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Violence, selection and infant mortality in Congo☆

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ABSTRACT

This paper documents the effects of the recent civil war in the Democratic Republic of Congo on mortality both in utero and during the first year of life. It instruments for conflict intensity using a mineral price index, which exploits the exogenous variation in the potential value of mineral resources generated by changes in world mineral prices to predict the geographic distribution of the conflict. Using estimates of civil war exposure on mortality across male and female newborn to assess their relative health, it provides evidence of culling effect (in utero selection) as a consequence of in utero shocks.

1. Introduction

The fetal origins hypothesis associated with Hales and Barker (1992) postulates that chronic, degenerative conditions of adult health, including heart disease and type 2 diabetes, may be triggered by adverse circumstance experienced in utero. Two broad sets of empirical literature developed around this fundamental intuition.[1] The first one, spanning across medicine, epidemiology and economics, investigates the long term health consequences of in utero shocks. There is a large consensus that shocks negatively affect children anthropometric measures and adults' self-reported health status, mental health, height, schizophrenia, and life expectancy.[2] The second strain of literature, developed mainly by economists, focuses instead on the long term socio-economic outcomes of early-life shocks. Most studies show evidence on how early shocks may result in poorer outcomes both in education and labor markets (e.g., Aguero and Deolalikar, 2012; Case et al., 2002, 2005; Case and Paxson, 2008; Lavy et al., 2016; Lee, 2014; Leon, 2012; Miller and Urdinola, 2010; Sanders, 2012; Shah and Steinberg, 2017; von Hinke Kessler Scholder et al., 2014).[3]

A common feature of many of these studies is the heterogeneity of their results across gender. Focusing on very recent studies, Akbulut-Yuksel (2017) studying the effect of WWII in utero exposure on recent health measures finds larger effects in the female sample. Similarly, Lavy et al. (2016) investigating the impact of a sudden migration of a large portion of the Ethiopian Jews to Israel

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[1] On the theory side, Heckman (2007) amended the standard Grossman (1972) model of the development of health capital to consider fetal origins, in which the production of health depends on health investments at different developmental stages of childhood. One implication of this model is that the effects of prenatal investment shocks may be non-monotonic in age and allow long term returns of early investments.

[2] See e.g., Akbulut-Yuksel (2017), Akresh et al. (2012a,b), Bobonis et al. (2016), Bozzoli and Quintana-Domeque (2014), Camacho (2008), Carlson (2015), Currie and Rossin-Slater (2013), Domingues and Barre (2013), Kesternich et al. (2014), Koppensteiner and Manacorda (2016), Lin and Liu (2014), Mansoor and Rees (2012), Minoiu and Shemyakina (2012), Torche (2011), van den Berg et al. (2016). Almond and Currie, 2011 provide an excellent review of this literature.

[3] Currie and Vogl (2013), reviewing this literature, also considers the effects of *early shocks for live children* on later outcomes, whereas Currie et al. (2014) reviews the literature focusing on the consequences of early *exposure to pollution*.

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(Operation Solomon) on the education of children in utero during the operation, reports significant improvements in education only for girls. van den Berg et al. (2016) instead find no effects of famine on the height of girls.

The accuracy of the estimates of the long term consequences of in utero shocks, and of their gender specific findings, hinges on the assumption that the composition of the population affected by the shock does not change as a result of the shock itself, i.e. that no selection occurs in utero *because of the shock*. Absent any shock during pregnancy, a share of fetuses at the bottom of the fetal health distribution are lost because their health endowment does not exceed the survival threshold, i.e. the level of health needed to reach birth. The resulting live births would generate the natural (secondary) sex ratio, i.e., the odds of a newborn's being male. Following the work of Trivers and Willard (1973), however, a number of studies suggest that in utero shocks (of different nature, including terrorism, pollution, maternal fasting, earthquake, cold waves) may increase fetal loss, and more specifically may reduce the number of boys relative to girls at birth, underlying the need to better grasp the mechanisms at play during pregnancy to correctly interpret outcomes emerging later in life (Shettles, 1961; Mizuno, 2000; Kraemer, 2000; Catalano et al., 2006; Roseboom et al., 2001; Almond et al., 2008; Hernández-Julián et al., 2014; Eriksson et al., 2010; Black et al., 2016; Fukuda et al., 1998; Torche and Kleinhaus, 2012; Catalano et al., 2006, 2008; Sanders and Stoecker, 2011; Almond and Mazumder, 2011).

Two separate mechanisms intertwine as a result of in utero shocks, both resulting in a reduction of the (secondary) sex ratio: the culling (or selection) and the scarring effects (Valente, 2015; Catalano and Bruckner, 2006).[4]

The culling effect directly links to the Trivers and Willard hypothesis. Trivers and Willard (1973) postulate that through the evolutionary process women developed the ability to (unconsciously) adjust their offspring's sex-ratio, favoring daughters when experiencing poor conditions, in order to maximize the number of surviving grandchildren. The logic behind the mechanism follows from the different reproduction probability for male and female individuals at times of hardship. In polygynous societies, having a daughter would maximize the reproduction chances of families not belonging to the elite. More formally, this implies that the additional losses of male fetuses in response to in utero negative shocks result from a "reduced maternal tolerance of males at the low end of a relatively constant distribution of survivability" (Catalano and Bruckner, 2006). In other words, the pre-shock distribution of fetal health would remain relatively unaffected, but the health survival threshold would move upwards and the sex ratio would change because the low end of the health distribution is disproportionally populated by male fetuses.

The scarring effect, instead, results from a downwards shift of the entire fetal health distribution as a consequence of the negative shock, reducing the health endowment of each fetus in the population at the time of the shock. Naturally, part of the distribution, which was marginally above the survival threshold before the shock, falls below the threshold and is lost in utero. As for the culling effect, since the low end of the health distribution is disproportionally populated by male fetuses, a reduction of the sex ratio follows. Even though both mechanisms result in a sex ratio reduction, they have different predictions on the health endowment of the surviving population: while the culling effect would generate a population of relatively more healthy individuals (notably among boys), the scarring effect would imply the opposite.

Based on these predictions a relatively narrow literature emerged recently to assess the degree of selection in utero and find evidence of culling vs. scarring effects. Catalano and Bruckner (2006) provide evidence of culling effect by showing that the sex ratio among Swedish cohorts born in the years 1751 through 1912 predicts male cohort life expectancy at birth. Similarly, Catalano et al. (2008) find that cold ambient temperatures during gestation predict lower secondary sex ratios and longer life span of males in annual birth cohorts composed of Danes, Finns, Norwegians, and Swedes born in the period 1878–1914, again suggestive of the culling effect operating.

Using sex differences in pollution-driven neonatal and infant mortality rates as an estimate of relative sex sensitivity, Sanders and Stoecker (2015) show that the 1970 Clean Air Act Amendments, by substantially improving air quality, caused a 0.47 percentage point increase in the probability of a live birth being male. Consistent with the culling effect, males display a greater post-natal mortality reductions than females.

Finally, Valente (2015) uses in utero exposure to the 1996–2006 Maoist insurgency in Nepal to assess culling and scarring effects. Her results show that exposed pregnancies are more likely to result in a miscarriage and in a female birth. Since exposed newborns do not seem to suffer of a poorer health (measured by a dummy for small babies and infant mortality), the author interprets her results as evidence of both scarring and culling effects operating.

Our study contributes to this recent literature by studying the effects of the recent civil war in the Democratic Republic of Congo (DRC) on mortality both in utero and during the first year of life. The contribution of this study to this literature is twofold. First, it addresses the potential endogeneity of conflict. Conflict is typically not randomly located and failing to properly account for this may lead to a bias in the estimated results. The fact that violence has been reported to target wealthier households in neighboring countries like Burundi (Bundervoet, 2010), Rwanda (Verpoorten, 2009), and Uganda (Blattman and Annan, 2010) suggests a bias that may push the estimated response of socioeconomic outcomes to conflict toward zero. Besides the likely nonrandomness of conflict, microlevel data on conflict events (based on news reports) may suffer from measurement errors. Conflict events in more remote and less connected locations will typically be underreported in the news and consequently in the data (Restrepo et al., 2006; Verpoorten, 2012). Controlling for the endogeneity solves these two issues at least partially. We instrument for conflict intensity using a mineral price index, which exploits the exogenous variation in the potential value of mineral resources generated by changes in world mineral prices to predict the geographic distribution of the conflict. Second, using estimates of civil war exposure on mortality across male and female newborns to assess their relative health, this study provides an additional test on the presence of culling and scarring effect as a consequence of in utero shocks.

In line with the above literature, our analysis confirms that in utero exposure (particularly during the first two trimesters of the pregnancy) increases fetal losses. More specifically, we find that conflict reduces the number of male live births, compatible with both scarring and culling effects. Zooming on the effects of conflict exposure on infant (0–12 months) mortality, however, reveals a gender imbalance in the impact. Strong and robust evidence, including mother fixed effects regressions, shows that conflict significantly increases infant mortality only among girls. Since newborn boys and girls are likely to undergo the same conflict exposure, we interpret our results as evidence of culling effect: maternal exposure to conflict during pregnancy determined an increase of the health survival threshold, with the consequence of increasing the average health endowment of the newborns, par-

[4] Notice that selection can occur also after birth, in the form of infant mortality affecting the weaker section of the cohort. This has been recently studied by Almond (2006) exploring the long term consequences of the 1918 Influenza pandemic in the US, and Bozzoli et al. (2009) reporting that the culling effect dominating in developing countries, resulting in even taller adults in response to early life shocks.

ticularly among boys, disproportionally populating the low end of the health distribution.[5] In other words, even though both boys and girls are exposed to conflict, only among (relatively weak) girls this increases mortality. As in Valente (2015) we cannot entirely exclude the presence of scarring effect.[6] However, if scarring did occur, its effects on the average health of the cohort exposed in utero was more than offset by the positive selection due to the culling effect.

Besides the literature discussed above, this study also contributes to the literature exploring the consequences of civil wars on children health. A large literature assesses the costs of civil wars for the affected societies, and argues that the impact of violence on the demography of a society can substantially increase the overall costs of a conflict, falling mainly on women and children (Ghobarah et al., 2003; Chen et al., 2008). The literature identifies a variety of factors which may negatively affect infants' health during a civil war. Malnutrition, resulting from the contraction of the internal supply of food and a partial collapse of trade in the regions in which violence unravels, for instance, worsens the general health status of the affected population (Alderman et al., 2006; Jenkins et al., 2007; Bundervoet et al., 2009; Akresh and Edmonds, 2011). Areas affected by violence are also characterized by losses of or poor access to health infrastructures, due to lack of equipment and human resources and health programs (such as prevention through vaccination and health education) are usually interrupted or implemented discontinuously, increasing the spread of vector-borne diseases. Finally, when displacement of large shares of a population occurs, lack of clean water and hygiene leads to a higher risk of diarrhea, one of the major causes of child morbidity and mortality.

In line with these intuitive considerations, infant mortality rates are regularly reported to increase in areas affected by civil war. The detrimental effect is consistent across humanitarian organization reports, medicine and economics literature (Toole et al., 1993; Goma Epidemiology Group, 1995; Danish Epidemiology Science Centre, 1999; Kiros and Hogan, 2001; Medecins sans frontieres, 2003; Coghlan et al., 2006; Guha-Sapir and van Panhuis, 2004; Guha-Sapir et al., 2005; Guha-Sapir and D'Aoust, 2010; Davis and Kuritsky, 2002; de Walque, 2005; Singh et al., 2005; Verwimp and Van Bavel, 2005). None of these studies, however, explicitly controls for the potential endogeneity of conflict location.

In the next section we provide the relevant background information on the armed conflict in the DRC. Section 3 presents the data. Section 4 lays out the empirical strategy, and results are presented in Section 5. Section 6 discusses two potential threats to our identification strategy, namely endogenous fertility decisions, and migration. The last section concludes.

2. Historical background

The DRC has experienced two of the most violent wars in recent history. The first Congolese war, which started at the end of 1996, is usually interpreted as a fight by the coalition of the Congolese rebellion led by Laurent-Desire Kabila with the foreign governments of Rwanda and Uganda not only to overthrow Mobutu but also to eradicate the presence of Rwandan Hutu refugees in eastern DRC, where they had escaped in the aftermath of the 1994 genocide (Vlassenroot and Raeymaekers, 2004; Prunier, 2009).

[5] In Appendix A, we test for gender discrimination behavior which may explain the gender unbalance in infant mortality, finding no support for it.

[6] The small and selected sample of children featuring anthropometric measures in the DHS 2007 covering the conflict period does not allow us to further dig into this issue.

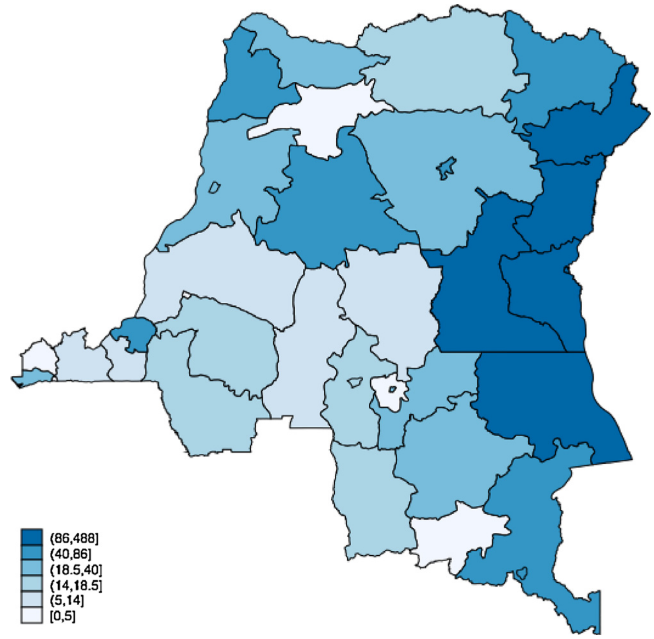

Fig. 1. Total conflict events by district in Democratic Republic of Congo, 1997–2004. *Source*: ACLED.

The second Congolese war unfolded between 1998 and 2004, with an astonishing estimated death toll of more than 3.8 million people (International Rescue Committee, 2011) and an estimated 1.7 million internally displaced people (Internal Displacement Monitoring Center, 2011). This magnitude of death and displacement is likely to have affected the health of infants. Interestingly, there is extensive anecdotal evidence of the role of minerals in shaping the dynamics of the conflicts, particularly during the second war (Congdon Fors and Olsson, 2004; Turner, 2007; Gambino, 2011; Stearns, 2011; Sanchez de la Sierra, 2013). This link will constitute the rationale of our instrumental variable strategy.

Fig. 1 reports the total number of conflict events recorded by ACLED in the period 1997–2004, aggregated at the district level. The provinces most heavily affected by the violence were Orientale and North and South Kivu (in the eastern part of the country), the areas in which the concentration of local and foreign armed groups was highest. Conflict was also concentrated in the territory of Pweto (Haut-Katanga, the southernmost district) in the Katanga province as well as in Kinshasa (the only dark region in the western part of the country in Fig. 1).

3. Data sources and sample construction

To assess the impact of in utero exposure to conflict we make use of the Demographic and Health Survey (DHS) on DRC carried out in 2007. Since we take advantage of the timing and location variations of conflict events, our main sample excludes children for whom we do not know exactly where their household was living at the time of their birth. This 2007 DHS dataset provides the number of years a household has been living in the village where the interview took place. Information about previous location is, however, not available, which means that we do not know whether households who migrated remained in the same district. In Appendix A (Table A3, A5, A7, and A9), we show our main results when including children belonging to nonresident households and children from an additional DHS wave released in 2014, whose main but critical weakness is to not distinguish between migrant and resident households.[7] We select those children in utero and born between 1997 and 2004, encompassing the two Congolese wars. DHS surveys are meant to be nationally representative and collect individual information on women aged 15 to 49 on education, demographic, and health issues as well as some information on the location of the interview, among which are GPS coordinates. Because only surviving women are interviewed, we are likely to underestimate the impact of conflict on infant health. Nonetheless, there is no reason to believe that this source of sample selection will affect gender imbalances in child mortality. Thanks to the inclusion of each woman's maternal history in the dataset, we have recovered detailed information such as when her children were born; whether they are still alive and if not, when they died; and whether they were part of a multiple birth. This enables us to create variables counting the number of brothers and sisters alive (of various age brackets) at the time of a child's birth.

For our panel regressions, we aggregate the above information to obtain a measure of the total number of births, the number of male and female births, the survival rates across boys and girls in the first year of life (defined as male or female (births-deaths)/births) for each month in each district.

We also take advantage of the geographical information linked to each DHS cluster to create three climatic variables. Given the emerging evidence on the links between weather shocks and violence (Hsiang et al., 2013), introducing these variables could potentially reduce the risk of confounding factors.

We include cumulative rainfall and average temperature based on monthly observations aggregated during the first 12 months of life of each child. In line with the use of weather data in economics, weather variables are expressed in terms of anomalies, i.e. deviation to long-term mean (that is, of the previous 25 years), divided by the correspondent long-term standard deviation (Dell et al., 2014; Marchiori et al., 2012; Barrios et al., 2010). The data used to construct the measure of precipitation and temperature come from Terrestrial Precipitation: 1900–2008 Gridded Monthly Time Series, Version 2.01, interpolated and documented by Matsuura and Willmott (2009). This dataset is a compilation of updated sources and provides monthly precipitation (and mean temperature) interpolated to a latitude/longitude grid of 0.5 degree by 0.5 degree.

We also create a third variable combining rainfall with daily temperature obtained from the Prediction of Worldwide Energy Resource (POWER) database of the US National Aeronautics and Space Administration (NASA): the number of months of potential malaria exposure in the first 12 months of life. To build such an index we apply the approach proposed by Kudamatsu et al. (2012). Four conditions have to be simultaneously satisfied for a month to be considered as malaria prone: The malaria index M_{dm} for district d and month m is set to 1 if and only if:

1 average monthly rainfall during the past 3 months is at least 60 mm,
2 rainfall in at least 1 of the past 3 months is at least 80 mm,
3 no day in the past 12 months has an average temperature below 5 °C, and
4 the average temperature in the past 3 months exceeds 19.5 °C plus the standard deviation of the monthly temperature in the past 12 months.

For the first stage of our instrumental variables estimates, we investigate the relationship between conflict events and mineral prices. To this end, we construct a panel dataset of conflict events and a price index. We filter the data from the Armed Conflict Location and Event Data Project (ACLED) dataset on the DRC and keep events from January 1, 1997, to December 31, 2004 (Raleigh et al., 2010).[8] A conflict event is defined as a single altercation wherein force is used by one or more groups for a political end (Raleigh et al., 2010). Thanks to the availability in the dataset of GPS coordinates for each conflict event, we assign each conflict to its respective district and time period, using the shapefiles on DRC from the Global Administrative Areas Database. For each month in the period considered, we create a district-level measure of conflict by summing all events taking place in a given district (*Conflict Event*).[9]

The data on mineral location is obtained from the mineral occurrences map of DRC, a comprehensive database on the mineral sector of the country provided by the geological service of the Royal Museum for Central Africa (RMCA) of Tervuren, Belgium. The database is the result of a recent effort to update and digitize the "Carte des gîtes minéraux du Zaïre", released in 1974 by the geological service of the country to allow for a general overview of mineral occurrences and potential (Lepersonne, 1974).[10] The intensive past exploration resulted in a vast amount of detailed reports and maps, most of which are held at the Royal Museum for Central Africa. Its archives contain most of the records of the former Geological Survey of Congo, the exploration and production records of mining companies and information based on its own fieldwork on the country. A systematic classification and assessment of this vast collection enriched substantially the map featuring 614 mineral deposits. The resulting database contains a considerably more detailed information on the mineral deposits in DRC as compared to any other existing source (e.g. the Metallogenic Map of Africa (2002), or the SNL dataset). Furthermore, by the very nature of the data collection, entries in the mineral occurrence map are less likely to be influenced by political processes and ongoing violence, as compared to data on contemporaneous mineral exploitations or mining concessions used in the recent literature (e.g. Maystadt et al., 2014).

We validate our first stage results using two alternative datasets on mineral locations, the US Geological Survey (USGS) and the SNL datasets, both used in the recent literature investigating the impact of minerals on civil war, but providing a much more restricted set of mineral deposits for DRC: 134 and 44, respectively.[11]

The location of ore deposits for various minerals is assigned to one of the 38 districts. We therefore know the mineral potential of each district of DRC, which we use to compute a price index.

Fig. 2 reports the geographical distribution by district of mineral location in DRC. Darker shaded districts feature more mineral deposits. Comparing it with Figure 1, it is rather striking that the

[7] Including migrants, as commonly stressed in the literature, is likely to create noise, as some migrants surveyed in the conflict affected region may not have experienced conflict at all, whereas some migrants in the control regions may have ended there escaping conflict after experiencing violence. In this respect, a critical weakness of 2013 DHS is to not distinguish between migrant and resident households. Moreover, recall data bias is much more likely to represent a problem in a survey collected 10 years after the end of the period studied. The lower coefficients in the specifications including the 2013 DHS are consistent with the attenuation bias expected, and substantiate our concerns.

[8] We exclude riots, defined in ACLED as violent, spontaneous grouping of unarmed groups, as they are not proper conflict events.

[9] ACLED reports the number of casualties only for a reduced subset of the events. An alternative source of conflict data from the Uppsala Conflict Data Program (UCDP—available at http://www.pcr.uu.se/research/ucdp/datasets/), which reports casualties, is put to use to offer robustness checks.

[10] The exploration surveys and mapping were conducted with the assistance of the UNDP, Belgium (mainly through the geological department of the Royal Museum for Central Africa of Tervuren) and France (through the Bureau de Recherches Géologiques et Minières).

[11] The USGS dataset is publicly available at: mrdata.usgs.gov/mineral-resources/mrds-global.html. The SNL dataset is available for sale at: snl.com/Sectors/MetalsMining.

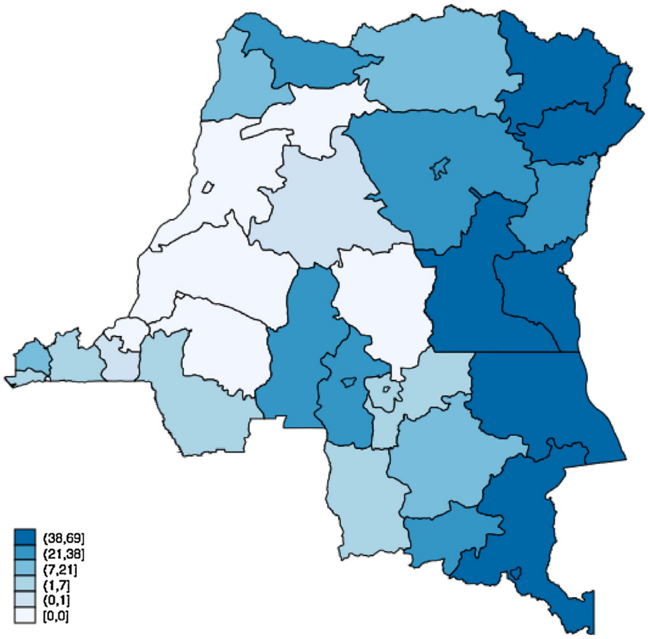

Fig. 2. Total mineral deposits by district in Democratic Republic of Congo. *Source*: Royal Museum for Central Africa of Tervuren, Belgium.

eastern part of the country, richer in minerals, experienced more conflict events. This correlation, however, does not account for time variations, which we intend to exploit by interacting the mineral potential of each district with the monthly price of the corresponding resource.

We turn to the United Nations Conference on Trade and Development (UNCTAD) dataset to get the monthly price series of 12 minerals (aluminum, copper, gold, iron, lead, manganese, nickel, oil, phosphate, tin, wolframite, and zinc) and compile information from *The Economics of Tantalum* (Roskill Information Services, 2009), Metal Pages, and the US Geological Survey to build our price series for tantalum. The number of potential extraction sites is interacted with the monthly mineral prices to obtain a time-varying measure of relative mineral value by district (*Price Index*). For each time period and district, we compute a price index taking account of the number of mineral deposits and the price for the set of 13 minerals, as follows:

$$Price\quad Index_{it} = \sum_{r} \omega_{ri} p_{rt}, \tag{1}$$

where $\omega_{ri} = minerals_{ri}/\sum_j minerals_{rj}$ is a weight measuring the relative importance of mineral r in district i with respect to all districts (j) and p_{rt} is the price of mineral r at period t with a price normalized to 100 for the first period (January 1997).[12]

Ideally, one would account for the size of the different deposits, which is likely to shape the response of conflict to price increase. For instance, lots of small mining sites in a district may be more easily contested by armed groups. Or alternatively, one large mining site may guarantee a much larger wealth in response to a price increase, but may also prove more difficult to conquer. However, despite the high quality of the mineral location dataset, the size of deposit is provided only for a small subset of the mining location. Since we have no information as to the realized or potential extraction of ores of each mineral location, we decided to weigh each monthly ore price by a ratio of the number of mineral deposits in the district over the total number of deposits of this particular mineral in the country. This way, we intend to proxy the extraction potential by ore of each district. We sum this potential over all minerals in a price index in order to reflect the district resource endowment value at each point in time. In other words, we capture how the monthly change in mineral prices alters the relative potential value of the mining sector across districts. The idea of using international commodity prices to predict local conflict is well established in the literature (Dube and Vargas, 2013; Berman et al., 2015; Maystadt et al., 2014). The strategy adopted here is similar to the one adopted by Bruckner and Ciccone (2010) in their study on conflict in sub-Saharan Africa.[13]

The main variables used in our analysis are described in Table 1. In order to obtain a nationally representative dataset, we resort to sampling weights provided in the DHS. These are needed to render the estimates independent from the sampling design.

4. Empirical strategy

The ultimate goal is to assess the presence of culling vs. scarring effects resulting from in utero exposure to conflict. Since the timing and location of conflict events is likely to be nonrandom and correlated to our dependent variable, we turn to an instrumental variable analysis. We first discuss in detail the first-stage relationship, wherein we predict conflict distribution based on the change in mineral prices. We then use this instrument to assess the impact of conflict on in utero and infant mortality.

4.1. Conflict

In the context of conflict in the DRC, we expect resources to shape the distribution of violence. We therefore use the previously described price index to predict the intensity of conflict by district and over time. Formally, the first-stage specification is as follows:

$$Conflict\quad Events_{dm} = \alpha_d + \beta_m(+\delta_d m) + \gamma Price\quad Index_{dm} + \varepsilon_{dm}, \tag{2}$$

where $Conflict\quad Events_{dm} = \sum_{j=0}^{11} Conflict\quad Events_{dm+j}$ is our preferred measures of conflict, and $Price\quad Index_{dm} = \sum_{j=0}^{11} Price\quad Index_{dm+j}$. Indeed, given the interest of this study, we run the first stage to predict, for each district d and month m in the sample, the conflict distribution over the following 12 months.[14] We control for district fixed effects, α_d, time fixed effects, β_m (a set of 96 dummies, one for each year-month in our sample), and, in some specifications, district-specific linear time trends, $\delta_d m$.[15]

The conflict literature identifies several channels through which the change in mineral prices can affect the intensity of violence. Intuitively, a price increase of a specific mineral raises its relative value, thereby increasing the incentives for armed groups to re-allocate their fighting effort toward controlling mining sites in which that mineral is extracted. This first channel, named by Dube and Vargas (2013) the rapacity effect, would then predict a positive relationship between mineral prices changes and conflict intensity (Berman et al., 2015; Maystadt et al., 2014).

[12] An alternative weighting matrix, $\omega_{ri} = minerals_{ri}/\sum_M minerals_{Mi}$, focussing on the district relative dependence from specific minerals (r) with respect to all minerals (M) in the district rather than the aggregate relative value across districts (and over time), also generates a significant (but substantially weaker) first stage.

[13] A logarithmic version of our price index produces similar results.

[14] In Fig. A1 in Appendix A, we report the average price index, the average price index computed on the 3TG minerals only, and the average level of Conflict Events over the period considered. We also report the average price index and Conflict Events for four of the most troubled districts in DRC in Fig. A2.

[15] In Appendix A we also adopt an alternative measure of conflict, which records for each month the number of months featuring violence in the districts over the following 12 months.

Table 1
Descriptive statistics.

Variable	N	Mean	Std. dev.	Min.	Max.
District level					
Number of births	2598	3.691	3.916	0.0297	32.534
Number of boy births	2598	1.834	2.386	0	20.366
Number of girl births	2598	1.857	2.492	0	28.760
Survival rate	2598	0.893	0.2372	0	1
Boy survival rate	2064	0.885	0.270	0	1
Girl survival rate	2007	0.905	0.250	0	1
Conflict events (12 m)	2598	5.955	12.890	0	118
Conflict events in utero (9 m)	2598	4.392	10.073	0	103
Conflict events in utero (1st and 2nd trimesters)	2598	2.960	7.328	0	76
Conflict events in utero (3rd trimester)	2598	1.431	4.237	0	53
Price Index (12m)	2598	422.240	595.557	0	3074.484
Individual level					
Boys birth	9496	0.504	0.500	0	1
Infant mortality	10,397	0.107	0.310	0	1
Boy Mortality	5242	0.117	0.321	0	1
Girl Mortality	5155	0.0976	0.297	0	1
Conflict events (12 m)	10,397	7.877	14.470	0	118
Conflict events in utero (9 m)	9,496	5.973	11.238	0	103
Conflict events in utero (1st and 2nd trimesters)	9,496	4.045	8.415	0	76
Conflict events in utero (3rd trimester)	9,496	1.928	4.825	0	53
Price Index (12 m)	10,397	495.967	630.795	0	3074.484

The second channel, centered around the concept of state capacity, focuses on the incentives of armed groups controlling mining sites for taxation, or more generally rent-seeking behavior. According to this mechanism, armed groups controlling mining sites would re-allocate part of their effort away from fighting toward taxing the local mining activity, in response to a price increase of minerals they control (Parker and Vadheim, 2016; Sanchez de la Sierra, 2013).[16] The relationship between mineral prices changes and conflict intensity would in this case be negative: armed groups reduce their violence to focus on taxing the mineral whose relative value increased.

The third channel, referred to as the opportunity cost effect in the literature, focuses on the supply side of the market for fighters (Dube and Vargas, 2013). An increase of mineral prices, by increasing the wage or creating more job opportunities in the local mining sector, would reduce the pool of individuals willing to join a rebel group and engage in violence. As for the previous mechanism, the relationship between mineral prices changes and conflict intensity would in this case be negative: higher mineral prices reduce the size of armed groups which map into an overall lower level of violence.

The estimate of γ in our first stage equation is likely to capture the joint effect of these mechanisms, and the sign of the coefficient will ultimately help us detecting the relative importance of the various effects.

4.2. In utero mortality

The predicted conflict is first used to estimate the impact of in utero exposure to conflict on in utero mortality. In particular, we estimate the effect of in utero exposure to conflict on the number of male and female births, and the probability for a newborn to be a boy, to test whether male and female fetuses are differently affected.

We implement two sets of regressions. The first set uses a collapsed version of the dataset at the month-district level to run panel level regressions (the observation in this context is a district-month). The second set of tests fully exploits the richness of the dataset to run cross-sectional regressions in which the observation is a child.

We run the following fixed-effect panel regressions:

$$V_{d\,m} = \alpha_d + \beta_m + \gamma Conflict_{dt} + \varepsilon_{d\,m}. \tag{3}$$

where m still denotes the month of birth, and t specifies the time bracket over which the conflict events are aggregated. As explained, we correct for endogeneity with two-stage least squares, instrumenting conflict by mineral prices, and estimate the following system of equations:

$$\begin{aligned} V_{d\,m} &= \alpha_d + \beta_m + \gamma Conflict_{dt} + \varepsilon_{d\,m} \\ Conflict_{dt} &= \delta_d + \rho_m + \eta Price\ Index_{dt} + \epsilon_{d\,m}. \end{aligned} \tag{4}$$

where $V_{d\,m}$ will be the number of (male/female) births in district d and month m when checking for the impact in utero exposure.

The alternative cross-sectional approach estimates the following model, in which the unit of observation is a child, i, born at month m, in district d.

$$W_{idm} = \alpha_d + \beta_m + \delta' X_i + \lambda' X + \gamma Conflict\ Events_{dm} + \varepsilon_{idm}, \tag{5}$$

where X_i is a vector of control variables including whether the child was part of a multiple birth, whether she was the first child, her number of siblings alive by gender and age groups, a malaria index (summing the number of months of exposure to malaria), and rainfall and temperature anomalies (with respect to a long-term average of 25 years). A further set of variables, X, controls for household characteristics including the education level of the mother, being a female headed household, the size of the household and a measure of the wealth of the household.

To deal with the potential endogeneity of conflict distribution, the conflict intensity measure is instrumented by the mineral price index described above. Formally, the following system of two equations is estimated by two-stage least square:

$$\begin{aligned} W_{idm} &= \alpha_d + \beta_m + \delta' X_i + \lambda' X + \gamma Conflict\ Events_{dm} + \varepsilon_{idm} \\ Conflict\ Events_{dm} &= \delta_d + \rho_m + \eta' X_i + \theta' X + \nu Price\ Index_{dm} + \upsilon_{idm}. \end{aligned} \tag{6}$$

where W_{idm} will be the probability to be a boy/girl when checking for the impact in utero exposure.

[16] It is worth pointing out that the literature discussing the latter channel relies exclusively on the Congo wars and empirical data from them to develop their arguments. A similar trade-off between rent-seeking (or taxation) and conflict is also described in the theoretical framework of De Luca et al. (2011) and Maystadt et al. (2014).

4.3. Infant mortality

We next study infant mortality rate during the first year of life to assess the presence of scarring and culling effects. Formally we replicate the estimation of models (4) and (6) where V_{dm} and W_{idm} will be the first year (male/female) survival rate in district d and month m, and (male/female) mortality, respectively. The latter is defined as the mortality of children by 12 months of age: we check 12 months after her birth whether she is still alive and assign value 1 to our binary variable $Mortality_{idm}$ if child i died during the first 12 months of her life.

The idea behind this empirical strategy is to exploit the timing and spatial variations of conflict events and to compare children born in the same month in districts affected differently by conflict. Standard errors are clustered at the district level, and sampling weights are used to render the estimates independent of the sampling design. Given the small number of clusters ($n = 38$) which might produce underestimated intra-group correlation, we turn to 1000 replications of wild bootstrap (percentile-t method), known to resist to heteroskedasticity, to compute confidence intervals (Cameron et al., 2008). As mentioned before, the regressions are run on the sample of children known to have been born in their mother's interview district.

Beyond the power of the instruments, this empirical strategy is based on a key identifying assumption. If mineral prices were to influence infant health in utero and the post natal year through another channel than the occurrence of violence, this would violate our exclusion restriction. While the relative weight of the first two mechanisms (rapacity vs. state capacity effects) does not affect the overall validity of our IV strategy, the opportunity cost channel may represent a serious threat to our empirical strategy. If an increase in mineral prices reduced violence by creating higher income opportunities in mining for locally sourced fighters, the potential income effects generated may directly affect infant mortality, thereby violating our exclusion restriction. There is a recent literature showing that starting mining activities in the context of developing countries may have some beneficial effects on the welfare of the population living nearby (Kotsadam and Tolonen, 2016). We consider this threat very seriously. First, even though mining industries are found to increase local welfare *during peace*, the local effects of a mineral price increase are theoretically much more ambiguous in the lawless context of an ongoing conflict, in which the vast majority of mining activities are directly or indirectly controlled by armed groups. More specifically, it is unclear what share of the surplus, generated by the price increase, would be captured by the different agents involved in the business (e.g., miners, taxing armed group, traders). The work by Parker and Vadheim (2016) and Sanchez de la Sierra (2013), focusing both on DRC conflicts, suggest that armed groups may adjust their taxing behavior fairly quickly in response to changes in the profitability of mining.

Second, our second-stage point estimates are roughly unchanged when controlling for household characteristics such as the education of the mother or household wealth, variables usually capturing household income. Such a heuristic approach to assess the risk of omitted variable is further supported by Oster (2016) bounding validation check, documented in Section 5. Even though the wealth index may fail to entirely capture transitory income shocks linked to mineral prices, it is a further indication that there may be not much of an income effect after all.

Third, assuming the potential income effect to be naturally concentrated in the areas around the mines we directly assess its impact by replicating the estimation of our empirical models in which the children living within 10, 20, and 30 km from the mineral deposits are removed from the sample. If income effects are present, we should observe a dramatic change in our estimates when removing the areas most concerned by the mining activities. The choice of the relevant distance (up to 30 km) is informed by the existing literature. Kotsadam and Tolonen (2016), studying the welfare effect of mining, restrict the analysis in the area within 20 km from the mines, arguing that beyond this distance they partially fail to capture the mine footprint. Furthermore, the existing literature on commuting distances in urban and rural Africa shows that areas of 5, 10 or 15 km are likely integrated markets (see e.g., Amoh-Gyimah and Aidoo, 2013; Kung et al., 2014, and Schafer, 2000).

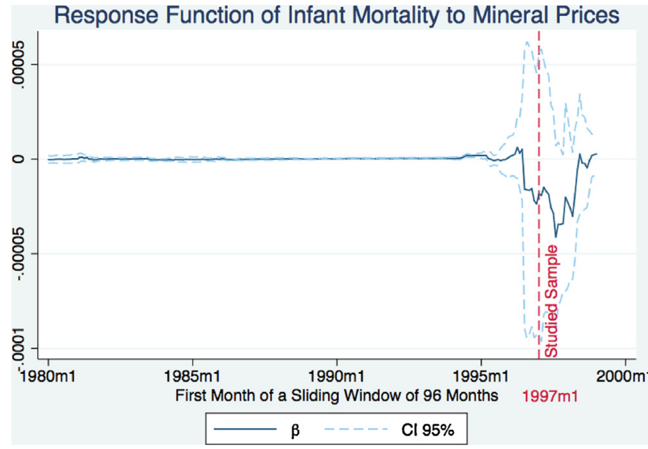

Fig. 3. Placebo test on full sample.

Fourth, even though our first stage relationship works well also if we predict conflict with the lagged price index or the average price index computed over the previous months, we chose to use the contemporaneous price index as a deliberate strategy to reduce the likelihood of income effects, which arguably would emerge with a lag.[17]

Finally, to further reinforce the plausibility of our exclusion restriction, we proceed to a placebo test. Regressions of infant mortality, our key variable used to disentangle between culling and scarring effects, on the same price index over the period 1981–1996 showed that the exclusion restriction does not seem to be at risk.[18] In particular, we fix the length of the sample to 96 months, as in the main analysis. Next, we run regressions starting on the first 96 months of the pre-conflict sample (January 1981–December 1988). Moving each time by one month our sliding window of 96 months from the start of the sample until the end, we test the reduced-form relationship in 95 regressions for the pre-conflict period.[19] The results of this exercise are graphically represented in Figs. 3–5, where on the horizontal axis we represent the first month of each 96-months sample considered, and on the vertical axis the corresponding estimated coefficient for the reduced form. The dashed lines define the 95% confidence interval. Fig. 3 uses the full sample of boys and girls, whereas Figs. 4 and 5 look at the reduced form among boys and girls only. In all three figures, the vertical dashed line identifies the starting point of the period of investigation. It is striking to see how the coefficients are consistently around zero in the pre-conflict period. The negative response of girls' mortality to mineral price shocks is particularly strong during the period of investigation, contrasting with the positive response for boys. Such a result is difficult to conciliate with price-induced wealth effects, as the one suggested by the opportunity cost channel.

[17] In Table A1 in Appendix A we report alternative first stage results with lagged and averaged price index.

[18] Infant mortality for this period is reconstructed retrospectively from the DHS.

[19] So, the second regression would focus on the period February 1981–January 1989, the third on March 1981–February 1989, and so forth.

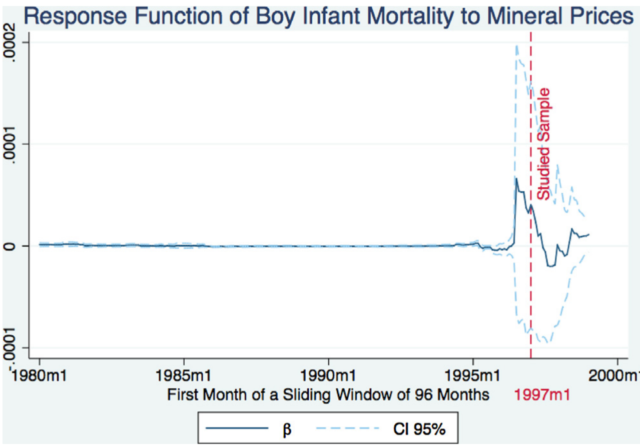

Fig. 4. Placebo test on boys sample.

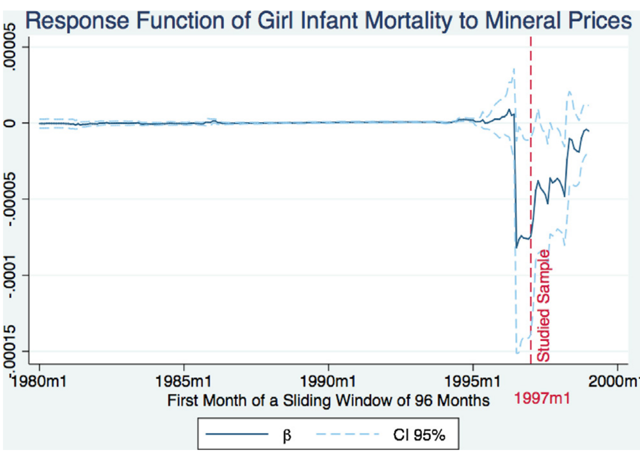

Fig. 5. Placebo test on girls sample.

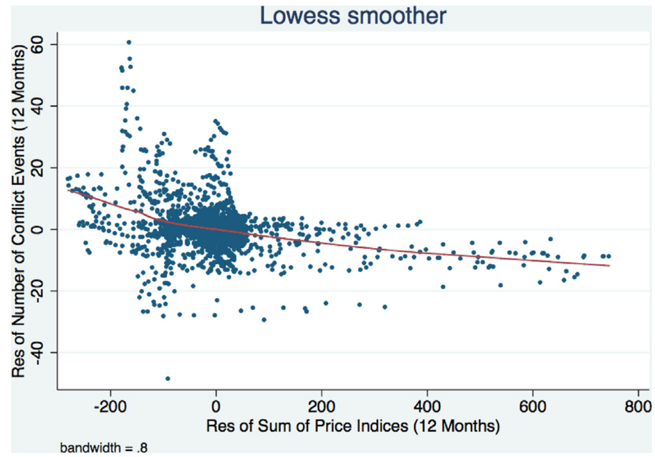

Fig. 6. Lowess estimation of first stage.

5. Results

5.1. Conflict

Table 2 exhibits that at the district level, the relationship between mineral prices and conflict events is highly significant and negative (first column and row of Panel A, which represents our benchmark first-stage results). This suggests, in line with the findings by Parker and Vadheim (2016) and Sanchez de la Sierra (2013), that in the context of DRC the rapacity effect is not the dominating mechanism mediating between mineral prices and conflict. In response to increasing mineral prices, armed groups redirect their effort from fighting to rent-seeking activities on the mining industry, thereby reducing the intensity of the violence.

Parker and Vadheim (2016) clearly argue that armed groups redirect their time allocation away from fighting to rent-seeking activities, whenever the latter secure larger profits. In the specific context of Eastern DRC, using the words of Sanchez de la Sierra (2013), "in response to an increase in the price of coltan, armed groups [. . .] start taxing and providing protection in the mines where coltan is produced. [. . .] Coltan is easy to tax [. . .] through taxation of output at the mines, simple to administer through custom borders, and carry minimal administrative growth". Similarly, he also discusses how the armed group NDC-R, which controlled an area in Eastern DRC, sustained its local monopoly of violence by collecting a number of mining output taxes and by issuing mining permits.[20]

To test the reliability of this negative relationship, we run a set of further robustness checks on the first stage. In the second row of column 1, we exclude tantalum from the price index, given the alleged relevance of this mineral in the conflict dynamics as stressed in the international press.[21] Conversely, in the third row we compute the price index only on tantalum. The fourth row focuses on the four "conflict" minerals, as defined by United States Geological Survey ("3TG" which stand for tantalum, tin, tungsten and gold) possibly due to a different level of extractability.

In column 2, we restrict the analysis to the 19 districts (half of DRC districts) hosting most of the violence in the period considered.[22] It is quite remarkable that despite the reduction in sample size, the relationship appears to be very strong in the 19 most conflict-exposed districts. In column 3 we restrict the analysis to the 19 districts richest in minerals (based on the price index).

In column 4–6 we focus even more on the role of tantalum in the relationship between mineral prices and conflict. In column 4 the analysis is conducted only on districts with tantalum deposits. Column 5 restricts the sample to the 5 districts most influenced by the tantalum price changes, and the last column looks at the districts excluded from column 5. Although somewhat weaker, the relationship remains negative when these 5 districts are excluded.[23] Hence, the robust negative relationship does not seem to be driven by a few districts or specific minerals, nor we find any evidence of non-monotonicity.

Adding linear district-specific time trends (Panel B) largely confirms the pattern presented in Panel A.

We also test the first stage using a nonparametric smoother (a locally weighted smoother, specifically the lowess estimator) on the demeaned and detrended versions of the conflict and mineral price measures. The result of this exercise, reported in Fig. 6, confirms that the linear specification constitutes a good approxi-

[20] Raeymaekers et al. (2008) also argues that armed groups in DRC develop state-like governance.

[21] Interestingly, removing any one mineral at a time from the analysis does not alter the results.

[22] Removing one district at a time does not qualitatively change the findings.

[23] Notice that tantalum districts typically also have other minerals. This explains, for instance, the negative and significant coefficient in the second row of column 4.

Table 2
Panel regressions on the impact of mineral prices on conflict.

Dependent variable	Conflict events					
Districts included	All	Most violent 19	Minerals richest 19	TA districts	5 districts with most TA price variation	Districts excluded in column (5)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: No trend						
Price index	−0.012***	−0.0154***	−0.0151***	−0.0218***	−0.0415**	−0.00406
	(0.00391)	(0.0048)	(0.00496)	(0.00393)	(0.0108)	(0.00251)
F-statistic	9.47	10.30	9.30	30.70	14.90	2.62
Price index - No TA	−0.00618**	−0.00874**	−0.00896**	−0.00755	−0.039*	−0.00441*
	(0.00256)	(0.00343)	(0.00312)	(0.0107)	(0.0166)	(0.00251)
F-statistic	5.85	6.50	8.24	0.50	5.55	3.09
TA price index	−0.0298***	−0.0303***	−0.0322***	−0.0309***	−0.0445**	0.144
	(0.00554)	(0.00619)	(0.00596)	(0.00591)	(0.0124)	(0.118)
F-statistic	28.80	24.00	29.10	27.30	12.90	1.51
3TG price index	−0.0237***	−0.0236***	−0.0256***	−0.0243***	−0.0341***	0.00864
	(0.00362)	(0.00403)	(0.004)	(0.00388)	(0.00681)	(0.037)
F-statistic	43.10	34.50	41.20	39.30	25.00	0.05
Panel B: Linear trends						
Price index	−0.0167***	−0.0189***	−0.0177***	−0.0251***	−0.0292	−0.00638**
	(0.00358)	(0.00315)	(0.00353)	(0.0036)	(0.0144)	(0.00298)
F-statistic	21.70	36.00	25.10	48.50	4.10	4.60
Price index - No TA	−0.0124***	−0.0156***	−0.0116***	−0.0286***	−0.025	−0.00719**
	(0.00395)	(0.00424)	(0.00394)	(0.00939)	(0.0251)	(0.00276)
F-statistic	9.90	13.50	8.65	9.26	0.99	6.76
TA price index	−0.0245***	−0.0244***	−0.0266***	−0.0256***	−0.0362***	0.152
	(0.00298)	(0.00371)	(0.00344)	(0.00339)	(0.00759)	(0.117)
F-statistic	67.50	43.30	59.80	57.00	22.80	1.69
3TG price index	−0.0251***	−0.0246***	−0.0263***	−0.0259***	−0.0234	0.0237
	(0.00303)	(0.00335)	(0.00337)	(0.00339)	(0.0164)	(0.0438)
F-statistic	68.80	54.20	60.70	58.30	2.05	0.29
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
N	3648	1824	1824	2112	480	3168

F-stats in square parentheses; FE = fixed effects; TA = tantalum; 3TG: Tantalum, Tin, Tungsten, Gold. For each panel, in the second row we exclude TA from the price index; in the third row we compute the price index only on tantalum; rows 4–6 replicate rows 1–3 but predict conflict in a 12-month period based on the sum of the price variations over a 12-month period. In column 2, we restrict the analysis to the 19 districts hosting most of the violence in the period considered; in column 3 we restrict the analysis to the 19 districts richest in minerals; in column 4 the analysis is conducted only on districts with tantalum deposits; in column 5 we restrict the sample to the 5 districts most influenced by the tantalum price changes; column 6 looks at the districts excluded from column 5. Panel B includes linear district-specific time trends.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

mation of a potentially more complex relationship.[24] The negative relationship is again confirmed using a nonparametric approach.[25]

Finally, in Table A2 in Appendix A we replicate our main first stage estimates on two alternative datasets adopted in the related literature: the USGS and SNL datasets.[26] The sign of the coefficients remain consistently negative across the different datasets, but it is non significant for both USGS and SNL. The non-negligible differences in the number of mining sites across the three datasets can account for the lack of statistical significance.

5.2. In utero mortality

Table 3 reports the results of our panel regressions on the impact of in utero conflict exposure on the number of births. Panel A displays the results of estimating the model with district and month-year fixed effects only, whereas Panel B includes also climatic variables. In the first two columns we test the impact on the aggregate number of births, while the next 4 columns reports the results specific to boys and girls. The overall number of births decreases as a result of in utero exposure, although the coefficient is never statistically significant for the 2SLS regressions. In line with the literature, we find an heterogeneity of the impact across gender. In utero conflict exposure significantly reduces the number of live male births, which is compatible with both scarring and culling effects.[27]

We find strong evidence for the nonrandom distribution of violence. Table 3 suggests a bias toward zero of 'naive' OLS regressions. These biases affecting the OLS estimates suggest that some unobserved factors simultaneously explain the number of conflict events by district and child mortality. A potential explanation may be that conflicts are more likely to target wealthier (and therefore healthier or more able to cope with adverse shocks) households in a looting-driven warfare. These biases may also arise from measurement errors.[28]

[24] Reducing the bandwidth of this estimator from 0.80 to 0.20, and hence producing less smoothing by focusing on closer points in the local regressions, leaves the general trend unchanged.
[25] In Table A1 (Panel C) we also show that our first stage is robust to adopting a non linear (Poisson) model.
[26] We thank Andreas Kotsadam for running these tests on the SNL dataset.

[27] A similar test on the effect of in utero exposure to conflict on sex ratio generates compatible results, with sex ratio decreasing in response to in utero conflict exposure. Given the nature of our test and data, however, sex ratio may be calculated off of too few births in several month-districts. We therefore prefer the number of birth results. We thank an anonymous referee for pointing out this potential issue.
[28] In the appendix Table A3 we report the results of several further tests including the use of separate month and year fixed effects (column 1), the addition of linear district-specific time trends (column 2), the inclusion of nonresident households in the sample and the 2013 DHS (column 3), and the adoption of a count (Poisson) model on the resident sample (column 4) and on the extended sample including nonresidents and 2013 DHS (column 5). In Table A4 we report the results of re-

Table 3
Panel regression of the impact of in utero exposure to conflict on number of births.

Dependent variable	Number of births					
	All		Boys		Girls	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Panel A: No controls						
Conflict Events in utero	−0.0106	−0.0442	−0.0062	−0.019	0.0043	0.0252
	(0.0064)	(0.033)	(0.0028)**	(0.0108)*	(0.0061)	(0.0263)
	[−0.032, −0.0000]**	[−0.13, 0.027]	[−0.013, 0.0033]	[−0.042, 0.002]*	[−0.025, 0.0059]	[−0.09, 0.034]
Kleibergen-Paap rk Wald F		21.30		21.30		21.30
Panel B: Climatic controls						
Conflict Events in utero	−0.0107	−0.0457	−0.0063	−0.0208	0.0043	0.0249
	(0.0069)	(0.0331)	(0.0028)**	(0.0118)*	(0.0067)	(0.0253)
	[−0.033, 0.0012]	[−0.13, 0.029]	[−0.012, 0.0034]	[−0.046, 0.0025]*	[−0.027, 0.0079]	[−0.088, 0.034]
Kleibergen-Paap rk Wald F		20.30		20.30		20.30
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
N	2598	2598	2598	2598	2598	2598

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$; Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of t-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Climatic controls include rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.

Table 4
Panel regression of the impact of in utero exposure to conflict by trimester on number of births.

Dependent variable	Number of births					
	All		Boys		Girls	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Panel A: No controls						
Conflict Events in utero	−0.0142	−0.0257	−0.0088	−0.0709	−0.0054	0.0452
(1st and 2nd trimesters)	(0.012)	(0.052)	(0.0048)*	(0.0477)	(0.0089)	(0.0524)
	[−0.053, 0.01]	[−0.15, 0.093]	[−0.023, 0.001]*	[−0.24, −0.0023]**	[−0.034, 0.011]	[−0.041, 0.25]
Conflict Events in utero	−0.0046	−0.0811	−0.0012	0.0857	−0.0034	−0.167
(3rd trimester)	(0.0212)	(0.151)	(0.0112)	(0.106)	(0.0127)	(0.137)
	[−0.059, 0.044]	[−0.49, 0.37]	[−0.026, 0.033]	[−0.043, 0.228]	[−0.033, 0.025]	[−0.406, 0.069]
Kleibergen-Paap rk Wald F		1.66		1.66		1.66
Panel B: Climatic controls						
Conflict Events in utero	−0.0149	−0.0227	−0.0088	−0.0713	−0.0061	0.0487
(1st and 2nd trimesters)	(0.012)	(0.0492)	(0.0048)*	(0.0438)	(0.0091)	(0.0494)
	[−0.055, 0.0087]	[−0.14, 0.09]	[−0.023, 0.0012]*	[−0.21, −0.0051]**	[−0.036, 0.01]	[−0.038, 0.23]
Conflict Events in utero	−0.0034	−0.0911	−0.0015	0.0788	−0.0019	−0.17
(3rd trimester)	(0.0214)	(0.144)	(0.0114)	(0.097)	(0.013)	(0.132)
	[−0.057, 0.044]	[−0.48, 0.3]	[−0.027, 0.034]	[−0.079, 0.55]	[−0.031, 0.029]	[−0.75, 0.053]
Kleibergen-Paap rk Wald F		1.96		1.96		1.96
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
N	2621	2621	2621	2621	2621	2621

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$; Standard errors clustered at the district level reported in parentheses; *p*-Values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Climatic controls include rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.

Following the related literature, we further investigate whether conflict exposure is particularly detrimental when it occurs during the early months of pregnancy. In Table 4, exposure to conflict is separately captured by two variables: conflict events during the first two trimesters of the pregnancy, and conflict events during the last trimester.[29] OLS results closely track the ones reported in Table 3, and confirm that indeed the first part of the pregnancy seems to be more sensitive to negative shocks. IV results are also consistent with the findings in Table 3, with conflict exposure during the first part of the pregnancy reducing the number of boy births. However, due to a combination of the selected sample

estimating the models in Table 3, using three alternative measures of conflict: the number of months featuring violence during the first year of life (ACLED), a corresponding variable created using the UCDP dataset, and the number of fatalities recorded by UCDP.

[29] The alternative strategy of capturing the first vs. the last two trimesters produces very similar but statistically less significant coefficients.

Table 5
The impact of in utero exposure to conflict by trimester on the probability of boy birth.

Dependent variable	Boy birth	
	OLS (1)	2SLS (2)
Panel A: No controls		
Conflict Events in utero (1st and 2nd trimesters)	−0.0005	−0.0127
	(0.0010)	(0.0087)
	[−0.0023, 0.0032]	[−0.041, −0.0001]**
Conflict Events in utero (3rd trimester)	0.0002	0.0262
	(0.0013)	(0.0171)
	[−0.0019, 0.005]	[−0.0037, 0.099]*
Kleibergen-Paap rk Wald F		2.45
Panel B: Full set of controls		
Conflict Events in utero (1st and 2nd trimesters)	−0.0005	−0.0138
	(0.0011)	(0.0083)*
	[−0.0026, 0.0036]	[−0.041, −0.0016]**
Conflict Events in utero (3rd trimester)	−0.0003	0.0266
	(0.0014)	(0.0166)
	[−0.0026, 0.0048]	[−0.0044, 0.097]*
Kleibergen-Paap rk Wald F		2.63
District FE	√	√
Month-year FE	√	√
N	9496	9496

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$; Standard errors clustered at the district level reported in parentheses; p-values are calculated on the empirical distribution of t-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.

(district-months in which some births occurred) and the need to instrument for two endogenous variables, our first stage becomes very weak, casting some doubts on the reliability of the IV results.

Table 5 presents further evidence of the gender specific impact of in utero shock produced by our cross-sectional regression. It shows that the probability for a newborn to be a boy is negatively affected by in utero exposure to conflict during the first two trimesters of pregnancy.[30] The positive and weakly significant coefficient for the conflict exposure during the third trimester may suggest the strong selection among boys occurring during the first part of the pregnancy may reduce the vulnerability of the survived boys during the last part of the pregnancy. But again, the low Kleibergen-Paap *F* statistic suggests caution in over-interpreting these results.

Overall the results of this section are consistent with a relatively well established result in the literature: (early) in utero exposure to negative shocks affects male fetuses disproportionally thereby reducing the number of male fetuses reaching birth. As argued in the introduction, since the low end of the fetal health distribution is more populated by male individuals, this pattern may result from two different mechanisms: the scarring effect, worsening the health of the entire distribution causing the individuals marginally above the survival threshold to fall below it, and the culling effect, whereby the survival threshold itself moves upwards claiming some fetal losses among the relatively weaker individuals. While the scarring effect would leave the affected cohort with a relatively poorer health, the culling effect would instead imply a positive selection on the surviving population, i.e. the newborn should feature on average better health, particularly boys. In the next section, we estimate the impact of conflict exposure on infant mortality (survival) rates to assess the presence of these two mechanisms.

5.3. *Infant mortality*

Table 6 reports the results of our panel regressions on the effects of conflict exposure on survival rates, for all children (columns 1–2), only boys (columns 3–4) or only girls (columns 5–6). Again, OLS results are biased toward zero. The results are gender specific, suggesting that the live infant population may start off with a different average health endowment. Exposure to conflict reduces survival rates only among girls.[31]

Exploiting the rich set of information in our dataset, Table 7 presents our preferred estimates, the cross-section regressions over the sample of children born between 1997 and 2004, for all children (columns 1–2), only boys (columns 3–4) or only girls (columns 5–6). Once more, we find strong evidence for the non-random distribution of violence. Table 7 suggests a bias to zero of 'naive' OLS regressions.[32]

The results confirm a gender-specific impact of conflict episodes on child mortality. Girls are more adversely affected by conflict than boys. According to column 6, a change by 1 standard deviation in the number of conflict events (that is, by about 14 conflict events) increases the probability of a girl's dying within the first year of life by about 8 percentage points. The magnitude of this effect is far from trivial, inasmuch as it constitutes almost a doubling of girls mortality (at mean value). Adding the full set of controls in Panel B of Table 7 leaves the coefficients of interest virtually unchanged (slight increase of the coefficient and the power of our instrument).[33] That is very reassuring with respect to the exclusion restriction. As discussed in Section 4, our exclusion restriction

[30] In the appendix Tables A5 and A6 we report the results of adopting alternative specifications and alternative measures of conflict, respectively.

[31] In the appendix Tables A7 and A8 we show the robustness of our results in Table 6 to alternative specifications and the adoption of alternative measures of conflict, respectively.

[32] In the appendix Tables A9 and A10 we show the robustness of our results on girls' mortality rate in Table 7 to alternative specifications and the adoption of alternative measures of conflict, respectively.

[33] For presentation purposes, coefficients of control variables are not shown here. Being part of a multiple birth and being a first child increase mortality. On the con-

Table 6
Panel regression of the impact of in utero exposure to conflict on survival rates.

Dependent variable	Survival rate					
	All		Boys		Girls	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Panel A: No controls						
Conflict Events	0.0001	−0.0033	0.0004	−0.0001	−0.0008	−0.0045
	(0.0004)	(0.0031)	(0.0006)	(0.0045)	(0.0008)	(0.0018)**
	[−0.0011, 0.0011]	[−0.016, 0.0032]	[−0.0007, 0.0024]	[−0.022, 0.01]	[−0.0026, 0.0008]	[−0.0098, −0.0011]**
Kleibergen-Paap rk Wald F		8.97		5.23		8.00
Panel B: Climatic controls						
Conflict Events	0.0002	−0.0036	0.0005	−0.0010	−0.0007	−0.0040*
	(0.0004)	(0.0036)	(0.0006)	(0.0051)	(0.0008)	(0.0023)
	[−0.001, 0.0015]	[−0.02, 0.0041]	[−0.0006, 0.0026]	[−0.0099, 0.0076]	[−0.0025, 0.0010]	[−0.011, 0.0005]*
Kleibergen-Paap rk Wald F		11.00		6.29		9.33
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
N	2598	2598	2598	2598	2598	2598

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$; Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. All regressions control for in utero exposure to conflict. Climatic controls include rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.

Table 7
The impact of conflict on infant mortality.

Dependent variable	Infant mortality					
	All		Boys		Girls	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Panel A: No controls						
Conflict Events	−0.0005	0.0009	−0.0016	−0.0042	0.0004	0.0055
	(0.0004)	(0.0032)	(0.0004)***	(0.0044)	(0.0009)	(0.0035)
	[−0.0013, 0.0003]	[−0.0044, 0.0064]	[−0.0024, −0.0008]***	[−0.0142, 0.0060]	[−0.0011, 0.00201]	[0.0006, 0.01036]**
Kleibergen-Paap rk Wald F		6.68		6.53		6.77
Panel B: Full set of controls						
Conflict Events	−0.0005	0.0023	−0.0015	−0.0020	0.0004	0.0061
	(0.0004)	(0.0036)	(0.0004)***	(0.0046)	(0.0009)	(0.0038)
	[−0.0013, 0.0003]	[−0.0030, 0.0078]	[−0.0021, −0.0007]***	[−0.0110, 0.0071]	[−0.0011, 0.0020]	[0.0010, 0.0107]**
Kleibergen-Paap rk Wald F		7.93		7.80		8.17
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
N	10,397	10,397	5242	5242	5155	5155

Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

is particularly endangered by the potential income effect of mineral prices. Therefore, conditional on household wealth, the exclusion restriction is more likely to hold.[34]

We next address more directly the potential existence of income effects due to mineral price changes by removing from our sample children born in the areas surrounding mines. In appendix Table A11 we replicate the instrumental variable estimations of Table 7 excluding the sample of children located within 10 (Panel A), 20 (Panel B) and 30 km (Panel C) from the mineral deposits. The results in Table A11 remain qualitatively identical to the ones in Table 7 suggesting that our exclusion restriction is not violated by income effects. Indeed, since households located further than 30 km from the mineral deposits are very unlikely to participate in any way to the mining activities, income effects cannot explain the impact of conflict on infant mortality.

Overall, the above results suggest that the male and female infant populations generated amidst violence start their life with

trary, and in line with the literature, mothers education and wealth decrease infant mortality (see, for instance, Behrman and Wolfe, 1987; Strauss and Thomas, 1995).

[34] As a further test, we apply the method proposed by Oster (2016) to assess the risk of omitted variable bias in our second-stage regression. Assuming the *R*-squared value generated from a mother fixed effect regression (with controls) as the variation that could be hypothetically explained by the full set of observed and unobserved factors, we obtain a degree of selection (δ in Oster, 2016) of 3.59. Such degree of selection suggests that the variation in unobservables should be more than 3 times higher than the variation in treatment due to observables to explain away the result (under a full hypothesized model). Such a value is well above the suggested value of one above which results could be considered as robust.

a different average health endowment, which is only consistent with the presence of culling effect. While a large share of relatively weak male fetuses are lost during pregnancy, thereby generating a positively selected male cohort at birth, the corresponding effect is much weaker among girls. Assuming that local violence affects similarly live boys and girls, the different mortality rate can be considered evidence of a different average health endowment.[35] As in Valente (2015), we cannot rule out that some scarring effect operated due to in utero effect. However, the lack of impact of conflict exposure on boys suggests that the net effect of the scarring and culling mechanisms produced a positively selected male population.

6. Threats to our identification strategy

In this section we address two main threats to our identification strategy. We first assess endogenous fertility decisions during the conflict in DRC, and then the extent to which migration may threaten the validity of our results.

6.1. Endogenous fertility

The fetal origin literature considers fertility decisions as a serious threat to all estimates of the impact of early negative shocks in life on later outcomes. If the negative shock affects the fertility decisions of mothers differently depending on some specific mother/household characteristics, then any estimate of the consequences of the shock would be biased. For instance, if educated mothers react to a conflict by reducing their fertility decisions, then the infant population generated during the conflict may feature poorer health or educational outcomes because of the different mothers' composition or characteristics rather than because of the conflict exposure. Notice first that it is very unlikely that such bias concerns the unbalanced effect across gender we presented in the previous section. Next, the stability of our estimated coefficients for girls' mortality when we include household characteristics such as the number of siblings or the household size is a first signal of the non-selected nature of our sample. Month fixed effects and the use of district-specific trends also deals with fertility trends and seasonality. Moreover, as Valente (2015) argues, it is unlikely that parents are able to predict month-to-month variations in violence when making their fertility decision. We nevertheless formally address this concern proposing two alternative strategies.

First, in our panel regressions we restrict our analysis to the sample of children of mothers who were pregnant at least twice during the period considered, facing different exposure to conflict across the two pregnancy, thereby reducing the concerns about composition effects.[36] Similarly, we include in our cross-sectional model a set of mother fixed effects.[37] Intuitively, we compare the mortality of siblings differently exposed to conflict, by taking account of their shared environment and common genetics.

Table 8, following this strategy, confirms the pattern presented in Tables 3 and 5 concerning the number of male/female births. Although within-mother variation is too limited to estimate the trimester-stratified exposure to conflict, we confirm a negative coefficient of conflicts occurring in the first and second trimester of pregnancy on the probability of a male birth.

The gender imbalance in infant mortality is further confirmed in Table 9.[38] The magnitudes of the effect remain similar. As in the previous specifications, the mother fixed effects results do not depend on the inclusion of climate and individual characteristics (Panel B of Table 9).

Second, we follow the common approach in assessing the characteristics of mothers who were exposed to violence before conceptions (3 months, 6 months, 9 months, 12 months), during pregnancy or during the first 12 months of life (see e.g. Akresh et al., 2011; Valente, 2015; Almond and Mazumder, 2011). We consider the following characteristics: the wealth index, household size, whether it is a female-headed household, mother's education level, the number of births, number of years of education, and whether the mother is a non-resident (migrant). In Table 10 we report the results of this exercise. With the exception of mother's education, exposure to conflict is uncorrelated with mothers' and households' characteristics. The fact that high-educated women are more likely to become pregnant when exposed to violence before conception suggests that we may underestimate the effect of violence on child mortality.[39]

Another exception is the negative correlation between conflicts before conception and the probability to be a migrant. We are less likely to encounter migrant mothers in district highly exposed to conflicts prior to the conception of the child. Such correlation further motivates the exclusion of the non-resident households from our main analysis. We further discuss the issue below.

6.2. Migration

Like endogenous fertility decisions, migration may also bias our results. If, for instance, the wealthier households are able to move to safer locations during conflict, then the treated group would be constituted by poorer household which may display higher mortality rates, irrespective of their conflict exposure. Taking seriously this issue, we restrict to the sample of resident only in our analysis.[40]

Descriptive statistics show that while the 5 districts most prone to conflict account for 26.4% of the total number of households, they represent precisely 1/3 of all the migrant households. In addition to that, the proportion of migrant households in the whole sample is 18.4% while it is 22.1% in these particular districts. It therefore seems that even though these districts accommodate proportionally more migrants than the others, they seem to move inside these districts.[41]

[35] In Table A12 in the appendix we test for the alternative explanation which could generate differential infant mortality rates across gender, namely gender discrimination. We interact our conflict exposure variable with a number of household/mother characteristics which have been flagged in the literature as potentially exacerbate female negative discrimination, i.e. female headed household, widow mother, number of sisters, number of brothers, and son preference (Thomas, 1990, 1994; Duflo, 2003, 2012; Morduch, 2000; Akresh and Edmonds, 2011; Jayachandran and Kuziemko, 2011). We find no evidence consistent with gender discrimination behavior.

[36] A similar strategy has been adopted by Hernández-Julián et al. (2014). Notice that while this sample restriction addresses composition effects, it somehow reduces the external validity of our findings.

[37] In order to ease the estimation process, we partial out each variable with the series of all the fixed effects.

[38] The *p*-values of the coefficients for conflict events in column 4 of Table 9 are 0.101 (Panel A) and 0.117 (Panel B), respectively.

[39] Such unexpected selection based on high education echoes the results found by Akresh et al. (2011) in the case of Ethiopia.

[40] The inclusion of non residents exacerbates the problems posed by migrations which could bias our results. We also chose not to include in the main analysis the 2013 DHS as it does not record whether an household is resident of migrant, which is all the more worrying in the context of DRC, Rwandan and Burundian civil wars, which triggered considerable migration in the area studied.

[41] This is also confirmed in the series of country reports released by the Internal Displacement Monitoring Center during the crisis available at: www.internal-displacement.org/database/country/?iso3=COD. For instance, the February 2000 report discussing the pattern of displacement argues that the vast majority of IDPs remain in proximity to their places of origin, "which makes them prone to be subjected to the same hazards and abuses that caused them to flee in the first place" (IDMC, 2000, p. 44).

Table 8
The impact of in utero exposure to conflict on gender specific natality – mother fixed effect sample.

Dependent variable	Number of births				Boy birth	
	Boys		Girls		OLS	2SLS
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	(5)	(6)
Panel A: No controls						
Conflict Events in utero	−0.0043	−0.0213	−0.0035	−0.0207		
	(0.0023)*	(0.0089)**	(0.0055)	(0.0216)		
	[−0.0094, 0.0036]	[−0.041, −0.0055]**	[−0.022, 0.0065]	[−0.081, 0.029]		
Conflict in utero (1st and 2nd trimesters)					0.0001	−0.0030
					(0.0012)	(0.0168)
					[−0.0021, 0.0024]	[−0.0336, 0.0305]
Conflit in utero (3rd trimester)					0.0007	0.0088
					(0.0012)	(0.0269)
					[−0.0016, 0.0030]	[−0.0389, 0.0561]
Kleibergen-Paap rk Wald F		24.1		24.1		2.50
Panel B: Including controls						
Conflict Events in utero	−0.0044	−0.0236	−0.0036	−0.0209		
	(0.0024)*	(0.0093)**	(0.0060)	(0.022)		
	[−0.0093, 0.0038]	[−0.044, −0.0067]**	[−0.024, 0.0076]	[−0.083, 0.032]		
Conflict in utero (1st and 2nd trimesters)					−0.0011	−0.0061
					(0.0009)	(0.0145)
					[−0.0027, 0.0006]	[−0.0323, 0.0218]
Conflict in utero (3rd trimester)					0.0008	0.0185
					(0.0013)	(0.0251)
					[−0.0016, 0.0035]	[−0.0244, 0.0609]
Kleibergen-Paap rk Wald F		22.20		22.20		3.00
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
Mother FE					√	√
N	2481	2481	2481	2481	8054	8054

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$; Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Analysis restricted to the sample of mothers having at least 2 children during the period considered. Panel B adds climatic controls in columns 1–4 and the full set of controls in columns 5–6. Climatic controls include rainfall anomalies, temperature anomalies, and the malaria index. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.

Still, to increase the confidence in our results, we formally assess the impact of migration. First, similarly to what we do to assess endogenous fertility, we study the characteristics of non resident mothers in our sample (see e.g. Akresh et al., 2011; Valente, 2015; Almond and Mazumder, 2011). Table 11, which displays the results of this exercise, indicates that residents and non-residents are very similar in terms of observed characteristics with one notable exception. Nonresident households are smaller in size. The results in Table 11, together with the results in Table 10 concerning migrants support our choice of restricting the main analysis to the resident population only.[42]

7. Conclusions

In this paper we analyze the impact of the armed conflict afflicting the DRC from 1997 to 2004 on in utero selection and infant mortality. We show that in utero exposure to conflict affects negatively the number of male (but not female) birth. According to the recent literature on in utero selection, this may result from two effects: culling and scarring. Based on the analysis of post natal infant mortality, we argue that culling effects may have positively selected the male population of newborn. Indeed, we find that conflict exposure increases infant mortality only among girls. This pattern is robust to many different specifications, controlling for district and month fixed effects, rainfall anomalies, and mother fixed effects. Assuming that local violence affects similarly live boys and girls, the different mortality rate can be considered as evidence of a better health endowment among male newborns.

This study differs from existing microlevel studies in a major way. Relying on a credible instrumental variable approach, we control for the nonrandom timing and location of conflict violence. This is particularly relevant when we exploit within-district variations inasmuch as we show that our instrumental variables results significantly differ from our ordinary least squares findings. Such contrasting results raise some concerns about potential bias in the existing studies relying on geographical variation in exposure to conflicts.

Overall, and in line with recent findings in the in utero selection literature, our results strongly suggest that more attention should be paid to understanding possible selection in utero in studies assessing the impact of early shocks in life (Catalano and Bruckner, 2006; Catalano et al., 2008; Sanders and Stoecker, 2015; Valente, 2015).

[42] In appendix we report the results of a further simulation exercise addressing migration. We simulate four scenarios randomly allocating migrant mothers to: (i) any of the 38 districts; (ii) one of the 19 most conflict intensive districts; (iii) one of the 10 most conflict intensive districts; and (iv) one of the 5 most conflict intensive districts, following the logic that most migration may have occurred due to ongoing violence. For each scenario, after having reallocated the migrating mothers we remove district/mother fixed effects and run 1000 2SLS regressions of conflict on girls mortality. The results of this exercise broadly confirm our main results: girls mortality is positively affected by the violence. The distribution of the coefficients of interest in these 1000 2sls regressions are plotted in Figs. A3 and A4 displayed in Appendix A.

Table 9
The impact of conflict on infant mortality – mother fixed effect sample.

Dependent variable	Survival rate				Infant mortality			
	Boys		Girls		Boys		Girls	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)	OLS (7)	2SLS (8)
Panel A: No controls								
Conflict Events	−0.00003	0.0003	−0.0002	−0.0055	−0.0014	−0.0036	−0.0011	0.0071
	(0.0007)	(0.0040)	(0.0007)	(0.0032)*	(0.0008)*	(0.0031)	(0.0008)	(0.0048)
	[−0.0014, 0.0025]	[−0.017, 0.0095]	[−0.0019, 0.0013]	[−0.015, 0.00099]	[−0.0029, 0.0002]*	[−0.0110, 0.0035]	[−0.0027, 0.0005]	[−0.0004, 0.01402]*
Kleibergen-Paap rk Wald F		5.71		7.55		8.16		9.93
Panel B: Climatic controls								
Conflict Events	−0.00005	−0.0007	−0.0002	−0.0053	−0.0014	−0.0040	−0.0002	0.0075
	(0.0007)	(0.0042)	(0.0008)	(0.0032)	(0.0007)*	(0.0032)	(0.0004)	(0.0047)
	[−0.0014, 0.0028]	[−0.013, 0.008]	[−0.002, 0.0015]	[−0.018, 0.0011]	[−0.0027, 0.00002]**	[−0.01134, 0.0035]	[−0.0010, 0.0005]	[0.0012, 0.0135]*
Kleibergen-Paap rk Wald F		4.61		6.56		8.5		12.2
Mother FE					√	√	√	√
Month-year FE	√	√	√	√	√	√		
N	1921	1921	1870	1870	4491	4491	4362	4362

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$; Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Analysis restricted to the sample of mothers having at least 2 children during the period considered. Panel B adds climatic controls and in utero exposure to conflict in columns 1–4 and the full set of controls in columns 5–8. Climatic controls include rainfall anomalies, temperature anomalies, and the malaria index. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares. OLS = ordinary least squares; 2SLS = two-stage least squares.

Table 10
Endogenous fertility – conflict exposure and the characteristics of pregnant women.

Dependent variable	Wealth index (1)	HH size (2)	Female headed HH (3)	Number of births (4)	Education level 1 (5)	Education level 2 (6)	Education level 3 (7)	Years of education (8)	Age of HH head (9)	Migrant (10)
Conflict events (12 months	9.913	−0.053	0.008	−0.449	−0.024	0.004	0.017	0.271	−0.561	−0.007*
before conception)	[−1.744, 44.126]	[−0.275, 0.115]	[−0.012, 0.045]	[−2, 0.921]	[−0.206, 0.018]	[−0.009, 0.023]	[−0.014, 0.091]	[−0.150, 1.816]	[−2.649, 0.342]	[−0.015, 0.000]
Conflict events (9 months	13.933	−0.074	0.009	−0.515	−0.034	0.006	0.023	0.378	−0.756	−0.009
before conception)	[−8.114, 68.043]	[−0.367, 0.161]	[−0.016, 0.060]	[−2.941, 1.614]	[−0.180, 0.018]	[−0.012, 0.034]	[−0.017, 0.144]	[−0.172, 3.258]	[−4.375, 0.446]	[−0.021, 0.000]
Conflict events (6 months	21.992	−0.103	0.016	−0.987	−0.058	0.011	0.040	0.638	−1.066	−0.013**
before conception)	[−2.702, 197.318]	[−0.596, 0.395]	[−0.024, 0.195]	[−6.95, 2.743]	[−0.407, 0.026]	[−0.021, 0.070]	[−0.024, 0.726]	[−2.225, −0.833]	[−29.74, 0.714]	[−0.032, 0.001]
Conflict events (3 months	44.556	−0.3358	0.0464	−1.286	−0.1329	0.0299	0.0866	1.471	−2.952	−0.0257
before conception)	[−23.781, 201.689]	[−1.342, 2.632]	[−0.050, 0.189]	[−14.130, 6.982]	[−0.837, 0.048]	[−0.074, 0.151]	[−0.047, 0.420]	[−14.13, 6.982]	[−9.861, 1.089]	[−0.071, 0.009]
Conflict events	3.758	−0.015	0.004	−0.179	−0.010	0.002	0.006	0.109	−0.175	−0.000
(in utero)	[−1.223, 12.601]	[−0.111, 0.107]	[−0.005, 0.013]	[−1.045, 0.536]	[−0.033, 0.009]	[−0.011, 0.012]	[−0.005, 0.021]	[−0.059, 0.326]	[−0.593, 0.173]	[−0.007, 0.006]
Conflict events (during first	−0.469	−0.015	−0.002	0.054	−0.003*	0.004*	0.00005	−0.0004	−0.117	0.0008
12 months of life)	[−1.236, 0.236]	[−0.044, 0.009]	[−0.006, 0.001]	[−0.133, 0.281]	[−0.008, 0.000]	[−0.000, 0.010]	[−0.003, 0.003]	[−0.025, 0.028]	[−0.289, 0.016]	[−0.001, 0.004]
District FE	√	√	√	√	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√	√	√	√	√

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$; *p*-Values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients. Each coefficient is produced with a separate 2SLS regression, using the price index as excluded instrument for the various conflict variables.

Table 11
Migration and the characteristics of mothers.

Dependent variable	Wealth index (1)	HH size (2)	Female headed HH (3)	Number of births (4)	Education level 1 (5)	Education Level 2 (6)	Education level 3 (7)	Years of education (8)	Age of HH head (9)
Non-resident	−3.897	−0.520***	0.010	−1.486*	−0.034	0.0001	0.034	0.417	−0.429
	[−12354, 5046]	[−0.743, −0.268]	[−0.034, 0.055]	[−3.423, 0.112]	[−0.105, 0.043]	[−0.071, 0.072]	[−0.053, 0.128]	[−0.357, 1.317]	[−1.585, 0.892]
District FE	√	√	√	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√	√	√	√
N	11,139	11,139	11,139	11,139	11,139	11,139	11,139	11,139	11,139

p-values are calculated on the empirical distribution of t-statistics; correction for sampling weights; sample of residents only; Standard errors clustered at the district level and confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Our analysis also delivers a critical policy recommendation. Gender-specific warfare damages have sometimes led scholars and policymakers to call for gender-based targeted interventions. Although they may be grounded on good motives and may help in reducing gender discrimination in general, our study warns that these policies may miss their targets if they fail to account for the possible selection in utero. In other words, despite providing some evidence of gender imbalances in infant mortality, our paper suggests that any policy should be drawn on a sound understanding of the sources of such gender bias. As biological factors in utero are found to be a prominent explanation, our paper resets the priority to policies aiming at enhancing the resilience of (pregnant) women to violent experiences. Policies ensuring high coverage of multiple micronutrient supplementation and other nutrition-sensitive programs directly addressing pregnant women (like cash transfers), family planning to delay the age of first pregnancy, or educational interventions designed to increase spacing between births may therefore all prove comparatively more effective in reducing infant mortality in times of violence than policies targeting gender discrimination during the first year of life (Black et al., 2013; Bhutta et al., 2013; Ruel and Alderman, 2013; WHO, 2014).

Appendix A.

Figures A1–A4

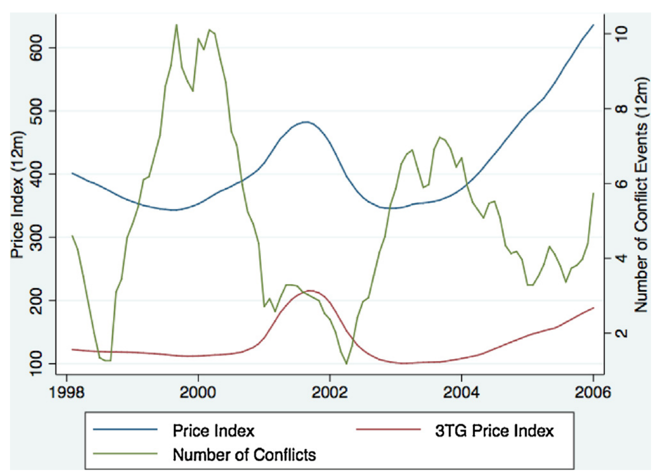

Fig. A1. Trends of average price index and conflict events. The figure reports the average price index, the average price index computed on the 3TG (tantalum, tin, tungsten and gold) minerals only, and the average level of Conflict Events over the period considered.

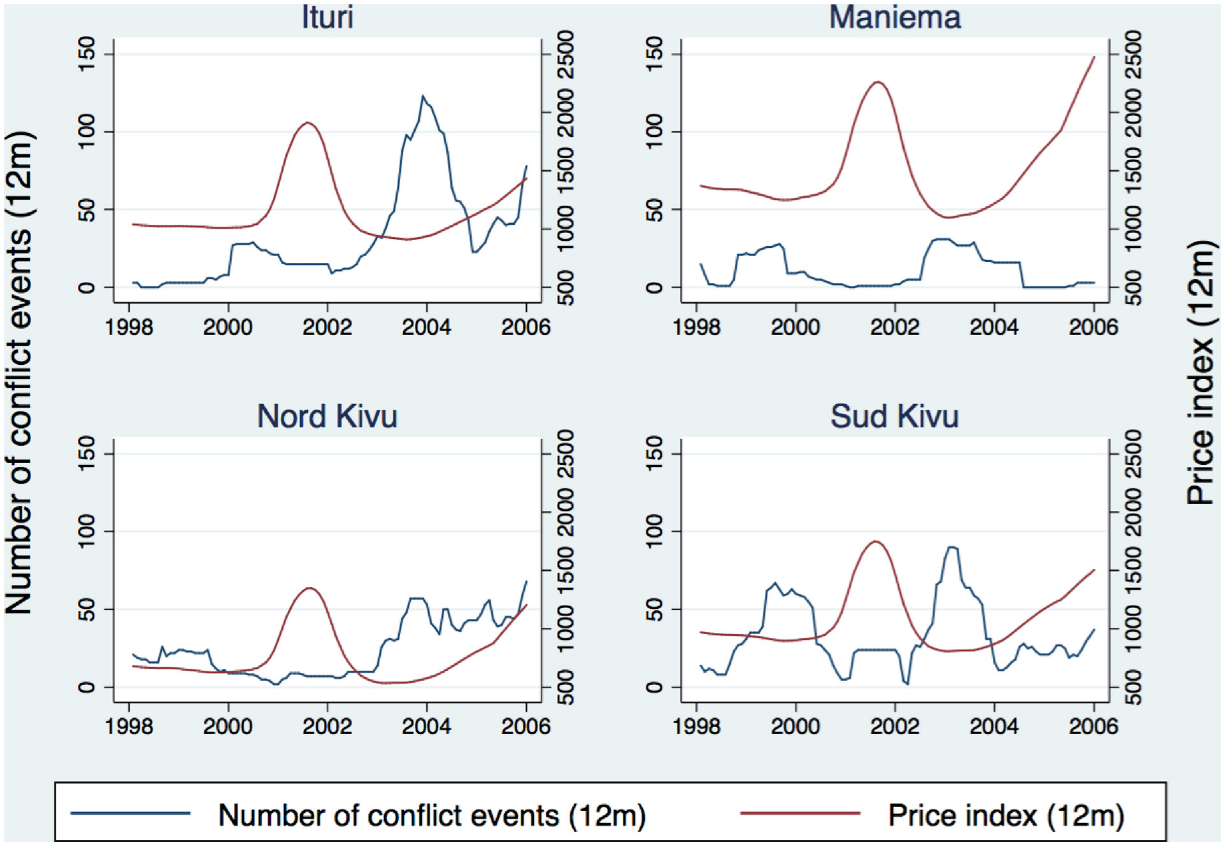

Fig. A2. Price index and conflict events across four districts. The figure reports the average price index and the average level of Conflict Events over the period considered for 4 districts troubled by the conflict.

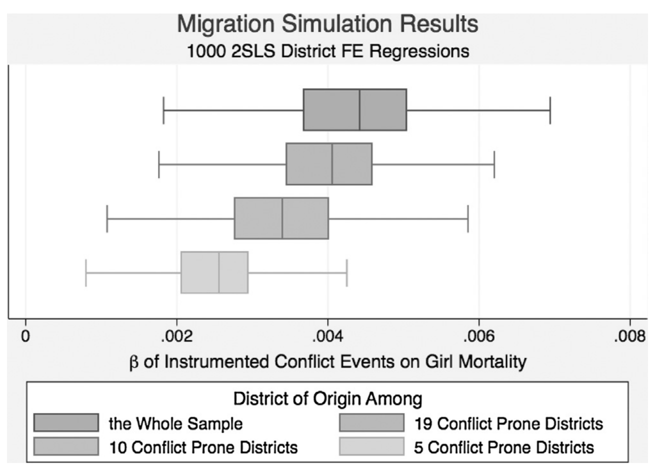

Fig. A3. Migration simulation – district FE. These boxplots represent distribution plots of 1000 estimated beta according to different scenarios: (1) all migrating mothers are randomly allocated to one district of origin; (2) all migrating mothers are randomly allocated to one of the 19 most conflict intensive districts; (3) all migrating mothers are randomly allocated to one of the 10 most conflict intensive districts; (4) all migrating mothers are randomly allocated to one of the 5 most conflict intensive districts. Bold vertical lines (inside the box) represent the median beta, colored boxes show the interquartile range (25–75%), and whiskers indicate the 5–95th percentile of simulations. After having reallocated the migrating mothers for the non resident sample, we remove district fixed effects and run 2SLS regressions of conflict on girls mortality. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

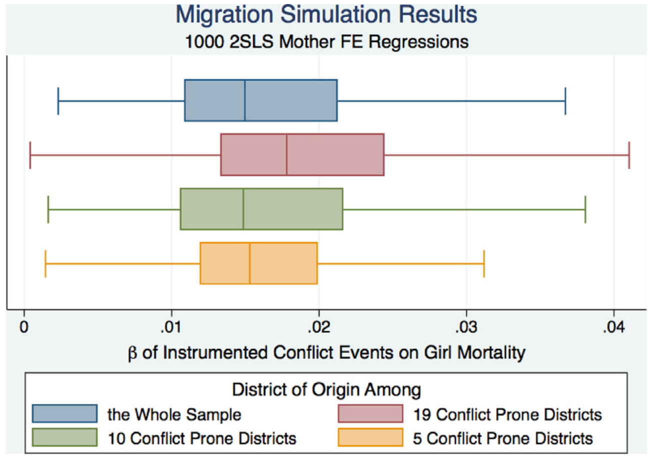

Fig. A4. Migration Simulation – mothers FE. These boxplots represent distribution plots of 1000 estimated beta according to different scenarios: (1) all migrating mothers are randomly allocated to one district of origin; (2) all migrating mothers are randomly allocated to one of the 19 most conflict intensive districts; (3) all migrating mothers are randomly allocated to one of the 10 most conflict intensive districts; (4) all migrating mothers are randomly allocated to one of the 5 most conflict intensive districts. Bold vertical lines (inside the box) represent the median beta, colored boxes show the interquartile range (25–75%), and whiskers indicate the 5–95th percentile of simulations. After having reallocated the migrating mothers for the non resident sample, we remove mothers fixed effects and run 2SLS regressions of conflict on girls mortality. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Tables A1–A12

Table A1
Alternative first stage specifications.

Dependent variable	Conflict events			
	(1)	(2)	(3)	(4)
Panel A: Lagged price index				
Price index	−0.0117***	−0.0121***	−0.0123***	−0.0124***
	(0.00391)	(0.00408)	(0.00423)	(0.00437)
Lag	$t-1$	$t-2$	$t-3$	$t-4$
Panel B: Averaged price index				
Price index	−0.0119***	−0.0121***	−0.0123***	−0.0125***
	(0.00401)	(0.00412)	(0.00423)	(0.00435)
Averaged periods	$t-1$ to $t-2$	$t-1$ to $t-3$	$t-1$ to $t-4$	$t-1$ to $t-5$
Panel C: Poisson model				
Price index	−0.00131***	−0.00126***		
	(0.00006)	(0.00007)		
Linear trend		√		
District FE	√	√	√	√
Month-year FE	√	√	√	√
N	3648	3648	3648	3648

Panel A reports the relationship between conflict events and several lagged versions of the price index, whereas Panel B reports the relationship between conflict events and several averaged versions of the price index. Panel C reports the relationship between conflict events and the price index using a Poisson model.
* $p<0.1$.
** $p<0.05$.
*** $p<0.01$.

Table A2
First stage specifications with alternative conflict measures.

Dependent variable	Conflict events					
	RMCA		USGS		SNL	
	(1)	(2)	(3)	(4)	(5)	(6)
Price index	−0.0120***	−0.0167***	−0.0026	−0.0029	−0.0037	−0.0013
	(0.0039)	(0.0036)	(0.0031)	(0.0037)	(0.0044)	(0.0034)
Price index - no TA	−0.0062**	−0.0124***	−0.0021	−0.0030	−0.0037	−0.0013
	(0.0026)	(0.0040)	(0.0020)	(0.0033)	(0.0044)	(0.0034)
TA price index	−0.0298***	−0.0245***	−0.0033	−0.0028	-	-
	(0.0056)	(0.0030)	(0.0079)	(0.0063)	(-)	(-)
3TG price index	−0.0237***	−0.0251***	−0.0052	−0.0057	−0.0255	−0.0646*
	(0.0036)	(0.0030)	(0.0056)	(0.0059)	(0.0198)	(0.0330)
Linear trends		√		√		√
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
N	3648	3648	3648	3648	3648	3648

RMCA denotes the Royal Museum for Central Africa dataset; USGS denotes the US Geologocal Survey dataset; SNL denotes the SNL dataset; TA stands for tantalum; 3TG: Tantalum, Tin, Tungsten, Gold; FE = fixed effects.
* $p<0.1$.
** $p<0.05$.
*** $p<0.01$.

Table A3
The impact of conflict on the number of boy births – alternative specifications.

Dependent variable	Number of boy births				
	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)
Panel A: No controls					
Conflict Events in utero	−0.0219 (0.0111)** [−0.045, −0.0001]**	−0.017 (0.0129) [−0.048, 0.013]	0.0314 (0.0461) [−0.05, 0.16]	−0.0326 (0.0127)** [−0.064, 0.021]	0.0191 (0.0403) [−0.065, 0.17]
Panel B: Climatic controls					
Conflict Events in utero	−0.0237 (0.0122)* [−0.049, 0.0009]*	−0.0227 (0.0147) [−0.058, 0.012]	0.0296 (0.0456) [−0.053, 0.15]	−0.0343 (0.0114)** [−0.059, 0.011]*	−0.0090 (0.0327) [−0.074, 0.049]
Month FE	√				
Year FE	√				
Linear trends		√			
Includes nonresidents &DHS 2013			√		√
Poisson model				√	√
Month-year FE		√	√	√	√
N	2598	2598	3168	2598	3168

Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Climatic controls include rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table A4
The impact of in utero exposure to conflict on the number of birth – alternative conflict measures.

Dependent variable	Number of births					
	All		Boys		Girls	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Panel A: No controls						
Number of months with conflict (ACLED) in utero	−0.0617 (0.0356)* [−0.14, 0.0068]*	−0.313 (0.223) [−0.86, 0.19]	0.0014 (0.0249) [−0.046, 0.055]	−0.135 (0.0761)* [−0.29, 0.015]*	−0.063 (0.0315)* [−0.14, −0.0019]**	−0.178 (0.179) [−0.62, 0.24]
Number of months with conflict (UCDP) in utero	−0.0098 (0.0434) [−0.1, 0.11]	−10.36 (10.3) [−3.5571, 0.8936]	−0.0154 (0.0358) [−0.083, 0.084]	−0.582 (0.492) [−1.3132, 0.1975]	0.0056 (0.0429) [−0.13, 0.1]	−0.774 (0.937) [−2.4820, 0.9547]
Number of casualties (UCDP) in utero	−0.0003 (0.0001)*** [−0.0008, −0.00004]**	−0.0013 (0.0009) [−0.0036, 0.0007]	−0.0002 (0.00006)*** [−0.0004, 0.00006]*	−0.0006 (0.0003)* [−0.0013, −0.000008]**	−0.0001 (0.00008) [−0.0005, −0.00001]**	−0.0007 (0.0007) [−0.0026, 0.0010]
Panel B: Climatic controls						
Number of months with conflict (ACLED) in utero	−0.064 (0.0373)* [−0.15, 0.007]*	−0.333 (0.229) [−0.89, 0.21]	0.0003 (0.0252) [−0.046, 0.056]	−0.152 (0.0859)* [−0.33, 0.018]*	−0.0643 (0.0339)* [−0.15, 0.0046]*	−0.181 (0.176) [−0.62, 0.25]
Number of months with conflict (UCDP) in utero	−0.0103 (0.0405) [−0.1,0.094]	−10.29 (10.19) [−3.3557, 0.7888]	−0.0159 (0.036) [−0.085, 0.086]	−0.598 (0.475) [−1.3490, 0.2058]	0.0056 (0.0417) [−0.12, 0.084]	−0.696 (0.825) [−2.2193, 0.8440]
Number of casualties (UCDP) in utero	−0.0003 (0.0001)*** [−0.0009, −0.00006]**	−0.0014 (0.0009) [−0.0039, 0.0007]	−0.0002 (0.00007)*** [−0.0004, 0.00007]	−0.0006 (0.0003)* [−0.0015, 0.000003]**	−0.0001 (0.0001) [−0.0006, 0.00003]	−0.0007 (0.0007) [−0.0027, 0.0009]
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
N	2598	2598	2598	2598	2598	2598

Notes: Each coefficient results from a separate regression.
Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Climatic controls include rainfall anomalies, temperature anomalies, and the malaria index.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table A5
The impact of conflict by trimester on the probability of being a boy at birth – alternative specifications.

Dependent variable	Boy birth				
	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)
Panel A: No controls					
Conflict Events in utero	−0.0081	−0.0082	−0.0057	−0.0054	−0.0009
(1st and 2nd trimesters)	(0.0059)	(0.0070)	(0.0063)	(0.0028)*	(0.0008)
	[−0.026, 0.002]	[−0.028, 0.0043]	[−0.029, 0.0061]	[−0.018, 0.0021]	[−0.0046, 0.002]
Conflict Events in utero	0.0165	0.0212	0.0158	−0.0028	−0.0006
(3rd trimester)	(0.0108)	(0.0144)	(0.0121)	(0.0052)	(0.0039)
	[−0.0057, 0.061]	[−0.0035, 0.077]	[−0.0074, 0.059]	[−0.012, 0.014]	[−0.012, 0.018]
Panel B: Full set of controls					
Conflict Events in utero	−0.0093	−0.0090	−0.0057	−0.0041	−0.0013
(1st and 2nd trimesters)	(0.0055)*	(0.0064)	(0.0059)	(0.0027)	(0.0007)*
	[−0.026, 0.0003]	[−0.025, 0.0024]	[−0.027, 0.0052]	[−0.016, 0.0051]	[−0.0038, 0.0009]
Conflict Events in utero	0.0177	0.0211	0.0144	−0.0030	−0.0016
(3rd trimester)	(0.0111)	(0.0138)	(0.0111)	(0.0043)	(0.0037)
	[−0.0063, 0.063]*	[−0.0054, 0.073]	[−0.0075, 0.051]	[−0.011, 0.014]	[−0.013, 0.018]
Month FE	√				
Year FE	√				
Linear trends		√			
Includes nonresidents &DHS 2013			√		√
Poisson model				√	√
Month-year FE		√	√	√	√
N	9496	9496	25829	9496	25829

* $p < 0.1$.
*** $p < 0.01$, ** $p < 0.05$; Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index. 2SLS = two-stage least squares.

Table A6
The impact of in utero exposure to conflict on the probability of a boy birth – alternative conflict measures.

Dependent variable	Boy birth		
	2SLS (1)	2SLS (2)	2SLS (3)
Panel A: No controls			
Conflict (1st and 2nd trimesters)	−0.4166	−0.1983	−0.0026
	[−0.778, −0.055]**	[−0.433, 0.037]*	[−0.005, −0.000]**
Conflict (3rd trimester)	1.5214	0.2088	0.0099
	[0.131, 2.912]**	[−0.089, 0.507]	[0.000, 0.020]**
Panel B: Full set of controls			
Conflict (1st and 2nd trimesters)	−0.4402	−0.2028	−0.0026
	[−0.811, −0.069]**	[−0.404, −0.002]**	[−0.005, −0.000]**
Conflict (3rd trimester)	1.4157	0.2208	0.0098
	[−0.000, 2.831]*	[−0.080, 0.521]	[−0.001, 0.021]*
Conflict measure	Number of months with conflict (ACLED)	Number of months with conflict (UCDP)	Number of Casualties (UCDP)
District FE	√	√	√
Month-year FE	√	√	√

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; Standard errors clustered at the district level and 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index.

Table A7
The impact of conflict on girls' survival rates – alternative specifications.

Dependent variable	Girls' survival rate				
	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)
Panel A: No controls					
Conflict events	−0.0051	−0.0053	−0.0025	−0.0027	−0.0041
	(0.0017)***	(0.0024)**	(0.0025)	(0.0007)***	(0.0014)***
	[−0.01, −0.002]**	[−0.013, −0.0007]**	[−0.012, 0.0027]	[−0.0044, −0.0004]**	[−0.0092, 0.0015]
Panel B: Climatic controls					
Conflict events	−0.0048	−0.0044	−0.0020	−0.0028	−0.0038
	(0.0022)**	(0.0027)	(0.0028)	(0.0009)***	(0.0012)***
	[−0.012, −0.0001]**	[−0.014, 0.0008]*	[−0.012, 0.0036]	[−0.0054, 0.0001]*	[−0.0077, 0.0015]
Month FE	√				
Year FE	√				
Linear trends		√			
Includes nonresidents &DHS 2013			√		√
Poisson model				√	√
Month-year FE		√	√	√	√
N	2007	2007	2919	2007	2919

Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. All regressions control for in utero exposure to conflict. Climatic controls include rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table A8
The impact of conflict on survival rates at 12 months – alternative conflict measures.

Dependent variable	Survival rate					
	All		Boys		Girls	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Panel A: No controls						
Number of months with conflict (ACLED)	0.0028	−0.0223	0.0031	−0.0029	0.0012	−0.0272
	(0.00238)	(0.0224)	(0.0038)	(0.0293)	(0.0026)	(0.0152)*
	[−0.0017, 0.0083]	[−0.14, 0.021]	[−0.0049, 0.013]	[−0.14, 0.099]	[−0.0048, 0.0066]	[−0.077, −0.003]**
Number of months with conflict (UCDP)	0.0033	−0.0088	0.0012	0.0032	0.0038	−0.0225
	(0.0045)	(0.0214)	(0.0037)	(0.0165)	(0.0049)	(0.0351)
	[−0.0047, 0.0116]	[−0.0330, 0.0161]	[−0.0056, 0.0079]	[−0.0295, 0.0345]	[−0.0053, 0.0130]	[−0.0527, 0.0097]
Number of casualties (UCDP)	−0.00002	−0.0002	0.00008	−0.00004	−0.00009	−0.0003
	(0.00006)	(0.0002)	(0.00005)	(0.0003)	(0.00005)*	(0.0002)*
	[−0.0002,0.0002]	[−0.0011,0.0002]	[−0.0001,0.0002]	[−0.0012,0.0006]	[−0.0003,0.0001]	[−0.0009, −0.00006]**
Panel B: Climatic controls						
Number of months with conflict (ACLED)	0.0038	−0.0236	0.0044	−0.0069	0.0023	−0.026
	(0.0023)	(0.0245)	(0.0032)	(0.0313)	(0.00266)	(0.0172)
	[−0.0007, 0.0092]*	[−0.15, 0.025]	[−0.0024, 0.013]	[−0.15, 0.083]	[−0.0034, 0.0077]	[−0.078, 0.0048]*
Number of months with conflict (UCDP)	0.0042	−0.0118	0.0020	−0.0016	0.0048	−0.0173
	(0.0043)	(0.0245)	(0.0037)	(0.017)	(0.0046)	(0.0326)
	[−0.0036, 0.0120]	[−0.0387, 0.0143]	[−0.0050, 0.0089]	[−0.0346, 0.0331]	[−0.0035, 0.0133]	[−0.0474, 0.0118]
Number of casualties (12m) (UCDP)	−0.00008	−0.0003	0.00008	−0.00007	−0.00008	−0.0004
	(0.00006)	(0.0003)	(0.00006)	(0.0004)	(0.00005)*	(0.0002)
	[−0.0002,0.0002]	[−0.0013, 0.0002]	[−0.00017,0.00027]	[−0.0012,0.0007]	[−0.0003,0.0001]	[−0.001, −0.00002]**
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√
N	2485	2485	1996	1996	1933	1933

Notes: Each coefficient results from a separate regression.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$. *p*-Values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; Standard errors clustered at the district level and confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index.

Table A9
The impact of conflict on girls' mortality – alternative specifications.

Dependent variable	Girls' mortality at 12 months				
	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)
Panel A: No controls					
Conflict events	0.0060	0.0063	0.0024	0.0023	0.0019
	(0.0032)*	(0.0036)*	(0.0013)*	(0.0010)**	(0.0004)***
	[0.0012, 0.01055]***	[0.0006, 0.01188]*	[0.0003, 0.0046]*	[0.0004, 0.0042]*	[0.0009, 0.0030]***
Panel B: Climatic controls					
Conflict events	0.0076	0.006	0.0032	0.0018	0.0019
	(0.0040)*	(0.0035)*	(0.0014)**	(0.0005)***	(0.0003)***
	[0.0026, 0.0125]**	[0.0003, 0.0115]*	[0.0012, 0.0053]***	[0.0008, 0.0027]***	[0.0011, 0.0027]***
Month FE	√				
Year FE	√				
Linear trends		√			
Includes nonresidents &DHS 2013			√		√
Poisson model				√	√
Month-year FE		√	√	√	√
N	5155	5155	13787	5155	13787

Standard errors clustered at the district level reported in parentheses; *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients reported in squared brackets. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index. OLS = ordinary least squares; 2SLS = two-stage least squares.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table A10
The impact of conflict on infant mortality – alternative conflict measures.

Dependent variable	Mortality rate at 12 months					
	All		Boys		Girls	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Panel A: No controls						
Number of months with conflict (ACLED)	−0.0045*	0.0050	−0.0091	−0.0324	−0.0094*	0.0688*
	[−0.009, 0.000]	[−0.036, 0.046]	[−0.022, 0.004]	[−0.106, 0.041]	[−0.019, 0.001]	[−0.008, 0.145]
Number of months with conflict (UCDP)	−0.0056**	0.0087	−0.0149**	−0.0555	−0.0018	0.1921*
	[−0.010, −0.001]	[−0.063, 0.080]	[−0.027, −0.003]	[−0.182, 0.071]	[−0.008, 0.004]	[−0.022, 0.406]
Number of casualties (UCDP)	−0.0001**	0.0001	−0.0002***	−0.0006	0.0001	0.0013*
	[−0.000, −0.000]	[−0.001, 0.001]	[−0.000, −0.000]	[−0.002, 0.001]	[−0.000, 0.000]	[−0.000, 0.003]
Panel B: Full set of controls						
Number of months with conflict (ACLED)	−0.0044**	0.0115	−0.0106*	−0.0040	−0.0048**	0.0485**
	[−0.008, −0.001]	[−0.033, 0.056]	[−0.022, 0.000]	[−0.013, 0.005]	[−0.009, −0.001]	[0.003, 0.094]
Number of months with conflict (UCDP)	−0.0055*	0.0182	−0.0142**	−0.0040	0.0033	0.0873**
	[−0.011, 0.000]	[−0.052, 0.088]	[−0.028, −0.001]	[−0.013, 0.005]	[−0.002, 0.008]	[0.006, 0.0169]
Number of casualties (UCDP)	−0.0001**	0.0003	−0.0002***	−0.0040	0.0002***	0.0010**
	[−0.000, −0.000]	[−0.001, 0.001]	[−0.000, −0.000]	[−0.013, 0.005]	[0.000, 0.000]	[0.000, 0.002]
District FE	√	√	√	√	√	√
Month-year FE	√	√	√	√	√	√

Each coefficient results from a separate regression. *p*-values are calculated on the empirical distribution of *t*-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table A11
The impact of conflict on infant mortality removing mines' neighborhood.

Dependent variable	Mortality rate at 12 months			
	Boys		Girls	
	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)
Panel A: excluding <10 km				
Conflict Events	−0.0043	−0.0015	0.0061**	0.0062**
	[−0.017, 0.008]	[−0.010, 0.007]	[0.001, 0.011]	[0.001, 0.012]

Table A11 (*Continued*)

Dependent variable	Mortality rate at 12 months			
	Boys		Girls	
	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)
Panel B: excluding <20 km				
Conflict Events	−0.0057 [−0.023, 0.012]	−0.0022 [−0.015, 0.011]	0.0064** [0.001, 0.012]	0.0066** [0.000, 0.013]
Panel C: excluding <30 km				
Conflict Events	−0.0104 [−0.035, 0.014]	−0.0052 [−0.024, 0.013]	0.0085** [0.001, 0.016]	0.0087** [0.002, 0.016]
District FE	√	√	√	√
Month-year FE	√	√	√	√
Full set of controls		√		√

Notes: Panel A, B and C exclude residents located within 10, 20 and 30 km from the mines, respectively. * $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$, p-values are calculated on the empirical distribution of t-statistics; correction for sampling weights; sample of residents only; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients. Full set of controls include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index. 2SLS = two-stage least squares.

Table A12
Exploring potential behavioral factors behind the gender-specific effect.

Dependent variable	Girls' mortality at 12 months					
	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)	2SLS (6)
Conflict*Female headed HH	−0.0009 [−0.006, 0.004]	0.0002 [−0.040, 0.041]	−0.0009 [−0.006, 0.004]	−0.0022 [−0.007, 0.003]	−0.0011 [−0.004, 0.002]	−0.0011 [−0.004, 0.002]
Conflict*Widow	0.0009 [−0.006, 0.008]	0.0006 [−0.007, 0.008]	0.0007 [−0.009, 0.011]	−0.0006 [−0.007, 0.006]	−0.0010 [−0.005, 0.003]	−0.0010 [−0.005, 0.003]
Conflict* # brothers	0.0009 [−0.001, 0.003]	0.0008 [−0.002, 0.003]	0.0009 [−0.001, 0.003]	0.0008 [−0.001, 0.003]	−0.0003 [−0.001, 0.000]	−0.0003 [−0.001, 0.000]
Conflict* # sisters	−0.0024 [−0.005–0.001]	−0.0024 [−0.006–0.001]	−0.0023 [−0.005, 0.001]	−0.0016 [−0.004, 0.001]	−0.0006 [−0.002, 0.001]	−0.0006 [−0.002, 0.001]
Conflict*Son preference	−0.0007 [−0.003, 0.001]	0.1499 [−0.259, 0.558]	0.1429 [−0.252, 0.538]	0.1741 [−0.200, 0.549]	−0.0162 [−0.083, 0.051]	−0.0162 [−0.083, 0.051]
Month FE		√				
Year FE		√				
Linear trends			√			√
Includes DHS 2013					√	√
Includes nonresidents				√	√	√
Month-year FE	√		√	√	√	√

Notes: Each coefficient is produced by a separate regression. Only the interaction term of interest is reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; p-values are calculated on the empirical distribution of t-statistics; correction for sampling weights; 95% confidence intervals produced by wild bootstrap (percentile-t method) and based on the empirical distribution of coefficients; All regressions include a dummy being part of a multiple birth, a dummy for being the first child, the number of brothers and sisters alive by age group (0–1, 1–2, 2–3, 3–4), mother's education, a measure of the wealth of the household, the household size, a dummy for female headed household, rainfall anomalies, temperature anomalies, and the malaria index.

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