Violence-Induced Migration and Peer Effects in Academic Performance

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Violence-Induced Migration and Peer Effects in Academic Performance

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Abstract

We document that local violence generates spillover effects beyond areas where violence takes place, via out-migration from violence-affected areas and peer exposure to violence. We study out-migration due to drug-trafficking-related violence in Mexico between 2006 and 2013. We use violence-induced student migration as an exogenous source of variation in peer exposure to violence to estimate its effects on student academic performance in relatively safe areas. Our results show that municipalities that face more violence experience higher rates of student out-migration. In receiving schools in areas not directly affected by violence, adding a new peer who was exposed to local violence to a class of 20 students decreases incumbents' academic performance by 1.2 percent of a standard deviation. Negative effects are more pronounced among girls and high-achieving students.

JEL classification: I24, I25, O15.
Keywords: Local violence; out-migration; in-migration; peer effects.

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1 Introduction

The effects of local violence can be disseminated through different channels and are unlikely to be restricted to the geographic area in which the violence takes place. Yet, most research on the effects of violence on educational outcomes has focused on the effects of exposure to violence in the area in which the violence occurs. Relatively little is known about spillovers into other areas that are not directly impacted by violence. We address this issue by studying how local violence affects human capital accumulation in other geographic areas via migration and schooling, in the context of the pronounced drug trafficking-related violence that Mexico experienced between 2006 and 2013. In doing so, we provide the first causal evidence on the effects of drug-related violence on students' outcomes in areas that are not directly affected by violence, through peer exposure to violence.

Between 2006 and 2013, the Mexican homicide rate more than doubled. Importantly, the increase in violence linked to Mexico's drug war was heterogeneous across municipalities, with some municipalities not experiencing increases in their homicide rates and others seeing their homicide rates escalate exponentially. Using linked administrative data and a difference-in-differences research design, we first show that students are more likely to out-migrate from municipalities that exhibited increases in their homicide rates. Then, we exploit this violence-induced migration as a natural experiment to examine how incumbent students in relatively safe municipalities are affected by the arrival of migrant peers who were exposed to drug-related violence in their municipalities of origin. The violence-induced migration of students to safer municipalities provides a setting in which exposure to violence is exogenous to incumbents' performance, which allows us to deal with the reflection problem (Manski, 1993) that typically arises in peer effect estimates.[1] To control for potential selection of migrants into schools, our peer effect estimates exploit the variation in exposure to peers

[1] Exposure to violence happens before students enroll in their new schools in safer areas, and hence it is not subject to the reflection problem, given that it is likely to be exogenous to incumbents' classroom performance and behavior. An extensive review of the literature on peer effects in education that covers both theoretical models and empirical applications can be found in Epple and Romano (2011).

arriving from violent municipalities across cohorts within a particular school-grade.[2]

Mexico offers an ideal context to study peer effects due to violence-induced migration because of a combination of features. First, there is significant geographic and temporal variation in the increase in local violence that we exploit in our analysis. Second, unlike studies carried out in other settings (for example, in the U.S.), we have access to student-level census data that allows us to follow students over time, across states, and across public and private schools, providing an accurate and complete measure of student migratory flows. Third, the large sample allows us to precisely estimate the effects of interest by implementing a set of fixed-effects models to control for unobservable factors that affect students' performance. Finally, we are able to combine different sources of information to examine heterogeneous impacts for different types of students and to explore potential mechanisms behind the estimated effects on migration and test scores.

Our results show that in the first year a municipality homicide rate reaches the fourth quartile of the cross-municipality distribution of homicide rates, it experiences a 0.9 percentage-point increase in the probability of a student out-migrating to a relatively safer municipality. This effect grows over time, reaching 3.9 percentage points after 6 years. Consistent with the pull-push forces discussed in the migration literature, where the threat of being exposed to violence acts as a push factor and the prevalence of safer environments acts as a pull factor, we also find that the probability of migrating to municipalities in the highest quartile of the national distribution of homicide rates declines.

Peer effect estimates indicate that the presence of new students who were exposed to local violence in their municipalities of origin significantly reduces incumbents' academic performance in safer municipalities. For a class of 20 students, these estimates are consistent with reductions in composite (math and reading) test scores of 1.2 percent of a standard deviation as a consequence of having one new peer who migrated from a violent municipality. Our results are robust to different specifications, and we show that they are unlikely to

[2] In the context of peer effect estimates, this approach has been followed by Hoxby (2000); Lefgren (2004); Carrell and Hoekstra (2010); Lavy and Schlosser (2011); and Carrell et al. (2018), among others.

be driven by endogenous responses such as the out-migration of incumbents or selection of migrant students to school-grades based on incumbents' observable characteristics. We also show that future influxes of peers exposed to violence are not related to incumbent students' academic achievement, which suggests that reverse causality is not a threat to our identification strategy.

We investigate potential heterogeneous impacts by gender and the ability of incumbent and migrant students. Consistent with the literature on the effects of exposure to violence on externalizing behaviors, boys exposed to violence generate stronger classroom and academic performance disruptions than girls. The arrival of new peers from violent municipalities has more detrimental effects on girls than on boys.[3] Unconditional quantile regression estimates indicate that the negative effects on incumbents are higher for students whose test scores are above the median of the national distribution.[4] Moreover, we find that students arriving from violent places whose (initial) test scores are below the median test score relative to the distribution in the school-grade they arrive to are particularly disruptive. Specifically, the estimated peer effect related to low-ability students who arrive from violent places is roughly twice as large as the estimated peer effect from exposure to low-ability peers arriving from other nonviolent municipalities. Finally, we provide evidence that these differences in the effects between peers who arrive from violent and nonviolent municipalities are not explained by the quality of the peers; on average, new peers coming from violent municipalities have higher initial test scores than those coming from schools not exposed to violence.

This paper contributes to different strands of the literature on migration and peer effects. We add to the literature that examines how influxes of relocating students affect students in the receiving areas. This includes the effects of immigrants, refugees, people who are

[3]These results are consistent with Chang and Padilla-Romo (2020), who show that exposure to local violence near schools decreases girls' performance on high-stakes standardized tests more than boys' performance.

[4]Different explanations may be generating this result. For example, if high-ability students are less likely to be exposed to violence outside the school than low-ability students, the arrival of peers exposed to violence may represent a more striking shock for them. This result could also be related to school inputs' having a higher impact on higher achieving students than on lower achieving students. Monteiro and Rocha (2017) present suggestive evidence in favor of this hypothesis for Brazil.

displaced due to natural disasters, and students relocated to different areas in the same city (Betts, 1998; Angrist and Lang, 2004; Gould et al., 2009; Jensen and Rasmussen, 2011; Imberman et al., 2012; Brunello and Rocco, 2013; Geay et al., 2013; Ohinata and van Ours, 2013; Schneeweis, 2015; Hunt, 2017; Figlio and Özek, 2019; Morales, 2019; Figlio et al., 2020). Among these studies, our paper is closely related to the peer effects literature on refugees and immigrants who move away from conflict zones. While immigrants and refugees tend to relocate to distant areas with different cultures and educational systems, we analyze relocation to municipalities in the same country, with a homogeneous educational system. This allows us to implement an identification strategy that is likely to provide a cleaner measure of peer effects due to violence exposure and not to adjustment to a new culture or educational system or lack of language proficiency. Moreover, given that incumbents and migrants are part of the same education system and subject to the same standardized tests, we are able to study heterogeneous impacts by comparing the relative achievement of both groups, and to observe a comparable measure of ability prior to migration for students who migrate.

In addition, we add to the relatively narrow literature that analyzes how having peers who were exposed to different types of violent environments may affect students' performance. Specifically, studies have analyzed the effects of having peers who have been exposed to domestic violence (Carrell and Hoekstra, 2010; Carrell et al., 2018); corporal punishment at home (Le and Nguyen, 2019); and neighborhood violent crime within a city (Burdick-Will, 2018). However, the peer exposure to violence we study in the context of Mexico's drugs war has distinct characteristics. These include extremely violent and public homicides, local increases in homicide rates significant enough to generate reductions in life expectancy for men (Aburto et al., 2016), and large degrees of variation in exposure to violence across both time and space.

Finally, our analysis contributes to the literature on the effects of drug-related violence in Mexico on education outcomes and migration decisions. Prior studies document that increases in local violence are associated with significant declines in test scores (Jarillo et al., 2016; Orraca-Romano, 2018; Chang and Padilla-Romo, 2020; Michaelsen and Salardi, 2020); significant reductions in years of education and the probability of school completion for young adults (Brown and Velásquez, 2017); and significant increases in grade repetition (Caudillo and Torche, 2014) in affected areas.[5] Importantly, we find positive selection in the out-migration of students (in terms of academic outcomes and socioeconomic status) in areas in which violence increased. Consequently, our estimates not only complement the literature on the local effects of exposure to violence on student outcomes, but also shed light on the important issue of selection.

In terms of the effects of violence on migration, the literature has found that increases in violence are associated with higher rates of out-migration (Ríos, 2014; Márquez-Padilla et al., 2019; Sobrino, 2019; Aldeco Leo et al., 2019).[6] While previous studies use a combination of survey data, population aggregates, and data on particular time periods to recover the effect of violence on migration, our estimates are obtained using a panel dataset that contains individual-level annual data for the universe of students enrolled in the Mexican school system. The nature of the data we use allows us to document out-migration patterns by directly observing individual school-location choices over time. One of the advantages of this approach is that a large sample allows us to recover precise estimates of the effects of interest for both the overall population of enrolled students and relevant subsamples. Also, not relying on geographic aggregates of population sizes to infer migration decisions has the advantage of not attributing changes in population sizes that are related to endogenous effects of violence on mortality or fertility to migration decisions. In this sense, our contribution is twofold: First, we are able to precisely estimate the effects of local violence on migration decisions, and second, we combine these estimates with panel data for students in relatively

[5] Similarly, in another Latin American context, Monteiro and Rocha (2017) find negative effects of violence on test scores in the context of drug battles in Rio de Janeiro, Brazil.

[6] One exception is Basu and Pearlman (2017), who find that violence did not increase domestic relocation. In addition, using survey data for 2005 and 2009, Brown and Velásquez (2017) find that while, on average, migration across municipalities was not driven by violence, violence increased the probability of migration among individuals with more educated mothers.

safe areas to create an exogenous measure of peer exposure to violence and estimate the spillover effects of local violence on safe areas.

The paper proceeds as follows. Section 2 provides background information on the Mexican war on drugs and the structure of the education system. Section 3 presents the data. Section 4 discusses the empirical strategy. Section 5 presents our main estimates for migration decisions (5.1), heterogeneous impacts on migration decisions according to students characteristics (5.2), peer effects for the complete sample (5.3), and peer effects and their interaction with gender (5.4) and student ability (5.5). Section 6 addresses potential threats to the identification. Section 7 discusses potential mechanisms, and Section 8 concludes.

2 Background

2.1 The War on Drugs

On December 11, 2006, during the second week of his term, then-Mexican president Felipe Calderón declared a war on drugs, citing drug trafficking as the "greatest threat to national security." Calderón's strategy consisted of a frontal attack led by the Mexican Army, the Navy, and the federal police to target areas considered highly dangerous. The goal was to disturb drug-trafficking organizations' (DTOs) capacity to produce and distribute drugs and to restore safety to places that had been "held hostage by the DTOs." The first operation took place in Calderón's home state, Michoacán, where more than 5,000 members of the federal armed forces were deployed across the state to eradicate drug crops, confiscate drugs and guns, and capture members of criminal organizations. Shortly after, operations similar to the *Operativo Conjunto Michoacán* were repeated in other parts of the country.

In the first few months after the first operation, Calderón's strategy seemed to be effective in reducing violence. The monthly homicide rate dropped by roughly 20 percent from December 2006 to January 2007 and remained lower than pre-war levels during 2007. Yet, starting in 2008, the war on drugs led to an unprecedented increase in violence in Mexico.

Moreover, the violence was brutal and well publicized, with heads and dead bodies displayed in public spaces (Williams, 2012). During Calderón's term, Mexico's annual homicide rate more than doubled, increasing from 10 to 23 homicides per 100,000 people between 2006 and 2012. Researchers across disciplines have attributed these increases in municipalities' levels of violence to the different strategies used in the war on drugs, including the deployment of federal armed forces (Escalante Gonzalbo, 2011; Merino, 2011); the targeting of high-ranked members of DTOs (Calderón et al., 2015; Phillips, 2015; Lindo and Padilla-Romo, 2018); the fragmentation of DTOs (Sobrino, 2019); and, more broadly, to the enforcement of drug policies (Dell, 2015).

Important to our analysis is that while some municipalities saw striking increases in violence during this period, many others remained relatively safe.[7] Considering the uneven geographic distribution of the drug war's impact on violence, we classify municipalities as either *never-violent* or *ever-violent* depending on whether their annual homicide rate was always below a critical threshold in the period 2006-2013.[8] Figures 1 and 2 show the geographic distribution and homicide rate trends for these two groups of municipalities, respectively.[9] It can be seen that the average homicide rate in ever-violent municipalities grew dramatically between 2006 and 2013; this was not the case for never-violent municipalities, where average homicide rate continued to be stable at less than 5 per 100,000 people per year.

[7]For example, according to the Citizens' Council for Public Security and Criminal Justice, Ciudad Juárez had the world's highest homicide rate in 2010 (229 homicides per 100,000 people) among cities with 300,000 or more inhabitants. In 2011, five Mexican cities were among the world's top 10 most violent cities, with homicide rates ranging between 78.04 and 147.77 per 100,000 people: Ciudad Juárez, Acapulco, Torreón, Chihuahua, and Durango. In contrast, more than 1,000 municipalities had no homicides in 2010.

[8]We define ever(never)-violent municipalities as those whose homicide rate was ever (never) above the 75th percentile (or 18.01 per 100,000 people), considering the distribution of average homicide rates across municipalities in the period 2006-2013. According to our definition, of the 2,454 municipalities in Mexico, 1,523 are classified as ever-violent and 931 as never-violent.

[9]To be consistent with the rest of the analysis, we define our variables in terms of academic years. For example, 2006 represents the academic year 2005-2006 (August 2005 to July 2006).

2.2 Education System

At first glance, Mexico's education system is similar to the U.S. in structure. It comprises preschool (grades K1-K3, ages 3-5); primary school (grades 1-6); middle school (grades 7-9); high school (grades 10-12); and higher education. Our analysis focuses on primary school children enrolled in grades 3-6. In Mexico, primary school is compulsory, required for progression to middle school, and coverage is almost universal. In the 2012-2013 academic year, 98.5% of children aged 6-11 were enrolled in school, of which 91.5% were enrolled in a public school.

Mexican education differs from U.S. education in several essential respects. First, and most important to our analysis, students are not tethered to a school district of residence but may enroll in any school in the country with available seats. Second, study plans and academic curricula for both public and private schools are set by the federal government through the Mexican Secretariat of Public Education (SEP). Finally, Mexican public primary school is provided in three modalities. In case of over-subscription, school principals decide which students to admit based on the SEP and local authorities' guidelines.

The three modalities—general, indigenous, and community courses (CONAFE)—enrolled 93.5%, 5.7%, and 0.8% of primary school students, respectively, in the 2012-2013 academic year (SEP, 2013). Their enrollment levels reflect how different their target populations are. The typical general school is located in an urban area and has one teacher per class. Indigenous and CONAFE schools, on the other hand, are often located in rural areas and are multi-grade.[10] Indigenous education is bilingual and bicultural and intended to promote and preserve the customs, traditions, and other elements of ethnic culture. CONAFE schools were created to offer education to children in rural areas in Mexico who, for various reasons, lacked access. Instructors are normally young people aged between 16 and 29 from rural areas and with a minimum education level of ninth grade. In exchange for a monthly stipend

[10] That is, their teachers have students from multiple grades in the same classroom. All CONAFE schools are multi-grade, and 95% are located in rural areas.

and a government-provided scholarship to continue their education, they settle and provide instructional services in a CONAFE community for 1 to 2 years (SEP, 2019).

3 Data

We construct an individual-level panel dataset that combines data on test scores, student school location choice, and exposure to local violence. To study peer effects, we analyze primary school students' performance using data from *ENLACE* (National Assessment of Academic Achievement in Schools), an annual standardized test administered to students enrolled in public and private schools in Mexico by the Mexican Secretariat of Public Education (SEP). In primary schools, ENLACE was administered to students in grades 3 to 6, in the academic years 2005-2006 to 2012-2013. The test covered math, reading, and a third rotating subject (i.e. science, history, geography, or civic education) each academic year. It consisted of different blocks of multiple-choice questions organized by subject, and was proctored by independent supervisors over a period of 2 days in different testing sessions. ENLACE was designed to provide diagnostic information on students' performance for the universe of students enrolled in the Mexican education system for parents, schools, and policymakers. Test scores were not used to determine students' course grades or admission to upper educational levels, however. Our main outcome variable is individual-level standardized composite scores, which are calculated as the sum of math and reading test scores from ENLACE.

We track students over time using anonymized individual identifiers that are linked to ENLACE test scores. We observe students' academic performance (measured by ENLACE standardized test results in math and reading) and the primary school in which they were enrolled on the exam date for each academic year they participated in ENLACE. As described earlier, the data contain information on students in all Mexican states and in all types of schools (i.e., private, public, urban, and rural). Using this information, we identify student migration across schools within municipalities and also across municipalities and states in Mexico. We build an individual-level unbalanced panel, in which the number of observations for a particular student depends on their grade in 2006 and the number of times they took the test when it was administered. Given that our measure of migration relies on observing school choices over time, we exclude students who are in the data only once and students who did not take the test in consecutive years.[11]

In order to examine the effects of the arrival of new peers on incumbents' academic performance, we consider students in grades 4 to 6 in public and private schools. Students in grade 3 are excluded from the sample because it is the first grade in which students take the test. This means that we are unable to identify whether these students were in the same school in the previous academic year, unless they are repeating the grade. For our peer effect estimates, we also drop from the main analysis schools that have the potential to be multi-grade–i.e., CONAFE and indigenous. In these types of schools, it is difficult to identify whether the school-grade composition was affected by the arrival of peers from violent municipalities. Finally, we also drop data for the academic year 2005-2006 from our out-migration sample, because a high fraction of students lack valid individual identifiers.[12]

ENLACE gathered information on school environment and on parents' levels of education for a sample of students and parents throughout the *Contexto* (Context) survey. We use this information to study selection in migration decisions and to explore different mechanisms that can potentially explain our results.[13] In addition, we combine the student-level data with school-level administrative data collected by the Mexican government at the beginning and end of each academic year in *Estadística 911* (Statistics 911). This dataset contains information for all schools in Mexico on the number of students by grade that allows us to compute measures of attrition in the ENLACE exam.

[11]See Appendix B for a more detailed description of ENLACE data and its attrition patterns over time. Overall, we show that municipalities' attrition patterns are not correlated with their levels of violence.

[12]Our results are robust to these exclusions from our sample.

[13]Given that the design of this survey has been modified over time, our analysis using Contexto includes individuals surveyed in the period 2008-2013, in which the questions used in our analysis are homogeneous.

An important data component of our empirical strategy is a measure of local exposure to violence. We use official mortality reports by the National Institute of Statistics and Geography of Mexico (INEGI). The number of homicides is calculated as the number of deaths that occurred due to violent or accidental causes that have been registered as presumed homicides. Combining information on the municipality in which the homicide occurred, the date of its occurrence, and official population counts from the National Population Council (CONAPO), we build a measure of exposure to violence that varies at the municipality and academic-year level.[14] Using homicide rates, Table 1, Panel A shows municipality-level descriptive statistics for 2005, considering municipalities classified as never-violent and ever-violent depending on whether their annual homicide rate was always below a critical threshold in the period 2006-2013 (18.01 homicides per 100,000 people).[15] Never-violent municipalities exhibit relatively better average development indicators summarized by the marginalization index,[16] as well as by municipal statistics on primary school completion and the proportion of families with running water, sewer, electricity, or dirt floors. Never-violent municipalities are (on average) 33% more populous than ever-violent municipalities.

4 Identification Strategy

To estimate the effects of exposure to violence on students' out-migration behavior, we use a difference-in-differences research design that leverages within-student variation in levels of violence in the municipality of school attendance. That is, we compare a student's decision to out-migrate with the same student before and after a municipality becomes violent. We estimate models that allow the effects to evolve over time using the following baseline specification:

[14]For example, the 2006-2007 academic year runs from August 2006 to July 2007.

[15]This threshold is the 75th percentile of the national homicide rate distribution in the period 2006-2013. In Appendix C, we show that our results are robust to using alternative thresholds (i.e., 65th and 70th percentiles).

[16]The marginalization index is a summary-measure of lack of access to education, inadequate housing, insufficient income, and rurality.

$$Migrate_{ismt} = \alpha_i + \gamma_t + \mu_m + \Sigma_{k=0}^{6} \delta_k Violent_{m,t-k} + \epsilon_{ismt}, \tag{1}$$

where $Migrate_{ismt}$ is an indicator variable of whether student i enrolled in school s in municipality m out-migrated in academic year t; α_i are student fixed effects; γ_t are academic year fixed effects; μ_m are municipality fixed effects; $Violent_{m,t-k}$ is an indicator variable that takes the value one k years after municipality m's level of violence is in the fourth quartile of the cross-municipality distribution of average homicide rates for the first time (i.e., the municipality "becomes violent" according to our classification) and zero otherwise; and ϵ_{ismt} is an error term we allow to be correlated within municipalities. The coefficients of interest are δ_k, and measure the average effect of a municipality becoming violent on the probability of out-migrating k years later. The validity of this difference-in-differences research design relies on the assumption of common trends in out-migration behavior for students in violent and nonviolent municipalities. We provide supporting evidence for this assumption by showing that out-migration trends for these two types of municipalities do not diverge prior to them being classified as violent.

We then use this violence-induced migration as a natural experiment to estimate the effects of having peers from violent municipalities on students not directly affected by the increase in violence in the host schools. Our variable of interest is $Share_{isgmt}$, the share of student i's peers exposed to violence in their municipality of origin. This variable is calculated as follows:

$$Share_{isgmt} = \frac{\Sigma_{h \neq i} Exposed_{hsgmt}}{n_{gst} - 1}, \tag{2}$$

where n_{gst} is the number of students in grade g at school s in academic year t, and $Exposed_{hsgmt}$ is an indicator variable equal to one if student h observed in school s in grade g and municipality m in period t was exposed to violence in their municipality of origin. It is important to note that our specification considers a student h to be exposed in each period after he or she arrives in the host school.[17]

To estimate the effects of having peers exposed to violence on *incumbent students*' academic performance in never-violent municipalities, we use a difference-in-differences research design. Intuitively, we compare differences in academic performance between students in school-grades with a high concentration of peers exposed to violence in their municipality of origin and students with a low concentration of such peers. In period t, we define incumbent students as those who are enrolled in school s, located in (a nonviolent) municipality m, provided they were enrolled in the same school in every period prior to t in which students took ENLACE.[18] We estimate the following model:

$$TS_{isgmt} = \lambda_{sg} + \eta_{gt} + \sigma Share_{isgmt} + \beta_1 TS_{isgm,t-1} + \beta_2 Female_i + u_{isgmt}, \tag{3}$$

where TS_{isgmt} is a standardized measure of academic performance (composite test score) for student i in school s, grade g, municipality m, and academic year t; λ_{sg} are school-by-grade fixed effects; η_{gt} are grade-by-year fixed effects; $Share_{isgmt}$ is the share of students who migrated from municipalities with a level of violence in the fourth quartile, in school s, grade g, municipality m, and academic year t; $TS_{isgm,t-1}$ is student i's composite test score in year $t-1$; $Female_i$ is an indicator for whether individual i is a female; and u_{isgmt} is an error term we allow to be correlated within schools. The coefficient of interest, σ, captures the effects of going from zero to all peers in-migrating from violent municipalities. To account for the fact that new students may be allocated to a particular class within a grade in a nonrandom fashion, we use variation at the school-grade-year level as opposed to school-class-year.

We exploit the panel structure of our dataset and use school-grade fixed effects to control for migrant students' selection into schools, which is likely to happen. Then, our identification strategy relies on comparing outcomes for the same grade in a particular school over time, and specifically on comparing cohorts with higher exposure to peers from violent mu-

[17] The dimension of our panel implies that students are considered to be exposed in never-violent municipalities for up to 3 years after out-migrating from violent areas.

[18] That is, migrating students never become incumbents.

nicipalities to cohorts with lower exposure to this type of peer. The validity of our estimates relies on the absence of selection of students into or out of school-grades based on the share of students migrating from municipalities with levels of violence in fourth quartile, and on the absence of correlation between the within-school-grade variation we are exploiting and other determinants of students' performance.[19] To support our identification, in Section 6 we show that incumbent student characteristics (such as initial performance or parental education) are not correlated with the share of peers exposed to violence, and that adding controls to our main identification does not significantly alter our results. We also show that the arrival of new students from violent environments did not generate the out-migration of incumbents. Finally, we show that future shares of students arriving from violent municipalities do not affect incumbents' current academic outcomes.

5 Results

We start by estimating the effect of local violence on the probability of students' out-migration. Then, we use this variation in out-migration to estimate how being exposed to new peers who migrated from violent environments affects incumbent students' test scores in safer (never-violent) municipalities.

We begin our analysis by showing how homicide rates evolve over time for ever-violent municipalities, relative to never-violent ones. Specifically, we estimate the effects on the homicide rate for the years prior to and after a municipality is first classified as violent (belonging to the fourth quartile of the homicide distribution across municipalities) using

[19]A potential threat to identification could emerge if there are shocks that explain both changes in the share of students from violent municipalities and outcomes of the incumbents. However, we believe that this is unlikely to be a significant concern in our context, for several reasons. First, the shocks that generate out-migration happen in a different geographic area from where incumbents live and are unlikely to affect them directly. Moreover, our preferred model includes grade-by-year fixed effects to control for the effect of common shocks at the grade-year level on academic performance. In addition, we construct the shares using not just the variation that comes from the arrival of new students to a classroom in a particular year, but the cumulative number of students who migrated from violent municipalities. Carrell and Hoekstra (2010) follow a similar approach and consider cumulative exposure to domestic violence to be a measure of peer exposure to violence.

a municipality-level model similar to Equation 1, but in which the outcome variable is municipalities' homicide rates. The estimated effects are shown in Figure 3. Homicide rates in ever-violent and never-violent municipalities were trending similarly prior to them becoming violent, and point estimates for the years prior to treatment are close to zero and statistically nonsignificant. The first year a municipality is classified as violent, its homicide rate increases by roughly 40 homicides per 100,000 people. The homicide rate is 20 units higher 1 year after treatment with respect to the pre-treatment year, then follows an increasing pattern over time and reaches 35 homicides per 100, 000 people 7 years after the first year a municipality is classified as violent. Overall, violent municipalities experienced a sudden, sharp, and sustained increase in violence that might affect students' migration decisions in the short and long run.

5.1 Effects of Local Violence on Migration

Before showing causal evidence for the effects of local violence on students' migration decisions, in Figure 4 we present the relationship between students' perceptions of safety near their school and the probability of migrating to a nonviolent municipality. Overall, there is a negative correlation between perceptions of safety and out-migration. This provides suggestive evidence that violence in ever-violent municipalities is correlated with students' perception of safety; at the same time, students who are more exposed to violence are more likely to migrate to a safer environment.

Using our main dataset, Figure 5 shows event-study estimates of the probability of out-migration for the years prior to and after a municipality is first classified as violent, expanding our baseline specification in Equation 1 to include leads and lags. All estimates are relative to the omitted year—i.e., 1 year prior to a municipality becoming violent. Overall, although not significant at conventional levels, the probability of out-migrating increases with every additional year since a municipality first became violent. In addition, the effects on the years prior to violence are close to zero and not statistically significant, providing support for our identification strategy.

While the effects shown in Figure 5 suggest an increase in the probability of out-migration due to local violence, they do not reflect the overall impact of the influx of students who move away from violence to safe municipalities. The reason is that violence not only increased the probability of out-migration but also shaped migration decisions regarding the destination. In Figure 6, we show that the probability of migrating to a safe municipality significantly increases once a municipality becomes violent, and that this effect is increasing over time. The opposite happens when considering the probability of migrating to another violent municipality, which declines over time. Moreover, the effects on out-migration to violent and safe municipalities in the years prior to violence are small in magnitude and not statistically significant, suggesting that there is no evidence of divergence among violent and safe municipalities in the years prior to treatment.

Table 2 shows the estimated effects on out-migration patterns. Column 1 shows the estimated effects on moving to a different municipality. Columns 2 and 3, respectively, show the estimated effects on migrating to a nonviolent and to a violent municipality. The estimates in Column 1 indicate that in the first year a municipality is classified as violent, the probability of out-migration increases by 0.2 percentage points and that this effect grows over time. The estimates in Column 2 indicate that students are 0.9 percentage points more likely to migrate to a nonviolent municipality when a municipality becomes violent, and that this effect grows to 3.9 percentage points 6 years after. Finally, the estimates in Column 3 indicate that a student is 0.7 percentage points less likely to migrate to a violent municipality when violence starts, and this negative effect increases to 3.2 percentage points after 6 years.

5.2 Who Migrates?

We have shown that for the overall sample, violence increases out-migration to nonviolent municipalities and decreases out-migration to other violent municipalities. A natural question is whether these effects are homogeneous across students. This analysis is motivated by the positive selection of migrants found in the migration literature (Bauernschuster et al., 2014; De la Roca, 2017; Aldeco Leo et al., 2019), in which migrants have higher levels of education and earnings than those who stay. Specifically, we estimate the effects of violence exposure on out-migration decisions by students' initial academic performance and socioeconomic status.

Figure 7 shows the estimated effects by student performance, measured by initial composite scores. We define high- and low-achieving students as those in the bottom and top quartile of the test score distribution, respectively. Overall, there is more mobility for high achievers than for low achievers. Students in the top quartile of the cumulative national initial test score distribution are more likely to migrate out of violent municipalities and to nonviolent municipalities than students in the bottom quartile. The estimated effects for high achievers grow from 0.9 percentage points in the year of treatment to 4.5 percentage points after 6 years. These results indicate that students out-migrating (new peers) from violent environments have higher test scores than the average student in Mexico.

To examine how socioeconomic status interacts with the probability of out-migration as a response to increased local violence, we combine information from our main dataset (ENLACE) with survey information on students' socioeconomic background and school environment collected for a subsample of the student population (Contexto). We define students as belonging to households with low-educated parents when the the maximum level of formal education of both parents is less than high school (i.e., less than the compulsory education level in Mexico).[20] Figure 8 shows that there is positive selection in out-migration. Students belonging to households with higher levels of parental education are approximately twice as likely to emigrate from violent municipalities than students in families with lower parental education attainment. This strong response holds when considering both the positive probability of out-migrating to nonviolent municipalities and the lower probability of

[20]The estimated effects on out-migration for students taking the survey follow the same patterns as those in our main sample (as shown in Figure A.1 in Appendix A), implying that the Contexto survey is carried out on a sample that is *as-good-as-random* for the purposes of examining the mechanisms behind our main results.

out-migrating to a different violent municipality. These results suggest that barriers to migration (such as information friction, relocation costs, and labor mobility costs) are difficult to overcome for poorer families.[21]

5.3 Peer Effects on Incumbents' Academic Performance

Considering the sample of never-violent municipalities, Table 3 shows the estimated effects of being exposed to peers who in-migrated from violent municipalities on incumbent students' test scores. Column 1 presents estimates from our baseline specification in Equation 3, which includes school-by-grade fixed effects, year-by-grade fixed effects, lagged test scores, and a female indicator. In Column 2, we additionally control for the share of peers who migrated from other nonviolent municipalities. In Column 3, we present our preferred specification, which additionally controls for the share of new peers migrating from other schools in the same municipality. In Column 4, we test whether the future arrival of peers exposed to violence affects current students' performance in a particular school-grade, by including the future share (i.e., the share in period $t + 1$) of peers exposed to violence for a particular school-cohort as a robustness exercise.

The results indicate that regardless of whether new peers migrate from violent municipalities, from other nonviolent municipalities, or from other schools in the same municipality, being exposed to new peers adversely affects incumbents' test scores.[22] However, the negative effect on test scores is twice as large when the new peers migrated from violent municipalities than from nonviolent municipalities, and roughly three times as large for those coming from other schools in the same municipality. To put the size of the estimates in context, having an additional peer from a violent municipality in a class of 20 students reduces test scores by 1.2 percent of a standard deviation.[23] In addition, the point estimate on the coefficient

[21] For example, De la Roca (2017) shows that there is positive selection of migrants in Spain, where those who move have higher levels of education and higher earnings that those who stay. In the context of Germany, Bauernschuster et al. (2014) find that mobility is higher among more educated individuals.

[22] These results are consistent with the literature on student turnover, which is found to be harmful for movers' and non-movers' academic performance (Hanushek et al., 2004).

[23] The effects are in line with the effects reported by Carrell et al. (2018), who use data for Alachua County

of the future share is economically and statistically insignificant, which suggests that reverse causality is not a threat to our identification strategy.

It is possible that high-achieving students are more likely to benefit from a disruption-free class environment, so we examine the possibility of having heterogeneous effects on high- and low-achieving students. In Figure 9, we show the estimated effects of having new peers from violent and nonviolent municipalities and from the same municipality on the unconditional quantiles of the test score distribution using the method developed by Firpo et al. (2009). These results indicate that having new peers negatively affects incumbent students' test scores at each point in the test score distribution, with significantly larger negative effects when new peers come from violent municipalities. The effects of having new peers from violent municipalities are more detrimental for students above the median of the performance distribution. As can be seen in Figure 9, having new peers from violent municipalities has strong effects on test scores at the high end of the test score distribution, with negative estimated effects that increase from 0.4 percent of a standard deviation at the 10th percentile to 1.7 percent at the 90th percentile when a new peer exposed to violence arrives to a class of 20 students.

Figure 10 presents additional heterogeneous impacts by grade, subject, and parental education. Specifically, we use our preferred specification in Table 3 Column 3, for each subsample. Overall, there are no statistically significant differences in the average effect of having peers exposed to violence between lower and higher grade levels, math and reading test scores, or between students with high and low parental education. However, point estimates are the largest for fifth graders, math test scores, and students for whom at least one parent has a high school diploma or higher.

(Florida) and find that adding one student exposed to domestic violence to a class of 20 students reduces test scores by 1.7 percent of a standard deviation.

5.4 Peer Effects and Gender

We now allow for the possibility that boys and girls emigrating from violent municipalities will affect their peers differently. This analysis is motivated by the literature on gender differences in coping mechanisms and internalizing and externalizing behaviors. Boys exposed to community violence are more likely than girls to present externalizing behaviors such as aggressiveness, acting out, and damaging property (Bacchini et al., 2011; Hardaway et al., 2012). Girls are more likely than boys to show internalizing symptoms such as depression and anxiety (Bacchini et al., 2011; Lambert et al., 2012). Thus, it is possible that a higher share of male peers from violent municipalities means more class disruptions and a more hostile learning environment and that how boys and girls respond to this environment may affect their academic performance differently.

Table 4, Panel A shows the estimated effects of the share of male and female peers from violent municipalities on test scores. In Column 1, we present our baseline specification, that is a modified version of Equation 3 in which we include the share of new peers by gender. In Column 2, we present our preferred specification, which controls for the share of peers from other nonviolent municipalities and from other schools in the same municipality. The point estimate for the share of male peers is larger (in absolute value) than the point estimate for the share of female peers. However, they are not statistically different from one another.

A complementary question is whether girls are more likely to be affected by peers who were exposed to violence than boys, and if their peer's gender affects them differently. To examine this issue, in Table 4, Panel B we present the estimated effects of being exposed to male and female peers from violent environments on composite test scores separately for girls and boys. Specifically, we estimate a modified version of Equation 3 in which we include the share of male and female peers and an interaction of those variables with an indicator variable equal to one if the incumbent is a girl. In this way, we are able to retrieve the estimated effects for males, females, and the gender gap.[24] Column 1 presents our baseline specification; in Column 2 we additionally control for the share of peers from other nonviolent municipalities and from other schools in the same municipality. Estimates in Table 4, Panel B indicate that both male and female incumbents are negatively affected by the arrival of male peers from violent environments regardless of their gender. However, female incumbents are the most affected. The gender gap effects indicate that the detrimental effects on test scores are 0.6 percent of a standard deviation larger for female incumbents when a new male peer arrives to a class of 20 students. The arrival of female students from violent municipalities also has a negative effect on incumbents, regardless of their gender. The point estimate measuring the gender gap is negative but statistically insignificant.

5.5 Effects by Peers' Achievement Level

The peer effects we estimate in Section 5.3 can be disseminated through various channels, including the quality of new peers from violent municipalities. In this section, we separate the share of peers into two groups: high and low achievers. A new peer is classified as a high achiever if their initial test score is above the median test score of the incumbents in a given school-grade-year; otherwise, they are classified as a low achiever.[25] Intuitively, if the new peer's past performance is above the performance of the median student in their new school, this peer might cause a positive externality, particularly among low achievers.

Figure 11, Panel (a), shows the estimated effects of having high- and low-achieving peers arriving from violent municipalities. In addition, it shows the effects of having peers arriving

[24] That is, we estimate

$$\begin{aligned} y_{isgmt} = \lambda_{sg} + \eta_{gt} + \sigma_1 ShareMale_{sgmt} + \sigma_2 ShareMale_{sgmt} \times Fem_i \\ + \sigma_3 ShareFem_{sgmt} + \sigma_4 ShareFem_{sgmt} \times Fem_i + \beta_1 y_{isgm,t-1} + \beta_2 Fem_i + u_{isgmt}, \end{aligned} \tag{4}$$

where Fem_i is an indicator variable equal to 1 if the incumbent is a female; σ_1 captures the estimated effect of the share of male peers on male incumbents; $\sigma_1 + \sigma_2$ captures the effect of male peers on female incumbents; and σ_2 captures the gender-gap effects of male peers from violent municipalities. Similarly, σ_3 captures the estimated effect of the share of female peers on male incumbents; $\sigma_3 + \sigma_4$ captures the effect of female peers on female incumbents; and σ_4 captures the gender gap effects of female peers.

[25] According to our definition, roughly 47% of students who in-migrate from violent municipalities are high achievers in their receiving schools, while 53% are low achievers.

from other schools in the same municipality and from schools located in other nonviolent municipalities.[26] We find that high-ability peers arriving from violent municipalities have a negative (but not statistically significant) effect on incumbents' achievement. Low-performing peers arriving from violent municipalities have a large (and statistically significant) negative effect on incumbents' test scores. Considering only low-ability new peers, the estimated effect of the share of students who arrive from violent areas is roughly twice as large as the effect of the share of peers who arrive from other nonviolent municipalities and from other schools in the same municipality.

Since low achievers from violent municipalities have more detrimental effects on incumbent students' test scores than low achievers arriving from nonviolent places, we examine whether the quality (measured by pre-migration test scores) of low-ability students arriving from violent places is lower than the quality of other low-ability new peers. In Figure 11, Panel (b), we show the estimated average differences between the initial achievement level of low achievers arriving from violent municipalities and low achievers arriving from other schools located in nonviolent municipalities. Panel (c) shows the estimated differences, excluding students arriving from other schools in the same municipality. We find that students arriving from violent municipalities have higher average initial test scores than students arriving from other schools located in nonviolent municipalities. When we exclude students arriving from other schools in the same municipality, the differences are not statistically significant. These results suggest that the stronger negative impact low-achieving new peers from violent places have on incumbents' achievement is unlikely to be explained by differences in peer quality between peers from violent and nonviolent municipalities.

[26]All estimates come from a single regression that extends our baseline specification in Equation 3 and considers peer shares by ability level.

6 Robustness Checks

Our peer effect estimates exploit the within-school-grade variation in the share of students who migrate from violent to nonviolent municipalities across different cohorts. The question we seek to answer is whether incumbents who belong to cohorts that receive a particularly high number of new students who were previously exposed to local violence perform differently from incumbents belonging to cohorts (in the same school) that received fewer students from violent municipalities.

The validity of our empirical approach relies on the assumption that the within-school-grade variation we are exploiting is not correlated with other determinants of students' performance. In this section, we explore the plausibility of this identifying assumption by considering the potential nonrandom sorting of students who moved from violent municipalities to school-grade-year combinations with incumbents of particular socioeconomic status, as well as the potential nonrandom out-migration of students in the incumbent pool. In addition, we use individual-level data on achievement to perform an event-study style analysis to provide additional support for a causal interpretation of our results.

We follow five complementary approaches. First, we regress the incumbents' initial achievement on the share of peers who arrived from violent municipalities, controlling for school-by-grade fixed effects and grade-by-year fixed effects. Second, we use parents' education as a proxy for socioeconomic status and regress the education of parents of incumbent students on the share of peers who arrived from violent municipalities. In this exercise, the sample includes students in grades 4-6 who participated in the survey that collected background information on students (Contexto) in at least 1 year.[27] Third, using our main estimation sample, we directly test for out-migration and selective out-migration of incumbents related to the arrival of new peers from violent municipalities. To that end, we estimate

[27] We consider the sample of students for whom parental education is reported. Importantly, given that parents' education can be assumed to be fixed in the period under analysis, we are able to extrapolate this variable to include observations across different years for students who participated in the survey in any given year.

the model in Equation 3, replacing the outcome variable with an indicator variable equal to one if the incumbent out-migrated and zero otherwise. Next, we show that the results are not driven by geographic proximity to violent areas by excluding safe municipalities that share borders with ever-violent municipalities. Lastly, we provide evidence that our results are unlikely to be driven by a declining trend in achievement for incumbent students prior to the arrival of peers from violent municipalities.

Table A.1 in Appendix A shows that the estimated effects using the subsample of students who participated in the survey in at least one period are similar to the main results presented in Table 3. Moreover, the results remain robust after controlling for parents' education level, as shown in Column 4. In Table 5, Column 1, we show that there is no evidence of nonrandom sorting of students from violent municipalities based on incumbents' initial achievement. In Column 2, we observe that the share of students from violent municipalities is not correlated with the socioeconomic status of incumbents, proxied by an indicator variable equal to one if the level of formal education of at least one parent is completed high school (or higher). These estimate provides support in favor of random exposure to peers who migrated from violent municipalities across cohorts in a particular school.

Columns 3 to 5 of Table 5 show the estimated effects on out-migration of incumbents and on the probability that high and low achievers out-migrate. In this exercise, high (low) achievers are students with initial performance levels above (below) the national median. The results indicate that the share of students from violent municipalities does not explain out-migration of incumbents. In addition, we do not find evidence in favor of selective endogenous out-migration based on incumbents' performance, implying that the estimated effects on incumbents' performance are unlikely to be explained by endogenous changes in the average ability of incumbents.

The results indicate that the share of students from violent municipalities does not explain out-migration of incumbents. We do not find evidence in favor of selective endogenous out-migration based on incumbents' performance, implying that the estimated effects on incumbents' performance are unlikely to be explained by endogenous changes in the average ability of incumbents.

Even after showing that the results are not driven by nonrandom sorting of students to school-grade combinations or by endogenous migratory responses of incumbents, a potential concern is that the negative effects we capture in Table 3 could be driven by incumbents' direct exposure to violence due to their proximity to violent municipalities. This could occur, for instance, if never-violent municipalities located geographically close to violent municipalities received larger influxes of students from violent environments and, at the same time, incumbent students became more aware of violence in the nearby municipalities. To address this concern, Table 6 shows the estimated peer effects for students who attend school on the subset of never-violent municipalities that are surrounded by never-violent municipalities. In Column 1, we present our baseline specification and in Column 2, we control for the share of students from other nonviolent municipalities and from other schools in the same municipality. The estimated negative effects are larger for this restricted sample, indicating that our main results are not being driven by direct exposure to violence and that for students for whom violence is far from being a norm, being exposed to one peer previously exposed to violence in a class of 20 students decreases test scores by 2.1 percent of a standard deviation.

Finally, we test whether the peer effects we estimate are driven by preexisting trends in test scores. Figure 12 presents an event-study analysis in which we examine the evolution of incumbent students' achievement for the years prior to and after they received at least one peer who arrived from a violent municipality. All estimates are relative to the 1 year prior to the first arrival. Incumbents' test scores showed similar trends before the arrival of peers previously exposed to violence. However, consistent with our peer effect estimates, there is a significant negative effect after exposure. This suggests that the reduction in incumbents' test scores observed after exposure to peers arriving from violent areas is unlikely to be explained by preexisting trends in incumbents' performance.

7 Effects on Class Environment

In Section 6, we showed that the estimated negative effect on incumbents' school performance is not consistent with an endogenous sorting of new students to specific cohorts in a school-grade. In addition, we found no evidence of out-migration responses to the arrival of peers from violent municipalities. Taken together, the reported evidence suggests that peers arriving from violent environments are likely to generate a disruption in the learning process of incumbents. We examine this issue by studying effects on potential mechanisms linked to school environment, as reported by incumbents. To that end, we use data from Contexto, a survey administered to students in a subset of schools when participating in ENLACE. Given that the schools chosen to participate in the Contexto survey change across academic years, in most cases we are not able to observe information for the same grade in a particular school over time. This means that we cannot exploit variations across cohorts within school-grade using time-variant responses from this data. Instead, we rely on within-school variation by including school fixed effects and grade-by-year fixed effects.

We consider the following school environment outcomes: physical aggression or fights in school, threats in school, making fun of students, making fun of teachers, damaging school property, robbery within the school, feeling safe in school, and feeling safe near school. Each outcome is a categorical variable that measures students' perceptions of the frequency of occurrence in school, and takes the values 0 (never), 1 (almost never), 2 (sometimes), 3 (almost always), and 4 (always). For the variables regarding feeling safe near or in school, we recode them such that a larger value implies a worse outcome (i.e., 0 for always and 4 for never).

Panel A of Table 7 shows the estimated effects on each of the standardized school-environment outcomes (at grade-year level) as well as an index, in Column 9, that is a standardized version of a simple average of the (standardized) school environment outcomes. The results suggest that the presence of new peers exposed to violence disrupts learning. There are significant effects on observing students making fun of their teachers or damaging school property, and reports of feeling unsafe near school. Most point estimates in Table 7, Panel A are positive, but some are not statistically significant. The school environment index significantly increases with increases in the share of students from violent municipalities, which indicates that the arrival of these new peers is likely to generate worse school environments.

Table 7, Panel B shows the estimated effects considering the gender of the new peers. We observe that the share of girls exposed to violent environments is not significantly associated with changes in school environment, whereas the share of boys generates significant disruption (measured by reports of threats in school, students making fun of other students or teachers, students damaging school property, and students feeling unsafe near school). These estimated effects suggest that a potential channel behind the estimated peer effects in test scores is via boys' externalizing behaviors exacerbated due to past exposure to violence.

In Section 5.5, we found that the presence of students whose past achievement is ranked below the median performance of incumbents generates large negative effects on incumbents' performance. In Table 7, Panel C, we examine whether high and low achievers are likely to affect the school environment differently. Consistent with the results reported in Section 5.5, though the share of high achievers does not have a significant impact on class environment, the share of low achievers explains significant declines in class environment measured by reports of feeling unsafe near school and of students making fun of teachers.

8 Conclusion

This paper studies the economic consequences of violence beyond its geographic boundaries. We do so by examining unprecedented increases in violence during Mexico's drug war between 2006 and 2013. We document violence-induced migration and negative spillover effects on students in receiving schools who are not directly affected by violence. We show that exposure to violence increases students' probability to migrate to nonviolent municipalities and that students' test scores in receiving schools are negatively affected by the arrival of these peers. In addition to a credible research design, we integrate measures of students' family background and school environment into our main results, which allows us to provide evidence on the mechanisms behind the effects on migration decisions and the detrimental effects on incumbent students' test scores.

We leverage variation in the timing of increased violence at the municipality level and a unique student-level panel of data to offer several important results. First, violence, measured by homicide rates, pushes students out of violent municipalities and pulls them into non-violent ones. Second, students in low-violence municipalities (never-violent) are negatively affected by the arrival of peers pulled out of their high-violence municipalities. Third, students' socioeconomic conditions, measured by parental education and initial test scores, are a driving mechanism of out-migration decisions, since students with more educated parents and higher past academic performance are more likely to migrate from violent to nonviolent areas. Fourth, we show that the detrimental effects on incumbent students' test scores are likely driven by new peers disrupting learning, as shown by negative effects on measures of a good class environment and safety near schools. Finally, we identify high achievers and female students as the groups of incumbent students who are more prone to suffer this negative spillover effect. These estimates, coupled with the information on potential mechanisms, can provide guidance in formulating policies with the objective of mitigating the negative effects that peers exposed to violence impose on incumbent students.

Our results demonstrate that externalities imposed on students in nonviolent areas are relevant to the analysis of how drug wars impact human capital accumulation and economic growth. The results highlight the importance of considering these negative spillovers, which extend beyond the geographic areas directly implicated in these types of conflicts, in policy design.

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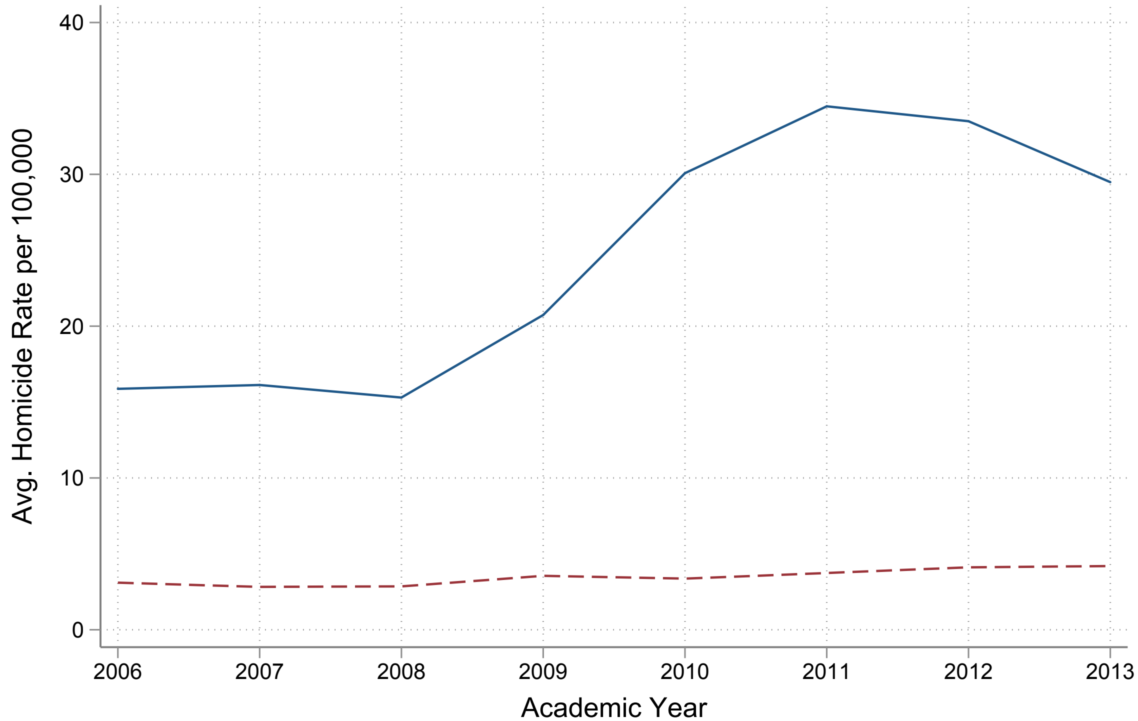
Figure 1: Geographic Distribution of Ever- and Never-violent Municipalities

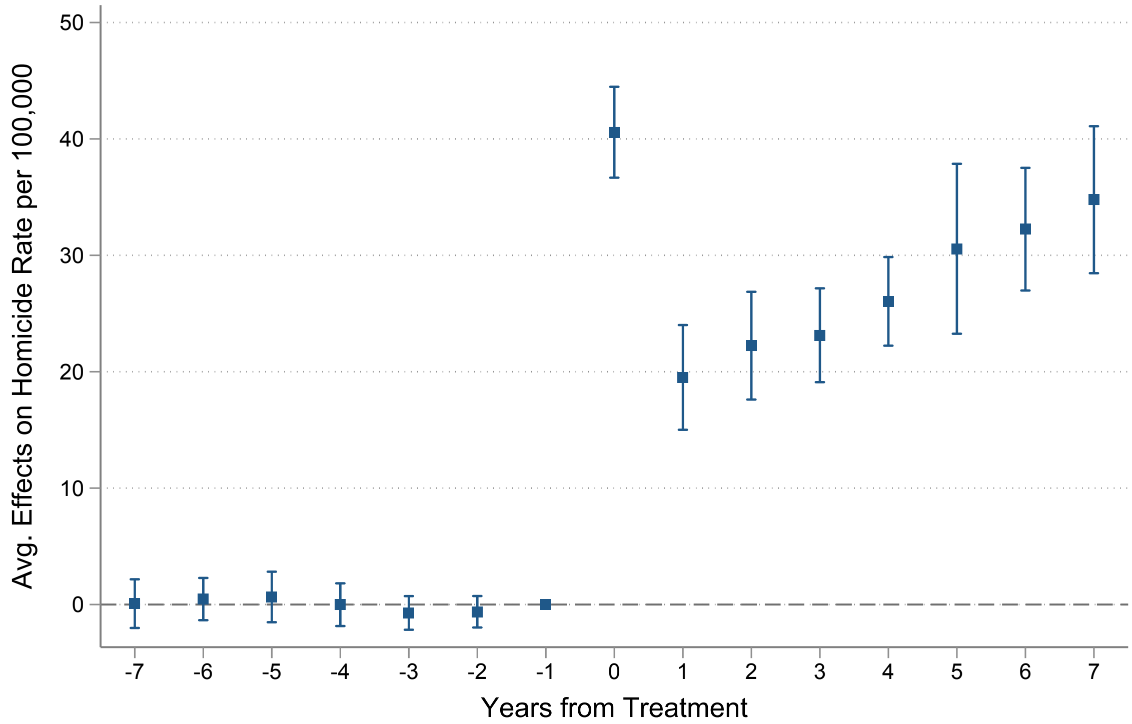
Notes: This figure shows the geographic distribution of ever (white) and never (green) violent municipalities. Never-violent municipalities are those that did not reach homicide rates above the 75th percentile between 2006 and 2013. Otherwise, municipalities are classified as ever-violent.

Figure 2: Homicide Rates for Ever- and Never-violent Municipalities



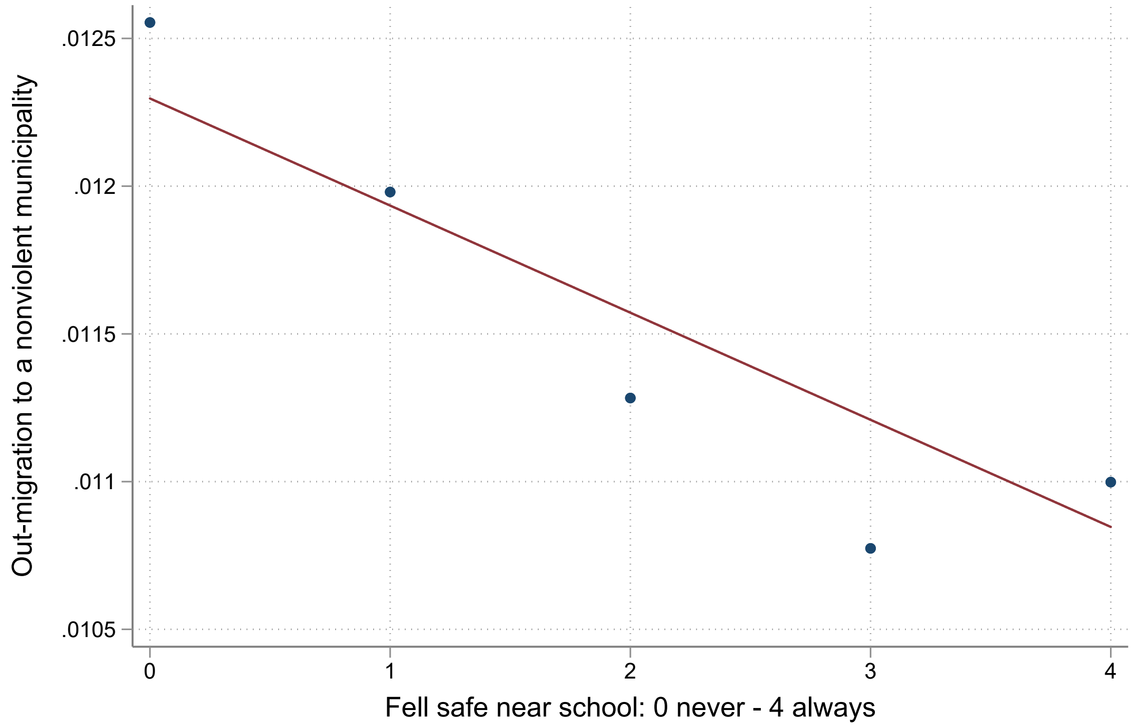
Notes: This figure plots average homicide rates (per 100,000 people) separately for ever- (solid line) and never- (dashed line) violent municipalities, which are shown in Figure 1. Homicide rates are calculated based on the universe of death certificates from INEGI and population counts from CONAPO. Homicide rates are measured in academic years.

Figure 3: Violent Municipalities and Homicide Rates



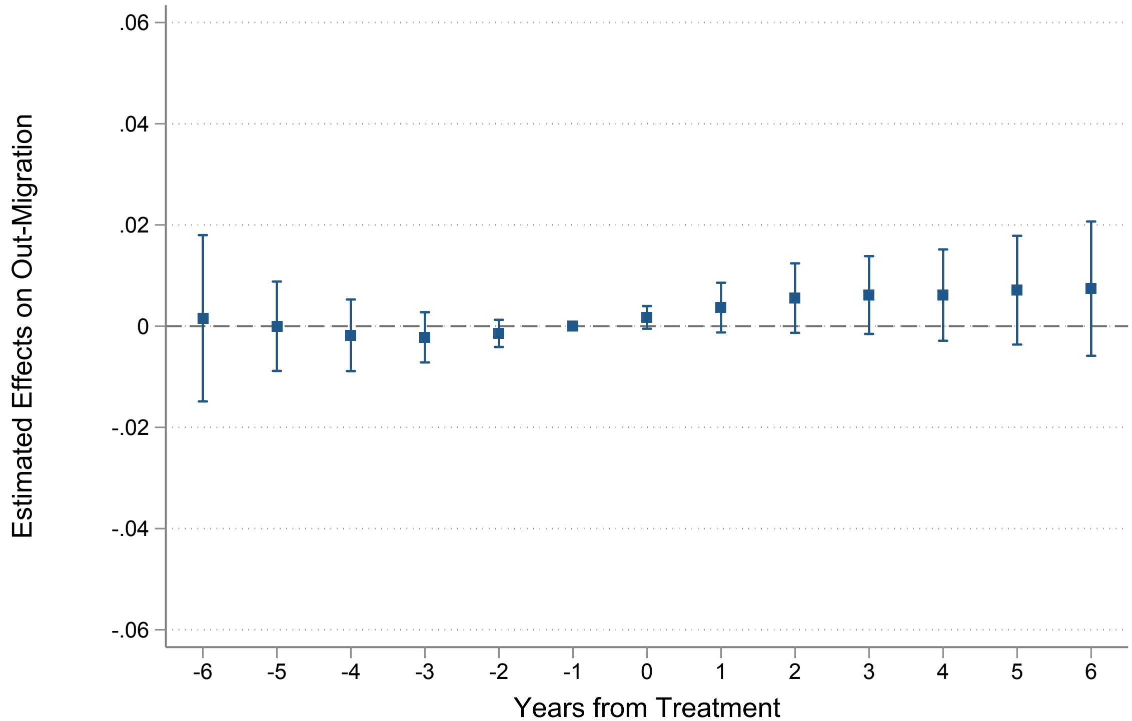
Notes: This figure plots coefficients and 95% confidence intervals for indicators of the years prior to and after a municipality was first classified as violent. All estimates come from a single regression and include municipality fixed effects and academic year fixed effects. All estimates are differences relative to the year prior to treatment. Standard errors are clustered at the municipality level.

Figure 4: Students' Perception of Safety Near School and Out-migration



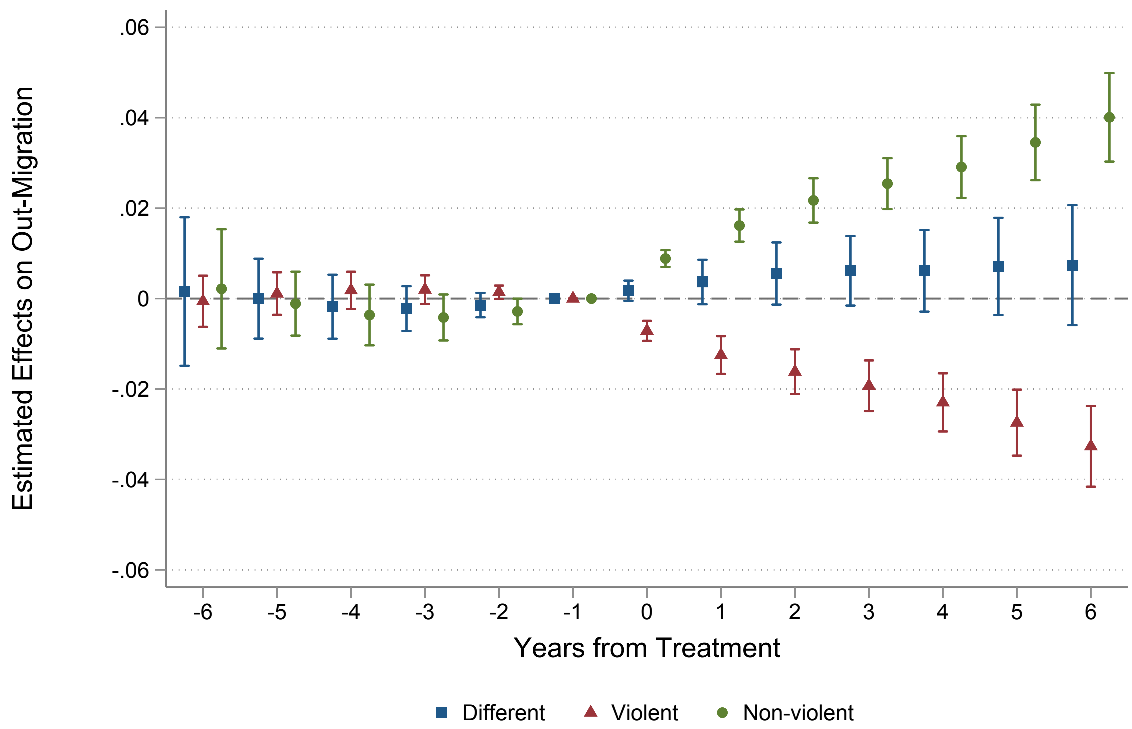
The sample includes students in ever-violent municipalities participating in the Contexto survey in the period 2008-2013. The horizontal axis represents possible answers to the statement "feel safe near school," taking the value 0 if never, 1 if almost never, 2 if sometimes, 3 if almost always, and 4 if always. The vertical axis shows the average values of out-migration to nonviolent municipalities for each value of students' perception of safety.

Figure 5: Exposure to Violence and Out-migration Behavior



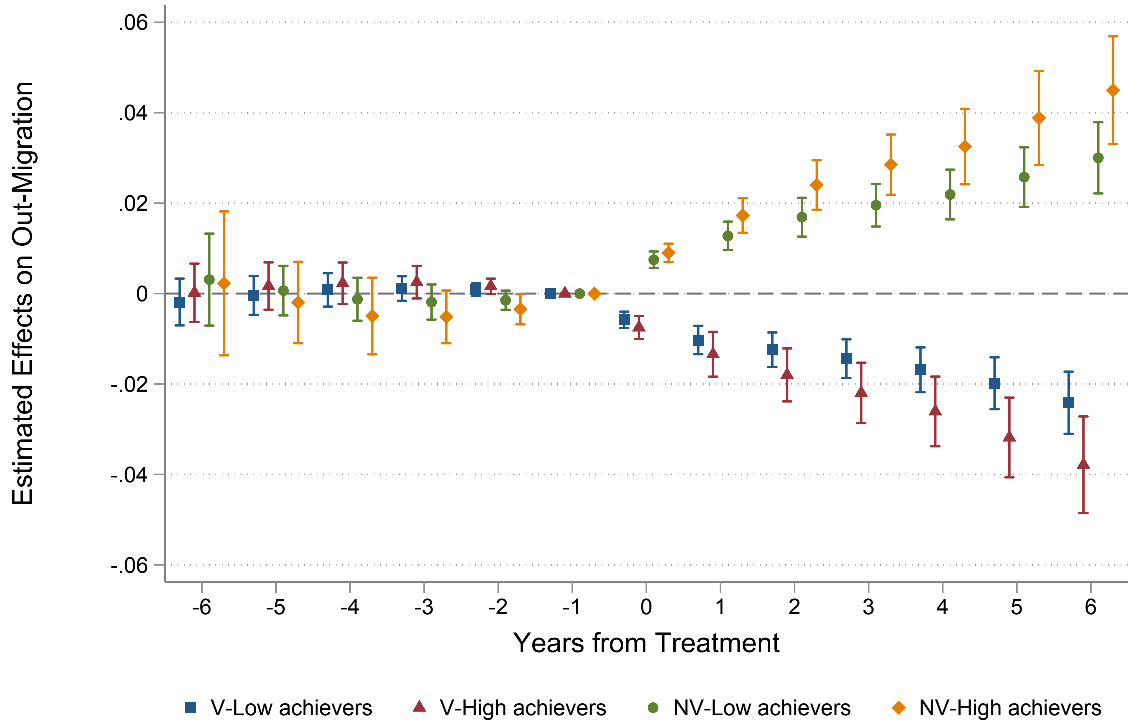
Notes: This figure plots coefficients and 95% confidence intervals for indicators for the years prior to and after a municipality was first classified as violent. All estimates come from a single regression and include student fixed effects, year fixed effects, and municipality fixed effects. All estimates are relative to the year prior to treatment. Standard errors are clustered at the municipality level.

Figure 6: Exposure to Violence and Out-migration Behavior by Destination



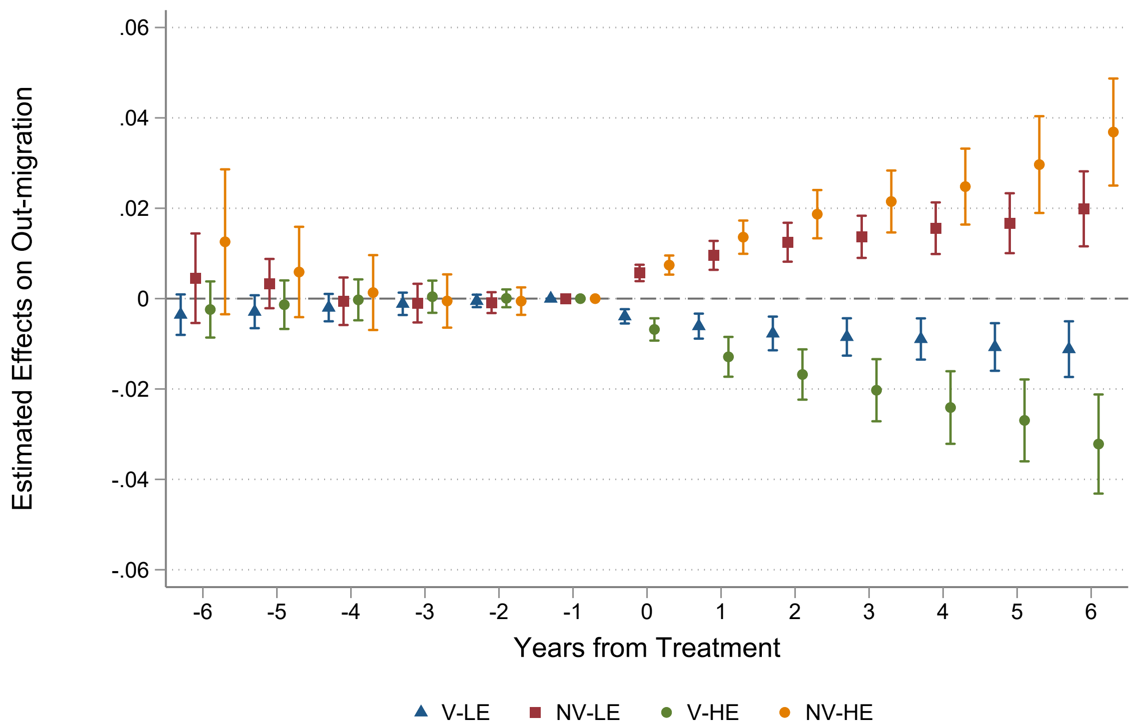
Notes: This figure plots coefficients and 95% confidence intervals for indicators for the years prior to and after a municipality was first classified as violent. All estimates for each destination type come from a single regression and include student fixed effects, year fixed effects, and municipality fixed effects. All estimates are relative to the year prior to treatment. Standard errors are clustered at the municipality level.

Figure 7: Exposure to Violence and Out-migration Behavior by Initial Performance



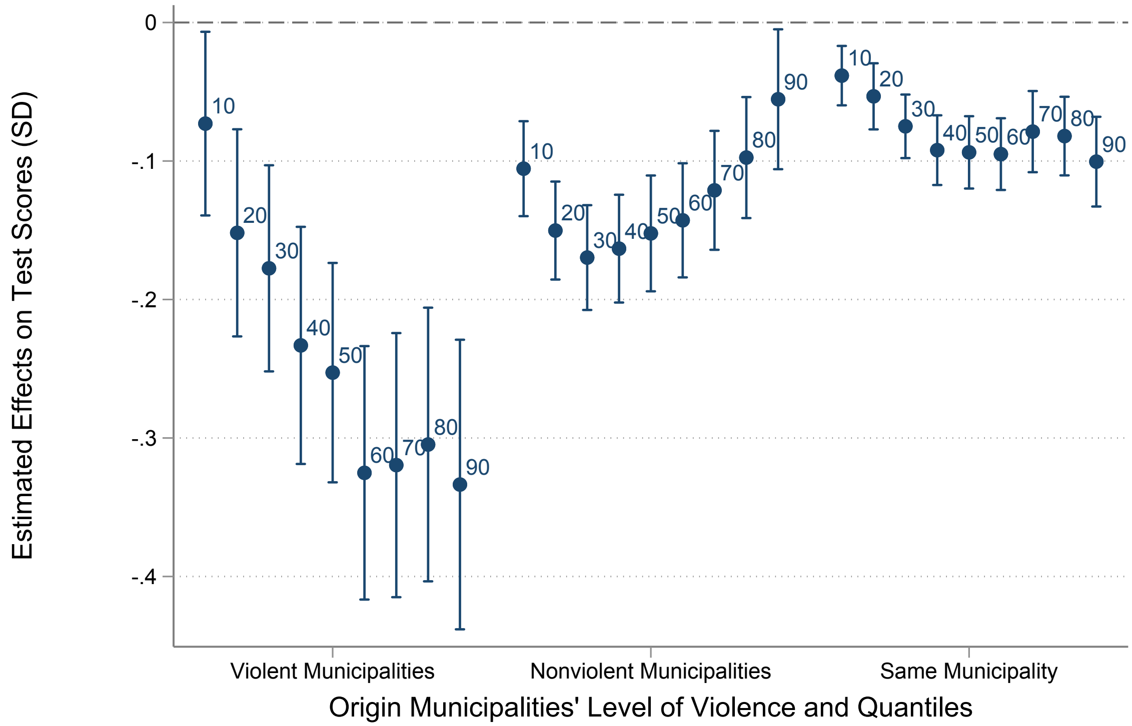
Notes: This figure plots coefficients and 95% confidence intervals for indicators for the years prior to and after a municipality was first classified as violent, separately for low- and high-performing students. Low- and high-achieving students are, respectively, defined as students in the bottom and top quartile of the national test score distribution of total performance (math + reading). All estimates for each performance level and destination type come from a single regression and include student fixed effects, year fixed effects, and municipality fixed effects. All estimates are relative to the year prior to treatment. Standard errors are clustered at the municipality level.

Figure 8: Exposure to Violence and Migration Patterns by Parents' Education



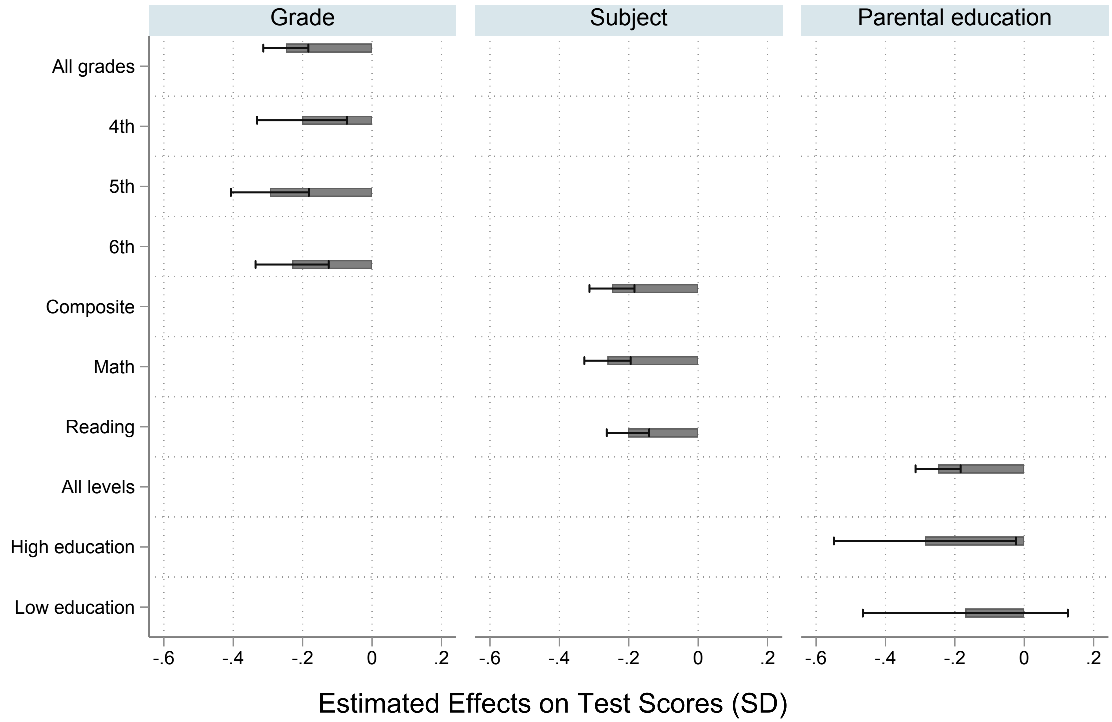
Notes: This figure plots coefficients and 95% confidence intervals for indicators for the years prior to and after a municipality was first classified as violent, separately for children whose formal parental education is low or high. All estimates for each group and destination type come from a single regression and include student fixed effects, year fixed effects, and municipality fixed effects. All estimates are relative to the year prior to treatment. Standard errors are clustered at the municipality level. HE indicates that the maximum level of formal education of at least one parent is completed high school (or higher). LE indicates that the maximum level of formal education between both parents is not higher than middle school.

Figure 9: Estimated Effects of Incoming Peers on Incumbent Students' Test Scores by Quantiles



Notes: This figure plots coefficients and 95% confidence intervals of unconditional quantile regression (Firpo et al., 2009) estimates on composite test scores. All quantile estimates come from different regressions and include school-grade fixed effects, grade-year fixed effects, a female indicator, and lagged test scores. Standard errors are bootstrapped based on 500 replications and clustered at the school level.

Figure 10: Estimated Effects of Incoming Peers from Violent Municipalities on Incumbent Students' Test Scores by Grade, Subject, and Parental Education



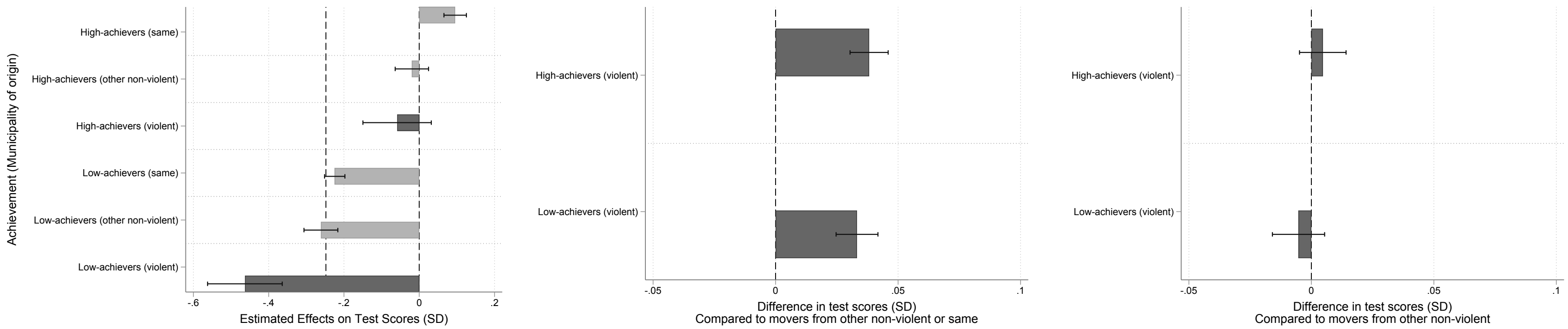
Notes: This figure plots coefficients and 95% confidence intervals for the share of students from violent municipalities. All estimates come from different regressions and include school-by-grade fixed effects, year-by-grade fixed effects, lagged test scores, and a female indicator. Standard errors are clustered at the school level.

Figure 11: Estimated Effects of Incoming Peers from Violent Municipalities on Incumbent Students' Test Scores by Peers' Initial Performance

(a) Peer effects

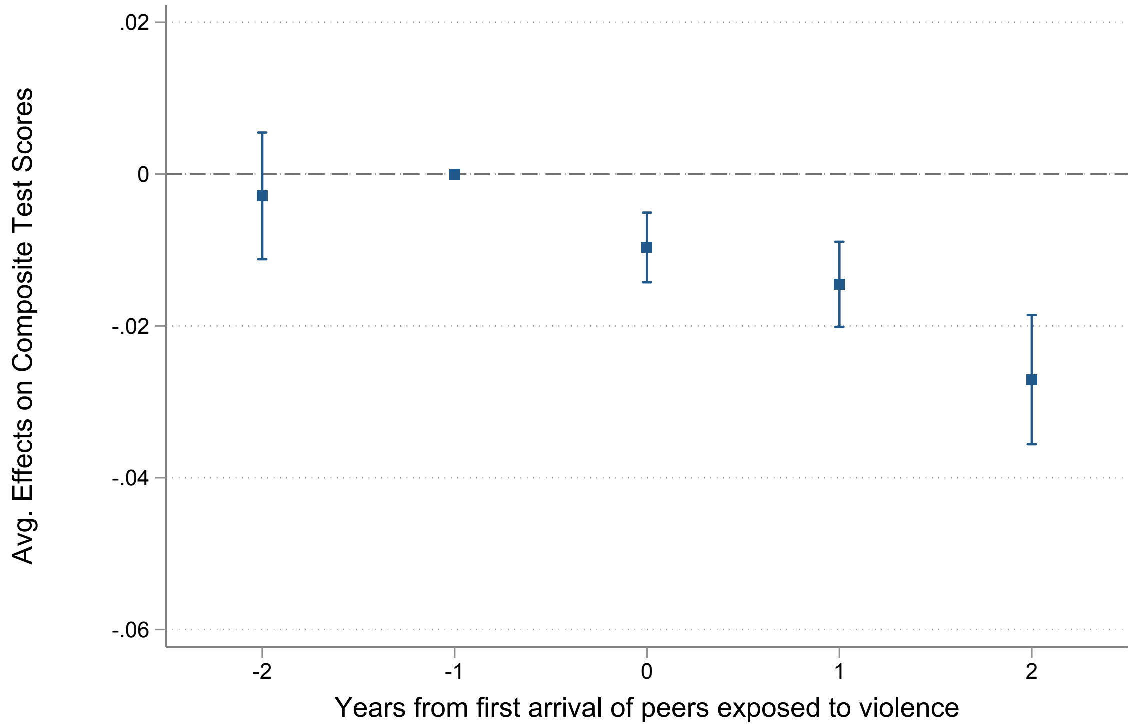
(b) Difference in peer quality (violent vs nonviolent)

(c) Difference in peer quality (violent vs other nonviolent)



Notes: This figure plots coefficients and 95% confidence intervals for the share of students from violent municipalities in Panel (a). Low- and high-achieving peers are, respectively, defined as students whose tests scores are below or above the median of incumbents' test score distribution of composite performance (math + reading) in a given school-grade-year. All estimates in Panel (a) come from a single regression that includes school-by-grade fixed effects, year-by-grade fixed effects, lagged test scores, and a female indicator. Estimates and 95% confidence intervals in Panels (b) and (c) show the estimated differences in peer quality, measured by initial composite test scores, between students who migrate from violent municipalities and students who migrate from nonviolent municipalities. Standard errors are clustered at the school level.

Figure 12: Event-study Estimated Effects of Incoming Peers from Violent Municipalities on Incumbent Students' Test Scores



Notes: This figure plots coefficients and 95% confidence intervals for the years prior to and after the first arrival of students from violent municipalities. All estimates come from different regressions and include school-by-grade fixed effects, year-by-grade fixed effects, lagged test scores, and a female indicator. Standard errors are clustered at the school level.

Table 1: Descriptive Statistics by Municipalities' Level of Violence

	Never-violent		*Ever-violent*		
	Mean	Std Dev	Mean	Std Dev	Difference
	(1)	(2)	(3)	(4)	(5)
Municipality Characteristics					
Total Population	49731.29	(131963.71)	37402.20	(122979.77)	-12329.09**
% Illiterate Population (above 15 years old)	16.46	(10.25)	16.86	(11.52)	0.41
% of Population without Completed Primary Education	38.04	(14.09)	40.25	(14.18)	2.21***
% of Population without access to Sewer	9.79	(11.88)	10.65	(13.03)	0.86*
% of Population without access to Electricity	4.47	(5.74)	6.10	(9.22)	1.63***
% of Population without access to Running Water	15.16	(18.51)	19.12	(20.73)	3.96***
% of Population with Dirt Floor	22.73	(20.53)	26.36	(23.34)	3.64***
Marginalization Index	-0.04	(0.91)	0.03	(1.05)	0.07*
Observations	931	931	1,523	1,523	

Notes: Ever-violent are municipalities with homicide rates in the upper quartile of the overall homicide rate distribution for at least 1 year over the period 2006-2013. Otherwise, municipalities are classified as never-violent. Calculations are based on estimations of CONAPO (2005).

Table 2: Estimated Effects on Out-migration Behavior

	Different (1)	Nonviolent (2)	Violent (3)
Year of treatment	0.002*	0.009***	-0.007***
	(0.001)	(0.001)	(0.001)
1 year after	0.004	0.016***	-0.012***
	(0.002)	(0.002)	(0.002)
2 years after	0.006*	0.022***	-0.016***
	(0.003)	(0.002)	(0.003)
3 years after	0.006*	0.025***	-0.019***
	(0.003)	(0.002)	(0.003)
4 years after	0.006	0.028***	-0.023***
	(0.004)	(0.003)	(0.003)
5 years after	0.007	0.034***	-0.027***
	(0.005)	(0.004)	(0.004)
6 years after	0.007	0.039***	-0.032***
	(0.006)	(0.004)	(0.004)
N	27752057	27752057	27752057

Notes: Each column represents a different regression. All specifications include student fixed effects, municipality fixed effects, and year fixed effects. Standard errors in parentheses are clustered at the municipality level.

Table 3: Estimated Effects of Being Exposed to New Peers on Incumbent Students' Test Scores

	(1)	(2)	(3)	(4)
Share of students from violent municipalities	-0.262***	-0.255***	-0.248***	-0.281***
	(0.033)	(0.033)	(0.033)	(0.065)
Share of students from nonviolent municipalities		-0.132***	-0.124***	-0.083***
		(0.016)	(0.016)	(0.022)
Share of students from the same municipality			-0.078***	-0.088***
			(0.010)	(0.014)
Share of students from violent municipalities (t+1)				-0.001
				(0.056)
Composite score (t-1)	0.682***	0.682***	0.682***	0.694***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	0.074***	0.074***	0.074***	0.058***
	(0.000)	(0.000)	(0.000)	(0.001)
N	12666823	12666823	12666823	6985038
School-by-grade FE	yes	yes	yes	yes
Grade-by-year FE	yes	yes	yes	yes

Notes: Each column represents a different regression. Standard errors in parentheses are clustered at the school level.

Table 4: Estimated Effects of Being Exposed to New Peers on Incumbent Students' Test Scores by Peers' and Incumbents' Gender

	(1)	(2)
Panel A: Peers' Gender		
Share of male students from violent municipalities	-0.282***	-0.268***
	(0.045)	(0.045)
Share of female students from violent municipalities	-0.249***	-0.235***
	(0.048)	(0.048)
Female	0.074***	0.074***
	(0.000)	(0.000)
Composite score (t-1)	0.682***	0.682***
	(0.001)	(0.001)
N	12622627	12622627
Panel B: Peers' Gender × Incumbents' Gender		
Share of male students from violent municipalities	-0.222***	-0.208***
	(0.048)	(0.048)
Share of male students from violent municipalities x Female	-0.126***	-0.125***
	(0.034)	(0.034)
Share of female students from violent municipalities	-0.225***	-0.211***
	(0.052)	(0.052)
Share of female students from violent municipalities x Female	-0.048	-0.048
	(0.035)	(0.035)
Female	0.075***	0.075***
	(0.000)	(0.000)
Composite score (t-1)	0.682***	0.682***
	(0.001)	(0.001)
N	12622627	12622627
School-by-grade FE	yes	yes
Grade-by-year FE	yes	yes
Controls for shares	no	yes

Notes: Each column in each panel represents a different regression. Standard errors in parentheses are clustered at the school level.

Table 5: Selection and Out-migration of Incumbents

			Out-migration		
	Initial Performance	High Parental Education	Overall	High-achievers	Low-achievers
	(1)	(2)	(3)	(4)	(5)
Share of students from violent municipalities	0.007	-0.012	-0.010	-0.006	-0.007
	(0.041)	(0.038)	(0.009)	(0.007)	(0.006)
N	13386566	1462205	8026579	7205772	7205772

Notes: Each column in each panel represents a different regression. Standard errors in parentheses are clustered at the school level. In Column 2, the sample is restricted to students in grades 4-6 who report parental education in at least one wave of the Contexto survey over the period 2008-2013. Parental education is defined as high if the level of formal education of at least one parent is completed high school (or higher). In Column 3, we report estimated results on out-migration for all incumbents. In Columns 4 and 5, the outcome variable is one for out-migrating incumbent students whose initial test scores are above and below the national median, respectively, and zero otherwise.

Table 6: Estimated Effects of Being Exposed to New Peers on Incumbent Students' Test Scores: Never-violent Neighbors

	(1)	(2)
Share of students from violent municipalities	-0.426***	-0.420***
	(0.125)	(0.126)
Share of students from nonviolent municipalities		-0.001
		(0.041)
Share of students from the same municipality		-0.058**
		(0.029)
Female	0.083***	0.083***
	(0.001)	(0.001)
Composite score (t-1)	0.665***	0.665***
	(0.003)	(0.003)
N	1,721,017	1,721,017
School-by-grade FE	yes	yes
Grade-by-year FE	yes	yes
Controls for shares	no	yes

Notes: Each column represents a different regression. The sample is restricted to the subset of never-violent municipalities that are surrounded by never-violent municipalities. Standard errors in parentheses are clustered at the school level.

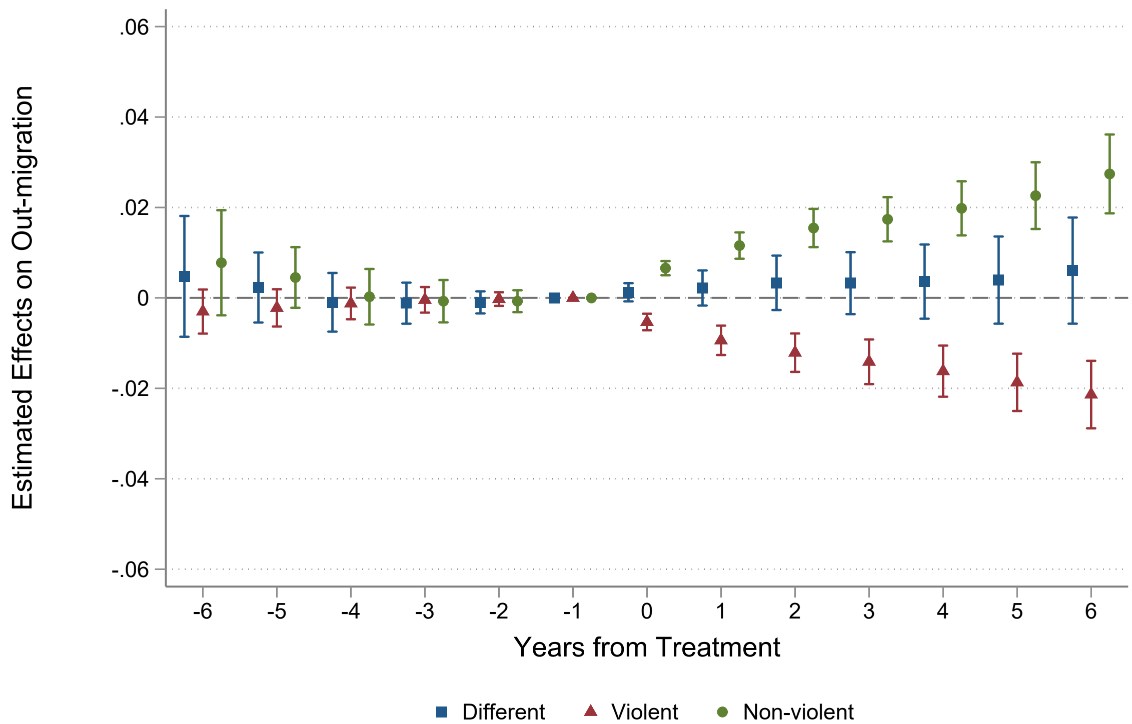
Table 7: Estimated Effects of Being Exposed to New Peers on Incumbent Students' School Environment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	Index (9)
Panel A: All									
Share of students from violent municipalities	-0.004	0.221	0.249	0.439**	0.379**	0.148	-0.042	0.419**	0.379*
	(0.191)	(0.190)	(0.202)	(0.196)	(0.177)	(0.159)	(0.206)	(0.170)	(0.215)
N	511414	509745	508878	508217	507288	505436	506248	505336	483276
Panel B: Effects by peers' gender									
Share of male students from violent municipalities	0.068	0.584**	0.624**	0.619**	0.539**	0.231	0.117	0.616***	0.704**
	(0.277)	(0.276)	(0.287)	(0.279)	(0.249)	(0.239)	(0.314)	(0.236)	(0.323)
Share of female students from violent municipalities	-0.004	-0.078	-0.072	0.319	0.209	0.020	-0.167	0.237	0.117
	(0.265)	(0.258)	(0.289)	(0.289)	(0.254)	(0.222)	(0.287)	(0.241)	(0.296)
N	507584	505928	505066	504402	503485	501667	502460	501560	479656
Panel B: Effects by peers' performance									
Share of high achievers from violent municipalities	-0.116	0.312	0.125	0.057	0.250	0.036	0.064	0.063	0.222
	(0.282)	(0.287)	(0.285)	(0.282)	(0.260)	(0.242)	(0.315)	(0.244)	(0.322)
Share of low achievers from violent municipalities	-0.071	0.169	0.424	0.830***	0.382	-0.016	-0.026	0.583**	0.434
	(0.289)	(0.295)	(0.316)	(0.293)	(0.264)	(0.239)	(0.305)	(0.248)	(0.323)
N	486268	484684	483877	483235	482365	480595	481359	480449	459431
School FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year-by-Grade FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls for Shares	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls for Lagged Test Scores and Gender	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: Each column in each panel represents a different regression, considering standardized outcomes that are recorded such that a larger value implies a worse school environment, as reported by incumbents. The outcome variables in each column measure (1) physical aggression or fights in school; (2) threats in school; (3) making fun of students; (4) making fun of teachers; (5) students damaging school property; (6) feel unsafe in school; (7) robbery in school; (8) feel unsafe near school. Column 9 shows the estimated results for the standardized index that summarizes information in (1)-(8). Low- and high-achieving peers are, respectively, defined as students whose tests scores are below or above the median of the incumbents' test score distribution of composite performance (math + reading) in a given school-grade-year. Standard errors in parentheses are clustered at the school level. The sample is restricted to students participating in the Contexto survey in the period 2008-2013.

Appendix A Additional Figures and Tables

Figure A.1: Exposure to Violence and Out-migration Behavior: Contexto Sample



Notes: This figure plots coefficients and 95% confidence intervals for indicators for the years prior to and after a municipality was first classified as violent. All estimates for each destination type come from a single regression and include student fixed effects, year fixed effects, and municipality fixed effects. All estimates are relative to the year prior to treatment. Standard errors are clustered at the municipality level.

Table A.1: Estimated Effects of Being Exposed to New Peers on Incumbent Students' Test Scores: Contexto Sample

	(1)	(2)	(3)	(4)
Share of students from violent municipalities	-0.267**	-0.259**	-0.256**	-0.251**
	(0.116)	(0.116)	(0.116)	(0.116)
Share of students from nonviolent municipalities		-0.085*	-0.081	-0.078
		(0.051)	(0.051)	(0.051)
Share of students from the same municipality			-0.052*	-0.054*
			(0.031)	(0.031)
Composite score (t-1)	0.710***	0.710***	0.710***	0.697***
	(0.003)	(0.003)	(0.003)	(0.003)
Female	0.065***	0.065***	0.065***	0.067***
	(0.001)	(0.001)	(0.001)	(0.001)
N	1246133	1246133	1246133	1246133
School-by-grade FE	yes	yes	yes	yes
Year-by-grade FE	yes	yes	yes	yes

Notes: Each column in each panel represents a different regression. Standard errors in parentheses are clustered at the school level. The sample is restricted to students who report parental education in at least one wave of the Contexto survey over the period 2008-2013.

Appendix B ENLACE Data

Although the ENLACE exam aims to be a census of students, for several reasons not every student takes the test in a given academic year. This attrition is because of either students', schools', or states' decision not to participate.[28] For every academic year between 2005-2006 and 2012-2013, Table B.1, columns 1-5, respectively, show the number of students enrolled in grades 3-6, those taking the ENLACE test, having a valid student identifier, being observed more than once, and being observed in consecutive years. ENLACE coverage was lowest in academic year 2011-2012, at 83.92%, and highest in academic year 2006-2007, at 90.19%.

The data-matching process used to build the anonymized student-level panel dataset is based on CURPs (Unique Population Registry Codes) and described by Xaber (2020). CURPs are unique individual alphanumeric codes assigned to citizens and residents of Mexico. It is important to note that not all students who take the ENLACE exam have valid CURPs or information that allows Xaber (2020) to create a valid student identifier.[29] In 2005-2006, only 74.77% of students had a valid identifier. However, starting in 2006-2007, more than 90% of students had a valid identifier with matching rates that range between 91.61% and 98.75%.

Given that we are estimating students' decisions to in- and out-migrate, to identify whether a student switched schools from one academic year to the next, we need to observe them for at least two consecutive periods. It is expected that matching rates between students with a valid identifier and those observed at least twice are low in the first (61.97%) and last (73.18%) academic years ENLACE was given. The reason is that all students in the sixth grade in 2005-2006 did not take the test in subsequent years, which is also the case for third grade students in 2012-2013 for whom, unless they were held back, that was the first and only year they took the ENLACE exam. Overall, more than 94% of students who are in the

[28] For example, in the states of Michoacán and Oaxaca the teachers union opposed the application of the ENLACE exam in their schools.

[29] We do not have access to CURPs but we are able to follow students over time using the anonymized student identifiers created by Xaber.

sample at least twice are observed in consecutive years. Finally, because of changes in the geographic borders of municipalities that occurred in the time frame we are analyzing, less than 0.2% of students are not assigned a valid municipality code.[30]

Nonrandom sample attrition could be a threat to identification if missing students are systematically enrolled in schools from violent or nonviolent municipalities. In Figure B.1, we assess whether the attrition processes described above are correlated with municipalities' levels of violence. Panels (a)-(d), respectively, show the estimated effects on the share of students in the municipality who did not take the ENLACE exam, have a missing identifier, appear once, and appear in nonconsecutive years. Specifically, we estimate a fixed-effects model in which our dependent variable is the attrition rate at the municipality and academic year level, and our treatment variables are indicators of when and whether a municipality has been classified as violent. Our model includes municipality fixed effects, year fixed effects, and state-by-year fixed effects.[31] Regressions are weighted by the number of students.[32] Results indicate that violence does not affect attrition rates. Of the 56 estimated coefficients, only one is statistically significant at conventional levels and other one is imprecisely estimated. The former corresponds to attrition in ENLACE participation in 2013 for municipalities that were violent in 2006. The later corresponds to having a missing identifier in 2006 for places that were violent in 2013. The remaining point estimates are close to zero and statistically insignificant, which suggests that nonrandom sample attrition is not a threat to our identification strategy.

[30]We use the 2005 INEGI's geostatistical framework to define municipalities. San Ignacio Cerro Gordo obtained its independence from Arandas in 2005, and in 2007 it became the 125th municipality in the State of Jalisco. Starting in 2007, we recode San Ignacio Cerro Gordo as Arandas. In Quintana Roo, two new municipalities were created, Tulum and Bacalar. However, unlike San Ignacio Cerro Gordo, none of these municipalities were fully contained in another municipality in 2005. We then drop students who were enrolled in schools in these two municipalities. Our results are not sensitive to these changes.

[31]State-by-year fixed effects control for circumstances in which most schools from a given state decided not to participate in the ENLACE exam in a particular year.

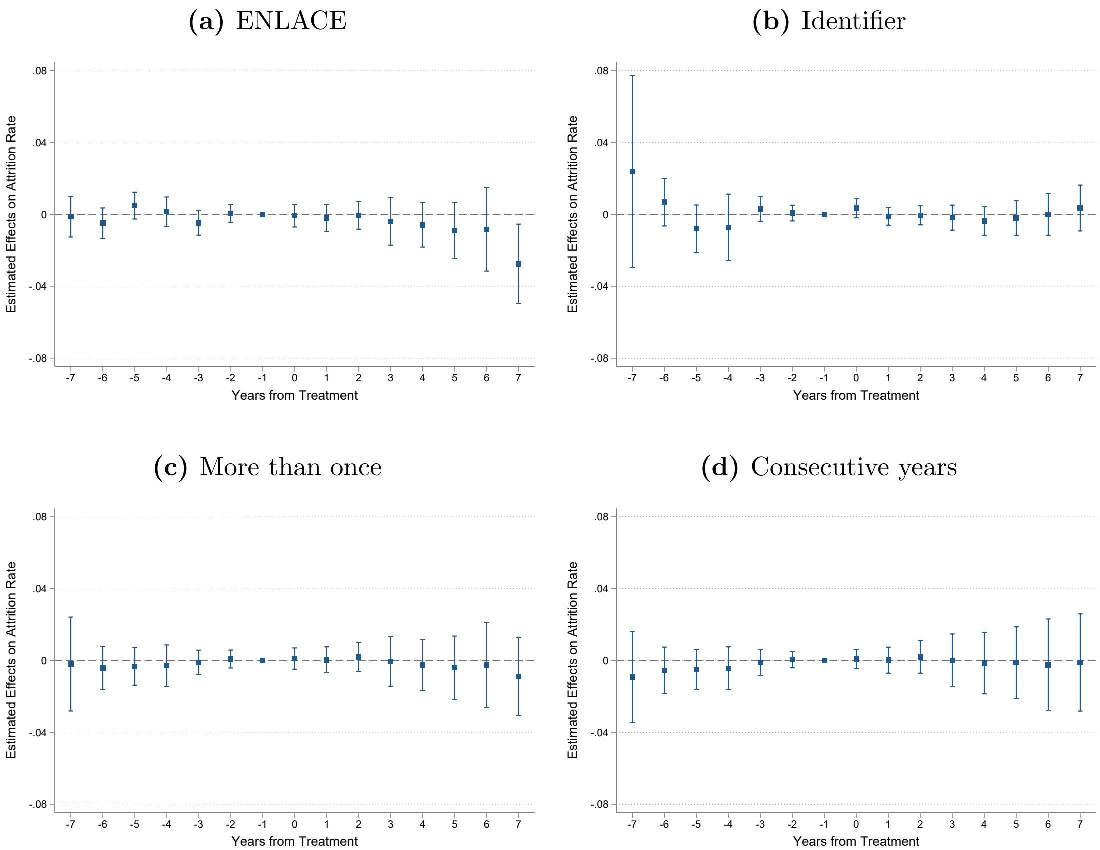
[32]Regressions weighted by the number of students produce estimated coefficients that are mathematically identical to those obtained from a regression using student-level data.

Table B.1: Sample Selection

Academic year	Enrolled (1)	Taking ENLACE (2)	Valid identifier (3)	More than once (4)	Consecutive years (5)
2005-2006	9,460,765	8,033,596	6,006,407	3,722,078	3,505,750
		(84.91%)	(74.77%)	(61.97%)	(94.19%)
2006-2007	9,395,015	8,473,451	7,762,275	6,518,148	6,321,387
		(90.19%)	(91.61%)	(83.97%)	(96.98%)
2007-2008	9,271,195	8,243,796	7,994,398	7,450,055	7,166,414
		(88.92%)	(96.97%)	(93.19%)	(96.19%)
2008-2009	9,344,698	7,963,907	7,871,726	7,669,680	7,406,467
		(85.22%)	(98.84%)	(97.43%)	(96.57%)
2009-2010	9,538,142	8,397,996	8,276,038	8,141,776	7,847,534
		(88.05%)	(98.55%)	(98.38%)	(96.39%)
2010-2011	9,766,978	8,688,671	8,580,363	8,363,438	8,063,874
		(88.96%)	(98.75%)	(97.47%)	(96.42%)
2011-2012	9,869,266	8,282,356	8,173,595	7,910,837	7,806,433
		(83.92%)	(98.69%)	(96.79%)	(98.68%)
2012-2013	9,818,418	8,551,858	8,177,194	5,984,326	5,648,399
		(87.10%)	(95.62%)	(73.18%)	(94.39%)

Notes: Each column shows the number of students and the percent, compared with the student count in the previous column, in parentheses. Information is based on ENLACE and *Estadística 911*.

Figure B.1: Estimated Effects of Exposure to Violence on Municipalities' Attrition Rates



Notes: This figure plots coefficients and 95% confidence intervals for indicators for the years prior to and after a municipality was first classified as violent. Estimates in each panel come from a single regression and include municipality fixed effects, academic year fixed effects, and state-by-year fixed effects. All regressions are weighted by the number of students in each municipality-year. Standard errors are clustered at the municipality level.

Appendix C Alternative Definitions of Violent

In our main analysis, we define violent municipalities as those whose homicide rate are above the 75th percentile (or 18.01 per 100,000 people), considering the distribution of average homicide rates across municipalities in the period 2006-2013. In this section, we present estimates for out-migration and peer effects using the 65th and 70th percentiles as alternative thresholds to define a municipality as violent.[33] The objective of this exercise is to show that our results are not sensitive to a particular threshold.

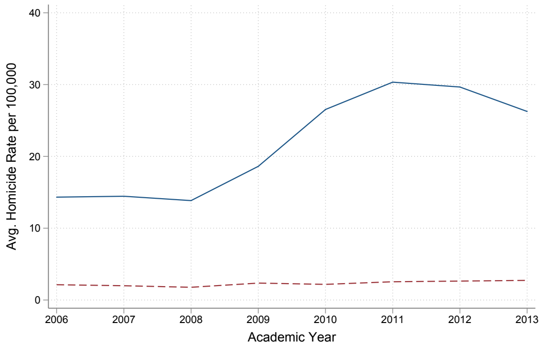
As shown in Figure C.1 panels (a) and (b), when we consider the 65th and 70th percentiles as the new thresholds, the average homicide rate in both never-violent and ever-violent municipalities are lower than in our main analysis. This implies that incumbent students are (on average) less likely to be exposed to violence themselves than incumbents in our main analysis. Moreover, movers from violent municipalities are exposed to less violence than movers in our sample.

In Figure C.1, panels (c) and (d), we show the estimated effects on out-migration mirroring those presented in Figure 6. In panels (e) and (f), we present the estimated peer effects using the same specification as in Figure 12. Overall, regardless of the threshold, the estimated effects on out-migration indicate that students exposed to violence are more likely to out-migrate to nonviolent municipalities and that migrating peers arriving to schools in never-violent municipalities negatively affect incumbents' test scores.

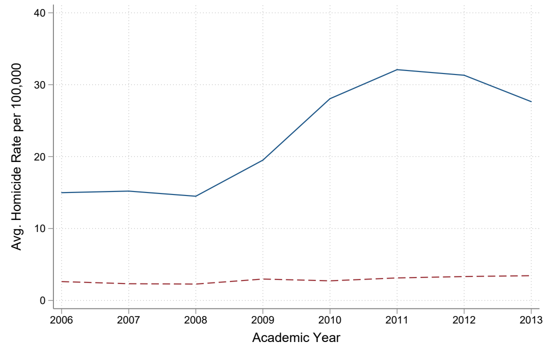
[33]The 65th and 70th percentile thresholds correspond to homicide rates of 12.93 and 15.13 per 100,000 people, respectively.

Figure C.1: Alternative Definitions of Violent Municipalities

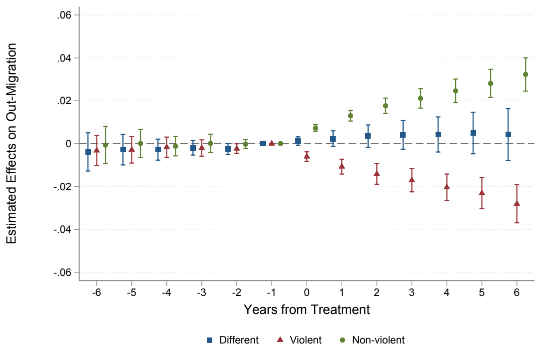
(a) Homicide Rate Trends: 65th



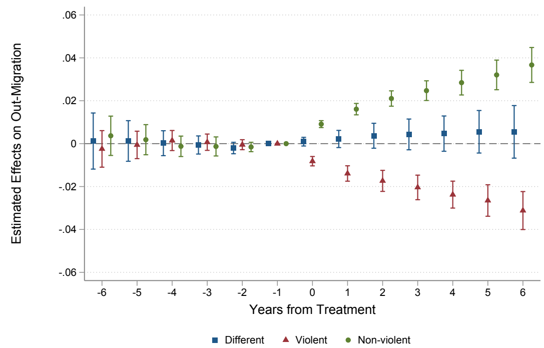
(b) Homicide Rate Trends: 70th



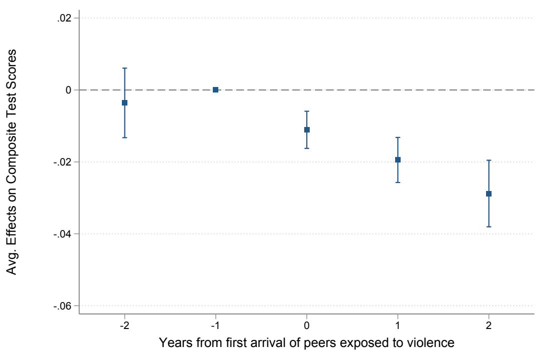
(c) Estimated Effects on Out-migration: 65th



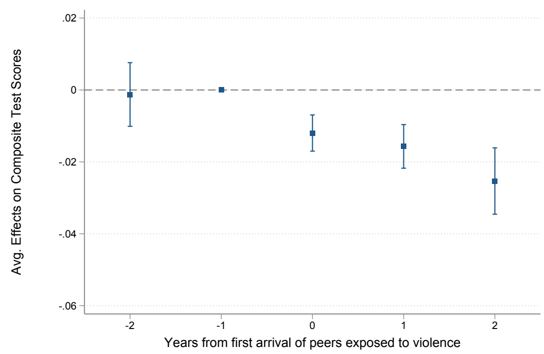
(d) Estimated Effects on Out-migration: 70th



(e) Estimated Peer Effects: 65th



(f) Estimated Peer Effects: 70th



Notes: See notes in figures 2, 6 and 12.