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The Effects of Conflict on Fertility in Rwanda

Kati Schindler[a]* and Tilman Brück[b]

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Abstract

The aim of this paper is to study the short and long-term fertility effects of mass violent conflict on different population sub-groups. The authors pool three nationally representative demographic and health surveys from before and after the genocide in Rwanda, identifying conflict exposure of the survivors in multiple ways. The analysis finds a robust effect of genocide on fertility, with a strong replacement effect for lost children. Having lost siblings reduces fertility only in the short term. Most interesting is the continued importance of the institution of marriage in determining fertility and in reducing fertility for the large group of widows in Rwanda.

Keywords: conflict, demography, fertility, gender, genocide, Rwanda

JEL codes: J13, O12

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1 Introduction

The aim of this paper is to analyze if, and in what way, mass violent conflict affects fertility. It is well known that shocks can have significant effects on fertility, with the nature of the shock driving the nature of the impact. In the case of mass violent conflicts, very little is known about their effects on fertility. Hence we aim to understand the effects of off-spring and sibling mortality, widowhood and sex imbalance on short- and long-term changes of fertility in the post-conflict period. This paper – a product of the Gender and Development Unit, Poverty Reduction and Economic Management Network – is part of a larger effort in the department to study gender differentials in response to conflict, with generous funding from the Government of Norway.

Our analysis contributes to two literatures. First, it teaches us how conflict affects individual human behavior and well-being, which is a small but growing area of research. Second, we learn more about the drivers of fertility in times of sudden structural change. The occurrence of mass violent conflict can be seen, from a methodological perspective, as an opportunity to analyze fertility responses to aggregate and individual shocks, and to understand fertility as a coping strategy. Given the importance of relative population shares of various ethnicities in conflict-prone societies and given the importance of fertility for individual and household well-being in very poor economies, understanding the relationship between conflict and fertility is also important from a policy perspective.

We study the specific case of the 1994 Rwandan genocide, using three waves of Rwanda Demographic and Health Surveys (RDHS) collected in 1992, 2000 and 2005. Our research strategy includes cross-sectional analyses of the post-genocide data as well as analyses of pooled data from before and after the genocide. This allows us to compare the determinants of fertility for sub-samples of women of the same age groups across the survey years. The most challenging identification of an effect of conflict is at the micro-level – especially if the survey does not ask about an individual's exposure to conflict explicitly (Brück et al. 2010). Two alternative categories of proxies for conflict exposure are employed to distinguish women who were and were not likely to be affected directly by violence. Each category represents a transmission mechanism of conflict: (1) replacement effects and (2) marriage market effects. We interpret the coefficients for these variables as the 'pure' conflict effects on fertility, bearing in mind that conflict may also shape fertility indirectly through the other variables included in the regressions.

We find very clear and robust effects of conflict on fertility. Three effects are particularly noteworthy. First, we can identify a strong replacement effect of conflict on fertility. This holds both for the short- and the long-term post-war period. Second, we find that sibling mortality, which we consider as a strong indicator of direct exposure to conflict, has a noticeable impact in the short-term only, perhaps associated with the stigma of being part of a genocide victim household. Third, we observe a clear impact of conflict on marital status and the marriage market and hence on fertility. The persistence of the institution of marriage in determining fertility is perhaps the most surprising finding of this study.

The paper advances the literature in several ways. By defining and contrasting several conflict exposure measures, our analysis disentangles the impact of conflict into the direct individual effects of conflict and cohort-specific excess mortality rates that operate through the marriage market. By pooling multiple, very detailed and large-N cross-sectional surveys we succeed in studying very differentiated impacts of mass violent conflict on fertility across time, cohorts and sub-groups. Population pressures on land and relative changes in ethnic population shares may have contributed to the genocide in Rwanda in 1994. Fertility, if not reduced sustainably, continues to be a potential driver of future tensions in Rwanda.

The paper proceeds as follows. Section 2 reviews the current knowledge on the links between violent conflict and fertility, followed by a section introducing the case study and summarizing the events of 1994 in Rwanda. The data used and the conflict proxies are discussed in Section 4 while Section 5 outlines the estimation approach. The next section presents and discusses descriptive and multivariate results. Section 7 concludes.

2 Review of previous research and hypotheses

Fertility in developing countries is shown to be determined by the socio-economic status of each woman and her partner (age, ethnicity, religion, education and employment opportunities, although the latter two may be endogenous to fertility), partnership characteristics (civil status, marital duration, number of previous unions), knowledge of and access to contraception, local characteristics (including health infrastructure and child mortality), and institutional arrangements (such as social support mechanisms).[1] Fertility and family planning are stochastic processes in themselves; this is also true of many of the drivers of fertility. There is a growing literature studying the effects of uncertainty and shocks on fertility.[2] A small number of studies investigate the determinants of fertility in (post-)conflict economies (for instance, Avogo and Agadjanian 2008; Henry 1966; Hynes et al. 2002; Khlat et al. 1997; Woldemicael 2008). A review of research in this field is provided by Hill (2004).

For example, Lindstrom and Berhanu (1999) study the specific impacts of conflict on fertility in Ethiopia. They find a sharp temporary decline in fertility during the early years of violent conflict and famine, followed by a rebound in fertility. During the second decade of conflict, which is paralleled with economic downturn, fertility decreases steadily and both sharp declines and rebounds are less pronounced. Lindstrom and Berhanu suggest that Ethiopian couples postpone births as a strategy to avoid impoverishment in the short term, thereby accepting higher risk in the long term, when fewer children are present to secure their own livelihood at old age. Verwimp and Van Bavel (2005) explore the drivers of fertility among Rwandan refugee women. Again, conflict, and the related processes of displacement and famine are found to affect fertility.

Randall (2005) finds that Tuareg people in Mali maintain constant patterns of fertility and age of marriage in a time period characterized by inter-ethnic conflict and change. Randall's interpretation of this phenomenon is that reproductive behavior is one important feature of cultural identity. By strengthening traditional marriage and fertility patterns, Tuareg people sharpen the boundaries towards other ethnic groups, thereby making themselves 'visible and readily identifiable in case of future conflicts' (Randall 2005: 326), thus perhaps turning around the decline in fertility that may have been observed in the case of peace. In a study of post-war Angola, Agadjanian and Prata (2002) find women living in conflict-affected regions have lower fertility rates during conflict, followed by a baby boom in the same regions once conflict ends. A similar pattern appears in Cambodia (de Walque 2006), where the genocide under the Khmer Rouge caused a severe shortage of eligible men, as in Rwanda, hence reducing fertility.

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[1] Examples are Ainsworth et al. (1996), Angeles et al. (2005), and Benefo and Schultz (1994).

[2] See Clay and Vander Haar (1993) for an early treatment of the issue.

While the cited studies demonstrate that conflict exposure significantly impacts fertility, there are three gaps in the empirical literature. First, despite these existing studies, still little is known about how violent conflict and its legacy affect fertility. In his review of the studies on health issues in post-war countries, Urdal (2010) concludes that most research in this field focuses on the immediate health consequences of conflict, which, in turn, may have indirect consequences on fertility (e.g. Bozzoli and Brück 2008; Bundervoet et al. 2009; Verwimp and van Bavel 2005).

Second, few of the existing studies analyzing fertility in post-war economies pinpoint the channels through which the mechanism works. As Urdal (2010) argues, these channels may vary with the type and duration of a given conflict. Hence there is a need to account for the specific ways in which conflict impacts fertility – ranging from the individual level to regional effects and to demographic issues. At the individual level, the loss of a brother or sister may affect a woman's fertility differently from the loss of a son or a daughter. While all of them are close kin, these persons may have different roles in a woman's livelihood, her old age security, in providing dowry or arranging a marriage. Fertility may be driven by demographic issues such as unbalanced sex ratios. This, in turn, reduces the number of unions or changes its characteristics, such as the age balance between partners, as argued by Shemyakina (2006), irrespective of a woman's immediate exposure of violence. At the regional level, conflict may, for example, destroy health infrastructure or regional employment opportunities.

Third, individuals may experience increased or decreased fertility at different points of time due to conflict. Such differences may emerge along ethnic, regional or cohort-specific lines. On the one hand, fertility can increase in times of conflict as a result of gender-based violence (such as war-rapes), a change in social norms making sex outside of marriage more common, the loss of family planning infrastructures, or lower opportunity costs for female time. At the individual level replacement fertility may be driven by requirements on household composition to perform various agricultural tasks and to secure a livelihood of mothers at old age. At the group level replacement fertility may be influenced by the perceived need to maintain or raise the population share of one's own group. In fact, the demographic balance between the opposing groups is often an implicit but important stake in the conflict (Fargues 2000; Tabeau and Bijak 2005). On the other hand, fertility may decline during conflict because of the debilitating effects of trauma on reproductive health, the break-up of marriages and widowhood, displacement, the separation of spouses, the loss of financial ability to support more children, and the loss of health infrastructure increasing numbers of still-born babies and maternal mortality. It is conceivable that these two effects average out in studies adopting a more aggregate perspective, hence underestimating the scale of the structural changes induced by mass violent conflict.

The reasons for these shortcomings may be related to a lack of suitable data in many cases. Often analyses can only be undertaken at a descriptive level, which prevents controlling for socio-economic factors (e.g. de Walque 2006) or because the different ways conflict impacts individuals (cf. Verwimp et al. 2009) cannot be identified clearly with the available data (Brück, et al. 2010).

We therefore contribute to this body of research in three ways. First, we disaggregate the impact of the Rwandan genocide into two possible channels, which we then test with individual data. Second, we disaggregate impacts for various groups and cohorts, hence distinguishing between what may be 'positive' or 'negative' effects of conflict on fertility. Third, we test the effects of conflict over the short- and the long-term. In contrast with the nascent literature on this theme, we do not predict a uniform trend for lower fertility due to conflict. Rather, we expect 'sub-patterns' to emerge. For example, war widows may have lower fertility while women less well entrenched in traditional culture may have more sexual partners and hence greater fertility.

3 Conflict and demographics in Rwanda

In the pre-colonial era, the terms Hutu and Tutsi mainly depicted different occupational groups – cultivators and pastoralists, respectively – who shared otherwise similar cultural practices and a common language (Newbury 1988; Prunier 1999).[3] Classification into either group depended largely on wealth, most importantly on cows. Accumulating cows and deriving a livelihood from pastoralism permitted people to transcend the categories, which allowed for considerable flexibility. The German colonial administration and later the Belgians (who ruled Rwanda after WWI) favored Tutsi whom they considered as the local

[3] For an analysis of the historical context, see Desforges (1999), Mamdani (2001), Newbury and Newbury (1999), Prunier (1999) and the special issue on Rwanda of ISSUE (1995).

elite. Under the influence of the United Nations, the Belgians increasingly shifted their support to Hutu from the 1950s onwards. Parallel to the cementing of a societal hierarchy in which the Tutsi minority dominated the Hutu majority, the meaning of the terms switched into ethnic categories (Desforges 1999).

Political power changed after the Hutu staged a successful coup and achieved independence from Belgium in 1962, with Grégoire Kayibanda, a Hutu, becoming the first president (Prunier 1999). In the following decades, ethnically motivated violence and political campaigns against Tutsi resulted in waves of Tutsi fleeing Rwanda for neighboring countries. Attempts by exiled Tutsi to regain power in Rwanda posed a constant threat to the Hutu government, which increased tensions between the two groups. The livelihoods of Hutu and Tutsi did not differ in post-independence Rwanda (Desforges 1999). However, Tutsi were discriminated against in the access to education and employment, and discriminatory policies forbade Hutu army members from marrying Tutsi. Also, Tutsi tended to marry later than Hutu (Jayaraman et al. 2009), which will be discussed again in Section 6d.

Up until the late 1980s, the government under President Juvénal Habyarimana (who forcefully took power in 1973) supported a peasant ideology that valued children and large families (Verwimp and van Bavel 2005). This paralleled the strong involvement of the Catholic Church in Rwandan politics and everyday culture, which effectively suppressed the availability of contraceptives. Also, cultural norms attached to a traditional lifestyle and the importance of family and kinship resulted in a low demand for family planning (May et al. 1990). Fertility in Rwandan ranked among the highest in the world, with total fertility rates of 8.5 in 1983 (INSR and ORC Macro 2006: 38). In turn, this aggravated pressure on already scarce farming land in a country with an extremely high population density, thus contributing to social tensions (André and Platteau 1998). Studies conducted in Rwanda in the early 1990s conclude that in the absence of public old-age social security, children contribute significantly to the economic well-being of the parents (Clay and Vander Haar 1993). The type of support differs by the gender of the child: Cash support is mostly given by sons, while daughters contribute more labor and gifts-in-kind than sons. Children continue supporting the parents even after forming independent households by themselves. Moreover, Rwandan households with larger land endowments tend to have higher fertility, as these households can afford better nutrition and health care, which in turn increases the supply of children (Clay and Johnson 1992).

After the genocide, young Rwandan men continue to be faced with notions of masculinity that depict men as providers and breadwinners (Sommers 2006a; 2006b). For instance, a young man in Rwanda can only get married in a culturally accepted manner after building a house and acquiring land to support his family. Given the pressure on land, few employment opportunities, and the loss of senior relatives during the genocide many young men could hardly achieve these prerequisites. Yet despite these challenges, the norms and gender roles may continue to exert significant effects on partnership and fertility.

The violence peaked with the 1994 genocide, when 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 of President Habyarimana. The human suffering during the genocide was enormous. Death toll estimates range between at least 500,000 deaths (Desforges 1999; Prunier 1999) to over a million deaths (African Rights 1995), about 10 percent of the 1994 population. Most of these individuals were Tutsi, killed in one-sided violence, causing the death of an estimated 75 percent of the Tutsi population (Desforges 1999). A smaller number of soldiers died in combat between the FAR and the rebel army, the Rwandan Patriotic Front (RPF), which eventually stopped the genocide and took power. The breakdown of the health care system and displacement also contributed (although to a much lower extent) to excess mortality (de Walque and Verwimp 2010). Sexual violence was widespread during the genocide, leading to a cohort of children conceived through rape (Nowrojee 1996).

Estimates of death tolls are politically sensitive in Rwanda and the available demographic data are scant. Reconciliation policies enacted after 1994 have strictly prohibited the collection of information on ethnicity that would facilitate the reconstruction the demographic impact of the genocide in more detail. The few studies that attempt to differentiate deaths according to gender (e.g. de Walque and Verwimp 2010; Ministry for Local Government 2002) conclude that adult men made up the majority of casualties.[4]

[4] Using population data from the province of Gikongoro, Verpoorten (2005) concludes that Tutsi women had only a moderately higher probability of survival than Tutsi men, with the respective probabilities being 0.29 and 0.21.

In the aftermath of the genocide, sex ratios (the ratio of males to females) became severely unbalanced. Primarily, this is because more men and boys than women and girls died. Further, about 2 million Rwandans, commonly referred to as *new caseload refugees*, escaped to the Democratic Republic of Congo and Tanzania immediately after the genocide in fear of revenge by the RPF and persecution for their crimes. Among the new caseload refugees were perpetrators of the genocide, who were mostly males, and Rwandan civilians. About 600,000 new caseload refugees returned from Congo to Rwanda and another 500,000 refugees returned from Tanzania to Rwanda in late 1996 (World Bank 2003). A smaller portion of the new caseload refugees, mostly former militias, became involved in the Congo wars and were repatriated to Rwanda in the period 1997-2000 (Verwimp and van Bavel 2005). On the other hand, about 700,000 Tutsi people returned to Rwanda from exile in Uganda shortly after the genocide (Newbury 2005). This group of *old caseload refugees* either fled Rwanda during waves of ethnic violence against Tutsi since independence or were the offspring of Rwandan exiles.

Grasping the demographic imbalance in numbers, Fig. 1 depicts sex ratios for five-year age groups calculated from the (pre-genocide) 1991 Census and the (post-genocide) 2002 Census for Rwanda. The graph allows comparing the relative distribution of men and women across age at the two points in time. Clearly, in 2002 there are shortages of men that *may be* attributable to genocide-related excess male deaths (i.e. shortages of men even larger than prior to the genocide for some age groups). The shortage of men is most pronounced in the groups of 20-45 year olds and the elderly older than 55 years. An immediate implication that follows from the unbalanced sex ratios is the reduced chance of women to get married to men of similar age for women in the age group most affected by genocide or to remarry after being divorced or widowed. This is hence a topic that we will investigate in more detail below.

4 Data

4.1 Rwanda Demographic and Health Surveys

The analysis builds on three cross-sectional Rwanda Demographic and Health Surveys (RDHS) collected in 1992 (before the genocide) (ONAPO and Macro International 1994), 2000 (after the genocide) (ONAPO and ORC Macro 2001) and 2005 (INSR and ORC Macro 2006). The data in each survey is representative of households at the national and in 1992 and 2005 at the provincial level, based on a stratified survey design. In the 2005 RDHS, each of Rwanda's twelve provinces was divided into an urban and a rural stratum, resulting in 23 strata (the province of Kigali City only consists of urban areas). In a first stage, primary sampling units were drawn from a listing of enumeration areas prepared for the 2002 Census. Primary sampling units were selected with probability proportional to size regarding the number of households in each enumeration area. This exercise was conducted separately in every stratum. In a second stage, 20 and 24 households within each urban and rural primary sampling unit were drawn, respectively.[5] In the following, all analyses account for the survey design and population weights are used as recommended by the data providers.

In every selected household, all women of age 15-49 years who were either usual household members or who were present in the household on the night before the interview were eligible for interviewing. In half of all selected households, an additional questionnaire was administered to survey all men aged 15-59 years about their health status. The questionnaire design remained broadly similar across the survey waves. Still, both the number of variables and the sample size increased over time with about 6,500, 10,600 and 11,300 prime age women included in the 1992, 2000 and 2005 survey, respectively.

The RDHS include detailed information on women's birth histories (permitting the calculation of a fertility indicator to be used as the dependent variable below), maternal and child health, marital history, access to health services, domestic violence, sibling mortality, and women's socio-economic characteristics, including schooling and main occupation. In contrast to LSMS-type household-surveys, the information captured on the characteristics of other household members and respondents' partners is limited to age, schooling, and occupation (the latter is only available for current partners). Data on community characteristics were not collected. Due to confidentiality policies we only know the province

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[5] This description of the sample design refers to the 2005 RDHS, with slightly different designs used in the two previous RDHS waves. The 1992 RDHS builds on the 1991 Census as a sampling frame. At the time of the 1992 survey collection, a civil war was ongoing, with most actions of warfare taking place along the Ugandan-Rwandan border. Due to security concerns, 44 rural sectors in the provinces of Byumba and Ruhengeri in northern Rwanda were excluded from the sample frame at the outset. The 2000 RDHS builds on the listing of enumeration areas outlined for another household survey, the *Enquête Intégrale sur les Conditions de Vie des Ménages* (EICV) collected in 2000, as no other population records were available at the time. The sampling frame of the EICV itself is based on the pre-genocide Census of 1991. Three strata were used – Kigali, other urban areas, rural areas – and rural areas were further stratified into provinces, resulting in 13 strata (Ministère des Finances et la Planification Economique 2003). The sample design of the 2000 RDHS is only representative of rural areas of each province and Kigali City.

in which a respondent currently resides, but not the administrative unit below the province level. This prevents us from merging the RDHS data with secondary data on geographical conflict intensity.

Moreover, no information on income or consumption expenditure is recorded and households' physical asset endowments, such as owning a radio or the quality of roofing materials, is the only implicit measure available on household wealth. Instead, we construct a wealth index based on recorded household assets. Components of the index include durables, such as radio and bicycle, source of drinking water, characteristics of floor materials, and type of toilet facility. While the 2000 and 2005 surveys record a larger number of assets than the 1992 survey, we construct the asset index based on the same categories of assets captured in every survey wave, ensuring full comparability over time. Most of these assets are recorded as dichotomous variables, taking 0/1 values, while the few categorical variables with multiple categories are manually reorganized along an ordinal scale according to costs. The asset variables are first normalized and then transformed into a single wealth index through principal component analysis, following an approach proposed by Kolenikov and Angeles (2009). Scree plots indicate that the first principal component is highly significant in every wave, while further components carry little information, as desired. The wealth-index is likely to indicate the long-term economic well-being, as many durables captured are typically held by households for many years and are not frequently replaced (Sahn and Stifel 2000).

In between the first and second survey waves an administrative reform took place in which the definition of urban areas was revised (Megill 2004), among other things. Some communities previously considered rural were now coded as towns, which, to some extent, explains the sharp increase in the proportion of urban population. As a consequence, results for urban areas are not comparable across the three RDHS waves in a strict sense.

4.2 Conflict proxies

One obvious predictor of being a genocide victim in 1994 is ethnicity (that is, being Tutsi). Yet, only the 1992 pre-genocide RDHS wave records ethnicity, as self-reported by respondents. In an effort to suppress further ethnic tensions, the post-genocide government of Rwanda forbids the usage and identification of ethnic categories. Hence, RDHS collected after 1994 does not record respondents' ethnicity.

In response to this challenge, we construct several 'conflict proxies' measuring likely exposure to conflict. These proxies allow us to differentiate two channels through which exposure to mass violence may influence fertility: replacement effects (where women choose to have children in the post-conflict period to compensate their lost children from during the conflict period) and marriage market effects (where a relative shortage of men to women creates a 'bottleneck' for women to get married). It is important to note that the conflict proxies do not necessarily identify victims of targeted genocidal violence. Rather, these proxies indicate individuals and age groups that were likely to be exposed to conflict-related violence. Given that the 1994 genocide occurred within a time span of just about 100 days, the duration or timing of conflict exposure is of less importance in the Rwandan genocide. Table 1 provides on overview of the definition and data source of all conflict exposure proxies discussed in the following.

a) Child and sibling mortality

A first proxy that captures replacement effects is whether or not a woman lost a child during the genocide (CHILDDEATH). The RDHS questionnaires record child mortality in great detail and even ask for the month of death. This allows us to precisely code CHILDDEATH to take the value one if a woman lost one or more children between April and July 1994. Moreover, we differentiate child death by gender in two further conflict proxies: SONDEATH indicates the death of at least one son during the genocide; DAUGHTERDEATH the death of at least one daughter. Fig. 2 displays the occurrence of child deaths over time as calculated from the 2000 and 2005 RDHS. Child deaths peak during the 1994 genocide, although child mortality remains relatively high in the immediate post-war period. Given that many mothers would have been killed in the genocide at the same time as their (young) children, this proxy somewhat underestimates the effects of genocide on child mortality. We interpret this proxy as capturing also the negative effects of conflict on health, sanitation and nutrition, leading to (even) higher child mortality.

A second proxy indicating replacement effects uses sibling mortality during the genocide (SIBLINGDEATH). Sibling mortality was recorded in the 2000 and 2005 surveys, but not in the

1992 RDHS. Every prime age woman was asked about all of her siblings born to the same mother. Of every sibling, information is available on the sex, date of birth, whether the sibling is still living, year of death, and whether the death was related to pregnancy or childbirth. This information is recorded irrespective of whether or not the sibling lives in the same household as the respondent and thus provides a good geographical coverage of deaths occurring across the whole country. However, no information is available on the sibling's place of living or the place of death. The occurrence of sibling deaths over time calculated from the 2000 and 2005 RDHS is displayed in Fig. 3. The graphs from both years exhibit one single and outstanding peak which coincides with the timing of the 1994 genocide. The peak of sibling deaths is somewhat less pronounced and spread over a slightly longer time period in the 2005 survey. We suggest that this is due to the imprecise way the time of death was captured in the questionnaire.[6] Accuracy tests on the sibling mortality data collected in the RDHS are discussed in more detail elsewhere.[7] Moreover, we disaggregate sibling deaths by gender, differentiating between women who lost a brother (BROTHERDEATH) and a sister (SISTERDEATH) in 1994. As discussed below in more detail, these variables are likely to capture the direct effects of genocide at the household most accurately. Women still living with their parents who recorded a sibling death during the genocide have a very high probability of having experienced the sibling death very closely. This may shape fertility negatively, for example through the stigma attached to having been part of a conflict victim household.

b) Marital status and sex ratios

The third proxy is a specific form of marital status, namely whether or not a woman is a widow (WIDOW). This proxy is available in both the pre-genocide and post-genocide RDHS. However, the RDHS questionnaire does not record the husband's cause of death or date of death. It is hence impossible to distinguish conflict widows from HIV/AIDS widows or other

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[6] The original question was 'How many years ago did [name of sibling] die?' The 2005 RDHS was collected between February and July 2005, while the genocide occurred between April and July 1994. Hence, a woman interviewed in February 2005 whose sibling died in May 1994 lost her sibling ten (discrete) years before the interview, which translates into 1995. In contrast, the 2000 RDHS was collected between June and November 2000, so most respondents who lost a sibling during the genocide period would have reported the death occurring six years ago, which translates into 1994.

[7] Verwimp and de Walque (2010) use the same RDHS data from 2000 to analyze excess mortality patterns related to the 1994 genocide. They find that the mean and median of both the siblings' sex and date of birth are similar and conclude that there is no evidence for a systematic bias in the reporting of sibling deaths.

widows. Still, the majority of widows in the 2000 and 2005 waves are very likely to be genocide widows (Brück and Schindler 2009) – that is, formerly wives of Tutsi husbands or moderate Hutu husbands. Given the gender-unbalanced mortality during the genocide, widowhood can be assumed to be exogenous. This point is underlined in Fig. 4, which depicts the distribution of current marital status (single, married, divorced, widowed) for women of various age groups in 1992, 2000, and 2005. The proportion of widows relative to married and unmarried women increased considerably after the genocide, particularly for younger women. For instance, the proportion of widows among women of age 25-34 years doubled between 1992 and 2000. Moreover, the proportion of widows rises steadily with older birth cohorts; a similar pattern is apparent for divorced women. This may indicate that once a woman becomes a widow, it is likely that she does not marry again given a lack of suitable partners of similar age.[8]

Finally, we calculate a demographic conflict proxy that captures the extent of deaths across age groups (SEXRATIO). The data on sex ratio comes from two secondary sources: the 1991 Census (which is matched with the 1992 RDHS) and the 2002 Census (which is matched with the 2000 RDHS and the 2005 RDHS). Women are assigned the average sex ratio (defined as the ratio of males to females) in the cohort of their potential partners in a given province, taking into account the typical age difference between spouses in Rwanda. More precisely, sex ratios in a woman's five-year age group, one younger age group and two older age groups were averaged. While this is our preferred measure, we also calculate the sex ratio in a women's five-year birth cohort and in her exact year of birth as a robustness test. These provincial, age group-specific sex ratios are the closest approximation to the local marriage market possible with publicly available data. Still, sex ratios derived from census data overestimate the number of men potentially available on the marriage market, as tens of thousands of male perpetrators of genocide were in jail (Ministry of Finance and Economic Planning et al. 2003).

It is important to note that our analysis is based on a sample of survivors. Households in which all (female) members died during the genocide are by definition not accounted for. In

[8] Unfortunately, most information on marital history available in the RDHS refers to the current partnership. Hence, we do not know whether a woman has ever been widowed and then remarried. Such a woman would simply appear as 'married' in the data.

other words, the impact of the genocide is underestimated in the present analysis. However, given that the focus of this paper is on the impact of the genocide on the fertility of the survivors in the post-genocide period (and not on estimating excess mortality), this does not bias our analysis.

5 Estimation strategy

We employ two slightly different estimation strategies to explore the impact of the two channels – replacement and marriage market – on fertility. This is due to the fact that proxies for conflict exposure measuring replacement effects are only available for post-genocide RDHS data, while conflict proxies measuring marriage market effects are available for both pre-genocide and post-genocide RDHS data. Table 2 provides a schematic summary of the estimation strategy.

To explore the replacement effects of conflict-related deaths on fertility, we conduct cross-sectional analyses with each of the post-genocide RDHS waves. The 2000 and 2005 RDHS provide insights into the short-term (five-year) and long-term (ten-year) effects of genocide on fertility, respectively. The dependent variable of interest is the number of children born alive in the aftermath of the genocide (while controlling for the number of children previously born). This estimation strategy builds on the idea that women may adjust their fertility in the post-conflict period, depending on the exact channel through which they were affected by the genocide. We exclude children conceived during the genocide (potentially through rape) from our analysis. Rather, the focus is on understanding how fertility decisions are made during the transition to peace. In the 2000 RDHS, the period of interest is May 1995 (when the first cohort of children conceived after the genocide is born) to June 2000 (the start of interviews for the 2000 RDHS). Similarly, in the 2005 RDHS, the dependent variable is the number of children born to a woman between May 1995 and February 2005 (when 2005 data collection began).

The determinants of fertility are estimated as a reduced-form equation, in line with the existing literature, as discussed above, with:

$$K_i = \alpha_0 + \beta_1 X_i + \beta_2 D + \beta_3 R + \beta_4 Conflict_i + \mu_i \qquad (1a)$$

where fertility *K* of woman *i* is a function of a constant α, a vector of the woman's socio-economic characteristics X_i, district-level and province-level characteristics *D*, region fixed effects *R*, a proxy for conflict exposure $Conflict_i$ (as discussed in Section 4.2), and a normally distributed error term u_i. The estimated coefficient β_4 measures the impact of *actual* conflict exposure during the genocide on fertility, relative to *all women after the genocide*.

To explore the marriage market effects of conflict on fertility, we exploit the fact that the conflict variables are available for all data waves, including the 1992 RDHS collected before the genocide. We pool the three cross-sectional RDHS waves pair-wise, combining the 1992 and 2000 waves and the 1992 and 2005 waves. Pooling surveys allows us to compare post-genocide fertility trends relative to a pre-genocide baseline period. For instance, inferences can be made about fertility of a population subgroup (e.g. widows) after the genocide in comparison to a similar population subgroup before the genocide. A similar approach to pooling of cross-sectional survey waves is applied by Akresh and de Walque (2008).

To ensure that the dependent variables are comparable from before and after the genocide, the periods are now defined in relative terms. In the pooled 1992-2000 data, the period of interest is the number of children born in the five years prior to the data collection. In the 2000 RDHS, this corresponds to the period between May 1995 and June 2000. This time span will be compared to the period between May 1987 and June 1992, when data collection for the 1992 RDHS began. Similarly, in the pooled 1992-2005 data, the time span of interest is the ten years prior to the starting date of the 2005 RDHS, which is the period between May 1995 and February 2005. This corresponds to the period from September 1982 to June 1992 in the 1992 RDHS.

To explore the marriage market effects of conflict on fertility, the following equation is estimated:

$$K_i = \alpha_0 + \beta_1 X_i + \beta_2 D + \beta_3 R + \beta_4 Year + \beta_5 Conflict_i + \beta_6 Conflict_i \text{ x } Year + \mu_i \quad (1b)$$

which additionally includes a time dummy *Year* for the survey year and an interaction term between the proxy for conflict exposure $Conflict_i$ and the time dummy. The estimated coefficient β_6 captures the impact of *actual* conflict exposure on fertility after the genocide relative to *women with similar risk of exposure to conflict before the genocide*.

Given that we control for the effects of conflict in addition to the usual socio-economic determinants of fertility and given that conflict may also affect these other variables, the estimated coefficient of the conflict variable can be interpreted as the 'pure conflict' effect. This makes our calculations conservative estimates of the total effects of conflict on fertility. For example, conflict is likely to reduce the educational attainment of children exposed to violence during their school-age (Akresh and de Walque 2008) and of girls in particular (Shemyakina 2006), hence inter alia raising their fertility.

In both (1a) and (1b), measures of each woman's socio-economic characteristics include three age categories (young: 15-24 years, middle: 25-34 years, and old: 35-49 years), the number of sons and daughters born before the time span of interest, her education (no education, some primary education, and some secondary or higher education), a dummy variable indicating whether she is currently in a union,[9] a dummy variable indicating whether she has had more than one union, a continuous household wealth index (see Section 4.1), a dummy variable indicating whether she has always lived in the same community, a dummy variable indicating whether the current location is urban, mortality rates of children under five years at the district level, population density at the province level, and region fixed effects. The return to these variables is assumed to remain constant over all three survey years (except age, education, and children previously born, which are interacted with the year dummy). Table 4 provides summary statistics of the variables used in the regressions.

The principle sample includes women aged 15-49 at the time of each survey collection; we further adjust the sample to match the definition of each conflict proxy (e.g. we only consider a sample of women who gave birth to their first child before the genocide when using the CHILDDEATH conflict proxy). The sample comprises both women who have not yet given birth to a child and women who have reached menopause. The youngest mother in the sample gave her first birth at age 12. In order to account for the different time spans that women are at risk of pregnancy, age 12 is used as the onset of exposure to conception.

The dependent variable in all regressions is a count variable with non-negative integer values ranging from 0 to 5 in the short term and 0 to 8 in the long term. We employ a Poisson

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[9] This variable is not included in the estimation of equation (1b), given that widowhood is strongly correlated with not being in union.

regression model that accounts for both censoring at zero and the integer nature of the values, assuming that the count variable follows a Poisson distribution. One key property of the Poisson distribution is that the conditional mean and conditional variance of the count variable are equal, which implies that counts occur independently from each other (Wang and Famoye 1997). The conditional expected value of the count variable is parameterized as an exponential function (cf. Cameron and Trivedi 2009). Alternative regression models are discussed in Section 6.3.

As a refinement of the original regression model, we estimate the determinants of fertility during the past five and ten years, respectively, separately for different age groups (see Table 2). As outlined above, we bundle women into three age groups based on their current age at each survey collection. The young age group (15-24 years) consists of those who were children or teenagers during the genocide. The women in the middle age group (25-34 years) were in the middle of their childbearing years in 1994. The women in the old age group (35-49 years) were at the late stage of their fertile period at the time of the genocide. Differentiating the sample population by age allows us to investigate further the impact of the timing of an exposure to violence on a woman's fertility.

An econometric problem arises if schooling is endogenous to fertility (e.g. Sander 1992). The reasoning is that mothers with children at home have fewer resources (both in terms of time and finance) to invest in their own schooling, while there may also be unobserved characteristics (such as ability) causing a selection into schooling that may also determine fertility. However, in the case of Rwanda, where in 2005 some 84 percent of the population lives in the countryside and average levels of schooling are very low, few young women continue education beyond age 15. Similar to research conducted in other developing countries characterized with low levels of education (e.g. Ainsworth, et al. 1996), education is considered to be exogenous to fertility in the following analysis.

6 Results and discussion

6.1 Descriptive statistics

Total fertility rates (TFR) changed considerably during the survey years, yet not in a linear pattern. TFR decreased sharply from 8.5 in 1983 (INS and ORC International 2006: 38) to 6.2

in 1992 and further reduced to 5.8 in 2000 but increased slightly to 6.1 in 2005. This pattern found in Rwanda is distinct from other conflict-affected countries where fertility declined during the war and then increased (at least for some time) in the post-war period, as previously discussed.

Some further unconditional statistics on fertility-related variables are displayed in Table 3. Both age at first marriage and age at first birth increased consistently between 1992 and 2005. In contrast, the ideal family size increased sharply after the genocide, from 4.32 children in 1992 to 5.04 in 2000. The use of modern contraception methods was at a very low level and even decreased over time. The proportion of polygamous marriages decreased slightly after the genocide. The proportion of women living in female-headed households rose by more than 15 percentage points in the post-war period. This again mirrors the demographic imbalances. Almost all socio-economic measures related to well-being improved over the years.

6.2 Multivariate analysis

Results of Poisson estimations of the determinants of fertility are displayed in Table 5 (short-term analysis), Table 6 (long-term analysis), and Table 7, 8, and 9 (for young, middle, and old women, respectively). The dependent variables used in all these tables are the number of children born alive to a woman during the five and ten years before the survey, respectively.

While Poisson is a natural candidate for estimating models in which the dependent variable is a non-negative integer count variable, the assumption of equidispersion is violated moderately. Equidispersion seems to be less of an issue in the short-term, where the variance-to-mean-ratio is about 1.1, but slightly more so in the long-term, where the variance-to-mean-ratio is about 1.7. A formal test establishes that over-dispersion is present in most estimations, indicating that the variance is greater than the mean, which would underestimate standard errors. We correct for this by computing robust (and cluster-specific) standard errors. This still retains consistent estimates if the conditional mean function is correctly specified (Cameron and Trivedi 2009: 561). The fitted probabilities of the Poisson model are acceptable, with the maximum difference in predicted and actual probabilities of the count variable ranging between 0.08 (short term) and 0.1 (long term). The Poisson prediction of zero counts is relatively precise, while the prediction of two children is the least precise. Other regression diagnostics (chi-squared value, log-likelihood statistics, AIC, BIC, and the squared coefficient of correlation between fitted and observed values of the dependent variable) confirm that Poisson estimations fit the data reasonably well. Overall, the model fit in the middle age group, which does not exhibit any natural censoring in its fertility, is much better than the fit for the young and old age groups.

a) Conflict and fertility in the short term

Women surveyed in 2000 who lost at least one child during the genocide (CHILDDEATH) gave birth to a significantly larger number of children in the short term than other women (Table 5). This findings supports the replacement hypothesis: The estimated fertility rate of these women is factor exp(0.14) = 1.15 times the predicted fertility rate of women not affected by conflict, controlling for the number of sons and daughters previously born and socio-economic factors. Moreover, the gender of the lost child matters: The death of a son (SONDEATH) results in significantly higher fertility in the post-war period, while the loss of a daughter (DAUGHTERDEATH) does not affect fertility. This result is in line with expectations, given that Rwanda is a patrilineal society. Women used to get access to land and resources primarily through men, and sons traditionally set up their own household close to their parental homestead (Hamilton 2000).[10]

Interestingly, we find the opposite effect for sibling death. Women in 2000 who lost a sibling (SIBLINGDEATH) during the genocide have significantly lower fertility (with a factor effect of exp(-0.06) = 0.94) in the post-war period compared to women did not loose siblings in 1994, holding other factors constant. We also differentiate sibling deaths by the age of the sibling relative to the respondent (results available upon request). The death of a younger sibling has a significantly negative impact on fertility (by factor exp(-0.09) = 0.91). In contrast, the death of older siblings of either gender does not significantly influence fertility.

We propose that (young) women were more likely to be at home with their parents and younger siblings at the time of the genocidal attacks, while men may have been relatively more likely to have been attacked away from their homes. The death of a sister or a younger

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[10] New legislation on succession and marital property regimes became law in 1999, granting women the right to hold ownership of property, including land, and to inherit (Burnet and RISD 2001).

sibling may imply having experienced an attack during the genocide at home. In turn, this variable may capture the effect of having witnessed violence committed against close family members (and possibly against the respondents as well, including gender-based violence), trauma, or stigmatization as a result of belonging to a victimized household.

As regards the marriage market effects of conflict on fertility, widows *before* the genocide (WIDOW) give birth to a significantly lower number of children than women of other civil status in 1992, holding other characteristics constant. Still, widows *after* the genocide (WIDOW x YEAR00) have even significantly lower fertility rates than widows before the genocide. The fertility rate of widows in 2000 during the past five years is exp(-0.18) = 0.83 times the fertility rate of widows in the pre-genocide period. This result suggests that childbirth continues to be strongly regulated through marriage. Households headed by widows (95 percent of widows in the sample are heads of household) thus have fewer members than male-headed households due to the death of the husband and fewer children born. Moreover, widows do not have a larger number of foster children or non-related young household members than other households. In the strongly gendered, labor-intensive, farm-based economy of rural Rwanda, this may render households of widows vulnerable to poverty, particularly at old age (Brück and Schindler 2009).

In the pre-genocide baseline period, a more balanced sex ratio in the province (SEXRATIO) significantly increases fertility. This pattern remains constant over time: The age-group and province-specific sex ratios in 2000 (SEXRATIO x YEAR00) do not differ significantly from the impact of sex ratios on fertility in 1992.[11] In 2000, the impact of sex ratios on fertility is still large (with a factor effect of exp(0.97-0.41) = 1.75). This is despite the fact that sex ratios became more unbalanced during the genocide: The mean sex ratio over all age groups of the sample is 0.95 in 1992 and 0.85 in 2000. In other words, a shortage of potential partners in a woman's cohort and province significantly reduces the number of children born both before and after the genocide.

Both results indicate that fertility is strongly linked with gender roles and partnership and that sexual activity outside of marriage did not become more common after the genocide.

[11] This finding is robust even when using slightly different methods of calculating sex ratios, such as the sex ratio in a woman's five-year birth cohort or in her exact year of birth.

Thus, a relative shortage of men in a given province correlates with a lower number of unions. It appears as if there are divergent trends for two groups of women. On the one hand, it seems that fertility among married women before and after the genocide remained unchanged with married women continuing to have many children. One may speculate that the high fertility after the genocide relates to the competition for marriageable partners. This may encourage married women to conform to the ideal female role model in Rwanda by becoming a mother of a large family (Jefremovas 1991; UNICEF 1997). A similar pattern – a local shortage of men causing a stricter gendered division of labor in household tasks – is found to influence intra-household time allocation patterns in Rwandan households (Schindler 2010). Another reason may be that, despite the genocide, the basic incentives for having large families in agricultural production and in providing old age security persisted in Rwanda, thus maintaining material benefits for parents of larger families. On the other hand, an increasing number of women are without partners in the post-genocide period. These women (and especially widows) have lower fertility levels, which may render them vulnerable at old age.

Most of the standard variables predicted by the literature to determine fertility have the expected signs. Fertility in the short term is strongly correlated with the number of children previously born. Also the sex of children matters: One additional son (SONNUMSHORT) changes the fertility rate in the short term by factor exp(-0.05) = 0.95, while one additional daughter (DAUGHTNUMSHORT) changes the fertility rate by factor exp(-0.08) = 0.92. Surprisingly, education (PRIMEDU, SECEDU) does not have a significant effect on fertility in the early post-war period. This result fits empirical evidence from other early post-war economies where education played a minor role in driving post-war socio-economic outcomes at the micro-level (e.g. Bozzoli and Brück 2009). In line with expectations, household wealth (WEALTHFUL) significantly decreases fertility. Being currently in union (INUNION) and living in a more densely populated province (PODENSITY) significantly increases fertility in the short term. Child mortality at the district level (DCHILDMORT) – one of the few exogenous measures of the health infrastructure that can be constructed from RDHS data – does not significantly affect fertility. Women living in cities (URBAN) do not have significantly lower fertility compared to their rural counterparts. This is likely due to the fact that the impact of the type of residence is already captured by region fixed effects, which also account for differences in wealth, infrastructure, and remoteness that we are unable to pinpoint more precisely due to the lack of community-level variables recorded in the RDHS.

There is a lack of information recorded in RDHS data on migration history that would allow us to identify whether a respondent was an old or new caseload refugee. However, we know if a woman has always lived in the same community (NATIVE), thus controlling for migration and displacement. Women who never left their community of origin have significantly lower fertility compared to women who at least once in their lives shifted the place of residence. This finding supports evidence from a previous study on Rwandan refugees (Verwimp and van Bavel 2005), which finds that new caseload refugees had 1.04 times the fertility rate of Rwandan women who never migrated, after controlling for age, civil status, education, place of residence and occupation. It also fits well into the findings of Verwimp and Van Bavel (2011) from Burundi, where conflict-related displacement leads to a faster transition into motherhood.

b) Conflict and fertility in the long term

Overall, the impact of conflict exposure becomes less pronounced in the long term (Table 6). The mean fertility rate in the past ten years of women who lost at least one child during the genocide (CHILDDEATH) was exp(0.12) = 1.12 times the fertility rate of women who did not lose children during the genocide. Women who lost at least one son (SONDEATH) have exp(0.13) = 1.13 more children than the reference group, while again the loss of a daughter in 1994 does not significantly influence fertility in the post-genocide period.

Unlike in the short term, sibling death in 1994 has no significant effects on fertility in the long term. These results may indicate that siblings are less important for livelihoods than own children, especially as siblings of women play less of a role in asset accumulation, land access, and old age security than do a woman’s own children. Alternatively, if sibling death captures the immediate exposure to violence at home, trauma, and victimization as argued above, these burdens appear to have been overcome in the subsequent five years.

In contrast, the negative impact of conflict on fertility for widows in 2005 (WIDOW x YEAR05) becomes even larger in the long-term compared to the short-term perspective, with a factor effect of exp(-0.40) = 0.67. This finding highlights the persistent vulnerability of widows as a population subgroup.

However, the opposite is true for the demographic impact of the conflict. A shortage of potential partners continues to limit fertility (with a factor effect of exp(0.98-0.61) = 1.44), but less so than in the immediate post-conflict period. This may be due to the fact that a larger share of sample respondents in the 2005 RDHS belongs to younger birth cohorts which are less severely affected by gender imbalances than in the 2000 RDHS. With respect to other control variables, many trends identified in the short term analysis also hold in the long term.

c) The impact of conflict exposure on fertility by age group

In order to explore the cohort-specific effects of conflict exposure on fertility, the original model is estimated by cohorts. Results for the youngest group of women are displayed in Table 7. Women in this age group surveyed in 2000 were 9-18 years old during the genocide; those surveyed in 2005 were 4-13 years old during the genocide. Very few sample women had given birth to a child before 1994 or were married at that time. Hence, CHILDDEATH, SONDEATH, DAUGHTERDEATH, and WIDOW are not used as proxies for conflict exposure in this age group. Interestingly, the only proxy for conflict exposure with a significant impact on fertility rates in the short term is the death of a sister during the genocide (SISTERDEATH). As argued above, this proxy possibly captures the effect of trauma and stigmatization as a victimized survivor of the genocide. However, this effect does not significantly impact fertility in the long term. Neither before nor after the genocide do sex ratios have a significant impact on the fertility of young women. Possibly, for many women of this age group, the matching of partners in the marriage market is not yet complete and gender imbalances among young women are not as severe compared to older birth cohorts.

Results show the opposite effect for women in the middle age grouping (of age 25-34 years during the survey collection, Table 8): On the one hand, a larger share of these women were likely exposed to conflict, while on the other hand, conflict exposure had a strong effect on their fertility in the post-conflict period. There is evidence for the replacing-the-lost hypothesis: Women of middle age who lost a child in 1994 (CHILDDEATH) give birth to a significantly larger number of children compared to other women in both 2000 and 2005, holding other factors constant. Widowhood significantly reduces the fertility of middle aged women compared to other women in this age group. Being a widow reduces fertility in the short term in both the pre- and post-genocide period. The fertility rate of widows in 2005 (WIDOW x YEAR05) was even lower (by a factor exp(-0.41) = 0.66) compared to widows before the genocide. In 1992, sex ratios (SEXRATIO) do not significantly influence fertility rates among middle age women. This is very different after the genocide: more balanced sex ratios significantly increase fertility rates in the early post-war period (SEXRATIO x YEAR00). Given that most of the women of middle age are in union, this finding again suggests that fertility and partnership are linked. Considering the number of living children in 2005, women of middle age who lost a child during the genocide do not have significantly fewer living children than women who were not exposed to conflict (holding other characteristics constant). In short, it seems that women who were exposed to the genocide aged between 14 and 28 fully adjusted their fertility in the post-war period.

This is not the case for women in the oldest age group of 35-49 years during the survey collection (Table 9). In this age group, the negative effects of conflict exposure on fertility prevail – through widowhood (WIDOW x YEAR00; WIDOW x YEAR05), the loss of a brother in the short term (BROTHERDEATH), and the loss of a child (CHILDDEATH) and a sister in the long term (SISTERDEATH). In fact, women in the oldest age group who lost a child or their husband during the genocide have 0.91 and 0.72 fewer living children in 2005, respectively compared to women not affected by the genocide. Considering the fact that the dependent variables of interest only capture births given during the past five and ten years – and that many of the women in the old age group may have reached menopause – this result is not surprising. Women who were exposed to conflict between age 29-43 (those surveyed in 2000) and 24-38 (those surveyed in 2005) have fewer chances to replace close kin who died in 1994. To conclude, it is the oldest group of women whose family structure is affected most strongly by mortality during the genocide.

d) Simulation results

In order to explore the magnitude of the effects, we predict the number of births and the number of living children for women exposed to conflict through different channels for each month between January 1990 and January 2005 (Fig. 5). All other socio-economic characteristics are held constant at their mean values. Women who lost at least one child during the genocide were able to adjust their fertility, although they could not fully make up for the lost child (Fig. 5a). However, conflict-affected women were not able to narrow the gap in the number of living children compared to women who did not lose children during the genocide (Fig. 5b). In part, this is due to higher mortality rates of children born to these women, possibly as a result of shorter intervals between births. Women who lost a sibling in 1994 do not differ significantly in the number of living children in the post-genocide period (Fig. 5c). In contrast, for widows the gap in the number of living children becomes larger over time compared to women of other civil status (Fig. 5d).

6.3 Robustness tests

We conduct multiple tests on the robustness of the findings (results available upon request). First, the determinants of fertility are estimated with a Zero-Inflated Poisson model to allow the probability to give birth to the first child to differ across (latent) groups of women.[12] Two variants of the Zero-Inflated Poisson model are estimated, with INUNION and the complete set of independent variables used as inflation variables determining the binary process. In both variants of the Zero-Inflated Poisson model the point estimates of the conflict proxies are of comparable magnitude and level of significance as in the original Poisson estimates.[13]

Second, we conduct a placebo genocide test, where we use child deaths and sibling deaths occurring between January 1990 and December 1993 as placebo conflict proxies. None of the placebo proxies significantly influences fertility in the short-term or long-term post-war period. This evidence confirms that our analysis captures fertility effects of the 1994 genocide.

Third, we define CHILDDEATH and SIBLINGDEATH as proportions of the total number of children and siblings born before the genocide, respectively. This allows the probability to

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[12] A Negative Binomial model would be our preferred alternative, as it relaxes the assumption of equidispersion. Yet, this model has difficulties to converge with the data at hand and the estimate of ln(alpha) is large and negative, which prevents the prediction of probabilities. Following Long and Freese (2006), we resort to the Zero-Inflated Poisson model.

[13] Although the Vuong test, fitted probabilities of the count variable, AIC, BIC, and log likelihood all indicate a slightly better fit of the Zero-Inflated Poisson model over the Poisson model, the latter allows for a more forward interpretation of the estimated coefficients and remains our preferred model.

lose a child and a sibling to vary with fertility at the time of the genocide and family size. The estimated coefficients are larger in magnitude, while the level of significance is similar.

Fourth, using the 1992 RDHS wave, we compare fertility among Hutu and Tutsi. Of the nationally representative sample of women surveyed in the 1992 RDHS, 8.6 percent are Tutsi.[14] Descriptive statistics point towards differences across both groups: On average, Tutsi women have 0.58 fewer children, they marry 1.73 years later and give birth to the first child 1.69 years later than Hutu women (all three figures are significantly different in means across Hutu and Tutsi). Other socio-economic characteristics differ significantly as well across Hutu and Tutsi, including education and wealth. However, it seems that these differences in fertility behavior and socio-economic status are driven by the place of residence: 14 percent of Tutsi women live in urban areas, compared to 5 percent of Hutu women. A regression analysis confirms this: When regressing the total number of children on multiple characteristics (including ethnicity, age, education, place of residence, and partnership characteristics), ethnicity is no longer statistically significant.

Fifth, we run all estimates based on the full sample of women in order to enhance the comparability and precision of the estimates. In both short-term and long-term analyses, the impact of conflict exposure is only slightly smaller in terms of the magnitude of effects and the level of significance compared to the original results derived from restricted samples.

7 Conclusions

The paper analyzes the effects of mass violent conflict on fertility for conflict-survivors in the case of the genocide in Rwanda in 1994 using individual-level data. To enable an identification of the genocide at the micro-level, two types of proxies for conflict exposure are constructed using three waves of RDHS data from before and after the genocide. With this estimation strategy, we identify the 'pure' effects of genocide on fertility over and above conflict-related effects like urbanization, destroyed infrastructure, and weaker health infrastructure – which in turn also affect fertility. We study the short- and long-term effects

[14] This self-reported ethnicity variable very likely underestimates the Tutsi population in 1992. Historical accounts on ethnic violence before 1994 suggest that many individuals tried to hide their ethnic identity in order to avoid discrimination and persecution (Desforges 1999). This may have also been the case in the RDHS data collection.

of genocide and differentiate the analysis by cohort, kinship relation and gender of the deceased persons.

Our approach is unique in that we estimate the effects of genocide on fertility using multiple measures of conflict exposure and disaggregating in more dimensions than done previously. In Rwanda, this is important as by law collecting a key conflict proxy, ethnicity, is not possible. In other contexts using conflict proxies may be important if data-sets, which do not contain any direct identification of conflict at the micro-level, are to be analyzed retrospectively.

The paper has five major findings. First, we find a significant direct (or 'pure') effect of mass violent conflict in determining fertility. Of all conflict proxies studied, the loss of a child during the genocide had the largest (and positive) effect on fertility in the aftermath of the genocide. We interpret this as evidence for a replacement effect.

Second, we observe differential effects of conflict on fertility in the shorter term (approximately five years) versus the longer term (approximately ten years) after the genocide. Given that we observe significant effects of conflict on fertility even ten years after the genocide ended, these findings suggest that the post-conflict period in Rwanda lasted just over ten years.

Third, there are gender-specific effects in both child deaths and sibling deaths. We only find evidence for replacement of lost sons, not of lost daughters. This result highlights the importance of patrilineality in Rwandan society. It can also be explained by the continued role of social norms for land access and old age security. As regards sibling deaths, the loss of a sister has a stronger negative impact on fertility than the loss of a brother. This may indicate the impact of having experienced genocidal violence at home.

Fourth, fertility is strongly linked with marital status and marriage markets. Widows face significantly lower fertility before and after the conflict, again emphasizing the role of social norms for fertility in Rwanda. Imbalanced sex ratios as a demographic conflict proxy appear to lead to lower fertility, pointing to the continued role of marriage markets and marital status for determining fertility. Some of these effects are more pronounced in the long-term, hence indicating that conflict legacies will continue to shape the population growth rate and population structure of Rwanda for many years to come.

Finally, there are cohort-specific effects in that the timing of conflict exposure during a woman's fertile years strongly influences fertility. The fertility of women who were children or teenagers during the genocide is not significantly affected by this early conflict exposure. Women who were in their mean childbearing years during the genocide are able to adjust their fertility and replace lost kin. However, women who were at the end of their fertile period during the genocide were too old to adjust fertility in the post-conflict period. Hence, the latter group of women is most persistently affected in their fertility by conflict exposure.

Our findings suggest four implications for the design of post-conflict reconstruction policies. First, the results help identify potentially vulnerable groups of survivors of the genocide, like widows and households with many killed sons. Understanding the effects of conflict across groups in general will help to target future assistance policies more accurately.

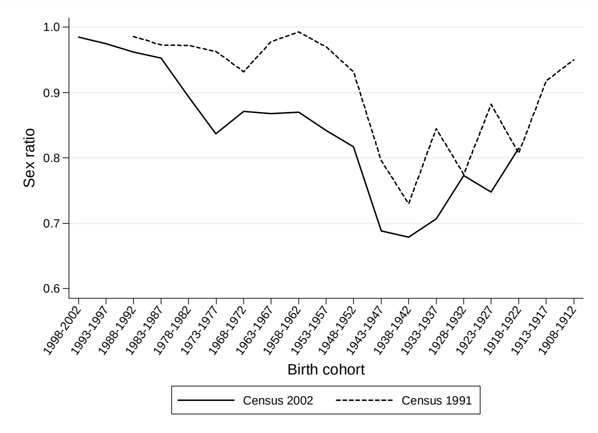
Second, the analysis suggests that within approximately ten years after the genocide, the effects of the conflict on fertility start to wear off. From this particular perspective, this dates the duration of the post-conflict period, during which policies may need to explicitly account for the previous conflict. More generally, there is a need to understand how long post-conflict periods last after different conflicts in different countries and with respect to different variables. For example, it is unlikely that a 'standard' population planning policy implemented soon after the genocide would have the same effect while the demand for children was still partly driven by the need for replacement of lost kin.

Third, and given that Rwanda has traditionally experienced very high levels of fertility by international comparisons, our results indicate the drivers of post-genocide fertility in Rwanda and may suggest possible avenues for interventions. Of all policy-related variables we considered in the analysis directly, education, migration and the balance of the sex ratios (in a country characterized by little internal migration) suggest areas for intervention to reduce fertility. Other targets for policy initiatives could include strengthening incentives for old age security beyond own children, reducing the reliance on family labor in land access and agricultural production and promoting the usage of contraception.

Fourth, we observe a strong persistence of traditions with some key variables showing a high degree of continuity. This is surprising when considering that the genocide induced significant structural changes in the economy in other respects, like increased rural-urban market integration and urbanization. From a policy perspective, this begs the question if these norms and traditions, to the extent that they may have contributed to the emergence of mass violent conflict in the first place, represent a potential source of future instability – or if the persistent social norms in the realm of fertility can be harnessed for increased stability and development in the future.

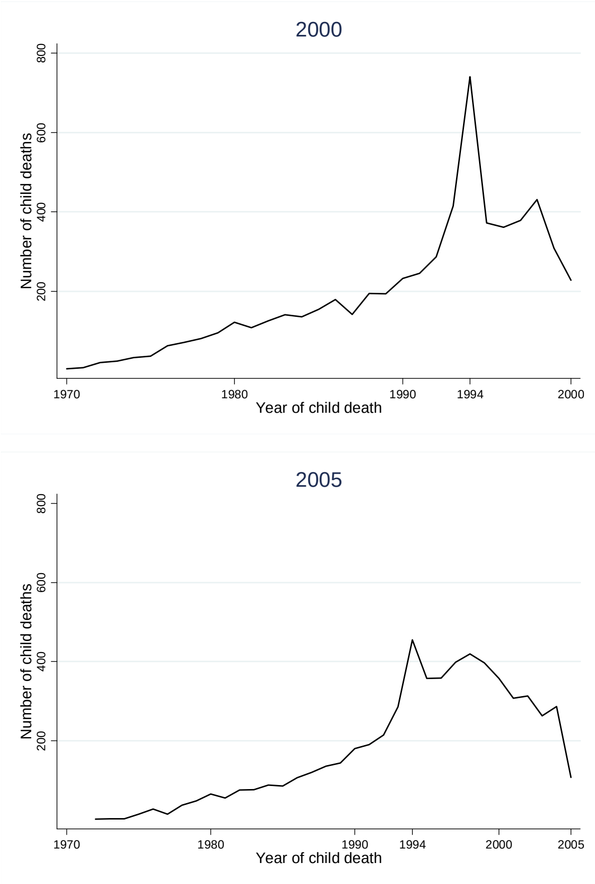
Appendix

Fig. 1: Sex ratio by age group



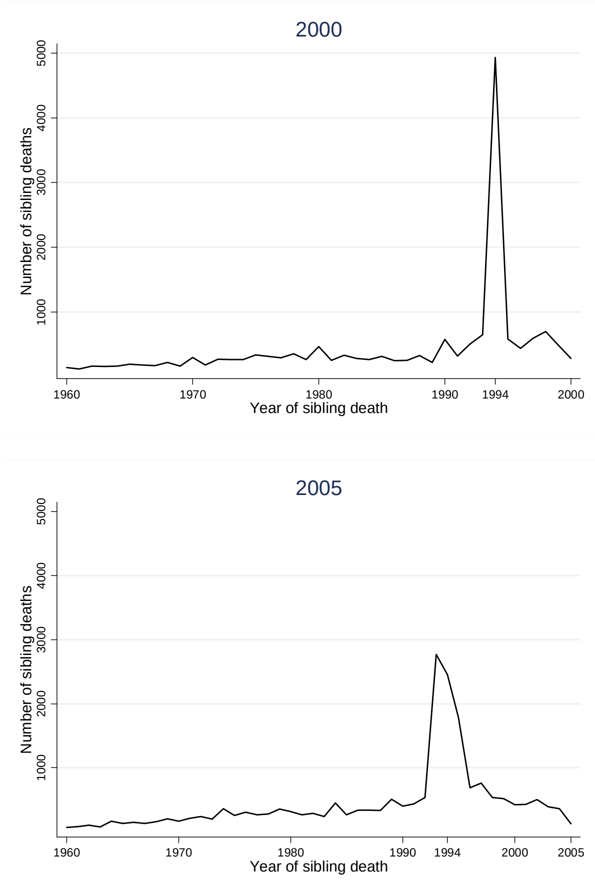
Source: Calculated from reports on the 1991 Census and the 2002 Census.

Fig. 2: Child mortality over time



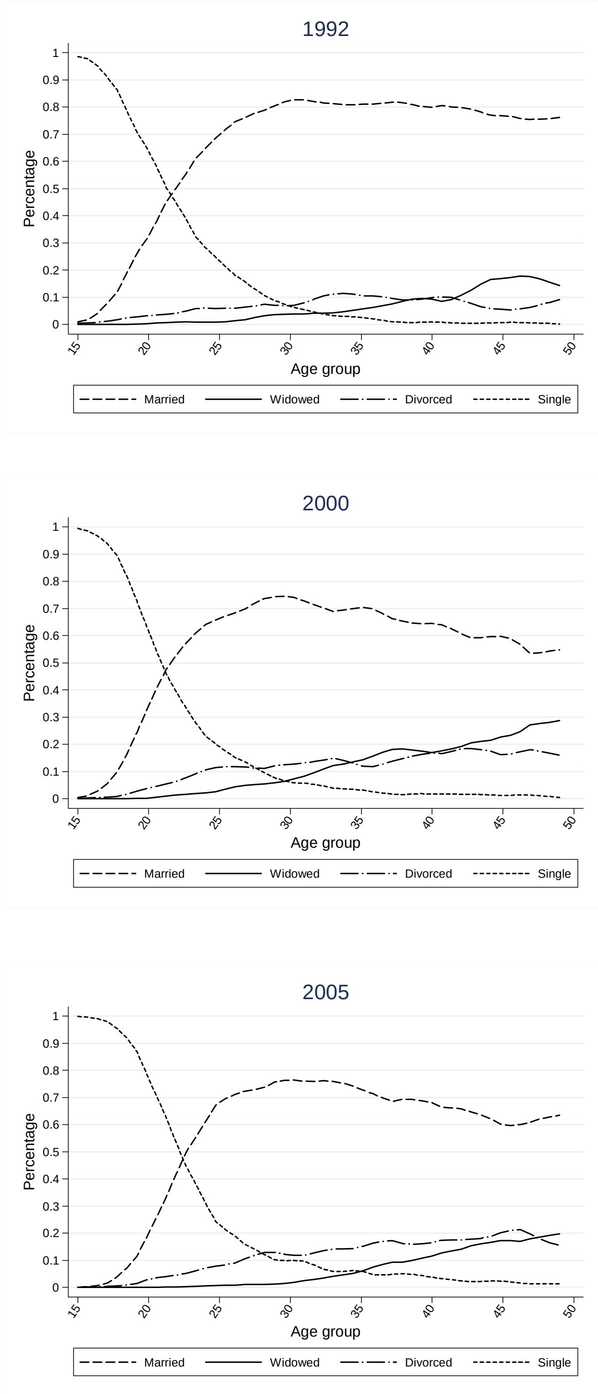
Source: RDHS 2000 and RDHS 2005. The graphs display the total number of child deaths and are not adjusted to a larger number of children born in recent years.

Fig. 3: Sibling mortality over time



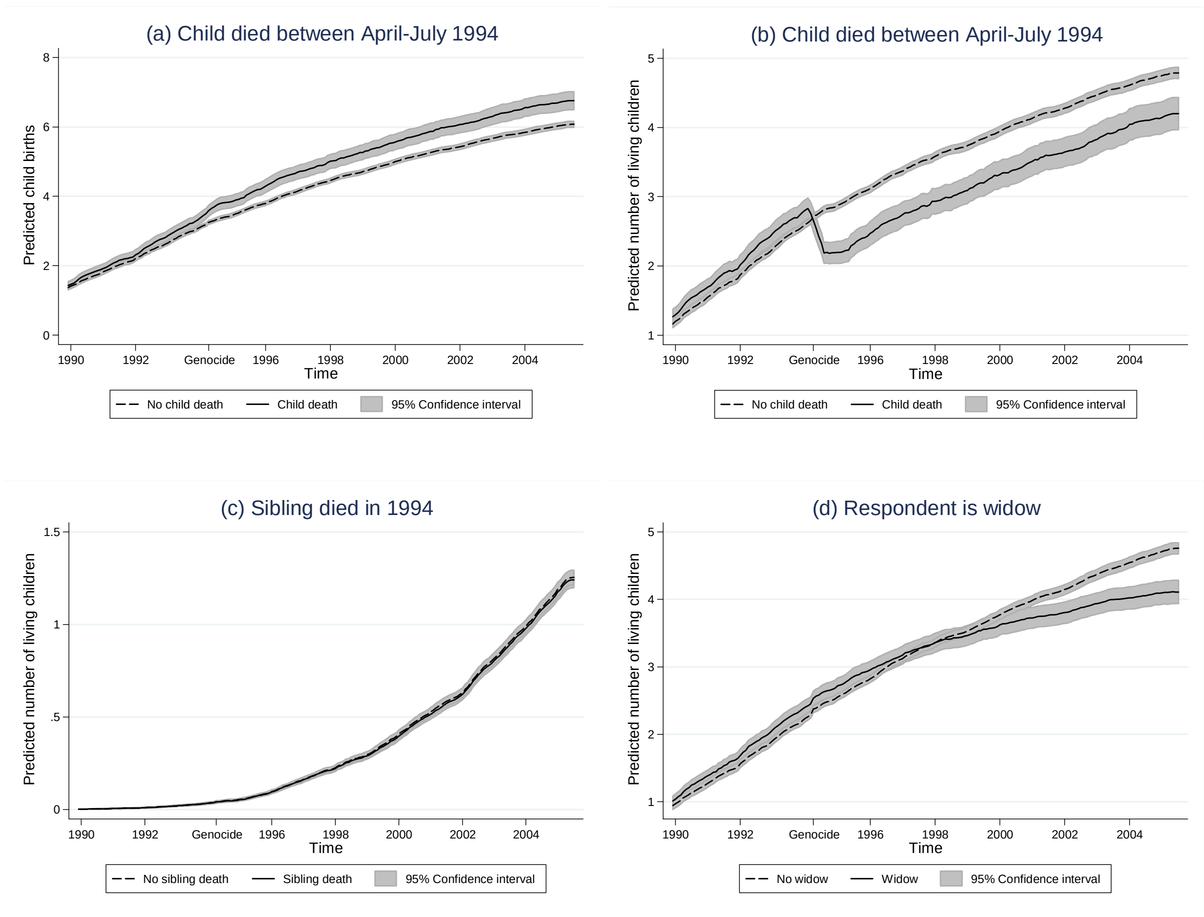
Source: RDHS 2000 and RDHS 2005. Double counts of the same siblings as reported by respondents living in the same household are excluded in these figures.

Fig. 4: Marital status over age



Source: RDHS 1992, RDHS 2000, and RDHS 2005. Non-parametric local polynomial regression with Epanechnikov kernel weights. Population weights were used.

Fig. 5: Predicted fertility over time



Source: RDHS 2005. Predicted values were calculated from Poisson regressions. All other covariates were held constant at their respective means. Robust and cluster-level standard errors were used. Sample: (a) and (b) women whose first child was born before the genocide; (c) women whose oldest sibling was born before the genocide; (d) women who had their first marriage before the genocide.

Table 1: Conflict proxies

Variable name	Variable definition in 1992	Variable definition in 2000 and 2005	Data source		
			1992	2000	2005
Replacement effects					
CHILDDEATH	–	Respondent's child died between April-July 1994	–	RDHS 2000	RDHS 2005
SONDEATH	–	Respondent's son died between April-July 1994	–	RDHS 2000	RDHS 2005
DAUGHTERDEATH	–	Respondent's daughter died between April-July 1994	–	RDHS 2000	RDHS 2005
SIBLINGDEATH	–	Respondent's sibling died in 1994	–	RDHS 2000	RDHS 2005
BROTHERDEATH	–	Respondent's brother died in 1994	–	RDHS 2000	RDHS 2005
SISTERDEATH	–	Respondent's sister died in 1994	–	RDHS 2000	RDHS 2005
Marriage market effects					
WIDOW	Respondent is widow	Respondent is widow	RDHS 1992	RDHS 2000	RDHS 2005
SEXRATIO	Sex ratio in respondents' cohort of potential partners per province	Sex ratio in respondent's cohort of potential partners per province	Census 1991	Census 2002	Census 2002

Table 2: Schematic representation of data sources and estimation strategy

<table>
<tr><th></th><th></th><th>1982</th><th>1987</th><th>RDHS 1992</th><th>Genocide 1994</th><th>1995</th><th>RDHS 2000</th><th>RDHS 2005</th></tr>
<tr><td rowspan="2">Replacement effects</td><td>Long-term analysis</td><td></td><td></td><td></td><td></td><td colspan="2">5 years</td><td></td></tr>
<tr><td>Short-term analysis</td><td></td><td></td><td></td><td></td><td colspan="3">10 years</td></tr>
<tr><td rowspan="2">Marriage market effects</td><td>Long-term analysis</td><td></td><td colspan="2">5 years</td><td></td><td colspan="2">5 years</td><td></td></tr>
<tr><td>Short-term analysis</td><td colspan="3">10 years</td><td></td><td colspan="3">10 years</td></tr>
<tr><td>Age group 1: Young</td><td>Age at survey collection</td><td></td><td></td><td>15-24</td><td></td><td></td><td>15-24</td><td>15-24</td></tr>
<tr><td></td><td>Age during genocide</td><td></td><td></td><td></td><td></td><td></td><td>9-18</td><td>4-13</td></tr>
<tr><td>Age group 2: Middle</td><td>Age at survey collection</td><td></td><td></td><td>25-34</td><td></td><td></td><td>25-34</td><td>25-34</td></tr>
<tr><td></td><td>Age during genocide</td><td></td><td></td><td></td><td></td><td></td><td>19-28</td><td>14-23</td></tr>
<tr><td>Age group 3: Old</td><td>Age at survey collection</td><td></td><td></td><td>35-49</td><td></td><td></td><td>35-49</td><td>35-49</td></tr>
<tr><td></td><td>Age during genocide</td><td></td><td></td><td></td><td></td><td></td><td>29-43</td><td>24-28</td></tr>
</table>

Table 3: Descriptive statistics

Variable	Description	Mean (1992)	Mean (2000)	Mean (2005)	Standard error of mean (1992)	Standard error of mean (2000)	Standard error of mean (2005)
Fertility							
CHILDNUM	Number of children ever born	3.06	2.77	2.68	0.056	0.034	0.030
CHILDDIED	Number of children who died	0.60	0.94	0.86	0.024	0.021	0.018
PREFCHILD	Ideal number of children	4.32	5.04	4.48	0.039	0.033	0.026
FAMPLANKNOW	Knows about any modern contraception	0.97	0.93	0.93	0.002	0.003	0.003
FAMPLANCUR	Currently uses any modern contraception	0.08	0.02	0.04	0.006	0.002	0.002
FIRSTBIRTHAGE	Age at first birth	21.13	21.55	21.57	0.085	0.055	0.050
BIRTHCLIN	Delivered last birth in clinic or health center	0.25	0.25	0.29	0.015	0.009	0.009
Civil status							
SINGLE	Has never been married	0.32	0.34	0.37	0.009	0.005	0.005
MARRIED	Is currently married	0.57	0.48	0.48	0.010	0.006	0.005
DIVORCED	Is currently divorced	0.06	0.09	0.09	0.003	0.003	0.003
WIDOWED	Is currently widowed	0.04	0.07	0.04	0.002	0.003	0.002
UNIONNUM	Had more than one union	0.11	0.10	0.09	0.005	0.003	0.003
HUSBANDHOUSE	Partner lives in the same household	0.95	0.87	0.89	0.004	0.006	0.004
POLYGAM	Lives in polygamous marriage	0.08	0.05	0.05	0.006	0.003	0.002
FIRSTMARAGE	Age at first marriage	19.77	20.30	20.44	0.083	0.052	0.052
Education							
CLASNUM	Years of education	3.40	3.71	3.74	0.113	0.064	0.052
GRADENUM	Number of grades completed	3.24	3.70	3.70	0.101	0.064	0.051
NOEDU	Has no education	0.37	0.29	0.23	0.013	0.007	0.005
LITERATE	Is literate	0.60	0.66	0.70	0.013	0.007	0.006
Respondents' socioeconomic characteristics							
AGE	Age	28.28	28.18	28.34	0.114	0.105	0.090
CATHOLIC	Is Catholic	0.62	–	0.45	0.020	–	0.011
PROTESTANT	Is Protestant	0.21	–	0.37	0.015	–	0.009
ADVENTIST	Is Adventist	0.13	–	0.14	0.014	–	0.008
MUSLIM	Is Muslim	0.01	–	0.01	0.008	–	0.002
FARMING	Self-employed farming is main occupation	0.85	0.70	0.62	0.010	0.012	0.009
NATIVE	Has always lived in this community	0.37	0.41	0.37	0.015	0.008	0.008
Household characteristics							
FEMHEAD	Household head is female	0.19	0.37	0.34	0.008	0.008	0.007
HHSIZE	Household size	6.18	5.55	5.54	0.075	0.040	0.035
BIKE	Household owns bike	0.06	0.08	0.11	0.007	0.005	0.005
QUALHOUSE	Housing floor is of good quality	0.12	0.16	0.14	0.010	0.010	0.007

Source: RDHS 1992, RDHS 2000, RDHS 2005. Population weights were used. Sample: women of age 15-49 years at the time of survey collection.

Table 4: Summary statistics of variables used in regressions

Variable	Description	Mean	Mean	Mean	Standard error of mean	Standard error o mean	Standard error o mean	Minimum	Maximum	Number of observations
		(1992)	(2000)	(2005)	(1992)	(2000)	(2005)	(2000)	(2000)	(2000)
Dependent variables										
CHILDBORNSHORT	Number of children born alive in last 5 years prior to survey collection	0.89	0.81		0.017	0.013		0	5	10421
CHILDBORNLONG	Number of children born alive in last 10 years prior to survey collection	1.70		1.45	0.030		0.018	0	8	11320
Previous children										
SONNUMSHORT	Number of sons born alive more than 5 years prior to survey collection	1.07	0.99		0.024	0.018		0	9	10421
DAUGHTNUMSHORT	Number of daughters born alive more than 5 years prior to survey collection	1.09	0.96		0.023	0.017		0	10	10421
SONNUMLONG	Number of sons born alive more than 10 years prior to survey collection	0.67		0.61	0.017		0.012	0	11	11320
DAUGHTNUMLONG	Number of daughters born alive more than 10 years prior to survey collection	0.68		0.61	0.016		0.010	0	9	11320
Individual characteristics										
YOUNG	Belongs to young age group (15-24 years) (d)	0.41	0.43	0.43	0.006	0.005	0.005	0	1	10421
MIDDLE	Belongs to middle age group (25-34 years) (d)	0.31	0.27	0.28	0.006	0.004	0.004	0	1	10421
OLD	Belongs to old age group (35-49 years) (d)	0.26	0.29	0.28	0.005	0.005	0.004	0	1	10421
NOEDU	Has no education (d)	0.37	0.29	0.23	0.012	0.007	0.005	0	1	10421
PRIMEDU	Has some primary education (d)	0.54	0.59	0.67	0.010	0.006	0.005	0	1	10421
SECEDU	Has some secondary or higher education (d)	0.07	0.10	0.09	0.006	0.006	0.004	0	1	10421
INUNION	Is currently in union (d)	0.57	0.48	0.48	0.009	0.006	0.006	0	1	10421
UNIONNUM	Respondent had more than one union (d)	0.11	0.10	0.09	0.004	0.003	0.003	0	1	10421
WEALTHFUL	Household wealth index factor score	-0.28	-0.24	-0.11	0.047	0.046	0.041	-2.38	10.6	10421
NATIVE	Has always lived in the current community (d)	0.37	0.41	0.37	0.013	0.008	0.008	0	1	10421
URBAN	Current place of residence is urban (d)	0.06	0.17	0.16	0.008	0.010	0.010	0	1	10421
DCHILDMORT	Under 5 child mortality rate at district level in 5 years preceding DHS survey	0.17	0.18	0.14	0.006	0.003	0.002	0	0.45	10421
Year dummy										
YEAR00	Survey collected in 2000 (d)	0	1	0	0	0	0	0	1	10421
YEAR05	Survey collected in 2005 (d)	0	0	1	0	0	0	0	1	11320
Conflict indices										
CHILDDEATH	Child died between April-July 1994 (d)		0.03	0.02		0.002	0.001	0	1	10421
SONDEATH	Son died between April-July 1994 (d)		0.02	0.01		0.004	0.001	0	1	10421
DAUGHTERDEATH	Daughter died between April-July 1994 (d)		0.01	0.01		0.001	0.001	0	1	10421
SIBLINGDEATH	Sibling died in 1994 (d)		0.29	0.27		0.006	0.006	0	1	10421
BROTHERDEATH	Brother died in 1994 (d)		0.21	0.19		0.005	0.005	0	1	10421
SISTERDEATH	Sister died in 1994 (d)		0.14	0.13		0.004	0.004	0	1	10421
WIDOW	Widow (d)	0.04	0.07	0.04	0.002	0.003	0.002	0	1	10421
SEXRATIO	Sex ratio in a woman's cohort of potential partners and province	0.95	0.85	0.88	0.003	0.002	0.004	0	1.64	10421

Source: RDHS 1992, RDHS 2000, RDHS 2005. Population weights were used. Minimum, maximum, and number of observations refer to RDHS 2000; if a variable is only defined for 2005, the reference is RDHS 2005. (d) indicates a dummy variable.

Table 5: Determinants of number of children born after genocide in short term (Poisson regression)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
YOUNG	0.56	1.17	1.16	1.07	0.54	0.55	0.54	1.16	0.30	1.05	0.28	1.00
	(12.07)***	(14.61)***	(9.11)***	(11.49)***	(11.52)***	(11.21)***	(11.17)***	(14.54)***	(4.76)***	(15.89)***	(4.40)***	(15.06)***
MIDDLE	0.69	0.64	0.59	0.62	0.68	0.68	0.67	0.63	0.73	0.69	0.69	0.63
	(21.03)***	(18.89)***	(15.15)***	(16.10)***	(20.43)***	(19.86)***	(19.75)***	(18.30)***	(18.37)***	(17.83)***	(17.36)***	(17.08)***
SONNUMSHORT	-0.05	-0.08	-0.09	-0.07	-0.05	-0.05	-0.05	-0.08	-0.04	-0.06	-0.04	-0.05
	(-5.49)***	(-7.08)***	(-7.02)***	(-6.35)***	(-5.52)***	(-5.09)***	(-5.67)***	(-7.22)***	(-4.19)***	(-5.45)***	(-3.87)***	(-4.86)***
DAUGHTNUMSHORT	-0.08	-0.11	-0.12	-0.12	-0.08	-0.08	-0.08	-0.10	-0.04	-0.06	-0.04	-0.06
	(-8.70)***	(-10.44)***	(-10.53)***	(-9.26)***	(-8.66)***	(-8.65)***	(-8.74)***	(-10.30)***	(-4.27)***	(-5.52)***	(-4.01)***	(-5.09)***
PRIMEDU	0.01	0.03	0.03	0.04	0.01	0.02	0.01	0.04	-0.09	-0.01	-0.10	-0.02
	(0.40)	(1.17)	(0.97)	(1.38)	(0.63)	(0.75)	(0.42)	(1.46)	(-3.57)***	(-0.36)	(-3.82)***	(-0.76)
SECEDU	-0.04	-0.05	-0.06	-0.04	-0.04	-0.03	-0.03	-0.05	-0.36	-0.11	-0.37	-0.13
	(-0.78)	(-0.76)	(-0.83)	(-0.53)	(-0.72)	(-0.63)	(-0.61)	(-0.80)	(-5.23)***	(-1.15)	(-5.40)***	(-1.41)
INUNION	1.21	0.80	0.80	0.84	1.21	1.21	1.22	0.79				
	(34.12)***	(22.53)***	(19.79)***	(20.99)***	(33.68)***	(32.96)***	(33.92)***	(22.37)***				
UNIONNUM	0.01	-0.03	-0.02	-0.03	0.02	0.02	0.03	-0.03	-0.01	-0.13	-0.01	-0.13
	(0.45)	(-0.90)	(-0.54)	(-0.68)	(0.67)	(0.52)	(0.98)	(-0.79)	(-0.47)	(-4.92)***	(-0.56)	(-5.00)***
WEALTHINDEX	-0.05	-0.05	-0.05	-0.06	-0.05	-0.06	-0.06	-0.05	-0.03	-0.03	-0.03	-0.03
	(-5.39)***	(-3.47)***	(-3.09)***	(-3.26)***	(-5.29)***	(-5.34)***	(-5.40)***	(-3.50)***	(-3.52)***	(-2.35)**	(-3.49)***	(-2.50)**
NATIVE	-0.08	0.03	0.04	0.04	-0.09	-0.10	-0.09	0.02	-0.37	-0.03	-0.37	-0.03
	(-3.36)***	(0.96)	(1.37)	(1.26)	(-3.57)***	(-3.80)***	(-3.67)***	(0.73)	(-14.09)***	(-1.44)	(-14.13)***	(-1.43)
URBAN	0.05	-0.05	-0.08	-0.02	0.05	0.06	0.05	-0.02	-0.09	-0.14	-0.09	-0.14
	(1.31)	(-0.89)	(-1.39)	(-0.34)	(1.43)	(1.57)	(1.32)	(-0.34)	(-2.07)**	(-3.08)***	(-2.06)**	(-3.02)***
DCHILDMORT	0.31	0.36	0.36	0.36	0.30	0.33	0.31	0.38	0.16	0.10	0.15	0.08
	(2.07)**	(1.85)*	(1.68)*	(1.66)*	(2.04)**	(2.24)**	(2.04)**	(1.93)*	(1.10)	(0.65)	(1.05)	(0.52)
POPDENSITY	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(1.70)*	(1.23)	(1.17)	(0.81)	(1.74)*	(1.75)*	(1.66)*	(1.28)	(1.40)	(1.59)	(2.26)**	(2.95)***
YEAR00									-0.18	-0.12	-0.15	-0.07
									(-2.43)**	(-1.66)*	(-1.92)*	(-0.94)
CHILDDEATH		0.14										
		(3.18)***										
SONDEATH			0.14					0.16				
			(2.49)**					(2.86)***				
DAUGHTERDEATH				0.09				0.09				
				(1.37)				(1.42)				
SIBLINGDEATH					-0.06							
					(-2.54)**							
BROTHERDEATH						-0.06		-0.10				
						(-2.40)**		(-2.95)***				
SISTERDEATH							-0.08	-0.05				
							(-2.85)***	(-1.28)				
WIDOW										-0.57		-0.57
										(-6.72)***		(-6.79)***
WIDOW x YEAR00										-0.18		-0.18
										(-1.73)*		(-1.75)*
SEXRATIO											0.97	1.54
											(3.53)***	(4.46)***
SEXRATIO x YEAR00											-0.41	-0.70
											(-1.20)	(-1.65)*
Constant	-4.12	-3.66	-3.57	-3.63	-4.10	-4.10	-4.09	-3.63	-2.91	-2.89	-3.82	-4.33
	(-58.26)***	(-45.09)***	(-36.85)***	(-38.51)***	(-57.32)***	(-57.06)***	(-56.50)***	(-44.69)***	(-35.38)***	(-35.91)***	(-14.00)***	(-12.57)***
AIC	20062.01	11333.88	9444.63	9479.31	19732.25	18883.06	18834.80	11131.71	36023.65	21120.44	36005.02	21091.19
BIC	20192.54	11455.10	9562.46	9597.05	19869.74	19019.62	18971.29	11271.70	36240.36	21329.21	36237.20	21313.88
Chi-square	3828.48	2567.98	2037.11	2060.77	3765.80	3574.23	3624.20	2543.14	3640.48	3933.68	3657.28	3964.26
Pseudo Log-Likelihood	-10013.01	-5647.94	-4703.31	-4720.65	-9847.13	-9422.53	-9398.40	-5543.85	-17983.83	-10530.22	-17972.51	-10513.60
Sq. correlation coefficient	0.38	0.32	0.30	0.33	0.38	0.38	0.38	0.32	0.25	0.16	0.25	0.16
Observations	10421	4358	3648	3630	10260	9772	9736	4287	16972	7777	16972	7777
RDHS data from	RDHS 2000	RDHS 2000	RDHS 2000	RDHS 2000	RDHS 2000	RDHS 2000	RDHS 2000	RDHS 1992 & 2000	RDHS 1992 & 2000	RDHS 1992 & 2000	RDHS 1992 & 2000	RDHS 1992 & 2000
Sample	All women of age 15-49 years	Women whose first child was born before genocide	Women whose first son was born before genocide	Women whose first daughter was born before genocide	Women whose oldest sibling was born before genocide	Women whose oldest brother was born before genocide	Women whose oldest sister was born before genocide	Women whose first child and whose oldest sibling were born before genocide	All women of age 15-49 years	Women who had first marriage before the genocide	All women of age 15-49 years	Women who had first marriage before the genocide

Note: Robust and cluster-level t-statistics in brackets with * p<0.1, ** p<0.05, *** p<0.01. Sample: women of age 15-49 years at the time of survey collection (sample is further adjusted to match each conflict proxy). OLD is the reference category in age groups. Region fixed effects included. Models 9-12 include additional interaction terms between YEAR00 and age group, education, children previously born (not shown).

Table 6: Determinants of number of children born after genocide in long term (Poisson regression)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
YOUNG	-0.18	1.21		1.17	-0.18	-0.16	-0.17	1.32	-0.53	-13.64	-0.56	-13.27
	(-5.04)***	(27.63)***		(23.52)***	(-4.93)***	(-4.24)***	(-4.57)***	(24.83)***	(-10.29)***	(-13.54)***	(-10.68)***	(-13.15)***
MIDDLE	0.41	0.44	0.42	0.43	0.41	0.41	0.41	0.43	0.35	0.42	0.31	0.36
	(15.73)***	(15.27)***	(11.57)***	(12.32)***	(15.64)***	(15.23)***	(15.17)***	(15.08)***	(11.29)***	(14.78)***	(10.23)***	(12.71)***
SONNUMLONG	-0.09	-0.11	-0.12	-0.12	-0.09	-0.09	-0.09	-0.11	-0.06	-0.07	-0.05	-0.06
	(-10.84)***	(-12.34)***	(-10.47)***	(-11.49)***	(-10.82)***	(-10.55)***	(-10.51)***	(-12.35)***	(-7.65)***	(-8.21)***	(-6.88)***	(-6.87)***
DAUGHTNUMLONG	-0.06	-0.09	-0.09	-0.09	-0.06	-0.06	-0.06	-0.09	-0.06	-0.07	-0.06	-0.07
	(-7.52)***	(-9.88)***	(-9.25)***	(-9.00)***	(-7.28)***	(-6.88)***	(-7.22)***	(-9.58)***	(-6.60)***	(-7.94)***	(-6.05)***	(-7.04)***
PRIMEDU	-0.03	0.00	0.00	-0.01	-0.03	-0.02	-0.02	0.00	-0.07	0.00	-0.08	-0.01
	(-1.63)	(0.12)	(0.11)	(-0.20)	(-1.47)	(-1.29)	(-1.11)	(0.17)	(-3.98)***	(0.02)	(-4.31)***	(-0.48)
SECEDU	-0.14	-0.10	-0.11	-0.11	-0.13	-0.13	-0.12	-0.09	-0.41	-0.20	-0.42	-0.21
	(-3.99)***	(-1.47)	(-1.45)	(-1.46)	(-3.82)***	(-3.70)***	(-3.59)***	(-1.34)	(-7.18)***	(-2.82)***	(-7.46)***	(-2.99)***
INUNION	1.08	0.78	0.81	0.78	1.08	1.07	1.06	0.78				
	(36.88)***	(23.44)***	(20.45)***	(21.54)***	(36.89)***	(36.23)***	(35.47)***	(23.31)***				
UNIONNUM	0.03	-0.06	-0.05	-0.04	0.03	0.04	0.03	-0.06	-0.01	-0.14	-0.01	-0.14
	(1.31)	(-2.01)**	(-1.46)	(-1.22)	(1.38)	(1.56)	(1.28)	(-1.94)*	(-0.66)	(-6.65)***	(-0.73)	(-6.77)***
WEALTHINDEX	-0.04	-0.02	-0.02	-0.03	-0.04	-0.04	-0.04	-0.02	-0.02	-0.00	-0.02	-0.01
	(-5.30)***	(-1.82)*	(-1.52)	(-2.04)**	(-5.27)***	(-5.09)***	(-5.17)***	(-1.88)*	(-3.41)***	(-0.51)	(-3.32)***	(-0.60)
NATIVE	-0.14	-0.03	-0.02	-0.03	-0.14	-0.14	-0.14	-0.03	-0.35	-0.04	-0.35	-0.04
	(-6.71)***	(-1.01)	(-0.56)	(-0.98)	(-6.84)***	(-6.78)***	(-6.67)***	(-1.21)	(-15.76)***	(-1.83)*	(-15.85)***	(-1.75)*
URBAN	0.03	0.01	0.03	-0.00	0.04	0.04	0.04	0.02	-0.05	-0.04	-0.05	-0.04
	(1.24)	(0.31)	(0.65)	(-0.02)	(1.36)	(1.46)	(1.29)	(0.48)	(-2.00)**	(-1.25)	(-2.03)**	(-1.23)
DCHILDMORT	0.04	-0.16	-0.31	-0.28	0.06	0.05	-0.02	-0.12	0.09	0.00	0.07	-0.04
	(0.23)	(-0.71)	(-1.18)	(-1.10)	(0.35)	(0.30)	(-0.11)	(-0.54)	(0.72)	(0.02)	(0.58)	(-0.30)
POPDENSITY	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(2.08)**	(1.64)	(1.63)	(1.82)*	(2.16)**	(2.51)**	(2.28)**	(1.51)	(2.18)**	(1.86)*	(3.06)***	(3.42)***
YEAR05									-0.28	-0.19	-0.25	-0.12
									(-5.48)***	(-3.55)***	(-4.88)***	(-2.15)**
CHILDDEATH		0.12										
		(2.84)***										
SONDEATH			0.13					0.17				
			(2.11)**					(2.91)***				
DAUGHTERDEATH				0.07				0.06				
				(1.01)				(0.98)				
SIBLINGDEATH					-0.00							
					(-0.21)							
BROTHERDEATH						0.01		0.02				
						(0.46)		(0.64)				
SISTERDEATH							-0.02	-0.12				
							(-1.04)	(-3.43)***				
WIDOW										-0.30		-0.30
										(-5.71)***		(-5.80)***
WIDOW x YEAR05										-0.40		-0.39
										(-5.42)***		(-5.41)***
SEXRATIO											0.98	1.63
											(4.42)***	(5.35)***
SEXRATIO x YEAR05											-0.61	-0.82
											(-2.37)**	(-2.33)**
Constant	-3.17	-2.78	-2.77	-2.76	-3.18	-3.19	-3.16	-2.77	-2.01	-2.01	-2.91	-3.54
	(-57.14)***	(-38.42)***	(-33.05)***	(-33.70)***	(-56.53)***	(-56.08)***	(-55.33)***	(-38.72)***	(-35.56)***	(-33.29)***	(-13.46)***	(-11.95)***
AIC	27540.10	11871.68	9599.27	9744.91	27199.54	26123.24	25982.44	11707.91	48304.59	22314.18	48271.94	22257.40
BIC	27672.12	11988.24	9705.93	9857.81	27338.62	26261.28	26120.36	11842.62	48522.73	22508.55	48505.67	22465.17
Chi-square	5767.92	20383.17	1388.66	17410.18	5704.09	5425.11	5325.66	20409.20	5225.13	3401.27	5232.66	3377.23
Pseudo log-likelihood	-13752.05	-5916.84	-4781.63	-4853.45	-13580.77	-13042.62	-12972.22	-5831.95	-24124.29	-11128.09	-24105.97	-11097.70
Sq. correlation coefficient	0.28	0.28	0.28	0.28	0.28	0.29	0.29	0.25	0.16	0.15	0.15	0.16
Observations	11320	3411	2768	2814	11161	10564	10495	3372	17871	6017	17871	6017
RDHS data from	2005	2005	2005	2005	2005	2005	2005	1992 & 2005	1992 & 2005	1992 & 2005	1992 & 2005	1992 & 2005
Sample	All women of age 15-49 years	Women whose first child was born before genocide	Women whose first son was born before genocide	Women whose first daughter was born before genocide	Women whose oldest sibling was born before genocide	Women whose oldest brother was born before genocide	Women whose oldest sister was born before genocide	Women whose first child and whose oldest sibling were born before genocide	All women of age 15-49 years	Women who had first marriage before the genocide	All women of age 15-49 years	Women who had first marriage before the genocide

Note: Robust and cluster-level t-statistics in brackets with * p<0.1, ** p<0.05, *** p<0.01. Sample: women of age 15-49 years at the time of survey collection (sample is further adjusted to match each conflict proxy). OLD is the reference category in age groups. Region fixed effects included. Models 9-12 include additional interaction terms between YEAR05 and age group, education, children previously born (not shown).

Table 7: Determinants of number of children born after genocide in youngest age group (Poisson regression)

	Short-term analysis				
	Dependent variable: CHILDBORNSHORT				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
SIBLINGDEATH	-0.04 (-0.79)				
BROTHERDEATH		-0.04 (-0.62)		-0.01 (-0.16)	
SISTERDEATH			-0.12 (-1.87)*	-0.11 (-1.63)	
SEXRATIO					-0.68 (-0.49)
SEXRATIO x YEAR00					-1.00 (-0.65)
AIC	4508.07	4341.52	4324.19	4508.84	8477.56
BIC	4623.86	4456.39	4438.93	4631.07	8671.04
Chi-square	1891.52	1851.95	1782.29	2166.88	2247.27
Pseudo Log-Likelihood	-2236.03	-2152.76	-2144.10	-2235.42	-4210.78
Sq. correlation coefficient	0.01	0.01	0.01	0.05	0.01
Observations	4596	4368	4335	4596	7406
RDHS data from	2000	2000	2000	1992 & 2000	1992 & 2000
Sample	Young women whose oldest sibling was born before genocide	Young women whose oldest brother was born before genocide	Young women whose oldest sister was born before genocide	Young women whose oldest sibling were born before genocide	All young women

	Long-term analysis				
	Dependent variable: CHILDBORNLONG				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
SIBLINGDEATH	0.02 (0.35)				
BROTHERDEATH		0.01 (0.16)		0.01 (0.17)	
SISTERDEATH			0.06 (1.06)	0.07 (1.17)	
SEXRATIO					-0.63 (-0.46)
SEXRATIO x YEAR05					-1.57 (-1.01)
AIC	4947.74	4704.90	4716.74	4948.67	9085.19
BIC	5058.08	4813.87	4825.70	5065.51	9265.82
Chi-square	2151.71	2008.40	1999.45	2160.21	3661.60
Pseudo Log-Likelihood	-2456.87	-2335.45	-2341.37	-2456.34	-4516.59
Sq. correlation coefficient	0.01	0.01	0.01	0.01	0.01
Observations	4870	4492	4489	4870	7688
RDHS data from	2005	2005	2005	1992 & 2005	1992 & 2005
Sample	Young women whose oldest sibling was born before genocide	Young women whose oldest brother was born before genocide	Young women whose oldest sister was born before genocide	Young women whose oldest sibling were born before genocide	All young women

Note: Robust and cluster-level t-statistics in brackets with * p<0.1, ** p<0.05, *** p<0.01. Only coefficients of conflict proxies are shown. CHILDDEATH, SONDEATH, DAUGHTERDEATH and WIDOW were not included as conflict proxies given that there was very little variation in these variables. The same set of socio-economic and region fixed effects as in the original model is used, but now also AGE is included. Sample: women of age 15-24 years at the time of survey collection (sample is further adjusted to match each conflict proxy).

Table 8: Determinants of number of children born after genocide in middle age group (Poisson regression)

	Short-term analysis									
	Dependent variable: CHILDBORNSHORT									
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
CHILDDEATH	0.11 (2.00)**									
SONDEATH		0.07 (0.95)					0.12 (1.78)*			
DAUGHTERDEATH			0.03 (0.30)				0.07 (0.85)			
SIBLINGDEATH				-0.02 (-0.86)						
BROTHERDEATH					-0.03 (-0.94)		-0.05 (-1.14)			
SISTERDEATH						-0.06 (-1.57)	-0.03 (-0.65)			
WIDOW								-0.50 (-4.31)***		-0.50 (-4.28)***
WIDOW x YEAR00								0.01 (0.04)		0.01 (0.05)
SEXRATIO									-0.55 (-1.27)	-0.79 (-1.97)**
SEXRATIO x YEAR00									1.13 (2.08)**	1.22 (2.23)**
AIC	4129.85	2836.06	2881.29	7510.39	7218.84	7197.71	4071.99	8765.83	13636.63	8767.11
BIC	4225.54	2924.91	2970.16	7617.41	7325.15	7303.95	4183.33	8935.05	13818.89	8948.42
Chi-square	273.42	170.12	215.41	605.84	588.59	590.95	274.27	606.98	513.99	614.50
Pseudo Log-Likelihood	-2046.93	-1400.03	-1422.65	-3737.19	-3591.42	-3580.85	-2015.00	-4354.92	-6790.31	-4353.55
Sq. correlation coefficient	0.43	0.43	0.42	0.44	0.44	0.44	0.43	0.15	0.29	0.15
Observations	1504	1029	1030	2823	2713	2704	1483	3114	4961	3114
RDHS data from	2000	2000	2000	2000	2000	2000	2000	1992 & 2000	1992 & 2000	1992 & 2000
Sample	Middle age women whose first child was born before genocide	Middle age women whose first son was born before genocide	Middle age women whose first daughter was born before genocide	Middle age women whose oldest sibling was born before genocide	Middle age women whose oldest brother was born before genocide	Middle age women whose oldest sister was born before genocide	Middle age women whose first child and whose oldest sibling were born before genocide	Middle age women who had first marriage before the genocide	All middle age women	Middle age women who had first marriage before the genocide

	Long-term analysis									
	Dependent variable: CHILDBORNLONG									
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
CHILDDEATH	0.11 (2.03)**									
SONDEATH		0.15 (2.04)**					0.14 (2.12)**			
DAUGHTERDEATH			0.08 (1.06)				0.04 (0.48)			
SIBLINGDEATH				0.01 (0.40)						
BROTHERDEATH					0.00 (0.00)		0.07 (1.35)			
SISTERDEATH						-0.00 (-0.04)	-0.08 (-1.12)			
WIDOW								-0.18 (-2.64)***		-0.18 (-2.66)***
WIDOW x YEAR05								-0.41 (-2.65)***		-0.41 (-2.61)***
SEXRATIO									-0.28 (-0.82)	-0.88 (-2.60)***
SEXRATIO x YEAR05									0.60 (1.39)	1.05 (2.02)**
AIC	2024.11	1137.70	1272.72	10585.40	10193.52	10086.40	1996.33	5370.59	18630.51	5371.93
BIC	2102.52	1205.70	1342.92	10694.47	10301.93	10194.58	2087.55	5519.03	18814.65	5530.97
Chi-square	148.78	118.91	109.25	690.77	650.29	646.63	148.87	311.94	470.14	324.70
Pseudo Log-Likelihood	-994.06	-550.85	-618.36	-5274.70	-5078.76	-5025.20	-977.17	-2657.30	-9287.25	-2655.97
Sq. correlation coefficient	0.46	0.39	0.45	0.42	0.41	0.42	0.46	0.17	0.26	0.15
Observations	576	323	365	3163	3050	3012	569	1482	5305	1482
RDHS data from	2005	2005	2005	2005	2005	2005	2005	1992 & 2005	1992 & 2005	1992 & 2005
Sample	Middle age women whose first child was born before genocide	Middle age women whose first son was born before genocide	Middle age women whose first daughter was born before genocide	Middle age women whose oldest sibling was born before genocide	Middle age women whose oldest brother was born before genocide	Middle age women whose oldest sister was born before genocide	Middle age women whose first child and whose oldest sibling were born before genocide	Middle age women who had first marriage before the genocide	All middle age women	Middle age women who had first marriage before the genocide

Note: Robust and cluster-level t-statistics in brackets with * p<0.1, ** p<0.05, *** p<0.01. Only coefficients of conflict proxies are shown. The same set of socio-economic and region fixed effects as in the original model is used, but now also AGE is included. Sample: women of age 25-34 years at the time of survey collection (sample is further adjusted to match each conflict proxy).

Table 9: Determinants of number of children born after genocide in oldest age group (Poisson regression)

	Short-term analysis									
	Dependent variable: CHILDBORNSHORT									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CHILDDEATH	0.09									
	(1.61)									
SONDEATH		0.11					0.12			
		(1.61)					(1.62)			
DAUGHTERDEATH			0.10				0.09			
			(1.10)				(1.04)			
SIBLINGDEATH				-0.05						
				(-1.38)						
BROTHERDEATH					-0.09		-0.09			
					(-2.00)**		(-2.00)**			
SISTERDEATH						-0.02	0.00			
						(-0.40)	(0.02)			
WIDOW								-0.46		-0.46
								(-4.88)***		(-4.91)***
WIDOW x YEAR00								-0.31		-0.30
								(-2.60)***		(-2.59)***
SEXRATIO									0.83	0.83
									(1.61)	(1.63)
SEXRATIO x YEAR00									-0.59	-0.69
									(-0.95)	(-1.16)
AIC	6432.93	5964.26	5939.59	6496.12	6163.75	6165.18	6330.53	10626.01	11070.35	10627.60
BIC	6539.81	6069.75	6044.92	6603.26	6269.90	6271.38	6454.87	10805.47	11250.53	10819.86
Chi-square	1287.19	1214.30	1192.72	1276.93	1208.41	1227.63	1260.54	1827.70	1604.99	1823.98
Pseudo Log-Likelihood	-3198.46	-2964.13	-2951.79	-3230.06	-3063.87	-3064.59	-3144.27	-5285.01	-5507.18	-5283.80
Sq. correlation coefficient	0.08	0.08	0.08	0.09	0.08	0.09	0.09	0.05	0.04	0.04
Observations	2802	2593	2570	2841	2691	2697	2754	4487	4605	4487
RDHS data from	2000	2000	2000	2000	2000	2000	2000	1992 & 2000	1992 & 2000	1992 & 2000
Sample	Old women whose first child was born before genocide	Old women whose first son was born before genocide	Old women whose first daughter was born before genocide	Old women whose oldest sibling was born before genocide	Old women whose oldest brother was born before genocide	Old women whose oldest sister was born before genocide	Old women whose first child and whose oldest sibling were born before genocide	Old women who had first marriage before the genocide	All old women	Old women who had first marriage before the genocide

	Long-term analysis									
	Dependent variable: CHILDBORNLONG									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CHILDDEATH	0.10									
	(2.17)**									
SONDEATH		0.11					0.15			
		(1.80)*					(2.46)**			
DAUGHTERDEATH			0.03				0.03			
			(0.43)				(0.43)			
SIBLINGDEATH				-0.03						
				(-1.17)						
BROTHERDEATH					0.02		0.03			
					(0.70)		(1.08)			
SISTERDEATH						-0.08	-0.10			
						(-2.53)**	(-3.02)***			
WIDOW								-0.29		-0.29
								(-5.41)***		(-5.43)***
WIDOW x YEAR05								-0.32		-0.31
								(-4.29)***		(-4.22)***
SEXRATIO									0.79	0.74
									(2.48)**	(2.32)**
SEXRATIO x YEAR05									0.11	0.05
									(0.28)	(0.14)
AIC	9132.32	7882.16	7902.25	10045.80	9686.50	9630.45	16959.23	15581.14	16938.83	15565.42
BIC	9239.41	7986.59	8006.70	10154.67	9794.75	9738.52	17128.03	15760.88	17120.62	15758.00
Chi-square	2028.85	1764.72	1722.89	2137.50	2086.08	2017.83	2160.72	2367.31	2151.02	2359.73
Pseudo Log-Likelihood	-4548.16	-3923.08	-3933.12	-5004.90	-4825.25	-4797.22	-8453.61	-7762.57	-8441.41	-7752.71
Sq. correlation coefficient	0.10	0.10	0.10	0.09	0.09	0.08	0.11	0.06	0.06	0.05
Observations	2834	2445	2448	3128	3022	2994	2802	4534	4878	4534
RDHS data from	2005	2005	2005	2005	2005	2005	2005	1992 & 2005	1992 & 2005	1992 & 2005
Sample	Old women whose first child was born before genocide	Old women whose first son was born before genocide	Old women whose first daughter was born before genocide	Old women whose oldest sibling was born before genocide	Old women whose oldest brother was born before genocide	Old women whose oldest sister was born before genocide	Old women whose first child and whose oldest sibling were born before genocide	Old women who had first marriage before the genocide	All old women	Old women who had first marriage before the genocide

Note: Robust and cluster-level t-statistics in brackets with * p<0.1, ** p<0.05, *** p<0.01. Only coefficients of conflict proxies are shown. The same set of socio-economic and region fixed effects as in the original model is used, but now also AGE is included. Sample: women of age 35-49 years at the time of survey collection (sample is further adjusted to match each conflict proxy).

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