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The Effects of Conflict on Fertility: Evidence from the Genocide in Rwanda

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Abstract

This paper analyzes the fertility effects of the 1994 genocide in Rwanda. We study the effects of violence on both the duration time to the first birth in the early post-genocide period and on the total number of post-genocide births per woman up to 15 years following the conflict. We use individual-level data from Demographic and Health Surveys, estimating survival and count data models. The paper contributes to the literature on the demographic effects of violent conflict by testing two channels through which conflict influences fertility. First, the type of violence exposure as measured by the death of a child or sibling. Second, the conflict-induced change in local demographic conditions as captured by the change in the district-level sex ratio. Results indicate the genocide had heterogeneous effects on fertility, depending on the type of violence experienced by the woman, her age cohort, parity, and the time horizon (5, 10, and 15 years after the genocide). There is strong evidence of a child replacement effect. Having experienced the death of a child during the genocide increases both the hazard of having a child in the five years following the genocide and the total number of post-genocide births. Experiencing sibling death during the genocide significantly lowers post-genocide fertility in both the short-run and the long-run. Finally, a reduction in the local sex ratio negatively impacts the hazard of having a child in the five years following the genocide, especially for older women.

Keywords: Child death, fertility, genocide, Rwanda, sex ratio, sibling death

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Introduction

Does violent conflict affect fertility? Various studies address this important issue, finding that violent conflict influences fertility during and after a conflict. Effects are shown to vary across empirical contexts (for instance, Agadjanian and Prata 2002; Heuveline and Poch 2007; Lindstrom and Berhanu 1999; Woldemicael 2008). Yet, little evidence is available on the mechanisms through which violent events may affect individual fertility, possibly explaining the differences in the direction of the effect identified in previous studies.

This paper uses individual-level data to provide empirical evidence on various mechanisms linking conflict to fertility. We focus on the effects of the 1994 genocide in Rwanda, one of the most devastating violent conflicts since World War II, during which at least 500,000 individuals died within just 100 days. There are two main reasons why the Rwandan genocide provides a suitable setting for exploring this question empirically. First, data on the fertility histories of Rwandan women are available from multiple post-genocide surveys that are representative at the national level and of high quality, which is rare for conflict-affected countries. Second, the fact that the Rwandan genocide was extremely violent and of very short duration reduces the possibility that other events may confound the causal identification of the fertility effects of the conflict.

In our analysis, we use survival and count data models to study the effect of violent conflict on the duration time to the first birth after genocide, i.e. the hazard of having a child in the first five years following the genocide, and on the total number of post-genocide births per woman up to 15 years after the conflict. We focus on two main channels through which conflict may affect fertility. First, we study the effect of different types of individual exposure to violence. In particular, we consider the effect of experiencing the death of a child or sibling during the genocide on a woman's fertility outcomes. Second, we consider the role of local demographic conditions. We focus on the genocide-induced change in the *commune*-level[1] sex ratio – with a severe imbalance of men to women in the post-conflict period – and test its effects on fertility outcomes.

Our main source of data consists of three cross-sectional waves of Demographic and Health Surveys (DHS) collected in Rwanda in 2000, 2005, and 2010. This allows us to disaggregate

1 A "*commune*" in Rwanda in 1991 denoted a local administrative unit akin to a district.

the effects of the genocide on fertility over time, distinguishing between the short- (1995-2000), medium- (2000-2005), and long-term (2005-2010) post-genocide periods. Thereby, we provide a comprehensive perspective on the conflict-induced adjustments in fertility.

This paper presents three main findings. First, there is strong evidence of a child replacement effect. Experiencing the death of a child during the genocide increases both the hazard of having a child in the five years following the genocide and the total number of births in the post-genocide period. Second, experiencing the death of a sibling during the genocide significantly lowers post-genocide fertility. The effect is the strongest if a woman loses a younger sibling, which suggests a psychological mechanism. Finally, the genocide-induced reduction in the local sex ratio has a negative impact on the hazard of having a child in the five years following the genocide. The effect is particularly strong for older women.

Literature

Literature on the Effects of Conflict on Fertility

Traditionally, researchers look at the impact of violent conflict on fertility using aggregate measures of fertility as the outcome variable. Most studies find a decline in fertility during conflict, followed by an increase in the early post-war period, as well as a gradual decline in fertility in the longer term for most, but not all, conflicts (Hill 2004). Yet, evidence is mixed. For instance, Iqbal (2010), examining cross-country data, finds no significant effects of war on aggregate fertility. Urdal and Che (2013), using time-series cross-country data for the 1970-2005 period, show that armed conflicts are associated with higher overall fertility in low-income countries.

More recent studies investigate the impact of violent conflict on fertility outcomes at the micro level. Some studies find that conflict tends to increase fertility, e.g. in Cambodia (Islam et al. 2016), the Occupied Palestinian Territories (Khawaja 2000), and Tajikistan (Shemyakina 2011). In the context of Rwanda, Verwimp and van Bavel (2005) show that female refugees have higher fertility rates than their non-refugee counterparts, while Rogall and Yanagizawa-Drott (2014) only find an increase in post-genocide fertility among young women.

In contrast, there is also evidence that exposure to conflict or periods of political instability may result in a decline in fertility.[2] Studies find this for Bangladesh (Curlin et al. 1976), Kazakhstan (Agadjanian et al. 2008), Angola (Agadjanian and Prata 2002), Cambodia (Heuveline and Poch 2007), Eritrea (Woldemicael 2008), Ethiopia (Lindstrom and Berhanu 1999), the Occupied Palestinian Territories (Khawaja et al. 2009), and Tajikistan (Clifford et al. 2010). Interestingly, several of these studies also find a rebound of fertility once the crisis ends (for instance, Agadjanian and Prata 2002; Heuveline and Poch 2007; Lindstrom and Berhanu 1999).

Literature on the Genocide in Rwanda

The Rwandan genocide is one of the most violent conflicts in the history of humanity. The genocide broke out on April 6, 1994, after the plane of President Habyarimana was shot down while approaching Kigali airport, killing all passengers.[3] An extremist Hutu militia known as Interahamwe, the Rwandan Armed Forces (FAR), and Rwandan police forces organized massacres against the Tutsi minority and, to a lesser degree, moderate Hutu intellectuals who were opposed to the regime. Death toll estimates range between 500,000 deaths to over 1 million deaths; about 10 % of the 1994 population (Desforges 1999; Verpoorten 2005). Most of these individuals were Tutsi, killed in one-sided violence, resulting in the death of an estimated 75 % of the Tutsi population (Desforges 1999). A smaller number of soldiers died in combat between the FAR and the Rwandan Patriotic Front (RPF), a rebel army of exiled Tutsi invading Rwanda from Uganda. The RPF eventually stopped the genocide in July 1994 and took power.

The Rwandan genocide is well studied. There is a very large literature on both its determinants and consequences (e.g., Akresh and de Walque 2008; Akresh et al. 2011; André and Platteau 1998; de Walque and Verwimp 2010; Justino and Verwimp 2013; La Mattina 2017; Lopez and Wodon 2005; Schindler and Verpoorten 2013; Verpoorten 2009, 2012; Yanagizawa-Drott 2014). Results show that the genocide severely impacted household income, poverty, education outcomes, health, and the incidence of domestic violence. The genocide also had a large impact on factors affecting demographic dynamics and fertility, such as sexual behavior (Elveborg Lindskog 2016) and refugee status (Verwimp and Van

[2] Caldwell (2004) notes that economic shocks generally have negative short-term effects on fertility.

[3] For a detailed account of the historical evolution of the tensions between Hutu and Tutsi, see Prunier (1999), Newbury (1988), Mamdani (2001), and Desforges (1999).

Bavel 2005). Rogall and Yanagizawa-Drott (2014) find evidence of a positive effect of the reception of radio waves – their proxy for exposure to violence – on total fertility for their young cohort, while they do not find significant effects for their two older cohorts. Yet, they only focus on the effect of violence on total fertility and do not explore the mechanisms explaining these effects at the micro level.

Conceptual Framework

The economic literature suggests that conflict may affect fertility through different demand and supply channels (Brück and Schindler 2009; Verwimp et al. 2009; Williams et al. 2012). On the one hand, conflict-induced deterioration in the economic conditions may reduce fertility because couples respond to a sudden decline in income by delaying marriage and birth in order to smooth consumption (Lee 1990; Rindfuss et al. 1978). On the other hand, conflict may increase the demand for children when parents suffer the loss of a child (Agadjanian and Prata 2002). Since children are a potential source of economic support for parents in old age (Caldwell et al. 1986), conflict may increase fertility because the value of the insurance role of children increases under conditions of economic insecurity (Cain 1983; Nugent 1985). Finally, conflict may affect the demand for children by decreasing a woman's education attainment, whereby encouraging early female marriage (La Mattina 2017).

On the supply side, conflict may affect fertility because it can impact the local sex ratio and the marriage market (Buvinic et al. 2013). In particular, if large numbers of young men are mobilized for warfare, this leads to both delayed marriages and a decline in marital fertility (Urdal and Che 2013). Moreover, conflict may influence fertility through its impact on reproductive health services (Verwimp and Van Bavel 2005).

The psychology literature suggests several further possible mechanisms linking conflict and fertility. Exposure to a tragic event, such as the death of a family member, may affect fertility because it reduces the desire for children, decreases coital frequency, and negatively affects the women's physiological capacity to carry a child to term, thereby leading to a higher incidence of miscarriages (Frankenberg et al. 2014; Nobles et al. 2015; Norris et al. 2002). Moreover, psychological stress and the decline in nutritional status associated with conflict may reduce fecundity and coital frequency (Kidane 1989). On the contrary, terror management theory suggests that people react to the exposure to violent events by adhering to traditional values more closely, e.g. focusing on their household and having more children (Rodgers et al. 2005; Vail et al. 2012).

Finally, sociological research emphasizes that fertility could also be influenced by variation in conflict-induced mortality at the level of community or ethnic group (Sandberg 2006). In particular, the positive association between mortality and fertility is expected to be stronger when a specific group is targeted or a large share of the population dies as a consequence of the conflict (Heuveline and Poch 2007).

The heterogeneous empirical evidence discussed above and the various possible mechanisms linking conflict to fertility we describe here suggest that both the effects and mechanisms are likely to vary with the specific conflict. In particular, the literature suggests that what matters is the type and duration of the conflict, the type of violence experienced by the population, and the induced changes in the local economic and social conditions (including the local sex ratio) (Nobles et al. 2015; Urdal and Che 2013; Verwimp et al. 2017). In our analysis, we capture the specific characteristics of the Rwandan genocide by focusing on two channels. The first is the type and intensity of individual exposure to violence, as measured by either child mortality or a woman's sibling's death. The second is the conflict-induced change in local demographic conditions, as measured by the *commune*-level sex ratio. The theoretical predictions we derive below regarding the expected impact of those forms of exposure to the genocide on fertility guide our empirical analysis.

Type and Intensity of Individual Exposure to Violence

Child Mortality

In general, household demand theory has no clear prediction as to the effect of child mortality on fertility (Schultz 1997). The target fertility model provides the intuitive basis for the mechanisms that predict a positive correlation between child mortality and fertility. The literature focuses on two main mechanisms leading to a positive correlation between child mortality and fertility: replacement (child replacement hypothesis)[4] and insurance (child survival hypothesis) (Bousmah 2017; Hossain et al. 2007; Montgomery and Cohen 1998; Nobles et al. 2015; Pörtner 2001; Preston 1978; Schultz 1969; Wolpin 1997). Instead, price

[4] In the literature, the term replacement effect includes both the physiological effect (associated with the truncation of lactation and the shortening of the length of the postpartum amenorrheic period) and the volitional replacement effect. Yet, it should be noted that the former would apply only to cases of infant death and cannot explain the relation between child death and fertility (Palloni and Rafalimanana 1999).

theory yields ambiguous predictions regarding fertility. The basic model indicates that parents respond to child mortality by increasing the number of births they demand (Ben-Porath 1976; Sah 1991). In particular, the positive effect of child mortality on subsequent fertility is reinforced by reduced expected returns on investments in child education, which induces a substitution of quantity for quality of children (Kalemli-Ozcan 2003). Yet, once the fact that children are costly is considered in the optimization problem, the optimal response to higher mortality varies with the properties of the utility function (Ben-Porath 1976). In this more general setting, the sign of the effect of a child's death depends on the relative strength of the replacement motive (which tends to increase fertility) and the income effect (which tends to reduce fertility).

The psychology literature suggests other possible explanations for the link between child death and fertility. Child death may reduce fertility because it reduces the desire for children and lowers the psychological capacity of women for childbearing (Nandi et al. 2017). On the contrary, it is possible that after a very dramatic event the desire for having another child may instead increase because fertility may take a symbolic meaning and represent a return to normality (Lindstrom and Kiros 2007; Norris et al. 2002; Rodgers et al. 2005).

The effects of conflict on fertility are likely to vary with parity, mother's age, and time horizon. Yet, theoretical elaborations on these aspects are still lacking. In general, it is expected that women who are younger and childless or at lower parities do, *ceteris paribus*, desire more children and exhibit stronger behavioral fertility response to child death (Nobles et al. 2015). As for the time horizon, Rodgers et al. (2005) argue that the duration of the effect would depend on the type of event. In particular, terror management theory suggests that the decay of the (positive) effect on fertility depends on how quickly the feelings of threat to life disappear and the situation returns to normality.

The existence of economic, social, physiological, and psychological mechanisms linking child death and fertility suggest that the sign of the effect of child mortality on fertility is theoretically ambiguous and needs to be determined empirically.

Women's Sibling Mortality

The effect of a sibling's death on the surviving sibling is also theoretically ambiguous. On the one hand, experiencing the death of a sibling could influence other siblings' outcomes because of the loss of positive (monetary and non-monetary) inputs or through bereavement

(Stroebe et al. 2006). On the other hand, the death of a sibling reduces competition for parental inputs among surviving siblings (Yi et al. 2015). Finally, a sibling's death may also reduce parental inputs because of grief. Fletcher et al. (2013) find that experiencing the death of a sibling during childhood influences various adult outcomes and that the cause of the sibling's death matters. Interestingly, surviving brothers and sisters seem to be differentially affected, with the effect stronger for surviving females. This result is in line with the fact that women usually report greater intimacy in sibling relationships than men (Kim et al. 2006). Finally, Fletcher et al. (2017) show that the effects are larger if the surviving sibling is older, suggesting sensitive periods of exposure, while the negative effects decline over time.

Changes in Local Demographic Conditions

Conflict may affect fertility by changing the local demographic conditions. In particular, it may influence the marriage market by changing the sex ratio (defined as the relative number of men to women).[5] In fact, a conflict-induced imbalance in the sex ratio is expected to negatively affect the marriage market and reduce fertility (Brainerd 2016). For instance, Bethmann and Kvasnicka (2013) show that in Bavaria, Germany, the decline in the sex ratio induced by WWII increased the proportion of out-of-wedlock childbearing but reduced overall fertility. As for Rwanda, there is robust evidence that the genocide reduced the sex ratio, that the effect was stronger in *communes* with a higher genocide intensity, and that this affected marital outcomes, domestic violence, and time use (La Mattina 2017; Schindler 2010; Schindler and Verpoorten 2013; Verpoorten 2005).

Data

Our analysis builds on three cross-sectional waves of the Rwandan DHS, collected by ORC Macro and the National Institute of Statistics of Rwanda in 2000, 2005, and 2010. The data in each survey is representative of households in Rwanda, based on a stratified survey design selected in two stages. In the following, all analyses account for the survey design and population weights are used as recommended by the data providers. In each selected household, all women aged 15-49 who were either usual household members or present in the household on the night before the interview were eligible for interviewing. The questionnaire

[5] Conflict may affect the marriage market in ways that go beyond the decline in the sex ratio (La Mattina 2017). First, conflict may decrease women's utility of being unmarried because of deteriorating economic conditions and increased risk of becoming a victim of sexual violence, thus increasing fertility. Second, the genocide may delay the age of first marriage, which would decrease fertility.

design remained broadly similar across the survey waves. The sample size increased over time with 10,421, 11,183, and 13,671 women included in the 2000, 2005, and 2010 survey waves, respectively. Our sample of analysis is restricted to women who were 10-45 years old at the time of the genocide.[6] Table 1 reports summary statistics for the main variables we use in the analysis.

[Table 1 about here]

The DHS provides detailed information on women's birth histories, maternal health, child health, marital status, and socio-economic characteristics, including educational attainment, main occupation, and place of living. The DHS also collects some information on respondents' partners, including age, education, and occupation. Income and consumption expenditures are not recorded. Therefore, we construct a wealth index based on household assets.[7]

We employ three alternative proxies for exposure to the genocide, which are described in the following.

Child mortality. The DHS questionnaire records child mortality in detail. For each sample woman who has ever lost a child, the month of death, gender, and age of the deceased child is recorded (while the cause of death is not asked for). This allows us to create a dummy variable $CHILD_{icw}$ that takes the value one if a woman *i*, living in *commune c*, interviewed in wave *w*, lost one or more children between April and July 1994 (the period of the genocide); and zero otherwise. Figure 1 shows the percentage of child deaths relative to the total number of living children reported by sample women for each year during the 1985-2010 period, separately for each DHS wave. The percentage of child deaths peaked during the genocide (increasing by more than twofold relative to the pre-genocide period), returned to pre-genocide levels in 1995, and then started decreasing further. Even though the percentage of child deaths is not low in the pre-genocide period (as is expected for a low-income country

6 We only keep women in the sample for whom fertility information is available for at least five years during the post-genocide period. This implies that the age of the women included in our sample is slightly different for each wave. For instance, consider the 2010 wave that interviews women aged 15-49 in 2010. By restricting the sample to women aged 10-45 years in 1994, the regression sample consists of women who are 25-49 years in 2010.

7 Components of the wealth index include durables and housing characteristics. This wealth index provides a proxy for long-term economic well-being as many durables and housing characteristics are typically held by households for many years and are infrequently replaced (Sahn and Stifel 2000).

like Rwanda), the figure reassuringly shows that there is no evidence of a positive trend in child mortality pre-genocide.

[Fig. 1 about here]

Women's sibling mortality. The DHS questionnaire also records detailed information on each woman's siblings born to the same mother. For every sibling, information is available on the gender, date of birth, whether the sibling is still alive, year of death, and whether the death is related to pregnancy or childbirth.[8] This allows us to create the dummy variable $SIBLING_{icw}$ taking the value one if a woman experienced the death of one or more siblings during the genocide; and zero otherwise. To ensure that this variable only captures deaths related to the genocide, we exclude all deaths related to pregnancy and childbirth for female siblings. Figure 2 shows the percentage of sibling deaths relative to the total number of living siblings reported by sample women for each year during the 1985-2010 period, calculated separately for all three DHS waves. The graph exhibits a single peak, which coincides with the 1994 genocide.

[Fig. 2 about here]

Genocide-induced change in the *commune*-level sex ratios. The third conflict proxy is a continuous variable capturing the change in the *commune*-level demographic conditions caused by the genocide. We construct the variable $\Delta Sex\ ratio_{c,1991-2002}$ as the difference between the pre-genocide and post-genocide sex ratios at the *commune* level. Data on sex ratios comes from two secondary sources: the 1991 Census (the most recent population data available from before the genocide) and the 2002 Census (the first population census collected after the genocide).[9] For each *commune*, the sex ratio – the ratio of males to females – is calculated for the population aged 15-60. We exclude individuals living in institutions, such as prisons, convents, and military camps. As shown in Fig. 3, the change in the *commune*-level sex ratio exhibits plenty of spatial variation across the 145 *communes* included in the analysis. On average, the sex ratio decreased by 15 percentage points, from 0.98 males per female in 1991 to 0.83 males per female in 2002. The sex ratio decreased in all 145 *communes*, with the value of this reduction ranging from a minimum of 0.002 to a

8 Accuracy tests on the sibling mortality module in the DHS are discussed in de Walque and Verwimp (2010).

9 For comparability, our analysis applies the administrative structure in place in 1991 to all DHS waves, when Rwanda's administrative structure consisted of 11 *préfectures* and 145 *communes*.

maximum of 0.32. Note that $\Delta Sex\ ratio_{c,1991-2002}$ takes only positive values, meaning that larger values reflect larger reductions in the sex ratio.

[Fig. 3 about here]

Estimation Strategy

We employ various estimation strategies to explore the impact of the genocide on fertility. The first is a Cox proportional hazard model to investigate the effects of conflict on the duration time to the first birth after genocide, i.e. on the hazard of having a birth after the genocide. The Cox proportional hazard is defined as:

$$h_{icw}(t) = h_0(t) \exp(\beta\ Conflict_{icw} + X'_{icw}\, \gamma + \delta_c + \theta_w) \quad (1)$$

where t is the duration (or survival) time, $h_{icw}(t)$ is the hazard rate at time t for woman i, living in *commune* c, interviewed in wave w, and $h_0(t)$ is the baseline hazard function that depends on t but not X. The baseline hazard function is assumed to be unknown and is un-parameterized.[10] The duration time is defined as the number of months between June 1995[11] and the first birth, if any, occurring up to and including May 2000. Note that we use data on births occurring during the same time window – June 1995 to May 2000 – in each DHS wave. The hazard rate at time t is the probability of occurrence of the event "having a birth" at time t if it has not already occurred. Our focus here is on the effects of the genocide on fertility in the short run, i.e. during the five years immediately following the genocide. Women having their first post-genocide birth after May 2000 are treated as right-censored observations.[12]

$Conflict_{icw}$ is a dummy variable capturing a woman's exposure to violence during the genocide. As a proxy for the type and intensity of exposure to conflict, we use two different measures: a dummy taking the value one if the woman experienced the death of a child during the genocide, and zero otherwise ($CHILD_{icw}$); and a dummy variable taking the value one if the woman experienced sibling death during the genocide, and zero otherwise ($SIBLING_{icw}$).

[10] Note that the duration time is parameterized in terms of the set of covariates, including the conflict proxy, but the particular distributional form of the duration time is not parameterized. Also note that there is no constant term; the latter is absorbed in $h_0(t)$, which is not directly estimated in the model.

[11] Using June 1995 as a starting point allows us to exclude children conceived during the genocide, potentially through rape, from our analysis.

[12] Note that the Cox regression analysis includes also right-censored observations, thus overcoming problems associated with censoring and preventing our estimates to be biased.

β is the key parameter of interest to be estimated, which captures the effect of violence exposure on the duration time to the first birth in the five years after genocide, i.e. on the hazard of giving a birth within the five years following the genocide. X'_{icw} is a matrix of covariates including i) woman-specific characteristics (age, age squared, marital status at the time of the genocide, education level, and previous fertility[13]); ii) household-specific characteristics (wealth index and an indicator for urban residence); and iii) *commune*-specific characteristics (mortality of children under age 5 during the five years preceding the genocide and the sex ratio before the genocide). Finally, δ_c is a vector of *commune* dummies, capturing all time-invariant factors at the *commune* level (*commune* fixed effects) and θ_w is a vector of dummies for the survey waves (wave fixed effects). Standard errors are clustered at the primary sampling unit (PSU) level.

Next, we perform a survival analysis, using the genocide-induced change in the *commune*-level sex ratio as conflict measure. In this case, the Cox proportional hazard is defined as:

$$h_{icw}(t) = h_0(t) \exp(\beta \, \Delta Sex\ ratio_{c,1991-2002} + \pi \, Sex\ ratio_{c,1991} + X'_{icw} \gamma + \delta_p + \theta_w) \quad (2)$$

where the variable $\Delta Sex\ ratio_{c,1991-2002}$ measures the change in the sex ratio from the pre-genocide period to the post-genocide period in *commune c*. This measures the genocide-induced reduction in the relative number of men in the *commune* and serves as proxy for a woman's chance of getting into a relationship. The variable $Sex\ ratio_{c,1991}$ accounts for the pre-genocide level of the sex ratio in the *commune*. X'_{icw} is the same matrix of individual, household, and *commune*-specific covariates as in Eq. 1. Finally, δ_p is a vector of *préfecture* fixed effects.[14] Standard errors are again clustered at the PSU level.

The second estimation strategy is a count data model to determine the effect of the conflict on the total number of post-genocide births. To account for both censoring at zero and the non-negative integer nature of the outcome values, we estimate the following Poisson regression model:

$$Y_{icw} = \alpha + \beta \, Conflict_{icw} + X'_{ict} \gamma + \delta_c + \theta_w + u_{icw} \quad (3)$$

[13] Number of children born before June 1995 and percentage of children ever lost before the genocide.

[14] Note that in Eq. 2, we use *préfecture* fixed effects (instead of *commune* fixed effects as in Eq. 1) because the sex ratio varies at the *commune* level.

where Y_{icw} denotes the post-genocide fertility of woman i, living in *commune* c, and interviewed in wave w. More specifically, the outcome variable contains the number of births a woman had between June 1995 and the date of each survey interview. Note that pooling all three DHS waves (2000, 2005, and 2010) here means that a different period for when births can occur is considered in each wave. This period is 5 years, 10 years, and 15 years long for the 2000, 2005, and 2010 waves, respectively. $Conflict_{icw}$ is a dummy variable indicating a woman's exposure to conflict during the genocide, as measured by the two different conflict proxies outlined above, $CHILD_{icw}$ and $SIBLING_{icw}$. X'_{icw} is a matrix of covariates including the same individual, household, and *commune*-specific characteristics as in Eq. 1. Finally, δ_c represents *commune* fixed effects, capturing all time-invariant factors at the *commune* level. θ_w is a vector of dummies for survey waves (wave fixed effects). u_{icw} is the error term. Standard errors are clustered at the PSU level.

Finally, we explore the effects of conflict on the total number of children born after the genocide, using the genocide-induced change in the *commune*-level sex ratio as proxy. To this end, we estimate the following Poisson regression model:

$$Y_{icw} = \alpha + \beta \, \Delta Sex\ ratio_{c,1991-2002} + \pi \, Sex\ ratio_{c,1991} + X'_{icw} \gamma + \delta_p + \theta_w + u_{icw} \quad (4)$$

where Y_{icw} denotes the number of children born between June 1995 and the date of interview to woman i, living in *commune* c, interviewed in wave w. As in Eq. 2, $\Delta Sex\ ratio_{c,1991-2002}$ measures the change in the *commune*-level sex ratio from the pre-genocide period to the post-genocide period. The variable $Sex\ ratio_{c,1991}$ measures the pre-genocide level of the sex ratio in each *commune*. X'_{icw} is the same matrix of individual, household, and *commune*-specific covariates as in Eq. 1. Finally, δ_p is a vector of *préfecture* fixed effects.

Results

The Effects of the Genocide on the Duration Time to the First Birth after Genocide

As a first step, we investigate the effects of the genocide on the duration time to the first birth after the genocide, i.e. on the hazard of having a birth in the five years following the genocide. Our sample includes all women aged 10-45 at the time of the genocide. As a refinement, we also estimate the effect separately by age cohort, based on a woman's age at the time of the genocide, and by parity, based on the number of children a woman had before June 1995.

Table 2 reports the results obtained from Cox regressions when we use, as measure of conflict exposure, $CHILD_{icw}$, a dummy taking the value one if a woman experienced the death of a child during the genocide; and zero otherwise. Column 1 reports the Cox regression coefficients from the baseline specification in which we only include the conflict proxy, the age (and age squared) of the woman, *commune* fixed effects, and wave fixed effects. Results show that for women exposed to child death during the genocide, the estimated coefficient of the conflict variable is 0.28. This corresponds to the hazard of having a birth within five years after the genocide being 32 % higher for women who lost a child during the genocide than for women who did not experience child death.[15] This means that the duration time to the event is shorter for women exposed to child death (compared to women not exposed), which is consistent with a replacement effect. As shown in column 2, this result is robust to the inclusion of the full set of control variables. Interestingly, when we look at the results by women's age cohort (columns 3-5), we find the effect to be highly statistically significant and large in magnitude for older women (aged 20-45 at the time of the genocide), while it is not significant for the young cohort (i.e. aged 10-19 at the time of the genocide). Finally, we look at the effects of child death on fertility by parity. The estimates reported in columns 6-8 confirm that there is a significant effect for all parity groups, though the effect is stronger for lower parities. Additional regressions (Table A1, columns 1-2 in the Supplementary Online Appendix) show that the effect of child death is significant and positive for both the death of a son and the death of a daughter. This indicates that the replacement effect is at work independent of the lost child's gender.

[Table 2 about here]

Next, we consider the effect of a woman's exposure to sibling death during the genocide on the duration time to the first birth after genocide. Results are displayed in Table 3. As before, we report the Cox coefficients for the baseline and main specifications (the one with all controls), and by age cohort and by parity. The variable of interest, $SIBLING_{icw}$, has a negative coefficient in all specifications. This indicates that the duration time to the first birth is longer, i.e. the hazard of having a birth is smaller for women exposed to a sibling death than to women not exposed. Yet, the estimated coefficient is only statistically significant in the specification including all controls (column 2) and for the youngest cohort (column 3). We

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[15] To compute the hazard ratio from the Cox coefficients, the following formula is applied: $100 * [\frac{(e^{\beta*1} - e^{\beta*0})}{e^{\beta*0}}]$ where β is the estimated regression coefficient.

interpret these results as suggesting that the possible negative psychological effect of sibling loss due to the genocide is more likely to affect fertility if the violent event occurs when the woman is young. Interestingly, when disaggregating the effects of sibling death by a sibling's age relative to each sample woman (Table A1, columns 3-8 in the Supplementary Online Appendix), we find that the effect is stronger (i.e. the duration time is longer) for the death of a younger sibling, although it is only significant in the case of the death of a younger brother. This finding is in line with the theoretical predictions suggesting a stronger effect for the death of a younger sibling. Since the death of a younger brother is likely to have occurred while the woman was still living with her parents, this variable may capture the trauma effect of having witnessed violence committed against close family members or stigmatization due to belonging to a victimized household.

[Table 3 about here]

As a final step in our survival analysis, Table 4 reports the Cox regression coefficients obtained when using the genocide-induced change in the *commune*-level sex ratio to measure conflict exposure. Results show that the coefficient of interest is negative in all specifications. This means that women exposed to a more severe local shortage of men because of the genocide have a lower hazard of having a birth in the five years after the genocide, i.e. the duration time to their first birth after genocide is longer. Moreover, reading across columns 3-5, our results show that – not surprisingly – the negative effect of the decline in the sex ratio on the hazard of having a birth is strongest for the oldest cohort.

[Table 4 about here]

Lastly, we comment on the covariates that turn out to be statistically significant across specifications (Tables 2, 3, and 4). The first is the household wealth index that – in line with results from previous studies – is significant and negative. This suggests that, under the assumption that current wealth is predicted by past wealth, the hazard of having a birth (the duration time to the birth) is smaller (longer) for women from relatively wealthier households. The second variable is the number of children born before June 1995. Its negative sign indicates that the hazard of having a birth after the genocide (the duration time to the first post-genocide birth) declines (increases) with the number of children conceived before the genocide. Finally, we find that the percentage of children ever lost before the genocide is significant and negatively correlated with the hazard of having a birth after the genocide. Yet, the correlation is positive for the youngest cohort. This is not surprising, because the replacement effect is more likely for those women.

The Effects of the Genocide on the Total Number of Post-Genocide Births

To complement the previous analyses, we now examine the effects of the conflict on the total number of post-genocide births, looking again at the effects of each of the three measures of conflict exposure. As in the survival analysis, our sample includes all women aged 10-45 at the time of the genocide. Again, we also estimate the model for the total number of births after the genocide separately by age cohort, based on a woman's age at the time of the genocide and by parity. In addition, we analyze the effects of the genocide on fertility in the short (1995-2000), medium (2000-2005), and long terms (2005-2010).

Table 5 reports results obtained from estimating Eq. 3 with a Poisson regression model and when using $CHILD_{icw}$ as a proxy for a woman's exposure to the genocide. Column 1 displays results for the baseline specification. Results show a strong and positive effect of $CHILD_{icw}$ on the number of births after the genocide. This again indicates a replacement effect at work. Women who lost at least one child during the genocide have significantly more births in the post-genocide period. This result is robust to the inclusion of the full set of control variables (column 2, the main specification). With respect to the magnitude of the estimated coefficient, we find that having experienced the death of a child during the genocide increases the predicted number of children born after the genocide by 10 %. Column 9 shows that the replacement effect is strong and significant in the short run, i.e. in the five years after the genocide, while the effect is reversed in the long-run. Interestingly, we find that while the effect of child death is positive for both son and daughter death, it is significant only for a deceased male child (Table A2, columns 1-2 in the Supplementary Online Appendix).

[Table 5 about here]

Next, we look at the effect of an exposure to sibling death during the genocide on the total number of births in the post-genocide period. To this end, we estimate Eq. 3 using the variable $SIBLING_{icw}$ as a measure of genocide exposure. Table 6 shows the results. Columns 1 and 2 report the estimates for the baseline and main specifications. The negative coefficients for $SIBLING_{icw}$ indicate that women who experienced the death of a sibling during the genocide have significantly lower fertility in the post-genocide period than women who did not lose a sibling. The magnitude of the estimated coefficient in column 2 indicates that being exposed to sibling death during the genocide decreases the predicted number of children by 5 %. The analysis by parity (columns 6-9) indicates that, while the effect is significant and decreases with an increase in parity, it is not significant for women who did not have children or had three or more at the end of the genocide. Finally, the analysis of the effect by time horizon (columns 10-12) indicates that, while the effect of the death of a sibling is always negative, it is larger in the long run. Interestingly, we find a significant and negative effect for the death of a sibling, irrespective of the sibling's gender (Table A2, columns 3 and 6 in the Supplementary Online Appendix). When we disaggregate sibling death by the age of the sibling relative to the sample women, we find the strongest effect for the death of a younger brother. This finding confirms results from the survival analysis and is in line with theoretical predictions suggesting a stronger effect for the death of younger (rather than older) sibling.

[Table 6 about here]

Finally, we examine the effect of the conflict-induced change in the *commune*-level sex ratio on the number of children born after the genocide. Results for Eq. 4 are reported in Table 7. In the baseline (column 1) and main specifications (column 2), the estimated coefficients for $\Delta Sex\ ratio_{c,1991-2002}$ are negative, with the latter being significant at 5 %. This indicates that a genocide-induced decrease in the local sex ratio (a relative reduction in the number of men with respect to the number of women in the *commune*) lowers the total number of births a woman had after the genocide. In particular, the effect is significant for the oldest cohort and for women with higher parity. Moreover, the effect is significant if we restrict the analysis to the short run, i.e. the five years following the genocide. This confirms the results obtained with survival analysis. Taken together, these results suggest that the genocide affected fertility in the short run by decreasing the possibility of marital matching due to the conflict-induced reduction in the local sex ratio.

[Table 7 about here]

As regards other covariates, we find some to be significant across results in Tables 5, 6, and 7. Both secondary (or higher) education and the number of children born before June 1995 tend to decrease post-genocide fertility. The coefficient for household wealth is always negative and significant. Again, the percentage of children lost before the genocide is negatively correlated with the number of total post-genocide births for the full sample, but not for the youngest cohort.

Robustness Tests

We conduct several tests on the robustness of results. First, we re-estimate the survival and count data models, this time including all three proxies for conflict exposure simultaneously. Results are reported in Tables A3 and A4, respectively, in the Supplementary Online Appendix. Interestingly, results are virtually unchanged for both models compared to those obtained with separate regressions, both in terms of significance levels and effect size. This suggests that the mechanisms captured by the three measures of genocide intensity are not substitutes; rather it appears that each affects fertility decisions independently from one another.

Second, we explore if our results capture the specific effect of the genocide on fertility or if our analysis simply picks up an effect that would materialize any time a woman loses a child or a sibling. Recall that in our main analysis, we already control for the percentage of children ever lost before the genocide. We now re-estimate the Cox regression using $CHILD_{icw}$ as the genocide measure and adding a dummy variable taking the value one if a woman experienced the death of a child during the 1990-1993 period, i.e. before the genocide, and zero otherwise, while accounting for the full set of control variables. Results in Table A5, column 1 in the Supplementary Online Appendix show that the replacement effect for child death during the genocide rather than in another period is significantly larger,[16] thus suggesting that exposure to the genocide does have a differential effect on fertility outcomes. Next, we conduct a similar test on the effect of sibling death. To this end, we construct a dummy variable taking the value one if the sibling death occurred in the 1990-1993 period (i.e. before the genocide); and zero otherwise. Again, we re-estimate our Cox regression using $SIBLING_{icw}$ as the genocide measure and adding the dummy to the full set of controls. Results in column 2 show that the variable for sibling death before the genocide is not statistically different from zero. This evidence confirms that our analysis captures fertility effects that are specific to a woman's sibling mortality during the 1994 genocide. Comparable results are obtained when carrying out this test with our Poisson regressions (Table A6 in the Supplementary Online Appendix). We interpret this evidence as convincingly showing that the effects of child and sibling deaths during the genocide are different from those during normal times. This could possibly suggest that while child death is not a rare event in Rwanda, if child death does not occur for "typical" reasons (such as health problems and poverty-related issues), then the

[16] An F-test rejects the null hypothesis that the coefficients are equal, with the p-value being 0.06.

desire for replacement may be stronger. Moreover, the effect of the genocide on fertility may also be reinforced by the objective to replace individuals of the same ethnic group who have been killed during the genocide (Heuveline and Poch 2007).

Third, we explore if results are driven by the choice of the regression sample. Recall that all regressions discussed so far are carried out on the full sample of women aged 10-45 years at the time of the genocide. Instead, we now re-estimate all regressions based on different samples that are tailored to each conflict proxy. For the analysis of the effects of child death, we restrict the sample to those women who had at least one child before the genocide began. For the analysis of the effects of sibling death, we restrict the sample to those women who had at least one sibling before the genocide. For the analysis of the genocide-induced change in the sex ratio, we restrict the sample to women who had their first marriage after the genocide. Results for the survival model (Table A7 in the Supplementary Online Appendix) and for the count data model (Table A8) show that using these restricted samples yields qualitatively similar results to those using the full sample, thus providing confidence in the robustness of our findings.

Fourth, since our analysis builds on retrospective data, we check whether recall bias is a serious concern. To test for this, we re-estimate the main specification in Table 2 (column 2) separately for each DHS wave. Results reported in Table A9 in the Supplementary Online Appendix show that the effect of child death on fertility is similar across each DHS wave. We interpret this as evidence supporting that the recall bias is not a major concern for our analysis.

Fifth, we re-estimate all main regression specifications with the addition of location-specific linear time trends to capture all time-varying characteristics at the *commune* and *préfecture* levels. Results in Table A10 (for the survival analysis) and Table A11 (for the count data model) in the Supplementary Online Appendix show that all main results for the effects of conflict – as proxied by $CHILD_{icw}$, $SIBLING_{icw}$ and $\Delta Sex\ ratio_{c,1991-2002}$ – are unchanged.

Finally, we explore the possibility that fertility differs across Hutu and Tutsi, examining how this difference may bias our results. Ideally, one would control for ethnicity in all models. Unfortunately, this is not possible because since the genocide, Rwandan law prohibits collecting information on the ethnicity of respondents. Thus, to shed light on the possibility that fertility outcomes differ between the two main ethnic groups, we turn to information included in the pre-genocide survey, namely the 1992 DHS. Of the nationally representative sample of women surveyed in the 1992 DHS, 8.6 % reported being Tutsi. Descriptive statistics suggest the existence of some differences across ethnic groups. On average, Tutsi women had 0.6 fewer living children, married 1.7 years later, and gave birth to their first child 1.7 years later than Hutu women in 1992 (all three figures are significantly different in means across Hutu and Tutsi). The other socio-economic characteristics differing between Hutu and Tutsi are education, place of living, and wealth. As a first step, we test the effect of being Tutsi on fertility in the pre-genocide period. We do this separately for the five years preceding the survey (1987-1992), the 10 years preceding the survey (1982-1992), and the 15 years preceding the survey (1977-1992), using the same set of covariates from our main specification as controls. We find that – *ceteris paribus* – being Tutsi has no effect on fertility in the 1987-1992 period, while it has a negative and significant effect on fertility in both the 1982-1992 and 1977-1992 periods (Table A12 in the Supplementary Online Appendix). This finding implies that because we cannot control for ethnicity, our results may be biased. Yet, the direction of the bias potentially introduced by the omitted Tutsi variable depends on the conflict proxy we use. When we use $SIBLING_{icw}$ or $\Delta Sex\ ratio_{c,1991-2002}$, we expect both measures to be negatively associated with fertility and positively associated with the Tutsi indicator (as discussed above, most people killed during the genocide were Tutsi). It follows that, if anything, the estimates obtained when using $SIBLING_{icw}$ or $\Delta Sex\ ratio_{c,1991-2002}$ are likely to be biased downwards. In other words, the estimated effect of conflict on fertility obtained in these two cases is likely to reflect the lower bound of the true effect of conflict, making our results conservative. Instead, in the case of $CHILD_{icw}$, the direction of the bias is ambiguous. While child death is positively associated with fertility, being Tutsi is negatively associated with fertility, and the two measures are positively correlated to each other (i.e. it is more likely that children from Tutsi households were killed during the genocide). Thus, the sign of the bias depends on which effect dominates, i.e. the positive effect of $CHILD_{icw}$ or the negative effect of being Tutsi. Yet, by reading our main results for this mechanism (Tables 2 and 5) together with those of the effect of being Tutsi on fertility, we can derive a clear conclusion. As ethnicity does not affect fertility in the short run, it is very unlikely that one of our main results – that there is a replacement effect in the five years after the genocide (Table 5, column 9) – is driven by the differing propensity for fertility between Tutsi and Hutu.

Conclusions

In this paper, we study the effects of the 1994 genocide in Rwanda on fertility, using detailed individual-level data and various measures of individual exposure to violence. Using both survival analysis and count data models, we investigate the effects of exposure to violence on both the duration time to the first birth in the five years following the genocide, i.e. the hazard of having a child within the first five years after the genocide, and the total number of births in the post-genocide period.

We find evidence that both channels – the type of violence a woman was exposed to and the conflict-induced change in local demographic conditions – influence post-genocide fertility outcomes. On the one hand, the death of a mother's child during the genocide increases both the hazard of having a child within five years and the total number of births within 15 years following the genocide. This is strong evidence for a replacement effect. At the same time, sibling death during the genocide significantly lowers the hazard of having a child in the five years following the genocide as well as total post-genocide fertility, especially if a woman lost a younger sibling. This suggests the existence of a psychological mechanism. On the other hand, the genocide-induced reduction in the local sex ratio has a strong negative impact on both the hazard of having a child in the five years after the genocide and on total fertility, with the effect being highly significant in the short run and for older women.

Taken together, these results suggest that both the type of violence experienced and the genocide-induced changes in the local demographic conditions matter for post-conflict fertility outcomes. Our analysis also highlights differential effects of the genocide in terms of age cohorts, parity, and time horizon (short-, medium-, and long-runs). In particular, our results by age group are informative. The genocide has no effect on the total number of children post-genocide for the youngest age group while all measures of genocide violence have an effect for the older age groups. At the same time, looking at the hazard of having a child during the five years following the genocide, the youngest and oldest age groups are those most affected in their fertility decisions. This heterogeneity in the effects of violent events on fertility suggests the importance of better understanding the precise mechanisms behind the aggregate effects of conflict on demographic changes.

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Tables

Table 1: Summary statistics

	Observations	Mean	Std. dev.	Min	Max
Dependent variables					
Number of children born after June 1995	25,770	1.832	1.728	0	10
Number of children born between June 1995 and May 2000 (short run)	25,770	0.870	0.979	0	6
Number of children born between June 2000 and May 2005 (medium run)	25,770	0.662	0.905	0	5
Number of children born between June 2005 and May 2010 (long run)	25,770	0.280	0.635	0	4
Conflict proxies					
Exposure to child death during genocide ($CHILD_{icw}$)	25,770	0.030	0.170	0	1
Exposure to sibling death during genocide ($SIBLING_{icw}$)	25,770	0.278	0.448	0	1
Genocide-induced change in *commune*-level sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$)	25,770	0.147	0.061	0.002	0.318
Women's characteristics					
Age	25,770	31.648	8.995	15	49
Cohort 1: women aged 10-19 during genocide	25,770	0.488	0.500	0	1
Cohort 2: women aged 20-29 during genocide	25,770	0.301	0.459	0	1
Cohort 3: women aged 30-45 during genocide	25,770	0.211	0.408	0	1
Never married at the time of the genocide	25,770	0.581	0.493	0	1
No education	25,770	0.262	0.440	0	1
Primary education	25,770	0.612	0.487	0	1
Secondary or higher education	25,770	0.126	0.331	0	1
No. of children born before June 1995	25,770	1.554	2.328	0	15
Parity 0: women with no children born before June 1995	25,770	0.568	0.495	0	1
Parity 1: women with one child born before June 1995	25,770	0.095	0.293	0	1
Parity 2: women with two children born before June 1995	25,770	0.076	0.264	0	1
Parity 3+: women with three or more children born before June 1995	25,770	0.261	0.440	0	1
Percentage of children ever lost before genocide	25,770	0.058	0.168	0	1
Household characteristics					
Household wealth index	25,770	-0.035	1.697	-2.375	13.579
Place of residence is urban	25,770	0.220	0.414	0	1
Commune characteristics					
Under-five mortality during 5 years before the genocide	25,770	0.166	0.089	0.015	0.522
Sex ratio before genocide	25,770	0.980	0.154	0.771	1.392
DHS wave					
Wave 2000	25,770	0.388	0.487	0	1
Wave 2005	25,770	0.323	0.468	0	1
Wave 2010	25,770	0.289	0.453	0	1

Data source: DHS 2000, 2005, and 2005.

Table 2: Survival analysis of post-genocide fertility with child death as conflict proxy

	All	All	Aged 10-19	Aged 20-29	Aged 30-45	Parity 1	Parity 2	Parity 3+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Child death during genocide ($CHILD_{icw}$)	0.28 (5.67)***	0.27 (5.45)***	-0.11 (-0.35)	0.27 (4.08)***	0.24 (3.77)***	0.30 (1.98)**	0.35 (3.11)***	0.22 (4.17)***
Age	0.55 (55.35)***	0.46 (45.52)***	0.70 (16.64)***	0.06 (1.74)*	0.09 (0.91)	0.15 (3.53)***	0.20 (3.49)***	0.12 (3.46)***
Age squared	-0.01 (-49.31)***	-0.01 (-41.63)***	-0.01 (-6.45)***	-0.00 (-2.41)**	-0.00 (-2.29)**	-0.00 (-4.79)***	-0.00 (-4.29)***	-0.00 (-5.24)***
Never married at the time of the genocide		-0.81 (-25.96)***	-0.68 (-6.44)***	-0.49 (-13.94)***	-0.51 (-4.05)***	-0.56 (-10.02)***	-0.57 (-4.67)***	-0.42 (-3.04)***
Primary education		0.03 (1.68)*	-0.16 (-3.32)***	0.05 (1.68)*	0.06 (1.83)*	0.05 (0.96)	0.06 (0.97)	0.05 (1.59)
Secondary or higher education		-0.02 (-0.62)	-0.45 (-5.81)***	-0.01 (-0.17)	-0.11 (-1.21)	0.06 (0.70)	-0.06 (-0.54)	-0.22 (-2.81)***
Number of children born before June 1995		-0.08 (-10.31)***	-0.17 (-2.15)**	-0.02 (-1.75)*	0.10 (10.56)***			
Percentage of children ever lost before the genocide		-0.19 (-3.39)***	0.38 (1.98)**	-0.16 (-2.70)***	-0.33 (-4.02)***	-0.29 (-2.12)**	-0.05 (-0.61)	-0.28 (-4.29)***
Household wealth index		-0.03 (-3.31)***	-0.10 (-6.18)***	-0.00 (-0.35)	0.01 (0.36)	0.01 (0.26)	0.00 (0.18)	0.02 (0.95)
Place of residence is urban		-0.01 (-0.25)	0.12 (1.48)	-0.05 (-0.86)	-0.11 (-1.31)	-0.09 (-0.88)	-0.18 (-1.60)	-0.08 (-1.11)
Under-five mortality during 5 years before the genocide		-0.14 (-1.03)	-0.06 (-0.21)	-0.47 (-2.26)**	0.11 (0.40)	-0.27 (-0.72)	-0.67 (-1.66)*	-0.09 (-0.40)
Commune fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves
Observations	25,770	25,754	12,570	7,759	5,425	2,442	1,948	6,741

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are Cox regression coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. Parity is defined based on a woman's number of children as of May 1995. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table 3: Survival analysis of post-genocide fertility with sibling death as conflict proxy

	All	All	Aged 10-19	Aged 20-29	Aged 30-45	Parity 0	Parity 1	Parity 2	Parity 3+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sibling death during genocide ($SIBLING_{icw}$)	-0.03 (-1.58)	-0.04 (-1.87)*	-0.08 (-1.79)*	-0.02 (-0.78)	-0.01 (-0.28)	-0.03 (-0.69)	-0.07 (-1.35)	-0.08 (-1.35)	-0.03 (-0.99)
Age	0.55 (55.50)***	0.46 (45.59)***	0.70 (16.70)***	0.06 (1.75)*	0.09 (0.90)	0.77 (33.06)***	0.15 (3.46)***	0.20 (3.32)***	0.12 (3.36)***
Age squared	-0.01 (-49.38)***	-0.01 (-41.76)***	-0.01 (-6.45)***	-0.00 (-2.43)**	-0.00 (-2.29)**	-0.01 (-26.39)***	-0.00 (-4.73)***	-0.00 (-4.13)***	-0.00 (-5.15)***
Never married at the time of the genocide		-0.82 (-26.08)***	-0.67 (-6.38)***	-0.50 (-14.01)***	-0.51 (-4.05)***	0.31 (2.83)***	-0.56 (-10.08)***	-0.57 (-4.67)***	-0.43 (-3.09)***
Primary education		0.03 (1.70)*	-0.15 (-3.26)***	0.04 (1.54)	0.07 (1.88)*	0.03 (0.71)	0.05 (0.92)	0.05 (0.77)	0.05 (1.58)
Secondary or higher education		-0.03 (-0.65)	-0.44 (-5.76)***	-0.01 (-0.29)	-0.11 (-1.22)	-0.04 (-0.64)	0.06 (0.71)	-0.07 (-0.67)	-0.23 (-2.84)***
Number of children born before June 1995		-0.07 (-9.80)***	-0.17 (-2.30)**	-0.02 (-1.37)	0.10 (11.00)***				
Percentage of children ever lost before the genocide		-0.19 (-3.37)***	0.38 (1.96)**	-0.16 (-2.69)***	-0.34 (-4.03)***		-0.30 (-2.18)**	-0.05 (-0.59)	-0.27 (-4.22)***
Household wealth index		-0.03 (-3.41)***	-0.10 (-6.18)***	-0.00 (-0.38)	0.00 (0.23)	-0.08 (-5.90)***	0.00 (0.19)	0.00 (0.10)	0.01 (0.85)
Place of residence is urban		-0.01 (-0.18)	0.12 (1.47)	0.05 (-0.87)	-0.10 (-1.23)	0.06 (0.75)	-0.09 (-0.92)	-0.18 (-1.54)	-0.08 (-1.10)
Under-five mortality during 5 years before the genocide		-0.14 (-0.99)	-0.06 (-0.20)	-0.47 (-2.26)**	0.11 (0.37)	-0.34 (-1.44)	-0.26 (-0.70)	-0.65 (-1.62)	-0.09 (-0.41)
Commune fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves
Observations	25,770	25,754	12,570	7,759	5,425	14,623	2,442	1,948	6,741

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. Parity is defined based on a woman's number of children as of May 1995. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table 4: Survival analysis of post-genocide fertility with change in the *commune*-level sex ratio as conflict proxy

	All	All	Aged 10-19	Aged 20-29	Aged 30-45	Parity 0	Parity 1	Parity 2	Parity 3+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$)	-0.57 (-2.49)**	-0.65 (-2.95)***	-0.75 (-1.79)*	-0.38 (-1.27)	-0.86 (-2.00)**	-0.47 (-1.24)	-0.38 (-0.70)	-1.09 (-1.92)*	-0.50 (-1.44)
Sex ratio before the genocide	0.47 (1.99)**	0.67 (2.88)***	1.17 (2.60)***	0.17 (0.55)	0.48 (1.07)	0.66 (1.63)	0.51 (0.90)	0.71 (1.25)	0.30 (0.82)
Age	0.54 (55.44)***	0.46 (45.55)***	0.69 (17.14)***	0.05 (1.57)	0.06 (0.62)	0.75 (33.60)***	0.12 (3.13)***	0.14 (2.74)***	0.12 (3.44)***
Age squared	-0.01 (-49.30)***	-0.01 (-41.75)***	-0.01 (-6.76)***	-0.00 (-2.26)**	-0.00 (-2.00)**	-0.01 (-26.78)***	-0.00 (-4.45)***	-0.00 (-3.55)***	-0.00 (-5.20)***
Never married at the time of the genocide		-0.83 (-26.22)***	-0.65 (-6.37)***	-0.51 (-14.27)***	-0.50 (-4.01)***	0.29 (2.70)***	-0.54 (-10.24)***	-0.53 (-4.95)***	-0.41 (-2.91)***
Primary education		0.02 (0.98)	-0.17 (-3.58)***	0.04 (1.26)	0.06 (1.61)	0.02 (0.41)	0.04 (0.75)	0.01 (0.25)	0.03 (1.08)
Secondary or higher education		-0.04 (-1.13)	-0.45 (-5.91)***	-0.03 (-0.58)	-0.13 (-1.41)	-0.06 (-0.85)	0.06 (0.73)	-0.11 (-1.10)	-0.24 (-3.00)***
Number of children born before June 1995		-0.07 (-9.39)***	-0.12 (-1.63)	-0.01 (-1.13)	0.10 (10.61)***				
Percentage of children ever lost before the genocide		-0.19 (-3.40)***	0.19 (0.69)	-0.16 (-2.63)***	-0.37 (-4.41)***		-0.34 (-2.60)***	-0.06 (-0.70)	-0.26 (-4.17)***
Household wealth index		-0.04 (-3.88)***	-0.09 (-6.15)***	-0.01 (-0.90)	-0.01 (-0.40)	-0.08 (-5.94)***	-0.01 (-0.27)	0.00 (0.11)	-0.00 (-0.10)
Place of residence is urban		-0.06 (-1.62)	0.10 (1.60)	-0.09 (-2.11)**	-0.17 (-2.52)**	0.01 (0.09)	-0.17 (-2.12)**	-0.17 (-1.78)*	-0.13 (-2.32)**
Under-five mortality during 5 years before the genocide		0.07 (0.67)	0.10 (0.41)	-0.05 (-0.31)	0.26 (1.15)	0.09 (0.47)	0.22 (0.87)	-0.42 (-1.37)	0.07 (0.37)
Préfecture fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves
Observations	25,770	25,754	12,570	7,759	5,425	14,623	2,442	1,948	6,741

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are Cox regression coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. Parity is defined based on a woman's number of children as of May 1995. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table 5: Count data analysis of post-genocide fertility with child death as conflict proxy

	All	All	Aged 10-19	Aged 20-29	Aged 30-45	Parity 1	Parity 2	Parity 3+	Short run	Medium run	Long run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Child death during genocide ($CHILD_{icw}$)	0.11 (2.83)***	0.10 (2.51)**	0.02 (0.17)	0.10 (1.32)	0.06 (1.14)	0.02 (0.17)	0.37 (3.32)***	0.04 (0.89)	0.10 (4.74)***	0.01 (0.14)	-0.17 (-1.78)*
Age	0.44 (54.27)***	0.42 (48.41)***	0.64 (36.81)***	-0.02 (-0.31)	0.10 (1.18)	-0.04 (-0.67)	0.11 (1.74)*	0.10 (2.92)***	0.19 (33.76)***	0.22 (18.78)***	0.18 (9.02)***
Age squared	-0.01 (-60.51)***	-0.01 (-54.95)***	-0.01 (-31.64)***	-0.00 (-3.60)***	-0.00 (-3.41)***	-0.00 (-3.34)***	-0.00 (-4.96)***	-0.00 (-7.53)***	-0.00 (-35.76)***	-0.00 (-22.81)***	-0.00 (-12.84)***
Never-married at the time of the genocide		-0.28 (-11.63)***	-0.10 (-3.02)***	-0.29 (-6.44)***	-0.30 (-2.31)**	-0.52 (-7.57)***	-0.64 (-3.75)***	-0.32 (-2.45)**	-0.36 (-21.33)***	-0.06 (-2.43)**	-0.06 (-1.81)*
Primary education		0.02 (1.33)	-0.06 (-3.27)***	0.11 (3.01)***	0.03 (1.04)	0.01 (0.20)	0.12 (1.85)*	0.06 (2.08)**	0.02 (1.69)*	0.01 (0.28)	0.01 (0.51)
Secondary or higher education		-0.19 (-6.01)***	-0.33 (-10.14)***	-0.06 (-0.96)	-0.16 (-2.08)**	-0.10 (-1.01)	-0.04 (-0.35)	-0.23 (-3.14)***	-0.04 (-1.92)*	-0.20 (-5.77)***	-0.15 (-3.68)***
Number of children born before June 1995		-0.04 (-6.80)***	-0.03 (-1.00)	0.05 (2.99)***	0.08 (10.56)***				-0.06 (-14.93)***	-0.04 (-5.41)***	-0.01 (-0.47)
Percentage of children ever lost before the genocide		-0.12 (-2.60)***	0.12 (1.70)*	-0.15 (-2.02)**	-0.27 (-3.84)***	-0.59 (-3.43)***	0.07 (0.80)	-0.19 (-3.45)***	-0.06 (-2.07)**	-0.04 (-0.86)	0.07 (0.87)
Household wealth index		-0.02 (-2.90)***	-0.05 (-7.02)***	0.03 (2.05)**	0.01 (0.75)	0.03 (1.26)	-0.02 (-0.54)	0.02 (1.48)	-0.01 (-1.37)	-0.02 (-2.23)**	-0.03 (-2.85)***
Place of residence is urban		-0.06 (-1.55)	-0.02 (-0.46)	-0.07 (-0.93)	-0.14 (-2.01)**	-0.05 (-0.43)	-0.29 (-2.13)**	-0.10 (-1.48)	0.00 (0.03)	-0.06 (-1.48)	-0.14 (-2.70)***
Under-five mortality during 5 years before the genocide		-0.09 (-0.76)	-0.03 (-0.23)	-0.27 (-1.12)	0.01 (0.03)	-0.08 (-0.25)	-0.34 (-0.84)	-0.15 (-0.68)	-0.09 (-1.09)	-0.01 (-0.07)	6.29 (3.65)***
Commune fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	2005 & 2010	2010 only
Observations	25,770	25,754	12,570	7,759	5,425	2,442	1,948	6,741	25,754	15,747	7,422

Note: Poisson regressions for the number of children born after June 1995. Displayed are marginal effects at the mean and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The dependent variable measures the number of children born between June 1995 and the time of each survey wave (columns 1-8), the number of children born between June 1995 and May 2000 (column 9), the number of children born between June 2000 and May 2005 (column 10), and the number of children born between June 2005 and May 2010 (column 11). Parity is defined based on a woman's number of children as of May 1995. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table 6: Count data analysis of post-genocide fertility with sibling death as conflict proxy

	All	All	Aged 10-19	Aged 20-29	Aged 30-45	Parity 0	Parity 1	Parity 2	Parity 3+	Short run	Medium run	Long run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sibling death during genocide ($SIBLING_{icw}$)	-0.05 (-3.05)***	-0.05 (-2.85)***	-0.02 (-0.94)	-0.09 (-2.50)**	-0.04 (-1.21)	-0.02 (-0.96)	-0.17 (-2.63)***	-0.12 (-1.90)*	-0.05 (-1.55)	-0.03 (-2.32)**	-0.02 (-1.26)	-0.05 (-2.36)**
Age	0.44 (54.48)***	0.42 (48.57)***	0.64 (36.77)***	-0.01 (-0.26)	0.10 (1.18)	0.51 (38.90)***	-0.03 (-0.61)	0.11 (1.67)*	0.10 (2.95)***	0.19 (33.85)***	0.22 (18.80)***	0.18 (9.01)***
Age squared	-0.01 (-60.51)***	-0.01 (-55.09)***	-0.01 (-31.60)***	-0.00 (-3.63)***	-0.00 (-3.41)***	-0.01 (-35.65)***	-0.00 (-3.37)***	-0.00 (-4.86)***	-0.00 (-7.58)***	-0.00 (-35.86)***	-0.00 (-22.84)***	-0.00 (-12.82)***
Never-married at the time of the genocide		-0.28 (-11.72)***	-0.10 (-2.98)***	-0.29 (-6.44)***	-0.30 (-2.31)**	0.18 (3.52)***	-0.51 (-7.47)***	-0.64 (-3.73)***	-0.32 (-2.45)**	-0.36 (-21.45)***	-0.06 (-2.43)**	-0.06 (-1.71)*
Primary education		0.02 (1.43)	-0.06 (-3.23)***	0.11 (3.07)***	0.03 (1.12)	-0.01 (-0.64)	0.02 (0.25)	0.11 (1.67)*	0.06 (2.17)**	0.02 (1.79)*	0.01 (0.33)	0.01 (0.62)
Secondary or higher education		-0.19 (-5.94)***	-0.33 (-10.12)***	-0.06 (-0.90)	-0.16 (-2.05)**	-0.23 (-6.84)***	-0.09 (-0.90)	-0.06 (-0.47)	-0.23 (-3.11)***	-0.04 (-1.89)*	-0.20 (-5.75)***	-0.14 (-3.52)***
Number of children born before June 1995		-0.04 (-6.60)***	-0.03 (-0.98)	0.05 (3.09)***	0.08 (10.63)***					-0.06 (-14.53)***	-0.04 (-5.45)***	-0.01 (-0.62)
Percentage of children ever lost before the genocide		-0.12 (-2.60)***	0.12 (1.70)*	-0.16 (-2.05)**	-0.27 (-3.83)***		-0.58 (-3.44)***	0.07 (0.75)	-0.19 (-3.42)***	-0.06 (-2.08)**	-0.04 (-0.85)	0.07 (0.91)
Household wealth index		-0.02 (-2.91)***	-0.05 (-7.03)***	0.03 (2.07)**	0.01 (0.75)	-0.04 (-5.19)***	0.03 (1.22)	-0.02 (-0.51)	0.02 (1.49)	-0.01 (-1.40)	-0.02 (-2.22)**	-0.03 (-2.88)***
Place of residence is urban		-0.06 (-1.54)	-0.02 (-0.46)	-0.07 (-0.92)	-0.14 (-2.00)**	-0.04 (-0.90)	-0.05 (-0.42)	-0.28 (-2.07)**	-0.10 (-1.49)	0.00 (0.05)	-0.06 (-1.47)	-0.14 (-2.66)***
Under-five mortality during 5 years before the genocide		-0.09 (-0.75)	-0.03 (-0.22)	-0.27 (-1.11)	0.00 (0.01)	-0.07 (-0.59)	-0.05 (-0.15)	-0.36 (-0.88)	-0.15 (-0.68)	-0.09 (-1.11)	-0.01 (-0.05)	6.55 (3.89)***
Commune fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	2005 & 2010	2010 only
Observations	25,770	25,754	12,570	7,759	5,425	14,623	2,442	1,948	6,741	25,754	15,747	7,422

Note: Poisson regressions for the number of children born after June 1995. Displayed are marginal effects at the mean obtained from Poisson regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The dependent variable measures the number of children born between June 1995 and the time of each survey wave (columns 1-9), the number of children born between June 1995 and May 2000 (column 10), the number of children born between June 2000 and May 2005 (column 11), and the number of children born between June 2005 and May 2010 (column 12). Parity is defined based on a woman's number of children as of May 1995. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table 7: Count data analysis of post-genocide fertility with change in the *commune*-level sex ratio as conflict proxy

	All	All	Aged 10-19	Aged 20-29	Aged 30-45	Parity 0	Parity 1	Parity 2	Parity 3+	Short run	Medium run	Long run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Change in sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$)	-0.33 (-1.49)	-0.46 (-2.30)**	-0.23 (-1.14)	-0.19 (-0.45)	-1.06 (-2.84)***	-0.22 (-1.02)	-0.41 (-0.53)	-0.71 (-1.04)	-0.75 (-2.19)**	-0.41 (-3.13)***	-0.15 (-0.69)	-0.25 (-0.97)
Sex ratio before the genocide	0.56 (2.65)***	0.83 (4.13)***	0.68 (3.49)***	0.43 (1.01)	0.76 (2.02)**	0.61 (2.78)***	1.34 (1.83)*	1.30 (1.88)*	0.50 (1.41)	0.52 (4.04)***	0.50 (2.38)**	0.65 (2.32)**
Age	0.44 (53.89)***	0.42 (48.32)***	0.63 (36.18)***	-0.01 (-0.29)	0.10 (1.23)	0.51 (38.38)***	-0.05 (-0.96)	0.08 (1.19)	0.10 (3.02)***	0.19 (33.67)***	0.22 (18.82)***	0.19 (9.20)***
Age squared	-0.01 (-59.60)***	-0.01 (-54.98)***	-0.01 (-31.19)***	-0.00 (-3.61)***	-0.00 (-3.47)***	-0.01 (-35.31)***	-0.00 (-2.90)***	-0.00 (-4.31)***	-0.00 (-7.62)***	-0.00 (-35.84)***	-0.00 (-22.97)***	-0.00 (-12.98)***
Never-married at the time of the genocide		-0.30 (-12.44)***	-0.13 (-3.62)***	-0.31 (-6.85)***	-0.30 (-2.31)**	0.17 (3.38)***	-0.56 (-8.12)***	-0.65 (-3.72)***	-0.35 (-2.71)***	-0.37 (-21.77)***	-0.08 (-3.12)***	-0.06 (-1.89)*
Primary education		0.01 (0.44)	-0.07 (-3.86)***	0.09 (2.56)**	0.01 (0.44)	-0.03 (-1.27)	0.03 (0.40)	0.06 (0.82)	0.04 (1.42)	0.01 (1.02)	-0.01 (-0.40)	0.01 (0.26)
Secondary or higher education		-0.21 (-6.62)***	-0.34 (-10.47)***	-0.08 (-1.28)	-0.18 (-2.30)**	-0.24 (-7.20)***	-0.09 (-0.84)	-0.12 (-1.00)	-0.25 (-3.25)***	-0.05 (-2.39)**	-0.22 (-6.30)***	-0.15 (-3.72)***
Number of children born before June 1995		-0.04 (-6.11)***	-0.02 (-0.57)	0.05 (3.19)***	0.09 (10.84)***					-0.06 (-14.08)***	-0.04 (-5.08)***	-0.01 (-0.77)
Percentage of children ever lost before the genocide		-0.12 (-2.65)***	0.12 (1.80)*	-0.14 (-1.86)*	-0.29 (-3.90)***		-0.59 (-3.38)***	0.05 (0.60)	-0.18 (-3.20)***	-0.06 (-2.14)**	-0.04 (-0.84)	0.08 (1.09)
Household wealth index		-0.03 (-3.43)***	-0.05 (-6.96)***	0.02 (1.13)	0.00 (0.18)	-0.04 (-5.44)***	0.02 (0.76)	-0.02 (-0.72)	0.01 (0.65)	-0.01 (-1.85)*	-0.02 (-2.59)***	-0.03 (-2.90)***
Place of residence is urban		-0.10 (-3.26)***	-0.04 (-1.24)	-0.17 (-2.66)***	-0.16 (-2.74)***	-0.06 (-1.90)*	-0.22 (-2.28)**	-0.29 (-2.56)**	-0.13 (-2.39)**	-0.02 (-1.23)	-0.07 (-2.11)**	-0.18 (-4.22)***
Under-five mortality during 5 years before the genocide		0.19 (2.15)**	0.11 (1.07)	0.46 (2.39)**	0.10 (0.56)	0.19 (2.02)**	0.38 (1.38)	0.20 (0.56)	0.06 (0.32)	0.06 (0.90)	0.20 (2.10)**	0.15 (1.61)
Préfecture fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	2005 & 2010	2010 only
Observations	25,770	25,754	12,570	7,759	5,425	14,623	2,442	1,948	6,741	25,754	15,747	7,422

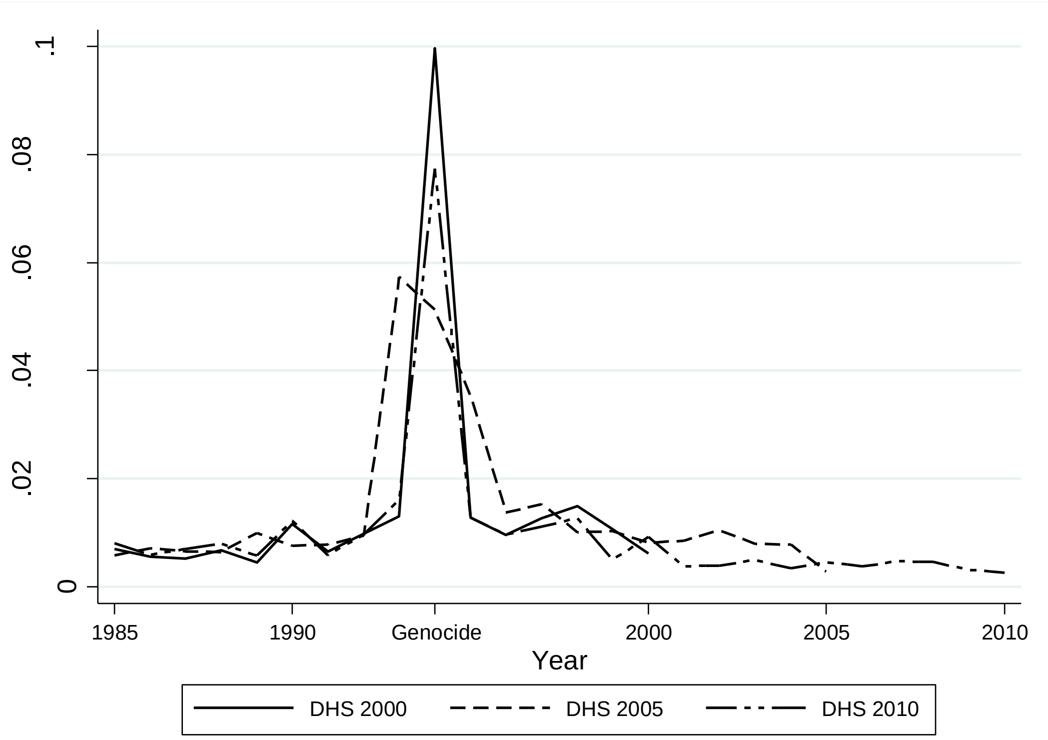
Note: Poisson regressions for the number of children born after June 1995. Displayed are marginal effects at the mean obtained from Poisson regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The dependent variable measures the number of children born between June 1995 and the time of each survey wave (columns 1-9), the number of children born between June 1995 and May 2000 (column 10), the number of children born between June 2000 and May 2005 (column 11), and the number of children born between June 2005 and May 2010 (column 12). Parity is defined based on a woman's number of children as of May 1995. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Figures

Fig. 1: Child mortality over time, 1985-2010

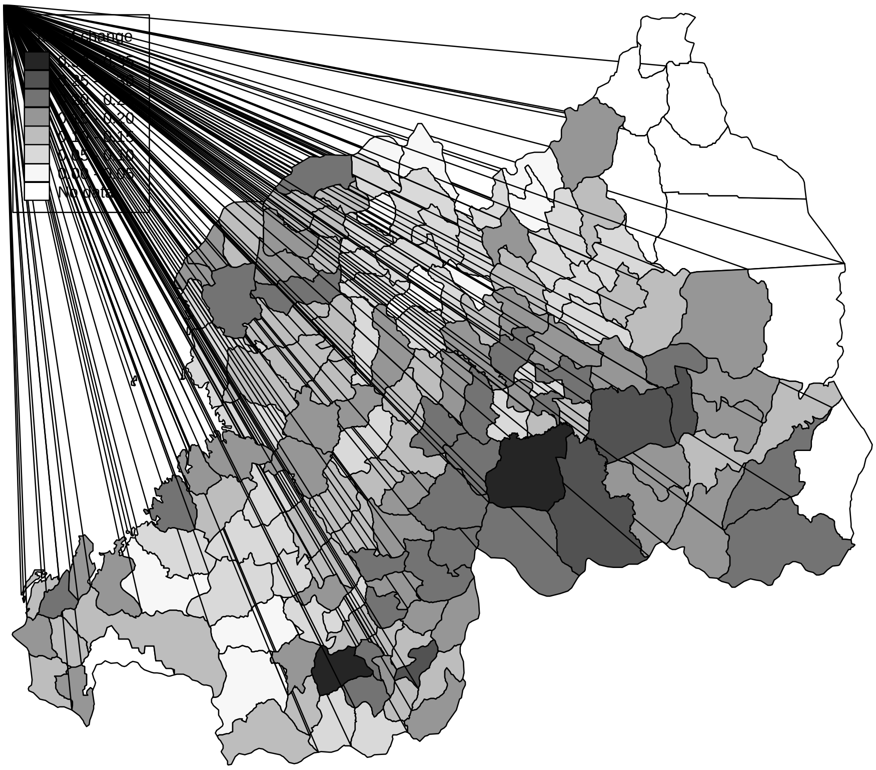
Note: The figure shows the percentage of child deaths relative to the total number of living children reported by sample women for each year during the 1985-2010 period. Data source: DHS 2000, 2005, and 2010.

Fig. 2: Sibling mortality over time, 1985-2010



Note: The figure shows the percentage of sibling deaths relative to the total number of living siblings reported by sample women for each year during the 1985-2010 period. Data source: DHS 2000, 2005, and 2010.

Fig. 3: Spatial variation in the genocide-induced change in the *commune*-level sex ratio, 1991-2002



Data source: Census 1991 and 2002.

Supplementary Online Appendix

Table A1: Survival analysis of post-genocide fertility with son/daughter death and brother/sister death as conflict proxy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Son death during genocide	0.20 (3.25)***							
Daughter death during genocide		0.29 (4.28)***						
Brother death during genocide			-0.04 (-1.89)*					
Older brother death during genocide				0.03 (1.39)				
Younger brother death during genocide					-0.09 (-3.07)***			
Sister death during genocide						-0.04 (-1.37)		
Older sister death during genocide							-0.03 (-0.96)	
Younger sister death during genocide								-0.04 (-1.23)
All controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves
Observations	25,754	25,754	25,754	25,754	25,754	25,754	25,754	25,754

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are Cox regression coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A2: Count data analysis of post-genocide fertility with son/daughter death and brother/sister death as conflict proxy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Son death during genocide	0.11 (2.13)**							
Daughter death during genocide		0.07 (1.32)						
Brother death during genocide			-0.04 (-2.22)**					
Older brother death during genocide				-0.01 (-0.47)				
Younger brother death during genocide					-0.06 (-2.45)**			
Sister death during genocide						-0.05 (-2.34)**		
Older sister death during genocide							-0.03 (-1.25)	
Younger sister death during genocide								-0.04 (-1.36)
All controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves
Observations	25,754	25,754	25,754	25,754	25,754	25,754	25,754	25,754

Note: Poisson regressions for the number of children born after June 1995. Displayed are marginal effects at the mean obtained from Poisson regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A3: Survival analysis of post-genocide fertility with simultaneous inclusion of all three conflict proxies

	All	Aged 10-19	Aged 20-29	Aged 30-45	Parity 1	Parity 2	Parity 3+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Child death during genocide ($CHILD_{icw}$)	0.26 (5.41)***	-0.04 (-0.12)	0.25 (3.78)***	0.24 (4.00)***	0.29 (2.02)**	0.35 (3.58)***	0.22 (4.06)***
Sibling death during genocide ($SIBLING_{icw}$)	-0.05 (-2.54)**	-0.08 (-1.93)*	-0.04 (-1.30)	-0.04 (-1.04)	-0.11 (-2.06)**	-0.08 (-1.45)	-0.05 (-1.59)
Change in sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$)	-0.63 (-2.86)***	-0.72 (-1.72)*	-0.35 (-1.15)	-0.88 (-2.07)**	-0.27 (-0.50)	-1.10 (-1.94)*	-0.49 (-1.39)
All controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Préfecture fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves
Observations	25,754	12,570	7,759	5,425	2,442	1,948	6,741

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are Cox regression coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. Parity is defined based on a woman's number of children as of May 1995. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A4: Count data analysis of post-genocide fertility with simultaneous inclusion of all three conflict proxies

	All	Aged 10-19	Aged 20-29	Aged 30-45	Parity 1	Parity 2	Parity 3+	Short run	Medium run	Long run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Child death during genocide ($CHILD_{icw}$)	0.10 (2.47)**	0.03 (0.25)	0.11 (1.39)	0.07 (1.54)	0.08 (0.58)	0.34 (3.13)***	0.05 (1.08)	0.10 (4.71)***	0.00 (0.08)	-0.16 (-1.68)*
Sibling death during genocide ($SIBLING_{icw}$)	-0.06 (-3.24)***	-0.01 (-0.86)	-0.10 (-2.85)***	-0.05 (-1.63)	-0.19 (-3.05)***	-0.14 (-2.14)**	-0.06 (-1.86)*	-0.03 (-2.95)***	-0.03 (-1.45)	-0.05 (-2.16)**
Change in sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$	-0.44 (-2.20)**	-0.23 (-1.12)	-0.14 (-0.33)	-1.05 (-2.81)***	-0.27 (-0.35)	-0.68 (-1.02)	-0.71 (-2.07)**	-0.39 (-3.03)***	-0.14 (-0.65)	-0.25 (-0.96)
All controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Préfecture fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All waves	All waves	All waves	All waves	All waves	All waves	All waves	All waves	2005 & 2010	2010 only
Observations	25,754	12,570	7,759	5,425	2,442	1,948	6,741	25,754	15,747	7,422

Note: Poisson regressions for the number of children born after June 1995. Displayed are marginal effects at the mean obtained from Poisson regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The dependent variable measures the number of children born between June 1995 and the time of each survey wave (columns 1-7), the number of children born between June 1995 and May 2000 (column 8), the number of children born between June 2000 and May 2005 (column 9) and the number of children born between June 2005 and May 2010 (column 10). Parity is defined based on a woman's number of children as of May 1995. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A5: Survival analysis of post-genocide fertility with placebo child/sibling death as conflict proxy

	(1)	(2)
Child death during genocide ($CHILD_{icw}$)	0.27 (5.40)***	
Child death during 1990-1993	0.03 (1.01)	
Sibling death during genocide ($SIBLING_{icw}$)		-0.04 (-1.86)*
Sibling death during 1990-1993		0.00 (0.08)
All controls	Yes	Yes
Commune fixed effects	Yes	Yes
Wave fixed effects	Yes	Yes
Sample	All waves	All waves
Observations	25,754	25,754

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are Cox regression coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A6: Count data analysis of post-genocide fertility with placebo child/sibling death as conflict proxy

	(1)	(2)
Child death during genocide ($CHILD_{icw}$)	0.10 (2.48)**	
Child death during 1990-1993	0.05 (1.94)*	
Sibling death during genocide ($SIBLING_{icw}$)		-0.05 (-2.78)***
Sibling death during 1990-1993		-0.00 (-0.22)
All controls	Yes	Yes
Commune fixed effects	Yes	Yes
Wave fixed effects	Yes	Yes
Sample	All waves	All waves
Observations	25,754	25,754

Note: Poisson regressions for the number of children born after June 1995. Displayed are marginal effects at the mean obtained from Poisson regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A7: Survival analysis of post-genocide fertility with restricted samples

	(1)	(2)	(3)
Child death during genocide ($CHILD_{icw}$)	0.25 (5.49)***		
Sibling death during genocide ($SIBLING_{icw}$)		-0.04 (-1.88)*	
Change in sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$)			-0.54 (-1.67)*
All controls	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	No
Préfecture fixed effects	No	No	Yes
Wave fixed effects	Yes	Yes	Yes
Sample	All waves	All waves	All waves
Observations	10,143	25,422	14,965

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are Cox regression coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. Parity is defined based on a woman's number of children as of May 1995. All controls include: age; age squared; never married at the time of the genocide (except in column 3); primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide and who had children before the genocide (column 1), who had siblings before the genocide (column 2), and who were never married at the time of the genocide (column 3). Data source: DHS 2000, 2005, and 2010.

Table A8: Count data analysis of post-genocide fertility with restricted samples

	(1)	(2)	(3)
Child death during genocide ($CHILD_{icw}$)	0.09 (2.19)**		
Sibling death during genocide ($SIBLING_{icw}$)		-0.05 (-2.86)***	
Change in sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$)			-0.29 (-1.29)
All controls	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	No
Préfecture fixed effects	No	No	Yes
Wave fixed effects	Yes	Yes	Yes
Sample	All waves	All waves	All waves
Observations	10,143	25,422	14,965

Note: Poisson regressions for the number of children born after June 1995. Displayed are marginal effects at the mean obtained from Poisson regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The dependent variable measures the number of children born between June 1995 and the time of each survey wave. All controls include: age; age squared; never married at the time of the genocide (except in column 3); primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide and who had children before the genocide (column 1), who had siblings before the genocide (column 2), and who were never married at the time of the genocide (column 3). Data source: DHS 2000, 2005, and 2010.

Table A9: Survival analysis of post-genocide fertility, exploring recall bias death

	(1)	(2)	(3)	(4)
Child death during genocide ($CHILD_{icw}$)	0.27 (5.45)***	0.15 (2.09)**	0.37 (4.31)***	0.25 (2.43)**
All controls	Yes	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	No	No	No
Sample	All waves	2000 only	2005 only	2010 only
Observations	25,754	10,007	8,325	7,422

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A10: Survival analysis of post-genocide fertility with time trends

	(1)	(2)	(3)
Child death during genocide ($CHILD_{icw}$)	0.27 (5.39)***		
Sibling death during genocide ($SIBLING_{icw}$)		-0.04 (-1.88)*	
Change in sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$)			-0.67 (-2.97)***
All controls	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	No
Commune-specific time trends	Yes	Yes	No
Préfecture fixed effects	No	No	Yes
Préfecture-specific time trends	No	No	Yes
Wave fixed effects	Yes	Yes	Yes
Sample	All waves	All waves	All waves
Observations	25,754	25,754	25,754

Note: Cox regressions on duration time to the first birth between June 1995 and May 2000. Displayed are coefficients and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The right-censored indicator takes the value one if the child birth has not occurred by May 2000; and zero otherwise. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A11: Count data analysis of post-genocide fertility with time trends

	(1)	(2)	(3)
Child death during genocide ($CHILD_{icw}$)	0.10 (2.54)**		
Sibling death during genocide ($SIBLING_{icw}$)		-0.05 (-2.66)***	
Change in sex ratio ($\Delta Sex\ ratio_{c,1991-2002}$)			-0.47 (-2.36)**
All controls	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	No
Commune-specific time trends	Yes	Yes	No
Préfecture fixed effects	No	No	Yes
Préfecture-specific time trends	No	No	Yes
Wave fixed effects	Yes	Yes	Yes
Sample	All waves	All waves	All waves
Observations	25,754	25,754	25,754

Note: Poisson regressions for the number of children born after June 1995. Displayed are marginal effects at the mean obtained from Poisson regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. All controls include: age; age squared; never married at the time of the genocide; primary education; secondary or higher education; number of children born before June 1995; percentage of children ever lost before the genocide; household wealth index; place of residence is urban; and under-five mortality in the five years before the genocide. Sample: women aged 10-45 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A12: Exploring differences in pre-genocide fertility across ethnic groups

	1987-1992	1982-1992	1977-1992
	(1)	(2)	(3)
Tutsi	-0.03 (-1.05)	-0.11 (-2.70)***	-0.15 (-3.60)***
All controls	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	Yes
Sample	1992	1992	1992
Observations	6,509	6,509	6,509

Note: Poisson regressions for the number of children born in the period reported in the first row. Displayed are marginal effects at the mean obtained from Poisson regressions and robust t-statistics, clustered at the PSU level, in brackets with * $p<0.1$, ** $p<0.05$, *** $p<0.01$. All controls include: age; age squared; primary education; secondary or higher education; household wealth index; place of residence is urban. Data source: DHS 1992.