AMERICAN SOCIOLOGICAL ASSOCIATION

Sociology of Education
87(2) 89–105
© American Sociological Association 2014
DOI: 10.1177/0038040714523795
http://soe.sagepub.com
SAGE

Exposure to Local Homicides and Early Educational Achievement in Mexico

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Abstract

We investigate the effect of children's exposure to local violence on grade failure in Mexico. We construct an annual panel of all elementary schools from 1990 to 2010 and merge municipality-level homicide rates to analyze the effect of exposure to local homicide. Using a variety of causal inference techniques, we consistently find that exposure to local violence increases the probability of failing a grade in elementary school. This effect is net of demographic, economic, and migratory trends usually associated with violence and is likely driven by heightened fear and anxiety and change in parenting practices. Our findings suggest that violent crime in children's environments compromises early educational achievement and may have long-lasting consequences on human capital formation and economic well-being.

Keywords

violence, grade failure, school dropout, causal inference, homicide

INTRODUCTION

Violent crime is a social problem with enormous direct costs in terms of death tolls, injuries, disability, and loss of property (Krug et al. 2002; Miller, Cohen, and Rossman 1993). But the consequences of crime do not stop with its direct victims. Given its ecological contours, effects likely extend to broader populations and multiple domains. In this article, we assess one of these indirect effects: the impact of local homicides on children's early educational achievement in Mexico.

Much research demonstrates a correlation between exposure to environmental violence and a myriad of children's and young adults' outcomes (Brooks-Gunn, Duncan, and Aber 1997; Harding 2009; Sampson, Morenoff, and Ganon-Rowley 2002), but evidence of a causal effect of violence on educational results is scarce. Most studies use cross-sectional data, rely on self-reported measures of crime, and use small or idiosyncratic samples. Although these studies are valuable, they are vulnerable to multiple sources of bias. This limitation is unfortunate because early educational achievement shapes human capital accumulation and socioeconomic well-being, particularly in Latin America and other developing regions (Hanushek and Woessman 2012). Furthermore, given that disadvantaged children are more likely than their advantaged peers to experience violence in their communities, exposure may contribute to the intergenerational reproduction of disadvantage.

Assessing effects of exposure to community violence on children's educational outcomes is challenging. A central difficulty is unobserved heterogeneity: Communities more likely to be exposed to homicides and other types of violence

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differ from nonviolent neighborhoods on factors that may also affect educational outcomes. While some of these factors can be (at least partially) observed—for example, socioeconomic advantage or school resources—many others, such as school quality and climate, families' social networks, and exposure to other environmental stressors, are unobserved. As a result, it is difficult to disentangle the effect of community violence from its unfortunately common correlates.

We study the effect of homicides in the municipality where a school is located on children's grade failure in Mexico between 1990 and 2010 using analytic strategies that alleviate the unobserved heterogeneity problem. We focus on homicide because it is the most visible and noticeable type of violent crime and its measurement is more reliable than that of other crimes (Sampson, Raudenbush, and Earls 1997). We construct a panel of schools by combining administrative records on all Mexican primary schools with data on all homicides over the 21-year period of observation, thus avoiding the potential measurement error of self-reported data. Unobserved heterogeneity is addressed by causal inference techniques for panel data. Even though all the models we use in this analysis yield consistent estimates, they provide different numerical answers, a fact usually overlooked by studies that rely on one single model. The comparison between models allows us to evaluate the robustness of the findings and to provide plausible bounds for the effects of interest.

The outcome of interest is primary school (grades 1 to 6) grade failure. Early school failure has severe, long-lasting consequences for children. U.S.-based research indicates it may result in stigma, disruption of peer relations, and social adjustment problems; early school failure also predicts long-term educational attainment and labor market outcomes (Jimmerson 1999; Jimmerson, Anderson, and Whipple 2002; Jimmerson et al. 1997; Roderick 1994; Rumberger 1995; see, however, Warren and Saliba 2012 for references to a recent literature using causal inference that suggests more mixed results). The literature is more limited in Latin America and other developing regions, but evidence shows early school failure strongly predicts poor educational outcomes, in particular premature school dropout (Marshall 2003; Randall and Anderson 1999; UNESCO 2012).

Mexico provides an interesting case to study the effect of exposure to community violence on school failure because the homicide rate changed markedly over the past two decades. The national homicide rate per 100,000 population dropped from almost 20 in the early 1990s to 8 in the mid-2000s, then rose dramatically to 23 in 2010 (see Figure 1). Furthermore, in 1990 homicides were more prevalent in small rural communities, but by 2010 violent crime touched virtually all regions of the country and was most prevalent in border states and areas located along drug trafficking routes. These dynamics induce substantial variation in exposure to violent crime at the local level, which we use to identify effects on children's educational outcomes.

The article is organized as follows. The next section offers the substantive background for this project. It describes the effects of local homicides on children's educational outcomes as well as the plausible mechanisms involved. Next, we discuss the Mexican context and the change in violence over the period considered. A description of the data and methods follows. Then, we offer the findings. Finally, we discuss the evidence in light of the data and methodological limitations as well as tasks for future research.

LOCAL HOMICIDES AND EARLY EDUCATIONAL OUTCOMES

Several bodies of research document a negative association between exposure to local violence—in particular, homicides—and children's outcomes and suggest mechanisms driving this association. Even if our analysis of aggregate trends at the school level does not allow detailed examination of individual-level mechanisms, it is important to discuss their plausibility in the Mexican case and to empirically evaluate them to the extent possible with the data at hand.

The mechanisms linking environmental violence to school outcomes operate at the individual, household, school, and neighborhood levels. At the individual level, children may experience heightened stress, fear, and anxiety generated by exposure to environmental violence, which research shows affects school performance (Margolin and Gordis 2000, Sharkey 2010). Developmental psychology studies show that exposure to violence predicts psychological

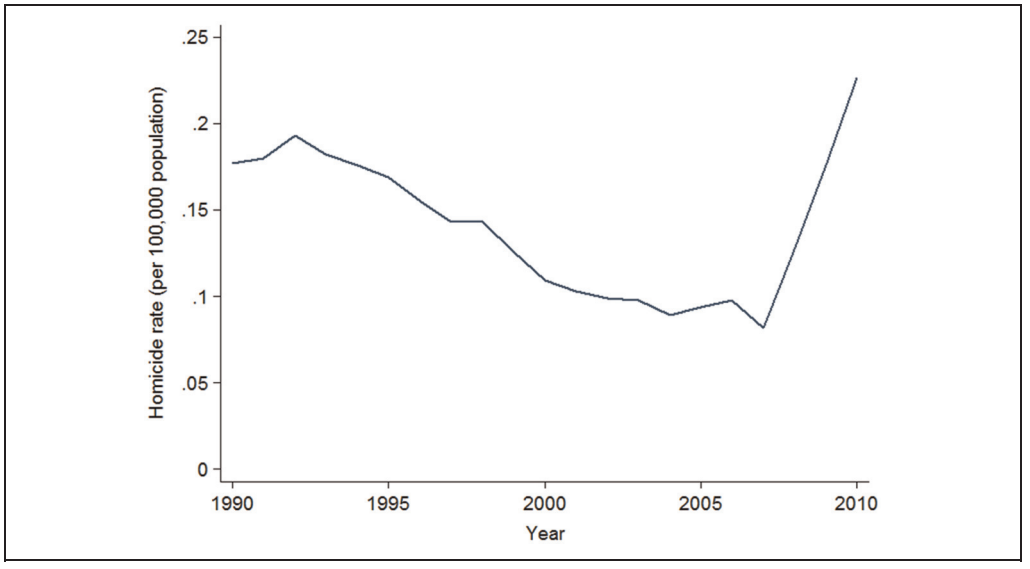

Figure 1. Homicide rate in Mexico 1990–2010
Source: Calculations based on municipal-level data made available by the Instituto Nacional de Estadística y Geografía (INEGI).

distress, sleep disturbances, aggressive behavior, attention and concentration disorders, and post-traumatic stress symptoms (Fowler et al. 2009; Guerra, Huesmann, and Spindler 2003).

At the household level, living in dangerous neighborhoods may alter parenting practices. Parents facing community violence may focus on ensuring basic safety and protecting children from danger, leaving less energy for enhancing academic performance (Harding 2010). Furthermore, parents exposed to high levels of violence may resort to harsh or inconsistent disciplining strategies due to the anxiety and depression triggered by environmental hostility (Hill and Herman-Stahl 2002). A common strategy used by parents to prevent children's victimization or engagement in violence is "bounding techniques," which restrict children—particularly younger ones—to the home environment and limit access to neighborhood interactions and influences (Jarrett 1997). These strategies, although useful to reduce environmental harm, may cut the social ties between families, school staff, and community that help monitor children (Bryk et al. 2010; Coleman 1988). In extreme circumstances, students may limit their school attendance in fear of neighborhood and school violence, thus compromising their learning (Bowen and Bowen 1999). Family migration to escape risky environments is an even more drastic household measure. If some population groups are more likely to out-migrate than others, these flows may alter the population at risk of exposure to homicide, inducing bias on the observed effect of violence. High levels of local violence may also disrupt economic activity. Closure of local businesses, difficulty in reaching places of employment, and higher transaction costs associated with violence may result in economic hardship fo households, which in turn may affect children's educational achievement and attainment (Gregg, McMillan, and Nasim 2012).

A rich tradition of urban ethnography in the United States studies the effect of violence at the community level and suggests that in contexts where economic opportunities are scarce, illegal activities are lucrative, leaving hard-working residents prey to local violence and children vulnerable to adopting gang-like values through cross-cohort socialization. These values may hurt children's ability or commitment to learning at school (Anderson 1999; Bourgois 1995; Harding 2010). Although these processes are more acute among adolescents, young children may also be vulnerable insofar as neighborhoods are easily accessible settings for early social interactions.

High environmental violence may also jeopardize educational achievement by reducing social capital and collective efficacy at the community

level. According to social disorganization theory, environmental violence undermines cooperation, trust, and community members' ability to exert informal social control to promote socially acceptable behavior that supports school engagement. As a result, community members will be less willing or able to intervene against nonnormative behaviors, such as children's skipping school, disrespecting adults, or vandalizing buildings (Anderson 1999; Sampson et al. 1997).

Violence may also affect disorder or safety at the school level, which has been linked to lower educational attainment (Grogger 1997). Children exposed to a "culture of violence" may bring to school attitudes and values that induce violent behavior toward peers and teachers and disorder in classrooms and hallways (Bowen and Van Dorn 2002; Lockwood 1997). These dynamics could foster anxiety and mistrust among teachers, jeopardizing a stable and supportive work environment that is critical for schools' successful functioning (Bryk et al. 2010) and inducing disengagement with pedagogic tasks (Devine 1996; Galand, Lecocq, and Philippot 2007).

These distinct analytic levels are not competing accounts but rather interrelated, mutually reinforcing social responses to a violent context (Burdick-Will 2013; Jencks and Mayer 1990). None of these pathways requires children living in high-violence municipalities to be direct victims or witnesses of crimes—in this sense they properly refer to an environmental exposure. No systematic research evaluating the relative relevance of these mechanisms exists to date in Mexico. However, as suggested by social disorganization theory, the depletion of social capital and collective efficacy at the local level is the product of long-term exposure to environmental violence. According to these approaches, over-time or chronic exposure to violence breaks down community organization and norms, reduces social integration among residents, and weakens mechanisms for social control (Bowen and Van Dorn 2002). The causal exposure captured in our analysis is not indicative of this kind of long-term violence in a community. Rather, we examine the effect of year-to-year variation in homicides, which captures precisely the departures from the average or baseline level of homicide for each municipality. We focus on young children, for whom anxiety or parenting styles may be more relevant than neighborhood influences in explaining educational outcomes, rather than adolescents. In fact, abundant media reports suggest Mexican parents routinely resort to bounding strategies, keeping children at home to avoid exposure in the streets and public spaces, and that children growing up in violent environments suffer from acute fear and anxiety (Monge 2012; Proceso 2009).

In summary, given this setting's characteristics, it is plausible that relatively short-term reactions and adaptations to environmental violence that likely affect younger children—such as heightened stress and anxiety, change in parenting styles, household isolation as a protective mechanism, and economic disruption—play important roles linking environmental violence to early educational outcomes.

THE MEXICAN CASE

Mexico experienced a substantial decline in violent crime between 1990 and the mid-2000s. The homicide rate reached about 17 per 100,000 in the early 1990s and dropped to 8 per 100,000 in 2007 (see Figure 1). Historically, homicides were more prevalent in rural than in urban areas in Mexico. Many rural communities were geographically isolated, extremely deprived, and difficult to reach by the rule of law (Villarreal 2004). These structural conditions contributed to violence's being deeply embedded in social life in rural Mexico. Furthermore, political and land-related conflicts in rural southern states, which have large indigenous populations, resulted in frequent episodes of violence (Hernández Navarro 2006).

The reduction in violent crime in Mexico since the mid-1990s was largely driven by a decline in homicides in rural communities. In 1991, the homicide rate per 100,000 population was 16 in urban areas but 27 in rural areas. By 2007, the urban-rural gap had virtually disappeared, with rates of 8 per 100,000 in urban areas and 11 per 100,000 in rural localities.

Evidence suggests the decline in rural violence was driven by multiple factors. First, depopulation of the Mexican countryside, pushed by market-oriented reforms implemented since the early 1990s (Escalante 2009; Fernández-Kelly and Massey 2007), seems to have reduced local disputes. Political reforms also appear to have contributed. After decades of violent conflict and repression, reforms intended to reduce violent control of rural,

indigenous communities were passed in the early 1990s (Trejo 2012). Anticipating violent rebellion as a response to NAFTA, governors of rural states implemented concessions for the peasantry as a preventive measure. For instance, the governor of the rural state of Oaxaca was instrumental in passing a constitutional reform protecting indigenous communities and reducing repression (Trejo 2012).

Macro-level processes such as urbanization and educational expansion may also have added to the decrease in violence. As education expanded and economic activity shifted from agriculture to manufacturing and services, traditionally violent disputes over land lost strength in rural areas. During this process, interpersonal disputes became less likely to end in homicides, as indicated by a drop in murders wherein the victim and perpetrator knew each other (Bergman 2012). The modest but steady growth in per capita GDP since the mid-1990s and the relatively low levels of unemployment in urban areas after the 1995 economic crisis appear to have contributed further to the decline in violent crime (González-Pérez et al. 2012).

In summary, although no conclusive evidence exists about the factors driving the decline in crime between 1990 and 2006, a combination of urbanization, economic transformation, migration, and political change likely contributed. In contrast, the sharp increase in homicides since 2007 was almost entirely driven by drug-trafficking-related violence. This increase was surprising because drug trafficking from Mexico into the United States had existed since at least the early twentieth century, and drug-trafficking organizations (DTOs) had historically been tolerated by the Mexican government.

The relative equilibrium in the relationship between DTOs, state authorities, and law enforcement began to change in the 1990s when the country started to decentralize and democratize, and it was drastically altered in 2006. That year, President Felipe Calderon came into power and launched an unprecedented crackdown on drug trafficking. The government deployed military and special police squads in cities with an organized crime presence, with the main objective of capturing or killing the leaders of DTOs (Astorga and Shirk 2010; Shirk 2010). This offensive against DTOs induced fragmentation and infighting between and within these organizations, triggering violent efforts to control trafficking routes. Violence was first targeted at members of rival organizations, but it rapidly extended to local authorities, journalists, and bystanders (Guerrero-Gutiérrez 2011; Shirk 2010). Largely due to drug-trafficking-related crime, the homicide rate rose from less than 10 per 100,000 in the mid-2000s to almost 25 in 2010. The changing source of violence since 1990 was accompanied by geographic dispersion (Guerrero-Gutiérrez 2011). In 1990, rural communities had the highest rates of violent crime, but by the late 2000s, border states and areas along drug-trafficking routes in the center of the country had the most homicides.

We exploit such temporal and geographic variation in the homicide rates across Mexico to ascertain the effect of exposure to local violent crime on primary school children. We cannot reasonably claim that local homicides are exogenous shocks, but by deploying a series of causal inference techniques, we alleviate the potential bias from unobserved characteristics of localities affected by violence that could confound the relationship between homicides and educational outcomes.

DATA AND METHODS

We created an annual panel of all elementary schools in Mexico (1990 to 2010) merged with the annual homicide rate in the municipality where each school is located. There are 2,456 municipalities in Mexico, with a median population of 12,731 (interquartile range: 4,264–32,717) and median area of 90.2 square miles (interquartile range: 33.5–255.6). School-level data come from the School Census, which the Mexican Department of Education applies twice a year to all schools in the country.[1] The School Census records students' enrollment and graduation. It also includes several school characteristics, such as number of teachers, whether the principal has teaching responsibilities, number of grades offered, and whether the school is public or private.

Variables

We constructed a 1990 to 2010 annual calendar-year homicide series at the municipal level using data from the Mexican Bureau of Statistics INEGI

(2012b). We obtained information about all homicides in the country from vital statistics, compiled by INEGI and coded according to the International Statistical Classification of Diseases. Population values come from the National Population Census of 1990, 2000, and 2010 and the National Population Counts of 1995 and 2005, from which we created an annual series through linear interpolation. Then, we calculated the homicide rate per 1,000 population for every year-municipality in the country. The outcome of interest is grade failure, measured as the proportion of elementary school students (grades 1 through 6) in each school who did not achieve the minimum overall grade necessary for passing to the next grade during each academic year (September through July).[2] Equation 1 calculates the school-level failure rate.

$$Failure\ Rate_{it} = \frac{(Enrollment_{it} - Passed\ students_{it})}{Enrollment_{it}}. \quad (1)$$

Enrollment refers to the total number of children enrolled in school *i* at the end of academic year *t*. This number excludes students who dropped out or left school at any point during the academic year and thus is not directly affected by migration. We consider the failure rate of all elementary school students in each school. Because both failure and homicide rate measures are right-skewed, we estimated alternative models using inverse hyperbolic sine, logarithmic (excluding zero values), and cubic root transformations of these variables. Results of these alternative models, available from the authors upon request, are substantively identical to those reported here.

Methods

We used several causal inference strategies for panel data. The first model is a school and year fixed-effects (FE) formulation. School fixed effects add a school-specific intercept term that accounts for any time-invariant source of heterogeneity at the school level, and year fixed effects account for any period affecting all schools. Time-varying controls account for trends at the school, municipality, and state levels plausibly correlated with violence and educational outcomes. For example, if declining economic conditions result in both an increase in violent crime and worse school outcomes, failing to account for economic trends may result in a spurious association between homicides and school failure.

Equation 2 presents the FE formulation.

$$\begin{aligned}(Failure\ Rate)_{ijt} &= \beta_0 + \beta_1 (Hom)_{jt} + Y_t + \beta_2 (\boldsymbol{SC})_{ijt} \\ &+ \beta_3 (\boldsymbol{MC})_{jt} + \alpha_{ij} + u_{ijt}. \end{aligned} \quad (2)$$

Failure Rate identifies the failure rate in school *i*, located in municipality *j*, at year *t*. *Hom* measures the homicide rate (homicides per 1,000 population) in municipality *j*, where school *i* is located, in year *t*. Y_t is a set of year-specific fixed effects, ***SC*** is a vector of time-varying school characteristics, ***MC*** is a vector of time-varying state and municipality characteristics, α_{ij} is a set of school-level fixed effects, and u_{ijt} is an idiosyncratic error term. Adding a term α_{ij} for each school is algebraically identical to taking over-time departures from the school-level means for all variables and running a regression on the de-meaned variables (Angrist and Pischke 2009). This results in using only change over time within school to identify the effect of interest (this is why the FE estimator is known as the "within" estimator). This and the following models are weighted using the total school enrollment for each year, and standard errors are clustered at the school level.

Time-varying covariates at the school level include whether the school offers all elementary grades, the teacher-student ratio, and whether principals do not have teaching responsibilities in year *t*. These covariates capture the change in economic and human resources at the school level. Time-varying municipality-level covariates include total population, the proportion of employed population earning less than twice the minimum wage, and the proportion of households without piped water and with crowded dwellings in year *t*. At the state level, time-varying controls include the unemployment rate, the net migration rate (emigration – immigration), the amount of public social spending, and the amount of public economic spending in year *t*. Table 1 offers a detailed explanation of all variables and presents descriptive statistics.

Panel data analysis may be affected by within-panel serial correlation, that is, the correlation between values of the error term in a temporal series and previous values of the same series. They also may be affected by contemporaneous

Table 1. Description of Variables

Variable Name	Level	Description	Source	Mean	*N* Schools
Homicide rate (predictor)	Municipality	Number of homicides per thousand residents in year *t*	System of state and municipal databases at the Mexican Bureau of Statistics (INEGI 2012b)	0.16 (0.23)	1,846,250
Failure rate (outcome)	School	Proportion of elementary school students in each school year who did not achieve the minimum grade necessary for passing to the next grade, obtained over the total number of students who did not drop out during the school year	Mexican School Census collected by the Ministry of Education (Secretaría de Educación Pública 2011)	0.07 (0.06)	1,568,183
All grades (control)		Equals 1 if the school offers all elementary school grades in year *t*		0.93 (0.25)	1,569,009
Teacher-student ratio (control)		Ratio of teachers to students in year *t*		0.05 (0.03)	1,568,261
Principals with no teaching responsibilities (control)		Equals 1 if school principals do not have teaching responsibilities in year *t*		0.51 (0.50)	1,575,418
Private (control)		Equals 1 if the school is private		0.12 (0.33)	1,862,658
Population (1,000) (control)	Municipality	Interpolated values to create an annual panel	National Council of Population estimations with original data from the censuses of 1990, 2000, and 2010 and the Population Count of 2005 (CONAPO 2012b)	254.6 (382.2)	1,846,250
% population in households without piped water (control)		Interpolated, denominator is total population living in private households		17.5 (19.1)	1,846,250
% population in households with crowded dwelling (control)		Interpolated, denominator is total population living in private households		49.4 (13.7)	1,844,170
% employed population earning less than twice minimum wage (control)		Interpolated, denominator is employed population 12 years or older		57.5 (18.4)	1,844,170

(continued)

Table 1.
(Continued)

Variable Name	Level	Description	Source	Mean	*N* Schools
Unemployment (control)	State	Available between 1996 and 2010, except for 1997, for 1990–1994 and 1997 state average over the available years was imputed, 1995 values replaced by the national unemployment average because Mexico experienced an economic crisis and imputing averages over time would be misleading	National Survey of Occupation and Employment made available through the Mexican Bureau of Statistics (INEGI 2012a)	3.72 (1.61)	1,855,875
Public social spending (control)		In thousands of dollars, available 1994–2010, extrapolated 1990-1993, includes public expenditure on education, health, environmental protection, and housing	System of state and municipal databases at the Mexican Bureau of Statistics (INEGI 2012b)	401,593 (1,101,704)	1,792,497
Public economic spending (control)		In thousands of dollars, available 1994–2010, extrapolated 1990-1993, includes public expenditure on industrial production and development, science, technology, innovation, and transportation		155,946 (214,406)	1,792,497
State net migration (control)		Sum of net interstate and international migration, net migration is defined as the difference between number of immigrants and emigrants, the calculations are projections beginning 2006	National Council of Population estimations (CONAPO 2012a)	–0.60 (0.70)	1,855,875

Note: Standard deviations are in parentheses. INEGI = Instituto Nacional de Estadística y Geografía; CONAPO = Consejo Nacional de Población.

serial correlation, the correlation within each year of observation. Serial correlation will not affect bias or consistency of the parameter estimates, but it will bias the standard errors and produce nonvalid test statistics. Clustered standard errors are robust to temporal but not to contemporaneous serial correlation. To account for this problem, model 2 in Table 2 replicates model 1 but adds standard errors robust to both types of serial correlation (Cameron, Gelbach, and Miller 2011; Thompson 2011). Parameter estimates from models 1 and 2 should be identical, but standard errors will differ.

Models 1 and 2 estimate one intercept for each school to address unobserved heterogeneity at the school level. Model 3 uses a first-difference (FD) strategy to address this problem. An FD formulation is a regression model in which all variables are expressed as changes from the prior year. Given that the term a_{ij} is constant over time, the FD formulation also eliminates any time-invariant unobserved attributes at the school level. The FE and FD models yield identical estimates when two time periods are analyzed, but they differ with more than two periods. Both estimates are consistent, and if the models are not affected by endogeneity, they should be similar within sampling error (Wooldridge 2002). FD models therefore provide a test of endogeneity and help us assess the robustness of results.

FE and FD approaches have one limitation that is usually neglected. Although they account for time-invariant unobserved heterogeneity at the school level, they assume over-time trends in children's educational outcomes are the same for all schools in the country. This assumption could be unrealistic, and it will induce bias if factors such as location of the municipality (e.g., border states versus rural southern states), level of poverty, or urbanization of the municipality shape different educational trajectories over the observation period. As an alternative strategy, we devised regression models that allow for group-specific intercepts and slopes (GSIS). GSIS models divide schools into groups that share common attributes, based on the socioeconomic status (SES) of the municipality (five SES quintiles) or the state where the school is located. We constructed the five municipality SES categories from a principal components factor analysis using eight indicators from the 1990 population census.[3] Schools were then classified into quintiles of the resulting SES index, weighted by elementary school enrollment in each municipality. Mexico is a federal system with 32 states. States are a natural way of classifying schools because they vary widely in terms of socioeconomic, cultural, and political factors.

GSIS models distinguish not only different intercepts by group (which would be equivalent to fitting an FE model at the aggregate group level instead of the school level) but also different baseline trajectories of violence over time across groups of schools by interacting the group-specific indicators with year dummies. We model school-specific departure in homicides and educational outcomes from these baseline trajectories. This strategy is an extension of the generalized FE model proposed by Morgan and Winship (2007) in which grouping is based on the allocation of a dichotomous treatment. Equation 3 presents the formulation for the GSIS models.[4]

$$\begin{aligned}(Failure\ Rate)_{ijkt} &= \beta_0 + \beta_1 (Hom)_{jkt} + \beta_2 (Group_k) \\ &+ Y_t + \beta_4 \left(Group_k \times Y_t\right) + \beta_5 (\boldsymbol{SC})_{ijkt} \\ &+ \beta_6 (\boldsymbol{MC})_{jkt} + e_{ijkt}. \end{aligned} \tag{3}$$

Failure rate in school i, municipality j, group k (k = state, municipality SES quintile), and year t is explained by the homicide rate in municipality j, group k, in year t, a set of dummies for $k - 1$ groups k (defined by municipality SES quintile or state), a set of Y dummies for $(t - 1)$ years, a set of $[(k - 1) \times (t - 1)]$ interaction terms between each group and year fixed effects, and all other variables included in previous models.

As shown in equation 3, GSIS models estimate not only one intercept but also one temporal trend for each group (municipality SES quintile or state). By so doing, GSIS models take into account the potentially divergent historical developments that groups of schools exhibit through the years. This provides a more plausible time-varying counterfactual for each school's trajectory, given by trends in each state or each municipal SES quintile. To illustrate, imagine a case in which school failure is more prevalent in poor municipalities and the school failure rate in poor municipalities increased substantially between 1990 and 2010 for reasons unrelated to local violence, whereas it remained constant in wealthy municipalities, so that these two groups of schools became more dissimilar over time. Assuming all schools followed the same trend—as the FE and FD models

Table 2. Effect of Municipal Homicide Rate on Elementary School Grade Failure, Mexico 1990–2010[a]

	1		2		3		4		5	
	School and Year Fixed Effects		School and Year Fixed Effects Serial-correlation Robust		School and Year First Differences		State GSIS		Municipal Socioeconomic Status GSIS	
	No Covariates	All Covariates	No Covariates	All Covariates	No Covariates	All Covariates	No Covariates	All Covariates	No Covariates	All Covariates
Homicide rate	0.0036**	0.0027**	0.0036	0.0027	0.0040**	0.0029**	0.0199**	0.0089**	0.0074**	0.0043**
	(0.0004)	(0.0004)	(0.0020)	(0.0016)	(0.0003)	(0.0003)	(0.0008)	(0.0007)	(0.0007)	(0.0006)
Private school								–0.0351**		–0.0350**
								(0.0003)		(0.0003)
All grades offered		–0.0073**		–0.0073**		–0.0045**		–0.0067**		–0.0058**
		(0.0005)		(0.0008)		(0.0006)		(0.0005)		(0.0005)
Principal does not teach		–0.0025**		–0.0025**		–0.0005**		–0.0160**		–0.0174**
		(0.0002)		(0.0003)		(0.0002)		(0.0003)		(0.0003)
Teacher-student ratio		–0.0939**		–0.0939*		–0.0521		–0.1042**		–0.1036**
		(0.0314)		(0.0387)		(0.0317)		(0.0188)		(0.0176)
State unemployment		0.0007**		0.0007		0.0003**		0.0004		–0.0055**
		(0.0000)		(0.0004)		(0.0000)		(0.0007)		(0.0001)
State Net migration		–0.0022**		–0.0022**		0.0000**		–0.0000		0.0009**
		(0.0003)		(0.0009)		(0.0000)		(0.0024)		(0.0002)
Municipal population		0.0000**		0.0000		–0.0008**		–0.0000**		0.0000
(thousands)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
% < twice minimum wage in municipality		–0.0006**		–0.0006**		0.0006**		0.0006**		0.0005**
		(0.0000)		(0.0001)		(0.0000)		(0.0000)		(0.0000)
% without piped water in municipality		0.0004**		0.0004**		0.0006**				
		(0.0000)		(0.0001)		(0.0001)				
% crowded dwellings in municipality		0.0004**		0.0004		–0.0000**				
		(0.0000)		(0.0002)		(0.0000)				
State social spending		–0.0000**		–0.0000**		–0.0000**				
		(0.0000)		(0.0000)		(0.0000)				
State economic spending		–0.0000**		–0.0000						
		(0.0000)		(0.0000)						
Constant	0.0927**	0.1100**			–0.0027**	–0.0024**	0.0760**	0.0669**	0.0507**	0.0829**
	(0.0002)	(0.0029)			(0.0001)	(0.0002)	(0.0022)	(0.0039)	(0.0006)	(0.0013)
R^2 within schools	0.2635	0.2772	0.2635	0.2772			0.3030	0.3976	0.2571	0.3575
Number schools	84,404	84,404	82,777	82,777	82,762	82,762	84,404	84,404	84,404	84,404

Note: GSIS = group-specific intercepts and slopes.

a. Parameter estimates associated with year fixed effects, Year × State dummies (model 4), and municipal socioeconomic status dummies (model 5) are not presented to conserve space but included in all models. *N* (students) = 1,556,636. Models 2 and 3 are estimated over a smaller number of schools because schools with only one valid observation over the period considered are dropped from the analysis.

*$p < .05$. **$p < .01$.

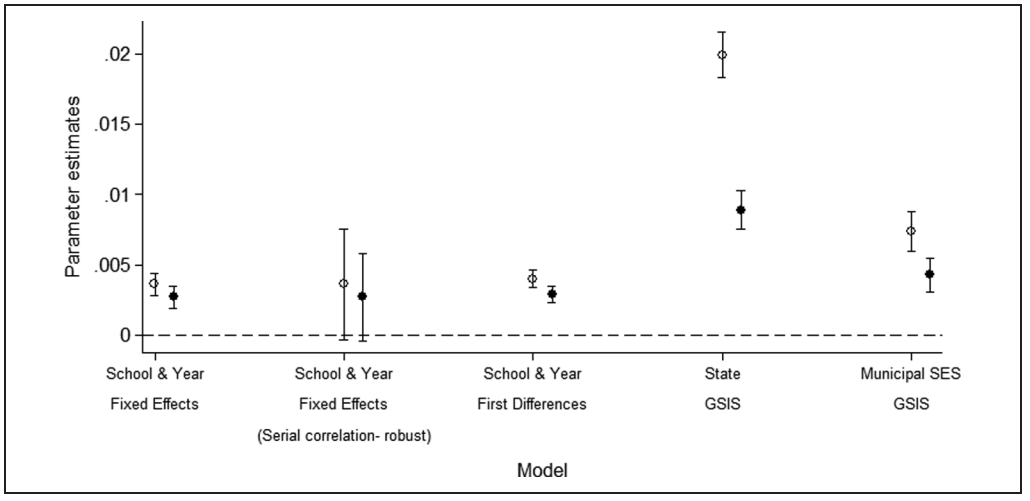

Figure 2. Effects of municipal homicides on primary school failure rate, Mexico 1990–2010
Note: Hollow circles identify parameter estimates from models without covariates (except for time); solid circles identify models with all covariates.

do—could lead one to misestimate the effect of homicides in both groups. In contrast, the GSIS models provide a more appropriate counterfactual for groups of schools with potentially dissimilar failure rate trends.

An assumption of the GSIS models, however, is that any unaccounted-for noise embedded within the school error term is random. In other words, GSIS models account for unobserved heterogeneity only across aggregate groups rather than among individual schools and are thus more vulnerable to time-invariant sources of unobserved selectivity within groups. In contrast, the FE and FD approaches account for school-specific unobserved heterogeneity as long as it does not change over time. Because assumptions of the FE, FD, and GSIS models are untestable, we use these different specifications to evaluate the stability of our findings and their sensitivity to different sources of bias.

We estimated two versions of each model. The first one includes only homicides and time variation as predictors, but it excludes time-varying covariates. This version captures a reduced-form effect. The second version adds the full set of time-varying covariates at the school, municipal, and state levels described earlier. This formulation accounts for potential determinants of spurious trends, but it likely also captures mediating factors. For example, if local homicides affect the teacher-student ratio by inducing teachers to leave the area, if economic downturn is a result rather than a cause of an increase in homicides, or if growing violence induces selective out-migration that alters the composition of the population exposed, then these time-varying factors would in fact be mediators rather than correlates of the effect of homicide on school failure. Controlling for these factors would thus account for some of the pathways of influence for the effect of violence.

Models without covariates likely produce an overestimate of the effect of interest and provide an upper bound for the causal effect of homicides. Models with full time-varying controls likely result in an underestimate of the total effect of homicides (ruling out strong suppressor effects) and provide a lower-bound estimate. Estimates with and without time-varying controls thus offer plausible bounds for the effect of interest.

FINDINGS: THE EFFECT OF LOCAL HOMICIDE ON EARLY SCHOOL FAILURE

Figure 2 presents the main findings, based on Table 2. Coefficients for the effect of local homicide from models without covariates are shown with hollow circles; solid circles represent the full set of covariates. Five models are estimated.

Model 1, using school and year fixed effects, indicates that an increase in 1 homicide per 1,000 population in the municipality where a school is located results in a .004 increase in the elementary school failure rate. This finding is consistent across models. In every case, an increase in the local homicide rate results in an increase in school failure. The effects of models without controls range in magnitude from .004 in FE and FD models to .020 in the state GSIS model. The much larger parameter estimates of the GSIS models suggest they may not provide an adequate counterfactual at the school level and may result in an overestimate of the effect of homicides.

Parameter estimates in models with the full set of covariates remain consistently positive and significant, but they are smaller in magnitude, ranging from .003 (FE and FD models) to .009 (state GSIS model). The decline in magnitude once time-varying covariates are added is most pronounced for GSIS models, suggesting that time-varying controls are essential to account for spurious trends in these models. After adding the full set of time-varying controls, the effect of homicides on the failure rate is very similar across models. Estimates from the FE and FD models are extremely similar in magnitude, suggesting the model is not affected by endogeneity. Models accounting for temporal and contemporaneous serial-correlation yield larger standard errors, providing conservative significance tests and result in parameter estimates significant only at the $p < .10$ level.

Substantive Relevance of the Effect of Homicide Exposure

Given that GSIS models may overestimate the effect of homicide exposure, we consider the parameter estimate from FE and FD models to be the most plausible measure of the effect of homicide exposure. Such estimates indicate that the increase in the homicide rate of 1 per 1,000 population results in an increase of a school's failure rate ranging between .003 (full covariates) and .004 (no covariates).

Is the effect of homicides on school failure large enough to be substantively relevant? We can use impact evaluations of several major interventions intended to reduce elementary school failure in Mexico as a benchmark. Gertler, Patrinos, and Rubio-Codina (2008) found that the School Management Support Program (an intervention designed to improve education quality launched in 1996) reduced failure rates by .004 among treated schools. Parker (2005) notes that cumulative exposure to Oportunidades (a large conditional cash transfer program for poor households) between 1996–1997 and 2002–2003 resulted in a .006 decline in the failure rate in primary school. Skoufias and Shapiro (2006) evaluated the School Quality Program (a school-level program including financial assistance, teacher development, and parental involvement launched in 2001) and found a .0024 decline in the failure rate after continuous participation in the program for three years.

Compared with the effect of these programs, the impact of exposure to community violence is substantial. Furthermore, effects estimated by these evaluations are treatment-on-the-treated (TOT) effects. In contrast, the effect of local homicides captured by this analysis is an intent-to-treat effect measured among all school children in each municipality, which likely contains substantial heterogeneity, with some children strongly affected by the environmental violence whereas others, either less exposed or more resilient, show no effect. If it were possible to estimate a TOT effect—that is, the effect only on children affected by homicides at the municipal level—this effect would be, necessarily, larger. The usual manipulation to obtain a TOT effect is to scale the intent-to-treat estimates up by the fraction of the population affected by the treatment.[5] Consequently, using the estimate in the model with all covariates, if 80 percent of children were affected, the effect would be 25 percent larger, reaching .004 (.003 / .8); if 20 percent of children were affected, the effect would reach .015.

However, we should consider that only about 1 percent of the approximately 14 million children attending elementary school in Mexico in 2010 experienced a change in homicide by 1 per 1,000 population (the measure used as treatment) in their municipality of residence. Nonetheless, as many as 53 percent of elementary school children experienced a change of 1 homicide per 10,000 population in their municipality between 2009 and 2010. We can easily adjust the parameter estimate to capture an effect of homicide rate using 10,000 (rather than 1,000) population by dividing by 10, which would result in an effect of .0003 (.003 / 10). Based on this calculation and the fact that 14.5 million Mexican children attended primary

school in 2010, an increase in local homicides of 1 per 10,000 from 2009 to 2010 would have resulted in between 2,306 and 3,074 (lower bound: .0003 × .53 × 14,500,000; upper bound: .0004 × .53 × 14,500,000) more children failing primary school. This effect is not trivial.

We have considered a long period—21 years—with much variation in terms of homicide dynamics. To assess potential change in the effect of homicides from 1990 to 2010, we further stratified our sample into three subperiods: 1990 to 1995, 1996 to 2007, and 2008 to 2010. The analysis by subperiod indicates a decline in the effect over time from .005 in the first period to .002 in the second and .001 in the third (for full results, see the online appendix). Given that the homicide rate dropped during the first two periods but sharply increased in the last one (see Figure 1), over-time variation suggests that the beneficial effect of a decline in violence on early educational outcomes is stronger than the pernicious effect of the increase in violence.

Causal Mechanisms

Our analysis provides some evidence about potential mechanisms driving the effect of local homicides on school outcomes. By controlling for changes in population size, net migration, and changes in local poverty and economic conditions, the models with full covariates address the possibility that the observed effect is driven by these demographic dynamics, selective compositional change, or economic trends. Controls for school-level factors capture, at least partially, the changing availability of financial and human resources at the school level, and controls for social expenditures at the state level provide a proxy for changing education budgets. But mechanisms referring to individual fear and anxiety, parenting strategies, school-level dynamics, and neighborhood influences remain potentially relevant. We conducted two additional analyses to explore their plausibility.

First, given that homicides are measured during calendar year t and the failure rate is measured during the school year from September of year t to July in $t + 1$, the model implies a relatively immediate effect of homicides on educational outcomes. We added further lagged terms of the predictor to assess potential delayed effects of homicide exposure, up to three years of prior exposure. The existence of lagged effects would be consistent with the over-time emergence and consolidation of a municipal- or school-level culture tolerating or promoting violence and processes of socialization or contagion emerging from such a culture. Lagged terms are consistently insignificant, and they do not add explanatory power to the models (see the online appendix). This suggests the effect of homicide exposure is short-term, occurring the year during and immediately after exposure. This time horizon is more consistent with immediate effects and adaptations, such as heightened anxiety and changing parenting styles and restrictions.

The second analysis examines the effect of homicide separately by grade in primary school. Given that factors such as engagement in deviant subcultures and gang-like behavior are more likely to emerge among older children and adolescents, an increase in the effect from first to sixth grade would support these mechanisms. Results (reported in the online appendix) show precisely the opposite pattern. The effect of homicide exposure declines across grade. This finding is more consistent with mechanisms prevalent among young children.

In summary, although it is not possible to fully disentangle the pathways linking environmental violence to children's early educational outcomes with the data at hand, this analysis suggests the effect is not driven by changes in economic conditions, demographic composition, migration, or schools' economic and human resources. Given that the impact of violence expresses itself shortly after exposure without lagged effects and that it is stronger among younger children, factors such as cultural changes or the ascription to nonconventional, gang-like values are not likely a major causal mechanism. Rather, the causal paths likely involve short-term reactions and adaptations, including heightened fear, stress, and anxiety and changes in parenting styles and strategies.

DISCUSSION

Homicide rates at the municipal level have a significant effect on the probability of failing a grade in elementary school in Mexico. The robustness of our estimates using different methodological formulations provides strong evidence for a detrimental effect of local violence on children's early educational achievement, even if they are not direct victims. These findings are a source of concern because exposure to environmental violence is

highly prevalent in contemporary societies and is unequally distributed along socioeconomic lines. To the extent that children living in poverty are more likely to experience environmental violence, its effect on early educational attainment will contribute to the intergenerational reproduction of poverty.

Moreover, the effect captured is likely a conservative estimate of the aggregate impact of local violence on children's educational outcomes. First, we estimated an "intent to treat" effect at the population level, which assumes all children in the municipality were exposed and thus underestimates the causal effect of homicide rates at the individual level. Furthermore, an outcome such as failing a grade is likely just the tip of the iceberg in terms of detrimental consequences of violence exposure on children's learning. As expected, a much larger number of children were affected in ways not severe enough to cause them to fail a grade.

We offer a methodological contribution by estimating a set of relevant causal inference models based on different counterfactual assumptions, and we found nontrivial variation across them. In principle, all models estimated are appropriate and provide consistent parameter estimates. However, their assumptions are not empirically testable. In general, researchers rely on a single model when examining an empirical question. We suggest that rather than relying on a single specification, researchers should routinely report results from alternative plausible models. This practice would help assess the robustness of the findings and provide a more realistic range of estimates.

Our study provides strong evidence about a causal influence of changes in local violence at the aggregate level. It also raises several further questions. First, the effect of local homicide likely reflects several interrelated pathways of influence. Our empirical examination suggests children's stress and anxiety and changes in parenting dynamics are the most likely mechanisms in the Mexican case. But this is only suggestive. Quantitative analysis of individual-level data could help further elucidate mechanisms, but this effort also requires in-depth qualitative examination in the affected communities. Second, the population-level effect captured is likely heterogeneous, varying across children along exposure, vulnerability, and resilience lines. More research using data at the individual level is necessary to identify populations most at risk. We hope this analysis has shown convincing evidence of the damage environmental violence causes on early educational achievement and will motivate further study of environmental determinants of human capital formation in different national contexts.

AUTHORS' NOTE

An earlier draft of this manuscript was presented at the 2013 annual meeting of the Population Association of America, April 11–13, New Orleans, Louisiana.

ACKNOWLEDGMENTS

The authors are grateful to Rajeev Dehejia, David Greenberg, Nicole Marwell, the *Sociology of Education* editor, and anonymous reviewers for their very helpful suggestions.

NOTES

1. We include all regular elementary schools in the country but exclude "indigenous" and "community" schools—serving about 6 percent of Mexican children—because information about these schools comes from a different source and their organizational characteristics differ from regular elementary schools.
2. Grades are assigned on the basis of academic performance and range from 5 to 10, with 6 being the minimum passing grade. If students do not reach a grade of 6, they automatically fail.
3. Variables used are percentage of illiterate population; population without elementary school; employed population earning less than twice the minimum wage; inhabitants living in households without drain network, without plumbing, without electricity, with dirt floor, and with overcrowding; and population living in localities with fewer than 5,000 inhabitants.
4. Group-specific intercepts and slopes models also add a time-invariant indicator coded 1 if the school is private.
5. In instrumental variable parlance, this is equivalent to excluding the "always takers" and "never takers" to focus on the "compliers" (Angrist, Imbens, and Rubin 1996).

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