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# Ethnic Violence and Birth Outcomes: Evidence from Exposure to the 1992 Conflict in Kenya

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# Ethnic Violence and Birth Outcomes: Evidence from Exposure to the 1992 Conflict in Kenya

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ABSTRACT

This study is an examination of the effect of intrauterine exposure to electoral violence on child birthweight; an outcome that has long-term effects on an individual's education, income, and health in later life. By considering the electoral violence that resulted from the introduction of multi-party democracy in Kenya as an exogenous source of shock and by adopting difference-in-differences method and mother-fixed effect model, we found that prenatal exposure to the violence increased the probabilities of low birth weight and a child being of very small size at birth by 19 and 6 percentage points, respectively. We found that violence exposure in the first trimester of pregnancy decreased birth weight by 271 grams and increased the probabilities of low birthweight and very small size at birth by 18 and 4 percentage points, respectively. The results reaffirm the significance of the nine months *in utero* as one of the most critical periods in life that shapes future health, economic, and educational trajectories.

Keywords: Kenya, elections, ethnic violence, child health, *in utero*.

JEL codes: O15, I15, J13, J24

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## 1. Introduction

Many countries in the developing world especially in Sub-Saharan Africa (SSA) have experienced episodes of conflict, mostly driven by the fight for political supremacy and control of resources. Although the occurrence of large conflicts has declined over the last thirty years, small-scale conflicts have had devastating effects on communities, especially women and children. The difference between the 21st and 20th Century violence is not only in the scale, but also that the nature of the violence takes multiple forms, such as political violence supported by criminal gangs, international ideological movements combined with local grievances and political movements financed by criminal organizations. Consequently, the risk of having violent conflicts is reported to be on the rise (World Bank, 2011).

Conflicts have very high economic and social costs that can persist for years even after the conflict has ended (Abadie and Gardeazabal 2003; Hoeffler and Reynal-Querol 2003; Collier 1999). Survivors of violent attacks not only lose their property, but also suffer physical injuries and psychological distress from exposure to violence and forceful displacement from their homes (Blattman and Miguel 2010). Given such high levels of exposure to violence that people across the world have experienced in recent years; whether this has had a persistent impact on population health, even after the violence ceases, has become a critical research question.

Existing studies find that children who were exposed to the civil wars in SSA *in utero* tend to be shorter in height (Akresh et al. 2011; Akresh et al. 2012; Bundervoet et al. 2009; Minou and Shemyakina 2014) and the negative effect on height is likely to remain even into their adulthood (Akresh et al. 2017; Akresh et al., 2012). Although there is an increasing body of literature showing that *in-utero* exposure to conflict or violent situations worsens birth outcomes in Latin America and developed countries, evidence from SSA is limited.[1] Furthermore, existing studies on conflicts in SSA have focused on political violence characterized by civil wars, detentions, and mass killings; incidences of which have, however, been on the decline in the recent past. In contrast, other forms of political violence, such as electoral violence, increased after the 1990s when many SSA countries introduced multi-party election, yet these forms of violence receive less attention in literature (Straus 2012).[2]

This paper contributes to increasing the visibility of these forms of violence by addressing the Kenyan case, where, despite decades of ethnically and politically instigated unrests, their negative effects on child health have not been adequately investigated.[3] By exploiting exposure to ethnic violence resulting from Kenya's first multi-party elections in 1992 as an exogenous source of prenatal shock, we statistically examine the impact of such shock on birth outcomes using difference-in-differences method and mother fixed effect model. We hypothesize that the

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[1] Prenatal shocks analyzed in relation to birth outcomes are: the September 11 terrorist attack in New York (Eskenazi et al. 2007); terrorist attacks in Colombia (Camacho 2008), and in Spain (Quintana-Domeque and Rodenas-Serrano 2017); homicides in rural Brazil (Foureaux Koppensteiner and Manacorda 2016); the Guatemalan civil war (Chamarbagwala and Morán 2011); Mexican drug war (Brown 2015); 1999-2007 massacres in Columbia (Duque, 2017); the al-Aqsa intifada in Jerusalem (Mansour and Rees 2012); and Germans exposed to WWII *in utero* and during early childhood (Akbulut-Yuksel, 2017; Akbulut-Yuksel and Yuksel, 2017).

[2] According to the African Electoral Violence Database, 60% of elections in SSA from 1990 to 2008 had violent intimidations. These include the 1992 election related violence in Angola which metamorphosed into a 10-year civil war with thousands of deaths; about 1,500 deaths in the 1992 Kenya elections; the 2005 election in Ethiopia led to about 200 deaths; the 2007/2008 Kenyan elections led to about 1,500 deaths; the 2010 elections in Ivory Coast led to about 3,000 deaths; and 800 deaths in the 2011 elections in Nigeria (Isola, 2018).

[3] The exception is Bell et al. (2012) who investigated the impact of the 2007 post-election violence in Kenya on birth weight. There are, however, methodological limitations to their study.

magnitude and sporadic nature of the violence and the fear of being attacked may have predisposed expectant mothers to psychological and physical stress, which may result in having small babies (Camacho 2008; Aizer et al. 2016). In areas affected by the violence, the economic consequences of the conflict were devastating. Many families were displaced and lost their homes and property. They lived in overcrowded camps that lacked health care and water services. There was also lack of food as grain stores and maize fields were razed down. These factors contributed to the loss of family resources, which could affect fetal development through reduced expenditure on nutritious food and medical services (Human Rights Watch 1993; National Council of Churches of Kenya 1992).

By utilizing the exogenous variations of the timing of the pregnancy and location of each child, we find that prenatal exposure to the violence increased the probabilities of low birth weight. In addition, exposure in the 1st trimester of the pregnancy had significant impact on the birth outcomes while the impact was insignificant when exposed in the other stages of pregnancy. The results suggest that maternal stress, not lack of nutrition during the violence, is the key channel through which mothers exposed to the violence had children with lower birth weight.

This study contributes to growing empirical literature that tests the fetal origins hypothesis proposed by Barker (1995) which showed that circumstances *in utero* have significant impact on the developmental health from infancy to adulthood. Particularly, *in-utero* exposure to armed conflict and political violence reduces school enrollment among school-age children (Akresh and de Walque 2008; Shemyakina 2011), educational attainments in adulthood (Lee 2014), and reduced lung capacity in old age (Islam et al. 2017), but increases female labor force participation (Shemyakina 2015).

The rest of the paper proceeds as follows. The next section describes the background, triggers, and consequences of ethnic and political violence in Kenya. Section 3 presents the data sources and explains the descriptive statistics. Section 4 explains the identification strategy while Sections 5 and 6 show estimation results and robustness checks. The last section provides policy implications and concludes.

## 2. Background of Kenya's Violence during the 1992 Elections

Kenya is often cited as an example of peace and stability in a tumultuous region. Post-independence Kenya was characterized by political stability and relative peace (Kimenyi and Ndung'u 2005); however, in the early 1990s, Kenya experienced ethnic clashes in response to the introduction of multi-party democracy.[4] The violence took place before the elections[5] mainly in Kenya's Rift Valley region. Here, the opposition had a strong presence and, according to reports, the objective of the perpetrators (Kalenjin community) was to ensure that the president, who was from their ethnic group, remained in power and that communities supporting the opposition parties did not register as voters and if registered would not be able to vote (Kimenyi & Ndung'u,

[4] The 1992 violence lasted from October 1991 up to the December 1992 elections and beyond throughout 1993. The districts with intense violent incidences were Nakuru, Kericho, Nandi, and Uasin Gishu in the Rift Valley region (Kimenyi and Ndung'u 2005).

[5] The 2007 elections resulted in ethnic violence in Kenya. The difference from the 1993 election violence was the timing and cause of violence. The 2007 violence occurred after the election results were announced, and hence was a protestation against the alleged rigging of the results.

2005). This was accompanied by the distribution of leaflets warning them to leave the Rift Valley or face the consequences (Human Rights Watch 1993). The conflicts led to hostility between different ethnic groups, compromised the credibility of the electoral process and slowed economic development. These conflicts, while small scale geographically, have shaped the country's political scene and their impacts mirror those of civil wars.

The aftermath of the violence was displacement, injuries, death, and loss of property. There was heightened insecurity as civilians took the law into their own hands by targeting their perceived enemies. This resulted in the indiscriminate loss of lives across all the ethnic groups, physical injuries, and psychological trauma (Human Rights Watch 1993; Kimenyi and Ndung'u 2005; Nyukuri 1997). It is estimated that 1,500 people lost their lives, while 300,000 others were displaced and many had signs of infectious diseases and malnutrition due to the poor living conditions that resulted from the unrest (Human Rights Watch 1993; Ndegwa 1997; Nyukuri 1997; Oyugi 1997).

The economic consequences of the violence were also devastating. From a fact-finding mission to the most affected parts of the Rift Valley region, Human Rights Watch (1993) reported that a majority of the agricultural farms were abandoned and attempts by the owners (immigrants to Rift Valley) to resume cultivation were thwarted by the Kalenjin (native ethnic group) who stole their crops and grazed their cattle in the farms. This led to food shortages not only in Rift Valley but also in the rest of the country.

In this study, therefore, the effects of *in-utero* exposure to the violence on birth outcomes include not only the violence per se (or stress caused by the violence) but, similar to existing studies, also the socio-economic factors such as food shortage and poor sanitation during the temporary displacement. It is normally the case that in severely affected areas, both violence and economic fallout is more serious, which makes it difficult to separate their effects. To control for economic conditions, we include district fixed effects and region-specific time trends. In addition, we examine heterogeneous effects between mothers with lower and higher education since mothers with higher education are less likely to be credit constrained, which increases access to nutritious food during pregnancy (Bozzoli and Quitana-Domeque 2014). Regarding the stress channel, we attempt to identify the effect by testing whether or not the timing of the exposure affects the birth weight differently. Torche (2011) found that stressful events in the first trimester of pregnancy affect birth weight more seriously than those unexposed or exposed later in the pregnancy.

## 3. Data and Descriptive Statistics

### 3.1 Data

This study uses 3 datasets. The first data set is the Uppsala Conflict Data Program (UCDP) Events Dataset, which records violent events occurring at a given time and place. This allows us to construct accurate measures of a conflict in terms of the location, timing, and severity. We use the information on the total number of fatalities due to the conflict in a district where an expectant mother resided as a measure of intensity to estimate the impact of the violence.

Our second data source is the 1993 Kenya Demographic and Health Survey (KDHS) conducted by the National Council for Population and Development (NCPD) and the Central Bureau of Statistics, Kenya. The KDHS forms part of the worldwide Demographic and Health Surveys (DHS) program designed to collect data on maternal and child health, fertility, and family planning.[6] The nationally representative survey contains information on 7,540 women between 15 and 49 years. Since our study examines the effect of the violence on birth weight and the data on birth outcomes are available only for children aged 0-5 years, our study focuses on women who have children aged 0-5 years. This accounts for 81% of the whole women sample. For our analysis, we define children whose mothers were pregnant in districts with pre- and post-election violence in 1992-1993 recorded in UCDP data as exposed to the violence ("violence-affected districts" hereafter), because although not everyone was a target of the violence, one does not have to be directly subjected to conflict to suffer the consequences (Islam et al. 2015). Living in a violent environment or fear of victimization and retaliation from an aggrieved party can be traumatizing, especially if there is loss of loved ones or neighbors as was the case in the retaliation by the Kikuyu against the Kalenjin (Human Rights Watch, 1993). We do not consider children whose mothers immigrated into violence-affected districts after the violence period as "exposed to the violence". The affected children in this study include those whose mothers had

[6] While KDHS1993 data contain the information on survey enumeration (primary sampling unit, PSU) and province where each household resides, GPS coordinates were not collected in the survey. We obtained names of districts for each stratum code from local DHS offices, which allows us to identify affected districts and to merge with the data with UCDP data. We failed to obtain names of the sub-location for each PSU code.

lived in violence-affected districts for more than two years, hence long enough to be exposed to the violence *in utero.*[7]

The child sample contains children born between March 1988 and July 1993 (aged 0-5 years during data collection). Due to the data limitation, this study focuses only on the subsample of pregnancies that resulted in live births. Given that the violence would presumably increase the risk of a non-live birth (miscarriage and stillbirth), this would lead to more conservative estimates of the impact that violence has on negative reproductive outcomes. For *in utero* exposure, children who were born before October 1991 were considered not exposed to the violence *in utero*.[8] Three sets of exposure variables are used in the analyses. The first exposure variable (exposed *in utero*) is a dummy variable taking '1' if fatalities were reported in the child's district of birth when the mother was pregnant and '0' otherwise. This captures the impact of the shock for children who were exposed to the violence *in-utero* during the high intensity period regardless of the length exposed. The second exposure variable (months *in utero*) identifies the number of months *in-utero* where fatalities were reported in the district in a given time. This estimates the possible different effect of the length of exposure (0-9 months) among the affected children. The third set contains three dummy variables; namely whether a child was exposed to the violence in the first trimester (0-3 months of pregnancy), second trimester (4-6 months of pregnancy), or third

[7] We use the variable "years lived in the current residence" for identifying the location at the time of the violence. Since the ethnic violence started in October 1991 and the survey was conducted in July 1993, we consider that women who had lived for more than two years in the affected district were exposed to the violence and their children who were born during the violent period were identified as affected by the violence. From the data set, 94% of the respondents answered that they had lived in the same residence for more than two years with 61% having always lived in the same district.

[8] DHS data does not contain information on the gestational stage of birth. We therefore assume a 9-month gestation period for identifying the timing of the in-utero exposure.

trimester (7-9 months of pregnancy).. The third measure allows for estimating the possible different effects of the violence depending on the timing of the pregnancy. This is an important measure for capturing the effects of the violence during the early pregnancy period as well. [9]

The third data source is rainfall and temperature data (1987-1993) from the Kenya Meteorological Department (Kenya Meteorological Department 2015). Out of the districts where DHS sample households reside, four do not have weather stations. For those districts, rainfall stations located in neighboring districts are assigned after considering similarity of climatic conditions. Based on the information on rainfall and temperature, we construct the number of months that were identified as higher risk of malaria infection for each child in the DHS dataset following Kudamatsu et al. (2012). This is particularly important because malaria is one of the leading causes of infant mortality worldwide and in Kenya accounts for 20% of all deaths in children under 5 years (Pathania 2014). In pregnancy, malaria increases the probability of infant mortality through low birth weight, and children in malaria epidemic areas who experience worse conditions *in utero* are at higher risk.

### 3.2 Descriptive Statistics

Table 1 Panel A shows a comparison of child characteristics in the full sample with those in the violence-affected districts and the non-affected districts. Children in the violence-affected districts

[9] We acknowledge that is it difficult to determine the exact time of conception using DHS data. Please keep in mind that there is some caveats to creating 3-month window of gestational age since gestational age at birth could be shorter than 9 months for premature labor. Therefore, we also conduct analyses by using a more general indicator "early" versus "mid/late" pregnancy. The results are similar to ones with trimesters.

are slightly younger than those in the non-affected districts. There are, however, no gender or birth order differences between the violence-affected districts and the non-affected districts.

Table 1 Panel B shows the parental characteristics. Regarding education status, 24.1% of the mothers in the violence-affected districts have no education attainment compared to 17.6% in non-affected districts. Similar results are reported for the husband or partner's education: 26.2% of the men in the violence-affected districts have never been to school in comparison with 16.5% in the non-affected districts. Mothers in the affected districts are taller than those in non-affected districts. It is necessary to control for intergenerational transmission of birth outcomes since height varies by ethnicity, hence the negative birth outcomes could be due to genetic factors and not exposure to the violence (Currie and Moretti 2007). In addition, fetal growth restriction, which could result in very small babies at birth, is determined by genetic, placental, and maternal characteristics (Peleg et al. 1998). Another notable difference is in the number of months of malaria exposure in pregnancy. The epidemiological climate of the violence-affected districts is less conducive for the spread of malaria as evidenced by 1.3 months' exposure in pregnancy compared to 5.4 months in the non-affected districts.

From Panel C on household characteristics, we can see that the violence-affected districts have younger household heads and a lower proportion of female heads. However, the distance to the nearest health center is further by approximately 2 kilometers. This could have hampered access to prenatal health care services in the violence-affected districts. In summary, Table 1 shows that violence affected districts are slightly poorer than non-affected districts with a higher share of women without education and longer average distance to the nearest hospital. However, as seen in Table 2, birth outcomes of the children in affected districts when they were not exposed to the violence are not significantly different from those in non-affected districts.

Table 2 presents the descriptive statistics of the main outcome variables, namely birth weights, low birthweights (whether a child was born with birth weight less than or equal to 2500 gram), and very small size at birth (mother's self-reported measure of a child's size at birth).

Panel A shows the birth weight separately for the violence-affected and non-affected districts. The differences in birth weight between the locations of the birth (affected vs. non-affected district) are not statistically significant among the given birth cohort. Similarly, mean birth weight for the birth cohort exposed *in utero* and that for those not exposed *in utero* are not statistically significant in a given location. However, the birthweight of those who were exposed in the first trimester of the pregnancy is lighter in affected districts than in non-affected districts.

As shown in Panel B, the low birth weight incidence in the violence-affected districts is higher among those who were exposed *in utero* (26%) than among those who were in the same exposed birth cohort but in non-affected districts (13%). Because there is no difference in low-birth-weight incidences between affected and non-affected districts among children who are not exposed *in utero*, the difference-in-differences estimator is significant. These effects are more prevalent in children exposed in the first and second trimesters of pregnancy while there is no difference in the incidence when the timing of the exposure was in the third trimester. This suggests that the exposure to violence especially in the early period of pregnancy contributed to the incidence of low birth weight.

Panel C reports the share of children who were of very small size at birth[10]. Although this is a mother's self-reported measure of the size of the child at birth, it is considered as a good proxy of birth weight and used in other studies (Molina and Saldarriaga 2017). The potential limitation of this measure is that it is subjective and dependent on a mother's interpretation and recall. Birth weight information is not available for 57% of the children, which does have the potential for selection bias; however those rare events in which children were born very small tend to be easy to recall for mothers. Around 9% of the children who were exposed to the violence *in utero* in the affected districts were born very small compared to 6% in non-affected districts. Similar to Panel B, the difference is found only in early pregnancy periods (1st and 2nd trimesters). This difference is statistically significant and points to worsened outcomes for violent districts.

## 4. Identification Strategy

Our empirical approach takes into account the possible disruption of life *in utero* and birth outcomes through unexpected exposure to shock among pregnant women, restriction of resources, and reduced access to health and social services due to the ethnic violence. Because this was the first time to introduce multi-party elections in Kenya, no one could predict the pre-election violence that occurred, hence, it was not likely that women would deliberately avoid pregnancy or

[10] A question of the size of a child at birth is chosen out of these sizes: very large; large; average; small; and very small; or does not know.

migrate to other districts in anticipation of violence. Thus, we consider the pre-election violence in 1992 as exogenous shock to mothers in affected districts.[11]

Because the violence is assumed to be exogenous shock, the effect on birth outcomes is estimated by difference-in-differences method. Our empirical model is as follows:

$$(1)\ y_{itd} = \beta_0 + \beta_1(R_d \times E_t) + \beta_x X_{idt} + \alpha_t + \delta_m + \omega_d + \alpha_t \times \omega_r + \varepsilon_{itd}\ ,$$

where *y* is the outcome variable, i.e., birth weights, low birthweight, or very small size at birth, of child *i* born in month-year *t* in district *d*. The interaction term $(R_d \times E_t)$ identifies children exposed to violence *in utero* ($E_t$) in the violence-affected districts ($R_d$)[12] and exposed to violence *in utero* ($E_t$); $X_{idt}$ are parental, child, and household characteristics; $\alpha_t$, are year of birth fixed effects; $\delta_m$, are month of birth fixed effects; and $\omega_d$ is district fixed effects to control for geographical and development differences across districts; $\alpha_t \times \omega_r$ is a set of region-specific birth-year dummies to control for differences in regions in time; βs are coefficients to be estimated; and ε is the error term.

While in the first specification, the variable indicating exposure to the violence ($E_t$) is a dummy variable whether a child was exposed to the violence *in utero* or not, we use other exposure variables that measure the timing and severity of the violence in other specifications. As

[11] We do not conduct our analysis using recent data since subsequent incidences of violence were predictable as Kenyans vote along ethnic lines, which makes identification of the violence on children's birth outcomes difficult.

[12] We acknowledge that district-level exposure may not be a precise measure of physical violence that households face. Due to the data availability, we cannot create more precise exposure measure. However, our exposure measures are more precise than other existing studies such as Akresh et al. (2012) and Akresh et al. (2011), which used region- and province-level measures.

explained earlier, different timing of exposure to violence during pregnancy allows us to test whether stress caused by the violence negatively affects birth weight or not. If children who were exposed in the first trimester of pregnancy tend to be smaller than those exposed later in the pregnancy or unexposed, the effect of exposure to violence on birth outcomes is partly due to maternal stress heightened by the violence (Torche 2011).

Kenya's electoral violence hit the maize production area and lasted a few months, and birth outcomes can be affected by nutritional deficits, which can affect fetal growth. Because households with credit constraints are less likely to have unimpaired access to nutritious food and are more likely to suffer from food insecurity, the effect of the electoral violence on birth outcome is expected to be greater for the credit-constrained households. In addition, there is evidence that birth weight is strongly associated with nutritional deficiencies in the third trimester of pregnancy (Stein and Lumey 2000; Almond et al. 2011). This difference in sensitivity of nutritional effect on birth outcome allows us to test whether the negative effect of electoral violence in Kenya is partly explained by the nutritional channel or not. In addition to the main specification of equation (1), we add the interaction term $(R_d \times E_t \times S_t)$ to identify the main channel of the negative effect of the violence stratified by the education level of the mother, as proposed by Bozzoli and Quintana-Domeque (2014). More educated mothers tend not only to have better knowledge about health care and nutrition, but also to have better health, which genetically leads to better health for their children (Strauss and Thomas 1999). Education level is also associated with income. During the violence, uneducated mothers might have failed to take adequate nutrition during the pregnancy and to prevent the adverse effects of the violence on their children. Thus, the effect of the violence on health can be different according to the mother's education. In Foureaux Koppensteiner and Manacorda (2016), the effect of homicides during the first trimester of pregnancy on the probability of children having low birthweight is only found among mothers with less than 7 years of education.

We also estimated the impact of the violence on gender to identify whether it varies by gender. In the existing literature, the gendered effects of the violence on child health are mixed. In terms of the effect on birth outcomes, Foureaux Koppensteiner and Manacorda (2016) find that exposure to homicides in Brazil increased the probability of low birthweight only among girls. For non-birth outcomes, Akresh et al. (2014) find that the impact of exposure to conflict on child height is slightly larger for girls in Ethiopia but smaller for girls in Eritrea though the gender-difference in these magnitudes is not statistically significant. Minoiu and Shemyakina (2012) find no evidence that the effect of war exposure on child's height is different between boys and girls in Cote d'Ivoire. Thus, whether exposure to the violence has a different impact on the health outcomes of boys and girls is inconclusive. In Kenya, the under-5 mortality rate for girls has been lower than that for boys. There is no evidence of serious sex-imbalances in the population unlike India and China. Even so, it is considered that in many parts of Africa, son preference is prevalent in areas with patriarchy; however, selective abortion is rarely conducted (Packer 2002). Since our focus is on birth weight, the gendered difference is likely to be explained by the strength of fetus, not the differential treatments parents engage in based on the sex of the child.

To correctly identify the impact of exposure to the violence, equation (1) relies on two assumptions. The first one is a common trend assumption.. The change over time in the children exposed to the violence in its absence is assumed to be the same as the change in the control group. The second assumption is that once *X* and other fixed effects are controlled for; the exposure variable does not correlate with the error term in equation (1). However, this assumption can be violated if some households selectively emigrated from affected areas to mitigate the negative effects from the violence (selective migration), if some mothers purposively delayed pregnancy during the violence to avoid its effects (selective fertility), or if there were some unobserved characteristics of those mothers likely to give birth during the violence (e.g., strong genes and more fertile), or if their children were healthier (unobserved characteristics). We acknowledge that our model relies on these assumptions. In the later section, we run the robustness checks of the results.

## 5. Results

### 5.1 Violence on Birth Outcomes

Columns (1) - (5) of Table 3 present the estimated impact of exposure to ethnic violence on birth weight in kilograms, columns (6) – (10) on low birth weight, and columns (11) – (15) on very small size at birth. All specifications control for child, parental, and household characteristics shown in Table 1, month of birth, year of birth, ethnicity, district of birth fixed effects, and region-specific time trend. Robust standard errors are clustered at the enumeration level (PSU).

The estimated coefficients in columns (1) and (2) show no evidence that *in utero* exposure to the violence reduced birth weight. Columns (3) and (4), however, show that exposure in the first trimester is associated with 250-271 grams reduction in birth weight. There is no evidence that exposure in the second and third trimesters affect birth weight. This is consistent with medical studies and other literature that show that the disruption of life in the first trimester has worse outcomes. In terms of magnitude, the negative effect from Kenya's electoral violence is far greater than the other studies.[13] This is probably because the other studies focus on more developed countries where more economic resources and advanced health care systems are available than Kenya, which enables them to cushion the people against some of the impact of conflicts.

The estimation results for low birthweight are presented in columns (6)-(10). We find a positive and significant impact of *in utero* exposure to the violence, meaning that *in-utero* exposure increased the probability of low birth weight. The results from column (6) show that children exposed to violence *in utero* had a higher probability of low birthweight by 19 percentage points while an extra month of exposure increased the probability of low birth weight by 2 percentage points than unexposed children. From columns (8) and (9) we can see that exposure in the first trimester increased the probability of low birthweight by 18 - 21 percentage point. Our estimates exceed those reported in previous studies. Mansour and Rees (2012) found that prenatal exposure to the 2001-2004 Israeli-Palestinian conflict in the first, second, and third trimesters increased the probabilities of low birth weight by 0.9%, 0.3%, and 0.14%, respectively.

[13] Exposure to landmine explosion in the residential municipality during the 2$^{nd}$ trimester of the pregnancy in Colombia decreases birth weight by 8 grams (Camacho 2008), while additional fatality from al-Aqsa Intifada during the first trimester of the pregnancy in the district decreased birthweight by 33 grams (Mansour and Ree 2012). Additionally, a study from Mexico showed that homicide in municipalities during the first trimester of the pregnancy decreases birth weight by 42 grams (Brown 2018).

Both Lauderdale (2006) and Eskenazi et al. (2007) report that *in utero* exposure to the September 11$^{th}$ 2001 New York terrorist attacks was associated with 1.50 and 1.44 increased odds of low birth weight, respectively. Comparing these findings with ours indicates that exposure to the ethnic violence in Kenya had worse outcomes.

The estimated coefficient in column (11) shows that *in utero* exposure to the violence increased the probability of a child being very small at birth by 5.9 percentage points. From column (12) we see that an extra month of *in utero* exposure to violence increased the likelihood of a child being very small at birth by 0.5 percentage points. Columns (13) and (14) show that the exposure in the first trimester increased the probability of a child being very small in size at birth by 3.7 – 4.6 percentage points. These findings suggest that nutritional deficiencies, due to resource constraints and restricted uterine growth, being the reason for the very small size of the exposed children.

### 5.2 Heterogeneous Effects of the Violence Stratified by Mother's Education

In this section, we examine whether there are heterogeneous effects of violence on child health outcomes. In columns (1) – (3) of Table 4, we examine the impact of the violence when the data on violence exposure is interacted with that of mother's education level. We interact the exposure variables with an indicator variable of whether the mother's education level is no greater than primary school. We found that children whose mothers had not completed secondary school had an increased probability of low birth weight. However, none of the coefficients of interaction terms were statistically significant. This finding differs from Bozzoli and Domeque-Quintana (2014) who analyzed the impact of the Argentine economic crisis on prenatal health and found that the low birth weight of children of less educated mothers was associated with the nutritional deficiencies experienced by the mothers.

### 4.3 Heterogeneous Effects of the Violence by Gender of Child

In columns (4) – (6) of Table 4, we present the results of the interaction of the violence variable with gender. There is no evidence that boys are less affected than girls. This may be because sonography to identify sex of the fetus was not common in our sample area in 1992 and preferential treatment towards sons over daughters during pregnancy is unlikely.

## 6. Robustness Checks

### 6.1 Selective Migration

Thus far, in order to make sure that those who were categorized as “affected” were in affected districts,, children who immigrated into violence-affected districts after the violence period was over are defined as “non-affected”. It is possible that some of those lived in the affected districts during the conflict but left those areas during or after the conflict by the time of the survey. If those who left the affected districts tend to be healthier, our estimates can be biased. Unfortunately, there is no information about the name of districts before they moved into the current residence in KDHS. Thus, we test the sensitivity of the estimates by dropping households who migrated during/ after the conflict from the sample.  The estimation results from subsample of those who did not migrate at least for 2 years are shown in Table 5. We obtained qualitatively similar results as those the in main results shown in Table 3.

### 6.2 Selective Fertility due to the Violence

Our estimation results could be accounted for by the fact that women who had children during the violent period were systematically different from others. For example, if those who were less healthy in the violent affected districts could not avoid being pregnant during the violent period, the estimated effect of the exposure to violence on birth outcome would be negative even when there was no actual impact. To rule out this possibility, we tested whether being pregnant during the violence is associated with observable characteristics of women. We ran a linear regression model using ordinary least squares (OLS) method looking at mothers' characteristics such as mother's age at child birth, years of education, height (long-term health measure), and marital status (never married), on exposure to violence (separately by trimesters). Regarding fertility behavior, we tested whether the birth interval preceding a given child's birth was different between children who were exposed to violence *in utero* and those who were not. The results in Table 6 show that there were no significant differences between women whose children were exposed to the violence and other women.

Using the 1999 Population Census data, we looked at the population composition of the affected children who were born after the 1992 violence, exposed to the violence *in utero*, and born before the violence (4, 6, and 8 years old in 1999, respectively) to see if there was any substantial population decline in groups from the affected districts. The proportions of children in the affected districts were: 9.5% for those who were born in 1995, 9.4 % for those who were born in 1993, and 9.2% for those in 1991. If the violence had affected the fertility behavior, the proportion of children in the violent districts who were born in 1993 should be significantly lower than the others, but that was not the case. Thus, the violence may not have affected the fertility choices in the districts.

### 6.3 Unobserved heterogeneity

The estimation model thus far does not control for potentially important unobserved characteristics such as a mother's genetic factors that might affect the health outcomes of their children. Without controlling for such unobserved heterogeneity, the estimated impact of exposure to the conflict could be biased. To eliminate a mother's time-invariant unobserved heterogeneity, we ran a mother fixed effects model. Here, we restricted the sample to mothers who had more than one child in the past five years and one of the children had been exposed to the violence *in utero*.

Table 7 shows the estimation results with the mother fixed effects model. As seen column (5) and (6), *in-utero* exposure increases the probability of a child being of low birth weight by 18 percentage points, which is almost same as the results shown in Table 3. The main results for low birth weight seem robust. Once the mother's unobserved heterogeneity is controlled for, the impact of exposure to the violence in the first trimester of the pregnancy on birth weight (column 3) and low birth weight (column 7) become insignificant while the effect on the probability of being very small at birth remains significant (column 11). Exposure to the violence during the second trimester increased the probability of low birth weight by 28 percentage points. Thus, we can conclude that the negative effects of exposure to violence on birth outcomes are not spurious.

### 6.4 Mechanism

The estimation results indicate that the 1992 Kenya's electoral violence resulted in negative birth outcomes. The question arises as to the mechanism behind this finding. One possibility is the disruption of access to health services during the violence, which made it impossible for pregnant mothers to access prenatal care. Prenatal care in general includes nutritional and health checks, which help fetal health, growth, and development. One such important prenatal recommendation to pregnant women is to obtain a tetanus vaccination during pregnancy as it reduces the risk of a child's death after birth (Mwabu 2008).

Table 8 shows the percentage of children whose mothers received antenatal care and tetanus injections during pregnancy and delivered at a health facility, separated by location (violence affected and non-affected districts), and timing of birth (exposed or non-exposed to the violence). Because the comparison of the means is inconclusive, we examined whether the mothers' utilization of health services changed due to the violence by applying the same estimation model (DID) as in the previous sections.

The results are shown in the last column of Table 8, where a dummy variable identifying women who received prenatal care during the pregnancy is the dependent variable in Panel A, while a dummy variable for being immunized with the tetanus toxoid vaccine during the pregnancy is the dependent variable in Panel B. In Panel C, a dummy variable for whether a child was delivered at a health facility or not is the dependent variable. All the coefficients are not statistically significant. We did not find evidence that the negative effect of exposure to the violence on child-birth outcomes was due to disruption of access to health care services during the violence. Combined with the results obtained in earlier sections, we can conclude that maternal stress early in pregnancy from the 1992 Kenya electoral violence negatively affected birth outcomes.

## 7. Conclusion

In this study we investigated the impact of prenatal exposure to pre-election violence on birth outcomes. We found that exposure to prenatal stress negatively affected the health of the exposed children. The results of our analysis show that babies exposed to violence *in utero* had an increased probability of low birth weight and being very small size at birth by 18.9 and 5.3 percentage points, respectively. An extra month of prenatal exposure to the violence increased the probabilities of low birth weight and a child being small at birth by 2.1 and 0.55 percentage points, respectively. This shows that the negative impact was stronger for children exposed for longer periods. Exposure to the violence in the first trimester decreased birth weight by 271 grams and increased the probabilities of low birth weight and very small size at birth by 18 and 4 percentage points, respectively. The findings are consistent with the hypothesis that prenatal exposure to conflict affects birth outcomes. Our findings reveal that the 1992 ethnic violence in Kenya had a greater impact on birth outcomes than did the violence described in the other studies. Further analyses have confirmed that the estimates presented in the study are robust.

While exposure in the first trimester of pregnancy significantly increased the incidence of low birth weight, exposure in the third trimester did not. Furthermore, we did not find evidence that the negative effect is greater for less educated mothers than for the more educated mothers. These results suggest that lack of proper nutrition during pregnancy due to the electoral violence is not the main reason for the increased incidence of low birth weight. We also dis not find evidence that those who were exposed to the violence were less likely to take antenatal care and tetanus injection and to deliver at health facilities. Therefore, decreased utilization of health services due to the violence does not explain the negative effect either. Rather, based on the results that *in-utero* exposure in the first trimester significantly decreased birth weight, we conclude that maternal stress is the main channel affecting the birth outcomes.

Ethnically- and politically-instigated conflicts are unfortunately a common phenomenon in Kenya, Africa, and other developing economies. It is, therefore, important to understand the health, educational, and economic consequences of such events that derail human and economic development efforts. These results have policy implications as they demonstrate the need for reducing violence. Free and fair elections are critical for preventing electoral violence. Electoral competition can work if there are checks and balances. There ought to be cooperation between the citizens, media, and the court system to ensure that elections are conducted according to the stipulated rules and where grievances arise, they are resolved according to the law or, as the findings of this study reveal, violent episodes can have a profoundly devastating effect on vulnerable women and children.

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**Table 1** Descriptive Statistics

| | Full Sample | | Affected districts | | Non-affected districts | | t value |
|---|---|---|---|---|---|---|---|
| | Mean | (SD) | Mean | (SD) | Mean | (SD) | |
| Number of observations | 6112 | | 1660 | | 4452 | | |
| **Panel A: Child characteristics** | | | | | | | |
| Male child | 0.499 | (0.500) | 0.509 | (0.528) | 0.496 | (0.491) | 0.76 |
| Single birth | 0.972 | (0.164) | 0.977 | (0.181) | 0.973 | (0.159) | -0.45 |
| First born child | 0.202 | (0.402) | 0.220 | (0.437) | 0.200 | (0.390) | 1.47 |
| Age in years | 2.000 | (1.400) | 1.896 | (1.495) | 2.000 | (1.364) | -3.33** |
| Mother's age at child's birth | 26.12 | (6.734) | 25.983 | (7.125) | 26.165 | (6.603) | -0.62 |
| **Panel B: Parental Characteristics** | | | | | | | |
| Mother's Educational level: No education | 0.192 | (0.394) | 0.241 | (0.452) | 0.176 | (0.374) | 1.80† |
| Primary level | 0.594 | (0.491) | 0.570 | (0.570) | 0.601 | (0.481) | -0.91 |
| Secondary level | 0.211 | (0.408) | 0.180 | (0.406) | 0.221 | (0.407) | -1.86† |
| Higher education | 0.004 | (0.059) | 0.009 | (0.102) | 0.002 | (0.040) | 1.68† |
| Father's Educational level: No education | 0.188 | (0.391) | 0.262 | (0.464) | 0.165 | (0.364) | 4.09** |
| Primary level | 0.490 | (0.500) | 0.491 | (0.440) | 0.32 | (0.507) | -2.72** |
| Secondary level | 0.309 | (0.462) | 0.280 | (0.474) | 0.319 | (0.457) | -1.66† |
| Higher education | 0.011 | (0.108) | 0.018 | (0.141) | 0.01 | (0.096) | 1.70† |
| Mother's Marital Status: Currently married | 0.850 | (0.357) | 0.842 | (0.385) | 0.853 | (0.375) | -0.68 |
| Never married | 0.074 | (0.262) | 0.096 | (0.311) | 0.067 | (0.245) | 2.59** |
| Formerly married | 0.076 | (0.265) | 0.061 | (0.254) | 0.08 | (0.267) | -1.66† |
| Mother's months of malaria exposure in pregnancy | 4.390 | (3.980) | 1.309 | (3.074) | 5.380 | (3.695) | -23.95** |
| Mother's height in centimeters | 159.15 | (9.15) | 159.945 | (8.364) | 158.89 | (9.324) | 2.44** |
| **Panel C: Household Characteristics** | | | | | | | |
| Age of household head | 39.97 | (13.97) | 39.078 | (14.69) | 40.257 | (13.716) | 1.63 |
| Female household head | 0.282 | (0.450) | 0.235 | (0.448) | 0.297 | (0.448) | -2.89** |
| Land ownership | 0.862 | (0.345) | 0.867 | (0.359) | 0.860 | (0.340) | 0.34 |
| Distance to hospital | 20.27 | (17.51) | 10.492 | (7.043) | 8.094 | (5.158) | 2.59** |
| Rural residence | 0.873 | (0.333) | 0.889 | (0.332) | 0.868 | (0.333) | 1.08 |
| Lived in the same region for many years | 0.933 | (0.25) | 0.838 | (0.389) | 0.964 | (0.183) | -6.32** |

Notes: Sampling weights are used in calculating all the means. The t-value tests whether means for affected districts and non-affected districts are statistically significant or not. †significant at 10%, * significant at 5%, ** significant at 1%.

**Table 2** Health Outcomes by Affected Districts and Period

| | Full sample | | Affected districts | | Non-affected districts | | t value |
|---|---|---|---|---|---|---|---|
| | Mean | (SD) | Mean | (SD) | Mean | (SD) | |
| **Panel A: Birth weight (kgs)** | | | | | | | |
| Number of observations | 2651 | | 614 | | 2037 | | |
| All | 3.282 | (0.747) | (3.280) | 0.771 | (3.283) | 0.74 | -0.09 |
| Born before violence (not exposed *in utero*) | 3.29 | (0.754) | 3.303 | (0.777) | 3.286 | (0.747) | 0.41 |
| Exposed *in utero* | 3.241 | (0.710) | 3.167 | (0.737) | 3.268 | (0.700) | -0.91 |
| Diff. between exposed and not exposed | -0.049 | (-1.10) | -0.136 | (-1.40) | -0.018 | (-0.36) | -1.28 |
| Exposed in 1st trimester | 3.158 | (0.727) | 3.01 | (0.663) | 3.203 | (0.740) | -1.67† |
| Exposed in 2nd trimester | 3.276 | (0.672) | 3.171 | (0.697) | 3.318 | (0.660) | -1.40 |
| Exposed in 3rd trimester | 3.273 | (0.700) | 3.270 | (0.665) | 3.274 | (0.711) | -0.04 |
| **Panel B: Low Birth weight** | | | | | | | |
| Number of observations | 2607 | | 604 | | 2003 | | |
| All | 0.158 | (0.365) | 0.165 | (0.382) | 0.156 | (0.360) | 0.46 |
| Born before violence | 0.157 | (0.364) | 0.146 | (0.365) | 0.161 | (0.364) | -0.76 |
| Exposed *in utero* | 0.161 | (0.368) | 0.255 | (0.440) | 0.130 | (0.336) | 2.19* |
| Diff. between exposed and not exposed | 0.004 | (0.16) | 0.11 | (2.09)* | -0.03 | (-1.25) | 2.30* |
| Exposed in 1st trimester | 0.186 | (0.390) | 0.346 | (0.483) | 0.141 | (0.348) | 2.69** |
| Exposed in 2nd trimester | 0.137 | (0.344) | 0.244 | (0.433) | 0.097 | (0.296) | 2.7** |
| Exposed in 3rd trimester | 0.137 | (0.344) | 0.139 | (0.351) | 0.136 | (0.342) | 0.06 |
| **Panel C: Very small size at birth** | | | | | | | |
| Number of observations | 6043 | | 1641 | | 4402 | | |
| All | 0.053 | (0.224) | 0.054 | (0.240) | 0.053 | (0.219) | 0.21 |
| Born before violence (not exposed *in utero*) | 0.050 | (0.219) | 0.045 | (0.219) | 0.052 | (0.218) | -0.84 |
| Exposed *in utero* | 0.065 | (0.246) | 0.092 | (0.301) | 0.055 | (0.224) | 1.68† |
| Diff. between not exposed and not exposed | 0.015 | (-1.58) | 0.047 | (2.44)** | 0.003 | (0.23) | 1.92† |
| Exposed in 1st trimester | 0.068 | (0.251) | 0.107 | (0.319) | 0.053 | (0.222) | 1.85† |
| Exposed in 2nd trimester | 0.053 | (0.224) | 0.091 | (0.302) | 0.039 | (0.190) | 2.26** |
| Exposed in 3rd trimesters | 0.056 | (0.230) | 0.056 | (0.242) | 0.056 | (0.226) | 0.02 |

Sampling weights are used in calculating all the means. a The t-value is for testing whether means of affected districts and non-affected districts are statistically different or not. †significant at 10%; * significant at 5%, ** significant at 1.

**Table 3** Impact of Ethnic violence on birth weight, low birth weight, size of the child at birth

| | Birth weight | | | | | Low birth weight | | | | | Very small size at birth | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) |
| Affected districts | -0.171 | | | | | 0.189** | | | | | 0.059† | | | | |
| × Exposed *in utero* | (-1.61) | | | | | (2.88) | | | | | (1.93) | | | | |
| Affected districts×Months | | -0.011 | | | | | 0.021* | | | | | 0.005† | | | |
| of exposure *in Utero* | | (-0.67) | | | | | (2.46) | | | | | (1.79) | | | |
| Affected districts× | | | -0.271† | | | | | 0.184** | | | | | 0.037† | | |
| Exposed in 1st trimester | | | (-1.83) | | | | | (3.08) | | | | | (1.82) | | |
| Affected districts | | | -0.039 | | | | | 0.065 | | | | | 0.039 | | |
| × Exposed in 2nd trimester | | | (-0.26) | | | | | (0.96) | | | | | (1.62) | | |
| Affected districts | | | -0.010 | | | | | -0.042 | | | | | -0.028 | | |
| × Exposed in 3rd trimester | | | (-0.10) | | | | | (-0.77) | | | | | (-1.44) | | |
| Affected districts× Exposed | | | | -0.250* | | | | | 0.208** | | | | | 0.046* | |
| in early pregnancy | | | | (-2.03) | | | | | (3.80) | | | | | (2.38) | |
| Affected districts× Exposed | | | | -0.091 | | | | | 0.013 | | | | | 0.012 | |
| in mid/late pregnancy | | | | (-0.76) | | | | | (0.21) | | | | | (0.57) | |
| Fatalities × Affected districts | | | | | -0.785† | | | | | 0.330† | | | | | 0.084 |
| × Exposed *in utero*/100 | | | | | (-1.75) | | | | | (1.83) | | | | | (0.89) |
| Observations | 2521 | 2521 | 2521 | 2521 | 2521 | 2472 | 2472 | 2472 | 2472 | 2472 | 5762 | 5762 | 5762 | 5762 | 5762 |

Notes: Other controls are all variables shown in Table 1. Year of birth, month of birth, ethnicity, and district fixed effects as well as region specific time trend are controlled. Estimated by Linear Probability Model. t-statistics in parentheses. Robust standard errors are clustered at the enumeration level (PSU). †significant at 10%, * significant at 5%, ** significant at 1%.

**Table 4** Impact of Ethnic violence on birth weight and the size of the child at birth by Mother's Education and Gender of Child

| | Less vs. more educated mothers (Z=low educated) | | | Boys vs. girls (Z=boys) | | |
|---|---|---|---|---|---|---|
| | Birth weight | 1 if low birth weight | 1 if very small size at birth | Birth weight | 1 if low birth weight | 1 if very small size at birth |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Affected districts× | -0.333 | 0.186* | 0.089** | -0.322 | 0.187* | 0.042 |
| Exposed in 1st trimester | (-1.22) | (2.02) | (2.85) | (-1.56) | (2.06) | (1.53) |
| Affected districts | 0.048 | 0.083 | 0.049 | -0.120 | 0.058 | 0.023 |
| ×Exposed in 2nd trimester | (0.17) | (0.83) | (1.43) | (-0.66) | (0.65) | (0.86) |
| Affected districts | 0.125 | -0.07 | -0.107 | 0.107 | -0.069 | -0.018 |
| ×Exposed in 3rd trimester | (0.72) | (-0.62) | (-1.06) | (0.81) | (-0.99) | (-0.68) |
| Affected districts× | 0.083 | -0.009 | -0.065 | 0.078 | 0.000 | -0.021 |
| Exposed in 1st trimester x Z | (0.28) | (-0.09) | (-1.55) | (0.31) | (0.00) | (-0.56) |
| Affected districts | -0.122 | -0.014 | -0.014 | 0.116 | 0.012 | 0.030 |
| ×Exposed in 2nd trimester x Z | (-0.40) | (-0.12) | (-0.37) | (0.53) | (0.11) | (0.78) |
| Affected districts | -0.226 | 0.045 | 0.093 | -0.186 | 0.024 | -0.044 |
| ×Exposed in 3rd trimester x Z | (-1.27) | (0.37) | (1.58) | (-1.21) | (0.23) | (-1.12) |
| Low educated =1 | -0.025 | 0.045* | -0.01 | -0.037 | 0.046* | -0.011 |
| | (-0.70) | (2.50) | (-1.22) | (-1.07) | (2.52) | (-1.36) |
| Boy =1 | 0.075* | -0.030* | -0.015* | 0.075* | -0.030† | -0.013† |
| | (2.25) | (-1.98) | (-2.36) | (2.15) | (-1.89) | (-1.95) |
| Observations | 2521 | 2472 | 5762 | 2521 | 2478 | 5776 |

Notes: Other controls are all variables shown in Table 1. Year of birth, month birth, ethnicity dummies, district of birth, region-specific time trends are controlled. Estimated by Linear Probability Model. t-statistics in parentheses. Robust standard errors are clustered at the enumeration level (PSU). †significant at 10%, * significant at 5%, ** significant at 1%.

**Table 5** Impact of the violence on birth outcomes using only those who did not migrate

| | Birth weight (kgs) | | Low birth weight | | Very small size at birth | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Affected districts × Exposed *in utero* | -0.030 | | 0.144† | | 0.030 | |
| | (-0.20) | | (1.94) | | (1.15) | |
| Affected districts× | | -0.363† | | 0.201* | | 0.008 |
| Exposed in 1st trimester | | (-1.71) | | (2.56) | | (0.30) |
| Affected districts | | -0.149 | | 0.101 | | 0.061* |
| ×Exposed in 2nd trimester | | (-0.66) | | (1.24) | | (2.16) |
| Affected districts | | 0.402 | | -0.162 | | -0.048 |
| ×Exposed in 3rd trimester | | (1.58) | | (-1.17) | | (-1.21) |
| Observations | 2343 | 2343 | 2288 | 2288 | 5287 | 5287 |

Notes: Other controls are all variables shown in Table 1, year of birth, month birth, ethnicity dummies, and district of birth. Estimated by OLS. t-statistics in parentheses. Robust standard errors are clustered at the enumeration level (PSU). †significant at 10%, * significant at 5%, ** significant at 1%.

**Table 6** Characteristics of women and household head with children born during the violence and fertility behavior

| | Mother's age at birth<br>(1) | No education<br>(2) | No education or primary education<br>(3) | Higher than primary education<br>(4) | Mother's height (cm)<br>(5) | Single (never married)<br>(6) | Preceding birth interval (months) before a given child<br>(7) |
|---|---|---|---|---|---|---|---|
| Affected districts× | -0.321 | -0.016 | 0.017 | -0.017 | 1.528 | 0.008 | |
| Exposed in 1st trimester | (-0.41) | (-0.39) | (0.49) | (-0.49) | (1.59) | (0.20) | |
| Affected districts | -0.413 | 0.026 | 0.026 | -0.026 | -1.486 | 0.006 | |
| ×Exposed in 2nd trimester | (-0.52) | (0.60) | (0.77) | (-0.77) | (-1.58) | (0.16) | |
| Affected districts | -0.194 | 0.035 | -0.015 | 0.015 | 0.343 | 0.020 | |
| ×Exposed in 3rd trimester | (-0.28) | (1.16) | (-0.49) | (0.49) | (0.70) | (0.67) | |
| Affected districts × Exposed *in utero* | | | | | | | -6.959 |
| | | | | | | | (-0.85) |
| Affected districts × Exposed *in utero* x | | | | | | | 0.249 |
| Mother's age at child's birth/100 | | | | | | | (0.83) |
| Mother's age at child's birth | | | | | | | 0.881** |
| | | | | | | | (12.56) |
| Observations | 6112 | 6112 | 6112 | 6112 | 5830 | 6112 | 4654 |

Notes: Other controls are year of birth, month birth, ethnicity dummies, and district of birth. Estimated by Linear Probability Model. t-statistics in parentheses. Robust standard errors are clustered at the enumeration level (PSU). †significant at 10%, * significant at 5%, ** significant at 1%.

**Table 7** Impact of the violence by mother fixed effects model

| | Birth weight | | | | Low birth weight | | | | Very small size at birth | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Affected districts | -0.192 | | | | 0.180* | | | | 0.027 | | | |
| ×Exposed *in utero* | (-1.41) | | | | (2.19) | | | | (1.08) | | | |
| Affected districts× | | -0.023 | | | | 0.015† | | | | 0.005 | | |
| Months of Exposure *in Utero* | | (-0.97) | | | | (1.69) | | | | (1.53) | | |
| Affected districts× | | | -0.088 | | | | 0.127 | | | | 0.066* | |
| Exposed in 1st trimester | | | (-0.43) | | | | (0.90) | | | | (2.51) | |
| Affected districts | | | -0.194 | | | | 0.281** | | | | -0.037 | |
| ×Exposed in 2nd trimester | | | (-1.11) | | | | (2.62) | | | | (-0.97) | |
| Affected districts | | | -0.028 | | | | -0.112 | | | | 0.032 | |
| ×Exposed in 3rd trimester | | | (-0.15) | | | | (-1.00) | | | | (0.93) | |
| Fatalities×Affected districts | | | | -1.14† | | | | 0.726† | | | | 0.178 |
| ×Exposed *in utero*/100 | | | | (-1.96) | | | | (1.66) | | | | (1.28) |
| Observations | 595 | 595 | 595 | 595 | 586 | 586 | 586 | 586 | 1643 | 1643 | 1643 | 1640 |

Notes: Other controls are child characteristics shown in Table 1. Estimated by Linear Probability Model. t-statistics in parentheses. Robust standard errors are clustered at the enumeration level (PSU). † significant at 10%, * significant at 5%, ** significant at 1%.

**Table 8** Antenatal care, tetanus injection, and delivery at health facility (means and estimation results by linear probability model)

| | Affected districts | | Non-affected districts | | Difference | T value[a] | Coefficient[b] of Affected districts x Exposed *in utero* (t-stats) |
|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | | | |
| **Panel A: Antenatal care** | | | | | | | |
| No. of Observations | 1647 | | 4415 | | | | |
| Not exposed *in utero* | 0.940 | 0.251 | 0.950 | 0.213 | -0.010 | -0.87 | |
| Exposed *in utero* | 0.928 | 0.270 | 0.932 | 0.224 | -0.004 | -0.20 | 0.013 |
| Difference (t-stats) | -0.012 | (0.68) | -0.018 | (1.53) | 0.006 | (1.59) | (0.68) |
| **Panel B: Tetanus injection** | | | | | | | |
| No. of Observations | 1631 | | 4343 | | | | |
| Not exposed *in utero* | 0.91 | 0.302 | 0.922 | 0.264 | -0.012 | -0.87 | |
| Exposed *in utero* | 0.846 | 0.375 | 0.884 | 0.315 | -0.038 | -1.40 | 0.0000121 |
| Difference (t-stats) | -0.064 | (2.99)*** | -0.038 | (2.70)*** | -0.026 | (0.28) | (0.00) |
| **Panel C: Delivery at a health facility** | | | | | | | |
| No. of Observations | 1660 | | 4452 | | | | |
| Not exposed *in utero* | 0.410 | 0.521 | 0.461 | 0.489 | -0.051 | -1.85* | |
| Exposed *in utero* | 0.351 | 0.497 | 0.405 | 0.483 | -0.054 | -1.27 | -0.0028 |
| Difference (t-stats) | -0.059 | (1.66)* | -0.056 | (2.28)** | -0.003 | (0.32) | (-0.06) |

Notes: [a] The t-value is for testing whether means of affected and non-affected districts are statistically different or not. Numbers in parentheses are t-value for testing whether means of exposed and non-exposed cohort are statistically different or not.

[b] The last column is estimated by Linear Probability Model. t statistics in parentheses. Robust standard errors are clustered at the enumeration level (PSU). All models include controls for child, parental, and household characteristics, as well as district-birth year, district, birth year, birth month fixed effects.