**Article:**

Kreif, Noemi, Mirelman, Andrew orcid.org/0000-0002-7622-0937, Suhrcke, Marc orcid.org/0000-0001-7263-8626 et al. (2 more authors) (2022) The Impact of Civil Conflict on Child Health : Evidence from Colombia. Economics and Human Biology. 101074. ISSN 1570-677X

https://doi.org/10.1016/j.ehb.2021.101074

# The Impact of Civil Conflict on Child Health: Evidence from Colombia[1]

Noemi Kreif[1], Andrew Mirelman[1], Marc Suhrcke[1,2], Giancarlo Buitrago[3], Rodrigo Moreno Serra[1]

[1]Centre for Health Economics, University of York, Heslington, York YO10 5DD, UK

[2]Luxembourg Institute of Socio-economic Research, 11 Porte des Sciences, 4366 Esch-sur-Alzette, Luxembourg

[3]Clinical Research Institute, Universidad Nacional de Colombia, Hospital Universitario Nacional de Colombia, Bogota, DC, Colombia

Corresponding author: Noemi Kreif: Noemi.kreif@york.ac.uk, Centre for Health Economics, University of York, Heslington, York, YO10 5DD, UK, +44 (0)1904 321401

[1] The research was part of the "War and Peace" grant (ref: MR/R013667/1), funded by the UK Medical Research Council (MRC), jointly with ESRC/DFID/Wellcome Trust, through the Joint Health Systems Research Initiative (HSRI).

## Abstract

Internal armed conflicts have become more common and more physically destructive since the mid-20th century, with devastating consequences for health and development in low- and middle-income countries. This paper investigates the causal impacts of the long-term internal conflict on child health in Colombia, following an identification strategy based on the temporal and geographic variation of conflict intensity. We estimate the effect of different levels of conflict intensity on height-for-age (HAZ), weight-for-age (WAZ), and weight-for-height z-scores among children under five years old, and explore the underlying potential mechanisms, through maternal health behavior and health care utilization. We find a harmful effect of exposure to conflict violence in utero and in early childhood for HAZ and WAZ, in the full sample and even more strongly in the rural sample, yet these estimates are smaller than those found for shorter term conflicts. The underlying pathways appear to operate around the time of the pregnancy and birth (in the form of maternal alcohol use, use of antenatal care and skilled birth attendance), rather than during the post-birth period (via breastfeeding or vaccination), and the impacts accumulate over the childhood. The most adverse impacts of conflict violence on child health and utilization of maternal healthcare were observed in municipalities which suffered from intermittent presence of armed groups.

Keywords: Conflict, Violence, Child health

## Highlights

- We study the impact of internal armed conflict on child anthropometric measures
- We use individual and municipality-level data from Colombia for the period 2000-2010
- Exposure to high conflict intensity in utero reduced height-for-age Z-score
- Conflict reduced antenatal care use and increased alcohol intake during pregnancy
- Harmful health effects were stronger in areas with intermittent armed group presence

# 1. Introduction

Since 1958 an estimated 220,000 people have died in Colombia and more than 6 million (13% of the population) have been displaced due to one of the longest civil conflicts in the world (ICRC 2016, World Bank 2016). Guerrilla groups, mainly the Revolutionary Armed Forces of Colombia (FARC) active in the majority of the country's regions and, to a lesser extent, the National Liberation Army (ELN), active mainly in rural areas of the north, have been involved in the conflict, alongside paramilitary groups, narco-traffickers and the Colombian military. Civilians have been victims of cross-fire, kidnappings, massacres, torture, extortion and other forms of violence and human rights violations, with an estimated 7 million direct victims of conflict violence between 1985 and 2015 (OECD, 2016). Following an extensive negotiation process, a peace accord between the FARC and the Colombian government was agreed in December 2016. However, violence associated with armed groups increased again in 2018, and civilians have suffered serious abuses at the hands of the ELN guerrillas, FARC dissidents, and paramilitary successor groups, leading to more than 48,000 individuals displaced between January and September 2018 (Human Rights Watch 2019). In light of these developments, it is imperative to understand the consequences of long-term conflict violence on health to inform health policy design in Colombia and in other similar settings, where harmful adverse consequences of protracted violence emerge. In this paper we assess the consequences of the long-term internal conflict on key indicators of child health in Colombia.

The primary aims of our paper are: (1) to estimate the causal effect of conflict intensity in the municipality where a child was born, during the periods just before and after the birth, on child health, and (2) to investigate the channels whereby exposure to conflict violence influences child health, through an examination of intermediate outcomes, including maternal health behavior and health system aspects. We use the Colombian Demographic and Health survey dataset that is nationally representative and spans a time period of 2000-2010, facilitating a "pseudo-panel" econometric strategy that allows controlling for common trends as well as unobservable characteristics through the use of area fixed effects, among other potential confounders. Following previous studies, we investigate impacts of conflict in utero, as well as conflict in the first five years of a child's life. We consider commonly used child anthropometric measures, and potential mechanisms related to the mother's health behavior and health service use.

The existing literature on the relationship between conflict and health has generally found exposure to conflict violence to entail severe, long lasting consequences on child and adult health (See Appendix Table 1 for a structured summary). Studies focusing on the effects of conflict on child health have regarded the latter either as an outcome in its own right or as an input into human capital formation. Two widely used indicators of child health in the empirical literature are stunting (proxied by low height-for-age) and wasting (low weight-for-height). Although they co-occur in many low- and middle-income countries (LMICs), and interrelationships between them do exist (Angood et al 2016), they have traditionally been considered as capturing distinct health problems. Stunting is the consequence of inadequate intake of food over a prolonged period of time and/or chronic or frequent illness, with adverse long-term developmental consequences. Wasting tends to reflect a short-term, imminent health issue resulting from poor current nutrient intake and/or severe disease e.g. diarrhea (UNICEF et al 2019). In this paper, in addition to using stunting and wasting as two main outcome variables, we also assess the consequences of conflict violence in terms of underweight (proxied by low weight-for-age), which can be interpreted as a composite indicator of malnutrition, since low weight-for-age could reflect wasting (indicating acute weight loss), stunting, or both (WHO 2010).

The underlying indicator used to assess stunting, i.e. the height-for-age Z score (HAZ), is a standard measure recorded in household surveys such as the Demographic and Health Surveys (DHS) (WHO 2006). The frequent use of HAZ in the conflict literature has been motivated by the fact that a low HAZ captures several aspects of conflict-related harm to mothers and children, including nutritional deprivation in utero (Stein and Lumey 2000) as well as the effect of the level and diversity of nutrition throughout childhood (Arimond and Ruel 2004). HAZ has also been shown to be a strong predictor of future health (Black et al. 2008), education and labor market outcomes (Case and Paxson 2008).

Most of the existing evidence on the effect of conflict on child health – and on health outcomes more generally – comes from relatively short, acute conflict periods, particularly in Sub-Saharan Africa. This includes work by Alderman et al. (2006) in Zimbabwe, Minoiu et al. (2014) in Côte d'Ivoire, Akresh et al. (2011,2012a, 2012b) in Nigeria, Rwanda and Eritrea-Ethiopia, as well as Bundervoet et al. (2009) in Burundi. These studies find that children exposed to conflict have lower HAZ compared to children unexposed to war, by between 0.2–0.5 standard deviation (SD).

However, this body of work provides limited insights for designing post-conflict health policies following long-term and intense internal violence. Protracted conflicts – i.e. long-running chronic confrontations, often defined as those lasting for eight or more consecutive years (ICRC 2016) – have become the norm in recent decades, affecting several LMICs (e.g. Colombia, DR Congo, Iraq, Lebanon, Libya, Myanmar, Syria, Yemen).

In contrast to the impacts from short-term or acute conflicts, the health consequences of exposure to protracted violence may differ in ways that require a differential response to the crisis. It is conceivable that upon exposure to protracted conflict, there is a double impact on population health: first, from the immediate direct effect of being harmed by attacks, kidnappings, displacement etc.; and second, more indirectly via the cumulative deterioration of the health system, other basic infrastructures and livelihoods (ICRC 2016). Such cumulative effects may materialize, for instance, in the form of a long-term experience of food insecurity, which in turn may increase the likelihood of chronic, more severe health consequences (e.g. stunting), rather than more transient ones (e.g. wasting). The longer the conflict endures, the more likely may conflict also risk overwhelming the capacity of public administration and services (FAO 2010). Countering such systemic failure and erosion will arguably require a particularly comprehensive and sustained policy response.

Moreover, because most of those directly exposed to protracted violence tend to be the poor living in rural areas (Bornemisza et al. 2010), such conflicts will likely aggravate poverty and further widen the poor vs. rich health gap. In Colombia, for example, poor and rural localities faced larger welfare losses from conflict-related violence and often showed worse access to health and other public services, despite the presence of a national health system (Ibáñez 2008). And at least in principle, the effects of longer-term conflict do not unambiguously have to be harmful for health. In some, perhaps exceptional cases, a persistent shortfall of government-provided health care services may stimulate an informal, grassroots mobilization of human, physical and financial resources that could compensate for part or all of the previously provided support (Uribe 2017; Stewart 2018). As a result, some degree of adaptation to life under protracted violence may emerge, to produce at least less pronounced conflict-induced damage to health outcomes than would be the case in contexts of acute, shorter-term conflict.

Only a minority of studies have investigated the impacts of protracted conflicts on child health. Leon (2012), using data for the long-term civil conflict in Peru, finds indicative, though not statistically significant, conflict-attributable declines in child HAZ. Kim (2019) examines the 2011 round of the Ugandan DHS, finding that each conflict event in a village during the long-run insurgency of the Lord's Resistance Army (1986-2006) is linked to 0.003 and 0.007 SD lower weight-for-age and weight-for-height z-scores (respectively) for children born after the conflict, with no impact found on HAZ. In Colombia, Camacho (2008) looks at the effect of stress caused by violence on low birth weight (LBW) using a dataset of 4 million births from 1998-2003. Camacho finds that a landmine explosion in the municipality of birth reduces birthweight by 8.7 grams. The study focuses on the effect of in utero stress on birth weight and does not examine growth and nutrition outcomes later in childhood. Duque (2017) uses a different Colombian dataset of 21,000 births between 1999 and 2007 to look at the impact of conflict violence (defined as number of massacres) on HAZ and other health outcomes. The author finds that a one SD increase in violence decreases HAZ by 0.1 SD, which is noted to be lower than estimates found in other settings.

We contribute to the literature in three ways. First, we add to the growing evidence base on the links between armed conflict and child health, yet focusing on the under-researched case of protracted conflict violence. Second, in order to go beyond the average effects of protracted violence in Colombia, where some regions endured more sustained violence than others, we explore the heterogeneity of conflict intensity impacts between rural and urban municipalities, and across municipalities with different persistence of the conflict over the study period. We define the extent of conflict persistence according to three categories capturing the degree of presence of armed groups, i.e. either permanent, intermittent or lack of their presence, thus allowing for an explicit exploration of the relationships between the protracted nature of the Colombian conflict violence and child health. Third, our research adds to the existing literature by examining the mechanisms through which conflict-related violence influences health outcomes. Understanding the pathways by which conflict-related violence influences child health in the general population, and in key population sub-groups, is crucial for the development of effective policy responses to address these health consequences, both during and in the aftermath of the conflict. However, evidence about these pathways is notably absent in most of the existing literature (Kadir et al. 2019), not least with regard to health system channels such as maternal and child healthcare provision. We explore these pathways by first, considering the dynamics of conflict exposure, by estimating the impact of not only in utero exposure, but also conflict exposure in the first five life years of a child. Second, we consider a range of potentially relevant intermediate outcomes, related to the mother's health behavior and health service during and after pregnancy.

We find a harmful effect of exposure to conflict violence in utero and in early childhood (the second year of the child's life), for HAZ and WAZ, both in the full sample and even more strongly in the rural sample. We find evidence of cumulative impacts of early life exposure to conflict violence, as the impacts are strongest among children who were older than two years of age the time of the survey. As for the potential mechanisms, we find an effect on pathways that refer to the time of the pregnancy and birth (maternal alcohol use, use of antenatal care and skilled birth attendance), rather than on the pathways referring to the time period after birth (breastfeeding, vaccination). We find that the detrimental impacts of higher conflict intensity on alcohol use and skilled birth attendance are particularly relevant for urban populations, while negative impacts on antenatal care play a greater role among rural populations. The biggest adverse impacts of conflict violence on child health and utilization of maternal healthcare were observed in municipalities which suffered from intermittent presence of armed groups (or even absence of stationed armed groups during most of the study period), while no such impacts were found for municipalities in which armed groups were permanently stationed between 2000-2010.

# 2. Data

### 2.1 The Colombian Demographic and Health Survey (DHS)

The Colombian Demographic and Health Survey (DHS) is representative of urban and rural populations. Samples are obtained through a stratified, multistage and cluster sampling design. The DHS surveys provide information for women aged 13-49, living in each of the 32 Colombian *departamentos* (provinces, formed by a grouping of municipalities) and the

Capital District (containing the country's capital, Bogota). The DHS data in Colombia is collected in the households of the respondents[2]. Five separate DHS rounds would be available in principle for our analysis (1986, 1995, 2000, 2005 and 2010), as they contain the required child anthropometric measures. However, due to data limitations, we constrain our analysis to the 2005 and 2010 waves only. Specifically, discrepancies in the coding of municipality IDs between those used by DHS and the official coding used by the National Statistical Office (DANE) limit the ability to use data from the DHS prior to 2005. The key outcome measures were not collected in the 2015 DHS.

The DHS records births as retrospective birth cohorts, i.e. all births taking place in the five years before a survey wave. To ensure that the conflict intensity measured at the municipality of current residence reflects conflict intensity at the time of the child's birth, we constrain the sample to births from mothers who lived in the same municipality at least from the year before the child's birth. This results in a sample size of approximately 23,000 children, from 340 municipalities, born between 2000 and 2010. The DHS also includes socioeconomic information for the women and the households they live in.

We use data from the Centro Nacional de Memoria Historica (CNMH) (Centro de Memoria Historica 2018) which is a leading data source for research about the Colombian conflict due to its reliability and completeness, to construct our conflict intensity variable. The CNMH contains comprehensive, publicly available information on all conflict-related violence events by location (terrorist attacks, war actions, attacks on populations, selective murders, kidnappings, child recruitment, massacres, enforced disappearance, damage to property, sexual violence and landmines). Each event has an associated calendar date and reported number of victims. The data is based on police reports from 592 different sources (e.g. military forces, national police, governmental sources, non-governmental organizations, newspaper information, victim declarations), with a timeframe going back to 1958, and with the municipality as the lowest geographic unit.

In addition to conflict intensity, we explore the protracted nature of conflict violence in Colombia by measuring the persistence of conflict. For this, we use a dataset constructed by

[2] The team of interviewers receive extensive training on the survey instruments, administration of the modules and safety procedures. The ethical guidelines of the survey focus on disclosure of family violence, crisis situations and how to emotionally prepare and respond in the fieldwork. To ensure guideline implementation, there is a team leader that verifies quality procedures with respect to informed consent, privacy while the family violence module is conducted, and referrals for women who report exposure to violence.

the “Centro de Recursos para el Analisis de Conflictos”(CERAC 2020) that defines 4 categories of municipalities according to the presence of armed groups between 2002 and 2010: “Presence of armed groups during the entire period”, “There are years with no armed group presence during the period”, “There is no evidence of armed group presence in the last 8 years of the relevant period” and “There is no evidence of armed group presence during the entire period”. We merge the latter two categories into a single one (“No armed group presence”), resulting in three subgroups (categories) of municipalities[3]. We construct two panel datasets with this information, one with data aggregated by month and another with data aggregated by calendar year, from 2000 to 2010. These datasets include all Colombian municipalities, containing the number of conflict events in each municipality in a given month and year.

**2.2 Measures of conflict intensity**

We examine two alternative measures of conflict intensity, both derived from the number of conflict events recorded at the municipality-month level. In order to account for the size of the municipality, we standardize these measures by municipality population in a given year. Hence, as in other studies (e.g. Urdinola 2004), all of our conflict intensity measures are expressed as the number of conflict victims per 1000 population. Our main measure of conflict intensity takes the monthly panel dataset and, for each birth, aggregates the monthly conflict events for the approximate duration of the pregnancy (calculated as the 9 calendar months before the month of birth), and for the child’s first year (12 months from birth), and for each subsequent year, up to age 5, with the variable set to zero for those years which the child has not yet reached. Due to the highly skewed distribution of the conflict intensity variable (see Figure 1), as an alternative we construct a second conflict measure that aims to capture the presence of high conflict in a municipality (see Robustness Checks).

[3] We must note that our persistence variable attempts to proxy the protractedness of violence in a given municipality exclusively through reports about armed groups who were stationed at that municipality (at some point or for the entirety of the relevant period). It is possible for a municipality to be categorised as having “no armed group presence” while at the same time still experiencing acute periods of conflict intensity, through violent incursions of one or more armed groups in the area e.g. for raids, selective murders or child recruitment.

## 3. Empirical strategy

To estimate the effects of exposure to conflict violence on child health, we use the repeated cross-sections data to create pseudo-panels of birth cohorts. This allows us to exploit regional and temporal variation of conflict intensity affecting cohorts of children. For the evaluation of the effects of relatively shorter conflicts, such framework can lead to traditional difference-in-differences specifications, through an assessment of how child health evolved in regions with conflict, in birth cohorts that were born before, during and after the conflict, and comparing these changes to the corresponding ones in regions that experienced no conflict (e.g. Akresh et al., 2011; Akresh et al. 2012). In the Colombian setting, however, most regions have been affected by the long-term conflict to some extent; therefore, the variation we take advantage of for the estimations comes from differences between low and high conflict intensity areas. The main econometric challenge to address is that armed groups often do not select areas at random: for example, they may prefer to be particularly active in more rural (densely forested) areas or in areas that are rich in resources that can be extracted (Torres and Urdinola 2018).

Our basic econometric specification is the following:

$$Y_{ijt} = \alpha_j + \delta_t + \tau * conf\_utero_{ijt} + \beta * \boldsymbol{X}_{ijt} + \varepsilon_{ijt}\ , [1]$$

where $i$ indexes the child, *j* is the municipality where the child was born and *t* is the birth cohort of the child. $Y_{ijt}$ is the outcome of interest: child health outcomes measured in the survey year (0 to 5 calendar years after *t*) as well as intermediate outcomes (retrospective information that relates to the period of the pregnancy and birth; see details in the following section). The variable $conf\_utero_{ijt}$ is the intensity of conflict to which the mother was exposed during the 9 months before child birth, and τ is the effect of such conflict intensity. $\boldsymbol{X}_{ijt}$ represents a vector of child, mother and household covariates. Finally, $\alpha_j$ is the municipality fixed effect and $\delta_t$ the cohort fixed effect.

To account for omitted variable bias due to possible serial correlation between conflict in utero and conflict in subsequent years of the child, and to directly explore the impact of conflict during the child's life, in Equation [2] we expand the previous specification by adding conflict exposure in the $k$th year of the child, life $conf\_chy_{ijtk}$, with a year-specific effect $\gamma_k$.

$$Y_{ijt} = \alpha_j + \delta_t + \tau * conf\_utero_{ijt} + \sum_{k=1}^{5} \gamma_k * conf\_chy_{ijtk} + \beta * \boldsymbol{X}_{ijt} + \varepsilon_{ijt} . \qquad [2]$$

For each outcome, we conduct ordinary least squares and fixed effect estimations, the latter using the -areg- Stata command to allow for a large number of municipality dummy variables (340), with standard errors clustered by municipality. We conduct the analyses for the estimation sample of children born between 2000 and 2010, restricting the sample to mothers who report to have lived in their current place of residence since at least one year before the year of their child's birth. This ensures that the conflict intensity measured in a given year, in the municipality of residence as recorded in the survey accurately captures the intensity of conflict violence to which the mother was exposed around the time of childbirth.[4]

To allow for the fact that conflict exposure in utero and in subsequent years can have a cumulative impact on child health, we also conduct an analysis which allows the effects of conflict during pregnancy and in the first five years of the child to vary by the child's age at the time of the survey, by interacting the conflict variables with child age.

We use the basic empirical specification (Eq1) for intermediate outcomes to explore mechanisms behind impacts on child health. We conduct subgroup analyses to explore heterogeneity in the impacts both for health outcomes and intermediate outcomes (See 3.2). In the robustness checks (3.3), we include an alternative definition of conflict intensity, to explore the potential impacts of sample selection, and seek to account for the potential remaining endogeneity of conflict exposure by using mother fixed effects.

[4] This strategy also alleviates the concern that our results could be driven by conflict-induced selective migration. For instance, if less healthy mothers and children who anticipated to suffer more due to the conflict were also more likely to migrate to low-intensity conflict municipalities, then our estimate of average conflict effect on child health using the full sample could be biased towards zero. On the other hand, our full-sample conflict effect estimate could be overestimated if healthier mothers and children were more likely to migrate to low-intensity conflict municipalities. Thus, the direction of the possible bias in the presence of selective migration is unclear a priori.

### 3.1 Health outcomes and control variables

We examine three main child health outcomes: HAZ, WAZ and WHZ. As secondary outcomes, we examine binary variables constructed based on the main measures, that indicate stunting, underweight and wasting, respectively, using the recommended cut-off z-score value of -2 (WHO 2019).

In our basic econometric model, we only include variables that are either known to be important predictors of our anthropometric measures (e.g. child age) or potentially influence both child anthropometric indicators and conflict intensity (e.g. rural/urban residence). We avoid including variables that are likely on the causal pathway from conflict to outcomes (potential mediators). Because child growth is a cumulative process, it is crucial to accurately control for the child's age, and we do so by using age expressed in months, and the square of age, allowing for a non-linear relationship[5]. We also control for the child's sex, the mother's education (in years of schooling) and whether the household is in a rural or urban area. To capture socioeconomic status, we compute an asset index following O'Donnell et al. (2007), based on whether the household has: electricity; refrigerator; television or radio; access to piped water; a toilet connected to sewer; and concrete flooring. Using these variables, we conducted a principal component analysis to classify households into wealth quintiles based on asset ownership and household characteristics, separately for urban and rural households, and by survey year. Due to the above-mentioned concerns around potential mediating effects, we do not include in the main analyses household size, the number of children in the household, or the sex of the household head.

### 3.2 Mechanisms and heterogeneity analysis

A systematic review by Kadir et al. (2019) highlighted that the effect of conflict on child health and development can run through various channels, including direct health effects (e.g. injury, illness and death), and indirect health effects arising from deficient access to basic services (e.g. via destruction of health and sanitation infrastructure) that put children

[5] We control for child age as a continuous measure, and consider children aged above and below 24 months. We have tested the sensitivity of the results to include child age as a categorical variable (<6m, 6-12 m, 12-18m, 18-24m, >24m), and found that this alternative specification did not change any of our conclusions.

and expectant mothers at risk of preventable diseases. We investigate differences in the provision of maternal (antenatal care use and skilled birth attendance) and child healthcare (vaccination) between areas with more and less intense conflict violence over time, as possible mediators of conflict effects on child health. We also examine the influence of behavioral aspects that may impact child health, in particular harmful maternal alcohol consumption, since alcohol abuse is a common coping mechanism among populations exposed to severe violence (Do and Iyer 2012). Furthermore, we look at the impact of violence on the duration of breastfeeding, a crucial factor in early life nutrition, which is related to health outcomes later in life (Horta et al. 2013). Finally, we investigate birth weight, which can be affected by maternal stress (Torche 2011) potentially induced by conflict through maternal health (care) and behavioral factors mentioned above, ultimately influencing child anthropometric indicators (Hack et al. 2003).

Hence, as intermediate outcomes (i.e. potential mechanisms for the main health effects), we consider the following: a binary variable indicating whether the mother consumed alcohol during pregnancy, a binary variable indicating whether the mother used antenatal care at least once during pregnancy[6], a binary variable indicating whether a skilled attendant (doctor or midwife) was present at birth, a binary variable indicating whether the child was underweight (<2500 grs) at birth, and the duration of breast feeding (in months), and a binary variable indicating whether the child received the full childhood vaccination schedule.

We split the full sample by rural and urban subgroups, expecting potentially heterogeneous conflict effects on health due to, for example, differences in health infrastructure availability, since health infrastructure in rural areas is likely to have suffered relatively more due to conflict violence than in urban areas. We then examine conflict intensity impacts according to the level of persistence of violence over time to which mothers and children were exposed in each municipality, by conducting analyses for subgroups of municipalities according to a categorisation of conflict persistence, based on the presence of armed groups between 2000 and 2012 (no presence, intermittent presence or permanent presence).

[6] The information on antenatal care use and alcohol use during pregnancy is only available for the most recent birth reported.

### 3.3 Robustness checks

We conduct four sets of robustness and specification checks. First, we explore an alternative definition of the conflict intensity variable, and use a binary variable for high conflict intensity in the year of the child's birth. We aggregate conflict events to the municipality-year level, and split the municipality-level yearly standardized conflict intensity measure into quintiles of the distribution over the entire study period, creating an indicator variable that takes the value of one if a given municipality in a given year belonged to the top quintile of the distribution (i.e. highest level of conflict intensity), and zero otherwise. We then assign to each child a binary variable which takes a value of one if there was high conflict intensity in the calendar year of birth, and zero otherwise.

While the municipality fixed effects control for unobserved factors that differ between municipalities and do not change over time, there may be unobserved differences between mothers across municipalities with high and low conflict intensity. To address this, as a second robustness check, we follow Camacho (2008) and Duque (2017) by using mother fixed effects to control for unobserved heterogeneity of mothers (e.g. deprivation not captured by the asset index, preference for breastfeeding) that in principle does not change between child births. This estimation identifies the effect of conflict intensity from between-mother variation in conflict exposure, due to changing conflict intensity over time within the municipality where a mother lives. Yet this analysis requires restricting the sample to those children who have at least one sibling in the dataset, which results in a substantial reduction in effective sample size (to 7666 children, from 4060 mothers), and we cannot conduct this sensitivity test for the intermediate outcomes (alcohol use, antenatal care use) where only one observation per mother is recorded. We note a further limitation of this analysis up front: parents may compensate for shocks to one child's growth, either by steering extra food to them, or by investing more in their siblings, hence there main still remain time-varying unmeasured confounding that is not taken care of with the mother fixed effects.

Third, we assess the potential implication of selecting our estimation sample. In brief, we have dropped those child-level observations where the mother has moved to the municipality later than the year prior the given child's birth, to make sure that our main explanatory variable – conflict intensity during pregnancy – is accurately measured. This reduces the sample from 23,480 mothers to 19,076, and the number of child level observations from 28,290 to 22,889. We introduced this sample selection to avoid measurement error in conflict intensity, but this procedure may introduce sample selectivity.  A potential concern is that mother who have moved to a municipality recently may differ from mothers who have lived there for long, and were potentially affected by different conflict intensity. Because we do not know the true observed conflict intensity for the movers, we cannot directly assess the implication of this selectivity issue, however we perform several indirect tests. First, we implement a regression analysis using the sample selectivity indicator (specifically, whether an observation has been excluded from the sample, due to conflict intensity during pregnancy being potentially measured with error) on the left-hand side, and the conflict intensity indicators, socioeconomic variables and fixed effects on the right hand side. To further explore this point, we also reanalyse the main regression specification for HAZ, the following ways: (1) we include all observations (not selecting the sample), and run the fixed effects regressions on the full sample, and compare the results of this analysis with the main analysis using the selected sample, and (2), we extend the analysis described above, but include indicator variables that capture whether conflict intensity in a given period is measured with error.

Fourth, for the main analyses, we report p-values corrected for multiple hypothesis testing, for the main analyses including the child health outcomes and mechanisms, and for the subgroup analyses, using the free step-down resampling algorithm by Westfall and Young, as well as p-values following Bonferroni correction, implemented in the pwyoung STATA package (Jones et al. 2019) (Results are presented in the Appendix Tables 2 and 3)

## 4. Results

Figure 2 shows how conflict intensity, measured both as continuous and binary variables, evolved over the time period examined (between 2000 and 2010). The number of victims per 1000 population grew sharply after 2000, peaked at 2002, decreased to lower levels by 2008 and remained relatively low thereafter. Table 1 describes the conflict variables, main and intermediate outcomes and covariates in the estimation sample. The main conflict intensity variable, conflict events during pregnancy, has a mean value of 0.38 per 1000 population over the study period, indicating that women giving birth in an average sized municipality of 63,000 people would have been exposed to approximately 24 conflict events on average during pregnancy. Figure 1 (left panel) plots the distribution of this variable. It is apparent that the variable has a highly skewed distribution, with a few observations coming from (typically small) municipalities that experienced extreme levels of conflict intensity. Figure 1 suggests a small negative correlation between z-scores and conflict intensity in utero; however, this correlation is not adjusted for cohort effects, municipality effects or observed covariates such as child age. Figure 3 illustrates the temporal and spatial variation in conflict intensity in utero, aggregating conflict intensity by departamentos and years. It is apparent that conflict intensity did not evolve in a parallel fashion across the various departamentos, and that enough variation in conflict intensity exists even after controlling for time and municipality fixed effects, thus permitting identification of conflict effects in the sample via our econometric specification.

**4.1 Results for child health outcomes**

Table 2 presents the regression results for HAZ, WAZ and WHZ outcomes, first using conflict events in utero as the explanatory variable, then adding the variables describing conflict intensity in the $k$th year of the child's life. The OLS analysis reports a seemingly beneficial effect of conflict exposure in utero on HAZ and a statistically significant reduction in stunting prevalence. These beneficial effects disappear after adjustment for municipality fixed effects, and a harmful effect of exposure to conflict violence in utero is uncovered for HAZ and WAZ. For HAZ, the estimated effect of an increase in conflict exposure by one event per 1000 during pregnancy is a reduction of HAZ of 0.06. These effects remain after we also include conflict exposure over the child's life, and we estimate significant harmful effects of conflict exposure in the second year of the child's life (0.07 and 0.04 for HAZ and WAZ, respectively). For WHZ, we find no effect of conflict exposure in utero, and the estimated significant impacts of conflict in the 3$^{rd}$ and 5$^{th}$ year of a child's life are opposite sign, cancelling each other out. For the binary outcome measures of stunting, wasting and underweight, we do not observe any effects of in utero exposure that are statistically significant at conventional levels.

Table 3 presents the results for the z-score outcomes, that allow the effect of conflict intensity to vary according to the child's age at the survey year. We find that for HAZ, and WAZ the negative impact of conflict in utero is dominated by the older children (2 to 3 and 4 to 5 years old), indicating a delayed impact of in utero exposure. In contrast, for HAZ, we also find an immediate effect of conflict exposure in the second year of the child's life, in the 1-2 age group. For WHZ, we find opposite effects of conflict exposure in utero and in the first 2 years of life, among the 2-3 year old children, and these effects cancel each other out.

#### 4.1 Results for mechanisms and heterogeneity analysis

Turning to an inspection of the potential mechanisms through which conflict violence may affect child health (Table 4), we find statistically significant impacts on the intermediate outcomes that are related to the duration of the pregnancy, but not for the intermediate outcomes that are measured after birth. We find statistically significant impacts for maternal health services utilized before or at birth: conflict during pregnancy decreased the probability of antenatal care use by the mother and also reduced the probability of a skilled health professional being present at birth, both by around two percentage points. We find some indicative evidence that exposure to one additional conflict event per 1000 population during pregnancy increased the risk of the mother consuming alcohol during pregnancy by one percentage point ($p<0.1$), but we do not find evidence of a mechanism through birth weight, duration of breastfeeding or vaccination coverage.

We conduct our subgroup analyses for the main health child health outcomes (Table 5) and for the intermediate outcomes (Table 6) for which we found a statistically significant effect in the main analysis. We find that for HAZ and WAZ, the adverse impacts of conflict exposure in utero were stronger among children born in rural communities, while the impacts of conflict exposure in the second year of a child's life were of similar magnitude across urban and rural communities. As for the mechanisms, the detrimental conflict effects on alcohol use and skilled birth attendance are particularly important for urban populations, while the negative impact on antenatal care use is stronger among rural populations. Disaggregating the effects of conflict intensity by groups of municipalities with different levels of persistence of conflict, we tend to find the strongest negative health effects (in terms of HAZ and WAZ, and for both in utero conflict and conflict in the second year of a child's life) – and negative effects on antenatal care use and skilled birth attendance – in municipalities that suffered from intermittent armed group presence. For HAZ, we also find a very strong negative effect of conflict intensity in utero in municipalities with no armed group presence, but this effect is cancelled out by an effect of opposite sign and similar magnitude in the first year of the child's life. By contrast, for alcohol consumption, we find the highest impact of conflict intensity in municipalities with permanent presence of armed groups.

**4.1 Results for robustness checks**

We present three sets of robustness checks here. In the first set (Table 7), we use a binary conflict intensity measure, and consider conflict intensity in the calendar year of the child's birth. The results are consistent with the main analyses: the fixed effects estimates show a significant reduction of the z-scores ($p<0.05$ for HAZ and $p<0.1$ for WAZ), if a mother gave birth in a municipality that belonged to the top quintile of conflict intensity in the year of birth, corresponding to a decrease of around 0.06 in the respective z-scores. For intermediate outcomes, similarly to the main analysis, we find a statistically significant negative impact on skilled birth attendance, and no significant impacts on antenatal care use and alcohol consumption (although the signs and magnitudes are similar to those from the main analysis).

In the second set of robustness checks (Table 8), we apply fixed effects regressions using mother fixed effects, and contrast them to the original analysis that used municipality fixed effects, as well as to a municipality fixed effect analysis where we restrict the sample size to those mothers with at last two children in the sample. Mother fixed effects allow to control for time-invariant household characteristics that are common to all children in the household (e.g. parental preferences).[7] Due to the substantially reduced sample size resulting from using information only for children with siblings, and the corresponding reduced variability in conflict intensity, in this specification the standard errors of conflict intensity in utero approximately double in magnitude compared to the main analyses. As a result, the effect of conflict exposure in utero is not statistically significant at the 5% level; however, it is still significant at the 10% level and is of similar magnitude to what was found

[7] It is important to acknowledge that while controlling for mother fixed effects is a common robustness check in the related literature (see e.g. Akresh, Lucchetti & Thirumurthy 2012; Bundervoet, Verwimp & Akresh 2009; Duque 2017), the underlying assumption of conflict exposure of a sibling not having consequences for parental investments in their sibling may in some cases be violated. To limit such violation, we have allowed for conflict exposures in different periods (in utero and at different years of life).

in the main analysis. For WAZ, the effects found for in utero exposure and exposure in the second year remain statistically significant and somewhat larger than in the main analyses.

In the third set of robustness checks (Table 9), we investigate the potential consequences of selecting the analytical sample to exclude children born before the mother moved to the municipality where they lived at the time of the survey. Overall, there is only weak evidence that sample selectivity might be systematically associated with conflict intensity: for all but one of the conflict intensity indicators, we find coefficients that are nowhere near statistical significance. Only for conflict intensity in the second year of the child is there a statistically significant association, for which we find no straightforward interpretation. In any case, when we re-estimate the main specifications for child health outcomes using the full (not selected) sample, with or without the "missingness" indicators, we find that our conclusions about the impact of conflict during pregnancy remain, despite somewhat smaller magnitudes of conflict effects (a HAZ reduction of 0.042 versus 0.055). We conclude that while sample selectivity may have an impact on the size of our estimates, it is unlikely to invalidate our conclusions.

# 4 Discussion

This paper aimed to estimate the causal effect of conflict intensity on child health in Colombia, for children born in the 2000-2010 period, and to investigate potential channels whereby exposure to conflict violence may have affected child health. Overall, we consistently found a negative effect of conflict on child health outcomes. Most notably, for HAZ, an increase in conflict exposure in utero by one event per 1000 during pregnancy entailed a reduction in HAZ of 0.06 (0.054 SD). This effect is somewhat smaller than the estimated magnitude of the effect on HAZ (0.09 SD) of a mother experiencing a massacre during pregnancy, as found by Duque (2017). The magnitude of our HAZ estimate is also smaller when compared to the range of effects found in studies that looked at shorter term conflicts: children exposed to war have been found to have between a 0.2–0.5 SD lower HAZ, compared to children unexposed (see Introduction). We found, nonetheless, that the negative effects of the Colombian conflict on WAZ were in line with those measured in

Uganda by Kim (2019), although it is difficult to compare both sets of estimates, since we look at a child's exposure to the ongoing conflict during pregnancy and over the first year of life, while Kim (2019) focuses only on children born after the conflict. Nevertheless, our estimates for a rural municipality (median population of 28,000) imply that one additional conflict event in the municipality would decrease WAZ by 0.093/28=0.003 SDs, which coincides with the lower bound of the estimate reported by Kim (2019).

One potential explanation for the smaller conflict effects on anthropometric measures that we find, compared to previous studies, is our different measure of conflict intensity. While other studies evaluating the health impact of conflict violence in Colombia (e.g. Urdinola 2004, Camacho 2008, Duque 2017[8]) used specific, narrowly defined set of conflict events to define conflict intensity (e.g. only homicides, landmines or massacres), we have constructed a conflict intensity measure that encompasses all types of conflict events listed in the CNMH database. This responds primarily to our intention of uncovering child health impacts that, in principle, may be caused by protracted conflict under all its main dimensions of violence, and we also add to the existing international evidence base by providing an extensive investigation of mechanisms through which conflict may plausibly impact child health, including health system-related channels. By contrast, we do not have clear priors from available theoretical or empirical literature about how specific conflict event types could influence the health and intermediate outcomes that we consider in this paper. By examining a broader conflict measure than other studies in Colombia, we avoid the cherry-picking of results and conclusions from our analyses based on any statistically significant, yet hard to interpret, estimated effects for particular conflict event types on a given outcome. Using this more general conflict measure may help explain the smaller effects we find, compared to those reported by Duque 2017, who focused massacres, one of the most extreme types of conflict event.

The second potential explanation for the smaller effects is the presence of partially compensating coping mechanisms – from the households and communities – or alternative routes of health care service provision that may enter into force during periods of protracted conflict. This possibility is supported by the findings of our heterogeneity

[8] Apart from Duque (2017), none of these studies looks at measures of child malnutrition, hence it is not possible to compare effect sizes in these cases.

analyses: these indicated that the strongest negative impacts of conflict violence on child health and utilization of maternal healthcare were observed in municipalities, which suffered from intermittent presence of armed groups (or even absence of stationed armed groups during most of the study period), while no such impacts were found for municipalities in which armed groups were permanently stationed between 2000-2010. It suggests that acute bouts of violence in a given locality – e.g. armed group incursions into contested areas for kidnappings or other violent acts – are a more important driver of poor child health than the chronic presence of armed groups in an area.

Adaptation to life under a known, chronic threat of violence may thus entail the establishment of longer-term coping mechanisms that are able to offer more protection to child health than it is the case in situations of acute conflict, where violent episodes are often characterized by high intensity and unpredictability. This is also in line with the findings from ongoing qualitative research which has found that, in order to compensate for difficulties in access to healthcare arising from the high turnover of medical staff and low availability of medicines, several articulation strategies emerged in Colombian areas affected by long-term conflict violence (Gonzalez-Uribe et. al 2020).· These strategies include cases of public institutions providing services alongside community-based organizations, as well as healthcare provision sponsored by the FARC armed group to the communities where they exerted territorial control. Previous evidence for Colombia points in the same direction (Leech 2011; Uribe 2017). The possible emergence of such coping strategies in contexts of chronic conflict constitutes, therefore, an additional element that policymakers need to consider when devising initiatives aimed at mitigating or preventing the adverse health effects of long-term violence. In the particular case of Colombia, where the 2016 peace accord brought about the end of FARC as an armed actor and its control over large sections of the country's territory, the above highlights the need for the Colombian state to step in rapidly into such localities to re-establish adequate levels of public healthcare provision, as the coping strategies established in conflict times start to lose relevance in many localities in the post-accord context. Doing so could help protect (child) health and – as a by-product – could promote public support to the peace accord in such areas (cf. e.g. Justino and Stojetz 2018).

When exploring the dynamics of the effect of conflict on child health, we found that the impacts of in utero exposure remained unchanged even after adding conflict exposure in later years. Beyond in utero exposure, we found significant conflict impacts of exposure in the second year of the child's life, potentially pointing towards a pathway running through nutrition in early life. This is in line with the finding by Duque (2017) of conflict violence in the first year of child being harmful for Z-scores. When exploring how the impacts differ by child age at the time of the survey, we found that average impacts were dominated by child age groups 2-3 and 4-5, suggesting a cumulative impact of early life conflict exposure on child anthropometric measures. This finding adds to the existing knowledge base on conflict and child health.

Our results for intermediate outcomes offer further insights about potential pathways for the effects of conflict on child health. The key findings are that conflict impacts are strongest for mechanisms that take place during pregnancy and around child birth, and that there is further heterogeneity between urban and rural populations in the relevance of pathways for effects. While impacts of increased maternal alcohol use during pregnancy and decrease in skilled birth attendance were stronger in the urban subgroup, the decrease in the use of antenatal care was found to be the stronger in the rural subgroup. Our finding regarding the deleterious effects of conflict exposure on alcohol use (and thereby child health) is supported by evidence elsewhere showing links between exposure to traumatic events and alcohol use disorders (cf. Roberts et al. 2014 and references therein). Such evidence points to increased alcohol consumption operating as a form of self-medication to cope with trauma and socio-economic insecurity among conflict-affected populations, in particular in contexts of long-term violence like in Colombia (e.g. Georgia). Our own finding that a strong conflict-induced increase in alcohol consumption during pregnancy occurred for mothers living in municipalities with permanent presence of armed groups lends further support to the aforementioned pathway, i.e. that alcohol consumption tends to be an important coping mechanism particularly among populations that are exposed to protracted violence. We find that urban mothers are at particular risk of increased alcohol consumption during pregnancy when exposed to conflict violence, which is in accordance with the fact that alcohol is generally more easily available in urban settings, where drinking among women may also be generally higher than in rural areas (Roberts et al. 2014). The negative consequences of alcohol use during pregnancy for infant and child health are well established in the literature (WHO 2018).

A recent review by Loubaba Mamluk et al. (2020) further concluded that small increases in prenatal alcohol consumption are associated with lower neurocognitive outcomes and (with weaker causal reliability) lower birthweight. Similar to some of the studies reviewed by Loubaba Mamluk et al. (2020), we were unable to identify in our setting any clear birthweight consequences arising from conflict exposure, with its associated increase in maternal alcohol consumption. This suggests that other pathways – e.g. fetal alcohol spectrum disorders, including postnatal growth delay (WHO 2018) – may have played a more important role in linking increased maternal alcohol use during pregnancy to worse child anthropometric measures in our context. In any event, the seemingly moderate *average* effect that we find of conflict exposure on higher maternal alcohol use highlights the likely relevance of other complementary mechanisms at play for explaining the worsening of child anthropometric indicators due to conflict exposure (discussed below), although it is also clear from our analyses that the average effect for maternal alcohol use masks important heterogeneities for specific subgroups, as mentioned previously.

Our finding that the conflict-induced reduction in antenatal care use was especially important for rural mothers agrees with the specific features of the Colombian conflict. As a general rule, rural areas tended to be more strongly affected by the conflict than urban areas, including through higher frequency of conflict-related violent events, tighter control exerted by armed groups on access to medical care by the local populations, and destruction of public (health) infrastructure (Beyrer et al. 2007; Ibáñez 2008).

In addition to antenatal care use, skilled birth delivery and maternal alcohol use, it is possible that further mechanisms may have mediated the negative impact of the Colombian conflict on child anthropometric measures. One such candidate mechanism is a potential child quantity-quality tradeoff in times of conflict, whereby maternal exposure to protracted conflict induces a temporary increase in the demand for children during the conflict period to secure family support under the uncertainties about offspring survival, with such increased fertility resulting in more “mouths to feed” with limited household resources, and hence negative impacts on child anthropometric measures (Nepal et al. 2018). While a similar child quantity-quality tradeoff may have occurred in Colombia in light of the protracted nature of its conflict, we are unable to investigate this possibility in depth for the study period with the available data, and must leave its investigation for future research.[9]

Following previous literature (e.g. Duque 2017, Kim 2019), we focused our investigation on mean child anthropometric Z-scores, finding reductions in these mean Z-scores due to exposure to higher conflict intensity during pregnancy. As complementary analyses, we also explored conflict-induced movements of children to the lower end of the Z-score distribution, by looking at binary versions of the Z-scores (indicating wasting, stunting and underweight) in our regression models. Here, we did not find statistically significant effects of conflict. Although future research is warranted to understand the specific mechanisms linking violence exposure during pregnancy to changes that are specific to the lower tail of these Z-scores distributions, one potential explanation in our context is the relatively low proportion of these more extreme child health outcomes in the dataset (stunting: 11%, wasting: 1%, underweight: 6%), making it more statistically challenging in our study to detect any relevant conflict effects that may exist.

The detrimental effects on child health and intermediate outcomes that we find were largely robust to the additional analyses where we defined conflict intensity as a binary variable or where we included conflict events during the child's first year of life. On the other hand, judged exclusively by the lack of statistical significance of the estimates at conventional levels, the analyses using mother fixed effects and instrumental variables were less conclusive, despite the signs and magnitudes of the estimated effects being mostly unchanged compared to the main results. While we took care in constructing an instrumental variable that likely meets the necessary exclusion restrictions, this variable

[9] This is mainly because (for reasons explained previously in the text, and unlike Nepal et al. 2018) we are in practice limited to only two DHS survey rounds which do not cover any post-conflict period, and there are no clearly defined conflict-affected/unaffected areas in Colombia. Just as a first approximation to the issue, we used the number of children born to the woman as the dependent variable in our models. These regressions (available upon request) presented mixed results, with relatively small coefficients of interest that were either far from statistical significance (Eq 1) or that tended to cancel each other in terms of signs and magnitudes (Eq 2). We concluded that these mixed results provide, at best, only preliminary evidence for the absence of a direct link during the period between conflict exposure and fertility in Colombia (and thus for a quantity-quality trade-off), given the data limitations previously mentioned. We thank an anonymous referee for suggesting the discussion of a child quantity-quality tradeoff as a possible mechanism for conflict effects on child health.

proved to be a relatively weak instrument for our conflict intensity indicator to permit drawing any strong conclusions.[10]

One limitation to this study is imposed by the availability of data, particularly about conflict incidence across localities and individual exposure to violent events. There is no consensus measure of conflict that is used in the literature (much depends also, of course, on the study objectives), and even in Colombia there are different databases and measures used in conflict-related research. We selected the CNMH database for our analyses here because it is arguably the most authoritative and comprehensive source for Colombia. We have explored other databases for reassurance, however. First, we obtained the Colombian Unified Victim Register (RUV) dataset which – as opposed to the CNMH dataset – is a database of violent episodes that are self-reported by victims, where displacement is counted as a type of victimization (Unidad Victimas 2020). Due to the requirement that victims come forward to report violent events for their inclusion in RUV (possibly resulting in important underreporting levels that are likely to be correlated with the degree of violence intensity where people live), and also because the DHS does not permit identification of individuals displaced by the conflict, we opted against using the RUV database in this study. Second, we explored the CEDE database, a panel dataset for municipalities that gathers information from a more limited number of sources than CNMH (mainly official institutions) and that also includes displacement episodes among conflict events (Uniandes 2020). We found that the general trends and across-municipality heterogeneity in conflict events are very similar to those found in the CNMH data (see the comparison of the trends in conflict intensity over the study period, using CNMH, RUV and CEDE data, in Figure 1 - Appendix 1).

Previous research about the conflict in Colombia has also used information on conflict episodes to test hypotheses about the presence of armed actors, instead of the levels of violence (Torres and Urdinola 2018). We incorporate information on the presence of armed groups into our analyses, and find that conflict events have a more detrimental effect on child health where armed groups are intermittently present. Future work may investigate further these issues. Future research may also take advantage of the rapidly evolving

[10] We also attempted to use IVs employed in other studies of the Colombian conflict, but found evidence indicating violations of exclusion restrictions. This includes changes in the prices of natural resources that may drive territorial disputes between armed groups (Dube and Vargas 2009) and homicide capture rates as a proxy for government deterrence measures (Rodriguez and Sanchez 2012).

situation regarding conflict data availability in Colombia, where emerging databases are consolidating information about conflict-related violence alongside spatial geolocated data on their occurrence (Wagner et al. 2018).

## References

Angood C, Khara T, Dolan C, Berkley JA, Group WT. Research priorities on the relationship between wasting and stunting. PloS one. 2016;11(5).

Akresh R, Bhalotra S, Leone M, Osili UO. War and stature: growing up during the Nigerian Civil War. American Economic Review. 2012a May;102(3):273-77.

Akresh R, Lucchetti L, Thirumurthy H. Wars and child health: Evidence from the Eritrean–Ethiopian conflict. Journal of development economics. 2012b Nov 30;99(2):330-40.

Akresh R, Verwimp P, Bundervoet T. Civil war, crop failure, and child stunting in Rwanda. Economic Development and Cultural Change. 2011 Jul 1;59(4):777-810.

Alderman H, Hoddinott J, Kinsey B. Long term consequences of early childhood malnutrition. Oxford economic papers. 2006 Jul 1;58(3):450-74.

Arimond M, Ruel MT. Dietary diversity is associated with child nutritional status: evidence from 11 demographic and health surveys. The Journal of nutrition. 2004 Oct 1;134(10):2579-85.

Beyrer C, Villar JC, Suwanvanichkij V, Singh S, Baral SD, Mills EJ. Neglected diseases, civil conflicts, and the right to health. The Lancet. 2007 Aug 18;370(9587):619-27.

Black RE, Allen LH, Bhutta ZA, Caulfield LE, De Onis M, Ezzati M, Mathers C, Rivera J, Maternal and Child Undernutrition Study Group. Maternal and child undernutrition: global and regional exposures and health consequences. The lancet. 2008 Jan 19;371(9608):243-60.

Bornemisza O, Ranson MK, Poletti TM, Sondorp E. Promoting health equity in conflict-affected fragile states. Social Science & Medicine. 2010;70(1):80-8.

Bundervoet T, Verwimp P, Akresh R. Health and civil war in rural Burundi. Journal of human Resources. 2009 Mar 31;44(2):536-63.

Camacho A. Stress and birth weight: evidence from terrorist attacks. American Economic Review. 2008 May;98(2):511-15.

Case A, Paxson C. Stature and status: Height, ability, and labor market outcomes. Journal of political Economy. 2008 Jun;116(3):499-532.

Centro de Memoria Historica. In Spanish: http://centrodememoriahistorica.gov.co/observatorio/`. Dataset downloaded in November 2018.

Centro de Recursos para el Análisis de Conflictos CERAC 2020. In English and Spanish: http://www.cerac.org.co/es/l%C3%ADneas-de-investigaci%C3%B3n/analisis-conflicto/tipologia-por-municipios-del-conflicto-armado.htm.

Do Q, Iyer L. Mental health in the aftermath of conflict. In: Skaperdas MGE, ed. Oxford Handbook of the Economics of Peace and Conflict. Oxford: Oxford University Press; 2012.

Dube O, Vargas JF. Commodity price shocks and civil conflict: Evidence from Colombia. The Review of Economic Studies. 2013 Oct 1;80(4):1384-421.

Duque V. Early-life conditions and child development: evidence from a violent conflict. SSM-population health. 2017 Dec 1;3:121-31.

FAO, UN World Food Program (WFP) (2010) The state of food insecurity in the world 2010: addressing food insecurity in protracted crises. FAO, Rome.

Gonzalez-Uribe C et al. Provision of health services during the armed conflict in Colombia (work in progress). 2020.

Hack M, Schluchter M, Cartar L, Rahman M, Cuttler L, Borawski E. Growth of very low birth weight infants to age 20 years. Pediatrics. 2003 Jul 1;112(1):e30-8.

Horta BL, Victora CG. Long-term effects of breastfeeding. Geneva: World Health Organization. 2013;74.

Human Rights Watch (2019). World Report 2019: Colombia. https://www.hrw.org/world-report/2019/country-chapters/colombia

Ibáñez AM. El Desplazamiento Forzoso en Colombia: Un Camino Sin Retorno a la Pobreza. Universidad de los Andes, Bogotá; 2008.

ICRC. Protracted Conflict and Humanitarian Action. Geneva, 2016.

Jones, D., D. Molitor, and J. Reif: What do workplace wellness programs do? Evidence from the Illinois Workplace Wellness Study. Quarterly Journal of Economics, 2019, 134 (4), 1747–1791.

Justino, P. & Stojetz, W., 2018. On the Legacies of Wartime Governance, HiCN Working Paper 263.

Kadir A, Shenoda S, Goldhagen J. Effects of armed conflict on child health and development: A systematic review. PLoS one. 2019 Jan 16;14(1):e0210071.

Kim H. In the wake of conflict: the long-term effect on child nutrition in Uganda. Oxford Development Studies. 2019 Jul 3;47(3):336-55.

Leech, G., 2011. The FARC: The Longest Insurgency. London: Zed Books.

Leon G. Civil conflict and human capital accumulation the long-term effects of political violence in Perú. Journal of Human Resources. 2012 Oct 2;47(4):991-1022.

Loubaba Mamluk et al: Evidence of detrimental effects of prenatal alcohol exposure on offspring birthweight and neurodevelopment from a systematic review of quasi-experimental studies, International Journal of Epidemiology, Volume 49, Issue 6, December 2020, Pages 1972–1995)

Minoiu C, Shemyakina ON. Armed conflict, household victimization, and child health in Côte d'Ivoire. Journal of Development Economics. 2014 May 1;108:237-55.

Nepal, Apsara and Halla, Martin and Stillman, Steven, Violent Conflict and the Child Quantity-Quality Tradeoff. IZA Discussion Paper No. 11690.

OECD. OECD Reviews of Health Systems: Colombia 2016. Paris, 2015.

O'Donnell O, Van Doorslaer E, Wagstaff A, Lindelow M. Analyzing health equity using household survey data: a guide to techniques and their implementation. The World Bank; 2007 Oct 27.

Roberts B, Murphy A, Chikovani I, Makhashvili N, Patel V, McKee M. Individual and community level risk-factors for alcohol use disorder among conflict-affected persons in Georgia. PloS one. 2014;9(5).

Rodriguez C, Sanchez F. Armed conflict exposure, human capital investments, and child labor: evidence from Colombia. Defence and Peace Economics. 2012 Apr 1;23(2):161-84.

Stein AD, Lumey LH. The relationship between maternal and offspring birth weights after maternal prenatal famine exposure: the Dutch Famine Birth Cohort Study. Human biology. 2000 Aug 1:641-54.

Stewart MA. Civil war as state-making: Strategic governance in civil war. International Organization. 2018;72(1):205-26.

Torche F. The effect of maternal stress on birth outcomes: exploiting a natural experiment. Demography. 2011 Nov 1;48(4):1473-91.

Torres AF, Urdinola BP. Armed Conflict and Fertility in Colombia, 2000–2010. Population Research and Policy Review. 2018:1-41.

Uniandes 2020: CEDE dataset. See in Spanish: https://datoscede.uniandes.edu.co/es/noticias/57-infografia-panel-de-violencia-y-conflicto. Dataset Downloaded in 2019.

Unidad Victimas: RUV dataset. 2020. In Spanish: https://rni.unidadvictimas.gov.co/RUV. Dataset downloaded in 2018.

United Nations Children's Fund (UNICEF), World Health Organization, International Bank for Reconstruction and Development/The World Bank. Levels and trends in child malnutrition: key findings of the 2019 Edition of the Joint Child Malnutrition Estimates. Geneva: World Health Organization; 2019.

Urdinola P. Could Political Violence Affect Infant Mortality? 2004. The Colombian Case. https://www.repository.fedesarrollo.org.co/handle/11445/1073

Uribe A. Governance Without Control: Insurgent Institutions and Rebel-Civilian Interaction in Contested Zones. CPW–MPSA Practice Session. 2017 Feb 15:1-28.

Wagner Z, Heft-Neal S, Bhutta ZA, Black RE, Burke M, Bendavid E. Armed conflict and child mortality in Africa: a geospatial analysis. The Lancet. 2018 Sep 8;392(10150):857-65.

WHO Multicentre Growth Reference Study Group, de Onis M. WHO Child Growth Standards based on length/height, weight and age. Acta paediatrica. 2006 Apr;95:76-85.

WHO (2019). Nutrition Landscape Information System (NLIS) country profile indicators: interpretation guide. Geneva. https://www.who.int/nutrition/nlis_interpretation_guide.pdf.

WHO (2018) Global Status Report on Alcohol and Health. Geneva: World Health Organization.

World Bank. Forcibly Displaced: Toward a Development Approach Supporting Refugees, The Internally Displaced, and Their Hosts. Washington, DC: World Bank; 2016.

# Tables

*Table 1. Descriptive statistics of the estimation sample*

| Conflict exposure variables | N | mean | sd | min | max |
|---|---|---|---|---|---|
| Conflict events in utero | 22889 | 0.38 | 0.71 | 0.00 | 10.64 |
| Conflict events during 1st year of child | 22889 | 0.44 | 0.85 | 0.00 | 12.75 |
| Conflict events during 2nd year of child* | 17616 | 0.42 | 0.85 | 0.00 | 12.75 |
| Conflict events during 3rd year of child* | 12787 | 0.39 | 0.79 | 0.00 | 12.66 |
| Conflict events during 4th year of child* | 8309 | 0.33 | 0.60 | 0.00 | 6.29 |
| Conflict events during 5th year of child* | 3891 | 0.28 | 0.50 | 0.00 | 5.84 |
| High conflict in year of birth | 22889 | 0.22 | | 0.00 | 1.00 |
| **Main outcomes** | | | | | |
| Height for age z score | 22889 | -0.72 | 1.10 | -5.88 | 5.85 |
| Weight for age z score | 22889 | -0.45 | 1.10 | -4.85 | 5.63 |
| Weight for height z score | 22889 | 0.05 | 0.98 | -3.97 | 5.90 |
| Stunting (zscore_ha < -2) | 22889 | 0.11 | | 0.00 | 1.00 |
| Wasting (zscore_wa < -2) | 22889 | 0.01 | | 0.00 | 1.00 |
| Underweight (zscore_wh < -2) | 22889 | 0.07 | | 0.00 | 1.00 |
| **Intermediate outcomes** | | | | | |
| Breastfeeding duration (months) | 21126 | 12.25 | 9.43 | 0.00 | 59.00 |
| Accessed prenatal care | 19021 | 0.94 | | 0.00 | 1.00 |
| Alcohol use during pregnancy | 19021 | 0.09 | | 0.00 | 1.00 |
| Low birth weight (<2500g) | 16470 | 0.07 | | 0.00 | 1.00 |
| Birth attended by health professional | 22889 | 0.89 | | 0.00 | 1.00 |
| Fully vaccinated | 22889 | 0.55 | | 0.00 | 1.00 |
| **Control variables** | | | | | |
| Population (thousand) | 22889 | 624.64 | 1615.03 | 2.33 | 7363.78 |
| Child age in months | 22889 | 28.69 | 17.17 | 0.00 | 59.00 |
| Child sex (male) | 22889 | 0.49 | | 0.00 | 1.00 |
| Birth order (1st) | 22889 | 0.35 | | 0.00 | 1.00 |
| Birth order (2nd) | 22889 | 0.28 | | 0.00 | 1.00 |
| Birth order (3rd) | 22889 | 0.17 | | 0.00 | 1.00 |
| Birth order (4th) | 22889 | 0.09 | | 0.00 | 1.00 |
| Birth order (5th +) | 22889 | 0.12 | | 0.00 | 1.00 |
| Mother's age at child birth | 22889 | 27.85 | 6.91 | 13.00 | 49.00 |
| Mothers education (years) | 22889 | 7.89 | 4.10 | 0.00 | 23.00 |
| Rural household | 22889 | 0.34 | | 0.00 | 1.00 |
| Asset index quintile 1 | 22889 | 0.37 | | 0.00 | 1.00 |
| Asset index quintile 2 | 22889 | 0.32 | | 0.00 | 1.00 |
| Asset index quintile 3 | 22889 | 0.14 | | 0.00 | 1.00 |
| Asset index quintile 4 | 22889 | 0.08 | | 0.00 | 1.00 |
| Asset index quintile 5 | 22889 | 0.09 | | 0.00 | 1.00 |
| Birth cohort: 2001 | 22889 | 0.07 | | 0.00 | 1.00 |
| Birth cohort: 2002 | 22889 | 0.08 | | 0.00 | 1.00 |
| Birth cohort: 2003 | 22889 | 0.08 | | 0.00 | 1.00 |

| | | | | |
|---|---|---|---|---|
| Birth cohort: 2004 | 22889 | 0.09 | 0.00 | 1.00 |
| Birth cohort: 2005 | 22889 | 0.10 | 0.00 | 1.00 |
| Birth cohort: 2006 | 22889 | 0.07 | 0.00 | 1.00 |
| Birth cohort: 2007 | 22889 | 0.11 | 0.00 | 1.00 |
| Birth cohort: 2008 | 22889 | 0.11 | 0.00 | 1.00 |
| Birth cohort: 2009 | 22889 | 0.12 | 0.00 | 1.00 |
| Birth cohort: 2010 | 22889 | 0.12 | 0.00 | 1.00 |
| **Subgroup variables** | | | | |
| Child age category: 0-1 | 22889 | 0.23 | 0.00 | 1.00 |
| Child age category: 1-2 | 22889 | 0.21 | 0.00 | 1.00 |
| Child age category: 2-3 | 22889 | 0.20 | 0.00 | 1.00 |
| Child age category: 3-4 | 22889 | 0.19 | 0.00 | 1.00 |
| Child age category: 4-5 | 22889 | 0.23 | 0.00 | 1.00 |
| Presence of armed groups: no armed presence | 22889 | 0.08 | 0.00 | 1.00 |
| Presence of armed groups: intermittent presence | 22889 | 0.62 | 0.00 | 1.00 |
| Presence of armed groups: permanent presence | 22889 | 0.30 | 0.00 | 1.00 |

*Note: Observations for conflict in the *k*-th year of the child where the child was not *k* years old yet set to missing for descriptive purposes.

*Table 2. Regression results for Z-score outcomes - the effect of exposure to conflict events during pregnancy and during the child's life*

| **Part A: Z scores** | **Height-for-age Z score** | | | **Weight-for-height Z score** | | | **Weight-for-age Z score** | | |
|---|---|---|---|---|---|---|---|---|---|
| | OLS | FE | FE | OLS | FE | FE | OLS | FE | FE |
| Conflict in utero | 0.0510** | -0.0583*** | -0.0555*** | -0.0387*** | -0.0093 | -0.0216 | 0.0033 | -0.0486*** | -0.0580*** |
| | (0.0218) | (0.0207) | (0.0195) | (0.0120) | (0.0170) | (0.0187) | (0.0144) | (0.0171) | (0.0202) |
| Conflict in y1 | | | 0.0228 | | | 0.0187 | | | 0.0302 |
| | | | (0.0240) | | | (0.0144) | | | (0.0184) |
| Conflict in y2 | | | -0.0746*** | | | -0.0006 | | | -0.0400** |
| | | | (0.0239) | | | (0.0192) | | | (0.0169) |
| Conflict in y3 | | | 0.0215 | | | 0.0504*** | | | 0.0313 |
| | | | (0.0359) | | | (0.0189) | | | (0.0208) |
| Conflict in y4 | | | 0.0098 | | | 0.0412* | | | 0.0469 |
| | | | (0.0412) | | | (0.0237) | | | (0.0318) |
| Conflict in y5 | | | 0.0196 | | | -0.0635** | | | -0.0196 |
| | | | (0.0536) | | | (0.0322) | | | (0.0374) |
| **N** | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 |

| **Part B: Binary variables** | **Stunting** | | | **Wasting** | | | **Underweight** | | |
|---|---|---|---|---|---|---|---|---|---|
| | OLS | FE | FE | OLS | FE | FE | OLS | FE | FE |
| Conflict in utero | -0.0147*** | 0.0048 | 0.0019 | 0.0019* | -0.0013 | -0.0014 | -0.0050 | 0.0003 | 0.0055 |
| | (0.0048) | (0.0051) | (0.0059) | (0.0011) | (0.0027) | (0.0032) | (0.0040) | (0.0049) | (0.0045) |
| Conflict in y1 | | | 0.0035 | | | 0.0001 | | | -0.0125** |
| | | | (0.0079) | | | (0.0026) | | | (0.0051) |
| Conflict in y2 | | | 0.0039 | | | 0.0010 | | | 0.0074* |
| | | | (0.0062) | | | (0.0022) | | | (0.0042) |
| Conflict in y3 | | | 0.0024 | | | -0.0036 | | | 0.0015 |
| | | | (0.0121) | | | (0.0026) | | | (0.0061) |
| Conflict in y4 | | | 0.0032 | | | -0.0036 | | | -0.0147 |
| | | | (0.0114) | | | (0.0032) | | | (0.0099) |
| Conflict in y5 | | | -0.0085 | | | 0.0054 | | | 0.0083 |
| | | | (0.0133) | | | (0.0053) | | | (0.0110) |
| **N** | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 |

**Notes**: OLS: ordinary least squares, FE: municipality fixed effects. All regressions include control variables and birth cohort fixed effects. Standard errors are clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

*Table 3 Regression results for Z-score outcomes, by child age groups - the effect of exposure to conflict events during pregnancy and during the child's life (results from fixed effects regressions)*

| **Z-scores:** | | **Height-for-age Z score** | **Weight-for-height Z score** | **Weight-for-age Z score** | **Stunting** | **Wasting** | **Underweight** |
|---|---|---|---|---|---|---|---|
| Conflict in utero: | age 0-1 | -0.0022 | -0.0383 | -0.0732 | -0.0215 | -0.0049 | -0.0304** |
| | | (0.0747) | (0.0719) | (0.0742) | (0.0229) | (0.0064) | (0.0143) |
| | age 1-2 | -0.0296 | 0.0053 | -0.0269 | -0.0179 | 0.0109 | -0.0053 |
| | | (0.0318) | (0.0531) | (0.0532) | (0.0116) | (0.0088) | (0.0120) |
| | age 2-3 | -0.0777*** | -0.0356* | -0.0869*** | 0.0065 | -0.0035 | 0.0140*** |
| | | (0.0229) | (0.0195) | (0.0177) | (0.0077) | (0.0026) | (0.0050) |
| | age 3-4 | 0.0241 | 0.0583 | 0.0509 | -0.0071 | -0.0131** | 0.0073 |
| | | (0.0553) | (0.0549) | (0.0636) | (0.0153) | (0.0055) | (0.0135) |
| | age 4-5 | -0.1178** | -0.0636 | -0.1095** | 0.0239 | -0.0034 | 0.0302*** |
| | | (0.0458) | (0.0417) | (0.0465) | (0.0160) | (0.0051) | (0.0116) |
| Conflict in y1: | age 0-1 | 0.0931 | 0.0005 | 0.0886 | 0.0241 | -0.0025 | 0.0014 |
| | | (0.0802) | (0.0880) | (0.0792) | (0.0248) | (0.0069) | (0.0147) |
| | age 1-2 | 0.0581 | -0.1095** | -0.0340 | 0.0181 | -0.0114 | 0.0137 |
| | | (0.0526) | (0.0550) | (0.0521) | (0.0136) | (0.0090) | (0.0165) |
| | age 2-3 | 0.0267 | 0.0621*** | 0.0621** | 0.0016 | -0.0005 | -0.0163 |
| | | (0.0455) | (0.0227) | (0.0259) | (0.0188) | (0.0040) | (0.0108) |
| | age 3-4 | -0.0812** | -0.0345 | -0.0747* | 0.0187 | 0.0102 | -0.0003 |
| | | (0.0350) | (0.0478) | (0.0449) | (0.0144) | (0.0066) | (0.0112) |
| | age 4-5 | 0.0907* | 0.0943** | 0.1227** | -0.0311* | 0.0000 | -0.0391*** |
| | | (0.0498) | (0.0433) | (0.0513) | (0.0160) | (0.0039) | (0.0120) |
| Conflict in y2: | age 1-2 | -0.2192*** | 0.1209 | -0.0173 | 0.0398 | 0.0039 | -0.0283 |
| | | (0.0669) | (0.0870) | (0.0807) | (0.0245) | (0.0101) | (0.0220) |
| | age 2-3 | 0.0379 | -0.1072* | -0.0587 | -0.0240 | 0.0010 | -0.0108 |
| | | (0.0660) | (0.0650) | (0.0496) | (0.0220) | (0.0060) | (0.0195) |
| | age 3-4 | 0.0110 | 0.0223 | 0.0168 | -0.0097 | -0.0009 | 0.0084 |
| | | (0.0343) | (0.0223) | (0.0238) | (0.0085) | (0.0037) | (0.0080) |
| | age 4-5 | -0.0582 | -0.0452 | -0.0730 | 0.0073 | -0.0046 | 0.0171 |
| | | (0.0479) | (0.0574) | (0.0646) | (0.0173) | (0.0033) | (0.0141) |
| Conflict in y3: | age 2-3 | -0.0435 | 0.1253 | 0.0480 | 0.0313 | -0.0014 | 0.0071 |
| | | (0.0902) | (0.0846) | (0.0653) | (0.0272) | (0.0078) | (0.0228) |
| | age 3-4 | 0.0786 | 0.0368 | 0.0919 | -0.0091 | -0.0024 | -0.0393** |
| | | (0.0820) | (0.0612) | (0.0586) | (0.0394) | (0.0073) | (0.0169) |
| | age 4-5 | -0.0343 | 0.0400 | 0.0057 | 0.0213** | -0.0000 | 0.0084 |
| | | (0.0272) | (0.0401) | (0.0299) | (0.0084) | (0.0030) | (0.0059) |
| Conflict in y4: | age 3-4 | -0.1043 | 0.0129 | -0.0729 | 0.0320 | -0.0080 | 0.0037 |
| | | (0.0958) | (0.0721) | (0.0760) | (0.0356) | (0.0091) | (0.0180) |
| | age 4-5 | 0.1335* | 0.0177 | 0.0997 | -0.0150 | 0.0009 | -0.0403** |
| | | (0.0803) | (0.0667) | (0.0686) | (0.0231) | (0.0075) | (0.0189) |
| Conflict in y5: | age 4-5 | -0.0571 | -0.0166 | -0.0492 | 0.0055 | 0.0028 | 0.0091 |
| | | (0.0949) | (0.0777) | (0.0866) | (0.0294) | (0.0071) | (0.0219) |
| **N** | | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 |

**Notes**: All regressions are municipality fixed effects regressions, including control variables and birth cohort fixed effects. Standard errors are clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

*Table 4. Regression results for intermediate outcomes – the effect of exposure to conflict events during pregnancy*

| | **Alcohol use during pregnancy** | | **Accessed prenatal care** | | **Birth attended by professional** | |
|---|---|---|---|---|---|---|
| | OLS | FE | OLS | FE | OLS | FE |
| Conflict in utero | 0.0061 | 0.0110* | -0.0096 | -0.0177** | -0.0203* | -0.0173** |
| | (0.0052) | (0.0065) | (0.0079) | (0.0076) | (0.0121) | (0.0085) |
| **N** | 19,021 | 19,021 | 19,021 | 19,021 | 22,889 | 22,889 |

| | **Low birth weight (<2500g)** | | **Breastfeeding (months)** | | **Fully vaccinated** | |
|---|---|---|---|---|---|---|
| | OLS | FE | OLS | FE | OLS | FE |
| Conflict in utero | -0.0035 | 0.0035 | -0.0542 | 0.1876 | -0.0032 | -0.0059 |
| | (0.0032) | (0.0051) | (0.1477) | (0.2091) | (0.0131) | (0.0107) |
| **N** | 16,470 | 16,470 | 21,126 | 21,126 | 22,889 | 22,889 |

**Notes**: OLS: ordinary least squares, FE: municipality fixed effects. All regressions include control variables and birth cohort fixed effects. Standard errors are clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

*Table 5 Subgroup analysis by rural/urban communities and persistence of conflict (presence of armed groups): health outcomes*

| Height-for-age Z score | Overall | Urban | Rural | No armed presence | Intermittent presence | Permanent presence |
|---|---|---|---|---|---|---|
| Conflict in utero | -0.0555*** | -0.0152 | -0.0931*** | -0.2365** | -0.0731*** | -0.0357 |
| | (0.0195) | (0.0253) | (0.0266) | (0.1032) | (0.0222) | (0.0523) |
| Conflict in y1 | 0.0228 | 0.0236 | 0.0098 | 0.1948** | 0.0004 | 0.0704 |
| | (0.0240) | (0.0266) | (0.0229) | (0.0944) | (0.0251) | (0.0509) |
| Conflict in y2 | -0.0746*** | -0.0862*** | -0.0733** | -0.0658 | -0.0738*** | -0.1134 |
| | (0.0239) | (0.0297) | (0.0294) | (0.1271) | (0.0242) | (0.0817) |
| Conflict in y3 | 0.0215 | 0.0236 | 0.0084 | 0.1165 | 0.0026 | 0.1007 |
| | (0.0359) | (0.0339) | (0.0430) | (0.1404) | (0.0338) | (0.0955) |
| Conflict in y4 | 0.0098 | -0.0069 | 0.0457 | -0.1403 | 0.0095 | -0.0293 |
| | (0.0412) | (0.0474) | (0.0555) | (0.2277) | (0.0461) | (0.0755) |
| Conflict in y5 | 0.0196 | 0.1207 | -0.0870 | -0.4445 | 0.0229 | 0.0311 |
| | (0.0536) | (0.0794) | (0.0662) | (0.4380) | (0.0619) | (0.0800) |
| N | 22,889 | 15,013 | 7,876 | 1,873 | 14,219 | 6,797 |
| **Weight-for-height Z score** | | | | | | |
| Conflict in utero | -0.0216 | -0.0209 | -0.0093 | -0.0402 | -0.0032 | -0.0800 |
| | (0.0187) | (0.0282) | (0.0232) | (0.1632) | (0.0195) | (0.0628) |
| Conflict in y1 | 0.0187 | 0.0304 | 0.0210 | 0.1791 | 0.0240 | -0.0147 |
| | (0.0144) | (0.0316) | (0.0186) | (0.1108) | (0.0164) | (0.0279) |
| Conflict in y2 | -0.0006 | -0.0158 | 0.0014 | 0.0155 | -0.0051 | 0.0957 |
| | (0.0192) | (0.0299) | (0.0235) | (0.0772) | (0.0166) | (0.0948) |
| Conflict in y3 | 0.0504*** | 0.0208 | 0.0820*** | 0.0704 | 0.0488** | 0.0340 |
| | (0.0189) | (0.0281) | (0.0212) | (0.1058) | (0.0197) | (0.0787) |
| Conflict in y4 | 0.0412* | 0.0691* | 0.0192 | -0.1510 | 0.0624** | -0.0589 |
| | (0.0237) | (0.0392) | (0.0435) | (0.2230) | (0.0261) | (0.0398) |
| Conflict in y5 | -0.0635** | -0.0403 | -0.0780 | -0.7719** | -0.0643* | 0.0293 |
| | (0.0322) | (0.0456) | (0.0513) | (0.3727) | (0.0366) | (0.0678) |
| N | 22,889 | 15,013 | 7,876 | 1,873 | 14,219 | 6,797 |
| **Weight-for-age Z score** | | | | | | |
| Conflict in utero | -0.0580*** | -0.0337 | -0.0735*** | -0.1938 | -0.0546** | -0.0962* |
| | (0.0202) | (0.0288) | (0.0271) | (0.1684) | (0.0212) | (0.0475) |
| Conflict in y1 | 0.0302 | 0.0370 | 0.0247 | 0.2363*** | 0.0218 | 0.0241 |
| | (0.0184) | (0.0267) | (0.0242) | (0.0765) | (0.0208) | (0.0413) |
| Conflict in y2 | -0.0400** | -0.0580* | -0.0416* | -0.0590 | -0.0477*** | 0.0506 |
| | (0.0169) | (0.0328) | (0.0218) | (0.1389) | (0.0174) | (0.0594) |
| Conflict in y3 | 0.0313 | 0.0076 | 0.0477* | 0.0955 | 0.0239 | 0.0296 |
| | (0.0208) | (0.0293) | (0.0261) | (0.1248) | (0.0226) | (0.0448) |
| Conflict in y4 | 0.0469 | 0.0597 | 0.0533 | -0.1939 | 0+F37.0600* | -0.0416 |
| | (0.0318) | (0.0475) | (0.0516) | (0.2785) | (0.0362) | (0.0566) |
| Conflict in y5 | -0.0196 | 0.0613 | -0.0986 | -0.7447* | -0.0195 | 0.0649 |
| | (0.0374) | (0.0459) | (0.0660) | (0.3887) | (0.0423) | (0.0835) |
| N | 22,889 | 15,013 | 7,876 | 1,873 | 14,219 | 6,797 |

Notes: OLS: ordinary least squares, FE: fixed effects, rural: rural household, urban: urban household. All regressions include control variables and birth cohort fixed effects. Standard errors are clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

*Table 6 Subgroup analysis by rural/urban communities and persistence of conflict (presence of armed groups): mechanisms*

| **Alcohol use during pregnancy** | **Overall** | **Urban** | **Rural** | **No armed presence** | **Intermittent presence** | **Permanent presence** |
|---|---|---|---|---|---|---|
| Conflict in utero | 0.0110* | 0.0203** | 0.0039 | 0.0064 | 0.0046 | 0.0288*** |
| | (0.0065) | (0.0080) | (0.0082) | (0.0386) | (0.0075) | (0.0104) |
| N | 19,021 | 12,904 | 6,117 | 1,522 | 11,677 | 5,822 |
| **Accessed prenatal care** | | | | | | |
| Conflict in utero | -0.0177** | -0.0102 | -0.0232* | 0.0482 | -0.0247*** | -0.0013 |
| | (0.0076) | (0.0066) | (0.0125) | (0.0362) | (0.0074) | (0.0079) |
| N | 19,021 | 12,904 | 6,117 | 1,522 | 11,677 | 5,822 |
| **Birth attended by professional** | | | | | | |
| Conflict in utero | -0.0173** | -0.0155** | -0.0122 | -0.0461 | -0.0239*** | 0.0075 |
| | (0.0085) | (0.0063) | (0.0121) | (0.0508) | (0.0077) | (0.0132) |
| N | 22,889 | 15,013 | 7,876 | 1,873 | 14,219 | 6,797 |

Notes: OLS: ordinary least squares, FE: fixed effects, rural: rural household, urban: urban household. All regressions include control variables and birth cohort fixed effects. Standard errors are clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

*Table 7. Robustness check 1: the effect of being born in a high conflict intensity municipality*

| **Outcomes** | **Height-for-age Z score** | | **Weight-for-height Z score** | | **Weight-for-age Z score** | |
|---|---|---|---|---|---|---|
| | OLS | FE | OLS | FE | OLS | FE |
| High conflict in year of birth | 0.0949** | -0.0577** | -0.0683** | -0.0327 | 0.0116 | -0.0599* |
| | (0.0368) | (0.0289) | (0.0268) | (0.0349) | (0.0282) | (0.0332) |
| N | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 | 22,889 |
| | **Alcohol use during pregnancy** | | **Antenatal care** | | **Birth attended by professional** | |
| VARIABLES | OLS | FE | OLS | FE | OLS | FE |
| High conflict in year of birth | 0.0121 | 0.0125 | -0.0075 | -0.0119 | -0.0218 | -0.0189** |
| | (0.0101) | (0.0088) | (0.0104) | (0.0087) | (0.0173) | (0.0095) |
| N | 19,021 | 19,021 | 19,021 | 19,021 | 22,889 | 22,889 |

Notes: OLS: ordinary least squares, FE: fixed effects. All regressions include control variables and birth cohort fixed effects. Standard errors are clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

*Table 8. Robustness check 2: mother fixed effects analysis for child health outcomes*

| | **Height-for-age Z score** | | | **Weight-for-height Z score** | | | **Weight-for-age Z score** | | |
|---|---|---|---|---|---|---|---|---|---|
| | Munic. FE | Munic. FE | Mother FE | Munic. FE | Munic. FE | Mother FE | Munic. FE | Munic. FE | Mother FE |
| Conflict in utero | -0.0555*** | -0.0347 | -0.0774* | -0.0216 | -0.0327 | -0.0047 | -0.0580*** | -0.0580* | -0.0689** |
| | -0.0195 | (0.0305) | -0.0396 | -0.0187 | (0.0256) | -0.0306 | -0.0202 | (0.0297) | -0.034 |
| Conflict in y1 | 0.0228 | 0.0460 | 0.0572* | 0.0187 | 0.0513** | 0.0641** | 0.0302 | 0.0697*** | 0.0807** |
| | -0.024 | (0.0310) | -0.0325 | -0.0144 | (0.0220) | -0.0302 | -0.0184 | (0.0239) | -0.0315 |
| Conflict in y2 | -0.0746*** | -0.1204*** | -0.1055*** | -0.0006 | -0.0060 | -0.0288 | -0.0400** | -0.0682** | -0.0769** |
| | -0.0239 | (0.0394) | -0.0348 | -0.0192 | (0.0326) | -0.0333 | -0.0169 | (0.0281) | -0.0335 |
| Conflict in y3 | 0.0215 | 0.0488 | 0.003 | 0.0504*** | 0.0544** | 0.0817** | 0.0313 | 0.0511 | 0.0408 |
| | -0.0359 | (0.0453) | -0.0456 | -0.0189 | (0.0274) | -0.0389 | -0.0208 | (0.0353) | -0.0406 |
| Conflict in y4 | 0.0098 | 0.0302 | 0.0282 | 0.0412* | 0.0315 | 0.0828 | 0.0469 | 0.0527 | 0.1043 |
| | -0.0412 | (0.0599) | -0.063 | -0.0237 | (0.0523) | -0.0639 | -0.0318 | (0.0557) | -0.0659 |
| Conflict in y5 | 0.0196 | -0.0459 | 0.01 | -0.0635** | -0.0249 | -0.1117 | -0.0196 | -0.0319 | -0.0744 |
| | -0.0536 | (0.0729) | -0.0826 | -0.0322 | (0.0585) | -0.076 | -0.0374 | (0.0719) | -0.0794 |
| N | 22,889 | 7,666 | 7,666 | 22,889 | 7,666 | 7,666 | 22,889 | 7,666 | 7,666 |
| N of mother id | | | 4,060 | | | 4,060 | | | 4,060 |

Notes: OLS: ordinary least squares, Munic. FE: municipality fixed effects. Mother. FE: mother fixed effects. All regressions include control variables and birth cohort fixed effects. Standard errors are clustered by municipality (for munic. FE) and mother (mother FE). *** p<0.01, ** p<0.05, * p<0.1

*Table 9. Robustness check 3: sample selection analysis*

| | **Selection equation (outcome: Observation excluded from sample)** | **FE model for selected sample** | **FE model for full sample** | **FE model for full sample, with selection indicator** |
|---|---|---|---|---|
| Conflict in utero | 0.0098 | -0.0555*** | -0.0424** | -0.0420** |
| | (0.0085) | (0.0195) | (0.0198) | (0.0197) |
| Conflict: y1 | 0.0015 | 0.0228 | 0.0176 | 0.0174 |
| | (0.0080) | (0.0240) | (0.0199) | (0.0200) |
| Conflict: y2 | 0.0155** | -0.0746*** | -0.0770*** | -0.0764*** |
| | (0.0068) | (0.0239) | (0.0212) | (0.0212) |
| Conflict: y3 | -0.0091 | 0.0215 | 0.0258 | 0.0246 |
| | (0.0098) | (0.0359) | (0.0335) | (0.0334) |
| Conflict: y4 | 0.0035 | 0.0098 | 0.0184 | 0.0199 |
| | (0.0124) | (0.0412) | (0.0322) | (0.0322) |
| Conflict: y5 | 0.0174 | 0.0196 | 0.0211 | 0.0211 |
| | (0.0161) | (0.0536) | (0.0356) | (0.0360) |
| Observations | 28,290 | 22,889 | 28,290 | 28,290 |

Notes: All regressions reported are fixed effects specifications, including control variables, municipality and birth cohort fixed effects. Standard errors are clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

# Figures

*Figure 1 (Top Left) Distribution of the conflict intensity variable (conflict events in utero) in the estimation sample. (Top Right to bottom right) Correlation between HAZ WAZ, WHZ and conflict intensity*

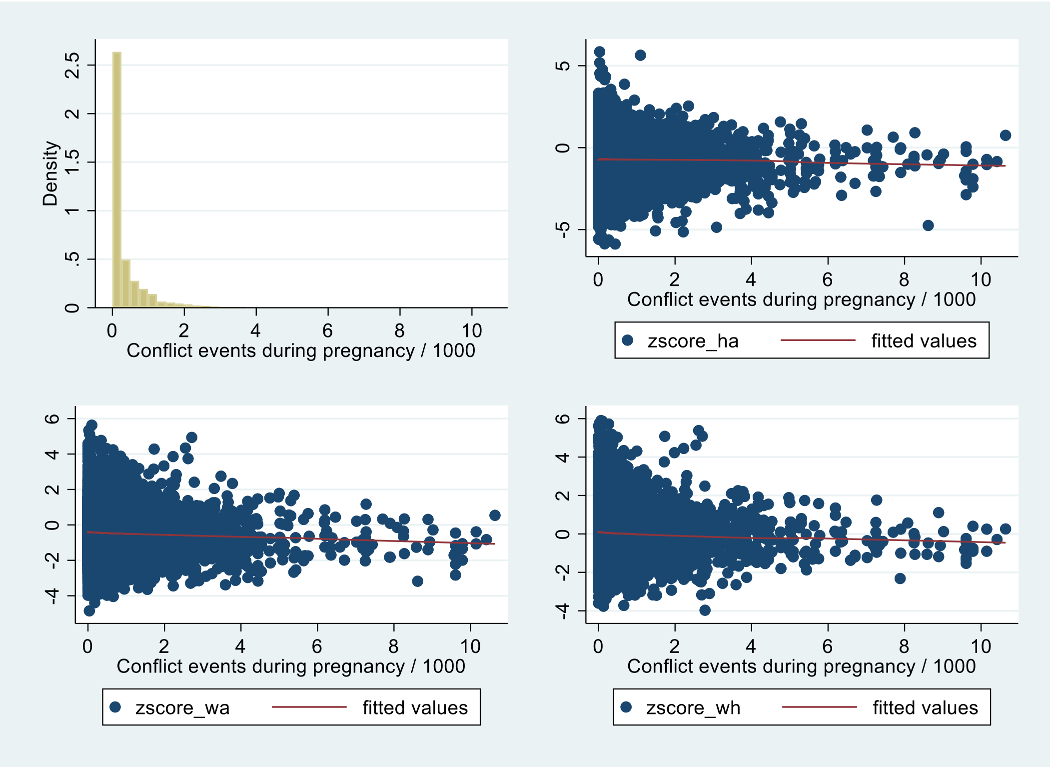


*Figure 2 Conflict experienced at the time of birth (solid line: total conflict events in utero, per 1000 population dashed line: high conflict intensity (top quintile) municipality in the year of birth)*

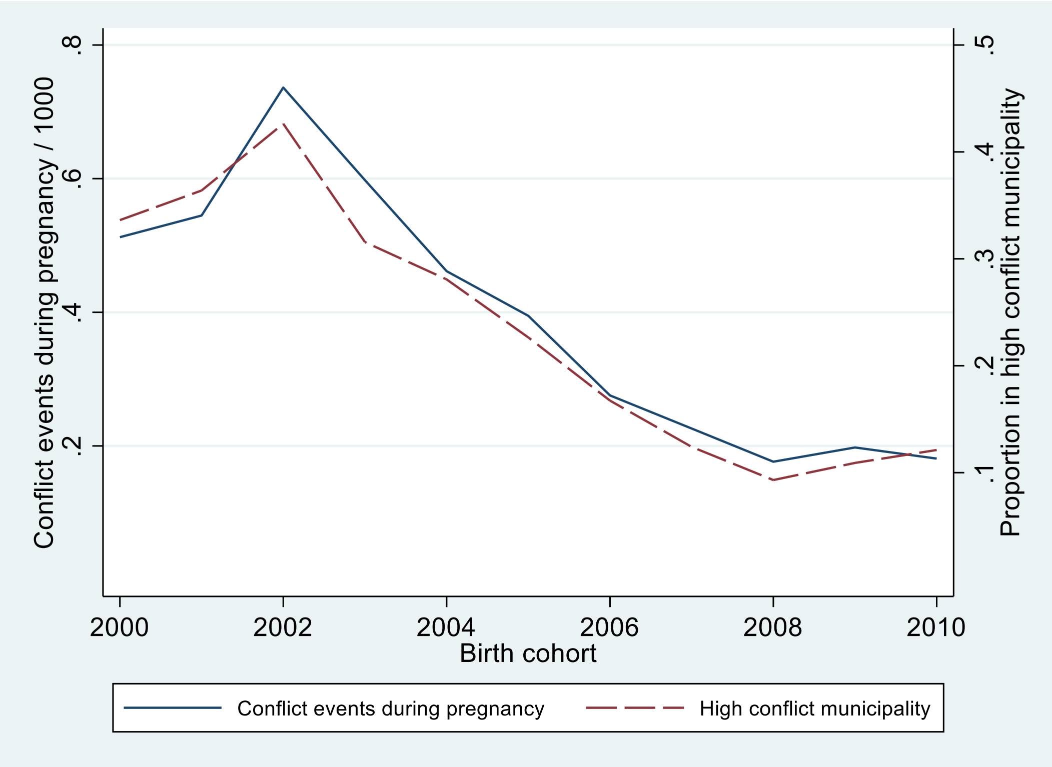
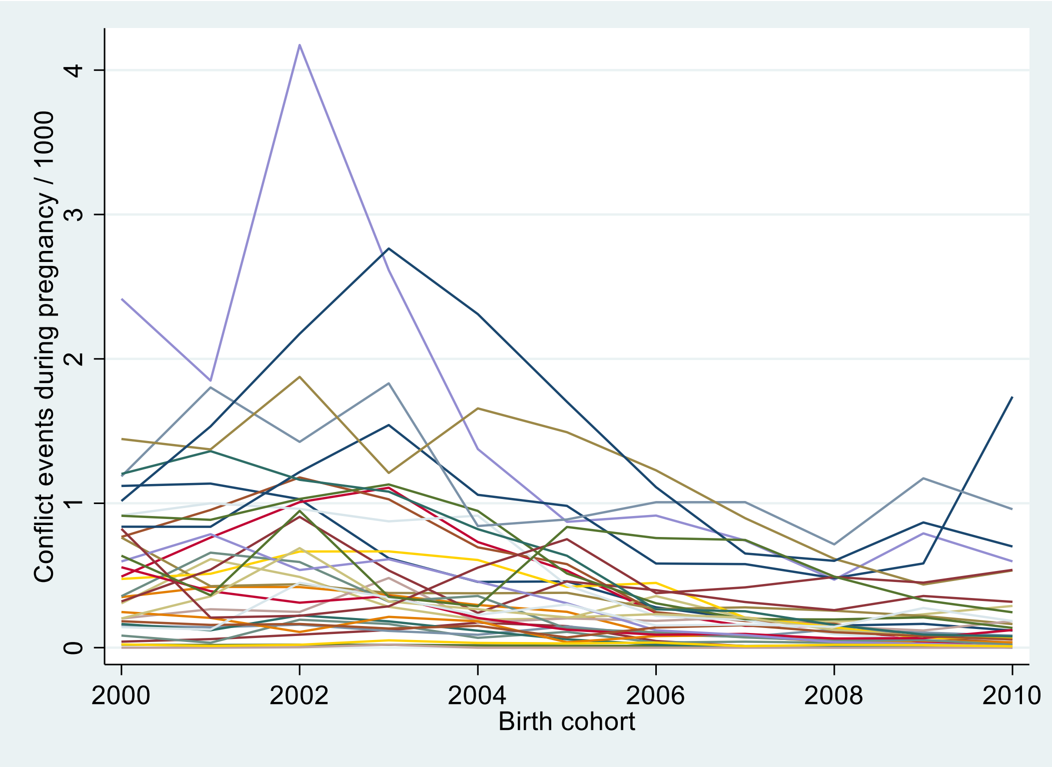
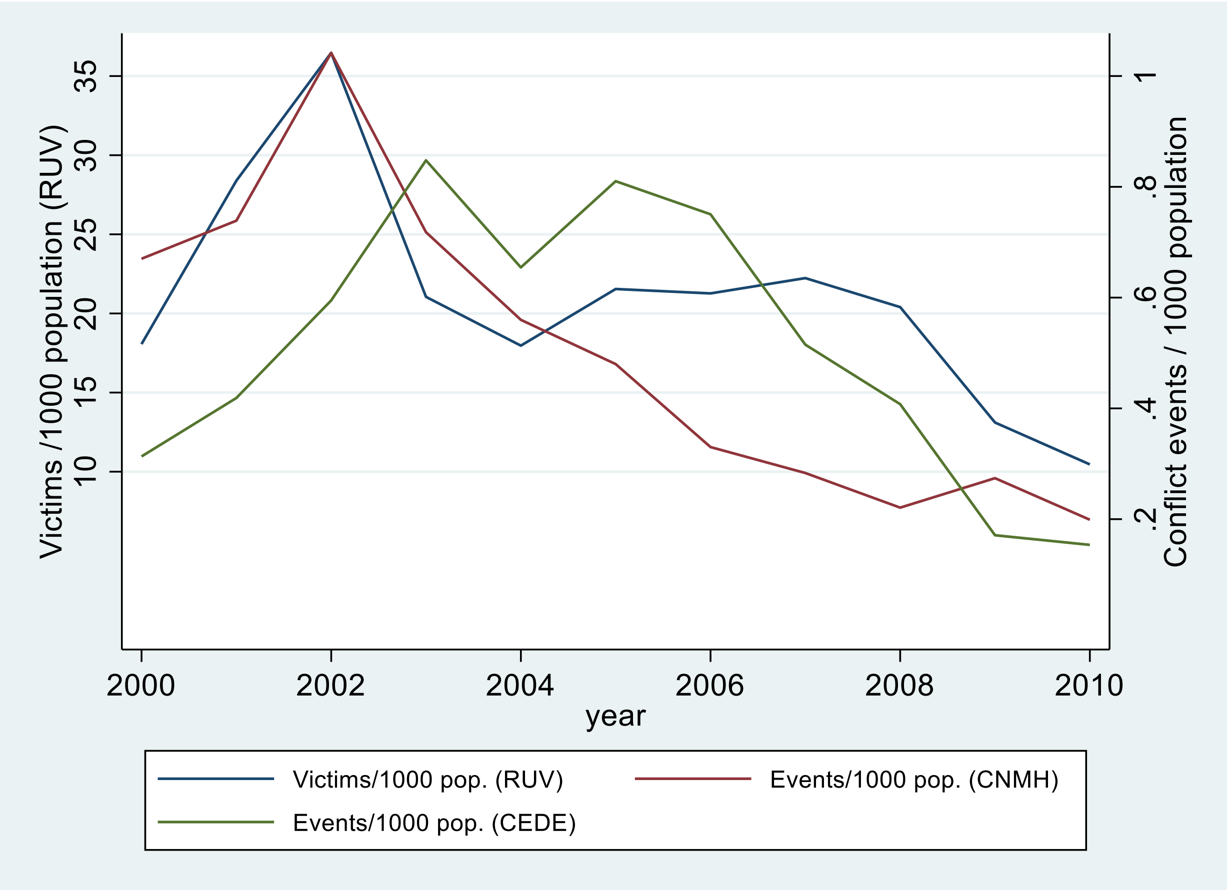


*Figure 3 Conflict intensity (conflict events in utero) by birth cohort, aggregated by departamento*



Appendix to the paper “The Impact of Civil Conflict on Child Health: Evidence from Colombia”

*Appendix 1 Figure 1: Comparison of conflict intensity measures derived from the CNMH, CEDE and RUV databases*



*Appendix Table 4: Targeted literature review of comparable studies investigating the impact of conflict exposure on child anthropometric measures*

| Reference | Location | Dates of study | Effect estimate |
|---|---|---|---|
| Akresh, Lucchetti, & Thirumurthy, 2012 | Ethiopian–Eritrean war | 2002 | HAZ: -0.42 SD |
| Bundervoet et al., 2009 | Burundi | 1994-98 | HAZ: -0.53 SD |
| Duque, 2017 | Colombia | 1997-2007 | *For children exposed to terrorist attacks in utero and in childhood:*<br>HAZ: -0.09 SD |
| Kim 2019 | Uganda Lord's Resistance Army (LRA) insurgency | War: 1986-2006<br>Data: 2011 | *For each conflict in a village, and for children born after the conflict:*<br>HAZ: not significant<br>WAZ: -0.0028 SD<br>WHZ: -0.0066 SD |
| Minoiu & Shemyakina, 2014 | Côte d'Ivoire | 2002-07 | HAZ: -0.25 to -0.41 SD |
| Tsujimoto & Kijima, 2020 | Mali | 1990-94 | *In utero exposure to conflict:*<br>HAZ: -0.574SD<br>WHZ: not significant<br>*After-birth exposure:*<br>HAZ: not significant<br>WHZ: not significant |

*Appendix Table 2: Multiple hypothesis correction for subgroup analyses results presented by armed group presence, for child health outcomes (Main manuscript Table 5)*

| **Outcome** | **Conflict variable** | **Subgroup** | **Coefficient** | **Standard error** | **p-value** | **p-value (Westfall and Young)** | **p-value (Bonferroni)** |
|---|---|---|---|---|---|---|---|
| HAZ | Conflict in utero | No armed presence | -0.237 | 0.103 | 0.026 | 0.817 | 1.000 |
| HAZ | Conflict in y1 | No armed presence | 0.195 | 0.094 | 0.044 | 0.889 | 1.000 |
| HAZ | Conflict in y2 | No armed presence | -0.066 | 0.127 | 0.607 | 1.000 | 1.000 |
| HAZ | Conflict in y3 | No armed presence | 0.117 | 0.140 | 0.410 | 1.000 | 1.000 |
| HAZ | Conflict in y4 | No armed presence | -0.140 | 0.228 | 0.540 | 1.000 | 1.000 |
| HAZ | Conflict in y5 | No armed presence | -0.444 | 0.438 | 0.315 | 1.000 | 1.000 |
| HAZ | Conflict in utero | Intermittent presence | -0.073 | 0.022 | 0.001 | 0.362 | 0.062 |
| HAZ | Conflict in y1 | Intermittent presence | 0.000 | 0.025 | 0.989 | 1.000 | 1.000 |
| HAZ | Conflict in y2 | Intermittent presence | -0.074 | 0.024 | 0.002 | 0.467 | 0.131 |
| HAZ | Conflict in y3 | Intermittent presence | 0.003 | 0.034 | 0.938 | 1.000 | 1.000 |
| HAZ | Conflict in y4 | Intermittent presence | 0.009 | 0.046 | 0.837 | 1.000 | 1.000 |
| HAZ | Conflict in y5 | Intermittent presence | 0.023 | 0.062 | 0.712 | 1.000 | 1.000 |
| HAZ | Conflict in utero | Permanent presence | -0.036 | 0.052 | 0.500 | 1.000 | 1.000 |
| HAZ | Conflict in y1 | Permanent presence | 0.070 | 0.051 | 0.177 | 0.992 | 1.000 |
| HAZ | Conflict in y2 | Permanent presence | -0.113 | 0.082 | 0.175 | 0.992 | 1.000 |
| HAZ | Conflict in y3 | Permanent presence | 0.101 | 0.095 | 0.300 | 1.000 | 1.000 |
| HAZ | Conflict in y4 | Permanent presence | -0.029 | 0.075 | 0.701 | 1.000 | 1.000 |
| HAZ | Conflict in y5 | Permanent presence | 0.031 | 0.080 | 0.700 | 1.000 | 1.000 |
| WAZ | Conflict in utero | No armed presence | -0.194 | 0.168 | 0.255 | 1.000 | 1.000 |
| WAZ | Conflict in y1 | No armed presence | 0.236 | 0.076 | 0.003 | 0.492 | 0.167 |
| WAZ | Conflict in y2 | No armed presence | -0.059 | 0.139 | 0.673 | 1.000 | 1.000 |
| WAZ | Conflict in y3 | No armed presence | 0.096 | 0.125 | 0.448 | 1.000 | 1.000 |
| WAZ | Conflict in y5 | No armed presence | -0.194 | 0.279 | 0.489 | 1.000 | 1.000 |
| WAZ | Conflict in y5 | No armed presence | -0.745 | 0.389 | 0.061 | 0.924 | 1.000 |
| WAZ | Conflict in utero | Intermittent presence | -0.055 | 0.021 | 0.011 | 0.675 | 0.530 |
| WAZ | Conflict in y1 | Intermittent presence | 0.022 | 0.021 | 0.296 | 1.000 | 1.000 |
| WAZ | Conflict in y2 | Intermittent presence | -0.048 | 0.017 | 0.007 | 0.597 | 0.333 |
| WAZ | Conflict in y3 | Intermittent presence | 0.024 | 0.023 | 0.292 | 1.000 | 1.000 |
| WAZ | Conflict in y5 | Intermittent presence | 0.060 | 0.036 | 0.098 | 0.962 | 1.000 |
| WAZ | Conflict in y5 | Intermittent presence | -0.020 | 0.042 | 0.645 | 1.000 | 1.000 |
| WAZ | Conflict in utero | Permanent presence | -0.096 | 0.047 | 0.051 | 0.902 | 1.000 |
| WAZ | Conflict in y1 | Permanent presence | 0.024 | 0.041 | 0.564 | 1.000 | 1.000 |
| WAZ | Conflict in y2 | Permanent presence | 0.051 | 0.059 | 0.400 | 1.000 | 1.000 |
| WAZ | Conflict in y3 | Permanent presence | 0.030 | 0.045 | 0.513 | 1.000 | 1.000 |
| WAZ | Conflict in y5 | Permanent presence | -0.042 | 0.057 | 0.468 | 1.000 | 1.000 |
| WAZ | Conflict in y5 | Permanent presence | 0.065 | 0.083 | 0.443 | 1.000 | 1.000 |
| WHZ | Conflict in utero | No armed presence | -0.040 | 0.163 | 0.806 | 1.000 | 1.000 |
| WHZ | Conflict in y1 | No armed presence | 0.179 | 0.111 | 0.112 | 0.968 | 1.000 |
| WHZ | Conflict in y2 | No armed presence | 0.016 | 0.077 | 0.841 | 1.000 | 1.000 |
| WHZ | Conflict in y3 | No armed presence | 0.070 | 0.106 | 0.509 | 1.000 | 1.000 |
| WHZ | Conflict in y4 | No armed presence | -0.151 | 0.223 | 0.501 | 1.000 | 1.000 |
| WHZ | Conflict in y5 | No armed presence | -0.772 | 0.373 | 0.043 | 0.889 | 1.000 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| WHZ | Conflict in utero | Intermittent presence | -0.003 | 0.020 | 0.868 | 1.000 | 1.000 |
| WHZ | Conflict in y1 | Intermittent presence | 0.024 | 0.016 | 0.144 | 0.986 | 1.000 |
| WHZ | Conflict in y2 | Intermittent presence | -0.005 | 0.017 | 0.759 | 1.000 | 1.000 |
| WHZ | Conflict in y3 | Intermittent presence | 0.049 | 0.020 | 0.014 | 0.706 | 0.675 |
| WHZ | Conflict in y4 | Intermittent presence | 0.062 | 0.026 | 0.017 | 0.753 | 0.839 |
| WHZ | Conflict in y5 | Intermittent presence | -0.064 | 0.037 | 0.080 | 0.947 | 1.000 |
| WHZ | Conflict in utero | Permanent presence | -0.080 | 0.063 | 0.212 | 0.998 | 1.000 |
| WHZ | Conflict in y1 | Permanent presence | -0.015 | 0.028 | 0.602 | 1.000 | 1.000 |
| WHZ | Conflict in y2 | Permanent presence | 0.096 | 0.095 | 0.321 | 1.000 | 1.000 |
| WHZ | Conflict in y3 | Permanent presence | 0.034 | 0.079 | 0.669 | 1.000 | 1.000 |
| WHZ | Conflict in y4 | Permanent presence | -0.059 | 0.040 | 0.149 | 0.986 | 1.000 |
| WHZ | Conflict in y5 | Permanent presence | 0.029 | 0.068 | 0.668 | 1.000 | 1.000 |

*Appendix Table 3: Multiple hypothesis correction for mechanisms - overall and subgroup results (Main manuscript Table 4 and 6)*

**Part A: mechanisms, overall**

| Outcome | Conflict variable | Subgroup | Coefficient | Standard error | p-value | p-value (Westfall and Young) | p-value (Bonferroni) |
|---|---|---|---|---|---|---|---|
| Alcohol use during pregnancy | Conflict in utero | All | 0.011 | 0.006 | 0.091 | 0.323 | 0.363 |
| Accessed prenatal care | Conflict in utero | All | -0.018 | 0.008 | 0.020 | 0.194 | 0.121 |
| Birth assisted by professional | Conflict in utero | All | -0.017 | 0.009 | 0.043 | 0.236 | 0.216 |
| Low birth weight (<2500g) | Conflict in utero | All | 0.003 | 0.005 | 0.494 | 0.754 | 1.000 |
| Breastfeeding (months) | Conflict in utero | All | 0.188 | 0.209 | 0.370 | 0.749 | 1.000 |
| Fully vaccinated | Conflict in utero | All | -0.006 | 0.011 | 0.582 | 0.754 | 1.000 |

**Part B: mechanisms, subgroup**

| Outcome | Conflict variable | Subgroup | Coefficient | Standard error | p-value | p-value (Westfall and Young) | p-value (Bonferroni) |
|---|---|---|---|---|---|---|---|
| Accessed prenatal care | Conflict in utero | No armed presence | 0.048 | 0.036 | 0.189 | 0.841 | 1.000 |
| Accessed prenatal care | Conflict in utero | Intermittent presence | -0.025 | 0.007 | 0.001 | 0.179 | 0.009 |
| Accessed prenatal care | Conflict in utero | Permanent presence | -0.001 | 0.008 | 0.867 | 0.988 | 1.000 |
| Alcohol use during pregnancy | Conflict in utero | No armed presence | 0.006 | 0.039 | 0.868 | 0.988 | 1.000 |
| Alcohol use during pregnancy | Conflict in utero | Intermittent presence | 0.005 | 0.007 | 0.536 | 0.964 | 1.000 |
| Alcohol use during pregnancy | Conflict in utero | Permanent presence | 0.029 | 0.010 | 0.009 | 0.378 | 0.064 |
| Birth assisted by professional | Conflict in utero | No armed presence | -0.046 | 0.051 | 0.368 | 0.924 | 1.000 |
| Birth assisted by professional | Conflict in utero | Intermittent presence | -0.024 | 0.008 | 0.002 | 0.231 | 0.017 |
| Birth assisted by professional | Conflict in utero | Permanent presence | 0.007 | 0.013 | 0.575 | 0.964 | 1.000 |