables are the percentage of absences in the year. Explanatory variable *Homicides* correspond to the number of homicides within a 25m radius from school in the year. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. **Individual controls** are age, sex and race fixed effects, dummies indicating whether at home the student has access to daily newspaper, magazines, dictionary, novels, poetry and short stories books and encyclopaedias; travel 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. **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, 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 2.8: Effect of exposure to violence in the school surroundings on standardised test scores - heterogeneous effects by gender

	Math proficiency (boys)	*Language proficiency (boys)*	*Math proficiency (girls)*	*Language proficiency (girls)*
	(1)	(2)	(3)	(4)
Homicides	−3.352	−1.522	−1.570	−0.978
	(1.292)***	(1.312)	(1.030)	(1.445)
	[1.088]***	[1.133]	[0.864]*	[1.209]
Observations	329,159	328,903	337,559	337,550
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m 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, teacher characteristics, school characteristics and classroom composition. **Individual controls** are age 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; travel 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, 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 2.9: Effect of exposure to violence in the school surroundings on attendance - heterogeneous effects by gender

	Absences (year - boys)	*Absences (year - girls)*	*Absences (1st sem. - boys)*	*Absences (1st sem. - girls)*	*Absences (2nd sem. - boys)*	*Absences (2nd sem. - girls)*
	(1)	(2)	(3)	(4)	(5)	(6)
Homicides (year)	0.019 (0.005)*** [0.004]***					
Homicides (year)		0.010 (0.004)*** [0.004]***				
Homicides (1st sem.)			0.019 (0.006)*** [0.005]***			
Homicides (1st sem.)				0.013 (0.004)*** [0.004]***		
Homicides (2nd sem.)					0.034 (0.005)*** [0.008]***	
Homicides (2nd sem.)						0.028 (0.005)*** [0.005]***
Observations	360,828	365,387	360,828	365,387	360,828	365,387
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Dependent variables are the percentage of absences in the year and in each semester. Explanatory variables *Homicides (year)* corresponds to the number of homicides within a 25m radius from school in the entire year; *Homicides (1st semester)* and *Homicides (2nd semester)* are the number of homicides within a 25m 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. **Individual controls** are age 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; travel 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. **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, 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 2.10: Effect of exposure to violence in the school surroundings on standardised test scores - heterogeneous effects by socioeconomic 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	−2.556	−0.058	−0.434	−2.780	−2.376	0.203	0.608	−1.626
	(1.852)	(1.470)	(1.629)	(1.977)	(1.666)	(1.779)	(1.390)	(2.105)
	[1.316]*	[1.534]	[1.308]	[1.704]	[1.255]*	[1.368]	[1.338]	[1.729]
Observations	181,704	200,614	200,043	102,982	181,667	200,491	199,944	102,992
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m 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 both parents have more than primary school. All regressions include time and school fixed effects. Controls include individual characteristics, teacher characteristics, school characteristics and classroom composition. **Individual controls** are age 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; travel 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, 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 2.11: Effect of exposure to violence in the school surroundings on attendance - heterogeneous effects by socioeconomic status

	Absences year				*Absences 1st semester*				*Absences 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	0.004	0.001	0.001	0.005								
(year)	[0.003]	[0.003]	[0.003]	[0.003]								
	(0.003)	(0.002)	(0.003)	(0.002)**								
Homicides					0.006	0.003	0.004	0.006				
(1st semester)					(0.002)***	(0.002)	(0.003)	(0.004)				
					[0.002]***	[0.002]*	[0.003]*	[0.004]*				
Homicides									0.021	0.024	0.022	0.025
(2nd semester)									(0.005)***	(0.011)**	(0.010)**	(0.010)**
									[0.005]***	[0.009]**	[0.008]***	[0.008]***
Observations	188,656	207,610	208,297	106,194	188,656	207,610	208,297	106,194	188,656	207,610	208,297	106,194
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Dependent variables are the percentage of absences in the year and in each semester. Explanatory variables *Homicides (year)* corresponds to the number of homicides within a 25m radius from school in the entire year; *Homicides (1st semester)* and *Homicides (2nd semester)* are the number of homicides within a 25m radius from school in the first and second semesters. 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 both parents have more than primary school. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. **Individual controls** are age 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; travel 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. **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, 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 2.12: Effect of exposure to violence in the school surroundings on standardised test scores: the role of students attendance

	Math proficiency		*Language proficiency*	
	(1)	(2)	(3)	(4)
Homicides	−2.251	−1.864	−1.285	−1.029
	(0.935)**	(0.843)**	(0.938)	(0.882)
	[0.789]***	[0.753]**	[0.931]	[0.918]
Observations	651,471	651,471	651,216	651,216
School/time fe	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Teacher controls	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes
Classroom 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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m 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, teacher 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; travel 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, 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 2.13: Effect of exposure to violence in the school surroundings on standardised test scores: the role of teachers attendance

	Math proficiency		*Language proficiency*	
	(1)	(2)	(3)	(4)
Homicides	−2.349	−2.558	−1.188	−1.227
	(0.967)**	(1.061)**	(0.977)	(0.991)
	[0.808]***	[0.869]***	[0.930]	[0.945]
Observations	666,718	666,718	666,453	666,453
School/time fe	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Teacher controls	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes
Classroom controls	Yes	Yes	Yes	Yes
Teacher 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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m 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, teacher 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; travel 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, 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.

Annex

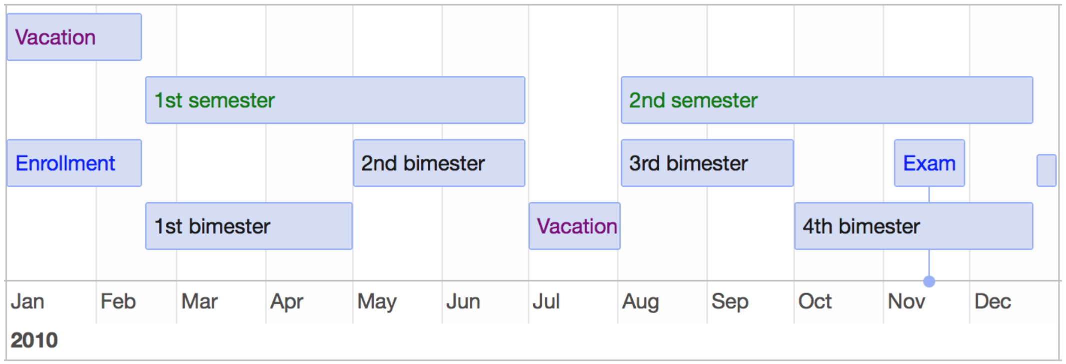


Figure 2.1: School Calendar in São Paulo

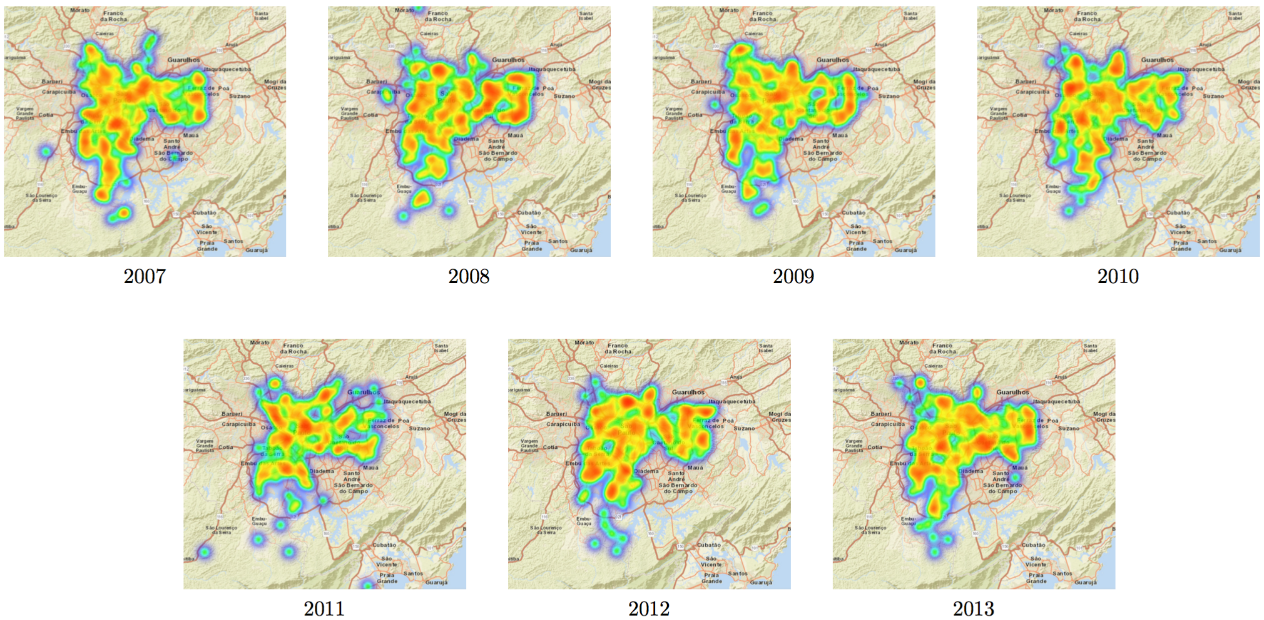
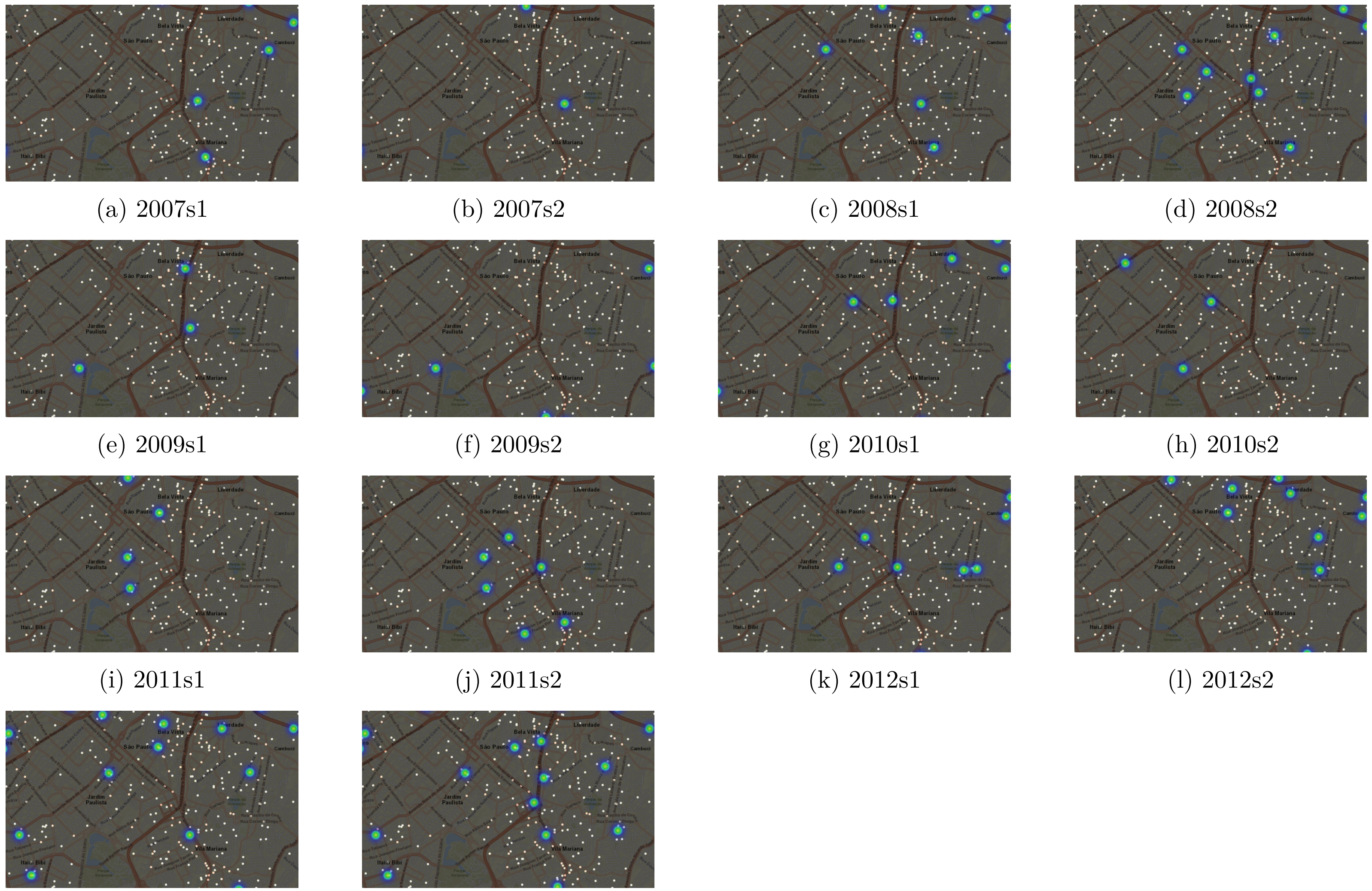


Figure 2.2: Homicides in the public way in São Paulo



(a) 2007s1 (b) 2007s2 (c) 2008s1 (d) 2008s2

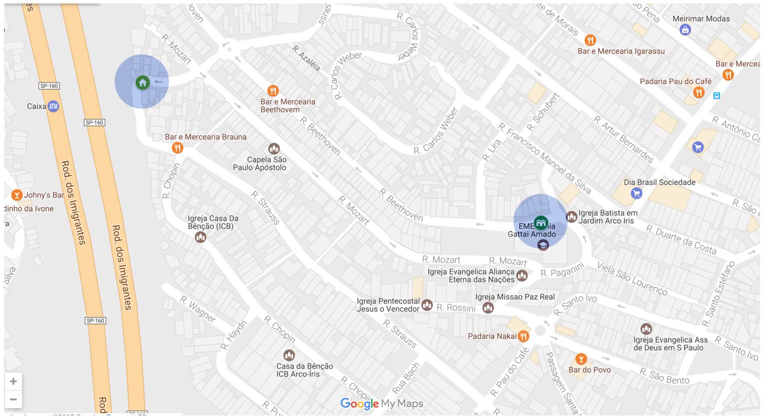
(e) 2009s1 (f) 2009s2 (g) 2010s1 (h) 2010s2

(i) 2011s1 (j) 2011s2 (k) 2012s1 (l) 2012s2

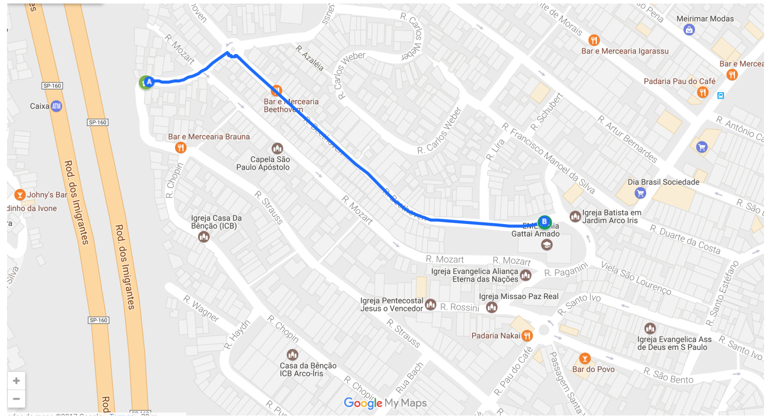
(m) 2013s1 (n) 2013s2

Figure 2.3: Homicides and schools in a São Paulo neighbourhood

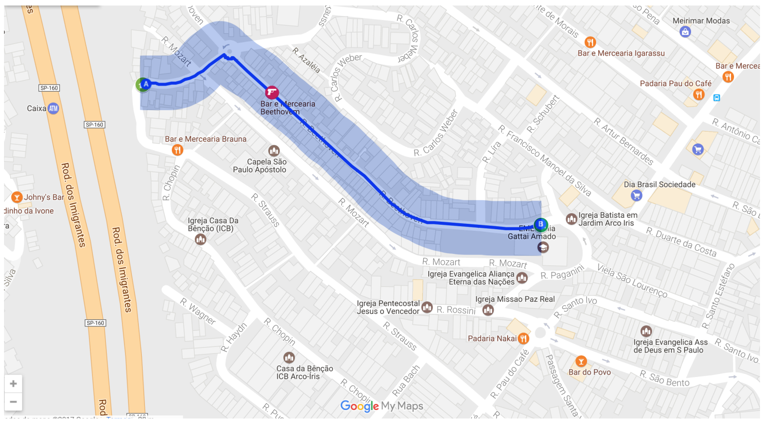
Note: Each individual map shows schools (white dots) and homicides (green circles) in a São Paulo neighbourhood in a semester.



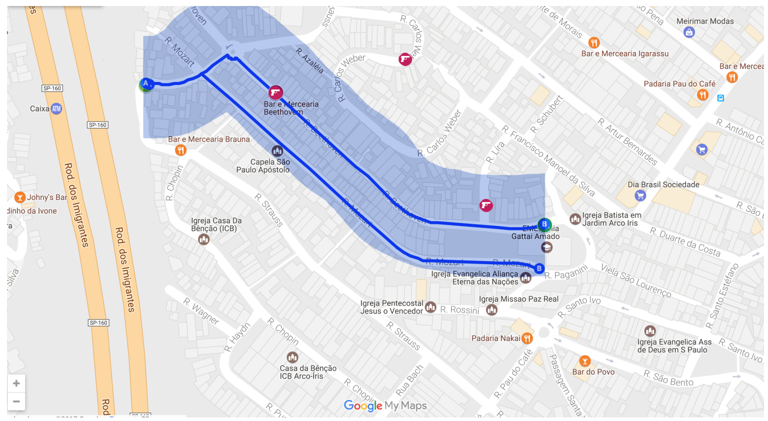
(a) School and residence radius



(b) Shortest walking distance from residence to school



(c) Corridor 1



(d) Corridor 2

Figure 2.4: Walking path from residence to school - Corridors

Table 2.14: Balancing tests

	Mean(Homicides=1)	Mean(Homicides=0)	Diff.	Std. Error
Students characteristics				
Age	14.2171	13.1497	-1.0675*	0.5734
Female	0.4830	0.4978	0.0149	0.0120
White	0.5470	0.5750	0.0280	0.0285
Black	0.0634	0.0572	-0.0063	0.0079
Mixed	0.3734	0.3564	-0.0170	0.0273
Income	1620.1821	1595.2596	-24.9225	66.8630
Own home	0.5024	0.4622	-0.0402	0.0307
Rent home	0.4976	0.5378	0.0402	0.0307
Father's education: low	0.5930	0.6067	0.0137	0.0258
Father's education: mid	0.2791	0.2785	-0.0007	0.0218
Father's education: high	0.0614	0.0504	-0.0110	0.0095
Mother's education: low	0.5678	0.5700	0.0022	0.0287
Mother's education: mid	0.3381	0.3464	0.0083	0.0238
Mother's education: high	0.0658	0.0558	-0.0100	0.0105
Father's employment: has a job	0.4290	0.4388	0.0098	0.0283
Father's employment: has a temp. job	0.1273	0.1530	0.0257*	0.0133
Father's employment: has no job	0.0355	0.0358	0.0002	0.0050
Mother's employment: has a job	0.3358	0.3715	0.0357	0.0255
Mother's employment: has a temp. job	0.1094	0.1251	0.0157	0.0113
Mother's employment: has no job	0.1282	0.1185	-0.0097	0.0122
Travel time from home to school (in min.)	34.9722	34.5635	-0.4087	0.9999
Number of people in the house	4.4453	4.4750	0.0297	0.0711
Has at home: newspapers	0.2182	0.2224	0.0042	0.0139
Has at home: magazines	0.3262	0.3337	0.0075	0.0166
Has at home: dictionary	0.8668	0.8636	-0.0032	0.0190
Has at home: books	0.8225	0.8040	-0.0184	0.0184
Has at home: scientific books	0.7640	0.7605	-0.0035	0.0159
Has at home: water supply	0.9712	0.9734	0.0023	0.0094
Has at home: sewage supply	0.8795	0.8893	0.0098	0.0198
Has at home: electricity supply	0.9734	0.9730	-0.0004	0.0067
Has at home: gas supply	0.2238	0.2346	0.0109	0.0224
Has at home: waste collection	0.9226	0.9247	0.0021	0.0111
Has at home: television	0.9620	0.9637	0.0016	0.0064
Has at home: radio	0.8135	0.8146	0.0011	0.0171
Has at home: bathroom	0.9241	0.9152	-0.0089	0.0124
Has at home: car	0.4838	0.4646	-0.0192	0.0263
Has at home: maid	0.0686	0.0795	0.0109	0.0085
Has at home: vacuum cleaner	0.3526	0.3424	-0.0102	0.0266
Has at home: washing machine	0.8547	0.8580	0.0033	0.0170
Has at home: DVD player	0.8655	0.8818	0.0163	0.0123
Has at home: refrigerator	0.9277	0.9313	0.0036	0.0099
Has at home: freezer	0.4785	0.4813	0.0028	0.0291
Has at home: telephone	0.7080	0.6659	-0.0421	0.0277
Has at home: computer	0.7611	0.7303	-0.0308	0.0322
Has at home: cable TV	0.5164	0.5225	0.0061	0.0352
Has at home: microwave	0.7707	0.7634	-0.0073	0.0219
Schools characteristics				
Computer lab	0.8947	0.9384	0.0437	0.0554
Science lab	0.3684	0.3023	-0.0661	0.1057
Library	0.1053	0.1006	-0.0047	0.0692
Internet	1.0000	0.9766	-0.0234	0.0347
School meals	1.0000	1.0000	0.0000	0.0000
Staff members	92.5263	75.2588	-17.2675**	7.1247
Number of school rooms in use	17.3684	15.4367	-1.9318	1.3975

Note: 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 2.15: Attendance at Math and Language tests

	Attendance at Math test	*Attendance at Language test*
	(1)	(2)
Homicides	−0.005	−0.001
	(0.008)	(0.007)
	[0.008]	[0.007]
Observations	767,069	767,069
School/time fe	Yes	Yes
Controls	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m 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, teacher 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; travel 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, 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 2.16: Effect of exposure to violence in the school surroundings on standardised test scores - 100 meters radius

	Math proficiency					*Language proficiency*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicides	−1.478	−1.365	−1.364	−1.382	−1.459	−0.986	−0.737	−0.694	−0.719	−0.802
	(1.133)	(0.770)*	(0.756)*	(0.745)*	(0.749)*	(1.188)	(0.670)	(0.660)	(0.654)	(0.644)
	[0.895]*	[0.622]**	[0.612]**	[0.599]**	[0.601]**	[0.855]	[0.566]	[0.573]	[0.567]	[0.561]
Observations	666,718	666,718	666,718	666,718	666,718	666,453	666,453	666,453	666,453	666,453
School/time fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Teacher controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
School controls	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Classroom controls	No	No	No	No	Yes	No	No	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 100m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 100m 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, teacher 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; travel 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, 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 2.17: Effect of exposure to violence in the school surroundings on standardised test scores - levels of proficiency

	Math high level	*Math low level*	*Language high level*	*Language low level*
	(1)	(2)	(3)	(4)
Homicides	−0.005	0.022	−0.002	0.001
	(0.002)**	(0.012)*	(0.003)	(0.011)
	[0.002]***	[0.009]**	[0.003]	[0.010]
Observations	666,718	666,718	666,453	666,453
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th, 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m radius from school. Variables *Math high level* and *Language high level* are dummy variables indicating whether the students reach the 'advanced' level in these subjects. Variables *Math low level* and *Language low level* show if the student's test scores are considered in the 'below the basic' level in these subjects. All regressions include time and school fixed effects. Controls include individual characteristics, teacher 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; travel 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, 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 2.18: Effect of exposure to violence in the school surroundings on standardised test scores - heterogeneous effects by cohort and gender

	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)	(7)	(8)	(9)	(10)	(11)	(12)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	−6.011	−1.872	−1.317	−1.799	−7.979	0.483	−5.707	0.982	0.547	−1.110	−2.245	−1.842
	(2.476)**	(2.024)	(1.468)	(0.692)***	(1.988)***	(3.295)	(3.018)*	(3.537)	(2.199)	(1.383)	(2.054)	(4.347)
	[2.036]***	[1.697]	[1.311]	[1.005]*	[1.856]***	[2.453]	[2.511]**	[2.793]	[1.674]	[1.446]	[2.617]	[3.120]
Observations	120,724	116,276	156,260	152,051	52,175	69,232	120,468	116,267	156,260	152,051	52,175	69,232
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* corresponds to the number of homicides within a 25m 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, teacher 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; travel 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, 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 2.19: Effect of exposure to violence in the school surroundings on attendance - heterogeneous effects by cohort and gender

	Absences 5th grade (primary school)		*Absences 9th grade (primary school)*		*Absences 3rd grade (secondary school)*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*
Homicides	0.014	0.011	0.022	0.017	0.004	−0.010
	(0.008)*	(0.006)*	(0.006)***	(0.003)***	(0.007)	(0.008)
	[0.006]**	[0.005]**	[0.006]***	[0.006]***	[0.006]	[0.007]
Observations	127,356	119,766	173,302	164,586	60,170	81,035
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides* correspond to the number of homicides within a 25m radius from school in the year. All regressions include time and school fixed effects. Controls include individual characteristics, school characteristics and classroom composition. **Individual controls** are age, sex and race fixed effects, dummies indicating whether at home the student has access to daily newspaper, magazines, dictionary, novels, poetry and short stories books and encyclopaedias; travel 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. **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, 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 2.20: Effect of exposure to violence in the school surroundings on standardised test scores - specific groups of victims

	Math proficiency			
	(1)	(2)	(3)	(4)
Homicides (all victims)	−2.349 (0.967)** [0.808]***			
Homicides (18+ victims)		−2.419 (1.019)** [0.848]***		
Homicides (male victims)			−2.761 (1.009)*** [0.782]***	
Homicides (gunshot victims)				−2.691 (1.251)** [0.938]***
Observations	666,718	666,718	666,718	666,718
School/time fe	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 25m cutoff distance in brackets.

Note: The analysis includes students in 5th and 9th of primary school and 3rd grade of secondary school, over the period between 2010 and 2013. Explanatory variable *Homicides (all victims)* corresponds to the number of homicides within a 25m radius from school; *Homicides (18+ victims)* corresponds to the number of homicides within a 25m radius from school for which the victims are 18 years old or older; *Homicides (male victims)* corresponds to the number of homicides within a 25m radius from school for which the victims are males; *Homicides (gunshot victims)* corresponds to the number of homicides within a 25m radius from school for which the victims were gunshot; Dependent variable *Math proficiency* are standardised test scores normalised at a (250,50) scale. All regressions include time and school fixed effects. Controls include individual characteristics, teacher 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; travel 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, 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.

Geographic coordinates and school-residence corridors

In order to define the measures of exposure to violence we use, it was necessary to geocode the addresses of the schools, residences and homicides. For the schools, we have available 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 in several postcodes. For the homicides we also have the precise location for each case.

We used 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 we geocoded, 96 percent of the schools and 97 percent of the residences were geocoded at the street level, 95 percent of the homicides in the public way were also geocoded at the street level.

We also used Google maps API's to calculate the corridors from residence to school. We used Google Directions API and calculated path polylines of walking transport mode for each school/residence pair, which we call *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 distance from residence to school and from residence to the *HEP*.

In order to make those calculations feasible and limit the time necessary to run them, we defined some filter rules:

- Define the threshold distance between the homicide points and the path polylines to 500m.
- Ignore walking mode if straightline distance is greater than 15km.
- Define $double\_distance = max(straightline\_distance * 2, 500 * 2)$: if $double\_distance$ is greater than 100km, ignore homicide point outside the circle with radius $double\_distance/2$ and centre as the middle of straight line between school and residence; if $double\_distance$ is less than or equal to 100km, ignore homicide point if the straight line distance between homicide point and either of school location and residence location is greater than the double distance.

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

Chapter 3

Estimating the Effect of Criminal Victimisation on Birth Outcomes

3.1 Introduction

Becoming victim of crime in a robbery or theft is a major concern for citizens around the world. The reality of Brazil, and indeed many other countries suffering from very high crime rates, is one of exposure to everyday violence and crime. Robberies often involve the use or threat of use of violence, including firearms or knives, often leading to traumatic experiences of the victims involved. This is reflected in crime being a top concern for citizens in the region.[1] In addition to the financial loss, victimisation in crime is linked to a myriad of adverse effects on victims, ranging from the direct health consequences due to injury (Miller et al. (1993)) and reductions in life expectancy (Auger et al. (2016)), the economic impact from lost productivity and material loss including criminal damage (Cohen et al. (2004)), and the psychological costs of becoming a victim (Hanson et al. (2010)).

[1] Available from `http://www.latinobarometro.org`

Most of literature relies on cross-sectional observations, where victimisation is often self-reported, and hence merely provide correlates of victimisation and a number of outcomes. The use of survey data also restricts the analysis from investigating rare events or events focussed on smaller populations, such as the effect of victimisation during pregnancy on birth outcomes. Given the violent nature of many of these incidents in the Brazilian context, being victimised in a robbery may provide for a traumatic experience for the mother and may in turn affect the development of the fetus in utero.

In this paper, we investigate the effect of individual victimisation in robbery and theft during pregnancy on health outcomes at birth, including birthweight, gestational length, and fetal and infant mortality making use of a unique dataset from Brazil linking the universe of crime reports - including information on all victims - with the universe of birth records for the period between 2012 and 2015.

There is a growing literature in economics estimating the effects of stressful events linked to violence on birth outcomes of children exposed in-utero. These events include terrorist attacks, war, crime waves and everyday violence. For example, Camacho (2008) estimates the effect of exposure to landmine explosions in Colombia during the time in-utero on birthweight. She finds that exposure to landmine explosion during the first trimester leads to a 9 gram reduction in birthweight. Quintana-Domeque and Ródenas-Serrano (2017) also focuses on terrorist attacks, but in the context of ETA terrorism in Spanish provinces and find that in-utero exposure early in gestation increases the prevalence of low birthweight deliveries and reduces gestational length.

There is also a number of papers which study the effect of maternal stress associated with the 9/11 attacks in the US on birth outcomes (Ecclestone (2012), Eskenazi et al. (2007)), although they struggle to isolate the effect of stress from possible confounders linked to exposure to pollutants for mothers resident in New York City (Currie and Schwandt (2015)); Mansour and Rees (2012) focus on pregnant women resident in Palestine exposed to violence linked to the Second Intifada using non-combatant fatalities as explanatory variable. They find a modest fall in birthweight for unborn babies of mothers exposed to higher fatalities. Brown (2018) focuses on the secular increase in homicides linked to the war on drugs in Mexico. Contrary to other papers, he finds a positive effect of the escalation of homicides over the period between 2008 and 2010 on birthweight and argues that this effect is due to increased pre-natal care utilisation. Foureaux Koppensteiner and Manacorda (2016) estimate the effect of exposure to homicides in rural Brazil on birthweight. They find that exposure early in gestation leads to a reduction in gestational length, a reduction in birthweight and an increase of the number of babies categorised as low birthweight. These papers have in common that they use indirect exposure to stressful events, and hence provide intention-to-treat estimates with often relatively small coefficients. The indirect exposure also makes it more difficult to investigate the underlying mechanisms.

In this paper, we depart from these previous studies in a number of ways. Firstly, by linking the universe of police reports that include information on all victims of crime to the universe of birth records, we are able to study the effect of individual victimisation, and hence exposure to stressful events at the individual level. Secondly, rather than focussing on large shocks, such as terrorist attacks, or conflict escalation, we focus on everyday crime, hence making the effects more generalisable to many other contexts. We make use of the crime categorisation from police records and focus on the two largest crime categories that involve a victim, robbery and theft. Because these two distinct types of crime have different implications for the channels underlying any estimated effect, this allows us to investigate a number of competing hypothesis on the mechanisms. To estimate the effect of victimisation on birth outcomes, we exploit that - conditional on place of residence, hospital of birth and time fixed effects - becoming a victim of crime is quasi-random.

We find that victimisation in robbery during the first trimester significantly reduces birthweight by about 60 grams, equivalent to approximately 10 percent of a standard deviation of birthweight. We also find that victimisation increases the prevalence of low birthweight. The effects are concentrated in the first trimester, a pattern consistent with findings elsewhere in the literature that maternal stress affects birth outcomes early in pregnancy. In contrast to robbery, victimisation in theft only has an effect late in pregnancy. We find that theft victimisation in the third trimester leads to a reduction of birthweight and a reduction in gestational length. The effects are particularly pronounced for very low birthweight, pointing to an underlying mechanism related to the economic shock related to theft.

We also find important heterogeneous effects along a number of maternal characteristics. For robberies, we find that estimates in the first trimester are driven by the effect for white mothers and the relatively more educated. This points to a potential adaptation to crime effect: mothers less likely to be exposed to crime may react stronger to their own victimisation. On the contrary, for the effects concentrated late in pregnancy due to theft, we find that the effects are more pronounced for non-white mothers and relatively less educated, supporting the interpretation of these effects as economic shocks. We also document a strong selection of live births. Victimisation in robbery and theft leads to a substantial increase in fetal deaths throughout pregnancy, with particularly strong effects for robbery. This also means that the estimated negative coefficients on measures of health at birth for robbery in the first and for theft in the third trimester are likely underestimating the true impact of victimisation.

The remainder of the paper is organised as follows. Section 3.2 describes the datasets used in the analysis. Section 3.3 details the identification strategy applied to estimate the causal effect of crime victimisation on birth outcomes. Section 3.4 analyses the results and section 3.5 presents the final remarks.

3.2 Data

To enable the estimation of the causal effect of criminal victimisation on birth outcomes, we construct a novel data set by combining data from two Brazilian institutions: the Brazilian Ministry of Health and the Secretariat of Public Safety. We combine these data using unique individual identifiers.

3.2.1 Birth data

We use data from the Brazilian Ministry of Health collected through the Life Birth Information System[2], over the period between 2012 and 2015. These data come from birth certificates, containing the universe of births in the state, and provide information on the characteristics of the mother, pregnancy, delivery and newborn's health.[3]

We present summary statistics in Table 3.1. The main measure of newborn's health we use is birthweight. Mean birthweight in our sample is around 3,150 grams.[4] We also look at the distribution of birthweight by creating indicator variables for low and very low birthweight, for a weight of the newborn up to 2,500 and 1,500 grams, respectively. The incidence of low birthweight is around 8.6 percent, whereas 1.2 percent of newborns are classified as very low birthweight.[5] We also calculated fetal growth, which is defined as birthweight divided by the number of gestation weeks; we find a mean of 81 grams for each week of gestation.

The data also contains information on APGAR scores, which evaluate newborns's health by considering their Activity (muscle tone), Pulse (heart rate), Grimace (reflex

[2] *Sistema de Informações sobre Nascidos Vivos (SINASC)*, in Portuguese.

[3] The data are collected from all births. Where the births occurs in the hospital - the vast majority of cases - the data is sent directly to the state secretariat of health; where the birth occurs in the residence, the attending midwife reports the information to the secretariat.

[4] This is about 300 grams less than mean birthweight in the US (Donahue et al. (2010)).

[5] This is roughly in line with rates for the US, with 8.2 and 1.4 percent, respectively. See Martin et al. (2016).

irritability), Appearance (skin colour) and Respiration (respiratory effort).[6] The test is performed at the 1st and 5th minutes after birth, and it is meant to be an objective way to determine whether the baby needs immediate medical care. Mean Apgar scores are 8.4 and 9.4 for 1st and 5th minute, respectively.

We determine the precise gestational length by using date of the mother's last menstrual period - reported in the birth records - and the date of delivery; we find an average gestational length of 272 days. To investigate the effect of victimisation along the distribution of gestation, we created indicator variables for preterm, very preterm and extremely preterm delivery, defined as gestational periods which last less than 259 days (37 weeks), less than 224 days (32 weeks) and less than 196 days (28 weeks), respectively. There is an incidence of 12.7 percent of low gestation and an incidence of very low of 1.8 percent, and extremely low gestation of 0.6 percent.[7]

In addition to information on health outcomes of live births, the SINASC data also contain information on pregnancy and delivery. Prenatal visits are free in the public health system and antenetal care is generally of high quality in Brazil (Victora et al. (2011)). On average, women have around just above 8 prenatal care visits.[8] There is a small share of twin and triplets or more pregnancies, 2.1 percent and 0.1 percent, respectively and 97.7 percent are singleton births. Almost 60 percent of deliveries were through C-section and only 21.1 percent are initiated after labour began, thus defined as emergency C-section.[9]

Table 3.1 also presents a range of characteristics of the mothers. Their mean age is 27 years, 39 percent of them is younger than 25. At least 50 percent of the

[6]Each criteria is scored 0, 1 or 2 depending on the observed newborn's condition. Overall, scores of 7 and above are considered normal and a score of 10 represents a baby in the best possible condition.

[7]This indicates a higher rate of preterm births (<37 weeks) in Brazil than in the US with a rate of 9.9 percent, but a lower rate for early preterm births (<34 weeks) with 2.8 percent for the US in 2016 (Martin et al. (2016)).

[8]These include extensive screening for risk factors including diabetes, pre-eclampsia, underlying infections and ultrasound scans of the fetus.

[9]Brazil has very high rates of planned caesarean section delivery. These are already historically documented (Barros et al. (1991)).

mothers declare themselves as mixed race, 34.3 percent are white and 6.7 percent are black. Detailed information on the marital status of the mother is also provided. 34.1 percent are single, 46.3 percent are married, 16.7 are in a stable union, 0.3 percent are widowed, and 1.5 percent separated. As to their education background, 21.6 percent have low education (up to 7 years of schooling), 57.4 mid education (8 to 11 years of schooling) and 18.1 percent high education (12 or more years of schooling).

3.2.2 Death in uterus and infant mortality data

We also use data from the Brazilian Mortality Information System[10]. This data set contains information on all natural and non-natural deaths in Brazil, including detailed cause of death and characteristics of the deceased. In case of fetal death and death occurring up to the age of one, these data also register the characteristics of mothers and birth outcomes, hence allowing us to link birth records with information on child mortality.

In case of infant mortality - after birth and up to the age of one year - we linked this information to the birth data using their unique birth identifier. Cases of death in utero do not have a birth identifier, as technically no life birth occurs, but a death certificate is issued. We identified all these cases from the mortality data and appended these observations to the birth data. We present summary statistics on cases of death in uterus in Table 3.11 in the Annex.[11]

[10] *Sistema de Informações sobre Mortalidade (SIM)*, in Portuguese.

[11] Not surprisingly, reported weight of the fetus and gestational length are far lower than for live births. Interestingly, mothers with miscarriages and stillbirths are three times more likely to be victimised in robbery and theft, but we cannot give these correlates a causal interpretation. This becomes apparent when comparing the fraction of mothers with low education for life and still births, indicating a clear socioeconomic gradient in miscarriage and still birth.

3.2.3 Crime data

We have access to all cases of robbery and theft reported to the police over the period between 2012 and 2015 from the Register of Occurrence[12]. Whenever police are called to an incidence or when a crime is reported to police, a new entry into the SRO is produced. These data are available on a daily basis and contain detailed information on all crimes registered with police and on the victims of crimes. Using unique individual identifiers, we were able to merge the crime data with the birth records data.

As mothers with longer gestational period are more likely to be victimised, we used the date of the mothers' last menstrual period to define trimesters of the same length to all mothers. In this case, we defined the first trimester as the date of last menstruation - considered by the medical literature as the conception day - plus 93 days. Similarly, we constructed second and third trimesters lasting 93 and 94 days, respectively. By doing this, we look at exposure to crime in the common time window of 280 pregnancy days.

We find that at least 1,234 mothers were victims of robbery and 2,948 were victims of theft at least once during their pregnancies. In order to separate the stress from the physical channel, we coded as missing some cases of robbery with injury, leaving us with 1,169 cases of robbery in the data set. In the bottom part of Table 3.1, we present the share of mothers that were victimised in each trimester and in Table 3.2 we present some of the crime characteristics.

3.3 Identification Strategy

In order to estimate the causal effect of crime victimisation on birth outcomes we need to deal with confounding factors. For instance, mothers of lower socioeconomic

[12] *Sistema de Registro de Ocorrência (SRO)*, in Portuguese.

status who live in poorer neighbourhoods, may be more likely to be victimised in crime. Their lower socioeconomic status may nevertheless directly impact the health of their unborn child in a myriad of ways, for example through a nutritional channel.[13] The literature has documented how low socioeconomic status is related with stress, health and short-sighted and risk-averse decision making, which in turn may negatively affect newborn's health (Dohrenwend (1973), Case et al. (2002), Deaton (2002), Haushofer and Ernest (2014)).

In addition, socioeconomic status is also related with levels of education. Higher levels of maternal education have been linked to improvements in birth outcomes, such as birthweight and gestational length. In particular, more educated mothers are more likely to be married, have more educated husbands, use prenatal care, reduce smoking, reduce fertility and thus invest more in their children (Currie and Moretti (2003)). Because of this, using cross-sectional variation on criminal victimisation and birth outcomes would lead to erroneous inference on the relationship between criminal victimisation and measures of health at birth.

We overcome the endogeneity problem of criminal victims by comparing mothers residing in the same municipality who give birth in the same hospital in a two-way fixed effect model. Municipality fixed effects deal with institutional differences across administrative units, including different policing and reporting of crime, as well as differences in the provision of prenatal care at the municipality level and socioeconomic conditions that may vary by municipality.[14] Hospital fixed effects capture the socioeconomic environment in the catchment area of hospitals. In addition, because we have birth records from all hospitals, public and private, hospital fixed effects

[13]There is a substantial literature showing how poor nutrition during gestation leads to adverse effects on later life outcomes, and impacts health beyond birth outcomes (Chen and Zhou (2007), Lindeboom et al. (2010)) or an adverse disease environment, including poorer sanitation (Rocha and Soares (2015), Maccini and Yang (2009), Almond and Doyle (2011), Almond et al. (2012), Amarante et al. (2016), Bozzoli and Quintana-Domeque (2016)).

[14]The provision of public health care and prenatal care is organised at the level of the municipality through the Family Health Program (Rocha and Soares (2010)).

will also deal with compositional differences in women accessing different hospitals in their neighbourhood. In all specifications, we include month of conception fixed effects to capture time trends. We also make use of a large set of controls which contains a range of observed characteristics of the mother and of the pregnancy, such as age, race, marital status, educational background, dummies for singleton, twins and triplets or more, and dummies for the number of children born alive and stillbirths from previous pregnancies.

Because gestational length may mechanically affect the propensity of victimisation towards the end of pregnancy, i.e. that mothers with longer gestational length have more chances to be victimised, we make use of the very rich information on the pregnancy in our data set. First, we construct date of conception from information on the date of the last menstruation.[15] Second, we assign equivalent gestational lengths to all mothers in an intention-to-treat framework: starting from conception date (defined by the medical literature as the date of the last menstrual period) we consider a full term gestation of 280 days. We then split the gestational period into three trimesters; the first and second trimesters last 93 days and the third 94 days. We then identify the number of times a mother was victimised in each of the above defined trimesters.

Equation 3.1 summarises the model we estimate:

$$y_{imht} = \beta_0 + \beta_1 victim_{trim1_i} + \beta_2 victim_{trim2_i} + \beta_3 victim_{trim3_i} + X_i\beta_4 + d_m + d_h + d_t + u_{imht} \tag{3.1}$$

y_{imht} is the outcome of interest related to mother i in municipality m, hospital h at time t; $victim_{trim1_i}$, $victim_{trim2_i}$ and $victim_{trim3_i}$ measure the number of times a

[15] This has the advantage of minimising the risk of mis-attributing victimisation to the wrong trimester or to periods before conception, when constructing trimesters by working backwards from date of birth using information of gestational length (Quintana-Domeque and Ródenas-Serrano (2017)).

mother i was a victim of robbery or theft in each trimester of gestation; X_i is a vector of mother and pregnancy observed characteristics; d_m, d_h and d_t are municipality of residence, hospital and month of conception (both linear and calendar) fixed effects; u_{imht} is the error term. Standard errors are clustered at the municipality level.

Conditional on municipality, hospital, and month of conception fixed effects mother's exposure to victimisation in crime is as good as random and the coefficients in the above model will provide us with causal estimates of victimisation in crime on birth outcomes. In addition, we have available a comprehensive set of controls on (predetermined) mother and pregnancy characteristics. Given the above identification strategy, their inclusion merely should reduce sampling variability. The estimation framework also allows us to test the identification strategy by including leads of victimisation indicators: victimisation after birth should not impact birth outcomes.

3.4 Results

In this section, we present the results of the effect of criminal victimisation during pregnancy on a number of outcomes. We start with birthweight and gestational length in Subsection 3.4.1. In Subsection 3.4.3, we look at additional birth outcomes including type of delivery, APGAR scores, and prenatal care. We also study miscarriage and stillbirth as outcomes, as well as a number of different infant mortality rates.

3.4.1 Effect of crime victimisation on birthweight and gestational length

We separately investigate how being victimised in a robbery or theft during the gestational period affects newborns' health. One may expect robbery and theft to have very different effects on birth outcomes, depending on the underlying mechanism. The main effect of theft likely arises from the economic damage caused by the theft, in particular the economic loss associated with burglaries. As theft does not include the element of use of force or threat of use thereof, we do not expect an accentuated stress reaction compared to being victimised in a robbery. Robberies in Brazil often involve the use of either firearms or knives and are known to carry the risk of serious injury or even death, in particular if victims are non-cooperative. We exclude a relatively small number of robberies that result in physical injury (n=65, this is equivalent to approximately 5 percent of all robberies).[16] Given the lack of detail on the nature of the injury, this is to insure that effects are not driven by the direct effect of victimisation through the direct physical harm of the unborn child in utero.

We first investigate the effect of victimisation on birthweight as measured in grams. We next look at the distribution of effects along the birthweight distribution and estimate the effect of victimisation on indicators for low (<2,500 grams) and very low birthweight (<1,500 grams) and the combined measure of weight-for-gestational-age fetal growth. Table 3.3 presents the results for robbery and Table 3.5 the results for theft.

In column (1) of Table 3.3 we estimate the effect of being a victim of robbery in each trimester of pregnancy on birthweight, when including only municipality of residence, month of conception fixed effects and a dummy for calendar month. Standard errors are clustered at the municipality level. We find a large and significant negative effect of victimisation in the first trimester on birthweight. Being a victim of robbery reduces birthweight by about 54 grams on average. This corresponds to an effect equivalent to 10 percent of a standard deviation of birthweight given the standard deviation of birthweight of 530 grams. In column (2), we add a large set of controls which includes dummies for mother's age, race, marital status and education;

[16] In Table 3.14 in the Annex we present the main estimates including these cases. Given the small number of robberies with injury, the difference in coefficients is minimal.

dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies; and in column (3), we add hospital of birth fixed effects. Across these specifications, the coefficients vary only little. The inclusion of individual controls strengthens the effect, possibly because of heterogeneous effects along a number of mother and birth characteristics. Including additional hospital fixed effects, which account for the conditions in the surroundings of the residence of pregnant mothers, does not change the coefficient in any meaningful way, lending extra credibility to our identification strategy. Considering the most saturated specification, for each additional case of robbery in the first trimester of pregnancy we find a reduction of around 59 grams of average birthweight. The estimated negative effects concentrated in the first trimester are in line with findings elsewhere in the literature that maternal stress manifests its negative effects on birth outcomes in the first trimester of pregnancy (Bozzoli and Quintana-Domeque (2016), Mansour and Rees (2012), Quintana-Domeque and Ródenas-Serrano (2017)).

Our results are much more pronounced compared to effects reported elsewhere in the literature. This is in part due because most of the papers looking at the relationship between stress caused by violence and birth outcomes estimate the intent-to-exposure for the entire population of pregnant women 'exposed' in a geographic unit making it difficult to compare the estimates.[17] To put the results into perspective, the effects are more than twice as large as the effects estimated by Black et al. (2016) on the effect of maternal stress caused by maternal bereavement during pregnancy. It is easier to compare their results with ours because Black et al. (2016) also use exposure defined at the individual level. The effects for victimisation in the second

[17]Quintana-Domeque and Ródenas-Serrano (2017) reports a reduction of 0.3 grams in birthweight for women exposed to bomb casualties of ETA terrorism in Spain, Foureaux Koppensteiner and Manacorda (2016) find a reduction in birthweight of 2 grams as response to a one-standard deviation increase in the homicide rate in Brazilian municipalities, Mansour and Rees (2012) find an statistically insignificant effect of 2.93 grams reduction in birthweight, but an increase in children classified as low birthweight as consequence of exposure to an additional non-combatant fatality in the Second Intifada.

and third trimesters are positive and roughly about half of the coefficient for the first trimester, but not significant at conventional levels of significance.

To learn about the effects on birth outcomes along the distribution of birthweight, we investigate the effect on low birthweight and very low birthweight. Victimisation in the first trimester increases the number of children classified as low birthweight by approximately 3.7 percentage points in the most satiated specification. Compared to the mean prevalence of low birthweight of 8.6 percent this corresponds to a 43 percent increase in the risk of low birthweight delivery. This constitutes a substantial increase in low birthweight births, which is well documented associated with long-lasting consequences for the affected individuals (Almond et al. (2005), Black et al. (2007), Figlio et al. (2014)). When considering very low birthweight, defined as birthweight less than 1,500 grams, we find a small positive effect of 0.014 percentage points for victimisation in a robbery in the first trimester, marginally significant. This points to very large effects taking into account the mean incidence for very low birthweight of 0.012. Considering the large associated costs of low and very low birthweight births (Almond et al. (2005)) these effects are economically very important and demonstrate the societal burden of criminal victimisation undocumented before.

The positive effects on birthweight for the second and third trimester are confirmed when looking at the effects on low and very low birthweight. We find that victimisation in second trimester reduces the risk for low birthweight. This could possibly indicate other underlying mechanisms at work for victimisation at different periods in gestation. Since the literature has to date found effects through biological stress mechanism to be concentrated in the first trimester, it is possible that stress at later stages leads to behavioural adjustments. Victims of robbery may for example avoid leaving their home and may generally be less active, which may positively contribute to the weight gain of the fetus.[18] Alternatively, there could be other be-

[18]There is small medical literature establishing the effect of activity during pregnancy, weight gain and birthweight (Hui et al. (2012)).

havioural responses to stress that lead to improvements in birth outcomes, such as through nutrition, or expectant mothers taking measures to invest in the quality of the pregnancy. The positive effect on birth outcomes could also, at least partially, be explained by selection. If victimisation has a negative effect throughout pregnancy, this could lead to a culling effect, such that the weakest pregnancies are terminated, leading to live births being positively selected. We will explore these different explanations further in the sections below.

In the last columns of Table 3.3 we present the estimates for fetal growth, defined as birthweight divided by the number of gestation weeks. We find that victimisation in the first trimester reduces fetal growth by 1.5 grams. This is an almost 2 percent reduction in the average weekly weight gain for affected fetuses. The coefficients for the second and third trimester are positive, much smaller, and not significant at conventional levels.

Foureaux Koppensteiner and Manacorda (2016) and Quintana-Domeque and Ródenas-Serrano (2017) show that reduced birthweight in the first trimester as a result of exposure to crime and terrorism is associated with reduced gestational length, possibly pointing out the underlying mechanics of the stress-birthweight relationship. We are therefore interested in understanding how criminal victimisation may affect gestational length. In addition to gestation in days, we also estimate whether the pregnancy is classified as preterm (< 259 days), very preterm (< 224 days) and extremely preterm delivery (< 196 days) and report the estimates in Table 3.4. We find that victimisation in robberies during the first trimester reduces gestation by about one day on average, and increases the propensity for preterm, very preterm and extremely preterm delivery, but the coefficients are not significant at conventional levels. For second and third trimester victimisation, we find effects that are aligned with the effects on birthweight: we find a positive and significant effect on gestation for victimisation in third trimester of about 2 days. Victimisation during the third trimester reduces the propensity for preterm births by 34 percent compared to the mean incidence. This is in line with the findings on birthweight in Table 3.3: victimisation in robberies in the first trimester has a negative effect on a number birth outcomes, and a positive effect, including on gestational length, towards the end of pregnancy.

Next, we investigate the effect of theft on a range of birth outcomes. We present the results in Table 3.5. Looking at birthweight, we do not find any effect of victimisation in theft on birthweight in the first trimester. The coefficients are negative, but very close to zero. We interpret this as evidence that theft does not impose the same level of stress to expectant mothers, setting off the biological mechanisms that leads to worse outcomes of health at birth. Indeed, depending on the type of theft, the victim may not even be confronted directly by the perpetrator or may not even be present during the incident. Interestingly, we find a reversal in the sign of victimisation in the third trimester on birthweight. Even though the coefficients are not statistically significant for birthweight, they are statistically significant for low and very low birthweight and economically meaningful, in particular for very low birthweight: mother victimised in theft in the third trimester have a substantially increased risk of very low birthweight delivery. Possibly, the economic shock from theft detrimentally affects (already vulnerable) pregnancies. The fact that the effects are concentrated towards the end of pregnancy is in line with a nutritional channel leading to intra-uterine growth retardation (Almond and Doyle (2011), Bozzoli and Quintana-Domeque (2016)).[19]

The effects on birthweight are largely supported by estimates on gestational lengths, presented in Table 3.6. We do not find an effect on any measure of ges-

[19] An exception to this is Almond and Mazumder (2011). They find that the observance of Ramadan during pregnancy affects birth outcomes stronger early in pregnancy. The underlying mechanism may nevertheless be different, as fasting during Ramadan not necessarily reduces overall calorie intake, but mainly affects the timing of prenatal nutrition and with it maternal glucose levels over the day.

tational length for victimisation during the first trimester; the coefficients are generally very small and not statistically significant. We also find small but precisely estimated effects for theft victimisation during the third trimester for the measures of very preterm and extremely preterm delivery. This could be interpreted as evidence in favour of an alternative mechanism underlying the negative effects on birthweight presented above. Victimisation and associated stress could possibly lead to spontaneous delivery close to the projected due date, reducing gestational length and hence mechanically birthweight. We will investigate this potential mechanism further below.

3.4.2 Robustness checks and sensitivity analysis

As a robustness check for the main outcomes on birthweight, we estimate equation 3.1 using the most satiated specification and include leads for victimisation for the three trimesters after birth. By design, victimisation after birth cannot affect birth outcomes. Any significant effect of the coefficients for the leads in victimisation may indicate problems with the identification strategy. We present the estimates for robbery in Table 3.12 in the Annex. As a first observation, the coefficients on birthweight, low birthweight and very low birthweight are largely unaffected by the inclusion of the leads. Second, the coefficients for the victimisation leads are generally small and not significant at any conventional levels of significance lending extra credibility to the identification strategy. We repeat the exercise for theft in Table 3.13 in the Annex. The estimates for theft generally tend to be noisier, making them harder to interpret. We find a few coefficients of the victimisation leads are significant, roughly in line with a small number being significant by chance.

We also re-estimate Table 3.3 including the small number of victims of robbery that sustained an injury. The results are presented in Table 3.14. The estimates are largely unchanged, and if anything slightly smaller. Because of the very small number of robberies that result in injury, and the heterogeneity of these cases, we prefer to exclude this small number of robberies and effectively shut the potential channel of transmission related to physical injury.

3.4.3 Effect of crime victimisation on additional outcomes

In this section we present additional birth outcomes separately for victimisation in robbery and theft. Some of these may shed additional light on the mechanisms at work. We present the outcomes for robbery in Table 3.7. All of the entries come from regressions using the most satiated specification including the full set of controls, time, municipality and hospital fixed effects. Hospital fixed effects are even more relevant for outcomes that are possibly related to the quality of the facilities and staff at point of delivery, and where reporting is of higher importance, for example for recording Apgar scores.

We start by looking at the fraction of birth delivered by emergency c-section. Here we only consider emergency c-section defined as caesarean delivery that were initiated after labour began. Apart from random factors for emergency c-section[20], it can also be induced by complications during pregnancy or labour, such as severe pre-eclampsia, labour not progressing or the mother feeling unwell during labour. We find a positive coefficient for victimisation in each trimester and large and significant coefficient for victimisation in robbery for the third trimester. Compared to the mean incidence, victimisation increases the chance for emergency c-section by 23 percent. We do not find an equivalent effect for victimisation in theft, results are reported in Table 3.8.

We do not find any effect on Apgar scores for robbery, with small and insignificant coefficients. Similarly for theft, the coefficients generally are very small. This is in

[20]These may include problems with the umbilical cord causing fetal distress or breech position of the fetus.

line with findings elsewhere in the literature, suggesting that one and five minute Apgar do not seem to reflect well fundamental health at birth.[21]

Next, we investigate whether victimisation impacts the number of prenatal visits. Prenatal visits are an important measure to identify pregnancy complications, such as gestational diabetes and pre-eclampsia. In the Brazilian context, prenatal care is free at the point of use and covered by the public health system (or by private insurance). We find that victimisation in robberies reduces the number of prenatal visits, with particularly strong effects in the last trimester: victimisation reduces prenatal visits by about 3 percent, compared to the mean. Victimisation possibly affects the willingness of victims to attend prenatal visits. We do not find an effect for theft.

Lastly, we investigate the effect on the sex composition and the fraction of singleton births. Both are indicators of selection.[22] We do not find an effect of the sex balance of live births for either robbery or theft victimisation. Turning to singleton births, we find that victimisation in robbery and theft reduce the number of multiple births, which generally tend to be more fragile pregnancies, substantially. The effects are particularly pronounced for robberies in the second trimester, but of similar magnitude for the first and third trimester. This points to a potential culling effect at work for pregnancies where the mother becomes the victim of crime. In the following section, we investigate in detail the effect of victimisation on miscarriage, stillbirth and infant mortality.

[21]Indeed, the Apgar score was designed to inform on a narrow aspect of infant health and the use as standard measure has been critiqued as misapplication of the measure. Doubts of the measure because of its subjective nature remain (Casey et al. (2001)).

[22]There is now a substantial literature that shows that the male fetus is biologically more fragile than the female fetus and more at risk of miscarriage or spontaneous abortion Kraemer (2000).

3.4.4 Effect of crime victimisation on fetal and infant death

To be able to investigate fetal death and infant mortality, we make use of the extraordinarily rich information in SINASC and SIM. As SINASC only contains information on live births, we combine birth records with death records that record fetal death. In addition, we can link birth records with death records, in cases where the infant dies within the first year of life. We separately investigate *Miscarriage* (where the fetal death occurs before the end of the 28th week of gestation), *Stillbirth* (where fetal death occurs after 28 weeks of gestation) and a combined measure, *Death in uterus*. A complex picture emerges from the results for exposure at different periods of gestation; we present the results in Table 3.9. We find that victimisation in robberies substantially increase the risk for miscarriage in the last trimester by 1.8 percentage points, tripling the risk for miscarriage.[23] Considering stillbirth, pregnancies that end in fetal death after 28 weeks of gestation, we find that victimisation in robbery during first or second trimester increases stillbirth by 1 and 0.9 percentage points respectively, roughly doubling the risk for stillbirth. When considering overall fetal death, victimisation in robbery substantially increases the risk of death in uterus in every trimester.

These outcomes are clearly important in their own right. Miscarriage and stillbirth are traumatic experiences for expectant mothers.[24] The results on fetal death are furthermore important for the interpretation of the estimates of birth outcomes because they may cause live births to be positively selected (Almond and Currie (2011)). Because mortality likely removes fetuses in poor health, this leads to survivors being positively selected. Regarding our findings for victimisation in robbery during the first trimester, where we estimate a negative effect on birthweight and

[23]While about 20 percent of pregnancies end in miscarriage, only a very small fraction of these are ever recorded in official statistics. In our case, we find that 0.6 percent of viable pregnancies end in miscarriages recorded in official death records.

[24]Miscarriage is also associated with substantial direct and indirect economic cost (Heazell et al. (2016)).

an increase in the likelihood of low and very low birthweight deliveries, this means that the estimated coefficients are likely lower bounds due to the bias from selective mortality. For the estimates on second trimester victimisation, where we find a reduction in the number of children being classified as low birthweight, this means that we cannot rule out that this effect is driven by selective mortality.

When considering the effect of theft on fetal mortality (results are presented in Table 3.10), we find no significant effect on miscarriage and small effects for second and third trimester victimisation on stillbirth. Overall, these coefficients are much smaller, about half the magnitude of the effects for robbery. Still there is some evidence for selective fetal mortality for the second and third trimester, consistent with an economic shock channel and these results need to be taken into account when interpreting the results on birthweight. Possibly, the positive effect (reduction) for second trimester victimisation on very low birthweight children could be explained by the selective mortality, whereas the reduction in birthweight and increase in very low birthweight children in the third trimester is a lower bound of the true impact of victimisation. The fact that these effects are concentrated at the lower part of the birthweight distribution is consistent with the culling explanation.

Lastly, we investigate whether victimisation in robbery (or theft) has an effect on different measures of infant mortality. We separately look at early neonatal ($<$ 1 week), neonatal ($<$ 4 weeks), perinatal ($<$ 22 weeks) and infant ($<$ 52 weeks) mortality. We do not find evidence for an effect of victimisation in robbery for any of these outcomes, apart from a negative coefficient for second trimester victimisation on early neonatal death. This negative effect is in line with selective mortality; given that children in relatively poorer health are removed before births, this could explain reductions in infant mortality due to positive selection.

Similarly for theft, we find no effect of first or third trimester victimisation, but a reduction in the mortality rates for second trimester victimisation, which is in line again with a positive selection of the surviving children.

3.4.5 Heterogeneous effects on birthweight

In this section, we briefly discuss heterogeneous effects for our main outcomes along with a number of mother characteristics. For this purpose, we split the sample by age, race, marital status and mother's education, for which we have information from the official birth register data. We find a small difference in the coefficients by age groups, with slightly smaller effects for relatively older mothers (> 24 years). Next, we investigate whether the effects vary by race of the mother.[25] The results for the heterogeneous effects for robbery and theft are presented in Tables 3.15 and 3.16, respectively.

We find that the effects are concentrated with white mothers, where we find pronounced negative effects of victimisation in robberies during the first trimester of around 100 grams. This could possibly indicate an adaptation effect to crime also documented in Foureaux Koppensteiner and Manacorda (2016). They find that indirect exposure to homicides on birth outcomes has a much stronger effect in rural areas, where homicides are very rare events, whereas in urban contexts the effects are much smaller. Possibly, for non-white mothers the effect is less pronounced because of similar adaptation to crime processes. This is also in line with the findings by mother's education. The effects on birthweight are concentrated on mother's with higher levels of education (with at least completed primary education), whereas we do not find an effect for low educated mothers suggesting the potential for a similar adaptation process. We do not find any differences by marital status.

[25] Race of the mother is self-declared, and generally includes white, mixed (pardo), black, asian and indigenous. We group mixed, black, asian and indigenous together. The distribution can be taken from Table 3.1.

Regarding victimisation in theft, we find some evidence contrary to robbery for the third trimester results. The negative effects on birthweight in third trimester are concentrated with non-white mothers and mothers with lower levels of education. This is consistent with the interpretation of the effects in third trimesters being caused by the economic shock. Because these estimates are at best significant at the 10 percent level, these differences need to be interpreted with caution.

3.5 Final Remarks

In this paper, we provide - to our best knowledge - the first evidence on the effect of victimisation in crime - robbery and theft - on birth outcomes using a unique dataset from Brazil, that allows us to link the universe of crime incidents from police reports to the universe of birth records for the period between 2012 and 2015.

We find that victimisation in robberies during the first trimester significantly reduces birthweight and indicators for poor health at birth, such as low birthweight. Controlling for municipality of residence, hospital of birth, and time fixed effects, and a large array of predetermined maternal characteristics, we find that robbery victimisation in the first trimester of gestation reduces birthweight by about 60 grams, equivalent to a reduction of about 10 percent of a standard deviation of birthweight. We find important variation along the birthweight distribution and document a substantial increase in the likelihood of being classified as low birthweight of 43 percent, leading to a substantial increase in the risk for complications and adverse later life outcomes of the children affected in-utero. The concentration of the effects in the first trimester for robbery, and the absence thereof for victimisation in theft, is consistent with the findings elsewhere in the literature that the effects are caused by the biological mechanisms induced by maternal stress and excess cortisol early in pregnancy (Camacho (2008), Aizer et al. (2016)).

We find that victimisation in theft during the third trimester reduces birthweight and increases the likelihood of very low birthweight births substantially. The particularly pronounced effect on very low birthweight is consistent with these effects being driven by the economic shock from theft. Because theft includes a very heterogeneous group of crimes, ranging from theft of mobile phone, to burglaries and the theft of motor vehicle, further analysis will be required to confirm this hypothesis.

We find strong evidence for selection of live births: criminal victimisation in robberies and theft increase the chance for fetal death substantially throughout pregnancy. This means, the negative effects on birth outcomes - concentrated in the first trimester for robbery and the third trimester for theft - are likely a lower bound of the true effect of victimisation, under the assumption of positive selection of surviving children.

We also document important heterogeneous effects along a number of maternal characteristics. For robberies, we find that the effects are concentrated with white mothers and the relatively more educated, possibly pointing to adaptation to crime effects: mothers' likely less exposed to crime may occur a stronger stress reaction to victimisation in robberies. We find the contrary for theft: effects in third trimester, which we interpret as economic shocks, are concentrated among non-white mothers and the lower educated. This is consistent with the economic shock from theft being relatively more important for mother's from on average lower socioeconomic background.

Our results contribute to a growing literature on the effects of maternal stress induced by violent events on birth outcomes. Rather than focussing on secular trends or extreme events, such as terrorism or war (Brown (2018), Quintana-Domeque and Ródenas-Serrano (2017), Mansour and Rees (2012)) we make use of individual level variation while including place of residence and hospital-fixed effects. Individual level exposure to crime and violence, rather than indirect exposure as in Foureaux

Koppensteiner and Manacorda (2016) or Camacho (2008) allows to shed additional light on the underlying mechanisms. For this purpose, we also make use of the very detailed vital statistics data and show that victimisation may also impact birth outcomes through behavioural channels, for example through avoidance strategies, affecting relevant inputs into pregnancies, for example prenatal visits.

Finally, our results contribute to the understanding of the societal cost of crime. Previous studies on the cost of victimisation in crime mostly focus on the health consequences and direct health costs of injury inflicted in violent crime. A few papers have tried to quantify the intangible costs of criminal victimisation brought on the victims through the psychological, social, educational, or occupational/professional consequences (Anderson (1999), Dolan et al. (2005), Brewster (2014)) mostly in accounting exercises. We add to this literature with the first estimates of individual victimisation in crime on birth outcomes documenting important cost of crime so far neglected in the literature.

3.6 Tables and Figures

Table 3.1: Birth outcomes, newborn and mothers characteristics

	Mean	Std.Dev.	Obs
Birth outcomes			
Birthweight	3150.105	530.612	517,689
Low birthweight	0.086	0.281	517,689
Very low birthweight	0.012	0.111	517,689
Fetal growth	81.022	12.905	517,689
1st minute APGAR	8.410	1.217	498,586
5th minute APGAR	9.376	0.860	498,894
Gestation days	271.776	16.564	517,689
Gestation days < 259	0.127	0.333	517,689
Gestation days < 224	0.018	0.132	517,689
Gestation days < 196	0.006	0.077	517,689
Newborn characteristics			
Female	0.489	0.500	517,689
Pregnancy and delivery characteristics			
Prenatal visits	8.145	2.462	513,678
Singleton	0.976	0.152	517,689
Twins	0.021	0.143	517,689
Triplets or more	0.001	0.025	517,689
C-section	0.597	0.491	517,689
Emergency C-section	0.211	0.408	517,689
Mothers characteristics			
Age	26.889	6.569	517,689
<= 24 years old	0.389	0.488	517,689
> 24 years old	0.611	0.488	517,689
White	0.343	0.475	517,689
Black	0.067	0.250	517,689
Asian	0.005	0.072	517,689
Mixed	0.503	0.500	517,689
Indigenous	0.002	0.047	517,689
Single	0.341	0.474	517,689
Married	0.463	0.499	517,689
Widowed	0.003	0.051	517,689
Separated divorced	0.015	0.120	517,689
Stable union	0.166	0.373	517,689
Low education	0.216	0.412	517,689
Mid education	0.574	0.494	517,689
High education	0.181	0.385	517,689
Mothers exposure to crime			
1st trimester: robbery victim	0.001	0.030	517,689
2nd trimester: robbery victim	0.001	0.027	517,689
3rd trimester: robbery victim	0.001	0.023	517,689
1st trimester: theft victim	0.002	0.047	517,689
2nd trimester: theft victim	0.002	0.043	517,689
3rd trimester: theft victim	0.001	0.038	517,689

Note: The table includes mothers over the period between 2012 and 2015. *Birthweight* is reported in grams. *Low birthweight* and *Very low birthweight* include newborns up to 2,500 and 1,500 grams respectively. *Fetal growth* is defined as birthweight divided by the number of gestation weeks. Variable *Low education* includes mothers with up to seven years of education; *Mid education* includes mothers with 8 to 11 years of education; and *High education* mothers with 12 or more years of education. Exposure to crime reports the share of mothers who were victims of robbery or theft in each trimester of pregnancy.

Table 3.2: Crime Characteristics

	Mean	Std.Dev.	Obs
Theft			
In the street	0.180	0.384	2,948
In a public space	0.154	0.361	2,948
In a residence	0.244	0.429	2,948
Place not reported	0.396	0.489	2,948
Robbery			
In the street	0.345	0.475	1,169
In a public space	0.276	0.447	1,169
In a residence	0.040	0.197	1,169
Place not reported	0.331	0.471	1,169

Note: The table includes mothers who were victims of robbery or theft, during gestational period, over the period between 2012 and 2015. The table does not include cases of robbery which resulted in an injured mother.

Table 3.3: Effect of crime victimisation on birthweight - robbery

	Birthweight			*Low birthweight*			*Very low birthweight*			*Fetal growth*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Victim	−53.869	−60.156	−59.410	0.031	0.038	0.037	0.013	0.014	0.014	−1.463	−1.509	−1.490
(1st trimester)	(27.173)**	(24.559)**	(24.224)**	(0.014)**	(0.013)***	(0.012)***	(0.009)	(0.009)	(0.009)*	(0.673)**	(0.614)**	(0.605)**
Victim	42.688	32.216	31.626	−0.038	−0.029	−0.026	−0.008	−0.006	−0.006	0.825	0.701	0.663
(2nd trimester)	(29.101)	(30.668)	(29.935)	(0.013)***	(0.014)**	(0.014)*	(0.004)*	(0.004)	(0.004)	(0.694)	(0.721)	(0.704)
Victim	27.305	26.700	30.140	−0.012	−0.007	−0.008	−0.006	−0.005	−0.005	0.054	0.140	0.159
(3rd trimester)	(35.634)	(31.664)	(32.091)	(0.018)	(0.017)	(0.017)	(0.004)	(0.004)	(0.004)	(0.837)	(0.752)	(0.770)
R^2	0.017	0.087	0.107	0.010	0.092	0.115	0.019	0.042	0.059	0.009	0.067	0.080
Clusters	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046
Observations	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Birthweight* is reported in grams. *Low birthweight* and *Very low birthweight* are dummies which indicate newborns up to 2,500 and 1,500 grams respectively. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of robbery in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception and municipality of residence fixed effects.

Table 3.4: Effect of crime victimisation on gestational length - robbery

	Gestation days			*Gestation days <259*			*Gestation days <224*			*Gestation days <196*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Victim	−0.491	−0.937	−0.924	−0.003	0.006	0.006	0.004	0.005	0.005	0.008	0.008	0.008
(1st trimester)	(0.819)	(0.772)	(0.767)	(0.013)	(0.013)	(0.013)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
Victim	1.099	0.513	0.560	−0.016	−0.004	−0.002	−0.007	−0.005	−0.004	−0.007	−0.007	−0.006
(2nd trimester)	(0.649)*	(0.658)	(0.650)	(0.020)	(0.021)	(0.020)	(0.005)	(0.005)	(0.005)	(0.001)***	(0.001)***	(0.001)***
Victim	2.307	1.957	2.189	−0.053	−0.043	−0.044	−0.006	−0.005	−0.004	0.002	0.002	0.002
(3rd trimester)	(0.813)***	(0.776)**	(0.764)***	(0.010)***	(0.011)***	(0.011)***	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
R^2	0.035	0.079	0.098	0.019	0.061	0.081	0.032	0.051	0.063	0.030	0.038	0.044
Clusters	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046
Observations	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of robbery in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception and municipality of residence fixed effects.

Table 3.5: Effect of crime victimisation on birthweight - theft

	Birthweight			*Low birthweight*			*Very low birthweight*			*Fetal growth*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Victim	−0.289	−7.086	−3.805	0.004	0.008	0.007	−0.001	−0.001	−0.001	0.031	−0.131	−0.079
(1st trimester)	(14.350)	(13.762)	(13.838)	(0.007)	(0.007)	(0.007)	(0.003)	(0.003)	(0.003)	(0.333)	(0.319)	(0.320)
Victim	10.053	4.654	6.289	0.003	0.006	0.005	−0.007	−0.006	−0.007	0.481	0.339	0.356
(2nd trimester)	(14.738)	(14.498)	(14.037)	(0.008)	(0.008)	(0.008)	(0.003)**	(0.003)**	(0.002)***	(0.379)	(0.380)	(0.368)
Victim	−19.039	−31.026	−28.765	0.015	0.021	0.019	0.015	0.016	0.015	−0.398	−0.713	−0.679
(3rd trimester)	(23.654)	(21.839)	(21.878)	(0.012)	(0.011)*	(0.010)*	(0.006)**	(0.006)***	(0.006)**	(0.539)	(0.499)	(0.499)
R^2	0.017	0.087	0.107	0.010	0.092	0.114	0.019	0.042	0.059	0.009	0.067	0.080
Clusters	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046
Observations	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Birthweight* is reported in grams. *Low birthweight* and *Very low birthweight* are dummies which indicate newborns up to 2,500 and 1,500 grams respectively. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of theft in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception and municipality of residence fixed effects.

Table 3.6: Effect of crime victimisation on gestational length - theft

	Gestation days			*Gestation days <259*			*Gestation days <224*			*Gestation days <196*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Victim	−0.138	−0.214	−0.096	−0.004	0.003	0.002	−0.003	−0.002	−0.002	−0.003	−0.002	−0.002
(1st trimester)	(0.454)	(0.444)	(0.444)	(0.012)	(0.012)	(0.012)	(0.003)	(0.003)	(0.003)	(0.002)*	(0.002)	(0.002)
Victim	−0.464	−0.454	−0.354	0.007	0.012	0.010	−0.008	−0.006	−0.007	−0.004	−0.004	−0.004
(2nd trimester)	(0.415)	(0.391)	(0.397)	(0.009)	(0.009)	(0.009)	(0.003)**	(0.003)*	(0.003)**	(0.001)***	(0.001)***	(0.001)***
Victim	−0.970	−0.978	−0.871	−0.005	0.003	0.000	0.012	0.014	0.013	0.009	0.010	0.010
(3rd trimester)	(0.677)	(0.659)	(0.663)	(0.014)	(0.013)	(0.013)	(0.007)*	(0.007)**	(0.007)*	(0.004)**	(0.004)**	(0.004)**
R^2	0.035	0.079	0.098	0.019	0.061	0.081	0.032	0.051	0.063	0.030	0.038	0.044
Clusters	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046
Observations	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852	489,852
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of theft in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception and municipality of residence fixed effects.

Table 3.7: Effect of crime victimisation on additional outcomes - robbery

	Emergency C-section	*1st minute APGAR*	*5th minute APGAR*	*Prenatal visits*	*Female*	*Singleton*
	(1)	(2)	(3)	(4)	(5)	(6)
Victim	0.008	−0.016	0.030	0.071	0.011	0.012
(1st trimester)	(0.017)	(0.046)	(0.039)	(0.113)	(0.018)	(0.005)**
Victim	0.021	0.071	0.027	−0.101	−0.014	0.018
(2nd trimester)	(0.023)	(0.065)	(0.038)	(0.098)	(0.027)	(0.003)***
Victim	0.049	−0.044	−0.042	−0.229	0.045	0.012
(3rd trimester)	(0.022)**	(0.066)	(0.048)	(0.127)*	(0.031)	(0.007)*
R^2	0.153	0.106	0.157	0.180	0.004	0.023
Clusters	1,032	1,043	1,043	1,046	1,046	1,046
Observations	462,417	472,179	472,455	486,232	489,766	488,940
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Emergency C-section* is a dummy which indicates if the C-section happened before labour began. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of robbery in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Table 3.8: Effect of crime victimisation on additional outcomes - theft

	Emergency C-section	*1st minute APGAR*	*5th minute APGAR*	*Prenatal visits*	*Female*	*Singleton*
	(1)	(2)	(3)	(4)	(5)	(6)
Victim	0.014	−0.034	−0.022	−0.047	−0.006	0.011
(1st trimester)	(0.011)	(0.033)	(0.024)	(0.069)	(0.014)	(0.003)***
Victim	0.005	0.084	0.027	0.039	0.003	0.009
(2nd trimester)	(0.012)	(0.034)**	(0.024)	(0.082)	(0.016)	(0.004)**
Victim	−0.004	0.038	−0.011	0.043	−0.003	0.011
(3rd trimester)	(0.015)	(0.043)	(0.026)	(0.095)	(0.018)	(0.004)***
R^2	0.153	0.106	0.157	0.180	0.004	0.023
Clusters	1,032	1,043	1,043	1,046	1,046	1,046
Observations	462,417	472,179	472,455	486,232	489,766	488,940
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Emergency C-section* is a dummy which indicates if the C-section happened before labour began. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of theft in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Table 3.9: Effect of crime victimisation on death in uterus and infant mortality - robbery

	Miscarriage (< 28 weeks)	*Stillbirth (>= 28 weeks)*	*Death in uterus*	*Early neonatal (1 week)*	*Neonatal (4 weeks)*	*Perinatal (22 weeks)*	*Infant (1 year)*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Victim	0.010	0.010	0.015	−0.001	0.004	0.003	0.003
(1st trimester)	(0.006)	(0.005)**	(0.005)***	(0.002)	(0.004)	(0.004)	(0.004)
Victim	−0.001	0.009	0.009	−0.003	0.004	0.003	0.003
(2nd trimester)	(0.005)	(0.005)*	(0.005)*	(0.000)***	(0.005)	(0.005)	(0.005)
Victim	0.018	0.005	0.014	0.001	0.003	0.002	0.002
(3rd trimester)	(0.009)**	(0.006)	(0.007)**	(0.003)	(0.004)	(0.004)	(0.004)
R^2	0.252	0.515	0.621	0.014	0.015	0.016	0.016
Clusters	1,053	1,052	1,053	1,053	1,053	1,053	1,053
Observations	524,832	521,627	524,832	524,832	524,832	524,832	524,832
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015 (including the ones whose babies died in uterus or before the age of 1 year). Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of robbery in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Table 3.10: Effect of crime victimisation on death in uterus and infant mortality - theft

	Miscarriage (< 28 weeks)	*Stillbirth (>= 28 weeks)*	*Death in uterus*	*Early neonatal (1 week)*	*Neonatal (4 weeks)*	*Perinatal (28 weeks)*	*Infant (1 year)*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Victim	−0.002	0.004	0.003	0.000	0.000	0.000	0.001
(1st trimester)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Victim	0.004	0.005	0.007	−0.003	−0.003	−0.004	−0.003
(2nd trimester)	(0.003)	(0.003)*	(0.003)**	(0.001)***	(0.001)***	(0.001)***	(0.001)**
Victim	−0.001	0.008	0.007	0.002	0.001	0.003	0.002
(3rd trimester)	(0.003)	(0.004)**	(0.003)**	(0.003)	(0.003)	(0.003)	(0.003)
R^2	0.252	0.515	0.621	0.014	0.015	0.016	0.016
Clusters	1,053	1,052	1,053	1,053	1,053	1,053	1,053
Observations	524,832	521,627	524,832	524,832	524,832	524,832	524,832
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015 (including the ones whose babies died in uterus or before the age of 1 year). Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of theft in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Annex

Table 3.11: Birth outcomes, fetus and mothers characteristics

	Mean	Std.Dev.	Obs
Birth outcomes			
Birthweight	1588.679	1054.245	9,260
Low birthweight	0.776	0.417	9,260
Very low birthweight	0.547	0.498	9,260
Fetal growth	61.459	143.273	9,260
Gestation days	211.999	50.260	9,260
Gestation days < 259	0.763	0.425	9,260
Gestation days < 224	0.524	0.499	9,260
Gestation days < 196	0.359	0.480	9,260
Fetus characteristics			
Female	0.446	0.497	9,260
White	0.068	0.252	9,260
Black	0.006	0.078	9,260
Asian	0.000	0.018	9,260
Mixed	0.106	0.308	9,260
Indigenous	0.000	0.018	9,260
Pregnancy and delivery characteristics			
Miscarriage (< 28 weeks)	0.359	0.480	9,260
Stillbirth (>= 28 weeks)	0.641	0.480	9,260
Singleton	0.921	0.270	9,260
Twins	0.069	0.253	9,260
Triplets or more	0.005	0.068	9,260
C-section	0.336	0.472	9,260
Mothers characteristics			
Age	27.343	7.228	9,260
<= 24 years old	0.388	0.487	9,260
> 24 years old	0.612	0.487	9,260
Low education	0.312	0.463	9,260
Mid education	0.448	0.497	9,260
High education	0.115	0.319	9,260
Mothers exposure to crime			
1st trimester: robbery victim	0.003	0.059	9,260
2nd trimester: robbery victim	0.002	0.045	9,260
3rd trimester: robbery victim	0.002	0.042	9,260
1st trimester: theft victim	0.004	0.064	9,260
2nd trimester: theft victim	0.004	0.060	9,260
3rd trimester: theft victim	0.003	0.058	9,260

Note: The table includes mothers over the period between 2012 and 2015 whose fetuses died in uterus. *Birthweight* is reported in grams. *Low birthweight* and *Very low birthweight* include fetuses up to 2,500 and 1,500 grams respectively. *Fetal growth* is defined as birthweight divided by the number of gestation weeks. Variable *Low education* includes mothers with up to seven years of education; *Mid education* includes mothers with 8 to 11 years of education; and *High education* mothers with 12 or more years of education. Exposure to crime reports the share of mothers who were victims of robbery or theft in each trimester of pregnancy.

Table 3.12: Effect of crime victimisation on birthweight (with trimester leads) - robbery

	Birthweight	*Low birthweight*	*Very low birthweight*
	(1)	(2)	(3)
Victim	−50.436	0.031	0.011
(1st trimester)	(20.440)**	(0.011)***	(0.007)
Victim	30.941	−0.024	−0.007
(2nd trimester)	(28.401)	(0.012)**	(0.003)***
Victim	37.163	−0.010	−0.008
(3rd trimester)	(29.193)	(0.016)	(0.002)***
Victim post-birth	13.761	0.001	−0.001
(1st trimester)	(36.766)	(0.019)	(0.009)
Victim post-birth	31.387	0.011	0.005
(2nd trimester)	(30.567)	(0.017)	(0.008)
Victim post-birth	−19.963	0.010	0.002
(3rd trimester)	(30.088)	(0.014)	(0.004)
R^2	0.106	0.114	0.057
Clusters	1,045	1,045	1,045
Observations	489,345	489,345	489,345
Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Birthweigth* is reported in grams. *Low birthweight* and *Very low birthweight* are dummies which indicate if newborns up to 2,500 and 1,500 grams respectively. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of robbery in the respective trimester of pregnancy; and *Victim post-birth (1st trimester)*, *Victim post-birth (2nd trimester)*, *Victim post-birth (3rd trimester)* indicate if the mother was a victim of robbery in the respective trimester post-birth. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Table 3.13: Effect of crime victimisation on birthweight (with trimester leads) - theft

	Birthweight	*Low birthweight*	*Very low birthweight*
	(1)	(2)	(3)
Victim	−6.126	0.008	0.000
(1st trimester)	(13.791)	(0.007)	(0.003)
Victim	2.634	0.006	−0.006
(2nd trimester)	(14.003)	(0.008)	(0.003)**
Victim	−31.365	0.019	0.016
(3rd trimester)	(21.740)	(0.010)*	(0.006)***
Victim - post-birth	−36.000	0.020	0.013
(1st trimester)	(22.932)	(0.012)	(0.006)**
Victim - post-birth	−28.122	0.022	−0.003
(2nd trimester)	(16.872)*	(0.010)**	(0.003)
Victim - post-birth	−9.557	−0.002	0.005
(3rd trimester)	(21.343)	(0.011)	(0.004)
R^2	0.106	0.114	0.057
Clusters	1,045	1,045	1,045
Observations	489,345	489,345	489,345
Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Birthweigth* is reported in grams. *Low birthweight* and *Very low birthweight* are dummies which indicate if newborns up to 2,500 and 1,500 grams respectively. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of theft in the respective trimester of pregnancy; and *Victim post-birth (1st trimester)*, *Victim post-birth (2nd trimester)*, *Victim post-birth (3rd trimester)* indicate if the mother was a victim of theft in the respective trimester post-birth. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Table 3.14: Effect of crime victimisation on birthweight and gestation (including injured victims) - robbery

	Birthweight	*Low birthweight*	*Very low birthweight*	*Fetal growth*	*Gestation days*	*Gestation days <259*	*Gestation days <224*	*Gestation days <196*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Victim	−53.181	0.032	0.012	−1.367	−0.721	0.003	0.003	0.007
(1st trimester)	(21.438)**	(0.011)***	(0.008)	(0.527)***	(0.745)	(0.012)	(0.005)	(0.006)
Victim	27.404	−0.023	−0.006	0.590	0.452	0.006	−0.005	−0.006
(2nd trimester)	(28.140)	(0.012)*	(0.004)	(0.659)	(0.670)	(0.020)	(0.005)	(0.001)***
Victim	30.029	−0.007	−0.006	0.135	2.278	−0.048	−0.005	0.002
(3rd trimester)	(30.750)	(0.016)	(0.004)	(0.737)	(0.746)***	(0.011)***	(0.005)	(0.004)
R^2	0.107	0.114	0.059	0.080	0.098	0.082	0.063	0.044
Clusters	1,046	1,046	1,046	1,046	1,046	1,046	1,046	1,046
Observations	489,908	489,908	489,908	489,908	489,908	489,908	489,908	489,908
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Birthweigth* is reported in grams. *Low birthweight* and *Very low birthweight* are dummies which indicate if newborns up to 2,500 and 1,500 grams respectively. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of robbery in the respective trimester of pregnancy; and *Victim post-birth (1st trimester)*, *Victim post-birth (2nd trimester)*, *Victim post-birth (3rd trimester)* indicate if the mother was a victim of robbery in the respective trimester post-birth. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Table 3.15: Effect of crime victimisation on birthweight (heterogeneous effects) - robbery

	Mother's age		*Mother's race*		*Mother's marital status*		*Mother's education*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<=24	*>24*	*White*	*Not white*	*Married*	*Not married*	*Low*	*Mid*	*High*
Victim	−62.355	−55.603	−105.803	−32.158	−64.795	−61.989	−4.792	−63.253	−60.397
(1st trimester)	(42.184)	(40.656)	(37.427)***	(33.008)	(34.897)*	(34.934)*	(91.512)	(33.417)*	(35.903)*
Victim	30.090	30.494	49.092	19.122	35.347	22.616	60.596	26.818	38.661
(2nd trimester)	(58.050)	(24.030)	(56.149)	(31.241)	(37.716)	(37.123)	(76.904)	(30.163)	(57.478)
Victim	−0.693	47.385	30.510	29.765	55.784	−7.970	118.703	1.070	63.007
(3rd trimester)	(45.120)	(46.193)	(72.413)	(28.102)	(53.268)	(40.483)	(62.546)*	(32.929)	(64.547)
R^2	0.093	0.119	0.122	0.103	0.115	0.100	0.114	0.104	0.149
Clusters	948	999	967	971	1,018	915	903	979	930
Observations	188,097	301,565	167,559	322,124	308,612	175,124	104,997	281,988	89,476
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Not married* category includes single, widowed and separated mothers. *Low education* includes mothers with up to 7 years of education; *Mid education* includes mothers with 8 to 11 years of education; and *High education* mothers with 12 or more years of education. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of robbery in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more; and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Table 3.16: Effect of crime victimisation on birthweight (heterogeneous effects) - theft

	Mother's age		*Mother's race*		*Mother's marital status*		*Mother's education*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<=24	*>24*	*White*	*Not white*	*Married*	*Not married*	*Low*	*Mid*	*High*
Victim	21.703	−18.141	−16.341	3.742	−20.795	16.194	−38.212	−1.792	8.301
(1st trimester)	(24.458)	(16.088)	(21.787)	(17.009)	(16.921)	(24.576)	(42.607)	(17.594)	(30.892)
Victim	22.668	0.494	9.006	0.342	11.819	−1.078	−21.123	22.157	−12.247
(2nd trimester)	(26.900)	(19.759)	(25.532)	(17.949)	(17.691)	(24.595)	(42.199)	(18.802)	(28.985)
Victim	5.079	−43.070	6.500	−50.157	−31.700	−29.643	−72.578	−40.006	−9.224
(3rd trimester)	(38.517)	(24.431)*	(28.331)	(29.565)*	(21.078)	(41.861)	(61.320)	(30.967)	(30.806)
R^2	0.093	0.119	0.122	0.103	0.115	0.100	0.114	0.104	0.149
Clusters	948	999	967	971	1,018	915	903	979	930
Observations	188,097	301,565	167,559	322,124	308,612	175,124	104,997	281,988	89,476
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: The analysis includes mothers over the period between 2012 and 2015. *Not married* category includes single, widowed and separated mothers. *Low education* includes mothers with up to 7 years of education; *Mid education* includes mothers with 8 to 11 years of education; and *High education* mothers with 12 or more years of education. Explanatory variables *Victim (1st trimester)*, *Victim (2nd trimester)*, *Victim (3rd trimester)* indicate the number of times the mother was a victim of theft in the respective trimester of pregnancy. Controls include dummies for mother's age, race, marital status and education; dummies for singleton, twins and triplets or more (when relevant); and dummies for the number of children born alive and stillbirths from previous pregnancies. All regressions include month of conception, municipality of residence and hospital fixed effects.

Chapter 4

Estimating the Effect of Criminal Victimisation on Workplace Performance and Turnover

4.1 Introduction

Crime victimisation generates losses at many levels: material, physical, psychological and social. In environments where the incidence of violence is high, fear of victimisation becomes part of individuals' lives. According to statistics from Latinobarometro[1], in 2016, delinquency was mentioned as one of the main problems for at least 22 percent of respondents. In Brazil, 68 percent of respondents stated that they were afraid all the time of becoming victims of violence, leading the country to occupy the first position among Latin American countries for fear of victimisation.

Indeed criminal victimisation is a serious problem for public policy in Brazil. In particular, rates of violent crimes and homicides are among the highest worldwide (Murray et al. (2013)). Being victimised in a crime, instils fear of (future) victimi-

[1] Available from `http://www.latinobarometro.org`

sation with victims, adding to the cost of the original incidence due to material or physical losses and the distress caused by becoming a victim. In turn, crime may generate numerous externalities which may spread its deleterious effects to a larger group of society. For instance, becoming a victim of crime may affect individuals performance at work. Such incidents may cause disruption in a typical work day and may affect workers' attendance at their workplace, their behaviour, their productivity or affect their willingness to remain in a particular job. Any productivity loss related to victimisation may then impact public service delivery, limiting these services in quantity or quality for users of these services.

This paper combines very rich Brazilian administrative data to estimate the effect of crime victimisation on labour market performance of public servants. These type of effects are not straightforward to measure, mainly because datasets containing workplace attendance for a large number of observations in relatively homogeneous jobs are very rare. For this reason, I focus on public servants, namely teachers in public schools, for which I have measures of workplace performance. Furthermore, I have access to the universe of identified crime reports from administrative police data that allow me to link individual victimisation to the workplace performance of public servants. Linking these two sets of administrative data, I can estimate the effect of individual victimisation in crime on monthly absenteeism from work while including individual and time fixed effects. The very detailed information available from the data allows me to investigate the differential effect of various crime types and the timing of victimisation on workers' monthly attendance, workplace transfer and job departure.

The public sector in Brazil is very large, with more than two million workers.[2] As public servants are hired by the government and are paid with public resources, there are legal requirements as set out in the Brazilian constitution to monitor their

[2]According to official statistics from the Brazilian Ministry of Planing, Development and Management.

work performance, in particular their attendance at work. Because there exists no universal monitoring system for the public sector as a whole, I focus on a large group of public servants for which there exists a common monitoring system: public school teachers. Focussing on this homogeneous group of public sector workers also allows for a valid comparison in attendance records and other outcome variables. The focus on this large group of public servants ensures that I have enough observations to obtain precise estimates of the effect of victimisation on workplace performance; after all, crime victimisation is still a relatively infrequent event, even in the context of the high-crime environment of Brazil.

There is a literature on the effects of individual victimisation on a range of outcomes, mostly outside of economics. Psychologists have been investigating the effect of crime victimisation on individuals life quality for some time (Norris (1992), Norris and Kaniasty (1994), Berman et al. (1996), Boudreaux et al. (1998), Hanson et al. (2010)). These studies mostly focus on mental wellbeing. For example, a range of psychological disorders have been associated with victimisation: post-traumatic stress disorder, depression, anxiety, obsessive compulsive disorder, hostility, social phobia and fear. In general, victims of violent crime are often found to be more severely distressed. These studies often rely on small samples in a cross-sectional setup and provide at best correlations, failing to address potential endogeneity. There is a small literature in economics that studies the effects of victimisation. Moya (2018) estimated the effect of violence on risk attitudes, he found that individuals become more risk averse after a traumatic event, affecting thus their economic decisions and, consistent with the psychology literature, the author concludes that the effect is mainly driven by anxiety disorders.

While there are numerous studies on the economic cost of crime and criminal victimisation using mostly accounting methodologies (Anderson (1999), Dolan and Peasgood (2007), Soares (2006)), these largely rely on aggregate statistics and do not allow disentangling the effect of victimisation from confounding factors. The literature aiming at estimating causal effects of crime on economic outcomes is limited. In particular, to the best of my knowledge, there is no previous study estimating the causal effect of crime victimisation on workplace absenteeism and turnover. The determinants of workplace absenteeism have received considerable attention from economists, particularly health related factors (Palme (2002), Puhani and Sonderhof (2010), Markussen et al. (2011), Markussen et al. (2013), Bratberg and Monstad (2015))[3]. There are few papers investigating the effects of sick leave policies on absenteeism of public servants and employee turnover, mostly from a personnel and human resource perspective (Winkler (1980), Pitts et al. (2011),De Paola et al. (2014)).

In this paper, I build a novel dataset by combining information on victims of robbery and theft from the universe of police incidence reports with administrative data from performance measures of a large group of public servants, namely public school teachers. I use this unique dataset to estimate the effect of exposure to day-to-day crime events of robbery and theft on monthly attendance and turnover of teachers in public schools.

I find that exposure to crime negatively impacts teachers performance at work. Using individual fixed effects estimates, I find that criminal victimisation in robbery or theft, reduces monthly attendance of public servants in the workplace. I focus on non-justified absences which have strong negative externalities on the efficiency of educational provision, because they limit the schools ability to plan for the absence. Individuals who were victims of crime are also more likely to change the workplace or to leave their job subsequently, possibly further impeding efficiency in the educational sector. Using the rich information on the timing and type of crime, and making use of rich information on the teacher characteristics, allows me to investigate heterogeneous

[3]There is also an extensive literature addressing employee absenteeism from a human resource management perspective (Ybema et al. (2010), Johns (2010)).

effects along a number of margins. The results are relevant for understanding a cost of crime so far neglected in the literature, workplace absenteeism and staff turnover.

The rest of the paper is organised as follows. Section 4.2 gives details on the institutional background. Section 4.3 describes the datasets used in the analysis. Section 4.4 presents the identification strategy applied to estimate the causal effect of crime victimisation on workplace performance and turnover. Section 4.5 analyses the results and section 4.6 presents the final remarks.

4.2 Institutional Background

Public servants play a key role to the functioning of society for the provision of a large number of public services. In Brazil, the general rules on the hiring process, and rights and duties of public servants are listed in the Federal Constitution[4], whereas the details are delimited by state or municipality regulations. In general, public servants are hired through a competitive public examination, which involves exams only or exams plus other prerequisites, for example a minimum level of education, depending on the requirements of the job.

Any individual older than 18 years, who fulfils his obligations as a citizen, and has passed in a public contest is entitled to become a public servant.[5] After starting the public role, individuals go through a probation period, which lasts two years. During this period, the following requirements need to be met: good moral character, attendance, discipline and efficiency.[6] If, at any stage, the worker is considered incapable or has exceeded a number of absences, she could be dismissed from the job.[7]

[4]Articles 37 to 41 of the Federal Constitution.

[5]According to Article 13 of Law 869, July 1952, the individual must meet the following requirements: fulfil the military obligations laid down by law; fulfil the voting obligations laid down by law and have good behaviour. To be entitled for the job, the individual also needs to have good health attested by a doctor.

[6]Article 23 of Law n. 869, July 1952.

[7]In those cases, the worker goes through an administrative trial and has the constitutional right to prepare her defence.

Attendance is monitored by the government and it is used to evaluate public servants' performance, calculate deductions in their salaries and account for contribution time towards retirement. Upon presenting credible evidence, workers have the right to go on sick leave, maternity leave and special leave.[8] They can also be excused from work without any penalty for up to eight days in case they are getting married and in case of death of a spouse, children, parents or siblings.[9]

State schools' teachers are public servants hired by the state, following all of the above rules. When they apply for a public contest, they are asked for their preferences on which school, region or municipality they would like to be allocated to. However, the final allocation depends on their ranking in the selection process. After being appointed to a job, teachers are allowed to transfer to a different school, as long as there is a vacancy or a teacher who is willing to swap workplaces.[10]

Male teachers can retire after reaching the age of 55 years if they have worked for at least 30 years as a teacher. For women, this threshold is reduced by 5 years, they can retire at the age of 50 years if they have been a teacher for at least 25 years.[11]

4.3 Data

In order to estimate the causal effect of criminal victimisation on work performance of public servants, I combine data from three Brazilian institutions: the Brazilian Ministry of Education, the State's Secretariat of Education and the State's Public Safety Secretariat. I combine these data using unique individual identifiers.

[8] Article 158 of Law n. 869, July 1952. Workers may apply to go on medical leave in case they need a health treatment and in case of accidents. Special leaves are in case of sickness of a family member, compulsory military service and in case they need to deal with private issues.

[9] Article 201 of Law n. 869, July 1952.

[10] Article 73 of Law n. 7109, July 1977. In case there is more than one candidate to the same vacancy, they are ranked according to the following criteria: the married, to the location where their spouses live; the sick, to the location where their medical treatment is available; the one who has either a sick spouse or a sick child, to the location where there is a medical treatment available; the individual, to the location where his family lives. After these criteria are checked, the next in line is seniority and then age.

[11] According to Article 40 of the Federal Constitution.

4.3.1 Educational data

I have access to unique data on all non-justified absences of state schools' teachers over the period between August 2008 and December 2015.[12] These data are available on a monthly basis and they are collected by the state's secretariat, which in turn uses this information to calculate deductions on teachers' monthly salaries. I combine these data with school census data, collected every year by the Brazilian Ministry of Education, which contains an array of characteristics of schools, classes, teachers and students for the universe of Brazilian primary and secondary schools. I combine information on attendance with school census records using a unique identifier for each teacher and detailed information on teachers and the subjects they teach, including information on their educational background, and demographic characteristics (among other).

Table 4.1 presents summary statistics of the state schools' teachers characteristics over the period between 2009 and 2015. The average age of teachers in the sample is around 40 years. The teacher profession in Brazil is dominated by female teachers, almost 80 percent of teachers are female, which is common across primary and secondary schools. Around 70 percent consider themselves white or mixed. More than three quarters of teachers have a university degree and the remainder at least secondary education. The table also summarises the different subjects teachers in the sample teach, including maths, science, history, geography and portuguese and foreign languages. Some teachers may teach more than one subject, or even all subjects to a given classroom depending on the grade. For consistency, I excluded from the sample: teachers who work with students with special needs and teachers who teach in rural areas, because they often have a different work regime; teachers who have been given authorisation to retire due to contribution time; and teachers who

[12] Absences are classified as non-justified if the worker cannot provide proof that her absence was a result of mitigating circumstances. In most of the cases, the justified absences are cases of medical leave.

are currently engaging in non-teaching activities (library managers for instance) due to medical reasons.

In 'work performance' I present some of the outcome variables, I focus on the 275,845 teachers employed in public schools. *Absenteeism* is the average number of days absent from work in a year. The data contain only non-justified absences, which means this number does not include medical leaves or any other planned and justified absences, for example for training. *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. I identify this from following teachers across schools using their unique identifier. I hence define a workplace transfer as disappearing as teaching staff from a school and appearing as teaching staff the subsequent year in another school. *Job departure* indicates whether a teacher leaves the profession in the subsequent year. I define this as disappearing as teaching staff from the database. Job departure could be due to a change in career, leaving the teaching profession, (early) retirement, or sabbatical leave.[13] These two variables were calculated using school census data, only available on an annual basis. To be able to identify workplace transfers and job departures, I restrict the sample to teachers employed at a single school reducing the sample to 170,379 observations.

4.3.2 Violence data

I use data on all cases of robbery and theft reported to the police over the period between August 2008 and December 2015. These data contain detailed information about the crimes and victims. I merge the crime reports using information on all victims with the teacher records using unique individual identifiers. I find that 5,851 teachers were victimised at least once during this period, and a few cases in which a teacher was victimised more than once, with a total number of cases equal to 6,422. This corresponds roughly to a victimisation rate of 3 percent over this period.

[13]For example, after two years active in a job, a public servant may apply to a leave designated to deal with private issues (Law 28039, May 1988).

In table 4.2, I present some of the characteristics of those crimes, separately for theft and robbery. The variable 'In week days' captures only crimes that happened from Monday to Friday, almost 75 percent of thefts and 79 percent of robberies, indicating that both theft and robbery are more likely to occur during week days than during the weekend. Figure 4.1 in the Annex shows the incidence of teacher victimisation by day of the week, which seems to be smaller during the weekend, especially on Sundays, probably aligning to the fact that individuals tend to stay more in the house on Sundays. The variable 'In work hours' captures only crimes occurring between 7am and 7pm. Crimes during this period are more likely to capture crimes occurring during work, on the way to work or returning home after work. Almost 67 percent of thefts and half of robberies happen during this period. Figure 4.2 in the Annex reports the incidence of crime by hour of the day. For robbery, there are peaks at 6am, 12pm, and after 4pm, which may coincide with the times teachers are travelling to and from their workplace. For theft, there is a peak at 3am, a sharp increase after 6am, starting to reduce after 5pm; there is a higher incidence of cases from 10am to 4pm, when the teachers are more likely to be at work, possibly related to burglary in their residence or theft occurring in the workplace. About 18 percent of all thefts are reported to occur in the public way and 6 percent at the workplace of teachers.

About 70 percent of robberies involved the use of a weapon; either a firearm or a sharp instrument, i.e. a knife. Around 4 percent of robberies resulted in injuries of the victim and the vast majority of robberies happened in the street. Only a small fraction of robberies happened at the workplace (0.6 percent), and 4 percent at the residence of teachers. I also present summary statistics of the characteristics of the teachers who were victimised in Table 4.11 and separately for theft and robbery in Tables 4.12 and 4.13 in the Annex.

4.4 Identification Strategy

In order to estimate the causal effect of criminal victimisation on workplace performance and turnover, it is necessary to deal with endogeneity of victimisation. Criminal victimisation depends on a number of factors that could also be related to work outcomes. For instance, individuals who work or live in neighbourhoods with high crime levels are more likely to be victimised, and they may also more likely to be absent at work, or to change workplaces because of reasons correlated with crime levels in their surroundings, for example, higher levels of socio-economic deprivation. This is the case, for example, if the quality of their workplace is systematically lower in these neighbourhoods, inducing these teachers to be more absent from their work and to change their job more frequently. Furthermore, differences in risk attitude and behaviour also affect the propensity for criminal victimisation, but may simultaneously also change workplace performance and turnover directly. These differences in factors determining individual victimisation are regularly unobserved by the researcher, leading to failed inference when relying on cross-sectional variation.

To deal with this problem, I use monthly variation of criminal victimisation and estimate panel models including individual fixed effects:

$$y_{it} = \beta_0 + \beta_1 victim_{it} + Z_{st}\beta_2 + d_i + d_t + u_{it} \tag{4.1}$$

y_{it} is the fraction of absences of teacher i in month t; $victim_{it}$ is a dummy variable which indicates teacher i was a victim of robbery or theft in month t; Z_{st} is a vector of school characteristics that may vary over time; d_i and d_t are individual and time fixed effects; u_{it} is the error term. Individual fixed effects hold constant any individual characteristics that may alter the risk for becoming a victim of crime. Standard errors are clustered at the individual level.

For the outcomes that only vary yearly, I use within school variation:

$$y_{ist} = \beta_0 + \beta_1 victim_{it} + X_{ist}\beta_2 + Z_{st}\beta_3 + d_s + d_t + u_{ist} \tag{4.2}$$

y_{ist} is the outcome variable for teacher i in year t; $victim_{it}$ indicates the number of times teacher i was a victim of robbery or theft in year t; X_{ist} and Z_{st} are vectors of individual and school characteristics that may vary over time; d_s and d_t are school and time fixed effects and u_{ist} is the error term. For the binary outcomes, I estimate linear probability models using ordinary least squares. The inclusion of school fixed effects ensures that systematic differences in the quality of the workplace (or neighbourhood of the workplace) are held constant, and individual victimisation is conditionally random. Standard errors are clustered at the school level.

4.5 Results

In this section, I first present, in Subsection 4.5.1, the results from equation 4.1 on workplace absenteeism, observed monthly. I will then move to outcomes observed annually - staff turnover - in Subsection 4.5.2, before investigating heterogeneous effects along a number of different margins in Subsection 4.5.3.

4.5.1 Effect of crime victimisation on workplace absenteeism

To start, I present the main results on workplace absenteeism of teachers. I estimate the effect of crime victimisation on non-justified work absences. Table 4.3 presents the estimates from model 4.1. I have estimated the model including teachers who work in more than one school. In this case, I sum their absences over the different workplaces.[14] As dependent variable *Absences* I use the percentage of absences of

[14]If teachers with more than one employment divide their jobs across different days of the week, summing of absences accounts best for the total absences from work. Because teachers possibly could also teach in two workplaces during the same day, I also provide estimates where I alternatively

teacher i in month t. I only have available information on non-justified absences of teachers. This excludes absences that are 'justified' by a medical letter or other justified excuses for their absence, which is the reason why the average absenteeism rate in a month is rather modest. However, those are the most disruptive kind of absences and generate a number of negative externalities for school coordinators and for students, since schools often do not have time to adjust to those events and either substitute the missing teacher with a replacement teacher at short notice[15] or, in some cases, send students home, both possibly negatively impacting the schooling of children. My explanatory variable *Victim(t)* is a dummy variable which indicates whether the teacher was a victim of robbery or theft in month t, the other explanatory variables are leads and lags of victimisation. All specifications include time and individual fixed effects, together with school characteristics as controls.[16]

In column (1) of Table 4.3, I include only contemporaneous exposure, i.e. the effect of victimisation in crime in a given month on absenteeism the same month. I find a significant and positive coefficient on the effect of victimisation on work absenteeism, the coefficient suggests an effect size, compared to the mean absence rate, of about 22 percent. In column (2), I include the lagged values of victimisation to investigate whether victimisation impacts workplace absenteeism beyond a contemporaneous effect. First, I find that the coefficient for contemporaneous exposure is virtually unchanged. Second, the coefficient for the one month lagged value of victimisation is roughly half of contemporaneous exposure, but is not significant at conventional levels of significance. This possibly indicates that victimisation has a positive effect on absenteeism beyond the contemporaneous month. Because I only have monthly

average absences across the different jobs. The estimates of this alternative definition of absences can be found in Table 4.14.

[15] In this case the replacement teacher likely may be drawn from a different subject specialisation or specialised in teaching different grade levels.

[16] I use school characteristics as controls for the main job, identified as the school with the largest number of hours. In case of an equal number of hours in two jobs, I randomly assign school characteristics from either job.

attendance records available, I cannot exclude the possibility though, that the effect is caused by misattributing victimisation on attendance records of crimes occurring late in the calendar month. The coefficient for the second lag of *Victim(t)* is very close to zero, so that I don't find any evidence for effects on absenteeism beyond two months.

As a falsification exercise, I also estimate the same equation including leads of the explanatory variable, presented in column (3). Mechanically, future victimisation should not affect current absenteeism. As expected, the coefficients for both leads are close to zero and insignificant. To investigate the coefficients visually, I plot the coefficients of column (3) in Figure 4.3 in the Annex, which shows that the contemporaneous effect is the most pronounced and statistically significant.

As a robustness check, I also exclude teachers with more than one employment. I present the results in Table 4.15 in the Annex. The results are very similar, possibly, because I lose observations by restricting the sample, I lose power and the coefficients are significant at the 10 percent level only.

Next, I make use of the very detailed information on the characteristics of crime available in the police bulletins of reported crime and investigate the effect of different types of criminal victimisation on the absenteeism of teachers at work. I created separate victimisation indicators according to a number of characteristics of the crime: crime categories (theft - robbery)[17], injury sustained, armed crime, timing of crime (during work hours - outside of work hours), and places of occurrence of the crime. I present the results in Table 4.4. All entries are from separate regressions and I follow the specification of column (1) of Table 4.3.

The first two columns look at victims of theft and robbery separately. By definition, in my sample thefts include crimes where a perpetrator dishonestly appropriates

[17]The data I have access to includes the two main crime categories, theft and robbery. These account for more than 50 percent of all police reported crime. The data excludes police reports on violent crime as another major crime category.

property from the victim.[18] Robberies are cases of theft that include an element of force or the threat to use force. Both coefficients are positive; only the coefficient on theft is significant, with a similar magnitude to the main results. The coefficient on robbery is smaller and not significant. Next, I look at victims with and without injury in columns (3) and (4). The coefficient for victims without injury are of similar magnitude to the main results. One might expect that a violent incident, which results in an injured victim would cause more disruption compared to an incident in which the victim was left unharmed. However, when cases are more extreme and possibly violent, it is also more likely that the teacher is able to justify the absence. This would be a possible explanation for the negative coefficient for *Victim (with injury)* in column (3). The number of injured victims, both in theft and robbery is small, hence the noisy estimates. This is also consistent with the the smaller coefficient for robbery, where victimisation in a robbery more likely leads to a justifiable absence, compared to victimisation in theft.

In columns (5) and (6), I compare events which involved the use of a weapon, either a firearm or a sharp object with cases that did not. Although not significant at standard levels of significance, the coefficient on armed crimes has a positive sign, similar in magnitude to the main effects, whereas the coefficient for not armed is much smaller and negative. Columns (7) and (8) compare the effect of crimes occurring during work hours - in week days and during the window between 7am and 7pm - with cases occurring during the weekend and during the night. Victimisation during work hours are to capture possible victimisation at work or during the commute to work or to home. One might expect crimes occurring during work hours to have a stronger effect on work related outcomes, everything else equal. Both coefficients are positive, with the coefficient for crimes during work hours being about half the size and insignificant. Possibly, crimes occurring outside of the work hours are systematically

[18] These cases include victimisation in pickpocketing, burglaries, theft of motor vehicle etc.

different and, for example, more likely include crimes occurring in one's home, hence the larger coefficient. Nevertheless, the interpretation of crimes occurring during work hours as work related may be erroneous. As a more direct way to investigate whether crimes linked to the workplace have a stronger impact on absenteeism, in the following columns I look at the different places where the crimes happened: at the workplace (school), in a public space, in the street or in a residence. Coefficients are all positive, but not significant. Victimisation at school has a smaller coefficient, possibly pointing to the fact that these crimes are substantially different. Victimisation in a public space has the largest coefficient on absenteeism, possibly pointing to a potential underlying avoidance mechanism. Possibly, victims that are victimised in the public, are more likely to avoid going to work to avoid facing a similar situation.

I also present results to compare the different circumstances of victimisation on work absenteeism separately for theft and robbery in Tables 4.16 and 4.17 in the Annex. The results are largely consistent with the findings from Table 4.4, in particular for theft. Because of the relatively small number of cases of robberies, splitting the cases in different categories may lead to much more noisy estimates making the interpretation of these coefficients difficult.

4.5.2 Effect of crime victimisation on turnover

Next, I investigate the effect of crime victimisation on workplace transfer and on job departure. Being victimised in a crime may impact teachers beyond a short-term effect of workplace attendance.[19] Criminal victimisation may, for example, lead to health-related (justified) absence from the workplace and may also aggravate underlying mental health issues.[20]

[19] Because I do not have access to justified absences, it is also more difficult to investigate the overall effect of victimisation on general absenteeism. For example, I cannot investigate the effect of victimisation that leads to stress-related longer-term absences that are medically justified, as non-justified absences are very unlikely in these instances.

[20] Poor mental health has been associated to be a major determinant of early retirement from paid employment (Van Rijn et al. (2014)).

To investigate the effect of victimisation on these longer-term outcomes, I combine information on victimisation with information from the school census, that allows me to investigate movements of teachers across schools and in and out of employment as teachers. As explanatory variable *Victim* I use the number of times a teacher was victimised in year t. *Workplace transfer* is a dummy variable which captures if in the following year the teacher transfers to a different school. The variable *Job departure* captures if in the following year the teacher does not appear in the dataset, which may be due to retirement, a year's sabbatical leave, or even starting a new job. To calculate these outcomes, I use the school census data, which contains a unique identifier for each teacher, thus allowing me to follow them over time and across schools. I present the results in Table 4.5. All specifications include time and school fixed effects; and specifications in columns (2) and (4) include, in addition, a set of controls which contains individual and school characteristics. I estimate equation 4.2 using a linear probability model.

For workplace transfers I find a positive and significant effect. Including a large set of time varying teacher and school characteristics reduces the effect only moderately, lending credibility to the identification strategy. Compared to the baseline workplace transfer rate, being victimised in a crime during a year increases workplace transfers by about 18.2 percent.[21] In columns (3) and (4) I present the estimates for job departure. Given the broad definition of job departure, including permanent and temporal departure, the coefficient needs to be interpreted keeping this in mind. I find a positive and significant effect of victimisation on job departure. Including the set of controls leaves the coefficient virtually unchanged. Compared to the mean departure rate of teachers, the effect is about 4 percent. Teachers transferring the school they teach at and leaving the teaching profession poses considerable costs to schools, as

[21] The school year in Brazil coincides with the calendar year, so that I can calculate exposure during the calendar year corresponding to exposure during the school year. I define workplace transfer as changing place of work for main job from original school to another school at the end of the school year.

they need to hire new teachers as replacement. This may create inefficiencies in the teaching system, for which teacher turnover is already a considerable problem (Ronfeldt et al. (2012), Akhtari et al. (2017), Watlington et al. (2010)).

In Table 4.6, I analyse the effects of different types of victimisation on workplace transfer, theft and robbery. Columns (1) and (2) confirm the main results for workplace transfer for both crime types. The coefficient on robbery is slightly larger, and significant. Victims with and without injury and victims of an armed incident are more likely to transfer to a distinct workplace the following year; coefficient for the variable *with injury* is almost 5 times larger than *without injury*, however, only the latter is significant. Comparing the effect of victimisation in an armed or unarmed incidence, reveals a much stronger effect on workplace transfer of being victimised in an armed robbery. The coefficient is twice the size of the coefficient of the main estimates and significant at the 10 percent level of significance. I also compare, victimisation within and outside work hours. The coefficients are presented in columns (7) and (8). While both coefficients are positive, the effect of *work time victimisation* is more pronounced and significant at the 5 percent level, while the effect of *out of work time victimisation* is smaller and not significant. Consistent with the findings on absenteeism, victimisation in the street and in public places have a more pronounced effect on workplace transfer than victimisation at school. I also estimate the effect of the different circumstances of victimisation on workplace transfer separately for theft and robbery, results are presented in Tables 4.18 and 4.19 in the Annex. The results for theft are consistent with the general findings. Because of the smaller number of incidences, the results for robbery need to be treated cautiously.

I repeat the exercise for *Job departure* in Table 4.7. Overall, the results are in line with the findings for workplace transfer. *Robbery* has a more pronounced effect than *theft*, but neither of the coefficients are significant. Incidents that lead to injury of the victims, have a more pronounced effect, but the coefficient is not significant, while the coefficient for incidents without injury is significant and in line with the main estimate. Coefficients in columns (5) and (6) analyse the effect of *armed* and *not armed* conflicts on job departure, they are both positive, with similar magnitudes, but not significant. Similarly, the coefficients for events during and outside of working hours are of similar magnitude. Considering the place of occurrence, *Victim (In a public space)* confirms the findings for workplace departure. Victimisation in the public and in the street have more pronounced effects on job departure. The coefficient on victimisation at the workplace is very close to zero and insignificant, possibly because crimes occurring at school are different from crimes occurring in the public space. I also estimate the effect of the different circumstances of victimisation on job departure separately for theft and robbery, results are presented in Tables 4.20 and 4.21 in the Annex. Results are largely in line with the general results.

4.5.3 Heterogeneous effects

I now investigate how crime victimisation affects different groups of teachers. I look at heterogeneous effects of crime victimisation on males and females; and by age of teachers, where I focus on individuals who are either 40 years of age or younger, compared to individuals who are older than 40.

I first analyse how the absenteeism of these groups is affected after victimisation, results are presented in Table 4.8. All regressions include time and individual fixed effects together with school controls as in the specification of the main estimates. I find no difference in the coefficients for female or male teachers on absenteeism; the coefficients are in fact identical. There is a literature that has identified that men and women have a different level of susceptibility of fear of crime (Snedker (2015)). Absences are nevertheless not equivalent to fear of crime, but may also be affected by how well a victim deals with experiences of victimisation, making it hence difficult to relate my findings to this literature.

Splitting the sample by age, reveals a much more pronounced effect of victimisation on absenteeism for individuals older than 40; the coefficient is more than twice the size compared to the coefficient for younger teachers. Again, it is difficult to interpret this result given that the incentives for absenteeism also may differ across age groups. Possibly the effects indicate that older teachers are more susceptible for the effects of victimisation. Alternatively, younger teachers, that are more likely to still be in their probation period, may choose not to be absent at work in a non-justified way than their older counterparts for the risk of dismissal.

I am also interested in the effect of victimisation on the turnover of these different groups. Table 4.9 presents the effect of crime victimisation on workplace transfer and job departure, separately for males and females. Starting with columns (1) and (2), the estimates suggest that males are more likely to transfer to another workplace after victimisation. The coefficient for male teachers is about twice the magnitude for female teachers, but not significant. The effect is even stronger for job departure; males are more likely to leave the job in the following year compared to females, as shown in columns (3) and (4). Overall, male teachers seem to be more affected than female teachers. From the data I have available, it is difficult to identify any underlying mechanism for these differences. The effects could be explained by levels of resilience to stressors and different coping strategies that vary by sex[22]. The differences are nevertheless also consistent with labour market opportunities to differ for male and female teachers, differentially impacting their workplace transfer or job departure decisions after victimisation. The effects could also be explained by differences in the underlying crime characteristics; possibly the type of crimes males are victimised in, differs from the characteristics of the crimes females are involved in as victims.[23]

[22]There is body of literature in psychology documenting sex differences in resilience to stress (Bale and Epperson (2015)).

[23]Females are known to be more risk averse in their behaviour, possibly leading to lower victimisation rates for specific types of crime (Eckel and Grossman (2008)).

Table 4.9 presents the results by age groups. Estimates show that there is no difference in workplace transfers across age groups. Younger and older workers are equally likely to transfer from one school to another after criminal victimisation. Older teachers though are more likely to leave the job after victimisation. Given that many job departures are likely linked to early retirement, it is not surprising that the effect is more pronounced for older teachers.

4.6 Final Remarks

This paper combines very detailed crime data on robberies and thefts from administrative police reports with data on the work performance of public servants in state schools in Brazil to estimate the causal effect of crime victimisation on teachers' absenteeism and turnover.

I find that crime victimisation worsens performance of teachers at work. In particular, I find that teachers are more likely to be absent at work after being a victim of robbery or theft in a given month. The effect for robbery is larger than it is for theft, although not statistically significant. I find that effects are concentrated for contemporaneous exposure, but there is some evidence for short-term persistence. I also compare different circumstances in which the crimes happened. For instance, I find that armed events have an effect on teachers absenteeism that is twice as large compared to non-armed events. The place of occurrence also plays an important role in explaining differences in responses. Victimisation at the workplace does not generally seem to drive any of the effects, most likely because the nature of crime at school differs from crimes occurring in public places.

With regards to teacher turnover, I find that teachers are more likely to transfer to a different school or to leave the job in the year following being victim of crime in a given year. Further results suggest that victims with injury and victims of an armed incident are more affected in their school transfer decision. Victims with injury are also more likely to leave the job. I find that victimisation in public places and in the street have the most profound effect on teacher performance. Such experiences may instil fear for repeat victimisation for some teachers, possibly leading to avoidance behaviour resulting in absenteeism and higher turnover at their current place of work.

I investigate heterogeneous effects for males and females and for different age groups. While I do not find a differential effect on absenteeism, the effect of victimisation on workplace transfers and job departure is much more pronounced for male teachers. I also find heterogeneous effects by age of teachers. Older teachers seem to be more susceptible for victimisation, with a coefficient more than twice as large than for younger teachers. Maybe not surprisingly, I find that job departure after victimisation is a more likely consequence for older teachers, possibly due to the possibilities for early retirement for this age group.

Absenteeism and turnover in the public sector are very disruptive not only for the workers, but also for the individuals who benefit from their services. On the public servant side, while job transfer and job departure may be part of coping strategies, these outcomes may nevertheless affect their careers and, in the Brazilian case, also affect their prospective to retire at their desired time. On the side of the beneficiaries of public services, these events may disturb the smooth delivery of services, possibly generating negative externalities for students in Brazil (Clotfelter et al. (2009), Herrmann and Rockoff (2012), Akhtari et al. (2017)). Hence, this paper also shows how violence and crime may contribute to the underlying problems with quality provision of primary and secondary education in Brazil.

4.7 Tables and Figures

Table 4.1: Teachers Characteristics

	Mean	Std.Dev.	Obs
Demographics			
Age	40.497	9.360	275,845
Female	0.778	0.416	275,845
White	0.393	0.488	275,845
Black	0.084	0.277	275,845
Mixed	0.300	0.458	275,845
Education			
Primary school	0.000	0.010	275,845
Secondary school	0.230	0.421	275,845
High education	0.770	0.421	275,845
Subject taught			
Mathematics	0.302	0.459	275,845
Science	0.339	0.473	275,845
History	0.261	0.439	275,845
Geography	0.263	0.440	275,845
Portuguese Language	0.314	0.464	275,845
Other Language	0.061	0.240	275,845
Other subject	0.339	0.473	275,845
Work performance			
Absenteeism	2.211	7.243	275,845
Workplace transfer	0.099	0.298	102,319
Job departure	0.399	0.490	170,379

Note: The table includes teachers from state schools over the period between 2009 and 2014. *Absenteeism* is the average number of days absent from work in a year. *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. *Job departure* indicates if in the following year the teacher leaves the job.

Table 4.2: Crime Characteristics

	Mean	Std.Dev.	Obs
Theft			
In week days	0.744	0.436	5,207
In work hous	0.669	0.471	5,207
In the street	0.183	0.387	5,207
In a school	0.058	0.234	5,207
In a public space	0.070	0.256	5,207
In a residence	0.207	0.405	5,207
Place not reported	0.460	0.498	5,207
Robbery			
Injured victim	0.044	0.204	1,215
In week days	0.794	0.404	1,215
In work hours	0.493	0.500	1,215
Armed robbery	0.691	0.462	1,215
In the street	0.390	0.488	1,215
In a school	0.006	0.076	1,215
In a public space	0.137	0.344	1,215
In a residence	0.042	0.201	1,215
Place not reported	0.404	0.491	1,215

Note: The table includes state schools' teachers who were victims of theft or robbery over the period between 2008 and 2015. *Injured victim* is a dummy variable which indicates if the crime resulted in an injured victim. *In week days* captures only crimes that happened from Monday to Friday. *In work hours* captures only crimes that happened from 7am to 7pm. *Armed robbery* indicates if the robbery involved the use of a weapon: either a firearm or a sharp instrument. *In the street, In a school, In a public space, In a residence* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other).

Table 4.3: Effect of crime victimisation on work absenteeism

	Absences		
	(1)	(2)	(3)
Victim(t+2)			0.020
			(0.082)
Victim(t+1)			0.053
			(0.085)
Victim(t)	0.206	0.209	0.212
	(0.091)**	(0.093)**	(0.095)**
Victim(t-1)		0.115	0.118
		(0.092)	(0.093)
Victim(t-2)		−0.026	−0.024
		(0.082)	(0.084)
R^2	0.248	0.248	0.248
Clusters	113,858	113,858	113,858
Observations	2,772,842	2,772,842	2,772,842
Controls	Yes	Yes	Yes

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

Note: The analysis includes state schools' teachers, over the period between August 2008 and December 2015. Explanatory variable *Victim(t)* indicates if the teacher was a victim of theft or robbery in month *t*. Dependent variable *Absences* is the percentage of absences from work in a given month. Controls are *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and individual fixed effects.

Table 4.4: Effect of crime victimisation on work absenteeism by type of victimisation

	Absences											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Victim (theft)	0.187											
	(0.088)**											
Victim (robbery)		0.078										
		(0.177)										
Victim (with injury)			−0.467									
			(0.466)									
Victim (without injury)				0.153								
				(0.082)*								
Victim (armed)					0.146							
					(0.221)							
Victim (not armed)						−0.074						
						(0.294)						
Victim (work time)							0.111					
							(0.114)					
Victim (not work time)								0.221				
								(0.108)**				
Victim (at school)									0.075			
									(0.407)			
Victim (in a public space)										0.304		
										(0.332)		
Victim (in the street)											0.215	
											(0.171)	
Victim (in a residence)												0.233
												(0.173)
R^2	0.233	0.233	0.233	0.233	0.233	0.233	0.233	0.233	0.233	0.233	0.233	0.233
Clusters	113,858	113,858	113,858	113,858	113,858	113,858	113,858	113,858	113,858	113,858	113,858	113,858
Observations	3,279,831	3,279,831	3,279,497	3,279,497	3,279,796	3,279,831	3,279,831	3,279,831	3,276,938	3,276,938	3,276,938	3,276,938
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 individual level in parentheses.

Note: The analysis includes state schools' teachers, over the period between August 2008 and December 2015. Explanatory variables *Victim (theft)* and *Victim (robbery)* are dummy variables which indicate if the teacher was a victim of theft or robbery in month t. *Victim (with injury)* indicate if the teacher was injured. *Victim (armed)* indicates if the crime involved the use of a weapon: either a firearm or a sharp instrument. *Victim (work time)* indicates if the teacher was victimised during week days and during the time window between 7am and 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Absences* is the percentage of absences from work in a given month. Controls are *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and individual fixed effects.

Table 4.5: Effect of crime victimisation on turnover

	Workplace transfer		*Job departure*	
	(1)	(2)	(3)	(4)
Victim	0.022	0.018	0.017	0.016
	(0.007)***	(0.007)***	(0.008)**	(0.008)**
R^2	0.079	0.115	0.081	0.094
Clusters	3,111	3,111	3,155	3,155
Observations	102,319	102,319	170,379	170,379
Controls	No	Yes	No	Yes

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

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variable *Victim* indicates the number of times a teacher was a victim of theft or robbery in year *t*. Dependent variable *workplace transfer* indicates whether the teacher transfers to a different school in the following year. *Job departure* indicates if the following year the teacher leaves the job. These two variables were calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

Table 4.6: Effect of crime victimisation on workplace transfer by type of victimisation

	Workplace transfer											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Victim (theft)	0.017											
	(0.007)**											
Victim (robbery)		0.024										
		(0.017)										
Victim (with injury)			0.083									
			(0.061)									
Victim (without injury)				0.016								
				(0.007)**								
Victim (armed)					0.038							
					(0.022)*							
Victim (not armed)						−0.006						
						(0.024)						
Victim (work time)							0.023					
							(0.010)**					
Victim (not work time)								0.014				
								(0.010)				
Victim (at school)									0.010			
									(0.035)			
Victim (in a public space)										0.031		
										(0.031)		
Victim (in the street)											0.042	
											(0.022)*	
Victim (in a residence)												0.000
												(0.016)
R^2	0.115	0.115	0.115	0.115	0.115	0.115	0.115	0.115	0.115	0.115	0.115	0.115
Clusters	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111
Observations	102,319	102,319	102,319	102,319	102,319	102,319	102,319	102,319	102,319	102,319	102,319	102,319
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.

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variables *Victim (theft)* and *Victim (robbery)* measure the number of times a teacher was a victim of theft or robbery in year t. *Victim (with injury)* is the number of times the teacher was injured after victimisation during the year. *Victim (armed)* measures the incidence of crime involving the use of a weapon: either a firearm or a sharp instrument. *Victim (work time)* indicates if the teacher was victimised during week days and during the time window between 7am and 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. This variable was calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

Table 4.7: Effect of crime victimisation on job departure by type of victimisation

	Job Departure											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Victim	0.014											
(theft)	(0.009)											
Victim		0.029										
(robbery)		(0.018)										
Victim			0.066									
(with injury)			(0.061)									
Victim				0.018								
(without injury)				(0.008)**								
Victim					0.029							
(armed)					(0.022)							
Victim						0.030						
(not armed)						(0.031)						
Victim							0.017					
(work time)							(0.011)					
Victim								0.016				
(not work time)								(0.011)				
Victim									−0.009			
(at school)									(0.037)			
Victim										0.059		
(in a public space)										(0.030)*		
Victim											0.031	
(in the street)											(0.020)	
Victim												0.010
(in a residence)												(0.020)
R^2	0.094	0.094	0.094	0.094	0.094	0.094	0.094	0.094	0.094	0.094	0.094	0.094
Clusters	3,155	3,155	3,155	3,155	3,155	3,155	3,155	3,155	3,155	3,155	3,155	3,155
Observations	170,379	170,379	170,379	170,379	170,379	170,379	170,379	170,379	170,379	170,379	170,379	170,379
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.

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variables *Victim (theft)* and *Victim (robbery)* measure the number of times a teacher was a victim of theft or robbery in year *t*. *Victim (with injury)* is the number of times the teacher was injured after victimisation during the year. *Victim (armed)* measures the incidence of crime involving the use of a weapon: either a firearm or a sharp instrument. *Victim (work time)* indicates if the teacher was victimised during week days and during the time window between 7am and 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Job departure* indicates if in the following year the teacher leaves the job. This variable was calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

Table 4.8: Effect of crime victimisation on work absenteeism by gender and age groups

	Absences (female)	*Absences (male)*	*Absences (younger than 40)*	*Absences (older than 40)*
	(1)	(2)	(3)	(4)
Victim	0.169	0.169	0.099	0.250
	(0.088)*	(0.164)	(0.103)	(0.122)**
R^2	0.214	0.262	0.234	0.243
Clusters	89,059	24,799	65,532	54,408
Observations	2,549,380	730,451	1,677,616	1,602,215
Controls	Yes	Yes	Yes	Yes

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

Note: The analysis includes state schools' teachers, over the period between August 2008 and December 2015. Explanatory variable *Victim(t)* indicates if the teacher was a victim of theft or robbery in the month *t*. Dependent variables *Absences* is the percentage of absences from work in the month, for male and females, and for teachers who are less than or equal to 40 years old and teachers who are more than 40 years old. Controls are *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and individual fixed effects.

Table 4.9: Effect of crime victimisation on turnover by gender

	Workplace transfer (female)	*Workplace transfer (male)*	*Job departure (female)*	*Job departure (male)*
	(1)	(2)	(3)	(4)
Victim	0.013	0.025	0.011	0.028
	(0.008)*	(0.016)	(0.009)	(0.015)*
R^2	0.125	0.235	0.101	0.161
Clusters	3,110	2,576	3,150	2,845
Observations	81,253	21,066	135,487	34,892
Controls	Yes	Yes	Yes	Yes

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

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variable *Victim* indicates the number of times a teacher was a victim of theft or robbery in year *t*. *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. *Job departure* indicates if in the following year the teacher leaves the job. These two variables were calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

Table 4.10: Effect of crime victimisation on turnover by age groups

	Workplace transfer (younger than 40)	*Workplace transfer (older than 40)*	*Job departure (younger than 40)*	*Job departure (older than 40)*
	(1)	(2)	(3)	(4)
Victim	0.017	0.017	0.015	0.022
	(0.011)	(0.009)*	(0.011)	(0.011)**
R^2	0.153	0.105	0.090	0.136
Clusters	3,082	3,086	3,144	3,128
Observations	48,203	54,116	85,347	85,032
Controls	Yes	Yes	Yes	Yes

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

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variable *Victim* indicates the number of times a teacher was a victim of theft or robbery in year *t*. Dependent variable *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. *Job departure* indicates if in the following year the teacher leaves the job. These two variables were calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

Annex

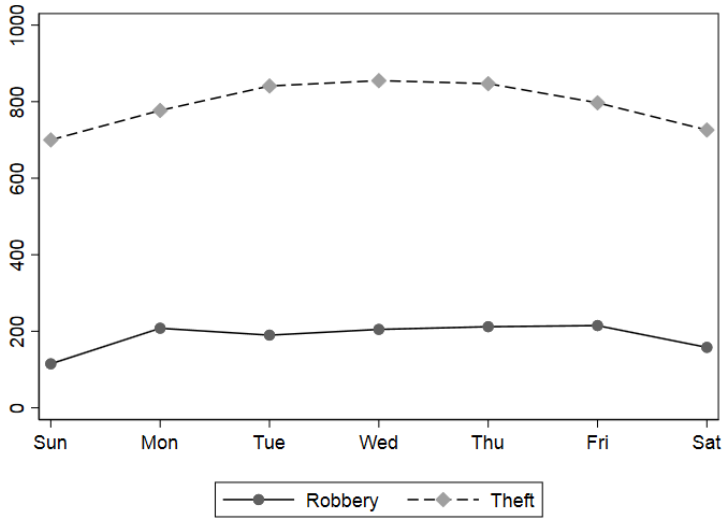


Figure 4.1: Crime incidence by day of the week

Note: Includes teachers who were victims of theft or robbery over the period between 2008 and 2015.

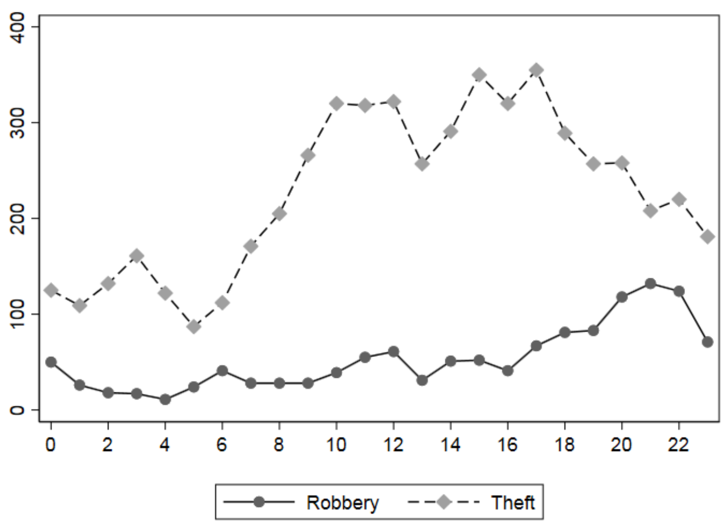


Figure 4.2: Crime incidence by hour of day

Note: Includes teachers who were victims of theft or robbery over the period between 2008 and 2015.

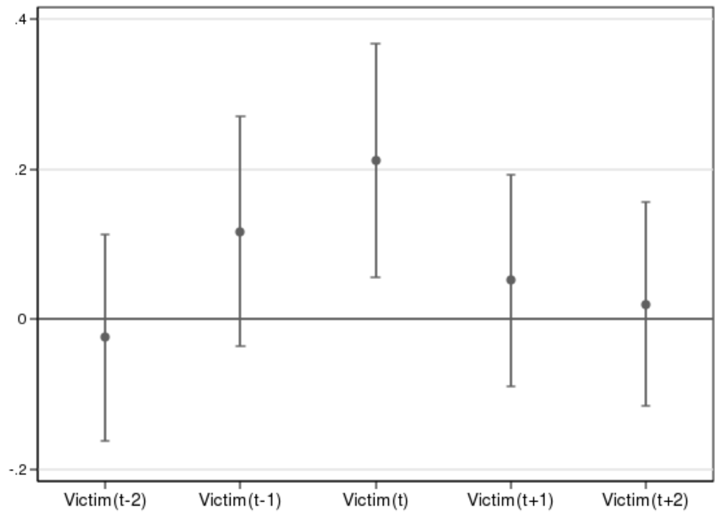


Figure 4.3: Effect of crime victimisation on work absenteeism - regression coefficients
Note: Figure 4.3 plots regression coefficients of column (3) in Table 4.3.

Table 4.11: Victims characteristics

	Mean	Std.Dev.	Obs
Demographics			
Age	40.175	9.406	5,992
Female	0.702	0.457	5,992
White	0.373	0.484	5,992
Black	0.085	0.279	5,992
Mixed	0.307	0.461	5,992
Education			
Primary school	0.000	0.018	5,992
Secondary school	0.247	0.431	5,992
High education	0.753	0.431	5,992
Subject taught			
Mathematics	0.267	0.443	5,992
Science	0.311	0.463	5,992
History	0.220	0.414	5,992
Geography	0.223	0.416	5,992
Portuguese Language	0.267	0.442	5,992
Other Language	0.055	0.228	5,992
Other subject	0.334	0.472	5,992
Work performance			
Absenteeism	3.025	8.659	5,992
Workplace transfer	0.127	0.333	2,158
Job departure	0.414	0.493	3,682

Note: The table includes characteristics of state schools' teachers who were victims of theft or robbery over the period between 2009 and 2014. *Absenteeism* is the average number of days absent from work in a year. *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. *Job departure* indicates if in the following year the teacher leaves the job.

Table 4.12: Theft victims characteristics

	Mean	Std.Dev.	Obs
Demographics			
Age	40.306	9.374	4,936
Female	0.698	0.459	4,936
White	0.382	0.486	4,936
Black	0.085	0.280	4,936
Mixed	0.308	0.462	4,936
Education			
Primary school	0.000	0.020	4,936
Secondary school	0.239	0.426	4,936
High education	0.761	0.427	4,936
Subject taught			
Mathematics	0.263	0.440	4,936
Science	0.310	0.462	4,936
History	0.219	0.413	4,936
Geography	0.218	0.413	4,936
Portuguese Language	0.263	0.441	4,936
Other Language	0.055	0.229	4,936
Other subject	0.336	0.472	4,936
Work performance			
Absenteeism	3.002	8.928	4,936
Workplace transfer	0.122	0.327	1,806
Job departure	0.410	0.492	3,061

Note: The table includes characteristics of state schools' teachers who were victims of theft over the period between 2009 and 2014. *Absenteeism* is the average number of days absent from work in a year. *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. *Job departure* indicates if in the following year the teacher leaves the job.

Table 4.13: Robbery victims characteristics

	Mean	Std.Dev.	Obs
Demographics			
Age	39.315	9.431	1,176
Female	0.707	0.455	1,176
White	0.335	0.472	1,176
Black	0.083	0.277	1,176
Mixed	0.304	0.460	1,176
Education			
Primary school	0.000	0.000	1,176
Secondary school	0.289	0.454	1,176
High education	0.711	0.454	1,176
Subject taught			
Mathematics	0.281	0.449	1,176
Science	0.314	0.464	1,176
History	0.233	0.423	1,176
Geography	0.242	0.429	1,176
Portuguese Language	0.279	0.449	1,176
Other Language	0.054	0.225	1,176
Other subject	0.338	0.473	1,176
Work performance			
Absenteeism	3.338	9.121	1,176
Workplace transfer	0.154	0.361	390
Job departure	0.437	0.496	693

Note: The table includes characteristics of state schools' teachers who were victims of robbery over the period between 2009 and 2014. *Absenteeism* is the average number of days absent from work in a year. *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. *Job departure* indicates if in the following year the teacher leaves the job.

Table 4.14: Effect of crime victimisation on work absenteeism

	Absences		
	(1)	(2)	(3)
Victim(t+2)			0.004
			(0.075)
Victim(t+1)			0.040
			(0.078)
Victim(t)	0.174	0.176	0.177
	(0.083)**	(0.084)**	(0.086)**
Victim(t-1)		0.102	0.104
		(0.084)	(0.085)
Victim(t-2)		−0.057	−0.056
		(0.072)	(0.073)
R^2	0.248	0.248	0.248
Clusters	113,858	113,858	113,858
Observations	2,772,842	2,772,842	2,772,842
Controls	Yes	Yes	Yes

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

Note: The analysis includes state schools' teachers, over the period between August 2008 and December 2015. Different from Table 4.3, regressions in this table consider the average (instead of the sum) number of absences across schools for teachers who work in more than one school. Explanatory variable *Victim(t)* indicates if the teacher was a victim of theft or robbery in month *t*. Dependent variable *Absences* is the percentage of absences from work in a given month. Controls are *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and individual fixed effects.

Table 4.15: Effect of crime victimisation on work absenteeism

	Absences		
	(1)	(2)	(3)
Victim(t+2)			0.010
			(0.093)
Victim(t+1)			0.043
			(0.096)
Victim(t)	0.191	0.194	0.197
	(0.102)*	(0.103)*	(0.106)*
Victim(t-1)		0.141	0.144
		(0.105)	(0.106)
Victim(t-2)		−0.073	−0.071
		(0.089)	(0.090)
R^2	0.253	0.253	0.253
Clusters	102,675	102,675	102,675
Observations	2,180,492	2,180,492	2,180,492
Controls	Yes	Yes	Yes

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

Note: The analysis includes state schools' teachers, over the period between August 2008 and December 2015. Different from Table 4.3, regressions in this table exclude all teachers who work in more than one school. Explanatory variable *Victim(t)* indicates if the teacher was a victim of theft or robbery in month *t*. Dependent variable *Absences* is the percentage of absences from work in a given month. Controls are *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and individual fixed effects.

Table 4.16: Effect of crime victimisation on work absenteeism - theft

	Absences					
	(1)	(2)	(3)	(4)	(5)	(6)
Victim (work time)	0.119 (0.124)					
Victim (not work time)		0.260 (0.125)**				
Victim (at school)			0.130 (0.414)			
Victim (in a public space)				0.548 (0.455)		
Victim (in the street)					0.028 (0.183)	
Victim (in a residence)						0.234 (0.180)
R^2	0.233	0.233	0.233	0.233	0.233	0.233
Clusters	113,858	113,858	113,858	113,858	113,858	113,858
Observations	3,279,831	3,279,831	3,277,426	3,277,426	3,277,426	3,277,426
Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: The analysis includes state schools' teachers, over the period between August 2008 and December 2015. Explanatory variables *Victim (work time)* indicates if the teacher was victimised during week days and during the time window between 7am and 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Absences* is the percentage of absences from work in a given month. Controls are *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and individual fixed effects.

Table 4.17: Effect of crime victimisation on work absenteeism - robbery

	Absences									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Victim (with injury)	−1.375 (0.806)*									
Victim (without injury)		0.156 (0.188)								
Victim (armed)			0.146 (0.221)							
Victim (not armed)				−0.074 (0.294)						
Victim (work time)					0.065 (0.310)					
Victim (not work time)						0.089 (0.211)				
Victim (at school)							−2.328 (1.782)			
Victim (in a public space)								−0.233 (0.333)		
Victim (in the street)									0.591 (0.361)	
Victim (in a residence)										0.205 (0.476)
R^2	0.233	0.233	0.233	0.233	0.233	0.233	0.233	0.233	0.233	0.233
Clusters	113,858	113,858	113,858	113,858	113,858	113,858	113,858	113,858	113,858	113,858
Observations	3,279,788	3,279,788	3,279,796	3,279,831	3,279,831	3,279,831	3,279,339	3,279,339	3,279,339	3,279,339
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 individual level in parentheses.

Note: The analysis includes state schools' teachers, over the period between August 2008 and December 2015. Explanatory variables *Victim (with injury)* indicate if the teacher got injured. *Victim (armed)* indicates if the crime involved the use of a weapon: either a firearm or a sharp instrument. *Victim (work time)* indicates if the teacher was victimised during week days and during the time window between 7am and 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Absences* is the percentage of absences from work in a given month. Controls are *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and individual fixed effects.

Table 4.18: Effect of crime victimisation on workplace transfer - theft

	Workplace transfer					
	(1)	(2)	(3)	(4)	(5)	(6)
Victim (work time)	0.020 (0.010)**					
Victim (not work time)		0.014 (0.011)				
Victim (at school)			0.004 (0.035)			
Victim (in a public space)				0.020 (0.037)		
Victim (in the street)					0.036 (0.026)	
Victim (in a residence)						−0.005 (0.016)
R^2	0.115	0.115	0.115	0.115	0.115	0.115
Clusters	3,111	3,111	3,111	3,111	3,111	3,111
Observations	102,319	102,319	102,319	102,319	102,319	102,319
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.

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variables *Victim (work time)* indicates if the teacher was victimised during week days and during the time window between 7am and 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. This variable was calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

Table 4.19: Effect of crime victimisation on workplace transfer - robbery

	Workplace transfer									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Victim (with injury)	0.062 (0.092)									
Victim (without injury)		0.022 (0.018)								
Victim (armed)			0.038 (0.022)*							
Victim (not armed)				−0.006 (0.024)						
Victim (work time)					0.041 (0.027)					
Victim (not work time)						0.015 (0.021)				
Victim (at school)							0.136 (0.216)			
Victim (in a public space)								0.056 (0.060)		
Victim (in the street)									0.057 (0.037)	
Victim (in a residence)										0.114 (0.097)
R^2	0.115	0.115	0.115	0.115	0.115	0.115	0.115	0.115	0.115	0.115
Clusters	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111
Observations	102,319	102,319	102,319	102,319	102,319	102,319	102,319	102,319	102,319	102,319
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.

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variables *Victim (with injury)* is the number of times the teacher was injured after victimisation during the year. *Victim (armed)* measures the incidence of crime involving the use of a weapon: either a firearm or a sharp instrument. *Victim (work time)* indicates if the teacher was victimised during week days and during the time window of 7am to 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Workplace transfer* indicates whether the teacher transfers to a different school in the following year. This variable was calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

Table 4.20: Effect of crime victimisation on job departure - theft

	Job departure					
	(1)	(2)	(3)	(4)	(5)	(6)
Victim (work time)	0.014 (0.012)					
Victim (not work time)		0.014 (0.012)				
Victim (at school)			−0.006 (0.038)			
Victim (in a public space)				0.022 (0.038)		
Victim (in the street)					0.041 (0.025)*	
Victim (in a residence)						0.017 (0.021)
R^2	0.094	0.094	0.094	0.094	0.094	0.094
Clusters	3,155	3,155	3,155	3,155	3,155	3,155
Observations	170,379	170,379	170,379	170,379	170,379	170,379
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.

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variables *Victim (work time)* indicates if the teacher was victimised during week days and during the time window between 7am and 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Job departure* indicates if the following year the teacher leaves the job. This variable was calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

Table 4.21: Effect of crime victimisation on job departure - robbery

	Job departure									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Victim (with injury)	0.025 (0.098)									
Victim (without injury)		0.033 (0.019)*								
Victim (armed)			0.029 (0.022)							
Victim (not armed)				0.030 (0.031)						
Victim (work time)					0.039 (0.029)					
Victim (not work time)						0.022 (0.023)				
Victim (at school)							−0.085 (0.207)			
Victim (in a public space)								0.124 (0.050)**		
Victim (in the street)									0.012 (0.036)	
Victim (in a residence)										−0.192 (0.098)**
R^2	0.094	0.094	0.094	0.094	0.094	0.094	0.094	0.094	0.094	0.094
Clusters	3,155	3,155	3,155	3,155	3,155	3,155	3,155	3,155	3,155	3,155
Observations	170,379	170,379	170,379	170,379	170,379	170,379	170,379	170,379	170,379	170,379
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.

Note: The analysis includes state schools' teachers, over the period between 2009 and 2014. Explanatory variables *Victim (with injury)* indicate if the teacher got injured. *Victim (armed)* indicates if the crime involved the use of a weapon: either a firearm or a sharp instrument. *Victim (work time)* indicates if the teacher was victimised during week days and during the time window between 7am and 7pm. *Victim (at school)*, *Victim (in a public space)*, *Victim (in the street)* and *Victim (in a residence)* indicate where the crimes happened. Public spaces include shops, banks, parks, clubs, markets (among other). Dependent variable *Job departure* indicates if the following year the teacher leaves the job. This variable was calculated using school census data, only available on an annual basis. Controls include *individual characteristics*: age, sex and race fixed effects; and *school characteristics*: number of staff members, number of school rooms in use and dummies indicating whether the school has computer room, science lab, library, internet connection and if the school offers school meals. All regressions include time and school fixed effects.

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