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Children of the Revolution: Fetal and Child Health amidst Violent Civil Conflict

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Abstract

This paper considers the impact of exposure to civil conflict on health inputs and outcomes from conception to age five, using the recent Maoist insurgency in Nepal as a case study. Conflict intensity is measured by the number of conflict deaths by district and month and merged with pregnancy histories from the 2001 and 2006 Demographic and Health Surveys. Within-mother estimates show that civil conflict increases the likelihood of miscarriage, so that exposure to conflict *in utero* has not only a scarring effect but also a selection effect on survivors, most likely due to a combination of maternal stress and malnutrition.

JEL codes: I10, J13, O15

Keywords: civil conflict, child health, fetal loss, Nepal

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1 Introduction

More than half of all countries have experienced at least one episode of civil conflict since 1950, but the body of knowledge on the causes and consequences of conflict is still limited (Blattman & Miguel 2010).

The exposure of children or their pregnant mothers to conflict can have deleterious effects on child health for a number of reasons, such as: direct effect of violence; malnutrition due to the destruction of sources of income and crops; poorer access to health care due to travel restrictions, destruction of infrastructure, and lower income; psychological distress; increased disease prevalence; and reduced investments in (health) human capital due to a reduction in life expectancy.

There is burgeoning microeconometric evidence of an adverse impact of violent conflict on health, and on child health in particular (Alderman, Hoddinott & Kinsey (2006); Camacho (2008); Bundervoet et al. (2009); Akbulut-Yuksel (2009); Akresh, Lucchetti & Thirumurthy (2010); Mansour & Rees (2011)). Health shocks before birth and in early life have persistent effects into adulthood, they can alter the accumulation of education, and worsen adult socioeconomic outcomes (Almond (2006); Strauss & Thomas (2008); Glewwe & Miguel (2008); Lindeboom, Portrait & Van den Berg (2010)). Countries affected by civil conflict may therefore bear its cost long after the fighting is over if child health is adversely affected.

A complicating factor in the study of the impact of health shocks is that such shocks may not only have an (adverse) scarring effect on those affected, but also lead to increased rates of mortality, and therefore to positive selection of survivors (see Bozzoli, Deaton & Quintana-Domeque (2009) for a formal exposition of the offsetting effects of scarring and selection on childhood mortality). In this paper, I explore the little studied hypothesis that there may be selection on mortality *in utero*. The possibility of increased fetal loss during times of war has been suggested before (e.g., Wynn & Wynn (1993); Rajab, Mohammad & Mustafa (2000); Roseboom et al. (2001)). But to the best of my knowledge this is the first study showing direct, arguably causal, evidence of the effect of conflict on fetal loss.

Fetal loss is by no means a negligible phenomenon: in clinical studies in developed countries, 12-15 percent of clinical pregnancies (i.e., more than 6 weeks after the last menstrual period) end in a miscarriage, much more if pre-clinical pregnancies are included, while typically less than one percent end in a still birth (Garcia-Enguidanos et al. (2002); Cnattingius, Haglund & Kramer (1998); Cai & Feng (2005); Nepomnaschy et al. (2006)). The *in utero* mortality rate is much larger than mortality rates after birth and so open the possibility of substantial selection. In addition, the fetal health threshold at which a pregnancy results in a live birth may be higher at times of civil conflict, e.g. because maternal health deteriorates or if access to antenatal care is reduced.[1] Previous research has found that maternal psychological stress and malnutrition can increase fetal loss as well as the likelihood of adverse birth outcomes for children carried to term (Herrmann et al. (2001); Siega-Riz et al. (2001); Mulder et al. (2002); Cai & Feng (2005); Camacho (2008); Helgstrand & Andersen (2005); Nepomnaschy et al. (2006); Maconochie et al. (2007); Wisborg et al. (2008)). This paper provides evidence that conflict increases the likelihood of miscarriage, and that exposure to conflict *in utero* has not only a scarring effect but also a selection effect on children carried to term, most likely due to a combination of maternal stress and malnutrition.

The experience of Nepal provides a particularly interesting case study. The country suffered prolonged conflict (1996-2006) but in contrast to other studies, the conflict was of moderate intensity.[2] For this reason and also due to the efforts of the Informal Sector Service Center (INSEC), exceptionally detailed conflict intensity data are available, namely deaths counts per district and month over the whole conflict period. In addition, one of the main limitations of studies investigating the impact of conflict, conflict-induced migration, is minimized in the case of Nepal where less than one percent of the population is estimated to have been displaced by the conflict (US Agency for International Development 2007). Finally, rarely available data on pregnancies not ending in a live birth are available from the 2001 and 2006 Demographic and Health Surveys of Nepal for the entire conflict period, so that it is possible to study the impact of conflict exposure *in utero* on pregnancy resolution (live birth, miscarriage, still birth).

Section 2 reviews the relevant literature. Section 3 depicts the conflict background, Section 4 presents the data and identification strategy, Section 5 reports the results, Section 6 checks the robustness of these findings, Section 7 explores underlying mechanisms, and Section 8 concludes.

2 Literature Review

Alderman, Hoddinott & Kinsey (2006) found that the number of months of exposure (since birth) to the Zimbabwean civil war had a negative effect on child height-for-age, which in turn negatively affected child education outcomes. A similar conclusion on the effect of conflict on child height-for-age was reached in two studies exploiting the variation over time and space in exposure to conflict in Burundi (Bundervoet et al. 2009) and in

[1] A similar point is made by Almond & Currie (forthcoming) about survival into adulthood with respect to disease outbreaks during childhood.

[2] The case studies analyzed in previous research tend to focus on conflicts of extremely high intensity, such as the bombing of Germany by allied forces during WWII (Akbulut-Yuksel 2009), the recent civil war in Burundi, where 200,000 people died (Bundervoet et al. 2009), and the Ethiopia-Eritrea war, where between 70,000 and 100,000 individuals lost their lives (Akresh, Lucchetti & Thirumurthy 2010). In Nepal, just over 13,000 casualties were recorded over the 10-year conflict.

Ethiopia and Eritrea (Akresh, Lucchetti & Thirumurthy 2010).

Two recent papers have also used a difference-in-difference approach to estimate the impact of conflict on birth weight. Camacho (2008) found that, in Colombia, landmine explosions taking place in the mother's municipality during the first trimester of pregnancy had a small negative impact on birth weight. Mansour & Rees (2011) similarly found that first-trimester conflict casualties increased the probability of low birth weight during the second Intifada in the Palestinian West Bank. Both findings are robust to the inclusion of maternal fixed effects, and both sets of authors argue that maternal stress is the most likely explanation for their findings.

The biomedical literature indeed suggests that maternal stress during pregnancy is likely to affect fetal and newborn health. The effect of maternal stress in animals on birth outcomes is well established (Mulder et al. 2002). The findings from (non-experimental) studies on humans are less consensual, but there is consistent biomedical evidence of the adverse effect of maternal stress on *pre-term delivery* (Austin & Leader 2000), while the effect on other pregnancy outcomes such as birth weight is less robust (Austin & Leader (2000); Mulder et al. (2002)). Less research has investigated the impact of maternal stress on pregnancy resolution. Rajab, Mohammad & Mustafa (2000) compare the incidence of spontaneous abortions in the 5 years before and the 5 years after the Gulf War based on hospital records from the main referral hospital of Bahrain, and find an increase in referrals in the post-war period. However, confounding factors that increase the occurrence of fetal loss and/or referral to the main hospital in the country over the 10-year period centered around the war cannot be ruled out in this simple before/after identification strategy. Wisborg et al. (2008) have also found that women who report experiencing a high level of psychological stress around 30 weeks of gestation are 80 percent more likely to suffer a still birth.

Since measures of maternal stress are not available in demographic and health surveys, further information on the period of pregnancy during which fetal health is most vulnerable to specific risk factors would help identifying the transmission channels at play.

Few medical studies shed light on whether and how the timing of maternal stress matters. Overall, it would appear that maternal stress has a more pronounced effect on pre-term delivery when mothers are exposed to stress early in pregnancy. Glynn et al. (2001) found that, in their sample of 40 Californian women who were pregnant during the 1994 earthquake, the reduction in gestational length increased the earlier in the pregnancy the earthquake took place. The biological transmission of maternal stress to the fetus is not yet fully understood (Kramer et al. 2009). The main mechanism suggested in the literature is an increased placental Corticotrophin-Releasing Hormone (CRH) around weeks 30-33 of gestation, which has been shown to predict pre-term delivery (Mulder et al. (2002); Sandman et al. (2006); Wadhwa et al. (2004)). The link between maternal stress and heightened CRH levels in the early third trimester has only been established for stress experienced up to early- to mid- second trimester.[3] Taken together, these results suggest that stress is most likely to affect the probability of miscarriage, stillbirth or other adverse birth outcomes when experienced up to mid-pregnancy.

On the contrary, evidence on the impact of maternal malnutrition on fetal and infant health tends to suggest that the latter is most detrimental the later it occurs in pregnancy.[4] In particular, Stein & Susser (1975), Roseboom et al. (2001), and Painter, Roseboom & Bleker (2005) find that exposure to the 1944-1945 Dutch famine in the third trimester of gestation decreased weight and length at birth, and increased the likelihood of infant mortality, while the effect of exposure during the first and second trimester are smaller, which Roseboom et al. (2001) suggest may be due to increased fetal loss. A few studies have analyzed the effect of maternal malnutrition on miscarriage, and suggested that malnourished mothers are more likely to experience fetal loss, but the evidence is mostly suggestive. [5]

A related, and growing body of research is concerned with understanding the extent to which adverse influences on maternal health are transmitted through to their offspring, whether they have occurred over the course of the mother's life before conception, or during the course of gestation. The influential "fetal programming" hypothesis, which originally linked fetal undernutrition in middle to late gestation to later coronary heart disease (Barker 1995) has paved the way to a number of studies of different pathways through which an unhealthy fetal environment during gestation - and famine in particular - can affect health and socioeconomic outcomes all the way to adult life (Barker (1995); Roseboom et al. (2001); Painter, Roseboom & Bleker (2005); Almond (2006); Maccini & Yang (2009); Meng & Qian (2009); Almond et al. (2010); Almond & Currie (forthcoming)).

Another strand of literature which the present analysis speaks to is that concerned with the effects of economic shocks on fetal and child health, which have been found to be either pro-cyclical or counter-cyclical, depending on the population and economic shock analyzed (Dehejia & Lleras-Muney (2004); Miller & Urdinola (2010)). The key themes of

[3]Hobel et al. (1999) and Sandman et al. (2006) find maternal stress at respectively 18-20 and 15 weeks of gestation to be correlated with placental CRH levels in the early third trimester.

[4]In a notable exception, Almond & Mazumder (forthcoming) recently find that *in utero* exposure to maternal fasting during the holy month of ramadan decreases birth weight most when it occurs during the first two trimesters.

[5]Wynn & Wynn (1993) offers graphical evidence of a sharp decrease in fertility and birth weight as well as of an increase in miscarriages in a number of German cities in the immediate post-war, which the authors attribute mostly to food shortages. Cai & Feng (2005) find that the famine caused by the Great Leap Forward increased fetal loss. These authors use Chinese data on retrospective fertility histories to estimate logistic regressions of miscarriage and still birth on a set of controls including a continuous year variable and dummies for the years where crises occurred, namely 1958-62 and 1966-68. The coefficients on the interaction between the year dummies and socioeconomic characteristics are then used to support the authors' claim that the year effects are linked to the famine of 1959-1961 and "the most intense year" of the Chinese Cultural Revolution, 1967 (Cai & Feng (2005), p.310).

this literature can be readily extended to civil conflict: if different types of mothers are more or less likely to postpone or experience difficulties in child bearing during periods of political instability, then this should influence the health of children conceived and born at times of civil strife, in a direction that is undetermined *a priori*. And if fetal and child health inputs are time-intensive, then a slowing down of the economic activity due to conflict may have a positive effect on fetal and child health, which could compensate for the loss of income (as in Miller & Urdinola (2010)).

To sum up, based on the previous literature, it can be expected that: (i) exposure to armed conflict after birth is likely to negatively affect child nutritional status; (ii) maternal stress during early to mid-pregnancy should worsen fetal health, with possible consequences both in terms of fetal wastage and in terms of newborn health; (iii) maternal malnutrition during pregnancy will adversely affect child health, especially when it occurs later in pregnancy; (iv) if armed conflict has an impact on maternal employment, this could have an effect of either sign on child health depending on the relative importance of income and maternal time in the relevant health production function.

3 Civil Conflict in Nepal

Nepal became a parliamentary monarchy in 1990. Despite the multiparty democratic elections that followed, a Maoist insurgency broke out in 1996, only to end in 2006. The insurgency started in February 1996 in the Rolpa district. At first, it was concentrated in a few Communist strongholds in Western Nepal, but by the end of the war, conflict-related casualties were recorded in 73 out of the 75 Nepalese districts. The Maoists' presence varied across districts from sporadic attacks to the organization of their own local governments and law courts, resulting in wide geographic variation in conflict deaths (Figure 1). Over the course of the conflict, Maoists attacked government targets such as army barracks, police posts, local government buildings (Do & Iyer 2010). They were also reported to terrorize, loot, abduct, and physically assault civilians (Human Rights Watch (2004); Bohara, Mitchell & Nepal (2006)). On the other hand, government security forces also killed civilians and were accused of using children for spying, torturing, displacing and summarily convicting civilians (Bohara, Mitchell & Nepal 2006).

A crucial moment in the conflict was the ending by the Maoists to a short-lived cease-fire in November 2001. From then on, the government's response intensified dramatically, involving the Royal Nepal Army, leading to an escalation of violence (see Figure 2). Building on opposition to King Gyanendra's authoritative reaction to the prolonged conflict, the Maoists joined forces with some of the country's major political parties, leading to the signature of a peace agreement in November 2006 and the creation of an interim government led by a power-sharing coalition including the Maoists. This put an end to a 10-year long conflict that led to the deaths of over 13,000 people and the displacement of an estimated 200,000 (US Agency for International Development 2007).

Several arguments have been put forward to explain the district variation in the intensity of the insurgency, including geography (Murshed & Gates (2005); Bohara, Mitchell & Nepal (2006); Do & Iyer (2010)), poverty (Murshed & Gates (2005); Do & Iyer (2010)), lack of political participation (Bohara, Mitchell & Nepal 2006), and inter-group inequality (Murshed & Gates (2005); Macours (2011)). Given that these variables are likely to also affect fetal and child health, it is important to control for district heterogeneity in the analysis, as discussed in Sections 4.2 and 6.

One would generally expect exposure to conflict to be detrimental to maternal and infant health. However, the predicted effect of the Nepalese conflict on maternal and child health is less clear-cut than in many violent conflict instances. Indeed, the impact of the conflict on the overall economy is unclear, child survival improved dramatically at the national level, and education was seemingly little affected (World Bank (2005); Macours (2011); Valente (2011)). In addition, most commentators agree that, although Maoist insurgents are said to have destroyed more than 1000 rural health posts as collateral damage to the destruction of other government buildings, and of having harassed, racketed, and stolen drugs from health workers, they also appear to have shown some commitment to their stated policy of avoiding disruption of "people's services", and are even said to have helped promote initiatives such as the national immunization programs and vitamin A supplementation and de-worming campaigns, contributed to reduced health staff absenteeism, and set-up their own health services in remote areas under their control (e.g., Beun & Neupane (2003); Devkota (2005); Collins (2006)).

4 Data and Identification Strategy

4.1 Data

Demographic and Health Surveys (DHS) have been carried out in a number of developing countries as part of the Measure DHS project, a worldwide USAID-funded project aimed essentially at providing detailed, reliable information on fertility, family planning, maternal and child health and mortality.

The second and third DHS carried out in Nepal took place in 2001 and 2006, respectively. These collected data from a nationally representative sample of women aged between 15 and 49 (if ever married in the case of the 2001 survey). Respondents were asked about their entire fertility history, including dates of all births and deaths of any liveborn child and dates of end and duration of all other pregnancies.[6] The questionnaires

[6] In these surveys, women were asked to report each of their pregnancies in turn, and, one by one, whether the baby was "born alive, born dead, or lost before birth". If they answered either of the two last options, the respondents were then asked about the month and year the pregnancy ended and its duration. If the child was born alive, I assume the duration of the pregnancy to be 9 months.

contain a number of probes for these, and enumerators were specifically trained to ensure that this information, that is central to the survey, is reliable.[7] These fertility histories are used here to create a panel dataset where mothers are the cross-sectional units and pregnancies the "longitudinal" unit, as in Bhalotra & van Soest (2008).

Due to the retrospective nature of the data, there may be measurement error in the dependent variable. Using Malaysian data from the 1970s and 1980s, Beckett et al. (2001) find that recall error in fertility histories is not an issue for live born children, except for some age heaping (e.g., rounding at one year old for children who die when 11 or 13 months old). As a consequence, I allow for age heaping such that the neonatal mortality indicator switches on for children who were reported to be up to one month old at the age of death.[8] I also address this issue by restricting the analysis to children born no longer than 5 years before the start of the survey.[9]

Data on pregnancies that do not result in a live birth are prone to more measurement error, especially in the form of underreporting (Beckett et al. 2001). By restricting the sample to the five years preceding each survey, underreporting should be as low as possible based on survey data. It is important to impose this stringent recall cut-off in the present analysis because conflict intensity varies substantially from month to month (see Figure 2), and so it is important to minimize measurement error in the dates of start and end of pregnancy.

Different women were interviewed in the 2001 and 2006 DHS surveys. However, the degree of measurement error in the reporting of neonatal deaths and miscarriages can be appraised by comparing the average rates of each of these outcomes obtained for children conceived in a given (Nepali) calendar year, but reported by different mothers 5 years apart (in 2001 and in 2006), as depicted by Figures A-1 and A-2. These graphs show that, for the recent period covered by the data used in this paper, average neonatal mortality and miscarriage rates are reasonably consistent across surveys, especially considering that the rates are based on comparatively small year samples and on different women. It is also worth noting the sharp increase in miscarriages coinciding with the conflict escalation, but the absence of such movement in neonatal mortality.

In addition to the entire fertility histories of all interviewed women, for the subsample of children born up to five years before the interview, the DHS also collected more detailed information on prenatal care, delivery, immunization status, and children height and weight (measured by enumerators using internationally recognized instruments). By pooling the 2001 and 2006 DHS cross-sections, I obtain a sample of children aged five or below at the time of either survey whose dates of birth span the whole period of the conflict, namely 1996-2006.

[7]See MOHP, New Era and Macro International Inc. (2007) for more information.

[8]Strictly speaking, neonatal mortality relates to mortality in the first 4 weeks of life.

[9]In the case of pregnancies that do not end in a live birth, the sample is restricted to pregnancies starting no longer than 5 years and 9 months before the start of the survey.

The individual data from the pooled Nepalese DHS are then merged with the number of conflict deaths per month per district of Nepal compiled by the Informal Sector Service Center (INSEC). INSEC is an independent, well-regarded, human rights NGO based in Kathmandu and with representatives in each of the 75 Nepalese districts, who monitor human rights violations. Data from INSEC has been extensively used in the media, international agencies and government reports, and in a number of academic studies, including Bohara, Mitchell & Nepal (2006) and Do & Iyer (2010). All conflict exposure variables are expressed in deaths per 1000 district inhabitants as per the last pre-conflict census (1991).

The DHS surveys took place either before or after the most intense conflict period, so that data collection was not greatly disrupted. In 2001, six out of 257 sampling units had to be dropped from the sample for security reasons (MOHP, New Era and ORC Macro (2002), p.6). But in 2006, the conflict had much abated, and so data collection could proceed without hindrance.

There are 14,107 pregnancies that started no more than 68 months before the month of interview. I restrict the analysis to singletons, as is standard in the demographic literature (dropping 201 pregnancies). I drop (132) pregnancies starting less than 9 months before the date of interview since, for this time period, only pregnancies that do not end in a live birth are recorded in the data. I also drop (771) pregnancies of women who are visiting the household. Finally, I restrict the analysis to children who were conceived in the place where their mothers were interviewed, in order to limit measurement error in exposure to conflict. This drops 1116 pregnancies, but my findings are not sensitive to including these children (see Section 6). The resulting pregnancies sample size is 11,887. After dropping pregnancies that did not end in a live birth (1041), children who were not born at least a full month before the interview (244), and those whose mothers did not report their (subjective) size at birth (11), the live births sample counts 10,591 children. Among these, prenatal care variables were only collected for the last child born to each mother. Delivery information was collected for all children under 5 years old. Anthropometric data were collected for all children under 5 years old at the time of the survey, with about 3 percent of missing observations (depending on the survey round). Finally, child immunization information was collected for all children under 5 years old who were alive at the time of the survey, but here I restrict the sample to children aged one to five for the immunization analysis because full immunization is normally achieved within 12 months.[10]

A number of health outcomes are considered, namely binary indicators for miscarriage, still birth, size of baby at birth as reported by the mother, neonatal mortality, the

[10] According to WHO guidelines, children are fully immunized when they have received one dose of the tuberculosis vaccine (BCG) and measles vaccine, and three doses of the polio and DPT vaccines. They are normally administered within the first 14 weeks of life except for the measles vaccine which is normally given shortly after the age of 9 months (MOHP, New Era and Macro International Inc. 2007).

standardized score of under-5 children for height-for-age (HAZ), and a binary variable for stunting, which switches on for children with a height-for-age z-score below 2 standard deviations of the reference population.

Miscarriages and still births are studied separately. For each pregnancy, mothers are asked whether the baby was "born alive, born dead, or lost before birth". If they answer that the baby was born dead or lost before birth, mothers are then asked whether they or someone else had done "something to end this pregnancy" (MOHP, New Era and Macro International Inc. 2007). The miscarriage (still birth) variable is equal to one if the mother answers that the baby was "lost before birth" ("born dead") without any action taken to end the pregnancy, and zero if the child was born alive. When the mother answered that some action was taken to end the pregnancy, or when she gave no answer about intent, the miscarriage and still birth indicators are set to missing in order to focus on biological mechanisms.

Although newborn health is difficult to capture with a single variable, birth weight is a commonly used measure. In a country like Nepal, where over 80 percent of babies are born at home (MOHP, New Era and Macro International Inc. 2007), birth weight is unknown for the majority of children. However, the DHS asked mothers to report whether at birth the child was 'very large', 'larger than average', 'average', 'smaller than average' or 'very small' at birth. I use this information to create a 'small baby' dummy equal to one if the child was 'smaller than average' or 'very small', and zero otherwise.

Child nutritional status is captured by children's height-for-age z-scores (HAZ) and a stunting indicator based on HAZ. These are widely used indicators of long-term growth retardation (see the seminal contribution of Martorell & Habicht (1986)).[11]

Summary statistics for the key variables used in the analysis are reported in Table 1. For the purpose of this table, the sample is divided into three groups, based on the district's position in the distribution of total district deaths (per 1000 inhabitants) over the conflict period, as in Figure 1. Fertility is lower in the low conflict-intensity tercile, as illustrated by the lower proportion of children of pregnancy order five and above in this district group. Children born in the low conflict-intensity tercile are more often born in urban areas, and to educated mothers. However, children in the low conflict-intensity tercile are less often born to parents from the more privileged classes (Brahmin and Chhetri), more likely born to somewhat less well-off other Tarai/Madhesi castes and Muslim parents, and less likely born to parents from indigenous groups (Janajati). They are less likely to be small babies at birth and, possibly as a consequence of this, are less often small for their age. Although not reported here for conciseness, their mothers receive more antenatal care and are more likely to receive care from a skilled birth attendant at delivery. However, there is no apparent difference in the probability of

[11] The reference population used to calculate the z-scores is the WHO Child Growth Standards, which is based on healthy children from Brazil, Ghana, India, Norway, Oman and the USA.

neonatal death across conflict-intensity groups, and no clear correlation between overall conflict intensity and the likelihood of miscarriage or still birth. Children from the lowest conflict-intensity tercile are also more often wasted, i.e., have lower weight for height, and do not receive better or worse immunization (not reported). Unless otherwise specified, the magnitude of the estimated effects are interpreted in relation to the change in the outcome of interest when going from the mean conflict exposure in the low conflict-intensity tercile to the high conflict-intensity tercile (e.g., in the case of exposure *in utero*, for a change of $0.011 - 0.002 = 0.009$ monthly average casualties per 1000 inhabitants).

4.2 Identification Strategy

I first estimate linear panel data fixed-effects models of the form:

$$\begin{aligned} Y_{idt} = &\beta_0 + \beta_{pre} pre_{dt} + \beta_{utero} inUtero_{dt} [+\beta_{post} post_{dt}] \\ &+ X'_{idt}\beta_X + Z'_{mdt}\beta_Z + Y'_t\beta_y + D_d + u_{idt} \end{aligned} \tag{1}$$

where Y_{idt} is one of several pregnancy, mortality, or health outcomes of interest for child i born in district d and conceived in month t; pre_{dt} is the *cumulated* number of (district) conflict deaths that have occurred up to the month before the conception of the index child conceived in district d in month t; $inUtero_{dt}$ is the *average monthly* number of casualties during pregnancy (so as to be comparable between pregnancies with different gestational periods); $post_{dt}$ is the average monthly number of conflict deaths occurring between birth and the interview date, which is included for outcomes realized beyond the neonatal period; X_{idt} are pregnancy-specific demographic controls, namely age of mother at conception and its square, pregnancy order indicators (for second, third, fourth, and "five and above"), and 11 calendar month of conception dummies. Z_{mdt} are time-invariant maternal characteristics, namely dummy variables for urban residence, maternal education, and caste. Y_t is a set of year of conception dummies, D_d a set of district fixed effects, and u_{idt} is an error term assumed to be independent between districts but not necessarily within district, and robust to heteroskedasticity of an arbitrary form.

The coefficients of interest are β_{utero} and, where relevant, β_{post}. β_{pre} is not given a causal interpretation, but pre_{dt} is included as a control for potential time-varying unobserved heterogeneity correlated with both changes in district conflict intensity over time and changes in the dependent variable.

When Y_{idt} is the nutritional or immunization status of children at the time of interview, I also include a control for age (in months) at measurement. Nutritional status captured by height-for-age is indeed a cumulative variable, and thus the distance to the healthy child of reference tends to deteriorate as the child gets older. It is therefore preferable to control for age in months at anthropometric measurement where, as is the case here, the treatment variable varies month-by-month and is correlated with age at measurement (due to the intensification of conflict over time). The same logic applies to immunization status, which may be correlated both with conflict intensity and age at interview.

For outcomes that are available for at least two siblings, I also estimate a variant of Equation 1 in which I replace district fixed effects with mother fixed effects (and exclude Z_{mdt}), so as to shed light on the degree to which estimates are due to selection of women who become pregnant or give birth at times of higher conflict intensity. This is the case for variables that are available over the entire fertile life of respondents, namely whether miscarriage or still birth and neonatal mortality, as well as for variables collected for *all* children under 5 years old at the time of survey (size at birth, help with delivery, immunization, and height-for-age), but not for antenatal care variables, which were only collected for the last birth. For clarity, in each mother fixed effects specification, I explicitly restrict the relevant samples to children with at least another sibling in the sample, and refer to this sample as the "siblings" sample. In an intermediate step between the district fixed-effects analysis and the maternal fixed-effects analysis, I estimate district fixed-effects models on the siblings sample in order to appraise the singularity of families who have at least two children in the five years preceding the survey.

Pregnancy-specific characteristics X_{idt} are included in all the main regressions. Pregnancy order and maternal age are indeed well-known determinants of obstetric outcomes, and pregnancy order has been found to have a large effect on parental investments in child quality (Black, Devereux & Salvanes 2005). The inclusion of these control variables should therefore increase the precision of my estimates. In addition, given the structure of the data (pregnancy histories), maternal age and pregnancy order increase over time, and this might bias the estimates of the impact of conflict if not controlled for. Similarly, there may be seasonal effects relevant both for conflict intensity and health outcomes, justifying the inclusion of month of conception dummies. As discussed in Section 6, results are robust to excluding these controls.

5 Results

5.1 Selection into Birth

I consider three types of selection into birth. First, I test the hypothesis that mothers with different observed characteristics may select into becoming pregnant at times of conflict, and find evidence of such selection. Second, I allow for the possibility that parents with different unobserved characteristics select into becoming pregnant at times of conflict. Finally, I estimate the impact of exposure to conflict on pregnancy resolution for a given mother, which I hypothesize to lead to positive selection on fetal health.

In order to shed light on selection on observable parental characteristics, I estimate variants of Equation 1, in which the dependent variable is, in turn, an indicator for wealth (measured by asset ownership quintile), maternal education, urban location, and caste/ethnicity group. Results presented in Table A-1 show that high-caste mothers (Brahmin or Chhetri) and mothers with some university education are comparatively less likely to become pregnant after they have been exposed to more conflict or when they anticipate more intense conflict during their pregnancy. This pattern of selection is consistent with the hypothesis that groups who felt most threatened by the Maoist insurgency postponed having children in reaction to the intensity of violence in their area. These results also suggest that the presence of parental selection is likely to extend to unobservable parental characteristics, and so it is important to account for this selection process when estimating the effect of conflict.

The impact of conflict on pregnancy resolution is investigated in Tables 2 and 3, where I estimate the effect of exposure to violent conflict on the likelihood of miscarriage (Table 2) and still birth (Table 3), against the alternative of a live birth. Each table is arranged as follows. In the first column, I estimate Equation 1 on the whole sample. In Column (2), I restrict the sample to pregnancies with at least another sibling in the relevant time period in order to assess whether families with at least two children conceived within 5 years and nine months of the survey are differently affected by violent conflict compared to the rest of the sample. In Column (3) I present maternal fixed effects estimates, which are robust to selection on time-invariant parental heterogeneity. Columns (4) and (5) repeat the regression in Column (3) but allowing for different effects of *in utero* exposure when it takes place at different stages of the pregnancy.

When selection on unobservable parental characteristics is not controlled for (Columns (1) and (2)), exposure to conflict does not appear to have a significant effect on the probability of miscarriage. However, once I allow exposure to conflict to be correlated with unobservable maternal heterogeneity (Column (3)), results indicate that maternal exposure to conflict during pregnancy significantly increases the probability of miscarriage. The direction of selection is as expected as these results suggest that more reproductively successful women are also more likely to become pregnant at times of more intense conflict. Going from mean *in utero* exposure in the low-intensity district group to the high-intensity group (i.e., an increase of 0.009 monthly average casualties per 1000 inhabitants) leads to an increase in the probability of miscarriage by 0.77 ppts (11.6 percent of the mean).

As would be expected, in the current sample, 90 percent of miscarriages occur in the first five months. The robustness of the estimated effect of exposure to conflict *in utero* on miscarriage can thus be tested by dividing the pregnancy period in two phases, the first one starting in the month of conception (mc) and lasting until $mc+4$, and the second spanning $mc + 5$ to $mc + 9$. The results are presented in Column (4) and confirm that the effect of conflict exposure *in utero* on miscarriage is driven by conflict during the first

5 months following conception. When trying to ask more of the data by splitting the potential pregnancy period in three trimesters (mc to $mc + 3$, $mc + 3$ to $mc + 6$, $mc + 6$ to $mc + 9$), the magnitude of the effect of exposure in the first trimester of pregnancy is the largest (p-value: 0.136), but the results are less clearly interpreted, with a negative coefficient for trimester two and none individually significant.

A similar analysis of the effect of conflict exposure on the probability of still birth (Table 3) shows that exposure to conflict *in utero* leads to a small decrease in the probability of still birth in absolute terms (0.22 ppts when controlling for maternal unobserved heterogeneity). Results in Columns (4) and (5) indicate that the decrease in the probability of still birth is driven by exposure to conflict up to mid-gestation, which is consistent with the interpretation that the probability of still birth decreases due to an increased probability of miscarriage. Contrary to the findings for miscarriage, however, controlling for maternal fixed effects does not affect the findings for exposure *in utero*, suggesting that the determinants of still births are less closely related to female reproductive capacity than determinants of miscarriage. It is interesting to note that the most robust effect of conflict exposure is found for the second trimester, which would suggest that it is those children miscarried due to exposure to conflict in the early to mid-second trimester who would have been at the highest risk of still birth, or that children who survive exposure to conflict in the first trimester to reach full term are more scarred by it than those experiencing the same level of exposure in the second trimester.

The magnitude of the within-district effect of conflict exposure on the probability of miscarriage and still birth doubles and, in the case of still birth, becomes significant, when restricting the sample to mothers with at least two pregnancies in the five years before the survey. These findings suggest that the obstetric outcomes of mothers who have comparatively close pregnancies are more affected by conflict, e.g., due to maternal depletion and more difficulties meeting the family's food and health care needs.

Combining the effect of conflict exposure *in utero* on both miscarriage and still birth suggests "positive" selection on fetal health with a net decrease in the probability of live birth of about 0.7 ppts.[12]

Further, indirect, evidence of the impact of exposure to conflict *in utero* on fetal loss and child health can be obtained by investigating the impact of conflict exposure on the gender of born children. It is well-established that the human male is more fragile than the female (Kraemer 2000). For this reason, a poor health environment around and during pregnancy is expected to decrease the male-to-female sex ratio at birth. A further reason to expect lower male-to-female ratios among mothers in worse health comes from evolutionary theory. Trivers & Willard (1973) have indeed hypothesized that wider

[12]This figure was obtained when estimating the effect of conflict on an overall indicator for pregnancy loss (miscarriage or stillbirth) using Equation 1 with mother fixed effects. Full results are available upon request.

variance in male reproductive success leads mothers in poorer conditions to favor female progeny. Evidence of a sex ratio skewed in favor of girls resulting from adverse maternal health shocks has been found in a range of circumstances (Fukuda et al. (1998); Almond & Mazumder (forthcoming); Almond, Edlund & Palme (2009)). Results reported in Table A-2 confirm that, when maternal unobserved heterogeneity is differenced-out,[13] the probability of a female live birth increases with exposure to conflict *in utero*.

5.2 Effect of Conflict on Child Health Outcomes

5.2.1 New Born Health

In Table 4, I investigate the effect of exposure to conflict on the health of new born babies, as captured by size at birth and neonatal mortality, following the same approach as in Section 5.1. Children exposed to more intense fighting while in the womb tend to enjoy *better* health at and soon after birth. On the contrary, there is a positive correlation between pre-conception, cumulated violence and neonatal mortality corresponding to an increase of 1.8 ppts or 36 percent of the mean, but the causal nature of this very large effect is unclear since it may be capturing long-term trends correlated with conflict intensity rather than a genuine causal effect.

When considering the effect of exposure to conflict during each trimester of pregnancy, exposure to conflict during the second trimester appears to have the largest "positive" selection effect on small size at birth (-1 ppt) and neonatal mortality (-0.2 ppt). Similar to the earlier finding on stillbirth, these results would suggest that babies who did not survive to birth due to a shock experienced in mid-gestation were those who would have been at the highest risk of adverse health outcomes around birth, or that children who survive exposure to conflict in the first trimester to reach full term are more scarred by it than those experiencing the same level of exposure in the second trimester.

Similar to the previous results on pregnancy resolution, restricting the sample to children with at least one sibling in the sample (i.e., comparing Columns (1) and (2)) leads to a sharp increase in the magnitude of the estimated effect of exposure to conflict, which gives further support to the hypothesis that the estimated effect of conflict intensity *in utero* on child health is linked to pregnancy resolution.

5.2.2 Child Height-for-Age

Before investigating the effect of exposure to conflict on child nutritional status using similar specifications as those used in this paper so far, I estimate a "standard" model

[13] For a discussion about the potential biological correlation between maternal characteristics and child gender, see Rosenfeld & Roberts (2004) and Fukuda et al. (2011). In addition, in a country such as Nepal, where sons are generally preferred to daughters, there may be a degree of sex-specific reporting bias such that some mothers are more likely to report the birth of a girl than others.

formulation akin to those used, for instance, by Bundervoet et al. (2009) and Akresh, Lucchetti & Thirumurthy (2010) in some specifications, and where the treatment variable is the number of months of conflict since the birth of the index child, controlling for region and year of birth fixed effects. There is a strong correlation between the number of months of conflict and worse height-for-age, with a decrease of 0.02 standard deviation for each additional month of exposure (Table 5, Column (1)). This figure is surprisingly similar to those reported for identical specifications in Bundervoet et al. (2009) for Burundi (-0.035, Column (3) in their Table 5) and Akresh, Lucchetti & Thirumurthy (2010) for Ethiopia (-0.023, in Column (6) in their Table 2). The findings reported in Bundervoet et al. (2009) and Akresh, Lucchetti & Thirumurthy (2010) hold across a range of alternative robustness checks provided in these papers. However, when including a control for age in months (Column (2)), the effect of the number of months exposed to conflict disappears in my data. As explained in Section 4.2, estimates using the baseline specification of Equation 1 may be misleading because older children (even within a given birth year cohort) are more likely to have low height-for-age due to the cumulative nature of this nutritional indicator.

From Column (3) onwards, conflict exposure variables are based on casualty counts. Monthly average conflict intensity since birth tends to have a negative effect on height for age, but this effect is only robust to controlling for age in months in the maternal fixed effects specification. In this specification, maternal exposure to conflict before conception is negatively and significantly correlated with lower height for age (by 0.09 standard deviations of the reference population for an increase in conflict intensity akin to moving between the first and third terciles of the conflict-intensity district distribution). Exposure to conflict during the child's life leads to a HAZ decrease of 0.05 standard deviations of the reference population for a move between the first and third terciles of the conflict-intensity district distribution. The effect of exposure to conflict *in utero* is not statistically significant, and very small in magnitude (-0.02 s.d. for a move between the first and third conflict terciles).

Similar to findings for pregnancy resolution, the adverse effect of conflict on child nutritional status is larger in magnitude for children with at least one other sibling in the sample (i.e., in Column (5) compared to Column (4)), suggesting that families with more young children were more severely affected by the conflict.

When considering exposure to conflict at different periods of gestation, higher conflict intensity in the second trimester of pregnancy is associated with higher HAZ (+0.017 s.d.), thus echoing findings for neonatal mortality and size at birth.[14] On the contrary, exposure to conflict in the third trimester decreases height-for-age (by 0.030 s.d.), which suggests the presence of a larger scarring effect due to maternal malnutrition (see Section

[14]Including a control for small size at birth only slightly weakens this effect, suggesting that the effect of conflict exposure is not fully mediated by birth weight.

2).

Except for the effect of maternal exposure to conflict before conception, which is insignificant in all specifications, similar findings are obtained when replacing the height-for-age z-score with a binary indicator for having a HAZ below two standard deviations of the reference population median.[15]

Across all health outcomes, the message from these results is that selection dominates for exposure to conflict in the second trimester, while scarring dominates in the third trimester, and the total effect of the two is undistinguishable from zero in the first trimester. It suggests either that those pregnancies miscarried well into the gestational period would have been at highest risk of poor health outcomes (more selection in trimester 2), or that exposure to conflict in the first trimester is more detrimental for children who are carried to term than exposure in the second trimester (more scarring in trimester 1). These results, together with that of increased fetal loss for exposure to conflict up to mid-gestation, could be explained by the effect of conflict on maternal stress. Indeed, the existing literature suggests that only maternal stress experienced up to mid-pregnancy is expected to have an impact on pregnancy resolution, and that stress experienced early in pregnancy has more deleterious effects on newborn health.

In much wealthier and healthier populations,[16] Camacho (2008) and Mansour & Rees (2011) both find that exposure to stressful conflict-related events during the first trimester are most detrimental to birth weight. These authors do not analyze the impact of these events on pregnancy resolution, but there are at least two reasons to expect different conclusions in populations characterized by different levels of development. Firstly, selection processes are more likely to intervene in higher mortality contexts (Bozzoli, Deaton & Quintana-Domeque 2009). Secondly, undernutrition may compound the effect of stress on fetal growth (Cliver et al. 1992).

6 Robustness Checks

The estimation framework proposed here follows a difference-in-difference identification strategy, and as such the main threat to identification is the existence of time-varying omitted factors correlated with the health outcomes of interest as well as conflict exposure. Figure 2 shows that casualties vary drastically from one month to the other within district, both increasing and decreasing sharply, so that contemporary changes in omitted determinants of health are less likely to drive the estimates of the effect of conflict exposure *in utero* than in applications relying on cruder measures of conflict intensity. In

[15] Results are not reported for conciseness, but are available from the author upon request.

[16] For instance, the under-5 stunting rate in Colombia (West Bank) is 12 (8.8) percent, and the neonatal mortality rate is 1.2 (1.35) percent (Ojeda et al. (2005); Palestinian Central Bureau of Statistics (2006)), compared to 55 and 3.5 percent, respectively, in the present data.

addition, I control for cumulated conflict intensity before conception (pre_{dt}), which should capture pre-conception trends correlated with conflict intensity. In this section, I carry out a number of robustness checks which bolster confidence in the findings presented so far.

I first investigate whether there is any evidence of pre-conflict differential trends in fetal and child health by estimating the following equation on the sample of children conceived in the ten years preceding the start of the conflict:

$$Y_{idt} = \gamma_0 + \sum_{j=1}^{5} \gamma_j PreYear_{j,t} \times totaldeaths_d + X'_{idt}\gamma_X + PreYear'_t\gamma_y + D_d + v_{idt} \quad (2)$$

where Y_{idt} is an indicator of fetal or child health available for the entire fertility history of interviewed women (miscarriage, still birth, neonatal and infant mortality), $PreYear_t$ is a set of conception year dummies, $PreYear_{j,t}$ is the year dummy corresponding to the jth pre-conflict year, and $totaldeaths_d$ is the total number of casualties (for 1000 inhabitants) over the course of the entire conflict in district d. X_{idt} and D_d are defined as in Equation 1. If the γ_j are jointly insignificant, and, *a fortiori*, if each γ_j is not statistically different from zero, then we can conclude that there was no pre-conflict trend in the dependent variable systematically correlated with future conflict intensity. Results confirm that all γ_j are statistically insignificant individually as well as being jointly insignificant (the lowest p-value for the joint test is equal to 0.43). Similar tests were performed using the maternal fixed effects estimator, with similar conclusions.[17]

I then check whether findings for the preferred mother fixed effects specifications are robust to a number of alterations, starting with excluding all controls except year fixed effects (and implicit maternal fixed effects). As can be seen in Panel A of Table 6, my previous results are robust to the exclusion of covariates.

I also try including a placebo treatment variable equal to the average monthly number of casualties during the same calendar period as the gestation period, but 12 months earlier. The magnitude and significance level of the coefficients on the conflict variables included in Equation 1 are largely unchanged, and the placebo effects are all insignificant except for small size at birth, where it indicates a correlation between conflict intensity 12 months before gestation and larger size at birth, so that caution should prevail when interpreting the effect of exposure to conflict *in utero* on size at birth as causal (Table 6, Panel B).

I then control for district-specific linear trends, which is bound to reduce the precision with which the effect of conflict exposure is estimated, but should also provide results that are less likely due to omitted, time-varying confounding factors (Table 6, Panel C). Conclusions are unchanged, but two remarks are warranted. First, the marginally

[17] Full results are available from the author upon request.

significant finding that exposure to conflict *in utero* reduces the probability of still birth becomes insignificant. Second, there is a very large increase in the magnitude of the adverse effect of conflict before, during and after pregnancy on height-for-age, but not on the stunting indicator, suggesting the possibility of omitted variables correlated with more intense conflict and nutritional improvements for the non-poor only. This is consistent with the finding that districts experiencing larger increases in inequality experienced more Maoist activity in Macours (2011).

For limited dependent variables, Equation 1 corresponds to the linear probability model (LPM), which does not take into account the nonlinear nature of the dependent variable, but provides more interpretable estimates. It is possible to estimate a nonlinear panel data model with (district or mother) fixed-effects that yields a consistent estimate of the effect of exposure to conflict on the log-odds ratio. However, this conditional logit model does not offer estimates for the fixed effects, and so it is not possible to estimate the partial effect of interest without making arbitrary assumptions on the value of the fixed effects (Wooldridge 2002). For rare events (neonatal mortality, miscarriage and still birth), I also estimated conditional logit models, which confirm the sign and significance of the findings obtained with the linear fixed-effects estimator.[18]

Finally, my conclusions are not altered when I include all children in the sample irrespective of whether or not their mothers already lived in their current place of residence when they became pregnant.[19] This bolsters confidence in the robustness of my findings to conflict-induced migration. While increasing measurement error, including these children addresses the potential issue that some women may have migrated within the district due to conflict. It does not remedy the potential concern that some mothers may have migrated away from their district in response to violent conflict because I do not observe where women have migrated from, and therefore I have to assign conflict exposure on the basis of their district of residence at the time of the survey. Recall however that concerns over conflict-induced migration are less relevant in the case of Nepal than for many other conflict episodes because population displacement was comparatively rare (less than one percent of the population). In addition, recent research investigating the direct effect of violence on migration decisions in one district of Nepal moderately affected by the conflict finds that conflict intensity does not increase the probability of migration outside the district, and that there is a U-shaped relationship for migration inside the district, after controlling for individual socioeconomic and demographic indicators (Bohra-Mishra & Massey 2011).

[18]Results are available from the author upon request.
[19]Results are available from the author upon request.

7 Analysis of Transmission Channels

7.1 Effect of Conflict on Health Care

Security concerns may discourage parents from seeking health care, and can disrupt the functioning of health facilities. In this section, I shed light on the impact of conflict intensity on use of key aspects of antenatal care (antenatal care checkups, iron and folic acid tablets, and anti-tetanus immunization), delivery assistance, and child immunization.

Antenatal care data was only collected for the latest pregnancy of the respondent (if it occurred within 5 years of the survey). Therefore, it is not possible to compare siblings, so that estimates comprise a behavioral response as well as a compositional effect due to potential changes in the characteristics of mothers giving birth at times of conflict. Conflict during pregnancy does not appear to have adversely affected the number of antenatal care visits, nor the likelihood of receiving iron and folic acid tablets or a tetanus toxoid injection (Table A-3). If anything, there is a small, generally statistically insignificant, positive correlation between conflict during pregnancy and the three antenatal care inputs considered. The exception is the effect of first-trimester conflict exposure on the probability of buying or being given iron/folic acid tablets, which is marginally significant, but corresponds only to an increase of 0.5 ppts (1.3 percent) for a move from the first to the third conflict tercile. Exposure to conflict before pregnancy could act as a deterrent to seek or give medical care but here it also captures time-varying omitted variables. It is positively correlated with antenatal care and the probability of receiving a tetanus toxoid injection (moving from the first to the third conflict tercile, they correspond to an increase in 0.11 antenatal checks and of 3.6 ppts in the probability of receiving a tetanus toxoid injection, or a 6 percent increase for both explained variables).

Data on circumstances of delivery, as well as immunization data, are available for all children born within five years of the survey. For these variables I therefore also estimate maternal fixed effects models. Once again, conflict intensity does not appear to have had a negative effect on health seeking behavior overall. As seen in Table A-4 and Table A-5, estimated effects are small in magnitude, and mostly statistically insignificant. Exceptions are, for a move from the first to the third tercile of district conflict intensity: conflict intensity in the first trimester of pregnancy is associated with a 0.4 ppt decrease in the likelihood of giving birth without any assistance (within mother, Table A-4 Column (6)), which could be due to an increase in pregnancy-related problems which in some cases we have seen have led to miscarriages; conflict intensity in the third trimester of pregnancy leads to a 0.3 ppt increase in the likelihood of giving birth without assistance (within mother), which could be due to conflict very near the time of birth discouraging travel of mothers, their family, or birth attendants; children exposed to more conflict *in utero* are somewhat less likely to receive no immunization at all (0.3 ppt from tercile 1 to tercile 3, within-mother, Table A-5 Column (6)); and children whose mothers have experienced more conflict before becoming pregnant are 1.6 ppt less likely to receive no immunization at all (from tercile 1 to tercile 3, within-mother, Column (6)).

The overall lack of adverse effect of violent conflict on health care might seem surprising. However, when respondents were directly asked in the 2006 DHS why they did not deliver in a health facility, security reasons where blamed in only 1 percent of cases. Poorer access to health facilities therefore does not seem to explain the contemporaneous effect of conflict on pregnancy loss, or that of conflict since birth on children's height-for-age. I now test whether the conflict has affected nutritional status indicators in the short run (i.e., within three months of exposure).

7.2 Effect of Conflict on Short-Term Nutritional Status

Nutritional status indicators are measured at the time of the survey. As a consequence, within a single DHS survey, there is no within-mother and very little within-district variation in conflict exposure shortly before measurement. The identification strategy used in this analysis thus differs from the main regressions as it relies on the variation in conflict intensity within-district between DHS surveys.

I consider the impact of cumulated conflict up to the last three months before the survey separately from that of conflict in the last three months. Given the finding that exposure to conflict in early pregnancy has a contemporaneous impact on miscarriage, I am particularly interested in the effect of conflict on maternal nutrition in the short run, although the existing evidence on the effect of malnutrition on miscarriage is weak (see Section 2). Conflict intensity in the three months preceding the survey is not meaningfully correlated with short-term nutritional status of children (weight-for-height) and women (BMI). As shown in Table A-6, there is a positive and statistically significant correlation between conflict exposure up to three months before the survey and the probability of a child being wasted (1.97 ppt), but any estimated effect of recent conflict is statistically insignificant and negligible in magnitude (e.g., the 95 percent confidence interval upper bound for the effect of conflict in the past three months on the probability of being wasted corresponds to an increase of 0.58 ppt, Column (2)).

There is no evidence of short-run nutritional distress due to conflict in the child and non-pregnant female adult populations. This suggests that the negative effect of conflict on long-term nutritional status is more likely due to a low-level, persistent nutritional deficit, than to short-term acute malnutrition. But this evidence does not exclude the possibility that pregnant women in their third trimester of pregnancy, when weight gain should be very rapid, suffer from malnutrition due to contemporaneous conflict.

7.3 Effect of Conflict on Female Labor Activity

Finally, Table A-7 shows that conflict intensity up to three months before the interview is correlated with an increased probability for mothers to be working in the seven days leading to the interview (by 3.4 ppts from tercile 1 to tercile 3, Column (1)), as also found in Menon & Rodgers (2011). Conflict intensity in the three months preceding the survey is however correlated with a lower probability of working (by 0.5 ppts, p-value of 0.102). When restricting the sample to women who report having worked at some point during the last 12 months (Column (3)), I find that women are significantly more likely to be off work in the previous week where there is more conflict in the three months preceding the interview, albeit only by 0.8 ppt (for a move from tercile 1 to tercile 3 of district conflict intensity). Finally, conflict intensity does not appear to have any effect on the probability that mothers work from home.

There is therefore evidence that mothers are somewhat less likely to work in response to recent conflict intensity. If the reasons that discourage mothers from going to work do not apply to health seeking behaviors (e.g., destruction of productive assets), this may mitigate the expected adverse effect of conflict on health care and explain part of the findings in Section 7.1.

8 Conclusion

This paper provides direct evidence of an increased probability of miscarriage due to exposure to conflict in early to mid-pregnancy. More specifically, I estimate that going from the mean conflict exposure in the third of districts with lowest conflict intensity to that prevailing in the third of districts with highest conflict intensity increases the probability of miscarriage by 0.77 ppts (11.6 percent of the mean). To cast this figure in a different light, it corresponds roughly to the increase in the probability of miscarriage when going from the 25th percentile of conflict intensity during pregnancy to the 75th percentile among children exposed to at least some conflict *in utero*. Although the possibility of increased fetal loss during times of war has been suggested in previous work (e.g., Wynn & Wynn (1993); Rajab, Mohammad & Mustafa (2000); Roseboom et al. (2001)), to the best of my knowledge this is the first study showing direct, arguably causal, evidence of this phenomenon.

Exposure to conflict in the first few years of life has an adverse effect on child nutritional status when holding unobserved maternal characteristics constant, at least for the sample of children with a sibling born within five years, who are a large, but more vulnerable, subsample. However, exposure to conflict *in utero* has both scarring and selection effects. This selection effect tends to dominate in the second trimester of pregnancy, for all health outcomes considered (stillbirth, size at birth, neonatal mortality, and height-for-age). On the contrary, scarring effects are stronger in the third trimester, especially for height-for-age, and selection and scarring influences cancel each other out for exposure to conflict in the first trimester.

The available data allow investigating three particular transmission channels directly: use of health care, maternal labor force participation, and short-term malnutrition. Use of antenatal care, (medical) help with delivery, and immunization do not appear to decrease when conflict intensifies. This lack of adverse impact of conflict on health care can be partly explained by the finding that recent conflict reduces female labor force participation. There is no evidence of acute nutritional distress caused by conflict in the three months preceding the survey among the general population, but it is difficult to extrapolate this finding to pregnant women. The biomedical literature suggests that the observed pattern of selection and scarring *in utero* is most likely due to a combination of maternal stress increasing miscarriages when experienced up to mid-pregnancy, stress having more deleterious effects on the health of children carried to term when experienced in the first trimester, and of maternal malnutrition being more detrimental when experienced later in pregnancy.

I also find direct evidence of selection on both observable and unobservable parental characteristics. The adverse effect of conflict is more marked when differencing-out maternal time-invariant heterogeneity, which suggests that more reproductively successful, healthier women are more likely to become pregnant and give birth at times of violent conflict.

Results reported here suggest a potentially large effect of maternal exposure to conflict *before* conception on neonatal mortality, although the causal nature of the observed correlation is unclear. Further research trying to pinpoint the causal effect of maternal conflict exposure before conception would be welcome.

Since conflict cannot be randomized, a difference-in-difference approach is generally used to obtain plausibly causal estimates, as it is the case here. This however comes with the caveat that civil conflict may have consequences for the whole country, so that within-district estimates are likely to underestimate the total effect of conflict. Nevertheless, this study suggests some important policy implications. First, in conflicts of moderate intensity, food aid is beneficial but may be more efficiently targeted if focussing on families with several young children and pregnant women. Second, it stresses the need to consider public health policies aimed at supporting women in conflict situations to deal with the trauma of pregnancy loss. More generally, this analysis adds to the scant body of knowledge on the impact of maternal psychological stress on miscarriage, and calls for research into preventive interventions.

Table 1: Summary Statistics

District Conflict-Intensity Tercile	(1) Low			(2) Intermediate			(3) High		
	mean	sd	count	mean	sd	count	mean	sd	count
Live births sample									
Selected explained variables									
=1 if female	0.508		3892	0.482		3403	0.498		3296
=1 if neonatal death	0.035		3892	0.035		3403	0.035		3296
=1 if baby smaller than average	0.165		3892	0.242		3403	0.239		3296
Height-for-age score	-1.997	1.3784	3548	-2.197	1.3334	3108	-2.204	1.2643	3013
Weight-for-height score	-0.911	1.0540	3548	-0.733	1.0694	3108	-0.767	1.0422	3013
Pregnancy characteristics									
=1 if first pregnancy	0.226		3892	0.210		3403	0.213		3296
=1 if second pregnancy	0.229		3892	0.216		3403	0.227		3296
=1 if third pregnancy	0.193		3892	0.169		3403	0.163		3296
=1 if fourth pregnancy	0.125		3892	0.127		3403	0.127		3296
=1 if fifth pregnancy and higher	0.226		3892	0.278		3403	0.270		3296
Maternal age at conception	24.106	5.9514	3892	24.885	6.3959	3403	24.485	6.3194	3296
Selected Maternal characteristics									
=1 if urban	0.186		3892	0.097		3403	0.119		3296
=1 if no education	0.663		3892	0.716		3403	0.755		3296
=1 if primary education	0.149		3892	0.154		3403	0.140		3296
=1 if secondary education	0.165		3892	0.116		3403	0.097		3296
=1 if higher education	0.023		3892	0.014		3403	0.007		3296
=1 if Dalit	0.170		3892	0.140		3403	0.165		3296
=1 if Brahmin or Chhetri	0.214		3892	0.379		3403	0.423		3296
=1 if Tarai/Madhesi Others	0.256		3892	0.046		3403	0.013		3296
=1 if Newar	0.034		3892	0.032		3403	0.037		3296
=1 if Janajati	0.195		3892	0.395		3403	0.334		3296

(Continued on next page)

(Continued)

	(1)			(2)			(3)		
District Conflict-Intensity Tercile	Low			Intermediate			High		
	mean	sd	count	mean	sd	count	mean	sd	count
=1 if Muslim	0.107		3892	0.005		3403	0.024		3296
=1 if Other Caste	0.025		3892	0.001		3403	0.005		3296
Conflict exposure - district casualties per 1000 inhabitants									
Before conception, total	0.039	0.0783	3892	0.118	0.2124	3403	0.423	0.7514	3296
During pregnancy, monthly average	0.002	0.0033	3892	0.005	0.0086	3403	0.011	0.0207	3296
Since birth, monthly average	0.002	0.0027	3892	0.004	0.0048	3403	0.008	0.0107	3296
During first year of life, total	0.028	0.0416	3892	0.063	0.1013	3403	0.143	0.2555	3296
Up to 3 months before interview, total	0.123	0.1488	3892	0.290	0.3242	3403	0.801	0.9791	3296
Last 3 months, total	0.007	0.0123	3892	0.009	0.0211	3403	0.016	0.0419	3296
Pregnancy sample									
Explained variables									
=1 if miscarriage	0.054		4226	0.048		3652	0.054		3564
=1 if stillbirth	0.013		4047	0.009		3507	0.014		3422

Mean values for each explained variable used in the paper are reported at the bottom of the corresponding results tables. The Nepal DHS contains 96 ethnicity categories. Here they are grouped following Bennett, L. and Ram Dahal, D. and Govindasamy, P. (2008). The miscarriage and still birth indicators are equal to zero for live births, and set to missing if the mother does not answer "No" when asked if any action was taken to end the pregnancy. Source: Author's calculations using Nepal DHS 2001 and DHS 2006.

Table 2: Effect of Conflict on the Probability of Miscarriage

	=1 if Miscarriage				
	(1)	(2)	(3)	(4)	(5)
Before conception	0.001	0.006	0.030	0.026	0.026
	(0.0086)	(0.0139)	(0.0236)	(0.0247)	(0.0239)
During pregnancy	0.203	0.465	0.854**		
	(0.2908)	(0.3790)	(0.3742)		
Conception to conception+4				0.705**	
				(0.2943)	
Conception+5 to conception+9				0.070	
				(0.4212)	
1st trimester					0.548
					(0.3640)
2nd trimester					-0.214
					(0.3094)
3rd trimester					0.443
					(0.3449)
Year FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Pregnancy vars	Yes	Yes	Yes	Yes	Yes
Maternal vars	Yes	Yes	No	No	No
Panel variable	District	District	Mother	Mother	Mother
Observations	11442	6594	6594	6594	6594
No. of Groups	75	74	3060	3060	3060
R-squared	0.0083	0.0126	0.0487	0.0487	0.0487
Mean Y_{idt}	0.052	0.066	0.066	0.066	0.066

All regressions are estimated using the linear fixed-effect estimator and include a constant. Pregnancy characteristics: age of mother at conception and its square, 4 pregnancy order indicators, and 11 calendar month of conception dummies. Maternal characteristics: a binary indicator for urban households, 3 maternal education dummies, and 6 caste/ethnicity indicators. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. * p<0.10, ** p<0.05, *** p<0.01

Table 3: Effect of Conflict on the Probability of Stillbirth

	=1 if Stillbirth				
	(1)	(2)	(3)	(4)	(5)
Before conception	-0.007	-0.013*	-0.007	-0.006	-0.005
	(0.0065)	(0.0077)	(0.0081)	(0.0081)	(0.0075)
During pregnancy	-0.106	-0.221**	-0.249*		
	(0.0872)	(0.1039)	(0.1329)		
Conception to conception+4				-0.234*	
				(0.1223)	
Conception+5 to conception+9				-0.001	
				(0.0530)	
1st trimester					-0.157
					(0.0974)
2nd trimester					-0.112**
					(0.0489)
3rd trimester					0.057
					(0.0494)
Year FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Pregnancy vars	Yes	Yes	Yes	Yes	Yes
Maternal vars	Yes	Yes	No	No	No
Panel variable	District	District	Mother	Mother	Mother
Observations	10976	6066	6066	6066	6066
No. of Groups	75	74	2866	2866	2866
R-squared	0.0091	0.0148	0.0155	0.0158	0.0158
Mean Y_{idt}	0.012	0.015	0.015	0.015	0.015

All regressions are estimated using the linear fixed-effect estimator and include a constant. Pregnancy and maternal characteristics: as listed under Table 2. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Effect of Conflict on Newborn Health

	(1)	(2)	(3)	(4)
Panel A	=1 if Small at Birth			
Before conception	-0.016	-0.022	-0.019	-0.012
	(0.0254)	(0.0317)	(0.0304)	(0.0292)
During pregnancy	-0.227	-0.859	-1.476*	
	(0.5537)	(0.5581)	(0.8827)	
1st trimester				-0.409
				(0.4024)
2nd trimester				-1.207**
				(0.4612)
3rd trimester				0.501
				(0.9075)
R-squared	0.0147	0.0234	0.0217	0.0232
Mean Y_{idt}	0.213	0.219	0.219	0.219
Panel B	=1 if Neonatal Death			
Before conception	0.007	0.015	0.046**	0.041**
	(0.0094)	(0.0142)	(0.0191)	(0.0199)
During pregnancy	-0.149	-0.327*	-0.272	
	(0.1341)	(0.1903)	(0.4475)	
1st trimester				-0.052
				(0.2903)
2nd trimester				-0.259*
				(0.1318)
3rd trimester				-0.122
				(0.3253)
R-squared	0.0094	0.0201	0.0319	0.0323
Mean Y_{idt}	0.035	0.049	0.049	0.049
Panel variable	District	District	Mother	Mother
Maternal vars	Yes	Yes	No	No
Observations	10591	5620	5620	5620
No. of Groups	75	74	2674	2674

All regressions are estimated using the panel data fixed effects estimator and include controls for pregnancy characteristics (as listed under Table 2), year of conception fixed effects, calendar month of conception dummies, and a constant. Maternal Characteristics: as listed under Table 2. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 5: Effect of Conflict on Child Height-for-Age Z-score (HAZ)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict months	-0.020***	0.002					
since birth	(0.0039)	(0.0049)					
Age in months		-0.025***		-0.024***	-0.028***	-0.040***	-0.041***
		(0.0029)		(0.0025)	(0.0028)	(0.0080)	(0.0079)
Before conception			0.008	0.032	-0.020	-0.244**	-0.233**
			(0.0969)	(0.0816)	(0.0866)	(0.1121)	(0.1094)
During pregnancy			-0.709	-0.372	-0.901	-2.341	
			(1.3973)	(1.3875)	(2.2707)	(2.1050)	
Since birth			-6.505*	2.203	-1.859	-8.273**	-7.782*
			(3.3231)	(2.9671)	(3.5669)	(3.8302)	(4.1185)
1st trimester							-1.442
							(1.6181)
2nd trimester							1.861**
							(0.8588)
3rd trimester							-3.287***
							(1.0630)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	Yes	Yes	No	No
Pregnancy vars	No	No	Yes	Yes	Yes	Yes	Yes
Maternal vars	No	No	Yes	Yes	Yes	No	No
Panel variable	District	District	District	District	District	Mother	Mother
Observations	9669	9669	9669	9669	4571	4571	4571
No. of Groups	75	75	75	75	74	2208	2208
R-squared	0.1303	0.1402	0.1700	0.1834	0.1968	0.3207	0.3222
Mean Y_{idt}	-2.126	-2.126	-2.126	-2.126	-2.180	-2.180	-2.180

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All regressions are estimated using the linear fixed-effect estimator and include a constant. Pregnancy and maternal characteristics: as listed under Table 2. There is no within-mother variation in date of interview. Therefore, any within-mother variation in age at interview (in months) is perfectly collinear with variation in month of conception when holding year of conception constant. Month of conception dummies are therefore excluded in the last two columns. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Robustness Checks

	(1) Miscarriage	(2) Still Birth	(3) Female	(4) Neo. Death	(5) Small Baby	(6) HAZ	(7) HAZ<-2
Panel A: No controls							
During	0.696**	-0.164*	1.626**	-0.640	-1.243	0.227	-0.118
pregnancy	(0.3253)	(0.0889)	(0.7574)	(0.4300)	(0.7577)	(2.0039)	(0.6339)
Since birth						-3.454	3.942**
						(5.6459)	(1.5489)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.0019	0.0044	0.0069	0.0093	0.0067	0.3088	0.2198
Panel B: Placebo experiments							
Before	0.033	-0.005	0.064	0.044**	0.011	-0.247**	0.047
conception	(0.0278)	(0.0076)	(0.0580)	(0.0205)	(0.0333)	(0.1168)	(0.0519)
During	0.867**	-0.236*	2.062**	-0.287	-1.276	-2.355	0.377
pregnancy	(0.3621)	(0.1287)	(1.0296)	(0.4426)	(0.8533)	(2.1076)	(0.7607)
During pregnancy	-0.134	-0.087	-0.674	0.100	-1.372***	0.113	-0.414
- 12 months[a]	(0.6593)	(0.0618)	(0.8279)	(0.2879)	(0.4766)	(1.0580)	(0.5799)
Since birth						-8.246**	4.425***
						(3.7971)	(1.0475)
Age in months						-0.040***	0.014***
						(0.0080)	(0.0035)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	No[b]	No[b]
Pregnancy vars	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.0487	0.0155	0.0105	0.0320	0.0230	0.3207	0.2292
Panel C: Controlling for district-specific linear trend							
Before	0.030	-0.004	0.107	0.044*	-0.047	-0.705***	0.134
conception	(0.0365)	(0.0103)	(0.0798)	(0.0262)	(0.0599)	(0.2515)	(0.0870)
During	0.911**	-0.139	2.469**	-0.449	-1.489	-5.438**	0.809

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(Continued)							
pregnancy	(0.4001)	(0.0962)	(1.0338)	(0.5038)	(1.2046)	(2.6913)	(0.9455)
Since birth						-15.525**	5.326***
						(7.1515)	(1.7460)
Age in months						-0.043***	0.014***
						(0.0080)	(0.0036)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	No[b]	No[b]
Pregnancy vars	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.0638	0.0312	0.0288	0.0459	0.0454	0.3560	0.2615
Observations	6594	6066	5620	5620	5620	4571	4571
No. of Groups	3060	2866	2674	2674	2674	2208	2208
Mean Y_{idt}	0.066	0.015	0.516	0.049	0.219	-2.180	0.573

[a] Average monthly number of casualties during the same calendar period as the gestation period, but 12 months earlier. [b] See notes under Table 5. All regressions are estimated using the linear (maternal) fixed-effect estimator and include a constant. Pregnancy characteristics: as listed under Table 2. Maternal characteristics: as listed under Table 2). District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. * p<0.10, ** p<0.05, *** p<0.01

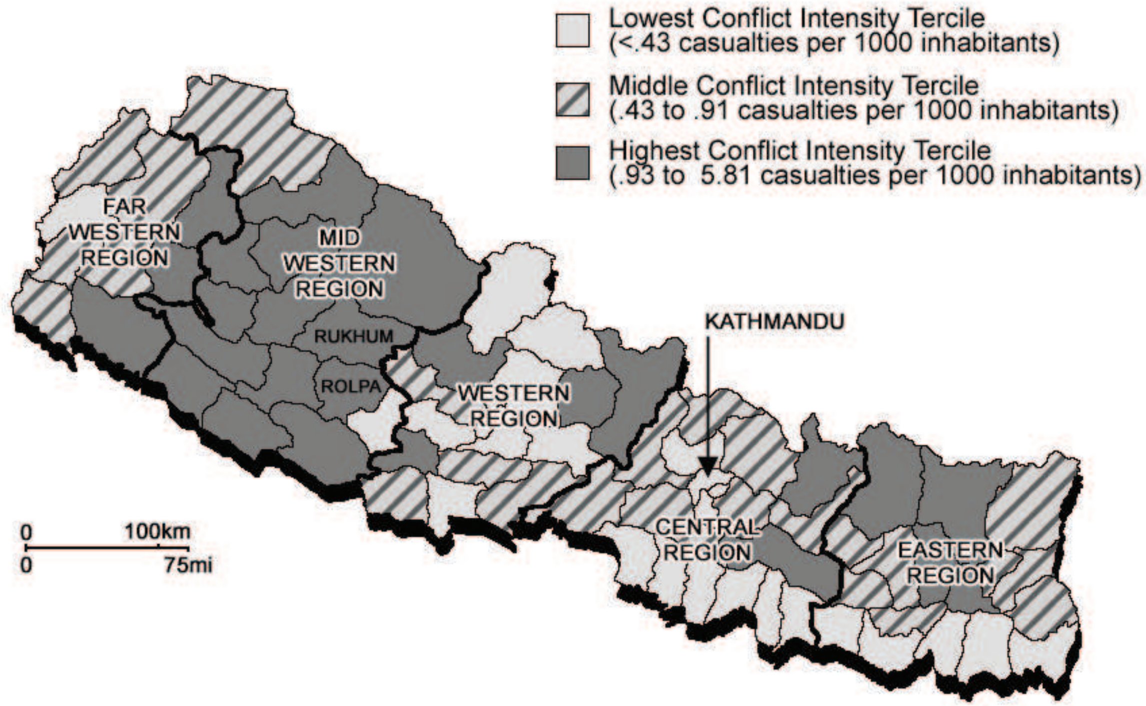

Figure 1: Between-District Variation in Casualties

Source: Author's calculation using data from Informal Sector Service Center (2009).

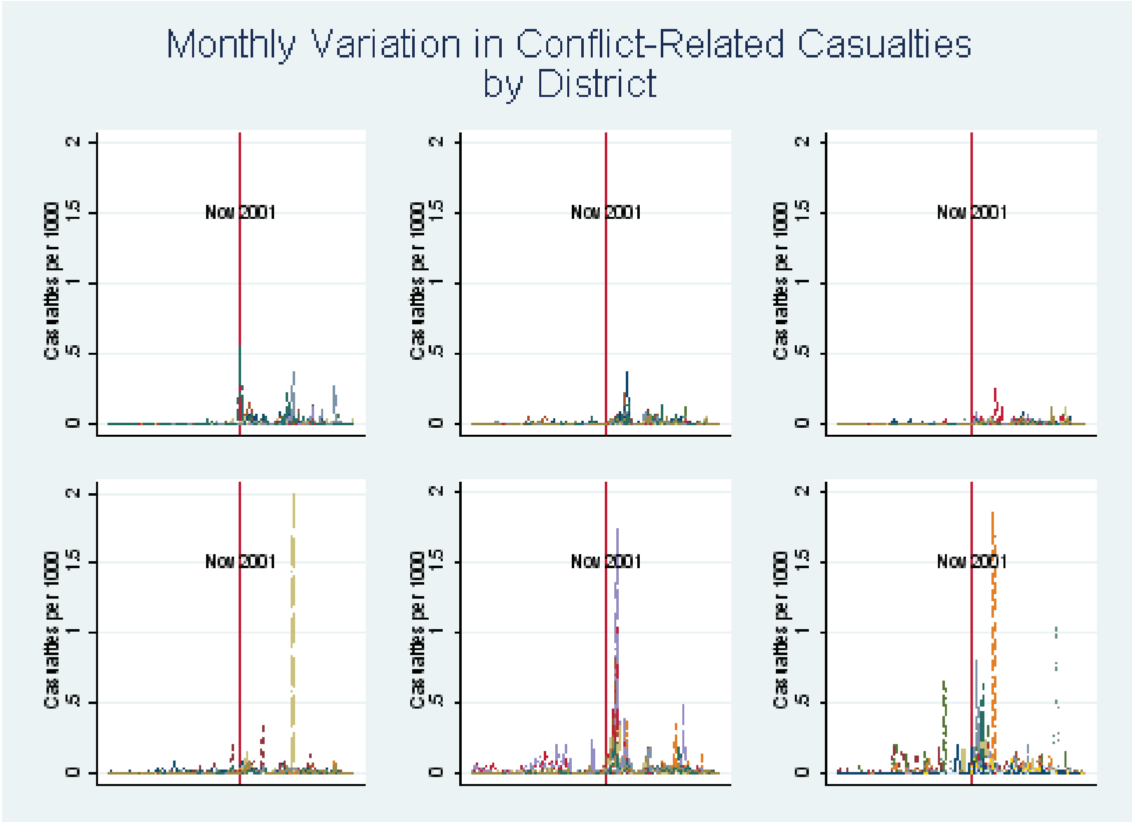

Figure 2: Within-District Variation in Casualties

The x-axis is a time line running from January 1996 to December 2006. November 2001 marks the start of conflict escalation. The 75 Nepalese districts are split in 6 groups and represented in separate diagrams, so that each line in each diagram plots monthly casualties for one of the 75 Nepalese districts over the course of the entire conflict.
Source: Author's calculation using data from Informal Sector Service Center (2009).

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Appendix - For Online Publication Only

Table A-1: Selection on Parental Characteristics

	(1)	(2)	(3)	(4)	(5)		
Wealth Quintile	First (Lowest)	Second	Third	Fourth	Fifth		
Before	0.060	-0.038	0.011	-0.031	-0.003		
conception	(0.0438)	(0.0372)	(0.0270)	(0.0270)	(0.0157)		
During	-0.374	0.348	0.221	0.046	-0.241		
pregnancy	(0.3868)	(0.3752)	(0.3589)	(0.3339)	(0.1996)		
Mean Y_{idt}	0.283	0.216	0.186	0.185	0.131		
	Maternal Education						
	=1 if None	=1 if 1ary	=1 if 2ary	=1 if higher	=1 if Urban		
Before	0.029	0.000	-0.013	-0.016**	0.003		
conception	(0.0221)	(0.0160)	(0.0186)	(0.0062)	(0.0334)		
During	0.091	0.158	-0.116	-0.133	-0.015		
pregnancy	(0.2163)	(0.1785)	(0.1738)	(0.0843)	(0.3027)		
Mean Y_{idt}	0.703	0.149	0.131	0.016	0.141		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Caste/Ethnicity	=1 if Brahmin/ Chhetri	=1 if Tarai/ Madhesi Others	=1 if Dalit	=1 if Newar	=1 if Janajati	=1 if Muslim	=1 if Other Caste
Before	-0.093**	0.025	0.009	0.006	0.045	0.011	-0.005
conception	(0.0401)	(0.0152)	(0.0376)	(0.0089)	(0.0332)	(0.0214)	(0.0058)
During	-0.830**	0.257	0.113	0.222	0.230	0.061	-0.054
pregnancy	(0.3340)	(0.1646)	(0.4040)	(0.1448)	(0.3356)	(0.2251)	(0.0577)
Mean Y_{idt}	0.340	0.111	0.160	0.034	0.296	0.048	0.011
Observations	11887	11887	11887	11887	11887	11887	11887

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(Continued)

All regressions are estimated using the linear (district) fixed-effect estimator and include a constant and include dummies for year of conception, month of conception and controls for pregnancy characteristics listed under Table 2. Binary indicators for wealth quintiles are provided in the DHS based on a principal component analysis of (i) ownership of consumer items such as television, bicycle, car, (ii) dwelling characteristics including source of drinking water, sanitation and type of housing materials. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A-2: Effect of Conflict on the Probability of a Female Birth

	=1 if Female				
	(1)	(2)	(3)	(4)	(5)
Before conception	-0.006	-0.008	0.050	0.049	0.036
	(0.0176)	(0.0208)	(0.0512)	(0.0515)	(0.0480)
During pregnancy	-0.330	-0.144	1.964*		
	(0.5019)	(0.6183)	(0.9949)		
Conception to conception+4				1.239**	
				(0.5049)	
Conception+5 to conception+9				0.723	
				(0.7872)	
1st trimester					1.027*
					(0.5333)
2nd trimester					-0.390
					(0.3322)
3rd trimester					1.007*
					(0.5947)
Year FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Pregnancy vars	Yes	Yes	Yes	Yes	Yes
Maternal vars	Yes	Yes	No	No	No
Panel variable	District	District	Mother	Mother	Mother
Observations	10591	5620	5620	5620	5620
No. of Groups	75	74	2674	2674	2674
R-squared	0.0048	0.0064	0.0103	0.0104	0.0105
Mean Y_{idt}	0.496	0.516	0.516	0.516	0.516

All regressions are estimated using the linear fixed-effect estimator and include a constant. Pregnancy and maternal characteristics: as listed under Table 2. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. The DHS only collected child gender data for live births. * p<0.10, ** p<0.05, *** p<0.01

Table A-3: Effect of Conflict on Antenatal Care

	(1) ANC Checks[a]	(2) ANC Checks[a]	(3) Iron/ Folic A. Tablets[b]	(4) Iron/ Folic A. Tablets[b]	(5) Tetanus Injection[c]	(6) Tetanus Injection[c]
Before	0.280**	0.278**	0.016	0.017	0.094***	0.094***
conception	(0.1219)	(0.1216)	(0.0291)	(0.0292)	(0.0323)	(0.0321)
During	2.421		-0.125		0.582	
pregnancy	(2.3290)		(0.7044)		(0.5896)	
1st trimester		1.589		0.531*		0.382
		(0.9786)		(0.2788)		(0.3018)
2nd trimester		-1.365		-0.714		-0.129
		(1.6295)		(0.4478)		(0.3660)
3rd trimester		2.219		0.217		0.335
		(1.7309)		(0.4175)		(0.3194)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Pregnancy vars	Yes	Yes	Yes	Yes	Yes	Yes
Maternal vars	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7454	7454	7455	7455	7455	7455
No. of Groups	75	75	75	75	75	75
R-squared	0.2593	0.2594	0.2550	0.2555	0.1540	0.1541
Mean Y_{idt}	1.908	1.908	0.372	0.372	0.598	0.598

[a]Number of antenatal care checks during pregnancy. [b]Dummy variable equal to one if the mother reports being given or having bought iron and folic acid tablets during pregnancy, and zero otherwise. [c]Dummy variable equal to one if the mother reports receiving at least one tetanus toxoid injection during pregnancy, and zero otherwise. All regressions are estimated using the linear (district) fixed-effect estimator and include a constant. Pregnancy and maternal characteristics: as listed under Table 2. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. Data collected only for the last birth. * p<0.10, ** p<0.05, *** p<0.01

Table A-4: Effect of Conflict on Help with Delivery

	(1) Trained Attdt[a]	(2) No Help[b]	(3) Trained Attdt[a]	(4) No Help[b]	(5) Trained Attdt[a]	(6) No Help[b]
Before conception	0.007	-0.007	0.007	-0.007	0.031	0.019
	(0.0120)	(0.0276)	(0.0118)	(0.0275)	(0.0304)	(0.0173)
During pregnancy	-0.057	-0.481				
	(0.3625)	(0.3298)				
1st trimester			-0.103	-0.367**	-0.128	-0.434*
			(0.2270)	(0.1595)	(0.3358)	(0.2188)
2nd trimester			0.064	0.150	0.059	0.033
			(0.1226)	(0.1744)	(0.1618)	(0.1646)
3rd trimester			-0.096	-0.223	0.269	0.335**
			(0.2758)	(0.1900)	(0.3035)	(0.1412)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Pregnancy vars	Yes	Yes	Yes	Yes	Yes	Yes
Maternal vars	Yes	Yes	Yes	Yes	No	No
Panel variable	District	District	District	District	Mother	Mother
Observations	10591	10591	10591	10591	5620	5620
No. of Groups	75	75	75	75	2674	2674
R-squared	0.2114	0.0399	0.2114	0.0401	0.0245	0.0172
Mean Y_{idt}	0.127	0.090	0.127	0.090	0.085	0.100

[a]Dummy variable equal to one if the mother reports that the delivery was assisted by a skilled birth attendant (doctor, nurse or midwife), and zero otherwise. [b]Dummy variable equal to one if the mother reports receiving no help at all with delivery, and zero otherwise. All regressions are estimated using the linear fixed-effect estimator and include a constant. Pregnancy and maternal characteristics: as listed under Table 2. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. Data collected for all children under 5 years old. * p<0.10, ** p<0.05, *** p<0.01

Table A-5: Effect of Conflict on Immunization

	(1) Full Immun.[a]	(2) No Immun.[b]	(3) Full Immun.[a]	(4) No Immun.[b]	(5) Full Immun.[a]	(6) No Immun.[b]
Before conception	0.005	0.008	-0.041	0.009	-0.053	-0.042**
	(0.0430)	(0.0120)	(0.0594)	(0.0226)	(0.0667)	(0.0204)
During pregnancy	0.561	-0.207**	0.954	-0.186	-0.505	-0.381*
	(0.5580)	(0.0993)	(0.6972)	(0.1955)	(0.7204)	(0.2247)
During first year	-0.013	0.002	-0.069	0.002	-0.006	-0.040*
	(0.0492)	(0.0155)	(0.0602)	(0.0200)	(0.0769)	(0.0220)
Age in months	0.020*	-0.004	0.026	-0.008	0.004	0.001
	(0.0103)	(0.0043)	(0.0158)	(0.0060)	(0.0033)	(0.0012)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	No	No
Pregnancy vars	Yes	Yes	Yes	Yes	Yes	Yes
Maternal vars	Yes	Yes	Yes	Yes	No	No
Panel variable	District	District	District	District	Mother	Mother
Observations	7969	7969	4005	4005	4005	4005
No. of Groups	75	75	74	74	2551	2551
R-squared	0.0858	0.0211	0.0877	0.0305	0.0134	0.0134
Mean Outcome	0.747	0.017	0.717	0.023	0.717	0.023

[a] Dummy variable equal to one if the child has received a full course of immunization, either as documented on a vaccinations card or as reported by the mother, and zero otherwise. [b] Dummy variable equal to one if the child has received no immunization at all, as reported by the mother, and zero otherwise. Following WHO guidelines, children are considered fully immunized when they have received one dose of the tuberculosis vaccine (BCG) and measles vaccine, and three doses of the polio and DPT vaccines. All regressions are estimated using the linear fixed-effect estimator and include a constant. Pregnancy and maternal characteristics: as listed under Table 2. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants and in monthly averages except for casualties occurring before conception. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. Data collected for all children under 5 years old alive at the time of the survey, but restricted here to those aged 1 to 5 years old since full immunization is normally achieved during the first year of life. * p<0.10, ** p<0.05, *** p<0.01

Table A-6: Short-run Effect of Conflict on Nutrition

	(1) Child WHZ[a]	(2) Child WHZ<-2	(3) Mother BMI[b]	(4) Mother BMI<18.5
Up to 3 months	-0.079	0.029**	-0.192	0.036
before interview	(0.0484)	(0.0144)	(0.1573)	(0.0223)
Last 3 months	-0.307	0.242	0.167	0.157
	(0.8705)	(0.2046)	(1.5034)	(0.2288)
Age in months	0.000	-0.002**		
	(0.0021)	(0.0008)		
Age in years			0.162**	-0.008
			(0.0644)	(0.0097)
Age in years squared			-0.002	0.000
			(0.0011)	(0.0002)
Year FE	Yes	Yes	Yes	Yes
Month (of conception) FE	Yes	Yes	No	No
Month of Interview FE	Yes	Yes	Yes	Yes
Pregnancy vars	Yes	Yes	No	No
Maternal vars	Yes	Yes	Yes	Yes
Observations	9669	9669	6866	6885
No. of Groups	75	75	75	75
R-squared	0.0577	0.0323	0.0602	0.0333
Mean Y_{idt}	-0.809	0.118	20.169	0.256

[a] Weight-for-height z-score. [b] Body Mass Index. All regressions are estimated using the linear (district) fixed-effect estimator and include a constant. Pregnancy and maternal characteristics: as listed under Table 2. Columns (3) and (4) include only one observation per mother and exclude women who are pregnant at the time of the survey. District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. * p<0.10, ** p<0.05, *** p<0.01

Table A-7: Short-run Effect of Conflict on Maternal Work

	(1) Work in last 7 days[a]	(2) Work at home[b]	(3) Off Work in last 7 days[c]
Up to 3 months	0.051*	-0.030	-0.011
before interview	(0.0268)	(0.0282)	(0.0192)
Last 3 months	-0.565	-0.253	0.877**
	(0.3411)	(0.4590)	(0.3464)
=1 if pregnant	0.007	0.002	0.014
	(0.0110)	(0.0144)	(0.0106)
Age in years	-0.014	0.020	0.032***
	(0.0100)	(0.0136)	(0.0063)
Age in years squared	-0.000	-0.000	-0.000*
	(0.0002)	(0.0001)	(0.0001)
Year FE	Yes	Yes	Yes
Month of Interview FE	Yes	Yes	Yes
Maternal vars	Yes	Yes	Yes
Observations	7645	6224	6570
No. of Groups	75	75	75
R-squared	0.1171	0.0355	0.0783
Mean Y_{idt}	0.814	0.150	0.062

[a] Dummy variable equal to one if the respondent reports having worked in the past seven days, and zero otherwise. [b]Dummy variable equal to one if the respondent reports usually working at home, and zero if the respondent has worked in the past 12 months but usually outside the home. [c] Dummy equal to one if the respondent has worked in the past 12 months but has not worked in the past 7 days, and zero if the respondent has worked in the past 7 days. All regressions are estimated using the linear (district) fixed-effect estimator and include a constant. Maternal characteristics: as listed under Table 2). District-correlated robust standard errors in parentheses. District casualties are expressed per 1000 inhabitants. Source: Author's calculations using Nepal DHS 2001 and DHS 2006. Samples include only one observation per mother. Column (2) is restricted to mothers who report working, and Column (3) is restricted to mothers who report having worked at some point in the 12 months preceding the survey. * p<0.10, ** p<0.05, *** p<0.01

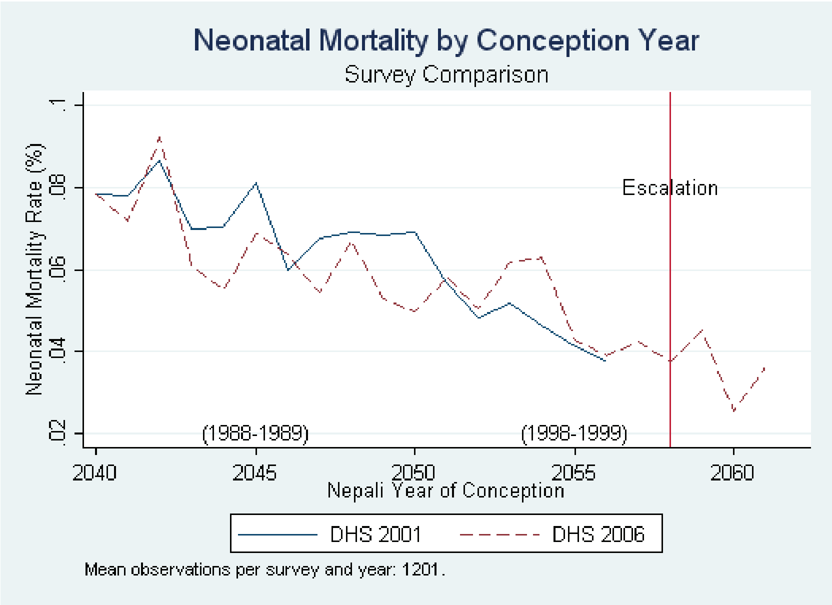

Figure A-1: Reported Neonatal Mortality Rates by Year and Survey Round

Dates in parentheses are the Western calendar years corresponding to the Nepali years used in the DHS. Source: Author's calculations based on DHS 2001 and DHS 2006. Different mothers were interviewed in each survey.

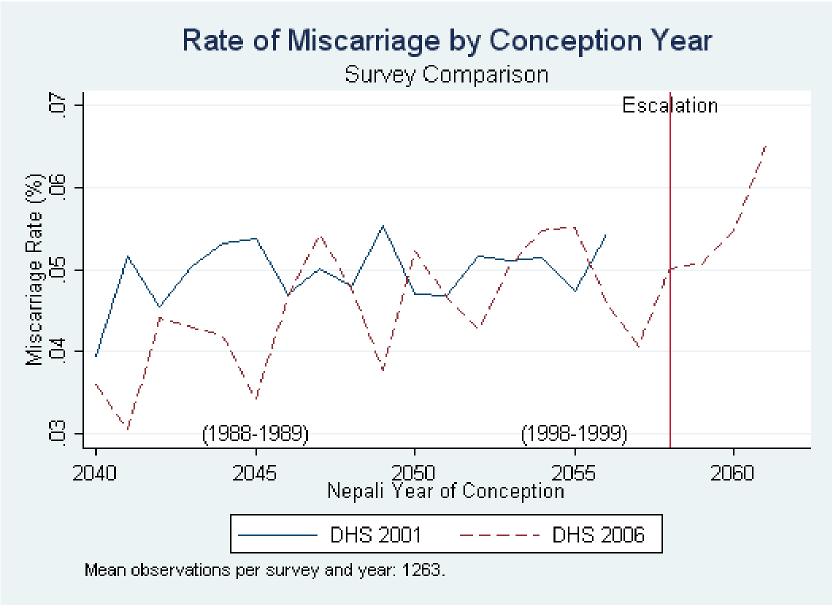

Figure A-2: Reported Miscarriage Rates by Year and Survey Round

Dates in parentheses are the Western calendar years corresponding to the Nepali years used in the DHS. Source: Author's calculations based on DHS 2001 and DHS 2006. Different mothers were interviewed in each survey.