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Violence, Psychological Stress and Educational Performance during the â•œWar on Drugsâ•š in Mexico

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Abstract: We provide evidence that violence in Mexico related to the "war on drugs" from 2006-2011 had a significant negative impact on educational performance that is primarily attributable to acute psychological stress among students in the immediate aftermath of local violence. Using geographically and temporally disaggregated data we demonstrate that the largest impacts of violence on educational performance result from homicides committed within the vicinity of schools during the week immediately prior to national standardized tests. This short-term impact increases with geographic proximity and levels of violence, and dramatically exceeds the effects of longer-term violence spread over a full school year.

Keywords: Violence, Primary Educational Performance, Psychological Stress, Mexico

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1 Introduction

In September 2011, five severed heads were found in front of Benito Juárez Elementary School in Acapulco, Mexico, accompanied by handwritten messages to drug traffickers in the area.[1] This event was one of many that occurred during the early years of the "war on drugs" that escalated after 2006, and which has included incidents that have directly targeted school children, spreading fear and terror in their immediate environment. These incidents speak to the potential for violence to generate significant consequences for children's educational performance. In Acapulco, for example, the act of graphic violence led to a temporary school closure and repeated strikes by teachers who demanded safety in schools, while there are parallel concerns about the possibility of significant psychological stress on students and their parents, which may amplify the negative effects of violence.

Reflecting these concerns, we investigate the effects of violence on educational performance among primary school students in Mexico during the peak of violence associated with the "war on drugs" from 2006-11.[2] Our central question is whether psychological stress caused by exposure to violence may be an important — but previously overlooked — mechanism linking violence to reduced educational performance. We focus particularly on the potential for acute short-term effects; more specifically, the possibility that homicides that occur in the week immediately prior to national exams may generate acute psychological stress and result in reduced exam performance. This is, to our knowledge, the first paper both to provide detailed tests of the role of psychological stress in linking violence to educational performance, and to focus attention on the potential for acute short-term effects of exposure to violence. In exploring this possibility we draw on a rich body of psychological research that has documented the potential for violence to contribute to both short-term acute psychological stress, and longer-term prolonged psychological stress.[3] Symptoms of both can include difficulty concentrating, anxiety, intrusive thoughts, sleep disturbances, reduced cognitive development and aggressive behavior, as well as psycho-biological effects (Margolin and Gordis, 2000).[4] Critically, given our focus on primary school students, there is evidence that parental stress disorders caused by community violence are a contributor to children's stress levels (Osofsky, 1995; Margolin and Gordis, 2000).[5]

Empirically, we combine data on violence from the Ministry of Health (*Secretaría de Salud*) with data about performance among primary school students on national standardized exams (*Evaluación Nacional del Logro Académico en Centros Escolares*) over the period of rapidly escalating violence from 2006-2011. Our analysis builds on the approach in Monteiro and Rocha (2016), as we assemble geo-located data on the positions of schools and homicides, and then add additional information on the precise timing of those homicides. This allows us to map the occurrence of homicides located within 2k, 5km and 10km of individual schools, and link higher levels of homicides, during specific time periods, to changes in student scores on standardized national exams within schools over time. In particular, we focus on the immediate impacts of homicides that occur in the seven days prior to exams, and on the longer-term impact of accumulated homicides over the entire school year (nine months). This allows us to establish more precise estimates of the links between homicides and educational performance, and to disentangle the previously unexplored importance of the short-term effects of violence on educational performance, which we attribute to acute psychological stress. While we cannot measure psychological stress directly, the fine-grained character of our data allows us to rule out alternative possibilities either because they operate over longer time horizons, or by testing for those alternatives directly.

We find a significant negative effect of violence on educational performance over both the short (seven days) and long (nine months) term, and show that the negative effect on exam scores generally increases in size as the level of violence increases and when the violence is geographically closer to the affected school. Critically, we find that homicides that occur in the week immediately prior to national standardized tests have dramatically larger impacts on performance than homicides that occur further from exam dates. Specifically, we find that schools that are exposed to at least three homicides within a 2km radius in the week immediately prior to exams see scores fall by an average of about 4.4 points (out of 800), or about 0.1 standard deviations. By contrast, when we focus on homicides that occur in the nine months prior to the exam exposure to at least 100 homicides generates roughly the same average decline in exam scores. This implies that the impact on educational performance of a homicide that occurs in the week immediately prior to exams is more than 30 times larger than an additional homicide that occurs earlier in the school year. We conduct a series of placebo tests in order to ensure the robustness of the results.

Overall, the results highlight the importance of focusing on the immediate short-term effects of violence on educational performance, which have previously been overlooked in the literature. It likewise reinforces the view that this short-term effect is attributable to acute psychological stress, as none of mechanisms previously studied in the literature could account for the much larger effect of violence over the short-term. An important question raised by the short-term results is whether we are capturing a decline in overall learning — as disruption prior to exams disrupts the consolidation of learning that has occurred over the course of the school year — or only a transitory decline in exam performance. Even if we are capturing only a transitory disruption of exam performance this can have important implications in so far as exam performance shapes future educational opportunities, teacher evaluations or school funding. In these scenarios, reduced exam performance resulting from exposure to violence may negatively and lastingly affect the educational trajectory of affected students, while it may result in reduced funding and evaluations for affected schools — potentially contributing to reduced performance and reinforcing prior inequalities. The results correspondingly suggest that where schools are exposed to significant violence in the lead up to important exams governments may consider providing various forms of additional psychological support to students and along with postponing exams, or else otherwise seeking to account for the negative impacts of exposure to violence.

In advancing this analysis we build upon a growing body of research. An increasingly large number of studies have sought to understand the impacts of violence on educational outcomes in countries that have been affected by violent conflicts, wars, and more general violence and crime. Prominent examples are Alderman et al. (2006), Akresh and Walque (2008), and Chamarbagwala and Moran (2011), and Shemyakina (2011), who show that violent conflict almost always reduces access to education, school attainment, and academic achievement. Akbulut-Yuksel (2014), Ichino and Winter-Ebmer (2004), and Leon (2012), and Justino et al. (2013) find that shocks to educational access can lead to significant and long-lasting detrimental effects on individual human capital accumulation, which in turn explain long-term trends in socioeconomic status (Case and Paxson, 2008; Maccini and Yang, 2009). The critical long-term importance of investments in children and adolescents for skill development and socioeconomic advancement is highlighted by Cunha and Heckman (2007).

While these studies highlight the negative impacts of violence on overall educational outcomes, only a more limited number of studies have looked explicitly at the impact of violence on educational performance among students. We build most explicitly on Monteiro and Rocha (2016), who explore the impact of variation in conflict intensity on children's educational performance in Rio de Janeiro's favelas, and argue that reduced school supply is the main driver of poor exam results after increases in violence intensity. They do not, however, explore the particular role of psychological stress, nor do they isolate short-term effects. In an unpublished working paper, Brück et al. (2014) find detrimental effects of violence on educational performance in affected regions in the Palestinian territories. Building upon our study, they consider the possibility that psychological stress may help to explain reduced student performance. However, they are only able to test this mechanism with significantly less precision owing to the more limited geographical and temporal disaggregation of their data, and more highly aggregated measures of educational performance.

In the Mexican context Brown and Velásquez (2017) draw on longitudinal and municipality-level data on drug-related violence over time, and find negative effects of drug-related violence on overall educational attainment and employment outcomes over time, while demonstrating that violence results in significant student migration. Of relevance here, they include an attempt to test for the impacts of violence on cognitive alertness — a potential result of psychological stress — and find no result, but note explicitly that their lack of any finding may be explained by their inability to capture the very short-term impacts expected to be associated with acute psychological stress. Jarillo et al. (2016) focus on the impact of exposure to drug related turf wars in Mexico on educational performance and establish a negative effect of violence on educational performance, particularly in poor urban settings affected by the presence of drug gangs. They attribute this effect to supply-side impacts, including teacher absenteeism and turnover, as well as demand-side impacts, including students' absenteeism and tardiness. However, their study employs a less constrained and demanding difference-in-difference estimation, while they rely on data aggregated at the municipal level, rather than more precisely geo-locating data on homicides and educational performance. As a consequence they are unable to capture short-term effects of violence on educational performance, and they do not explore the role of psychological stress as a transmission mechanism. Finally, in an unpublished working paper Márquez-Padilla et al. (2015) investigate the impact of violence in Mexico on human capital accumulation and schooling decisions. Focusing on municipality-level effects of violence on school enrolment, they do not find any statistically significant impact for most age groups and time periods. Our results suggest that their null findings reflect the comparatively aggregated nature of their measures, and a corresponding failure to capture effects that depend on both geographic and temporal proximity.

The paper is organized as follows. In the next section, we provide background information on the violent conflict in Mexico. In Section 3 we describe the data and descriptive statistics. Section 4 describes the empirical strategy. Section 5 presents the core empirical results for violence over both seven days and nine months prior to exams. In Section 6 we discuss the mechanisms underpinning the relationship between violence and educational performance, and test alternatives to the psychological stress mechanism. In Section 7 robustness checks are presented. We conclude in Section 8.

2 Violence in Mexico

Violence associated with the illicit narcotics trade in Mexico first emerged as a significant issue in the 1980s when Colombian drug trafficking organizations (DTOs) began to co-operate with Mexican cartels in order to traffic drugs from Latin America to the United States. Violence was relatively low and geographically confined for the two decades that followed. With increasing political decentralization initiated by electoral reforms in 1997 (Rios, 2012), traditional patterns of power shifted in many local governments. This altered previously relatively stable corruption patterns and caused conflicts between DTOs to multiply (Guerrero-Gutiérrez, 2011).

By 2006, the beginning of this study, six major drug cartels were active in more than two thirds of Mexico's 2,457 municipalities. These cartels controlled different geographical areas of the drug trafficking network, with drug-related violence concentrated in states near the Mexico-U.S. border and along the Pacific coast (Shirk, 2010). In 2006 President Felipe Calderón was elected, and declared a "war on drugs", giving the military a mandate to disrupt trafficking patterns and to detain cartel leaders (drug lords). The arrest of cartel leaders, and disruption of existing drug trade networks, contributed to factionalization among the DTOs, expanding turf wars, and escalating levels of violence. This was accompanied by an increase in a variety of criminal activities among the cartels including kidnapping, torture, human and weapon trafficking and money laundering (Guerrero-Gutiérrez, 2011).

A critical feature of this escalating violence and criminality from the perspective of this study was its broad reach and focus on sowing generating fear and anxiety — psychological stress — within affected communities. Actual violence was frequently accompanied by threats, was often relatively public and graphic in order to sow fear, and increasingly targeted local politicians, journalists as well as the police and military. Additionally, while broad campaigns of kidnapping and extortion targeted a wide range of citizens — including, for example, individuals who participate in the conditional cash transfer program *Oportunidades* (Díaz-Cayeros et al., 2011). Meanwhile, at least some possible mechanisms by which violence may affect educational performance seem to play little role: there has been little destruction of educational infrastructure, there have not been broad based and long-term school closures and, as education funding is controlled by the federal government, it is thus not vulnerable to local political dynamics.

Notwithstanding the diversity of violence and threats, it is widely held in the literature that these broader criminal activities are proxied effectively by overall levels of homicides, which is our focus here. An overview of the geographic evolution of violence in Mexico's municipalities is provided in Figure 1 using the official municipal-level database (SIMBAD - *Sistema Estatal y Municipal de Bases de Datos*) from Mexico's autonomous governmental statistical agency (INEGI - *Istituto Nacional de Estadística and Geografía*). The maps show the distribution and dispersion of homicides across municipalities over time: violence is more intense and covers more municipalities as time goes by. Molzahn et al. (2013) document that at the end of the relevant period men were the largest share of the victims of drug-related killings (only 9% were women), with an average age of 32 years. This contrasts with the common perception that most victims were among unemployed youth without future prospects — and, as these adult victims are connected with other members of their communities, it is consistent with drug related violence having broader psychological impacts within the areas affected.

[Insert Figure 1 here]

We can place this expansion in violence in comparative context by comparing official homicides data from Mexico to cross-country data from the United Nations Office on Drugs and Crime (UNODC).[6] During this period Mexico's homicide rate moved from 7.8 per 100,000 of population in 2007, a level slightly higher than that of the United States (5.6/100,000), to 22.5 per 100,000 in 2011 — a level roughly on par with Brazil (23.5/100,000), which is widely regarded as one of the world's most violent countries. That said, it remained significantly lower than in either Honduras (91.4/100,000 in 2011) or Guatemala (38.6/100,000 in 2011), which have consistently been home to among the highest murder rates in the world.

In seeking to paint a more detailed view of the evolution of violence over time, we have access to a variety of alternative data sources on homicides in Mexico. They include several official sources, such as those from Mexico's statistical agency (INEGI), Mexico's National Security System (SNSP), and the Ministry of Health (SALUD — *Secretaría de Salud*), and a number of others from non-governmental sources, like the newspapers *Reforma* and *Milenio*. The INEGI dataset has been most widely used in the literature, but reports homicide data at the municipality level, and on a monthly basis. We correspondingly rely in what follows on source data from SALUD, which contains underlying death records disaggregated at the locality level, and reported on a daily basis. In order to ensure comparability with the INEGI dataset we classifying deaths in the SALUD dataset according to the International Classification of Diseases system (ICD-10) — the same classification employed by INEGI. In Figure 2, we plot homicide data at the national level from the often-used SIMBAD dataset from INEGI, the dataset that we assemble using data from SALUD, and the dataset of drug related homicides maintained by the *Reforma* newspaper. We see that in aggregate terms the INEGI and SALUD datasets are overlapping, confirming that the SALUD data that we employ is comparable with earlier studies using INEGI data, while providing more spatially and temporally disaggregated information on homicides.

[Insert Figure 2 here]

A key challenge in working with homicide data lies in identifying the precise extent of drug related violence, as distinct from other homicides. This challenge is reflected in Figure 2: the INEGI and SALUD datasets report a broader range of intentional homicides, while the Reforma dataset focuses on drug related homicides. While homicides increase rapidly in each case, the overall level of homicides is significantly higher than the level of homicides that can confidently be attributed to the drug trade. This reflects the existence of homicides unrelated to the drug trade, as well as the difficulty of classifying the motive for homicides across categories. That said, it is clear that the large increase in violence over time is overwhelmingly attributable to increases in drug related violence. Heinle et al. (2014) offer a detailed analysis of a range of alternative data sources, and conclude that while it is not possible to say with certainty which homicides are strictly drug related, this share has certainly increased over time: between 22% and 32% of all intentional homicides were classified by one of the data sources that they consider as drug-related in 2007, while this proportion had reached 45% to 72% in 2011. An alternative is to consider the share of homicides committed using firearms, which are generally associated with the drug trade (McDougal et al., 2013; Dube et al., 2013). In Figure 3 we present two figures using the SALUD data: the evolution of total homicides using three alternative definitions (homicides that were intentional or of unknown intent, intentional homicides, and homicides committed with firearms), and a graph of total homicides broken down by type of murder. In both cases the rapidly increasing share of homicides using firearms is apparent, with the increase in homicides using firearms accounting for most of the overall increase in homicides over time.

[Insert Figure 3 here]

3 Data and Descriptive Statistics

In the analysis to follow we rely on data on homicides from SALUD, on data from the ENLACE national standardized tests to measure educational performance, and on a range of control variables drawn from primarily from INEGI. We describe each in turn.

We rely on the SALUD dataset on homicides because it offers the highest level of spatial and temporal disaggregation, which is necessary to support our identification strategy. Homicides are reported at the locality level, on a daily basis, making it possible for us to link homicides to specific time periods, localities and schools. These localities are, in turn, small enough to allow for relatively accurate mapping of homicides with school locations: the average locality has a population of only 576 individuals, while municipalities, which are the level of aggregation used in earlier studies, are comprised of an average of 98 separate localities. As described above, we classify each homicide in the SALUD dataset according to the International Classification of Diseases system (ICD-10), which is also used by INEGI. In the core analysis we focus on homicides classified as intentional. In the sensitivity tests that follow we also consider a broader definition, which also incorporates homicides of unknown intent, and a more restrictive definition based only on deaths from firearms. The trends in these alternative variables are reported in Figure 3.

In order to measure educational performance we focus on results from *Evaluación Nacional del Logro Académico en Centros Escolares* (ENLACE) exams, which are standardized tests coordinated by the Minister of Public Education (SEP — *Secretaría de Educación Pública*), which have been conducted in all of Mexico's primary and secondary schools since 2005/06. At the primary school level students in grades 3 to 6 (corresponding to ages 8 through 11) have taken these exams in Mathematics and Spanish in every school year, while each year they also take an exam in one additional subject: Science in 2007/08; Civic Education and Ethics in 2008/09; History in 2009/10 and Geography in 2010/11. The tests are held during national evaluation week, which occurred in April in 2006 to 2010, and in late May in 2011. While they were not used for allocating school funding, assigning student grades, or evaluating teacher performance, they were widely cited and discussed in the media and among politicians (Agüero and Beleche, 2013).

Notwithstanding debates about the overall educational value of standardized tests, the ENLACE results are ideal for our purposes. Average scores by school, grade level and subject are downloadable from the SEP website and are directly comparable over time and across schools, allowing us to pool all schools and academic years into one data set and control for school fixed effects. Critically, Vázquez and Romero (2011) show that ENLACE results appear to be an accurate reflection of student learning, and thus a good measure of students' educational performance. Reflecting this assessment, ENLACE results have been used in several other academic studies (Agüero and Beleche, 2013; Alcaraz Pribaz et al., 2017; Estrada and Gignoux, 2014). Extensive measures are taken to insure the integrity of the tests results, including administration by independent teachers. There have been some accusations of fraud related to the exams, resulting from teachers urging weaker students to stay home and the illegal trading of exam copies before the exam date (Navarro, 2013). However, this effect appears limited. Further, it should not affect our overall identification strategy, as we have no reason to believe that such fraud is correlated with the intensity of violence, as discussed below.

Ultimately, our sample consists of average scores by school, subject (Math and Spanish) and grade level in order to create a panel dataset for all urban public schools, each of which is attached to state (32 in total), municipality (2,454) and locality (299,455) codes that allow us to match school locations to our data on homicides. Average scores range from 200 to 800, with significant variation in educational outcomes across schools and geographic areas. We are also able to retrieve attendance rates during the exam sessions, numbers of students enrolled and numbers of students who failed the exam, the administrative structure of the school (i.e. the role of the principal and other staff), as well as the number of schoolteachers, and the number of those instructors that have degrees. We employ these data to address alternative mechanisms linking violence to educational performance.

We focus exclusively on primary school because this avoids concerns about reverse causation running from educational performance to violence among older students. We focus on urban schools owing to the greater challenges of geo-matching violence and schools data in sparsely populated rural localities, and the fact that the surge in violence from 2006 occurred primarily in urban areas (Figure B1, in the Appendix). We likewise exclude private schools because the geographic coupling between students' places of residence and school location is less clear for the private school system: different private schools may draw students from long distances, while others may serve much smaller neighborhoods. We correspondingly use the private schools sample as a robustness check to illustrate the validity of our identification strategy in Section 7.

While the datasets are relatively clear, the key to our use of the data, and identification strategy, lies in geo-matching data on homicides with education performance at nearby schools. This allows us to identify the level of homicides occurring within a specific geographic radius around the school during a specific time period. This type of detailed matching has only appeared in one other paper, from Monteiro and Rocha (2016) for Brazil, and allows for the more precise identification of links between violence and educational performance.

Specifically, we map a series of concentric circles around each school, with radii of 2km, 5km and 10km. We then identify all homicides that occur during a given time period (i) within the overall radius covered by each circle (i.e. distances 0-2km, 0-5km and 0-10km), and (ii) within each individual "ring" created by those concentric circles (e.g. 0-2km, 2-5km and 5-10km). In the analysis to follow this allows us to understand the aggregate effect of all homicides within a given radius, as well as the incremental impact of homicides within each individual ring — something which, most importantly, allows us to assess whether violence occurring closer to schools has a larger impact on educational performance. Because homicides are identified only down to the locality level, rather than to a specific geo-location, our methodology requires us to match the homicides that occur within specific localities to specific "rings" around individual schools. Appendix A offers a more detailed description of the process used for achieving this linking.

Finally, while our analysis includes municipality fixed effects we also include a set of variables drawn from INEGI that are intended to control for time variant municipality level differences which could be related to both primary educational performance and the intensity of violence. We include the number of registered automobiles per capita, gross municipality expenditure per capita, gross income per-capita, population density at the municipality level, the population of the municipality, the amount of net public works and social expenditure per capita as a share of total net expenditure per capita, the value added per worker, an indicator for whether the area is a "low development" area, and the amount per capita spent on the conditional cash transfer program, *Oportunidades*. Following Dell (2015) we also include a variety of electoral indicators to ensure that we have appropriately captured the influence of the political environment of the school's location. Finally, we also calculate the distance between each school and the municipality seat (the midpoint of the locality that is the center of the municipality), as a proxy for the type of neighborhood (i.e. central to the city center and very dynamic vs. far outside the core and more likely to be close to shipping and industrial areas). Collectively, these variables seek to control for changes in the level of economic wealth and vibrancy of an area, which may affect both violence and educational performance.

Our final sample consists of an unbalanced panel of 17,632 public, urban, primary schools, across 1301 Mexican municipalities, observed beginning with the 2006/07 school year and ending in 2010/11.[7] The average ENLACE exam score is 515 with a standard deviation of 44. The average attendance rate for the exam in our representative sample is 94% and in the average school approximately 194 students are enrolled at exam time. These figures are shown in the upper panel of Table B1 in the Appendix B. For most municipal characteristics our sample is in line with or somewhat better off than the national average. Our measure of value-added per worker averages about 207 (1000$ MEX/Worker), while the national average in 2013 was closer to 170. This is consistent with our focus on an urban sample. Meanwhile, spending on social programs is somewhat lower in our sample, consistent with somewhat less poverty in urban areas.

Turning to the frequency of homicides near schools, the lower panel of Table B1 in Appendix B reports that on average schools were exposed to roughly 15 homicides over a school year of nine months within a 2km radius, increasing to roughly 30 homicides within a 5km radius and 58 within a 10km radius. This average masks a significant increase in violence over time. The mean value of the homicide count over a nine month period, within a 5km radius, increased from about 16 in 2006/07 to about 49 in 2010/11. Treating homicides as a dichotomous variable, about 9.1% of all schools experienced at least three homicides within a 2km radius in the week prior to exams at least once, with that figure increasing to

19.1 % of schools within a 5km radius and 29.1% within a 10km radius. Again this disguises a significant increase over time: in 2006/07, 1.7% of schools experienced at least three homicides in a 2km radius during the week before the exam, while that share had increased to 7.6% by 2010/11. Figure 4 offers an overview of the aggregate evolution of violence over time, graphing the weekly number of homicides nationally over the entire period, along with relevant exam dates.

[Insert Figure 4 here]

4 Identification Strategy

Our core focus is on estimating the marginal impact of increases in violence (measured by homicides) on educational performance (measured by exam scores) of primary school students, and in exploring the roles of acute and prolonged psychological stress in driving that relationship. We cannot directly measure the extent of acute and prolonged psychological stress affecting students, nor, by extension, the impact of such stress on exam scores. As such, our approach is to attempt to rule out alternative mechanisms that may explain any association between violence and exam scores, thus leaving acute and prolonged psychological stress, respectively, as the residual explanations for observed outcomes.

The existing literature describes a variety of potential mechanisms, which can be usefully classified into supply and demand side mechanisms. Supply-side mechanisms focus on possible drivers of declines in the quality or availability of education. Violence may result in the destruction of schools and related infrastructure or in school closures and broader disruption of academic activities. It may also result in declines in the quality of the learning environment owing to increases in principal or teacher turnover, or increased teacher absenteeism (Monteiro and Rocha, 2016).

Demand-side factors focus on the extent to which the performance of students may independently decline as a result of violence. Most research has focused on the possibility that violence may result in increased student absenteeism, withdrawal from school or out-migration. As discussed in the introduction, the roles of acute and prolonged psychological stress offer the primary alternative demand-side mechanisms, though they have yet to be compellingly estimated in existing research. In both cases exposure to violence may result in anxiety, intrusive thoughts, sleep disturbances, reduced cognitive development and aggressive behavior, as well as psycho-biological effects. This may occur through direct awareness of violence, or may be transmitted to younger children via parental stress (Margolin and Gordis, 2000). Most relevant to our study, this may collectively result in significant difficulties concentrating and, as a result, reduced exam performance.

Our core focus is on identifying the impact of acute psychological stress caused by exposure to homicides on exam scores. To do so we consider the number of homicides within the immediate vicinity of individual schools — that is, within a radius of 2km, 5km or 10km — over the course of the week (seven days) prior to the ENLACE standardized tests, and link that violence to educational performance, as measured by average scores on the ENLACE tests. We argue that evidence of such a temporally proximate link between local violence and decreased educational performance is most plausibly explained by an acute stress mechanism. The other mechanisms that are proposed in the literature to link violence to decreased performance are unlikely to operate over such a short time period: such violence does not produce destruction of infrastructure and is too temporally proximate to drive significant migration, while risks of reduced learning related to teacher or principal absenteeism, turnover or school closures are minimized by the very short time horizon — at most a few school days — relative to the longer period over which learning takes place. The only plausible alternative mechanism is through reduced attendance at exams, which we are able to test for directly.

In order to contextualize this short-term analysis we subsequently look at the impact of accumulated homicides over a period of nine months (roughly the length of the school year) on exam scores. Any decline in performance in this case is plausibly also attributable to psychological stress associated with prolonged exposure to violence. However, because of the longer time horizon alternative explanations become possible. We correspondingly attempt to test for alternative transmission mechanisms, including degradation of the learning environment and out-migration of better students. As importantly, if the short-term effect of homicides on exam scores is significantly larger than the long-term effect this would suggest the importance of an acute stress mechanism: it is the only hypothesized mechanism that is unique to the short-term, whereas if results are driven by alternative mechanisms from the literature that operate over both the short- and longer-term we would expect the magnitude of the short- and longer-term effects to be relatively similar.

For both the short- and longer-term analysis the equation that identifies the effect of violence on educational performance — represented by the ENLACE exam score, y_{ijt} , is:

$$y_{ijt} = \beta_1 H_{itd} + \beta_2 X_{ijt} + \beta_3 Z_{jt} + \gamma_i + \delta_t + \epsilon_{ijt}$$

where i, j, and t are subscripts for schools, municipalities, and school years, respectively, and d is a subscript for the radius or ring around a school in which homicides are counted. X_{ijt} is a vector of time-variant school characteristics and Z_{jt} is a vector of time-variant municipality characteristics. γ_i and δ_t are school and school year fixed effects, respectively. H_{itd} is the violence measure of interest: exposure to homicides for a given school i and school year t within a distance radius or ring d around the school.

We construct several measures of violence intensity that vary in the time period prior to the ENLACE tests — the seven days or nine months before the exam — and the distance from the school, defined by the radius (2km, 5km or 10km) or ring (0-2km, 2-5km or 5-10km) of interest. The generic formula for these measures of violence intensity can be seen as:

$$H_{it} = \sum_{l} \sum_{s} 1_{\{D_{il} \leq A\} \cap \{P_{e(t),s} \leq B\}} \cdot h_{lst}$$

where l is a subscript for localities, s is the date, and $e(t)$ is the exam date for a given school year t, and $D_{i,l}$ is the distance between a given school and a locality centroid. A is the cutoff threshold for the radius of interest. $P_{e(t),s}$ is the time between the date of a homicide and the exam date for the school year of interest. B is the cutoff threshold for the time range of interest. h_{lst} is a count of the number of homicides that occurred in a given locality on a given date in a given school year. This formula tells us the sum of the homicides that occurred during the time and distance range of interest in a radius around each school. Measures of the violence intensity in a ring of interest are generated by subtracting the smaller radius values — i.e. the violence intensity in a 2-5km ring is the conflict intensity in the 5km radius less that of the 2km radius. Testing both rings and radii around each school allows us to focus carefully on the linkages between where homicides happen and their impacts on children's educational performance: radii allow us to focus on "cumulative" effects, while rings enable us to delve further into the independent effects of violence in each geographic range, while controlling for violence in other distance ranges.

For the short-term analysis we focus on the raw number of homicides in the week prior to the exam, and construct dummy variables for three threshold levels of violence: three, five or seven homicides. We employ these dummy variables, rather than a continuous measure of homicides, for clarity of exposition, and in order to focus attention on the impact on educational performance of exposure to relatively high levels of violence. Table B1 in Appendix B reports homicide data during the period one week before the exam, for different levels of violence and different distances from schools. For the long-term analysis we initially employ a continuous measure of the number of homicides, reflecting the higher number of homicides overall (see Table B1 in Appendix B). While some previous studies have employed the homicide rate as their key variable of interest, we prefer to split the homicide rate into its component parts: the homicide count, and the log of population. This allows us to isolate the impact of each additional homicide, for comparison to the short-term results, and is thus clearer for purposes of exposition and interpretation. As critically, reliance on the homicide count then allows us to neatly capture the potentially non-linear relationship between homicides and exam scores by dividing the schools for analysis into "bins" based on the level of homicides in the preceding nine months (0-25 homicides, 25-100 and greater than 100).

We consider several potential threats to our identification strategy. The first issue is unobserved heterogeneity, such as average socioeconomic development in an area or the political power of the residents.[8] This could, for example, affect the distribution of public spending, which may affect both the intensity of local violence and educational performance. To account for this possibility we include municipality fixed effects, school year fixed effects, and a range of control variables, which account for these municipality-specific time-varying effects.

The second potential problem arises from reverse causality between educational performance and the local level of violence. While an increase in violence is likely to worsen performance, poor educational performance could also contribute to violence as violent groups can more easily recruit young individuals from badly performing schools.[9] As described earlier this is why we focus on primary school children, under the age of 12, who are far less likely to be targeted for recruitment or involvement in homicides. That said, it is important not to overstate this risk: reverse causation should not have any affect on our short-term estimates given the short time horizon.

Third, our identification strategy may be vulnerable to bias caused by teachers or students cheating. Among the ways that this might occur, teachers may strategically encourage and/or force low-performing students to stay home on exam days in order to have better school-level performance. Meanwhile, students may seek to gain access to exams in advance. This would be a threat to our identification strategy if there is a systematic relationship between cheating behavior and the intensity of violence, but there is no evidence that that is the case. To further protect against any risk we focus our analysis on the "representative sample" of schools recommended by SEP, for which exam attendance is at least 80% and there is no evidence of significant cheating.[10]

Finally, there may be a risk of measurement error in so far as our measure of homicides does not exclusively capture drug related violence. The SALUD data do not indicate whether a homicide is related to drug trafficking specifically, owing to the difficulty of such coding. As a result, we certainly include some non-drug related homicides. We do not, however, think that this poses a threat to our approach. The data presented in the previous section makes clear that the increase in violence from 2006 to 2011 is explained overwhelmingly by increases in drug related violence. In turn, we expect drug related violence to be more likely to cause psychological stress among students owing, for example, to it being more graphic, or associated with wider threats. As such, the inclusion of a broader range of homicides would, if anything, cause us to underestimate the impact of drug related violence on educational performance, making our estimates a lower bound.

5 Results

We focus first on estimating the impact of violence in the seven days prior to exams on exam scores, in an effort to isolate the role of acute psychological stress on educational performance. We rely on thresholds of three, five or seven homicides in the seven days prior to the exams to identify the effects of violence. The key message about the frequency of violence is that violence at these levels is far from universal, but nonetheless meaningful: 9.11% (9.73%, 17.19%) of schools experienced three or more homicides within a 0-2km (2-5km, 5-10km) ring around their school in the seven days before the exam for at least one of the years that we consider.

[Insert Table 1 here]

Table 1 presents the first set of results, which show a significant and meaningful negative impact of homicides on exam performance. Pooled regressions reported in columns (1), (3) and (5) show a positive correlation between increased violence and average exam scores. This is consistent with violence being concentrated in larger and more vibrant urban areas, which also tend to be home to stronger educational performance. Once we control for school fixed effects in order to properly estimate our relationship of interest the coefficients are rendered strongly negative: within-school estimations report statistically significant negative impacts of violence on educational performance that hold across different distances and thresholds of violence. The substantive magnitude of the effects is plausible and substantial. For schools with at least three homicides within a 2km radius, for example, the average exam score is roughly 4.4 points lower.

We can then look at how the magnitude of these effects varies across distance and levels of violence. To highlight the effects of homicides that occur at different distances from respective schools, Table 2 reports the results by rings — that is, the marginal impact of homicides 0-2km, 2-5km and 5-10km from schools. To measure these effects we control, in each case, for homicides within the smaller rings when computing the impact of additional homicides in the outer rings. At all of the violence thresholds we find that homicides that occur further from schools have a smaller effect on exam performance. Illustratively, if we focus on schools that experience at least three homicides, that level of violence within 2km is associated with a decline in exam scores of roughly 4.4 points, whereas homicides 2-5km and 5-10km from the school are associated with 2.3 and 1.6 point declines.

[Insert Table 2 here]

Focusing on changes in the size of the effect as the number of homicides increases we generally see larger effects at higher levels of violence. We see a large increase in the magnitude of the effect when we move from the three homicide to five homicide cut-off. As reported in Table 1, the magnitude of the effects within the 2km radius increases from 4.4 to 4.6, within the 5km radius it increases from 2.4 points to 5.0 points and within the 10km radius it increases from 1.5 points to 5 points. The fact that the effect of higher levels of violence becomes more important with greater distance seems intuitive. Within a 2km ring even three homicides is likely to be very psychologically stressful, whereas with greater distance it is only higher levels of violence that generate the same level of acute psychological stress. When we move to the seven homicide cut-off we do not see a further increase in in the magnitude of the impact on exam scores, but instead see a magnitude very similar to that for the five-homicide cut-off. This could be explained by the fact that the number of schools exposed to seven or more homicides is comparatively small, while in any case the overall pattern is broadly consistent with expectations.

Finally, we look at how these overall patterns vary by subject (math or Spanish) and grade level (3-6), with results reported in Table 3. We see a slightly stronger effect on math scores, though the effect is negative and significant for both subjects. This is consistent with previous literature generally suggesting that the impacts of psychological stress might be reflected more strongly in math scores as Spanish exercises (or other wording exercises) allow more 'flexible grading' (Baker and Hoekstra, 2010; Schwartz and Gorman, 2003). We similarly see slightly larger effects for younger students, which is encouraging in ruling out concerns about endogeneity between poor educational performance and violence. This is, again, consistent with the literature on violence and psychological stress, which has generally suggested that the effects may be largest for younger students (Rønholt et al., 2013; Allwood et al., 2002; Schwab-Stone et al., 1999).[11]

[Insert Table 3 here]

In order to place these results in broader context, we turn to estimating the longer-term impact of accumulated violence over the course of nine months prior to the exams. This serves two purposes: estimating this effect of accumulated violence is independently important, while also offering a perspective of the relative importance of the short-term effects reported so far.

Table 4 reports these longer-term results, again using 2km, 5km and 10km radii around the schools. As with the short-term effects, we find a positive relationship in a linear pooled model (column 1), while the relationship between homicides and exam scores becomes negative and significant after including school fixed effects (column 2). This holds true for homicides within 2km, 5km or 10km of the schools. The magnitude of the effect is relatively small: an increase of one homicide within 2km of a school over the preceding nine months is associated with a decrease in exam scores of only 0.009 points. Because we suspect that the impact of homicides on educational performance may be non-linear we then implement a non-linear model (column 3). The non-linear model appears to offer a better fit, with the estimated impact increasing about five fold, such that an additional homicide is associated with a 0.044 point decline in exam scores.

[Insert Table 4 here]

In order to more clearly capture the non-linear relationship between homicides and educational performance we divide our sample into three bins, capturing schools exposed to below 25 homicides, 25-100 homicides, and greater than 100 homicides — with the latter two categories including 17.8% and 5.8% of school-years, respectively. Consistent with a non-linear relationship, we find a larger and more significant negative association between homicides and exam scores in areas with at least 100 homicides. Schools that are exposed to at least 100 homicides within a 2km radius in the preceding nine months have, on average, exam scores that are 3.5 points lower. As we expand the radius, this effect declines to 2.2 points for the same level of homicides within a 10km radius. We also see some evidence of a smaller negative effect for schools exposed to 25-100 homicides, with a negative and significant coefficient when focusing on the 5km radius — and an estimated effect on scores of about 1.4 points.

Taken together the short- and longer-term results reveal a significant relationship between violence and educational performance, while pointing in particular toward the importance of relatively overlooked short-term effects, which we argue are associated with acute psychological stress. Our results suggest that relatively small numbers of homicides in the week immediately prior to the exams have the same effect on exam performance as much larger levels of violence over the preceding nine months. More specifically, we find that among schools that are exposed to at least three homicides within a 2km radius in the week prior to exams there is a decline in exam scores of 4.4 points. Within this group the average number of homicides is 6.8. Yet this comparatively small number of homicides has a larger effect on exam scores than being in a school that was exposed to at least 100 homicides over the preceding nine months (3.5 points, with an average of about 206 homicides). This suggests that the effect of each homicide is roughly 38 times larger when they occur in the week immediately prior to the exam. This pattern is illustrated graphically in Figure 5, which plots the impact of one additional homicide on exam scores over progressively longer time periods.[12]

[Insert Figure 5 here]

The magnitude of the effects that we identify is strikingly similar to those reported by Monteiro and Rocha (2016) in Brazil, in the only other study to have employed similarly geographically fine grained data and within school estimates of the effects of increases in violence. They focus on the impact of homicides that occur in the academic term prior to exams, using schools within 250m of favelas that experience gun-fights. Their measure is thus somewhat more geographically, and less temporally, proximate, but bears important similarities to our analysis. They ultimately report an effect equivalent to 0.051 standard deviations. This is about half of the magnitude that we report for the impact on exam scores of experiencing three homicides within 2km in the seven days prior to exams, which it is five or more times larger than our long-term effect. If anything the fact that their estimated long-term effect is smaller than our short-term effect — despite a very tight geographic focus — offers further support for the importance of a very short-term acute psychological stress mechanism.

The most similar previous study of the Mexican context comes from Jarillo et al. (2016), who estimate only the longer-term effect of violence on educational performance. They look at the impact of homicides over the course of the previous year on exam scores using municipal level data, and focus exclusively on math scores and homicides committed with firearms. They employ a difference-in-difference estimation strategy which is less constrained and demanding than our fixed effects specification and, consistent with that difference, report an effects of -0.045 standard deviations. As with Monteiro and Rocha (2016), this estimated effect is about half the size of our short-term effect, and significantly larger than our long-term effect. While our estimation strategy is more robust overall, we can usefully think of our long-term estimates as capturing a lower bound, while Jarillo et al. (2016) capture an upper bound. Meanwhile, their results again highlight the substantial magnitude of the short-term effect that we capture.

Finally, it is important to note that we are capturing the marginal impact of increases in violence over seven days or nine months, as distinct from the still longer-term effects on learning of living in areas affected by chronic violence. This has important implications, as explained in a similar study by Monteiro and Rocha (2016, p. 220): "It is worth emphasizing that our analysis estimates the effect of exposure to extreme but temporary episodes of violence, and does not take into account the cross-sectional variation in violence and the impact of being under the rule of drug dealers for extended periods. Consequently, one might reasonably interpret our estimates as a lower bound for the impact of drug-related violence on student achievement."

6 Understanding Mechanisms

We argue that the impact of homicides occurring in the seven days prior to exams on exam performance is best explained by an acute psychological stress transmission mechanism. While we cannot measure student stress levels directly, the short time horizon between homicides, and the significant decline that we observe in scores, seems to rule out most alternative mechanisms. On the supply-side there is no destruction of educational infrastructure, while broader impacts on student learning resulting from teacher turnover, teacher stress or absenteeism would be unlikely to have large effects given the short timeline — particularly for younger students for whom learning is likely to be cumulative over the school year. Meanwhile, demand side factors related to out-migration of the best students seem unrealistic over such a short time horizon. Critically our confidence in the relevance of the acute stress mechanism is reinforced by the magnitude of our empirical findings: the fact that homicides that occur immediately prior to exams are associated with dramatically larger declines in exam scores — more than 30 times larger — is highly consistent with acute stress being the driving factor. The acute psychological stress mechanism is the only hypothesized mechanism that operates exclusively over the short-term, and which could thus explain the much larger short-term effect. By contrast, if the effect were driven by alternative mechanisms that operate over both the short- and long-term we would expect the magnitudes of the short and long-term effects to be relatively similar.

There is, however, one possible alternative explanation for our short-term results: we can imagine stories in which local violence may result in reduced exam attendance concentrated among higher income and better performing students, thus resulting in lower average scores within affected schools. To rule out this possibility we run tests of the impact of homicides in the week prior to exams on exam attendance, which we are able to access alongside exam scores. Results are reported in Table 5, and we do see evidence of a very small negative impact of violence on exam attendance, with attendance in areas affected by violence declining by an average of about 0.3%. However, even if we make the extreme assumption that the 0.3% of students who do not attend would have all otherwise received perfect scores on the exam (800, as compared to a national average of 515), this would only result in the average score for that school declining by 0.85 points — as compared to a total decline in those areas of about 4.4 points. Under more realistic assumptions any effect of reduced attendance on exam scores would be marginal.[13]

[Insert Table 5 here]

Untangling the mechanisms underlying the longer-term effect of accumulated homicides over the course of nine months on exam scores is more complex. Prolonged psychological stress is one possibility, but a wide variety of other mechanisms from the literature are likewise possible. To gain some insight we run tests looking at two of the most common mechanisms suggested by the literature: out-migration of high performing students, and a decline in the quality of the learning environment. In both cases we fail to find significant and meaningful support for these alternative mechanisms, though in both cases our measures are subject to important limitations.

Table 6 reports results when looking at the impact of accumulated violence over nine months on levels of student migration, which we proxy by looking at declines in enrollment over the course of the school year. The intuition here is straightforward: violence may induce some students to move to new schools in safer areas, and higher income students — who tend to be better performing — are more likely to be able to move. In practice, we see a small and sometimes statistically significant increase in migration, of at most 0.5% of students. As with the case of reduced attendance at exams discussed above, this scale of migration would explain at most a small part of the total decline in exam scores even if we assume that all of the migrating students would be extremely high performing. In turn, this relatively low rate of migration is unsurprising: most migration is likely to occur between school years, whereas we are only interested in migration that occurs during the school year. That said, it is important to note that we are measuring gross changes in enrollment, rather than net migration: we thus cannot entirely rule out the possibility that larger numbers of high income and higher performing students migrate away, but are replaced by lower performing students — such a scenario would cause us to underestimate the potential role of migration in explaining the decline in exam scores.

We next consider whether there is evidence of violence contributing to reduced quality of the learning environment. Table 6 reports results when we look at the impact of homicides on teacher attrition, which has been reported to increase alongside violence in other contexts (Brück et al., 2014). The results are almost universally insignificant while the point estimates are of negligible magnitude, thus making clear that there is no decline in aggregate teaching resources.[14] However, this does not entirely rule out other mechanisms by which violence might undermine the quality of the learning environment, but for which we lack the data to conduct effective tests. While we are able to test the aggregate number of teachers at the beginning and end of the year, there may be increased turnover over the course of the year, which could undermine learning outcomes. Likewise, the quality of instruction could decline owing to psychological stress among teachers, increased absenteeism or school closures. Other studies offer reasons to believe that these factors may, in fact, play an important role. For example, Monteiro and Rocha (2016) find significant impacts in Brazil of violence on both teacher absenteeism (38% of the sample mean) and temporary school closings (31%).

[Insert Table 6 here]

Overall, these additional tests offer significant support for the relevance of an acute stress mechanism linking violence immediately prior to exams to reduced performance. Most alternative mechanisms proposed by the literature are not plausible over this short time period — or at least seem incapable of explaining the relatively large magnitude of the effect — while we are able to rule out reduced exam attendance as the most plausible alternative explanation of our results. Unpacking the mechanisms underpinning the longer-term relationship is more difficult, owing to the wider range of potentially confounding factors. It remains possible that this effect is attributable to the accumulated effects of prolonged stress caused by nearby violence. However, we are not able to rule out all alternative possibilities for the long-term effect.

7 Robustness Tests

To see if our results are dependent on the particular definition of violence used, we re-run our core short-term analysis using two alternative definitions of violence. Whereas our core results rely only on intentional homicides, we now consider, first, a combined measure of both intentional homicides and homicides of unknown intent, and, second, a measure of only those homicides committed with a firearm, which are most strongly associated with drug related violence. Results are presented in Table 7. We find very similar results in both cases, as the coefficients on homicides are negative and significant in all specifications, spanning differences in both distance and the extent of violence, while the magnitude of the coefficients is very similar to the core analysis.

[Insert Table 7 here]

Next we perform a series of placebo tests in order to ensure that our results are reliable. First, we run tests to see if we find any association between accidental deaths and educational performance. While accidental deaths may be traumatic within small circles of personal connections, we would not generally expect them to be related to the war on drugs, or to generate equivalent levels of psychological stress among students. And, indeed, results reported in columns (1)-(3) in Table 8 reveal no consistent connection between accidental deaths and exam scores. We find marginally significant results in some specifications, though even then with much weaker significance and smaller magnitudes than the core results. This is consistent with the potential for accidental deaths to cause some local psychological stress, but far less so than homicides. This offers confidence in our core results, and reinforces the fact that it is homicides specifically that are strongly associated with lower exam scores.

[Insert Table 8 here]

Second, we test the effects of homicides that occurred immediately after the exam period on educational performance, as homicides that occur after the exam date should not affect exam scores. This formulation is a common test in the literature (see, for example, Monteiro and Rocha (2016)). We focus on the second week following the exam, as we worry that data for the first week may be misleading: bodies that are found in the days immediately after the exam may actually have been murdered before the exam took place, or have been reflected in escalating threats and tensions within the community in the week before the exam. As reported in columns (4)-(6) of Table 8 we find no association between violence following the exam and average scores.

Third, we look at the impact of homicides on educational performance at private schools. Private schools do not have the same tight geographic links to their surrounding environments (i.e. their catchment areas are much less precisely defined and they may draw students from relatively far away). As such, our prior is that we should not see the same impact on exam scores of homicides that occur close to these schools because students themselves are not uniformly from those same communities. Results reported in columns (7)-(9) of Table 8 are consistent with this story. We see only very weak evidence of an association between homicides and exam scores, with only two of nine specifications yielding significant results, and the coefficients universally smaller in magnitude that in the core results. Moreover, the two significant results that we do find are for higher levels of violence, and larger radii around the schools, both of which are consistent with private school students being more widely dispersed and thus less affected by violence occurring directly around the schools.[15]

8 Conclusions

A growing strand of the literature has provided evidence that violence reduces educational achievement, attainment or performance. However, only a small part of this literature has focused specifically on students' educational performance, while evidence on the mechanisms underlying such a relationship has remained limited. The strongest existing evidence has focused on the impacts of violence on the supply of education, and on student migration, over the course of full school years. While a few recent studies have highlighted psychological stress as a potential alternative mechanism linking violence to educational performance, none of them have been able to test this potential relationship convincingly.

Against this background our key contribution lies in employing temporally and geographically disaggregated data on homicides and exam scores to provide evidence of the role of acute psychological stress as a key transmission mechanism linking violence and educational performance. While we cannot measure psychological stress, our detailed data allows us to implement an identification strategy that rules out competing explanations for the short-term connection between geographically proximate homicides and reduced exam performance. In turn, the comparison of the magnitude of our short- and longer-term estimates makes clear the importance of the timing of homicides in shaping exam performance — a dynamic that has been entirely overlooked by previous studies, and highlights the importance of acute psychological stress as a critical part of any story about violence and educational performance.

A final important question for our study is whether we are capturing a relationship between exposure to violence and transitory changes in exam performance, or broader negative impacts on student learning. In so far as the literature is correct in viewing the ENLACE exams as a genuine reflection of student learning, we feel relatively confident in asserting that our long-term results are capturing a reduction in student learning as a result of exposure to ongoing high levels of violence. The interpretation of our short-term results is more complex. We may be capturing a decline in long-term learning if acute psychological stress disrupts studying prior to exams in a way that results in reduced internalization of information from the preceding school year. Alternatively, we may be capturing a more transitory negative effect on exam scores owing to difficulty studying and writing exams, but without any significant decline in overall learning. In the latter case the impact of acute psychological stress on long-term human capital accumulation may be comparatively limited. However, even if we are capturing primarily a transitory effect of violence on exam scores this could have important consequences that demand policy responses. In so far as exam scores shape subsequent opportunities for students — such as admission to better schools as they age — then students living in areas heavily affected by violence could be systematically disadvantaged. Likewise, if exam scores are used, formally or informally, to evaluate teachers or assign school funding those in high violence areas may similarly face a systematic disadvantage over time — potentially reinforcing future violence and patterns of inequality. The results reported here minimally point toward the importance of considering the potential for violence to disrupt short-term educational performance, and the need to adapt accordingly through, for example, postponing exams, taking exposure to violence into account in evaluating student performance, or offering extra support to students (and, potentially, parents) exposed to such violence.

Notes

[1]https://justiceinmexico.org/severed-heads-found-outside-of-elementary-school-in-acapulco/

[2]In 2012 national elections saw the victory of Enrique Peña Nieto, who oversaw a progressive de-escalation of drug related violence, with overall levels of violence remaining high, but declining progressively beginning in 2012-13. In order to isolate our study from the effects of this broader transition we thus focus on the period of rapidly escalating violence from 2006-2011.

[3]In their most acute forms exposure to violence may give rise to Acute Stress Disorder (ASD) and Post Traumatic Stress Disorders (PTSD), though our interest is also in capturing the impacts of psychological stress that may fall short of these clinical thresholds but nonetheless have negative impacts on concentration and learning (Colman, 2009; Pynoos, 1994; Osofsky, 1995).

[4]One psycho-biological effect is cortisol disruption. Cortisol is a hormone produced by the human body, which is released in response to stress, and has negative consequences on the immune system, bone formation and increased blood pressure, among other effects, at elevated levels — an effect which was found among adolescents after exposure to community violence in Boston (Suglia et al., 2010).

[5]For example, children of parents who were exposed to war in Bosnia and Herzegovina demonstrated this effect (Bratti and Mendola, 2014).

[6]Data on homicides from the UNODC for Mexico is in line with official data from INEGI, thus ensuring the broad validity of these comparisons.

[7]In total, there are 2,454 municipalities in Mexico; 1,150 municipalities are not covered by our sample due to missing or implausible data or because none of the primary schools in

our representative, urban, public school sample reside in these municipalities.

[8]For a discussion of the relationship between crime and educational investment in Mexico, see Hansen (2010).

[9]For an overview of youth crime in Mexico and other Latin American countries, see e.g. World Bank (2011).

[10]We also confirm that there is no relationship between the sample of schools that are excluded from this representative sample and the intensity of violence.

[11]These patterns by subject and grade level are similar in the long-term, and are therefore not repeated here to avoid redundancy. The results are available on request from the author.

[12]Strictly speaking this figure likely understates the actual impact of one additional homicide over longer time periods, as it relies on a linear specification. The results in Table 4 reveal a larger coefficient when using a non-linear specification, while our analysis divided by bins yields an estimate per homicide that falls between the two. However, relative to the size of the effects when homicides occur closer to the exam date these differences are small, and the Figure thus accurately reflects the overall pattern of results.

[13]We also experiment with including attendance as a control in our main tests, and find a negligible effect on the results.

[14]We also run similar tests of the effect of violence intensity on student/teacher ratios, and on attrition among principals and administrative staff, and find similarly insignificant results.

[15]We conduct similar placebos tests for the long-term results, using both accidents and private schools. In both cases the placebo tests similarly yield insignificant results. These figures are available upon request from the author.

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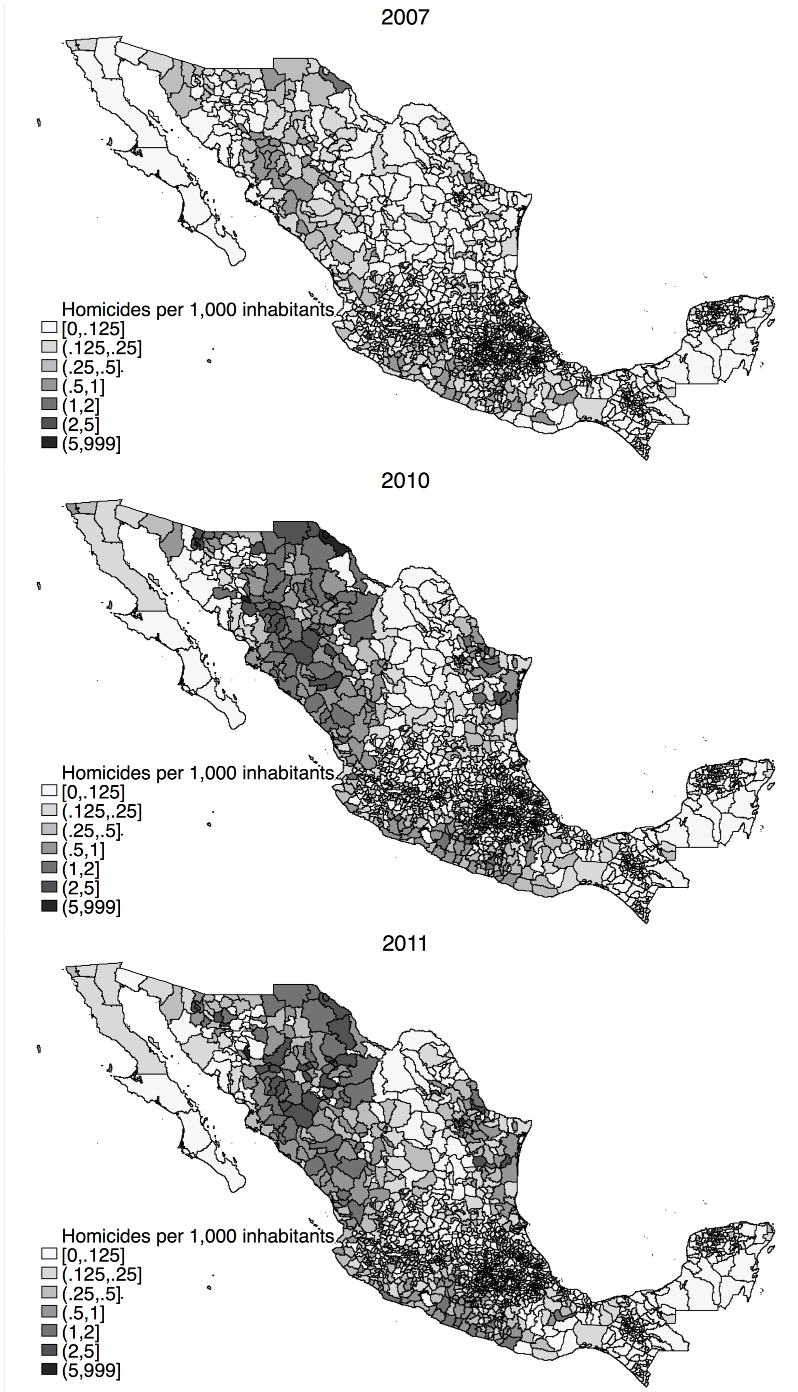
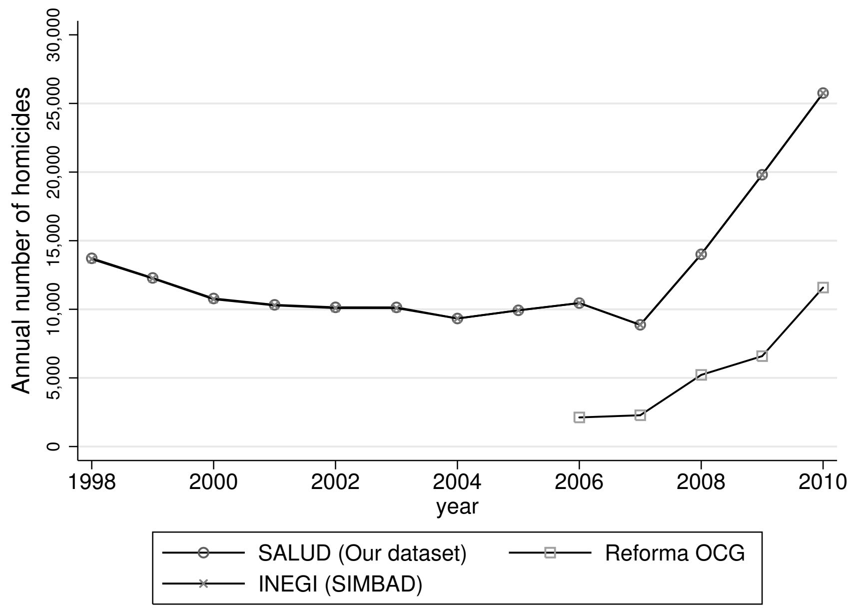
List of Figures and Tables to be inserted in the text

Figure 1: Homicides per 1,000 inhabitants by municipality over time

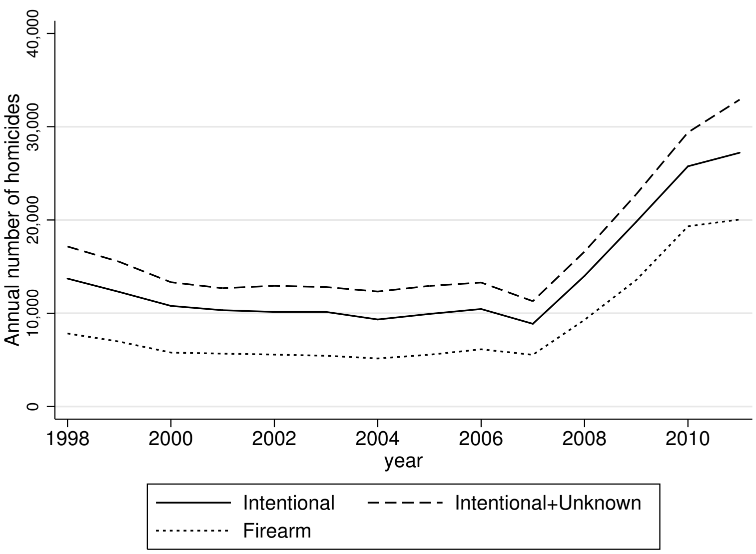


Notes: Authors' construction based on SIMBAD data. See appendix for details on the construction of the data.

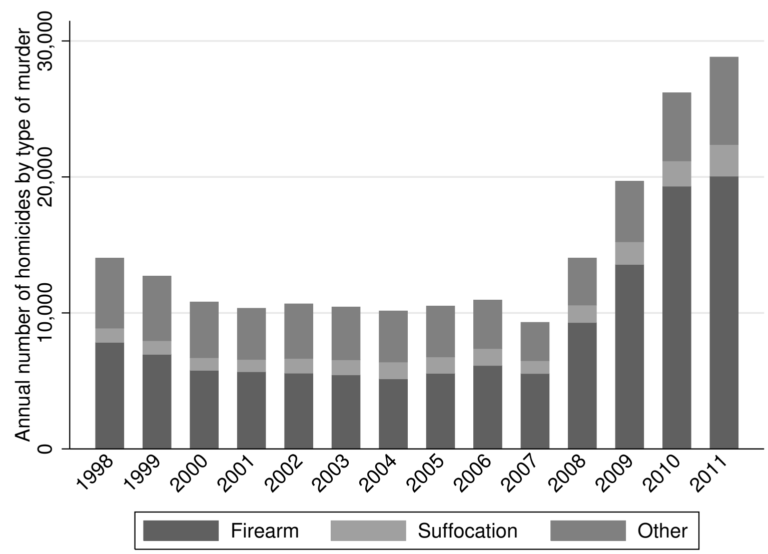
Figure 2: Comparison across different sources of homicide data



Notes: Authors' construction using SALUD (our preferred specification), *Reforma OCG* (source : https://justiceinmexico.org/data-portal/homicides/), and INEGI (SIMBAD) data. See appendix for details on construction of data.



(a) Comparison of different measures of homicide

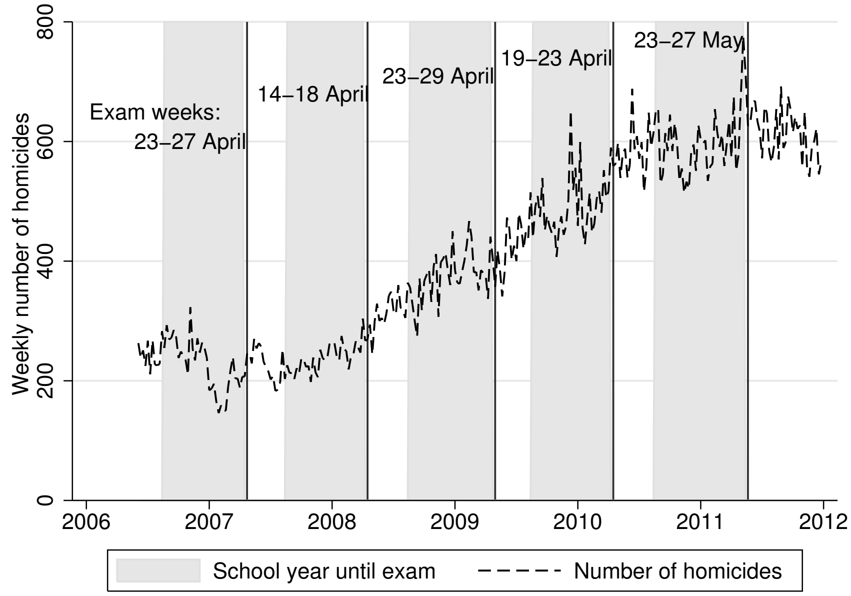


(b) Annual homicides by type of killing

Figure 3: Comparison of homicide measures and types

Notes: Authors' construction using SALUD data. See appendix for details on construction of data.

Figure 4: Weekly homicides count and school dates



Notes: Authors' construction using SALUD data. See appendix for details on the construction of the data. The shaded area indicates the duration of the school year, and the vertical line indicates the exam date for each year.

Table 1: The effect of violence on educational performance (using 7-day homicide measure and Km radii)

	DEPENDENT VARIABLE: Z-Average score					
	(1) $\lambda = 3$	(2) $\lambda = 3$	(3) $\lambda = 5$	(4) $\lambda = 5$	(5) $\lambda = 7$	(6) $\lambda = 7$
Distance radius up to: 2km						
No. of Homicides $\geq \lambda$	0.0663***	-0.0999***	0.1039***	-0.1027***	0.0987***	-0.1001***
	(0.0172)	(0.0160)	(0.0232)	(0.0222)	(0.0288)	(0.0243)
Distance radius up to: 5km						
No. of Homicides $\geq \lambda$	0.0742***	-0.0549***	0.0677***	-0.1141***	0.0995***	-0.1083***
	(0.0121)	(0.0112)	(0.0157)	(0.0142)	(0.0201)	(0.0163)
Distance radius up to: 10km						
No. of Homicides $\geq \lambda$	0.0008	-0.0346***	-0.0018	-0.1135***	-0.0005	-0.0983***
	(0.0107)	(0.0109)	(0.0113)	(0.0113)	(0.0124)	(0.0116)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	No	Yes	No	Yes	No	Yes
Municipality Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
School year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	56,262	56,262	56,262	56,262	56,262	56,262
Number of Schools	.	17,632	.	17,632	.	17,632

Notes: Each coefficient stems from a separate regression. The violence variable is a binary indicator of whether homicides numbering over 3 (5, 7) occurred in the previous 7 days to the exam in a 2 (5, 10) km radius around each school. The violence variable uses the INEGI definition of homicides. The λ for each column indicates the homicides threshold used in that column to generate the binary homicides indicator. Control variables are: Number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.

Table 2: The effect of violence on educational performance (using 7-day homicide measure and Km rings)

	DEPENDENT VARIABLE: Z-Average score					
	(1) $\lambda = 3$	(2) $\lambda = 3$	(3) $\lambda = 5$	(4) $\lambda = 5$	(5) $\lambda = 7$	(6) $\lambda = 7$
Distance rings up to: 2km						
No. of Homicides $\geq \lambda$, 0-2km	0.0663***	-0.0999***	0.1039***	-0.1027***	0.0987***	-0.1001***
	(0.0172)	(0.0160)	(0.0232)	(0.0222)	(0.0288)	(0.0243)
Distance rings up to: 5km						
No. of Homicides $\geq \lambda$, 0-2km	0.0709***	-0.1020***	0.1086***	-0.1088***	0.1024***	-0.1028***
	(0.0173)	(0.0160)	(0.0233)	(0.0222)	(0.0288)	(0.0243)
No. of Homicides $\geq \lambda$, 2-5km	0.0464***	-0.0569***	0.0386**	-0.1145***	0.0766***	-0.0903***
	(0.0152)	(0.0143)	(0.0195)	(0.0178)	(0.0276)	(0.0216)
Distance rings up to: 10km						
No. of Homicides $\geq \lambda$, 0-2km	0.0697***	-0.0991***	0.1028***	-0.1089***	0.1002***	-0.1028***
	(0.0173)	(0.0159)	(0.0235)	(0.0223)	(0.0289)	(0.0244)
No. of Homicides $\geq \lambda$, 2-5km	0.0504***	-0.0529***	0.0376*	-0.1093***	0.0770***	-0.0873***
	(0.0152)	(0.0143)	(0.0195)	(0.0179)	(0.0276)	(0.0216)
No. of Homicides $\geq \lambda$, 5-10km	-0.0611***	-0.0363***	-0.0466***	-0.0742***	-0.0398**	-0.0374**
	(0.0115)	(0.0128)	(0.0136)	(0.0156)	(0.0172)	(0.0169)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	No	Yes	No	Yes	No	Yes
Municipality Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
School year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	56,262	56,262	56,262	56,262	56,262	56,262
Number of Schools	.	17,632	.	17,632	.	17,632

Notes: Each coefficient stems from a separate regression. The violence variable is a binary indicator of whether homicides numbering over 3 (5, 7) occurred in the previous 7 days to the exam in a 0-2 (2-5, 5-10) km ring around each school. The violence variable uses the INEGI definition of homicides. The λ for each column indicates the homicides threshold used in that column to generate the binary homicides indicator. Control variables are: Number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.

Table 3: The effect of violence on educational performance by subjects and grades

	DEPENDENT VARIABLE: Z-Average score by Subject/Grade					
	(1) Math	(2) Spanish	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6
Distance radius up to: 2km						
No. of Homicides ≥ 5	-0.0498**	-0.0308	-0.0971***	-0.0798***	-0.0889***	-0.0149
	(0.0211)	(0.0197)	(0.0285)	(0.0295)	(0.0293)	(0.0295)
Distance radius up to: 5km						
No. of Homicides ≥ 5	-0.0673***	-0.0421***	-0.1227***	-0.0945***	-0.0553***	-0.0436**
	(0.0139)	(0.0131)	(0.0196)	(0.0196)	(0.0197)	(0.0196)
Distance radius up to: 10km						
No. of Homicides ≥ 5	-0.0792***	-0.0638***	-0.1282***	-0.1113***	-0.0942***	-0.0672***
	(0.0115)	(0.0107)	(0.0159)	(0.0158)	(0.0161)	(0.0161)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
School year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	44,588	44,589	44,617	44,542	44,476	44,389
Number of Schools	17,473	17,473	17,499	17,465	17,432	17,376

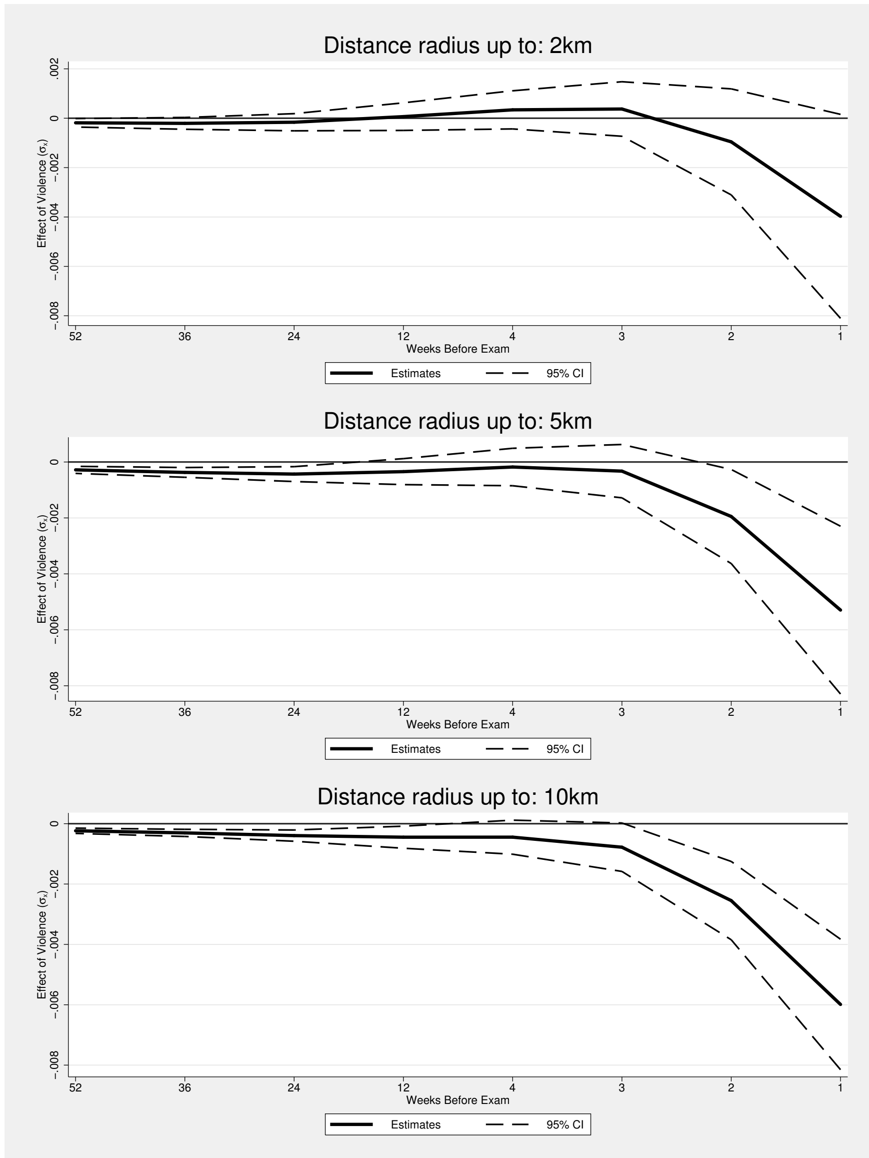
Notes: Each coefficient stems from a separate regression. The violence variable is a binary indicator of whether homicides numbering over 5 occurred in the previous 7 days to the exam in a 2 (5, 10) km radius around each school. Each column is a different dependant variable, reflecting the standardized score for the subject/grade indicated in that column. The violence variable uses the INEGI definition of homicides. Control variables are: Number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.

Table 4: The effect of violence on educational performance (using 9-month homicide measure and Km radii)

	DEPENDENT VARIABLE: Z-Average score			
	(1)	(2)	(3)	(4)
Distance radius up to: 2km				
Homicide Count	0.0004***	-0.0002*	-0.0010***	
	(0.0001)	(0.0001)	(0.0003)	
Homicides Count^2			0.0000***	
			(0.0000)	
Up To 25 Homicides				0.0229**
				(0.0102)
25-100 Homicides				-0.0151
				(0.0186)
Over 100 Homicides				-0.0799***
				(0.0291)
Distance radius up to: 5km				
Homicide Count	0.0005***	-0.0004***	-0.0011***	
	(0.0001)	(0.0001)	(0.0002)	
Homicides Count^2			0.0000***	
			(0.0000)	
Up To 25 Homicides				0.0142
				(0.0108)
25-100 Homicides				-0.0327*
				(0.0179)
Over 100 Homicides				-0.0945***
				(0.0240)
Distance radius up to: 10km				
Homicide Count	0.0003***	-0.0003***	-0.0010***	
	(0.0000)	(0.0001)	(0.0002)	
Homicides Count^2			0.0000***	
			(0.0000)	
Up To 25 Homicides				0.0366**
				(0.0147)
25-100 Homicides				0.0103
				(0.0202)
Over 100 Homicides				-0.0490*
				(0.0254)
Controls	Yes	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Municipality Time Trend	Yes	Yes	Yes	Yes
School year FE	Yes	Yes	Yes	Yes
Sample Size	56,262	56,262	56,262	56,262
Number of Schools	.	17,632	17,632	17,632

Notes: Each coefficient stems from a separate regression. The violence variables are defined using the INEGI definition of homicides over the 9 months prior to the exam (i.e. 8-267 days prior to the exam) in a 2 (5, 10) km radius around each school. The variable 0-25 homicides is a dummy variable indicating that the nine month homicide count fell within that range, similarly for 25-100 homicides, and over 100 homicides. Control variables are: log(population) at the municipality level, the number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.

Figure 5: The effect of violence over time



Notes: The violence variables are defined using the INEGI definition of intentional homicides in a 2 (5, 10) km radius around each school. Date range is from the week listed (e.g. 52) until one week before the exam. The estimates plotted in this graph are shown in Figure B3 in the Appendix. Control variables are: Number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school. The sample used is the "representative" sample, as defined by the SEP.

Table 5: The exam attendance mechanism

	DEPENDENT VARIABLE: Exam Attendance		
	(1) $\lambda = 3$	(2) $\lambda = 5$	(3) $\lambda = 7$
Distance radius up to: 2km			
No. of Homicides $\geq \lambda$	-0.2945***	-0.3724***	-0.2161
	(0.0975)	(0.1267)	(0.1464)
Distance radius up to: 5km			
No. of Homicides $\geq \lambda$	-0.2624***	-0.1773**	0.0435
	(0.0689)	(0.0852)	(0.1023)
Distance radius up to: 10km			
No. of Homicides $\geq \lambda$	-0.3309***	-0.2559***	-0.0584
	(0.0630)	(0.0659)	(0.0693)
Controls	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Municipality Time Trend	Yes	Yes	Yes
School year FE	Yes	Yes	Yes
Sample Size	56,262	56,262	56,262
Number of Schools	17,632	17,632	17,632

Notes: Each coefficient stems from a separate regression. The violence variable is a binary indicator of whether homicides numbering over 3 (5, 7) occurred in the previous 7 days to the exam in a 2 (5, 10) km radius around each school. The violence variable uses the INEGI definition of homicides. The λ for each column indicates the threshold used in that column. The dependant variables are the exam attendance rate as calculated using ENLACE data. Control variables are: Number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.

Table 6: The student migration and teacher attrition mechanisms

	DEPENDENT VARIABLE:					
	Student Migration			Teacher Attrition		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance radius up to: 2km						
Homicides Count	0.0000***	0.0000		-0.0000	-0.0000	
	(0.0000)	(0.0000)		(0.0000)	(0.0000)	
Homicides Count^2		-0.0000			0.0000	
		(0.0000)			(0.0000)	
Up To 25 Homicides			-0.0003			-0.0010
			(0.0008)			(0.0016)
25-100 Homicides			0.0037**			0.0009
			(0.0015)			(0.0026)
Over 100 Homicides			0.0050*			0.0005
			(0.0028)			(0.0038)
Distance radius up to: 5km						
Homicides Count	0.0000*	0.0000		-0.0000	-0.0000	
	(0.0000)	(0.0000)		(0.0000)	(0.0000)	
Homicides Count^2		-0.0000			0.0000	
		(0.0000)			(0.0000)	
Up To 25 Homicides			-0.0007			-0.0030
			(0.0010)			(0.0019)
25-100 Homicides			0.0015			-0.0057**
			(0.0016)			(0.0026)
Over 100 Homicides			0.0036*			-0.0034
			(0.0021)			(0.0036)
Distance radius up to: 10km						
Homicides Count	0.0000	0.0000**		-0.0000	-0.0000	
	(0.0000)	(0.0000)		(0.0000)	(0.0000)	
Homicides Count^2		-0.0000***			0.0000	
		(0.0000)			(0.0000)	
Up To 25 Homicides			-0.0017			-0.0046*
			(0.0015)			(0.0025)
25-100 Homicides			-0.0009			-0.0049
			(0.0018)			(0.0031)
Over 100 Homicides			0.0041			-0.0021
			(0.0026)			(0.0037)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
School year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	56,262	56,262	56,262	56,148	56,148	56,148
Number of Schools	17,632	17,632	17,632	17,590	17,590	17,590

Notes: Each coefficient stems from a separate regression. The violence variables use the INEGI definition of homicides. The dependant variables are the percentage difference in number of students between start and end of term (% Δ No. Students) and the percentage difference in number of teachers between start and end of term (% Δ No. Teachers). The variable 0-25 homicides is a dummy variable indicating that the nine month homicide count fell within that range, similarly for 25-100 homicides, and over 100 homicides. Control variables are: log(population) at the municipality level, the number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.

Table 7: Sensitivity checks using alternative violence definitions

	DEPENDENT VARIABLE: Z-Average score					
	Intentional + Unknown Intent			Firearms		
	(1) $\lambda = 3$	(2) $\lambda = 5$	(3) $\lambda = 7$	(4) $\lambda = 3$	(5) $\lambda = 5$	(6) $\lambda = 7$
Distance radius up to: 2km						
Homicide Count $\geq \lambda$	-0.0631***	-0.1070***	-0.0914***	-0.0967***	-0.0664***	-0.0732***
	(0.0133)	(0.0200)	(0.0231)	(0.0187)	(0.0245)	(0.0248)
Distance radius up to: 5km						
Homicide Count $\geq \lambda$	-0.0394***	-0.1045***	-0.0853***	-0.0931***	-0.0725***	-0.0748***
	(0.0098)	(0.0130)	(0.0144)	(0.0126)	(0.0162)	(0.0173)
Distance radius up to: 10km						
Homicide Count $\geq \lambda$	-0.0279***	-0.1062***	-0.1028***	-0.1118***	-0.0846***	-0.0743***
	(0.0099)	(0.0108)	(0.0113)	(0.0104)	(0.0116)	(0.0131)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
School year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	56,262	56,262	56,262	56,262	56,148	56,262
Number of Schools	17,632	17,632	17,632	17,632	17,632	17,632

Notes: Each coefficient stems from a separate regression. The violence variable is a binary indicator of whether homicides numbering over 3 (5, 7) occurred in the previous 7 days to the exam in a 2 (5, 10) km radius around each school. The violence variables are defined using the INEGI definition of intentional homicides and those of unknown intent in (1), (2), and (3) and using the firearms homicides definition in (3), (4), and (5) over the 7 days prior to the exam in a 2 (5, 10) km radius around each school. The λ for each column indicates the threshold used in that column. Control variables are: Number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.

Table 8: Placebo tests

	DEPENDENT VARIABLE: Z-Average score								
	Using Accidents			After-Exam Effect			Using Private Schools Sample		
	(1) $\lambda = 3$	(2) $\lambda = 5$	(3) $\lambda = 7$	(4) $\lambda = 3$	(5) $\lambda = 5$	(6) $\lambda = 7$	(7) $\lambda = 3$	(8) $\lambda = 5$	(9) $\lambda = 7$
Distance radius up to: 2km									
No. of Homicides $\geq \lambda$	-0.0212*	-0.0173	-0.0406**	0.0265	0.0218	0.0247	0.0184	0.0094	-0.0435
	(0.0120)	(0.0157)	(0.0183)	(0.0197)	(0.0205)	(0.0257)	(0.0277)	(0.0341)	(0.0422)
Distance radius up to: 5km									
No. of Homicides $\geq \lambda$	-0.0242**	0.0040	-0.0122	-0.0050	-0.0129	-0.0156	-0.0039	-0.0224	-0.0195
	(0.0101)	(0.0109)	(0.0127)	(0.0130)	(0.0154)	(0.0172)	(0.0193)	(0.0227)	(0.0254)
Distance radius up to: 10km									
No. of Homicides $\geq \lambda$	-0.0217**	-0.0192**	-0.0012	0.0130	0.0243**	-0.0199*	-0.0194	-0.0489**	-0.0323*
	(0.0098)	(0.0095)	(0.0108)	(0.0122)	(0.0121)	(0.0116)	(0.0206)	(0.0206)	(0.0190)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	56,262	56,262	56,262	56,262	56,262	56,262	14,215	14,215	14,215
Number of Schools	17,632	17,632	17,632	17,632	17,632	17,632	4,768	4,768	4,768

Notes: Each coefficient stems from a separate regression. The violence variable is a binary indicator of whether deaths numbering over 3 (5, 7) occurred in the previous 7 days to the exam in a 2 (5, 10) km radius around each school. The placebo variables are the number of accidental deaths in (1), (2), and (3); the number of homicides in the second week after the exam (i.e. 8-14 days after) in (3), (4), and (5); and the INEGI homicides definition over the 7 days prior to the exam for the private schools sample in (6), (7), and (8). In each case, regressions are performed at a 2 (5, 10) km radius around each school. The λ for each column indicates the threshold used in that column. Control variables are: Number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.

Appendix A

A9 Classification of Drug-Related Homicides

We use three different definitions of homicides: intentional homicides; intentional homicides and those of unknown intent; and homicides committed with firearms. In all cases we draw on INEGI classifications, which are based on the following definitions from the International Classification of Diseases system (ICD-10):

Intentional homicides:

ICD-10 X85-Y09
Source: *http://www.inegi.org.mx*

Intentional homicides and homicides of unknown intent:

Intentional homicides: ICD-10 X85-Y09
Homicides for which intent is undetermined: Y10-Y34, Y87.2, Y89.9
We also include homicides of state security personnel (legal intervention/war) among those of unknown intent, as they seem comparatively likely to be associated with the war on drugs: Y35-Y36, Y89(.0, .1)
Source: External Cause of Injury Mortality Matrix for ICD-10,
www.cdc.govnchsdataiceicd10_ transcode.pdf

Homicides by firearm:

X93-X95, Y22-Y25 and Y350
We rely on homicides using firearms as a proxy for drug-related violence. The total number

of homicides using firearms lies between official figures on intentional homicides and unofficial statistics from media outlets on drug related homicides.

Source: External Cause of Injury Mortality Matrix for ICD-10,

www.cdc.govnchsdataiceicd10_ transcode.pdf

A10 Homicide Measure Construction & Data Imputation Strategy

Because data on homicides are reported at the locality level, rather than having specific location coordinates, we need to employ a strategy for matching locality level data on homicides as precisely as possible to specific geographic areas around individual schools.

To do this we rely on two key pieces of data. First, we have access to geographic coordinates for individual schools. Second, we have coordinates for the centroid for each locality – the point such that equal amounts of area would be located on either side of any straight line passing through that point. Because homicides are recorded at the locality level, the best estimate of the distance between a school and a particular homicide is the distance between the school's coordinates and the coordinates of the locality centroid for the locality in which the homicide occurred. Because localities are generally very small, with an average population size of 576, this mapping is normally straightforward.

However, in some case where localities are geographically larger the mapping can be slightly more complex. In some cases this counting process means that a school may not necessarily be assigned the homicides for its own locality. This can occur if there are other locality centroids which are within the radius (or ring) but the "home" locality of the school is not within the radius (or ring). In such cases only those localities inside the radius or ring are assigned.

In other cases, schools are located in areas such that one or more of the radius (or ring) values of interest do not contain the centroid of any locality. In this case, an imputation strategy is employed to generate a similar measure of the number of homicides within the radius (or ring). Specifically, a school is assigned the homicide values for the locality within which it is located if the radius of interest does not contain any locality centroids. Because this might be true of the 2km, 5km, or 10km radius measures (or rings), when more than one of the measures do not contain a single locality centroid, then the homicide count of the home locality is assigned to both radius or ring measures in proportion to the percentage of the area covered by each radius/ring. For example, if both the 0km – 2km ring and the 2km – 5km ring are missing, because there are no locality centroids within those rings around the school, then 1/5 of the homicides for the home locality are assigned to the 0km – 2km ring, and 4/5 the values are assigned to the 2km – 5km ring, as this roughly corresponds to the percentage of the total area that falls within each ring.

A11 Population Measure Construction & Data Imputation Strategy

The INEGI Census of Population and Housing is decennial. Thus, data is only available for 2000 and 2010, at the locality level. In order to estimate the population of each locality in the intervening years, we calculate the population for each locality in each year under a constant growth rate, according to the following formula:

$$pop_{lt} = pop_{l,2000} * \left[\left(\frac{pop_{l,2010}}{pop_{l,2000}} \right)^{0.1} \right]^{t-2000}$$

where l is a subscript for localities, and t is the academic year. We use this to construct measures of the population inside each radius or ring around a school.

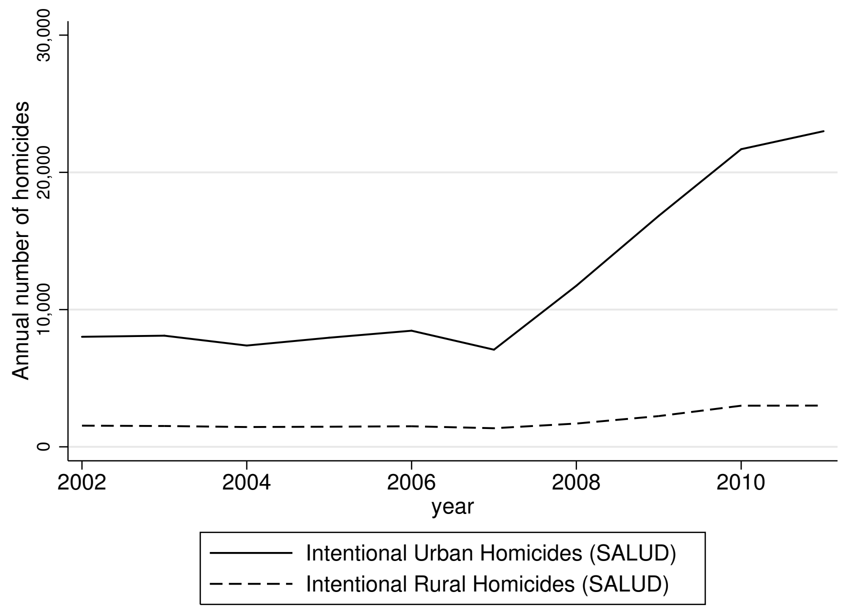
Similar to the data construction for homicides discussed in Appendix A Section A10 the number of people residing within a given radius (or ring around) of a school cannot be precisely determined, but can be estimated using the coordinates of the school and the localities that are inside the population radius (or ring) of interest. If a locality's centroid falls within the radius (or ring) of interest, then the full population of the locality is counted as falling within the radius or ring. As in the case of homicides, if no locality centroid falls within the radius (or ring) of interest, then the population of the "home" locality is used, in proportion to the area of the radius (or ring) that is missing data.

In some cases, there are schools that are missing population data, but not homicide data. These cases arise primarily when schools are in "fringe" or boundary areas of a municipality – i.e. near rural localities, but still themselves in areas that are defined as urban. This is a plausible cause of missing data, because Mexico employs random sampling techniques to construct census data in rural areas, rather than mandating door to door enumeration (INEGI, 2003). Because of how rural localities are defined (a rural locality is any that includes at least one dwelling place that is in a rural municipality/rural geographical basic statistical division) they can have very low populations (in some cases down to a single household). Combined with the sampling methodology for the census, this can result in missing population data for some rural localities. This can, in turn, imply missing population data in some of the rings around urban school that lie near the boundaries of urban areas. In order to address this problem when there is a missing population value we use the mean population of the five nearest non-missing neighboring localities in place of the missing value.

In some cases, these strategies can cause the 2km radius to be assigned a larger population than the 5km radius (or the 5km radius larger than the 10km radius). When this happens, the ring measures (i.e. the 2-5km ring or the 5-10km ring) are recorded as having negative populations. When logarithms of these populations are taken, these generate missing values. In order to mitigate this issue, the population values are smoothed. Effectively, if the 5km radius has a lower population than the 2km radius, for example, then the two population values are reversed. An alternate interpretation of this procedure is that the excess population that is assigned to the 2km radius is re-assigned to the 5km radius. This ensures that the population is monotonically increasing in radius around every school, which in turn ensures that there are no negative population value rings, and no missing logarithms.

Appendix B: List of Figures and Tables for the Online Appendix

Figure B1: Comparison of Homicide Counts in Urban vs Rural Areas



Notes: Authors' construction using SALUD data. See appendix for details on the construction of the data.

Table B1: Descriptive statistics

	Mean	Std.Dev.	Min	Max
School Characteristics				
Average Exam Score	515	(44.26)	334	782
Exam Attendance Rate	94	(4.30)	80	100
Number of Students Examined	194	(123.50)	8	1,183
School-Municipality Centre Distance (Km)	4	(10.31)	0	204
Municipality Characteristics				
Share w/o Social Security	0.56	(0.19)	0	1
Cars per capita	0.16	(0.10)	0	1
Density (Inhabitants/km^2)	32.77	(100.57)	0	949
Gross Income per capita (1000s of $MXN)	884.34	(1232.41)	2	7,495
Share of Expenditure on Social Programs	0.29	(0.17)	0	1
Oportunidades Expenditure per capita (1000s of $MXN)	0.43	(0.45)	0	22
Value Added (1000s of $MXN/Worker)	207.21	(527.16)	3	17,570
Homicides: Dichotomous Measures				
No. of Homicides $\geq$ 3, 7 Days, 2km Radius	0.05	(0.21)	0	1
No. of Homicides $\geq$ 3, 7 Days, 5km Radius	0.11	(0.31)	0	1
No. of Homicides $\geq$ 3, 7 Days, 10km Radius	0.21	(0.41)	0	1
No. of Homicides $\geq$ 5, 7 Days, 2km Radius	0.02	(0.16)	0	1
No. of Homicides $\geq$ 5, 7 Days, 5km Radius	0.06	(0.23)	0	1
No. of Homicides $\geq$ 5, 7 Days, 10km Radius	0.14	(0.35)	0	1
No. of Homicides $\geq$ 7, 7 Days, 2km Radius	0.02	(0.12)	0	1
No. of Homicides $\geq$ 7, 7 Days, 5km Radius	0.03	(0.18)	0	1
No. of Homicides $\geq$ 7, 7 Days, 10km Radius	0.09	(0.29)	0	1
Homicides: Continuous Measures				
Homicide Count, 9 Months, 2km Radius	15.27	(47.88)	0	614
Homicide Count, 9 Months, 5km Radius	29.56	(66.48)	0	614
Homicide Count, 9 Months, 10km Radius	57.94	(108.22)	0	804
Sample Size	56,262			

Note: Authors' calculations using ENLACE, INEGI, and SALUD data. For details on the construction of the variables, please see the appendix. Monetary values are reported in deflated $MXN, except the value added figure, which is reported in nominal $MXN.

Table B2: Number of public schools exposed to homicides in the 7 days before exam

Homicide Count	2007		2008		year 2010		2011		Total	
	No.	%	No.	%	No.	%	No.	%	No.	%
0	11,850	87.40%	9,690	83.47%	12,597	80.85%	12,465	80.35%	46,602	82.83%
1	995	7.34%	1,397	12.03%	1,315	8.44%	1,376	8.87%	5,083	9.03%
2	480	3.54%	294	2.53%	705	4.52%	484	3.12%	1,963	3.49%
3	148	1.09%	74	0.64%	127	0.82%	297	1.91%	646	1.15%
4	69	0.51%	43	0.37%	214	1.37%	243	1.57%	569	1.01%
5	16	0.12%	53	0.46%	137	0.88%	256	1.65%	462	0.82%
6	0	0.00%	0	0.00%	0	0.00%	79	0.51%	79	0.14%
7 or More	0	0.00%	58	0.50%	486	3.12%	314	2.02%	858	1.53%
Total	13,558	100.00%	11,609	100.00%	15,581	100.00%	15,514	100.00%	56,262	100.00%

Notes: Authors' calculations using SALUD data on intentional homicides. The sample considered here is urban public schools. See appendix for details on the construction of the data.

Table B3: The effect of violence over time (estimates for Figure 5)

	DEPENDENT VARIABLE: Z-Average score							
	(1) $\lambda = 52$	(2) $\lambda = 36$	(3) $\lambda = 24$	(4) $\lambda = 12$	(5) $\lambda = 4$	(6) $\lambda = 3$	(7) $\lambda = 2$	(8) $\lambda = 1$
Distance radius up to: 2km								
Homicide Count, λ Weeks Before	-0.0002**	-0.0002*	-0.0002	-0.0000	0.0005	0.0005	0.0001	-0.0040*
	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0004)	(0.0004)	(0.0009)	(0.0021)
log(Population)	0.6165***	0.6166***	0.6167***	0.6187***	0.6176***	0.6176***	0.6186***	0.6201***
	(0.0771)	(0.0771)	(0.0771)	(0.0771)	(0.0771)	(0.0771)	(0.0771)	(0.0771)
Distance radius up to: 5km								
Homicide Count, λ Weeks Before	-0.0003***	-0.0004***	-0.0005***	-0.0004**	-0.0001	-0.0001	-0.0010	-0.0053***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0004)	(0.0004)	(0.0007)	(0.0015)
log(Population)	1.0671***	1.0687***	1.0681***	1.0801***	1.0809***	1.0809***	1.0816***	1.0800***
	(0.1112)	(0.1112)	(0.1112)	(0.1111)	(0.1111)	(0.1111)	(0.1111)	(0.1111)
Distance radius up to: 10km								
Homicide Count, λ Weeks Before	-0.0002***	-0.0003***	-0.0004***	-0.0005***	-0.0004	-0.0004	-0.0017***	-0.0060***
	(0.0000)	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0003)	(0.0006)	(0.0011)
log(Population)	1.4062***	1.4024***	1.3986***	1.4024***	1.3937***	1.3937***	1.4056***	1.4112***
	(0.1346)	(0.1346)	(0.1346)	(0.1346)	(0.1348)	(0.1348)	(0.1348)	(0.1347)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	56,262	56,262	56,262	56,262	56,262	56,262	56,262	56,262
Number of Schools	17,632	17,632	17,632	17,632	17,632	17,632	17,632	17,632

Notes: Each coefficient stems from a separate regression. Date range is from the week listed (e.g. 52) until one week before the exam. The violence variables are defined using the INEGI definition of intentional homicides. Control variables are: log(population) at the municipality level, the number of enrolled students (school level), student/teacher ratio (school level), teachers with a degree (school level), principals groups (school level), groups (school level), registered automobiles per capita (municipality level), gross expenditure (municipality level), expenditure *Oportunidades* (municipality level), social expenditure as share of total net expenditure (municipality level), population(municipality level), population density (municipality level), low development indicator (municipality level), value added per worker (municipality level), the school's distance from the municipality center, and a variety of indicators for election years and victories by particular parties. Standard errors are clustered by school and are reported in parentheses. *,** and *** denote significance level of 10%, 5% and 1%, respectively. The sample used is the "representative" sample, as defined by the SEP.