This paper is a draft submission to the

WIDER Development Conference

Human capital and growth

6-7 June 2016 Helsinki, Finland

The (Non-) Effect of Violence on Education: Evidence from the "War on Drugs" in Mexico

Fernanda Márquez-Padilla[*], Francisco Pérez-Arce[†], and Carlos Rodríguez-Castelán[‡]

April 13, 2016

Abstract

This paper studies the sharp increase in violence experienced in Mexico after 2006, known as "The War on Drugs" and its effects on human capital accumulation. The upsurge in violence is expected to have direct effects on individuals' schooling decisions, but not indirect effects, because there was no severe destruction of infrastructure. The fact that the marked increases in violence were concentrated in some municipalities (and not in others) allows for implementation of a fixed-effects methodology to study the effects of violence on educational outcomes. Different from several recent studies that have found significant negative effects of violence on economic outcomes in Mexico, the paper finds evidence that this is not the case, at least for human capital accumulation. The paper uses several sources of data on homicides and educational outcomes and shows that, at most, there are very small effects on total enrollment. These small effects may be driven by some students being displaced from high-violence municipalities to low-violence municipalities; but the education decisions of individuals do not seem to be highly impacted. The analysis discards the possibility that the effects on enrollment of young adults appear small because of a counteracting effect from ex-workers returning to school. The results stand in contrast with recent evidence of the negative effects of violence on short-term economic growth, since minimal to null effects on human capital accumulation today should have little to no adverse effects on long-term growth outcomes in Mexico.

[*]Princeton University

[†]RAND Corportation

[‡]World Bank

1 Introduction

There is a growing interest in the economic literature on the pervasive effects of exposure to violence and the presence of armed conflicts on human capital accumulation. Social scientists have suggested that hostile environments may have a detrimental effect on education by reducing enrollment, years of schooling, academic achievement, and even long-term labor market performance. However, the literature has come short on disentangling the direct effect of individuals' schooling decisions from the indirect effects related to the destruction of infrastructure which inevitably accompanies armed conflict.

In this paper we study the sharp increase in violence experienced in Mexico after 2006 and its effects on human capital accumulation. This upsurge in violence is associated with the Federal Government's launch of a military offensive against drug trafficking organizations (DTOs) known as the "War on Drugs". Months after the start of this operation in December 2006, that deployed 6,500 federal troops and continued to expand to approximately 45,000 troops by 2011, violent confrontations between DTOs and official armed forces and between competing DTOs became more frequent and homicide rates increased quickly.

This raise in violence affected significantly some municipalities, while it virtually left others untouched. Though concentrated in some municipalities (that became fighting grounds between DTOs), the municipalities affected in this period are spread across the country. The northern border, and the Pacific and Gulf of Mexico coasts, all saw violence spikes in some of their cities. The increase in violence affected both places that had historically observed high levels (such as Ciudad Juárez in the north) and places that had not experienced high levels before (including the prosperous city of Monterrey). Other cities, including Mexico City, that had suffered significant crime rates in earlier years, did not experience increases during this period.

The fact that the marked increases in violence were concentrated in some municipalities (and not in others) allows us to implement a fixed effects methodology to study the effects of violence. Also, the fact that Mexico's War on Drugs has not had the destruction of infrastructure which usually accompanies large scale armed conflicts allows us to disentangle the direct effect of violence on individuals' schooling decisions. Our empirical strategy then consists of comparing changes in educational outcomes with changes in the number of homicides at the municipality level. We find evidence that changes in the level of homicide rates do not seem to be explained by prior trends in homicide rates, economic growth, or other variables, which gives credence to our empirical strategy.

The analysis combines homicides data from official statistics reported by Mexico's Technical Secretariat of the National Public Security Council (SNSP) with the official education enrollment data from the National Institute of Statistics, INEGI (as reported the Ministry of Public Education, SEP).

We show that increases in violence had a very small impact on the number of students enrolled in a municipality. An increase of 8 homicides per 100,000 individuals (which corresponds to the nationwide increase in the homicide rate during the period of analysis, 2007 to 2011) is associated with no decrease in the number of enrolled students in basic education comprised by primary and lower secondary school (*primaria* and *secundaria*, or years 1 to 9), and a 0.3% decrease in the number of enrolled students in upper-secondary school (*preparatoria* or *bachillerato*, or years 10 to 12). Because our results are tightly estimated, we can conclude that the decreases are not steeper than 0.067% and 0.59%, respectively.

We find that these small effects are explained by migration of students rather than by changes in enrollment. We conduct a second set of analyses where the education variable comes from household surveys and censuses. In this case, the dependent variable is an indicator of whether the individual is attending school or not. With these specifications, we cannot reject the null hypothesis of no impact of violence (as proxied by the homicide rates) on enrollment rates.[1] At the 5 percent level, we estimate that the effect of an increase of 8 homicides per 100,000 individuals on the enrollment rates of 15-17 year olds is smaller than 0.032%, for example. We hypothesize that the small effect on the number of enrolled students but null impact on the rate of enrollment is explained by an impact on migration. We corroborate this by showing that increases in violence are associated with outmigration: municipalities with higher increases in homicide rates observed relative reductions in their population size. Thus, these results are consistent with the hypothesis that a small fraction of families reacted to increases of violence by moving out (with their school-age children) of their municipality of residence to less-affected municipalities, but that it did not affect the probability of being enrolled in school.

Several studies have found significant effects of violence on economic outcomes in Mexico in the same period, though none of these (to our knowledge) look at education outcomes. Using similar methodologies, focusing on the same population and using the same homicide data, Arias-Vazquez and Esquivel (2013), Robles, Calderón and Magaloni (2013), Velásquez (2014), Dell (2015) and Enamorado, López-Calva and Rodríguez-Castelán (2014) find significant negative effects of homicide rates on labor

[1] The terms violence and crime describe different concepts. According with the World Health Organization, violence refers to the intentional use of physical force or power, threatened or actual, against oneself, another person, or against a group or community that either results in or has a high likelihood of resulting in injury, death, psychological harm or deprivation. While crime includes actions that may or may not involve the use of any force or injury to another person—e.g. most property crimes such as theft, embezzlement, fraud, tax crimes, some forms of racketeering, and bribery—. This paper uses the term violence throughout, but does do not attempt to distinguish the impacts of types of violence or crime different than homicides.

market outcomes and economic activities. Given this evidence, we expected to find significant effects on education. However, we find evidence that this is not to be the case. By using several sources of data we show, at most, very small effects on total enrollment rates. A small number of students are being displaced from high violence municipalities to low violence municipalities, thus reducing the total *number* of students in high violence municipalities; but the education decisions of individuals does not seem to be highly impacted. We explore whether the lack of effect on enrollment may arise due to a counteracting effect on the labor force. That is, we explore whether a reduction in employment frees some young individuals to enroll in school. We find no evidence of this: increases in violence were not associated with reductions in employment or labor force participation for school-aged individuals. Our results stand in contrast with the above-described literature finding negative effects of violence on short-term economic growth; since minimal to null effects on human capital accumulation today should have little to no adverse effects on long-term growth outcomes in Mexico.

Violence could affect human capital formation through learning given enrollment. This second channel would occur if stress associated with violence affects the learning for those in school. We test the effect of homicide rates on national test scores and do not find an effect on test scores either. However, these results are less precisely estimated. In addition, the selection issues caused by the effect of violence on migration further complicate the interpretation of this result. Thus, we cannot conclude with certainty whether there has been such effect.

The rest of this paper is organized as follows. Section 2 presents a brief discussion on the theoretical and empirical evidence on this subject. Section 3 presents some stylized facts on the spike in violent crime rates observed during Mexico's Drug War. Section 4 describes the data used for this study. Section 5 lays out the empirical strategy. Section 6 presents our main findings, and Section 7 concludes.

2 Evidence on the Effects of Violence on Human Capital and Labor Market Outcomes

The literature identifies two main potential channels on how violence can affect educational outcomes. First, some theories predict that crime and violence can negatively affect enrollment rates. Second, some theories focus on the negative effect of violence on learning given enrollment. This second channel would occur if stress associated with violence affects the learning for those in school. A good number of studies analyze the potential effects of violence on both enrollment and student attainment. This body of literature includes Shemyakina (2011) who studies the effect of the armed conflict in Tajikistan on individuals' school attainment and enrollment, Chamarbagwala and Morán (2011) who analyze the impact of Guatemala's 36-year-long civil war between 1960 and 1996 on human capital accumulation, and Ichino and Winter-Ebmer (2004) who study the effects of World War II on schooling and labor market outcomes of German and Austrian school-age children. All these studies find significant evidence that exposure to armed conflict had a negative impact on human capital accumulation and learning outcomes.

Nevertheless, the literature studying the effect of violence in large scale armed conflicts has an important shortcoming. Given the nature of the settings being analyzed, it is hard—if not impossible—to disentangle the direct effects of violence on individuals' schooling decisions and academic performance from the indirect effects, as human capital accumulation decisions are usually severely affected by the destruction of infrastructure (like schools and roads) which inevitably accompanies these war episodes. In fact, most of these studies suggest that physical capital destruction is one of the main mechanisms through which education is negatively affected. While Mexico's War on Drugs has taken an important toll in human lives, it has not had the destruction of infrastructure which usually accompanies large scale armed conflicts, thus making Mexico's setting different from traditional armed-conflict scenarios. Our research is thus able to explore the direct effect of violence on human capital accumulation in a highly violent setting through mechanisms besides the destruction of infrastructure.

Recent studies have attempted to use Mexico's War on Drugs to study the effects of violence on economic outcomes. Arias-Vazquez and Esquivel (2013) assess the economic impact of drug-related violence by looking at the relationship between organized crime-related homicides and labor market indicators. They find that drug-related violence increases unemployment (ten additional homicides per 100,000 inhabitants lead to an increase of a half percentage point in the unemployment rate), and that the impact is disproportionally larger for women than for men. Additionally, their evidence suggests that the increase in violence has destroyed both formal and salaried jobs, while increasing self-employment.

Velásquez (2014) also studies the effect of drug-related homicides on labor market outcomes and household expenditures in Mexico. Taking into consideration endogenous migration, this study finds heterogeneous labor market effects by gender and occupation. In particular, she presents evidence that increases in the homicide rates increases the probability that self-employed women leave the labor market and reduce their hours worked. By contrast, this study finds that violence does not appear to affect the labor market participation of self-employed men, but does negatively affect their hourly and total earnings. Finally, the paper also concludes that these negative labor outcomes caused by violence had a negative impact in per capita expenditure at the household level.

Another study that uses the same data set, by Dell (2015), shows that homicide rates and diversion of drug traffic had negative impacts on informal sector earnings and female labor force participation, but it finds no significant effect on formal sector wages and male labor force participation (in the same line as Velasquez). The study concludes that while economic effects may be noisily estimated, they are consistent with qualitative evidence that DTOs extort informal sector producers via protection rackets.

Robles, Calderón and Magaloni (2013) study the effect of drug-trafficking related homicides in Mexico on economic activity (measured using electricity consumption) and unemployment. They suggest that drug-related crime may be affecting the economy by increasing extortion, inducing migration of businesses and business owners to safer territories, a decrease in capital investment and creation of new businesses. They find that an increase of 10 homicides per 100,000 inhabitants generates a decrease in the proportion of people working of 2-3 percentage points. Additionally an increase of 1 homicide per 100,000 inhabitants decreases municipal income by 1.2%.

Finally, Enamorado, López-Calva and Rodríguez-Castelán (2014) combine municipality-level data on incomes (from poverty maps) and crime data for Mexico, and study the effects of the spike in violent crime on income convergence. They find evidence indicating a negative impact of drug-related homicides on income growth in Mexican municipalities over the period from 2005 to 2010. Non-drug related crimes, on the other hand, are not found to have any effect on the economic growth rate of municipalities during the same time period.

We complement this body of evidence by studying the effects of violence in human capital accumulation. This is important in order to understand whether the channel that explains these findings on the negative effects of violence on economic activity might be linked to education; and also since it could preview longer term impacts of the crisis.

3 Recent Spike in Violent Crime in Mexico

After 2007, there has been a dramatic increase in the level of violence in Mexico. The number of homicides per 100,000 inhabitants almost tripled between 2005 and 2011 (from 9,921 to 27,199 homicides). The sharp increase in homicides experienced in Mexico after 2006 began right after the Federal Government declared a "War on Drugs" and launched of a military offensive against DTOs drug trafficking organizations. Figure 1 shows the monthly number of homicides as reported by INEGI since

1990 until 2011.[2]

The recent spike in violence in Mexico has drawn attention both in the policy and academic arenas. It has been argued that the increase in violence was triggered as a response to aggressive government policies (Guerrero (2010); Guerrero (2011); Merino (2011)), although no consensus has been reached. Dell (2015) examines the direct and spillover effects of Mexican policies towards the drug trade. She exploits variation from close mayoral elections to identify the counties that experienced a more intense anti-drug policy. Her analysis suggests that the violence reflects rival traffickers' attempts to wrest control of territories after policies have weakened incumbent criminals.

However, there does not exist a consensus regarding whether the increase in violence has indeed been triggered by government security policies. In fact, there seems to exist some evidence suggesting that the increase of violence has not been a response to the government's aggressive policy to fight DTOs: Ríos (2012*b*) and Sota and Messmacher (2012) argue that it does not appear to be the case that violence has increased as a response to a targeted security policy, which suggests that counties where violence increased the most need not be affected by different government policies as those with no increase in violence.

Rios (2012*a*) suggests that the main reason behind the recent escalation of violence is that the illegal drug industry evolved from one in which DTOs were stable oligopolies into one in which DTOs wanted to compete against each other. Rios (2012*a*) provides empirical evidence that the propensity of criminal organizations to engage in damaging criminal activities increases when municipal and state governments are not coordinated. She argues that coordinated political institutions lead criminal organizations to behave and organize in less violent ways. Mexico's political decentralization has decreased the coordination between levels of government which in turn has increased drug related violence.

Dube, Dube and García-Ponce (2013) analyze the effect of an increase of the availability of guns on violence in Mexican municipalities near the U.S. border. They find differential increases in homicides in municipalities that were exposed to the spillover of an increased gun supply. They find that the increments were most marked in municipalities with a high degree of political competition in high drug trafficking areas.

Whatever the reason that violence grew substantially in some places and not in others, it is not obvious that trends on education enrollment would have caused and or

[2] The figure also shows a measure of "drug-related homicides" collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government. It includes all deaths by "presumed delinquent rivalry". Homicides are catalogued into "deaths by execution" (violent deaths where the deceased presumably belongs to a DTO, and where no government authority was involved), "deaths from confrontations" (violent deaths resulting from a confrontation between DTOs and official authorities), and "deaths by aggression" (deaths resulting from an assault on official government authorities). These data cover the period from December 2006 until June 2012.

predicted them. The suddenness and drastic nature of the violence spikes on violence in selected municipalities (which did not have particularly different trends in educational attainment) give us confidence in our methodology, which assumes that changes in violence are orthogonal to what the changes in education enrollment would have been.[3]

4 Data and Key Variables

4.1 Violence Indicators

We use the municipal homicide rate per 100,000 inhabitants as our measure of violence. Using homicides as our measure for violence has several advantages. First, homicides are a form of crime which is generally both violent and visible. Additionally, it does not suffer from reporting bias as other types of crime. Finally, homicides have been consistently reported at the municipality level. Homicide statistics were gathered from administrative data and cover the period from 1990 to 2011. They include all homicides at the municipality (*municipal*) level. Its source is the vital statistics registry from INEGI, the Mexican National Institute of Statistics and Geography.

An additional variable we use is the drug-related homicide rate. These data on total number of homicides at the municipal level comes from official figures made public by Mexico's Technical Secretariat of the National Public Security Council (SNSP), a federal entity dependent of the Ministry of Interior. This variable has the advantage that aims to be more closely related to "War on Drugs" spikes of violence that is the motivation of this study. However, this variable was created by individual officials' assessments of the relatedness to drug trafficking of the given homicide, and thus it is likely to have substantially more measurement error. Thus, although we include it in some models, we use the standard homicide rate variable for our main specification.

4.2 Demographic Characteristics at the Municipal Level

Demographic characteristics at the municipality level were obtained from the Censos de Población y Vivienda for 1990, 2000, 2005, and 2010, which include data on total population, share of rural population, share of population between 15 and 29 years of age, share of population over 60, median age, number of households, male to female ratio and fertility rate. For the years between censuses, we interpolated values assuming linear growth.

We have also gathered data on aggregate figures of public expenditures, government transfers, and other public finance variables at the municipal level in Mexico. The

[3]Figures 2 and 3 graphically show the diverse nature of the changes in violence in certain places.

data on public expenditures was obtained from the State and Municipal System of Databases (SIMBAD) produced by the National Institute of Statistics, Geography, and Information (INEGI).

4.3 Data on Education (schooling, enrollment rates)

The yearly education data from the first data set includes student enrollment (total students), passing rates, and retention rates at the municipality level. These data were obtained from INEGI and are consistently reported from 1994 to 2010. The variables are decomposed by the different levels of schooling, namely for *primaria* (primary school, grades 1-6), *secundaria* (lower secondary school, grades 7-9), and, *preparatoria* or *bachillerato* (upper-secondary school, grades 10-12). Historically in Mexico, grades 1-9 have been compulsory and as of constitutional changes made in 2013 grades 10-12 are now mandatory as well. The education variables considered in this data set include the total number of students for the aforementioned schooling levels (and the log of this number), the passing rate (# passing students / # total students), and the retention rate (# total students / # enrolled students).

While we know the total number of students at each school level, we do not have precise data on the corresponding school age population. Thus, we cannot calculate enrollment rates from this source with reliable precision. In order to include enrollment rates in the analysis, we use data from the Micro-Sample of the Census (2000 and 2010) and the interim census update, the *Conteo de Población* for 2005. We also use the quarterly *Encuesta Nacional de Ocupación y Empleo* (ENOE), which provides household level data of participation in schooling and workforce.

When we analyze the effects on the number of students enrolled in the municipality, we only do this at the level of basic and upper-secondary school only, and not at the college or beyond levels. This is because students are more likely to reside outside of the municipality where the university is located at the university level. Thus, the outcome variable (number of students enrolled in the municipality) may not be indicative of the schooling decisions of the population in question. However, the analysis using the Census and ENOE data do allow us to look at the effects on the education decisions of college-age individuals.

These data were merged with the homicides database, resulting in a panel with 2,457 counties followed over the 1994-2010 period, yielding a total of 41,769 observations.

The first data set we constructed is a large panel data set at the municipality level covering the period from 1994 to 2010. *Municipios* are the third-level administrative division in Mexico (the second-level being *Estados*, or States, and the first-level the Federal Government). There are 2,457 municipalities in the Mexican territory. This original data set was constructed from SIMBAD produced by INEGI and includes yearly education variables (from the official records of the Ministry of Public Education) as well as monthly crime variables and demographic characteristics for census years.

The second data set was constructed using data from the records of a standardized test applied to all students nationwide. These records allow us to follow schools over the 2009-2011 period. By merging these data with information from the administrative school census we are able to locate schools at the municipality level, and thus combine the education variables with the municipality violence data.

Our second data set allows us to look at student performance and school size. These data come from the ENLACE, *Evaluación Nacional del Logro Acadeémico en Centros Escolares* (National Evaluation of Academic Achievement in School Centers) results. ENLACE is a nationwide test applied to all students in *educación básica* (basic education which is composed of primary and lower secondary education, grades 1-9). We use the results from ENLACE 2009-2011.[4]

The data from these tests is reported at the student level, but due to the nature of the data it is impossible to follow a particular student through time. However, using administrative data from the School Census 2009 (*Formato 911*) we are able to match students to their schools and locate the municipality where these are located. This allows us to create a novel panel at the school and municipal level and use the aggregate test scores through time. Additionally, these data provide us with information about the school size and mean test scores. The database follows 78,830 primary schools and 25,989 lower secondary schools over three years.

5 Identification and Econometric Specification

There is huge variation in terms of the changes in violence across municipalities from the start of the "War on Drugs". Some municipalities with originally high homicide rates, saw a reduction or no change in the homicide rates (our primary proxy measure for violence, although some specifications also use drug-related homicide rates). Among those with originally low violence, some remained relatively peaceful while others saw the homicide rate explode.

This huge variation in the paths of homicide rates across municipalities allows us to have enough power to estimate very precisely the effect of violence (as proxied by homicide rates) on education enrollments. Given that these large municipality-specific changes are not likely to have been brought about by the small differences in the trends in educational attainment, it is unlikely that our fixed effects approach will suffer from

[4]The database constructed includes ENLACE 2008. However, the results from this year have been greatly questioned and invalidated. The results presented therefore only include ENLACE 2009-2011. Regressions including 2008 were also run, and results do not differ significantly.

reverse-causality bias.

We use a fixed effects model to assess whether greater exposure to violence has had an impact on human capital accumulation at the municipality level. The fixed effects models that we estimate are of the form:

$$y_{it} = \alpha_i + \beta_t + \gamma homicides_{it} + \varepsilon_{it} \tag{1}$$

where y_{it} represents the educational variable of interest, α_i a set of municipality fixed effects, β_t a set of year fixed effects and $homicides_{it}$ the homicide rate per 100,000 inhabitants for municipality (or a normalized transformation of the homicide rate) i at time t. The municipality-year specific error term is given by ε_{it}. The coefficient of interest is γ. All standard errors are clustered at the municipality level.

The education variables in our first set of specifications are the log of one plus the total number of students at different education levels for each municipality over the period 1994-2010. They are reported separately for the different school levels, which allows us to run our regressions separately for primary, lower secondary, and upper-secondary. This approach is useful as it allows us to explore whether violence is affecting education in different ways according to school level. Running the specification presented in Equation 1 separately for each school level would allow us to get some preliminary suggestive evidence about the effect of violence on education.

We also run the fixed effects model for the education variables from our second data set which was constructed using the information from ENLACE. When using this data set, we can aggregate the student information to the (i) municipality level, or (ii) school level. When using the data aggregated at the school level we include school fixed effects as opposed to municipality fixed effects. The homicide variable corresponds to the annualized homicide rate per 100,000 inhabitants, normalized according to the national homicide rate per 100,000 inhabitants in 2007 (general homicide rate of 8.3 and drug related homicide rate of 2.6 $\times$100). All regressions are run with clustered standard errors at the municipality level.[5]

Including municipality fixed effects removes all observed and unobserved municipality characteristics that are constant in time, thus removing the bias in the estimation that is caused by municipality-invariant characteristics. In particular, if certain municipality characteristics are correlated with both an increase in violence and a decrease in schooling, a model without fixed effects would yield a downward biased estimator

[5] Oaxaca is excluded from the analysis using ENLACE data. The education data for the state of Oaxaca is known to be unreliable and incomplete. Oaxaca is a highly rural state, and although it only accounts for less than 3.4% of the national population (Census, 2010) it is divided into 571 counties (23.2% of the total number of counties). Oaxaca has not experienced a particularly high increase in violence as of 2006. The state is excluded from our econometric analysis, although the regressions were also run including the state of Oaxaca and results are robust.

of the impact of homicides on education. By including municipality fixed effects—and assuming that the trends of these characteristics are time invariant—the bias would be eliminated. The yearly fixed effects eliminate biases that may be occurring at the national level for any given year.

By including municipality and year fixed effects, we are effectively controlling for factors that are constant in time for any given municipality and constant across counties for any given year. The underlying assumption for this model to be correctly specified is that there are no omitted time-varying municipality specific characteristics that are correlated with our violence variables.

One potential mechanism of an impact of violence on total enrollments in the municipality schools is migration. If those municipalities most affected by increasing violence presented a change in migration patterns (assuming an increase in migration, which would imply a negative effect on population) this would cause a decrease in the number of students.

To test this, we first run regressions of the form specified in Equation 1 including municipality and year fixed effects but use total population and other demographic municipality characteristics as our dependent variable y_{it}. By using demographic characteristics as our dependent variables we analyze whether the homicide rate appears to be affecting total population and its composition.

Second, we use age specific enrollment rates calculated from Census and a labor force survey (the *Encuesta Nacional de Ocupación y Empleo*, ENOE) to test whether the homicide-rate has affected the share of individuals attending school, as opposed to the actual number of students. This empirical strategy allows us to address migration concerns; to the extent that violence-induced migration is not selective on propensity to education, migration would not affect this result. Of course, it is quite possible that certain types of families are more likely to migrate (i.e. high socio-economic status families). However, as we will see, we find that there is no impact of violence on enrollment rates, so it seems that this is not the case.

Other potential mechanism that could confound the effects of violence on enrollment rates at the municipality level is the potential reinsertion into school of former labor force participants who lost their employment or their desire to find a job due to the negative effects of violence in the local economy, as this would cause either higher enrollment rates or higher rates of idleness. Previous studies for Mexico that include Arias-Vazquez and Esquivel (2013), Velásquez (2014) and Dell (2015) show that drug-related homicides increased unemployment and labor force participation, particularly for women. However, these authors only study the effects at the mean and do not provide estimates of potential differentiated effects of violence on labor market outcomes by age group, and thus this effect may not be driven by the school aged population.

We test whether this phenomenon may be causing an increase in enrollment rates.

To test for these potential confounding effects, we estimate the model defined in Equation 1 (with municipality and year fixed effects) using as our dependent variable y_{it}, first, the employment rate, and second, the rate of idle-youth, those in the 15 to 24 age range who are neither in school nor in the labor market. We further divide these rates by gender and also by smaller groups (12-14, 15-17, 18-20 and 21-24 years of age) to identify if any trends correspond to changes in enrollment rates in lower- and upper-secondary school. By using idle-youth and employment rates as our dependent variables we additionally test if violence affected the employment or idleness decisions of the school-aged population.

6 Results

6.1 Impact of Violence on Total Enrollment

Table 1 shows that there has been a small impact on the number of students enrolled in basic and upper-secondary schools in Mexico as a result of the increase in violence. An increase in the homicide rate is associated with decreases in the number of students in the district (*municipio*).

In order to clearly analyze the magnitude of the effect, we normalized the independent variable of interest, namely the homicide rate, so that increases of one represent the average increase in the homicide rate of the country. Through this linear transformation (dividing the homicide rate by the country-level increase in the homicide rate between 2007 and 2010), we can interpret the coefficients as representing the impact of an increase in violence comparable to the one experienced by Mexico as a whole.

The first four columns present the results when we do not include fixed effects for municipality and year. Thus, these may not be interpreted as difference-in-difference results and are driven by both the cross-sectional and longitudinal changes in homicide-rates. The first column shows the impact on students of basic school (grades 1 to 9 of formal schooling). The coefficient of -1.00 would be interpreted as showing that a municipality that experienced a typical increase in homicide rates (i.e. equal to the average of the country) would observe a 1% reduction in the number of children enrolled in basic education. This number arises from similar reductions in primary school students (0.95%—column 2—) as secondary school students (1%—column 3). Column 4 shows the effect on the number of upper-secondary school students (grades 10 to 12). The effect on that group is somewhat larger at 1.5%.

These effects, though may be seen as small in economic magnitude, are statistically significant. Some may even argue that these impacts are of substantial economic significance given the large importance of education on labor market earnings (i.e. Card 1999).

A different picture emerges once we control for year and municipality fixed effects, which is our preferred empirical strategy. In this manner, we are effectively comparing the changes in the number of students enrolled in the municipality against the changes in the homicide rate. Columns 5 to 8 present these results. The effect for basic school disappears. The coefficient is not only small (less than 0.001 percent) but also quite accurately estimated. We can reject, for example, the hypothesis that the effect is larger than 0.06%. Zooming in to the impacts on primary (grades 1 to 6) or secondary (7 to 9 grades) we find the same null or miniscule effects.

The results for upper-secondary school, however, are different. The coefficient for that regression (column 8) is not insignificant in a statistical sense (at least not at the 10% level). The 0.3% negative effect of the homicide rate is still much smaller than what we obtain when we do not control for state and fixed effects, and we can reject a negative impact larger than 0.5%.

The results presented in columns 9 to 16 rely solely on the period after 2007. We present these because one may hypothesize that the effects are different in the "War on Drugs" period since the issue became much more prominent. In addition, it allows us to use the "Drug Related Homicide Rate" variable in columns 13 to 16. The results are qualitatively similar as before. Although coefficients are generally of the expected sign, (i.e. consistent with a reduction of enrollments when violence is higher), the magnitudes are very small and statistically insignificant. In these cases, in addition, even when the dependent variable is the number of upper-secondary school students, the coefficients are statistically undistinguishable from zero.

We present a graphical analysis of this in the figures below, to allow the reader to visualize the magnitude of these effects. In Figure 4 Panel A, we present the evolution of upper-secondary school total enrollments (using an index where the value for 2007 is set to 100), for municipalities where violence has greatly increased and for municipalities where violence has stayed relatively constant. For this purpose, municipalities were divided into "high violence increase" and "low change in violence" according to their homicide rate trend from 2007 to 2011. Counties classified as those where violence increased (471 totals) represent 24.9% of our sample while counties with stable violence levels (945 totals) represent 51% of our sample. The fitted values correspond to the estimated time trend for the 2005-2007 period (using an extrapolation of a linear trend). We can see that while the enrollment for the counties less affected by the increase in violence stayed at levels close to the previous trend, for those counties most affected by the upsurge in violence, the number of enrolled students seems to divert, though only slightly, from the previous trend. Figure 4 Panel B aims to make this clearer by showing the residuals between actual enrollment and the enrollment as predicted by the 2000-2007 trend. These graphs show the residuals for counties with a low change in homicide rates, oscillating around zero for the whole period, while the residuals for counties with a high violence increase descending after 2007. After 2007, in all but one 2008 quarter, the actual number of upper-secondary students remains below the trend.

Although the difference-in-difference approach used in the regressions reported in Table 1 allows us to disentangle the effect of violence from cofounders that are either fixed in time or change at the same pace in all the country, it does not allow us to disentangle from cofounders that change at the municipality level. Table A.1 in the Appendix also shows that these results are not being driven by confounders that vary at the state level, by presenting the results of regressions with the addition of state-by-year dummy variables.

Given that our finding from the effects shown in Table 1 is that there is at most a small impact in the number of students enrolled in a municipality, we would like to control for variables that may be biasing the effect downwards. One potential issue is that governments (at the federal, state or municipality level) may be responding to the increases in violence by augmenting spending in social services. Such higher level in social spending could increase enrollments if, for example, it includes construction of new schools or improvement in roads that lead to schools. This, in fact, has been occurring as evidence of the recently launched National Program for the Social Prevention of Violence and Crime (*Programa Nacional para la Prevención Social de la Violencia y la Delincuencia*) headed by the Ministry of Interior with a total budget of 118,801 million MX$ to implement programs and actions to reduce violence in metropolitan areas as well as in rural areas.

In order to account for this, we estimated a set of models that include public expenditure variables. Table 2 shows pairs of results, the first of which does not include the public expenditure variables and one that does. For example, column one shows the same result as the one presented in the previous table in the fifth column; while column 2 presents the result of the same regression but including the level of public expenditure in the municipality.

Even though the effect of public expenditures is strongly significant on the number of enrolled students, none of the violence coefficients change in any considerable degree. The coefficient for the logarithm of the number of students of basic education in column 2 (and primary and secondary students separately —columns 4 and 6 respectively—), remains statistically insignificant and of roughly the same magnitude. The statistically significant effect for the number of upper-secondary school students remain significant (and now at the 5% level), but its magnitude remains small (0.32% reduction instead of 0.29%).

We estimated additional models with different sets of municipal-time varying variables. In none of these models was there a substantially different result (these results are presented in Table A.2 in the Appendix). Thus, the takeaway remains that there seems to have been no or very small effect on the number of students at the basic-school level, and a small effect on the number of upper-secondary school students.

Furthermore, we estimate our preferred model which includes municipality and year fixed effects to identify if there are any lagged effects of homicide rates on the number of students enrolled in school (these results are included in the Table A.3 in the Appendix). As in the case of contemporaneous homicide rates, the estimation with homicide rates that correspond to the previous school year has no effect on primary and lower-secondary school, and it has a negative but small effect on upper-secondary school of about 0.47%. An interesting case is the model that includes as independent variable the two-period lagged homicide rates. The results of this estimation find a statistically significant negative effect of homicide rates on the number or students enrolled in all school levels, however these are very small, specifically, 0.1% for basic education, and 0.3% for upper-secondary school. Notably, the small negative effect for upper-secondary school persists over time.

6.2 Impact of Violence on Enrollment Rates

Albeit small, the effect on the number of students (weakly found at the upper-secondary school level) may be a consequence of would-be students in the municipality moving to a different area. To the extent that families with school-aged children flee the areas that are more aggravated, we would expect to see an increase in the number of enrolled students in relatively safer areas and a small reduction in the number of enrolled students in areas getting more violence. This does not necessarily imply, however, that there is an effect on student decisions but rather that there is migration of students.

In order to test this, we use household-survey data where we can analyze, not what is the impact of violence on the number of students enrolled, but on what share of children of a certain age group is enrolled.

We ran separate regressions for different age groups so that they match the schooling levels tested in the previous tables. For each set of regressions, we present results for 6-11 year olds (roughly the age of primary school children), 12-14 year olds (secondary school age) and 15-17 year olds (upper-secondary school age). In addition, using data from the household level allows us to look at the decisions of individuals of ages higher than typical high school students. Therefore, we can test whether increasing violence reduces the likelihood of a college-age youth being enrolled. For this purpose, we include two regressions, one for 18-20 year olds, and 21-24 year olds; both groups fall within common age-range of college-level students.

Because these regressions are using Census (and Conteo) data, we can only use data for 2000, 2005 and 2010. Columns 1-5 of Table 3 show the regressions that include data from 2000, 2005 and 2010, while columns 6-15 show 2005-2010 (which is the closest we can get to the 2007-2010 comparisons made in analysis form previous tables). From Column 11 onwards, we use the "drug-related homicide rate" variable. However, this variable only starts in 2007. Thus, regressions in columns 11-15 assign the violence variable of 2007 to the enrollment data of 2005. Though this is not ideal, it is the best that can be done with the available data.

The results again do not show an effect. Among the 15 regressions, only one yields a statistically significant result at the 1% level of significance. This result is for the effect of homicide rates on the enrollment of children ages 6-11 and, though significant, is extremely small (a reduction of the share of enrolled children of about 0.00002 for a municipality that suffers an increase in violence of the magnitude of that experienced in Mexico from 2007 to 2010). Furthermore, that result disappears when we use only data from 2005 and later.

The result that had been found to be more robust in terms of the number of high school students does not have its counterpoint in terms of an effect on the enrollment rates of 15-17 year olds. This pattern of results is consistent with a null effect on education enrollments, but with a small effect on migration away from increasingly violent areas. Interestingly, there is also no effect on the enrollment rates of college-age young adults (18-20 and 21-23).

These data allow us to separate regression results for male and female students. One of the hypotheses is that more crime attracts young men to participate in the lucrative but illicit activity, and could thus incentivize them to leave school. To the extent that crime does not attract young women in the same degree, we would expect the effect to exist for men but not for women. On the other hand, if it was the case that women are more vulnerable and feel that going to school exposes them to more risks, we could expect there to be an effect for female but not for male.

The results shown in Table 4 are very similar for each gender. There is only one statistically significant result in any of the regressions, and there is no result for which the magnitude of the coefficient is major.

One potential concern with the results reported in Tables 3 and 4 is that it only uses data from two or three points in time (2000, 2005 and 2010). In Table 5, we estimate the same regressions but instead use the ENOE which provides quarterly data. We focus on the 2006-2010 period.

The results provide further confirmation to the finding described above: namely, that there was no effect on enrollment rates. The coefficients are all small, and most are not statistically significant. The exceptions are the outcomes for the 15-17 year old groups, which correspond to the typical age range of upper-secondary school, where we found the small but significant impact on the number of students (Tables 1 and 2). However, the effect goes on the opposite direction than expected. This gives further strength to the interpretation that the small effect of violence on the number of enrolled students, even if it exists, is likely not a result of individual schooling decisions. In the following sections, we present some evidence that there has been some migration resulting from the increases in homicide rates. Such migration could explain the coexistence of a small effect on the number of students enrolled in the municipality and the lack of an effect on enrollment rates.

6.3 Effects on Migration

In order to test the hypothesis that violence did not affect education decisions but affected the location of students, we carry out the following analyses that show that violence affected the residence of individuals.

The models estimated are similar to the ones shown above, but have as dependent variable the number of individuals in the population group living in the municipality. Table 6 shows the coefficients of homicide rates (and drug-related homicide rate) on the logarithm of the total number of people resident in the municipality. Each row shows the result of a separate regression where the dependent variable refers to a different population group (the first row shows the coefficient when the dependent variable is total population, the second and third show the regressions when the dependent variables are total male and total female population; from the fourth onwards show the results by age group). The first two columns show the 2000 to 2010 results (the first one is the bare-bones differences-in-difference approach, whereas the second one adds time-varying public expenditure controls). The last four columns restrict to the 2005 and 2010 data points, and alternate the "homicide rate" and "drug-related homicide rate" variables.

The regressions for total population show a strongly significant impact on total population. On average, a municipality suffering an increase in its homicide rate of 8.3 per 100,000 inhabitants would experience a reduction of 0.17% in its population according to the 2000-2010 specifications (columns 1 and 2). Columns 3 and 4 show the results for the 2005-2010 specifications and though the magnitude is somewhat smaller, they are not statistically different to those on columns 1 and 2. The corresponding results using the variable "drug-related homicide rate", however, do show a substantially smaller impact. This may be a result of a higher measurement error in that variable, which would bias the coefficients towards zero. In any case, all specifications are consistent with there being outmigration for increasingly more violent municipalities and towards relatively safer ones. This result is consistent with Ríos (2014), who estimates that a total of 264,693 individuals have migrated fearing organized crime activities in Mexico between 2005 and 2010. In addition, that study presents anecdotal evidence whereby a significant number of these migrants migrated from more violent municipalities to cities with lower levels of violence.

Rows two and three show nearly identical effects for the male and female populations, a result that is perhaps not surprising if families are moving entirely. All population groups show a statistically significant effect of the expected direction. Interestingly, however, upper-secondary-age children (as well as college age youth) seem more likely to be relocated than basic-school aged children as a response to spikes in homicide rates. In the first column, the coefficient for 15-17 year olds, equals -0.22, about twice as high as the coefficient for 6-14 year olds (-0.11). Although there are slight variations in those coefficients across the specifications (different columns in the table), in all cases the coefficient for 15-17 year olds is at least double that of 6-14 year olds.

The larger effect on migration of 15-17 year olds than 6-14 year olds is also consistent with our result that there was an effect on the number of upper-secondary school and not on the number of basic school students, while there was a zero effect on enrollment rates in both groups.

6.4 Effects on the Labor Market

Previous studies on the effects of violence on labor market outcomes for Mexico found negative effects on employment and labor force participation, particularly of women (Arias-Vazquez and Esquivel (2013); Robles, Calderón and Magaloni (2013); Velásquez (2014); Dell (2015); and Enamorado, López-Calva and Rodríguez-Castelán (2014)). Thus, a potential channel that could act in the opposite direction of lower enrollment rates due to higher levels of violence is the re-enrollment into school of discouraged workers. Under the assumption that this hypothesis is valid, then the small/null effect on enrollment/enrollment rates could be a result of two counteracting forces (less willingness to go to school on the one hand, but a larger out of work population who could potentially enroll). This would downward bias our previous results and would thus suggest that there is in fact a negative effect of homicides on enrollment rates.

An important point to notice is that previous studies that found negative effects of violence on employment and labor force participation only estimated such an effect at the mean and by gender. They find that both employment and labor force participation declined for women while it did not change significantly for men. However, they do not study potential differentiated effects of violence on labor outcomes by age group, and thus the effect they find may not be driven by school aged population. Next, using the

ENOE data, we test the hypothesis that violence affected the employment or idleness decisions of school-aged population. To do so, we divide the population between 12 and 24 into four groups that may correspond to students in lower-secondary and upper-secondary education (12-14, 15-17, 18-20 and 21-24). We also divide the sample by gender to test whether there are differentiated results for women.

Table 7 Panel A estimates the regression of homicide rates on employment rates of men using the ENOE. The coefficients are all small, and most are not statistically significant, similar to what other studies have predicted. The exception is the outcome for drug-related homicides and employment rate for men between 18 and 20 years old, which correspond to the age range in the last year of upper-secondary school. Table 7 Panel B presents the results of the model of the effects of violence on employment rates of women. Differently to Arias-Vazquez and Esquivel (2013), our estimates show no statistically significant effects of both homicides and drug-related homicides on employment rates of school-aged women.

Moreover, to test if the results we observe on enrollment rates are explained by idleness caused by violence, we present a model that regresses homicide rates on the share of young individuals that are out of both school and work. More precisely, we test whether there is an effect on the NEET rate ("Not in Education, Employment or Training", NEET) for those in the 15 to 24 age range. Table 8 Panel A and Panel B present the estimates of the effects of violence on the rates of idle-youth divide by gender and by four groups that may correspond to students in lower-secondary and upper-secondary education (12-14, 15-17, 18-20 and 21-24). As in the case of employment rates, we do not find any significant effect of homicide rates (including drug-related homicides) on NEET rates. Thus, all together, the outcomes of violence on employment and youth NEET rates (presented in Tables 7 and 8) further validate our results that education decisions are not the channel that explain the small negative effect of violence on the number of enrolled students.

6.5 Impacts on Educational Achievement: Inputs and Learning

Educational attainment is not the only potential educational consequence of violence. It is possible to hypothesize that although children are going to school at the same rates, there is a lower or higher quality of education being provided to them. Alternatively, one could think that due to stress, even the same quality of education produces lower levels of learning. In that case, we would expect to see an impact on learning outcomes and cognitive ability.

We cannot directly test the effects on education quality. However, we can look at the effect on inputs. Table 9 shows the differences-in-difference estimate on the impact of homicide rates (and "drug-related homicide rates") on the number of teachers in the municipality (columns 1 to 5); and on the number of schools (columns 6 to 10). We do not find any significant impact, although the coefficients are less precisely estimated than in the case of students and enrollment rates. The largest significant coefficient appears in column 1 and it is rather small at 0.6%. However, this is the least preferred specification as it does not include fixed effects.

The effect on the number of teachers could be explained by either teachers moving out of crime-ridden areas, or through the changes in demand for schooling services. Interestingly, there is also a (very small) effect in the number of schools. This could be driven by demand due to the migration of students, which may cause fewer school openings in places where outmigration is commonplace. Alternatively, it could also be driven by extortion to private schools (in fact, some businesses have closed in response to DTO gangs requiring private businesses to pay them in exchange for protection).

Table 10 looks at the impact of homicide rates on learning, as measured by the Enlace test. We do not find a statistically significant impact on learning. Two things are important to note, however. First is that the results are much less precisely estimated. Second is that the reported migration changes the selection of students who take the test, so that even tighter estimated results would not be easy to interpret without knowing the test scores of those who migrate.

The results do not point to large changes in learning as a result of increases of violence. However, given that these results are less tightly estimated, and the selection issues mentioned above, we interpret the result of no effect on learning with more care than the effect on enrollment, and we recognize the possibility that there is a significant impact on learning that we are unable to uncover.

7 Conclusions

Based on a growing literature that documents the negative impacts of violence (for example, (Arias-Vazquez and Esquivel (2013), Robles, Calderón and Magaloni (2013), Velásquez (2014), andDell (2015)), one could expect a negative impact on education attainment. However, we find no such detrimental effect of homicide rates on education enrollments. The wealth of available data, and the large within-municipality variability in homicide rates, allows us to estimate very precisely a null or small impact on the number of students enrolled. Furthermore, we find that the school enrollment in affected municipalities goes down because of an effect of violence on migration out of those municipalities (and immigration into safer ones), and not because of a direct effect on individuals' schooling decisions.

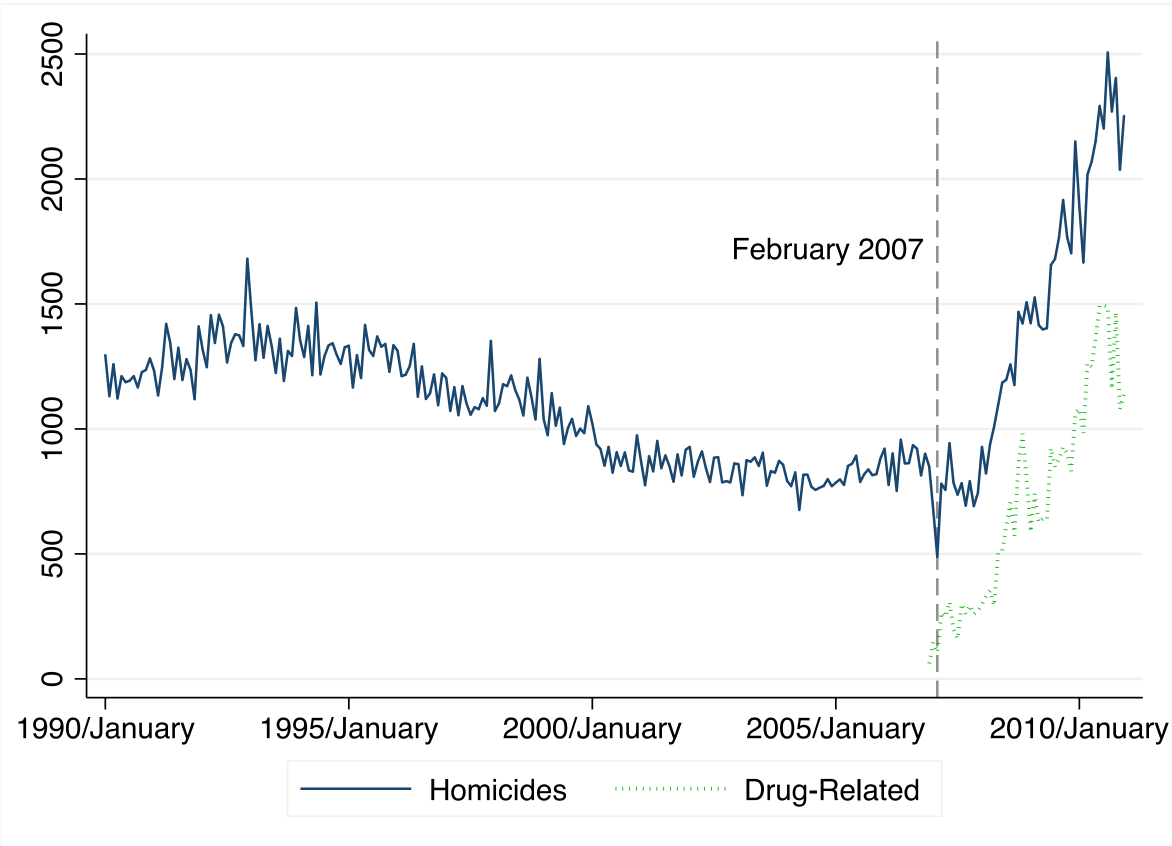
Preparatory is the only school-level where we found an effect, albeit very small and not robust, on the number of students enrolled. However, even this result seems to arise from migration of would-be students from municipalities that are suffering increases in violence to safer places. When we estimate models on enrollment rates, we find no effect of violence. Increases in homicide rates are associated with some migration out of the municipalities that experienced more severe increases in violence, particularly of families with upper-secondary school aged (and college aged) children.

We cannot rule out effects of violence on other measures of human capital formation, such as learning. The effects of violence on migration would caution against causally interpreting small changes in mean test scores, since the composition of students and test takers is also affected.

Our findings show that educational decisions of families have been robust to the increase of violence. They stand in contrast with recent evidence of the negative effects of violence on short-term economic growth, since minimal to null effects of violence on human capital accumulation today should have little to no adverse effects on long-term growth outcomes in Mexico.

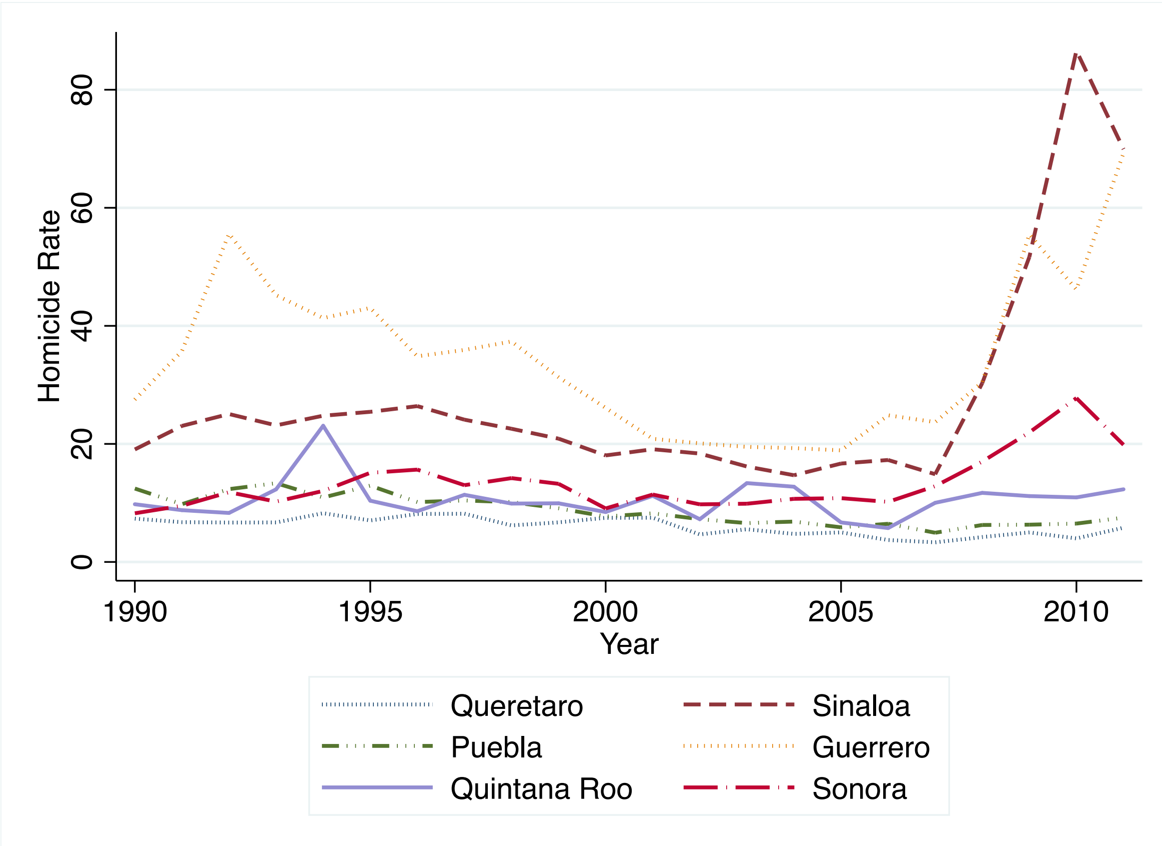
8 Figures and Tables

Figure 1: Total Number of Homicides. Monthly Data, 1990-2010



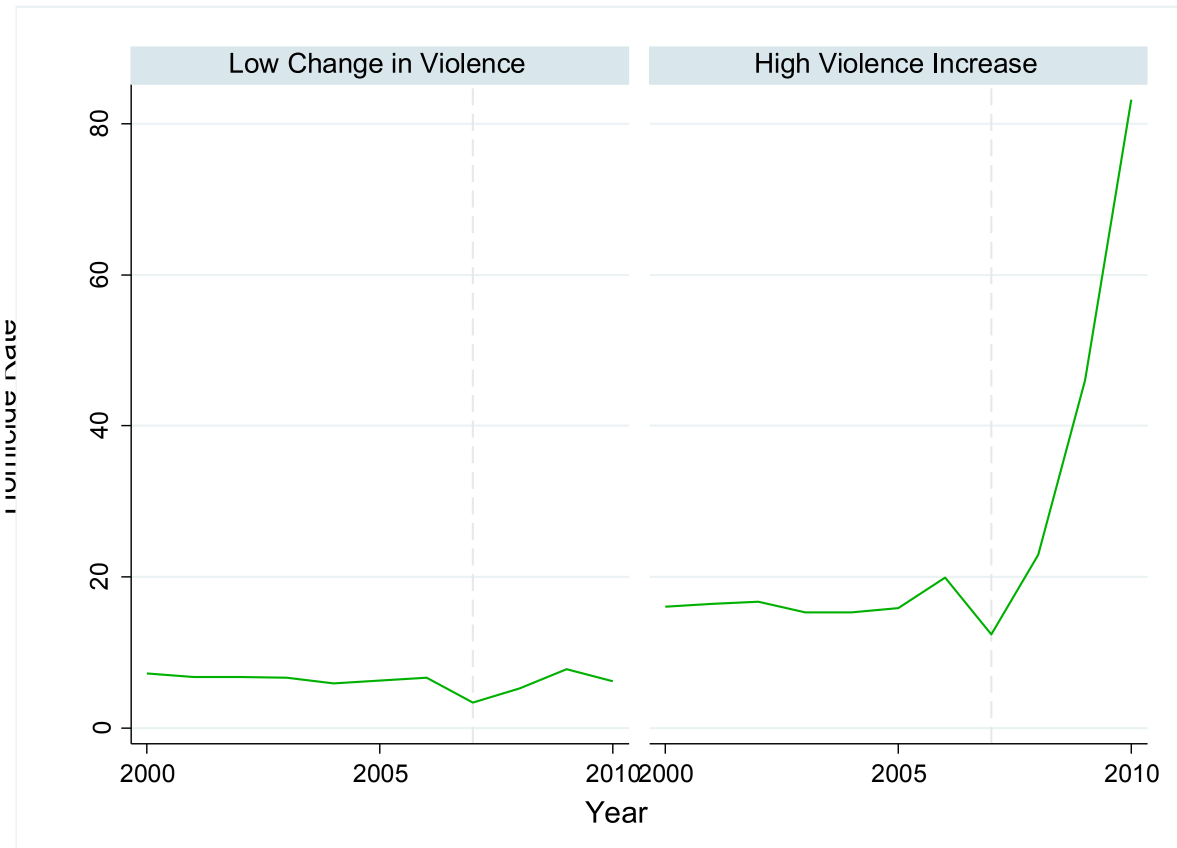
Source: INEGI (2014), SNSP, 2011, 2012 and 2014

Figure 2: Homicide Rate Evolution. Selected States



Source: INEGI (2014)

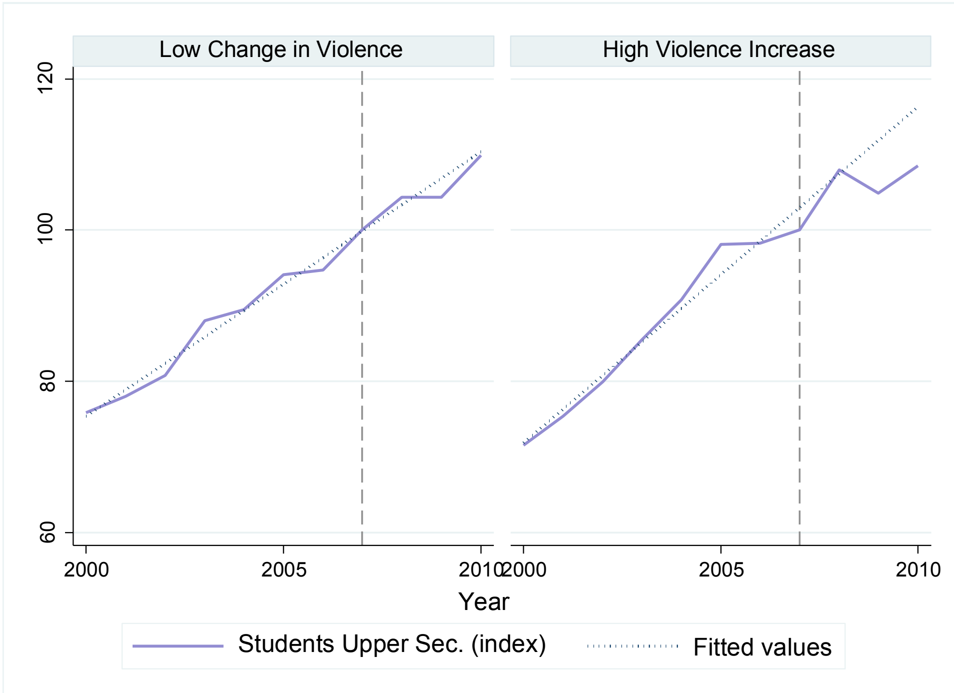
Figure 3: Mean Homicide Rate for Selected Municipalities



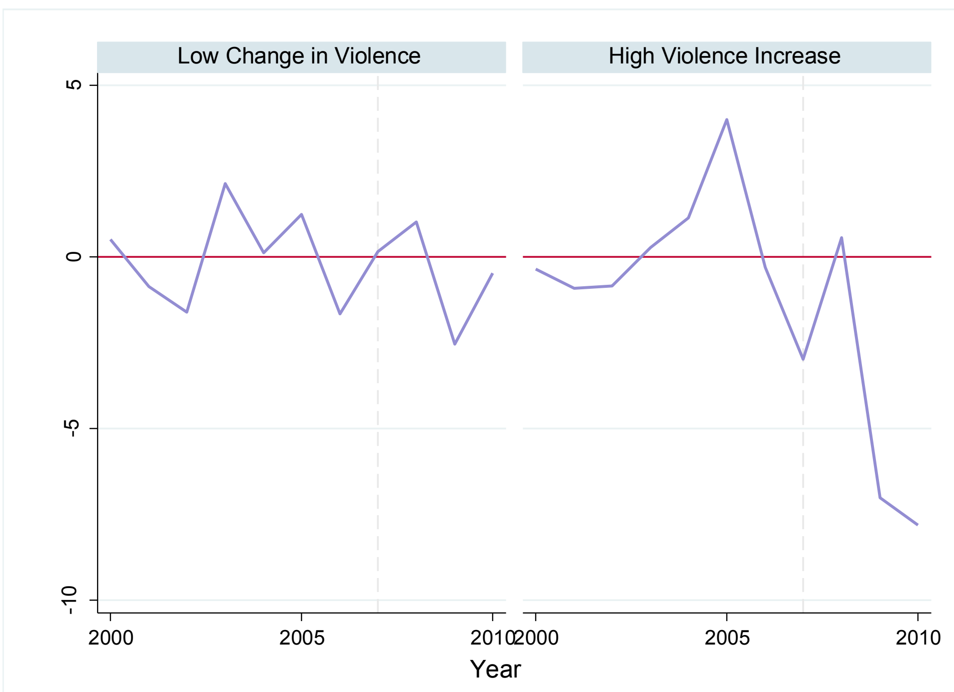
Source: INEGI (2014)
Note: In this graph, municipalities are divided into "high violence increase" and "low change in violence" according to their homicide rate trend percentile from 2007 to 2011. Municipalities classified as violent-increasing (471 total) represent 24.9% of our sample while non-violent municipalities (945 totals) represent 51% of our sample.

Figure 4: Total Enrollment in upper-secondary School in Municipalities with Low and High Changes in Violence

Panel A: Trends for upper-secondary School Enrollment (index 2000=100)



Panel B: upper-secondary School Total Enrollments, Residuals from Trend



Source: INEGI (2014)

Note: In this graph, municipalities are divided into "high violence increase" and "low change in violence" according to their homicide rate trend percentile from 2007 to 2011. Municipalities classified as violent-increasing (471 total) represent 24.9% of our sample while non-violent municipalities (945 totals) represent 51% of our sample.

Table 1: The impact of homicide rates on the number of enrolled students

	2000-2010								2007-2010							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Homicide Rate	-1.00***	-0.95***	-1.04***	-1.55***	0.00	0.02	-0.05	-0.29*	-0.00	0.02	-0.04	-0.05				
	(0.30)	(0.31)	(0.32)	(0.59)	(0.03)	(0.04)	(0.04)	(0.15)	(0.03)	(0.03)	(0.06)	(0.09)				
Drug Hom. Rate													-0.01	-0.01	-0.02	-0.02
													(0.01)	(0.01)	(0.01)	(0.04)
Mean dept. var.	7.881	7.574	6.478	4.869	7.881	7.574	6.478	4.869	7.772	7.445	6.423	5.088	7.771	7.444	6.423	5.087
Observations	25,194	25,195	25,208	25,041	25,194	25,195	25,208	25,041	9,807	9,807	9,807	9,703	9,811	9,811	9,811	9,707
R-squared	0.001	0.001	0.001	0.001	0.985	0.984	0.983	0.936	0.998	0.997	0.991	0.967	0.996	0.995	0.990	0.966
Fixed Effexts	no	no	no	no	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Edu. Level	Basic	Prim.	Sec.	Prep.	Basic	Prim.	Sec.	Prep.	Basic	Prim.	Sec.	Prep.	Basic	Prim.	Sec.	Prep.

Notes: Dependent variable is the logarithm of county total students +1 (INEGI) by school level. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by "presumed delinquent rivalry". Standard errors clustered at county level in parentheses. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Table 2: The impact of homicide rates on the number of enrolled students, controlling for public expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homicide Rate	0.005	0.005	0.019	0.021	-0.047	-0.047	-0.291*	-0.321**
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.15)	(0.16)
Public expenditure		0.002***		0.003***		0.001***		-0.008***
		(0.00)		(0.00)		(0.00)		(0.00)
Mean dept. var.	7.881	7.921	7.574	7.614	6.478	6.525	4.869	4.945
Observations	25,194	23,494	25,195	23,495	25,208	23,508	25,041	23,348
R-squared	0.985	0.984	0.984	0.983	0.983	0.984	0.936	0.934
Edu. Level	Basic	Basic	Prim	Prim	Sec	Sec	Prep	Prep

Notes: All regressions include municipality and year fixed effects. Dependent variable is the logarithm of county total students +1 (INEGI) by school level. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by "presumed delinquent rivalry". Controls in Million MX$. Standard errors clustered at county level in parentheses.
$^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Table 3: The impact of homicide rates enrollment rates using Census Data

	2000-2010					2005-2010									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Homicide Rate	-0.02***	-0.00	0.02	-0.01	-0.00	-0.01	0.02	0.07	-0.01	-0.00					
	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.06)	(0.04)	(0.03)					
Drug Hom. Rate											-0.00	0.01	0.03*	-0.00	0.00
											(0.00)	(0.01)	(0.02)	(0.01)	(0.01)
Mean dept. var.	0.954	0.864	0.539	0.227	0.095	0.961	0.889	0.587	0.257	0.108	0.961	0.889	0.587	0.257	0.108
Observations	7,350	7,348	7,342	7,337	7,333	4,908	4,906	4,900	4,895	4,891	4,908	4,906	4,900	4,895	4,891
R-squared	0.665	0.740	0.798	0.797	0.750	0.733	0.784	0.826	0.833	0.804	0.733	0.784	0.826	0.833	0.804
Age	6-11	12-14	15-17	18-20	21-24	6-11	12-14	15-17	18-20	21-24	6-11	12-14	15-17	18-20	21-24

Notes: Dependent variable is the the enrollment rate by age group, calculated from census data. All regressions include municipality and year fixed effects. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by "presumed delinquent rivalry". Data from years 2000, 2005 and 2010 included in columns (1) to (5) , and for years 2005 and 2010 in columns (6) to (15). Standard errors clustered at county level in parentheses.
$^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table 4: The Impact of Violence on Enrollment Rates by Gender Using Census Data

Panel A: Men

	2000-2010					2005-2010									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Homicide Rate	-0.023**	0.015	0.040	0.004	-0.005	-0.013	0.057	0.088	-0.008	-0.006					
	(0.01)	(0.03)	(0.04)	(0.03)	(0.03)	(0.01)	(0.05)	(0.07)	(0.05)	(0.06)					
Drug Hom. Rate											0.000	0.021*	0.037*	-0.003	0.010
											(0.00)	(0.01)	(0.02)	(0.01)	(0.01)
Mean dept. var.	0.953	0.871	0.541	0.232	0.099	0.959	0.890	0.581	0.259	0.113	0.959	0.890	0.581	0.259	0.113
Observations	7,341	7,331	7,318	7,307	7,288	4,899	4,889	4,876	4,865	4,847	4,899	4,889	4,876	4,865	4,847
R-squared	0.595	0.656	0.727	0.725	0.692	0.675	0.726	0.776	0.779	0.754	0.675	0.726	0.776	0.779	0.754
Age	6-11	12-14	15-17	18-20	21-24	6-11	12-14	15-17	18-20	21-24	6-11	12-14	15-17	18-20	21-24

Panel B: Women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Homicide Rate	-0.023*	-0.013	-0.008	-0.016	-0.003	-0.014	0.012	0.026	-0.011	-0.009					
	(0.01)	(0.02)	(0.05)	(0.03)	(0.02)	(0.02)	(0.04)	(0.08)	(0.06)	(0.04)					
Drug Hom. Rate											-0.007	-0.002	0.012	-0.005	-0.007
											(0.00)	(0.01)	(0.02)	(0.01)	(0.01)
Mean dept. var.	0.955	0.856	0.536	0.223	0.091	0.962	0.888	0.592	0.256	0.104	0.962	0.888	0.592	0.256	0.104
Observations	7,346	7,325	7,324	7,306	7,313	4,904	4,883	4,882	4,865	4,871	4,904	4,883	4,882	4,865	4,871
R-squared	0.615	0.698	0.768	0.746	0.677	0.668	0.719	0.792	0.787	0.744	0.668	0.719	0.792	0.787	0.744
Age	6-11	12-14	15-17	18-20	21-24	6-11	12-14	15-17	18-20	21-24	6-11	12-14	15-17	18-20	21-24

Notes: Dependent variable is the the enrollment rate by age group, calculated from census data. All regressions include municipality and year fixed effects. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by “presumed delinquent rivalry”. Data from years 2000, 2005 and 2010 included in columns (1) to (5) , and for years 2005 and 2010 in columns (6) to (15). Standard errors clustered at county level in parentheses.
$^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Table 5: The impact of homicide rates on enrollment rates using the ENOE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Homicide Rate	0.07	0.45**	-0.02	-0.01	0.08	0.45*	-0.03	-0.01				
	(0.17)	(0.23)	(0.23)	(0.08)	(0.18)	(0.23)	(0.23)	(0.08)				
Public expenditure					-0.00	-0.00	-0.00	0.00				
					(0.00)	(0.00)	(0.00)	(0.00)				
Drug Hom. Rate									0.03	0.14*	-0.06	-0.04
									(0.06)	(0.08)	(0.07)	(0.02)
Mean dept. var.	0.824	0.541	0.253	0.092	0.824	0.538	0.250	0.091	0.824	0.541	0.253	0.092
Observations	5,248	5,247	5,175	5,208	4,983	4,980	4,914	4,949	5,248	5,247	5,175	5,208
R-squared	0.416	0.477	0.476	0.438	0.425	0.478	0.477	0.435	0.416	0.477	0.477	0.438
Controls	no	no	no	no	yes	yes	yes	yes	no	no	no	no
Age Group	12-14	15-17	18-20	21-24	12-14	15-17	18-20	21-24	12-14	15-17	18-20	21-24

Notes: Dependent variable is the the enrollment rate by age group, calculated from ENOE data. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by "presumed delinquent rivalry". All regressions include municipality and year fixed effects. Data available from 2006 to 2010. Standard errors clustered at county level in parentheses. Controls in Million MX$. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 6: The Impact of Violence on the Number of Residents in Municipalities

	2000-2010		2005-2010			
	Hom. Rate	Hom. Rate	Hom. Rate	Hom. Rate	Drug H.R.	Drug H.R.
Dep. variable: Log Pop.						
Total Pop.	-0.166***	-0.168***	-0.131**	-0.131**	-0.0535***	-0.0536***
	(0.0495)	(0.0499)	(0.0556)	(0.0558)	(0.0171)	(0.0171)
Total Male Pop.	-0.167***	-0.168***	-0.139**	-0.139**	-0.0563***	-0.0564***
	(0.0497)	(0.0500)	(0.0563)	(0.0564)	(0.0164)	(0.0165)
Total Female Pop.	-0.165***	-0.166***	-0.122**	-0.123**	-0.0506***	-0.0507***
	(0.0503)	(0.0508)	(0.0559)	(0.0560)	(0.0180)	(0.0180)
1-5 Year Olds	-0.173***	-0.174***	-0.137**	-0.138**	-0.0581***	-0.0582***
	(0.0502)	(0.0507)	(0.0549)	(0.0551)	(0.0158)	(0.0159)
1-5 Male	-0.173***	-0.174***	-0.146**	-0.147**	-0.0604***	-0.0605***
	(0.0512)	(0.0516)	(0.0569)	(0.0571)	(0.0156)	(0.0157)
1-5 Female	-0.172***	-0.174***	-0.129**	-0.129**	-0.0557***	-0.0558***
	(0.0501)	(0.0506)	(0.0538)	(0.0540)	(0.0162)	(0.0163)
6-14 year olds	-0.107**	-0.109**	-0.0692	-0.0703	-0.0413**	-0.0415**
	(0.0489)	(0.0488)	(0.0569)	(0.0566)	(0.0175)	(0.0174)
6-14 Male	-0.105*	-0.107*	-0.0704	-0.0716	-0.0441***	-0.0442***
	(0.0562)	(0.0561)	(0.0595)	(0.0593)	(0.0164)	(0.0164)
6-14 Female	-0.108**	-0.109**	-0.0662	-0.0673	-0.0375*	-0.0377*
	(0.0438)	(0.0436)	(0.0586)	(0.0582)	(0.0219)	(0.0218)
15-17year olds	-0.223***	-0.224***	-0.201***	-0.201***	-0.0817***	-0.0817***
	(0.0586)	(0.0590)	(0.0612)	(0.0615)	(0.0195)	(0.0196)
15-17 Male	-0.263***	-0.264***	-0.240***	-0.240***	-0.0896***	-0.0896***
	(0.0581)	(0.0583)	(0.0589)	(0.0591)	(0.0193)	(0.0193)
15-17 Female	-0.190**	-0.191**	-0.170**	-0.171**	-0.0778***	-0.0779***
	(0.0747)	(0.0752)	(0.0766)	(0.0769)	(0.0256)	(0.0257)
19-24	-0.227***	-0.228***	-0.199**	-0.199**	-0.0718***	-0.0718***
	(0.0699)	(0.0702)	(0.0835)	(0.0835)	(0.0192)	(0.0192)
19-24 Male	-0.264***	-0.264***	-0.265***	-0.265***	-0.0878***	-0.0877***
	(0.0774)	(0.0777)	(0.0989)	(0.0988)	(0.0210)	(0.0210)
19-24 Female	-0.195***	-0.196***	-0.146*	-0.147*	-0.0601***	-0.0601***
	(0.0667)	(0.0669)	(0.0769)	(0.0770)	(0.0198)	(0.0199)
25+	-0.198***	-0.199***	-0.155**	-0.156**	-0.0633***	-0.0634***
	(0.0559)	(0.0565)	(0.0616)	(0.0619)	(0.0169)	(0.0170)
25+ Male	-0.205***	-0.206***	-0.164***	-0.165***	-0.0642***	-0.0643***
	(0.0570)	(0.0576)	(0.0628)	(0.0631)	(0.0173)	(0.0174)
25+ Female	-0.207***	-0.208***	-0.164**	-0.165**	-0.0655***	-0.0656***
	(0.0572)	(0.0578)	(0.0637)	(0.0640)	(0.0174)	(0.0175)
Controls	No	Yes	No	Yes	No	Yes
N	7,352	7,352	4,910	4,910	4,910	4,910

Notes: All regressions include municipality and year fixed effects. All are linear regressions. Each column represents a separate regression using the log of the population size in the municipality for each subgroup as dependent variable. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security of the Federal Government and include all deaths by "presumed delinquent rivalry". The control included is the (time-varying) municipality-level public expenditure. Standard errors clustered at county level in parentheses.
$^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 7: The impact of homicide rates on employment rates using the ENOE

Panel A: Men

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homicide Rate	0.05	0.02	0.24	0.02				
	(0.18)	(0.36)	(0.26)	(0.33)				
Drug Hom. Rate					-0.01	0.08	0.23**	0.02
					(0.05)	(0.13)	(0.10)	(0.12)
Mean dept. var.	0.171	0.426	0.666	0.808	0.171	0.426	0.666	0.808
Observations	5,018	4,949	4,731	4,743	5,018	4,949	4,731	4,743
R-squared	0.395	0.435	0.381	0.349	0.395	0.435	0.382	0.349
Age Group	12-14	15-17	18-20	21-24	12-14	15-17	18-20	21-24

Panel B: Women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homicide Rate	0.06	0.02	-0.16	0.21				
	(0.14)	(0.31)	(0.31)	(0.27)				
Drug Hom. Rate					-0.03	-0.07	-0.07	0.03
					(0.05)	(0.07)	(0.08)	(0.09)
Mean dept. var.	0.072	0.179	0.317	0.389	0.072	0.179	0.317	0.389
Observations	4,989	4,965	4,840	4,970	4,989	4,965	4,840	4,970
R-squared	0.305	0.334	0.352	0.371	0.305	0.334	0.352	0.371
Age Group	12-14	15-17	18-20	21-24	12-14	15-17	18-20	21-24

Notes: Dependent variable is the employment rate by age-group, calculated from ENOE survey data. All regressions include municipality and year fixed effects. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by "presumed delinquent rivalry". Data available from 2006 to 2010. Standard errors clustered at county level in parentheses. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table 8: The impact of homicide rates on NEET rates using the ENOE

Panel A: Men

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homicide Rate	0.04	0.03	0.02	0.09				
	(0.04)	(0.08)	(0.07)	(0.12)				
Drug Hom. Rate					0.00	-0.03	-0.03	-0.00
					(0.01)	(0.03)	(0.03)	(0.07)
Mean dept. var.	0.007	0.021	0.018	0.016	0.007	0.021	0.018	0.016
Observations	5,018	4,949	4,731	4,743	5,018	4,949	4,731	4,743
R-squared	0.264	0.254	0.240	0.274	0.264	0.254	0.240	0.274
Age Group	12-14	15-17	18-20	21-24	12-14	15-17	18-20	21-24

Panel B: Women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homicide Rate	-0.04	-0.04	0.09	0.05				
	(0.06)	(0.17)	(0.23)	(0.18)				
Drug Hom. Rate					-0.01	-0.03	0.11	0.09
					(0.02)	(0.04)	(0.10)	(0.07)
Mean dept. var.	0.010	0.044	0.070	0.073	0.010	0.044	0.070	0.073
Observations	4,989	4,965	4,840	4,970	4,989	4,965	4,840	4,970
R-squared	0.272	0.328	0.318	0.317	0.272	0.328	0.318	0.317
Age Group	12-14	15-17	18-20	21-24	12-14	15-17	18-20	21-24

Notes: Dependent variable is the rate of NiNi (individuals that do not work nor study) prevalence by age-group, calculated from ENOE survey data. All regressions include municipality and year fixed effects. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by "presumed delinquent rivalry". Data available from 2006 to 2010. Standard errors clustered at county level in parentheses. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table 9: The impact of homicide rates on the supply of schools and teachers

	Total Teachers					Total Schools				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	-0.64**	-0.04	-0.00		-0.05*	0.33	-0.06**	-0.02		-0.06**
	(0.30)	(0.03)	(0.03)		(0.03)	(0.36)	(0.02)	(0.02)		(0.02)
Drug Hom. Rate				-0.01					-0.01	
				(0.01)					(0.01)	
Public expenditure					0.00***					0.00***
					(0.00)					(0.00)
Mean dept. var.	5.196	5.196	5.160	5.159	5.235	3.797	3.797	3.753	3.753	3.834
Observations	24,962	24,962	9,807	9,811	23,295	24,957	24,957	9,807	9,811	23,290
R-squared	0.000	0.992	0.999	0.998	0.992	0.000	0.993	0.999	0.998	0.993
Fixed Effects	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Controls	no	no	no	no	yes	no	no	no	no	yes
Years	2000-2010	2000-2010	2007-2010	2007-2010	2007-2010	2000-2010	2000-2010	2007-2010	2007-2010	2007-2010

Notes: Dependent variable is the log of total number of teachers for columns (1) to (5) and the log of the total number of schools for columns (6) to (10). Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by "presumed delinquent rivalry". Standard errors clustered at county level in parentheses. Controls in Million MX\$. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 10: The impact of homicide rates on test scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homicide Rate	10.17	1.56			2.96	-14.54		
	(9.20)	(14.60)			(9.04)	(11.43)		
Drug Hom. Rate			1.75	4.34			2.47	-0.59
			(2.40)	(4.72)			(3.26)	(3.95)
Mean dept. var.	519.263	506.073	519.246	506.066	507.847	509.576	507.838	509.577
Observations	5,641	5,598	5,645	5,602	261,455	88,802	261,571	88,843
R-squared	0.858	0.757	0.858	0.757	0.791	0.751	0.791	0.751
Fixed Effects	Muni	Muni	Muni	Muni	School	School	School	School
Level	Prim.	Sec.	Prim.	Sec.	Prim.	Sec.	Prim.	Sec.

Notes: Dependent variable is the mean math test score on ENLACE. It is aggregated at the county level for columns (1)-(4), and at the school level for columns (5)-(8). All regressions include year fixed effect and either school or municipality fixed effects. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Drug-related homicides are collected by the National Council of Public Security (Consejo Nacional de Seguridad Publica) of the Federal Government and include all deaths by "presumed delinquent rivalry". The state of Oaxaca is excluded due to the unreliability of the test scores. Standard errors clustered at county level in parentheses. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

References

Abadie, Alberto and Javier Gardeazabal. 2003. "The economic costs of conflict: A case study of the Basque Country." *American economic review* pp. 113–132.

Aizer, Anna. 2008. Neighborhood violence and urban youth. Technical report National Bureau of Economic Research.

Akresh, Richard, Philip Verwimp and Tom Bundervoet. 2011. "Civil war, crop failure, and child stunting in Rwanda." *Economic Development and Cultural Change* 59(4):777–810.

Alderman, Harold, John Hoddinott and Bill Kinsey. 2006. "Long term consequences of early childhood malnutrition." *Oxford economic papers* 58(3):450–474.

Alderman, Harold, Miriam Babita, Gabriel Demombynes, Nthabiseng Makhatha and Berk Özler. 2002. "How low can you go? Combining census and survey data for mapping poverty in South Africa." *Journal of African Economies* 11(2):169–200.

Allensworth, Elaine M. 2005. "Dropout rates after high-stakes testing in elementary school: A study of the contradictory effects of Chicago's efforts to end social promotion." *Educational Evaluation and Policy Analysis* 27(4):341–364.

Arias-Vazquez, J and G Esquivel. 2013. "A Note on the Side Effects of the War on Drugs: labor Market Outcomes in Mexico." *El Colegio de Mexico, mimeo* .

Becker, Gary S. 1974. Crime and punishment: An economic approach. In *Essays in the Economics of Crime and Punishment*. NBER pp. 1–54.

Bell, Carl C and Esther J Jenkins. 1991. "Traumatic stress and children." *Journal of Health Care for the Poor and Underserved* 2(1):175–185.

Besley, Timothy J and Hannes Felix Mueller. 2009. "Estimating the Peace Dividend: The impact of violence on house prices in Northern Ireland." *LSE STICERD Research Paper No. EOPP011* .

Blattman, Christopher and Jeannie Annan. 2010. "The consequences of child soldiering." *The review of economics and statistics* 92(4):882–898.

Card, David. 1999. "The causal effect of education on earnings." *Handbook of labor economics* 3:1801–1863.

Carrell, Scott E and Mark L Hoekstra. 2010. "Externalities in the classroom: How children exposed to domestic violence affect everyone's kids." *American Economic Journal: Applied Economics* 2(1):211–228.

Chamarbagwala, Rubiana and Hilcías E Morán. 2011. "The human capital consequences of civil war: Evidence from Guatemala." *Journal of Development Economics* 94(1):41–61.

Currie, Janet and Erdal Tekin. 2006. Does child abuse cause crime? Technical report National Bureau of Economic Research.

Currie, Janet and Tom Vogl. 2012. Early-life health and adult circumstance in developing countries. Technical report National Bureau of Economic Research.

Dell, Melissa. 2015. "Trafficking networks and the Mexican drug war." *The American Economic Review* 105(6):1738–1779.

Di Tella, Rafael and Ernesto Schargrodsky. 2004. "Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack." *The American Economic Review* 94(1):115–133.

Dube, Arindrajit, Oeindrila Dube and Omar García-Ponce. 2013. "Cross-border spillover: US gun laws and violence in Mexico." *American Political Science Review* 107(03):397–417.

Enamorado, Ted, Luis F López-Calva and Carlos Rodríguez-Castelán. 2014. "Crime and growth convergence: Evidence from Mexico." *Economics Letters* 125(1):9–13.

Grogger, Jeffrey. 1997. "Local violence and educational attainment." *Journal of human resources* pp. 659–682.

Guerrero, Eduardo. 2010. "Cómo reducir la violencia en México." *Nexos. México DF* 3.

Guerrero, Eduardo. 2011. "La raíz de la violencia." *nexos, junio de* .

Heller, Sara, Harold A Pollack, Roseanna Ander and Jens Ludwig. 2013. Preventing youth violence and dropout: A randomized field experiment. Technical report National Bureau of Economic Research.

Ichino, Andrea and Rudolf Winter-Ebmer. 2004. "The long-run educational cost of World War II." *Journal of Labor Economics* 22(1):57–87.

Justino, Patricia. 2011. "Violent conflict and human capital accumulation." *IDS Working Papers* 2011(379):1–17.

Le, Anh T, Paul W Miller, Andrew C Heath and Nick Martin. 2005. "Early childhood behaviours, schooling and labour market outcomes: estimates from a sample of twins." *Economics of Education Review* 24(1):1–17.

Leon, Gianmarco. 2012. "Civil conflict and human capital accumulation the long-term effects of political violence in perú." *Journal of Human Resources* 47(4):991–1022.

Lynch, Michael and Dante Cicchetti. 1998. "An ecological-transactional analysis of children and contexts: The longitudinal interplay among child maltreatment, community violence, and children's symptomatology." *Development and psychopathology* 10(02):235–257.

Magaloni, Beatriz, Alberto Diaz-Cayeros, Aila M Matanock and Vidal Romero. 2011. "Living in Fear: Mapping the Social Embeddedness of Drug Gangs and Violence in Mexico." *SSRN Working Paper Series* .

Mani, Anandi, Sendhil Mullainathan, Eldar Shafir and Jiaying Zhao. 2013. "Poverty impedes cognitive function." *science* 341(6149):976–980.

Merino, José. 2011. "Los operativos conjuntos y la tasa de homicidios: Una medición." *nexos, junio de* .

Miguel, Edward and Gerard Roland. 2011. "The long-run impact of bombing Vietnam." *Journal of development Economics* 96(1):1–15.

Osofsky, Joy D. 1999. "The impact of violence on children." *The future of children* pp. 33–49.

Rios, Viridano. 2012*a*. "Why Are Mexican Traffickers Killing Each Other? Government Co-ordination and Violence Deterrence in Mexico's Drug War." *Unpublished manuscript. Harvard University, Department of Government* .

Ríos, Viridiana. 2012*b*. "How government structure encourages criminal violence: the causes of Mexico's drug war." *PhD diss., Harvard University, Cambridge, MA* .

Ríos, Viridiana. 2014. "Security issues and immigration flows: Drug-violence refugees, the new Mexican immigrants." *Latin American Research Review* 49(3):3.

Robles, Gustavo, Gabriela Calderón and Beatriz Magaloni. 2013. "The economic consequences of drug trafficking violence in Mexico." *Poverty and Governance Series Working Paper, Stanford University* .

Sharkey, Patrick. 2010. "The acute effect of local homicides on children's cognitive performance." *Proceedings of the National Academy of Sciences* 107(26):11733–11738.

Sharkey, Patrick T, Nicole Tirado-Strayer, Andrew V Papachristos and C Cybele Raver. 2012. "The effect of local violence on children's attention and impulse control." *American journal of public health* 102(12):2287–2293.

Shemyakina, Olga. 2011. “The effect of armed conflict on accumulation of schooling: Results from Tajikistan.” *Journal of Development Economics* 95(2):186–200.

Sota, Alejandra and Miguel Messmacher. 2012. “Operativos y violencia.” *Nexos en Linea* 1(12):2012.

Velásquez, Andrea. 2014. “The economic burden of crime: Evidence from Mexico.” *Job Market Paper, Department of Economics, Duke University, Durham, NC* .

Willis, Robert J and Sherwin Rosen. 1978. “Education and self-selection.”.

A Appendix Tables

Table A.1: The impact of homicide rates on the number of enrolled students (including state-year fixed effects)

	2000-2010			
	(1)	(2)	(3)	(4)
Homicide Rate	-0.03	-0.03	-0.02	-0.25
	(0.04)	(0.04)	(0.05)	(0.18)
Mean dept. var.	7.881	7.574	6.478	4.869
Observations	25,194	25,195	25,208	25,041
R-squared	0.987	0.986	0.985	0.943
Fixed Effexts	yes	yes	yes	yes
State-Yr. FE	yes	yes	yes	yes
Edu. Level	Basic	Prim.	Sec.	Prep.

All regressions include municipality and year fixed effects, as well as state-year fixed effects. Dependent variable is the logarithm of county total students +1 (INEGI) by school level. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table A.2: The impact of homicide rates on the number of enrolled students, controlling for time-varying municipality-level public finance indicator.

Basic School

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	0.00	0.01	0.01	0.01	-0.00	-0.04	0.01	0.01	0.01	-0.01
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)
Public expenditure		0.00***								
		(0.00)								
Wages			0.01***							
			(0.00)							
Transfers				0.00***						
				(0.00)						
Investment					0.00***					
					(0.00)					
Debt						0.00				
						(0.00)				
Public Revenue							0.00***			
							(0.00)			
Taxes								0.01***		
								(0.00)		
Uncond. Fed. Transf									0.01***	
									(0.00)	
Cond. Fed. Transf										0.01***
										(0.00)
Mean dept. var.	7.881	7.921	7.922	7.920	7.984	8.144	7.921	7.947	7.921	7.984
Observations	25,194	23,494	23,466	23,208	22,293	14,976	23,494	23,023	23,486	20,480
R-squared	0.985	0.984	0.984	0.984	0.984	0.989	0.984	0.984	0.984	0.989

All regressions include municipality and year fixed effects, as well as state-year fixed effects. Dependent variable is the logarithm of county total students +1 (INEGI) by school level. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times$100 (general homicide rate of 8.3 and drug related homicide rate of 2.6). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX$.
$^{*}p < 0.1, ^{**} p < 0.05, ^{***} p < 0.01$

Table A2: (cont'd) **Primary School**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	0.02	0.02	0.02	0.02	0.01	-0.03	0.02	0.02	0.02	0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Public expenditure		0.00***								
		(0.00)								
Wages			0.01***							
			(0.00)							
Transfers				0.01***						
				(0.00)						
Investment					0.01***					
					(0.00)					
Debt						0.00				
						(0.00)				
Public Revenue							0.00***			
							(0.00)			
Taxes								0.01***		
								(0.00)		
Uncond. Fed. Transf									0.01***	
									(0.00)	
Cond. Fed. Transf										0.01***
										(0.00)
Mean dept. var.	7.574	7.614	7.615	7.612	7.677	7.834	7.614	7.639	7.614	7.672
Observations	25,195	23,495	23,467	23,209	22,294	14,977	23,495	23,024	23,487	20,481
R-squared	0.984	0.983	0.983	0.983	0.983	0.989	0.983	0.983	0.983	0.988

All regressions include municipality and year fixed effects, as well as state-year fixed effects. Dependent variable is the logarithm of county total students +1 (INEGI) by school level. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX$.
$^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Table A2: (cont'd) **Secondary School**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	-0.05	-0.05	-0.05	-0.05	-0.04	-0.08*	-0.05	-0.04	-0.05	-0.06*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Public expenditure		0.00***								
		(0.00)								
Wages			0.00							
			(0.00)							
Transfers				0.00						
				(0.00)						
Investment					0.00***					
					(0.00)					
Debt						0.00				
						(0.00)				
Public Revenue							0.00***			
							(0.00)			
Taxes								0.00**		
								(0.00)		
Uncond. Fed. Transf									0.00	
									(0.00)	
Cond. Fed. Transf										0.00***
										(0.00)
Mean dept. var.	6.478	6.525	6.526	6.526	6.593	6.775	6.525	6.555	6.525	6.610
Observations	25,208	23,508	23,480	23,222	22,307	14,990	23,508	23,037	23,500	20,494
R-squared	0.983	0.984	0.984	0.984	0.984	0.987	0.984	0.984	0.984	0.987

All regressions include municipality and year fixed effects, as well as state-year fixed effects. Dependent variable is the logarithm of county total students +1 (INEGI) by school level. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX$.
$^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Table A2: (cont'd) **Preparatory School**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	-0.29*	-0.32**	-0.32**	-0.32**	-0.33**	-0.32	-0.32**	-0.31*	-0.32**	-0.32*
	(0.15)	(0.16)	(0.16)	(0.16)	(0.16)	(0.20)	(0.16)	(0.17)	(0.16)	(0.17)
Public expenditure		-0.01***								
		(0.00)								
Wages			-0.02***							
			(0.00)							
Transfers				-0.02***						
				(0.01)						
Investment					-0.02***					
					(0.00)					
Debt						-0.01***				
						(0.00)				
Public Revenue							-0.01***			
							(0.00)			
Taxes								-0.03***		
								(0.01)		
Uncond. Fed. Transf									-0.03***	
									(0.01)	
Cond. Fed. Transf										-0.03***
										(0.00)
Mean dept. var.	4.869	4.945	4.948	4.948	5.035	5.295	4.945	4.999	4.945	5.082
Observations	25,041	23,348	23,320	23,065	22,147	14,889	23,348	22,881	23,340	20,340
R-squared	0.936	0.934	0.934	0.933	0.933	0.933	0.934	0.933	0.934	0.934

All regressions include municipality and year fixed effects, as well as state-year fixed effects. Dependent variable is the logarithm of county total students +1 (INEGI) by school level. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX$.
$^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Table A.3: The impact of homicide rates on the number of enrolled students (lagged homicide rates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Hom. Rate (L1)	-0.03	-0.02	-0.06	-0.47***					-0.04	-0.04	-0.08	-0.48***
	(0.04)	(0.04)	(0.06)	(0.17)					(0.03)	(0.03)	(0.06)	(0.17)
Hom. Rate (L2)					-0.11**	-0.08	-0.20**	-0.39*	-0.12**	-0.09	-0.23**	-0.44*
					(0.05)	(0.06)	(0.10)	(0.21)	(0.05)	(0.06)	(0.10)	(0.23)
Homicide Rate									-0.01	0.00	-0.06	-0.27*
									(0.02)	(0.03)	(0.04)	(0.14)
Mean dept. var.	7.882	7.575	6.479	4.870	7.884	7.576	6.481	4.871	7.882	7.575	6.480	4.871
Observations	25,158	25,159	25,172	25,005	25,120	25,121	25,134	24,967	25,038	25,039	25,052	24,885
R-squared	0.990	0.989	0.986	0.936	0.996	0.995	0.988	0.937	0.996	0.995	0.988	0.937
Fixed Effexts	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Hom. Rate L3 & L4	no	no	no	no	no	no	no	no	yes	yes	yes	yes
Edu. Level	Basic	Prim.	Sec.	Prep.	Basic	Prim.	Sec.	Prep.	Basic	Prim	Sec.	Prep.

All regressions include municipality and year fixed effects, as well as state-year fixed effects. Dependent variable is the logarithm of county total students +1 (INEGI) by school level. Basic school level includes years 1-9, primary 1-6, secondary 7-9, and preparatory 10-12. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3 and drug related homicide rate of 2.6). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table A.4: The impact of homicide rates on the enrollment rates, controlling for time-varying municipality-level public finance indicators

Ages 6-11

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	-0.023***	-0.023***	-0.023***	-0.023***	-0.023***	-0.023***	-0.023***	-0.023***	-0.023***	-0.023***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Public expenditure		-0.000***								
		(0.00)								
Wages			-0.001***							
			(0.00)							
Transfers				-0.000**						
				(0.00)						
Investment					-0.000					
					(0.00)					
Debt						-0.000***				
						(0.00)				
Public Revenue							-0.000***			
							(0.00)			
Taxes								-0.001***		
								(0.00)		
Uncond. Fed. Transf									-0.001***	
									(0.00)	
Cond. Fed. Transf										-0.000**
										(0.00)
Mean dept. var.	0.954	0.954	0.954	0.954	0.954	0.954	0.954	0.954	0.954	0.954
Observations	7,350	7,350	7,350	7,350	7,350	7,350	7,350	7,350	7,350	7,350
R-squared	0.665	0.666	0.666	0.665	0.665	0.665	0.666	0.666	0.666	0.665

All regressions include municipality and year fixed. Dependent variable is the enrollment rate by age group using census data. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX\$. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table A4: (cont'd) **Ages 12-14**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.003	-0.004	-0.004
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Public expenditure		-0.000***								
		(0.00)								
Wages			-0.002***							
			(0.00)							
Transfers				-0.001**						
				(0.00)						
Investment					-0.001***					
					(0.00)					
Debt						-0.001***				
						(0.00)				
Public Revenue							-0.000***			
							(0.00)			
Taxes								-0.003***		
								(0.00)		
Uncond. Fed. Transf									-0.002***	
									(0.00)	
Cond. Fed. Transf										-0.001***
										(0.00)
Mean dept. var.	0.864	0.864	0.864	0.864	0.864	0.864	0.864	0.864	0.864	0.864
Observations	7,348	7,348	7,348	7,348	7,348	7,348	7,348	7,348	7,348	7,348
R-squared	0.740	0.742	0.742	0.741	0.741	0.741	0.742	0.742	0.741	0.741

All regressions include municipality and year fixed. Dependent variable is the enrollment rate by age group using census data. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX\$. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table A4: (cont'd) **Ages 15-17**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	0.023	0.024	0.024	0.023	0.024	0.023	0.024	0.024	0.024	0.023
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Public expenditure		-0.001***								
		(0.00)								
Wages			-0.003***							
			(0.00)							
Transfers				-0.002***						
				(0.00)						
Investment					-0.002***					
					(0.00)					
Debt						-0.001***				
						(0.00)				
Public Revenue							-0.001***			
							(0.00)			
Taxes								-0.004***		
								(0.00)		
Uncond. Fed. Transf									-0.003***	
									(0.00)	
Cond. Fed. Transf										-0.003***
										(0.00)
Mean dept. var.	0.539	0.539	0.539	0.539	0.539	0.539	0.539	0.539	0.539	0.539
Observations	7,342	7,342	7,342	7,342	7,342	7,342	7,342	7,342	7,342	7,342
R-squared	0.798	0.799	0.799	0.798	0.799	0.798	0.799	0.799	0.799	0.799

All regressions include municipality and year fixed. Dependent variable is the enrollment rate by age group using census data. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX$. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table A4: (cont'd) **Ages 18-20**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	-0.007	-0.006	-0.006	-0.006	-0.006	-0.006	-0.006	-0.006	-0.006	-0.007
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Public expenditure		-0.001***								
		(0.00)								
Wages			-0.001***							
			(0.00)							
Transfers				-0.001						
				(0.00)						
Investment					-0.001***					
					(0.00)					
Debt						-0.001**				
						(0.00)				
Public Revenue							-0.001***			
							(0.00)			
Taxes								-0.002***		
								(0.00)		
Uncond. Fed. Transf									-0.002***	
									(0.00)	
Cond. Fed. Transf										-0.002***
										(0.00)
Mean dept. var.	0.227	0.227	0.227	0.227	0.227	0.227	0.227	0.227	0.227	0.227
Observations	7,337	7,337	7,337	7,337	7,337	7,337	7,337	7,337	7,337	7,337
R-squared	0.797	0.798	0.798	0.798	0.798	0.798	0.798	0.798	0.798	0.798

All regressions include municipality and year fixed. Dependent variable is the enrollment rate by age group using census data. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX$. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table A4: (cont'd) **Ages 21-24**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Homicide Rate	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Public expenditure		-0.000								
		(0.00)								
Wages			-0.000							
			(0.00)							
Transfers				-0.000						
				(0.00)						
Investment					-0.000**					
					(0.00)					
Debt						-0.000				
						(0.00)				
Public Revenue							-0.000			
							(0.00)			
Taxes								-0.000		
								(0.00)		
Uncond. Fed. Transf									-0.000	
									(0.00)	
Cond. Fed. Transf										-0.000*
										(0.00)
Mean dept. var.	0.095	0.095	0.095	0.095	0.095	0.095	0.095	0.095	0.095	0.095
Observations	7,333	7,333	7,333	7,333	7,333	7,333	7,333	7,333	7,333	7,333
R-squared	0.750	0.750	0.750	0.750	0.750	0.750	0.750	0.750	0.750	0.750

All regressions include municipality and year fixed. Dependent variable is the enrollment rate by age group using census data. Homicide rates are normalized according to the national homicide rate per 100,000 inhabitants in 2007 $\times 100$ (general homicide rate of 8.3). Regressions cover the 2000-2010 period. Standard errors clustered at county level in parentheses. Controls in Million MX$. $^{*}p < 0.1, ^{**} p < 0.05, ^{***} p < 0.01$