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The Impact of Civil Conflict on Child Health: Evidence from Colombia

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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 during pregnancy 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). 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.

Key words: Conflict, Violence, Child health

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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 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. 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. Whilst Duque argues that this could be due to the protracted nature of the Colombian conflict, these differences could also be driven in large part by limitations of the data used in her study, which is not nationally representative, referring mostly to urban municipalities.

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 contribute to the literature in several 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, we explore the potential heterogeneity in conflict effects along relevant population subgroups: urban vs. rural households and different child age groups. Third, 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 across municipalities with different persistence of the conflict over the study period. We define the extent of conflict persistence as the permanent, intermittent or lack of presence of armed groups, thus allowing us to explore further and explicitly the relationships between the protracted nature of the Colombian conflict violence and child health.

Fourth, our research adds to the existing literature by examining the mechanisms through which conflict-related violence influences health outcomes. 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 and expectant mothers at risk of preventable diseases. Our dataset allows an investigation of the role played by many of

these indirect pathways for conflict effects on health. 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). 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.

As a final contribution, we use a dataset that is nationally representative and spans a longer time period (2000-2010) than those used in previous work in Colombia and elsewhere, 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.

We find a harmful effect of exposure to conflict violence during pregnancy for HAZ and WAZ, both in the full sample and even more strongly in the rural sample. 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 assisted birth 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.

[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.

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 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. We then examine conflict intensity impacts across these three municipality subgroups, to explore the heterogeneity of impacts according to the level of persistence of violence over time to which mothers and children were exposed in each municipality.[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.

[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.

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 first 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 birth month) and for the child's first year (12 months from birth). 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. 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 (i.e. highest level of conflict intensity) of the distribution, and zero otherwise. We then assign to each child three binary variables depending on whether there was high conflict intensity in the calendar year of birth.

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 primary econometric specification is the following:

$$Y_{ijt} = \alpha_j + \delta_t + \tau_k conflict_{ijt} + \beta \boldsymbol{X}_{ijt} + \varepsilon_{ijt}\,, \quad [1]$$

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 $conflict_{ijt}$ is the intensity of conflict to which the mother was exposed during the 9 months before child birth. $\boldsymbol{X}_{ijt}$ represents a vector of child, mother and household covariates. Finally, α_j is the municipality fixed effect and δ_t the cohort fixed effect. For each outcome and definition of conflict intensity, 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] As a first subgroup analysis, we constrain the sample only to those children who were at least 24 months old at the time of the survey, to examine the long term, cumulative effects of conflict on anthropometric indicators. In further analyses, we split the full sample by rural and urban subgroups, expecting

[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.

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 conduct analyses for subgroups of municipalities based on the persistence of conflict, defined as the presence of armed groups between 2000 and 2012 (no presence, intermittent presence or permanent presence).

We conduct a number of robustness checks (as detailed below), including using alternative definitions of conflict intensity, as well as accounting for the potential remaining endogeneity of conflict exposure by using mother fixed effects and instrumental variables.

3.1 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). 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[5], a binary variable indicating whether a skilled attendant (doctor or midwife) was present at birth, a binary variable indicating whether the child received the full childhood vaccination schedule, as well as the child's birth weight (in grams) and the duration of breast feeding (in months).

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,

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

and the square of age, allowing for a non-linear relationship[6]. 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 Robustness checks

We conduct four sets of robustness and specification checks. First, we explore the timing of the effects of conflict intensity on the various outcomes, by including in our main specification an additional conflict variable representing the cumulative number of conflict events that occurred in the municipality in the first 12 months of the child's life. Second, 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. 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 third 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

[6] 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.

6888 children, from 3291 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.

A final consideration is the possibility that our main models do not fully account for unobserved, time-varying differences across municipalities, which may be correlated with changes in both conflict intensity and child health outcomes. Examples include economic shocks leading to shortage of food or shortages of health care supply that differ across municipalities with different conflict intensities. To address this, as a fourth robustness check, we pursue an instrumental variables (IV) strategy for the possibly endogenous conflict intensity measures. We exploit the fact that armed groups in Colombia (both paramilitaries and guerrillas) often relied on financing from the cocaine trade; thus, municipalities that are located in areas relatively more suitable to coca cultivation may experience higher conflict violence (Dube and Vargas 2013). However, using the actual quantities of coca produced as an IV would be problematic as this is influenced by the anti-narco trafficking measures carried out by the government (e.g. spraying of coca crops), which in turn are influenced by conflict intensity, making such an instrument violate the exclusion restrictions. Instead, we construct a measure that captures the suitability to coca cultivation of the area where a municipality is located, considering that optimal growth of the plant is a function of elevation, humidity, precipitation and temperature. Combining digitized satellite images (capturing elevation and slope), information on humidity, temperature and precipitation, as well as forest density, we constructed a coca suitability index ranging between 1 and 4, which we geo-linked to municipalities based on their centroids. We classify the municipalities with an index of 3 or 4 as high coca suitability (see the Appendix for further details on the construction of this variable). We perform the IV analysis for our main conflict intensity variable (conflict events during pregnancy) and control for all potential confounders and cohort effects specified before, except for the municipality fixed effects, since the coca suitability measure only varies by municipality and not by time. Hence in the resulting IV analysis, we rely on cross sectional variation only (by municipality).[7]

[7] While incorporating coca prices could add some heterogeneity over time, this could also introduce endogeneity due to prices driven by conflict levels (Dube and Vargas 2013). We therefore choose not to use price information.

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, covariates and the IV 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; 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, 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.

Table 2 presents the regression results for HAZ, WAZ and WHZ outcomes, for the full sample, using conflict events during pregnancy as the explanatory variable. The OLS analysis reports a seemingly beneficial effect of conflict exposure during pregnancy 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 during pregnancy is uncovered for HAZ and WAZ. For HAZ, the estimated effect in the full sample of an increase in conflict exposure by one event per 1000 during pregnancy is a reduction of HAZ of 0.06. For WAZ and WHZ, the harmful effects of conflict exposure during pregnancy hold in the sample of children who were older than 2 years at the time of the survey. On the other hand, for the binary outcome measures of stunting, wasting and

underweight, we do not observe any effects that are statistically significant at conventional levels.

Turning to an inspection of the potential mechanisms through which conflict violence may affect child health (Table 3), 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. Specifically, 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, but we do not find statistically significant effects on birth weight or the duration of breastfeeding. 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 no effects of conflict exposure during the child's first year of life on vaccination coverage.

We conduct our subgroup analyses (Table 4) for the main health outcomes and for the intermediate outcomes for which we found a statistically significant effect in the main estimations. We find that for HAZ and WAZ, the negative conflict impacts were stronger among children born in rural communities. 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 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 municipalities with no armed group presence. By contrast, for alcohol consumption, we find the highest impact of conflict intensity in municipalities with permanent presence of armed groups.

We conducted four sets of robustness checks for the main health outcome measures and for the intermediate outcomes for which we found a significant impact in the main analyses. In the first set (Table 5), we add to the models a conflict intensity variable defined as the cumulative number of conflict events that occurred in the municipality during the first year

after the birth of the child, in addition to our original measure of conflict events that occurred during pregnancy. For the intermediate outcome variables (in particular antenatal care and alcohol use during pregnancy), we conduct this analysis as a form of placebo test, as we do not expect exposure to conflict violence in the child's first year of life to have an impact on these intermediate variables that were measured during pregnancy. Our overall finding is consistent with the main analysis: the deleterious effect of conflict on anthropometric measures and intermediate outcomes is driven by conflict events that take place during pregnancy, and not through events that happen during the first year of the child's life. For the mechanisms, as expected, we do not find statistically significant impacts of conflict events in the first year of the child, thus increasing our confidence in the model specification.

In the second set of robustness checks (Table 6), we use the 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 the ones from the main analysis).

In the third set of robustness checks (Table 9), we apply fixed effects regressions using mother fixed effects, and contrast them to the original analysis that used municipality fixed effects, in the main samples. Due to the substantially reduced sample size (from 22805 to 6881) resulting from using information only for children with siblings, and the corresponding reduced variability in conflict intensity, in this specification the standard errors approximately double in magnitude compared to the main analyses. Although this reduction in the precision of estimates leads to conflict effect coefficients that are not statistically significant at conventional levels for HAZ and WAZ, the coefficient point estimates are practically unchanged compared to the main findings. For the skilled birth attendance variable, the mother FE analysis yields a statistically significant estimate that is larger in

absolute value than the main estimate (approximately three percentage points, compared to approximately two percentage points).

Finally, the last column of Table 9 shows the results of applying IV estimation (two-stage least squares – 2SLS) in the main sample, for the main health outcomes and three intermediate outcomes. The cluster-robust F-statistic from the first stage regression is 5.85 for the health outcomes and the skilled birth attendance variable, and 5.71 for the other two intermediate outcomes (due to the slightly smaller sample size). When contrasting estimates obtained for the OLS, FE and 2SLS specifications, we find that the sign and magnitude of the estimated effects are similar to what is found in the FE regressions. The IV estimates fail to reach statistical significance however, which can be explained by the relatively low F-statistics reported in the first stage – indicating relatively weak performance of the coca suitability instrument in our context – and much inflated standard errors of the coefficients compared to the main FE models.

5. 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, the estimated effect of an increase in conflict exposure by one event per 1000 during pregnancy was a reduction of HAZ of 0.06 (0.054 SD). This result is in line with the estimated magnitude of the effect on HAZ (0.09 SD) of a mother experiencing a massacre during pregnancy, found by Duque (2017). Yet, the magnitude of our HAZ estimate is small 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 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 0.073/28=0.003 SDs, which coincides with the lower bound of the estimate reported by Kim (2019).

A potential explanation for the smaller conflict effects on anthropometric measures that we find, compared to previous studies, 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 analysis: 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).

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 violence exposure 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).

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).

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 proved to be a relatively weak instrument for our conflict intensity indicator to permit drawing any strong conclusions.[8]

[8] 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).

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).

While other studies evaluating the health impact of conflict violence in Colombia (e.g. Urdinola 2004, Camacho 2008, Duque 2017) 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.

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 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).

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Tables

Table 1. Descriptive statistics of the estimation sample

Conflict exposure variables	**N**	**mean**	**sd**	**min**	**max**
Conflict events during pregnancy	22805	0.38	0.71	0	10.64
Conflict events during 1st year of child	22805	0.44	0.85	0	12.75
Conflict events in year before birth	22805	0.53	0.95	0	13.56
Conflict events in year of birth	22805	0.48	0.93	0	13.56
Conflict events in year after birth	22805	0.42	0.82	0	12.75
High conflict in year before birth	22805	0.24	0.43	0	1
High conflict in year of birth	22805	0.22	0.42	0	1
High conflict in year after birth	22805	0.19	0.39	0	1
Main outcomes					
Height for age z score	22805	-0.72	1.10	-5.88	5.85
Weight for age z score	22805	-0.45	1.10	-4.85	5.63
Weight for height z score	22805	0.05	0.98	-3.97	5.90
Stunting (zscore_ha < -2)	22805	0.11	0.32	0	1
Wasting (zscore_wa < -2)	22805	0.01	0.11	0	1
Underweight (zscore_wh < -2)	22805	0.07	0.25	0	1
Intermediate outcomes					
Breastfeeding duration (months)	21050	12.3	9.4	0	59
Accessed prenatal care	18956	0.94	0.24	0	1
Alcohol use during pregnancy	18956	0.09	0.29	0	1
Birth weight (grams)	16409	3250.4	595.1	500	7000
Low birth weight (<2500g)	16409	0.07	0.26	0	1
Covariates					
Population (thousand)	22805	625.3	1615.5	2.3	7363.8
Child age in months	22805	28.7	17.2	0	59
Child sex	22805	0.489	0.500	0	1
Birth order (1st)	22805	0.351	0.477	0	1
Birth order (2nd)	22805	0.278	0.448	0	1
Birth order (3rd)	22805	0.166	0.372	0	1
Birth order (4th)	22805	0.087	0.282	0	1

Birth order (5th +)	22805	0.118	0.322	0	1
Mother's age at child birth	22805	27.85	6.91	13	49
Mothers education (years)	22805	7.89	4.09	0	23
Rural household	22805	0.344	0.475	0	1
Asset index quintile 1	22805	0.372	0.483	0	1
Asset index quintile 2	22805	0.322	0.467	0	1
Asset index quintile 3	22805	0.136	0.342	0	1
Asset index quintile 4	22805	0.085	0.279	0	1
Asset index quintile 5	22805	0.086	0.280	0	1
Birth cohort: 2000	22805	0.066	0.249	0	1
Birth cohort: 2001	22805	0.081	0.273	0	1
Birth cohort: 2002	22805	0.085	0.278	0	1
Birth cohort: 2003	22805	0.089	0.285	0	1
Birth cohort: 2004	22805	0.098	0.297	0	1
Birth cohort: 2005	22805	0.075	0.263	0	1
Birth cohort: 2006	22805	0.109	0.311	0	1
Birth cohort: 2007	22805	0.108	0.311	0	1
Birth cohort: 2008	22805	0.117	0.321	0	1
Birth cohort: 2009	22805	0.123	0.328	0	1
Birth cohort: 2010	22805	0.050	0.219	0	1
Years in current residence	22805	27.44	14.95	2	45
Instrumental variable					
Suitability of municipality to coca cultivation	22805	0.555	0.496	0	1

Table 2. Regression results for Z-score outcomes - the effect of exposure to conflict events during pregnancy

Health outcomes	**Conflict variable**	**OLS**	**FE**	**FE >24m**
Height-for-age Z score	Conflict events during pregnancy	0.0513**	-0.0588***	-0.0124
		(0.0219)	(0.0207)	(0.0238)
Weight-for-age Z score	Conflict events during pregnancy	0.0033	-0.0492***	-0.0403**
		(0.0145)	(0.0171)	(0.0196)
Weight-for-height Z score	Conflict events during pregnancy	-0.0391***	-0.0098	-0.0430***
		(0.0120)	(0.0171)	(0.0158)
Stunting	Conflict events during pregnancy	-0.0148***	0.0051	0.0002
		(0.0048)	(0.0051)	(0.0069)
Wasting	Conflict events during pregnancy	0.0020*	-0.0013	-0.0015
		(0.0012)	(0.0027)	(0.0021)
Underweight	Conflict events during pregnancy	-0.0050	0.0006	0.0036
		(0.0040)	(0.0049)	(0.0062)
Observations		22,805	22,805	13,143

Notes: OLS: ordinary least squares, FE: fixed effects, >24m: child older than 24months at the time of survey. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Regression results for intermediate outcomes – the effect of exposure to conflict events during pregnancy

Intermediate outcomes	**Conflict variable**	**OLS**	**FE**	**FE >24m**
Alcohol use during pregnancy	Conflict events during pregnancy	0.0064	0.0112*	0.0195**
		(0.0051)	(0.0065)	(0.0079)
Antenatal care	Conflict events during pregnancy	-0.0098	-0.0178**	-0.0182**
		(0.0079)	(0.0076)	(0.0079)
Observations		18,956	18,956	9,548
Assisted delivery	Conflict events during pregnancy	-0.0207*	-0.0174**	-0.0177
		(0.0121)	(0.0086)	(0.0110)
Observations		22,805	22,805	13,143
Birth weight	Conflict events during pregnancy	33.7234***	2.9510	3.2271
		(9.4178)	(11.1280)	(15.1445)
Low birth weight	Conflict events during pregnancy	-0.0037	0.0036	0.0016
		(0.0032)	(0.0051)	(0.0062)
Observations		16,409	16,409	8,915
Breastfeeding (months)	Conflict events during 1st year of life	-0.0815	0.1198	0.0130
		(0.1134)	(0.1447)	(0.2267)
Fully vaccinated	Conflict events during 1st year of life	0.0005	0.0097	0.0077
		(0.0118)	(0.0106)	(0.0087)
Observations		21,050	21,050	11,495

Notes: OLS: ordinary least squares, FE: fixed effects, rural: rural household, urban: urban household, >24m: child older than 24months at the time of survey.
*** p<0.01, ** p<0.05, * p<0.1

Table 4 Subgroup analysis by persistence of conflict (presence of armed groups) and rural/urban communities

Outcomes	Conflict variable	FE overall	FE rural	FE urban	FE no armed presence	FE intermittent presence	FE permanent presence
Health outcomes							
Height-for-age Z score	Conflict events during pregnancy	-0.0588***	-0.0888***	-0.0329	-0.1852*	-0.0779***	-0.0157
		(0.0207)	(0.0264)	(0.0226)	(0.1049)	(0.0221)	(0.0256)
Weight-for-age Z score	Conflict events during pregnancy	-0.0492***	-0.0732***	-0.0290	-0.1699	-0.0532***	-0.0351
		(0.0171)	(0.0249)	(0.0252)	(0.1303)	(0.0189)	(0.0339)
Weight-for-height Z score	Conflict events during pregnancy	-0.0098	-0.0163	0.0008	-0.0485	-0.0002	-0.0273
		(0.0171)	(0.0221)	(0.0321)	(0.1258)	(0.0198)	(0.0324)
Observations		22,805	7,849	14,956	1,868	14,157	6,780
Intermediate outcomes							
Alcohol use during pregnancy	Conflict events during pregnancy	0.0112*	0.0037	0.0207***	0.0061	0.0048	0.0289***
		(0.0065)	(0.0082)	(0.0080)	(0.0387)	(0.0075)	(0.0104)
Antenatal care	Conflict events during pregnancy	-0.0178**	-0.0238*	-0.0102	0.0500	-0.0250***	-0.0013
		(0.0076)	(0.0124)	(0.0066)	(0.0358)	(0.0074)	(0.0079)
Observations		18,956	6,095	12,861	1,518	11,629	5,809
Assisted birth	Conflict events during pregnancy	-0.0174**	-0.0126	-0.0153**	-0.0486	-0.0240***	0.0075
		(0.0086)	(0.0121)	(0.0063)	(0.0512)	(0.0077)	(0.0132)
Observations		22,805	7,849	14,956	1,868	14,157	6,780

Notes: OLS: ordinary least squares, FE: fixed effects, rural: rural household, urban: urban household, *** p<0.01, ** p<0.05, * p<0.1

Table 5. Robustness check 1: the effect of exposure to conflict events during pregnancy and in the first year after birth

Outcomes	**Conflict variable**	**OLS**	**FE**	**FE >24m**
Health outcomes				
Height-for-age Z score	Conflict events during pregnancy	-0.0017	-0.0546***	-0.0259
		(0.0188)	(0.0187)	(0.0195)
	Conflict events in first year	0.0538**	-0.0079	0.0298
		(0.0229)	(0.0225)	(0.0191)
Weight-for-age Z score	Conflict events during pregnancy	-0.0446**	-0.0613***	-0.0537**
		(0.0205)	(0.0208)	(0.0211)
	Conflict events in first year	0.0486**	0.0226	0.0296
		(0.0216)	(0.0203)	(0.0188)
Weight-for-height Z score	Conflict events during pregnancy	-0.0553***	-0.0292	-0.0510***
		(0.0160)	(0.0179)	(0.0188)
	Conflict events in first year	0.0165	0.0364**	0.0176
		(0.0130)	(0.0171)	(0.0198)
Observations		22,805	22,805	13,143
Intermediate outcomes				
Alcohol use during pregnancy	Conflict events during pregnancy	0.0044	0.0080	0.0153*
		(0.0049)	(0.0063)	(0.0079)
	Conflict events in first year	0.0021	0.0057	0.0088
		(0.0050)	(0.0048)	(0.0068)
Antenatal care	Conflict events during pregnancy	-0.0091	-0.0151**	-0.0216***
		(0.0073)	(0.0071)	(0.0069)
	Conflict events in first year	-0.0008	-0.0048	0.0070
		(0.0051)	(0.0059)	(0.0055)
Observations		18,956	18,956	9,548
Assisted birth	Conflict events during pregnancy	-0.0144	-0.0145	-0.0169
		(0.0135)	(0.0105)	(0.0127)
	Conflict events in first year	-0.0064	-0.0054	-0.0017
Observations		22,805	22,805	13,143

Notes: OLS: ordinary least squares, FE: fixed effects, rural: rural household, urban: urban household, >24m: child older than 24months at the time of survey.
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Robustness check 2: the effect of being born in a high conflict intensity municipality

Outcomes	**Conflict variable**	**OLS**	**FE**	**FE >24m**
Health outcomes				
Height-for-age Z score	High conflict in year of birth	0.0950**	-0.0590**	-0.0538
		(0.0369)	(0.0289)	(0.0355)
Weight-for-age Z score	High conflict in year of birth	0.0110	-0.0625*	-0.0421
		(0.0282)	(0.0327)	(0.0349)
Weight-for-height Z score	High conflict in year of birth	-0.0692***	-0.0347	-0.0195
		(0.0267)	(0.0344)	(0.0378)
Observations		22,805	22,805	13,143
Intermediate outcomes				
Alcohol use during pregnancy	High conflict in year of birth	0.0126	0.0133	0.0178
		(0.0101)	(0.0088)	(0.0118)
Antenatal care	High conflict in year of birth	-0.0076	-0.0113	-0.0149
		(0.0105)	(0.0088)	(0.0111)
Observations		18,956	18,956	9,548
Assisted birth	High conflict in year of birth	-0.0221	-0.0181*	-0.0099
		(0.0174)	(0.0097)	(0.0113)
Observations		22,805	22,805	13,143

Notes: OLS: ordinary least squares, FE: fixed effects, >24m: child older than 24months at the time of survey. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Robustness check 3-4: Mother fixed effects and instrumental variables regressions

Outcomes	**OLS**	**FE (municipality)**	**FE (mother)**	**2SLS**
Health outcomes				
Height-for-age Z score	0.0513**	-0.0588***	-0.0577	-0.0794
	(0.0219)	(0.0207)	(0.0399)	(0.2848)
Weight-for-age Z score	0.0033	-0.0492***	-0.0495	-0.0904
	(0.0145)	(0.0171)	(0.0308)	(0.1690)
Weight-for-height Z score	-0.0391***	-0.0098	0.0039	-0.0473
	(0.0120)	(0.0171)	(0.0284)	(0.1552)
Observations	22,805	22,805	6,881	22,805
Intermediate outcomes				
Alcohol use during pregnancy	0.0064	0.0112*	NA	0.0459
	(0.0051)	(0.0065)		(0.0598)
Antenatal care	-0.0098	-0.0178**	NA	-0.0090
	(0.0079)	(0.0076)		(0.0537)
Observations	18,956	18,956	NA	18,956
Assisted birth	-0.0207*	-0.0174**	-0.0277**	-0.0406
	(0.0121)	(0.0086)	(0.0132)	(0.0832)
Observations	22,805	22,805	6,881	22,805

Notes: OLS: ordinary least squares, FE: fixed effects, 2SLS: Two stage least squares. NA: Alcohol use during pregnancy and antenatal care use only available for the last pregnancy, precluding a mother FE analysis. *** p<0.01, ** p<0.05, * p<0.1

Figures

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

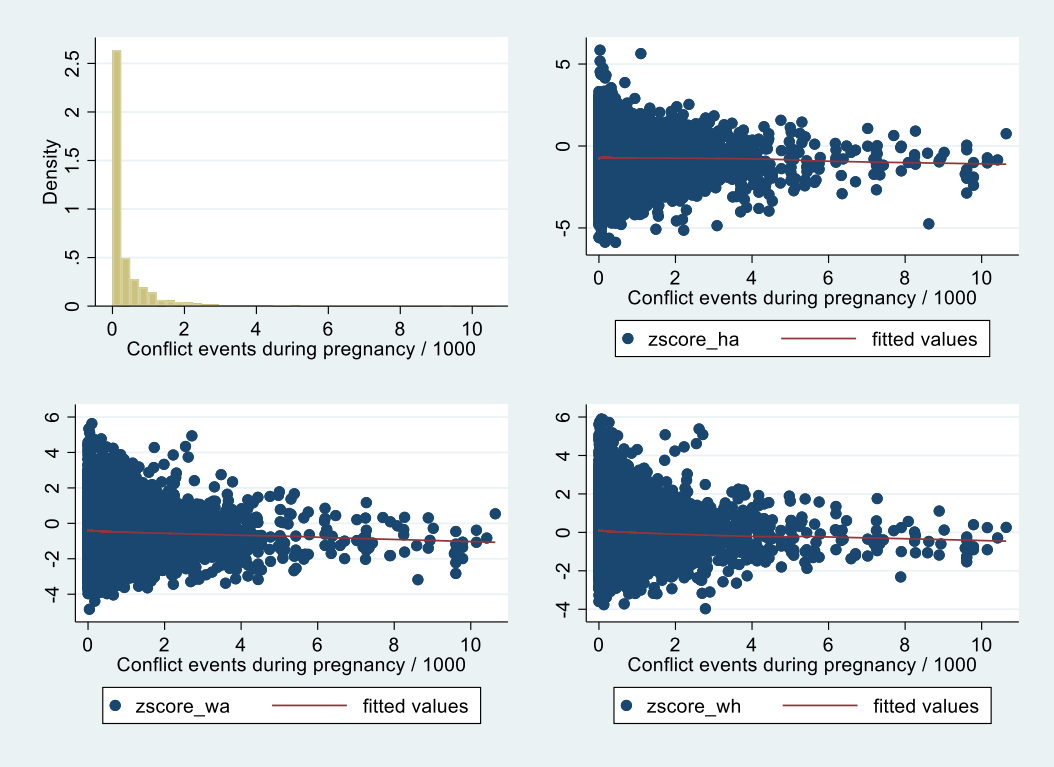


Figure 2 Conflict experienced at the time of birth (solid line: total conflict events during pregnancy, per 1000 population dashed line: high conflict intensity (top quintile) municipality in the year of birth)

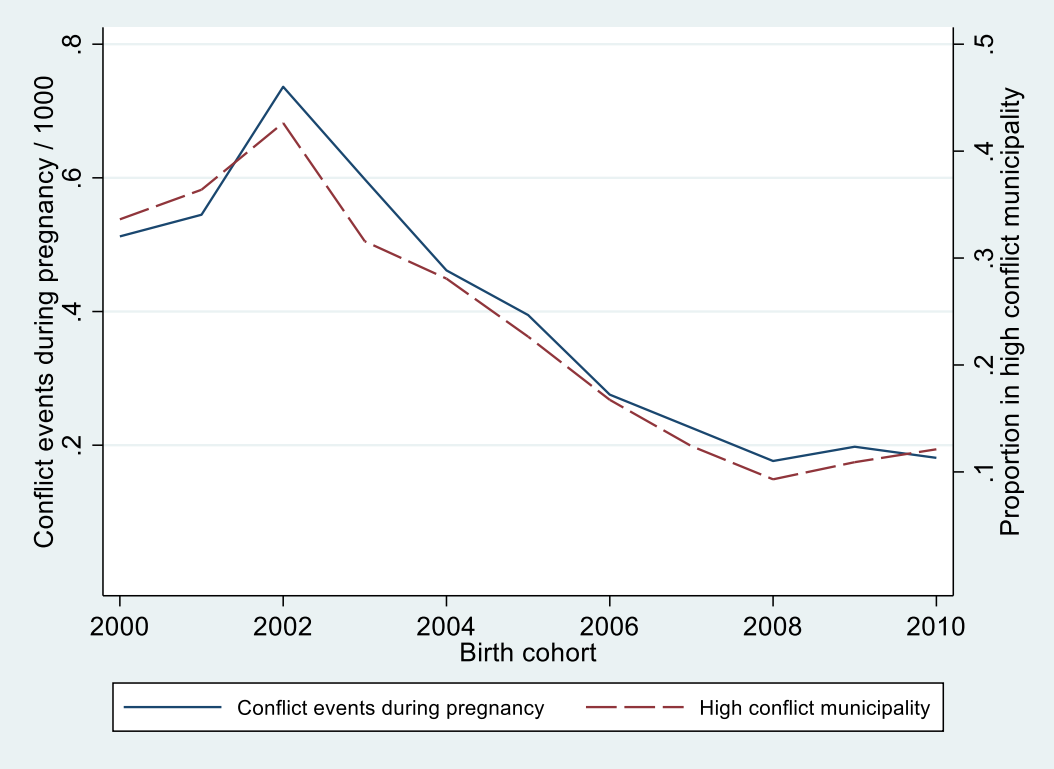
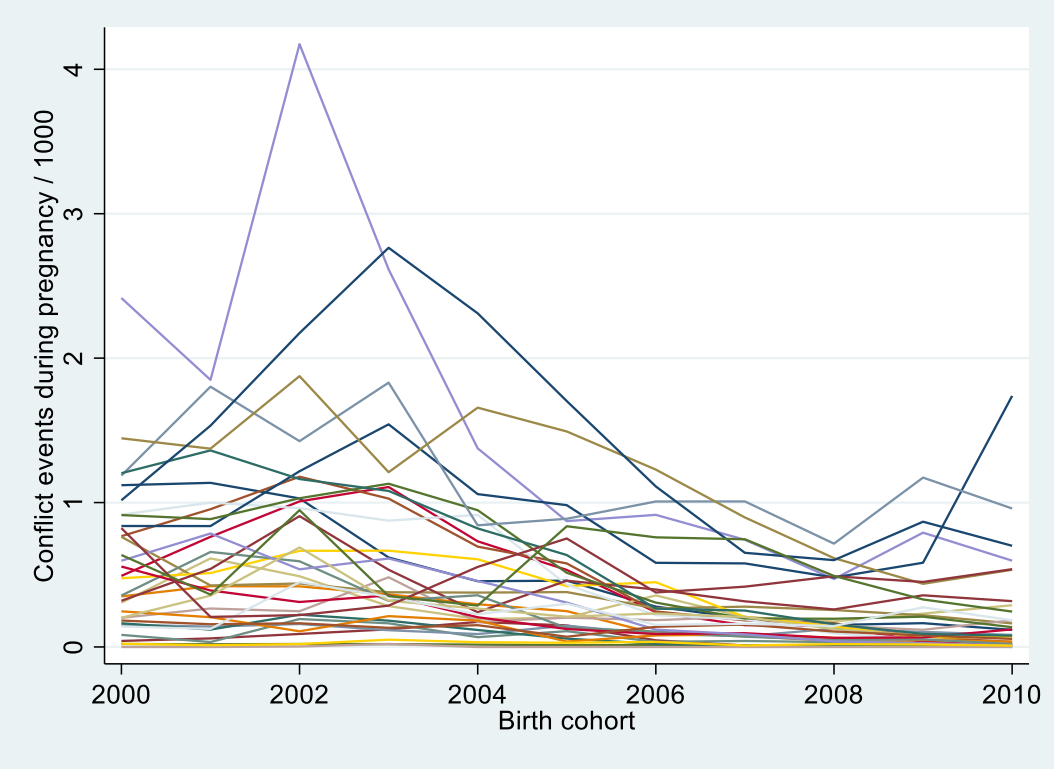


Figure 3 Conflict intensity (conflict events during pregnancy) by birth cohort, aggregated by departamento



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