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Schooling Under Threat: Intra-Camp Insecurity and School Attendance among Rohingya Children

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Abstract

Quantitative evidence on violence inside refugee camps and children's schooling is scarce, partly because camp incidents are under-recorded in media-based event datasets. Nor is the direction of the relationship evident a priori: violence raises the risk of the journey to school, but also the risks attached to the alternative uses of a child's time, and the balance shifts with age and gender. This paper links two rounds of the Cox's Bazar Panel Survey of Rohingya refugee children to camp-level protection incidents recorded by UNHCR's Joint Protection Monitoring exercise. Exploiting within-household variation across rounds, one additional incident in a child's camp is associated with a 0.35-percentage-point decline in attendance. That average masks a reversal of sign, and it is the variation rather than the average that the data establish: equality of the association across age bands is rejected decisively. Among children aged seven to eight the estimate is positive; the association is null at nine to ten, negative from eleven, and reaches -1.41 points at thirteen to fourteen, a difference of 1.83 points that survives correction for multiple comparisons. The association is precisely estimated among girls and among households with no employed member. The reasons households give for withdrawal divide along the same lines: withdrawal attributed to a restriction imposed by the household rises with violence for girls and falls for boys, a differential that survives familywise correction and emerges only at the ages at which restriction is expected to bind. A placebo on the pre-escalation period returns coefficients indistinguishable from zero. The association between violence and schooling in protracted displacement is not uniform, and the children whose attendance moves with it are identifiable by age, sex and household employment.

Keywords: Refugees; Violence; Education; Gender; Rohingya
JEL codes: I21, I25, O15, D74

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1 Introduction

Conflict disrupts human capital accumulation well beyond the destruction of schools and learning materials: through psychological trauma, household income shocks, displacement, and a persistent insecurity that makes attendance costly or dispensable (Justino et al., 2014; Verwimp & Van Bavel, 2014; Vesco et al., 2025). In displacement settings, where educational infrastructure is fragile and household resources are severely constrained, exposure to violence may disrupt attendance further still.

The scale of the problem is large. More than 117 million people were forcibly displaced worldwide at the end of 2025 (UNHCR, 2026). Among refugees, 46 per cent of the 12.4 million children of school age are out of school: the gross enrolment rate is 67 per cent at primary level, against 102 per cent in low-income countries and 94 per cent in fragile and conflict-affected states, and falls to 37 per cent at secondary level (UNHCR, 2025b). The gap widens in protracted displacement, where refugees may remain for decades without durable solutions (World Bank, 2023). Refugee children face barriers that host-community children do not: overcrowded learning spaces, few qualified teachers, an unfamiliar language of instruction, the psychosocial legacy of displacement (Kaplan et al., 2016; Margolin & Gordis, 2000), and household poverty that can make child labour or early marriage the binding alternative to school (Al-Hroub, 2014). These barriers explain the level of enrolment; they do not explain how enrolment responds to violence, and the camp is an unusually clean setting in which to observe that response.

When insecurity rises around a household in a refugee camp, the direction in which the decision about a child's schooling should move is not obvious. Attending a learning center means leaving the shelter and, above primary level, crossing between blocks, so violence raises the cost of going. It also raises the cost of staying: the learning center places the child among adults present in a professional capacity, whereas supervision at the shelter falls back on a household whose capacity to provide it is most stretched when insecurity is highest. And violence compresses household income, raising the value of a child's time – but only of a child old enough to earn.

Which force binds depends on what the alternative to school is. For a young child it is unsupervised time around the shelter, so supervision is the only channel with purchase. For an adolescent girl the alternative is the shelter as the safer place, and the household's protective response – restricting her movement – withholds her from school altogether. For an adolescent boy it is casual work, which the short double shifts of the learning centers accommodate, and which compresses attendance rather than ending it. Attendance does not register these forces symmetrically: it responds to restriction while remaining comparatively insensitive to economic pressure.

The existing evidence is consistent with this reading but has not been organized around it. The direction is predominantly negative – in Tajikistan (Shemyakina, 2011), Burundi (Verwimp & Van Bavel, 2014), Timor-Leste (Justino et al., 2014), Nigeria (Bertoni et al., 2019), Colombia (Rodríguez & Sánchez, 2012) and Mexico (Brown & Velásquez, 2017) – while the cross-country evidence establishes context dependence rather than a magnitude (Unfried & Kis-Katos, 2023; Vesco et al., 2025). Departures from that pattern are not rare: Israeli closures raised child labour in the West Bank while reducing attendance only weakly at ages ten to fourteen (Di Maio & Nandi, 2013), and girls' attainment in Nepal rose during the Maoist insurgency, except where the insurgents stepped up abductions (Valente, 2014).

The closest thing to a regularity is that the negative effects concentrate among older children. Rodríguez and Sánchez (2012) estimate a reduction in schooling of 8.8 per cent for Colombian children aged six to seventeen, but of 17.0 per cent at sixteen to seventeen. Brown and Velásquez (2017) find that Mexican boys aged fourteen to seventeen lost years of schooling and were less likely to complete compulsory education, with no corresponding effect at ten to thirteen. Swee (2015) finds that the Bosnian war depressed secondary but not primary attainment, the primary null plausibly owing to war schools, which sustained a standardized curriculum where secondary provision could not. Shemyakina (2011) finds a significant effect for Tajik girls and little or none for boys. The explanations offered – rising opportunity cost, longer and less secure journeys, recruitment – are all increasing in age. The exception is instructive: where belligerents target children directly, enrolment falls at school entry itself (Haer et al., 2026). What this literature has not done is treat the age profile as the object of inference rather than as a heterogeneity check, so the small effect among younger children has been read as the absence of a response rather than as a possible channel of opposite sign.

On sex there is no regularity. Buvinić, Das Gupta and Shemyakina (2014) conclude that the direction of the differential is context-specific, shaped by pre-war gender gaps in attainment and by the opportunities that would have obtained in the absence of war. Girls fare worse in Tajikistan (Shemyakina, 2011), in Punjab (Singh & Shemyakina, 2016) and where tactics target minors (Haer et al., 2026); boys fare worse in Bosnia (Swee, 2015), Timor-Leste (Justino et al., 2014), São Paulo (Koppensteiner & Menezes, 2021), Mexico (Brown & Velásquez, 2017) and the Tigray war (Demeke & Wie, 2025). Ben Salah and Saxena (2025) find no gender differential in grade-six proficiency across ten sub-Saharan countries, but girls' attendance falls, so their null holds only among those still enrolled. The divergence is informative rather than inconclusive: the settings in which boys lose more are those in which supply-side explanations are ruled out directly and the response is traced to household finances (Koppensteiner & Menezes, 2021; Brown & Velásquez, 2017), which is the channel expected to bind on boys.

Whether low-intensity violence matters at all is likewise unsettled. Unfried and

Kis-Katos (2023) find that high-intensity conflict depresses attainment while localized low-intensity conflict does not, and that boys lose substantially more at high intensity; Ben Salah and Saxena (2025) report effects only from events threatening life or property. Haer et al. (2026), by contrast, find enrolment effects from proximity alone, and Koppensteiner and Menezes (2021) find effects on test scores in low-homicide neighbourhoods but none in high-homicide ones, where violence is routine rather than novel. Violence in the camps is of the second kind: routine rather than novel, and chronic.

This paper examines the Rohingya refugee camps of Cox's Bazar, Bangladesh, the world's largest and most densely populated refugee settlement, where the displacement that escalated in August 2017 now affects more than one million refugees (UNICEF, 2026). Two rounds of the World Bank Cox's Bazar Panel Survey are linked to camp-level records of reported protection incidents from the UNHCR Joint Protection Monitoring exercise. The source is generated by actors physically present in each camp, and it therefore captures intra-camp violence that the media-based datasets on which the conflict literature relies record less consistently. Coverage is not the only difference. ACLED (Raleigh et al., 2010) geocodes events occurring inside these camps to the centroids of the host-community settlements that administer them, so camp events are displaced by distances of the same order as the camps themselves – a scope condition of the dataset rather than a defect, but one that leaves buffer-based exposure measures and distance-based instruments unidentified in camp settings.

Attendance is compared within households across the two rounds, against differences between camps in recorded violence over the interval, with household and round fixed effects. Standard errors are clustered at the primary sampling unit, the level at which the survey sample was drawn; because exposure varies by camp, inference at camp level, based on the wild cluster bootstrap, is reported alongside. What is reported is a conditional association rather than a causal effect: with two rounds parallel trends cannot be tested within the estimating window, and no credible instrument was available.

One additional recorded incident in a child's camp is associated with a fall of 0.35 percentage points in the probability of attendance. That figure conceals more than it conveys, and it is the variation around it that the data establish: equality of the association across age bands is rejected at $F(4, 120) = 12.49$. The association is positive among children aged seven and eight – and since every child in the sample was attending at baseline, positive means retained rather than newly enrolled – indistinguishable from zero at nine and ten, and negative from eleven onward, reaching -1.41 percentage points at thirteen and fourteen. The difference between the youngest band and that one, 1.83 points, survives correction for multiple comparisons. The association is estimated with precision among girls and not among boys, though the difference between the two is not itself established ($p = 0.113$), and

it is concentrated among households with no employed adult member. The reversal at the youngest ages is the least anticipated of these results. Its sign is the one the supervision channel would predict, though attendance data alone cannot establish that supervision is what produces it.

The survey also records why a household says a child stopped attending, and the stated reasons divide more sharply than the attendance estimates. Withdrawal reported as a restriction imposed by the household rises with violence for girls and falls for boys; the differential survives correction across the five stated reasons, and it is absent among the youngest children and present from the ages at which mobility norms begin to bind. For boys the reported margin is economic and strengthens through adolescence, though that pattern is a direction the estimates display rather than an established differential. The raw counts show the division before any model is fitted: almost every withdrawal attributed to familial and social restriction concerns a girl. These reports are not a substitute for identification, and the reasons they record are interdependent rather than separate channels; but they are elicited independently of the exposure measure, and they are read here as suggestive evidence on the mechanisms at work.

Two main contributions follow. First, the aggregate relationship between violence and refugee children's schooling appears to be not a weak effect but a composite of responses of opposite sign, small in the pooled estimate because its components offset; null results recurring in this literature may therefore be artefacts of aggregation rather than evidence that violence is inconsequential. The camp makes the decomposition legible: educational provision does not vary substantially with violence across camps, so what is isolated is the household's allocation of the child's time, in a literature where supply and demand are ordinarily confounded (Vesco et al., 2025; Ben Salah & Saxena, 2025). Second, the paper locates the estimate on chronic, low-intensity insecurity, a form of violence this literature has largely found inert (Unfried & Kis-Katos, 2023; Koppensteiner & Menezes, 2021), recorded here by monitors present in each camp rather than in the media-based event data on which that null rests. The reported reasons for withdrawal then associate each channel with the group the framework predicts, in a population for which quantitative evidence on this margin is scarce (Dryden-Peterson, 2011, 2016; World Bank, 2023).

The distributional finding has an operational counterpart. Programming is ordinarily delivered uniformly across the school-age population, whereas the response is concentrated among children entering adolescence, among girls, and among households with no employed member – groups that can be reached through safe access to learning facilities, provision closer to dwellings, and attendance-conditional incentives calibrated to close rather than reproduce the gender gap. For the youngest children the learning center appears to function as a protective option, so keeping it open during periods of heightened insecurity is protection as well as education.

The children concerned are identifiable in advance.

The paper is organized as follows. Section 2 presents the theoretical framework. Section 3 describes the context of Cox's Bazar. Section 4 introduces the data and the empirical strategy. Section 5 reports the main results, the heterogeneity analysis, the robustness checks, and suggestive evidence on potential mechanisms. Section 6 concludes with policy implications.

2 Theoretical Framework

Violence affects educational outcomes through several interconnected mechanisms. Displaced children carry the trauma of forced flight, usually face constrained access to educational infrastructure, and live in settings where household coping mechanisms are already stretched to their limits. For this population, each additional exposure to community violence may trigger educational disruption out of proportion to that experienced by children in stable host communities. The mechanisms are analytically distinct but operate simultaneously; they are considered in turn.

Safety and fear. Nearby violent events generate acute parental fear for children's physical safety, leading to withdrawal from educational activities even in the absence of direct attacks on schools (Milam et al., 2010; Caudillo & Torche, 2014). Landa-Blanco (2025) finds that children exposed to violence face a 13 per cent higher likelihood of not completing their education, with perceived insecurity on the route to school among the most frequently cited barriers. In refugee camps, where mobility is tightly constrained and children must cross dense shared public spaces to reach a learning center, this channel is likely to be particularly salient: a generalized atmosphere of insecurity is sufficient to trigger precautionary withdrawal even when no learning facility has been attacked.

Psychological trauma, cognitive disruption and disengagement from schooling. Exposure to violence produces acute and chronic psychological responses that impair the cognitive capacities learning requires – concentration, working memory, executive function (Margolin & Gordis, 2000; Kaplan et al., 2016). Sharkey et al. (2012, 2014) find worse assessment performance among children exposed to community violence in the preceding week, concentrated among those residing closest to the incident. Under repeated exposure these deficits compound into deteriorating classroom performance and grade repetition, which erode the perceived return to continued attendance, for child and household alike, where the instrumental value of schooling is already uncertain. Disengagement is therefore the terminal stage of the trauma channel rather than an independent mechanism. For refugee children the psychological burden is compounded by pre-migration trauma, grief from family separation, and the chronic stress of uncertain legal status, so that additional violent shocks may push already fragile children into chronic disengagement.

Economic shock and child labour. Violence disrupts household income through the destruction of productive assets, the loss of adult employment, and the reallocation of labour to caregiving after the injury or death of a household member (Justino et al., 2014; Vesco et al., 2025). Such shocks raise the opportunity cost of schooling and the marginal value of child labour as a coping response. Rodríguez and Sánchez (2012) find that exposure to armed conflict in Colombia increases child labour and reduces enrolment, operating primarily through household income shocks rather than through direct safety concerns; Brown and Velásquez (2017) document reduced human capital investment among young adults aged 15–25 exposed to violent crime in Mexico, with a shift toward immediate labour market participation at the expense of continued schooling. Where refugees are formally excluded from local labour markets and income depends almost entirely on humanitarian transfers and informal activity, the channel is amplified: disruption to these fragile sources translates into educational withdrawal more rapidly than where income is diversified.

School supply disruption. Violence also reduces the physical availability of education, through attacks on learning centers, teacher displacement and damage to infrastructure (Fiszbein & Stanton, 2018; Bertoni et al., 2019), with effects that outlast the disruption itself: interrupted schooling produces grade repetition, permanent dropout and long-term human capital scarring (Ben Salah & Saxena, 2025). Contractions in supply are not distributionally neutral. Where provision is suspended or consolidated into fewer facilities, the marginal cost of attendance rises most for children whose access is already constrained. The clearest case is that of children with disabilities, already excluded in stable settings and 10 to 40 per cent less likely to attend in camps (UNESCO, 2020; Crea et al., 2022; Guglielmi et al., 2020) – a gap that any violence-induced rise in the cost of reaching a learning center widens.

Mobility and administrative barriers. Violence that triggers new displacement, or relocation within the camp, severs the organizational and social ties that sustain educational participation. These disruptions accumulate: a child displaced repeatedly faces successive re-enrolment barriers and long waiting periods, and may eventually drop out of educational tracking systems altogether (Dryden-Peterson, 2011; Al-Hroub, 2014). Documentation requirements and administrative procedures designed for settled populations do not accommodate the mobility and legal precarity of displaced households, and thus operate as structural barriers to access (Taylor & Sidhu, 2012). The consequences are magnified where refugees lack legal recognition and access to formal documentation, as is the case for the Rohingya under Bangladeshi law.

Gendered and age-graded allocation within the household. These channels bear on the household's decision, but not on every child in it alike. Once violence raises the cost of leaving the shelter, the household must decide which

children continue to leave, and that allocation is structured by age and gender. For young children the learning center is itself a supervised, adult-staffed space, so the relevant comparison is not between school and safety but between school and an unsupervised shelter; violence may therefore raise attendance at the youngest ages, against the sign the safety channel implies on its own. For adolescents the calculus reverses. Adolescent boys carry the highest marginal value in the informal labour market and are the natural margin of adjustment when the economic channel binds (Rodríguez & Sánchez, 2012; Brown & Velásquez, 2017) – a margin demonstrably active in the camps, where 26 per cent of adolescent boys report wage work in the previous year against 3 per cent of girls (Guglielmi et al., 2019). Adolescent girls face a different constraint. Where norms of female seclusion govern, the perceived risk to a post-pubescent girl in transit, and the consequences of an incident for her and for her household, are substantially higher than for her brothers, and the response is confinement rather than labour reallocation (Al-Hroub, 2014; Dryden-Peterson, 2011). The binding threshold is puberty rather than age as such, and it constrains mobility well before it constrains schooling (Guglielmi et al., 2019). The prediction is therefore not a single average effect but effects that differ in sign across the age distribution and in channel across gender, with girls' withdrawal beginning earlier and attributable to household prohibition rather than to cost or to insecurity as such. This is consistent with evidence that the risk of sexual violence depresses girls' attendance in these camps while showing no such association – if anything the reverse – among boys (M. Hossain, 2024).

As noted at the outset, these mechanisms are analytically distinct but empirically intertwined, and their simultaneous operation in camp settings produces cumulative disadvantages qualitatively unlike those facing children in conflict-affected host communities. Households in stable contexts may compensate in part for one disrupted channel; in protracted displacement, where assets are minimal, social networks fragmented and institutional support constrained, such compensation is largely unavailable. The intertwining also has an empirical corollary, taken up in Section 5.4: the reason a household gives for withdrawing a child records the form its response takes, not which of these channels set it in motion. The prediction is therefore a negative average effect of community-level violence on attendance, with the compensatory capacity that would attenuate it largely absent, and substantial systematic variation around that average along the age and gender dimensions set out above.

3 The Cox's Bazar Refugee Camp Context

The study is set in the refugee camps of Cox's Bazar, Bangladesh, which host the largest concentration of Rohingya refugees in the world. With the mass influx of August 2017 – during which an estimated 700,000 Rohingya fled targeted military

violence in Myanmar's Rakhine State – the camp population swelled to nearly one million, making the Kutupalong–Balukhali expansion site one of the most densely populated refugee settlements anywhere. At the time of the study, over 930,000 registered Rohingya refugees resided in the camps of Cox's Bazar district (ACAPS, 2023). The camps are characterized by extremely high population density, limited economic opportunities, constrained freedom of movement and limited access to formal legal systems. Children under 18 make up approximately half of the refugee population, so that the camps hold one of the largest concentrations of displaced children in the world (Mithun, 2021; UNHCR, 2025a).

Educational services within the camps are provided through a network of approximately 3,400 learning centers, of which around 2,800 are directly supported by UNICEF (UNICEF, 2023a). The remainder are operated by a constellation of international and national NGOs under the Cox's Bazar Education Sector, a coordination mechanism co-led by UNICEF and Save the Children. The Government of Bangladesh restricts the use of the national curriculum, requiring instead a modified Myanmar curriculum adapted to the Rohingya context, the Learning Competency Framework and Approach (LCFA). The restriction creates obstacles to educational continuity and to future integration (Taylor & Sidhu, 2012; Dryden-Peterson, 2016), compounding the structural barriers of funding, teacher quality and the absence of credible curriculum certification documented within the camps themselves (Rahman et al., 2022, 2023).

In November 2021 UNICEF and its partners launched a Myanmar Curriculum Pilot for secondary-level children, which reached 10,000 students by May 2022 and was extended to all school-aged children for 2023/24 (Education Sector, 2022; UNICEF, 2022). Coverage nonetheless remained partial: some 100,000 school-aged Rohingya children were out of school as of May 2022, and a record enrolment of 300,000 for 2023/24 still left a substantial share without access to formal learning (A. Z. Hossain, 2023; UNICEF, 2023a). Attendance is steeply age-graded and increasingly gendered: it falls from 68 per cent among adolescents aged 10–12 to 15 per cent among those aged 15–18, and in the oldest group 28 per cent of boys attend against 2 per cent of girls (Guglielmi et al., 2020). Families frequently withdraw daughters at puberty, with distance to the learning center and the perceived risk of harassment en route operating as binding constraints (Melnikas et al., 2020; Islam et al., 2021; Human Rights Watch, 2022; UNICEF, 2023b). The restriction binds on mobility before it binds on schooling – girls are roughly 65 per cent less likely than boys to leave their camp block – whereas boys' withdrawal tracks earnings (Guglielmi et al., 2019). From December 2021 the authorities additionally closed or dismantled some 30 community-led schools operating outside the formal humanitarian system (Human Rights Watch, 2022). Insecurity does not appear among the correlates of attendance before the escalation: in January 2019 the correlates of girls' attendance were individual and household characteristics –

age, disability, child marriage in the household, psychological distress – with no detectable association between community violence and attendance (M. Hossain, 2024).

The security environment deteriorated markedly over the study period, from mid-2021 to early 2023. Bangladeshi police recorded nearly 2,500 cases of crime and violence in the camps between August 2017 and September 2022, including 116 targeted killings, and roughly 60 per cent of all incidents recorded through April 2023 date from 2022 onward (ACAPS, 2023). The Ministry of Defence has reported at least eleven armed groups operating simultaneously (Human Rights Watch, 2023), among them ARSA and the RSO alongside territorial gangs engaged in extortion, kidnapping and drug trafficking, which use killings to control the drug trade and to assert dominance (Rezvi et al., 2025). The murder of the community leader Mohib Ullah in September 2021 marked an escalation in violence against leaders and rights advocates (Human Rights Watch, 2021). By late 2023 the intra-Rohingya homicide rate was approximately double that of Bangladeshis in Cox's Bazar district and nearly five times the national average, with at least 76 intra-community murders between January and October 2023 alone (ACAPS, 2023; Human Rights Watch, 2023; IISS, 2023).

Recurrent assault, abduction and forced recruitment produced a severe deterioration of safety across almost all camps (IISS, 2023; ISCG/Rohingya Response, 2024; MSF, 2024). Attacks remained largely selective, targeting majhis, activists and rival gang members, but they generated pervasive fear, aggravated by tactics designed to spread panic: masked attackers stabbing victims indiscriminately and firing shots into the air to terrify residents (Human Rights Watch, 2021, 2023). Of direct educational relevance is the forced recruitment of adolescent boys and young men: MSF (2024) estimates that more than 1,000 were taken from the camps in the early months of 2023 to fight in Myanmar, with abduction and blackmail used to extort families for a relative's return or to forestall conscription. Refugees reported staying awake to guard their households and being unable to move within the camp after dark (MSF, 2024). Over the same period Bangladesh imposed increasingly coercive restrictions on livelihoods, mobility and education, including checkpoint harassment and the closure of markets, community schools and vocational training centers (Human Rights Watch, 2023). Combined with sharply deteriorating food security after the WFP ration cuts, these produced acute multidimensional deprivation in which violence and educational exclusion were closely intertwined (ACAPS, 2023).

The physical destruction of educational infrastructure links violence to schooling directly. The camps recorded 222 fire incidents between 2021 and 2022, 60 of them classified as deliberate sabotage (ISCG/Rohingya Response, 2024), and fires that burned down learning centers were among the principal obstacles to educational continuity in 2022–2023 (UNICEF, 2023a). The destruction of infrastruc-

ture and shelters through arson and armed confrontation disrupted educational services across the camp system, so that armed-group activity, criminal violence and sabotage together undermined both the supply of and the demand for education throughout the study period.

4 Data and Methodology

4.1 Data sources

The analysis combines three sources.

Cox's Bazar Panel Survey and High-Frequency Tracking Survey. The household panel is drawn from the World Bank's Cox's Bazar Panel Survey (CBPS) and from the High-Frequency Tracking Survey (HFT) implemented on the CBPS 2019 sample frame. The baseline is round 3 of the HFT, collected in April 2021. Earlier rounds (April and October 2020) are excluded because they coincide with the initial phase of COVID-19 restrictions, when learning centers were fully closed; by April 2021 home-based and remote learning had begun to operate more systematically, which makes that round the more suitable comparison for the follow-up.

The follow-up is the CBPS 2022 wave, fielded between December 2022 and March 2023, which re-interviewed 85.3 per cent of the refugee households surveyed in 2019; most attrition consists of households no longer present in Cox's Bazar. The public files do not report household-level interview dates, so the date up to which exposure should be measured is uncertain within the fieldwork window. The main analysis uses 31 December 2022 as the reference date, and 31 March 2023 is used as a robustness check. The association is negative and significant under both and stable across control sets within each, so neither sign nor significance depends on where in the window exposure is dated. Magnitudes are not comparable across the two: the later date cumulates a longer exposure window and a larger incident count, and the comparison requires a restricted sample of camps, so the per-incident coefficients should not be read against one another. Section 5.3.7 sets out the comparison.

Both surveys collect detailed information on household composition, children's school attendance, socioeconomic characteristics and welfare indicators. Household coordinates are released anonymized to the level of the enumeration block.

UNHCR Joint Protection Monitoring. Data on violence come from the Joint Protection Monitoring (JPM) exercise coordinated by UNHCR and its protection partners in Cox's Bazar. Protection monitors record incidents occurring within the camps and classify them by type – including abduction, killing, assault, shooting and extortion – and the counts are aggregated by camp. The data are released as three separate quarterly files, which are harmonized and stacked into a single camp-by-

quarter panel. Coverage begins in the second quarter of 2022, and the camp is the finest spatial unit available, so all households resident in a given camp are assigned the same exposure.

Unlike media-based event catalogues, protection monitoring is generated by actors physically present in each camp, and it therefore captures forms of intra-camp violence that rarely reach national or international reporting. The corresponding cost is that an incident enters the record only if it is reported to, and registered by, a protection actor: coverage depends on monitoring presence and on the community's willingness to report, and it is uneven across camps and reporting quarters. The implications of that unevenness for the estimation sample are set out in Section 4.2. These limitations are real, but they are not specific to this source: any measure of violence in a setting where much of it goes unreported and unpoliced inherits them. What distinguishes the JPM exercise is that it is the only systematic collection of incident-level data covering the entire camp system with a consistent classification and camp-level disaggregation. The relevant alternative is therefore not a cleaner dataset but the absence of any measurement of intra-camp violence at this resolution, and the exercise is used here on that basis.

Camp geography (Humanitarian OpenStreetMap Team). Camp boundaries georeferenced by the Humanitarian OpenStreetMap Team are used to characterize each household's position within the camp system. From these boundaries a measure of the household's distance from the edge of the camp in which it resides is constructed. The measure is included among the controls, and it is used to describe where within the camp system the estimated association is concentrated.

4.2 Sample Construction

The sample comprises children resident in the main Kutupalong site who were enrolled at baseline in any school other than madrasas (Islamic school) and Bangladeshi schools. The analysis focuses on children aged 7–17 at baseline, consistent with the target ages for formal education programming in the Education Sector Cox's Bazar Strategy 2022–2024. At endline the sample includes 18-year-olds, reflecting the prevalence of delayed or interrupted schooling in refugee contexts (UNHCR, 2017).

School type is recorded in the 2019 CBPS. Bangladeshi schools are excluded because they lie outside the camp perimeter and Rohingya children have been formally barred from them since the expulsion order of January 2019, so that attendance is either wrongly recorded or requires other type of strategems to be enrolled in such institutions (Human Rights Watch, 2019). Attendance at such schools therefore selects on household resources, on ties to the host community and on residence near the camp edge – the same dimension along which the exposure measure varies. Madrasas are excluded on different grounds: religious schooling sits outside coordinated provision, is not covered by sector policy and carries no certification (ERICC,

2024), and, being community-run, stands in a different relation to the mobility and safety mechanism through which violence is expected to operate here. The two restrictions yield a sample of children whose attendance is exposed to the mechanism under study, and for whom provision is coordinated and comparable across camps.

Data cleaning required resolving substantial mismatches in individual panel identifiers within households in the follow-up, a documented problem in humanitarian panel datasets collected in dynamic displacement settings (McKenzie, 2012; Calderón-Mejía & Ibáñez, 2016). The final sample comprises 498 children from 285 households, resident in 22 camps and 121 primary sampling units, yielding 996 child-round observations in a balanced two-wave panel. Retention stands at approximately 86 per cent of the original CBPS 2019 sample, which compares favourably with panel studies in conflict and displacement contexts, where attrition of 20–30 per cent is common (Alderman et al., 2001; Thomas et al., 2012). Tests of covariate means between the included and excluded samples return a difference significant at the 5 per cent level only for age, indicating that the cleaning procedure does not introduce systematic bias on observables other than age. The cleaned sample may therefore skew slightly younger, which bears on generalizability to older adolescents – a group whose dropout decisions are more likely to reflect individual agency than parental or safety-driven withdrawal (Di Maio & Nandi, 2013).

The estimation sample is restricted to the Kutupalong–Balukhali site on substantive rather than incidental grounds. Kutupalong presents the settlement geography characteristic of large refugee camps – a contiguous, high-density built area bordered by settled residential land – so that a household's distance from the camp edge, and hence its proximity to the host population, varies comparably across the sample. The Teknaf camps sit in more isolated locations, where that boundary carries a different meaning and educational provision is harder to place on the same scale. The estimates should accordingly be read as applying to Kutupalong–Balukhali, which hosts the large majority of the refugee population of Cox's Bazar, rather than to the camp system as a whole.

Within the site, 22 camps are retained. Camps 7 and 1W are dropped: the JPM data return no information for either until the end of 2022, yet the qualitative record places both among the camps substantially exposed to violence, so their recorded zeros more plausibly reflect absence from data collection than absence of incidents. No coverage metadata are released that would separate the two cases.

Baseline exposure normalized to zero. Reported violence is set to zero for every camp in the baseline round. This is a construction rather than a measurement, and it follows from two facts. First, the monitoring series does not extend back to April 2021, so no camp-level count can be assigned to the baseline. Second, the baseline falls at the end of the period of COVID-19 restrictions, during which movement within and around the camps was severely curtailed and learning

centers had only recently begun to reopen; the escalation documented in Section 3 postdates it. The auxiliary ACLED series is consistent with this reading: it records a single violent event involving Rohingya refugees, with no associated fatalities, over the entire period preceding April 2021 – a lower bound rather than a complete record, since media-based cataloguing rarely captures intra-camp violence, but corroborative nonetheless.

The normalization has a structural consequence for the design. Because exposure does not vary across camps at baseline, the household fixed effects and the round indicator absorb the level and the common shift, and β is identified from the cross-camp distribution of endline exposure. The estimating window therefore contains no pre-period of its own. The placebo test of Section 5.3.4 supplies one from outside it, and survey data collected in these camps in January 2019, which record no association between community violence and attendance (M. Hossain, 2024), point the same way.

4.3 Descriptive Statistics

Table 1 compares the refugee and host-population samples of the 2019 Cox's Bazar Panel Survey, which served as the sample frame for the study.

Table 1: Household Characteristics: Refugees vs Host Communities

	Refugee		Host Community	
Variable	Mean %	SE	Mean %	SE
Household Head				
Female	26.82	1.01	17.73	0.96
Education				
No education	65.74	2.31	40.31	1.58
Primary	32.68	1.26	29.54	1.25
Secondary	1.47	0.31	22.97	1.29
Tertiary	0.11	0.06	4.71	0.75
Other	0.00	0.00	2.46	0.39
Households with earnings from productive activities	61.33	1.22	86.65	0.90
Households enrolling all children (age 5–12)	49.39	1.04	57.93	1.10
Households enrolling all children (age 5–15)	41.15	1.14	59.33	1.10
Enrolment rate (age 5–12)				
Female	78.98	1.12	86.39	1.32
Male	84.39	1.04	91.38	0.90
Enrolment rate (age 5–15)				
Female	66.59	1.17	88.57	0.98
Male	78.27	1.02	91.10	1.18
Observations (children aged 5–15)	4,362		3,958	
Households with children 5–15	1,352		2,303	

Note: Enrolment rates refer to the share of children enrolled in formal or non-formal education.

The two populations differ markedly on every dimension. Nearly two thirds of refugee household heads report no formal education (65.7 per cent, against 40.3 per cent in host communities), and secondary or tertiary completion is virtually absent (1.6 against 27.7 per cent) – a gap that matters here because the literacy of the household head is a well-established predictor of children's attendance (Saasa, 2018). Only 61.3 per cent of refugee households report earnings from productive activities, against 86.7 per cent among hosts, reflecting formal restrictions on Rohingya participation in the labour market and near-total dependence on humanitarian assistance; any violence-induced income shock therefore falls on households with little capacity to absorb it.

Enrolment is lower among refugees in both age ranges, but not uniformly so. Among children aged 5–12 the two populations are separated by roughly seven percentage points within each sex (79.0 against 86.4 per cent for girls, 84.4 against 91.4 for boys), and the gender gap within each population is of similar size: 5.4 points among refugees, 5.0 among hosts. The 5–15 range is the one that matters for what follows, since it identifies the cohort observed at ages 7–17 in the estimation rounds, and extending the range changes the picture. Refugee girls' enrolment falls to 66.6 per cent, against 88.6 among host-community girls, and refugee boys' to 78.3: the refugee gender gap doubles to 11.7 points precisely where the host-community gap halves to 2.5. The same pattern holds at household level. The share of refugee households enrolling all their children falls from 49.4 to 41.2 per cent as the range widens, while the host-community share rises slightly, from 57.9 to 59.3.

Table 2 presents summary statistics for the analytical sample in the two survey rounds. The household income variables, by contrast, change substantially, and the shifts are themselves informative about conditions over the study period. The share of households with at least one working member falls from 78 to 59 per cent, and the mean number of working members from 0.88 to 0.77, while the share reporting non-agricultural income rises from 65 to 76 per cent. The two series are not in conflict: the employment variable records whether a household member is engaged in work reported as employment, whereas the income module records sources of household income irrespective of employment status, and therefore captures self-employment, petty trade and casual activity within the camp. Together they indicate a shift in the composition of household income away from wage employment and towards informal, camp-based activity, rather than an expansion of work. Agricultural income contracts from 12 to 3 per cent of households and remittance or pension income from 31 to 7 per cent, a marked reduction in income originating outside the camp.

Cash assistance from government or humanitarian actors rises from 57 to 96 per cent, which indicates near-universal coverage by the time of the follow-up and is consistent with the scaling-up of the emergency response to the deteriorating

Table 2: Summary Statistics – Child and Household Sample Characteristics

	Round 1		Round 2	
Variable	Mean	SD	Mean	SD
Child Characteristics				
Age	10.03	2.39	12.03	2.39
Female (%)	0.44	0.50	0.44	0.50
Household Characteristics				
Household size	6.94	1.91	6.99	2.08
Household with at least an employed member (%)	0.78	0.42	0.59	0.49
Number of working members	0.88	0.56	0.77	0.76
Head literacy (%)	0.26	0.44	0.22	0.42
Household Income Sources				
Non-agricultural (%)	0.65	0.48	0.76	0.43
Agricultural (%)	0.12	0.32	0.03	0.17
Remittances/pension (%)	0.31	0.46	0.07	0.26
Cash assistance (Govt/NGO/WFP/UNHCR) (%)	0.57	0.50	0.96	0.19
No sort of income (%)	0.00	0.00	0.01	0.09
Observations	498		498	

Note: Reported age is unusable in the later rounds: values jump implausibly and in some cases decrease across rounds. Age is therefore anchored to the 2019 face-to-face CBPS and projected forward by the interval elapsed between rounds. It consequently varies across children only through age at that anchor, its within-child variation over time being common to all children and absorbed by the round indicator; the age-based heterogeneity reported in Section 5.2 is accordingly heterogeneity in age at baseline.

security and food security situation documented in Section 3. Household head literacy declines from 26 to 22 per cent, a difference plausibly attributable to changes in the identity of the household head rather than to any change in literacy status.

Descriptive statistics for the violence exposure measure are reported in Table 3. Because the measure counts reported protection incidents at camp level, its distribution is summarized across camps rather than across households. Exposure is normalized to zero in Round 1 for every camp, so all the variation used for identification is cross-camp variation at endline.

Exposure is highly dispersed. Across the twenty-two camps the five-type measure ranges from zero to forty-seven incidents, with a mean of 15.2 and a standard deviation of 14.7, almost as large as the mean. The distribution is right-skewed, with a median of 10.5 well below the mean, and a small number of camps account for a disproportionate share of the recorded total. Only three camps record no incident of any type at endline. The construction of the measure is set out in Section 4.4.

4.4 Empirical Strategy

The specification compares changes in attendance within households against differences across camps in the intensity of reported violence between the two survey

Table 3: Violence Exposure – Reported Protection Incidents by Camp and Round

	From April to December 2022			
Exposure measure	Mean	SD	Median	Max
Killing and shooting	1.36	1.79	0.5	6
+ assault	3.82	4.22	3.0	14
+ abduction	7.73	8.18	4.5	32
+ extortion (five-type measure)	15.18	14.69	10.5	47
Camps recording zero incidents		3		
Camps		22		

Notes: Camp-level counts of reported protection incidents across the twenty-two camps in the estimating sample. The rows are cumulative: each adds the named incident type to the row above, so the last row is the five-type measure used in the main analysis. The number of camps recording zero incidents refers to the five-type measure. A total of 334 incidents is recorded across the twenty-two camps at endline.

rounds, within a two-way fixed effects framework. The estimating equation is

$$\text{School}_{ihct} = \alpha_h + \theta_t + \beta\,\text{Violence}_{ct} + \gamma' X_{ihct} + \varepsilon_{ihct} \tag{1}$$

where School_{ihct} is a binary indicator equal to one if child i in household h, resident in camp c, attends school in round t, and Violence_{ct} is the total number of protection incidents recorded in camp c over the period preceding round t. The latter is constructed as the unweighted sum of the incident categories reported by the Joint Protection Monitoring exercise and listed in Table 3, aggregated over the quarters falling within the reference window. X_{ihct} is a vector of child and household controls, including the distance of the household dwelling from the camp boundary; α_h are household fixed effects; and θ_t is a round indicator. The household effects absorb every characteristic of the household that is fixed over the two rounds and, because households do not change camp within the sample, every fixed characteristic of the camp in which the household resides. β therefore summarizes the extent to which changes in attendance within a household differ according to how much reported violence its camp of residence recorded over the interval, net of what is fixed at household level and of any change common to all households.

The preferred specification includes the full control vector: child age and sex, household size, household head literacy, the number of household members in work, and the household's income source categories. Distance from the camp edge is time-invariant and therefore enters interacted with the round indicator rather than in levels. A more parsimonious specification, restricted to child age and sex, household size and head literacy, is reported alongside it. All covariates are used at baseline level. Covariates at current level are reported as robustness check.

Because reported violence is normalized to zero at baseline, β is equivalently the coefficient on the interaction between camp-level exposure and the round indicator, estimated by two-way fixed effects from the cross-camp distribution of

exposure intensity at endline. Reading β as a causal response requires that, absent the escalation, attendance in camps that came to record more reported violence would have followed the same trajectory as in camps recording less. That condition cannot be tested inside the estimating window, which contains no pre-period, although a placebo on the preceding interval (Section 5.3.4) returns coefficients indistinguishable from zero and well below the estimated association. β is in addition a single slope fitted to a relationship that need not be linear, and it weights intermediate levels of exposure more heavily than the tails (Callaway, Goodman-Bacon & Sant'Anna, 2024). The estimates are interpreted throughout as conditional associations, subject to the qualifications set out in Section 5.3.4.

Standard errors are clustered at the level of the primary sampling unit (PSU), of which there are 121. The choice follows the sampling design: the PSU is the unit at which the CBPS sample was drawn, so clustering there accounts for the dependence the design induces among households selected together, and with 121 clusters the asymptotic approximation on which cluster-robust inference rests is reliable. It is also what the framework of Abadie, Athey, Imbens and Wooldridge (2020, 2023) indicates for the estimand reported here: the level of clustering is determined by the sampling and assignment processes rather than by within-group correlation in outcomes, and for a descriptive estimand – which is how β is interpreted throughout – uncertainty arises from sampling alone, which in the CBPS is clustered at the PSU, since PSUs are drawn from a larger population of PSUs and households are then sampled within them. Exposure, however, varies by camp, and under a causal reading of β the estimand depends on potential outcomes whose sampling is governed by the assignment mechanism, so that the level of assignment becomes the relevant level of clustering (Abadie et al., 2023). Inference at camp level is therefore reported alongside the main results, in the same table. Because the sample covers most of the camps on the site rather than a small draw from a larger population of camps, the cluster variance at that level is, if anything, conservative in large samples (Abadie et al., 2023). With only 22 camps, however, the analytical cluster-robust standard error is not reliable, so inference at that level is based on the wild cluster bootstrap with Rademacher weights and 9,999 replications (Cameron, Gelbach & Miller, 2008; Roodman et al., 2019); randomization inference, which reassigns exposure across camps and relies on no asymptotic approximation, is reported in Section 5.3.3. The conclusions drawn from the preferred specification do not depend on the choice: they hold under the more conservative inference at camp level as they do under the design-based clustering.

4.5 Heterogeneity and mechanism specifications

Heterogeneity is assessed by interacting the violence measure with a moderator Z, within the same fixed effects structure:

$$\text{School}_{ihct} = \alpha_h + \theta_t + \beta\,\text{Violence}_{ct} + \lambda\,(\text{Violence}_{ct} \times Z_{ihct}) + \gamma' X_{ihct} + \varepsilon_{ihct} \qquad (2)$$

The moderators examined are child age, child sex, their interaction, distance to the camp boundary, household employment status, the sex of the household head, the household head's education, and household size. Average marginal effects are computed across the support of Z and reported in Section 5.2. Because the number of interactions examined is large relative to the sample, the heterogeneity results are subject to multiple-testing concerns; where a family of contrasts is tested, adjusted p-values are reported with the estimates.

Section 5.4 turns to the reasons households give for a child's withdrawal. Each stated reason is recoded as a separate binary outcome defined on the full estimating sample – equal to one if the child left school for that reason and zero otherwise, including for children who remained enrolled – and estimated within the same specification:

$$\mathbf{1}[\text{Reason}_{ihct} = k] = \alpha_h + \theta_t + \beta_k\,\text{Violence}_{ct} + \gamma' X_{ihct} + \varepsilon_{ihct} \qquad (3)$$

Defining the outcome on the full sample rather than on the subsample of leavers is deliberate. Under this construction β_k measures the association between reported violence and the probability that a child leaves school for the reason the household reports as k. Estimating the same relationship on the subsample of children observed to have dropped out would instead condition on an endogenous outcome and would answer a different question, namely how leavers distribute across reasons. Coding the outcome as zero for children who remained enrolled imputes no unobserved reason to them: it records only that no withdrawal for reason k occurred. Since the reason-specific outcomes form a family of related hypotheses, Romano–Wolf step-down p-values are reported alongside the conventional ones.

5 Results

5.1 Main Results

Table 4 reports the main estimate. The exposure measure is the camp-level count of reported protection incidents across all five recorded types: abduction, killing, assault, shooting and extortion. Column (3), which includes the full control vector, is the preferred specification; column (2) restricts the controls to predetermined child and household characteristics, excluding the labour and income variables; column (1) omits controls altogether. Panel A reports standard errors clustered at the primary sampling unit, the level of the sampling design. Panel B reports standard errors clustered by camp, the level at which exposure varies, together with wild cluster bootstrap p-values and confidence intervals.

Table 4: Reported Violence and School Attendance

	(1) No controls	(2) Base model	(3) Full model
A. Standard errors clustered at primary sampling unit level			
Violence	−0.00334**	−0.00322**	−0.00351**
	(0.00143)	(0.00140)	(0.00156)
B. Standard errors clustered at camp level, with wild cluster bootstrap			
Violence	−0.00334	−0.00322	−0.00351
	(0.00151)	(0.00150)	(0.00120)
Wild bootstrap p-value	0.085	0.089	0.030
WCB 95% confidence interval	[−0.0069, 0.0006]	[−0.0067, 0.0007]	[−0.0071, −0.0004]
Controls and sample (both panels)			
adj. R^2	0.202	0.276	0.352
Controls	No	Yes	Yes
Fixed effects	HH, t	HH, t	HH, t
N	996	996	996

Notes: The dependent variable is a binary indicator for school attendance. The exposure measure is the camp-level count of reported protection incidents across the five recorded types (abduction, killing, assault, shooting, extortion). All columns include household and round fixed effects. Column (3) is the preferred specification. In Panel B significance is assessed by the wild cluster bootstrap (Rademacher weights, 9,999 replications) rather than by the analytical standard error, so no stars are reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In the preferred specification, one additional reported protection incident in the camp is associated with a fall of 0.35 percentage points in the probability that a child attends school, significant at the 5 per cent level. The estimate is stable across the three columns. Moving from the parsimonious control set of column (2) to the full set of column (3) changes the coefficient from -0.00322 to -0.00351, roughly a fifth of a standard error, and in the direction opposite to the one the post-treatment concern of Section 4.4 implies: conditioning on a mediator would attenuate the estimate toward zero, whereas the full specification is larger in magnitude. The estimate does not, therefore, depend on the choice of covariates.

Panel B addresses the inference question set out in Section 4.4. The sample was drawn by primary sampling unit, which is why Panel A clusters at that level; but exposure is assigned by camp, and under a causal reading of the estimate the camp is the relevant level of clustering. With twenty-two camps the analytical cluster-robust standard error is unreliable in either direction, so the wild cluster bootstrap replaces the asymptotic test. In the preferred specification the bootstrap p-value is 0.03 and the 95 per cent bootstrap confidence interval excludes zero: the estimate on which the paper's conclusions rest is robust to the more conservative inference. In the specifications without the full control set significance is at the 10 per cent.

5.2 Heterogeneity

The population-average coefficient in Table 4 conceals a systematic pattern: the association reverses sign across the age distribution, is estimated with precision among girls and not among boys, strengthens with distance from the camp boundary, and is concentrated among households with no employed member. These regularities, rather than the average, are the paper's principal empirical finding. All heterogeneity estimates use the five-type exposure measure and the full control set, within the household and round fixed effects structure of equation (2).

5.2.1 Child age

The association between violence and attendance is not constant across childhood, and its variation across the age range is the result on which the paper rests. The specification interacts exposure with indicators for five age bands defined at baseline, allowing the association to differ freely across them (Table 5, Panel A).

Table 5: Marginal Effect on School Attendance, by Age of Child (age bands)

A. Age band at baseline		Marginal effect	Std. error
7–8		0.00418*	(0.00243)
9–10		−0.00171	(0.00217)
11–12		−0.00830***	(0.00278)
13–14		−0.01410***	(0.00271)
15–17		−0.00712	(0.00635)
B. Pairwise contrasts against ages 7–8	Difference	Std. error	p (Sidak)
9–10 vs 7–8	−0.00589	(0.00279)	0.140
11–12 vs 7–8	−0.01248	(0.00274)	0.000
13–14 vs 7–8	−0.01825	(0.00296)	0.000
15–17 vs 7–8	−0.01130	(0.00656)	0.306
Joint	$F(4, 120) = 12.49$		0.000
N	996		

Notes: Panel A: average marginal effects of one additional reported protection incident on the probability of school attendance, by age band at baseline. Panel B: contrasts of the marginal effect in each band against the 7–8 band, corrected for four comparisons. Unadjusted p-values are 0.037, 0.000, 0.000 and 0.087 respectively. Household and round fixed effects and the full control set excluding child age. Age at baseline is time-invariant and absorbed by the household fixed effects, while current age differs from age at baseline by an increment common to all children and is therefore collinear with the round indicator; including it leaves the estimates unchanged. Standard errors clustered at the primary sampling unit level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Equality of the association across bands is rejected: $F(4, 120) = 12.49$, $p < 0.0001$. The estimates do not describe a common association measured with varying precision, and no average taken over the age range represents the response of any group within it.

No negative association is found among young children. In the 7–8 band the

point estimate is positive, at 0.42 percentage points per additional incident, and the confidence interval runs from -0.06 to $+0.90$ percentage points. The interval excludes harm of any material size: its lower bound is more than an order of magnitude smaller than the association estimated in early adolescence, and the estimate at 13–14 lies far outside it. What the data rule out at these ages is a negative association of any consequence, and the point estimate lies on the positive side of zero.

The negative association is concentrated in early adolescence. Among children aged 11–12 an additional incident is associated with a fall of 0.83 percentage points in the probability of attendance, and among those aged 13–14 with a fall of 1.41 points, both significant at the 1 per cent level. These are the ages at which the sign, and not merely the magnitude, of the association differs from that observed among the youngest children.

The contrast between the youngest children and each of the two early-adolescent bands survives correction for multiple comparisons decisively (Table 5, Panel B). The difference between ages 7–8 and 13–14 is 1.83 percentage points, the largest in the table, and the difference against 11–12 is 1.25 points. The variation across the age range is therefore not an impression formed by comparing significance levels across cells but a difference tested directly and robust to correction.

The two bands that do not differ from the youngest are the 9–10 band, adjacent to it, and the 15–17 band, the least precisely estimated in the sample. Neither is informative about the contrast the paper advances. The 9–10 band sits between the two regimes: its estimate is not distinguishable from zero, and it differs from the 7–8 band before correction but not after, which places the transition between the two regimes in this interval without locating it more precisely.

Table 6 estimates the association separately at each year of age, imposing neither cutpoints nor a functional form. The pattern it returns is the one the bands describe. No estimate below age twelve is negative and significant at any conventional level; every significant negative estimate falls at age twelve or above. The age at which the negative association appears is therefore located by the data rather than by the choice of bands, and the absence of a negative association among younger children is not a product of grouping. The single-year estimates are computed on cells of a few dozen children and are correspondingly imprecise; the band specification averages that noise out.

Read against Section 2, the profile is not a gradient in the intensity of one response but a change in which force binds. At ages at which a child's time carries no market return and the alternative to attendance is an unsupervised shelter, the supervisory channel operates alone and points toward retention; by early adolescence restriction and opportunity cost enter and dominate. The sign reversal is what that change would produce, and Section 5.4 examines the stated reasons for withdrawal

Table 6: Marginal Effect on School Attendance, by Age of Child

Age of child at baseline	Marginal effect	Std. error
7	0.00446	(0.00292)
8	0.00326	(0.00300)
9	−0.00462	(0.00294)
10	0.000232	(0.00265)
11	−0.00497	(0.00454)
12	−0.0113***	(0.00330)
13	−0.0152***	(0.00361)
14	−0.0122***	(0.00364)
15	−0.0109	(0.00692)
16	0.0124*	(0.00633)
17	−0.0299***	(0.00708)
N		996

Notes: Marginal effects of one additional reported protection incident on the probability of school attendance, estimated freely at each single year of age at baseline. Household and round fixed effects and the full control set excluding age. Standard errors clustered at the primary sampling unit level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

with this reading in mind. The estimates are consistent with the negative effects reported for conflict-affected settings elsewhere over most of the age range, and depart from them only at its lower end, where those samples are either restricted to adolescents or pooled across childhood.

5.2.2 Child sex

Table 7 reports the association separately by sex of the child. For boys it is −0.14 percentage points and statistically indistinguishable from zero; for girls it is −0.58 points, significant at the 1 per cent level. The population-average coefficient in Table 4 is therefore not the common response of two similar groups but the average of one group in which the association is precisely estimated and one in which it is not.

Table 7: Marginal Effect on School Attendance, by Sex of Child

	Marginal effect	Std. error
Boys	−0.00141	(0.00186)
Girls	−0.00581***	(0.00186)
N	996	

Notes: Average marginal effects of one additional reported protection incident on the probability of school attendance, by sex of the child, from the specification interacting violence with sex. Household and round fixed effects and the full control set. Standard errors clustered at the primary sampling unit level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The two marginal effects are reported rather than the interaction term alone, because the pair is the more informative summary of what the specification implies. The estimates establish the association among girls without establishing that it is

absent among boys: the difference between the two, 0.44 percentage points, is not significant on a two-sided test, with a confidence interval running from -0.93 to $+0.10$ percentage points. The direction is the one Section 2 anticipates, where norms of female seclusion make the perceived risk attaching to a girl in transit, and the consequences of an incident for her and for her household, exceed those attaching to her brothers.

5.2.3 Age and sex jointly

The sample supports one partition at a time. Splitting the age bands by sex halves the number of children per cell, and the standard errors roughly double: the point estimates below are informative about direction but not, individually, about significance. They are reported for that reason, and not as a second test of the age result.

Figure 1 reports the marginal effect of violence on attendance by age band, estimated separately for boys and girls, with 95 per cent confidence intervals.

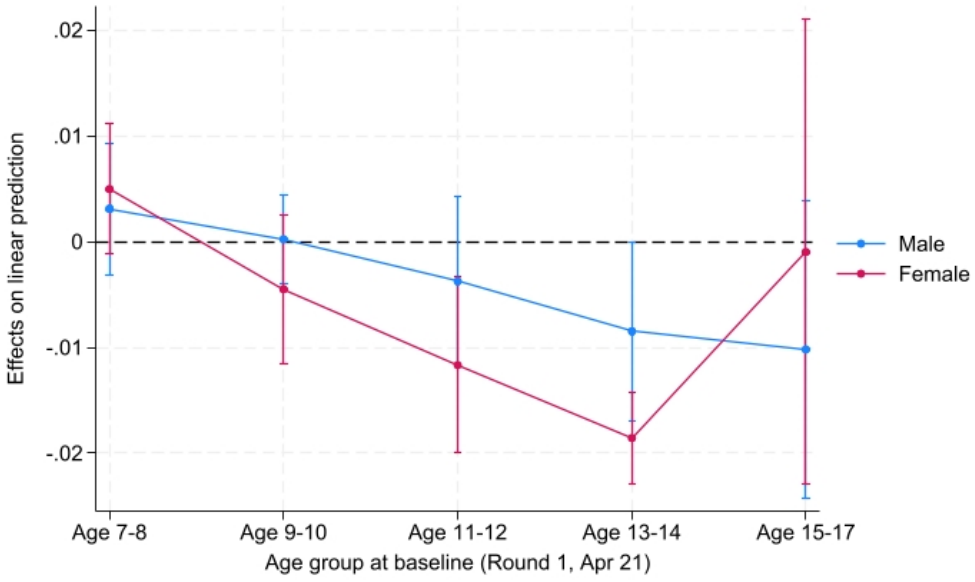


Figure 1: Marginal Effect on School Attendance, by Age and Sex

Notes: Average marginal effects of one additional reported protection incident on the probability of school attendance, with 95% confidence intervals, from the specification interacting violence with child age band and child sex. Household and round fixed effects and the full control set excluding child age and sex, both of which enter the interaction. Standard errors clustered at the primary sampling unit level. $N = 996$.

Both sexes follow the same broad pattern. In the youngest band the estimate is non-negative for boys and for girls alike, and both profiles turn negative and remain so through early adolescence. The reversal by age is the feature the figure shows most clearly: it occurs for both sexes, which locates the sign change in

age itself rather than in the norms that differentiate the sexes. Sex appears to govern the timing and the amplitude of the reversal rather than its occurrence. The profiles diverge from the 9–10 band onward, where the estimate for girls has already turned negative while that for boys has not, and the gap widens through early adolescence until the estimate for girls is roughly twice that for boys at ages 13–14. The divergence is an adolescent phenomenon, absent in childhood and emerging as the constraints on mobility that bind girls begin to apply.

The confidence intervals are wide and overlap in every band, so the figure does not establish that the two profiles differ; the earlier transition and the larger estimates for girls are a consistent direction rather than a tested difference. No within-band sex contrast survives correction for multiple comparisons, the smallest adjusted p-value being 0.215 at ages 13–14, and equality of the sex differential across bands is not rejected ($F(4, 120) = 1.41$, $p = 0.234$). Nor does the pattern extend to the oldest band, where the estimate for girls returns to zero with an interval spanning four percentage points in each direction. That band is uninformative rather than contradictory: as Section 3 documents, attendance among adolescent girls in these camps is already very low by the mid-teens, which leaves little extensive margin on which violence can operate.

5.2.4 Distance to the camp boundary

Table 8 reports the association evaluated at distances from the boundary of the camp of residence, over the range observed in the sample.

Table 8: Marginal Effect on School Attendance, by Distance to the Camp Boundary

Distance to camp boundary	Marginal effect	Std. error
0 km	0.0000426	(0.00290)
0.3 km	−0.000889	(0.00242)
0.6 km	−0.00182	(0.00200)
0.9 km	−0.00275	(0.00169)
1.2 km	−0.00368**	(0.00155)
1.5 km	−0.00461***	(0.00162)
1.8 km	−0.00554***	(0.00189)
2.1 km	−0.00648***	(0.00229)
2.4 km	−0.00741***	(0.00276)
2.7 km	−0.00834**	(0.00327)
N	996	

Notes: Average marginal effects of one additional reported protection incident on the probability of school attendance, evaluated at each distance from the boundary of the camp of residence. The marginal effects are generated by a single interaction coefficient and are therefore constrained to lie on a line: the estimated quantity is the slope of the association in distance, not an empirically estimated distance profile. Household and round fixed effects and the full control set. Standard errors clustered at the primary sampling unit level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The association is essentially zero at the boundary and becomes more negative

with distance, reaching -0.83 percentage points at 2.7 kilometres. It is not distinguishable from zero within 0.9 kilometres of the boundary and is significant from 1.2 kilometres onward. The pattern is therefore a gradient in magnitude and precision rather than a change in sign: households near the perimeter are not estimated to respond in the opposite direction, but their response is smaller and measured less precisely.

The gradient is consistent with the concentration of armed-group activity in camp interiors documented in protection reporting, and with the perimeter being where access roads, service points and external institutional presence are concentrated. As Section 4.4 notes, distance is not absorbed by the household fixed effects: it enters interacted with the round indicator, and in a small number of cases its measured value differs between rounds, reflecting limited changes in the mapped camp geography between baseline and endline whose source is not fully resolved.

5.2.5 Household economic capacity

Table 9 distinguishes households by whether at least one adult member is employed. In households with no employed member the association is -0.66 percentage points, significant at the 1 per cent level; in households with at least one employed member it is -0.11 points and indistinguishable from zero.

Table 9: Marginal Effect on School Attendance, by Household Employment

	Marginal effect	Std. error
No employed members	-0.00657***	(0.00231)
At least one employed member	-0.00109	(0.00180)
N	996	

Notes: Average marginal effects of one additional reported protection incident on the probability of school attendance, by whether any household member is employed. Household and round fixed effects and the full control set. Standard errors clustered at the primary sampling unit level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10 crosses employment status with the sex of the child. Within households with no employed member the association is significant at the 5 per cent level for both sexes and larger for girls (-0.74 percentage points) than for boys (-0.64 points). Within households with at least one employed adult member the estimate for boys is positive and indistinguishable from zero, and that for girls is -0.40 points, significant only at the 10 per cent level.

The direction is what the economic channel of Section 2 predicts: households without wage income have no buffer other than a child's time. The contrast should nonetheless be read with care. The interaction between violence and employment is not itself significant, so the difference between the two rows of Table 9 reflects the precision with which each subgroup estimate is identified as much as a difference in the underlying response.

Table 10: Marginal Effect on School Attendance, by Household Employment and Child Sex

	Boys	Girls
No employed members	−0.00639**	−0.00735**
	(0.00274)	(0.00360)
At least one employed member	0.00289	−0.00402*
	(0.00253)	(0.00225)
N	996	

Notes: Average marginal effects of one additional reported protection incident on the probability of school attendance, by household employment status and sex of the child. Both columns are estimated on the full sample of 996 child-round observations. Household and round fixed effects and the full control set. Standard errors clustered at the primary sampling unit level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2.6 Sex of the household head

Table 11 reports the association by sex of the household head. In male-headed households it is −0.34 percentage points, significant at the 5 per cent level; in female-headed households the point estimate is larger in magnitude, at −0.54 points, but the standard error is more than twice as large and the estimate is not distinguishable from zero.

Table 11: Marginal Effect on School Attendance, by Sex of the Household Head

	Marginal effect	Std. error
Male household head	−0.00335**	(0.00162)
Female household head	−0.00539	(0.00427)
N	996	

Notes: Average marginal effects of one additional reported protection incident on the probability of school attendance, by sex of the household head. Household and round fixed effects and the full control set. Standard errors clustered at the primary sampling unit level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This should be read as an absence of evidence rather than as evidence of no difference. Female-headed households are a minority of the sample, and the imprecision of the estimate is consistent with a wide range of true values, including one substantially larger in magnitude than the estimate for male-headed households. The result is reported because the null is informative about what the data can support, not because an underlying difference has been shown to be absent.

5.3 Robustness Checks

5.3.1 Alternative fixed effects structures

Equation (1) absorbs household and round effects, so that identification rests on the within-household change in attendance set against the cross-camp change in

reported violence. Table A1 re-estimates the coefficient under seven alternative structures, holding the exposure measure, the control vector and the clustering unchanged. Four replace the household effects with a different partition of the data – camp, camp area, sex interacted with baseline age, and the primary sampling unit interacted with the camp. Two retain them and add a group-specific time path, by baseline age and by sex, so that children of different ages, and boys and girls, may follow different trajectories between the rounds. The last interacts each baseline characteristic with the round indicator, the most permissive treatment of differential trends these data allow.

The coefficient is negative throughout, ranging from -0.00278 to -0.00371 (the preferred estimate), with six of the eight rows significant at the 5 per cent level and two at the 10 per cent level. Discarding the within-household differencing and absorbing camp effects instead gives -0.00350, close to the preferred estimate. The smallest magnitudes arise where children are allowed different time paths by sex (-0.00278) and where each baseline characteristic is interacted with the round indicator (-0.00298), both significant only at the 10 per cent level. That the most permissive treatment of differential trends attenuates the coefficient by roughly a quarter without eliminating it is the informative feature of the table.

5.3.2 Sensitivity to individual camps

Exposure varies at camp level across twenty-two camps, so a single camp combining an unusual number of reported incidents with an unusual change in attendance could in principle generate the entire result. Table A2 re-estimates the preferred specification twenty-two times, excluding one camp at a time.

Dropping camps one at a time moves the coefficient little in most cases. Across the twenty-two re-estimations it ranges from -0.00248 to -0.00408, against -0.00351 in the main specification, with a median of -0.00352. Nineteen estimates remain significant at the 5 per cent level and two more at the 10 per cent level, the latter obtained by excluding Camp 19 and Camp 4. The exception is Camp 8W: excluding it moves the coefficient to -0.00248 and the p-value to 0.148, so on that subsample the association would not be reported as significant. This is the largest departure in the table, and no other single camp changes the reading. The camps are administrative units within a single contiguous settlement rather than separate sites, so a camp contributing a large share of the identifying variation reflects where incidents were concentrated rather than a distinct setting whose removal tests the result; and the estimate excluding it retains its sign and about two thirds of its magnitude.

5.3.3 Randomization inference

Exposure varies across twenty-two camps while standard errors are clustered on 121 primary sampling units. If the relevant dependence operates at camp level, conventional standard errors rest on an asymptotic approximation in a number of clusters at which that approximation is known to be unreliable. Randomization inference dispenses with it: the observed vector of camp-level doses is randomly reassigned across the twenty-two camps, the specification is re-estimated on each of five thousand reassignments, and the observed coefficient is compared with the resulting distribution. Table A3 in the appendix reports the exercise in full.

The permutation distribution behaves as it should under the null, centred on zero with a standard deviation of 0.00196. Of the five thousand reassignments, 361 produce a coefficient at least as large in absolute value as the observed one, giving a two-sided p-value of 0.072; allowing for Monte Carlo error, the exact permutation p-value lies between 0.065 and 0.080. The observed coefficient therefore falls just inside the central 95 per cent of the permutation distribution, and this criterion places the estimate at the 10 per cent level rather than the 5 per cent. The dispersion of the permutation distribution exceeds the clustered standard error of the main estimate, 0.00196 against 0.00156, which is why randomization inference is the most demanding of the criteria applied here and the one on which the estimate is weakest.

5.3.4 Placebo test on the pre-baseline period

The estimating window contains no pre-period over which differential trends could be observed, because exposure is normalized to zero at baseline (Section 4.2). The survey series, however, provides a window that precedes the escalation entirely: the same children are observed in April 2019 and in April 2021, the latter being the baseline round of the main analysis. Assigning each camp its endline dose and interacting it with the later round asks whether camps that would subsequently record more violence were already diverging in attendance before any of it had occurred. Under parallel trends the coefficient should be zero.

None of the three coefficients is distinguishable from zero, at -0.0015 without controls, -0.0015 with baseline characteristics interacted with the round indicator, and -0.0025 once distance to the camp boundary is added; all are smaller in magnitude than the preferred estimate. The null also carries information: with standard errors of approximately 0.0018, a pre-baseline divergence equal to the estimated association would have been detected with probability about one half, and divergences of 0.005 or larger are excluded at conventional power.

Two qualifications attach. The confidence interval on the most demanding specification runs from -0.0061 to 0.0012 and still contains the preferred estimate, so

Table 12: Placebo Test – Future Violence Exposure and Pre-Baseline Attendance

	(1) No controls	(2) Baseline full controls	(3) + distance
Violence (placebo dose)	−0.00149	−0.00151	−0.00247
	(0.00174)	(0.00178)	(0.00185)
N	1,666	1,666	1,666
Controls	No	Yes (2019 × t)	Yes (2019 × t)
Distance to boundary	No	No	Yes
Fixed effects	HH, t	HH, t	HH, t

Notes: The dependent variable is a binary indicator for school attendance. The sample is the panel of children observed in both April 2019 and April 2021; it covers the interval preceding the estimating window and is therefore not the estimating sample of Table 4. Each camp is assigned its endline exposure, interacted with the later of the two rounds. Column (2) adds child and household characteristics measured in April 2019, interacted with the round indicator; column (3) adds distance from the dwelling to the camp boundary, measured on the 2021 release and applied to both rounds because the 2019 boundary extract is no longer retrievable, so that it postdates part of the window under test. Household and round fixed effects throughout. Standard errors clustered at the primary sampling unit level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the exercise excludes a large pre-existing differential trend across camps but not one of the same order of magnitude as the association in Table 4. And the window spans the closure of learning centers under COVID-19 restrictions, comparing a period of full in-person provision with one of partial and remote provision, so it does not hold the schooling regime fixed as a test of trends ideally requires. The placebo is therefore reported as evidence consistent with the absence of a pre-existing divergence, not as a demonstration that none exists.

5.3.5 Reweighting on baseline observables

The placebo addresses divergence in trends before the escalation. A distinct question is whether the estimate is driven by differences in baseline characteristics between camps that subsequently experienced more and less violence. Marginal mean weighting through stratification (Hong, 2020) provides one way of posing it. The camp-level dose is discretized into ordered categories, an ordered logit on baseline characteristics generates a propensity score, and observations are reweighted so that the baseline covariates are balanced across dose categories. Weights are computed on the baseline round and carried forward to the follow-up, and the specification is re-estimated on the weighted sample.

Reweighting leaves the estimate essentially where it was. Under the full control set the weighted estimate is −0.00359, against −0.00351 unweighted, a difference of less than a tenth of its magnitude; in the two more parsimonious columns the weighted estimate is larger in magnitude than its unweighted counterpart, −0.00391 against −0.00334 and −0.00380 against −0.00322. All three remain significant, the first two at the 5 per cent level and the third at the 10 per cent,

Table 13: Reweighting on Baseline Observables (MMWS)

	(1) No controls	(2) Base model	(3) Full model
Violence	−0.00391**	−0.00380**	−0.00359*
	(0.00182)	(0.00179)	(0.00185)
adj. R^2	0.236	0.302	0.383
Controls	No	Yes	Yes
Weights	MMWS	MMWS	MMWS
Fixed effects	HH, t	HH, t	HH, t
N	996	996	996

Notes: As Table 4, with observations weighted by marginal mean weights through stratification. Weights are constructed from an ordered logit of the discretized camp-level dose on baseline child and household characteristics and the distance control, estimated on the baseline round and carried forward to the follow-up. Standard errors clustered at the primary sampling unit level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

on the same 996 child-round observations as the main specification. Selection on baseline observables is therefore not what generates the association: balancing those observables leaves the estimate intact.

5.3.6 Alternative exposure measure

The exposure measure sums five incident categories with equal weight, a restriction imposed by the source rather than chosen (Section 4.1). Whether the association depends on that choice can be assessed by re-estimating the specification on a narrower measure that counts only the two most severe types, killing and shooting (Table A4).

The association is negative in every column, with a point estimate of -0.0224 in the preferred specification. Under clustering at the primary sampling unit it is significant at the 10 per cent level throughout, and under camp-level clustering the wild cluster bootstrap returns p-values between 0.052 and 0.065, so on this measure the association is established at the 10 per cent level rather than the 5 per cent. Moving from the parsimonious control set to the full one changes the coefficient from -0.0178 to -0.0224, roughly half a standard error.

The two measures cannot be compared per incident, since they differ in scale. Expressed at one standard deviation of each measure in the estimating sample – 11.6 incidents for the five-type measure, 1.6 for the two-type – they imply falls of 4.3 and 3.6 percentage points respectively in the probability of attendance. The two are of comparable magnitude, so the result does not turn on which incident types the measure counts, and the equal weighting imposed by the source is not what generates it.

5.3.7 Timing of exposure measurement

The follow-up round was fielded between December 2022 and March 2023 and interview dates are not released at household level (Section 4.1), so exposure must be cumulated to a date chosen by the analyst. The main specification uses 31 December 2022, the end of the monitoring quarter in which fieldwork began, so that few of the incidents it counts can postdate an interview. The opposite convention, 31 March 2023, is reported alongside it in Table 14.

Table 14: Alternative Dating of Exposure – Camps Covered in Both Windows

	(1) No controls	(2) Base model	(3) Full model
A. Exposure cumulated to 31 December 2022			
Violence	−0.00625***	−0.00611***	−0.00499***
	(0.00164)	(0.00165)	(0.00177)
adj. R^2	0.207	0.284	0.357
B. Exposure cumulated to 31 March 2023			
Violence	−0.00213***	−0.00211***	−0.00206***
	(0.000664)	(0.000697)	(0.000685)
adj. R^2	0.200	0.278	0.358
Controls and fixed effects (both panels)			
Controls	No	Yes	Yes
Fixed effects	HH, t	HH, t	HH, t
N	534	534	534

Notes: The dependent variable is a binary indicator for school attendance. Both panels are estimated on the camps for which protection monitoring returns data in both windows, so that the two dating conventions differ only in the reference date and not in the composition of camps. All columns include household and round fixed effects. Standard errors clustered at the primary sampling unit level in parentheses. Because the later date cumulates a longer window and assigns each camp a larger count, the coefficients are not comparable per incident across panels; see the text. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The two conventions cannot be compared on the full estimating sample, because the coverage of protection monitoring is not constant over time. A number of camps recorded throughout 2022 return no data for the first quarter of 2023, and their absence is a gap in reporting rather than an absence of incidents: the source records explicit zeros where a covered camp had none, and several of the camps that disappear had recorded substantial counts in the preceding quarters. Extending the window to 31 March 2023 therefore changes the set of camps as well as the length of the exposure period. Both conventions are consequently estimated on the camps covered in both windows, which isolates the dating choice from the change in composition and retains 534 of the 996 child-round observations.

On that common sample the association is negative under both conventions and across all control sets, and significant at the 1 per cent level throughout: −0.00499

in the full specification when exposure is cumulated to 31 December 2022, and -0.00206 when it is cumulated to 31 March 2023. The estimate does not depend on which end of the fieldwork window is used to date exposure.

The two coefficients remain incomparable in magnitude: the later date cumulates an additional quarter and assigns each camp a larger count, so a coefficient expressed per incident is mechanically smaller, and the ratio between the two – close to two and a half to one – matches the growth in recorded incidents over that quarter. That growth is itself informative, since the first quarter of 2023 falls inside the sharpest phase of the escalation (Section 3), and abduction, extortion and killing, three of the five incident types counted, are the categories rising fastest within it. The common-coverage sample is selected on the presence of protection monitoring in both periods, so the larger coefficient it produces is not offered as a preferred estimate.

5.4 Suggestive Evidence on Potential Mechanisms: Stated Reasons for Withdrawal

The preceding sections document that reported violence is associated with lower school attendance and that the association varies systematically across children, but they are silent on the channels behind it. This section offers suggestive evidence on that question, using the reasons households give for a child's withdrawal. The quantitative literature on conflict and education documents effects on enrolment and attainment and infers mechanisms from the pattern of heterogeneity, but it rarely observes the household's own account of why a child stopped attending. The Cox's Bazar Panel Survey records that account, and this appears to be among the first attempts to use stated reasons for withdrawal to unpack the relationship between violence and schooling in a refugee camp.

The evidence is suggestive by construction, and one limitation governs how every estimate below should be read: the stated reasons are not independent channels. Only one of the five, safety concerns, names insecurity as the proximate cause. Each of the others is, on the argument of Section 2, a route through which violence may operate – restriction is one way a household acts on fear for a daughter's safety, economic pressure is how an income shock reaches the child, and disengagement is the terminal stage of the trauma channel – and each is also reported where violence plays no part. A stated reason therefore identifies the form a withdrawal takes in the household's account, not its cause, and the estimates below describe how the composition of stated reasons co-moves with reported violence. Section 5.4.6 returns to the point.

School attendance is universal at baseline and falls to 68 per cent at endline, so every withdrawal is observed in the second round; 153 of the approximately 160 children not attending are assigned to one of the five categories (Table 15). Two

Table 15: Withdrawal Events by Stated Reason and Sex

Stated reason	Pooled	Boys	Girls	Share at endline
Disengagement	35	26	9	7.0%
Safety concerns	14	5	9	2.8%
Familial and social restriction	68	2	66	13.7%
Economic reason	32	25	7	6.4%
Supply-side constraints	4	2	2	0.8%
Total categorized	153	60	93	30.7%

Notes: Counts of children observed to have left school at endline, by stated reason and sex. Withdrawal is observed only in the second round, so shares are expressed relative to the 498 children observed at endline rather than to the 996 child-round observations of the estimating sample. The five categories do not exhaust the recorded reason codes; see the text.

features of the counts matter. The events are rare – the largest category comprises sixty-eight children and the smallest four – so the estimates below are sparse and are read as indications of direction and relative magnitude rather than as precise quantities. And the division the regressions recover is already visible without them: restriction is reported for sixty-six girls and two boys, economic pressure for twenty-five boys and seven girls, disengagement for twenty-six boys and nine girls. The sex composition of the restriction category is close to complete separation, and it is this feature of the data that underlies the results that follow.

The design follows Section 4.5. Each of the five stated reasons – disengagement, safety concerns, familial and social restriction, economic reasons, supply-side constraints – is recoded as a binary outcome on the full estimating sample, equal to one if the child left school for that reason and zero otherwise, including for children who remained enrolled, and estimated within the same household and round fixed effects structure. Throughout this section the specification uses the control set measured at baseline, as in Section 5.1, because baseline covariates are predetermined with respect to the exposure measured between the two rounds; conditioning on contemporaneous household characteristics would risk absorbing part of the variation under examination.

The pooled estimates are close to zero for every stated reason, and the disaggregation shows why: on familial and social restriction the estimate is negative for boys and positive, and more than twice as large, for girls, so that averaged over the sample the two largely offset. The section is organized around that disaggregation, with the pooled estimates reported first as the benchmark against which the offsetting is visible.

5.4.1 Pooled estimates

Table 16 presents the pooled estimates for the entire sample.

No estimate is significant at conventional levels except that on supply-side con-

straints, which carries a small negative coefficient significant at the 10 per cent level. It is not interpreted: the point estimate is a fraction of those on disengagement, restriction and economic reasons, it rests on four events, and the category shows no systematic pattern in the disaggregated results below.

Table 16: Withdrawal Reasons and Reported Violence – Pooled Estimates

	(1) Disengagement	(2) Safety	(3) Restrictions	(4) Economic	(5) Supply
Violence	0.00137	−0.000349	0.00165	0.00128	−0.000401*
	(0.00115)	(0.000454)	(0.00134)	(0.00131)	(0.000220)
adj. R^2	0.074	0.051	0.182	0.151	0.133
Controls	Baseline	Baseline	Baseline	Baseline	Baseline
Fixed effects	HH, t	HH, t	HH, t	HH, t	HH, t
N	996	996	996	996	996

Notes: Separate linear probability models; the outcome equals one if the child left school for the stated reason, defined on the full estimating sample. Exposure is the camp-level count of reported protection incidents (five types). Household and round fixed effects and baseline controls throughout. Standard errors clustered at the primary sampling unit level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.4.2 Estimates by sex

Disaggregation by sex separates the stated reasons sharply (Table 17).

Table 17: Withdrawal Reasons by Sex of Child

	(1) Disengagement	(2) Safety	(3) Restrictions	(4) Economic	(5) Supply
Boys	0.00282*	−0.000673	−0.00278***	0.00230	−0.000449*
	(0.00169)	(0.000622)	(0.000957)	(0.00144)	(0.000241)
Girls	−0.000210	0.0000072	0.00651***	0.000166	−0.000348
	(0.000971)	(0.000791)	(0.00179)	(0.00144)	(0.000252)
Female–Male difference	−0.00303	0.000680	0.00930	−0.00214	0.000101
	(0.00160)	(0.00108)	(0.00143)	(0.00114)	(0.000222)
p-value of difference	0.061	0.532	<0.001	0.063	0.651
Controls	Baseline	Baseline	Baseline	Baseline	Baseline
Fixed effects	HH, t	HH, t	HH, t	HH, t	HH, t
N	996	996	996	996	996

Notes: Average marginal effects of one additional reported protection incident on the probability of withdrawal for each stated reason, estimated separately by sex of the child from the specification interacting violence with sex. The Female–Male difference is the coefficient on the interaction term, with its own standard error and two-sided p-value. Household and round fixed effects and the baseline control set. Standard errors clustered at the primary sampling unit level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

For girls, only one stated reason moves with violence: an additional reported

incident raises the probability of withdrawal attributed to familial and social restriction by 0.65 percentage points, significant at the 1 per cent level, while disengagement, safety, economic pressure and supply constraints are all indistinguishable from zero. For boys the restriction estimate is of opposite sign, at -0.28 points and also significant at the 1 per cent level, and disengagement carries a positive coefficient significant at the 10 per cent level. The two opposite-signed coefficients are the arithmetic behind the near-zero pooled estimate in Table 16.

The sex difference on restriction, 0.93 points, is the largest and best-determined contrast in the table and the only one to survive correction for the five hypotheses tested (Section 5.4.5). Two others reach the 10 per cent level: disengagement (-0.30 points), reflecting a positive coefficient for boys against a null for girls, and economic reasons (-0.21 points), where neither marginal effect is individually distinguishable from zero but the contrast is estimated more precisely than either response, as is common for contrasts and not an inconsistency. The direction of the latter – boys responding more than girls – is the one Section 2 anticipates.

The pattern for girls is what the evidence from these camps would lead one to expect. Where norms of female seclusion govern adolescent mobility, a deterioration in security is absorbed by restricting a daughter's movement, and the household reports it as such. In Cox's Bazar the restriction binds on mobility before it binds on schooling: adolescent girls are roughly 65 per cent less likely than boys to leave their camp block (Guglielmi et al., 2019); families commonly withdraw daughters at puberty, citing distance to the learning center and harassment en route (Melnikas et al., 2020; Islam et al., 2021; UNICEF, 2023b); and the risk of sexual violence is associated with lower attendance among girls but not boys (M. Hossain, 2024). Confinement rather than labour reallocation is likewise the documented reaction to insecurity among adolescent girls in other displaced populations (Al-Hroub, 2014; Dryden-Peterson, 2011), and a safety-based reading has been advanced for the girl-specific losses in Tajikistan (Shemyakina, 2011).

Boys are not confined by the same response. Their withdrawal tracks earnings – 26 per cent of adolescent boys report wage work in the previous year, against 3 per cent of girls (Guglielmi et al., 2019) – the margin on which violence-induced income shocks operate elsewhere (Rodríguez & Sánchez, 2012; Brown & Velásquez, 2017), and they are in addition the group exposed to forced recruitment (MSF, 2024). Their negative restriction coefficient carries little independent weight, resting on only two reported withdrawals (Table 15); it is the contrast between the sexes, driven by girls, that the data support. The boys' estimate is at most consistent with their withdrawal being governed by other considerations.

5.4.3 Estimates by age

The same exercise is repeated across age bands, with the marginal effect of exposure allowed to differ freely across the five bands defined at baseline. Table 18 reports the estimates and, for each stated reason, the joint test of equality across bands.

Table 18: Withdrawal Reasons by Age Band of Child

Age band at baseline	(1) Disengagement	(2) Safety	(3) Restrictions	(4) Economic	(5) Supply
7–8	0.00247	−0.000413	−0.00336***	−0.00213**	−0.000322*
	(0.00188)	(0.000633)	(0.00114)	(0.00103)	(0.000194)
9–10	0.00157	−0.00101**	0.00130	−0.0000449	−0.000379
	(0.00129)	(0.000435)	(0.00169)	(0.00113)	(0.000263)
11–12	0.000992	0.000975	0.00553**	0.00123	−0.000413*
	(0.00158)	(0.00127)	(0.00278)	(0.00158)	(0.000213)
13–14	0.00110	−0.000819*	0.00705**	0.00721**	−0.000232
	(0.00212)	(0.000490)	(0.00332)	(0.00325)	(0.000218)
15–17	−0.000545	0.00325	−0.000322	0.00597	−0.000908
	(0.00210)	(0.00377)	(0.00340)	(0.00405)	(0.000600)
Joint test, $F(4, 120)$	0.38	1.00	9.32	4.13	0.64
p-value	0.820	0.412	<0.001	0.004	0.632
Controls	Baseline	Baseline	Baseline	Baseline	Baseline
Fixed effects	HH, t	HH, t	HH, t	HH, t	HH, t
N	996	996	996	996	996

Notes: Average marginal effects of one additional reported protection incident on the probability of withdrawal for each stated reason, by age band at baseline, from the specification interacting violence with age-band indicators. The bands impose no restriction on the shape of the profile. The joint test is of equality of the marginal effect across the five bands. Household and round fixed effects and the baseline control set, excluding age. Standard errors clustered at the primary sampling unit level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Equality of the marginal effect across age bands is rejected for two stated reasons and no others: familial and social restriction, and economic pressure. For disengagement, safety and supply-side constraints the joint tests do not reject a flat profile, and the isolated significant coefficients in those columns are not interpreted.

The two reasons that vary are the two that Section 2 places in adolescence, and both reverse sign rather than merely strengthening. Restriction is −0.34 percentage points among children aged 7–8, significant at the 1 per cent level, turns positive from the 9–10 band, and reaches 0.55 points at 11–12 and 0.71 points at 13–14, both significant at the 5 per cent level. Economic pressure follows a slightly later

timetable: −0.21 points at ages 7–8, significant at the 5 per cent level, indistinguishable from zero at 9–10 and 11–12, and 0.72 points at 13–14, again significant at the 5 per cent level.

The reasons that dominate adolescent withdrawal are therefore not merely unresponsive among young children: their association with violence has the opposite sign. Among the youngest, higher reported violence is associated with less withdrawal attributed to restriction and to economic pressure, which is what the supervisory interpretation of Section 2 would lead one to expect: a learning center is an adult-staffed space, a seven-year-old's time carries no market return, and the household has little to gain from keeping that child at home when conditions deteriorate. Both profiles have turned by early adolescence, on the timetable the camp evidence suggests. Restriction turns at the approach of puberty, the threshold at which families in these camps curtail daughters' movement and withdraw them from learning centers (Melnikas et al., 2020; Guglielmi et al., 2019); economic pressure turns when a boy's time acquires a market value (Guglielmi et al., 2019), the age pattern also found for income-driven withdrawal in Colombia and Mexico (Rodríguez & Sánchez, 2012; Brown & Velásquez, 2017). The bands in which the two profiles turn bracket the interval in which the attendance association changes sign in Section 5.2.1.

The 15–17 band is imprecise for every stated reason, with standard errors two to three times those of the adjacent band, and no claim is made on it.

5.4.4 Estimates by age and sex

Disaggregating by age band and sex jointly divides the sample into ten cells and is correspondingly demanding. The specification is reported for the three stated reasons that respond in the preceding sections; safety and supply-side constraints are omitted, since neither varies systematically by age or by sex. Table 19 reports the marginal effects and, for each reason, the test of whether the sex differential varies across age bands.

Table 19: Withdrawal Reasons by Age Band and Sex – Selected Reasons

	Disengagement		Restrictions		Economic	
Age band at baseline	Boys	Girls	Boys	Girls	Boys	Girls
7–8	0.295	0.237	−0.283***	−0.399***	−0.176*	−0.245**
	(0.261)	(0.255)	(0.106)	(0.137)	(0.104)	(0.111)
9–10	0.352*	−0.161*	−0.302***	0.802**	−0.0001	−0.0070
	(0.186)	(0.0870)	(0.0976)	(0.328)	(0.138)	(0.157)
11–12	0.285	−0.0432	−0.168	1.09***	0.430	−0.0890
	(0.325)	(0.0776)	(0.143)	(0.400)	(0.319)	(0.0877)
13–14	0.269	−0.0329	−0.267	1.43***	0.970**	0.516
	(0.358)	(0.112)	(0.177)	(0.448)	(0.412)	(0.378)
15–17	0.119	−0.361**	−0.211	0.728	0.865	0.0963
	(0.298)	(0.145)	(0.156)	(1.09)	(0.573)	(0.213)
Triple interaction, $F(4, 120)$	0.56		8.29		0.98	
p-value	0.694		<0.001		0.421	
Controls	Baseline					
Fixed effects	HH, t					
N	996					

Notes: Coefficients and standard errors are multiplied by 100 and expressed in percentage points.
Average marginal effects of one additional reported protection incident on the probability of withdrawal for the stated reason, by age band at baseline and sex, from the specification interacting violence with age band and sex. The triple interaction test is of equality of the sex differential across age bands and is common to the two columns of each reason. Household and round fixed effects and the baseline control set, excluding age and sex. Standard errors clustered at the primary sampling unit level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The joint disaggregation is supported for one stated reason only: equality of the sex differential across age bands is rejected for familial and social restriction ($F(4, 120) = 8.29$), but not for disengagement ($F = 0.56$) or economic pressure ($F = 0.98$).

On restriction the two sexes separate early, and the separation is itself estimated. In the youngest band both are negative and significant at the 1 per cent level, −0.28 percentage points for boys and −0.40 for girls. From the 9–10 band the profiles part: the estimate for girls reverses and rises through adolescence, from 0.80 percentage points at 9–10 to 1.09 at 11–12 and 1.43 at 13–14, significant at the 5 per cent level or better throughout, while that for boys stays negative at every age, significant in the first two bands and not thereafter – and, as noted in Section 5.4.2, resting on two events, so carrying little independent weight. The implied Female–Male difference is −0.12 percentage points in the youngest band and then 1.10, 1.26, 1.70 and 0.94 across the remaining four. The sexes are alike where the argument of Section 2 says restriction has no purchase, and diverge from the

ages at which it says restriction begins to bind, which is what the rejected triple interaction records.

The youngest band is also where the pooled estimates conceal most. For both sexes, higher violence among children aged 7–8 is associated with less withdrawal attributed to restriction and to economic pressure, the four estimates running from -0.18 to -0.40 percentage points and all significant at conventional levels. At those ages the household does not appear to be restricting a child's movement in response to insecurity, which is consistent with the learning center being an adult-staffed space whose value rises rather than falls when conditions deteriorate.

On economic pressure the point estimates place the response among adolescent boys, for whom the coefficient rises from -0.18 percentage points at ages 7–8 to 0.97 at 13–14, significant at the 5 per cent level, while the profile for girls is flat above the youngest band. The triple interaction is not rejected, so the direction is the one Section 2 anticipates but the age-by-sex pattern is not established. The same holds for disengagement, where the estimates for boys are positive and those for girls negative above the youngest band, but only two cells reach the 10 per cent level and the joint test returns nothing.

5.4.5 Adjustment for multiple hypothesis testing

The exercise tests five outcomes within each specification, and the risk of a false rejection accumulates accordingly: at a 5 per cent threshold, five independent tests carry a probability of approximately 23 per cent that at least one rejects when no association is present. Romano–Wolf step-down adjusted p-values (Romano & Wolf, 2005a, 2005b, 2016) are therefore reported for each specification, computed by bootstrap resampling at the level of the primary sampling unit. The procedure controls the familywise error rate and exploits the correlation structure across the five outcomes, which is substantial here because the categories are mutually exclusive. Four families are reported, corresponding to the specifications of Sections 5.4.1 to 5.4.3: the pooled estimate, the sex differential, the age profile across the five bands, and the contrast between adolescence and childhood. Each comprises five hypotheses, one per stated reason. The two families on which the reading of those sections rests are reported here; the pooled family and the adolescence contrast are in the appendix (Tables A5 and A6).

The correction leaves the reading of the preceding sections largely intact. In the pooled specification nothing survives, and nothing was significant beforehand: the adjustment confirms what Table 16 already showed.

The sex differential on familial and social restriction survives comfortably (Romano–Wolf $p = 0.001$), and is the only one of the five that does. It is therefore not an artefact of testing five outcomes at once. The differentials on disengagement and

Table 20: Romano-Wolf Adjusted p-values – By Sex

	Unadjusted	Romano-Wolf
Disengagement	0.0608	0.1323
Safety concerns	0.5318	0.7202
Restrictions	$<$0.001	0.0010
Economic	0.0630	0.1323
Supply-side	0.6513	0.7202

Notes: The tested coefficient is the interaction of exposure with the indicator for girls. Romano-Wolf step-down adjusted p-values (Romano & Wolf, 2005a, 2005b), computed with 999 bootstrap replications of which 989 were valid, resampled at the primary sampling unit level. Each family comprises the five stated-reason hypotheses tested jointly. The procedure controls the family-wise error rate, that is, the probability of at least one false rejection within the family.

Table 21: Romano-Wolf Adjusted p-values – By Age Band

	Unadjusted	Romano-Wolf
Disengagement	0.4270	0.4838
Safety concerns	0.2730	0.3889
Restrictions	0.0003	0.0242
Economic	0.0005	0.0253
Supply-side	0.7764	0.6990

Notes: The tested coefficient is the interaction of exposure with the age-band index, that is, a linear trend in the association across the five bands. Romano-Wolf step-down adjusted p-values (Romano & Wolf, 2005a, 2005b), computed with 999 bootstrap replications of which 989 were valid, resampled at the primary sampling unit level. Each family comprises the five stated-reason hypotheses tested jointly. The procedure controls the familywise error rate, that is, the probability of at least one false rejection within the family.

economic pressure, both significant at the 10 per cent level before adjustment, do not survive, with adjusted p-values of 0.13.

The age dimension is tested under two parameterizations of the same hypothesis – a trend across the five bands (Table 21), and the narrower contrast between adolescence and childhood that Section 2 predicts (Table A6) – and both return the same answer. Familial and social restriction survives in each, with adjusted p-values of 0.024 and 0.022; economic pressure survives the first (0.025) and reaches the 10 per cent level in the second (0.064). The two reasons whose age profiles Section 5.4.3 documents are thus the two that hold up under correction, and the sign reversal both display is not a consequence of testing five outcomes at once.

No adjustment is applied to the joint disaggregation of Table 19. An interaction of violence, age and sex is identified from comparisons between siblings of different ages and sexes within the same household, and few households contribute such comparisons; with binary outcomes observed for a small minority of children and cells defined at two-year age bands, a familywise correction across five such terms would return non-significant values whether or not the underlying differences are real. Table 19 is read as descriptive throughout.

5.4.6 Interpretive limitations

Three qualifications apply to the evidence in this section, of which the first is the most consequential.

Interdependence of the stated reasons. The five categories partition the answers households give, not the causes of withdrawal. Only safety concerns is defined as a consequence of violence; restriction, economic pressure, disengagement and supply constraints are each plausibly downstream of it (Section 2) but are equally reported in its absence, and each child is assigned a single reason. A household that withdraws a daughter because of assaults in the camp, and assigns her domestic responsibilities in the interim, may report the outcome as a familial or social restriction, because that is an accurate description of the arrangement from its point of view. Two implications follow. First, the absence of a response in the safety category cannot be read as evidence that safety is irrelevant to girls' withdrawal: the concentration of their response under restriction, alongside no response under safety, is what the confinement reading predicts, and it matches accounts from these camps in which the risk of harassment en route is the reason families give for keeping adolescent daughters at home (Section 3). Second, because the categories are mutually exclusive, a rise in the share of one is mechanically accompanied by a fall in the share of the others, so the reason-specific coefficients are not independent pieces of evidence. With self-reported data the causes of withdrawal and the household's description of it cannot be fully separated, and the estimates are informative about the second.

Sparseness of the withdrawal margin. The counts in Table 15 are distributed across 121 primary sampling units, and cluster-robust inference on outcomes this sparse is fragile. The supply-side category rests on four events, which is why its estimates are not interpreted here, including where they reach the 10 per cent level; the restriction estimates for boys rest on two. The same caution applies with more force to the age-by-sex cells of Table 19, and most of all to the 15–17 band, where standard errors run two to three times those of the adjacent band.

Respondent bias. The reasons are reported by a household respondent rather than by the child. A restriction imposed by the household is a socially unremarkable answer; a report that a child was frightened, or has ceased to care about school, is not equally so, and may be understated for reasons unrelated to what occurred. Bias of this kind cannot be signed or corrected with the available data, and it is a further reason to read this section as locating where the adjustment occurred in the household's own account rather than as identifying causes.

The claims the section supports are graded accordingly. The firmest is descriptive and depends on no coefficient: withdrawals attributed to familial and social restriction concern girls almost exclusively, and those attributed to economic pressure and to disengagement concern mostly boys. The sex differential on restriction,

and the age profiles of restriction and of economic pressure, survive familywise correction. The divergence between the sexes across age bands in Table 19 is supported by the rejected triple interaction on restriction, but on disengagement and economic pressure it is a direction the estimates display rather than a tested differential. None of these results identifies a causal channel; together they indicate that the household accounts are consistent with the mechanisms the framework proposes.

6 Policy Implications and Conclusion

This paper has examined how community-level violence relates to the schooling of Rohingya refugee children in Cox's Bazar. Across the estimating sample, one additional recorded incident in a child's camp is associated with a fall of 0.35 percentage points in the probability of attendance – the least informative quantity the data produce. Disaggregated, the association is positive among children aged seven and eight, null at nine and ten, and negative and growing thereafter, reaching -1.41 points at thirteen and fourteen; equality across age bands is rejected decisively. The association is estimated with precision among girls and not among boys, and it is concentrated among households with no employed member. The reasons households give for withdrawal divide along the same lines: for girls the response appears under familial and social restriction and under no other stated reason, while for boys the estimates point to economic pressure through adolescence, a direction they display rather than a tested differential.

What emerges is less a uniform contraction of schooling than a set of selective withdrawals: which children stop attending, and the reason a household reports when they do, vary systematically with age, with sex and with whether the household has any income other than a child's time. Three implications follow, and each depends on that structure rather than on the average.

Insecurity is an education constraint, and should be budgeted as one. The reasons under which the association appears – a rule confining an adolescent daughter, economic pressure on an adolescent son – are household responses to the security environment rather than to the education system. Measures that reduce the frequency of incidents in the camps, or the exposure of the journey to a learning facility, may therefore be expected to act on attendance through the same margin as measures that expand provision. In the Rohingya response, as in most humanitarian architectures, protection and education are coordinated through separate sector structures with separate indicator frameworks and separate funding lines. The evidence here argues for treating school participation as a reported outcome of protection programming, and camp-level security as a planning input for education programming.

Provision for young children should be defended when insecurity rises, not reduced. No negative association is detected at any age below eleven, and among the youngest children the estimate is positive. Whatever weight is placed on that estimate, its operational implication does not depend on the interpretation: for a young child the learning center is a fixed location with adults present in a professional capacity, and the alternative to attendance is time spent in and around the shelter and the lanes of the camp, with the supervision it requires falling back on the household. Periods of heightened insecurity are also the periods in which a household's capacity to provide that supervision is most stretched. Suspending or consolidating provision at such moments therefore withdraws an arrangement at the point of greatest demand. The implication runs against the operational reflex, which is to curtail activities when the security situation deteriorates, and against the pattern of funding, under which education is among the first components to be reduced. Continuity of provision for the youngest age groups has a protection rationale independent of its learning rationale.

The two adolescent margins are different, and so are the interventions that reach them. The decision documented here is reported as the household's, through the main respondent, not the child's, and it takes a different form for daughters and for sons. For girls it is reported as a restriction on movement, and the pattern is age-graded: the estimate on restriction is negative among the youngest children, turns positive at nine and ten, and rises to 1.43 percentage points at thirteen and fourteen. The sex differential on this reason survives familywise correction, and it is absent among the youngest children, emerging from the ages at which mobility norms begin to bind. The constraint therefore appears to lie on the journey and on the household's assessment of it. It is addressed by provision at block rather than camp level, by female teachers and accompaniment arrangements, and by engagement with the adult who takes the decision – not by measures directed at the child, which leave the rule untouched. For boys the reported margin is economic and concentrated in early adolescence, and points instead to household income support or to transfers conditional on attendance.

These conclusions are bounded by what the design supports. The estimates are conditional associations rather than effects, although a placebo on the interval preceding the escalation returns coefficients indistinguishable from zero; exposure is measured at camp level, so every household in a camp is assigned the same value; the sample does not support disaggregation by age and sex jointly on attendance; and the evidence on mechanisms is suggestive, resting on a sparse withdrawal margin and on stated reasons that are interdependent and that record how a household describes a decision rather than what produced it. Additional survey rounds, exposure measured below the level of the camp, and direct measurement of provision would each extend what can be claimed. Until then, the finding to carry forward is not the magnitude of the average association but its structure: the association

between violence and schooling in protracted displacement is not uniform, and the children whose attendance moves with it are identifiable in advance.

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Appendix

Table A1: Alternative Fixed Effects Structures

Absorbed effects	Coefficient	Std. error	N	adj. R^2
Household, round (Table 4)	−0.00351**	(0.00156)	996	0.348
Camp, round	−0.00350**	(0.00157)	996	0.332
Sex × baseline age, round	−0.00317**	(0.00155)	996	0.335
PSU × camp, round	−0.00347**	(0.00170)	996	0.323
Child, baseline age × round	−0.00341**	(0.00159)	996	0.300
Child, sex × round	−0.00278*	(0.00161)	996	0.307
Baseline characteristics × round	−0.00298*	(0.00152)	994	0.363
Camp area, round	−0.00350**	(0.00157)	996	0.332

Notes: Each row reports the coefficient on the five-type exposure measure from a re-estimation of the preferred specification with the indicated effects absorbed. The first row reproduces column (3) of Table 4 for comparison. The full control set is included throughout and standard errors are clustered at the primary sampling unit level. Baseline characteristics denotes the child and household covariates measured at baseline, each interacted with the round indicator. The estimating sample varies across rows because the absorbed effects drop singleton groups. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Leave-One-Camp-Out Estimates

Camp excluded	Coefficient	Std. error	N
Camp 1E	−0.00408**	(0.00158)	924
Camp 2E	−0.00349**	(0.00159)	944
Camp 2W	−0.00352**	(0.00167)	936
Camp 3	−0.00352**	(0.00157)	940
Camp 4	−0.00306*	(0.00156)	966
Camp 4 Extension	−0.00349**	(0.00156)	982
Camp 6	−0.00374**	(0.00158)	940
Camp 8E	−0.00338**	(0.00157)	934
Camp 8W	−0.00248	(0.00170)	940
Camp 9	−0.00371**	(0.00160)	976
Camp 10	−0.00330**	(0.00157)	954
Camp 11	−0.00398**	(0.00174)	938
Camp 12	−0.00392**	(0.00173)	974
Camp 13	−0.00378**	(0.00156)	896
Camp 14	−0.00354**	(0.00158)	948
Camp 15	−0.00341**	(0.00163)	936
Camp 16	−0.00330**	(0.00157)	972
Camp 17	−0.00329**	(0.00158)	972
Camp 18	−0.00369**	(0.00163)	920
Camp 19	−0.00330*	(0.00171)	968
Camp 20	−0.00362**	(0.00156)	978
Camp 20 Extension	−0.00353**	(0.00157)	978

Notes: Each row reports the coefficient on the five-type exposure measure from a re-estimation of the specification in column (3) of Table 4, excluding the indicated camp. Household and round fixed effects and the full control set. Standard errors clustered at the primary sampling unit level in parentheses. Camps are identified by their official designation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Randomization Inference across Camps

	Value
Observed coefficient	−0.00351
Mean of permutation distribution	0.00009
Std. deviation of permutation distribution	0.00196
2.5th–97.5th percentile of distribution	−0.00356 to 0.00398
Permutations with $\lvert b\rvert \geq \lvert$observed$\rvert$	361
Two-sided p-value	0.072
Monte Carlo std. error of p-value	0.0037
95% interval for p-value	0.065 to 0.080
Reassignments	5,000
Camps	22

Notes: The camp-level exposure vector is randomly permuted across the twenty-two camps and the specification in column (3) of Table 4 re-estimated on each permutation, holding the outcome, the controls and the fixed effects structure fixed. The two-sided p-value is one plus the number of permutations with a coefficient at least as large in absolute value as the observed one, divided by one plus the number of permutations. The interval for the p-value is the point estimate plus or minus 1.96 Monte Carlo standard errors.

Table A4: Alternative Exposure Measure – Killing and Shooting Only

	(1) No controls	(2) Base model	(3) Full model
A. Standard errors clustered at primary sampling unit level			
Hard violence	−0.0177*	−0.0178*	−0.0224*
	(0.00932)	(0.00909)	(0.0120)
B. Standard errors clustered at camp level, with wild cluster bootstrap			
Hard violence	−0.0177	−0.0178	−0.0224
	(0.0103)	(0.00965)	(0.0118)
Wild bootstrap p-value	0.059	0.052	0.065
WCB 95% confidence interval	[−0.0701, 0.0010]	[−0.0655, 0.0004]	[−0.0823, 0.0018]
Controls and sample (both panels)			
adj. R^2	0.200	0.274	0.352
Controls	No	Yes	Yes
Fixed effects	HH, t	HH, t	HH, t
N	996	996	996

Notes: As Table 4, but the exposure measure counts only the two most severe incident types, killing and shooting. Because the two measures differ in scale, the coefficients are not directly comparable in magnitude; see the text. Wild cluster bootstrap p-values and confidence intervals are computed at camp level with Rademacher weights and 9,999 replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Romano-Wolf Adjusted p-values – Pooled Specification

	Unadjusted	Romano-Wolf
Disengagement	0.2361	0.4081
Safety concerns	0.4437	0.4081
Restrictions	0.2217	0.4081
Economic	0.3273	0.4081
Supply-side	0.0706	0.1879

Notes: Romano-Wolf step-down adjusted p-values (Romano & Wolf, 2005a, 2005b), computed with 999 bootstrap replications of which 989 were valid, resampled at the primary sampling unit level. Each family comprises the five stated-reason hypotheses tested jointly. The procedure controls the familywise error rate, that is, the probability of at least one false rejection within the family.

Table A6: Romano-Wolf Adjusted p-values – Adolescence versus Childhood

	Unadjusted	Romano-Wolf
Disengagement	0.381	0.431
Safety concerns	0.183	0.361
Restrictions	$<$0.001	0.022
Economic	0.002	0.064
Supply-side	0.379	0.431

Notes: The tested coefficient is the difference between the marginal effect at ages 11–14 and that at ages 7–10, from a specification that also includes a separate term for ages 15–17. Romano-Wolf step-down adjusted p-values (Romano & Wolf, 2005a, 2005b), computed with 999 bootstrap replications of which 989 were valid, resampled at the primary sampling unit level. Each family comprises the five stated-reason hypotheses tested jointly. The procedure controls the familywise error rate, that is, the probability of at least one false rejection within the family.