Educational Consequences of Firsthand Exposure to Armed Conflict: The Case of the Sri Lankan Civil War*

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This study examines the educational consequences of exposure to conflict using the case of the 1983-2009 Sri Lankan Civil War. Based on a strategy of within-sibling comparison, we estimate the educational impacts of households' firsthand experiences of different types of conflict events: human victimization, property damage, and war-induced migration. The estimation results show that the impacts of conflict exposure vary depending on the type of conflict event and the timing of exposure. In a worst-case scenario in which a household experiences two events that cause human victimization and property damage when a child is school-aged, the probability that the child completes upper secondary or higher education decreases by 97.2 percentage points, lowering educational attainment by 3.49 years (29.4% of the sample mean). Furthermore, the prolonged and significant impacts of property damage that we found suggest that providing immediate assistance to children in damaged households could be an effective measure for mitigating the loss of human capital in the next generation.

Keywords: armed conflict, education, household victimization, war-induced migration, Sri Lanka

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1. Introduction

Living in a conflict area is tantamount to being deprived of fundamental human rights. Children living in these areas are more likely to lose their lives (Kiros & Hogan, 2001; Gates et al., 2012; Dagnelie, Luca, & Maystadt, 2018; Wagner et al., 2018; Bendavid et al., 2021).[1] People who survive their childhood tend to be physically underdeveloped (Bundervoet, Verwimp, & Akresh, 2009; Akresh, Verwimp, & Bundervoet, 2011; Akresh et al., 2012; Akresh, Lucchetti, & Thirumurthy, 2012; Domingues & Barre, 2013; Akbulut-Yuksel, 2014; Minoiu & Shemyakina, 2012, 2014), suffer from mental disorders (Hadi & Llabre, 1998; Husain, Allwood, & Bell, 2008; Panter-Brick et al., 2009; Singhal, 2019),[2] and have lower educational attainment (Ichino and Winter-Ebmer 2004; Lai and Thyne, 2007; Elbert et al., 2009; Chamarbagwala & Morán, 2011; Shemyakina, 2011; León 2012; Akbulut-Yuksel, 2014; Dabalen & Paul, 2014; Verwimp & van Bavel, 2014; Swee, 2015; Islam et al., 2016; Singh & Shemyakina, 2016; Bertoni et al., 2019). Thus, the influence of deteriorated human capital may persist in the future and into the next generation through poor household circumstances, even after the conflict ends.[3]

Meanwhile, the international community still cannot vanquish the demon of absolute

[1] Dagnelie, Luca, and Maystadt (2018) found the heterogeneous impact of in-utero exposure to conflict events between the sexes based on the case of the Congolese Wars. The adverse impact on child mortality was found only among female children. They posited this impact being the result of the presence of the culling (or selection) effect; that is, weak male fetuses are more likely to be lost in utero under severe conditions, and hence, male infants who are born are relatively healthier than female infants. In addition to the linkage between conflict exposure and child mortality, some studies have documented that in-utero exposure to conflict events decreases birth weight (Mansour & Rees, 2012; Torche & Shwed, 2015).

[2] On the other hand, Do and Iyer (2012) found no impact of conflict exposure on mental health using the case of the Bosnian War.

[3] For instance, several studies have documented that conflict exposure causes decreases in earnings (Ichino & Winter-Ebmer, 2004; Akbulut-Yuksel, 2014) and consumption (Serneels & Verpoorten, 2015), which can be attributed to deteriorated human capital. Moreover, some empirical studies have found that parents' education plays an important role in improving child welfare, and therefore, loss of education due to war can prolong the damage due to armed conflicts into postconflict generations (Kiros & Hogan, 2001; Akbulut-Yuksel, 2014). Meanwhile, studies on the aggregate impact of conflict show that, despite the short- or medium-run detrimental impact of the war, within a period of 20 to 30 years, prewar trends of economic development could be revived (Davis & Weinstein, 2002; Miguel & Roland, 2011).

deprivation two decades into the twenty-first century. Instead, since the end of WWII, the number of armed conflicts has not decreased, as civil war (or intrastate conflict) has become the most prevalent form of warfare (Strand et al., 2019). Although the scale of conflicts has reduced in terms of total casualties per conflict per year, they tend to be prolonged, and populated civilian areas have increasingly become a battlefield or target for terrorism, resulting in increased civilian victimization, including death, injury, or displacement (Epps, 2013; IOM, 2018; UNHCR, 2018). Therefore, given the devastating nature of armed conflicts, elucidating their consequences has become an increasingly important line of research in its own right and in terms of providing effective humanitarian aid and reconstruction policies in wartime and postwar periods.

In this study, we examine the impact of exposure to conflict events on education using the case of the Sri Lankan Civil War between 1983 and 2009, which was one of the longest ethnic conflicts in the world (D'Costa, 2013). The war was fought between the Sri Lankan government forces and a separatist ethnic group, which aimed to build an independent state in the Northern and Eastern provinces of the island. In the several months of the war's final phase alone, the period of our main interest, it is estimated that 40,000 civilians were killed and nearly 300,000 were involuntarily displaced (UN, 2011).

This empirical study adds to the literature by quantitatively examining the educational impact of firsthand experiences of exposure to conflict events. Most quantitative studies of the consequences of war exposure, with a few exceptions mentioned later, utilize regional aggregate measures, such as the number of violent events and casualties at some point in time within a certain distance or administrative unit, based on media reports. However, studies using such aggregate measures of conflict exposure have been unable to correctly determine people's actual exposure to conflict, thus potentially ending up estimating average treatment effects on both victims and

nonvictims.[4] This may lead to misleading results in which the actual impacts of conflict exposure are understated and specific causal mechanisms remain unanswered. Focusing on experiences of being directly exposed to conflict may enable us to address these drawbacks.

At the same time, however, estimating the causal impact of firsthand experiences of conflict exposure involves empirically difficult issues because individual/household decisions may mediate the cause and effect. For example, conflict-induced migration is partly a result of household decisions, and the loss of family members may also be an outcome of residential selection, including such migration decisions. In the current context, family beliefs on child welfare, which are often unobservable and potentially affect residential choice, could be a confounder of firsthand conflict exposure. This study addresses the issue by employing a strategy of within-sibling comparison; the source of identifying variation stems from differences in the timing of exposure between siblings in the same decision unit—the household. Furthermore, our data were collected from only conflict-affected regions, and hence, the control group comprises individuals who have been indirectly exposed to conflict. Therefore, geographical heterogeneity between the treated and control groups is considered to be very small. Given the limitations of the empirical approach to conflict studies, in which conducting experiments is absolutely impossible, our empirical strategy is one of the most reliable approaches available utilizing observational data.

Moreover, this study distinguishes among types of conflict events (namely, human victimization, property damage, and displacement) and differences in the timing of exposure to the events. While some quantitative studies have examined the impact of individual/household direct exposure to conflict, studies examining several different channels of exposure simultaneously are

[4] In addition, many studies employ a difference-in-differences (DID) approach based on comparisons between conflict-affected and nonaffected areas, often with assertive assumptions that people in both areas have been living in the current place since wartime and have the same trend in the potential outcome.

sparse in the literature.[5] For example, Ichino and Winter-Ebmer (2004), who explored the impact of WWII on education, employed information about fathers' involvement in the army and the loss of fathers during the war in the analysis using the 1984–1986 waves of the German Socioeconomic Panel (GSOEP).[6] In addition, Minoiu and Shemyakina (2014) examined the impact of self-reported experiences of economic loss, mental illness, and displacement on child health utilizing the case of the 2002-2007 civil war in Côte d'Ivoire.

These studies, however, share a common feature: They do not address the issue discussed above, namely, the impact of unobserved heterogeneity among households. Moreover, no study has taken into consideration differences in the impacts due to differences in the timing of exposure. A lack of understanding of the cause-and-effect linkage between specific forms of conflict victimization and their consequences hinders the design and implementation of essential policy measures to mitigate the ravages of armed conflicts. Thus, based on a rigorous methodology, this study further advances the literature by elucidating the differences in the effects of conflict through the specific channels and timing of war exposure.

The estimation results show that the adverse impacts of war exposure on education vary depending on the type of conflict event and the timing of exposure. In a worst-case scenario in which a household experiences two events that cause both human and property damage when a

[5] There is a large body of literature in medicine and psychology that has examined the consequences of direct individual/household exposure to war events based on qualitative or t test/correlation analysis, often focusing on short-term effects, with relatively small sample sizes. For studies on the association between war exposure and education-related outcomes, see, for example, Hadi and Llabre (1998), Rousseau and Drapeau (2000), Elbert et al. (2009), and Diab and Schultz (2021).

[6] For other studies addressing a single pathway (or a couple of similar pathways) of conflict exposure, for example, Shemyakina (2011), who investigated the educational impact of the armed conflict in Tajikistan from 1992 to 1998 employing households' reports of damage to their own dwellings. Akbulut-Yuksel (2014) also investigated the impact of WWII using the 1985 wave of the GSOEP, with variables similar to those used in Ichino and Winter-Ebmer (2004). While Shemyakina (2011) addressed unobserved heterogeneity among households by controlling for household fixed effects, the other studies controlled for unobserved fixed effects at the birth cohort or administrative unit level but not the household (sibling) level.

child is school-aged, the probability that the child completes upper secondary or higher education decreases by 97.2 percentage points, resulting in a reduction of 3.49 years of schooling. Moreover, a comparison with the results employing war exposure aggregated at the district level suggests that the possible underestimation of the impacts of conflict exposure in conventional analyses is relatively small, although such analyses cannot disentangle the specific causal pathways.

The remainder of the paper is structured as follows. In the next section, we provide a brief overview of the Sri Lankan Civil War. In Section 3, we explain the dataset used in the analysis and empirical framework and discuss the identification issues. Section 4 presents empirical evidence for the educational impacts of exposure to the war. Finally, Section 5 concludes the paper.

2. Brief Overview of the Civil Conflict in Sri Lanka

The Sri Lankan Civil War was fought between the separatist Tamils, the Liberation Tigers of Tamil Eelam (LTTE), and the Sinhala-dominant government from 1983 to 2009. The LTTE aimed to create an independent Tamil state in the Northern and Eastern provinces of the island.[7] The timeline of the war is divided into four phases: Eelam Wars I, II, III, and IV.[8] Figure 1 shows the geographical transition of the war in terms of casualties. The LTTE was in control of the majority of the Northern and Eastern provinces at its peak before the fourth phase of the war started.

[Insert Figure 1 Here]

Eelam War IV began in July 2006, when the government commenced hostilities by bombing several LTTE camps with a casus belli that the LTTE cut off water supply to the rice-

[7] The Northern Province comprises five districts: Jaffna, Kilinochchi, Mannar, Mullattivu, and Vavuniya. The Eastern Province consists of three districts: Trincomalee, Batticaloa, and Ampara.
[8] See Li, Yamazaki, and Ito (2019) for more details on the history and evolution of the Sri Lankan Civil War.

growing fields in the Mavil Aru area in the eastern Trincomalee district. In December 2006, the government started to eliminate the LTTE from the Eastern Province and eventually took control of the province in July 2007 (Gardner, 2006, 2007). This was the beginning of the final defeat and retreat of the LTTE. The LTTE lost control over territories in the north, including Kilinochchi, its *de facto* capital, in January 2009 (Sirilal, 2009). After controversial battles involving the use of Tamil civilians as "human shields" (Reuters Staff, 2009), the government forces eventually defeated the LTTE, and the civil war ended in May 2009.

Of the four phases, the fourth phase, which is most closely related to this study because our sample consists mainly of children who were school-aged during this phase, was the most devastating. Although the estimation of human damage during the Eelam War, especially the fourth phase, has been a controversial issue and there are no official figures for civilian casualties and IDPs, a UN report (UN, 2011) indicates that 40,000 civilians may have been killed and nearly 300,000 displaced from their homes.

3. Research Design

(a) *Data*

To examine the educational impact of household-specific exposure to conflict events during the Sri Lankan Civil War, we carried out an original household survey in the conflict zones, the Northern and Eastern provinces, in March and April 2018. The survey was conducted through a local research company under the direction of the authors, employing multistage stratified random sampling. To obtain a representative sample given the size of our budget, we set the sample size at 1,600 (250 households from eight conflict-affected districts in the two provinces).[9]

[9] See Online Appendix A for details on the sampling method and sample characteristics.

The questionnaire used in the survey contained questions regarding conflict victimization as well as standard questions such as family roster, labor activity, and education. The main respondents were heads of the household or their spouses, and female members with children were also included as respondents for several questions about conflict victimization and migration and pregnancy histories. At the beginning of the survey, we obtained informed consent from the respondents and explained that they could refuse to answer any question and terminate their participation at any time.[10]

Regarding conflict victimization, this study focuses on conflict events associated with (a) the loss of family, friends and neighbors, (b) property damage, and (c) conflict-induced migration. Regarding the loss of family/friends/neighbors, respondents were asked about experiences in which family members (including relatives) and friends/neighbors were killed or are still missing.[11] Property damage included damage to homes and land. For questions regarding human and property damage, the target respondents were heads of the household or their spouses, and we obtained answers from 1,505 households (or 94.1% of all households).[12] Among them, 950 households (or 63.1%) reported at least one relevant event. Regarding migration history, the target respondents were mothers with children aged 25 or younger. This was to collect the migration history of school-age children during the conflict. There were 1,286 households with an eligible mother. Of them, 456 households (or 35.5%) reported experiences related to war-induced migration. Note that war-induced migration is defined as migration for which the respondent

[10] We obtained written informed consent from the main respondents (usually household heads or head's spouses) and oral informed consent from other subrespondents. In addition, the research project including the survey design was approved by the Research Ethics Committee of the Graduate School of International Cooperation Studies, Kobe University.

[11] In the questionnaire, friends and neighbors killed (or still missing) were asked in the same question, and therefore, we cannot distinguish them.

[12] In the remaining 95 households, respondents are other household members than the heads and heads' spouses.

specified war as the cause or migration whose destination was an IDP camp, following the definition used by the International Organization for Migration (IOM).[13]

Note that, as explained above, our conflict exposure variables are based on the respondents' answers to retrospective questions. In the context of violent conflict, the safety of enumerators often leaves no choice but to rely on retrospective questions that collect information during the conflict in postconflict areas, which has led to many existing studies of conflict relying on recall data (de Walque, 2011). However, when using subjective data from retrospective questions, reporting issues such as response refusals or recall errors may arise. According to the enumerators' reports, there were no cases in which respondents refused to answer conflict-related questions. However, there may have been several cases in which respondents did not refuse explicitly but answered that they had no experiences, despite having them. This implicit refusal may then cause the impact of conflict exposure to be understated, as unreported victims are included in nonvictims. On the other hand, recall errors can lead to either underestimation or overestimation. If recall ability declines over time, then longer recall periods may result in little or no recollection of a particular event. Conversely, overestimation may occur when strong influences from an experience are deeply linked to memory consolidation.

Unfortunately, we cannot verify the presence of reporting problems and the magnitude of the bias they create. However, regardless of underestimation or overestimation, misreporting may be less significant. For example, Moreno-Serra et al. (2022), who examined the issue of recall errors using postconflict surveys in Colombia, found no systematic differences in respondents'

[13] IOM defines war-induced migration or displacement as "a migratory movement in which an element of coercion exists, including threats to life and livelihood, whether arising from natural or man-made causes." Note that in reality, it is very difficult to distinguish between voluntary (precautionary) migration and forced migration in many cases, especially when using self-reported information. Thus, we admit that our variable of war-induced migration cannot exclude voluntary migrations.

ability to recall or in patterns of misreporting of postconflict health data across levels of conflict intensity.[14] Furthermore, as explained in the next section, our empirical strategy is based on the comparison of siblings within the same household (i.e., of the same respondent). Thus, the possible influences of unobserved heterogeneity at the household (respondent) level, if any, can be eliminated to some extent, leading to relatively less bias than in a naïve comparison across households.

[Insert Figure 2 Here]

Figure 2 presents the distribution of the number of events for each case. Regarding the loss of family/friends/neighbors in Panel A, 915 households (or 57.2%) reported one event or more. Among all killed/missing persons, only respondents' spouses were identified, with 47 cases. This number implies that 1.97% of couples (heads and their spouses) who had been married before the end of the war experienced partner death.[15] Regarding property damage in Panel B, only 148 households (9.3%) experienced damage to their property, such as homes and land. Regarding war-induced migration reported in Panel C, the majority of 456 households that experienced war-induced migration migrated two times or more.

(b) ***Empirical Framework***

To estimate the effect of firsthand exposure to conflict events on children's educational attainment,

[14] It should be noted, however, that the retrospective questions in their study were about information from four to five years ago, while our conflict data are based on retrospective information from about 10 years ago (when limited to the final phase of the conflict).

[15] The estimated death toll in the final phase of the Sri Lankan Civil War is reported to be approximately 40,000 (UN, 2011) and the total population in the Northern and Eastern provinces in Census 2012 was 2,616,825. Therefore, the death rate was estimated to be 1.51% (= 40,000/(40,000 + 2,616,825). Thus, the 1.97% marital death rate calculated from our survey data is slightly higher than the estimate from the UN report. Although our estimate does not include children, one of the most vulnerable groups, it is considered a reasonable estimate given that the survey data cover the entire period of the civil war.

this study employs a strategy of within-sibling comparison using the following equation:

$$Educ_{ijc} = \alpha + \sum_{s} Exposure_{is}\,\beta_s + \boldsymbol{X}_i\boldsymbol{\gamma} + \delta_j + \delta_c + \varepsilon_{ijc}, \quad (1)$$

where $Educ_{ijc}$ is an educational outcome of child i, household (parents) j, and birth cohort c; $Exposure_{is}$ denotes household/parents-level conflict exposure when child i was at a lifecycle stage s (e.g., before and during school ages); $\boldsymbol{X}_i$ is the vector of individual characteristics (birth order and gender); δ_j and δ_c are sibling and birth cohort fixed effects; and ε_{ijc} represents an unobserved component. This DID-type framework enables us to eliminate the influence of household-level unobservables. Thus, the source of identifying variation stems from differences in the timing of exposure between siblings in the same household.

For educational outcomes ($Educ_{ijc}$) in Equation (1), we employ several measures. First, we focus on the grade last attended (or currently attending) rather than the actual years of schooling to distinguish grade progression as educational performance from grade repetition. The second measure is the number of grade repetitions. Third, to understand the mechanism of the impact of conflict exposure precisely at different education levels, we focus on the completion of lower secondary (grades 6 to 9) and upper secondary (grades 10 to 13) education.[16] The fourth measure is whether or not the person has still not completed their education, that is, whether they are currently attending school/university or preparing for the next education level.

For the variable of firsthand experiences of conflict exposure ($Exposure_{is}$), as mentioned in the previous section, we focus on three different aspects: (a) the loss of family/friends/neighbors, (b) property damage, and (c) war-induced migration. Regarding the first measure, we employ the

[16] The Sri Lankan education structure broadly comprises five stages: primary (grades 1 to 5), junior secondary (grades 6 to 9), senior secondary (grades 10 to 11), collegiate (grades 12 to 13), and tertiary levels. In this study, grades 6 to 9 are classified as lower secondary education, and grades 10 to 13 are classified as upper secondary education.

number of events in which individuals lost people close to them, such as family members and friends/neighbors. As a variable for property damage, we use the number of times that houses and assets were damaged. The third measure, war-induced migration, is proxied by the number of times children migrated with mothers from their living place at that time due to conflict. For the timing of exposure to these experiences, we focus on three different periods: the periods of prebirth (from 5 years before birth to birth), preschool age (from birth to 5 years of age), and school age (from 6 to 16 years).[17] The first two periods are expected to capture the prolonged influences of war exposure on education through a deteriorated in utero and early-life environment.[18]

We also note that, in the above equation, we do not control for changes in the supply factors of education caused by conflict, such as the destruction of school facilities, the closure of schools, or the loss of teachers. There is evidence in the literature that educational facilities in conflict areas are often subject to destruction (UNDP, 2005, p. 159; Akbulut-Yuksel, 2014), and Sri Lanka is no exception (UNESCO, 2010, pp. 226-227). However, our survey data contain no information on supply-side conditions of education, and therefore, we cannot rule out the possibility that the effects of our conflict exposure variables are confounded by the effects of supply-side changes. This possibility is further discussed in Sections 4(a) and 4(b).

The sample used in the analysis comprises children born between 1992 and 2001 (aged between 16 and 25 years at the time of the survey). People who were potentially affected by the

[17] Regarding the timing of events, anticipating that many respondents would not be able to remember the exact year of events, we allowed respondents to choose a five-year range for the timing and 68.71% of answers were in the range form. In such cases, we assign a fraction (i.e., 1/5 event) to each year in the range. In addition, 85.02% of answers have no information on the month the event happened. When the month information is missing, we inserted 6 by assuming that it happened in the middle of the year, June. This also hinders the use of shorter periods such as fetal and infant periods, and we focus on the three relatively longer periods above.

[18] A sizable body of literature has documented the profound influence of in-utero and early-life health conditions on socioeconomic outcomes in later life, including educational attainment (see Currie and Almond 2011).

conflict during their school-age period were aged 5/6 years or older in 2009. In addition, the enrollment rate at the lower secondary education level is considerably high in Sri Lanka,[19] and therefore, we focus on children born in 2001 or earlier (age 16/17 years old, the standard age at which they have already completed secondary education, or older). Moreover, older children are more likely to leave their parents' house for a job or marriage; hence, we set children aged 25 years (mainly born in 1992) as the oldest cohort and included questions about all children aged 25 or less in the roster and education section of the questionnaire, regardless of their current place of residence.[20] Furthermore, we restrict the sample to sons and daughters of household heads. This is because the target respondents for conflict-related questions were set to be household heads or spouses, and respondents in 1,505 households (or 94.1%) were either of them or both. Among the 1,505 households, only 601 had sons or daughters who were born between 1992 and 2001. Finally, excluding those whose education data are missing, the sample we use in the analysis comprises 898 children from 572 households. Table 1 shows the summary statistics of the variables regarding educational attainment ($Educ_{ijc}$) and household-level war intensity ($Exposure_{is}$).

[Insert Table 1 Here]

(c) ***Issues in the Empirical Strategy***

Our empirical strategy, a within-sibling comparison, still relies on the assumption that there are no systematic differences in the *potential* educational attainments of children between households and

[19] According to UIS (2023), the gross intake ratio to the last grade of secondary education (as a percentage of the relevant age group) in Sri Lanka in 2016 was 94.75%. For comparison, the gross enrollment rate at the secondary level calculated from our survey data is over 100%, while that at the tertiary level is 49.2%.
[20] Nevertheless, male-to-female ratio among children aged 25 or less increases as age increases in our sample. This may be because respondents tend to forget to include married children and marriage age is on average lower for females than males. The distorted gender ratio may lead to a possible sample selection problem, which we discuss later.

between siblings within the same household, regardless of whether they have been directly exposed to conflict. In other words, between- and within-household unobserved heterogeneities in children's education need to be independent of the degree and timing of exposure to conflict events.

To validate the strategy, we first implement balancing tests (Table 2). In Panel A of Table 2, we compare the characteristics of households with firsthand experiences of conflict events (loss of family/friends/neighbors, property damage, and war-induced migration) and those without. Panels B and C compare individual characteristics by classifying the sample individuals into two groups according to conflict exposure during preschool-age (Panel B) and school-age (Panel C) periods. Significant differences in variables between groups indicate that conflict exposure is associated with those characteristics, which may cause our estimates to be biased.

[Insert Table 2 Here]

Although no statistically significant differences in characteristics are found at the household level (Panel A), the timing of conflict exposure at the individual level is associated with age (Panels B and C) and birth order (Panel B) in our sample. However, given the nonexperimental nature of our research design, possible correlations between conflict exposure and these variables are not surprising. The results in Panels B and C show that individuals who were exposed to conflict events during preschool and school age were older than those who were not. This may be simply because the war had slowed in some areas before children in younger cohorts were born. As explained in Section 2, for example, the war in the Eastern Province ended in 2007. Alternatively, parents of the exposed individuals tended to have fewer children after experiencing conflict events, or their younger siblings may have died, given that infants are more likely to be vulnerable. Thus, as shown in Panel B, the sample individuals exposed to conflict during preschool age had a relatively earlier birth order and fewer siblings than those who did not.

In our sibling comparison strategy, although we can include individual characteristics as control variables, it is not possible to control any (time-invariant) household characteristics. Therefore, the lack of a significant difference in household-level variables found in Panel A is favorable because it implies no differences in preexisting trends in children's educational outcomes between exposed and nonexposed households. Meanwhile, possible correlations between conflict exposure and individual characteristics such as age and birth order, although they are thought to be "spurious" relationships, as mentioned above, may confound the impact of firsthand exposure to conflict events. Therefore, in the analysis, we carefully address possible biases due to heterogeneous trends caused by birth cohort.

Note also that Table 1 and Panels B and C of Table 2 show that the female proportion in our sample is relatively small. As already explained in Section 3(b), our sample consists of sons and daughters of household heads, who tend to leave their parents' homes for jobs or marriages as they mature. Although questions about all children aged 25 years old or younger (regardless of their current living place) were included in the roster and education section of the questionnaire, the male-to-female ratio in the sample increases with age, which might reflect that girls tend to marry and leave home earlier than boys, so the main respondents might have forgotten to include them in their answers.[21] If conflict exposure indeed lowers educational attainment and females with lower levels of education leave home for marriage earlier than those who were not affected by conflict (because education level is often negatively associated with marriage age), gender may be associated with the treatment status. This may cause a sample selection problem, although in the current context, we underestimate the adverse impact of conflict on education for females. In the analysis, we also address possible differences in trends between the sexes.

[21] In fact, the male-to-female ratio in our sample is 113.6% for those aged between 16 and 20 (born between 1997 and 2001), and 160.6% for those aged between 21 and 25 (born between 1992 and 1996).

Related to the above point, there is another issue due to our identification strategy. As several studies pointed out, war exposure may have heterogeneous impacts across genders.[22] However, we cannot allow the impact of conflict exposure to differ according to gender: Only 243 of the 898 sample children have same-gender siblings, and hence, there is little variation in the timing of the exposure between siblings of the same gender. Furthermore, for the same reason, we cannot investigate the impact of the death of a parent. Although it is easy to imagine that parental death in childhood has a considerable influence on childhood education (Ichino and Winter-Ebmer 2004; Akbulut-Yuksel 2014), there were only 47 cases in which household heads' spouses were dead (or still missing) due to conflict events. Therefore, we cannot explore these issues in the analysis, which is one of the drawbacks of our sibling comparison strategy.

4. Estimation Results

(a) *Baseline Results*

Table 3 reports the estimated impact of household-level firsthand exposure to conflict events on educational attainment measured as the grade last attended (or currently attending). Regarding the treatment variables (exposure to conflict events), we estimate different specifications, including loss of family/friends/neighbors (Columns 1 to 2), property damage (Columns 3 to 4), war-induced

[22] Gender differences in the impacts of war exposure on education can vary depending on the circumstances surrounding the conflict or traditional gender role in the society (Shemyakina, 2011; Buvinić, Das Gupta, & Shemyakina, 2014). In the context of developing countries, the role of children in the family is generally determined by gender, and this can be one of main causes of gender-based educational inequality (Levison, Moe, & Marie Knaul, 2001; Assaad, Levison, & Zibani, 2010; Dhital et al., 2022). However, the gender gap in educational attainments in Sri Lanka has been quite small; for example, according to the 1997 Human Development Report (UNDP, 1998), the adult literacy rate and the combined gross enrollment rate in primary, secondary, and tertiary education for males are 93.2% and 65%, respectively, while those for females are 86.9% and 68%, indicating gender gaps that are much smaller than the average gaps for developing countries. Therefore, gender-based heterogeneity in the impacts of war exposure through this mechanism is considered to be low.

migration (Columns 5 to 6), and all conflict events (Column 7). We also focus on the timing of exposure, dividing the period into three periods: prebirth (from 5 years before birth to birth), preschool-age (from birth to 5 years old), and school-age (6 to 16 years old) periods. The sibling and birth cohort fixed effects are controlled for in all specifications. In addition, we further control for individual-level variables (such as sex and birth order) and district-specific linear time trends (i.e., interaction terms between birth year and district dummies) in Columns 2, 4, 6, and 7. The inclusion of district-specific linear time trends is intended to account for heterogeneity in time trends across districts. As discussed in Section 3(b), this is expected to eliminate the possible influence of district-level educational supply factors. It also aims to address the results of the balancing test in Table 2, which suggested that treatment effects could be confounded by birth cohort trends.

[Insert Table 3 Here]

We start by investigating the impact of human damage (Columns 1 and 2). The estimated impacts of the number of conflict events with the loss of family members and friends/neighbors are always negative, and exposure during the school-age period has the largest influence. Column 1 shows that an increase in the number of events during preschool- and school-age periods decreases educational attainment by 0.898 and 1.504 years, respectively. When we control for individual-level variables and district-specific linear time trends (Column 2), the size of the coefficients remains almost unchanged, although the statistical significance of the coefficient for exposure during the preschool period decreases. Note also that firsthand experiences of deaths of close persons are expected to affect educational attainments mainly through mental stress due to traumatic events. If this is the case, it is plausible that the events during the after-birth periods affect educational attainment more than those during the prebirth period.

Turning to property damage (Columns 3 and 4), the estimated impacts are negative and statistically significant in all three periods. After controlling for individual-level variables and district-specific linear trends, the estimates change slightly, but most of them increase in magnitude. The estimated coefficients in Column 4 suggest that an increase in the number of times properties were damaged reduces the number of years of education by approximately 2.3 to 2.5 when the damage occurs during the prebirth and early childhood periods and by 3.2 years when it occurs during the school-age period. Thus, in the case of property damage, the experience during the prebirth period has a prolonged impact on subsequent educational outcomes. This may be because it takes several years to recover from damage to property.[23]

Regarding the influences of war-induced migration (Columns 5 and 6), the estimated impacts of migration during the prebirth period are positive and statistically significant. This positive impact may reflect that those who were born in safer places (possibly outside the conflict area) are more likely to achieve slightly longer years of schooling than those who did not migrate and those who migrated during the school-age period. Another possible explanation is that households with outside options for immediate evacuation may be wealthier, and this may lead to a positive income effect. In addition, government announcements and several case reports, as well as our field interviews conducted in 2017 in an IDP camp, indicate that during the war, the government and international organizations were committed to continuing education in IDP camps.[24] Taken together, these factors may have mitigated and offset the potential negative effects

[23] This prolonged adverse impact on education can also be attributed to poor maternal health during pregnancy (Almond 2006; Black, Devereaux & Salvanes 2007; Almond, Edlund & Palme 2009; Aizer, Stroud, & Buka 2016). However, this possibility may be less likely given the results in Columns 1 and 2 that loss of close people during the prebirth period has little impact.

[24] See, for instance, Schack (2000) and Uyangoda (2013). On the other hand, Williams (2010) reported that the quality and quantity of education provided in the camps was not of a high enough standard and that the interventions had limited results.

of conflict displacement. Here, again, controlling for individual characteristics and district linear time trends does not change the coefficient estimates of war-induced migration.

Next, when we include all three measures of conflict exposure (Column 7), the estimated coefficients of events with loss of family/friends/neighbors and property damage slightly decrease in magnitude. This indicates that these different measures of conflict exposure are correlated with each other to some extent. Notably, the results in Table 3 that economic loss has a large negative impact on education and that displacement has a positive impact are consistent with the findings of Minoiu and Shemyakina (2014).

It should be noted that our conflict exposure variables do not necessarily elucidate the detailed mechanisms of causality. For example, the main channel of the impact of damage to property (houses and land) is likely to be through income effects, but as already noted, it may also include the impact of damage to school facilities. Alternatively, the death of family/friends/neighbors could include the death of school teachers and may capture its consequences, such as school closure. Furthermore, parents may have refrained from sending their children to school for fear that their children would be involved in combat.[25] Although it is impossible to disentangle all possible channels, we attempt to unravel the pathways in more detail in the following two sections.

(b) *Robustness Checks*

First, we further examine the validity of the identification assumption. Although birth year fixed effects and district-specific linear time trends are controlled for in Table 3, the possibility remains that the effects of war exposure are confounded with unobserved time trends within districts. For

[25] For example, anecdotal evidence of such cases is reported in Boyden (1996).

example, as noted above, the estimated effects of our war exposure variables may also capture the effects of war-induced changes in educational supply conditions. To check this possibility further, we additionally control for district-specific cubic trends or district-year fixed effects. If district-specific unobserved heterogeneity, including changes in the supply side of education, confounded our estimates, the inclusion of these additional trend terms would change the estimates substantially. In addition, as discussed in Section 3(c), older cohorts contain more male than female subjects, and therefore, we also include gender-specific trends to allow time (birth cohort) trends to differ by gender.

Furthermore, we limit the sample to 716 children from 432 households who had experienced at least one conflict event. Although the sample used in Table 3 consists of children in conflict-affected areas, households with firsthand experience of conflict events may have different characteristics or preferences for education than those without. These differences may create heterogeneity in educational investment. Therefore, to mitigate the heterogeneity between the treatment and control groups and hence any resulting potential bias, we use the subsample comprising children in households who reported that they experienced loss of family/friends/neighbors, property damage, or war-induced migration.

In Figure 3, the estimated coefficients on the treatment variables, i.e., the number of events resulting in the deaths (or missing) of family/friends/neighbors (Panel A), properties damaged (Panel B), and war-induced migration (Panel C), are reported. The point estimates are denoted by symbols, and lines represent the 90% confidence intervals. The baseline estimates in Column 7 of Table 3 are indicated by circles, and those from the robustness checks discussed above are indicated by squares, triangles, and crosses. The graphs show that most of the coefficient estimates are considerably stable across all specifications. The estimates of property damage when

controlling for district-year (and gender-year) fixed effects are the only exceptions, but the estimates tend to be larger in absolute terms. Thus, we found no evidence that the adverse impacts of property damage on education (Table 3) are driven by time-varying unobserved heterogeneity at the district level. Such heterogeneity includes changes in the supply-side conditions of education at the district level. In summary, no systematic changes are found in the estimates after controlling for district- and gender-specific heterogeneity that varies over time and after restricting the sample to children from exposed households only.

[Insert Figure 3 Here]

Finally, we estimate the bias-adjusted treatment effects proposed by Oster (2019) to address the concern that our identification strategy is invalid and that the estimated coefficients contain bias from unobserved characteristics. A detailed explanation of this estimation and the corresponding results is provided in Online Appendix B, and here, we report the results briefly. The bias-adjusted coefficient estimates reported in Figure A1 are very stable across all the scenarios examined regarding the influence of unobservables. In particular, the estimated impacts of property damage in all three periods and of human victimization during the school-age period are always negative, implying that the main results in Table 3 are highly robust. Together with the results presented in Figure 3 above, these results mean that the main findings are less likely to be driven by possible confounders or pre-existing trends.

(c) ***Further Discussion***

To further investigate how and at which education stage conflict events affect individuals the most, we employ different educational outcomes, such as the number of grade repetitions, dummies for the completion of lower secondary (grades 6 to 9) and upper secondary (grades 10 to 13) education,

and education continuation (current attendance at a school/university or preparing for a higher level of education). We also examine the impact of conflict exposure on employment status, which is measured as an indicator variable that takes a value of one if individuals are self-employed or wage employed and zero otherwise. If negative income shocks are the main driver of the adverse impacts of human victimization and property damage, then children who are exposed more to conflict events are more likely to engage in wage work than those who are exposed less. Conversely, if the main pathway is that human and property damage have harmed children's mental health, this may reduce their participation in the labor market.

The estimation results based on the full specification as in Column 7 of Table 3 are reported in Table A3 in Online Appendix C. In Table 4, we report the impacts of war exposure under two scenarios: one in which a household experiences a human victimization event (Panel A) and the other in which a household experiences both a human victimization event and a property damage event (Panel B). While the first scenario can be considered relatively common, the second scenario is one of the worst cases. In our sample, there are 564 children who have experienced one or more of the three conflict events in any of the three periods. Of these children, 363 (or 64.4%) experienced at least one human victimization event, and only 34 (or 6.0%) experienced two or more events that caused human and property damage in the same period.

The results in Table 4 can be summarized as follows. Conflict exposure has the largest negative impact on educational attainment when the two events occur during the school-age period, reducing the completion rate of upper secondary education by 97.2 percentage points and resulting in a 3.49-year reduction in education attainment, which accounts for 29.4% of the average years of education in the sample (Columns 1 and 4 of Panel B, respectively). Conversely, conflict exposure during the school-age period has limited influences on grade repetitions, the completion

of lower secondary education, and education continuation (Columns 2, 3, and 5, respectively). Considered together, these results indicate that school-age children from households affected by conflict events are more likely to drop out of school during their upper secondary education without repeating grades or continuing education. This finding is also partially confirmed by the results in Column 6 that children exposed to conflict events during school age are more likely to work.[26] The positive impact on labor participation suggests that children in exposed households are more likely to leave school early primarily because of their contribution to household income rather than because of the negative impact on their mental health.

[Insert Table 4 Here]

Finally, we compare the results using household-level conflict exposure (reported in Column 7 of Table 3) with those using geographical variations in conflict exposure. As Akbulut-Yuksel (2014) implied, studies that utilized objective measures of conflict exposure, such as the aggregated number of violent events and casualties within a certain distance or administrative unit, might tend to underestimate the impact of war exposure because what they estimated is the average treatment effect on both victims and nonvictims.[27] To investigate this possibility, in Table 5, we compare the estimation results using firsthand conflict exposure from our survey (Panel A) and those using district-level exposure from the Global Terrorism Database (Panel B). The results

[26] The mean of the dependent variables reported in Columns 6 and 7 of Table 4 indicates that 64.7% of the sample children aged between 16 and 25 are continuously studying (39.5%) or working (25.2%), and the remaining children accounts for 35.2%. Among them, 19.9% are looking for a job or taking a temporary leave; 7.0% are homemakers or helping with domestic chores, and 5.7% did not answer.

[27] As the size and intensity differ from conflict to conflict, the results in this study and those in previous studies based on geographical variations in conflict exposure should be compared with caution, but the estimated impact of conflict exposure in this study seem larger than their impacts. For example, a relatively comparable study is León (2012) that explored the civil conflict in Perú from 1980 to 1993 with approximately 70,000 deaths. Whereas our result in Column 1 of Table 4 shows that the effect for the average child exposed to conflict events at least once after birth is 1.99 fewer years of education (= – 0.444– 1.546), that in León (2012) is 0.56 (according to the result for school-age children shown in Column 1 of Table 7).

based on the baseline specification (as in Column 7 of Table 3) are shown in Column 1, and those based on the full specification where district cubic and gender cubic trend terms are additionally controlled are shown in Column 2.

[Insert Table 5 Here]

For ease of comparison, Table 5 reports the impacts of an average level of exposure in each of three periods. The results in Column 2 of Panel A indicate that if a household with no exposure to conflict events experiences an average level of exposure when the child is of preschool and school age,[28] the child's educational attainment decreases by 0.44 and 1.57 years, respectively. Turning to the results in Panel B using district-level war exposure, none of the estimates are statistically significant, but the magnitudes are relatively stable in both specifications. Column 2 of Panel B shows that if the aggregated measure of war intensity is the average level in each period (27.4 and 36.6 events during the preschool- and school-age periods, respectively), children's years of schooling are reduced by 0.869 in the preschool-age period and by 0.889 in the school-age period.

Comparing the results of Panels A and B, the total impacts during the preschool- and school-age periods estimated using district-level war exposure (Panel B) are comparable to those using household-level exposure (Panel A) to some extent. Although the estimates in Panel B become smaller (in magnitude) and imprecise once district-specific and gender-specific cubic trends are controlled for, the total estimated impact during these periods explains 87.4% of this study's estimate (Column 2). This suggests that while studies employing aggregated measures of

[28] Regarding household-level firsthand conflict exposure, 30.6% (or 275), 34.0% (or 305), and 55.2% (or 496) of the sample children were exposed to one or more conflict events in each period, and the average numbers of the three types of events (i.e., the loss of family/friends/neighbors, property damage, and war-induced migration) were 0.47, 0.11, and 0.46 in the preschool-age period, respectively, and 1.13, 0.11, and 1.09 in the school-age period.

war exposure based on a geographical unit cannot identify the specific cause of adverse educational impacts of conflict exposure, their estimates do not necessarily understate the impacts of war exposure.

5. Conclusion

In this study, taking previous studies as the point of departure, we elucidated the specific channels of the influences of household-level war exposure. The estimation results indicate that the impacts of conflict exposure vary depending on the type of conflict event and the timing of exposure. Human victimization and property damage during the school-age period are the main causes of the adverse educational impacts of war exposure: Experiencing conflict events that cause human and property damage deprives children of most opportunities for upper secondary education or higher. We also found that early migration (i.e., before school age) could alleviate the adverse impacts on children's education to some extent. In general, whether a household has an option to evacuate can vary greatly from household to household, and therefore, it could be important to promptly establish safer places for people in conflict areas, including IDP camps, and to quickly disseminate information on those places to households. Moreover, the prolonged and significant effects of property damage that we found also suggest that providing immediate assistance to children in damaged households could be an effective measure for mitigating the loss of human capital in the next generation. These are the main implications of this study for current or future conflicts around the world.

At the same time, however, our investigation of the impact of war exposure still leaves many questions unanswered. As already mentioned in Section 3(c), the within-sibling comparison does not allow us to investigate gender heterogeneity in the impact of war exposure and the impact

of parental death due to a lack of sufficient variations in the timing of exposure between siblings. In addition, this study does not adequately address the changes in the supply factors of education and their impact, such as the destruction of educational facilities that may have been caused by the conflict. To discuss concrete and effective reconstruction policies in more detail, further research must be conducted.

One direction of future research may be to investigate the cognitive and noncognitive abilities of exposed children. The mechanism of lowered educational attainment can be broadly classified into two categories: the loss of educational opportunities and impaired learning ability. Our findings imply that the former may be the main mechanism, as we found that property damage, if it occurs even once, can be the largest contributor to an adverse educational impact. However, we cannot explore this further, mainly due to data limitations. Subsequent analyses using anthropometric and health information, as well as data on cognitive and noncognitive abilities, focusing on household-level conflict exposure, can help to comprehensively explore the impact of war exposure on children's educational attainment.

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Tables

Table 1: Summary Statistics of the Empirical Variables

Variable	Obs.	Mean	Std. Dev.	Min	Max
[Educational outcome]					
Years of education	898	11.87	1.60	5.00	18.00
[Experience of conflict exposure]					
Loss of family/friends/neighbors					
Prebirth period (5 years before birth to birth)	898	0.20	0.48	0.00	4.00
Preschool-age period (0 to 5 years)	898	0.15	0.36	0.00	5.00
School-age period (6 to 16 years)	898	0.51	0.88	0.00	5.40
Property damage					
Prebirth period (5 years before birth to birth)	898	0.03	0.17	0.00	1.00
Preschool-age period (0 to 5 years)	898	0.04	0.17	0.00	1.20
School-age period (6 to 16 years)	898	0.05	0.22	0.00	2.20
War-induced migration					
Prebirth period (5 years before birth to birth)	898	0.08	0.29	0.00	2.00
Preschool-age period (0 to 5 years)	898	0.14	0.45	0.00	4.00
School-age period (6 to 16 years)	898	0.50	1.03	0.00	6.00
[Individual characteristics]					
Year of birth	898	1997.31	2.59	1992.00	2001.00
Female	898	0.40	0.49	0.00	1.00
Birth order	898	2.31	1.22	1.00	7.00

Source: Authors' calculation using data from the original survey conducted in 2018.

Table 2: Balancing Test for Household/Individual Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Exposed to conflict events		Unexposed to conflict events		(2) − (4)	
	Obs.	Weighted mean	Obs.	Weighted mean	Diff.	Std. Err.
A) Household (HH) level						
HH head's age	442	51.129	130	51.669	−0.541	[1.191]
HH head's schooling years	442	7.516	130	7.937	−0.422	[0.595]
Female-headed HH	442	0.136	130	0.134	0.002	[0.042]
Mother's age	432	47.217	128	48.206	−0.989	[0.849]
Mother's schooling years	432	8.082	128	7.554	0.528	[0.590]
Single parent	442	0.044	130	0.067	−0.024	[0.026]
Agric. land owned (acres)	442	0.713	130	0.696	0.017	[0.237]
B) Individual (child) level – during the preschool-age period (aged between 0 to 5)						
Female	305	0.432	593	0.391	0.041	[0.038]
Age	305	20.184	593	19.826	0.358*	[0.212]
Birth order	305	2.208	593	2.352	−0.144*	[0.078]
# of siblings	305	2.994	593	3.144	−0.150	[0.105]
C) Individual (child) level – during the school-age period (aged between 6 and 16)						
Female	496	0.375	402	0.428	−0.054	[0.043]
Age	496	20.151	402	19.758	0.393*	[0.222]
Birth order	496	2.295	402	2.317	−0.022	[0.154]
# of siblings	496	3.108	402	3.089	0.019	[0.182]

Source: Authors' calculation using data from the original survey conducted in 2018.

Table 3: Impact of Conflict Exposure on Years of Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loss of family/friends/neighbors							
Prebirth period	−0.038	−0.087					0.092
	[0.180]	[0.223]					[0.219]
Preschool-age period	−0.898***	−0.876*					−0.739
	[0.329]	[0.523]					[0.533]
School-age period	−1.504***	−1.599**					−1.071*
	[0.481]	[0.647]					[0.605]
Property damage							
Prebirth period			−1.450***	−2.322***			−2.134***
			[0.310]	[0.532]			[0.769]
Preschool-age period			−2.053***	−2.469***			−2.012**
			[0.517]	[0.652]			[0.782]
School-age period			−3.242***	−3.223***			−2.416**
			[0.607]	[0.751]			[0.936]
War-induced migration							
Prebirth period					0.442*	0.404*	0.361*
					[0.249]	[0.229]	[0.208]
Preschool-age period					0.164	0.278	0.281
					[0.223]	[0.208]	[0.203]
School-age period					0.023	−0.046	−0.061
					[0.221]	[0.209]	[0.200]
Sibling & birth year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual-level controls	No	Yes	No	Yes	No	Yes	Yes
Linear hetero. district trends	No	Yes	No	Yes	No	Yes	Yes
Observations	898	898	898	898	898	898	898
R-square	0.110	0.176	0.101	0.172	0.095	0.170	0.202
Groups (siblings)	572	572	572	572	572	572	572

Notes: This table reports only the coefficients of interest based on Equation (1). Standard errors in the square brackets are clustered at the village (GN division) level. Triple asterisks (***), double asterisks (**), and a single asterisk (*) denote that the coefficient is statistically significant at the 1%, 5%, and 10% levels, respectively.

Source: Authors' estimation using data from the original survey conducted in 2018.

Table 4: Impacts of Conflict Exposure in Two Scenarios

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Years of education	Grade repetitions	Completion of: Lower secondary or higher	Completion of: Upper secondary or higher	Continuing their studies	Working
Mean (Std. Dev.):	11.87 (1.60)	0.137 (0.365)	0.973 (0.162)	0.282 (0.450)	0.395 (0.489)	0.252 (0.434)
A) Exposed to a human victimization event						
Prebirth period	0.092	0.010	−0.024	0.043	−0.078	0.130
	[0.219]	[0.041]	[0.018]	[0.102]	[0.063]	[0.079]
Preschool-age period	−0.739	0.022	−0.072*	−0.100	−0.054	0.265
	[0.533]	[0.069]	[0.040]	[0.188]	[0.127]	[0.170]
School-age period	−1.071*	0.020	−0.130*	−0.085	−0.012	0.377*
	[0.605]	[0.106]	[0.067]	[0.234]	[0.183]	[0.206]
B) Exposed to both a human victimization event and a property damage event						
Prebirth period	−2.042***	0.143	0.006	−0.549**	−0.564*	−0.098
	[0.630]	[0.167]	[0.051]	[0.253]	[0.300]	[0.272]
Preschool-age period	−2.750***	0.266	0.130	−0.861***	−0.493	0.424
	[0.857]	[0.251]	[0.079]	[0.300]	[0.319]	[0.373]
School-age period	−3.487***	−0.085	−0.199	−0.972***	0.129	0.725*
	[0.934]	[0.239]	[0.130]	[0.313]	[0.338]	[0.371]

Notes: This table reports only the coefficients of interest based on Equation (1). All estimations are based on the same specifications as in Column 7 of Table 3. Standard errors in the square brackets are clustered at the village (GN division) level. Triple asterisks (***), double asterisks (**), and a single asterisk (*) denote that the coefficient is statistically significant at the 1%, 5%, and 10% levels, respectively.

Source: Authors' estimation using data from the original survey conducted in 2018.

Table 5: Comparison with the Impacts of District-Level Conflict Exposure

	(1)	(2)
A) Using household firsthand experience of war exposure		
Conflict exposure:	# of events with loss of family/friends/neighbors, property damage, & war-induced migration	
Educational Impact of the average exposure level		
Preschool-age period	−0.444* [0.266]	−0.444 [0.283]
School-age period	−1.546*** [0.672]	−1.568** [0.720]
Specification:	Baseline (as in Col. 7 of Table 3)	Baseline + District cubic & gender cubic trends
B) Using aggregated measure of war exposure at the district level		
Conflict exposure:	# of conflict events with deaths	
Educational impact of the average exposure level		
Preschool-age period	−0.999 [1.000]	−0.869 [0.999]
School-age period	−1.011 [1.415]	−0.889 [1.382]
Specification:	Baseline (as in Col. 7 of Table 3)	Baseline + District cubic & gender cubic trends

Notes: This table reports the educational impact of the average exposure level. Standard errors in the square brackets are clustered at the village (GN division) level. Triple asterisks (***), double asterisks (**), and a single asterisk (*) denote that the coefficient is statistically significant at the 1%, 5%, and 10% levels, respectively. The results reported in Columns 1 and 2 of Panel B are calculated based on the estimation results in Columns 3 and 4 of Table A4 in Online Appendix C. Source: Authors' estimation using data from the original survey conducted in 2018 (Panel A) and data from the Global Terrorism Database (Panel B).

Figures

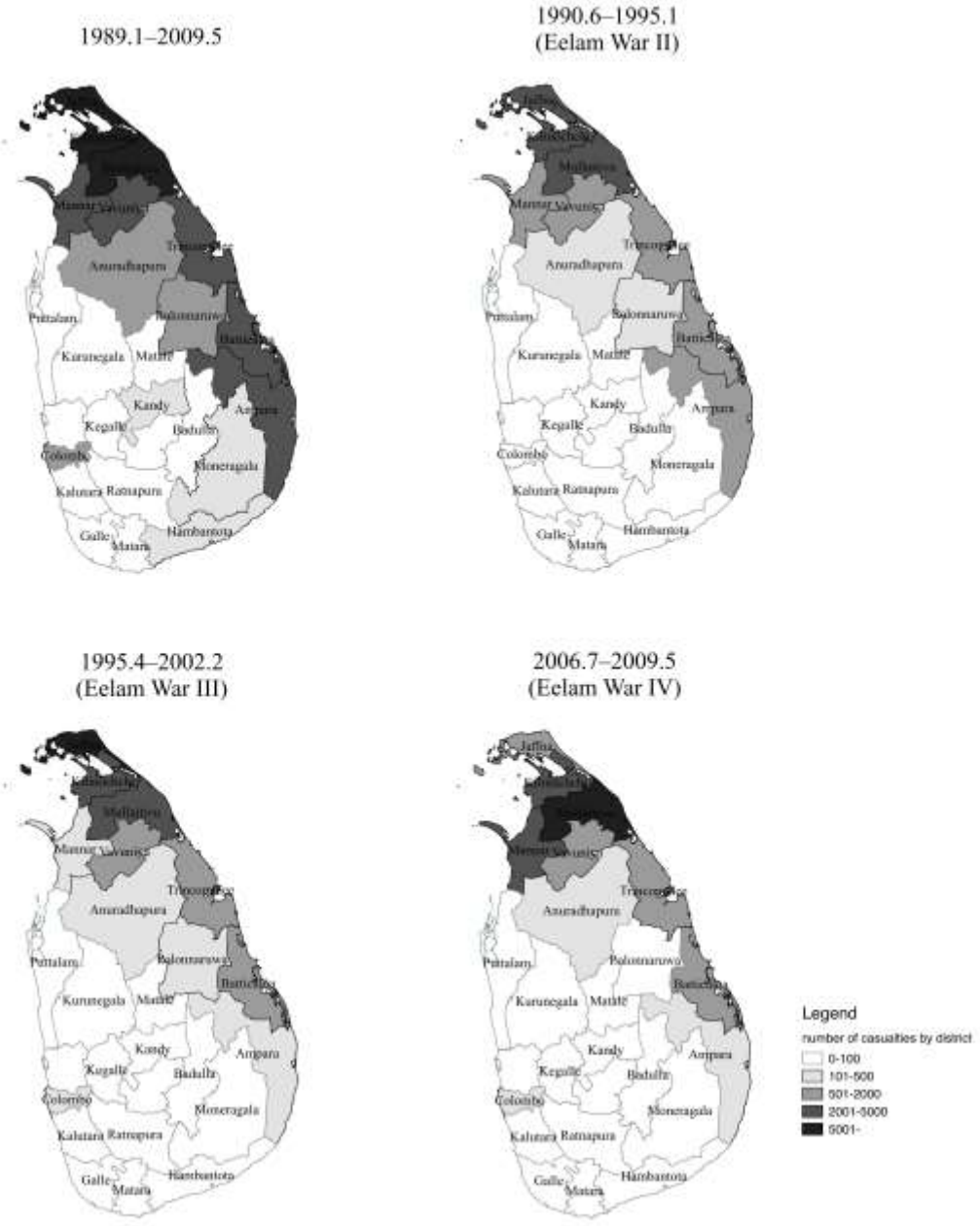
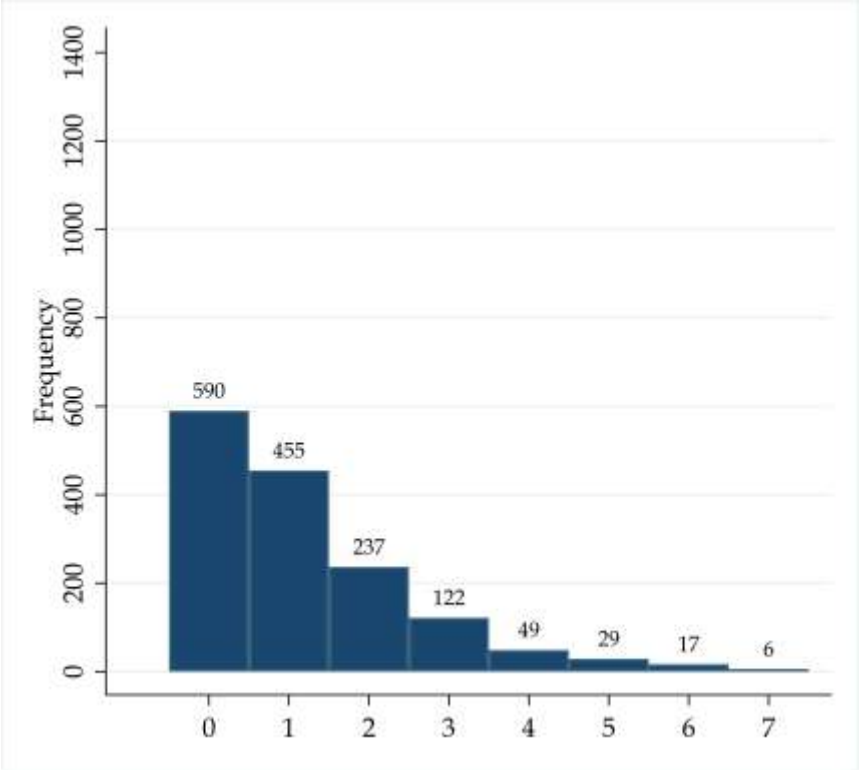


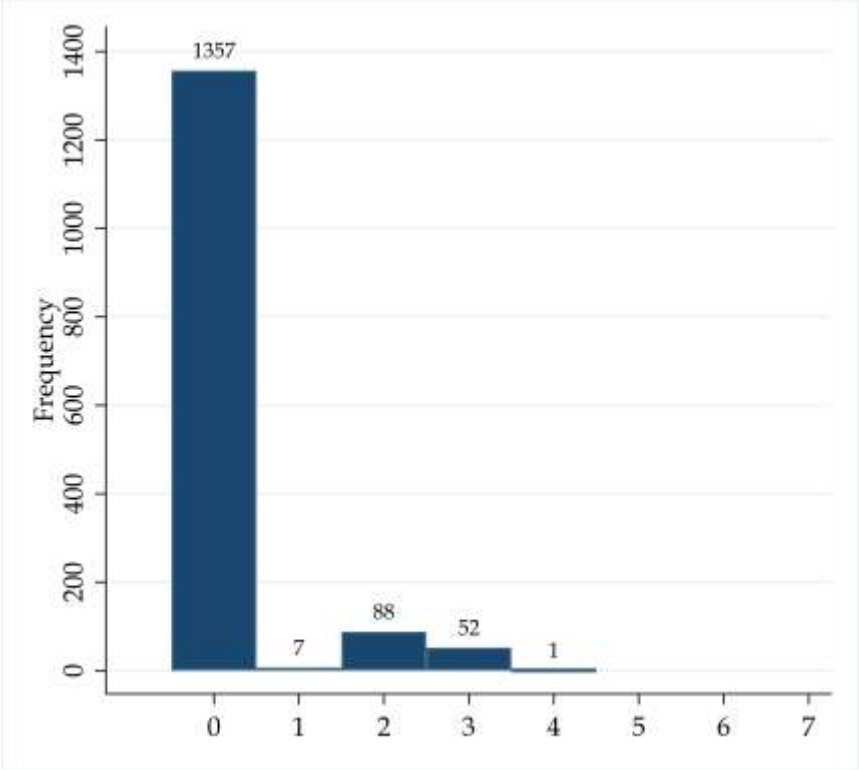
Figure 1: Geographical Transition of the War

Note: The LTTE was in control of the majority of the Northern and Eastern provinces at its peak. The Northern Province comprises five districts: Jaffna, Kilinochchi, Mannar, Mullattivu, and Vavuniya. The Eastern Province consists of three districts: Trincomalee, Batticaloa, and Ampara. Because the data contain only conflict events from 1989 to the present, the maps mainly cover Eelam Wars II to IV.

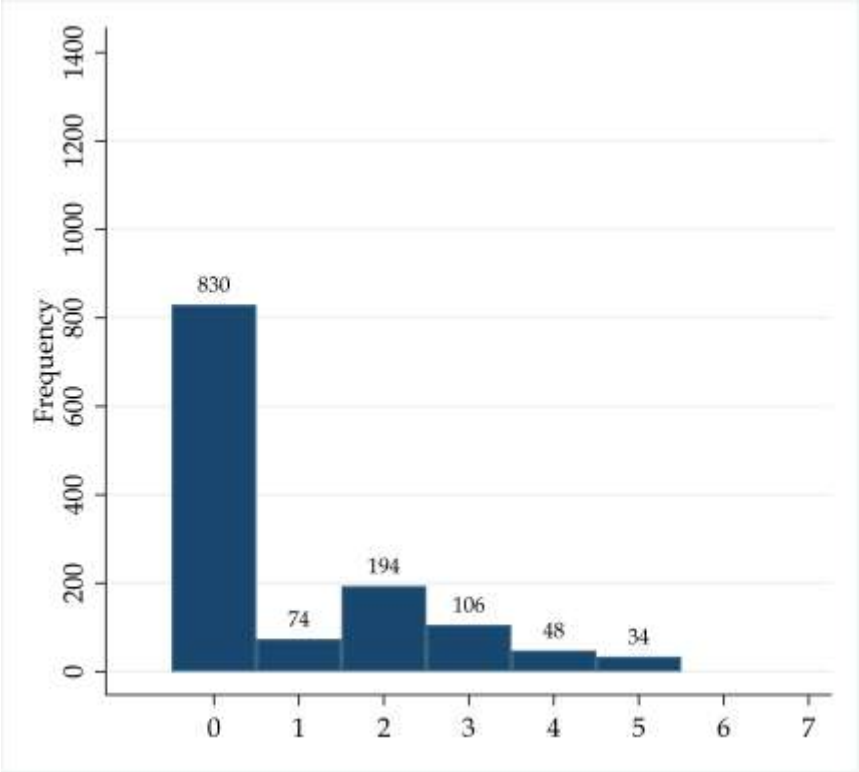
Source: Authors' drawing using data from the Uppsala Conflict Data Program Georeferenced Event Dataset Global version 20.1 (UCDP GED 20.1).



A) Loss of family/friends/neighbors



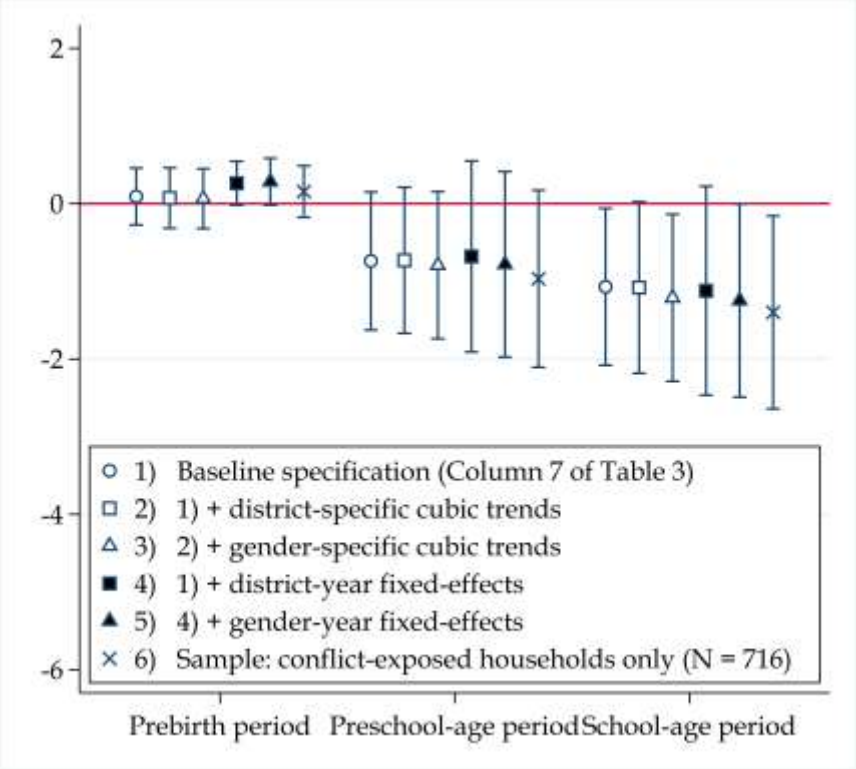
B) Property damage



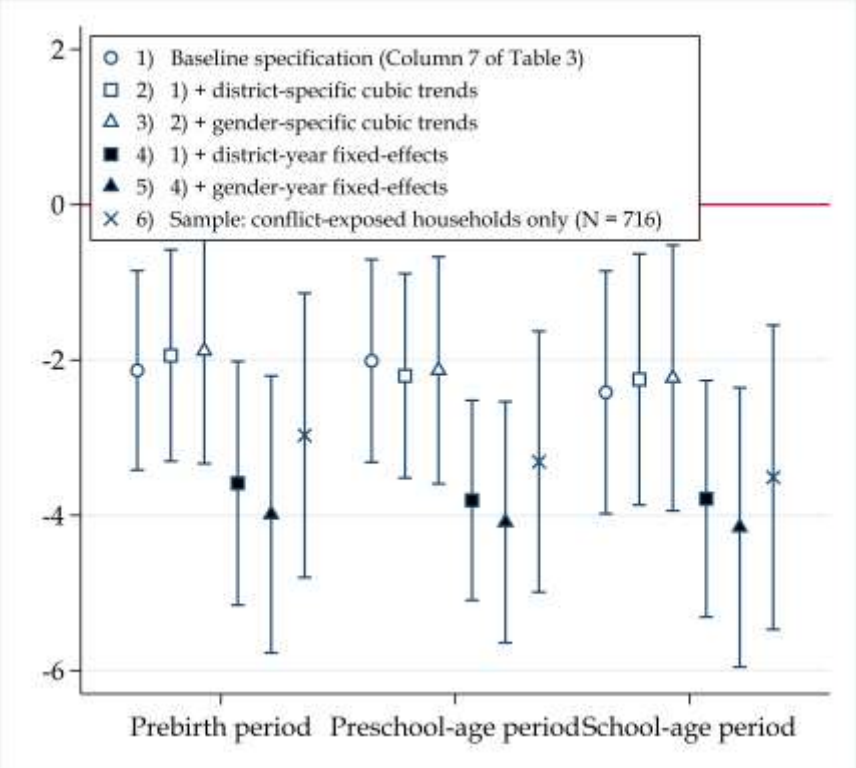
C) War-induced migration

Figure 2: Distribution of Conflict Events

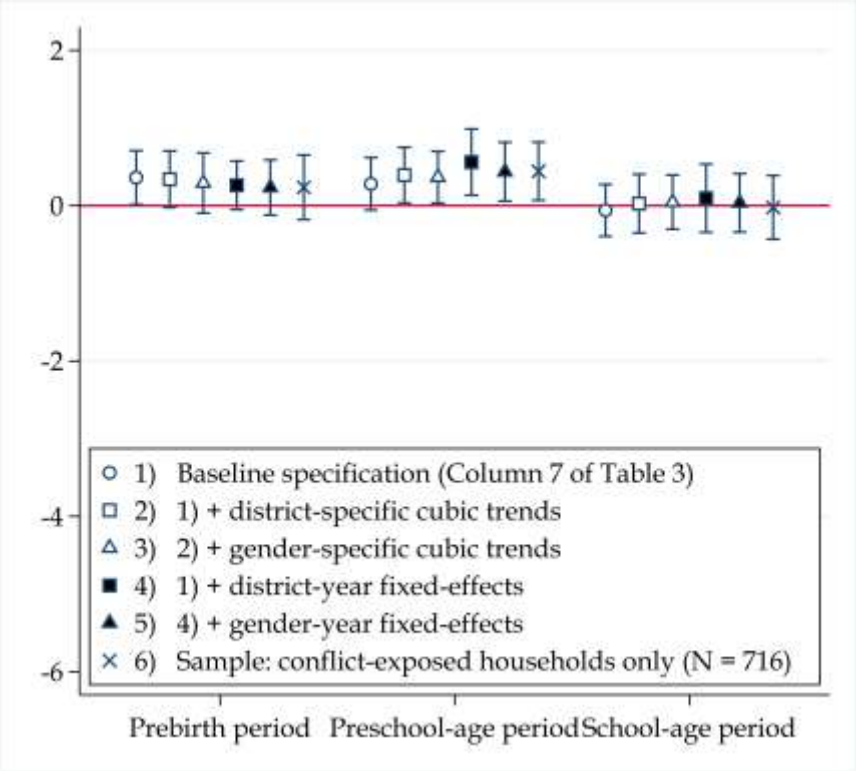
Source: Authors' drawing using data from the original survey conducted in 2018.



A) Loss of family/friends/neighbors



B) Property damage



C) War-induced migration

Figure 3: Robustness Checks: Educational Impacts of Conflict Exposure

Note: Vertical lines represent 90% confidence intervals.

Source: Authors' drawing using data from the original survey conducted in 2018.

Online Appendix for
"Educational Consequences of Firsthand Exposure to Armed Conflict: The Case of the Sri Lankan Civil War"

Takahiro Ito, Jia Li, Ramila Usoof-Thowfeek, Koji Yamazaki

A. Survey Details

In this appendix, we briefly explain our original household survey conducted in the Northern and Eastern provinces of Sri Lanka in March and April 2018. For detailed information about the survey, see Yamazaki et al. (2021).

The survey was conducted based on the multistage stratified random sampling method according to the following procedure. In the study region, there are 79 DS divisions in eight districts: Ampara, Batticaloa, and Trincomalee in the Eastern Province and Jaffna, Kilinochchi, Mannar, Mullaitivu, and Vavuniya in the Northern Province. The first stratum is the district. Next, to ensure that our sample contains diverse ethnic groups as in the population, we classified the 79 DS divisions into four categories based on the composition of ethnic groups, and the category was set as the second stratum. This is because Grama Niladaris (GNs), the primary sampling unit (PSU), are often monoethnic, and simple random sampling may hinder us from recovering the actual ethnic composition.

The four categories of DS divisions are Tamil-, Sinhalese-, Moor-dominant, and mixed-ethnicity divisions. The "dominant" ethnic group is defined as the one with at least 90% of the total population in the DS division, which is based on the 2012 census data. Next, we assigned a number of GNs to be chosen for the categories, roughly in proportion to their population share in the district (see Table A1). From each category, the assigned number of GNs was randomly chosen,

and 25 households were randomly chosen from the voters' list in each chosen GN. However, note that for GNs chosen from the Tamil-, Sinhalese-, and Moor-dominant categories, if a selected household is not the same ethnicity as the dominant ethnicity, we replaced it with another until we obtained a household with the dominant ethnicity. Thus, we have 1,600 households (eight districts × eight GNs × 25 households). Note also that the sample size was designed to ensure a confidence level of 95% with a margin of error of 0.025 in the case of simple random sampling. The estimated number of households in the population calculated from the 2012 census data was 676,819, and the required sample size was 1,534.

Table A1: Selection of the Primary Sampling Unit, Grama Niladaris (GNs)

District	Category of DS divisions: # of DSs (population share, %)	The number of GNs in category
Jaffna	Tamil-dominant: 15 DSs (100.0)	8 GNs from 15 Tamil-dominant DSs
Kilinochchi	Tamil-dominant: 4 DSs (100.0)	8 GNs from 4 Tamil-dominant DSs
Mannar	Tamil-dominant: 1 DS (18.0) Mixed-ethnicity: 4 DSs (72.0)	1 GN from 1 Tamil-dominant DS 7 GNs from 4 mixed-ethnicity DSs
Mullaitivu	Tamil-dominant: 3 DSs (54.3) Sinhalese-dominant: 1 DS (7.5) Mixed-ethnicity: 2 DSs (39.1)	4 GNs from 3 Tamil-dominant DSs 1 GN from 1 Sinhalese-dominant DS 3 GNs from 2 mixed-ethnicity DSs
Vavuniya	Tamil-dominant: 2 DSs (75.0) Sinhalese-dominant: 1 DS (7.6) Mixed-ethnicity: 1 DS (17.4)	6 GNs from 2 Tamil-dominant DSs 1 GN from 1 Sinhalese-dominant DS 1 GN from 1 mixed-ethnicity DS
Ampara	Tamil-dominant: 2 DSs (7.4) Sinhalese-dominant: 7 DSs (38.4) Moor-dominant: 6 DSs (29.6) Mixed-ethnicity: 5 DSs (24.7)	1 GN from 2 Tamil-dominant DSs 3 GNs from 7 Sinhalese-dominant DSs 2 GNs from 6 Moor-dominant DSs 2 GNs from 5 mixed-ethnicity DSs
Batticaloa	Tamil-dominant: 7 DSs (42.0) Moor-dominant: 3 DSs (16.8) Mixed-ethnicity: 4 DSs (41.2)	3 GNs from 7 Tamil-dominant DSs 2 GNs from 3 Moor-dominant DSs 3 GNs from 4 mixed-ethnicity DSs
Trincomalee	Tamil-dominant: 1 DS (3.0) Sinhalese-dominant: 2 DSs (5.1) Moor-dominant: 1 DS (17.0) Mixed-ethnicity: 7 DSs (74.9)	1 GN from 1 Tamil-dominant DS, 1 GN from 2 Sinhalese-dominant DSs 1 GN from 1 Moor-dominant DS 5 GNs from 7 mixed-ethnicity DSs

Table A2 reports sample features such as sex ratio, child population ratio, and ethnic composition calculated from our survey data (Column 2) and from census data (Column 1) for

comparison. We observe small demographic changes between the two datasets. The male–female ratio increased by 2.94%, and the child population ratio (population aged less than 20 years) decreased by 4.56%. However, these may be natural changes in the post-conflict periods, in part due to the increased life expectancy of male adults. Looking at the ethnicity of respondents, the ethnic compositions in our sample and the census are consistent, indicating that our sample is not far from the sample from the nationally representative survey.

Table A2: Sample Features in the Census and Our Survey

	Census (2012)	Our survey (2018)
Sex ratio (Male/Female)	93.33%	98.05%
Child population ratio	38.73%	33.63%
Sinhalese	15.01%	17.64%
Sri Lankan Tamil	61.07%	58.46%
Moor	23.20%	23.74%
Other	0.72%	0.17%

Note: Figures calculated from our survey data are adjusted using the sampling weight ($N = 6{,}828$).
Source: Authors' calculation using data from the original survey conducted in 2018 (Column 1) and data from the 2012 census (Column 2).

B. Issue of Selection on Unobservables

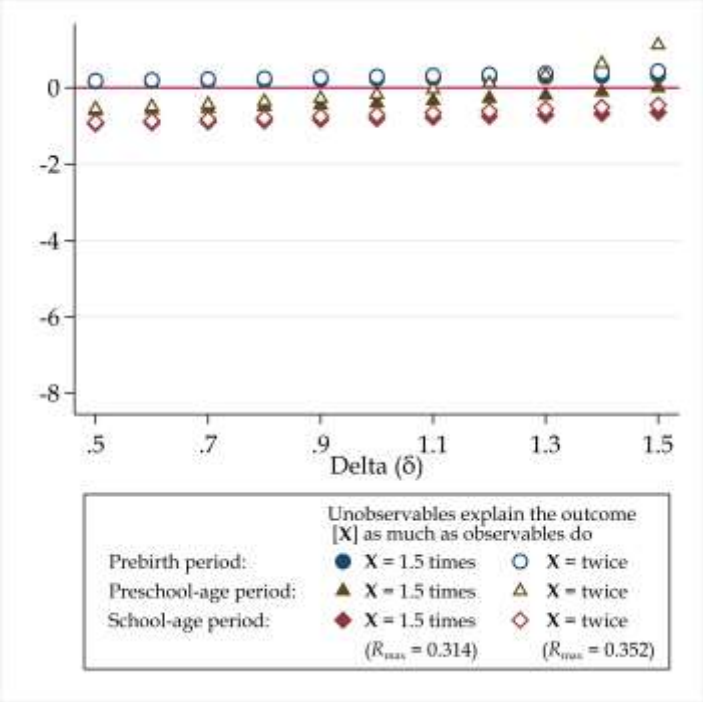
In this appendix, we check our identification strategy by calculating the bias-adjusted treatment effects proposed by Oster (2019). If our identification assumption is valid, the estimated impacts of the treatment variables from a simple regression with only those variables related to the identification assumption should be almost the same as the estimated impacts from a regression with the full set of controls. By contrast, if there are significant differences between the two regressions, there may also be an issue of "selection on unobservables." Thus, following Oster (2019), we conduct a robustness check by calculating the lower (or upper) bound of the treatment

effects under certain assumptions. These effects, denoted by β^*, are approximated by (or expressed by, if we assume Assumption 2 from Oster, 2019)

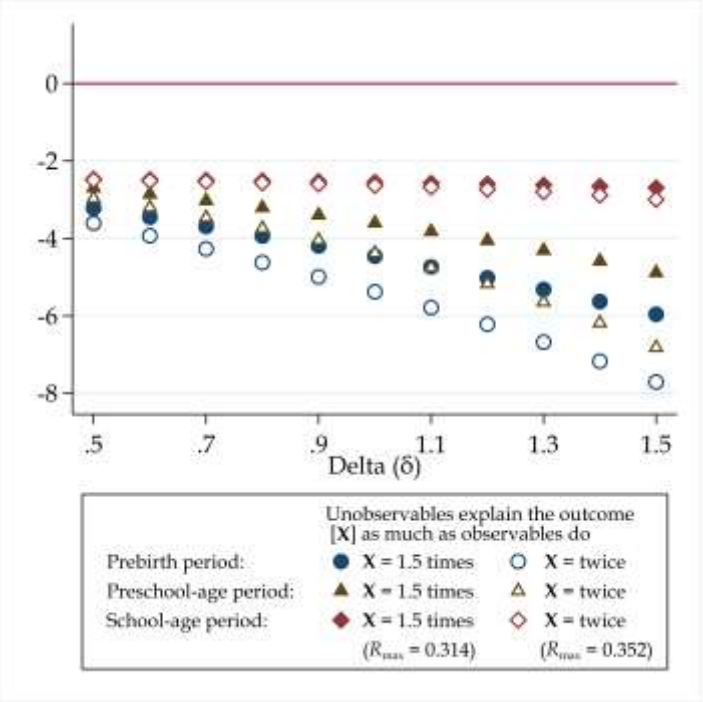
$$\beta^* = \tilde{\beta} - \delta\left(\overset{o}{\beta} - \tilde{\beta}\right)\frac{R_{\max} - \tilde{R}}{\tilde{R} - \overset{o}{R}},$$

where $\tilde{\beta}$ and $\tilde{R}$ are the coefficient estimate of interest and the R-squared from the fully controlled specification (as in Column 7 of Table 3), $\overset{o}{\beta}$ and $\overset{o}{R}$ are those from the *uncontrolled* specification that includes no observable controls but does include those related to the identification assumption (in our context, sibling and birth year fixed effects), and δ and $R_{\max}$ are parameters that depend on the influences of unobservables and are determined according to the context of the study. δ is defined as the ratio of the selection on unobservables to the selection on observables, that is, $\delta = \frac{\sigma_{2,X}/\sigma_2^2}{\sigma_{1,X}/\sigma_1^2}$, where $\sigma_{j,X}$ is the covariance of the treatment variable with the observables ($j = 1$) or unobservables ($j = 2$), and σ_j^2 is the variance of the observables ($j = 1$) or unobservables ($j = 2$). For δ, we use values ranging from 0.5 to 1.5. $R_{\max}$ is the regression R-squared assuming that both the observed and unobserved variables can be controlled for. We use two values for $R_{\max}$, 0.314 and 0.352, which are the values when we assume that the unobservables explain the variation in the outcome 1.5 and 2 times as much as the controls do (i.e., $R_{\max} = 2.5\tilde{R} - 1.5\overset{o}{R}$ and $R_{\max} = 3\tilde{R} - 2\overset{o}{R}$).

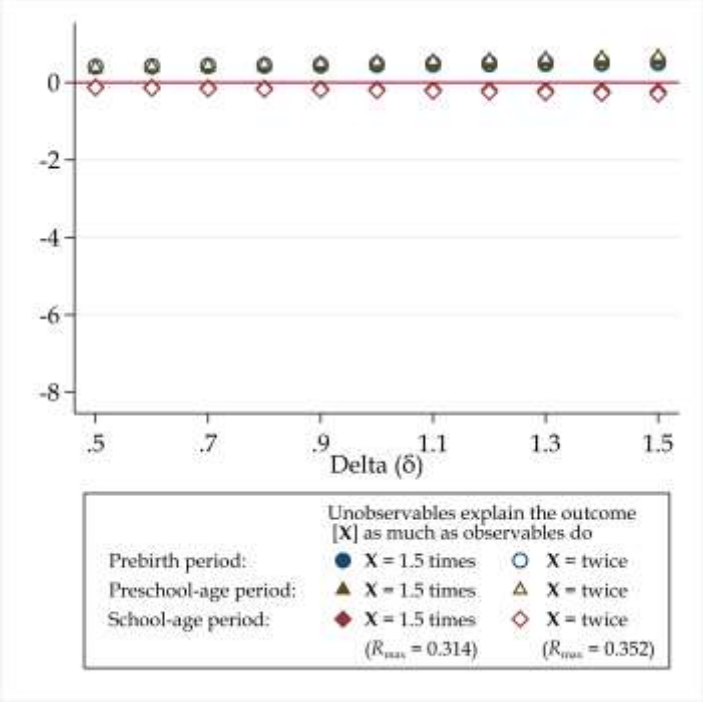
Figure A1 reports the estimated bias-adjusted coefficients on conflict events. As shown in the figure, in most cases, the estimates are fairly stable across δ and $R_{\max}$ values. Thus, it is implied that the endogeneity issues caused by violations of the identification assumption are not very problematic or are negligible if they exist.



A) Loss of family/friends/neighbors



B) Property damage



C) War-induced migration

Figure A1: Bias-Adjusted Impacts of Conflict Exposure

C. Additional Analysis of the Impacts of Conflict Exposure

The results in Columns 2 to 7 of Table 4 are based on the estimation results presented in Table A3.

Table A3: Impact of Conflict Exposure on other Educational Outcomes and Labor Participation

	(1)	(2)	(3)	(4)	(5)
Dep. var.:	Grade repetitions	Completion of		Continuing their studies	Working
		Lower secondary	Upper secondary		
Mean (Std. Dev.)	0.137 (0.365)	0.973 (0.162)	0.282 (0.450)	0.395 (0.489)	0.252 (0.434)
Loss of family/friends/neighbors					
5 to 1 years before birth	0.010	−0.024	0.043	−0.078	0.130
	[0.041]	[0.018]	[0.102]	[0.063]	[0.079]
Preschool-age period	0.022	−0.072*	−0.100	−0.054	0.265
	[0.069]	[0.040]	[0.188]	[0.127]	[0.170]
School-age period	0.020	−0.130*	−0.085	−0.012	0.377*
	[0.106]	[0.067]	[0.234]	[0.183]	[0.206]
Property damage					
5 to 1 years before birth	0.134	0.029	−0.591*	−0.486	−0.227
	[0.195]	[0.066]	[0.323]	[0.327]	[0.318]
Preschool-age period	0.243	0.202**	−0.761**	−0.439	0.159
	[0.243]	[0.091]	[0.367]	[0.354]	[0.395]
School-age period	−0.106	−0.069	−0.887**	0.141	0.348
	[0.256]	[0.127]	[0.376]	[0.376]	[0.418]
War-induced migration					
Prebirth period	0.036	0.004	0.009	0.085	0.020
	[0.045]	[0.012]	[0.076]	[0.069]	[0.056]
Preschool-age period	0.033	−0.039*	0.149*	0.025	−0.079
	[0.041]	[0.021]	[0.079]	[0.062]	[0.075]
School-age period	0.027	−0.029	0.013	−0.013	−0.064
	[0.029]	[0.021]	[0.074]	[0.066]	[0.080]
Sibling & birth year fixed effects	Yes	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes	Yes
District linear trends	Yes	Yes	Yes	Yes	Yes
Observations	898	898	898	898	898
Groups (siblings)	572	572	572	572	572
R-square	0.113	0.087	0.304	0.302	0.334

Notes: This table reports only the coefficients of interest based on Equation (1). All estimations are based on the same specifications as in Column 7 of Table 3. Standard errors in the square brackets are clustered at the village (GN division) level. Double asterisks (**) and a single asterisk (*) denote that the coefficient is statistically significant at the 1%, 5%, and 10% levels, respectively.
Source: Authors' estimation using data from the original survey conducted in 2018.

In addition, the results in Panels B and C of Table 5 are based on the estimation results presented in Table A4.

Table A4: Impact of District-Level Conflict Exposure on Education

	(1)	(2)	(3)	(4)
# of events with deaths (at the district level)				
Prebirth period	−0.018	−0.011	−0.021	−0.013
	[0.025]	[0.036]	[0.026]	[0.038]
Preschool-age period	−0.031	−0.026	−0.037	−0.032
	[0.033]	[0.033]	[0.037]	[0.037]
School-age period	−0.027	−0.025	−0.028	−0.024
	[0.036]	[0.034]	[0.039]	[0.038]
Sibling & birth year fixed effects	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
Hetero. district trends: linear	No	—	No	—
Hetero. district trends: cubic	No	Yes	No	Yes
Hetero. sex trends: cubic	No	Yes	No	Yes
Sample	Incl. households who evacuated outside the district		Excl. households who evacuated outside the district	
Observations	1,029	1,029	774	774
R-square	0.154	0.197	0.170	0.202
Groups (siblings)	672	672	515	515

Notes: This table reports only the coefficients of interest based on Equation (1). Standard errors in the square brackets are clustered at the village (GN division) level. Triple asterisks (***), double asterisks (**), and a single asterisk (*) denote that the coefficient is statistically significant at the 1%, 5%, and 10% levels, respectively.
Source: Authors' estimation using data from the Global Terrorism Database.

References

Oster, Emily. 2019. "Unobservable Selection and Coefficient Stability: Theory and Evidence." *Journal of Business and Economic Statistics*, 37(2): 187–204.

Yamazaki, Koji, Takahiro Ito, Jia Li, and Ramila Usoof-Thowfeek. 2021. "Survey of Conflict-Affected Regions in Sri Lanka: An Overview." GSICS Working Paper Series, No.37.