

Hometown Conflict and Refugees' Integration Efforts*

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Abstract

How does violence in origin areas affect the educational outcomes of refugees in their destinations? Using administrative panel data, we find that heightened violence in the hometowns of Syrian students leads to improvements in their school outcomes in Türkiye. Turkish language and Math scores of refugees improve, with larger impacts on Turkish language scores. Naturalized Syrian students, however, exhibit negligible effects. Syrians' increased effort after home violence partially offsets classroom congestion effects and generates positive spillovers on Turkish classmates. Parents play an important role: higher hometown conflict is associated with greater maternal internet use, less male-biased educational priorities, and more maternal time investment in children. These patterns are consistent with ongoing violence in refugee-origin areas reducing the perceived likelihood of returning home and motivating greater investment in host-country-specific human capital.

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1 Introduction

Amidst an unprecedented global displacement crisis, where 1.4 percent of the world’s population was forcibly displaced by 2022, more than tripling the 0.4 percent recorded in 1990 (UNHCR, 2022), the humanitarian challenges are profound and extensive. Notably, developing countries shoulder most of the burden, hosting approximately 75 percent of the world’s refugees, with nearly half being children. Persistent crises in countries like Syria, Afghanistan, Ukraine, Venezuela, Libya, Palestine, and Yemen indicate that achieving political stability remains elusive in the foreseeable future.

The conflict in Syria began in 2011 and rapidly escalated into a full-scale civil war, triggering a mass exodus and leading to one of today’s most significant refugee and displacement crises (World Bank, 2023). Millions sought refuge in neighboring Türkiye, Lebanon, and Jordan. Türkiye hosted the largest population of Syrian refugees, reaching approximately 3.8 million by June 2022.¹ By about 2023, the likelihood of return to Syria had diminished significantly due to ongoing political instability, persistent conflict, proxy wars, and the emergence of extremist groups.

Syrian children have borne a disproportionate share of the costs of violent conflict, forced displacement, and persistent instability. At the beginning of the 2022–23 school year, approximately 1.4 million school-age (5–17) refugee children were present in Türkiye, with lower test scores, lower enrollment, and higher absenteeism than their native peers (Tumen et al., 2026). These gaps are often linked to language barriers, which remain a central obstacle to the educational integration of refugee children (UNHCR, 2019). At the same time, Syrian households in Türkiye remain closely connected to developments in Syria, and existing evidence suggests that smartphones and digital/social media are important channels through which conflict-related information circulates within refugee communities (UNHCR, 2016; Narlı, 2018; Jauhainen et al., 2022).² By 2022, an estimated 306,887 civilians, or about 84 per day, had been killed since the onset of the conflict, underscoring its devastating human toll (SNHR, 2023). As we discuss later in the paper, violence and casualties in Syria vary substantially across time and space.

Against this backdrop, we focus on a previously under-explored dimension: how contemporaneous violence in the district of origin affects refugee integration.³ In the Syrian context, persistent conflict casts a long shadow over the feasibility of return, making it important to understand how prolonged instability shapes refugee children’s human capital investments in

¹See <https://data.unhcr.org/en/situations/syria/location/113> for data and the latest trends.

²Our administrative data do not contain a direct measure of household media consumption or news exposure, so we do not separately identify media exposure as a mechanism. Instead, we interpret violence in the place of origin as conflict-related information that is plausibly observed and processed by refugee households, consistent with external evidence on connectivity and information-seeking among Syrians in Türkiye.

³In the context of this paper, “hometown” refers to the district of birth of Syrian students. The district (mintaqah) is the level of observation available in our data and serves as the primary geographical reference for analysis. The 14 governorates of Syria, or muhafazat, are divided into 65 districts, or manatiq, including the city of Damascus. Each district comprises multiple subdistricts (nahiya). Given data availability at the district level, we use “district” to define students’ hometowns throughout the paper.

the host country.

We estimate the causal effect of ongoing violence in Syria on refugee children’s educational outcomes in Türkiye. The heightened levels of concurrent violence may influence educational attainment through at least two opposing mechanisms. On the one hand, the trauma experienced may worsen schooling outcomes. On the other, the continued violence may alter expectations, prompting refugee households to reassess their beliefs about the likelihood of returning to Syria. This despair, in turn, may lead to more investments in their educational development, as an effort to integrate more effectively into their host communities.

To examine the various competing hypotheses, we obtain individual-level administrative panel data that comprehensively document the academic performance of Syrian and Turkish students, and merge them with detailed information on the timing, location, and intensity of violence in students’ hometowns in Syria. Because we observe Syrian students’ districts of birth, our identification strategy exploits variation in conflict intensity across origin districts and academic semesters.

Controlling for individual fixed effects, years-since-arrival fixed effects, and high dimensional classroom–grade–academic-year fixed effects, we find that heightened violence in refugee students’ Syrian hometowns is associated primarily with higher Turkish language scores, alongside smaller gains in Math. The stronger response in Turkish is consistent with increased investment in host-country-specific human capital and integration. By contrast, naturalized Syrian students show a much smaller, or no, response to hometown violence, and absenteeism remains unaffected.

Several additional findings help interpret these patterns. Event-study estimates show no evidence of differential pre-trends and indicate that the effects emerge gradually after salient spikes in violence. The response is stronger for later arrivals and for students who have spent less time in Türkiye, indicating that conflict exposure matters more when return plans are less settled. Parental behavior potentially plays an important role in explaining the results. The effects are larger for younger children and for students from lower-education households, while survey-based evidence shows that higher conflict intensity is associated with greater maternal internet use, less male-biased stated educational priorities, and greater maternal time investment in children. Overall, these results are potentially consistent with a mechanism in which worsening origin conditions reduce the perceived likelihood of return and lead households to invest more in their children’s education, so that they can better integrate into the host country.

We also find no evidence that ongoing violence leads to differential attrition among Syrian students. The Math effects are concentrated among female students. In addition, cumulative exposure, whether since migration or since the start of the conflict, has a larger association with Turkish language and Math scores than contemporaneous violence alone, suggesting that households respond not only to short-run spikes in violence but also to its sustained persistence. Finally, Syrian students’ stronger academic effort in response to violence in their

home districts generates positive spillover effects for Turkish students. While a larger share of Syrian classmates is negatively associated with Turkish students’ academic performance, plausibly because of congestion effects, this negative association is partly offset when Syrian classmates are exposed to greater hometown violence and increase their own academic effort.

The estimates remain robust across a broad range of specifications and sensitivity checks. The results are similar when we use alternative measures of conflict intensity, including violent events and fatalities, and when we apply alternative transformations of the treatment and outcome variables, including the inverse hyperbolic sine transformation, specifications that drop zero-violence observations, and raw test scores instead of log scores. We also implement a “clean control” estimator to address potential biases in two-way fixed-effects settings with continuous treatments (de Chaisemartin et al., 2022). The findings are further confirmed in fixed early-arrival cohorts, leave-one-province-out and leave-one-semester-out exercises, and placebo tests using future violence, which yield null effects. Additional event-study analyses using alternative thresholds and staggered-treatment estimators by Sun and Abraham (2021); de Chaisemartin et al. (2022) produce the same qualitative pattern.

Related literature and contributions. These findings contribute to three distinct strands of the migration and conflict literature. To our knowledge, we are the first to document how ongoing violence in refugees’ hometowns affects the academic performance of refugee students in the host country. Whereas previous research has mainly examined the effects of past cumulative *direct* exposure to violence, highlighting its adverse impacts on a wide range of life outcomes (Verwimp and Van Bavel, 2014; Akbulut-Yuksel, 2014; Bertoni et al., 2019), our study shifts attention to contemporaneous *indirect* exposure to violence while refugees are abroad, which can reshape expectations and alter human capital investments.⁴ In this sense, our paper also complements the literature showing that local exposure to crime and police activity around schools worsens educational outcomes (Brown and Velasquez, 2017; Ang, 2021; Bharadwaj et al., 2021; Koppensteiner and Menezes, 2021; Chang and Padilla-Romo, 2023; Cabral et al., 2026).⁵ Our setting is different: we study the effects of large-scale violence occurring simultaneously in refugee students’ hometowns, but from a *distant* country. Although this violence does not directly threaten students’ physical safety, it plausibly lowers the perceived likelihood of return. Our findings suggest that ongoing violence in the hometown can increase human capital accumulation in the host country.⁶

Our paper also contributes to the refugee integration literature by showing that worsening conditions in the home country can strengthen refugees’ integration efforts in the host country. More broadly, the results support theoretical ideas suggesting that human capital investment abroad rises with the expected duration of stay. Existing evidence shows that refugees are less likely to return than economic or voluntary immigrants (Cortes, 2004). More generally, the

⁴See also Angrist and Kugler (2008), Chamarbagwala and Moran (2011), Shemyakina (2011), Leon (2012), Rodriguez and Sanchez (2012), Justino et al. (2014), and Bruck et al. (2019).

⁵See also Monteiro and Rocha (2017), Casey et al. (2018), and Michaelsen and Salardi (2020).

⁶In related work, Alfano and Görlach (2024) causally identify how terrorist attacks reduce school participation through fears and safety concerns. Alfano and Görlach (2023) show that media coverage amplifies the negative effects of terrorism on schooling by increasing perceived risks.

longer immigrants expect to remain in the host country, the more they invest in human capital (Dustmann, 1996; Khan, 1997; Adda et al., 2022; Aksoy et al., 2023) and local language acquisition (Abramitzky et al., 2023), while saving less and sending fewer remittances to their home country (Galor and Stark, 1990; Merkle and Zimmermann, 1992). Our paper adds to this literature by showing that these dynamics are also visible in refugee children’s educational outcomes, even at relatively early stages of integration.

Finally, our study speaks to a growing literature showing that shocks in migrants’ home countries shape integration outcomes at destination. Recent work finds that deteriorating security or political conditions at origin can increase immigrants’ labor-market attachment, marriage, political engagement, or well-being in host countries, while reducing return to the origin country (Akay et al., 2017; Barsbai et al., 2017; Zaiour, 2024; Bassetto and Monteiro, 2026; Adema et al., 2026). We complement and extend this literature in several ways. We focus on refugee children and compulsory-school outcomes, examining human capital investment rather than adult labor-market or political behaviour. In our context, labor-market and social integration outcomes may reflect not only refugees’ own responses but also the reactions of the host population, including discrimination, acceptance, and bargaining power. Academic test scores, therefore, provide a cleaner setting to study our mechanism, as they more directly capture effort and investment within a structured institutional environment. We also leverage rich administrative microdata that allow us to compare students within the same classroom and academic year, using within-student variation to isolate changes in exposure to hometown violence over time. Overall, our results, consistent with a return-expectations channel, provide causal micro-level evidence that prolonged conflict can reshape refugee families’ human capital investments, highlighting an important and previously unexplored channel of integration.

The plan of the paper is as follows. In Section 2, we provide an overview of the background. Section 3 delves into the specifics of our data, while Section 4 outlines our empirical strategy. Our findings are discussed in Section 5, and Section 6 concludes.

2 Background and Institutional Setting

2.1 Armed conflict and violence in Syria

The Syrian conflict began in early 2011 as a peaceful protest triggered by discontent with the government. However, it swiftly escalated, encompassing a vast geographical area and evolving into a full-scale civil war. This conflict gave rise to one of the largest refugee waves in human history. The origins of the crisis are rooted in a complex array of political, historical, religious, and economic factors.⁷

Here, we discuss two descriptive patterns in the violence data. First, the conflict displayed significant temporal variation, as illustrated in Figure A1. It began escalating in early 2012, peaking by the end of 2014. Although it gradually subsided thereafter, intermittent spikes

⁷For further reading, see Phillips (2015), Van Dam (2017), Abel et al. (2019), Daoudy (2020), and Tumen (2023).

persisted. A key factor contributing to this temporal variation was the direct involvement of foreign countries, such as Russia, the US, and Iran. Additionally, the timing of Syrian refugee flows into Türkiye aligns with the pattern of violent events in Syria.

Second, the conflict and its temporal variation exhibit significant regional disparities, based on the presence of various rebel groups and Kurdish forces. Figure A2 illustrates cumulative violence across regions, showcasing considerable regional variation. Meanwhile, Figures A3 and A4 reveal significant variation in the patterns of violence across regions, with conflict events igniting and dissipating at different times in different areas. The ethnic and religious diversity of Syria’s demographic structure plays a pivotal role in creating these regional differences.⁸ Figure A5 displays the distribution of Syrian students in our data set based on their district of birth.

We measure hometown violence using two alternative indicators: (i) the number of violent events and (ii) the number of conflict-related fatalities. Our results are robust to using either measure. For clarity and simplicity, we lead the analysis with violent events as our primary violence measure, after first presenting the baseline estimates using both indicators in the main table. We do so because, in the conflicts dataset, violent events are documented from various news sources, so their number is more accurate, while the number of deaths is estimated, making the second measure noisier.

2.2 School integration of refugee children

The integration of refugee children into schools has proven challenging, primarily due to language barriers. Syrians speak Arabic, and there are significant differences between Arabic and Turkish, including their alphabets. This makes it particularly difficult for Syrian children to learn Turkish. Typically, Syrian children are exposed early to the Arabic alphabet at home or through preschool attendance in local Quran courses (Boucher et al., 2021).

The number of Syrian refugees in Türkiye was relatively small during the initial stages of the conflict. The Turkish government constructed refugee accommodation camps near the Syrian border, primarily to provide emergency humanitarian assistance, with the initial expectation that refugees would eventually return to Syria.

By mid-2014, the Syrian conflict had intensified significantly, leading to a more than threefold increase in refugees by early 2016. Initially, educational services for Syrian refugee children were limited to small-scale, project-specific initiatives by non-governmental organizations within camps, supervised by the Ministry of National Education of the Republic of Türkiye (MoNE). However, recognizing the urgent need for educational integration as the crisis deepened, the Turkish government overhauled its policies to fully integrate Syrian children into the public education system, implementing nationwide programs funded by the EU Facility for Refugees in Türkiye (FRIT). A crucial aspect of this policy change was ensuring free access to education for all Syrian refugee children, granting them the same rights as

⁸For information about the pre-war heterogeneity in population, see Khalifa (2013).

Turkish citizens to attend public schools without paying tuition fees.

3 Data

Administrative data on educational outcomes. The primary data source for refugee and native students' academic performance is the administrative data obtained from MoNE, for the academic years between 2011-12 and 2018-19.⁹ In our main analysis, we focus on refugee students; but for analyzing spillovers, we also use data on Turkish students.

Our outcomes include the end-of-semester scores from all courses taken by all registered students, and their absenteeism records. The end-of-semester score is derived from a weighted average of exam results, quizzes, homework assignments, and other graded activities for each course. Absenteeism is quantified in terms of days missed per academic year for each student. Additionally, the dataset contains school names, classroom and grade-year information, and key individual-level characteristics, including gender, date of birth, and place of birth (governorate and district). While data on parental characteristics is available, it has several missing entries.

Our data set includes all refugee and native students registered in Turkish schools across four provinces: Ankara, Bursa, Gaziantep, and Şanlıurfa. Ankara, Türkiye's capital, is the second-largest city, with a population exceeding 5.7 million. Bursa, situated in the northwestern region of Türkiye, ranks as the fourth-largest city, with a population of over 3.1 million. It is an appealing destination for refugees due to the presence of large manufacturing companies, which provide employment opportunities. Gaziantep and Şanlıurfa, neighboring provinces to Syria, are known for hosting significant refugee populations, with some schools in these areas experiencing substantial refugee enrollments. Initially, during the early stages of the Syrian crisis, these provinces housed large refugee camps. Following the closure of these camps, a significant number of refugees remained in Gaziantep and Şanlıurfa due to geographical and cultural proximity to Syria.¹⁰

We focus on grade levels 4 through 10. In grade levels 1 to 3, grading is rather informal and employs a three-category scale (good, intermediate, should be improved) primarily intended for guidance rather than evaluation. Conversely, for grade levels 4 to 10, a formal grading scale ranging from 0 to 100 is used.

Our empirical analysis focuses on mandatory courses (Turkish language and Math) taken by all students in grades 4 to 10, along with absenteeism, as key variables. Turkish language scores measure communication skills in the local language, also making them a valuable indicator of social integration. Additionally, language proficiency serves as a proxy for non-

⁹The panel is unbalanced by design, as students enter at different times and are followed across semesters; all available student-semester observations are used in the main regressions with individual fixed effects.

¹⁰Overall, these provinces are substantively central to the refugee context. Two (Gaziantep and Şanlıurfa) are border provinces that have consistently been among the largest host locations. The other two (Ankara and Bursa) are major interior labour markets and prominent destinations for secondary settlement. Together, they capture both border-intake and inland-absorption settings and include a sizeable share of refugee students, which supports the external relevance of the findings.

cognitive skills and enhances learning efficiency across other subjects. Math scores, on the other hand, assess students' cognitive and analytical capabilities. In combination, Math and Turkish language scores provide a comprehensive measure of the core components of refugee students' academic capacity, school performance, and human capital. Absenteeism serves as an indicator of school attachment or school integration for refugee students.

Conflict data. The conflict data is from the Uppsala Conflict Data Program (UCDP) Georeferenced Event Dataset, which collects information on state-based armed conflicts, non-state conflicts, and one-sided violence. This data set specifically includes conflicts that have exceeded the threshold of 25 battle-related deaths in a single calendar year.¹¹ We concentrate on violent incidents and related fatalities that occurred during the Syrian civil war between 2011 and 2019, specifically at the district level, covering 44 Syrian refugee hometown districts.

Constructing the study sample. We construct the final analysis sample in three steps. First, using the UCDP Georeferenced Event Dataset, we measure conflict exposure in each Syrian district by semester s or academic year y , using counts of conflict-related events and fatalities. In Türkiye, the fall semester runs from September to January and the spring semester from February to May. We therefore exclude conflict exposure occurring during the summer holiday period (June to August). Second, we define the student sample based on outcome availability. Some outcomes, such as Math and Turkish language scores, are recorded at the semester level, whereas others, such as absenteeism, are observed only at the academic-year level and cannot be disaggregated by semester. Finally, we merge the conflict data with the administrative education data using each Syrian student's district of birth and semester (or academic year).

Demographic and Health Surveys. To study the mechanisms underlying our main results, we use data from the 2018 Türkiye Demographic and Health Survey (DHS). This round is particularly valuable in our context because, in addition to the native sample, it includes a nationally representative sample of Syrian refugees residing in Türkiye. The survey contains a household roster with basic demographic information on all household members and a women's questionnaire administered to women aged 15–49, which collects richer information on migration history, marriage, fertility, child-related outcomes, and women's status.

We use these data to construct measures of mothers' information-seeking, educational attitudes, and parental investments in children. Specifically, we create indicators for any internet use, regular internet use, and whether the mother agrees that sons' education is more important than daughters' education. We also use the survey's time-use modules to capture parental investments in children, constructing indicators for whether nobody in the household spends time on child care, being with children at home, or being with children outside the home, as well as corresponding indicators for whether the mother spends time on each of these activities. Together, these outcomes allow us to examine whether worsening conflict conditions in Syria are associated with greater information-seeking, changing educational priorities, and increased parental time investment among Syrian refugee households in

¹¹If a conflict exceeded the 25-death threshold in a single year, but produced events in either preceding or subsequent years, all events associated with that conflict are considered, even if the threshold was not surpassed in those years.

Türkiye.

Descriptive statistics. Figure A6 shows the distribution of Math and Turkish scores among Syrian students. The red vertical dashed line illustrates the average scores achieved by native students, while the vertical blue solid line represents the corresponding scores for Syrian students. On average, Syrian students achieve a Math score of 57.8, whereas Turkish students score 64.9. Notably, the difference in Turkish language scores is more substantial, exceeding 10 points, with Syrian students averaging 58.3 and Turkish students 69.9.¹²

Figure A7 shows the distribution of the number of school days missed per academic year among Syrian students for the full sample (Panel A) and for observations below the 95th percentile (Panel B).¹³ The red dashed vertical line indicates the average number of school days missed per academic year by Turkish students. Both panels show that Syrian students are absent more frequently than their Turkish classmates.

We measure concurrent ongoing violence in the Syrian students’ district of birth during each semester by the number of events, and separately the number of conflict fatalities. On average, Syrian students in Türkiye are exposed to 81 attacks, with 346 corresponding deaths in their hometowns during a semester.

4 Empirical Strategy

To investigate the impact of ongoing violence in the Syrian students’ district of birth on their educational outcomes in Türkiye, we estimate the following specification with individual fixed effects:

$$\ln Y_{i,s} = \beta \ln V_{d(i),s} + \gamma_i + \lambda_t + \rho_{c,g,y} + \epsilon_{i,s}, \quad (1)$$

where i , s index individuals and semesters; $d(i)$ denotes students’ districts of birth in Syria. γ_i index individual fixed effects (FE), λ_t capture years since arrival in Türkiye FE, and $\rho_{c,g,y}$ denote classroom-grade-academic year FE. Y is the end-of-semester Math or Turkish language score in logarithm, V is a violence measure, and ϵ an error term.

The treatment variable, V , is defined as the number of violent events in the district of birth d , during semester s . For robustness, we also use the number of conflict fatalities in the district-semester.¹⁴ Note that violent events that happened during the summer holidays (from June to August) are excluded. Figure 1 shows a timeline for variable construction.

Our administrative data uniquely positions us in including a rich set of fixed effects that may account for confounding factors. Individual fixed effects (γ_i) capture the effect of time-invariant student characteristics on outcomes (such as innate ability or motivation, past

¹²Differences in Math and Turkish language scores between refugee and native students are statistically significant by t -test at the 1% level.

¹³Panel A in Figure A7 shows the full distribution of annual absences among Syrian students. The right tail indicates that some students missed more than 150 days of school, suggesting that they were enrolled but attended very little or not at all. Panel B excludes observations at and above the 95th percentile.

¹⁴We apply a logarithmic transformation to the values, adding 1 to accommodate zero values. We report a set of alternative parameterizations in Table A4.

conflict exposure, and parental background). Overall, it controls for the possibility that differences between students drive the observed outcomes. The years since arrival fixed effects (λ_t) account for the influence of time-related factors (such as acculturation, language proficiency, education system familiarity, social integration) associated with a student’s duration of residence in the host country on their academic outcomes.

Finally, we include a fixed effect for each unique combination of classroom \times grade \times academic year, $\rho_{c,g,y}$. This high-dimensional fixed effect absorbs any factors common to students sharing the same host learning environment in that academic year, including time-varying classroom conditions (e.g., class size and classroom composition), scheduling and grade-by-year features (e.g., curriculum or testing changes), and teacher-specific practices (e.g., pedagogy, teacher lenience, and grading standards).¹⁵ In particular, it also absorbs any grading changes that operate at the classroom–grade–year level (e.g., a teacher becoming more lenient or supportive in that classroom-year). For grading to bias our estimates even after these controls, it would have to vary *within* a classroom–grade–year cell *across Syrian students over time* in a way that systematically tracks each student’s Syrian district of birth and semester-by-semester changes in violence in that district, a demanding pattern that we view as unlikely given the granularity of the treatment variation.¹⁶

Because $\rho_{c,g,y}$ already nests classroom and academic-year components, including separate classroom or academic-year fixed effects would be redundant and lead to collinearity. To illustrate that there is meaningful identifying variation, Figure A8 shows that students from the same Syrian hometown are dispersed across many Turkish schools, rather than clustered in a small number of schools or classrooms. This dispersion helps mitigate concerns that our results are driven by endogenous sorting or by school-level confounders specific to students from particular origin districts.

Standard errors are clustered at the Syrian district of birth. We also report robustness to alternative inference procedures (including bootstrapped standard errors and alternative clustering levels) in appendix exhibits.

Our identification assumption is that semester-to-semester changes in violence in a student’s Syrian district of birth are independent of other determinants of that student’s contemporaneous school performance in Türkiye, conditional on individual fixed effects, years-since-arrival fixed effects, and $\rho_{c,g,y}$. Intuitively, the estimates compare refugee students who share the same classroom–grade–year environment, while differing in the violence exposure associated with their (fixed) district of birth over time.

¹⁵This fixed-effect structure absorbs any time-varying features of the student’s *host* classroom in that academic year, including changes in class size, the Syrian/native composition of classmates, shifts in grading standards or pedagogy, schedule changes, and classroom- or school-level shocks, thereby addressing potential confounding channels (congestion, composition, local diaspora effects) without introducing post-treatment (“bad control”) bias.

¹⁶Because we also include individual fixed effects, any time-invariant student-specific differences (including persistent teacher-specific favoritism toward a given student) are absorbed as well.

5 Results

5.1 Main estimates

In Table 1, we focus on two key performance indicators that capture the academic achievements of Syrian children: (log) Turkish language scores and (log) Math scores. These scores represent overall outcomes at the end of the semester and encompass a composite evaluation of exams, assignments, class participation, and other graded components.

In columns 1, 3, 5, and 7, our estimates incorporate fixed effects for individuals, time since arrival, and grade level. For columns 2, 4, 6, and 8, we use a more detailed fixed effects specification that combines classroom-grade-academic year instead of grade-level effects. This latter specification is more stringent since it captures all unobserved characteristics at the classroom level, including peer effects, and any temporal factors specific to that learning environment. Our analysis uses two different measures of the treatment: the logarithm of the number of violent events and the logarithm of the number of fatalities.

Across all columns, for both measures of violence, we consistently find that the Turkish language and Math scores of Syrian students improve in response to heightened levels of violence in their place of origin.¹⁷ Column 2 indicates that for a shift from the 50th to 75th percentile of violent events, we would expect Turkish language scores among Syrian students to increase by 2.7 percent. At the mean, this is an increase of around 2 points or equivalently closing the gaps between Syrian and Turkish students by 16 percent.¹⁸

The shift from the 50th to the 75th percentile in the casualties treatment, leads to an average 1.8 percent increase in Turkish language scores. This effect is meaningful, comparable to a 1-point improvement at the mean, or equivalently, would close the gap between the average Syrian and Turkish student by 8 percent.

While the effect of violence on Math scores is positive and statistically significant, it is somewhat less sizable. If the number of violent events shifts from the 25th to the 75th percentile, Math scores increase by a little under 1 percent, on average. This corresponds to an approximately half point increase for Syrian students who score around the mean. While this is economically small, the gap between Syrian and Turkish students is only around 7 points when it comes to Math scores; therefore, a half-point increase would close the gap by around 7 percent. Similarly, if the number of fatalities rises from the 25th to the 75th

¹⁷In Table A1, we report results from models that progressively replace individual fixed effects with coarser controls. Without individual fixed effects, the relationship between origin conflict and academic performance becomes smaller and statistically insignificant. However, once we control for students' birth-district fixed effects, the estimates become statistically significant again and are close to the baseline results with individual fixed effects; adding father's education fixed effects changes them little further. This pattern indicates that much of the selection absorbed by individual fixed effects in our setting reflects fixed differences across origin districts, rather than broader unobserved individual-level heterogeneity.

¹⁸The percent change in the outcome due to a shift from the 50th to 75th percentile of violent events, that is an increase from 33 to 96 violent events per semester, is equal to $(96/33)^{0.0261} = 1.027$, therefore a 2.7 percent increase. The mean Turkish language score among Syrian and Turkish students is 58 and 70 points, respectively. A 2.7 percent increase at the mean among Syrian students is approximately 2 points or 17 percent of the gap between Syrian and Turkish students $2/(70 - 58) = 0.17$.

percentile, Math scores are likely to increase by less than 1 percent, on average.

Our results suggest that heightened violence in hometowns is associated with a larger improvement in Turkish language scores compared to Math scores among Syrian refugees. This may reflect the fact that children put considerable effort to integrate into their new society. As violence intensifies in their hometowns, Syrian students proactively invest in Turkish language skills, a crucial step in achieving effective integration, which enhances their communication and participation in educational and social activities. We also find a modest increase in Math scores, which may reflect the increased academic effort stemming from the reduced likelihood of return, or reflect improved language skills needed for Math.

Do higher levels of violence affect absenteeism among Syrian students?

In Figure 2, we analyze the number of school absences per academic year, serving as an indicator of school attachment among Syrian students. While school attachment does not directly reflect academic achievement, it captures the students' willingness to attend school and engage in educational activities, including exams. We are unable to detect effects statistically different from zero, which indicate that school attachment might not change meaningfully in response to heightened violence in students' hometowns.

Heterogeneity by student characteristics

Table 2 reports heterogeneity in the effect of hometown violence on refugee students' end-of-semester test scores. The outcomes are *Log Turkish score* (column 1) and *Log Math score* (column 2). Each panel augments our fully saturated specification with an interaction between $\log(\# \text{ violent events})$ and an indicator for the relevant subgroup. For ease of interpretation, we also report the implied *total effect* for the subgroup (baseline coefficient plus interaction) and its p -value.

Panel A interacts exposure with an indicator for whether the student continues schooling through the end of compulsory education. Because continuing is a post-treatment outcome, we treat this split as a descriptive check rather than a causal estimand. The positive response is concentrated among continuers: the implied total effects are 0.0277 for Turkish ($p = 0.000$) and 0.0077 for Math ($p = 0.031$). Reassuringly, we also find no evidence that hometown violence predicts dropout or chronic absenteeism (Figures 2 and A9), reducing concerns that selective attrition is mechanically driving our results.

Panel B of Table 2 interacts exposure with an indicator for whether the student is naturalized (holds Turkish nationality). The interaction term is negative for Turkish, indicating attenuation among naturalized students relative to non-naturalized students. This pattern is consistent with legal status moderating the marginal incentive to adjust host-country investments in response to additional origin conflict shocks.¹⁹

¹⁹Naturalized students are positively selected in levels (with higher test scores on average), but this is not a concern for identification as individual fixed effects absorb time-invariant differences across students. We therefore interpret the naturalization heterogeneity as complementary evidence rather than as being driven by baseline level differences.

Panel C shows larger responses for girls: the interaction with *Female* is positive and statistically significant for both outcomes. The implied total effects for girls are 0.0286 for Turkish and 0.0149 for Math (with p -values reported as 0.000 and 0.001, respectively). Labor force opportunities are higher for women in Türkiye than in Syria. One plausible interpretation is that when return becomes less feasible, the expected returns to girls' education become more closely tied to host-country opportunities, where schooling is more strongly linked to continued education and eventual labor-market participation.

Overall, Table 2 documents systematic heterogeneity by student and household characteristics, with effects consistently larger for Turkish language and patterns that are consistent with increased host-country investment when origin-country violence intensifies.

Table 3 shows larger effects for students whose fathers have primary education or below. The interaction is positive and statistically significant for both Turkish and Math, implying a stronger response in lower-education households. One contextual interpretation is that when origin conditions worsen, households may increase emphasis on host-country schooling and language acquisition, and this margin of adjustment may be larger for families that had placed relatively less weight on these investments at baseline.

Heterogeneity by arrival timing and baseline violence at origin

We next examine heterogeneity along two dimensions that are informative for interpretation: (i) *arrival timing*, which proxies for differences in perceived return feasibility as the conflict became protracted, and (ii) *baseline violence at origin*, which captures variation in districts that were already highly exposed to conflict prior to migration (so that additional violence may carry different informational content across origins). Table 4 reports these results.

Panel A interacts exposure to hometown violence with an indicator for arriving in Türkiye in September 2017 or later. We focus on 2017 because it marks roughly five years into the war, by which point the conflict had become entrenched and multi-sided, and return within a short horizon was widely viewed as less feasible. Consistent with this interpretation, the interaction term is positive and statistically significant for *Log Turkish score*, implying larger Turkish-language gains for later arrivals. The implied total effect for late arrivals is 0.0349, compared to 0.0148 for earlier arrivals. For *Log Math score*, the interaction term is small and statistically insignificant, and the implied total effect for late arrivals is not statistically different from zero at conventional levels. Overall, the arrival-timing heterogeneity is concentrated in Turkish language acquisition, the most direct host-country-specific integration margin.

Panel B turns to heterogeneity by origin backgrounds. We construct a measure of cumulative violence in Syria *prior to arrival in Türkiye* and interact contemporaneous exposure with an indicator for being below the median of this pre-period violence distribution. This split is motivated by the idea that additional violence may carry different informational content depending on what families have already experienced: for students from historically high-violence origins, renewed violence may reinforce the persistence of conflict, whereas for

historically lower-violence origins, marginal changes may be less strongly linked to longer-run expectations. Empirically, the interaction term is negative and statistically significant for both outcomes, implying attenuation among students from historically lower-violence origins and correspondingly stronger responses among students from historically higher-violence origins. Overall, it shows that the relationship between contemporaneous violence and learning outcomes varies with pre-migration conflict exposure at origin.

Does the effect vary by perpetrator type?

An important question for interpretation is whether students' responses are tied to particular political actors (e.g., regime versus opposition violence) or whether they reflect broader insecurity at origin. To shed light on this, Figure 3 examines whether the estimated relationship between hometown violence and students' test scores depends on the type of perpetrator. We classify violent events by whether they involve state actors, non-state actors, or civilian actors, and then re-estimate our baseline specification in Equation 1 replacing overall violence with actor-specific measures (i.e., the log number of violent events attributed to each actor category). The figure plots the point estimates and 95% confidence intervals for both outcomes.

Across perpetrator categories, the estimated coefficients are positive and of similar magnitude to the baseline specification: the confidence intervals overlap substantially across actor-specific measures, and none of the differences across actor types are statistically distinguishable at conventional levels. This pattern indicates that the results are not driven by violence attributed to a particular actor; rather, what matters empirically is the persistence and intensity of violence in the place of origin, regardless of perpetrator type. In other words, the results are consistent with the idea that conflict primarily operates by sustaining uncertainty and insecurity at origin, and therefore lowering the feasibility of return in the relevant horizon, rather than through actor-specific political developments.

Do effects accumulate with continued violence?

To assess whether the relationship we document reflects only short-run fluctuations or instead sustained responses to a protracted conflict environment, Table 5 replaces contemporaneous violence in Equation 1 with cumulative exposure measures. Specifically, columns (1) and (3) use the log cumulative number of violent events in the student's hometown *since arriving in Türkiye*, while columns (2) and (4) use the log cumulative number of violent events *since the start of the Syrian conflict*.

Across both cumulative measures, the estimated coefficients are positive and statistically significant for Turkish and Math. For Turkish, the coefficient on cumulative violence since arrival is 0.0406, and the coefficient on cumulative violence since the start of the conflict is 0.0980. For Math, the corresponding coefficients are smaller in magnitude but remain statistically significant. Relative to specifications using only contemporaneous violence, these results indicate that the association with learning outcomes is stronger when violence is

summarized as an accumulating process rather than as a purely contemporaneous shock.²⁰

These findings are broadly consistent with the dynamic patterns in the event-study analysis: the event-study estimates show no evidence of differential pre-trends and a gradual build-up of effects over post-event semesters, rather than a one-time jump at impact. Taken together, the cumulative-exposure results and the event-study dynamics suggest that the relevant margin is not purely immediate, but evolves over time as conflict persists and additional violence occurs, consistent with sustained origin-country insecurity shaping integration investments.

5.2 Parental mediation and human capital investment

We interpret the main results through a simple organizing idea: changes in conflict conditions in refugees' places of origin can shift households' perceived feasibility and timing of return, thereby affecting the perceived returns to host-country-specific human capital investments, especially Turkish language acquisition. Importantly, the relevant behavioral actors are primarily parents, who make household decisions about settlement planning and allocate educational inputs, rather than children independently tracking and processing developments in Syria. At the same time, our administrative education data do not contain direct measures of parental return intentions or children's media consumption, so the evidence in this section is necessarily complementary and suggestive rather than definitive.

A parent-mediated channel is plausible in our setting for two reasons. First, our administrative sample covers grades 4–10, an age range in which children's study routines and educational inputs are plausibly shaped by parental decisions and household constraints. Second, even when children are aware of events in Syria, parents are more likely to be informed about origin conditions, to reassess the feasibility of return, and to translate these assessments into changes in educational investments, such as homework supervision, time spent with children at home, and emphasis on language acquisition. We therefore focus on evidence that is informative about parental information-seeking, beliefs, and time investments, and we relate this evidence to heterogeneity patterns in the administrative test-score data that are consistent with parental mediation.

In the absence of direct survey measures of return intentions in our data, we bring in new evidence from the 2018 Türkiye Demographic and Health Survey (DHS), the only standardized and nationally representative household survey that covers a sample of Syrian refugees living in Türkiye and provides individual-level information on mothers and children. While the DHS does not ask a direct question about intended return, it contains outcomes that are informative about parental information-seeking, educational attitudes, and time investments in children.

²⁰Because these coefficients come from log–log specifications, they should be interpreted as elasticities. Since cumulative violence is much larger in levels than contemporaneous violence, a 1% increase in cumulative violence corresponds to a larger absolute increase in events. We therefore do not interpret the larger cumulative coefficients as implying a larger marginal effect of an additional violent event, but rather as evidence that sustained exposure is more strongly associated with outcomes when violence is measured cumulatively.

Information-seeking and educational attitudes. Table 6 relates contemporaneous conflict intensity in the child’s Syrian birth province (measured as the logarithm of the number of violent events in 2018) to DHS-based measures of mothers’ internet use and beliefs. The dependent variables are constructed directly from DHS questions: *Some internet* equals reporting internet use either irregularly or regularly, while *Regular internet* equals reporting regular use. We interpret these as proxies for parental information-seeking. The measure *Sons’ education more important* equals the mother agreeing that educating sons is more important than educating daughters. This margin is informative because attitudes toward daughters’ education may become especially relevant when return prospects decline, given that women’s labor force participation is substantially higher in Türkiye than in Syria. The regressions include residence-region-by-residence-type fixed effects (urban/camp), mother’s age fixed effects, mother’s education fixed effects, family size fixed effects, and child arrival-time fixed effects. Standard errors are clustered at the child’s birth province.

Two patterns are noteworthy. First, higher conflict intensity is associated with greater reported internet use, particularly among mothers with primary education or less (columns (2) and (4)), where the interaction term with low maternal education is positive and statistically significant. This pattern is consistent with increased information-seeking as expected life trajectories change. Second, higher conflict intensity is associated with a lower probability that mothers report that sons’ education is more important than daughters’ (column (5)), with an additional negative effect among less-educated mothers (column (6)). This pattern is in line with mothers becoming more open to investing in their daughters’ education, and reflect the fact that our test-score results are stronger for girls as well. The stronger effects for girls potentially reflect the fact that opportunities for women are larger in Türkiye than in Syria, and so the returns to investing in girls’ education have improved. Our DHS patterns are also consistent with the heterogeneity results in Table 3, which show stronger effects among students from lower-education households.

Overall, we interpret these DHS results cautiously as suggestive evidence of greater information-seeking and shifting educational priorities when conflict in the place of origin is more severe.

Parental time investments in children. Table 7 uses DHS time-use modules to construct indicators for whether nobody in the household spends time on (i) child caring, (ii) being with children at home, and (iii) being with children outside (Panel A), and analogously whether the mother spends time on each activity (Panel B).

The key regressor is conflict exposure in the child’s origin province *since arriving in Türkiye* (measured as the logarithm of the number of violent events since arriving in Türkiye). Crucially, the specification also controls for the logarithm of the number of violent events before arriving in Türkiye, which helps separate contemporaneous variation from fixed differences across origins. The regressions include residence-region-by-residence-type fixed effects, mother’s age fixed effects, mother’s education fixed effects, family size fixed effects, mother and child arrival-time fixed effects, and child birth-province fixed effects.

The results show that higher conflict exposure *since arrival* is associated with a lower probability that nobody in the household spends time on childcare (Panel A, column (1)), and with a higher probability that the mother spends time on childcare and with children at home (Panel B, columns (1) and (2)). In contrast, we do not find meaningful associations for time with children outside the home (Panel B, column (3)). These patterns are consistent with greater parental investment in children, especially at home, where studying is likely to take place. Importantly, the coefficients on pre-arrival conflict exposure are small and statistically indistinguishable from zero across outcomes, which is consistent with the interpretation that these associations are driven by contemporaneous origin conditions rather than by time-invariant differences across origin provinces.

Younger children, time since arrival, and external evidence. We complement the DHS evidence with heterogeneity patterns in the administrative data that are informative about whether a parent-mediated channel is plausible. First, effects are stronger for younger students and attenuate for older cohorts (Table 8). This age gradient is difficult to reconcile with a mechanism driven primarily by older students independently following news and updating beliefs, but it is consistent with parental mediation because parents typically exert greater influence over younger children’s routines and study time. Second, the response is stronger for more recent arrivals (Table 9), consistent with the idea that origin news may be especially salient when families’ settlement plans are less settled shortly after arrival.

We also relate our interpretation to external evidence on refugee return intentions and educational choices. Large-scale refugee intention surveys consistently show that short-term return intentions among Syrian refugees remain low and are driven primarily by conditions inside Syria, especially safety, security, and livelihoods (Arababah et al., 2023; UNHCR, 2024b; International Rescue Committee, 2025). These surveys therefore support the idea that worsening origin-country conditions can shift settlement planning toward a longer, if not permanent, stay. Complementing this evidence, research on Syrian refugees in Türkiye shows that parents adapt their educational strategies as displacement becomes more prolonged, including by placing greater emphasis on Turkish language acquisition (Karaagac et al., 2022).

In addition, the steady rise in Syrian children’s enrollment in Turkish public schools is difficult to reconcile with expectations of near-term return and instead points to substantial parental investment in host-country schooling (Karaagac et al., 2022; UNHCR, 2024a). Our interpretation does not require that baseline return intentions were high early in the conflict; rather, it relies on the idea that families update beliefs about the feasibility and timing of return, and that these updates can affect incentives to invest in host-country-specific skills even when average return intentions remain low.²¹

²¹We also considered whether digital trace data, such as Google searches or online activity, could proxy for return intentions. In our context, however, such measures are unlikely to be reliable. Digital traces do not allow us to identify who within the household is conducting searches, nor to link online behavior credibly to individuals’ Syrian districts of origin, which makes it difficult to align such measures with our treatment definition. For these reasons, we focus instead on DHS-based measures and administrative heterogeneity patterns.

Overall, the evidence in this section points in a consistent direction. The DHS results show that higher conflict intensity in the place of origin is associated with greater parental information-seeking, shifts in stated educational priorities, and greater maternal time investment in childcare and time with children at home (Table 6 and Table 7). The administrative heterogeneity results show that the main effects are stronger where parental mediation should plausibly matter more, especially for younger children (Table 8 and Table 9). Finally, external survey and qualitative evidence indicates that prolonged insecurity in Syria pushes refugee families toward longer-term settlement planning and greater investment in host-country schooling. Taken together, these patterns make the return-expectations mechanism, in our view, the interpretation most consistent with the full set of evidence. At the same time, the evidence remains suggestive rather than definitive and does not allow us to isolate a single psychological or behavioural pathway.²²

5.3 Additional results and robustness checks

In this section, we present our event-study estimates and a range of additional robustness checks to assess the stability of our main findings.

Event-study estimates: pre-trends and dynamic effects

Our baseline specification in Equation 1 uses the contemporaneous continuous measure of hometown violence together with our richest fixed-effects structure. We view that specification as our preferred design because it preserves the full variation in treatment intensity and is therefore best suited to estimating the relationship between ongoing violence and refugee students' outcomes, as well as the heterogeneity patterns we examine above. The event-study analysis is complementary. By discretizing violence into a binary event, it necessarily uses less variation, but it allows us to assess pre-trends and to trace the dynamic response to a salient increase in hometown violence. We therefore use the event study as a diagnostic and complement to the main continuous-treatment framework, rather than as a substitute for it.

Specifically, the event-study estimates address two questions: whether the identifying variation is consistent with parallel pre-trends, and how quickly any effects emerge after exposure. The relevant assumption is that, absent heightened violence in the hometown, students exposed earlier and later would have followed similar *within-student* trajectories over event time, after accounting for fixed differences across students and common semester shocks.

We report semester-level event-study estimates that trace outcomes from three semesters before treatment to six semesters after treatment. We define the event as the *first* semester in which a student's hometown experiences violence above a pre-specified threshold. Our baseline threshold is 96 violent events, corresponding to the 75th percentile of the violence

²²For this reason, we do not interpret the results as uniquely identifying one mechanism. Rather, we view them as most consistent with a parental mediation and return-expectations channel, while allowing for other adaptive responses, such as psychological adjustment, greater future orientation, or an increased desire for stability under persistent uncertainty.

distribution. We also examine alternative thresholds of 80, 90, 96, 100, and 110 events. Standard errors are clustered at the district level.

Figure 4 presents the main event-study estimates using the estimator of Sun and Abraham (2021). Panel (a) uses *Log Turkish score* as the dependent variable and Panel (b) uses *Log Math score*. The omitted reference period is the semester immediately prior to treatment ($k = -1$), so each coefficient is interpreted relative to that baseline. The estimates show that the pre-treatment coefficients are close to zero and statistically insignificant in both outcomes, providing no evidence of differential pre-trends in the periods leading up to treatment. Post-treatment, the coefficients turn positive and increase gradually over event time. This pattern is consistent with effects that build over time rather than appearing immediately on impact.

Figure 5 examines sensitivity to the definition of treatment. Using the same estimator (Sun and Abraham, 2021) and the same fixed effects and clustering choices as in Figure 4, we re-estimate the event study under alternative thresholds for heightened violence. The qualitative pattern is stable across definitions: the pre-treatment coefficients remain close to zero, and the post-treatment coefficients are generally positive. Although the magnitudes and precision vary somewhat across thresholds, as expected when treatment timing is redefined, the main conclusions regarding pre-trends and post-treatment dynamics remain unchanged.

Figure 6 examines robustness to alternative estimators for staggered treatment timing with potentially heterogeneous effects. Holding the event definition fixed at the baseline threshold, we report event-study coefficients and 95% confidence intervals using the approaches of Sun and Abraham (2021) and De Chaisemartin and d’Haultfoeuille (2020). The resulting profiles are similar across methods.

Taken together, Figures 4–6 yield two main conclusions. First, there is no strong indication of differential pre-trends. Second, the effects of heightened hometown violence are positive and emerge gradually over time. These findings support the interpretation that ongoing violence in origin areas affects refugee students’ subsequent investment and integration behavior. We next turn to the main estimates from the continuous-treatment specification in Equation 1.

Clean-sample analysis

A related issue arises once the continuous violence measure is converted into a binary treatment: treatment status may switch on and off over time. This differs from the event-study framework above, where treatment is defined by first crossing the threshold and then treated as absorbing. To assess whether our results are sensitive to such treatment-path variation, we report an additional clean-sample analysis in Table A2.

As in the event study, this exercise begins by discretizing the continuous violence measure using thresholds from the treatment distribution. To maintain comparability with the event-study analysis, we include the 75th-percentile threshold, alongside lower cutoffs. We then restrict the sample to observations with cleaner treatment paths. The control group consists

of students who are never treated. The treated group consists of students who are initially untreated, become treated, and remain treated thereafter. We therefore exclude students whose treatment status later switches off, as well as students who are treated throughout the sample period. The goal is to focus on cleaner comparisons that are less affected by unstable treatment histories and to document that the main findings are not driven by observations with irregular treatment paths or by repeated transitions into and out of treatment.

Table A2 shows that the estimates remain qualitatively similar in this restricted sample. Across alternative threshold definitions, the relationship between heightened hometown violence and refugee students' academic outcomes remains positive. This provides additional evidence that our findings are not an artifact of unstable binary treatment paths.

Ruling out influential observations

We rule out the importance of influential observations by plotting the coefficients of our preferred specifications as each province is omitted at a time. Figure A10 shows that our coefficient estimates are quite stable as a specific province is eliminated from our main sample in each iteration. We repeat a similar analysis with Figure A11 in which we drop one semester-year at a time and again find that our estimates are not driven by any single semester-year.

Placebo tests: Leads of exposure to violence

Figure A12 shows no detectable effects of one-year leads of violence on Turkish language or Math scores. Specifically, within the subset of students observed for at least three academic years, the estimated effect of contemporaneous violence remains positive and statistically significant. Reassuringly, the coefficient on the one-year lead of violence is close to zero and not statistically significant at the 90% confidence level. This complements the event-study analysis, which likewise shows no evidence of pre-trends.

Re-estimating the results on a fixed cohort of early arrivals

Our results show (a) no evidence of anticipatory effects using treatment leads, (b) robustness to alternative clustering choices and functional-form transformations, and (c) that dropping one Turkish province or one semester at a time leaves the estimates stable. In addition, a clean-sample analysis based on threshold-defined binary treatments yields qualitatively similar results. Together, these findings argue against spurious effects driven by changes in the composition of later-arriving cohorts from specific origin regions.

To further strengthen this case and to rule out any remaining concern that results are driven by selection into migration, we also re-estimate the main specifications on *fixed cohorts* of early arrivers: (i) students who arrived by the 2013–2014 academic year; and (ii) students who arrived by the 2014–2015 academic year. We report these results in Table A3 and find that they are qualitatively similar to those in the full sample.

Alternative parametrization of treatment and outcomes

To assess whether our findings are driven by the log–log specification or by the $\log(\cdot + 1)$ adjustment applied to the treatment, we report a set of alternative parameterizations in Table A4. It is useful to distinguish between the treatment and outcome variables here. On the outcome side, our baseline specification uses $\log(\text{test score})$, not $\log(\text{test score} + 1)$. Because all students in the regression sample have strictly positive test scores, our main results are not subject to concerns associated with log-transforming outcomes that contain zeros. On the treatment side, however, the baseline specification uses $\log(\#\text{events} + 1)$, since roughly 3% of observations in the regression sample have zero conflict in the district–semester. The robustness exercises below, therefore, focus primarily on alternative parameterizations of the treatment, and in the final panel, we also show results using test scores in levels.

Panel A keeps the dependent variables in logs, *Log Turkish score* and *Log Math score*, but replaces the treatment variable $\log(\#\text{events} + 1)$ with $\text{asinh}(\#\text{events})$, which allows for zero values while preserving a log-like interpretation at higher values. Panels B and C instead use raw test scores on the 0–100 scale. In both panels, we drop observations with zero violent events. Panel B keeps the baseline treatment definition, $\log(\#\text{events} + 1)$, while Panel C uses $\log(\#\text{events})$ and therefore removes the “+1” adjustment. Panel D also uses raw test scores and retains the baseline treatment definition.

Across these alternatives, the estimates remain qualitatively similar. This indicates that our findings are not sensitive to the precise functional form of the treatment, the treatment of zero-conflict observations, or the use of log-transformed rather than level test scores.

Alternative clustering of standard errors

Table A5 demonstrates the robustness of the main findings using three different approaches to clustering standard errors: district-level bootstrapping, district and academic year level clustering, and a district and academic year level bootstrapping. Across all specifications, the effects remain statistically significant.

5.4 Are there spillover effects to Turkish students?

To explore potential spillovers to Turkish students, we collapse the semester-level data to the school–academic year–grade–semester–classroom level. At this level, we compute: (i) the average Turkish language and Math scores of Turkish students in the classroom–semester cell, and (ii) the share of Syrian students in that classroom–semester cell.

Our analysis proceeds as follows. First, we calculate average test scores among Turkish students at the classroom–semester level. This will serve as the outcome variables in the spillover analysis. Then, we obtain the share of Syrian students, which will serve as the control variable used to capture potential congestion effects due to higher refugee concentration, again at classroom–semester level. Throughout this analysis, we focus on classrooms with at least 1 Syrian student. Next, we construct a *weighted violence index*, as in Equation 2, measuring contemporaneous violence experienced by Syrian classmates in each classroom–semester. An

important property of this measure is that it does not increase with the share of Syrian students in the classroom, making it a robust measure of the average Syrian conflict intensity within a classroom during a semester.

The weighted violence index is calculated as the sum of the shares of Syrian students from each origin district, weighted by contemporaneous violence in that origin district:

$$WeightedIndex_{c,s} = \sum_{d \in \mathcal{D}} s_{d,c} \times V_{d,s}, \quad (2)$$

where $s_{d,c}$ is the share of Syrian students in classroom c who are born in Syrian district d , \mathcal{D} is the set of all Syrian districts, and $V_{d,s}$ is contemporaneous violence in district d during semester s . The shares here must add up to 1 (i.e., $\sum_d s_{d,c} = 1$) as the denominator includes only Syrian students.

Third, we allow the association between origin-district violence exposure (through Syrian classmates) and Turkish students' outcomes to vary with the overall Syrian share in the classroom, consistent with the idea that peer spillovers may depend on the intensity of refugee presence. That is, the violence-induced spillover is likely to be meaningful when there are more Syrian students in the class.

We estimate:

$$Y_{c,s} = \beta_1 Share_{c,s} + \beta_2 WeightedIndex_{c,s} + \beta_3 (WeightedIndex_{c,s} \times Share_{c,s}) + \Gamma_y + \gamma_{ss} + \theta_g + \lambda_j + \alpha_{yp} + u_{c,s}, \quad (3)$$

where $Y_{c,s}$ is the average Turkish or Math score among Turkish students in classroom c at semester s , and $Share_{c,s}$ is the share of Syrian students in that classroom-semester. Notice that this is a different share from the shares used to calculate the weighted index on violent exposure. The specification includes academic year fixed effects (Γ_y), a summer-semester indicator (γ_{ss}), grade fixed effects (θ_g), school fixed effects (λ_j), and academic year-by-province fixed effects (α_{yp}). Standard errors are clustered at the school level.

The coefficient β_1 captures the association between the Syrian share and Turkish students' outcomes (a congestion-type effect or peer effects). β_2 captures the effect of having a Syrian classmate who, on average, come from origins with more violence. As we have explained before, our weighted index on violence exposure is constructed in a way that it will not mechanically increase when a classroom has more Syrian refugee students. Because we are investigating the effects of more intensive violence exposure on Turkish students, the coefficient of interest is, β_3 , which captures the marginal effect of this violence index in classrooms with an increasing share of Syrian classmates. This is what we call the spillover effect. Consider two classrooms with the same share of Syrian students (thus, the direct effects of these refugees enjoying more educational resources are controlled), β_3 captures the marginal effects of Syrian classmates experiencing more violent events in their hometown. Analogously, consider two classrooms with the same violence index; β_3 captures how this violence has greater

impacts on Turkish classmates as the share of Syrian classmates increases.

The estimates are reported in Table 10 across three panels. Panel A reports results for the full sample of classroom–semester cells with at least one Syrian student. Panel B restricts to schools with an above-median number (i.e., 8) of Syrian refugees during the school-semester, while Panel C focuses on schools with a below-median number.

Congestion effects. In the violent-events specifications, β_1 is negative and statistically significant in Panels A and B. In Panel A, a one-unit increase in $Share_{c,s}$ (from 0 to 1) is associated with a -0.271 change in *Log Turkish score* and a -0.345 change in *Log Math score*; equivalently, a 10 percentage point increase in the Syrian share is associated with about a 2.71% lower Turkish score and a 3.45% lower Math score. In Panel B, these negative associations are larger in magnitude (-0.388 for Turkish and -0.469 for Math), consistent with stronger congestion pressures in higher-refugee settings. In Panel C, where we only include those schools that have below-median numbers of Syrian students, the share coefficients are much smaller in magnitude and not statistically significant.

Spillovers through violence exposure among Syrian classmates. In Panel A (full sample), β_3 , the spillover effect is positive and statistically significant. That is, higher violence exposure among Syrian classmates is associated with modest positive changes in the negative direct exposure effects of Syrian classmates on Turkish students' average outcomes. This suggests peer interactions between Syrian students who are more incentivized to integrate into Türkiye due to more intensive hometown conflicts at least partly offsets their direct negative effects on average Turkish students. In Panel B (higher-refugee schools), the same pattern is more pronounced: β_3 is positive and statistically significant for both outcomes, and as expected, they are larger than the magnitudes in the full sample. In Panel C (lower-refugee schools), β_3 is no longer statistically significant.

Taken together, the violent-events specifications indicate two forces operating in opposite directions. Higher Syrian shares are associated with lower Turkish students' performance (consistent with congestion), but the marginal spillover effects associated with higher violence exposure among Syrian classmates become positive, and this pattern is clearest in higher-refugee schools. This is consistent with spillovers that operate through peer interactions in settings where Syrian classmates constitute a meaningful share of the classroom.

6 Conclusion

We investigate how ongoing violence in the places of origin of Syrian refugee children shapes their integration in Türkiye, focusing on educational outcomes. Using administrative data, we find that higher contemporaneous hometown violence is associated with better academic performance, with consistently larger effects on Turkish language than on Math. We find no effect on absenteeism. The response is attenuated among naturalized students, and the broader set of results is consistent with a parent-mediated mechanism: higher conflict intensity is associated with greater maternal internet use, less male-biased educational prior-

ities, and greater maternal time investment in children. We also document positive spillover effects on Turkish classmates that partly offset the congestion effects associated with a higher share of Syrian students in the classroom.

Taken together, these results suggest that ongoing violence at origin can strengthen refugee families' incentives to invest in host-country-specific human capital. We interpret the evidence as most consistent with a return-expectations channel, whereby worsening origin conditions reduce the perceived feasibility of return and shift attention toward skills that matter in the host country, especially local language acquisition.

More broadly, our findings suggest that refugee integration is more responsive to incentives and expectations than a static view of assimilation would imply. Integration may therefore be stronger when host countries lower the costs and raise the returns to investing in host-country-specific human capital. The cumulative nature of the effects also speaks to debates over asylum and protected status: when return remains unsafe and conflict persists, temporary or uncertain arrangements may weaken longer-term integration efforts. Overall, these findings imply that when displacement becomes prolonged, policies that support stability and host-country-specific human-capital investment may be especially important for sustaining refugee integration.

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Table 1: Effects of hometown conflicts on students' test scores

	Log Turkish score				Log Math score			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log. # of violent events	0.0155*** (0.0022)	0.0261*** (0.0046)			0.0037** (0.0014)	0.0075** (0.0029)		
Log. # of fatalities			0.0108*** (0.0020)	0.0137*** (0.0038)			0.0034*** (0.0009)	0.0041** (0.0018)
<i>R</i> -squared	0.744	0.839	0.744	0.838	0.754	0.839	0.754	0.839
# of observations	53,973	53,973	53,973	53,973	53,973	53,973	53,973	53,973
# of clusters	44	44	44	44	44	44	44	44
Mean baseline (2011-2014)	60.49	60.49	60.49	60.49	58.33	58.33	58.33	58.33
Mean sample	58.31	58.31	58.31	58.31	57.77	57.77	57.77	57.77
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years since arrival FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grade FE	Yes	No	Yes	No	Yes	No	Yes	No
Classroom-grade-year FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes. This table reports estimates from the main specifications, controlling for different sets of fixed effects. The sample is at student-semester level. The dependent variable is *Log Turkish score* in columns (1)-(4) and *Log Math score* in columns (5)-(8). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester (plus 1) in logarithm; while *Log. # of fatalities* is the number of deaths at the district-semester (plus 1) in logarithm. Columns (1), (3), (5) and (7) incorporate fixed effects for individuals, time since arrival and grades while columns (2), (4), (6) and (8) include Classroom-grade-academic year FE instead of Grade FE. Mean baseline and Mean sample refer to the mean of the dependent variable calculated over the academic years from 2011-2012 to 2013-2014 and for the regression sample, respectively. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 2: Heterogeneous effects on test scores, by students' characteristics

	Log Turkish score (1)	Log Math score (2)
Panel A: Heterogeneity by whether students continued their education		
Log. # of violent events	-0.0097 (0.0077)	0.0026 (0.0163)
Log. # of violent events \times Continuing	0.0374*** (0.0074)	0.0051 (0.0147)
Total effect	0.0277	0.0077
<i>p</i> -value	0.0000	0.0031
Panel B: Heterogeneity by whether students are naturalised		
Log. # of violent events	0.0273*** (0.0045)	0.0075** (0.0033)
Log. # of violent events \times Naturalised	-0.0119** (0.0055)	0.0001 (0.0061)
Total effect	0.0154	0.0075
<i>p</i> -value	0.0073	0.0922
Panel C: Heterogeneity by whether students are female		
Log. # of violent events	0.0233*** (0.0040)	-0.0004 (0.0025)
Log. # of violent events \times Female	0.0053** (0.0025)	0.0153*** (0.0023)
Total effect	0.0286	0.0149
<i>p</i> -value	0.0000	0.0001
# of observations	53,973	53,973
Individual FE	Yes	Yes
Years since arrival FE	Yes	Yes
Classroom-grade-year FE	Yes	Yes

Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in column (1) and *Log Math score* in column (2). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester in logarithm. In panel A, *Continuing* equals to 1 if a student is continuing their education and zero if the student dropped out of schooling before the end of their compulsory education (i.e., before grade 12). In panel B, *Naturalised* equals to 1 if the Syrian refugee student has a Turkish nationality and zero otherwise. In panel C, *Female* equals to 1 if the student is a female and zero otherwise. Total effect, which is the sum of the coefficient on the treatment variable and the coefficient on the interacted term, together with its *p*-value, is also reported. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 3: Heterogeneous effects on test scores, by fathers' education level

	Log Turkish score (1)	Log Math score (2)
Log. # of violent events	0.0238*** (0.0048)	0.0054** (0.0026)
Log. # of violent events × Father's education is primary or below	0.0176*** (0.0038)	0.0169*** (0.0042)
Total effect	0.0414	0.0223
<i>p</i> -value	0.0000	0.0000
# of observations	53,973	53,973
Individual FE	Yes	Yes
Years since arrival FE	Yes	Yes
Classroom-grade-year FE	Yes	Yes

Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom × grade × academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in column (1) and *Log Math score* in column (2). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester in logarithm. *Father's education is primary or below* equals to 1 if the highest education of the student's father is primary school or below, and 0 otherwise. The interaction term between violence exposure and father's education is missing is also controlled (not reported). Total effect, which is the sum of the coefficient on the treatment variable and the coefficient on the interacted term, together with its *p*-value, is also reported. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 4: Heterogeneous effects on test scores, by students' migration patterns

	Log Turkish score (1)	Log Math score (2)
Panel A: Heterogeneity by students' arrival time		
Log. # of violent events	0.0148*** (0.0029)	0.0065*** (0.0018)
Log. # of violent events × Arrival time is September 2017 onwards	0.0202** (0.0098)	0.0018 (0.0067)
Total effect	0.0349	0.0083
<i>p</i> -value	0.0003	0.1568
Panel B: Heterogeneity by students' origins		
Log. # of violent events	0.0345*** (0.0010)	0.0122*** (0.0006)
Log. # of violent events × Past violence in Syria is below median	-0.0178*** (0.0054)	-0.0099*** (0.0034)
Total effect	0.0166	0.0022
<i>p</i> -value	0.0019	0.4952
# of observations	53,973	53,973
Individual FE	Yes	Yes
Years since arrival FE	Yes	Yes
Classroom-grade-year FE	Yes	Yes

Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom × grade × academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in column (1) and *Log Math score* in column (2). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester in logarithm. In panel A, *Arrival time is September 2017 onwards* equals to 1 if a student first appeared in the administrative dataset from 2017/18 academic year onwards and zero otherwise. In panel B, *Past violence in Syria is below median* equals to 1 if the cumulative number of violent events experienced in Syria (i.e., before arriving in Türkiye) is fewer than the median. Total effect, which is the sum of the coefficient on the treatment variable and the coefficient on the interacted term, together with its *p*-value, is also reported. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 5: Effects on test scores, with cumulative measures of hometown conflicts

	Log Turkish score		Log Math score	
	(1)	(2)	(3)	(4)
Log. cumulative # of violent events since arriving in Türkiye	0.0406*** (0.0051)		0.0131*** (0.0029)	
Log. cumulative # of violent events		0.0980*** (0.0307)		0.0677*** (0.0210)
<i>R</i> -squared	0.840	0.838	0.840	0.839
# of observations	53,973	53,973	53,973	53,973
Individual FE	Yes	Yes	Yes	Yes
Years since arrival FE	Yes	Yes	Yes	Yes
Classroom-grade-year FE	Yes	Yes	Yes	Yes

Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in column (1) and *Log Math score* in column (2). The explanatory variable *Log. cumulative # of violent events since arriving in Türkiye* is the logarithm of the number of all violent events from arriving in Türkiye to current semester, while *Log. cumulative # of violent events* is the logarithm of the number of all violent events from the beginning of the Syria conflicts to current semester. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 6: Correlation between hometown conflicts and mothers' internet usage and opinion

	Some internet		Regular internet		Sons' educ more important	
	(1)	(2)	(3)	(4)	(5)	(6)
Log. # of violent events	0.0610** (0.022)	-0.0353 (0.031)	0.0347 (0.035)	-0.0124 (0.032)	-0.0498** (0.021)	0.0116 (0.012)
Log. # of violent events × Mothers' education is primary or below		0.1441*** (0.043)		0.0704** (0.027)		-0.0919** (0.037)
R-squared	0.402	0.424	0.344	0.350	0.379	0.404
N	866	866	866	866	866	866

Notes. The regression sample is a cross section of Syria refugee children's mothers, from 2018 Türkiye Demographic and Health Survey (which especially samples Syria refugees in Türkiye). The explanatory variable, *Log. # of violent events*, is the logarithm of the number of violent events in the child's birth province in 2018. Dependent variables of columns (1)-(4) are constructed from the survey question on the frequency of internet use ("No" or "Irregularly" or "Regularly"). *Some internet* is 1 if the mother reports using internet irregularly or regularly. *Regular internet* is 1 if the mother reports using internet regularly. Dependent variables of columns (5)-(6) are constructed from the survey question on mother's opinion on: educated son better than daughter ("Disagree" or "Agree" or "No idea"). *Sons' education more important* is 1 if the mother says "agree" to this question. Control variables include residence region (Istanbul, Aegean, East Marmara, West Anatolia, Mediterranean, Central Anatolia, Central East Anatolia, Southeast Anatolia) interacted with residence type (Urban, Camp) fixed effects (FE), mother's age FE, mother's education level FE, family size FE, and children's arrival time FE. Standard errors are clustered at children's birth province. Weights are applied to match the population distribution across provinces in 2004 Syria census. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 7: Correlation between hometown conflicts and households' time with children

Panel A: Nobody in the household spends time on			
	Child caring (1)	With children at home (2)	With children outside (3)
Log. # of violent events, since arriving in Türkiye	-0.2731** (0.100)	-0.1589 (0.109)	0.0100 (0.118)
Log. # of violent events, before in Türkiye	-0.0117 (0.016)	0.0041 (0.034)	-0.0089 (0.037)
R-squared	0.382	0.351	0.345
N	840	840	840

Panel B: Mom spends time on			
	Child caring (1)	With children at home (2)	With children outside (3)
Log. # of violent events, since arriving in Türkiye	0.2474** (0.104)	0.2368** (0.082)	0.0597 (0.090)
Log. # of violent events, before in Türkiye	0.0140 (0.019)	-0.0089 (0.033)	-0.0156 (0.037)
R-squared	0.378	0.378	0.365
N	840	840	840

Notes. The regression sample is a cross section of Syria refugee children's mothers, from 2018 Türkiye Demographic and Health Survey (which especially samples Syria refugees in Türkiye). The main explanatory variable, *Log. # of violent events, since arriving in Türkiye*, is the logarithm of the number of violent events in children's birth province since they arrived in Türkiye. The logarithm of the number of violent events before they arrived in Türkiye, *Log. # of violent events, before in Türkiye*, is also controlled. Dependent variables are constructed from survey questions on mothers' housework activities. For each question, each mother needs to choose from the list: "Always herself," "Usually herself," "Together with her (ex) husband," "Always her (ex) husband," "Other females in HH," "Other males in HH," "Other," "No one." In panel A, the value of an outcome variable is 1 if the mother answers "No one" to the corresponding question; and 0 otherwise. In panel B, the value of an outcome variable is 1 if the mother answers "Always herself," or "Usually herself," or "Together with her (ex) husband," to the corresponding question; and 0 otherwise. Control variables include residence region (Istanbul, Aegean, East Marmara, West Anatolia, Mediterranean, Central Anatolia, Central East Anatolia, Southeast Anatolia) interacted with residence type (Urban, Camp) fixed effects (FE), mother's age FE, mother's education level FE, family size FE, children's arrival time FE, and children's birth province FE. Standard errors are clustered at children's birth province. Weights are applied to match the population distribution across provinces in 2004 Syria census. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 8: Heterogeneous effects on test scores, by students' age

	Log Turkish score			Log Math score		
	(1)	(2)	(3)	(4)	(5)	(6)
Log. # of violent events	0.0101*** (0.0032)	0.0058 (0.0039)	0.0086 (0.0069)	-0.0108*** (0.0029)	-0.0156*** (0.0040)	-0.0180*** (0.0054)
Log. # of violent events × Student age ≤ 11	0.0331*** (0.0085)			0.0379*** (0.0060)		
Log. # of violent events × Student age ≤ 12		0.0289*** (0.0084)			0.0330*** (0.0070)	
Log. # of violent events × Student age ≤ 13			0.0203* (0.0105)			0.0296*** (0.0080)
Total effect	0.0432	0.0347	0.0289	0.0271	0.0173	0.0116
<i>p</i> -value	0.0000	0.0000	0.0000	0.0000	0.0002	0.0030
<i>R</i> -squared	0.839	0.839	0.839	0.840	0.840	0.840
# of observations	53,973	53,973	53,973	53,973	53,973	53,973
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Years since arrival FE	Yes	Yes	Yes	Yes	Yes	Yes
Classroom-grade-year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom × grade × academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in columns (1)-(3) and *Log Math score* in columns (4)-(6). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester in logarithm. It is interacted with dummies indicating student's age is smaller than or equal to 11, 12, or 13. The dummy indicating student age group is also included in the regressions (not reported). Total effect, which is the sum of the coefficient on the treatment variable and the coefficient on the interacted term, together with its *p*-value, is also reported. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 9: Heterogeneity effects on test scores, by time since migration

	Log Turkish score		Log Math score	
	(1)	(2)	(3)	(4)
Log. # of violent events	0.0217*** (0.0040)	0.0070 (0.0043)	0.0055** (0.0022)	-0.0022 (0.0025)
Log. # of violent events × Years since arrival ≤ 1	0.0105** (0.0049)		0.0048 (0.0038)	
Log. # of violent events × Years since arrival ≤ 3		0.0208*** (0.0072)		0.0106** (0.0050)
Total effect	0.0321	0.0278	0.0102	0.0084
<i>p</i> -value	0.0000	0.0000	0.0411	0.0148
<i>R</i> -squared	0.839	0.839	0.839	0.839
# of observations	53,973	53,973	53,973	53,973
Individual FE	Yes	Yes	Yes	Yes
Years since arrival FE	No	No	No	No
Classroom-grade-year FE	Yes	Yes	Yes	Yes

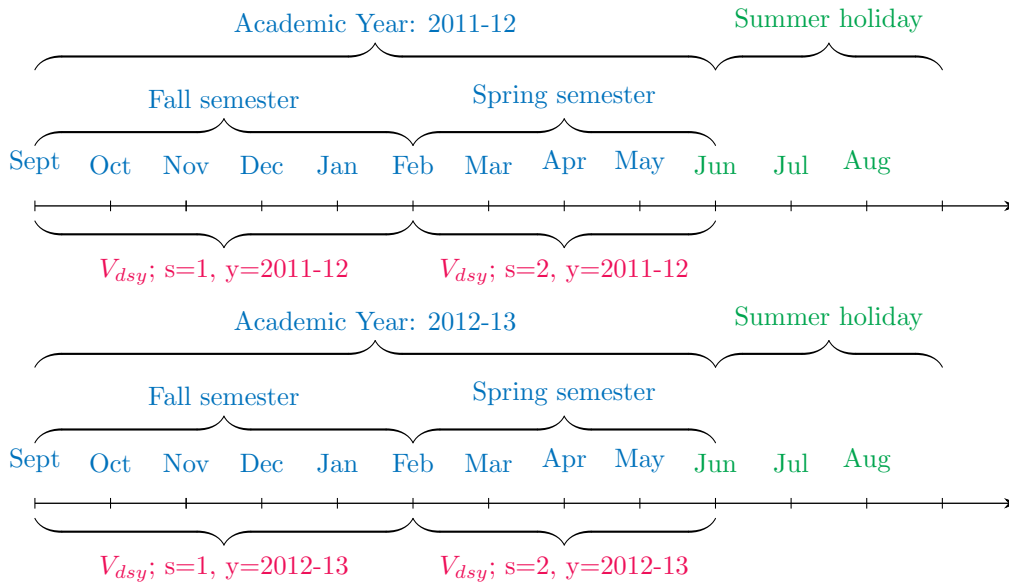
Notes. This table reports estimates of a specification where individual FE and classroom × grade × academic year FE are controlled. Compared with the main specification, years since arrival FE is not controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in columns (1)-(2) and *Log Math score* in columns (3)-(4). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester in logarithm. It is interacted with dummies indicating students' time in Türkiye is smaller than or equal to 1, or 3. The dummy on students' years since arrival is also included in the regressions (not reported). Total effect, which is the sum of the coefficient on the treatment variable and the coefficient on the interacted term, together with its *p*-value, is also reported. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table 10: Spillovers to Turkish students

	Log Turkish score (1)	Log Math score (2)
Panel A: All classroom-semester observations with at least one Syrian student		
Share of Syrian students	-0.2712*** (0.0920)	-0.3451*** (0.1104)
Weighted index on violent events	-0.0044*** (0.0014)	-0.0032** (0.0016)
Share of Syrian students \times Weighted index on violent events	0.0549*** (0.0202)	0.0654*** (0.0252)
R-squared	0.619	0.613
# of observations	27,812	27,812
# of clusters	1,967	1,967
Mean sample	72.78	67.77
Panel B: At least median number of Syrian refugees in the school-semester		
Share of Syrian students	-0.3880*** (0.1022)	-0.4687*** (0.1088)
Weighted index on violent events	-0.0067*** (0.0020)	-0.0042* (0.0023)
Share of Syrian students \times Weighted index on violent events	0.0788*** (0.0224)	0.0895*** (0.0249)
R-squared	0.552	0.547
# of observations	14,816	14,816
# of clusters	571	571
Mean sample	72.49	67.19
Panel C: Below median number of Syrian refugees in the school-semester		
Share of Syrian students	-0.1191 (0.1529)	-0.0159 (0.1778)
Weighted index on violent events	-0.0014 (0.0021)	0.0003 (0.0024)
Share of Syrian students \times Weighted index on violent events	0.0131 (0.0375)	-0.0001 (0.0438)
R-squared	0.720	0.716
# of observations	12,996	12,996
# of clusters	1,887	1,887
Mean sample	73.12	68.42
School FE	Yes	Yes
Grade FE	Yes	Yes
Academic year FE	Yes	Yes
Summer semester FE	Yes	Yes
Province \times academic year FE	Yes	Yes

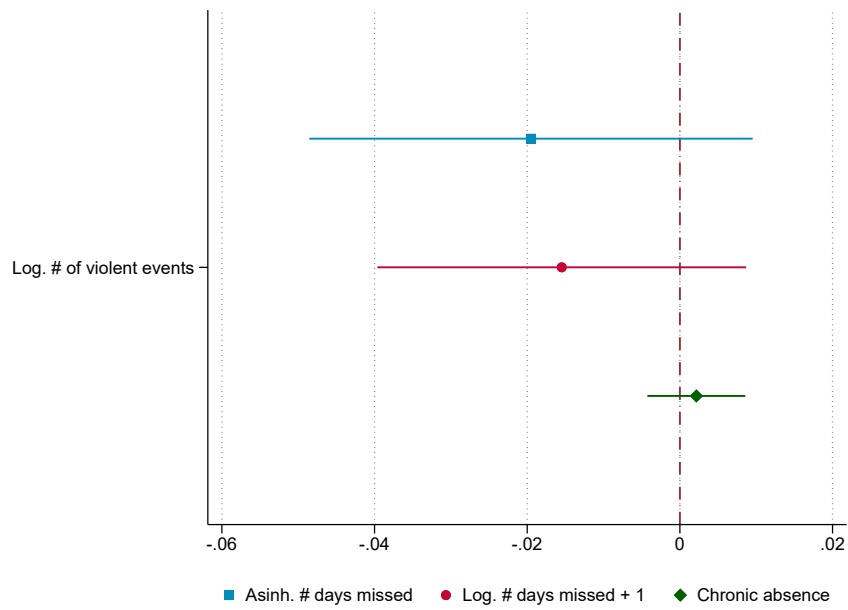
Notes. This table reports estimates of spillover effects. The sample is at classroom-semester level. The dependent variable is *Log Turkish score*, the logarithm of average Turkish scores among Turkish students, in column (1); and *Log Math score*, the logarithm of average math scores among Turkish students, in column (3). *Weighted index on violent events* is the weighted logarithm of the number of violent events in that classroom-semester, where weights are shares of Syrian students across districts. It is interacted with *Share of Syrian students*, which is the share of Syrian students in that classroom-semester. School FE, grade FE, academic year FE, whether the semester is in summer, and the interaction between province and summer semester FE are controlled. In panel A, all classroom-semester observations with at least one Syrian student are included in the regression. Panel B is the sub-samples of classrooms with at least the median number of Syrian students (i.e., 8 Syrian students) in that school-semester; while panel C is the sub-sample with below median number of Syrian students (i.e., 8 Syrian students). Standard errors are clustered at the school level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Figure 1: Treatment timing example



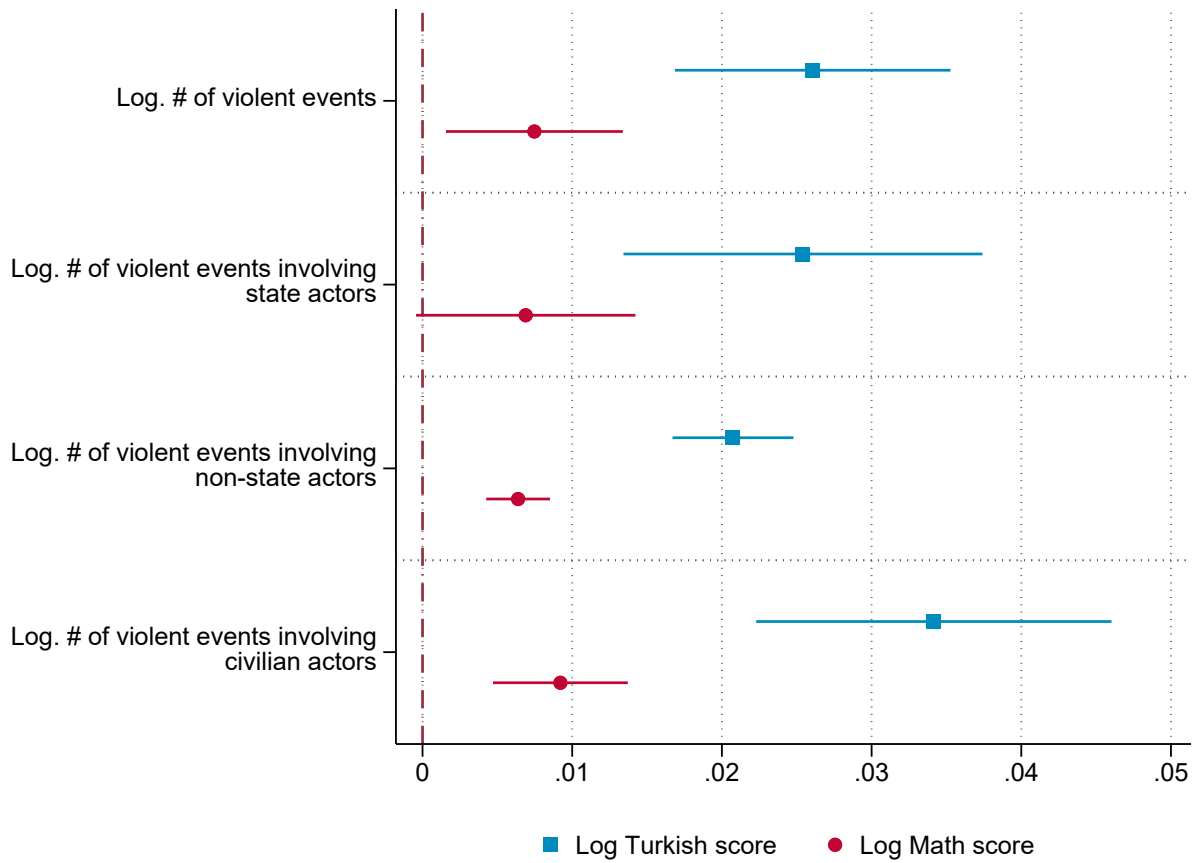
Notes. This figure visualizes how the treatment variable is constructed at the semester level, focusing on two of the academic years covered in the data. Here, V_{dsy} denotes the intensity of conflict in a student's Syrian district of birth d in semester s (or academic year y), measured by the number of violent events.

Figure 2: Effects of hometown conflicts on school absence



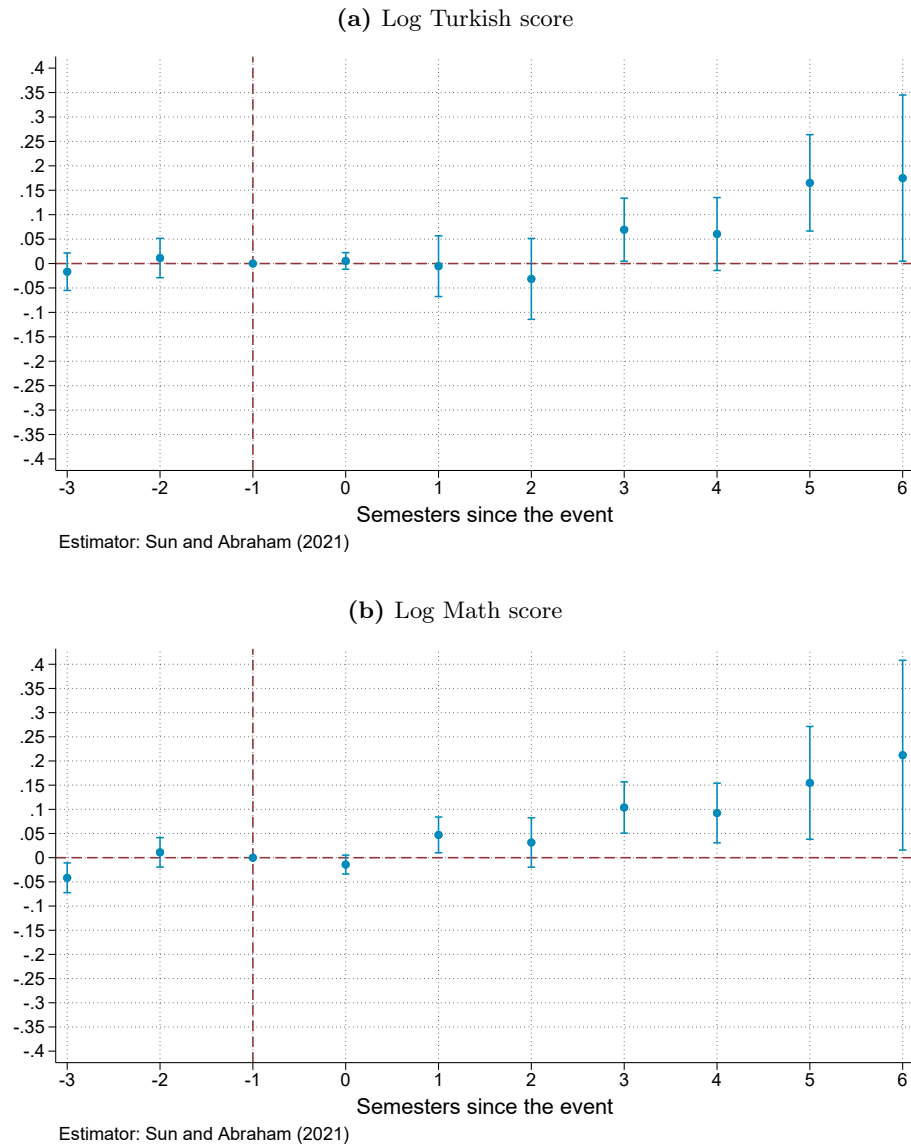
This figure reports point estimates and 95% confidence intervals from the main specification, which controls for individual fixed effects, years-since-arrival fixed effects, and classroom \times grade \times academic-year fixed effects. The sample is at the student-academic-year level. *Asinh. # days missed* denotes the inverse hyperbolic sine transformation of days missed. *Log # days missed + 1* denotes the logarithm of days missed plus 1. *Chronic absence* is an indicator equal to 1 if the student missed more than 20 days of school, corresponding to the 75th percentile of the sample distribution.

Figure 3: Heterogeneous effects on test scores, by perpetrator type



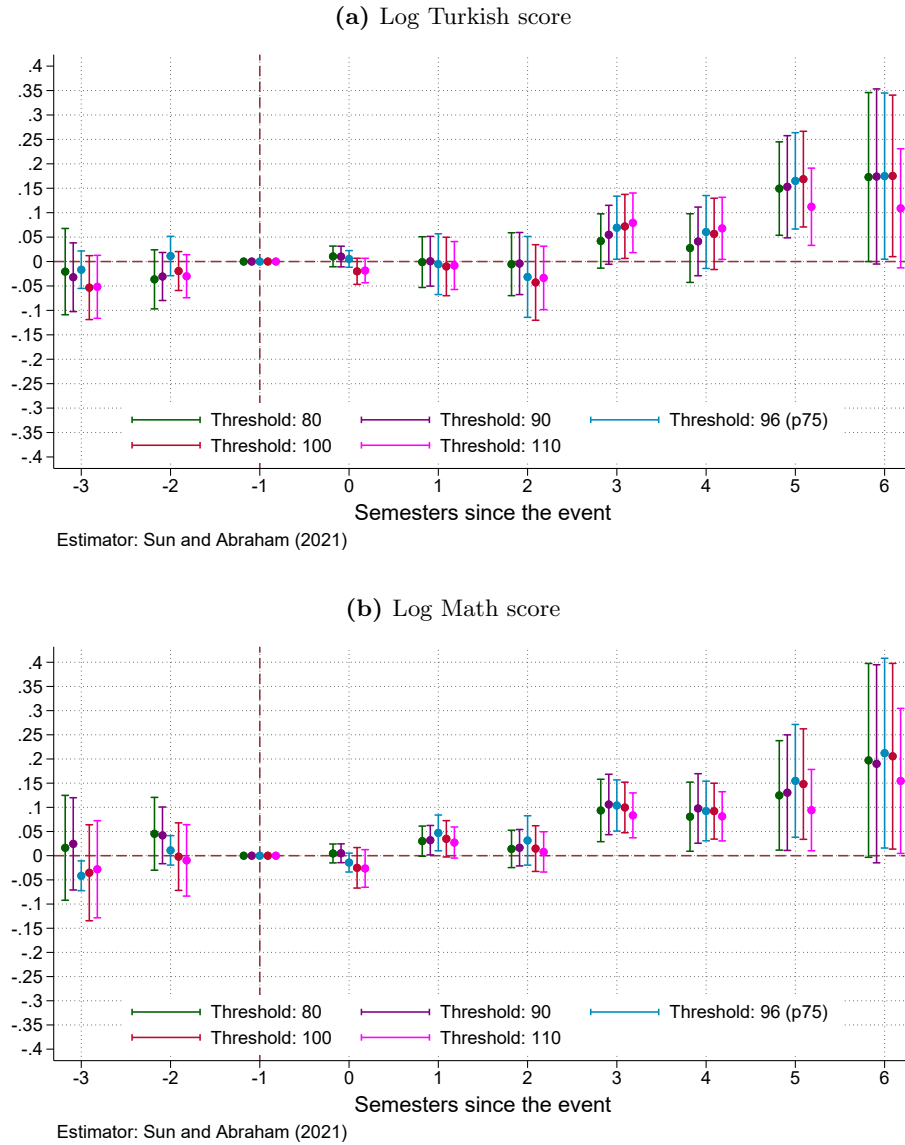
Notes. This figure reports point estimates together with the 95% confidence intervals of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. Standard errors are clustered at the district level. The dependent variable is *Log Turkish score* in blue and *Log Math score* in red. Beyond the main explanatory variables where all violent events are counted, three additional variables, where only conflicts involving state, non-state, or civilian actors are counted, are used as explanatory variables.

Figure 4: Effects of hometown conflicts on education outcomes in the event-study framework



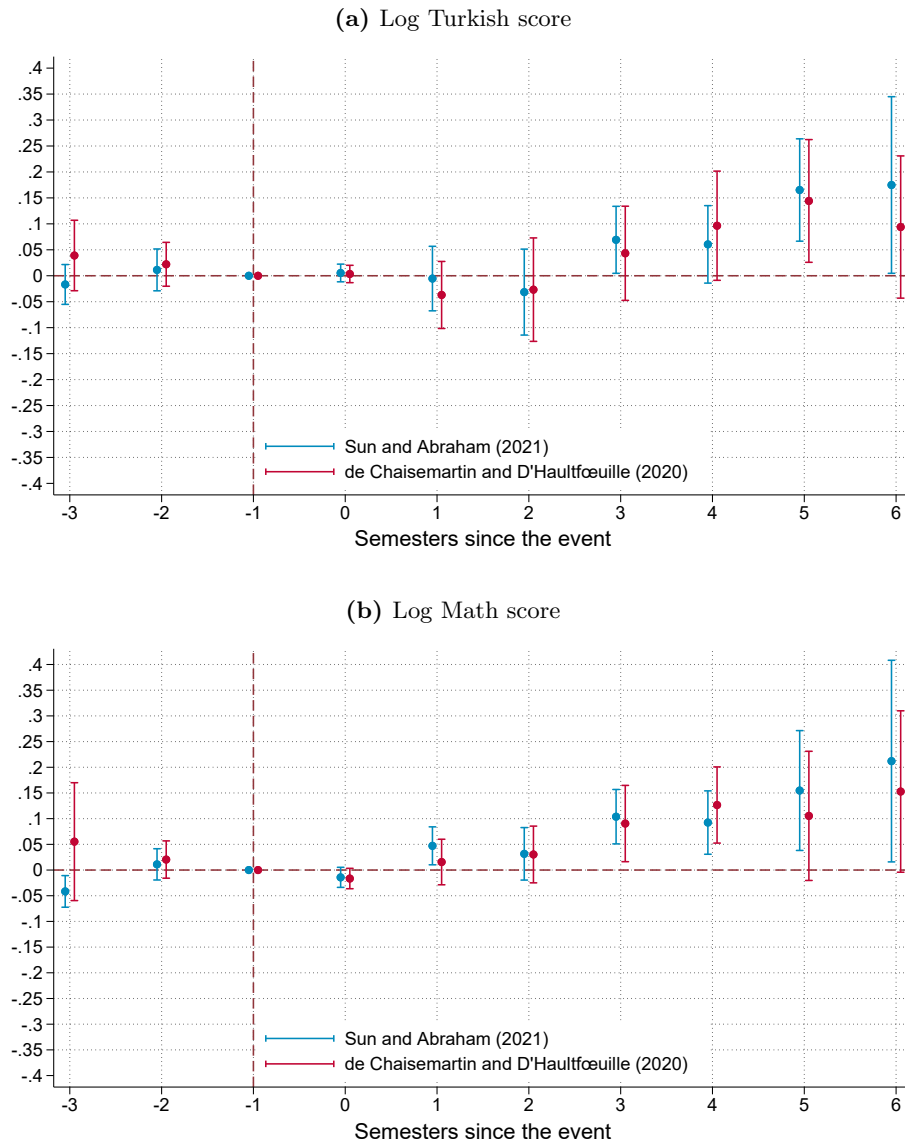
Notes. This figure reports event study coefficients together with the 95% confidence intervals using the estimator proposed by [Sun and Abraham \(2021\)](#), controlling for individual and semester fixed effects. The dependent variable is *Log Turkish score* in panel (a), and *Log Math score* in panel (b). The sample is at student-semester level. Standard errors are clustered at the district level. An event is defined as the first semester when students first experienced more than 96 (p75) violent events in their hometown.

Figure 5: Robustness in event studies: Different event definitions



Notes. This figure reports event study coefficients together with the 95% confidence intervals using the estimator proposed by Sun and Abraham (2021), controlling for individual and semester fixed effects. The dependent variable is *Log Turkish score* in panel (a), and *Log Math score* in panel (b). The sample is at student-semester level. Standard errors are clustered at the district level. An event is defined as the first semester when students experienced more than 80, 90, 96 (p75), 100, and 110 violent events in their hometown.

Figure 6: Robustness in event studies: Different estimators



Notes. This figure reports event study coefficients together with the 95% confidence intervals. Estimators included [Sun and Abraham \(2021\)](#); [De Chaisemartin and d'Haultfoeuille \(2020\)](#). Individual and semester fixed effects are controlled. The dependent variable is *Log Turkish score* in panel (a), and *Log Math score* in panel (b). The sample is at student-semester level. Standard errors are clustered at the district level. An event is defined as the first semester when students first experienced more than 96 (p75) violent events in their hometown.

A Appendix tables and figures

Table A1: Different fixed effects

	Log Turkish score				Log Math score			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log. # of violent events	0.0087 (0.0070)	0.0261*** (0.0046)	0.0235*** (0.0048)	0.0233*** (0.0049)	-0.0018 (0.0049)	0.0075** (0.0029)	0.0067** (0.0029)	0.0064** (0.0030)
<i>R</i> -squared	0.589	0.839	0.591	0.595	0.572	0.839	0.574	0.580
# of observations	53,973	53,973	53,973	53,973	53,973	53,973	53,973	53,973
# of clusters	44	44	44	44	44	44	44	44
Individual FE	No	Yes	No	No	No	Yes	No	No
Years since arrival FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Classroom-grade-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth district FE	No	No	Yes	Yes	No	No	Yes	Yes
Father education FE	No	No	No	Yes	No	No	No	Yes

Notes. This table reports coefficients with different fixed effects. The sample is at student-semester level. The dependent variable is *Log Turkish score* in columns (1)-(4) and *Log Math score* in column (5)-(8). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester in logarithm. Columns (2) and (6) report results from the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. In columns (1) and (5), individual FE are excluded. In columns (3) and (7), students' birth district FE replace the individual FE. In columns (4) and (8), students' birth district and father education FE replace the individual FE. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table A2: Robustness – ‘clean’ control and treated students

Panel A: Effects of high hometown conflicts, estimated on the full sample						
	Log Turkish score			Log Math score		
	(1)	(2)	(3)	(4)	(5)	(6)
# of violent events \geq 25th percentile	0.0264 (0.0175)			-0.0008 (0.0085)		
# of violent events \geq 50th percentile		0.0316*** (0.0056)			0.0044 (0.0035)	
# of violent events \geq 75th percentile			0.0344*** (0.0041)			0.0079* (0.0045)
<i>R</i> -squared	0.838	0.838	0.839	0.839	0.839	0.839
Mean sample	58.31	58.31	58.31	57.77	57.77	57.77
# of observations	53,973	53,973	53,973	53,973	53,973	53,973
# of students	17,857	17,857	17,857	17,857	17,857	17,857
Panel B: Effects of high hometown conflicts, estimated on the clean sample						
	Log Turkish score			Log Math score		
	(1)	(2)	(3)	(4)	(5)	(6)
# of violent events \geq 25th percentile	0.0558*** (0.0187)			0.0398*** (0.0070)		
# of violent events \geq 50th percentile		0.0583*** (0.0032)			0.0326*** (0.0021)	
# of violent events \geq 75th percentile			0.0405*** (0.0015)			0.0132*** (0.0020)
<i>R</i> -squared	0.825	0.826	0.836	0.834	0.832	0.840
Mean sample	57.45	57.19	57.50	56.90	56.43	56.87
# of observations	8,299	23,207	39,374	8,299	23,207	39,374
# of students	3,388	7,387	14,964	3,388	7,387	14,964
# of treated students	842	4,841	10,614	842	4,841	10,614
# of control students	2,546	2,546	4,350	2,546	2,546	4,350
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Years since arrival FE	Yes	Yes	Yes	Yes	Yes	Yes
Classroom-grade-year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in columns (1)-(3) and *Log Math score* in columns (4)-(6). The explanatory variables are all binary, indicating whether students' experienced hometown conflicts are greater than 25th, 50th, and 75th percentiles. In panel A, all Syrian students are included in the regressions; while in panel B, the regression sample includes only those ‘clean’ control and treated students in the sense that they are either never treated or they are treated only once and the treatment status is never turned off. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table A3: Robustness – Re-estimating the results on a fixed cohort of early arrivals

	Log Turkish score (1)	Log Math score (2)
Panel A: Arrived time is before or in the 2013/2014 academic year		
Log. # of violent events	0.0160** (0.0076)	0.0199*** (0.0064)
<i>R</i> -squared	0.886	0.877
# of observations	2,002	2,002
Panel B: Arrived time is before or in the 2014/2015 academic year		
Log. # of violent events	0.0145* (0.0078)	0.0139** (0.0054)
<i>R</i> -squared	0.894	0.889
# of observations	3,227	3,227
Individual FE	Yes	Yes
Years since arrival FE	Yes	Yes
Classroom-grade-year FE	Yes	Yes

Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in column (1) and *Log Math score* in column (2). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester in logarithm. In panel A, only those refugee students who arrived in Türkiye before or in the 2013/2014 academic year are included in the regression. In panel B, only those refugee students who arrived in Türkiye before or in the 2014/2015 academic year are included in the regression. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table A4: Robustness – different parameterizations other than log-log transformation

Panel A: IHS transformation on violence exposure		
	Log Turkish score (1)	Log Math score (2)
asinh(# of violent events)	0.0245*** (0.0049)	0.0070** (0.0029)
<i>R</i> -squared	0.839	0.839
# of observations	53,973	53,973
Panel B: Dropping observations with zero violence event		
	Turkish score (1)	Math score (2)
Log(# of violent events + 1)	1.3882*** (0.2197)	0.3594*** (0.1048)
<i>R</i> -squared	0.891	0.894
# of observations	52,415	52,415
Panel C: Dropping observations with zero violence event, real logarithm transformation		
	Turkish score (1)	Math score (2)
Log(# of violent events)	1.3156*** (0.2281)	0.3329*** (0.1096)
<i>R</i> -squared	0.891	0.894
# of observations	52,415	52,415
Panel D: Using raw test scores as outcome variables		
	Turkish score (1)	Math score (2)
Log(# of violent events + 1)	1.3343*** (0.2446)	0.3458*** (0.1069)
<i>R</i> -squared	0.890	0.894
# of observations	53,973	53,973
Individual FE	Yes	Yes
Years since arrival FE	Yes	Yes
Classroom-grade-year FE	Yes	Yes

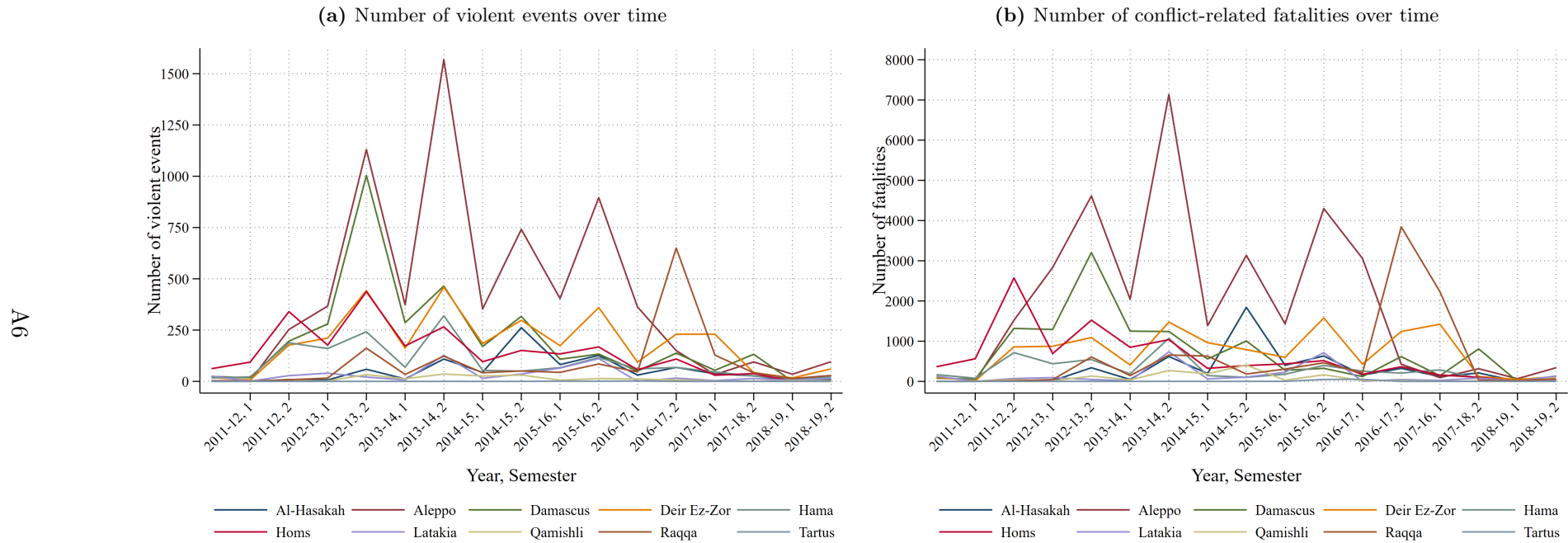
Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in column (1) and *Log Math score* in column (2) for panels A-C, and is raw test scores in panel D. Some Syria districts have zero violent event in certain semesters, and to accommodate these observations (around 3% of observations in the regression sample), the treatment variable is actually defined as *Log(# of violent events + 1)* in main specifications. In panel A, the IHS transformation is applied. In panel B, those observations with zero violent event are dropped. In panel C, in addition to dropping these observations, the regressor is re-defined as *Log(# of violent events)*. In panel D, the outcome variable is raw test scores in the scale of 0 to 100. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

Table A5: Robustness – different standard errors

	Log Turkish score (1)	Log Math score (2)
Panel A: District level bootstrapping		
Log. # of violent events	0.0261*** (0.0062)	0.0075** (0.0031)
<i>R</i> -squared	0.839	0.839
# of observations	53,973	53,973
# of clusters	44	44
Panel B: District and academic year level clustering		
Log. # of violent events	0.0261*** (0.0079)	0.0075* (0.0041)
<i>R</i> -squared	0.839	0.839
# of observations	53,973	53,973
# of clusters	219	219
Panel C: District and academic year level bootstrapping		
Log. # of violent events	0.0261*** (0.0079)	0.0075* (0.0039)
<i>R</i> -squared	0.839	0.839
# of observations	53,973	53,973
# of clusters	219	219
Individual FE	Yes	Yes
Years since arrival FE	Yes	Yes
Classroom-grade-year FE	Yes	Yes

Notes. This table reports estimates of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. The dependent variable is *Log Turkish score* in column (1) and *Log Math score* in column (2). The treatment variable, *Log. # of violent events*, is the number of violent events at the district-semester in logarithm. Panel A reports cluster bootstrap standard errors at the district level. Panel B reports clustered standard errors at the district-academic year level. Panel C reports cluster bootstrap standard errors at the district-academic year level. Standard errors are clustered at the district level. ***, **, and * denote significance levels at the 1%, 5%, and 10%, respectively.

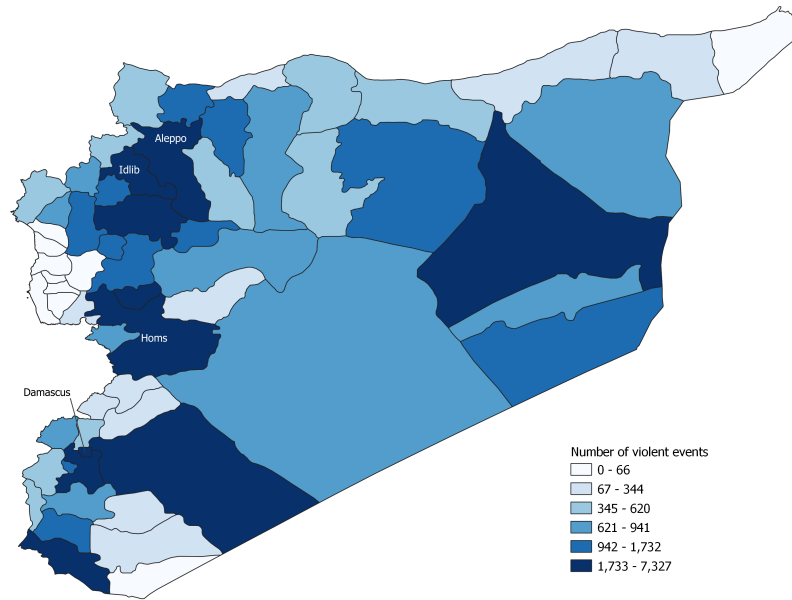
Figure A1: Temporal variation of violence



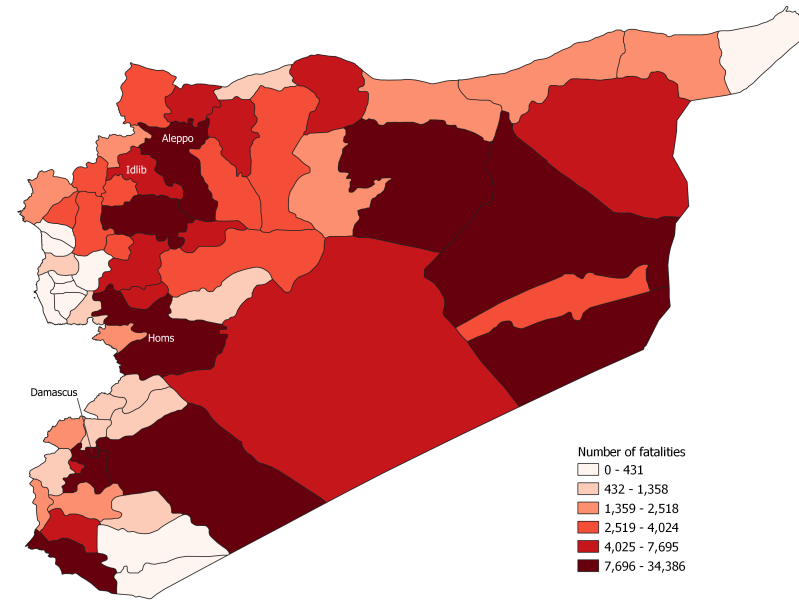
Notes. Panel (a) shows the number of violent events that occurred over time at the district level for our sample period. Panel (b) shows the number of conflict-related fatalities over time at the district level for our sample period. On the *x*-axis, the academic year is followed by the semester; “2011-12, 1” and “2011-12, 2” indicate the fall and spring semesters, respectively. For visual clarity, we focus on the most populated origin districts based on population figures from the 2004 official census.

Figure A2: Regional variation in cumulative violence

(a) Cumulative number of violent events

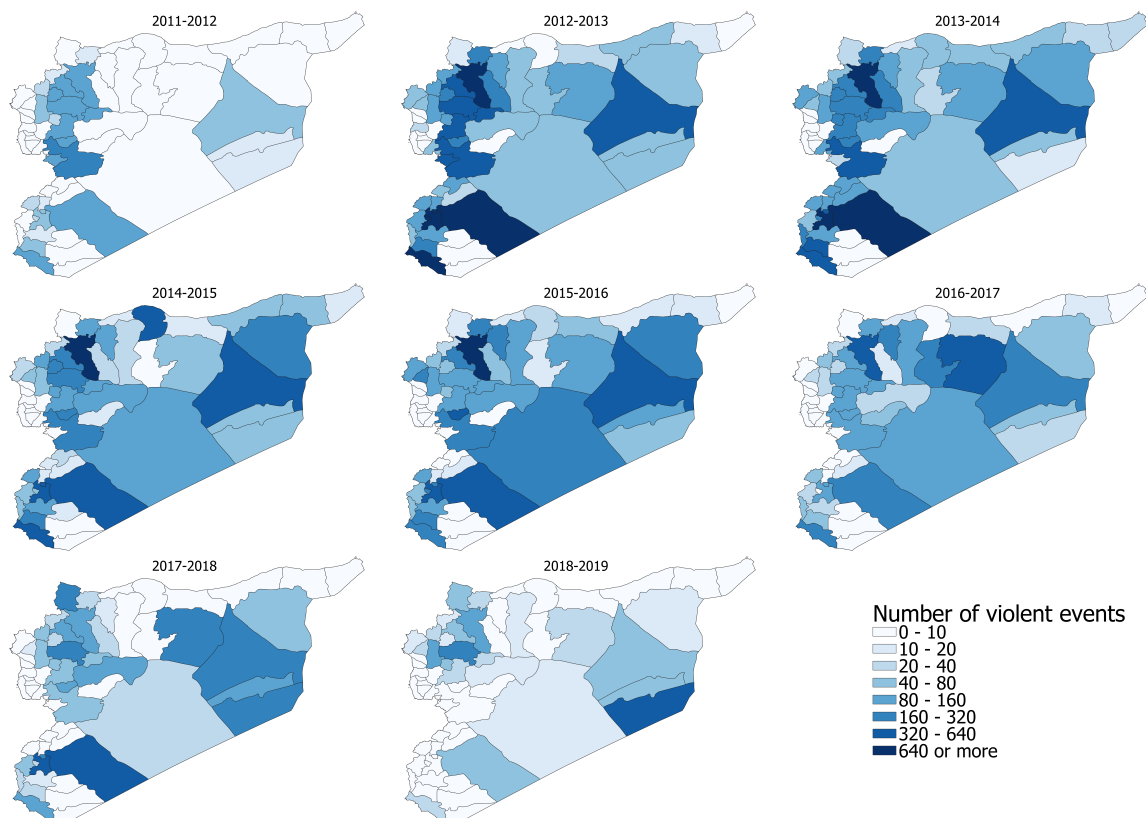


(b) Cumulative number of fatalities



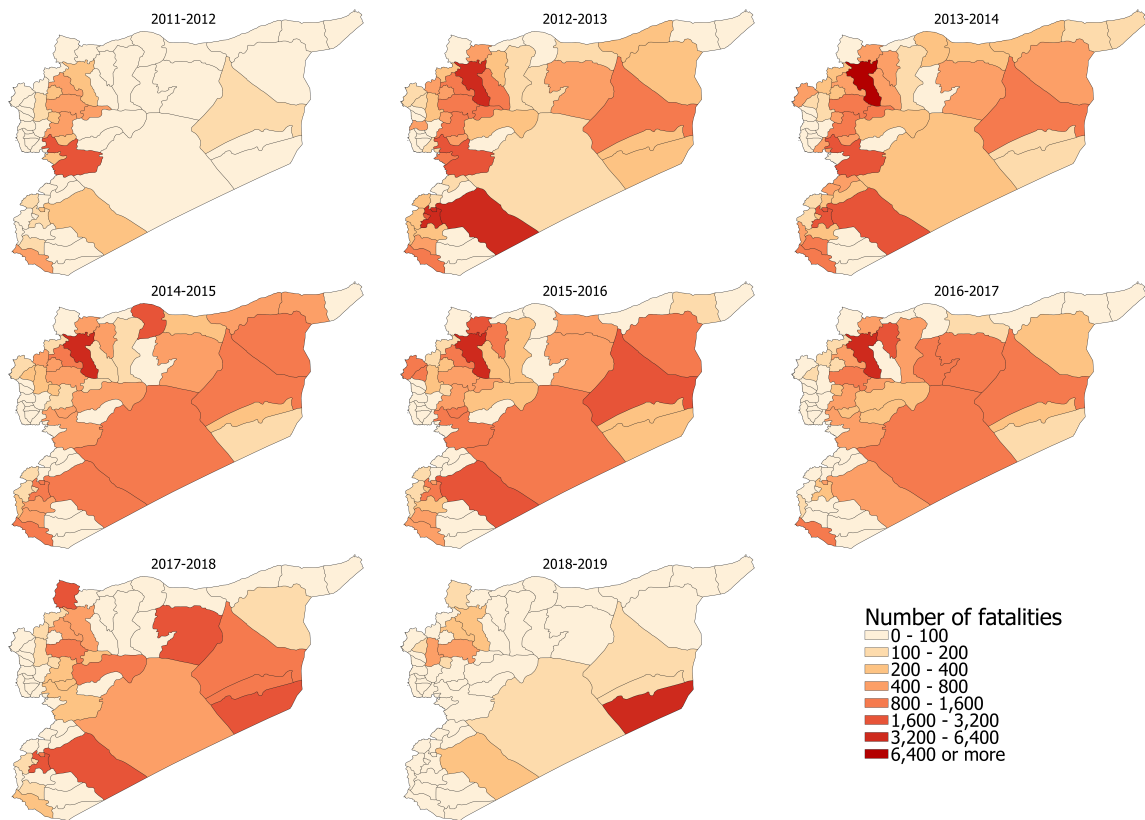
Notes. This figure shows cumulative violence during the academic years from 2011–2012 to 2018–2019. Panel A and Panel B correspond to the number of violent events and fatalities, respectively. Syria is divided into 65 districts (*manatiq*), including the city of Damascus, and these districts are nested within the country's 14 governorates (*muhafazat*).

Figure A3: Significant variation in violent events across regions and over time



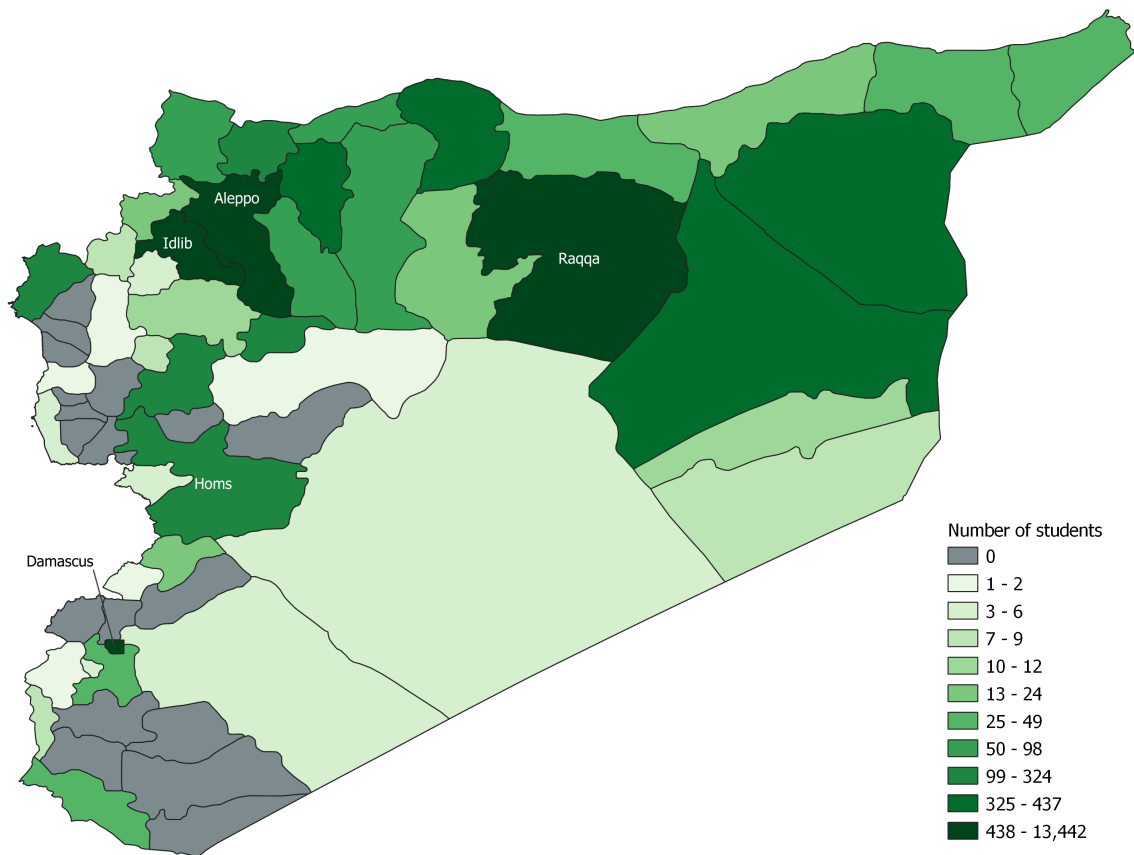
Notes: These maps show the number of violent events at the district level for the academic years from 2011-2012 to 2018-2019. Syria is divided into 65 districts (*manatiq*), including the city of Damascus, and these districts are nested within the country's 14 governorates (*muhafazat*).

Figure A4: Significant variation in fatalities across regions and over time



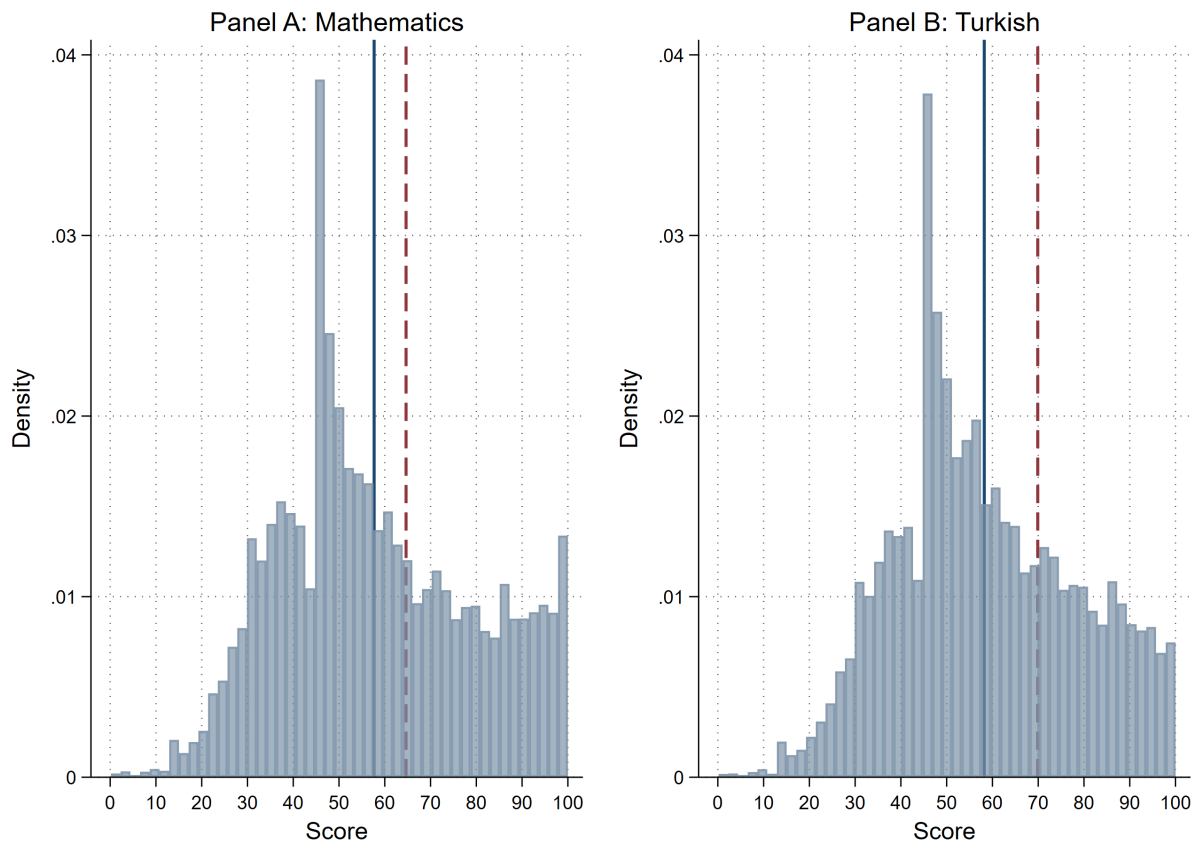
Notes: These maps show the number of conflict-related fatalities at the district level for the academic years from 2011-2012 to 2018-2019. Syria is divided into 65 districts (*manatiq*), including the city of Damascus, and these districts are nested within the country's 14 governorates (*muhafazat*).

Figure A5: Distribution of students based on district of birth



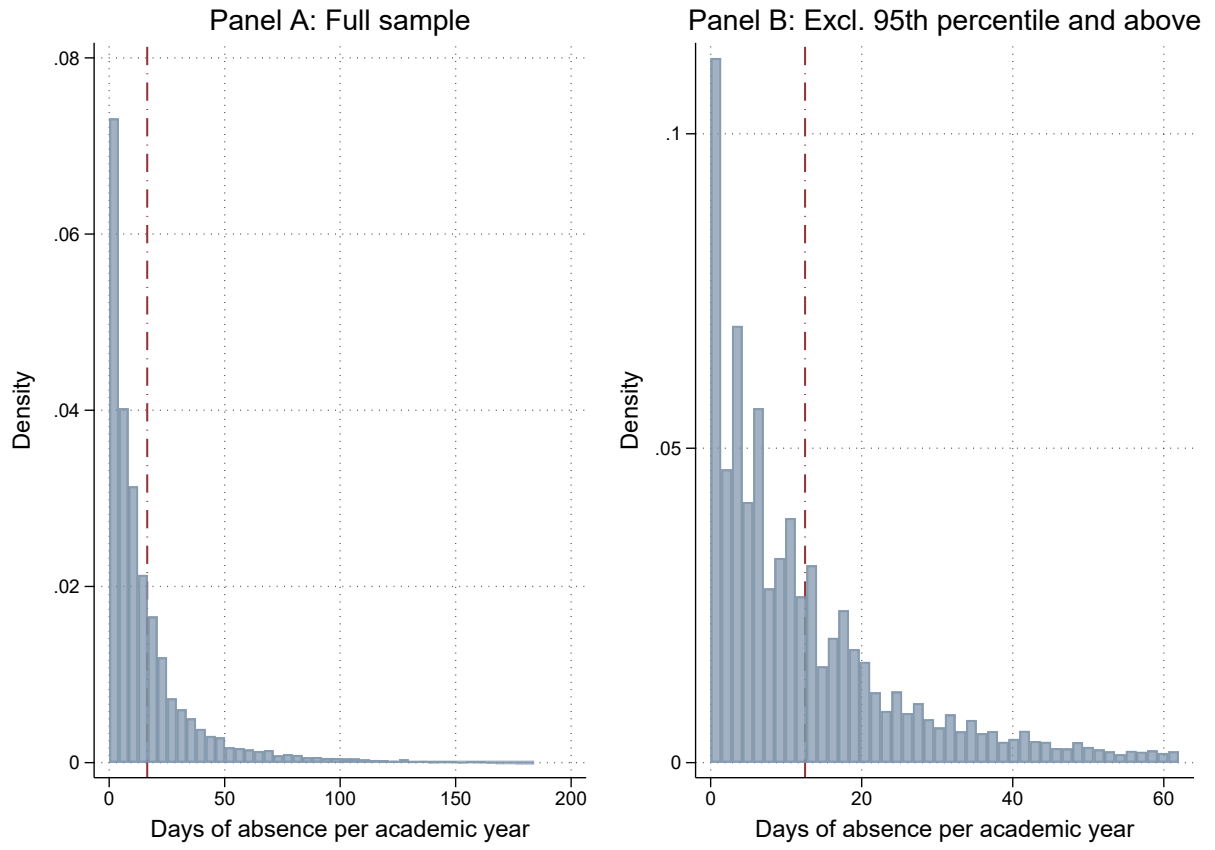
Notes: Figure shows the distribution of Syrian students according to their district of birth. District boundaries are indicated using thin black lines. Syria is divided into 65 districts (*manatiq*), including the city of Damascus, and these districts are nested within the country's 14 governorates (*muhafazat*).

Figure A6: Turkish language and Math scores among Syrian students



Notes: This figure shows the distribution of Math and Turkish language scores in Panel A and Panel B, respectively. Sample means are indicated by the blue solid lines and red dashed lines for Syrian and Turkish students, respectively. The sample is the semester-level data and corresponds to academic years between 2011-2012 and 2018-2019.

Figure A7: The number of school days missed among Syrian students



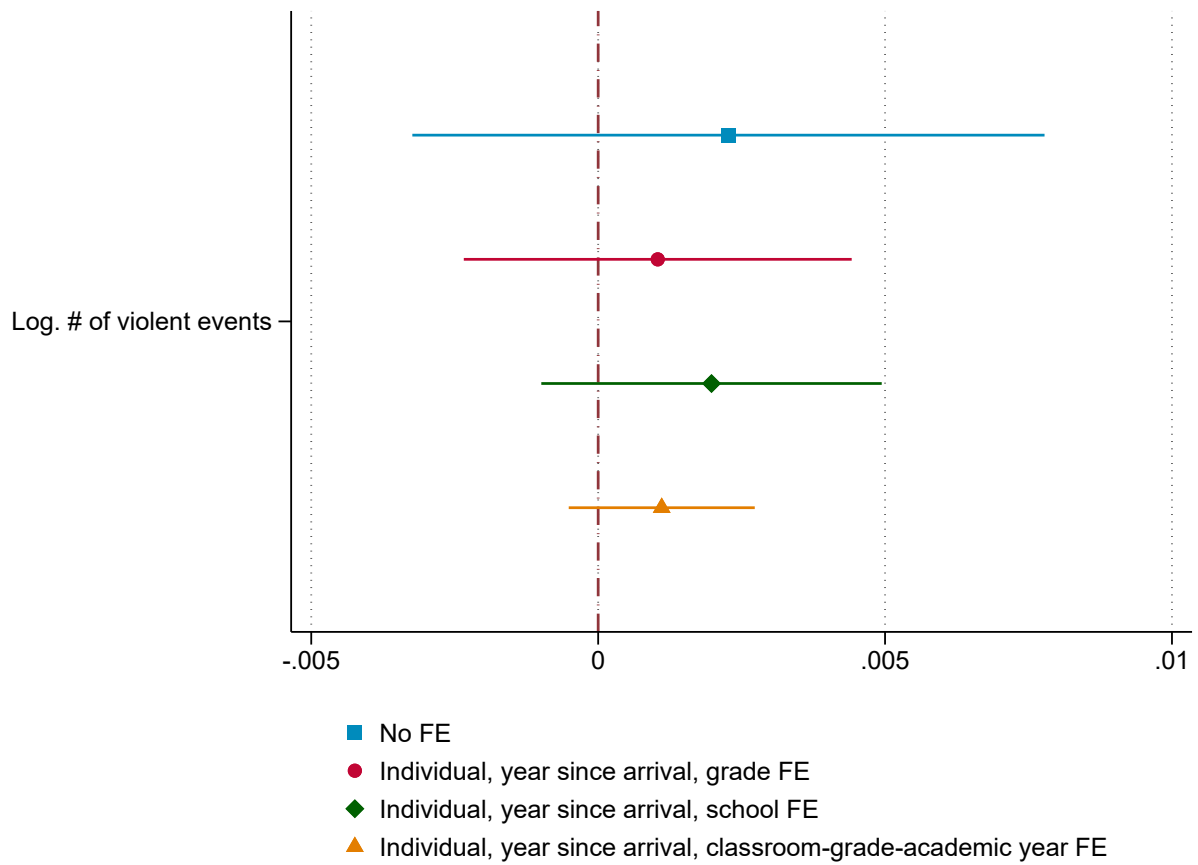
Notes. This figure shows the distribution of the number of school days missed per school year. Panel A is the full sample, while Panel B shows observations below the 95th percentile. Sample means for Turkish students are indicated by the red dashed lines. The sample is at student-academic year level. In panel A, the mean is 16.45 and median is 9; in panel B, the mean is 12.50 and median is 8.5.

Figure A8: Students from the same hometown tend to disperse into different schools



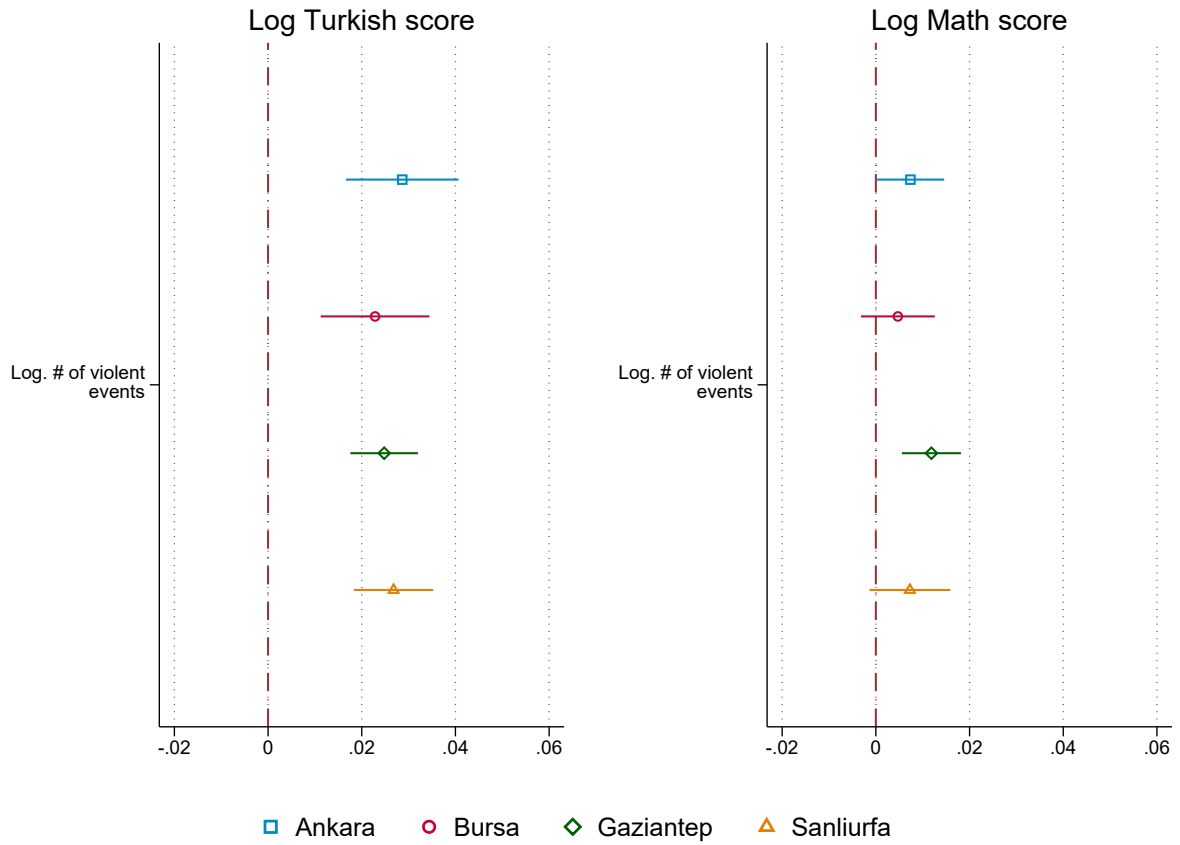
Notes. This figure shows the extent to which students from the same hometown disperse into different schools. Each plot focuses on students from one and the same hometown, within each plot, cells represent a distinct school and cell sizes are proportional to the number of students that attend the same school out of all students from the same hometown. For visual clarity, we focus on the most populated origin districts based on population figures from the 2004 official census. Syria is divided into 65 districts (*manatiq*), including the city of Damascus, and these districts are nested within the country's 14 governorates.

Figure A9: Effects of hometown conflicts on dropout



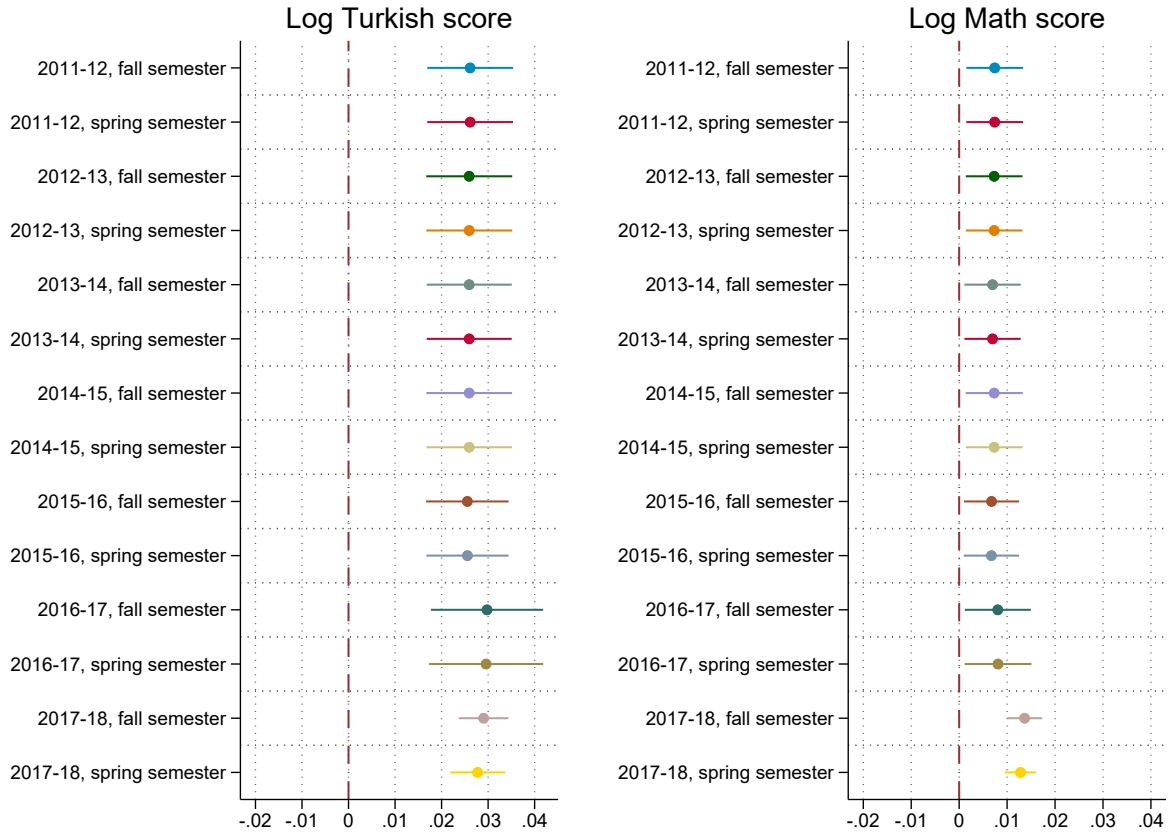
Notes. This figure reports point estimates together with the 95% confidence intervals with different fixed effects as specified in the legend. The sample is at student-semester level. The dependent variable, *Dropout*, equals 1 if either Math or Turkish scores are missing and the grade is less than or equal to 10, and 0 otherwise. Standard errors are clustered at the district level.

Figure A10: Robustness – Leaving out one (Turkish) province at a time



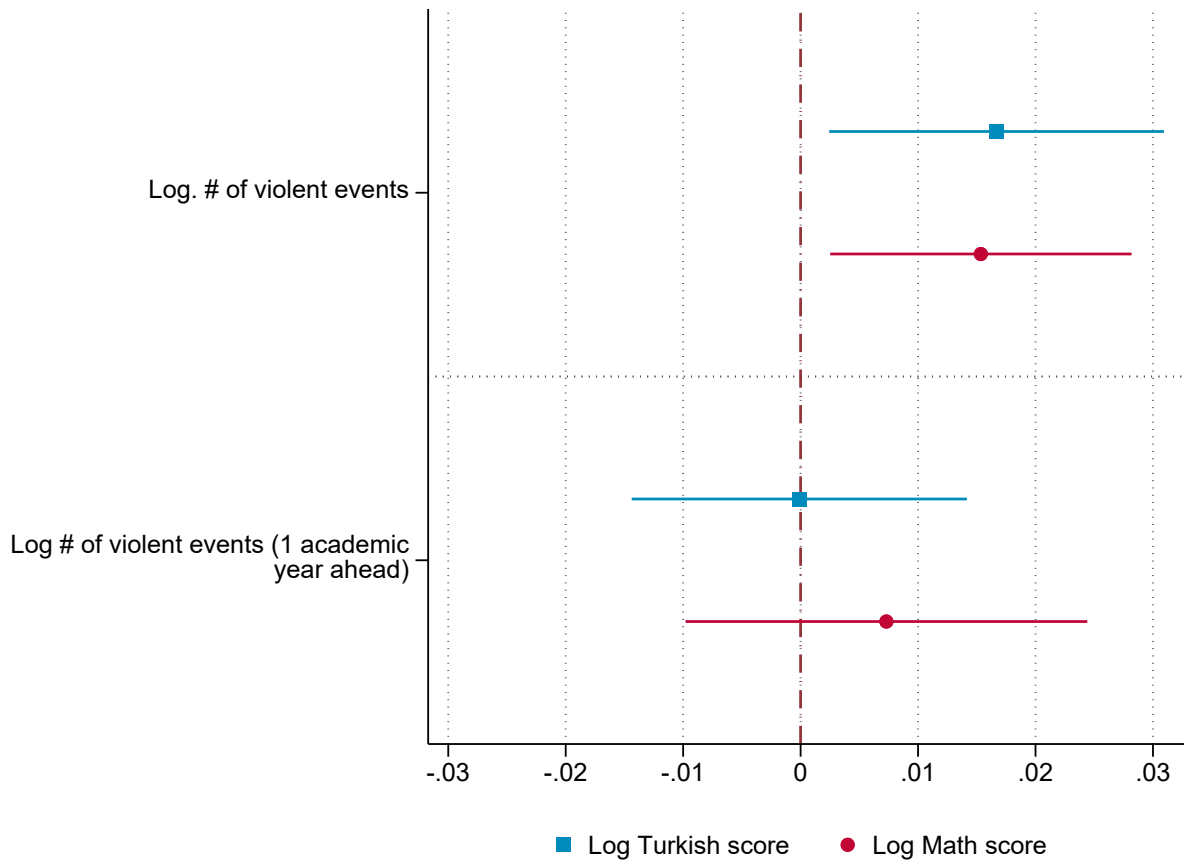
Notes. This figure reports point estimates together with the 95% confidence intervals of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. Each coefficient is estimated on a sub-sample of the data where one of the four provinces, namely, Ankara (remaining number of observations, $N = 47,823$), Bursa ($N = 38,519$), Gaziantep ($N = 27,990$), and Şanlıurfa ($N = 47,587$) is dropped. Standard errors are clustered at the district level. The dependent variable is *Log Turkish score* on the left panel and *Log Math score* on the right panel.

Figure A11: Robustness – Leaving out one semester at a time



Notes. This figure reports point estimates together with the 95% confidence intervals of the main specification, where individual FE, years since arrival FE, and classroom \times grade \times academic year FE are controlled. The sample is at student-semester level. Each point is estimated on a sub-sample of the data where the semester indicated on the vertical axis has been dropped. Standard errors are clustered at the district level. The dependent variable is *Log Turkish score* on the left panel and *Log Math score* on the right panel.

Figure A12: Null effects of treatment leads



This figure reports point estimates together with 90% confidence intervals from the main specification, which controls for individual fixed effects, years-since-arrival fixed effects, and classroom \times grade \times academic-year fixed effects. The sample is at the student-semester level and is restricted to students observed for at least seven semesters. Standard errors are clustered at the district level. The dependent variable is *Log Turkish score* in blue and *Log Math score* in red. The lead of the treatment is the logarithm of the number of violent events in the spring or fall semester one academic year ahead.