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Abstract

We examine the indirect effect of violent conflict on child health in non-conflict zones for the case of Sri Lankan civil war. Using variations in timing, location, and intensity of violent events happened in the neighboring districts, we found evidence for the negative indirect effect on children living in adjacent areas to conflict zones. Our findings suggest the existence of a vulnerable population often neglected in the process of war reconstruction, that is, people living in neighboring areas of conflict zones. The causal pathway of the indirect effect is different from the one for the direct impact of violence on people in conflict zones. Our analysis of causal mechanisms indicates that inflows of internally displaced persons may have caused a short-run nutritional deficiency due to an increase in food prices, which suggests the importance of early policy responses to mitigate the negative impacts on people in neighboring non-conflict zones.

JEL classification: D74, I15, O15

Keywords: civil conflict, child health, indirect effect, internally displaced persons, Sri Lanka

Declarations of Interest: none.

1. Introduction

Armed conflict has devastating impacts on society and people living in conflict zones. Recent studies have provided robust evidence that conflict hinders the accumulation of human capital of children in conflict zones. Negative impacts on health (Akbulut-Yuksel, 2014; Akbulut-Yuksel, 2017; Bundervoet, et al., 2009; Mansour and Rees, 2012; Minoiu and Shemyakina, 2014; Weldeegzie, 2017) and education (Chamarbagwala and Morán, 2011; León, 2012; Shemyakina, 2011) during early childhood have irreversible and lasting impacts on their wellbeing (Akresh et al., 2012). Therefore, it is clear that people in conflict-affected areas should be made a priority in public policies for war reconstruction. At the same time, however, there may exist another vulnerable population, people living in neighboring areas of conflict zones, which is subject to the impact of violent conflict and is often neglected in the process of reconstruction.

Previous studies show that conflicts spill over both spatially and temporally, highlighting significant effects on the economic development of neighboring countries (Carmignani and Kler, 2016; de Groot, 2011; Dunne and Tian, 2014; Murdoch and Sandler, 2002; Murdoch and Sandler, 2004), which may lengthen conflict (Carmignani and Kler, 2017). However, these studies primarily focus on the diffusion of violent conflict itself across countries or spillover effects of neighboring conflicts on macroeconomic conditions. So far, little attention has been paid to spillover effects of violent conflict on people in non-conflict zones within the same country. Rather, these people are treated as a control group to measure the direct impacts of violent conflict in previous studies. However, they are not necessarily free from impacts of the conflict; refugee inflows (Alix-Garcia and Saah, 2010; Alix-Garcia et al., 2018; Baez, 2011; Maystadt and Verwimp, 2014; Taylor et al. 2016; Young et al., 2014) and food insecurity (Breisinger et al., 2015) may have spillover effects on people in non-conflict zones in the same country; but, such indirect effects have not been thoroughly investigated yet. If the control group is negatively affected as well, the estimated impact of war exposure is underestimated and provides lower bound of the impact. But we are ignorant of the magnitude of such underestimation.

In this study, we examine the indirect effects of violent conflict on the well-being of children in non-conflict zones within the country. To this end, we explore the case of civil conflict in Sri Lanka using the Sri Lanka Demographic and Health Survey (SLDHS) conducted in 2006/2007 (Department of Census and Statistics and Ministry of Healthcare and Nutrition, 2009) and the Uppsala Conflict Data Program Georeferenced Event Dataset (UCDP GED) Global version 18.1 (Sundberg and Melander, 2013; Croicu and Sundberg, 2018). Sri Lanka experienced one of the longest civil conflicts in Asia; the Sri Lankan conflict started in 1983 and ended in 2009 with the victory of the Sri Lankan government. Even though 10 years have passed since the end of the conflict, the human capital consequences of the war are still underexplored. Exploiting variations in the timing, location, and intensity of combat, we compare health indicators of children who live in districts in non-conflict zones neighboring conflict zones with those who do not live adjacent to conflict zones to identify indirect effects of the conflict.

This study contributes to the literature in the following three ways. First, we examine an indirect effect of civil conflict on children in non-conflict zones within the same country, which has been largely neglected in extant conflict-related literature. Recent studies investigate the impacts of refugee inflows on hosting communities in neighboring countries across the national border (Alix-Garcia et al., 2018; Fallah et al., 2019; Maystadt and Verwimp 2014; Taylor et al., 2016). These cross-border refugees have limited freedom of movement and opportunities for employment in hosting countries. Nevertheless, these studies find that, on average, people in hosting communities gain economic benefits from the inflow of the refugees. In this study, we analyze the indirect impact of violent events on people in neighboring non-conflict zones. One possible mechanism is the inflow of internally displaced persons (IDPs) to the non-conflict zones. Since IDPs from a neighboring region of the same country may have friends and relatives in hosting communities, may not need to stay in IDP camps, and may freely interact with local people, it is worth investigating how the inflow of IDPs affects people in hosting communities.

Second, we focus on impacts on child health, which has attracted limited attention in the literature on refugee inflows. Contrary to studies showing economic benefits to local populations, previous studies on health impact show negative effects of refugee inflows. Using civil war in a refugee-sending country as an instrumental variable in cross-country panel regressions, Montalvo and Reynal-Querol (2007) show that refugee inflows cause higher malaria incidence in refugee-hosting countries. Baez (2011) analyzes the impact of refugee inflows on local children in Tanzania, and finds negative and persistent impacts in terms of height, morbidity, school attainment, and literacy. He suggests that the burden of health and sanitation on hosting communities could worsen the health environment for local children, and overpopulation might put pressure on resources such as food, land, and wood. However, he provides no quantitative evidence of which mechanism is at work. Therefore, it is worth investigating, in different settings, the indirect effects of civil war on child health in non-conflict zones and causal pathways of such effects.

Third, we try to distinguish possible causal pathways of how children in non-conflict zones are affected by violent events in neighboring conflict zones. There are three possible mechanisms suggested by previous literature. The first two mechanisms work through refugee inflows. One is deteriorated health environment due to refugee inflows, as suggested by Montalvo and Reynal-Querol (2007) and Baez (2011). In areas with limited local capacity of public health services and sanitation infrastructure, huge refugee inflows could damage the health environment. The second mechanism is food insecurity. Higher demand for food by refugees puts upward pressure on food prices (Alix-Garcia and Saah, 2010), which can be a source of positive welfare impacts on local farmers but hurts local consumers. Temporary reduction in food consumption is critical to pregnant mothers and new-born babies. Even without inflows of refugees, security concerns around violent conflict drive agricultural production away from conflict zones (Alix-Garcia et al., 2013). Such changes in land-use patterns could also raise food prices near conflict areas, due to reduced agricultural production as well as increased transportation costs in unsecure areas.

The third mechanism is suggested by the literature on the direct impact of violence on birth outcomes. Previous studies emphasize that psychological stress during early pregnancy causes premature birth and lower birthweight. Studies on landmine explosions in Colombia (Camacho 2008), homicide in Brazil (Koppensteiner and Manacorda, 2016), Basque nationalist terrorism in Spain (Quintana-Domeque and Ródenas-Serrano, 2017), and al-Aqsa Intifada in Palestine (Mansour and Rees, 2012) show that exposure to violence in the first trimester leads to low birthweight.[1] Camacho (2008) emphasizes that the importance of exposure during early pregnancy provides support for the stress hypothesis, because an alternative explanation based on worse nutritional condition due to violence should show the strongest effect in the third trimester when babies grow the most.[2]

In this study, we shed light on the pathways through which an indirect effect works against child health by analyzing the impacts across different child developmental stages, the impact on health environment, the number of IDPs, inflation of paddy prices, and changes in

[1] However, Endara et al. (2009) find no evidence that the first-trimester exposure to the terrorist attacks on September 11, 2001 in the U.S. were associated with infant health outcomes.

[2] According to Camacho (2008) and Koppensteiner and Manacorda (2016), the underlying mechanism is that prenatal stress increases Corticotrophin-releasing hormone, which accelerates the fetal maturation leading to preterm delivery and lower birthweight.

land use patterns. Understanding the exact causal pathway can provide important insights on appropriate policy responses for post-war reconstruction.

The remainder of this paper is organized as follows. Section 2 details evolution of the conflict in Sri Lanka. Section 3 explains empirical methodology and data. Section 4 shows our empirical results. Section 5 presents evidence of possible casual mechanisms. Section 6 concludes the paper.

2. Background on Civil Conflict in Sri Lanka

The 26 year-long ethnic conflict between the separatist Tamils and the Sinhala-dominant government, called the Eelam War, was one of the longest civil conflicts in the world (D'Costa, 2012). The Liberation Tigers of Tamil Eelam (LTTE) fought to build an independent Tamil state in the Northern and Eastern provinces of the island. The Eelam War is estimated to have caused between 80,000 and 100,000 deaths (Human Rights Watch, 2010). At its peak in 2001-02, the Eelam War also displaced 800,000 people within the country (Norwegian Refugee Council/ Internal Displacement Monitoring Centre, 2010).

During the colonial era, independence movements were driven by Sinhala nationalism, which caused tension between Tamils and Sinhalese after independence in 1948, partly because of the "divide and rule" policy under the British colonial government. In 1956, the government announced the Sinhala Only Act, making Sinhalese the only official language of the government. Tension between the two groups escalated over time. In 1976, Velupillai Prabhakaran created the LTTE to fight for a separate Tamil state in the northern and eastern parts of the island. They started guerilla attacks on government officials, police, army, and civilians. During this pre-civil war period (1976-1982) the numbers of attacks and victims were limited, and violent events mainly happened in the Jaffna peninsula in the Northern Province (Dower et al., 2017).

2.1 Evolution of the Eelam War

The Sri Lankan Ministry of Defense separated the whole process of the Eelam War into four phases (Dower et al., 2017), which are named Eelam War I, II, III, and IV. The "Black July" in 1983, the worst anti-Tamil riots, marked the onset of Eelam War I (Brun and van Hear, 2012), which ended with the Indian intervention under the Indo-Sri Lanka Peace Accord (D'Costa, 2012) in July 1987. After failed peace talks, Eelam War II broke out in June 1990, when hundreds of Muslim policemen were murdered by the LTTE (McGilvray, 1997).

In January 1995, a cease-fire agreement was reached between the two parties, which marked the end of Eelam War II (Ganguly, 2018). A brief interlude of peace was disrupted again by the hijacking of a civilian ferry by the LTTE in August 1995,[3] and then Eelam War III started. Due to the efforts by Norway to bring the government and the Tamil Tigers to the negotiating table, both the LTTE and the Sri Lankan government announced a 30-day cease-fire in December 2001 and then signed a permanent cease-fire agreement in February 2002, which marked the end of Eelam War III.[4, 5]

The period following the cease-fire agreement was considerably peaceful (Figure 1), and peace talks between the Sri Lankan government and the LTTE were held in six rounds. They began in Phuket in Thailand in September 2002[6] and continued until March 2003 in

[3] "Rebels Hijack Civilian Ferry In Sri Lanka." The New York Times. August 31, 1995. Retrieved on 16 June 2018. We partly rely on news articles in this section. The internet addresses of the news web sites used in this section are listed in Appendix A.

[4] "Sri Lanka rebels announce truce." BBC News. 19 December 2001. Retrieved on 16 June 2018.

[5] "Sri Lanka enters truce with rebels." BBC News. 21 December 2001. Retrieved on 16 June 2018.

[6] "Upbeat opening for Sri Lanka talks." BBC News. 16 September 2002. Retrieved on 16 June 2018.

Kanagawa in Japan. Despite the fact that the peace talks broke down in April 2004,[7] the ceasefire was largely held due to the Indian Ocean tsunami and the split of the LTTE thereafter.

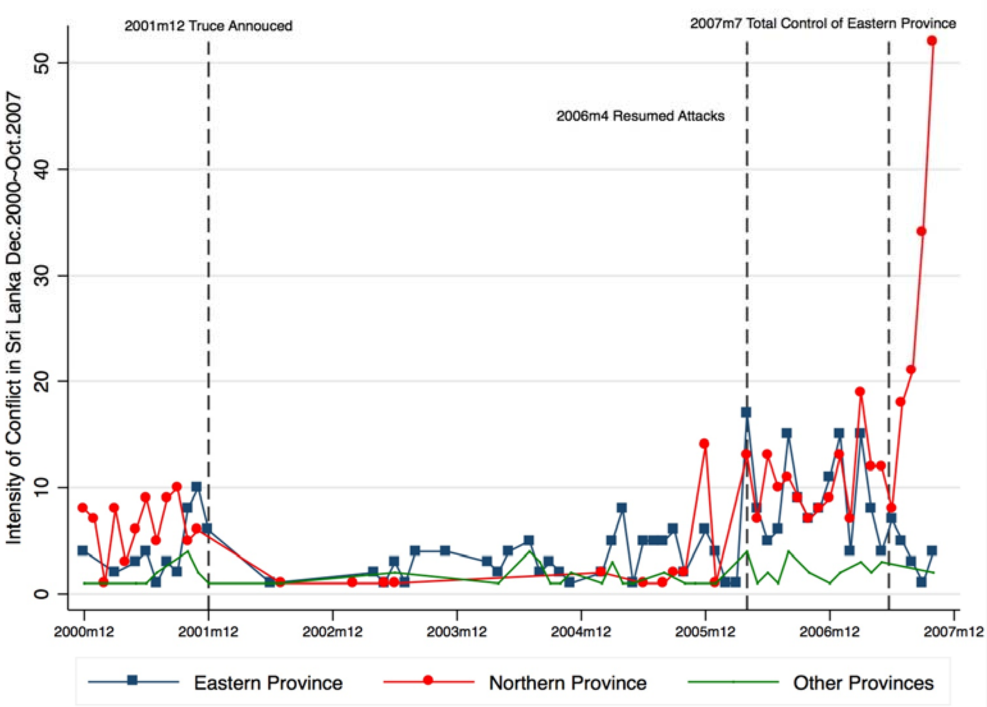

Figure 1: Intensity of the Eelam War across Regions over Time (December 2000 to October 2007)

Note: The data source is the UCDP GED. Intensity of the civil conflict denotes the number of conflict events in each month. Representative month of an event is determined based on the middle date of the start and end dates of each event. Eastern Province consists of three districts, namely Trincomalee, Batticaloa, and Ampara. Northern Province comprises five districts, namely Jaffna, Kilinochchi, Mannar, Mullattivu and Vavuniya. Other Provinces consist of the rest of 17 districts.

The LTTE officially pulled out of peace talks in April 2006,[8] which marked the beginning of Eelam War IV. Further violence followed, including the Vankalai massacre[9] and the Kebithigollewa massacre.[10] The renewed hostilities began to escalate, especially after the failure of a Norway-brokered ceasefire on July 21, 2006, when the LTTE cut off the water supply to the rice growing fields in the Mavil Aru area in the eastern Trincomalee district. In December 2006, the Sri Lankan government commenced the removal of the LTTE from of the Eastern Province,[11] eventually taking control of the province in July 2007.[12] This was the

[7] "Tamil Tigers call off peace talks." BBC News. 21 April 2003. Retrieved on 16 June 2018.
[8] "Bomb targets Sri Lanka army chief." BBC News. 25 April 2006. Retrieved on 16 June 2018.
[9] "People terrorized after massacre of Tamil family." AsiaSat News. 13 June 2006. Retrieved on 16 June 2018.
[10] "Over 60 dead in Sri Lanka bus bombing." The Guardian. 15 June 2006. Retrieved on 16 June 2018.
[11] "Sri Lanka military vows to drive Tigers from east coast." Reuters. 14 December 2006. Retrieved on 16 June 2018.

beginning of the defeat and retreat of the LTTE. The LTTE kept losing control over the territories in the north, including Kilinochchi, the *de facto* capital of the LTTE, in January 2009. After controversial battles involving the use of Tamil civilians as "human shields," the government announced victory over the LTTE on May 16, 2009.

2.2 Temporal and Spatial Variations of War Intensity

Since our main sample of empirical analysis consists of children under five years of age at the time of the interview in the SLDHS, relevant conflict episodes are limited to the period between December 2000 and October 2007, which covers the periods both in utero and after birth of all children under five in the sample. These children experienced the end of Eelam War III and part of Eelam War IV, with the cease-fire period in between. This range allows a large temporal variation of war intensity (Figure 1).

During this period, 737 conflict events[13] are recorded for the whole island, among which 672 (over 91%) happened in Northern and Eastern provinces, while the total number of events in other provinces is only 65 (Appendix Table B). Figure 2 displays spatial distribution of the conflict events. It is obvious that the majority of the battles were fought in the LTTE-occupied areas, namely the Northern and Eastern Provinces. Therefore, we consider eight districts in the Northern and Eastern provinces as the conflict zone, and the rest of the districts as the non-conflict zone. There are 17 districts in the non-conflict zone in Sri Lanka, out of which seven districts have common borders with the districts in the conflict zone. The number of violent events happened in these seven districts is 29. The same number of events also happened in the national capital area, Colombo, which has no common border with the districts in the conflict zone (Appendix Table B).

[12] "Sri Lanka declares fall of rebel east, Tigers defiant." Reuters. 11 July 2007. Retrieved on 16 June 2018.

[13] In the UCDP GED, a conflict event is defined as "The incidence of the use of armed force by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death" (Sundberg and Melander, 2013, p.524).

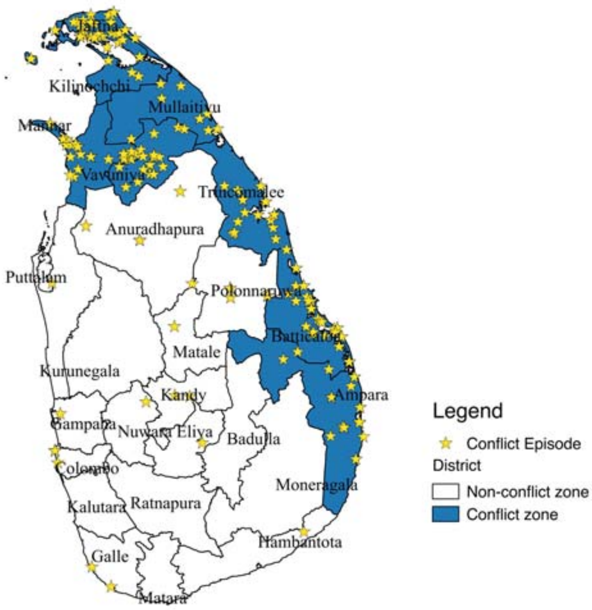

Figure 2: Spatial Distribution of the Conflict Events in Sri Lanka (December 2000 to October 2007)
Note: Conflict zone consists of Northern and Eastern Provinces. The location of each event is taken from the UCDP GED. When exact location is not identified in the UCDP GED, the representative location (i.e., centroid) of the district or the province is used instead. In such cases, a star represents multiple events.

3. Research Design

3.1 Estimation of Indirect Effect of Conflict

To examine the indirect effect of conflict (i.e., an influence of violent events broke out in the conflict zone on people who live in the non-conflict zone) we focus on an influence of conflict events occurred in *neighboring* districts in the conflict zone.[14] As we explained before, we define the conflict zone as eight districts in the Northern and Eastern provinces. Among the other 17 districts in the non-conflict zone, seven districts have common borders with the districts in the conflict zone. Thus, children living in these seven districts are the treatment group in our

[14] When measuring war intensity, whether "district" or some other administrative unit should be employed may be a controversial issue. However, the use of district can be justified in two ways. First, among 4,575 conflict episodes of the Sri Lankan civil war coded in the UCDP GED, 4,089 events (or 89%) can be identified at the district level, while only 1,390 events (or 30%) have exact geographical information which can be identified at a smaller unit than district, such as divisional secretariats and grama niladhari divisions. Second, in the literature, war intensity has been commonly measured at some sort of administrative unit. With only a few exceptions, previous studies have used a larger unit than a village/town/community. Thus, for ease of comparison with previous studies (i.e., comparison between the indirect spillover effect of war intensity in this study and the direct influences found in the literature), there are advantages to measuring war intensity at the district level.

analysis, while the control group consists of children living in the remaining ten districts in the non-conflict zone. The SLDHS covered part of the districts in the conflict zone. However, due to security concerns, the SLDHS only surveyed households in three districts in the conflict zone of the Eastern Province. Because of our focus on indirect spillover effects of conflict, we excluded observations of the children living in these three districts from our analysis.

Our empirical strategy hinges on two sources of variation: spatial and temporal differences in war intensity in neighboring districts, which are considered to be plausibly exogenous. Since citizens cannot control the time, place, and intensity of conflict events, these are expected to be orthogonal to observed and unobserved household characteristics. In this study, we focus on the indirect effect of conflict on people living in the non-conflict zone. Therefore, the assumption of exogeneity of war intensity in the neighboring conflict zone seems plausible. The validity of our identification assumption will be discussed further in section 4.2.

As we mentioned before, violent events also broke out in the non-conflict zone, although the number was considerably smaller than those in the conflict zone. Therefore, we control for direct influences of violent events happened in the district of residence of a child. We denote war intensity in district j's neighboring districts in the conflict zone when a child born in year-month c was in a developmental stage s as $WarNeighbor_{jcs}$. We denote war intensity in district j for a child born in year-month c and in a developmental stage s as War_{jcs}. Our variable of interest is $WarNeighbor_{jcs}$, which is set to zero for children in the 10 districts without common borders with the conflict zone. Our main estimation equation is

$$(1) \qquad Health_{ijc} = \alpha + \textstyle\sum_s \left(\beta_s \times WarNeighbor_{jcs}\right) + \sum_s \left(\rho_s \times War_{jcs}\right) + \boldsymbol{X}'_{ij}\boldsymbol{\gamma} + \delta_j + \eta_c + \varepsilon_{ijc},$$

where $Health_{ijc}$ is a measure of health status of child i in district j who was born in year-month c (year and month birth cohort), $\boldsymbol{X}_{ij}$ is a vector of individual and household characteristics, δ_j and η_c are district and birth (year-month) cohort fixed effects respectively, ε_{ijc} is an unobserved component, and α, β_s, ρ_s and $\boldsymbol{\gamma}$ are parameters to be estimated. The developmental stages of children, denoted by s, which this study focuses on are fetal (the 9 months from conception to birth) and after-birth (0 to 59 months) periods. In the analysis below, we also disaggregate the fetal and after-birth periods into shorter sub-periods.

3.2 Empirical Variables and Summary Statistics

The individual, household, and community data used in the analysis are from the SLDHS conducted in 2006/2007. This study limits the sample to children under five years of age (i.e., age in months is less than or equal to 59 and the eldest birth cohort was born in September 2001), which amounts to 7,487 children.

To measure $Health_{ijc}$ in Equation (1), we employ three different indicators of child health: height-for-age Z-score (HAZ), weight-for-age Z-score (WAZ), and birthweight. HAZ reflects health and nutritional conditions in the past and, thus, is often interpreted as a long-term measure of child health. HAZ is shown to have a positive association with standardized cognitive test scores in school, adult height, and labor market outcomes (Case and Paxson, 2008). WAZ is more likely to be affected by recent shocks and, thus, is seen as a short-term measure of child health. On the other hand, birthweight reflects growth experience only *in utero* and is affected by the health condition of the mother during pregnancy. Low birthweight is shown to decrease school attainment and leads to lower adult height and worse labor market outcomes (Behrman and Rosenzweig, 2004). Since birthweight cannot be affected by after-birth shocks, we exclude after-birth war intensity variables in empirical analysis for birthweight below.

Regarding war intensity variables, $WarNeighbor_{jcs}$ and War_{jcs}, this study uses the number of conflict events that broke out in neighboring districts in the conflict zone and in the district of residence, respectively. We decided not to use the number of casualties as a measure of war intensity because, in Sri Lanka, many people are still missing even after 10 years have passed since the end of the war. Actually, the UCDP GED provides three different estimates of the number of deaths in each event: highest, lowest, and the best estimates. Among the data for Sri Lanka, more than 11 percent of the events have different estimates of the highest and the lowest number of deaths, and the mean difference is 28.8 persons. Therefore, we doubt the reliability of the estimated number of casualties in this data and use the number of events in each month as a measure of war intensity instead.

Other controls in $\boldsymbol{X}_{ij}$ are individual and household characteristics. As individual controls, we use gender of the child and information on birth, such as whether the child is one of multiple births and cesarean delivery. Household level controls are height, age, and education level of the mother; gender and ethnicity of the household head; household size; asset ownership dummies; an urban dummy; and a dummy for tsunami affected households. Assets include house, having electricity, radio, TV, mobile phone, telephone, bicycle, motorcycle, car, and agricultural land.

After dropping observations with incomplete information or extreme values on a health outcome,[15] the numbers of children in our sample are 5,229, 5,221, and 4,883 for the analysis of HAZ, WAZ, and birthweight, respectively. Appendix Table C shows summary statistics of the empirical variables mentioned in this section.

4. Empirical Results

4.1 Main Regression Results

Table 1 reports the results of the estimation of equation (1). Columns (1) to (3) show estimation results with the whole sample for HAZ, WAZ, and birthweight respectively. We find that the estimated coefficients on $WarNeighbor$ (after birth) are both negative and statistically significant for height (HAZ) and weight (WAZ) of children. These estimates imply that one violent event after birth in the neighboring war districts reduces both HAZ and WAZ of children by 0.004 standard deviations. Since the mean number of conflict events happened in the neighboring war districts among children in the treatment group is 45.5, these estimates imply average reduction of HAZ and WAZ by 0.17 standard deviations, which is smaller than reduction of HAZ by 0.3 standard deviations found by Baez (2011). His finding of the larger impact is reasonable, because he analyzes the inflow of over 500,000 refugees from Burundi and Rwanda to Tanzania, and the inflow of Rwandan refugees is described by UNHCR as "the largest and fastest refugee movement in modern history" (p.392).

All of the coefficients for the war intensity variables *in utero* are not statistically different from zero at conventional significance levels. It seems that the negative indirect effect is caused only by the shocks after birth. Since after-birth shocks are not a significant factor in most of the previous literature on the direct impacts of violence, the causal mechanism of

[15] For the main sample for HAZ, 597 children were excluded due to missing values on height; 46 children were excluded due to extreme values in HAZ (i.e., HAZ less than -6); 268 children were excluded due to lack of information on mother's height; and 552 children were excluded due to incomplete information on child birth and mother characteristics. We drop 788 children living in the conflict zone, seven children due to extreme values of mother's height (identified from BMI less than 12 or greater than 60), leaving 5,229 children in the final sample. Additionally, we excluded eight children due to lack of information of weight or extreme values in WAZ (i.e., WAZ less than -6), and 346 children without information on birthweight, respectively.

indirect influence of violent conflict on children in non-conflict zones seems to be different from the mechanism of direct exposure to conflict.

Table 1: Main estimation results

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	HAZ	WAZ	Birthweight	HAZ	WAZ	Birthweight
Sample:	Whole sample	Whole sample	Whole sample	Rural	Rural	Rural
War (in utero)	-0.0154	-0.0042	-1.4231	0.0215	0.0412	2.7807
	0.0193	(0.0194)	(6.5497)	(0.0326)	(0.0308)	(10.7851)
War (after birth)	-0.0128	0.0037		0.0191	0.0230	
	0.0078	(0.0083)		(0.0199)	(0.0218)	
WarNeighbor (in utero)	-0.0004	0.0005	-0.4428	-0.0012	-0.0024	-1.1914
	0.0041	(0.0038)	(1.1772)	(0.0046)	(0.0042)	(1.3259)
WarNeighbor (after birth)	-0.0038*	-0.0037**		-0.0049**	-0.0047**	
	0.0019	(0.0017)		(0.0023)	(0.0020)	
No. of observations	5,229	5,221	4,883	4,174	4,167	3,880
R^2	0.2297	0.1737	0.1577	0.2368	0.1785	0.1638

(Note) All regressions include individual and household characteristics, and district and birth cohort fixed effects. All standard errors are adjusted for clustering within the enumeration area, and shown in parentheses. Asterisks, *, **, and ***, next to an estimate imply that each estimate is significantly different from 0 at the 10, 5, and 1 percent level respectively.

The estimation results for the whole sample need to be interpreted carefully. It is well-known that the LTTE has been using suicide attacks as one of their main strategies. During the Eelam War, beginning with the first suicide attack in 1987, the LTTE committed at least 137 suicide attacks.[16] These violent attacks in the non-conflict zone may have targeted affluent urban areas and economic and political nerve centers, where people are relatively wealthier than other non-conflict zones.[17] Therefore, these estimates may be biased.

In order to eliminate the bias, we limit the sample to children living in the rural area, and the estimation results are shown in columns (4) to (6) in Table 1. We still find that the indirect spillover effect of violent events remains negative and significant for HAZ and WAZ. Moreover, the absolute magnitude of the coefficients on $WarNeighbor$ (after birth) becomes larger, and now implies that conflict events after birth in the neighboring war districts reduce HAZ and WAZ of children by more than 0.2 standard deviations on average.

Previous studies on direct exposure to violent conflicts also find larger impacts than the indirect effects in our results. For example, Minoiu and Shemyakina (2014) show that direct exposure to conflict reduces HAZ of children by 0.2 to 0.4 standard deviations in Cote d'Ivoire. Weldeegzie (2017) finds that HAZ is lower by 0.30 to 0.38 standard deviations among Ethiopian children due to direct exposure to the conflict with Eritrea. It is natural that our estimate of the indirect spillover effect is smaller. Nonetheless, if we compare the estimate effects with the average HAZ in our sample in Appendix Table C, our estimate implies 18 to 20 percent reduction in HAZ on average.

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[16] See the article covering an interview of Robert Pape, entitled "Tamil Tigers: Suicide Bombing Innovators," on NPR at https://www.npr.org/templates/story/story.php?storyId=104391493 (accessed on July 7, 2019). Only one of 137 suicide attacks happened abroad, killing the former Prime Minister of India, Rajiv Gandhi, in 1991. All other attacks were committed in Sri Lanka.

[17] Actually, this could be the reason why Colombo district had the largest number of violent events in the non-conflict zone. The Western Province, consisting of the Colombo, Gampaha, and Kalutara districts, has been producing around 40 percent of total national GDP in Sri Lanka (Central Bank of Sri Lanka, 2018; p.36).

4.2 Validity of Identification Strategy

One concern for our identification strategy is possibility of endogenous residential choice. People can choose a safer place to live by fleeing from violent conflict. Therefore, individual exposure to conflict events is partly an outcome of individual choice, although this potential threat to the identification is common to most studies on the impact of conflict exposure. In order to deal with the problem of endogenous residential choice, we conducted estimations in Table 2 with the sample further limited to children of mothers without migration experience across districts.[18] The results show that the indirect effects, indicated by the coefficients on $WarNeighbor$ (after birth), remain negative and significant for HAZ and WAZ.[19] We also confirm that all of the estimated coefficients for the war intensity variables during the fetal period are not statistically different from zero at conventional significance levels. In column (2) on WAZ, the coefficient on War (after birth) is positive and significant at 10 % level. This result may reflect positive survival bias.

Table 2: Robustness checks with non-migrant samples

	(1)	(2)	(3)
Outcome:	HAZ	WAZ	Birthweight
Sample:	Rural & Non-Migrants	Rural & Non-Migrants	Rural & Non-Migrants
War (in utero)	0.0577	0.0631	7.0229
	(0.0404)	(0.0439)	(13.3077)
War (after birth)	0.0295	0.0406*	
	(0.0238)	(0.0244)	
WarNeighbor (in utero)	-0.0042	-0.0040	-2.1795
	(0.0054)	(0.0051)	(1.5276)
WarNeighbor (after birth)	-0.0044*	-0.0047**	
	(0.0026)	(0.0023)	
No. of observations	3,205	3,199	2,959
R^2	0.2503	0.1797	0.1750

(Note) All regressions include individual and household characteristics, and district and birth cohort fixed effects. All standard errors are adjusted for clustering within the enumeration area, and shown in parentheses. Asterisks, *, **, and ***, next to an estimate imply that each estimate is significantly different from 0 at the 10, 5, and 1 percent level respectively.

[18] Though the SLDHS did not directly ask for the specific locations of residence for each household during the war, it included district-level information on the former residence of eligible women and the length of time spent in the current place of residence, which can be used to trace the migration experience of a mother prior to the current residence.

[19] We also conducted estimation with mother-fixed effects, and use only variations across siblings of the same mother for estimation. Among the whole sample of 5,229 children in Column (1) of Table 1, 3,920 children do not have any siblings under the age of five from the same mother. For the rest of the 1,309 children, 100 children were born in multiple births. Then, out of the remaining 1,209 children, only 305 children were residents in the seven districts of the non-conflict zone having neighboring war districts. With more than 70 birth cohorts, the size of identifying observations is too small for the estimation of parameters on birth-cohort fixed effects. The estimates are naturally very imprecise and not shown here.

Since our sample of analysis consists of children less than five years of age, it is hard to posit any unobservable child characteristics that would be correlated with war intensity variables. Thus, if any unobservable factors cause bias in our estimation, such factors must be part of household characteristics and should also affect the health outcomes of other household members. For example, if some households do not have access to social networks when they collect information about violent conflict happening in neighboring districts, their reaction to negative events tends to be slow, and the negative consequences would be larger.

In order to test possibility of unobservable confounding factors, we conducted placebo tests. We replaced the outcome variable, $Health$, in equation (1) with the height and weight of the mother of the child, and estimated the same equation as in Tables 1 and 2. Table 3 shows the results of the placebo tests. In all columns except for column (4), the estimated coefficients for the war intensity variables are not statistically significant. In column (4), the estimated coefficient for $WarNeighbor$ (after birth) for mother's weight with the whole sample is statistically significant at 10 percent level. However, as we limit the sample in columns (5) and (6), the coefficient becomes insignificant. Moreover, contrary to our findings on the negative indirect spillover effects in Tables 1 and 2, the estimated indirect effects after birth are positive. Thus, the results in Table 3 provide additional evidence for our assumption of exogeneity of the war intensity variables.

Table 3: Results of placebo tests with height and weight of a mother as outcome

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Height	Height	Height	Weight	Weight	Weight
Sample:	Whole sample	Rural	Rural & Non-Migrants	Whole sample	Rural	Rural & Non-Migrants
War (in utero)	-0.0845	-0.1607	-0.056	0.0193	-0.3901	-0.2704
	(0.1068)	(0.1698)	(0.2387)	(0.2086)	(0.3156)	(0.3696)
War (after birth)	0.0087	-0.0051	0.0413	0.0835	-0.0877	0.0181
	(0.0422)	(0.0926)	(0.1295)	(0.0829)	(0.1639)	(0.1934)
WarNeighbor (in utero)	-0.0005	-0.0006	0.0017	0.0421	0.0354	0.0605
	(0.0215)	(0.0221)	(0.0271)	(0.0457)	(0.0466)	(0.0541)
WarNeighbor (after birth)	0.0079	0.0078	0.0113	0.0393*	0.0319	0.0400
	(0.0138)	(0.0151)	(0.0188)	(0.0222)	(0.0243)	(0.0283)
No. of observations	5,229	4,174	3,205	5,229	4,174	3,205
R^2	0.0954	0.0976	0.1100	0.1906	0.1788	0.1974

(Note) All regressions include individual and household characteristics, and district and birth cohort fixed effects. The only difference from Table 1 is that we excluded the dependent variable, mother's height, from the control variables in these estimations. All standard errors are adjusted for clustering within the enumeration area, and shown in parentheses. Asterisks, *, **, and ***, next to an estimate imply that each estimate is significantly different from 0 at the 10, 5, and 1 percent level respectively.

5. Causal Mechanism

The results in the previous section suggest that the negative indirect effect of war intensity in the neighboring conflict zone on child health did exist. Previous literature suggests three possible mechanisms through which conflict may have a negative effect on child health, namely deteriorated health environment, mother's stress, and nutritional deficiency. We will examine these mechanisms in turn.

5.1 Deteriorated Health Environment

As mentioned in the introduction, previous studies suggest that the deteriorated health environment caused by an inflow of IDPs may put a burden on the health of vulnerable people in the non-conflict zone. We investigate this possibility by estimating the following equation with district-level annual panel data:

(2) $$Y_{jt} = \gamma_0 + \gamma_1 War_{jt} + \gamma_2 WarNeighbor_{jt} + \delta_j + \mu_t + \upsilon_{jt},$$

where Y_{jt} is an infant/ within-one-month mortality rate in district j in year t, δ_j and μ_t are district and year fixed effects, and υ_{jt} is an error term.[20] Since we cannot exclude possibility of district-level time-variant confounders, empirical evidence provided by estimation of equation (2) needs to be taken as suggestive.

Table 4: Estimation on mortality rates

	(1)	(2)	(3)	(4)
Outcome variables	Infant Mortality Rate		Within-One-Month Mortality	
Period of analysis	2000-2007		2000-2007	
War	-0.3456		-0.2902	
	(0.4330)		(0.4460)	
	{0.4853}		{0.4983}	
WarNeighbor	-0.0281		-0.0240	
	(0.1190)		(0.1320)	
	{0.1338}		{0.1164}	
War (t-1)		0.2546		0.2475
		(0.2970)		(0.2760)
		{0.1384}		{0.1288}
WarNeighbor (t-1)		-0.1065		-0.0907
		(0.0160)**		(0.0260)**
		{0.0246}**		{0.0216}**
No. of observations	136	136	136	136
No. of districts	17	17	17	17
R^2	0.3088	0.3242	0.2921	0.3053

(Note) All regressions include district and year fixed effects. We adjust standard error for clustering at the district level, and clustered standard errors are used for p-values of t statistics in parentheses. Since the number of districts is 17 and small, wild bootstrap estimates of p-values are also shown in curly brackets. Asterisks, *, **, and ***, next to p-values imply that each estimate is significantly different from 0 at the 10, 5, and 1 percent level respectively.

The estimation results are shown in Table 4. In columns (1) and (3), we find that the point estimates of coefficients on contemporary war intensity variables, War and $WarNeighbor$, are negative but not statistically significant at conventional levels. When we take a one-year lag of these variables, the indirect effects become negative and significant in columns (2) and (4). These results suggest that IDP inflows do not worsen health environment, and after one year, the health environment in these non-war districts seems to have improved

[20] Annual data on infant and within-one-month mortality rates are calculated from the data on vital statistics available at http://www.statistics.gov.lk/ (accessed on August 11, 2019).

possibly due to assistance from the government and international organizations. Thus, the health environment hypothesis is not supported by the data.

5.2 Stress Hypothesis and Nutritional Deficiency

The empirical results in section 4 suggest that the indirect spillover effect of war intensity in the neighboring war districts on child health takes place after birth. Hence, the psychological stress of mothers cannot be the causal factor of the indirect effect. In order to confirm this, we further disaggregate the periods in utero and after birth to see in which period violent events in the neighboring war districts are crucial. The pregnancy period is divided into three trimesters, and the period after birth is also separated into three sub-periods with ages in months less than 12, from 12 to 35, and from 36 to 59 months. Table 5 shows estimation results of equation (1) for HAZ, WAZ and birthweight.

Table 5: Estimation results with disaggregated child development periods			
	(1)	(2)	(3)
Outcome:	HAZ	WAZ	Birthweight
Sample:	Rural & Non-Migrants	Rural & Non-Migrants	Rural & Non-Migrants
War (1st trimester)	0.0512	0.0588	22.8310
	(0.0556)	(0.0598)	(22.6585)
War (2nd trimester)	0.0522	0.0305	-21.7609
	(0.0642)	(0.0674)	(22.9804)
War (3rd trimester)	0.0986*	0.0933	21.9903
	(0.0583)	(0.0781)	(23.1932)
War (after birth, less than 12 months)	0.0346	0.0385	
	(0.0393)	(0.0462)	
War (after birth, 12 to 35 months)	0.0372	0.0472	
	(0.0290)	(0.0347)	
War (after birth, 36 to 59 months)	0.0211	0.0333	
	(0.0328)	(0.0347)	
WarNeighbor (1st trimester)	-0.0046	-0.0048	4.4223
	(0.0120)	(0.0123)	(4.9887)
WarNeighbor (2nd trimester)	0.0110	0.0087	-0.8217
	(0.0121)	(0.0125)	(4.8490)
WarNeighbor (3rd trimester)	-0.0138	-0.0086	-6.6005*
	(0.0119)	(0.0102)	(3.3769)
WarNeighbor (after birth, less than 12 months)	-0.0082**	-0.0067*	
	(0.0041)	(0.0039)	
WarNeighbor (after birth, 12 to 35 months)	-0.0045	-0.0045	
	(0.0029)	(0.0029)	
WarNeighbor (after birth, 36 to 59 months)	-0.0055**	-0.0043	
	(0.0028)	(0.0027)	
No. of observations	3,205	3,199	2,959
R^2	0.2513	0.1551	0.1781

(Note) All regressions include individual and household characteristics, and district and birth cohort fixed effects. All standard errors are adjusted for clustering within the enumeration area, and shown in parentheses. Asterisks, *, **, and ***, next to an estimate imply that each estimate is significantly different from 0 at the 10, 5, and 1 percent level respectively.

The estimates of the indirect effect are consistently negative after the third trimester and are significant after birth before the age of one for HAZ and WAZ. For birthweight, the estimated coefficient of the war intensity in the neighboring districts in the conflict zone during the third trimester is negative and significant. These periods just before and soon after the birth are the periods when the babies grow most. Therefore, we conclude that nutritional deficiency is the cause of the negative indirect effect. Since none of the impact during the first trimester is significant, the stress hypothesis can be rejected.

5.2.1 Inflow of IDPs

How did nutritional deficiency happen? It is reasonable to expect that districts geographically close to the conflict zone may experience larger inflows of IDPs, which put upward pressure on food prices. Here, we investigate whether IDP inflows are caused by violent events in the neighboring war districts. Due to limited information on IDPs during the wartime, we could only collect annual data on the number of IDPs during the period from 2003 to 2007.[21] Using annual panel data of 17 districts in the non-conflict zone, we analyze how the war intensity variables, War and $WarNeighbor$, affect the number of IDPs by estimating equation (2) with the dependent variable, Y_{jt}, replaced by natural log of (1 + the number of IDPs) in district j in year t.

Table 6: Estimation on IDPs

	(1)	(2)
Outcome variables	Ln Number of IDPs	
Period of analysis	2003-2007	
War	-0.3386	
	(0.026)**	
	{0.1294}	
WarNeighbor	0.0170	
	(0.009)***	
	{0.1172}	
War (t-1)		-0.4465
		(0.0040)***
		{0.1798}
WarNeighbor (t-1)		0.0412
		(0.0300)**
		{0.1046}
No. of observations	85	85
No. of districts	17	17
R^2	0.7231	0.7417

(Note) All regressions include district and year fixed effects. We adjust standard error for clustering at the district level, and clustered standard errors are used for p-values of t statistics in parentheses. Since the number of districts is 17 and small, wild bootstrap estimates of p-values are also shown in curly brackets. Asterisks, *, **, and ***, next to p-values imply that each estimate is significantly different from 0 at the 10, 5, and 1 percent level respectively.

Table 6 shows estimation results. The point estimates of coefficients in column (1) are intuitive; violent events occurring within a district reduce the number of IDPs there, but such events in a neighboring conflict zone cause an inflow of IDPs. However, when we use the wild bootstrap method to adjust for the small number of districts, these estimates become marginally

[21] The data sources used in section 5 are detailed in Appendix E.

insignificant at 12 and 13 percent levels.[22] The point estimates imply that violent events in the district of residence reduce the number of IDPs by 20.7%, but violent events in neighboring war districts increase the number of IDPs by 51.0% on average, both evaluated at the mean values of the war intensity variables in the treatment districts.[23]

Column (2) of Table 6 takes lagged effects into account and uses one-year lagged values of war intensity variables as predictors. The estimation results show that violent events within a district during the previous year reduce the number of IDPs by 26.4%, but violent events in the neighboring war districts during the previous year increase the number of IDPs by as much as 171.3% on average. Again, the wild bootstrap estimates of p-values are larger than conventional levels at 18.0% and 10.5%, respectively. These results provide suggestive evidence of a huge inflow of IDPs caused by violent events in the neighboring war districts, which may put a burden on local resources.

5.2.2 Inflation of Food Prices

Next, we examine how violent conflict influences local food prices. Given that paddy is the main staple food in Sri Lanka, we collect annual data on producers' price for paddy from 2000 to 2007 and investigate whether conflict events in the neighboring war districts lead to inflation of paddy prices by estimating equation (2) with the outcome variable, Y_{jt}, replaced by natural log of paddy price.

Table 7: Estimation on paddy price and production

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome variables	Ln Paddy Price		Ln Paddy Sown Area		Ln Paddy Harvest Area	
Period of analysis	2000-2007		2001-2007		2001-2007	
War	-0.0041		-0.0172		-0.0168	
	(0.2200)		(0.055)*		(0.1140)	
	{0.5913}		{0.0724}*		{0.0818}*	
WarNeighbor	0.0006		0.0019		0.0021	
	(0.058)*		(0.000)***		(0.000)***	
	{0.1718}		{0.0264}**		{0.0232}**	
War (t-1)		-0.0025		-0.0191		-0.0216
		(0.4760)		(0.049)**		(0.036)**
		{0.4475}		{0.1060}		{0.1186}
WarNeighbor (t-1)		0.0009		0.0020		0.0025
		(0.098)*		(0.012)**		(0.000)***
		{0.1698}		{0.0486}**		{0.0332}**
No. of observations	126	126	119	102	119	102
No. of districts	17	17	17	17	17	17
R^2	0.8849	0.8835	0.3101	0.3075	0.2736	0.2663

(Note) All regressions include district and year fixed effects. We adjust standard error for clustering at the district level, and clustered standard errors are used for p-values of t statistics in parentheses. Since the number of districts is 17 and small, wild bootstrap estimates of p-values are also shown in curly brackets. Asterisks, *, **, and ***, next to p-values imply that each estimate is significantly different from 0 at the 10, 5, and 1 percent level respectively.

Columns (1) and (2) of Table 7 show the estimation results of the regression of paddy prices on contemporaneous and lagged war intensity variables, respectively. If we use cluster-robust standard errors, the estimated coefficients on $WarNeighbor$ are significant at the 10 percent level. However, the wild bootstrap estimates make all of the coefficients insignificant. Moreover, point estimates show small effects; if we evaluate the estimated coefficients in

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22 See Roodman et al. (2019).

23 See Appendix Table D for summary statistics.

column (1) at mean values of war intensity variables in the treatment districts, violent events within a district reduce the paddy price by 0.27%, but violent events in neighboring war districts increase the paddy price by 1.03%, both in the same year on average.

One possible reason for small effects on paddy price is existence of government intervention. Given the importance of rice, the Sri Lankan government started price controls of paddy via the Guaranteed Price Scheme (GPS) in the late 1940s.[24] Though the Paddy Marketing Board was abolished in 1977, the GPS kept its role of setting a floor price of rice. This mechanism of price stabilization might have kept fluctuation in paddy prices low. However, the existence of a floor price does not explain small inflationary pressure caused by inflow of IDPs.

Another explanation can be our use of annual price data. Due to data limitations, monthly paddy prices at the district level are not available. However, it is well-known that seasonal fluctuations in paddy prices are large (Dayaratna-Banda et al., 2008). The use of annual data may hide short-run price hikes due to shortages. Therefore, we cannot exclude the possibility of short-run nutritional deficiency in mothers caused by conflict events in the neighboring war districts. Annual fluctuation of paddy prices can also be mitigated by the supply responses of rice farmers, a possibility we investigate next.

5.2.3 Supply Responses

Here we examine the supply responses of rice farmers in terms of sown and harvested areas. We collect annual data on sown and harvested areas of paddy from 2001 to 2007.[25] Our estimation equation is equation (2) with the outcome variable Y_{jt} replaced by the natural log of sown or harvested areas.

Columns (3) and (5) of Table 7 show the estimated coefficients for contemporaneous war intensity variables. Columns (4) and (6) are the results with lagged war intensity variables. All of the coefficients on the indirect effect are positive and significant at 5 percent level, even with the p-values estimated by wild bootstrap method. Evaluation of these estimated coefficients at mean values implies that, on average, violent events in the neighboring war districts increase sown area of paddy by 3.8 to 4.0 percent and harvested area of paddy by 4.2 to 5.0 percent. Such supply responses reduce food shortages after the harvesting season. However, the supply response needs time to take effect, and we conclude that temporary shortages of food may have caused nutritional deficiency of new-born babies in the short run.

6 Concluding Remarks

In this study, we examined the indirect effect of violent conflict on child health in the non-conflict zone for the case of Sri Lankan civil war. Using variations in timing, location, and intensity of violent events, we found evidence for the negative indirect effect. Due to violent events that happened in the neighboring conflict zone, height-for-age and weight-for-age Z-scores of children in the non-conflict zone are reduced by 0.05 standard deviations on average. Thus, we found existence of a vulnerable population often neglected in the process of war reconstruction, that is, people living in neighboring areas of conflict zones.

While previous studies on the impacts of direct exposure to violence emphasize importance of mother's stress caused by violent events in early pregnancy, the indirect effect works during the period after birth before the age of one, when a child grows most. The causal pathway of the indirect effect seems to be different from the one for the direct impact. Our analysis on causal mechanisms suggests that inflow of IDPs may have caused a short-run nutritional deficiency of newborn babies, which suggests the importance of early policy

[24] See Henegedara (2002).

[25] See Appendix E for data sources.

responses in mitigating negative impacts on people in the neighboring non-conflict zones. However, our evidence on causal mechanism is still suggestive, and further investigation is needed to identify the exact causal pathways of the indirect effect of violent conflicts.

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Appendix A. Data Sources on New Web Sites

- The New York Times: https://www.nytimes.com/
- BBC News: https://www.bbc.com/news
- AsiaSat News: https://www.asiasat.com/news/
- The Guardian: https://www.theguardian.com/
- Reuters: https://www.reuters.com/

Appendix Table B: Number of conflict events by district (December 2000 to October 2007)

Area	Province	District	Number of Events	Total
War Area	Northern	Jaffna	142	672
		Kilinochchi	38	
		Mannar	62	
		Vavuniya	115	
		Mullaittivu	31	
	Eastern	Batticaloa	146	
		Ampara	41	
		Trincomalee	97	
Non-War Area	Western	Colombo	29	65
		Gampaha	1	
		Kalutara	0	
	Central	Kandy	2	
		Matale	1	
		Nuwara Eliya	1	
	North Western	Kurunegala	0	
		Puttalam	1	
	North Central	Anuradhapura	8	
		Polonnaruwa	18	
	Uva	Badulla	0	
		Moneragala	0	
	Sabaragamuwa	Ratnapura	0	
		Kegalle	1	
	Southern	Galle	2	
		Matara	0	
		Hambantota	1	

Appendix Table C: Summary statistics of individual-level data

	Number of observations	Mean	Standard deviation	Minimum	Maximum
Health outcomes					
HAZ	5,229	-0.9974	1.2110	-5.99	5.95
WAZ	5,221	-1.1921	1.0937	-5.94	4.58
birthweight	4,883	2,893.2810	492.7264	800	8,130
War intensity					
War (in utero)	5,229	0.4695	1.4366	0	9
War (in utero, treatment group only)	1,537	0.2726	0.9131	0	6
War (1st trimester)	5,229	0.1465	0.6472	0	7
War (2nd trimester)	5,229	0.1482	0.6581	0	7
War (3rd trimester)	5,229	0.1748	0.7371	0	7
War (after birth)	5,229	2.3372	5.6850	0	27
War (after birth, treatment group only)	1,537	1.5329	3.1621	0	13
War (after birth, less than 12 months)	5,229	0.6835	1.8835	0	10
War (after birth, 12 to 35 months)	5,229	1.1670	3.1918	0	16
War (after birth, 36 to 59 months)	5,229	0.4867	1.9034	0	15
WarNeighbor (in utero)	5,229	1.7694	6.7972	0	86
WarNeighbor (in utero, treatment group only)	1,537	6.0195	11.4741	0	86
WarNeighbor (1st trimester)	5,229	0.4833	2.0664	0	31
WarNeighbor (2nd trimester)	5,229	0.5835	2.5948	0	43
WarNeighbor (3rd trimester)	5,229	0.7026	3.0934	0	43
WarNeighbor (after birth)	5,229	13.3829	35.1514	0	235
WarNeighbor (after birth, treatment group only)	1,537	45.5296	52.3549	0	235
WarNeighbor (after birth, less than 12 months)	5,229	3.4146	11.9551	0	109
WarNeighbor (after birth, 12 to 35 months)	5,229	6.5211	21.3177	0	169
WarNeighbor (after birth, 36 to 59 months)	5,229	3.4471	15.5490	0	165
Individual controls					
female	5,229	0.4905	0.5000	0	1
age in months	5,229	29.3729	17.0243	0	59
multiple biirth	5,229	0.0197	0.1390	0	1
Cesarean delivery	5,229	0.2391	0.4265	0	1
Characteristics of mother					
height of mother	5,229	152.4239	5.6499	104.3	199.1
weight of mother	5,229	52.4902	10.8148	30.2	116.5
age of mother	5,229	30.1964	5.9186	15	49
dummies for different education level:					
no schooling	5,229	0.0008	0.0277	0	1
incomplete primary schooling	5,229	0.0681	0.2519	0	1
complete primary schooling	5,229	0.0337	0.1804	0	1
incomplete secondary schooling	5,229	0.1977	0.3983	0	1
complete secondary schooling	5,229	0.3257	0.4687	0	1
more than secondary schooling	5,229	0.3490	0.4767	0	1
Household characteristics					
male-headed household	5,229	0.8028	0.3979	0	1
household size	5,229	5.2241	1.8405	2	17
Sinhalese	5,229	0.7644	0.4244	0	1
Sri Lankan Tamil	5,229	0.0587	0.2351	0	1
Moor	5,229	0.0920	0.2890	0	1
urban	5,229	0.2018	0.4014	0	1
tsunami-affected household	5,229	0.0300	0.1707	0	1
dummies for asset ownership:					
house	5,229	0.7675	0.4225	0	1
electricity	5,229	0.8026	0.3980	0	1
radio	5,229	0.7778	0.4158	0	1
TV	5,229	0.7958	0.4032	0	1
mobile phone	5,229	0.4022	0.4904	0	1
telephone	5,229	0.3351	0.4721	0	1
bicycle	5,229	0.3622	0.4807	0	1
motorcycle	5,229	0.2576	0.4374	0	1
car	5,229	0.0857	0.2799	0	1
agricultural land	5,229	0.3253	0.4685	0	1

Appendix Table D: Summary statistics of district-level panel data (Tables 4, 6 and 7)					
	Number of observations	Mean	Standard deviation	Minimum	Maximum
Table 4					
Infant mortality rate (‰)	136	10.31	5.29	1.88	27.57
Within-one-month mortality rate (‰)	136	7.72	4.54	0.61	24.69
War	136	0.61	1.60	0.00	9.00
War (treatment districts only)	56	0.66	1.37	0.00	6.00
WarNeighbor	136	7.53	25.18	0.00	227.00
WarNeighbor (treatment districts only)	56	18.29	36.83	0.00	227.00
Table 6					
Number of IDPs (persons)	85	2,161.81	9,393.94	0.00	61,763.00
War	85	0.67	1.76	0.00	9.00
War (treatment districts only)	35	0.69	1.55	0.00	6.00
WarNeighbor	85	9.98	30.89	0.00	227.00
WarNeighbor (treatment districts only)	35	24.23	44.75	0.00	227.00
Table 7					
Column (1)					
Producer price of paddy (samba) (Rs./kg)	126	13.77	2.32	9.54	20.97
War	126	0.59	1.59	0.00	9.00
War (treatment districts only)	55	0.65	1.38	0.00	6.00
WarNeighbor	126	7.98	26.07	0.00	227.00
WarNeighbor (treatment districts only)	55	18.27	37.17	0.00	227.00
Columns (3) and (5)					
Sown area (hectares)	119	33,074.97	29,317.45	4,233.00	134,158.00
Harvested area (hectares)	119	31,587.03	27,553.85	4,227.00	118,125.00
War	119	0.58	1.54	0.00	9.00
War (treatment districts only)	49	0.63	1.39	0.00	6.00
WarNeighbor	119	7.97	26.68	0.00	227.00
WarNeighbor (treatment districts only)	49	19.37	39.05	0.00	227.00

Appendix E. Data Sources for Estimation in Tables 5 and 6

E.1 Number of IDPs by District

The number of IDPs in each district is derived from the maps published by the UN Refugee Agency in August 2003[26], December 2004, August 2005[27], December 2006[28] and October 2007[29]. IDPs covered in the maps not only include those living with the relatives but also include those living in the welfare centers (i.e. IDP camps).

E.2 Price of Paddy by District

The data of producer's prices for paddy (samba) is derived from Table 13 of Bulletin of Selected Retail and Producer Price 1999-2003, Table 13 of Bulletin of Selected Retail and Producer Price 2002-2005, and Table 12 of Bulletin of Selected Retail and Producer Price 2006-2009, all of which were published by Department of Census and Statistics, Ministry of Finance and Planning, Sri Lanka.

E.3 Sown and Harvested Areas of Paddy by District

We collected data on paddy extent sown and harvested from the Paddy Statistics[30] Agriculture and Environment Statistics Division, Department of Census and Statistics.

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[26] Sri Lanka IDP Movements by District in August 2003. https://reliefweb.int/map/sri-lanka/sri-lanka-idp-movement-district-aug-03. (Access on July 31st, 2018)

[27] Sri Lanka Displaced Population by District. https://reliefweb.int/sites/reliefweb.int/files/resources/8B187221491A7864852570DF007077E4-unhcr_IDP_lka011205.pdf (Access on July 31st, 2018)

[28] IDP Movement Trends between Districts. https://reliefweb.int/map/sri-lanka/sri-lanka-idp-movement-trends-between-districts-18-dec-2006. (Access on July 31st, 2018).

[29] IDPs and Returnees Trends by District. https://reliefweb.int/map/sri-lanka/sri-lanka-idps-and-returnees-trends-district-31-oct-2007. (Access on July 31st, 2018).

[30] Paddy Statistics http://www.statistics.gov.lk/agriculture/Paddy%20Statistics/PaddyStats.htm (Access on July 31st, 2018).