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Abstract

This paper examines the impact of urban violence on the standardized test scores of public school students in Medellin. I use the school-level variation in exposure to local homicides for years 2004-2013 in a model that includes school and year fixed effects, allowing me to control for the endogeneity of violence. I find that each additional homicide per year occurring within 500 meters of a school reduces student achievement by slightly over 0.01 standard deviations on a variety of tested academic subjects. For an average school, this implies dropping from the 50th percentile to the 47th percentile in a typical violent period. The effect does not appear to be driven by bias from student migration, and evidence from differential effects estimates is more consistent with supply-side channels. Examining the causal pathways of the effect, the impact of local violence is shown to induce higher levels of teacher turnover, although this may actually improve the average qualifications of teachers in the school in the long run.

Keywords: *violence, urban crime, education outcomes, teacher turnover*

JEL Classification: I21, I25, D74, J28, O12, O15

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El efecto de la violencia urbana sobre el logro de estudiantes en Medellín, Colombia

Gregory L. Haugan*

Resumen

Este artículo estudia el impacto de la violencia urbana sobre los puntajes en una prueba estandarizada para estudiantes de colegios públicos en Medellín. Se usa la variación en la exposición a la violencia a nivel de colegio para los años 2004-2013 en un modelo con efectos fijos de año y colegio, lo que permite controlar por la endogeneidad de la violencia. Los resultados muestran que un incremento en un homicidio por año que ocurre dentro de 500 metros a un colegio reduce el logro de los estudiantes en un poco más de 0.01 desviaciones estándares. No encuentro evidencia que el efecto se debe al sesgo por la migración de estudiantes, y la evidencia es más consistente con los canales de oferta. Los resultados también muestran que la violencia aumenta la deserción de profesores de los colegios, aunque esto podría resultar en un grupo de profesores más calificados a largo plazo.

Palabras claves: *violencia, crimen urbano, educación, deserción de profesores*

JEL: I21, I25, D74, J28, O12, O15

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1. Introduction

An unfortunate reality of many recent and ongoing armed-conflicts across the globe is that noncombatants are not only accidental victims, but are often actively targeted. In cities located in the Western Hemisphere, this victimization frequently stems from mafias and gangs engaged in illegal drug trafficking. For a variety of reasons and to differing levels of intensity, urban communities have been or continue to be targeted by armed gangs in countries such as Colombia, Mexico, Honduras, and the United States, to mention only a few. In particular, students, teachers, and educational institutions themselves have been specifically targeted by gangs or armed groups in these countries. The manifestations of this violence are diverse. In some contexts, students and teachers may be extorted on their daily commute, while in other cases students are targeted for recruitment by criminal groups, teachers are targeted for their roles as community leaders, and schools are closed due to internal violence between gangs (O'Malley, 2014). The consequences can be disastrous for the human capital accumulation of those affected.

The Colombian city of Medellin presents an interesting case study for identifying these effects. While overall levels of violence in the city have seen a dramatic drop from peak levels in the 1990s, Medellin continues to see sporadic spikes in violence, often linked to arguably exogenous factors that create power vacuums in the city's illegal economy, generating the potential for armed conflict between rival groups. Within this context, gangs, armed groups, and organized criminal mafias may target students for a variety of reasons: refusal to participate in a group's activities, carrying messages or arms for a rival group, disobeying the orders of a local group, or directing sexual violence at female students. Additionally, students' paths to school may put them at risk as they are forced to cross invisible gang boundaries (Personeria de Medellín, 2011). Even further, gang activities may have indirect effects on students, as school quality drops due to teacher turnover in conflict areas, students are traumatized by violence in their communities, or as violence and criminal activities change their perceptions regarding the returns to education.

Understanding these effects on student outcomes is important, particularly as Colombia transitions to a post-conflict scenario. As the country negotiates a peace agreement and the demobilization of the FARC, Colombia's oldest and largest guerrilla group, commentators have warned of the potential for former combatants to swell the ranks of criminal gangs.

A recent, growing body of literature examines the effects of internal conflict and organized violence on human capital accumulation. Using outcome measures such as exam results, dropout decisions, and labor market outcomes, researchers have found evidence for significant, negative effects of violence on human capital accumulation, using data from conflicts in Peru (León, 2012), Colombia (Rodriguez and Sanchez, 2012), Tajikistan (Shemyakina, 2011), and Uganda (Blattman and Annan, 2010), among others. Importantly, however, these studies mostly focus on rural conflict and use aggregate data at the regional level, such as total homicides by year and municipality.

In this respect, this paper contributes to the literature on the effect of violence on education outcomes in three important ways. First, I create a new dataset with the exposure to homicides for public schools in Medellin for 2004-2013. By focusing on school-level measures of violence, my data provides a more micro indicator of violence than is typically used in other studies. Secondly, I focus on urban violence, which has been studied less frequently, but which is potentially more important for countries in the Western hemisphere. Finally, I explore the possibility that local violence increases teacher turnover within affected schools, a previously ignored channel in the literature on the impact of violence on education. Using time and school fixed effects that control for the endogeneity of violence, the results show that each additional homicide occurring within 500 meters of a school reduces student performance by slightly over 0.01 standard deviations for a number of academic subjects on a standardized exam. While it is impossible to rule out the possibility that homicide exposure affects academic achievement via psychological impacts on students, overall the results are more consistent with supply-side channels.

The remainder of this paper is organized as follows: In the next section, I review the related literature. I then describe my data and econometric strategy in section three, followed by a discussion of my results in section four. Section five explores the heterogeneous nature of the effects, and section six explores the potential causal pathways of the effect, focusing in particular on the effect of violence on teacher turnover. Section seven presents the paper's conclusions and discusses the implications of the findings.

2. Related Literature

Although recent in its development, the literature studying the effects of violence and armed conflict on human capital accumulation has grown rapidly and been consistent in its findings of negative consequences for the impact of violence on measures such as labor market outcomes, years of schooling, and test scores. The results hold across developed and underdeveloped countries in a variety of violent scenarios, including traditional war (WWII Germany), lower-intensity guerrilla conflicts (Peru and Colombia), urban gang rivalries (Brazil), and genocide (Rwanda). While the studies are consistent in finding a negative impact for violence on human capital accumulation, two significant debates within the literature emerge: (1) whether these effects merely exist in the short term, or if they persist in the long term, and (2) which channels are significant for explaining the effects of violence on human capital outcomes.

2.1. The effect of armed conflict on education

By and large, the literature dealing with the relationship between violence and the accumulation of human capital is empirical in nature. An exception is Barrera and Ibañez (2004), who provide an intertemporal model for how the impact of violence on an individual's utility, family income, and the returns to education may work to affect investment in education. Taking Colombia as an empirical setting, the authors attempt to test the implications of the model by regressing the probability of school enrollment on municipality-level homicide rates, finding a negative correlation between the two. Yet because the focus of their work is mostly theoretical, they do little to control for the endogeneity of violence beyond including several control variables. Importantly, they do not test the relevance of the channels proposed in their theoretical model for explaining the results.

Rodriguéz and Sánchez (2010, 2012) and Vanegas (2014) have conducted more empirically rigorous work for Colombia. Given that all three studies use an instrumental variables approach, the work of Vanegas is perhaps the most convincing. Using rainfall as an instrument, the study finds that the instrumented effect of municipality-level violence during a mother's pregnancy impacts an individual's cognitive development via stress hormones released by the mother, increasing the probability of school dropout many years later. Under the assumption that rainfall occurring before an individual's birth is unrelated to school

dropout decisions via any other channel, Vanegas provides convincing evidence for the effect of prenatal exposure to violence. In two papers, Rodriguéz and Sánchez show that the intensity of armed conflict in Colombian municipalities reduces school enrollment and standardized test scores, using natural disasters, estimated local cocaine revenues, and lagged homicide capture rates as instruments for violence. Especially given the focus on the more contemporaneous nature of the effect of violence, the exogeneity assumptions required are much stronger, although the authors go further in exploring the causal pathways of the effect. Using growth in municipal tax revenues, homicides of 15-29 year olds, and standardized test scores in the municipality as proxies, they find a statistically significant relationship between violence and all three variables, suggesting that violence negatively impacts family budget constraints, the returns to education via reduced life expectancy, and reduces the quality of education. In the paper from 2010, the authors examine several supply-side channels through which violence might impact test scores, including student-teacher ratios and the qualifications of area teachers, but find no evidence that the effect functions through these mechanisms.

Other studies have produced similar results. For Guatemala and Rwanda, respectively, Chamarbagwala and Morán (2010) and Akresh and Walque (2011) use cross-sectional data and a differences-in-differences strategy to compare the educational outcomes of individuals exposed to varying intensities of conflict, depending on their year and place of birth. Chamarbagwala and Morán find that at-risk populations with the most exposure to the Guatemalan conflict completed approximately one year less of education than those with no exposure to conflict during their school years. This compares with the finding that young children exposed to violence during the Rwandan genocide completed 0.5 fewer years of schooling and were 15 percentage points less likely to complete fourth grade. However, for both studies the authors are unable to empirically identify the causal channels of the effect.

Using a similar empirical strategy for Bosnia and Herzegovina, Swee (2009) finds that an increase of one standard deviation in the war casualty rate reduces the probability of completing secondary schooling by 4 percentage points, with stronger effects for males. Investigating the causal pathways, Swee uses the percentage of damaged housing structures and regional displacement as proxies for damaged school buildings and displaced teachers, respectively, finding no statistical evidence that the effect functions through these supply-side channels. Shemyakina (2011) uses two cross-sectional surveys of households in Tajikistan and

different measures of a household's exposure to the Tajik conflict, and finds that school-aged girls from households whose dwellings were damaged in the conflict were 11 percent less likely to be enrolled in school. Conditional on a household reporting structural damage to the home as a result of the conflict, children of widowed mothers were 22.9 percent less likely to be enrolled in school, evidence that losing a parent in the conflict is one of the effect's causal channels.

For Peru, León (2012) examines whether the effects of violence on education persist in the long term, finding that exposure to violence may increase school dropouts in the short term, but finds evidence that individuals exposed at an older age catch up to their peers in the long term, suggesting these individuals may return to school later on. On the other hand, individuals exposed to violence at a very early age experience effects that persist long into adulthood, which points to the effect of violence on cognitive development as a likely pathway.

With the exception of Shemyakina, each of the aforementioned authors uses regional variation in conflict intensity. Due to limited data on teachers, students, and schools, all of these studies are limited in the extent to which they can explore the causal pathways of the effect of conflict on education. Perhaps the best attempt at identifying these causal channels comes from Monteiro and Rocha (2013), using data from schools located in the slums of Rio de Janeiro. Under the assumption that the events that trigger gang conflicts are exogenous to time-varying neighborhood conditions, the study exploits the variation in gang conflicts occurring near schools, finding that exposure to gang violence reduced test scores in math by 0.054 standard deviations. Further, with detailed information on teachers and principals, the authors are able to explore the relative importance of supply-side and demand-side channels, finding that an increase in violence increases absences for teachers, but not for students, and increases the probability that schools temporarily suspend classes.

2.2. State of the literature and opportunities for further investigation

As shown in Table 1, just over half (8) of these studies use measures of attacks or deaths at the regional level as their primary indicator of violence, while 80% (12) of the studies use years of schooling or dropout decisions as their measure of human capital accumulation. The focus on these variables is important. The use of regional-level violence data demonstrates the common reliance on aggregated violence measures, creating data-weighted averages of individuals within the same region, but with different levels of exposure, which may

increasingly undermine the identification of the true effect of violence on the individual as the level of aggregation increases. At the same time, focusing on the impact of violence on the quantity of education is important when viewed in the context of the debate on the relative importance of quantity versus quality in education (Hanushek, 2005). Indeed, low school quality has been found to be a primary determinant of dropout decisions (Hanushek, Lavy, and Hitomi, 2008), suggesting that the impact of regional violence on school quality could be a primary driver of the reduced enrollment rates in violent areas.

Dealing with these issues provides an opportunity for contributing to a literature that has already seen a great deal of growth. Of the five studies I find that use more micro-level violence indicators, two examine the effects of violence on combatants themselves (Angrist, Chen, and Song, 2011; Blattman and Annan, 2010), which does little to answer the question of how violence impacts the human capital accumulation of non-combatants. A third paper (Grogger, 1997) uses school-level data to measure the impact of in-school violence in the United States on high school graduation and university enrollment, although the measures of violence come from subjective responses from school principals characterizing violence in their schools as "serious", "moderate", or "non-existent", and is unable to separate the effects of in-school violence from its likely correlation with neighborhood violence. There is therefore a notable lack of studies in the literature using non-subjective microdata as a measure of violence.

Furthermore, additional work is needed to disentangle factors constraining individual investment in education from other factors more related to the quality of education. Schools in conflict zones may experience a variety of problems that constrain individual investment in education, even when demand for education investment remains constant. The destruction or closing of schools (Swee; Shemyakina) and risk of leaving home to attend school in a conflict zone (Shemyakina; Chamarbagwala and Morán; Monteiro and Rocha) have been mentioned as possible constraints to individual investment in education. Still, assuming that reduced demand for education is a principal driver of dropout rates in violent areas, an exogenous increase in enrollment - such as one provided by a conditional cash transfer from the government based on the enrollment status of a household's children, perhaps - will do little to improve human capital accumulation if the quality of schooling is extremely poor. However, relatively few studies thus far have examined the effect of violence on the quality of education.

Finally, although Table 1 shows that several papers have examined the potential for violence to affect education via teacher supply channels, the results are far from conclusive. Swee uses overall region out-migration as an imprecise proxy for teacher displacement and finds no statistically significant evidence to support this as a causal pathway. León (2013) uses the death of a teacher in the region as a result of the conflict and finds that this may be correlated with delayed school entry for children, though the results may suffer from potential bias if teacher deaths are correlated with regional teachers working with guerrilla groups. Rodríguez and Sánchez (2010) find that regional violence is correlated with a lower-qualified body of teachers in the region, but are unable to conclude that teacher qualifications constitute a channel through which violence impacts test scores. Importantly, there is a lack of work examining the potential for conflict to increase teacher turnover, which could affect quality of education via channels related to the disruption of institutional processes within schools, as suggested by Ronfeldt, Loeb, and Wyckoff (2013).

3. Data and Empirical Strategy

The data used in this paper comes from two principal sources: the Colombian Institute for the Promotion of Higher Education, ICFES for its Spanish acronym, and a database on criminal activity in Colombian cities from Colombia's National Police, including GPS coordinates for these activities provided by the Center for Drug and Security Studies (CESED) at the Universidad de Los Andes in Bogotá. I use ICFES data for the period 2006-2013 and the National Police data for 2004-2013, focusing solely on data available for Medellin.

The ICFES is responsible for administering standardized tests in Colombian schools. In particular, all Colombian students must sit the Saber 11 standardized test at the end of 11th grade as a requirement to graduate from high school.[1] The exam tests students' knowledge on a range of subjects, including languages (Spanish), mathematics, and sciences. In addition to providing scores for each student on the core subjects tested, the Saber 11 asks students for personal and socioeconomic information, including the student's gender, date of birth, the level of education completed by the student's parents, family income, ethnic minority status, household internet and computer access, and the strata of the student's dwelling[2], among other information. Unfortunately, data for most of these socioeconomic indicators is not available

[1] 11th grade is the final year of high school in Colombia.
[2] Buildings in Colombia are assigned a strata level on a scale of 1-6. Utilities are billed at different rates depending on the strata assigned.

for 2006 and 2007. The information in the database also includes basic information on the school the student attended, including the school's name, unique identification code, and the shift the student attended[3]. The final database for 11th grade public school students in Medellin who took the Saber 11 between 2006 and 2013 includes 142,506 students from 245 schools. Collapsing the individual data to the school-year level, the database contains 1,731 observations of school-year cohorts.

In order to measure the distance from each school to the location of each of the homicides reported in the National Police data, it was also necessary to obtain GPS coordinates for Medellin's public schools. I obtained the addresses for each school from a public school directory available from the Secretary of Education of Medellin, allowing me to map the GPS coordinates for each school and merge the information with the ICFES database using the school's unique identification code. Using ArcGIS software to map the data on school and homicide locations, I counted the number of homicides occurring within a distance of 500, 1000, 1500, and 2000 meters of each school for each year, giving me a yearly school-level measure of exposure to violence.

3.1. Descriptive Statistics

Table 2 presents the descriptive statistics for Medellin's public schools during the 2006-2013 period. The table is divided into four panels according to the general categories of the different variables. Descriptive statistics are presented for the pooled sample, schools in violent areas, and schools in non-violent areas, as well as the results for the relevant difference in means tests for determining the statistical equality of the traits of schools in both areas. To classify schools as located in either high-violence or low-violence areas, I take the mean number of homicides within 500 meters for all schools during each year, and classify a "violent year" for a school if the number of homicides it is exposed to is above the citywide mean for that year. Finally, a school is classified as being located in a high-violence area in the descriptive statistics table if it appears as a violent year for at least half the years it appears in the dataset. Formally:

[3] Particularly in Colombian public schools, schedules are sometimes divided into shifts to allow more students to attend, given limited physical space. Students may be assigned to a morning or afternoon shift, although some public schools do not use this system and are able to provide full-time education. Evening shifts also exist, usually attended by adult learners who didn't complete high school during their teenage years.

$$Violent\ Year_{s,t} = \begin{cases} 1\ if\ \sum h_{s,t} > \overline{H_t} \\ 0\ if\ \sum h_{s,t} \leq \overline{H_t} \end{cases} \quad \textbf{(1)}$$

and

$$High\ Violence\ Area_s = \begin{cases} 1\ if\ \frac{\sum_{t=1}^{T} Violent\ Year_{s,t}}{T_s} \geq \frac{1}{2} \\ 0\ if\ \frac{\sum_{t=1}^{T} Violent\ Year_{s,t}}{T_s} < \frac{1}{2} \end{cases} \quad \textbf{(2)}$$

Where $\sum h_{s,t}$ is the total number of homicides occurring within 500 meters of school *s,* in year *t*, and $\overline{H_t}$ is the citywide average number of homicides occurring within 500 meters of schools during year *t*.

The main takeaway from Table 2 is that although a large number of variables are statistically different between schools in high- and low-violence areas, the differences are surprisingly small. While some of the results shown are to be expected – Panel A shows that not only are the neighborhoods within 500 meters of violent schools significantly more violent than non-violent schools, but the wider surrounding areas up to 2000 meters from the school are significantly more violent, as well – other results appear counterintuitive.

This is best demonstrated in Panel C. We would expect schools in violent areas to be poorer, and show higher levels of vulnerability in terms of their socioeconomic characteristics – a higher percentage of students from ethnic minorities, with parents who failed to graduate from high school, who work, and living in poorer neighborhoods. Yet this is not necessarily a conclusion that can be drawn from the results in this panel. There is only limited evidence that students in violent areas are more likely to have parents who were high school dropouts, and there is no evidence for statistical differences in ethnic minority status or the likelihood that these students are working. Contrary to what we would expect, schools in violent areas actually show a lower percentage of students living in poorer, Strata 1 neighborhoods. No general conclusion can be drawn as to whether the overall composition of students in violent areas is richer or poorer than the composition of students attending school in less-violent areas.

Far from allowing us to conclude that the distribution of violence is random, the relative statistical similarity between schools in violent and non-violent areas is driven by two systematic patterns in the data. Firstly, a large number of both schools and homicides are located in the city center. While the students living and studying in this area may be exposed to

a disproportionate amount of urban crime or violence, they do not necessarily come from such vulnerable or poor backgrounds as students in other high-violence areas, such as the slums on the urban periphery. This would likely pull average observable characteristics away from the high-levels of vulnerability that we would expect to see in high-violence areas.

Secondly, and perhaps most importantly, the results shown in Table 2 reflect a missing data problem. While the homicide data provided by the National Police includes street addresses for each event, the GPS coordinate matching software was unable to provide coordinates for all addresses, either because the address was incomplete or because the coordinate matching software failed to recognize the address (Mejía, Ortega, and Ortiz, 2015). It is likely that the inability to match coordinates with an address is correlated with the complicated addresses of streets located in the hilly slums on the urban periphery. If this is the case, a proportion of the homicides occurring near schools located in these areas would go systematically uncounted, perhaps leading these schools to be classified as located in low-violence areas.

Table 3 shows how this might work, aggregating the data at the *comuna* level and comparing against *comuna*-level data obtained from a municipal government report (Municipio de Medellin, 2013).[4] Panel A shows that for most *comunas* there is no systematic over- or under-classification of violence levels, misclassifying the *comuna*-year as either "high-" or "low-violence" compared to the annual mean. However, *Comunas* 7, 8, and 13 have under-classified violence levels for several periods. In fact, this leads to these neighborhoods being misclassified as low-violence according to equation (2), when in fact they should be classified as high-violence. *Comunas* 8 and 13 have high incidences of poverty, and their misclassification could be affecting the results presented in Table 2. As such, it is important to note that the probability of successfully georeferencing homicides does not change significantly from year to year. This is shown in Table 3, Panel B. The percentage of homicides successfully matched by the GPS software hovers between a narrow range of 69 and 75 percent over the period.

Thus, the correlation between missing homicides and factors that negatively affect education, such as poverty and lack of infrastructure, is positive, while the expected correlation between missing homicides and students' exam scores is negative. This implies that the model described in the next subsection may underestimate the absolute value of the negative effect of local violence on exam scores, resulting in a conservative estimate of the true effect.

[4] A *comuna* is roughly comparable to the idea of a borough in New York City. There are 16 *comunas* in urban Medellin.

3.2. Identification

As the discussion in the previous subsection suggests, traditional OLS methods regressing exam results on school-level exposure to violence are likely to be biased. To overcome the dual problem of systematic differences between violent and non-violent schools and the correlation of missing homicide data with school characteristics, I propose a model that includes year and school fixed effects. While unobserved variables influencing neighborhood conditions might influence both local student outcomes and neighborhood violence, confounding the identification of the effect of violence on education between neighborhoods, I argue that variations in local violence within neighborhoods from one year to the next are exogenous. Similarly, while the GIS software might be less likely to identify coordinates for homicides in peripheral urban areas, the probability of identifying coordinates for homicides occurring in the same neighborhood should not vary from year to year. To identify the effect, I estimate the following:

$$\mathrm{R}_{st}^{a} = \beta * V_{st} + \partial * C_{st} + \gamma * X_{st} + S_s + T_t + \mu_{st} \qquad \mathbf{(3)}$$

Where R_{st}^{a} is the average result on the Saber 11 in academic area *a,* for students in school *s*, in year *t*, expressed in standard deviations. I estimate the effects on students' results on the math, language, biology, physics, chemistry, social sciences, and philosophy portions of the Saber 11, as well as on the rank for the total exam score, where 1 is the highest and 1000 is the lowest. The parameter of interest, β, is the effect of V_{st}, the accumulated exposure to violence in school *s* during the year of the exam. V_{st} is defined as:

$$V_{st} = \sum \mathbb{I}\,\{D_{hst} < M\} \qquad \mathbf{(4)}$$

Where $\mathbb{I}\,\{D_{hst} < M\}$ is a function that takes the value of one if the distance between school *s* and homicide *h* occurring in year *t* is less than *M* meters. V_{st} is therefore the sum of homicides occurring with *M* meters of school *s* in year *t*. I initially set *M* equal to 500 meters, and then run the regression for larger values of *M* to test whether the effect diminishes as the distance to homicide events increases.

C_{st} is a vector that includes time-varying school characteristics. It includes the total number of 11th grade students in year *t* and the percentage of male students. Because the exam asks students for personal information, I can also control for the average composition of student

characteristics in a school by including X_{st}. This vector includes the average student age, percentage of students belonging to an ethnic minority group, percentage of students who work, percentage of students whose parents never graduated from high school, and the percentage of students in different family income and household strata categories. This allows me to control for changes in the average composition of these observable variables within schools from year to year.

The model also includes S_s and T_t, school and year fixed effects, respectively. The inclusion of T_t allows me to control for time-varying aspects common to all public schools in the city, such as variations in education policies or the municipal budget. By including S_s, I am able to control for fixed school characteristics, such as infrastructure or management traits. This also controls for fixed characteristics of the neighborhood around the school, and controls for the fixed characteristics of students' communities under the assumption that students attend schools located near their homes. Therefore, rather than comparing the cross-sectional variation of violence between schools, which is almost certainly endogenous, the inclusion of the fixed effect terms in the model controls for this endogeneity and allows the parameter of interest to capture the effect of exogenous variations in violence within schools over time.

Still, to consider μ_{st} as an idiosyncratic error requires strict assumptions about the variation in violence within neighborhoods: (A1) that variations in violence are not due to variations in the quality of schools; (A2) that there are no systematic differences between student cohorts from one year to the next that are themselves driving variations in violence; (A3) that variations in in neighborhood violence levels are not driven by confounding variables that also influence educational outcomes, such as changes in the neighborhood unemployment rate; and (A4) that students do not systematically drop out or change schools due to variations in local violence.

A detailed explanation of why assumptions A1-A3 should hold for the general case of a typical city affected by gang violence can be found in Monteiro and Rocha, including references to previous studies on the subject. For the specific case of Medellin in the period under study, a number of reports suggest that exogenous factors, such as paramilitary demobilizations, extraditions of gang and paramilitary leaders, and truces between gangs, have been primary factors driving variations in urban violence (Giraldo Ramirez, 2008; Medina,

Posso, and Tomayo, 2011). Graph 1 shows the homicide rate per 100,000 residents for Medellin during the period 2002-2013, along with the rates for other major Colombian cities and for Colombia as a whole. As the graphic shows, Medellin saw a much greater degree of variation in homicide rates over this period than other parts of the country. The extremely high homicide rates in Medellin up to 2002 were largely due to confrontations between urban militias of illegal armed actors in Colombia's conflict, with the large drop-off starting in 2002 occurring after government efforts combating these groups, and the low points beginning in 2005 corresponding to the demobilization of local paramilitary groups (Arango, Prado, and Dyner, 2009). Observers such as peace think tank Ideas Para la Paz also suggest that the uptick in 2008 corresponds to the extradition of Don Berna, a local crime boss and head of the Oficina de Envigado mafia, which fractured after the extradition, leading to violent confrontations between different mafia factions, while an outside criminal organization known as "Los Urabeños" attempted to take advantage to exert their own presence in the city. These conflicts were eventually resolved as certain gangs consolidated territorial control and a number of local gangs began making truces (Llorente et al., 2014; McDermott, 2014).

These trends are also shown in Graph 2, which breaks down the tendencies in average annual homicides occurring within a 500-meter radius of schools by geographic region of the city. The Central, Western, and Southwestern geographic regions show trends that are roughly parallel over the period, trending upward beginning in 2008 and peaking in 2011. The Northeastern and Northwestern zones have much more dramatic increases and earlier peaks, perhaps reflecting the lucrativeness of these territories to gangs and mafias owing to their easier access to drug trafficking routes to the Caribbean coast. The wealthier area in the south of the city sees little variation over the period. These trends can also be seen spatially in Maps 1-3.

I provide specific evidence supporting the assumption that these shocks are exogenous to school context by regressing the average number of homicides occurring within 500 meters of each school over periods *t* and *t-1* on lagged observable school characteristics. If the effect of the lagged observable variables is statistically different from zero, assumptions A1-A4 would likely be unreasonable.

The results are shown in Table 4, with observable characteristics lagged one period in Column 1 and lagged two periods in Column 2. The lagged observable characteristics do not

appear significant for explaining average violence levels. The obvious exception is that we see a negative correlation between the number of students from relatively wealthier Strata 4 neighborhoods, and a positive correlation between the number of Strata 2 students and violence. In absolute terms these differences are quite small, but they may suggest that wealthier students could be leaving these schools in high-violence years, perhaps in favor of private schools in safer areas, given their increased ability to pay.

Taken together, the timing and degree of the variation in violence, the "on-off-on-off" nature of the effect, and the results shown in Table 4 are at least suggestive of exogenous shocks, supporting assumptions A1-A3. To deal with the possibility that these assumptions fail to hold, I also construct a Bartik Shock instrument as a robustness check. Due to a weak first stage, the description of this approach and the results are not presented here, but are available upon request.

The violation of A4 is more problematic, and Table 4 provides some limited evidence suggesting that this assumption may in fact fail to hold. If more highly motivated students with more supportive families systematically leave schools with increased exposure to violence in favor of schools in safer areas, then $\text{Cov}(V_{st}, \mu_{sct}) < 0$ in equation (3) and the estimated effect overestimates the absolute value of the negative impact of violence. On the other hand, if less-motivated or less-able students are more apt to drop out of school entirely due to violence, as has been suggested by numerous authors mentioned in Section II of this paper, then we expect $\text{Cov}(V_{st}, \mu_{sct}) > 0$ because students at the lower end of the standardized test score distribution leave school and violence results in an overall more highly capable group of students taking the exam. The overall direction of the bias that results if A4 is violated is therefore difficult to predict *a priori*. I discuss this problem in greater detail in Section IV.

4. Results

Panel A of Table 5 presents the results for the regression of equation (3), with coefficient sizes expressed in terms of standard deviations. To produce these results, I set the distance threshold at 500 meters, counting only homicides occurring within this radius for each school. As shown, the estimates for the effects of exam-year and lagged violence on test scores in Panel A are consistently negative across all academic subjects. The significance is a bit more robust for lagged violence than for exam-year violence. The overall size of the coefficients

changes little when controls are added, supporting the claim that the variation in violence within neighborhoods over time is exogenous to the time-varying aspects of school context. School controls include the total number of students in the cohort and the percentage of all the cohort's students who are male; socioeconomic controls include the percentage of students who also work, the percentage of students from ethnic minority groups, whose mothers never finished high school, and the percentage of students from different income brackets and neighborhood strata classifications.

While the estimates are consistently negative, the overall size of the effect appears quite small. Estimates do not change significantly in regressions that include only either exam-year or lagged violence, nor are important differences in levels of significance seen when clusters are set at the *comuna* level, allowing for correlation in errors between schools within the same area of the city (results not shown).

These results suggest that exam-year and lagged violence are comparable in the impact they have on education. This is important in the consideration of the channels through which the effect functions, and suggests that the effects seen here are persistent, as opposed to the short-term cognitive effects of violence observed by Sharkey (2010). Thus, I also estimate equation (3) using average exposure to homicides during t and $t-1$ and present these results in Panel B. The results are consistently negative and significant, and the size of the effect varies little across academic subjects. Throughout the remainder of the paper I work with this two-year average indicator of school exposure to violence.

Overall, I estimate that each additional homicide per year decreases results on various areas of the ICFES standardized exam by slightly over 0.01 standard deviations. While the effect is small, it is important to note that in high exposure years the schools in my sample saw an average increase of 6.1 homicides occurring within a 500-meter radius. For an average school, in the 50th percentile of the score distribution and with average homicide exposure, this would imply that scores fall by about 0.07 standard deviations in a typical violent period, dropping to the 47th percentile.

These findings are comparable to those found by other authors. For example, Monteiro and Rocha find that schools' test scores in Rio de Janiero fell by .054 standard deviations in violent periods for the math portion of a standardized exam, although they find no effect for language scores. To put the extent of this effect in context, in a randomized controlled trial

Angrist *et al.* (2002) find that winners of a lottery for private school vouchers in Colombia scored 0.2 standard deviations higher on achievement tests three years after the lottery.

As mentioned however, my results could be driven by student migration problems. If this is the case, violence should not only change the mean of the distribution, but also its shape. Taking a non-parametric approach to this problem, I divide the students in each school between those exposed to violence that is above the school's mean and those exposed to violence below the school mean and plot the graphs for the effect on the distribution. Graph 3 shows the effect on the distribution of math scores, representative of the graphs for other test areas. Although the distributions appear to shift down slightly with higher exposure to violence, the shape does not change in any way that is obvious upon visual inspection. This is confirmed by taking a parametric approach and estimating the effect of violence on higher moments of the ICFES score distribution, including kurtosis and skewness as dependent variables.[5] Together, the results indicate that the effect of homicides on exam scores functions by shifting the entire distribution downwards, and support the claim that student migration is not driving the estimated negative effect of homicides on average exam scores.

I also conduct checks to test the robustness of results to two different specifications of equation (3). First, I change in the definition of exposure to violence by increasing the distance threshold from schools. Second, I conduct placebo tests by using lead indicators of violence in the place of the averaged exam-year and lagged violence.

If the results in Table 5 are indeed driven by local exposure to violence, the effect should diminish as the distance threshold set for counting homicides increases. To test this hypothesis, I run the regressions of equation (3) again for distances of 1000, 1500, and 2000 meters. Table 6 shows that for every academic subject, the absolute value of the effect of violence on test scores is lower than the estimates in Table 5, and the effect diminishes as distance increases, although the effect remains statistically significant even when the threshold is set at 2000 meters. Even though results are only shown for selected subjects, this pattern is consistent across all academic areas.

Clearly, if these neighborhood-level shocks are driving results, we should not find significant estimates when substituting the average of exam-year and lagged-year violence with lead indicators of violence for the two years after the exam. Resetting the distance threshold at

[5] Results available upon request.

500 meters, I return to equation (3), this time regressing exam results in year *t* on average violence in years *t+1* and *t+2*. Table 7 shows the results. Lead homicide indicators do not appear significantly different from zero for any other subjects, suggesting that it is not only neighborhood-level shocks driving the results, but that the specific timing of these shocks is important. To some extent, this also provides evidence supporting assumption A1, referring to reverse-causality problems. If lower school quality in period *t* causes violence, the effect should also be seen in period *t+1*. The results in Table 7 suggest this is not the case.

5. Heterogeneous Effects

From a public policy perspective, there may be relatively little that local officials can do to respond to these findings by substantially reducing neighborhood violence. This is particularly true to the extent that homicide trends are driven by more powerful economic and political forces at the national and international levels, which may include extraditions and narcotics legislation. In this sense, identifying the causal pathways of the effect of violence on education outcomes and the way the effect differs according to subsets of the population becomes particularly important, as this may lead to more practical policy recommendations.

I explore the possibilities for heterogeneous effects of violence on test scores, depending on student characteristics. Specifically, I return to the fixed effects model used in equation (3), this time running the regression at the student level, and explore whether a student's sex, parents' educational background, and family income show evidence of heterogeneous effects. Importantly, these results will provide further evidence that student migration problems are not likely to be the driving factor behind the results in Table 5.

5.1. Male and Female Students

Violence may affect male and female students differently. Although female students are more likely to face deplorable levels of harassment and sexual violence in violent and gang-controlled neighborhoods (Personeria de Medellin, 2011), there is no obvious reason why violence against female students would increase within a neighborhood in high-homicide years. However, male students are more likely to be targeted for gang recruitment, forced to carry messages or arms for local gangs, and face physical violence when crossing invisible gang boundaries, all of which would be expected to increase in high-homicide years with increased inter-gang conflict. To the extent that homicide victims are disproportionately young men,

male students could also change their perceptions regarding the returns to education, as life expectancy is reduced. While we would expect this to reduce the performance of male students, the effect might also work to increase dropout rates among low-performing students, perversely raising average scores (Rodriguez and Sanchez; Barrera and Ibañez; Monteiro and Rocha). The net effect is *a priori* ambiguous.

The results for the differential effect of violence on students by sex are presented in Panel A of Table 8. Although the differential effect for male students is significant for some subjects, the effect does not go in a single, obvious direction. The universal effect for homicides loses some of its significance compared to previous regressions, but is consistently negative. This suggests that while a portion of the effect may owe to the demand-side factors previously discussed, another portion likely comes from supply-side factors related to a school's ability to provide quality education in high-violence periods, affecting male and female students equally. Such channels might include school closings or the supply of teachers.

5.2. Vulnerable Populations

When episodes of local violence break out, students from low-income households and those with less-educated parents may be particularly vulnerable and disproportionately bear the weight of the effects. Following a similar logic to that described for the differential effects of violence on male students, the direction of the estimated effect on these vulnerable populations is difficult to predict *a priori* owing to potential sample selection problems stemming from student dropout. I present the results for the differential effect of violence on students whose mothers never completed high school and differential effects by household socioeconomic strata in Panels B and C, respectively.

The estimates for the differential effect of violence depending on mother's education show consistently positive coefficients for the interaction of homicides and the student's mother never having completed high school, significant for language, physics, and chemistry. While I am unable to formally test the hypothesis, when combined with the existing evidence on the relationship between violence and student dropout decisions, these results strongly suggest that by inducing dropout decisions amongst less-motivated, low-performing students, violence perversely raises average test scores amongst some segments of the population. This would generate an upward bias in the estimates, implying the results shown might be conservative estimates of the true effect.

Finally, I estimate the model including heterogeneous effects that account for the interaction between violence and the socioeconomic strata of the student's home, classified by the government for the purpose of utilities pricing. *A priori,* the expected direction of the effect is ambiguous for the reasons previously argued for males and students whose parents never finished high school. At the same time, sample selection problems could also bias estimates downward if students from more supportive families systematically migrate from violent to less violent schools in high-violence years. To the extent that this is correlated with families' ability to pay for the costs of migrating to a new school, either because the new school is private or because additional transportation and time costs are imposed, we would expect to see a greater negative effect for students from households classified at higher socioeconomic levels. Another possibility is that students from high-strata homes attending schools disproportionately affected by violence reduce their attendance in high-violence years, lowering exam scores.

Setting the baseline at the mid-socioeconomic level Strata 3 and including heterogeneous effects for lower socioeconomic level Strata 1-2 households and higher socioeconomic level Strata 4-6 households, the results are shown in Panel C of Table 8. The differential effect for the more vulnerable populations from low-strata households is consistently positive and shows some significance. Again, this may be due to higher dropout rates among the least motivated students from low-strata households, or because the best students from middle- and high-strata households migrate to safer schools. Yet the results suggest that the latter is not likely to be the case; the direction of the estimate for the interaction of homicides with high-strata households is not consistent and does not provide convincing evidence of statistical significance. Taken as a whole, these results further support the claim that sample selection issues are not behind the finding of negative effects of violence on test scores. Again, we may be justified in interpreting the results as conservative estimates.

6. Causal Pathways – Local violence and teacher turnover

As described in section two of this paper and shown in Table 1, the existing literature on the relationship between violence and education outcomes has examined a number of psychological, behavioral, and institutional channels through which violence may impact the quality and quantity of education received. Existing literature on the psychological and cognitive impacts of violence has been conducted in natural experiment settings, and

convincingly shows that the effect is greatest when the violence occurs closer to cognitive and psychological evaluations, and that the effect may not be permanent (Sharkey, 2010; Moya, 2014). As I have shown, however, my results are not driven by violence occurring closer to the exam date, and the effect shows only limited convincing evidence of negative heterogeneous effects for students who might be most exposed to violence due to their socioeconomic characteristics. Although I cannot rule out the possibility that repeated exposure to violence has a psychological impact, the results are more suggestive of institutional channels, affecting students equally via supply-side factors.

The effect of violence on school closings and the composition of teachers would appear as the most obvious institutional channels. The data does not allow me to observe school closings. However, data from Annex 3a of Colombia's *Resolución 166* provides longitudinal data following all public school teachers in the country from 2008-2013, and can be used to observe teachers' movements between schools over the period.

As a final contribution to the literature on the effect of violence on education, I examine the possibility that exposure to violence induces higher rates of teacher turnover. Previous studies have examined the potential for violence to increase teacher absences (Monteiro and Rocha), lower the average qualifications of the body of teachers (Rodriguez and Sanchez, 2010), and delay school entry for children in rural areas due to the death of a community teacher (León, 2012). Although a number of studies discuss the impact of school characteristics on teacher turnover (Hanushek *et al.,* 2002; Falch and Strom, 2004; Ondrich *et al.,* 2008) and examine the potential for teacher turnover to interrupt institutional processes and reduce the quality of education (Ronfeldt *et al,* 2013), to my knowledge no previous study has examined the impact of local violence on teacher attrition.

My analysis is anchored in the one-period school choice problem described by Hanushek, Kain, and Rivkin (2002), to which I add neighborhood violence as an additional factor motivating school transition decisions:

$$\begin{gathered} max\, pv\left[u^i(X_s, Z_i, V_s)\right] \\ \text{given: } s \in \{S\}_i \qquad \textbf{(5)} \\ c_s = c(z_i, s*) \end{gathered}$$

Individual i chooses school s from the set of school options, $\{S\}_i$, in order to maximize the present value of expected utility, with school characteristics, X_s, individual characteristics, Z_i,

and violence in the school's local community, V_s, included in the utility function. c_s describes the cost of moving schools or leaving the teaching profession altogether, which depends upon individual traits. The teacher is assumed to update the problem at the end of each school year, at which point they recalculate the decision to remain in school *s** or to work elsewhere. No transition is made as long as $pv\left[u^i(X_{s*}, Z_i, V_{s*})\right] > pv\left[u^i(X_s, Z_i, V_s)\right] - c_s$. V_{s*} is assumed to enter negatively in the utility function, so an increase in violence raises the probability of turnover.

With Annex 3a of the *Resolución 166,* I use the data for the 14,023 teachers who worked in Medellin from 2008-2013 (62,438 total observations), and identify the years in which teachers changed schools or stopped teaching in public schools. Because Medellin public schools' academic calendar runs from January to December, it is likely that lagged violence would impact teachers' labor market decisions in the exam year. Thus, if teachers face high exposure to violence in *t-1* and make job transition decisions at the end of the period, the composition of a school's body of teachers will change in period *t*, the year a student takes the exam. As such, for teacher *i* working in school *s*, year *t-1* is identified as a turnover year if the teacher is not working in *s* in year *t*.

Table 9 presents the descriptive statistics for these teachers. Using observations of teachers in year *J,* defined as job transition years for turnover teachers and year 2012 for those who never made a school transition, teachers who made job transition decisions at some point during the period were exposed to more violence, were younger, and had less teaching experience in the public school system than those who remained in the same school for the entire 2008-2013 period. They were also significantly more likely to be secondary school teachers, teach classes in science, technology, or mathematics (STEM), and less likely to be female.

I use a discrete time representation of a proportional hazards survival model to estimate the impact of violence on the risk that a teacher working at school *s* in period *t-1* does not return to work in period *t*.[6] This model is more appropriate than either OLS or traditional binary dependent variable models for modeling teachers' job transition decisions because it

[6] Following Hanushek, Kain, and Rivkin, I also estimate a linear probability model and run the regression separately for teachers in different age groups, accounting for the fact that teachers in different stages of their lives and careers may evaluate the decision differently. The results are quite similar to those for the logistic survival model, and are thus not presented here, but are available upon request.

accounts for right censoring in the data, allows individuals to have time-varying independent variables, and explicitly accounts for an individual's exposure time (the number of years a teacher is "at-risk" for making a job transition decision). The hazards are estimated using the complementary log-log regression method described for discrete time hazards analysis by Jenkins (2005):

$$\theta_i(j) = P\big(J_i = j \mid J_i \geq j, V_{st,} X_{st}, Z_{it}\big) = 1 - \exp(-\exp[\theta_0(j) + \theta_0(s) + \theta_0(t) + \lambda V_{st} + \gamma X_{st} + \delta Z_{it}]) \qquad \textbf{(6)}$$

Thus, I estimate the hazards $\theta_i(j)$, or the probability that an individual leaves at the end of the *j*th year[7], given survival up until *j* and a set of school and individual characteristics. The model includes three baseline hazards terms: $\theta_0(j)$ is the baseline hazard for all teachers in their *j*th year, $\theta_0(s)$ is the baseline for teachers in school *s,* and $\theta_0(t)$ is the baseline for year *t*. For greater flexibility, I use non-parametric $\theta_0(j)$, making no assumptions or impositions on the functional form the baseline hazards take. The parameter of interest, λ, captures the effect of average violence over the previous two years, after controlling for school and individual teacher characteristics, X_{st} and Z_{it}. Teacher controls include teacher's level of completed studies, salary, age group, and sex; I use the same school controls as in previous regressions. In addition, I also include the school's average performance on the math and language portions of the ICFES exam, accounting for the fact that teachers' attrition decisions may be driven by poorly performing students, which is correlated with violence.

Panel A in Table 10 shows the results. Columns 1-3 show the results for all teachers, high school teachers, and primary and pre-school teachers, respectively. Average violence is significant across all teacher types, and the size of the coefficient varies little. The positive coefficient means that violence increases the hazards, or the probability that a teacher leaves after their *j*th year at the school, given they have continued working up until *j*.

However, these estimations fail to take into account that the data is also left truncated. Observations of all teachers begin in 2008, and though we can observe when a teacher began teaching in the public school system, we cannot observe when they began teaching at the current school for teachers that began teaching before 2008. This could induce survivor bias, because for older teachers we do not observe their colleagues who made transition decisions

[7] Note that *j* here refers to survival time, not *t* years (i.e. 2006, 2007).

before the period of study. Thus, I also estimate the model for a restricted sample, including only teachers who began teaching in 2008 or later, for whom I observe all transition decisions during their careers up until 2013. Results for the restricted sample of teachers are presented in columns 4-6 for all teachers, high school teachers, and non-high school teachers, respectively. The estimates for all teachers and high school teachers are marginally significant (for high school teachers $p<.06$), although not significant for primary and pre-school teachers.

To aid in interpreting the implications these findings have on the hazard of teacher turnover, Graph 4 plots the hazards rates. I take the mean two-year homicide exposure for each school over the entire period and define a year as "more violent" if the school's average two-year exposure for period *t* is above the mean. The effect is estimated only for high school teachers who began teaching in 2008 or later. As the graph shows, teacher turnover is approximately five percentage points higher in more violent years. In particular, the first two years of teaching appear particularly hazardous, with nearly 60% of second-year teachers not returning for a third year if their school faced a high-violence period over the previous two years, compared with approximately 52% of second-year teachers working in schools facing less-violent periods.

Violence may also change the composition of the faculty in terms of their average professional qualifications. Panel B of Table 10 shows that violence increases turnover for teachers with no post-secondary education ("*LowQuals*") to a lesser extent than it does for other teachers. The effect is significant for the overall body of teachers in the sample and for secondary teachers, although I fail to reject the null hypothesis of no differential effect when the sample is restricted to primary teachers (Columns 3 and 6). For highly qualified teachers, defined as those with a post-graduate education, the coefficient for the differential effect appears positive, suggesting that violence may have a larger effect on turnover decisions for these teachers, although the estimates are not significant.

However, despite violence increasing turnover for more-qualified teachers at a higher rate than for less-qualified teachers, it does not appear to result in a body of teachers that is less qualified on average, and may even increase average qualifications in a school. Table 11 shows the effect of violence on the percentage of teachers without a post-secondary education (Columns 1-2), with a four-year degree (Columns 3-4), and with a post-graduate degree (Columns 5-6). The results suggest that although teachers with a four-year post-secondary

degree may be more likely to leave after a high violence period, they are replaced by teachers with similar qualifications. For the least qualified teachers that leave after a high violence period, the results suggest they are replaced by teachers with a post-secondary education.

This is also important when viewed in the context of the Colombian conflict. Rodriguez and Sanchez (2010) suggest that the composition of area teachers may change due to conflict, with more violence resulting in a less-qualified body of teachers. This may be possible in rural areas, with more-qualified being more likely to move to a different municipality as a result of violence and their schools unable to replace them with similarly qualified teachers. However, my results suggest that in Medellin's context, with a larger pool of qualified educators, teachers that leave are easier to replace, although the higher turnover of teachers may still disrupt institutional processes.

7. Conclusions

This paper has presented evidence suggesting that extremely localized violence in the neighborhoods around Medellin's public schools reduces performance on standardized exams taken at the end of students' final year of high school. I estimate that each additional homicide per year occurring within a 500-meter radius of the school reduces student performance by approximately 0.01 standard deviations on a variety of tested academic subjects. Because results on the exam are an important determinant of the probability of university acceptance and the quality of university to which students have access, it is reasonable to assume the impact may have long-term consequences for the education and labor market outcomes of affected students.

The work provides a contribution to the literature in several respects. Unlike most previous studies, I have used non-subjective microdata to produce a school-level indicator of violence, allowing a more precise estimate of the true impact of violence. Though much of the existing related literature focuses on rural civil conflict, my focus on urban violence has fewer precedents, and may provide more relevant evidence for Western hemisphere countries, particularly looking forward for Colombia. Finally, I have shown that increased violence around a school is correlated with higher teacher turnover from one year to the next, an important but previously unexplored channel within the literature. These results imply that

teachers are approximately five percentage points less likely to return the following year when violence around the school is above its historical average.

My methods exploit the panel nature of the data to control for unobserved fixed effects of schools and time that are potentially correlated with education outcomes. Still, a number of assumptions are required to interpret these effects as causal. Other studies in this area of the literature using similar identification strategies have argued for the interpretation of their findings as causal estimates, and I have attempted to provide convincing evidence that the within-community variation in violence over time is exogenous in this paper's context.

The results in this paper may have a number of public policy implications, the most interesting and important of which can be inferred from the estimations of heterogeneous effects of violence. The most significant and consistent estimates suggest that it is the long-term effect of exposure to violence, not temporary exposure resulting in short-term cognitive impairment, which most heavily affects student outcomes. This shows only limited evidence of having heterogeneous effects depending on student-level characteristics, which could mean that the effect of violence on interrupting institutional processes, affecting all students in a school equally, is a primary channel.

In particular, my estimates point to the effect of violence on teacher turnover as one of the affected institutional processes. Colombia as a whole, and Medellin in particular, has a long history of violence directed against educators (Valencia and Celis, 2012). The country has only recently begun to closely monitor this violence and implement programs to protect teachers. As more of this data becomes available, it will deserve close scrutiny, particularly to determine what programs work for both keeping teachers safe and limiting teacher attrition. Reforming recruitment processes to identify and hire teachers more likely to be successful and less likely to burn out in difficult contexts, and incentivizing teachers appropriately should receive special attention. No less important will be identifying and evaluating the effectiveness of reforms aimed at keeping students and teachers safe, both during their commutes and in classrooms. Examples of programs to be evaluated include programs like the recently implemented *Estrategia RIO* in Bogotá.

Additionally, the results show some evidence for heterogeneous effects of violence on at-risk populations. This could be the result of cognitive impairment on those living in homes or neighborhoods that disproportionately bear the weight of violence, while in other cases it

could be evidence that violence induces dropout decisions for at-risk students. It is therefore important for counseling, mentoring, and academic orientation programs to be directed at these students, especially during high-violence periods.

Finally, these public policy implications take on added importance in the coming years, as members of the FARC guerrilla group demobilize and Colombia transitions to a post-conflict scenario. As a major city located near several of the FARC's traditional strongholds, it is likely that Medellin will become the new home of a large number of newly demobilized, relatively uneducated, and socially stigmatized FARC members. The potential for ex-combatants to be easy recruits for criminal gangs has already been seen in the country, and there is certainly a possibility for such a migration to serve as the type of exogenous shock to the structure of the criminal economy that causes spikes in violence similar to those seen during the period evaluated in this study.

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Tables, Graphs, and Maps

Table 1: Characteristics of the Related Literature

Strategy	#	Channel	#	Human Capital Measures	#	Violence Measures	#
2SLS	4	**Physical Infrastructure**	2	**Labor Market Outcomes**	4	**Casualties or Attacks (Region-Level)**	8
Dif-in-Dif	5	**Supply of Teachers**	4	**Years of Schooling**	12	**War-Period/War-Region dummy**	3
Fixed Effects	4	**Lost Schooling or Labor Experience from Soldier Status**	2	**Test Scores**	2	**In-School Violence (School-Level)**	1
Natural Experiment	1	**Risk of Leaving Home to Attend School**	2	**University Attendance**	1	**Neighborhood Gun Crime (School-Level)**	1
Multivariate Regression	1	**Early Childhood Development**	3			**Combatant Status (Individual-level)**	2
Total Papers	15	**Psychological Impact of Violence**	1			**Victimization Dummy (Individual-Level)**	1
		Home Resources	3				
		Ophanhood	3				
		Returns to Education	2				
		School Closing and Interruptions to Institutional Processes	1				

Table 2: Descriptive Statistics of School Characteristics

Panel A: Descriptive Statistics of Local Violence

	Pooled Sample		**Schools in High-Violence Areas**			**Schools in Low-Violence Areas**			**Difference**	
	Obs	Mean	Obs	Mean	SE	Obs	Mean	SE	Dif.	SE
Homicides within 500 meters	1,731	6.890	787	11.174	0.262	944	3.319	0.093	7.855***	0.260
Homicides within 1000 meters	1,731	25.308	787	39.197	0.764	944	13.729	0.342	25.468***	0.791
Homicides within 1500 meters	1,731	51.994	787	76.879	1.378	944	31.247	0.742	45.632***	1.498
Homicides within 2000 meters	1,731	86.964	787	122.781	2.109	944	57.103	1.292	65.679***	2.390

Panel B: Descriptive Statistics of Academic Characteristics

	Pooled Sample		**Schools in High-Violence Areas**			**Schools in Low-Violence Areas**			**Difference**	
	Obs	Mean	Obs	Mean	SE	Obs	Mean	SE	Dif.	SE
Full Day Schedule	1,731	0.100	787	0.082	0.010	944	0.115	0.010	-0.034**	0.014
Morning Schedule	1,731	0.306	787	0.309	0.016	944	0.303	0.015	0.006	0.022
Evening Schedule	1,731	0.122	787	0.149	0.013	944	0.099	0.010	0.05***	0.016
Weekend Schedule	1,731	0.011	787	0.012	0.004	944	0.010	0.003	0.002	0.005
Afternoon Schedule	1,731	0.462	787	0.449	0.018	944	0.473	0.016	-0.024	0.024
Academic Curriculum	1,507	0.725	685	0.709	0.017	822	0.738	0.015	-0.029	0.023
Mixed Curriculum	1,507	0.165	685	0.219	0.016	822	0.120	0.011	0.099***	0.019
Normal School	1,507	0.005	685	0.000	0.000	822	0.009	0.003	-0.009**	0.003
Technical Curriculum	1,507	0.105	685	0.072	0.010	822	0.133	0.012	-0.061***	0.015

*** p<0.01, ** p<0.05, * p<0.1

Table 2: Descriptive Statistics of School Characteristics (continued)
Panel C: Student Characteristics

	Pooled Sample		Schools in High-Violence Areas			Schools in Low-Violence Areas			Difference	
	Obs	Mean	Obs	Mean	SE	Obs	Mean	SE	Dif.	SE
%Male	1,731	0.461	787	0.447	0.018	944	0.472	0.016	-0.025	0.024
%Ethnic Minority	1,731	0.037	787	0.038	0.007	944	0.036	0.006	0.002	0.009
%Working	1,428	0.246	657	0.257	0.017	771	0.237	0.015	0.020	0.023
%Father Dropout	1,430	0.648	658	0.664	0.018	772	0.635	0.017	0.029	0.025
%Mother Dropout	1,429	0.608	657	0.633	0.019	772	0.588	0.018	0.045*	0.026
%Strata1	1,431	0.172	658	0.142	0.014	773	0.197	0.014	-.055***	0.020
%Strata2	1,431	0.488	658	0.514	0.019	773	0.465	0.018	0.049*	0.026
%Strata3	1,431	0.300	658	0.329	0.018	773	0.275	0.016	0.054**	0.024
%Strata4	1,431	0.033	658	0.012	0.004	773	0.051	0.008	-.039***	0.009
%Strata5	1,431	0.007	658	0.002	0.004	773	0.011	0.004	-0.009**	0.004
%Family Income < 1MMS	1,430	0.223	658	0.219	0.016	772	0.227	0.015	-0.008	0.022
%1MMS<Fam Inc<2MMS	1,430	0.568	658	0.589	0.019	772	0.550	0.018	0.039	0.026
%2MMS<Fam Inc<5MMS	1,430	0.198	658	0.185	0.015	772	0.210	0.015	-0.025	0.021
%Family Income > 5MMS	1,430	0.010	658	0.007	0.003	772	0.013	0.004	-0.006	0.005

Panel D: ICFES Results

	Pooled Sample		Schools in High-Violence Areas			Schools in Low-Violence Areas			Difference	
	Obs	Mean	Obs	Mean	SE	Obs	Mean	SE	Dif.	SE
Total Students	1,731	82.326	787	89.371	4.034	944	76.452	2.498	12.919***	4.588
ICFES Language Score	1,731	46.457	787	46.132	0.088	944	46.729	0.079	-0.597***	0.119
ICFES Math Score	1,731	43.956	787	43.491	0.097	944	44.343	0.094	-0.852***	0.133
ICFES Rank (1-1,000)	1,731	527.375	787	546.271	3.603	944	511.622	3.361	34.649***	4.937

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Missing Data

Panel A: Misclassification of *Comunas*

Comuna	2006	2007	2008	2009	2010	2011	2012	2013
1								
2								
3			OVER					
4								
5			UNDER					
6	OVER							OVER
7		UNDER		UNDER			UNDER	
8				UNDER	UNDER			UNDER
9								
10								
11								
12								
13		UNDER	UNDER	UNDER				
14								
15	UNDER	UNDER						
16		OVER						

Note: "UNDER" if data classifies a high-violence area as low-violence; "OVER" if data classifies a low-violence area as high-violence

Panel B: GPS Matching Success

Year	Total Homicides	Not Matched	Pct Matched (%)
2006	669	197	70.55
2007	626	171	72.68
2008	779	190	75.61
2009	1257	318	74.70
2010	1250	371	70.32
2011	1377	415	69.86
2012	1111	344	69.04
2013	831	208	74.97

Table 4: Homicide Exposure and Lagged School Traits

Lagged Variables	(1) Average Violence	(2) Average Violence
Family Income<1MMS (%)	0.042	0.853
	(1.475)	(1.625)
1MMS<Family Income<2MMS (%)	-0.392	0.003
	(1.336)	(1.452)
2MMS<Family Income<5MMS (%)	-0.388	0.262
	(1.407)	(1.519)
Mother Dropout (%)	-0.400	-0.049
	(0.856)	(0.980)
Male Students (%)	-0.130	1.431
	(1.430)	(1.601)
Ethnic Minority Students (%)	0.410	-1.183
	(1.241)	(1.509)
Working Students (%)	-0.676	-0.867
	(0.883)	(0.695)
Total Students Strata1	0.004	-0.001
	(0.013)	(0.013)
Total Students Strata2	0.017**	0.018*
	(0.008)	(0.009)
Total Students Strata3	0.003	-0.000
	(0.010)	(0.011)
Total Students Strata4	-0.096**	-0.118**
	(0.039)	(0.049)
Total Students Strata5	-0.032	-0.049
	(0.077)	(0.087)
Total Students Strata6	0.409	0.474
	(0.541)	(0.500)
Language Exam Score	-0.142	-0.082
	(0.087)	(0.112)
Mathematics Exam Score	0.020	0.120
	(0.067)	(0.084)
Lag	1 Period	2 Periods
Observations	1,174	933
R-squared	0.239	0.248
Number of Schools	240	231
Year FE	YES	YES
School FE	YES	YES

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

Table 5: The effect of homicides on ICFES scores
Panel A: Exam-year and lagged violence

VARIABLES	(1) Math	(2) Math	(3) Language	(4) Language	(5) Biology	(6) Biology	(7) Physics	(8) Physics
Homicide Exposure	-0.004	-0.003	-0.008**	-0.010**	-0.003	-0.004	-0.004	-0.007
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Lagged Homicide Exposure	-0.008*	-0.008**	-0.007*	-0.010***	-0.005	-0.008**	0.003	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Observations	1,472	1,285	1,472	1,285	1,472	1,285	1,472	1,285
Number of Schools	240	240	240	240	240	240	240	240
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
School FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES	NO	YES

	(9) Chemistry	(10) Chemistry	(11) Social Sciences	(12) Social Sciences	(13) Philosophy	(14) Philosophy	(15) Rank	(16) Rank
Homicide Exposure	-0.004	-0.003	-0.006*	-0.006*	-0.001	0.000	0.003	0.005
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Lagged Homicide Exposure	-0.006	-0.008**	-0.003	-0.008**	-0.006	-0.010**	0.003	0.007**
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Observations	1,472	1,285	1,472	1,285	1,472	1,285	1,472	1,285
Number of Schools	240	240	240	240	240	240	240	240
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
School FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES	NO	YES

Robust standard errors clustered at the school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Positive coefficient for Rank is actually a negative effect. Dependent variable is the average school score on the different portions of the ICFES exam, expressed in terms of standard deviations. Controls include total number of students, percentage of all students who are male, percent who work, who are from ethnic minority groups, whose mothers never completed high school, and who are from the different income bracket and neighborhood strata classifications.

Table 5: The effect of homicides on ICFES scores
Panel B: Two-year average violence exposure

	(1) Math	(2) Math	(3) Language	(4) Language	(5) Biology	(6) Biology	(7) Physics	(8) Physics
Two-Year Avg Homicide Exposure	-0.011**	-0.011**	-0.015***	-0.019***	-0.008	-0.012**	-0.001	-0.010
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.007)
Observations	1,472	1,285	1,472	1,285	1,472	1,285	1,472	1,285
Number of Schools	240	240	240	240	240	240	240	240
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
School FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES	NO	YES

	(9) Chemistry	(10) Chemistry	(11) Social Sciences	(12) Social Sciences	(13) Philosophy	(14) Philosophy	(15) Rank	(16) Rank
Two-Year Avg Homicide Exposure	-0.010*	-0.011**	-0.010**	-0.014***	-0.007	-0.010	0.006	0.012**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)	(0.005)
Observations	1,472	1,285	1,472	1,285	1,472	1,285	1,472	1,285
Number of Schools	240	240	240	240	240	240	240	240
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
School FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES	NO	YES

Robust standard errors clustered at the school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Positive coefficient for Rank is actually a negative effect. Dependent variable is the average school score on the different portions of the ICFES exam, expressed in terms of standard deviations. Controls include total number of students, percentage of all students who are male, percent who work, who are from ethnic minority groups, whose mothers never completed high school, and who are from the different income bracket and neighborhood strata classifications.

Table 6: Diminishing marginal effects at greater distances (Two-year homicide exposure average)

	(1) Math	(1) Math	(2) Math	(4) Language	(5) Language	(6) Language	(7) Biology	(8) Biology	(9) Biology	(10) Rank	(10) Rank	(12) Rank
Homicides	-.004**			-.006***			-.005**			.005***		
(M= 1000 meters)	(0.002)			(0.002)			(0.002)			(0.002)		
Homicides		-.002**			-.003***			-.003**			.002**	
(M=1500 meters)		(0.001)			(0.001)			(0.001)			(0.001)	
Homicides			-.002*			-.002***			-.002**			.002**
(M=2000 meters)			(0.001)			(0.001)			(0.001)			(0.001)
Observations	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285
Number of Schools	240	240	240	240	240	240	240	240	240	240	240	240
Year & School FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors clustered at the school level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Positive coefficient for Rank is actually a negative effect. Dependent variable is the average school score on the different portions of the ICFES exam, expressed in terms of standard deviations. Controls include total number of students, percentage of all students who are male, percent who work, who are from ethnic minority groups, whose mothers never completed high school, and who are from the different income bracket and neighborhood strata classifications.

Table 7: Placebo Regressions – The effect of homicides in lead years on ICFES scores

	(1) Math	(2) Math	(3) Language	(4) Language	(5) Biology	(6) Biology	(7) Physics	(8) Physics
Avg Homicide Exposure in t+1, t+2	0.000	0.004	-0.009	-0.006	-0.001	0.003	0.000	0.004
	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
Observations	1,224	933	1,224	933	1,224	933	1,224	933
Number of Schools	231	231	231	231	231	231	231	231
Year & School FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES	NO	YES

	(9) Chemistry	(10) Chemistry	(11) Social Sciences	(12) Social Sciences	(13) Philosophy	(14) Philosophy	(15) Rank	(16) Rank
Avg Homicide Exposure in t+1, t+2	-0.002	0.000	0.001	-0.000	0.001	0.001	0.004	0.002
	(0.005)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)
Observations	1,224	933	1,224	933	1,224	933	1,224	933
Number of Schools	231	231	231	231	231	231	231	231
Year & School FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES	NO	YES

Robust standard errors clustered at the school level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Positive coefficient for Rank is actually a negative effect. Dependent variable is the average school score on the different portions of the ICFES exam, expressed in terms of standard deviations. Controls include total number of students, percentage of all students who are male, percent who work, who are from ethnic minority groups, whose mothers never completed high school, and who are from the different income bracket and neighborhood strata classifications.

Table 8: Differential effect of violence (Student-level regressions)

Panel A: Student Gender

VARIABLES	(1) Math	(2) Language	(3) Biology	(4) Physics	(5) Chemistry	(6) Social Sciences	(7) Philosophy	(8) Rank
Homicides (2 yr avg)	-0.004*	-0.004*	-0.002	-0.002	-0.003	-0.002	-0.004	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Male*Homicides	-0.002	0.001	-0.003**	0.001	0.001	-0.003*	0.003*	-0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Male	0.347***	0.023*	0.192***	0.232***	0.157***	0.157***	-0.042***	-0.218***
	(0.014)	(0.012)	(0.012)	(0.015)	(0.013)	(0.012)	(0.012)	(0.013)
Observations	96,544	96,544	96,544	96,544	96,544	96,544	96,544	96,544

Panel B: Mother High School Dropout

Homicides (2 yr avg)	-0.005**	-0.005**	-0.003*	-0.003*	-0.004**	-0.003	-0.002	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
MomDropout* Homicides	0.001	0.002**	0.001	0.003**	0.002*	0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mom Dropout	-0.111***	-0.130***	-0.116***	-0.084***	-0.110***	-0.105***	-0.083***	0.146***
	(0.012)	(0.010)	(0.012)	(0.014)	(0.013)	(0.011)	(0.013)	(0.011)
Observations	96,544	96,544	96,544	96,544	96,544	96,544	96,544	96,544

Robust standard errors clustered at the school level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Positive coefficient for Rank is actually a negative effect. Dependent variable is the average school score on the different portions of the ICFES exam, expressed in terms of standard deviations. Controls include total number of students, percentage of all students who are male, percent who work, who are from ethnic minority groups, whose mothers never completed high school, and who are from the different income bracket and neighborhood strata classifications.

Table 8: Differential effect of violence (Continued)
Panel C: Household Socioeconomic Level

VARIABLES	(1) Math	(2) Language	(3) Biology	(4) Physics	(5) Chemistry	(6) Social Sciences	(7) Philosophy	(8) Rank
Homicides (2 yr avg)	-0.006**	-0.004**	-0.004**	-0.003	-0.005**	-0.004*	-0.005**	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Homicides*Strata1_2	0.002	0.002	0.002*	0.002*	0.003**	0.003**	0.003***	-0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Homicides*Strata4_6	0.000	-0.005	-0.001	-0.006*	0.003	0.002	-0.002	0.003
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
Strata1_2	-0.074***	-0.078***	-0.086***	-0.051***	-0.065***	-0.074***	-0.060***	0.101***
	(0.013)	(0.014)	(0.014)	(0.014)	(0.012)	(0.014)	(0.013)	(0.015)
Strata4_6	0.050**	0.046**	0.067**	0.051**	0.035	0.034	0.026	-0.076***
	(0.025)	(0.023)	(0.030)	(0.026)	(0.031)	(0.024)	(0.029)	(0.027)
Observations	96,531	96,531	96,531	96,531	96,531	96,531	96,531	96,531
For Panels A-C								
Number of Schools	240	240	240	240	240	240	240	240
School & Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors clustered at the school level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Positive coefficient for Rank is actually a negative effect. Dependent variable is the average school score on the different portions of the ICFES exam, expressed in terms of standard deviations. Controls include total number of students, percentage of all students who are male, percent who work, who are from ethnic minority groups, whose mothers never completed high school, and who are from the different income bracket and neighborhood strata classifications.

Table 9: Descriptive Statistics of Teachers and Teacher Turnover

Panel A: Annual Teacher Turnover

		Total	**Leavers (quit at year's end)**		**Stay put (continue in t+1)**	
Year	Obs	Pct. (%)	Obs	Pct. (%)	Obs	Pct. (%)
2008	10,730	100	1520	14.166	9,210	85.834
2009	10,416	100	2033	19.518	8,383	80.482
2010	10,461	100	560	5.353	9,901	94.647
2011	10,416	100	3541	33.996	6,875	66.004
2012	10,066	100	1933	19.203	8,133	80.797

Panel B: Descriptive Statistics of Teachers

	All Teachers		**Leavers**			**Stay Put**			**Difference**	
	Obs	Mean	Obs	Mean	SE	Obs	Mean	SE	Dif.	SE
Homicides within 500 meters	19,818	7.093	13,641	7.679	6.493	6,177	5.800	4.410	1.879***	0.091
Age	19,936	45.223	13,757	44.783	10.248	6,179	46.201	9.747	-1.418***	0.155
Years of Experience	19,936	13.265	13,757	12.253	12.247	6,179	15.517	11.729	-3.265***	0.185
Highest Education Completed										
Secondary School=1	19,936	0.177	13,757	0.183	0.004	6,179	0.163	0.005	0.02***	0.006
Undergraduate Degree=1	19,936	0.630	13,757	0.621	0.004	6,179	0.652	0.006	-0.031***	0.007
Post-Graduate Degree=1	19,936	0.173	13,757	0.182	0.003	6,179	0.154	0.005	0.027***	0.006
Teacher Type										
Permanent Contract=1	19,936	0.672	13,757	0.669	0.004	6,179	0.679	0.006	-0.01	0.007
Secondary School Teacher=1	19,936	0.554	13,757	0.582	0.004	6,179	0.491	0.006	0.091***	0.008
STEM Teacher=1	19,936	0.212	13,757	0.224	0.004	6,179	0.185	0.005	0.039***	0.006
Female=1	19,936	0.635	13,757	0.611	0.004	6,179	0.688	0.006	-0.078***	0.007

*** p<0.01, ** p<0.05, * p<0.1

Note: Observations in Panel B include only transition years for teachers who made turnover decision (Leavers) at some point in the 2008-2013 period, and only 2012 for teachers who remain in the same school for the entire 2008-2013 period (Stay put).

Table 10, Panel A: Violence and teacher attrition decisions at the end of t-1 (Proportional Hazards Model)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\theta_i(j)$	$\theta_i(j)$	$\theta_i(j)$	$\theta_i(j)$	$\theta_i(j)$	$\theta_i(j)$
Two-year Homicide Average	0.030***	0.031***	0.029***	0.019*	0.026*	-0.004
	(0.007)	(0.008)	(0.008)	(0.011)	(0.014)	(0.019)
Observations	51,493	27,827	22,806	8,126	5,166	2,715
Teacher Type	All Teachers	High School	Primary & Pre-K	All Teachers	High School	Primary & Pre-K
Start Date	All Teachers	All Teachers	All Teachers	Post 2007	Post 2007	Post 2007

Panel B: Violence, teacher qualifications, and attrition decisions at the end of t-1 (Proportional Hazards Model)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\theta_i(j)$	$\theta_i(j)$	$\theta_i(j)$	$\theta_i(j)$	$\theta_i(j)$	$\theta_i(j)$
Two-year Homicide Average	0.031***	0.033***	0.028***	0.024**	0.032**	-0.002
	(0.007)	(0.008)	(0.009)	(0.011)	(0.015)	(0.020)
HomicideAverage*LowQuals	-0.008*	-0.011*	-0.004	-0.019**	-0.023*	-0.005
	(0.004)	(0.006)	(0.006)	(0.009)	(0.013)	(0.015)
HomicideAverage*HighQuals	0.001	-0.000	0.009	0.006	0.016	0.014
	(0.004)	(0.005)	(0.007)	(0.017)	(0.021)	(0.040)
LowQuals	0.122**	0.143**	0.153**	1.508***	1.429***	2.012***
	(0.051)	(0.070)	(0.076)	(0.151)	(0.221)	(0.276)
HighQuals	-0.093**	-0.072	-0.203**	0.025	-0.075	0.057
	(0.047)	(0.061)	(0.082)	(0.262)	(0.284)	(0.712)
Observations	51,493	27,827	22,806	8,126	5,166	2,715
Teacher Type	All Teachers	High School	Primary & Pre-K	All Teachers	High School	Primary & Pre-K
Start Date	All Teachers	All Teachers	All Teachers	Post 2007	Post 2007	Post 2007

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

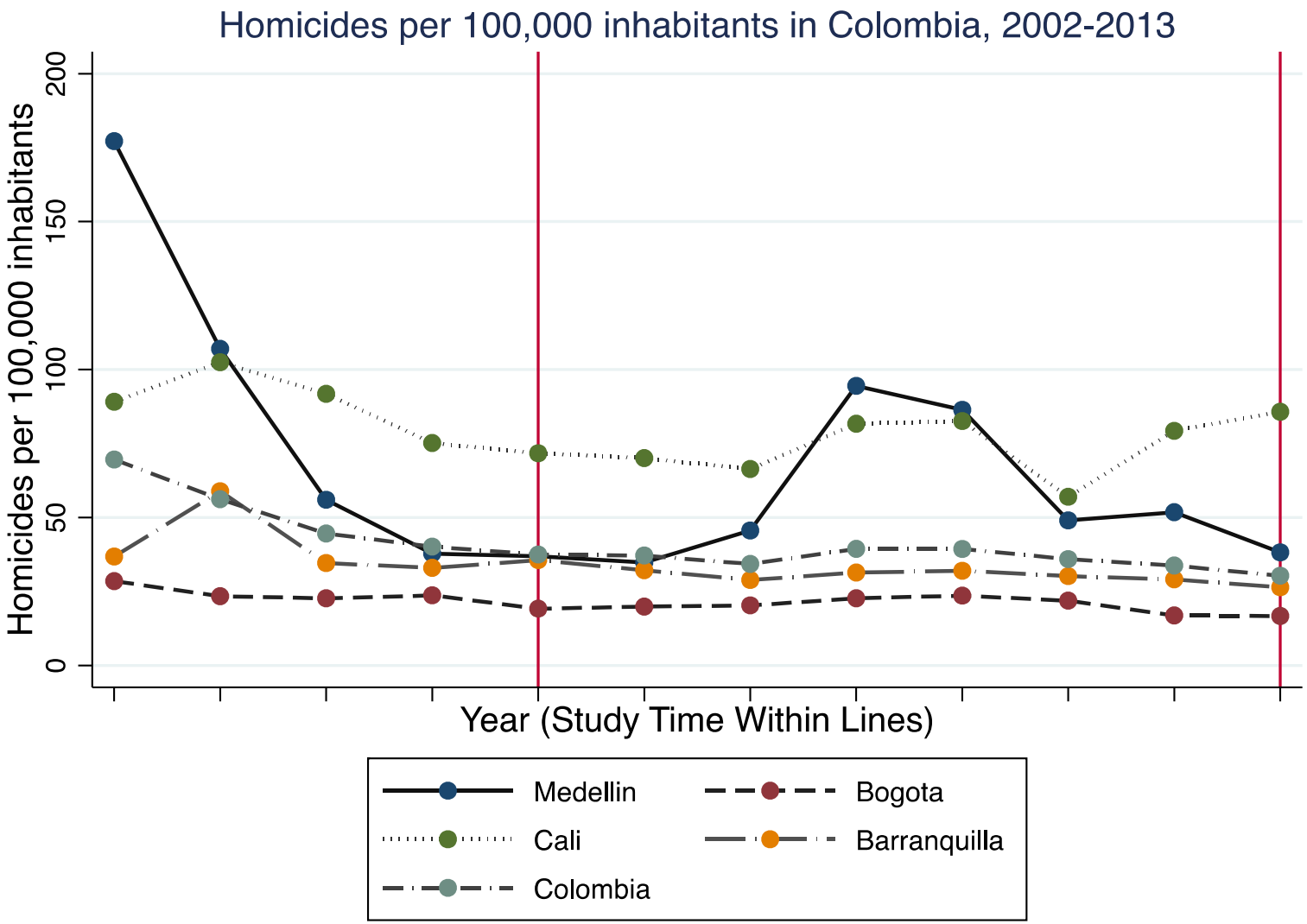
Note: Regressions include baseline hazards for survival time, year, and school. Teacher-level controls include teacher's level of completed studies, salary, age, and sex. School controls include average score on the language and math portions of the ICFES exam, total number of students, percentage of students who also work, and percentage of students whose mothers never graduated from high school. For Panel B, LowQuals denominates teachers with the lowest professional qualifications, including teachers with no post-secondary degree. HighQuals denominates teachers with the highest professional qualifications, and includes teachers with a post-graduate degree. The baseline is those teachers whose highest completed education is a two- or four-year university degree.

Table 11: Average violence in years t-1 and t-2 and composition of teachers in t

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Percentage of teachers whose highest qualification is…	No Post-Secondary	No Post-Secondary	Four-year Degree	Four-year Degree	Post-Graduate Degree	Post-Graduate Degree
Two-year Homicide Average	-0.001	-0.003***	0.001*	0.002**	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Observations	1,076	910	1,076	910	1,076	910
Year & School FE	YES	YES	YES	YES	YES	YES
School Controls	NO	YES	NO	YES	NO	YES

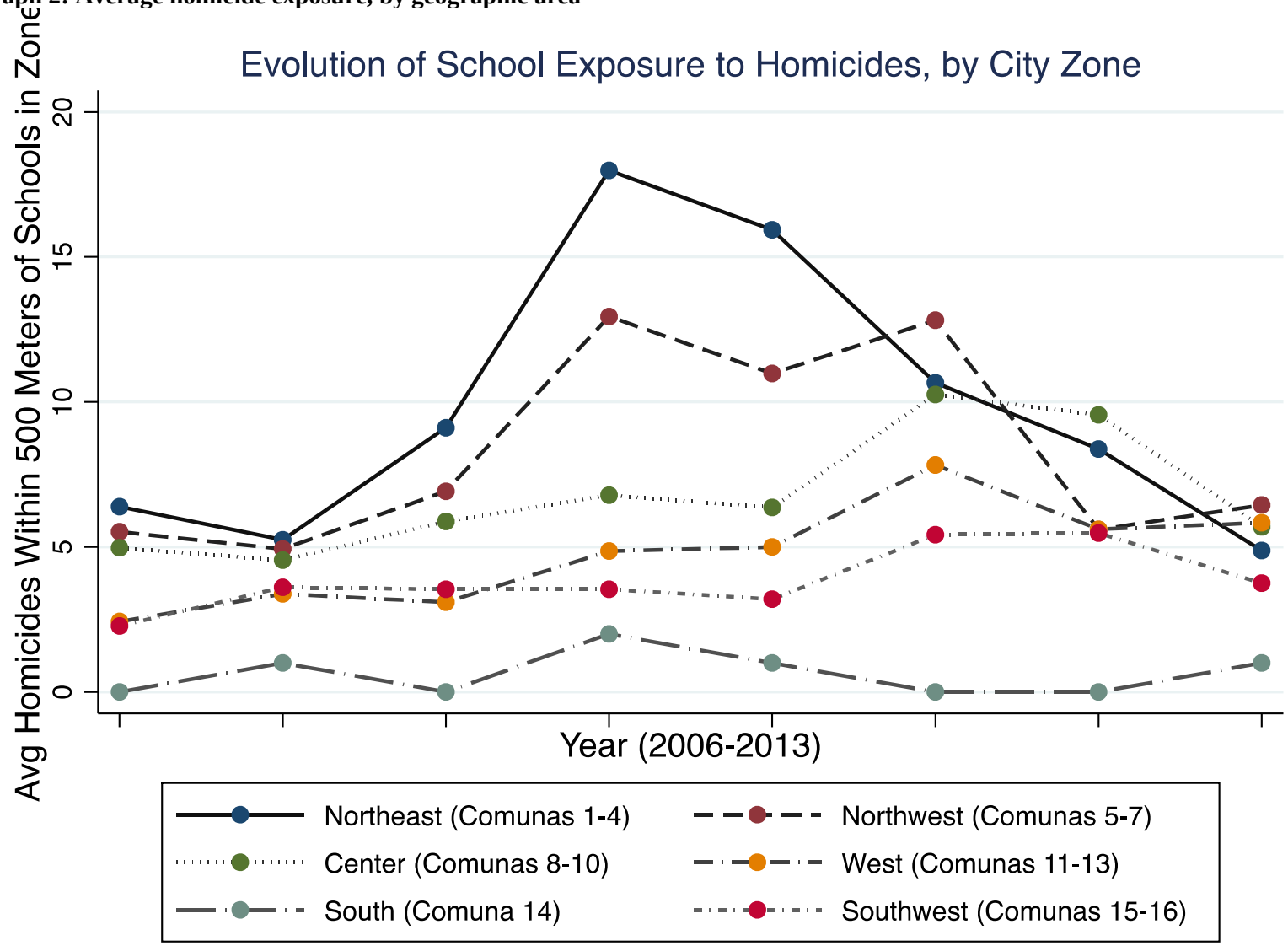
Robust standard errors clustered at the school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Graph 1: Colombia homicide trends, by city



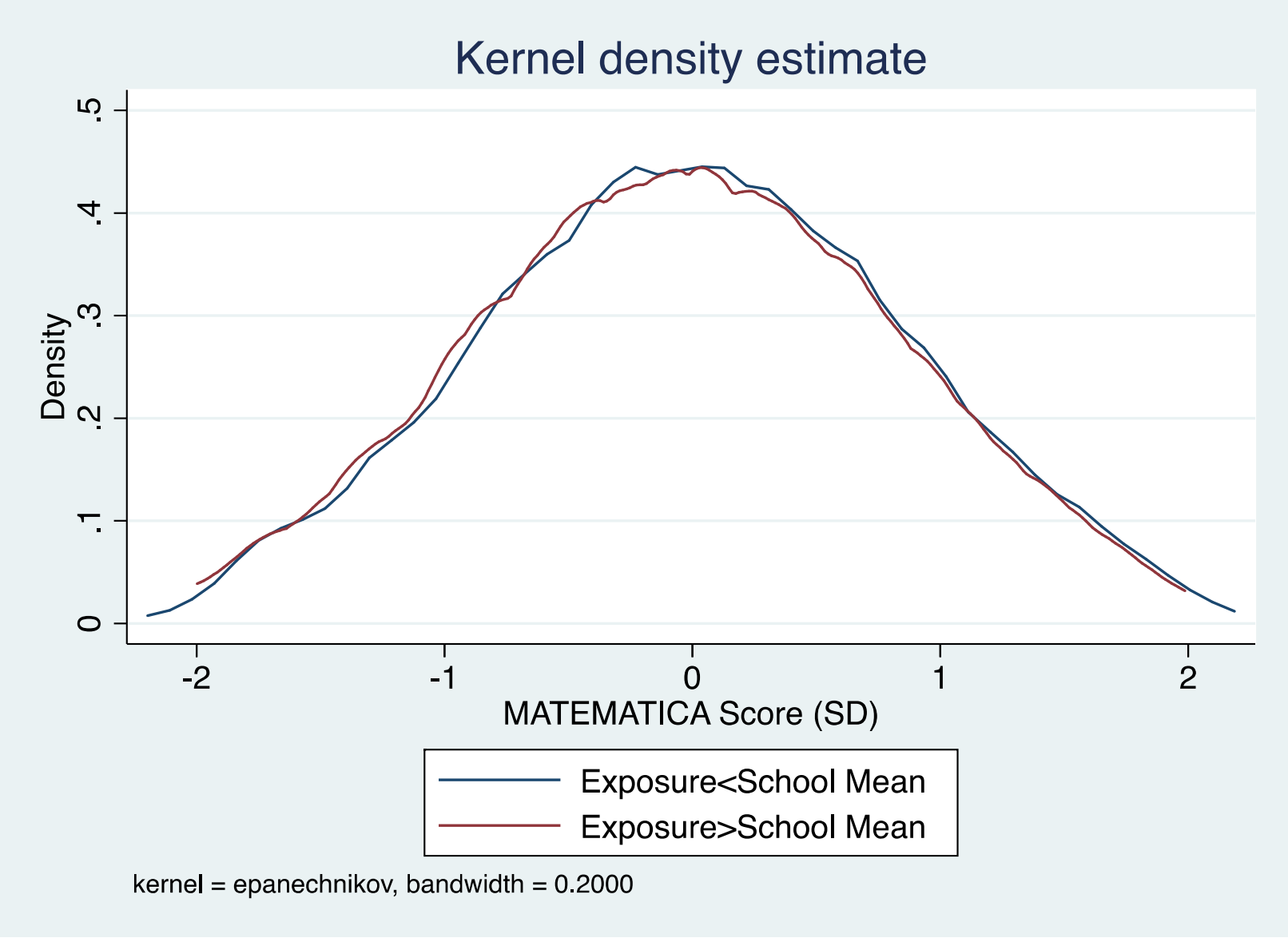
Source: Author's calculations with data from Colombia's Instituto Nacional de Medicina Legal

Graph 2: Average homicide exposure, by geographic area



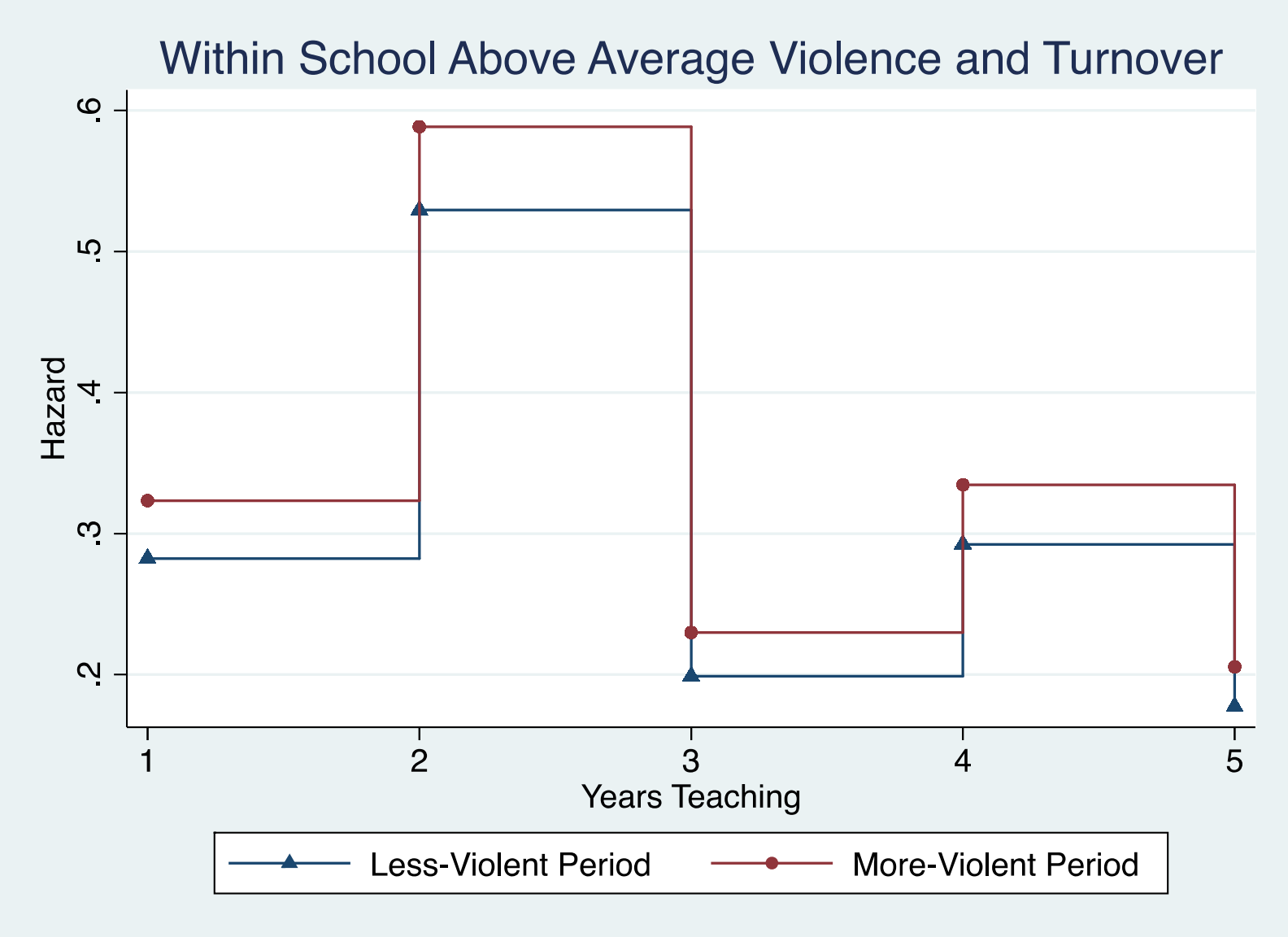
Source: Author's calculations from National Police data

Graph 3: Kernel Density of Math Score Distribution, by homicide exposure



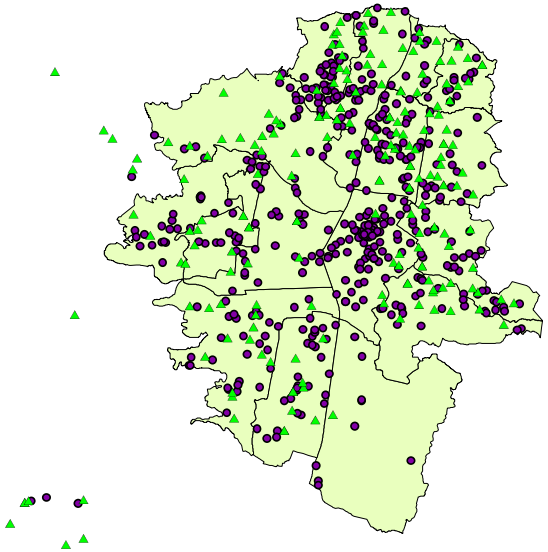
Source: Author's calculations from National Police and ICFES data

Graph 4: Teacher turnover hazards, by exposure to local violence (only high school teachers with post-2007 start dates)

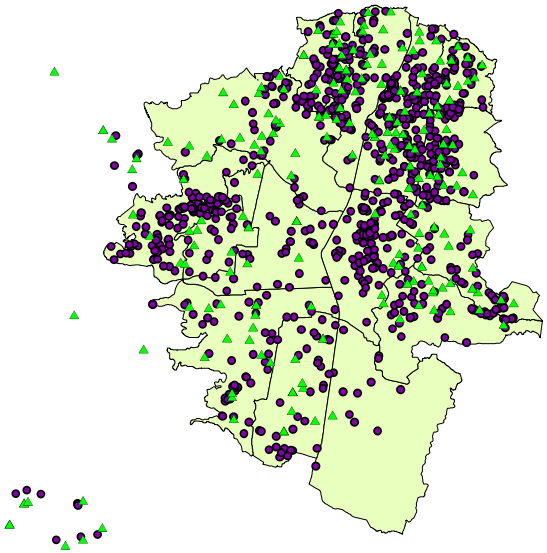


Source: Author's calculations from Resolución 166 Anexo 3a and National Police data

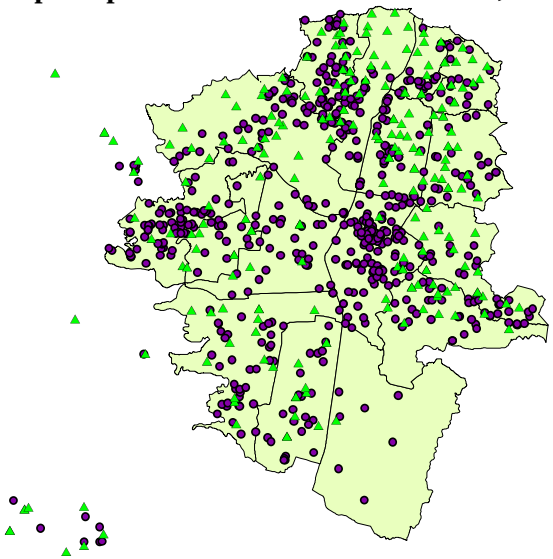
Map 1: Spatial distribution of homicides, 2007



Map 2: Spatial distribution of homicides, 2010



Map 3: Spatial distribution of homicides, 2013



Legend

Schools

Homicides

Comuna Border