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

Kati Kraehnert , Tilman Brück , Michele Di Maio and Roberto Nisticò

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University of Naples Federico II

University of Salerno

Bocconi University, Milan

CSEF - Centre for Studies in Economics and Finance
DEPARTMENT OF ECONOMICS – UNIVERSITY OF NAPLES
80126 NAPLES - ITALY
Tel. and fax +39 081 675372 – e-mail: csef@unisa.it

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Abstract
This paper analyzes the effects of the 1994 genocide in Rwanda on fertility outcomes. We study the effects of violence on both the timing of the first birth after the genocide and the total number of post-genocide births. We analyze individual-level data from several Demographic and Health Surveys, using event history 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 subsequent fertility. First, the type of violence exposure as measured by child death as well as by the death of a woman's sibling. Second, the conflict-induced change in local demographic conditions as captured by the change in the commune-level sex ratio. Results indicate that the genocide has 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 replacement effect. Having experienced the death of a child during the genocide reduces the time to the first birth after the genocide and increases the total number of births in the post-genocide period. Experiencing a sibling death during the genocide significantly lowers fertility in the long run. The effect is strongest if a woman loses a younger sister. Finally, the genocide-induced reduction in the sex ratio has a strong negative impact on fertility, both in terms of the timing of the first birth and the total number of births after the genocide.

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

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* German Institute for Economic Research (DIW Berlin). Postal address: DIW Berlin, Mohrenstr. 58, 10117 Berlin, Germany. E-mail kkraehnert@diw.de, phone +49-30-89789-442, fax +49-309789-108

** ISDC - International Security and Development Center, Berlin, Germany and Leibniz Institute of Vegetable and Ornamental Crops (IGZ)

*** Università di Napoli Parthenope. E-mail: michele.dimaio@uniparthenope.it.

**** Università di Napoli Federico II and CSEF. E-mail: roberto.nistico@unina.it

Table of contents

1. Introduction

2. Literature

2.1 Literature on the Effects of Conflict on Fertility

2.2 Literature on the Genocide in Rwanda

2.3 Conceptual Framework

2.4 Type and Intensity of Individual Exposure to Violence

2.5 Changes in the Local Demographic Conditions ....

3. Data

4. Estimation Strategy

5. Results

5.1 The Effects of the Genocide on the Timing of the First Birth after the Genocide

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

5.3 Robustness Tests

6. Conclusions

References

Introduction

Does violent conflict affect fertility? A number of studies address this important issue, finding that violent conflict influences fertility during and after the conflict. Effects are shown to vary across empirical contexts (see 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 microdata to provide empirical evidence on various mechanisms linking conflict to fertility. As a test bed, we focus on the 1994 genocide in Rwanda, one of the most devastating conflicts since World War II, during which at least 500,000 individuals died in just hundred days. There are two main reasons why the Rwandan genocide provides a suitable setting for exploring this question. First, data on fertility histories for Rwandan women are available for multiple post-genocide surveys that are representative at the national level and of high quality, which is something unique for conflict-affected countries. Second, the fact that the Rwandan genocide was extremely violent and of extremely short duration reduces the possibility that other relevant events may confound the identification of its effect on fertility outcomes.

In our analysis, we use event history and count data models to study the effect of violence on the timing of the first birth after the genocide as well as on the total number of post-genocide births. We focus on two main channels through which conflict may affect fertility. First, we look at the effect of different types of individual exposure to violence. In particular, we consider the effect of experiencing a child death or a sibling death 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 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 are cross-sectional waves of Demographic and Health Surveys (DHS) collected in Rwanda in 2000, 2005, and 2010, which are representative of all households in Rwanda. This allows us to disaggregate 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 period, thereby providing a comprehensive perspective on the conflict-induced adjustments in fertility.

The paper has three main findings. First, there is strong evidence of a replacement effect. Having experienced the death of a child during the genocide reduces the time to the first birth after the genocide and increases the total number of births in the post-genocide period. Second, experiencing a sibling death during the genocide significantly lowers fertility in the long run. The effect is strongest if a woman loses a younger sister. This result suggests that a psychological mechanism may be at work, influencing fertility outcomes of surviving individuals. Finally, the genocide-induced reduction in the sex ratio has a strong negative impact on fertility, both in terms of the timing of the first birth and the total number of births after the genocide.

The paper proceeds as follows: The next section reviews research on both the links between violent conflict and fertility as well as the Rwandan genocide. The following section presents the conceptual framework on which our empirical analysis is based. We then describe the data and variables used in our analysis. The following section presents the estimation strategies employed, followed by a discussion of the empirical results and robustness tests. The last section concludes.

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, and 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 recently, a number of studies are investigating the impact of violent conflict on fertility outcomes at the micro level. Some studies find that conflict tends to increase fertility, for instance 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) find an increase in post-genocide fertility only 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.[1] 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 (see for instance Agadjanian and Prata 2002; Heuveline and Poch 2007; Lindstrom and Berhanu 1999)

Literature on the Genocide in Rwanda

The Rwandan genocide was 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 the Kigali airport, killing all passengers.[2] 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;

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

[2] 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).

Yanagizawa-Drott 2014). Results show that the genocide had a severe impact on 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 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 the young cohort, while they do not find significant effects for the two older cohorts. Yet, they only focus on the effect on total fertility and do not explore the mechanisms explaining these effects at the micro level.

Conceptual Framework

Conflict may affect fertility through different demand and supply channels (Brück and Schindler 2009; Verwimp et al. 2009; Williams et al. 2012). First, on the one hand, conflict may reduce fertility by delaying childbirth because of the possibility that women are exposed to violence and harm. On the other hand, demand for children may increase because of the parents' desire to replace lost children as a risk-insurance strategy (Agadjanian and Prata 2002). Since children are a potential source of economic support for parents at 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). At the same time, deteriorating economic conditions may instead reduce fertility because couples may respond to a sudden decline in income by delaying marriage and birth in order to smooth consumption (Lee 1990; Rindfuss et al. 1978). Finally, conflict may affect the demand for children by decreasing a woman's education attainment, thereby encouraging early female marriage (La Mattina 2017).

On the supply side, fertility depends on 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). Finally, psychological stress and a decline in nutritional status associated with conflict may reduce fecundity and the frequency of intercourse (Kidane 1989).

The heterogeneous empirical evidence discussed above and the various possible mechanisms linking conflict to fertility we described here suggest that 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 by a woman's sibling 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 for 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) and insurance (child survival hypothesis) (Hossain et al. 2007; Preston 1978; Schultz 1969; Wolpin 1997). Instead, the price 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). Moreover, the positive effect of child mortality on subsequent fertility is reinforced by reduced expected returns from investments in child education, which induces a substitution of quantity for quality of children. 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 death depends on the relative strength of the replacement motive (which tends to increase fertility) and the income effect (which tends to reduce fertility). This implies 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 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 reason for the sibling death matters. Moreover, surviving brothers and sisters are 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 and with same-sex dyads being closer than male-female sibling pairs (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 the 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).[3] 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 the decline in the sex ratio induced by WWII increased the proportion of out-of-wedlock childbearing but reduced overall fertility in Bavaria, Germany. As for Rwanda, there is robust evidence that the genocide reduced the sex ratio, that the effect was stronger in communes with 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).

[3] 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 single because of deteriorating economic conditions and increased risk of becoming a victim of sexual violence, thus increasing fertility. Second, the genocide may affect the age at the time of marriage: if conflict induces couples to delay marriage, this would decrease fertility.

Data

Our analysis builds on three cross-sectional waves of Rwanda Demographic and Health Surveys (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 who were present in the household on the night before the interview were eligible for interviewing. The questionnaire 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. Table 1 reports summary statistics for the main variables we use in the analysis.

[Table 1 about here]

The DHS provide detailed information on women's birth histories, maternal and child health, marital status, and women's socio-economic characteristics, including educational attainment, main occupation, and place of living. The DHS also collect some information on respondents' partners, including age, education, and occupation. Income and consumption expenditures were not recorded. Therefore, we construct a wealth index based on household assets.[4]

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 great detail. For each sample woman who has ever lost a child, the month of death, gender, and age of the deceased child were recorded (while the cause of death is not asked for). This allows us to create a dummy variable $CHILD_{ict}$, which takes the value one if a woman *i*, living in commune *c*, interviewed in wave *t*, 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

[4] Components of the wealth index include durables (radio, refrigerator, bicycle, motorcycle, car), source of drinking water, characteristics of floor materials, and type of toilet facility. 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 not frequently replaced (Sahn and Stifel 2000).

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 in the pre-genocide period the percentage of child deaths is not low (as is expected for a low-income country, such as 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.[5] This allows us to create the dummy variable $SIBLING_{ict}$ 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, for female siblings we exclude all deaths related to pregnancy and childbirth. 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 one 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 $\Delta Sex\ ratio_{c1991-2002}$ as the difference between the pre-genocide and post-genocide sex ratio 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).[6] 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 128 communes included in the analysis. On average, the sex ratio decreased by 15 percentage points, from 0.98 males per

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

[6] 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.

female in 1991 to 0.83 males per female in 2002. The sex ratio decreased in all 128 communes, with the value of this reduction ranging from a minimum of 0.002 to a maximum of 0.32. Note that $\Delta Sex\ ratio_{c1991-2002}$ takes positive values only, 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 an event history analysis in which we investigate the effects of conflict on the timing of the first birth after the genocide. We estimate the following Cox regression model:

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

where $h_{ict}(t)$ is the hazard rate for woman *i*, living in commune *c*, interviewed in wave *t*. The variable $h_0(t)$ is the baseline hazard function that is assumed to be unknown and is un-parameterized.[7] The duration time is defined as the number of months between June 1995[8] and the first birth, if any, occurring before May 2000, inclusive. Note that here we are using data on births occurring during the same time window – June 1995 to May 2000 – in each DHS wave. 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 giving birth after May 2000 are treated as right-censored observations. $Conflict_{ict}$ is a dummy variable indicating a woman's exposure to violence during the genocide. As 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 a child death during the genocide, and zero otherwise ($CHILD_{ict}$); and a dummy variable taking the value one if the woman experienced a sibling death during the genocide, and zero otherwise ($SIBLING_{ict}$). X'_{ict} is a matrix of covariates including i) woman-specific characteristics (age, age squared, education level, marital status,[9] and

[7] 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.

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

[9] This is proxied by whether the woman is *currently* in a union and whether she has been in more than one union. Note that the DHS does not capture the date of the current marriage (or informal union) if it is not the first marriage (or informal union). Thus, for sample women who have been married more than once at the time of the

previous fertility[10]); 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 prior to the survey). Finally, δ_c is a vector of commune dummies, capturing all time-invariant factors at the commune level (commune fixed effects) and θ_t is a vector of dummies for survey wave (time fixed effects). Standard errors are clustered at the primary sampling unit (PSU) level.

Next, we perform an event history analysis, using as conflict measure the genocide-induced change in the commune-level sex ratio. We estimate the following Cox regression model:

$$h_{ict}(t) = h_0(t)\,\exp(\beta\ \Delta Sex\ ratio_{c1991-2002} + \pi\ Sex\ ratio_{c1991} + X'_{ict}\,\gamma + \delta_p + \theta_t) \quad (2)$$

where $h_{ict}(t)$ is the hazard rate for woman *i*, living in commune *c*, interviewed in wave *t*, as defined in Eq. 1. The variable $\Delta Sex\ ratio_{c1991-2002}$ 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 $Sex\ ratio_{c1991}$ accounts for the pre-genocide level of the sex ratio in the commune. X'_{ict} 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.[11] 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:

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

where Y_{ict} denotes the post-genocide fertility of woman *i*, living in commune *c*, and interviewed at time *t*. 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

survey interview, there is the possibility that a woman lost her husband during the genocide and then remarried. In this case, a positive effect of the genocide on fertility would be a lower bound of the true effect.

[10] Number of children born before May 1995 and number of children ever lost, except those lost during the genocide.

[11] 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.

three DHS waves (2000, 2005, and 2010) here means that in each wave, a different time period for when births can occur is considered. This period is 5 years, 10 years, and 15 years long for the 2000, 2005, and 2010 wave, respectively. $Conflict_{ict}$ is a dummy variable indicating a woman's exposure to conflict during the genocide, as measured by the two different conflict proxies outlined above, $CHILD_{ict}$ and $SIBLING_{ict}$. X'_{ict} 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_t is a vector of dummies for survey waves (time fixed effects). u_{ict} 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 as proxy the genocide-induced change in the commune-level sex ratio. To this end, we estimate the following Poisson regression model:

$$Y_{ict} = \alpha + \beta\, \Delta Sex\ ratio_{c1991-2002} + \pi\, Sex\ ratio_{c1991} + X'_{ict}\, \gamma\ + \delta_p +\ \theta_t + u_{ict} \quad (4)$$

where Y_{ict} denotes the number of children born between June 1995 and the date of interview to woman *i*, living in commune c, and interviewed at time *t*. As in Eq. 2, $\Delta Sex\ ratio_{c1991-2002}$ measures the change in the commune-level sex ratio from the pre-genocide period to the post-genocide period. The variable $Sex\ ratio_{c1991}$ measures the pre-genocide level of the sex ratio in each commune. X'_{ict} 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 Timing of the First Birth after the Genocide

As a first step, we investigate the effects of genocide exposure on the timing of the first birth in the five years after the genocide. Our sample includes all women aged 10-49 at the time of the genocide. As a refinement, we also estimate the determinants of the timing to the first birth 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 May 1995.

Table 2 reports results obtained from estimating Eq. 1 with a Cox regression model when we use, as measure of conflict exposure, $CHILD_{ict}$, a dummy taking the value one if a woman experienced a child death during the genocide and zero otherwise. Column 1 reports results from the baseline regression specification in which we only include the conflict proxy, the age (and age squared) of the woman, commune fixed effects and time fixed effects. Results show that for women exposed to child death during the genocide, the risk of giving birth is higher than for women not exposed to child death, i.e. the survival time measured as the number of months to the first birth after genocide is shorter. This is consistent with a replacement effect at work. As shown in column 2, this result is robust to the inclusion of a large number of control variables. The estimated coefficient of the conflict variable (0.31) implies that the hazard of giving a birth within the five years immediately after the genocide is 36 % higher for women who lost a child during the genocide than for women who did not experience a child death.[12] Interestingly, when we look at the results by women's age cohort (columns 3-5), we find the effect to be stronger for women aged 20-29 at the time of the genocide relative to women in the older cohort (i.e. aged 30-49), while it is not statistically significant for women who were very young during the genocide (i.e. aged 10-19). Finally, we look at the effects of child death on the survival time 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. This suggests that the replacement effect is stronger for women with fewer children. Interestingly, 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 independently 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 timing of the first birth after the genocide. Results are displayed in Table 3. As before, we report results for the baseline and main specifications (the one with all controls), and by age cohort and parity. The variable of interest, $SIBLING_{ict}$, has a negative coefficient in most specifications. This indicates that the risk of giving a birth is smaller, i.e. the survival time is longer, for women exposed to a sibling death. Yet, the estimated coefficient is never significant at conventional levels. We interpret these results as suggesting that the possible negative psychological effect of a sibling loss due to the genocide does not affect post-genocide fertility in the short run. Interestingly, when disaggregating the effects of sibling

[12] This is computed as: 100*[(exp(0.31*1)-exp(0.31*0))/exp(0.31*0].

death by a sibling's age relative to each sample woman (Table A1, columns 3-8 in the supplementary online Appendix), we find a significant and negative effect for the death of a younger sibling – irrespective of the gender of the deceased younger sibling – on fertility (i.e. a longer survival time). This is in line with the theoretical prediction suggesting a stronger effect for the death of a younger sibling.

[Table 3 about here]

As a final step in our event history analysis, Table 4 reports results obtained when using the genocide-induced change in the commune-level sex ratio to measure conflict exposure (Eq. 2). Results show that the coefficient of interest is negative in all specifications. This suggests that the time until the first birth after the genocide increases with the genocide-induced reduction in the relative number of available men. In other words, women exposed to a more severe local shortage of men because of the genocide have a lower risk of giving birth in the five years after the genocide. Moreover, reading across columns 3-5, our results show that – not surprisingly – the negative effects on the fertility risk of the decline in the sex ratio is higher and more significant for older women.

[Table 4 about here]

Lastly, we comment on the covariates that turn out to be significant across the various 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 risk of giving birth is smaller for women from relatively wealthier households, i.e. the time to the first birth after genocide is longer. The second variable is the number of children born before the genocide: its negative sign indicates that the fertility risk declines with the number of children born before the genocide. Finally, we find that the number of children ever lost is always significant and positive, indicating that the replacement motive is always present. In particular, controlling for this variable when employing child death as conflict proxy (Table 2) ensures that $CHILD_{ict}$ picks up the specific effect of child death due to the genocide.

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

All results discussed so far focus on the first birth occurring in the five years immediately after the genocide. To complement the previous analysis, 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 event history analysis, our sample includes all women aged 10-49 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 women'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 term (2005-2010).

Table 5 reports results obtained from estimating Eq. 3 with a Poisson regression model and when using $CHILD_{ict}$ as 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 $CHILD_{ict}$ 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 give significantly more births in the post-genocide period. The result is robust to the inclusion of the full set of control variables (column 2, the main specification). As regards the magnitude of the estimated coefficient, we find that having experienced a child death during the genocide increases the predicted number of children born after the genocide by 13 %. The analysis by cohort (columns 3-5) shows that the effect is larger and highly significant for the group of women aged 20-29. The analysis by parity (columns 6-8) indicates that, as expected, the effect disappears for women with three or more children, i.e. when the replacement motive is weaker. Finally, columns 9-11 show that the replacement effect is stronger and only significant in the short run, i.e. in the five years after the genocide. Interestingly, as in the event history analysis, we find that the effect of child death is significant and positive for both son and daughter death, even if in this case the magnitude of the effect is larger 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 as measure of genocide exposure the variable $SIBLING_{ict}$. Table 6 shows the results. Columns 1 and 2 report the estimates for the baseline and main specifications. The negative coefficients for $SIBLING_{ict}$ indicates 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 a sibling death during the genocide decreases the predicted number of children by 3 %.

When we look at the effect by age cohort (columns 3-5), the sign of the effect remains negative for all groups, but the coefficient is no longer significant at conventional levels. The analysis by parity (columns 6-9) indicates that, while the effect is large and significant for parity 1 and 2, it is not significant for women who either did not have children as of May 1995 or who had three or more children as of May 1995. We interpret these results as suggesting that the negative psychological effect of experiencing a sibling death on the decision of having an additional birth is likely to be less relevant for women who do not have children yet or who already have several children. 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 large and significant only in the long run, i.e. when we analyze fertility outcomes during the 2005-2010 period. Interestingly, when disaggregating sibling death by the age of the sibling relative to sample women, we find a significant and negative effect for the death of a younger sister (Table A2 in the supplementary online Appendix). This finding confirms results from the event history analysis and is in line with theoretical predictions suggesting a stronger effect for same-sex and younger sibling deaths. Note that the death of a younger sister is likely to have occurred while the woman was still living with her parents. Thus, this variable may capture the trauma effect of having witnessed violence committed against close family members or stigmatization as a result of belonging to a victimized household.

[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 $\Delta Sex\ ratio_{c1991-2002}$ are negative. 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. Yet, this result is only significant when controlling for a number of variables, including the sex ratio level before the genocide (column 2). Looking at the results in the other columns, we find that the effect of the reduction in the sex ratio is only significant for the oldest age cohort and for women with higher parity. This confirms the theoretical predictions linking the effects of the genocide on fertility through the channel of marital matching.

[Table 7 about here]

As regards other covariates, we find some to be significant across results in Tables 5, 6, and 7. The number of children a woman has ever lost (apart from those lost during the genocide) is always positive and significant. In line with results from the event history analysis, this indicates that the replacement motive is always present. Finally, results show that both having secondary (or higher) education and the number of children born before the genocide tend to decrease fertility.

Robustness Tests

As a first test, 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 level. Results in Table A3 (for the event history model) and Table A4 (for the count data model) in the supplementary online Appendix show that all main results for the effects of conflict – as proxied by $CHILD_{ict}$, $SIBLING_{ict}$, and $\Delta Sex\ ratio_{c1991-2002}$ – are unchanged.

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 anytime a woman loses a child or a sibling. Recall that in our main analysis, we already control for the number of lost children (excluding those lost during the genocide). We re-estimate Eq. 3 with $CHILD_{ict}$ as genocide measure, but now adding a dummy variable taking the value one if a woman experienced a child death before the genocide period (1990-1993), 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 a child death during the genocide rather than in another period is significantly larger,[13] 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 on fertility in the post-genocide period. To this end, we construct a placebo dummy variable taking the value one if the sibling death occurs in the 1990-1993 period (i.e. before the genocide), and zero otherwise. Again, we re-estimate Eq. 3 with $SIBLING_{ict}$ as genocide measure, adding the placebo dummy to the full set of controls.[14] Results in Table A5, column 2 in the supplementary online Appendix show that the placebo variable for sibling death is not statistically different from zero. This evidence confirms that

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

[14] In theory, sibling death may affect fertility also during peacetime, i.e. by reducing the capacity of a woman's family to pay a decent bride price, which in turn may reduce her chances of getting married and having children.

our analysis captures fertility effects that are specific to a woman's sibling mortality during the 1994 genocide.

Third, since our analysis builds on retrospective data, we check whether recall bias is a serious concern. To test for this, we re-estimate the specification in Table 5, column 9 separately for each DHS wave. This way, we can compare results for exactly the same time window. Results reported in Table A6 in the supplementary online Appendix show that the effect of child death on fertility is remarkably similar across the different DHS waves. We interpret this as supporting evidence that the recall bias is not a major concern for our analysis.

Finally, we explore the possibility that fertility differs across Hutu and Tutsi and how this difference may bias our results. Ideally, one would control for ethnicity in all models. Unfortunately, this is not possible because Rwandan law since the genocide prohibits the collecting of 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). 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) and using as controls the same set of covariates as in our main specification. 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 A7 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 $SIBLING_{ict}$ or $\Delta Sex\ ratio_{c2002-1991}$, we expect both measures to be negatively associated with fertility and positively associated with the Tutsi indicator (as discussed above, the large majority of people killed during the genocide were Tutsi). It follows that, if anything, the estimates obtained when using $SIBLING_{ict}$ or

$\Delta Sex\ ratio_{c1991-2002}$ are likely to be downward biased. 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 $CHILD_{ict}$, 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 $CHILD_{ict}$ or the negative effect of being Tutsi. Yet, by reading our main results for this mechanism (Tables 2 and 5) together with those on the effect of being Tutsi on fertility, we can derive a clear conclusion. Because 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 different fertility propensity 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. By using both event history and count data models, we investigate the effects of exposure to violence on both the timing of the first birth and the total number of births in the post-genocide period.

The paper has three main findings. First, there is strong evidence of a replacement effect. Having experienced the death of a child reduces the time to the first child after the genocide and increases the total number of births in the post-genocide period. Second, experiencing a sibling death during the genocide significantly lowers post-conflict fertility in the long run (2005-2010) and the effect is stronger if a woman loses a younger sister. This result suggests that a psychological mechanism is at work in this case. Finally, the genocide-induced reduction in the local sex ratio has a strong negative impact on fertility, both in terms of the timing of the first birth and total number of births after the genocide.

Taken together, these results suggest that both the type of violence experienced and the genocide-induced changes in the demographic conditions matter for fertility outcomes. Our analysis also highlights differential effects of the genocide in terms of age cohorts, parity, and time horizon (short-, medium-, and long-run). 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. Our paper is a contribution in this direction.

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Tables

Table 1: Summary statistics

	Observations	Mean	Std. dev.	Min	Max
Dependent variables					
No. of months to the first child birth between June 1995 and May 2000	25,770	22.853	16.090	1	60
No. of children born after June 1995	25,770	1.832	1.728	0	10
No. of children born between June 1995 and May 2000 (short run)	25,770	0.870	0.979	0	6
No. of children born between June 2000 and May 2005 (medium run)	25,770	0.662	0.905	0	5
No. 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 ($CHILD_{ict}$)	25,770	0.030	0.170	0	1
Exposure to sibling death during genocide ($SIBLING_{ict}$)	25,770	0.278	0.448	0	1
Genocide-induced change in commune-level sex ratio (ΔSex $ratio_{c1991-2002}$)	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 30-49 during genocide	25,770	0.211	0.363	0	1
Currently in union (married or in informal relationship)	25,770	0.605	0.489	0	1
Had more than one union	25,770	0.119	0.323	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
No. of children ever lost except deaths during genocide	25,770	0.613	1.086	0	10
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 preceding the survey	25,770	0.131	0.075	0.012	0.447
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: Event history analysis of post-genocide fertility with child death as conflict proxy

	Dependent variable: No. of months to the first child birth between June 1995 and May 2000							
	All	All	Aged 10-19	Aged 20-29	Aged 30-49	Parity 1	Parity 2	Parity 3+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Child death during genocide ($CHILD_{ict}$)	0.28 (6.58)***	0.31 (7.17)***	-0.18 (-0.85)	0.36 (5.67)***	0.26 (4.05)***	0.47 (3.53)***	0.34 (2.97)***	0.26 (5.06)***
Age	0.55 (54.66)***	0.47 (45.74)***	0.51 (11.17)***	0.06 (1.65)*	0.11 (1.02)	0.15 (3.59)***	0.21 (3.71)***	0.13 (3.44)***
Age squared	-0.01 (-49.64)***	-0.01 (-38.84)***	-0.00 (-2.58)***	-0.00 (-1.62)	-0.00 (-2.14)**	-0.00 (-4.34)***	-0.00 (-4.23)***	-0.00 (-4.72)***
Currently in union		0.77 (35.79)***	0.99 (21.72)***	0.65 (20.45)***	0.71 (17.68)***	0.65 (11.21)***	0.56 (8.59)***	0.64 (18.43)***
Had more than one union		0.04 (1.45)	0.36 (6.77)***	-0.13 (-3.81)***	-0.11 (-2.42)**	-0.27 (-4.71)***	-0.36 (-5.43)***	-0.15 (-3.79)***
Primary education		0.03 (1.39)	-0.11 (-2.65)***	0.06 (1.88)*	0.05 (1.21)	0.02 (0.40)	0.02 (0.38)	0.04 (1.30)
Secondary or higher education		0.00 (0.05)	-0.28 (-4.00)***	-0.01 (-0.22)	-0.05 (-0.51)	0.03 (0.35)	-0.07 (-0.68)	-0.18 (-2.25)**
No. of children born before June 1995		-0.12 (-17.62)***	-0.01 (-0.25)	-0.02 (-1.37)	0.06 (5.35)***			
No. of children ever lost except genocide deaths		0.19 (22.84)***	0.44 (19.87)***	0.16 (12.18)***	0.07 (5.41)***	0.28 (9.97)***	0.22 (7.98)***	0.08 (6.71)***
Household wealth index		-0.05 (-6.74)***	-0.08 (-5.63)***	-0.03 (-2.84)***	-0.04 (-2.16)**	-0.01 (-0.36)	-0.02 (-0.82)	-0.02 (-1.34)
Place of residence is urban		0.08 (1.91)*	0.18 (2.25)**	0.01 (0.15)	-0.02 (-0.19)	-0.02 (-0.16)	-0.10 (-0.81)	0.01 (0.20)
Under-five mortality		-0.35 (-1.87)*	-0.66 (-1.72)*	-0.24 (-0.83)	-0.09 (-0.23)	-0.35 (-0.65)	0.08 (0.13)	0.10 (0.32)
Commune fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,770	25,770	12,582	7,763	5,425	2,443	1,948	6,741

Note: Displayed are coefficients obtained from Cox regressions and robust t-statistics, clustered at the PSU level, in brackets with * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The duration time measures the number of months between June 1995 and the first child birth after the genocide. 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 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

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

	Dependent variable: No. of months to the first child birth between June 1995 and May 2000								
	All	All	Aged 10-19	Aged 20-29	Aged 30-49	Parity 0	Parity 1	Parity 2	Parity 3+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sibling death during genocide ($SIBLING_{ict}$)	-0.03 (-1.63)	-0.03 (-1.34)	-0.06 (-1.53)	0.00 (0.04)	-0.01 (-0.18)	0.00 (0.02)	-0.06 (-1.13)	-0.05 (-0.91)	-0.02 (-0.54)
Age	0.55 (54.80)***	0.48 (45.89)***	0.51 (11.13)***	0.06 (1.65)*	0.12 (1.04)	0.61 (29.33)***	0.15 (3.49)***	0.20 (3.59)***	0.12 (3.25)***
Age squared	-0.01 (-49.67)***	-0.01 (-39.00)***	-0.00 (-2.52)**	-0.00 (-1.64)	-0.00 (-2.16)**	-0.01 (-23.35)***	-0.00 (-4.25)***	-0.00 (-4.13)***	-0.00 (-4.55)***
Currently in union		0.76 (35.61)***	0.98 (21.71)***	0.64 (20.22)***	0.70 (17.59)***	1.01 (26.20)***	0.63 (11.04)***	0.56 (8.57)***	0.63 (18.22)***
Had more than one union		0.04 (1.69)*	0.36 (6.72)***	-0.12 (-3.62)***	-0.11 (-2.32)**	0.40 (8.46)***	-0.26 (-4.48)***	-0.35 (-5.35)***	-0.14 (-3.64)***
Primary education		0.03 (1.40)	-0.11 (-2.67)***	0.05 (1.69)*	0.05 (1.26)	0.07 (1.78)*	0.02 (0.41)	0.01 (0.14)	0.04 (1.31)
Secondary or higher education		0.00 (0.00)	-0.28 (-4.02)***	-0.02 (-0.40)	-0.05 (-0.50)	0.11 (1.96)**	0.03 (0.31)	-0.09 (-0.80)	-0.18 (-2.27)**
No. of children born before June 1995		-0.11 (-17.07)***	-0.02 (-0.51)	-0.01 (-0.84)	0.06 (5.62)***				
No. of children ever lost except genocide deaths		0.20 (23.10)***	0.44 (19.90)***	0.16 (12.26)***	0.07 (5.56)***	0.53 (27.57)***	0.28 (9.86)***	0.22 (8.02)***	0.08 (6.94)***
Household wealth index		-0.06 (-6.87)***	-0.08 (-5.63)***	-0.03 (-2.85)***	-0.04 (-2.29)**	-0.08 (-6.61)***	-0.01 (-0.38)	-0.02 (-0.90)	-0.02 (-1.45)
Place of residence is urban		0.08 (2.00)**	0.18 (2.26)**	0.01 (0.16)	-0.01 (-0.11)	0.11 (1.58)	-0.03 (-0.27)	-0.10 (-0.78)	0.02 (0.27)
Under-five mortality		-0.36 (-1.91)*	-0.67 (-1.75)*	-0.24 (-0.84)	-0.10 (-0.27)	-0.60 (-1.84)*	-0.30 (-0.57)	0.08 (0.13)	0.07 (0.23)
Commune fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,770	25,770	12,582	7,763	5,425	14,638	2,443	1,948	6,741

Note: Displayed are coefficients obtained from Cox regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The duration time measures the number of months between June 1995 and the first child birth after the genocide. 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 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

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

	Dependent variable: No. of months to the first child birth between June 1995 and May 2000								
	All	All	Aged 10-19	Aged 20-29	Aged 30-49	Parity 0	Parity 1	Parity 2	Parity 3+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in sex ratio (ΔSex ratio$_{c1991\text{-}2002}$)	-0.57 (-2.75)***	-0.77 (-3.63)***	-0.54 (-1.36)	-0.54 (-1.74)*	-0.89 (-2.05)**	-0.47 (-1.39)	-0.20 (-0.36)	-1.18 (-1.90)*	-0.46 (-1.26)
Sex ratio before genocide	0.47 (2.28)**	0.71 (3.28)***	0.79 (1.93)*	0.22 (0.71)	0.46 (1.02)	0.66 (1.85)*	0.43 (0.76)	0.48 (0.75)	0.17 (0.47)
Age	0.54 (54.66)***	0.54 (53.91)***	0.71 (16.53)***	0.09 (2.53)**	0.07 (0.67)	0.73 (36.55)***	0.15 (3.81)***	0.15 (2.82)***	0.13 (3.40)***
Age squared	-0.01 (-49.56)***	-0.01 (-47.05)***	-0.01 (-7.17)***	-0.00 (-2.82)***	-0.00 (-1.88)*	-0.01 (-29.94)***	-0.00 (-4.77)***	-0.00 (-3.53)***	-0.00 (-4.95)***
Primary education		0.04 (1.81)*	-0.15 (-3.58)***	0.07 (2.19)**	0.07 (1.84)*	0.05 (1.30)	0.04 (0.70)	0.03 (0.46)	0.05 (1.57)
Secondary or higher education		-0.03 (-0.88)	-0.36 (-5.31)***	-0.02 (-0.33)	-0.11 (-1.26)	0.05 (0.90)	0.06 (0.66)	-0.06 (-0.63)	-0.21 (-2.69)***
No. of children born before June 1995		-0.10 (-15.00)***	0.02 (0.45)	0.00 (0.29)	0.07 (7.39)***				
No. of children ever lost except genocide deaths		0.21 (24.91)***	0.49 (23.12)***	0.18 (13.51)***	0.07 (5.46)***	0.60 (33.39)***	0.29 (10.56)***	0.20 (7.71)***	0.08 (6.85)***
Household wealth index		-0.03 (-4.29)***	-0.09 (-6.25)***	-0.01 (-0.63)	0.00 (0.15)	-0.07 (-6.04)***	0.00 (0.07)	0.02 (0.91)	0.01 (0.86)
Place of residence is urban		-0.04 (-1.16)	0.11 (1.91)*	-0.11 (-2.34)**	-0.15 (-2.35)**	0.02 (0.44)	-0.19 (-2.40)**	-0.17 (-1.81)*	-0.12 (-2.14)**
Under-five mortality		-0.15 (-1.00)	-0.18 (-0.61)	-0.34 (-1.51)	-0.03 (-0.10)	-0.38 (-1.50)	-0.45 (-1.13)	0.04 (0.09)	-0.06 (-0.26)
Préfecture fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,770	25,770	12,582	7,763	5,425	14,638	2,443	1,948	6,741

Note: Displayed are coefficients obtained from Cox regressions and robust t-statistics, clustered at the PSU level, in brackets with * p<0.1, ** p<0.05, *** p<0.01. The duration time measures the number of months between June 1995 and the first child birth after the genocide. 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 10-49 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

	Dependent variable: No. of children born after June 1995										
	All	All	Aged 10-19	Aged 20-29	Aged 30-49	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 ($CHILD_{ict}$)	0.11 (2.73)***	0.13 (3.74)***	0.05 (0.55)	0.21 (3.32)***	0.06 (1.31)	0.22 (1.97)**	0.42 (4.48)***	0.06 (1.37)	0.12 (5.49)***	0.01 (0.19)	-0.12 (-1.50)
Age	0.45 (50.37)***	0.28 (34.63)***	0.41 (22.37)***	-0.05 (-1.27)	0.17 (2.04)**	-0.05 (-1.05)	0.08 (1.36)	0.12 (3.73)***	0.17 (28.90)***	0.10 (9.73)***	0.09 (4.98)***
Age squared	-0.01 (-56.11)***	-0.00 (-38.01)***	-0.01 (-20.22)***	-0.00 (-2.28)**	-0.00 (-4.05)***	-0.00 (-2.48)**	-0.00 (-4.32)***	-0.00 (-8.04)***	-0.00 (-27.30)***	-0.00 (-13.13)***	-0.00 (-8.89)***
Currently in union		1.17 (58.19)***	0.88 (38.13)***	1.50 (33.35)***	0.80 (28.07)***	1.39 (20.07)***	1.18 (15.49)***	0.93 (30.70)***	0.50 (35.64)***	0.89 (44.19)***	0.81 (30.54)***
Had more than one union		-0.01 (-0.41)	0.06 (2.92)***	-0.18 (-4.83)***	-0.04 (-1.07)	-0.24 (-4.14)***	-0.18 (-2.80)***	-0.09 (-2.72)***	-0.02 (-1.18)	0.01 (0.53)	0.04 (1.59)
Primary education		0.02 (1.08)	-0.03 (-1.89)*	0.10 (3.08)***	0.01 (0.24)	-0.04 (-0.75)	0.12 (2.16)**	0.04 (1.82)*	0.02 (2.16)**	-0.01 (-0.33)	-0.00 (-0.18)
Secondary or higher education		-0.11 (-3.91)***	-0.18 (-6.97)***	0.01 (0.11)	-0.11 (-1.45)	-0.14 (-1.40)	0.02 (0.15)	-0.08 (-1.14)	-0.00 (-0.16)	-0.13 (-4.21)***	-0.11 (-3.25)***
No. of children born before June 1995		-0.11 (-19.55)***	-0.06 (-3.14)***	-0.03 (-2.62)***	0.02 (2.73)***				-0.09 (-23.25)***	-0.09 (-14.17)***	-0.04 (-3.04)***
No. of children ever lost except genocide deaths		0.23 (38.78)***	0.23 (24.06)***	0.29 (25.43)***	0.10 (11.10)***	0.38 (15.94)***	0.29 (12.15)***	0.12 (14.07)***	0.14 (30.68)***	0.15 (23.32)***	0.12 (14.19)***
Household wealth index		-0.04 (-5.83)***	-0.03 (-5.74)***	-0.02 (-1.42)	-0.03 (-1.75)*	0.02 (0.87)	-0.06 (-1.72)*	-0.02 (-1.52)	-0.01 (-2.45)**	-0.03 (-3.86)***	-0.04 (-4.27)***
Place of residence is urban		0.02 (0.73)	0.01 (0.30)	0.03 (0.55)	-0.04 (-0.76)	0.05 (0.43)	-0.11 (-0.95)	0.03 (0.51)	0.04 (1.99)**	0.01 (0.27)	-0.10 (-2.37)**
Under-five mortality		-0.45 (-3.11)***	-0.27 (-1.85)*	-0.47 (-1.49)	-0.45 (-1.81)*	0.12 (0.25)	-0.31 (-0.59)	-0.26 (-1.04)	-0.28 (-2.84)***	-0.35 (-1.96)*	-4.28 (-5.43)***
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	25,770	12,582	7,763	5,425	2,443	1,948	6,741	25,770	15,763	7,438

Note: 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 1-8), 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 10-49 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

	Dependent variable: No. of children born after June 1995											
	All	All	Aged 10-19	Aged 20-29	Aged 30-49	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 ($SIBLING_{ict}$)	-0.06	-0.03	-0.01	-0.05	-0.04	-0.01	-0.11	-0.12	-0.04	-0.02	-0.02	-0.05
	(-3.14)***	(-2.42)**	(-0.65)	(-1.63)	(-1.56)	(-0.67)	(-1.90)*	(-1.99)**	(-1.61)	(-1.61)	(-0.98)	(-2.65)***
Age	0.45	0.28	0.41	-0.05	0.17	0.29	-0.05	0.08	0.12	0.17	0.10	0.09
	(50.48)***	(34.73)***	(22.34)***	(-1.25)	(2.06)**	(22.19)***	(-1.02)	(1.31)	(3.76)***	(28.97)***	(9.74)***	(4.99)***
Age squared	-0.01	-0.00	-0.01	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(-56.11)***	(-38.08)***	(-20.19)***	(-2.29)**	(-4.07)***	(-22.05)***	(-2.49)**	(-4.25)***	(-8.08)***	(-27.38)***	(-13.14)***	(-8.89)***
Currently in union		1.17	0.88	1.49	0.80	1.05	1.39	1.18	0.93	0.50	0.89	0.81
		(58.16)***	(38.13)***	(33.20)***	(28.07)***	(43.58)***	(19.94)***	(15.48)***	(30.62)***	(35.56)***	(44.18)***	(30.49)***
Had more than one union		-0.00	0.06	-0.17	-0.03	0.05	-0.24	-0.17	-0.09	-0.01	0.01	0.04
		(-0.24)	(2.93)***	(-4.68)***	(-0.99)	(2.30)**	(-4.01)***	(-2.72)***	(-2.66)***	(-0.96)	(0.57)	(1.66)*
Primary education		0.02	-0.03	0.10	0.01	0.01	-0.04	0.10	0.05	0.02	-0.00	-0.00
		(1.13)	(-1.88)*	(3.09)***	(0.33)	(0.45)	(-0.68)	(1.87)*	(1.90)*	(2.17)**	(-0.30)	(-0.09)
Secondary or higher education		-0.11	-0.18	0.01	-0.10	-0.09	-0.13	0.00	-0.08	-0.00	-0.13	-0.11
		(-3.86)***	(-6.97)***	(0.11)	(-1.40)	(-3.26)***	(-1.32)	(0.04)	(-1.11)	(-0.14)	(-4.18)***	(-3.08)***
No. of children born before June 1995		-0.11	-0.06	-0.03	0.02					-0.09	-0.09	-0.04
		(-19.34)***	(-3.13)***	(-2.34)**	(2.82)***					(-22.94)***	(-14.32)***	(-3.23)***
No. of children ever lost except genocide deaths		0.23	0.23	0.29	0.10	0.29	0.37	0.29	0.12	0.14	0.15	0.12
		(38.86)***	(24.04)***	(25.29)***	(11.23)***	(28.14)***	(15.96)***	(12.16)***	(14.17)***	(30.87)***	(23.32)***	(14.17)***
Household wealth index		-0.04	-0.03	-0.02	-0.03	-0.03	0.02	-0.06	-0.02	-0.01	-0.03	-0.04
		(-5.80)***	(-5.74)***	(-1.35)	(-1.72)*	(-5.42)***	(0.92)	(-1.73)*	(-1.48)	(-2.46)**	(-3.84)***	(-4.27)***
Place of residence is urban		0.02	0.01	0.03	-0.05	-0.02	0.04	-0.12	0.03	0.04	0.01	-0.09
		(0.70)	(0.29)	(0.47)	(-0.78)	(-0.73)	(0.37)	(-1.00)	(0.48)	(1.98)**	(0.27)	(-2.31)**
Under-five mortality		-0.46	-0.27	-0.47	-0.45	-0.44	0.13	-0.36	-0.26	-0.28	-0.35	-4.47
		(-3.12)***	(-1.84)*	(-1.50)	(-1.82)*	(-2.97)***	(0.27)	(-0.66)	(-1.07)	(-2.85)***	(-1.97)**	(-5.74)***
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	25,770	12,582	7,763	5,425	14,638	2,443	1,948	6,741	25,770	15,763	7,438

Note: 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 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

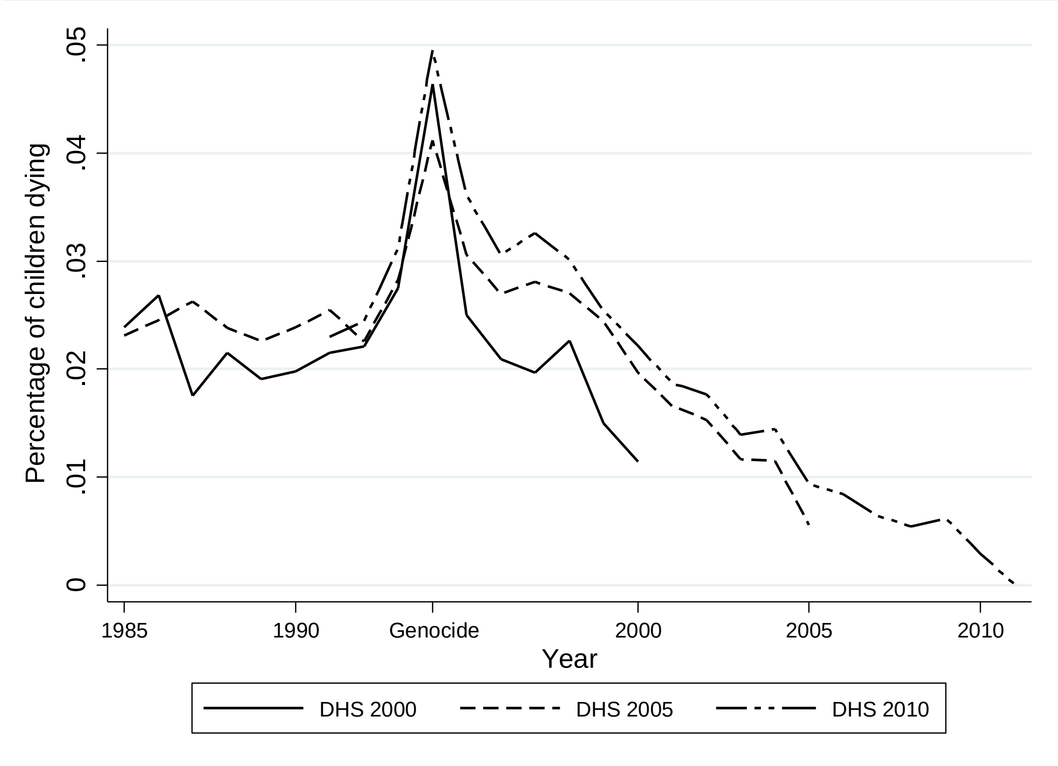
Table 7: Count data analysis of post-genocide fertility with change in the commune-level sex ratio as conflict proxy

	Dependent variable: No. of children born after June 1995											
	All	All	Aged 10-19	Aged 20-29	Aged 30-49	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 (ΔSex ratioc1991-2002)	-0.24 (-1.01)	-0.42 (-1.95)*	-0.15 (-0.67)	-0.27 (-0.59)	-0.97 (-2.54)**	-0.22 (-0.95)	0.41 (0.54)	-1.17 (-1.59)	-0.71 (-1.94)*	-0.38 (-2.66)***	-0.09 (-0.42)	-0.36 (-1.36)
Sex ratio before genocide	0.55 (2.34)**	0.66 (3.15)***	0.56 (2.70)***	0.25 (0.56)	0.53 (1.40)	0.63 (2.76)***	0.43 (0.60)	0.91 (1.25)	0.28 (0.82)	0.40 (2.97)***	0.36 (1.72)*	0.70 (2.60)***
Age	0.45 (50.59)***	0.42 (48.60)***	0.67 (35.95)***	-0.01 (-0.18)	0.16 (1.69)*	0.49 (34.98)***	-0.00 (-0.01)	0.05 (0.74)	0.14 (3.80)***	0.23 (40.58)***	0.19 (16.21)***	0.15 (7.08)***
Age squared	-0.01 (-56.14)***	-0.01 (-52.21)***	-0.01 (-32.06)***	-0.00 (-3.44)***	-0.00 (-3.72)***	-0.01 (-33.55)***	-0.00 (-3.48)***	-0.00 (-3.71)***	-0.00 (-8.04)***	-0.00 (-38.38)***	-0.00 (-19.74)***	-0.00 (-10.87)***
Primary education		0.03 (2.02)**	-0.04 (-2.41)**	0.13 (3.63)***	0.02 (0.67)	0.01 (0.38)	0.03 (0.52)	0.12 (1.96)**	0.05 (1.65)*	0.03 (2.45)**	0.00 (0.23)	0.02 (0.90)
Secondary or higher education		-0.17 (-5.10)***	-0.27 (-8.25)***	-0.01 (-0.11)	-0.20 (-2.35)**	-0.15 (-4.51)***	-0.10 (-0.90)	0.01 (0.08)	-0.17 (-2.03)**	-0.03 (-1.26)	-0.18 (-5.34)***	-0.13 (-3.03)***
Household wealth index		-0.00 (-0.18)	-0.03 (-4.13)***	0.05 (3.23)***	0.03 (2.30)**	-0.02 (-2.86)***	0.08 (2.71)***	0.02 (0.72)	0.04 (2.49)**	0.01 (1.08)	-0.00 (-0.35)	-0.02 (-2.03)**
Place of residence is urban		-0.09 (-2.90)***	-0.01 (-0.29)	-0.20 (-3.19)***	-0.19 (-3.17)***	-0.05 (-1.63)	-0.21 (-2.10)**	-0.32 (-2.90)***	-0.15 (-2.78)***	-0.01 (-0.56)	-0.06 (-1.96)*	-0.18 (-4.25)***
No. of children born before June 1995		-0.11 (-16.31)***	-0.06 (-2.80)***	-0.02 (-1.32)	0.04 (4.85)***					-0.09 (-20.69)***	-0.10 (-12.42)***	-0.05 (-3.44)***
No. of children ever lost except genocide deaths		0.28 (43.52)***	0.30 (26.10)***	0.35 (28.27)***	0.11 (11.88)***	0.39 (29.56)***	0.45 (18.45)***	0.32 (14.24)***	0.13 (14.53)***	0.16 (33.55)***	0.19 (28.28)***	0.16 (16.49)***
Under-five mortality		-0.19 (-1.15)	0.00 (0.02)	-0.21 (-0.64)	-0.28 (-1.11)	-0.17 (-0.96)	0.12 (0.27)	0.40 (0.75)	-0.23 (-0.86)	-0.16 (-1.48)	-0.08 (-0.46)	0.15 (0.46)
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	25,770	12,582	7,763	5,425	14,638	2,443	1,948	6,741	25,770	15,763	7,438

Note: 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 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

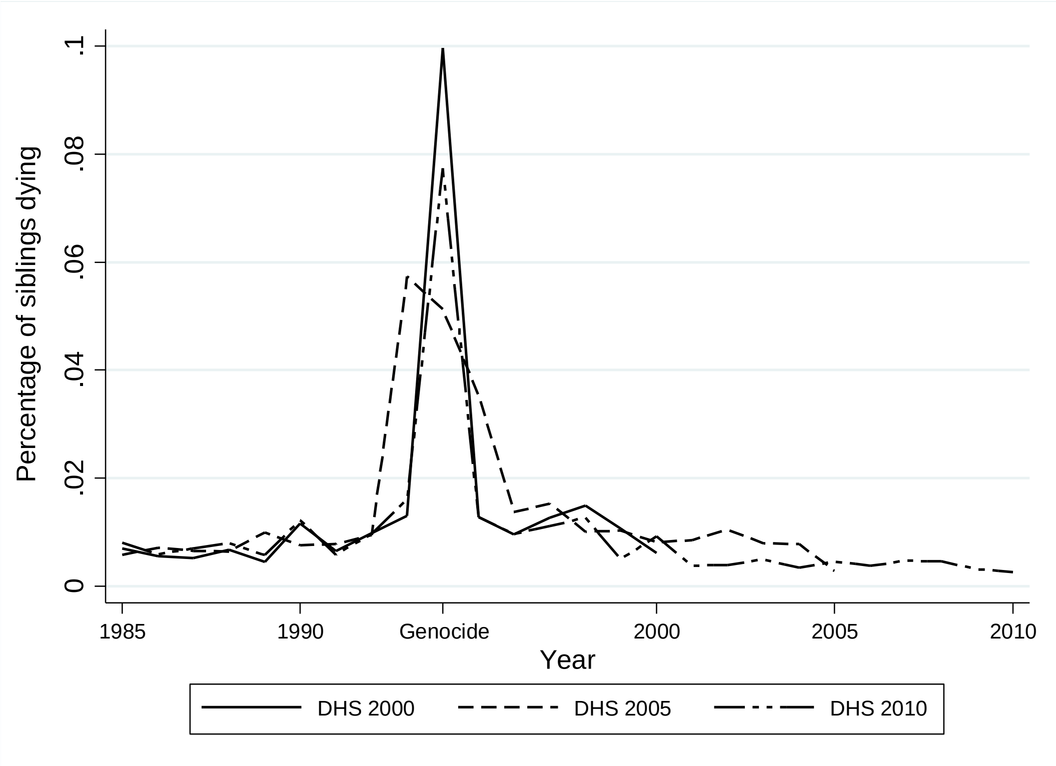
Figures

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



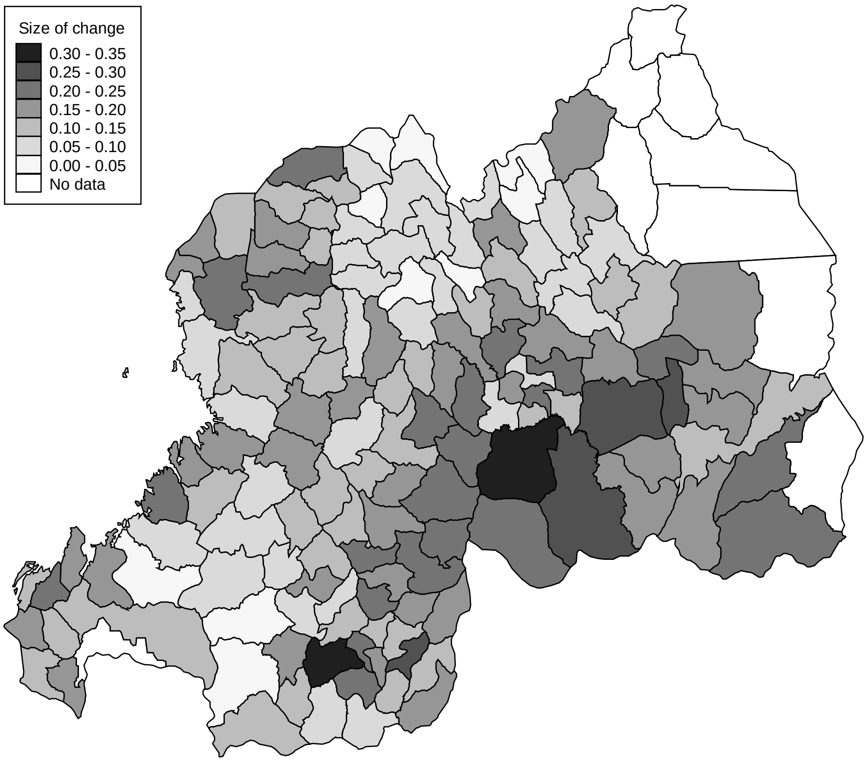
Note: The figure shows the percentage of child deaths relative to the total number of living children reported by sample women for each year during the 1985-2010 period. Data source: DHS 2000, 2005, and 2010.

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



Note: Only communes included in the DHS 2000, 2005, and 2010 are shown. Data source: Census 1991 and 2002.

Supplementary Online Appendix

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

	Dependent variable: No. of months to the first child birth between June 1995 and May 2000							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Son death during genocide	0.25 (4.31)***							
Daughter death during genocide		0.34 (5.57)***						
Brother death during genocide			-0.02 (-1.12)					
Older brother death during genocide				0.05 (2.04)**				
Younger brother death during genocide					-0.07 (-2.53)**			
Sister death during genocide						-0.03 (-1.36)		
Older sister death during genocide							-0.02 (-0.48)	
Younger sister death during genocide								-0.07 (-2.12)**
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	25,770	25,770	25,770	25,770	25,770	25,770	25,770	25,770

Note: Displayed are coefficients obtained from Cox regressions 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. Sample: women aged 10-49 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

	Dependent variable: No. of children born after June 1995							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Son death during genocide	0.16 (3.41)***							
Daughter death during genocide		0.10 (1.92)*						
Brother death during genocide			-0.03 (-1.62)					
Older brother death during genocide				0.01 (0.42)				
Younger brother death during genocide					-0.03 (-1.44)			
Sister death during genocide						-0.04 (-2.32)**		
Older sister death during genocide							-0.03 (-1.27)	
Younger sister death during genocide								-0.05 (-1.97)**
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	25,770	25,770	25,770	25,770	25,770	25,770	25,770	25,770

Note: 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. Sample: women aged 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A3: Event history analysis of post-genocide fertility with time trends

	Dependent variable: No. of months to the first child birth between June 1995 and May 2000		
	(1)	(2)	(3)
Child death during genocide ($CHILD_{ict}$)	0.31 (7.07)***		
Sibling death during genocide ($SIBLING_{ict}$)		-0.03 (-1.33)	
Change in commune-level sex ratio (ΔSex $ratio_{c1991\text{-}2002}$)			-0.78 (-3.65)***
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	25,770	25,770	25,770

Note: Displayed are coefficients obtained from Cox regressions 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. Sample: women aged 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A4: Count data analysis of post-genocide fertility with time trends

	Dependent variable: No. of children born after June 1995		
	(1)	(2)	(3)
Child death during genocide ($CHILD_{ict}$)	0.13 (3.81)***		
Sibling death during genocide ($SIBLING_{ict}$)		-0.03 (-2.36)**	
Change in commune-level sex ratio ($\Delta Sex\ ratio_{c1991\text{-}2002}$)			-0.45 (-2.09)**
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	25,770	25,770	25,770

Note: 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. Sample: women aged 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A5: Count data analysis of post-genocide fertility with placebo child/sibling death

	Dependent variable: No. of children born after June 1995	
	(1)	(2)
Child death during genocide	0.15 (4.31)***	
Child death during 1990-1993	0.07 (2.96)***	
Sibling death during genocide		-0.03 (-2.28)**
Sibling death during 1990-1993		0.00 (0.02)
All controls except "No. of children ever lost except those during genocide"	Yes	Yes
Commune fixed effects	Yes	Yes
Wave fixed effects	Yes	Yes
Sample	All waves	All waves
Observations	25,770	25,770

Note: 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. Sample: women aged 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A6: Count data analysis of post-genocide fertility, exploring recall bias

	Dependent variable: No. of children born after June 1995			
	(1)	(2)	(3)	(4)
Child death during genocide ($CHILD_{ict}$)	0.12 (5.49)***	0.06 (2.33)**	0.10 (3.18)***	0.09 (2.77)***
All controls	Yes	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes
Sample	All waves	2000 only	2005 only	2010 only
Observations	25,770	10,007	8,325	7,438

Note: 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. Sample: women aged 10-49 during the genocide. Data source: DHS 2000, 2005, and 2010.

Table A7: Count data analysis of post-genocide fertility, exploring differences across ethnic groups

	Dependent variable: No. of children born in the period…		
	1987-1992	1982-1992	1977-1992
	(1)	(2)	(3)
Tutsi	0.00 (0.02)	-0.07 (-1.70)*	-0.12 (-2.62)***
All controls	Yes	Yes	Yes
Commune fixed effects	Yes	Yes	Yes
Sample	1992	1992	1992
Observations	6,508	6,508	6,508

Note: 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. Data source: DHS 1992.