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MOVING FOR GOOD:
EDUCATIONAL GAINS FROM LEAVING VIOLENCE BEHIND

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ABSTRACT

This paper estimates the effects of moving away from violent environments into safer areas on migrants' academic achievement in the context of the Mexican war on drugs. Using student location choices across space and over time, we recover individual-level migration paths for elementary school students across all municipalities in Mexico. We find that students who were induced to leave violent areas due to increased violence experience academic gains after relocating to safer areas. Students who migrated from municipalities in the 90th percentile of the violence distribution to municipalities in the 10th percentile experienced improvements of 5.3 percent of a standard deviation in their test scores two years after they migrated. These results appear to be explained by increases in school attendance and improvements in the learning environment after they moved.

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1 Introduction

Exposure to community violence has detrimental effects on children's educational trajectories. When local violence escalates, families may attempt to mitigate these detrimental effects of local violence by migrating to safer areas. However, migration involves significant costs, including up-front relocation expenses, forgone earnings, and assimilation costs in the new location. Moreover, students who switch schools may experience learning losses as they adjust to a new school environment.[1] As a result, it is unclear whether the welfare gains from relocating to a safer environment outweigh the associated migration costs.

In this paper, we provide causal evidence on the returns to migration, examining the potential academic gains from leaving violence behind. To do so, we focus on the role of violence as a determinant of out-migration. Using violence-induced migration as a quasi-exogenous shock, we analyze how relocating to safer areas affects students' academic performance. Our study takes place in the context of the Mexican war on drugs, a period marked by high levels of local violence in affected areas in Mexico.

Estimating the returns to migration presents several empirical challenges. Movers and non-movers are likely to differ in unobservable characteristics, and naïve comparisons between movers and stayers can lead to biased estimates. Moreover, the same concern arises when comparing movers who migrate to different destinations, as the destination choice may be correlated with unobservables that affect the outcomes. To address these potential concerns, we estimate a model that compares test scores before and after migration for the same student and leverages the variation within destination schools across students who relocate from municipalities with different levels of local violence. We compare students who migrate to the same destination schools located in areas that remained safe during the period of analysis. This strategy allows us to separately identify the effects of changes in exposure to local violence (safety gains) from individual time-invariant traits that affect academic

[1] For example, Hanushek et al. (2004) documents negative effects for students who change schools within a district. Similarly, Booker et al. (2007) finds that students moving from traditional public to charter schools face temporary achievement declines during the transition.

performance.

Our setting and empirical approach allow us to overcome challenges typically present in the migration literature. Following Padilla-Romo and Peluffo (2023b), we use students' location choices to build comprehensive migration flows over time. By doing so, and in contrast to most papers in the migration literature, we are able to analyze migration using individual-level data that target the population of school-aged children in Mexico (for children enrolled in grades 3-6). Unlike Padilla-Romo and Peluffo (2023b), who study the effects of migration on incumbent (non-migrant) students, in this paper, we track the academic trajectories of those who migrate. Moreover, we control for unobservable individual characteristics and time-varying characteristics common to the destination schools by introducing student and school of destination-by-time relative to moving fixed effects in our analysis.[2] Importantly, migration happens in a context with a centralized educational system, which implies that, unlike refugees, international migrants, and national migrants in countries with decentralized school systems, movers do not need to adjust to a new language, significant cultural changes, or a new education system. This context mitigates potential sources of bias arising from heterogeneity in post-migration adaptation costs.

Our results indicate that among migrant students in elementary schools in Mexico, relocation to safer local environments causes significant increases in test scores, which appear to be explained by increases in school attendance and improvements in the school environment. We find that a one standard deviation increase in safety upon migration, relative to the mean, leads to an increase in students' test scores of four percent of a standard deviation two years after they move.[3] For example, students who migrated from municipalities in the 90th percentile of the violence distribution to municipalities in the 10th percentile experi-

[2] Aizer (2007) highlights that children exposed to more community violence tend to be disadvantaged in other respects, which imposes challenges to the identification of causal effects of exposure to violence and children's outcomes. Being able to include student fixed effects allows us to control for individual characteristics (such as family background or baseline poverty levels) that can confound the estimated effects.

[3] In our analysis, safety is defined considering changes in the seven-year pre-migration average homicide rates between origin and destination municipalities (Section 3 provides details). The standard deviation in safety gains is 13 homicides per 100,000 people.

enced improvements of 5.3 percent of a standard deviation in their test scores two years after they migrated.

We evaluate the robustness of our results to potential threats to identification and test the plausibility of the assumptions on which the model relies. Specifically, using a stacked difference-in-differences specification, we show that our estimates are unlikely to be biased by treatment effect heterogeneity or dynamic effects. Then, because our baseline model imposes symmetry with respect to increases and decreases in violence, we relax this assumption by allowing the treatment effect to vary depending on whether students moved to safer or more violent destinations. Consistent with symmetry, we find similar point estimates for students who move to safer and more violent destinations (relative to their municipality of origin), but the estimates are noisier for the latter (smaller) sample. Moreover, because our baseline model assumes linearity in the effects of safety gains, we estimate a flexible specification that allows the effect of safety gains to vary across their distribution to examine whether this assumption is supported by the data. We do not find substantial departures from linearity, suggesting that the relationship between improvements in safety and academic achievement is approximately linear. Finally, we show that the gains in test scores associated with increased safety upon relocation are unlikely to be driven by broader improvements in local economic conditions.

The paper contributes to the literature that estimates the effects of migration from (broadly defined) disadvantaged areas on individual outcomes.[4] Among these studies, the experimental literature has studied the effects of moving out of poor neighborhoods in the U.S. in the context of the randomized program Moving to Opportunity (Sanbonmatsu et al., 2006; Chetty et al., 2016; Kling et al., 2007; Clampet-Lundquist and Massey, 2008; Ludwig et al., 2013; among others) and the effects of temporary migration in Bangladesh during the lean season (Bryan et al., 2014). Studies on the effects of relocation on migrants' outcomes using non-experimental settings include the effects of relocation due to natural disasters

[4]In our case, a disadvantaged area is defined in terms of levels of violence, which are not necessarily correlated with economic deprivation.

(Deryugina et al., 2018; Deryugina and Molitor, 2020), due to public housing demolitions (Jacob, 2004; Chyn, 2018; Haltiwanger et al., 2020), in the context of the great migration (Collins and Wanamaker, 2014; Countryman, 2017), and the effects of moving to better neighborhoods (Chetty and Hendren, 2018) in the U.S. or to districts with higher educational attainment in Indonesia (Schwank, 2024).[5] However, little is known about the causal effects of improvements in safety on students' academic performance. This paper fills this gap in the literature by examining the academic returns to migration for students who relocate in response to rising violence in their municipalities of origin. While related to the neighborhood effects literature, this context is distinct in that migration is induced by a specific shock characterized by local increases in violence that occur at different points in time and across locations rather than by broader neighborhood disadvantage. This study provides novel evidence by focusing on the role of safety in shaping students' educational outcomes.

We also contribute to understanding how drug-trafficking-related violence affects human capital accumulation in the context of Mexico. It has been documented that increases in local violence lead to significant reductions in academic achievement in terms of test scores, years of education, school completion, and grade retention (Caudillo and Torche, 2014; Jarillo et al., 2016; Brown and Velásquez, 2017; Orraca-Romano, 2018; Chang and Padilla-Romo, 2022; Michaelsen and Salardi, 2020). Moreover, in previous studies, we have shown that the effects of violence are not restricted to the areas in which violence occurs. Specifically, violence-induced migration of previously violence-exposed students generates negative spillover effects on academic achievement among incumbent students in safe areas in the short run (Padilla-Romo and Peluffo, 2023b), which persist later in life (Padilla-Romo and Peluffo, 2023a). We add to this literature by examining the academic returns to violence-induced migration to safe destinations, showing that the harm of exposure to violence is not permanent and that mitigation strategies, such as out-migration to safe areas, can be effective.

[5] Chyn and Katz (2021) provides an excellent review of the literature on neighborhood effects with a focus on evidence from high-income countries.

The remainder of the paper is organized as follows. Section 2 discusses the institutional background. Section 3 describes the data used in our analysis. Section 4 presents the empirical strategy. Section 5 discusses the main results. Section 6 examines potential mechanisms, and Section 7 reports robustness analyses. Section 8 concludes.

2 Institutional Background

The Mexican war on drugs started at the end of 2006, under the presidency of Felipe Calderón. This national security strategy against drug trafficking organizations (DTOs) was characterized by the deployment of federal armed forces to confront DTOs and to restore safety in areas that were particularly affected by these criminal organizations. Homicide rates temporarily declined at the beginning of this process. However, starting in 2008 violence escalated. The years that followed were marked by a sharp increase in salient public displays of violence, with the national homicide rate more than doubling between 2006 and 2012.

The increase in violence in Mexico was not uniform: some areas remained relatively safe, while others experienced unprecedented increases in violence. In response, significant relocation occurred across different areas of the country, with people migrating out of municipalities with high levels of violence (Ríos, 2014; Márquez-Padilla et al., 2019; Sobrino, 2019; Padilla-Romo and Peluffo, 2023b). In our analysis, we leverage the heterogeneous increase in violence across space and over time to recover the educational returns to out-migration for elementary school students who relocate to safe areas.

In Mexico, elementary school is part of the broader basic education system, which comprises three levels: preschool (ages 3–5), elementary (ages 6–12), and lower secondary school (ages 12–15). The academic curriculum for public and private elementary schools is set at the federal level by the Mexican Secretariat of Public Education (SEP). In the years included in our analysis, elementary school students in public and private schools took ENLACE, a national standardized diagnostic test administered annually to students in grades 3 through 6. The test was designed to have universal coverage for all students in the targeted grades, and elementary school enrollment was nearly universal.[6]

3 Data

To identify migrants, we use data provided by *Xaber* from ENLACE for students in grades 3-6 of elementary school in Mexico. The data include students enrolled in schools that belong to the public and private systems who took the test once per academic year between 2007/08 and 2012/13. ENLACE contains anonymized student identifiers that allow us to track students over time.[7] We use the location of the school in which a student is enrolled when taking the test each year to identify migratory flows. Students are classified as movers if they switch schools between academic years y and $y + 1$, moving to a school located in a municipality different from the one in which they were enrolled during academic year y. The new municipality may be in the same state or in a different state.

ENLACE contains questions on math, reading, and a third rotating subject. Our main outcome variable is the standardized composite test score (math and reading). Our sample includes only movers, i.e., students who changed their municipality of school enrollment exactly once between 2007/08 and 2012/13, excluding students with multiple moves. We further restrict our analysis to students observed in four consecutive years of elementary school.[8]

The ENLACE exam is administered over two days in eight 45-minute sessions. Consequently, students who are absent on either day may miss parts of the test, particularly one

[6]Mexico's net (gross) primary school enrollment rate averaged 98% (110.6%) during 2008–2013, according to World Bank (UNESCO) data. Participation rates in ENLACE were high: over the same period, the average attrition rate at the school-grade level was 13%. Furthermore, Padilla-Romo and Peluffo (2023b) provide evidence that attrition rates across municipalities are uncorrelated with increases in the homicide rate during the Mexican war on drugs.

[7]The construction of the anonymized student-level panel dataset is described in Xaber (2020).

[8]To avoid giving more weight to some students, for those observed for more than four consecutive years, we include the first four consecutive observations in our analysis.

of the subject sections (either math or reading), or may not complete all questions in a given subject. Furthermore, after the administration of the exam, the results undergo automated verification to assess their reliability.[9] The ENLACE dataset flags students who either did not answer at least half of the questions in a given subject or whose exam results were classified by the verification system as unreliable. We restrict our main analysis to students without flagged test scores.[10]

To shed light on the mechanisms driving the main results, we use ENLACE's context questionnaire, which contains self-reported information regarding attendance behavior, incidence of bullying, and school environment for a sample of students taking the ENLACE exam from 2007/08 to 2012/13. Specifically, students answer the following questions with possible answers never (0), hardly ever (1), sometimes (2), almost always (3), and always (4): How often do you skip school? How often do you receive physical aggression from your classmates? How often are there physical aggression or fights in your school? How often are there threats in your school? How often do students make fun of other students? How often do students make fun of teachers? How often do students damage school property? For each of these questions, we standardize the outcome for each academic year to have a mean of zero and a standard deviation of one.

To measure exposure to local violence at the municipality and academic-year level, we rely on official mortality records from the National Institute of Statistics and Geography of Mexico (INEGI). We construct homicide rates as the number of deaths registered as presumed homicides per 100,000 inhabitants in an academic year (August to July), using population data from the National Population Council (CONAPO). To identify safe municipalities, we first calculate the maximum annual homicide rate observed between the 2005/06 and 2012/13 academic years for each destination municipality. We then consider the distribution of these

[9] One of the benefits of ENLACE test scores is that the exam administration was overseen by individuals not affiliated with the schools, reducing the scope for manipulation (De Hoyos et al., 2021). To detect potential cheating, the verification system analyzes response patterns using the *K-index* and *Scrutiny* methods, focusing specifically on similarities in incorrect answers among test-takers.

[10] For our sample of movers to safe municipalities, 8.7% of all observations are flagged. In Section 7, we show that the results are robust to including students with fewer than four reliable observations.

maximum homicide rates across all destination municipalities among movers. Municipalities with a maximum homicide rate below the median of this distribution are classified as safe.[11]

Figure 1 shows the annual homicide rate per 100,000 people separately for safe and violent municipalities between the academic years 2001/02 and 2012/13. Before 2007, the homicide rate remained relatively stable in each group. However, the level of violence was significantly lower in municipalities classified as safe, with a homicide rate of around 4 homicides per 100,000 (which is slightly below the U.S. national homicide rate during the same period). Starting in 2007/08, while the homicide rate in safe municipalities remained stable, homicide rates in violent municipalities rose sharply, reaching more than 30 homicides per 100,000 people. Figure 2 shows the location of safe municipalities across Mexico.

To examine the possibility that students who arrive at the same school in a safe municipality from more violent minicipalities might experience differential gains in their families' economic opportunities relative to those coming from less violent municipalities, we complement our analysis with data from Mexico's National Survey of Occupation and Employment (Encuesta Nacional de Ocupación y Empleo, ENOE). To do so, we restrict the analysis to municipalities that, in each academic year, have at least 100 observations for individuals between 15 and 65 years old. For each municipality and academic year, we then calculate municipal averages of labor force participation, employment, weekly hours worked, and monthly earnings in Mexican pesos using the individual survey weights.[12]

Table 1 presents students' test scores, as well as schools' and municipalities' characteristics, separately for those who moved to a safe municipality that was either more or less violent than their municipality of origin. We define pre-migration violence as the average homicide rate in the municipality of origin over the seven years prior to the move.[13] Although

[11] The homicide rate in safe municipalities did not exceed 19.38 homicides per 100,000 people between 2005/06 and 2012/13 academic years. When there is a tradeoff between sample size and a cutoff, the median is a commonly used threshold. While this definition is arbitrary, in Section 5.3, we show that our results are robust to using other thresholds.

[12] ENOE has four waves per year, one per quarter. We define an academic year as the fourth quarter of the calendar year and the first three quarters of the following year (e.g., the academic year 2008/09 includes the fourth quarter of 2008 and the first three quarters of 2009).

[13] For example, for a student who moved to a safe municipality in the academic year 2010/11, the level of

students who moved to less violent municipalities have slightly higher baseline test scores on average, the differences are small and not statistically significant. A similar pattern is observed in Figure 3, which shows that for students who migrate to safe municipalities, the change in average seven-year pre-migration homicide rates between origin and destination municipalities is not systematically related to students' baseline (pre-migration) performance rank. Students moving to safer municipalities are slightly more likely to be enrolled in private schools and to move away from schools located in urban settings and from municipalities with lower poverty levels, as indicated by the pre-migration (2007) share of Progresa beneficiaries, compared to students who move to more violent municipalities.[14] Considering labor market conditions, destination municipalities for students who move to more violent municipalities exhibit, on average, slightly higher labor force participation, employment, weekly hours worked, and monthly earnings relative to their municipalities of origin. In contrast, destination municipalities have, on average, lower weekly hours worked and monthly earnings than their origin municipalities for students moving to safer municipalities. As with the other variables, however, these differences are small and not statistically significant.

4 Identification Strategy

We restrict our sample to movers to estimate the relative gains of moving out of violence. A mover is a student who was enrolled in school A in academic year y and in school B in academic year $y+1$. We require schools A and B to be located in different municipalities, and school B to be located in a municipality that was safe during the analysis period.

Our main estimation equation is as follows:

$$TS_{isgt} = \alpha_i + \gamma_{gt} + \eta_{s_dt} + \sum_{j \neq -1} \beta_j 1(t=j) \Delta Safety_{im_om_dt} + \epsilon_{isgt} \quad (1)$$

violence exposure is measured as the average homicide rate between 2003/04 and 2009/10.

[14]Progresa is a conditional cash transfer program aimed at low-income households. Data on the number of Progresa beneficiaries at each school in 2007 were provided by the Ministry of Education.

where TS_{isgt} represents the standardized test score for student i, enrolled in school s, grade g in year relative to moving t, α_i, γ_{gt}, and η_{s_dt}, are student, grade-by-year relative to moving, and relative year by school of destination fixed-effects. $\Delta Safety_{im_om_dt}$ measures the standardized pre-migration difference of the seven-year average in homicide rates in the municipality of origin relative to the destination municipality for student i. Standard errors are clustered at the student level.[15] In this model, β_j captures the effects on test scores of a one standard deviation increase in safety ($\Delta Safety_{im_om_dt}$) j years after moving, relative to the mean across all movers.

Suppose we observe that new students who relocate from municipalities experiencing high levels of violence to safe municipalities perform better, on average, than those who migrate from other relatively safe municipalities. In that case, we cannot conclude that safety gains generate an improvement in academic achievement because these correlations may be due, at least in part, to selection into specific destination areas. To address this concern, we control for the school of destination-by-time relative to moving fixed-effects. Intuitively, we are comparing students who migrated from areas that experienced higher levels of violence relative to students who are enrolled in the same destination school and migrated from relatively safe areas the same number of years ago. Thus, the model allows for selection into destination schools and leverages the variation in the level of violence in the municipality of origin to recover the estimates of interest. In addition, including student fixed effects allows us to control for time-invariant individual characteristics that may affect academic achievement.

In some specifications, we control for the differences in quality of education between the origin and destination, interacted with the indicators for each year relative to moving. First, we include $\Delta Score_{im_dm_o}$, defined as the baseline difference in average standardized test scores between the municipality of destination (m_d) and the municipality of origin (m_o).

[15] Figure A.1 shows the distribution of $\Delta Homicide_{im_om_dt}$ and $\Delta Safety_{im_om_dt}$ for all students in our sample, who migrated to safe municipalities. Overall, 65% of students move to destination municipalities with lower homicide rates than their origin municipalities.

This variable captures broader location-level differences in educational performance that may be correlated with both migration decisions and subsequent student outcomes. In our preferred specification, we include $\Delta Score_{is_d s_o}$, defined as the baseline difference in average standardized test scores between the school of destination (s_d) and the school of origin (s_o), capturing school-level differences in academic quality.

The identification of the effects relies on the assumption that, in the absence of migration (conditional on the controls included in the model), test scores would not exhibit differential trends among students who eventually enrolled in the same school but had lived in areas with different levels of violence before migrating. By analyzing trends in student outcomes prior to migration, in Section 5, we provide supporting evidence for this assumption. Specifically, we show that students in high-violence areas (relative to the destination) were not experiencing improvements in test scores before migration.

Because students move at different times, a potential concern is that treatment effects may vary across cohorts or change over time (Goodman-Bacon, 2021; Sun and Abraham, 2021). To address this possibility, in Section 7, we re-estimate our results using a stacked difference-in-differences approach, in which we saturate the model with cohort-specific fixed effects by interacting all fixed effects with indicators for the year of the move.

5 Results

5.1 Main Estimates

Considering all movers to safe municipalities, Column 1 of Table 2 reports estimates from our baseline specification in Equation (1). The specifications in columns 2 and 3 additionally include controls for differences in the quality of education between the origin and destination municipalities and schools, respectively. In each case, these are measured by the pre-migration differences in average test scores between the destination and origin municipalities or schools.[16] Regardless of the specification, the results indicate that moving away from violent municipalities improves students' academic achievement. The estimates from our preferred specification in Table 2, Column 3, indicate that a one standard deviation increase in safety, relative to the mean, increases students' test scores by 1.9 percent of a standard deviation the year of the move. This effect rises to 4 percent two years after moving.[17,18] The table also shows that controlling for the relative gains in education quality leaves the point estimates largely unchanged, suggesting that differences in educational quality between origin and destination are not driving the results.

Figure 4 presents the estimated results using our preferred specification in Column 3 of Table 2, when including the pre-treatment effects. One potential concern is that the post-migration differential gains for students previously exposed to higher levels of violence could be explained by differential trends in academic achievement among students arriving from areas with heterogeneous homicide rates relative to the destination. However, the coefficients for the years before migration show that this is unlikely, as they are close to zero and statistically insignificant. Figure 5 examines whether the effects of safety improvements on academic achievement vary by subject by estimating the same model as in Figure 4 separately for math and reading. We find that the effects are similar across subjects, suggesting that improvements in safety benefit student performance broadly.

We also evaluate whether the effects presented in Table 2 mask meaningful heterogeneity for students across the test score distribution. To do so, Figure 6 presents unconditional

[16] In Figure A.2, panels (a) and (b), we show the distribution of the relative change in education quality between the origin and destination municipalities and schools, respectively. In Figure A.3, we further show that these changes in education quality are not correlated with students' baseline (pre-migration) test score rank.

[17] For example, for a student who in 2010/11 migrated from Ciudad Juárez (where the average homicide rate between 2003/04 and 2009/10 was 71.69) to Guadalajara (where this average was 9.06), the average estimated increase in test scores is 18.8 percent of a standard deviation two years after migrating. This number is calculated using the seven-year standardized change in average pre-moving homicide rates (4.7) and the estimated effects in Column 3 of Table 2. That is, $4.7 \times 0.04 = 0.188$.

[18] Recall that a one standard deviation increase in safety is measured by an increase in one standard deviation in the pre-migration difference of the seven-year average in homicide rates between the origin municipality and the destination municipality (13 homicides per 100,000 people), relative to the mean safety increase across movers.

quantile regression estimates using the method proposed by Firpo et al. (2009). To improve precision, when estimating effects by quantile, the analysis in Figure 6 focuses on the average treatment effects by interacting the post-move indicator with the standardized change in safety rather than considering dynamic effects. The estimated effects are relatively stable across quantiles, suggesting that the average effect we estimate is broadly representative of impacts experienced by students across the performance distribution. While the estimates are not statistically different across quantiles, the point estimates are smaller for students in the top 20 percent of the distribution, suggesting that the benefits of moving to safer areas are less pronounced at the upper end of the performance distribution.

In Figure 7, we examine how relative increases in safety translate into contemporaneous homicide rate reductions for students leaving violent areas behind. We find that a one standard deviation increase in the seven-year average pre-migration difference in homicide rates between the origin and the destination municipality (relative to the mean) is associated with an average reduction of approximately 25 homicides per 100,000 people for elementary school movers.

5.2 Returns to Migration and Persistence in Violence Exposure

Our estimates show that safety gains following relocation result in improvements in academic achievement. A related question is whether these academic gains depend on the duration of violence exposure in the municipality of origin. Table 3 presents how the estimated effects of moving to a safer municipality on test scores vary depending on the length of time used to measure prior exposure to violence. Specifically, each column reports estimates from our main regression in Equation (1), considering our preferred specification (Column 3 of Table 2) and varying the number of years over which the homicide rate in the origin and destination municipalities is calculated prior to migration (from 1 year in Column 1 to 7 years in Column 7).[19] By increasing the number of years over which we calculate our measure for exposure to

[19]Figure A.4 presents estimates similar to those in Columns 1-6 of Table 3, but including effects for the years prior to migration. In all cases, the estimates for the pre-migration years are close to zero and not

violence, we examine whether students who experienced violence over longer periods benefit more from migrating. The results show that the gains in test scores after moving tend to be larger and more precisely estimated as the years of exposure in the origin increase. This suggests that students exposed to persistent community violence prior to migration experience larger academic improvements after relocating to safer areas than those who left areas that only recently became violent.

5.3 Heterogeneous Effects by Safety Levels in the Destination

To better understand the role of safety in driving academic gains, we examine whether the estimated effects of relocation change when we consider alternative thresholds for classifying destination municipalities as safe. To do so, we estimate our preferred specification (Column 3 of Table 2) using different definitions of a safe municipality.

Table 4 presents the estimated results considering four thresholds corresponding to the maximum homicide rate (between 2001/02 and 2012/13) below which a given percentage of movers in the sample relocated (specifically, 45%, 50%, 55%, and 60% of students). As we move from columns 1 to 4, the definition of a safe municipality becomes broader, including destination municipalities with higher homicide rates. The estimated results show that the academic returns of safety gains from relocation are generally higher as the absolute safety at the destination increases.[20] The overall effects of moving to a safer area on test scores are positive and statistically significant across all thresholds, suggesting that our main conclusions are robust to the cutoff used to define a municipality as safe.

statistically significant. The effects are robust to changes in the number of years used to average pre-migration homicide rates, and the estimates become more precise as the exposure window increases.

[20]For example, a one standard deviation increase in safety (relative to the mean) leads to an increase in students' test scores of 5.2 percent of a standard deviation two years after moving when considering the 45% threshold. The corresponding increase is 3 percent of a standard deviation two years after moving when considering the 60% threshold.

6 Mechanisms

Since exposure to community violence has been shown to have detrimental effects on school attendance (Koppensteiner and Menezes, 2021) and has been linked to internalizing and externalizing behavioral problems among children (Margolin and Gordis, 2000; Dustmann and Fasani, 2015; Chang and Padilla-Romo, 2022), improvements in safety can help increase attendance and enhance the school environment. In this section, we provide suggestive evidence of potential mechanisms underlying the improvements in the academic performance of students who migrated to safer areas. To do so, we leverage self-reported data on attendance behavior, bullying, and school environment, available for a subset of movers who are included in ENLACE's context questionnaire.

We use this information in a regression setting where we estimate a modified version of our preferred specification. We focus our analysis on all movers who migrated to a safe municipality with lower levels of violence (i.e., m_d is safe and $\Delta Homicide_{im_om_dt} > 0$) and estimate a model that includes the municipality of destination-by-academic-year fixed effects (α_{m_dy}).[21] Specifically, we estimate the following model:

$$Y_{im_dy} = Post_{iy}\beta + \alpha_{m_dy} + Post_{iy} \times \Delta Score_{is_ds_o}\delta + \epsilon_{im_dy} \tag{2}$$

where Y_{im_dy} are our different standardized measures of attendance behavior, bullying, and school environment, $Post_{iy}$ is an indicator for the years after the move for student i, and $\Delta Score_{is_ds_o}$ is the difference in test scores between the school of origin and destination for student i. We allow the error term to be correlated within the municipality of destination.

The results of this specification are shown in Figure 8. The estimates suggest that when students move to safe municipalities with lower levels of violence than their origin municipality, they report improvements in school attendance, a lower incidence of bullying, and a better school environment. That is, they report skipping school and being physically

[21] Since most students and schools in this sample are observed at most once, we are unable to include student fixed effects or school of destination-by-time relative to the move fixed effects.

abused by their classmates less often. They also report lower incidences of physical aggression or fights in their schools, students making fun of students, and students damaging school property. These results suggest that increased attendance and an educational environment more favorable to learning are potential mechanisms behind the improvements in academic performance we document in Section 5.[22]

7 Robustness

Our main specification in Equation (1) relies on an event-study specification, where time is defined relative to the year in which students migrate. The model includes a rich set of controls such as student, grade-by-year-relative to moving, and school of destination-by-year-relative to moving fixed effects. Since the identification allows for comparisons across cohorts in the same relative time period, a potential concern can arise if treatment effects are heterogeneous across cohorts or evolve over time (Goodman-Bacon, 2021; Sun and Abraham, 2021). Considering this possibility, we re-estimate our results using a stacked difference-in-differences (DiD) specification, in which we saturate the model with cohort-specific fixed effects (interacting all the fixed effects in the model with year of the move fixed effects).

Figure A.6 shows estimates for our preferred specification for the standard event study and the stacked DiD specifications. Due to the inclusion of cohort-specific fixed effects, some observations are dropped in the stacked DiD regression compared to the main regression in Figure 4. To match the sample, we use the same observations in the standard event study as in the stacked DiD specification. We obtain estimates that are close in magnitude across both specifications. Similar conclusions can be drawn when considering heterogeneous effects by years of exposure prior to migration (Table A.1), or when using alternative definitions of safety in the destination municipality (Table A.2). This suggests that our estimates are

[22] In Figure A.5 panels (a) and (b), we show the estimated effects on test scores and homicide rates, respectively, using this alternative specification. Consistent with our main results, test scores increase, and the homicide rate decreases when students move to safe municipalities with lower levels of violence than the municipality of origin.

unlikely to be biased due to treatment effect heterogeneity and dynamic effects.

Next, to evaluate the plausibility of the symmetry assumption we rely on in our baseline specification, we allow treatment effects to differ depending on whether students move to safe municipalities that are safer or to more violent than their origin municipalities. Table A.3 presents estimates, disaggregated by the direction of the change in safety. Column 2 reports estimates for students who moved to more violent areas (i.e., who experienced a decrease in safety), while Column 3 focuses on those who moved to safer areas. In both cases, there is a positive point estimate for the relationship between changes in safety and test scores. However, the coefficient for the smaller group of students who moved to more violent areas is less precisely estimated. These results suggest that the relationship between changes in safety and academic achievement is approximately symmetric.

We also test the linearity assumption of our baseline model by estimating a more flexible specification that allows the effect of safety gains to vary across the distribution of safety changes. Figure A.7 presents the estimated effect of migration on test scores by decile of the change in safety, relative to the fifth decile.[23] While there is some variation across deciles, the effects are generally monotonic and increase with greater improvements in safety, particularly in the upper part of the distribution. Importantly, we do not observe sharp nonlinearities or reversals in sign, and the confidence intervals across most deciles overlap with a linear prediction. Taken together, these results support the use of a linear specification in our main analysis.

A potential concern is that the positive relationship between safety gains and academic performance could be driven by a small number of students at the extremes of the safety change distribution. To examine this, in Figure A.8 we present a binned scatter plot of the relationship between residualized test scores and residualized safety changes. Both variables are residuals obtained after controlling for student fixed effects, changes in school quality, grade-by-relative-time fixed effects, and school-of-destination-by-relative-time fixed effects.

[23]The 5th decile corresponds to standardized changes in safety close to zero, with $\Delta Safety_{im_om_d} \in [-0.042, 0.045]$

The pattern in the plot suggests that extreme values at both ends of the distribution may attenuate the slope, suggesting that outliers may bias the results towards zero for a large share of students. In Table A.4, we present estimates separately, excluding students in the bottom and top percentiles of the residualized change in safety. Column 1 presents the baseline specification using the full sample of movers, columns 2 and 3 show estimates after dropping students in the top and bottom 1% and 5% of the safety change distribution. The coefficient on the interaction between the post-move period and the standardized change in safety is positive and statistically significant across all specifications. Importantly, the magnitude of the effect increases when extreme values are excluded: the coefficient rises from two percent of a standard deviation in the full sample to 3.8 and 4.8 percent of a standard deviation when excluding the top and bottom 1% and 5%, respectively.[24]

Given the possibility that violence spills over to nearby geographical areas, we estimate the effects excluding students who relocate to schools in adjacent municipalities. The results, presented in Figure A.10, are close to our main estimates, but are slightly larger in magnitude. Two years after migration, one standard deviation increase in safety relative to the mean translates into an improvement of 5.7 percent of a standard deviation in test scores for students migrating to non-neighboring municipalities, whereas the effect for all students is 4 percent.

A relevant question is whether students arriving from more violent municipalities might also experience larger improvements in economic opportunities, which could confound the estimated effects on test scores. To examine this, in Table A.5 we show the estimated effects from regressions using our preferred specification, but replacing the outcome variable with municipality-level labor market measures, such as labor force participation, employment, weekly hours worked, and monthly earnings in the working-age population. The point estimates are very small and negative, suggesting that if anything, among students who arrive at

[24] Figure A.9, in the Appendix, presents the event study estimates under these sample restrictions. Overall, we observe a similar pattern as in our main results, with stronger positive gains when excluding extreme observations.

the same school, those relocating from more violent municipalities tend to experience lower gains in economic opportunities than peers coming from less violent municipalities. These findings indicate that differential improvements in economic opportunities are unlikely to explain the positive effects we document on test scores.[25]

Finally, in Table A.7, we evaluate the robustness of our estimates to relaxing the sample restrictions imposed in the analysis. The sample in our baseline specification includes only students observed in four consecutive years of elementary school and excludes any student with at least one test score flagged as unreliable in the ENLACE dataset. This restriction guarantees that all students in the sample have valid outcomes observed before and after they move and addresses concerns about partial or potentially invalid test histories. Columns 1 and 2 present estimates using a less restrictive sample that includes all reliable test scores for students observed in four consecutive years, considering the standard difference-in-differences and the stacked difference-in-differences specifications, respectively. In this case, students are included in the analysis for the years in which their test scores are not flagged, even if scores from other years are excluded due to unreliability. The estimates for the unrestricted sample are similar to our estimates using the restricted sample that only includes students without flagged test scores. Taken together, the estimates suggest that our results are not driven by sample selection based on complete or fully reliable test histories.[26]

8 Conclusion

This paper studies the causal effects of moving from violent areas to safer ones on students' academic performance. We rely on an administrative longitudinal dataset that includes in-

[25] In Column 1 of Table A.6, we restrict the analysis to municipalities included in the ENOE sample (as defined in Section 3) and find results similar to those obtained in the main sample. While these estimates should be interpreted with caution—because the labor market measures are likely to be endogenous—Column 2 shows that our main results are robust to controlling for municipality-level measures of labor market opportunities.

[26] Figure A.11 presents event-study estimates for the unrestricted sample. As in the case of our main results, pre-trends estimates are close to zero and statistically insignificant, and we find positive and significant effects for the years after treatment.

formation on students' location choices and results from a standardized test administered to the same student in consecutive years. Our research design, combined with the richness of the data, allows us to compare students with themselves before and after they migrate. Leveraging students' location choices and the variation in the level of violence across origin and destination municipalities in the context of the Mexican war on drugs, we identify the returns to migration for students who are pushed to out-migrate from municipalities that experience increases in violence. We find that improvements in safety following migration lead to higher test scores, with larger gains for students who were exposed to violence for longer periods prior to moving. To put our results in context, we find that, on average, students who migrated from municipalities in the 90th percentile of the violence distribution to municipalities in the 10th percentile experienced improvements of 5.3 percent of a standard deviation in test scores two years after they migrated. These increases in test scores are potentially driven by increases in school attendance, reductions in bullying, and improvements in the school environment after moving. Altogether, our results highlight the importance of neighborhood safety for children's human capital accumulation.

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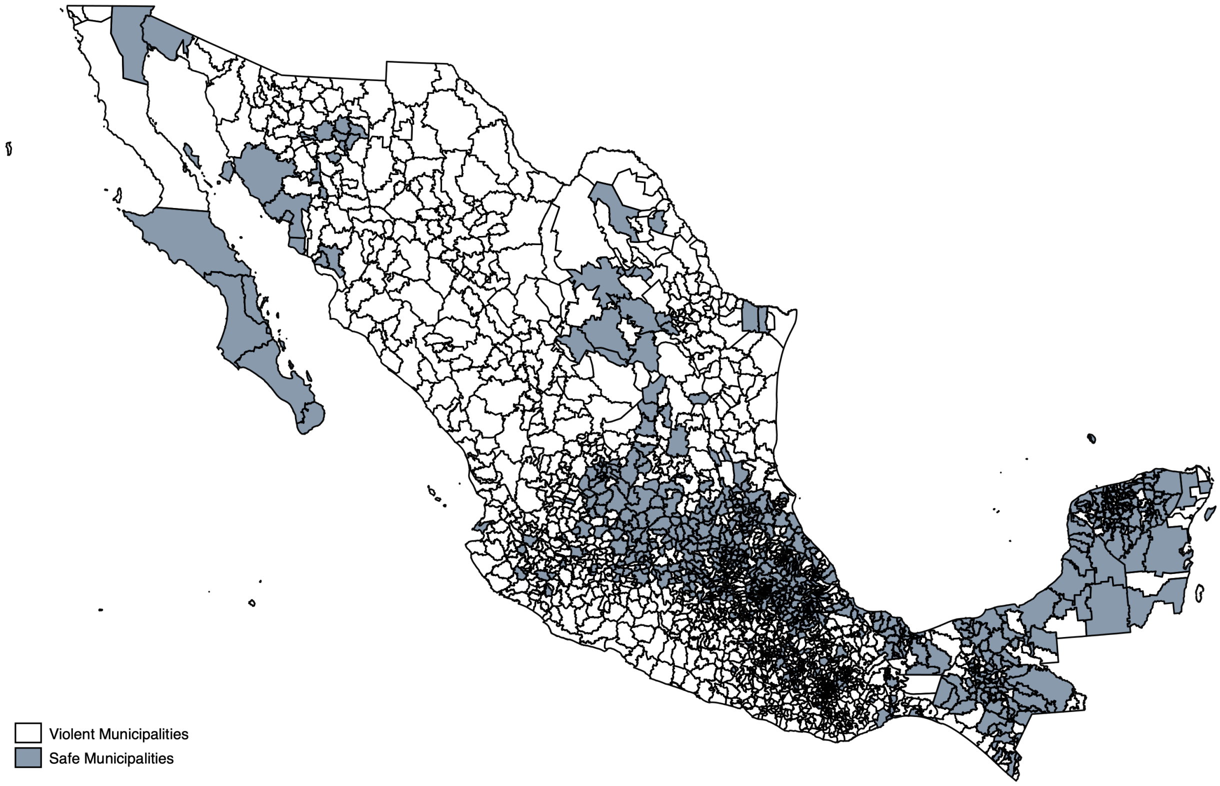
Figure 1: Annual Homicide Rate per 100,000 People by Municipality Safety

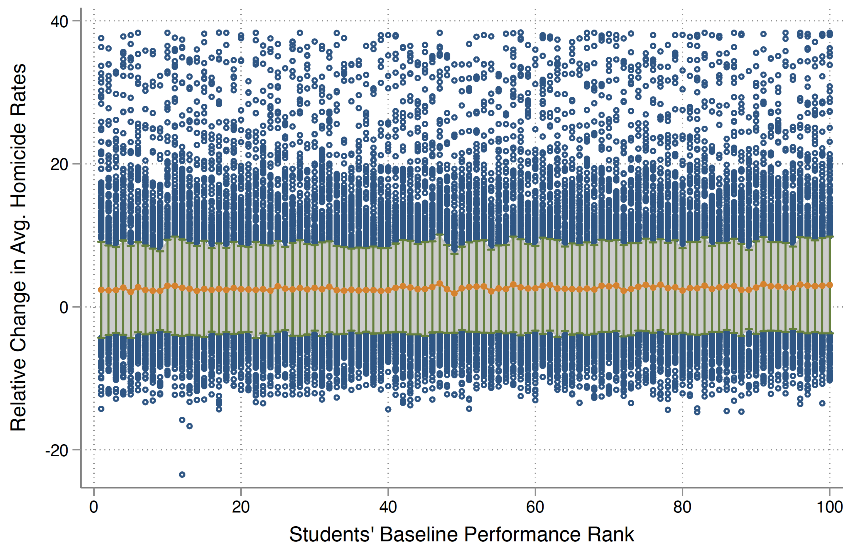
Notes: This figure shows the annual homicide rate per 100,000 people separately for safe (dashed line) and violent (solid line) municipalities.

Figure 2: Geographic Distribution of Safe and Violent Municipalities



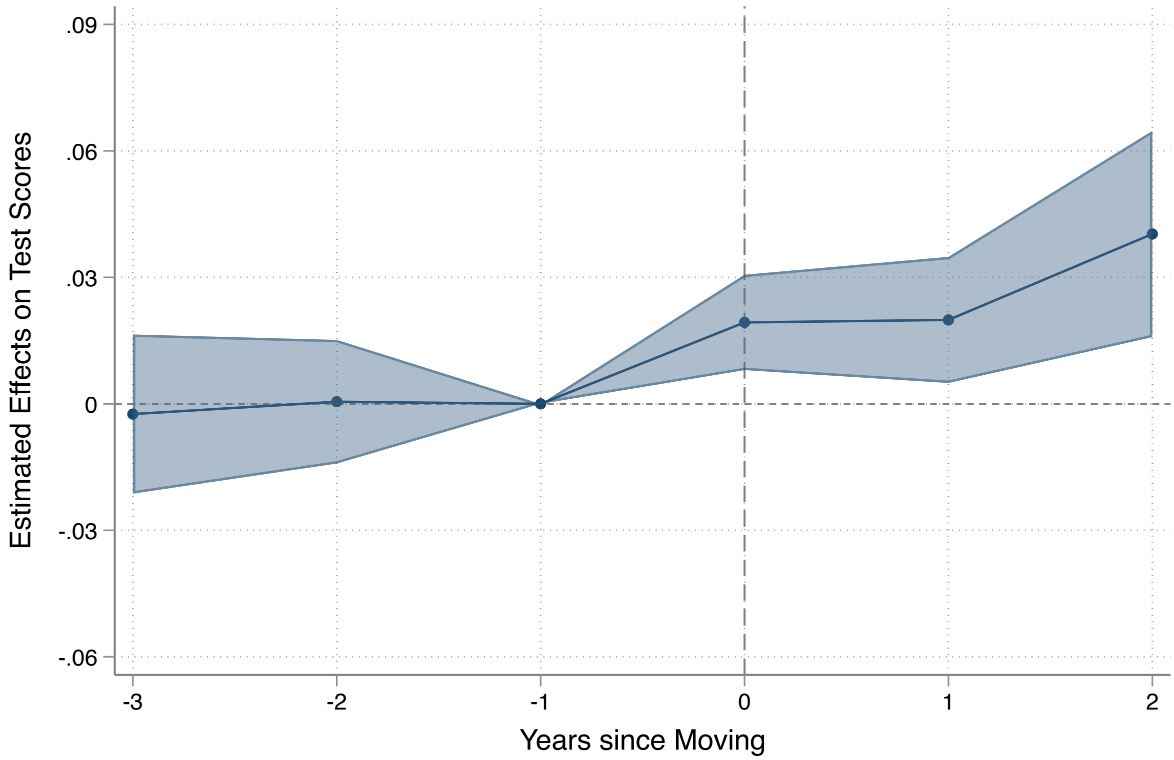
Notes: This figure shows the geographic distribution of safe (blue) and violent (white) municipalities. Out of the 2,454 municipalities in Mexico, 836 municipalities are classified as safe.

Figure 3: Baseline Performance Rank and Change in Average Homicide Rate ($\Delta Homicide_{im_o m_d}$)



Notes: The figure shows a scatter plot together with the mean and confidence intervals. The vertical axis shows the relative change in test scores between the municipality of origin and destination. The horizontal axis displays the students' baseline (prior to moving) performance rank in the percentile distribution of movers' test scores.

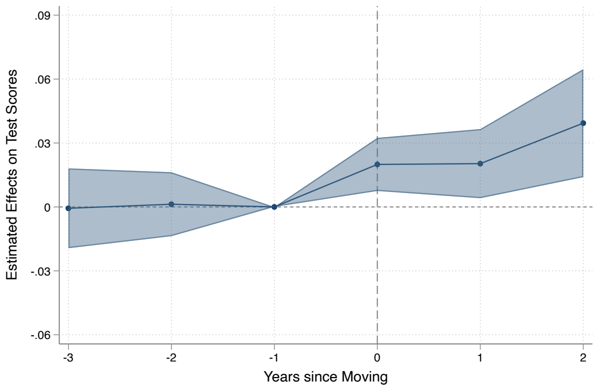
Figure 4: Estimated Effects on Test Scores



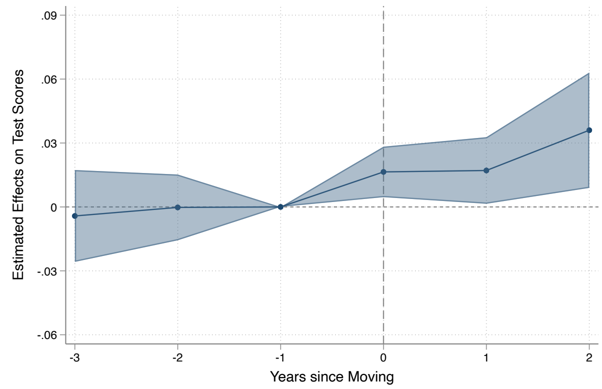
Notes: Estimates and confidence intervals come from the same regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and controls for the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. Standard errors for confidence intervals are clustered at the individual level.

Figure 5: Estimated Effects on Test Scores by Subject

(a) Math

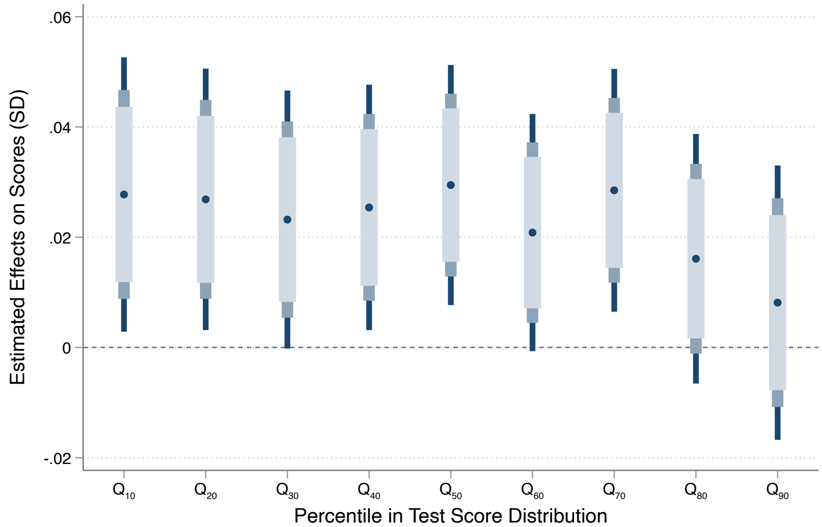


(b) Reading



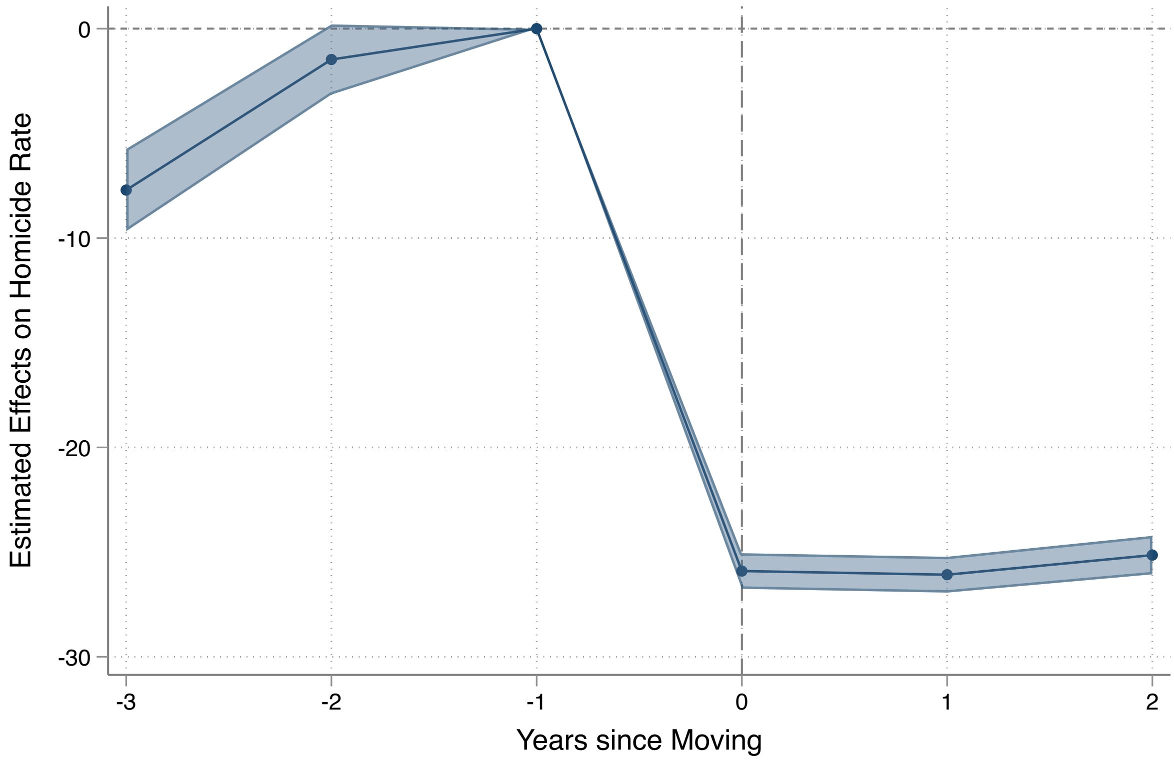
Notes: Estimates and confidence intervals in each panel come from a separate regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and controls for the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. Standard errors for confidence intervals are clustered at the individual level.

Figure 6: Estimated Effects on Test Scores by Quantiles



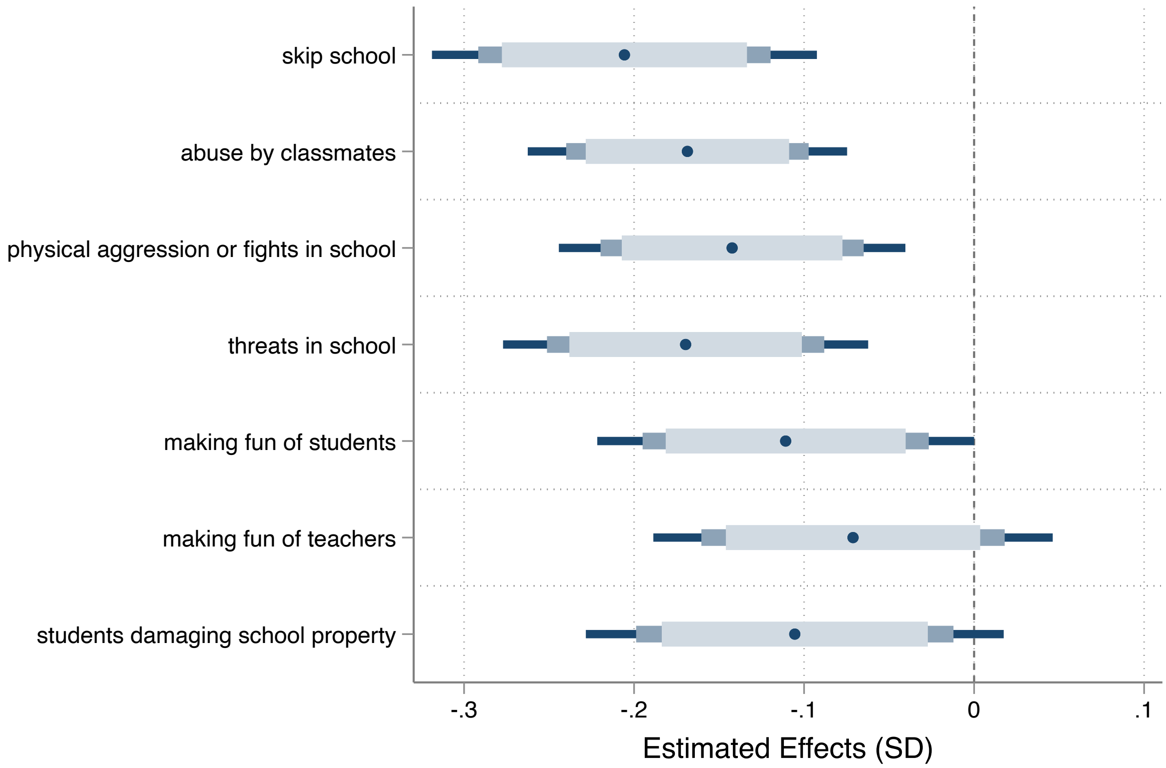
Notes: Each coefficient comes from a different regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and controls for the differences in school quality between the origin and destination interacted with a post-moving indicator. We show 90% (thicker), 95%, and 99% (thinner) confidence intervals. Standard errors for confidence intervals are clustered at the individual level.

Figure 7: Estimated Effects on Homicide Rate



Notes: Estimates and confidence intervals come from the same regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and controls for the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. Standard errors for confidence intervals are clustered at the individual level.

Figure 8: Estimated Effects on Attendance Behavior, Bullying, and School Environment



Note: Each coefficient comes from a different regression that includes the municipality of destination-by-academic year fixed effects and school average change in baseline test scores after the move. Dependent variables on the vertical axis are normalized for each academic year and described in Section 3. We show 90% (thicker), 95%, and 99% (thinner) confidence intervals. Standard errors for confidence intervals are clustered at the destination municipality level.

Table 1: Summary Statistics for All Movers to Safe Municipalities

	Move to more violent $\Delta Homicide_{im_o m_d} < 0$		Move to safer $\Delta Homicide_{im_o m_d} \geq 0$	
	Mean (1)	SD (2)	Mean (3)	SD (4)
Baseline Composite Test Score	0.19	(0.96)	0.22	(0.96)
Baseline Math Test Score	0.17	(0.95)	0.19	(0.95)
Baseline Spanish Test Score	0.19	(0.97)	0.23	(0.97)
Maximum Homicide Rate in Destination Municipality	13.22	(3.78)	11.47	(4.31)
Private School of Origin	0.19	(0.39)	0.21	(0.41)
Private School of Destination	0.17	(0.38)	0.19	(0.39)
Rural School of Origin	0.17	(0.38)	0.11	(0.31)
Rural School of Destination	0.11	(0.32)	0.14	(0.35)
Share of Progresa Beneficiaries in Origin School	0.13	(0.18)	0.08	(0.14)
Share of Progresa Beneficiaries in Destination School	0.10	(0.14)	0.12	(0.16)
Avg. Labor Force Participation in Origin Municipality	0.63	(0.05)	0.64	(0.04)
Avg. Labor Force Participation in Destination Municipality	0.64	(0.04)	0.64	(0.05)
Avg. Employment in Origin Municipality	0.60	(0.05)	0.60	(0.04)
Avg. Employment in Destination Municipality	0.61	(0.04)	0.60	(0.05)
Avg. Weekly Hours Worked in Origin Municipality	25.31	(2.57)	25.71	(2.20)
Avg. Weekly Hours Worked in Destination Municipality	25.81	(2.40)	25.38	(2.42)
Avg. Monthly Earnings (MXN) in Origin Municipality	2316.98	(735.74)	2472.00	(824.27)
Avg. Monthly Earnings (MXN) in Destination Municipality	2473.15	(765.97)	2389.04	(730.31)
Observations	24167		45352	

Notes: The table shows summary statistics for students who migrated to safe municipalities, distinguishing between those whose destination was more violent (columns 1 and 2) and less violent (columns 3 and 4) than their municipality of origin. Homicide Rate is constructed using official mortality records from INEGI and population counts from CONAPO. The school-level variables come from the Ministry of Education. Students' test scores are reported considering the first observation (pre-migration) for each student in the sample of movers to safe municipalities. Municipality-level variables are calculated using data from ENOE and are reported considering average municipality characteristics for the first observation of each student.The rest of the variables are time-invariant.

Table 2: Estimated Effects on Test Scores

	(1)	(2)	(3)
Year of move $\times \Delta Safety_{im_om_dt}$	0.018***	0.020***	0.019***
	(0.005)	(0.005)	(0.005)
1 year after $\times \Delta Safety_{im_om_dt}$	0.017**	0.020***	0.020***
	(0.007)	(0.007)	(0.007)
2 years after $\times \Delta Safety_{im_om_dt}$	0.038***	0.042***	0.040***
	(0.012)	(0.013)	(0.012)
Year of move $\times \Delta Score_{im_dm_o}$		0.109***	
		(0.018)	
1 year after $\times \Delta Score_{im_dm_o}$		0.105***	
		(0.022)	
2 years after $\times \Delta Score_{im_dm_o}$		0.126***	
		(0.032)	
Year of move $\times \Delta Score_{is_ds_o}$			0.325***
			(0.010)
1 year after $\times \Delta Score_{is_ds_o}$			0.347***
			(0.012)
2 years after $\times \Delta Score_{is_ds_o}$			0.344***
			(0.017)
N	240025	240025	239292
Student FE	yes	yes	yes
Grade-by-relative time FE	yes	yes	yes
School of destination-by-relative time FE	yes	yes	yes

Notes: Each column represents a different regression. Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 3: Estimated Effects on Test Scores by Number of Years of Exposure Pre-Move

Average Homicide rate over:	1 year (1)	2 years (2)	3 years (3)	4 years (4)	5 years (5)	6 years (6)	7 years (7)
Year of move $\times \Delta Safety_{im_om_dt}$	0.012**	0.013**	0.014***	0.016***	0.016***	0.018***	0.019***
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
1 year after $\times \Delta Safety_{im_om_dt}$	0.014*	0.012	0.012*	0.012*	0.015**	0.018**	0.020***
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
2 years after $\times \Delta Safety_{im_om_dt}$	0.021	0.032**	0.035**	0.040***	0.043***	0.041***	0.040***
	(0.015)	(0.015)	(0.016)	(0.015)	(0.014)	(0.013)	(0.012)
Year of move $\times \Delta Score_{is_ds_o}$	0.325***	0.325***	0.325***	0.325***	0.325***	0.325***	0.325***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
1 year after $\times \Delta Score_{is_ds_o}$	0.346***	0.346***	0.346***	0.346***	0.346***	0.346***	0.347***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
2 years after $\times \Delta Score_{is_ds_o}$	0.344***	0.344***	0.344***	0.344***	0.344***	0.344***	0.344***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
N	239292	239292	239292	239292	239292	239292	239292
Student FE	yes	yes	yes	yes	yes	yes	yes
Grade-by-relative time FE	yes	yes	yes	yes	yes	yes	yes
School of destination-by-relative time FE	yes	yes	yes	yes	yes	yes	yes

Notes: Each column reports estimates from a separate regression using Equation (1), using our preferred specification (Column 3 of Table 2) and varying the window over which pre-move homicide rates are averaged to calculate pre-migration exposure to violence, from 1 to 7 years (Columns 1 to 7, respectively). Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 4: Estimated Effects on Test Scores by Definition of Safe Municipality

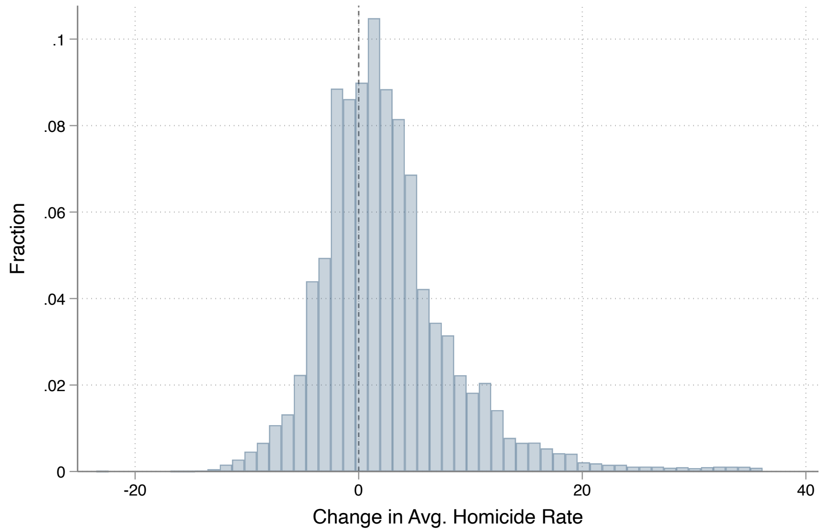
Percent of students:	45	50	55	60
Maximum homicide rate in destination municipality:	≤ 16.81	≤ 19.38	≤ 20.76	≤ 21.86
	(1)	(2)	(3)	(4)
Year of move $\times \Delta Safety_{im_om_dt}$	0.022***	0.019***	0.018***	0.013***
	(0.006)	(0.005)	(0.005)	(0.005)
1 year after $\times \Delta Safety_{im_om_dt}$	0.020***	0.020***	0.022***	0.017***
	(0.008)	(0.007)	(0.007)	(0.006)
2 years after $\times \Delta Safety_{im_om_dt}$	0.052***	0.040***	0.036***	0.030***
	(0.013)	(0.012)	(0.012)	(0.011)
Year of move $\times \Delta Score_{is_ds_o}$	0.321***	0.325***	0.324***	0.325***
	(0.011)	(0.010)	(0.010)	(0.009)
1 year after $\times \Delta Score_{is_ds_o}$	0.337***	0.347***	0.343***	0.342***
	(0.013)	(0.012)	(0.012)	(0.011)
2 years after $\times \Delta Score_{is_ds_o}$	0.336***	0.344***	0.345***	0.354***
	(0.018)	(0.017)	(0.017)	(0.016)
N	214185	239292	261452	287910
Student FE	yes	yes	yes	yes
Grade-by-relative time FE	yes	yes	yes	yes
School of destination-by-relative time FE	yes	yes	yes	yes

Notes: Each column reports estimates from a separate regression using Equation (1), using our preferred specification (Column 3 of Table 2), varying the definition of a safe destination. Columns 1 to 4 correspond to thresholds below which 45%, 50%, 55%, and 60% of movers relocated. Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively.

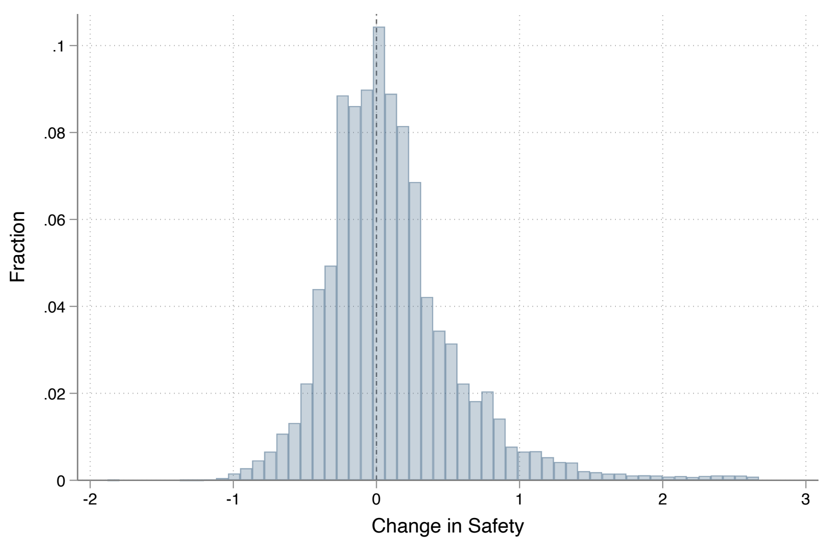
Appendix A Additional Figures and Tables

Figure A.1: Distribution of the Change in Average Homicide Rates in Safe Destinations ($\Delta Homicide_{im_om_d}$)

(a) $\Delta Homicide_{im_om_d}$



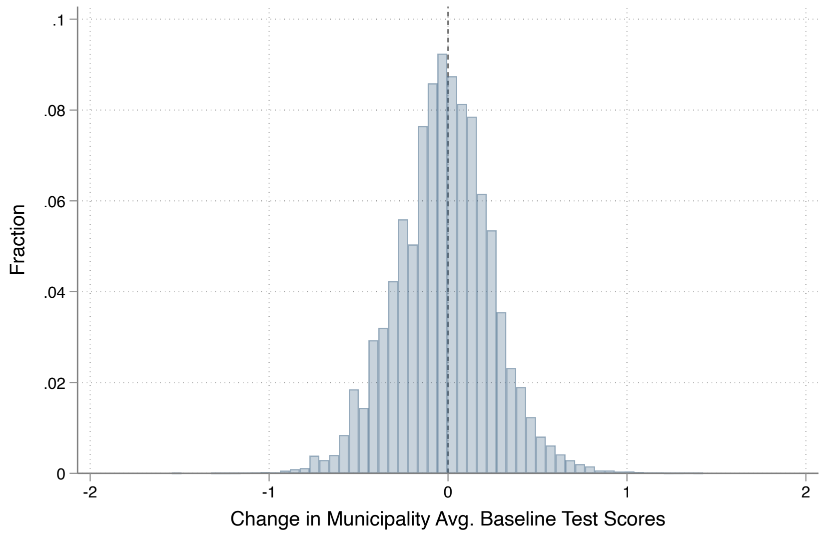
(b) $\Delta Safety_{im_om_d}$



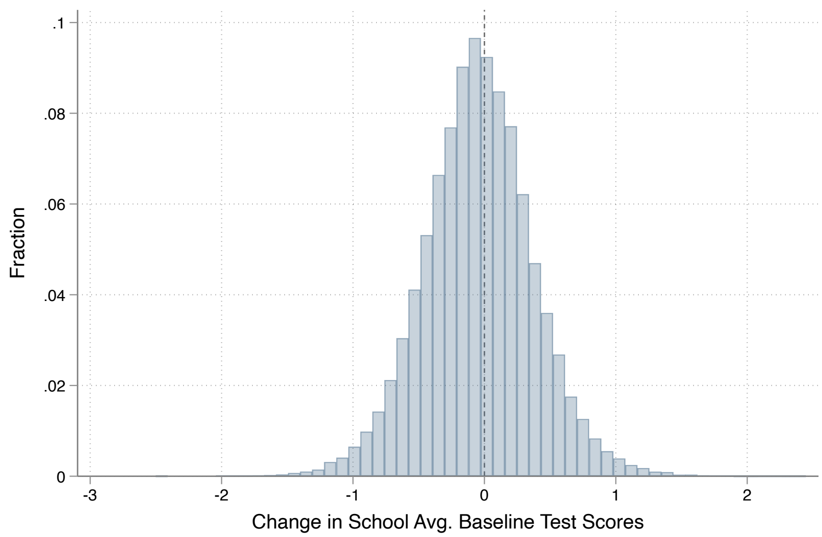
Notes: Panel (a) shows the distribution of students' pre-migration differences in the seven-year average homicide rates between their municipality of origin and destination. Positive values indicate improvements in safety in the municipality of the destination relative to the origin. Panel (b) shows the standardized pre-migration differences in the seven-year average homicide rates between their municipality of origin and destination. We exclude changes in the top 1% to improve clarity in the visualization.

Figure A.2: Distribution of the Change in School Average Test Scores ($\Delta Score_{is_os_d}$)

(a) Municipality ($\Delta Score_{im_om_d}$)



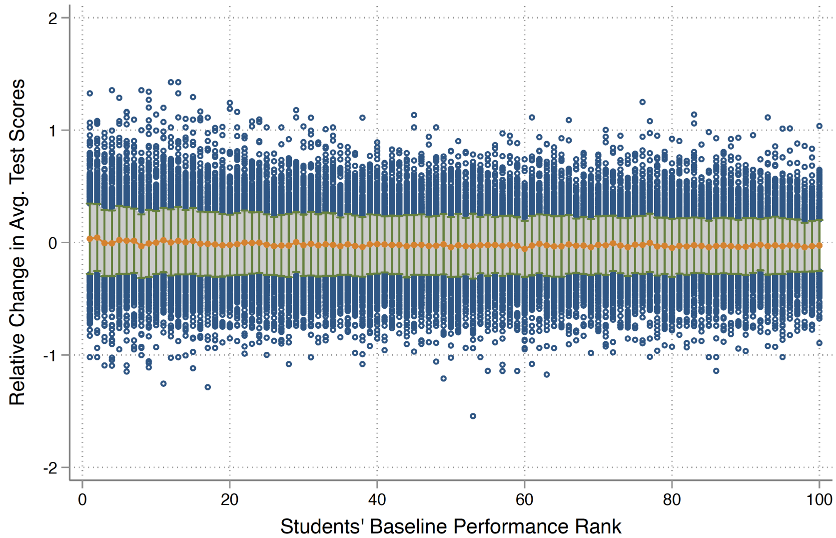
(b) School ($\Delta Score_{is_os_d}$)



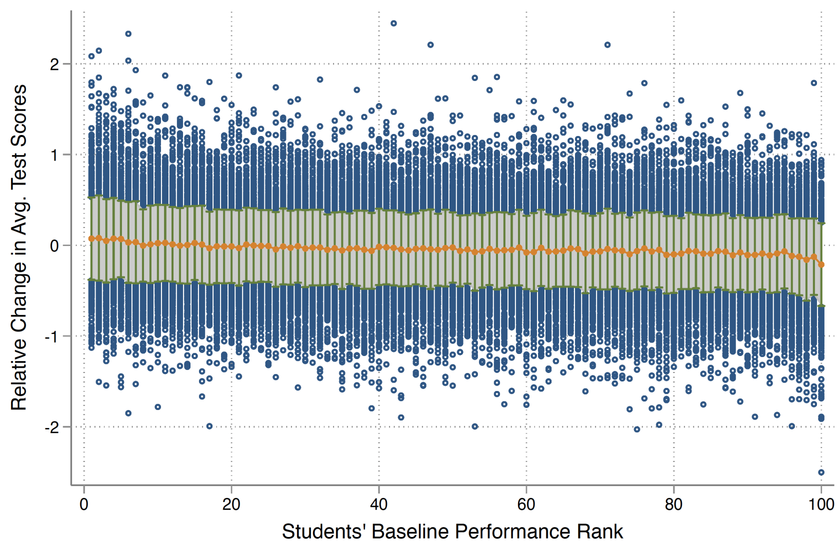
Notes: This figure shows the distribution of students' average change in baseline test scores between origin and destination municipalities (Panel a) and schools (Panel b). Positive values indicate higher average test scores in the destination relative to the origin.

Figure A.3: Baseline Performance Rank and Relative Change in Average Test Scores

(a) Municipality ($\Delta Score_{im_o m_d}$)



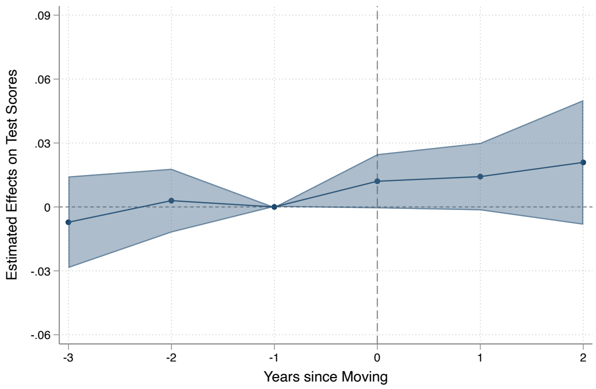
(b) School ($\Delta Score_{is_o s_d}$)



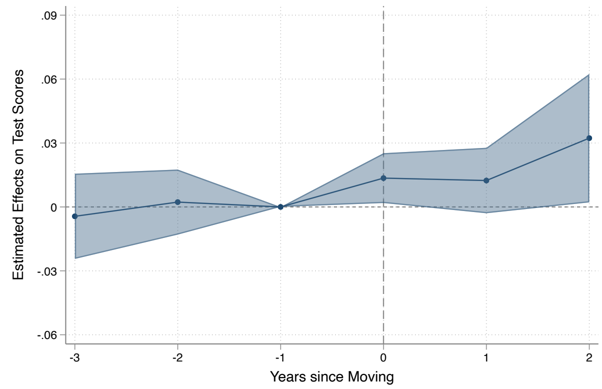
Notes: The figure shows a scatter plot together with mean and confidence intervals. The vertical axis shows the relative change in test scores between the municipality of origin and destination. The horizontal axis displays the students' baseline (prior to moving) performance rank in the percentile distribution of movers' test scores.

Figure A.4: Estimated Effects on Test Scores by Number of Years of Exposure

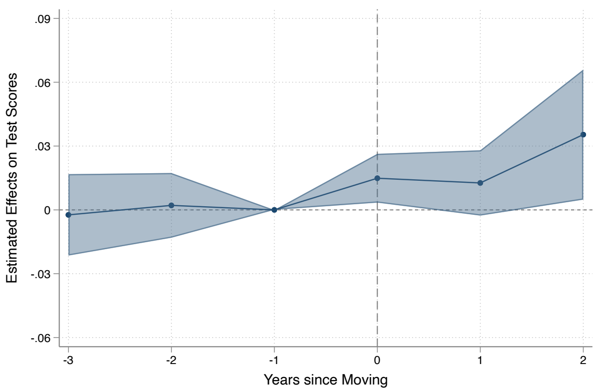
(a) 1 year



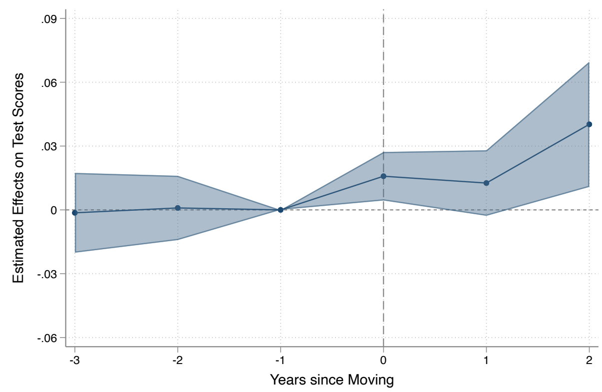
(b) 2 years



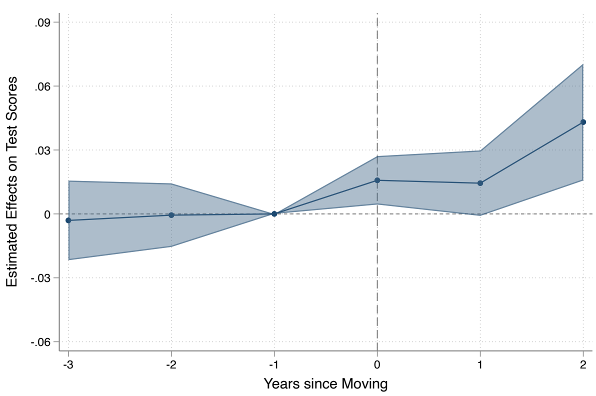
(c) 3 years



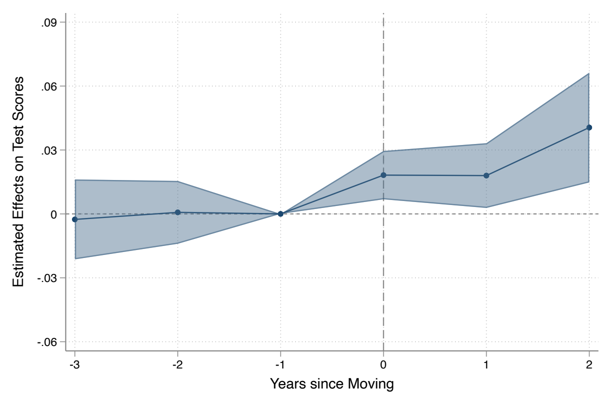
(d) 4 years



(e) 5 years



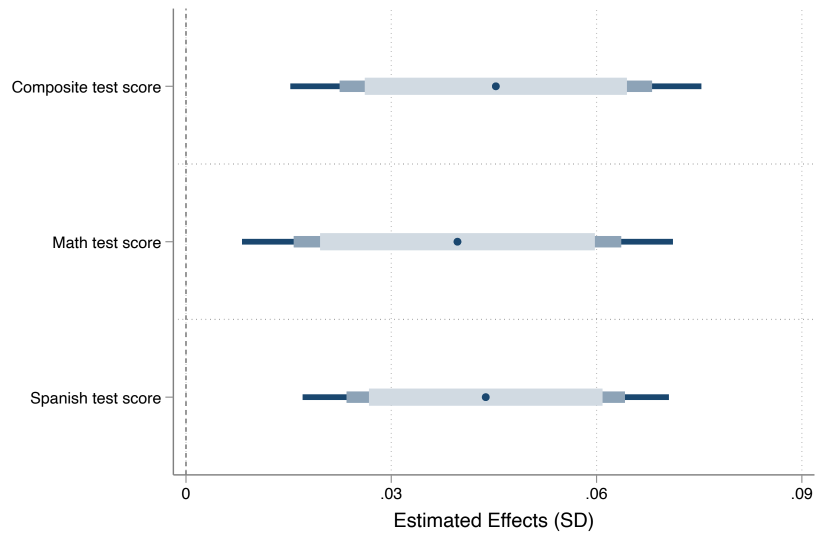
(f) 6 years



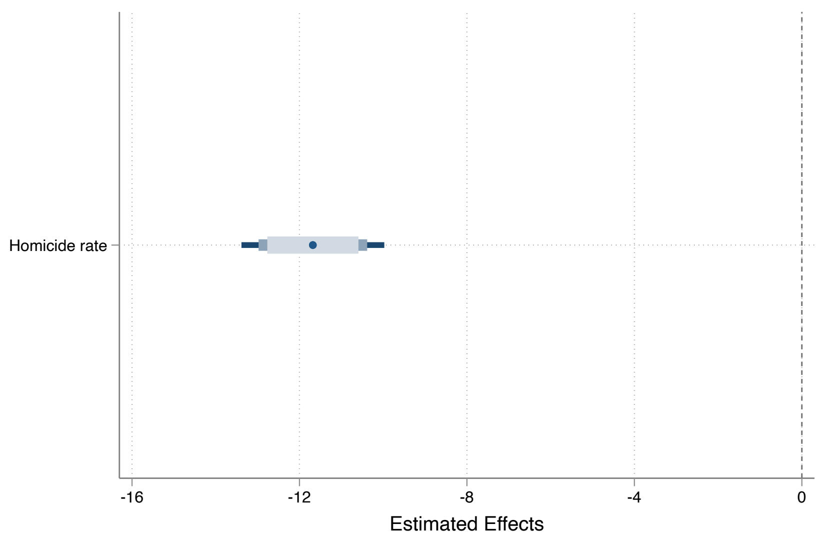
Notes: Estimates and confidence intervals in each panel come from a separate regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and controls for the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. Each panel varies the window over which pre-move homicide rates are averaged to calculate pre-migration exposure to violence, from 1 to 6 years (Panels a to f, respectively). Standard errors for confidence intervals are clustered at the individual level.

Figure A.5: Estimated Effects on Test Scores and the Homicide Rate using Alternative Specification

(a) Test Scores

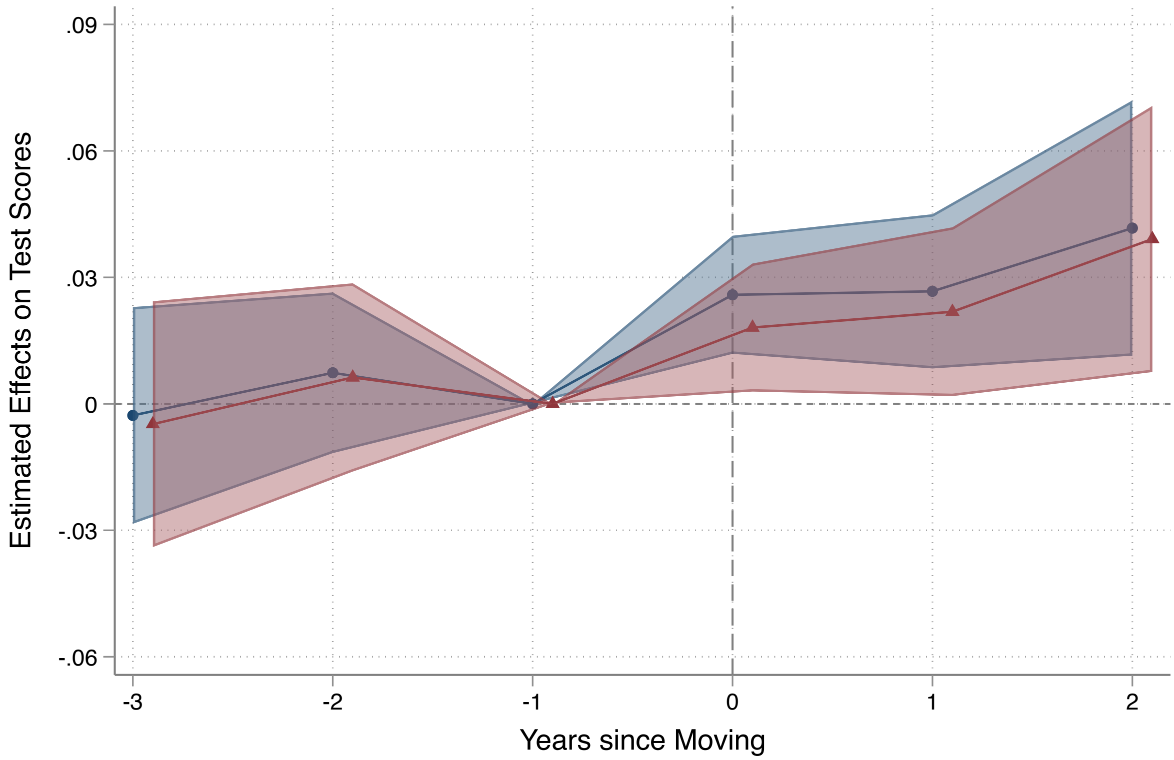


(b) Homicide Rate



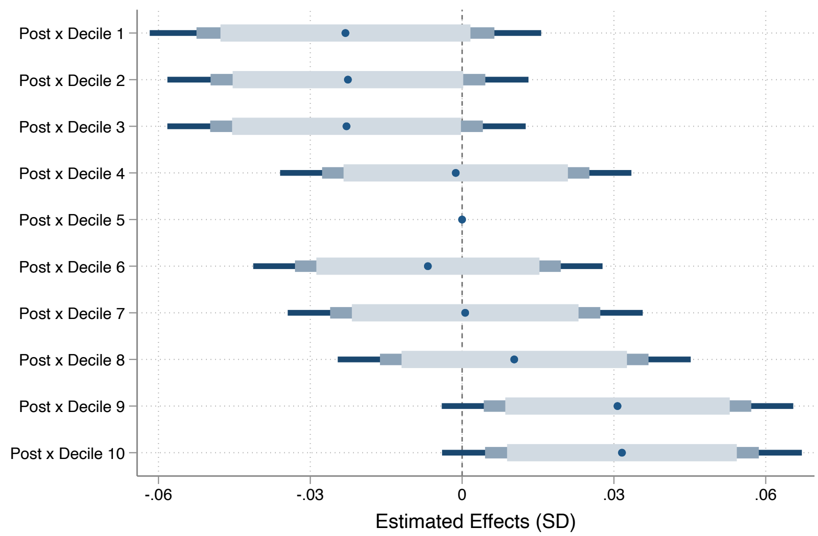
Notes: Each coefficient comes from a different regression that includes the municipality of destination-by-academic year fixed effects and school average change in baseline test scores after the move. We show 90% (thicker), 95%, and 99% (thinner) confidence intervals. Standard errors for confidence intervals are clustered at the destination municipality level.

Figure A.6: Estimated Effects on Test Scores: Standard DiD vs. Stacked DiD



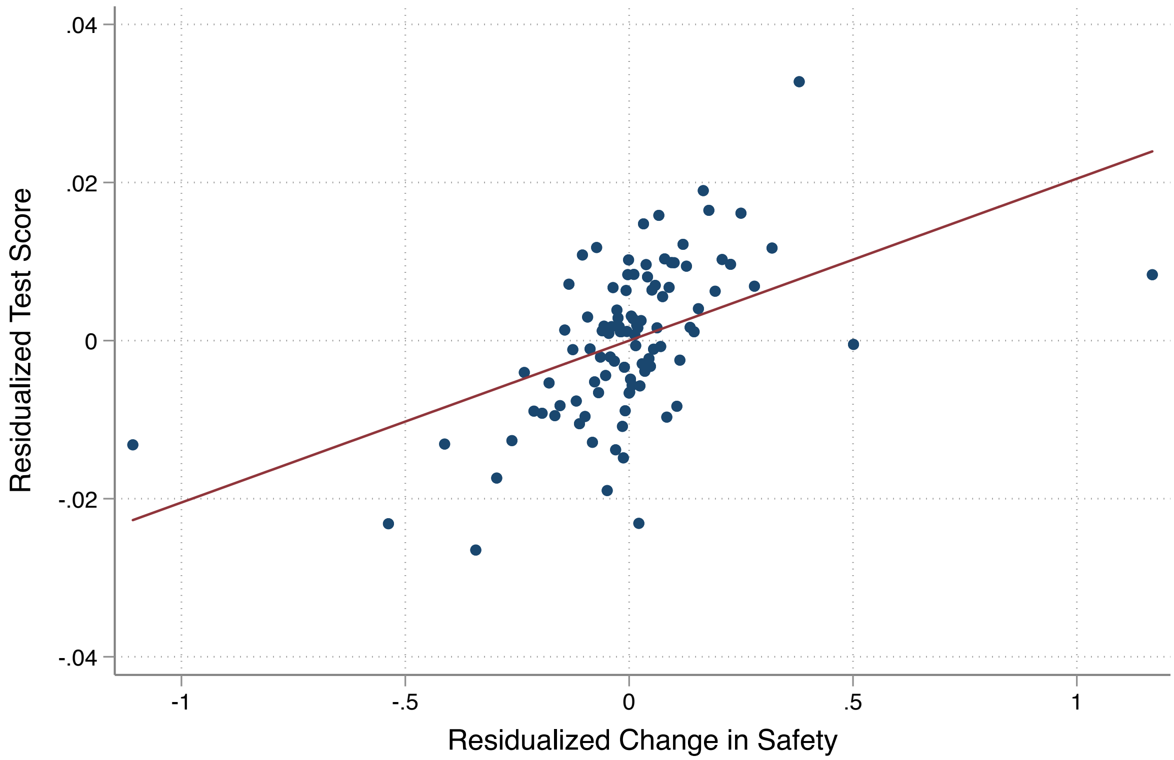
Notes: Circle markers show estimates from the standard DiD specification in Figure 4. Triangle markers show estimates from a stacked DiD specification that includes student fixed effects, grade-by-relative time-by-year moved fixed effects, school of destination-by-relative time-by-year moved fixed effects, and controls for the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. Standard errors for confidence intervals are clustered at the individual level. For comparison, we restrict the same to be the same for the standard and stacked estimations.

Figure A.7: Estimated Effects on Test Scores by Decile of Change in Safety



Notes: Estimates and confidence intervals come from the same regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and school average change in baseline test scores after the move. All estimates are relative to changes in safety in the 5th decile (i.e., $\Delta Safety_{im_om_d} \in [-0.039, 0.05]$). Standard errors for confidence intervals are clustered at the individual level.

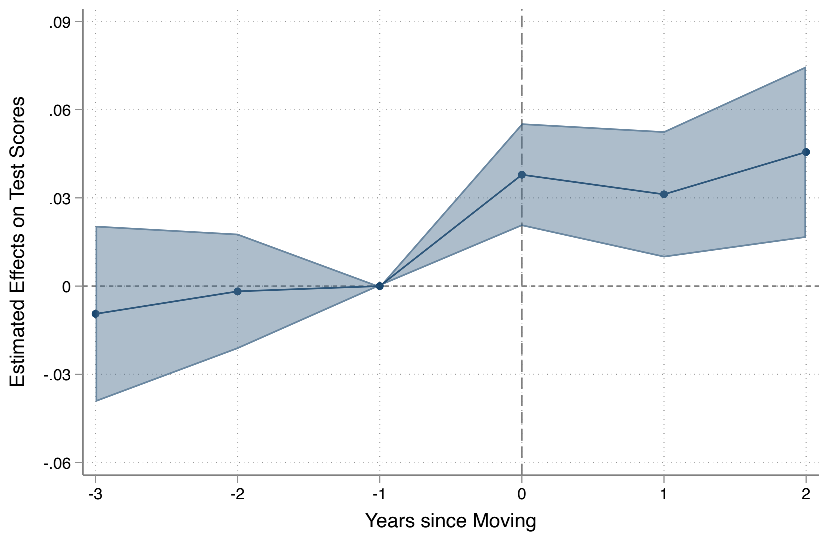
Figure A.8: Binned Scatter Plot of the Relationship Between Test Scores and Changes in Safety



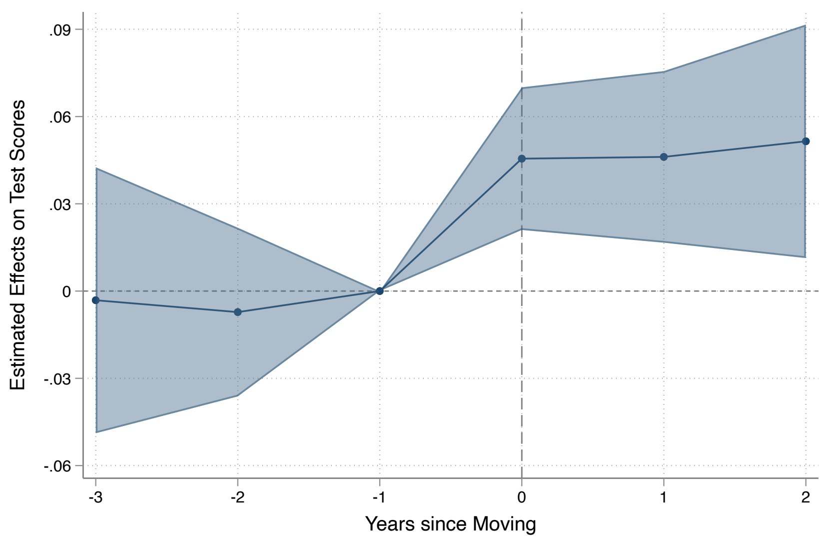
Notes: Residualized test scores and change in safety are the remaining variation after controlling for student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. The figure shows a binned scatter plot using centile bins of residualized change in safety. The red line shows the linear relationship between residualized change in safety and residualized test scores for our baseline sample.

Figure A.9: Estimated Effects on Test Scores Excluding Outliers

(a) Dropping Top & Bottom 1%

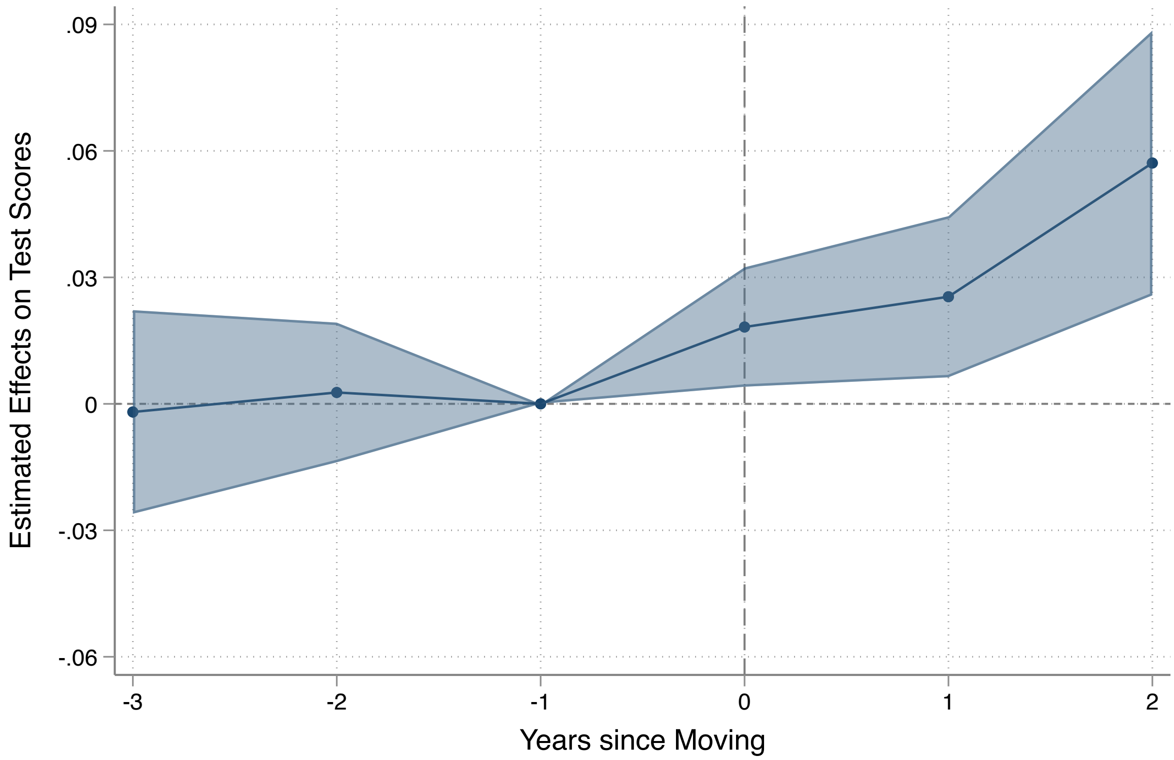


(b) Dropping Top & Bottom 5%



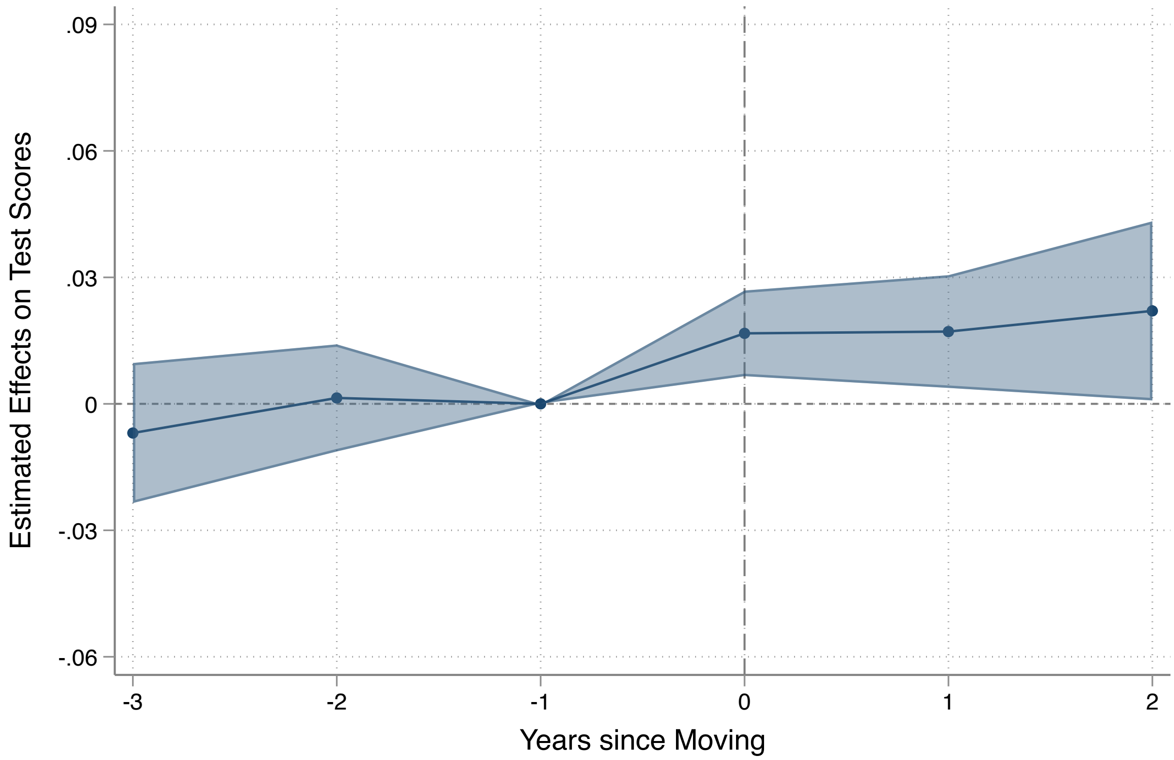
Notes: Estimates and confidence intervals in each panel come from a separate regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and controls for the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. Panels (a) and (b) exclude students in the top and bottom 1% and 5%, respectively, of the residualized change in safety. Standard errors for confidence intervals are clustered at the individual level.

Figure A.10: Estimated Effects on Test Scores Without Movers from Neighboring Municipalities



Notes: Estimates and confidence intervals come from the same regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and controls for the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. The sample includes students whose municipality of origin is not a neighboring municipality of the destination. Standard errors for confidence intervals are clustered at the individual level.

Figure A.11: Estimated Effects on Test Scores: Unrestricted Sample



Notes: Estimates and confidence intervals come from the same regression that includes student fixed effects, grade-by-relative time fixed effects, school of destination-by-relative time fixed effects, and controls for the differences in school quality between the origin and destination interacted with the indicators for each year relative to moving. Standard errors for confidence intervals are clustered at the individual level.

Table A.1: Estimated Effects on Test Scores by Number of Years of Exposure Pre-Move: Stacked DiD

Average Homicide rate over:	1 year (1)	2 years (2)	3 years (3)	4 years (4)	5 years (5)	6 years (6)	7 years (7)
Year of move $\times \Delta Safety_{im_o m_d t}$	0.008	0.011	0.012	0.014*	0.014*	0.015**	0.017**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
1 year after $\times \Delta Safety_{im_o m_d t}$	0.008	0.010	0.010	0.012	0.015	0.018*	0.020**
	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
2 years after $\times \Delta Safety_{im_o m_d t}$	0.010	0.022	0.032	0.036*	0.038**	0.039**	0.038**
	(0.018)	(0.019)	(0.019)	(0.019)	(0.018)	(0.017)	(0.016)
Year of move $\times \Delta Score_{is_d s_o}$	0.314***	0.315***	0.315***	0.315***	0.315***	0.315***	0.315***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
1 year after $\times \Delta Score_{is_d s_o}$	0.327***	0.327***	0.327***	0.327***	0.327***	0.327***	0.327***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
2 years after $\times \Delta Score_{is_d s_o}$	0.347***	0.347***	0.347***	0.347***	0.347***	0.347***	0.347***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
N	172965	172965	172965	172965	172965	172965	172965
Student FE	yes	yes	yes	yes	yes	yes	yes
Grade-by-relative time-by-year moved FE	yes	yes	yes	yes	yes	yes	yes
School of destination-by-relative time-by-year moved FE	yes	yes	yes	yes	yes	yes	yes

Notes: Each column reports estimates from a separate regression using a stacked DiD specification and varying the window over which pre-move homicide rates are averaged to calculate pre-migration exposure to violence, from 1 to 7 years (Columns 1 to 7, respectively). Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table A.2: Estimated Effects on Test Scores by Definition of Safe Municipality: Stacked DiD

Percent of students:	45	50	55	60
Maximum homicide rate in destination municipality:	≤ 16.81	≤ 19.38	≤ 20.76	≤ 21.86
	(1)	(2)	(3)	(4)
Year of move $\times \Delta Safety_{im_o m_d t}$	0.018**	0.017**	0.017**	0.009
	(0.008)	(0.007)	(0.007)	(0.007)
1 year after $\times \Delta Safety_{im_o m_d t}$	0.022**	0.020**	0.024***	0.015*
	(0.010)	(0.010)	(0.009)	(0.008)
2 years after $\times \Delta Safety_{im_o m_d t}$	0.042**	0.038**	0.037**	0.026*
	(0.017)	(0.016)	(0.015)	(0.014)
Year of move $\times \Delta Score_{is_d s_o}$	0.305***	0.315***	0.315***	0.317***
	(0.013)	(0.013)	(0.012)	(0.011)
1 year after $\times \Delta Score_{is_d s_o}$	0.312***	0.327***	0.323***	0.326***
	(0.017)	(0.016)	(0.015)	(0.014)
2 years after $\times \Delta Score_{is_d s_o}$	0.330***	0.347***	0.343***	0.356***
	(0.023)	(0.022)	(0.021)	(0.020)
N	154800	172965	189586	210064
Student FE	yes	yes	yes	yes
Grade-by-relative time-by-year moved FE	yes	yes	yes	yes
School of destination-by-relative time-by-year moved FE	yes	yes	yes	yes

Notes: Each column reports estimates from a separate regression using a stacked DiD specification, varying the definition of a safe destination. Columns 1 to 4 correspond to thresholds below which 45%, 50%, 55%, and 60% of movers relocated. Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table A.3: Estimated Effects on Test Scores by Change in Safety

	All	Move to more violent $\Delta Homicide_{im_om_d} < 0$	Move to safer $\Delta Homicide_{im_om_d} \geq 0$
	(1)	(2)	(3)
Post $\times \Delta Safety_{im_om_dt}$	0.020***	0.012	0.016**
	(0.005)	(0.041)	(0.006)
Post $\times \Delta Score_{is_ds_o}$	0.333***	0.358***	0.318***
	(0.009)	(0.019)	(0.012)
N	239292	69833	145848
Student FE	yes	yes	yes
Grade-by-relative time FE	yes	yes	yes
School of destination-by-relative time FE	yes	yes	yes

Notes: Each column reports average post-moving effects from a separate regression. Column 1 uses the full sample of movers, while Columns 2 and 3 include movers who move to more violent municipalities and safer municipalities, respectively. Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table A.4: Estimated Effects on Test Scores Dropping Outliers

	All	Dropping Top & Bottom 1%	Dropping Top & Bottom 5%
	(1)	(2)	(3)
Post $\times \Delta Safety_{im_o m_d t}$	0.020***	0.038***	0.048***
	(0.005)	(0.008)	(0.011)
Post $\times \Delta Score_{is_d s_o}$	0.333***	0.333***	0.331***
	(0.009)	(0.010)	(0.010)
N	239292	233738	211826
Student FE	yes	yes	yes
Grade-by-relative time FE	yes	yes	yes
School of destination-by-relative time FE	yes	yes	yes

Notes: Each column reports average post-moving effects from a separate regression. Column 1 uses the full sample of movers, while Columns 2 and 3 exclude students in the top and bottom 1% and 5%, respectively, of the residualized change in safety. Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table A.5: Estimated Effects on Local Economic Conditions

	Labor Force Participation (1)	Employment (2)	Hours Worked (3)	Earnings (4)
Post $\times \Delta Safety_{im_om_dt}$	-0.002***	-0.002***	-0.037***	-23.206***
	(0.000)	(0.000)	(0.014)	(3.308)
Post $\times \Delta Score_{is_ds_o}$	-0.000	-0.001	-0.055**	53.773***
	(0.000)	(0.000)	(0.025)	(5.176)
N	208368	208368	208368	208368
Student FE	yes	yes	yes	yes
Grade-by-relative time FE	yes	yes	yes	yes
School of destination-by-relative time FE	yes	yes	yes	yes

Notes: Each column reports estimates from a separate regression. The outcome variable in each regression is indicated at the top of each column. Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.6: Estimated Effects on Test Scores With and Without Controlling for Local Economic Conditions

	(1)	(2)
Post $\times \Delta Safety_{im_o m_d t}$	0.021***	0.021***
	(0.005)	(0.005)
Post $\times \Delta Score_{is_d s_o}$	0.325***	0.326***
	(0.010)	(0.010)
N	208368	208368
Student FE	yes	yes
Grade-by-relative time FE	yes	yes
School of destination-by-relative time FE	yes	yes
Controls for Local Economic Conditions	no	yes

Notes: Each column reports estimates from a separate regression. Local economic conditions include municipality-level labor force participation, employment, weekly hours worked, and monthly earnings. Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.7: Estimated Effects on Test Scores by Sample

	Unrestricted		Restricted	
	DiD	Stacked DiD	DiD	Stacked DiD
	(1)	(2)	(3)	(4)
Post $\times \Delta Safety_{im_om_dt}$	0.017***	0.017***	0.020***	0.019***
	(0.004)	(0.006)	(0.005)	(0.007)
Post $\times \Delta Score_{is_ds_o}$	0.352***	0.343***	0.333***	0.321***
	(0.008)	(0.010)	(0.009)	(0.012)
N	315304	239002	239292	172965
Student FE	yes	yes	yes	yes
Grade-by-relative time FE	yes	yes	no	no
School of destination-by-relative time FE	yes	yes	no	no
Grade-by-relative time-by-year moved FE	no	no	yes	yes
School of destination-by-relative time-by-year moved FE	no	no	yes	yes

Notes: Each column reports estimates average post-treatment effects from a separate regression. Columns 1 and 2 use an unrestricted sample that includes all reliable test scores for students observed in four consecutive years, even if some scores are flagged and excluded. Columns 3 and 4 use the restricted sample from our main analysis, which excludes students with any flagged scores. Estimates for the unrestricted sample are weighted by the inverse of the number of times a student's score is observed. Estimates in columns 1 and 3 come from a standard DiD specification, and estimates in columns 2 and 4 come from a stacked DiD specification. Standard errors in parentheses are clustered at the student level. *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively.