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From missing brothers to educated sisters: The effects of victimization during the Rwandan genocide

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ABSTRACT

How do civil conflicts affect female empowerment? I study the effects of household-level victimization during the 1994 genocide in Rwanda on the human capital of surviving children. Identification stems from differential mortality rates by age groups and sex. I construct a Bartik-style instrument for household-level victimization based on whether the siblings of a child are likely to be targeted during the genocide due to their demographic characteristics. Victimization leads to a large increase in schooling, especially for surviving girls relative to surviving boys. Victimization mostly takes the form of losing male siblings. These results can be explained by the impact of relief programs and by increased parental investments in the human capital of surviving children following the loss of a child. This paper underscores the role of education in empowering female survivors, highlighting that rapid reconstruction of educational infrastructure and targeted scholarship programs were essential in fostering female educational gains and mitigating the long-term impacts of victimization.

1. Introduction

Civil conflicts and interstate wars have profoundly influenced the advancement of women's rights in various countries. In the United States and the United Kingdom, World War I and World War II not only catalyzed economic empowerment but also enhanced political engagement among women (Acemoglu et al., 2004; Goldin, 1991; Pugh, 1992). Similarly, in Sub-Saharan Africa, countries emerging from conflicts have witnessed some of the most dramatic increases in women's political representation in recent decades (UN Women, 2012). The reasons why conflicts provide a window of change for women's empowerment are multiple. Among other channels, conflicts create a vacuum on the labor market that opens the door for increased female labor force participation, they disrupt entrenched social orders, and facilitate political renewals that can further empower women (Hughes & Tripp, 2015; Webster et al., 2019).

In this paper, I argue that conflicts change the societal role of women through intra-household reallocation of resources. The heavy toll of war on human lives often results in children losing their siblings, prompting shifts in resource distribution within families. This leads to greater investments in the human capital of surviving siblings, and disproportionately enhance the education levels of women.¹ This is especially the case when women's empowerment is actively promoted in the aftermath of a conflict, as is increasingly the case (Bell & O'Rourke, 2010).

I empirically test this hypothesis in the context of the 1994 Rwandan genocide, investigating how household-level victimization affected the human capital of surviving children.² In less than four months, thousands of Tutsis were systematically slaughtered across the country, along with moderate Hutu and political opponents to the extremist Hutu faction in power. In total, around 0.5–1 million individuals died during the genocide (Prunier, 1996; Verpoorten, 2005). As a result, one of the main ways through which individuals were victimized was through the loss of relatives, and through the loss of siblings in particular.³ The sheer scale of the genocide and its relative brevity allow me to use Rwanda as a case study to understand the long-term effects of victimization. I show that children from targeted families in 1994 became significantly more educated, with a particularly strong

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¹ Large mortality shocks had growth-enhancing effects in multiple settings, including the AIDS epidemic (Young, 2005), the Black Death (Voigtländer & Voth, 2013a, 2013b), the Holocaust (Acemoglu et al., 2011) or mass displacement (Chaney & Hornbeck, 2016).

² Relatedly, several studies have documented the detrimental effect on education of the genocide in Rwanda (Akresh & de Walque, 2008; Guariso & Verpoorten, 2018; La Mattina, 2018). The genocide in Rwanda also had negative effects on other socio-economic outcomes such as malnutrition (Akresh et al., 2011), economic outcomes (Hodler, 2019; Justino & Verwimp, 2013; Lopez & Wodon, 2005; Serneels & Verpoorten, 2015), or marriage market outcomes of women (Jayaraman et al., 2009; La Mattina, 2017).

³ More generally, the loss of siblings is a common source of victimization during civil conflicts. In multiple conflict-affected countries, the share of individuals losing at least one sibling before turning 25 exceeds 50% (Smith-Greenaway & Weitzman, 2020).

effect on women, and that sibling loss plays a key role in this outcome.⁴

The genocide profoundly disrupted the Rwandan society as a whole. Schooling in particular was heavily impacted: the destruction of schools and changes in the returns to schooling are just a few examples of the many ways education was impacted.⁵ Household-level victimization alone does not capture the full extent of the genocide's impact on education; it represents just one form of suffering in a conflict that devastated the entire society. Children who lost siblings and those who did not were often neighbors, and both groups were severely affected by the genocide's overall consequences. Nonetheless, household-level victimization brought an additional burden to surviving children. This paper focuses on this particular dimension of the conflict.⁶

To explore the impact on education of household-level victimization during the 1994 Rwandan genocide, I build on multiple waves of the Rwandan Demographic and Health Survey (DHS). Crucially, for female respondents only, the DHS provides detailed information on all everborn siblings, which is crucial for identifying the number of siblings lost in 1994, one of the main forms of household-level victimization.⁷

The empirical analysis has two parts. The first part focuses on more than 40,000 female respondents who were born by 1994 and for whom I have detailed information on siblings characteristics. I link their household-level victimization, measured by the number of siblings lost in 1994, to their education. The second part focuses on women living with their parents at the interview time, and uses relationships to household head to identify interviewed brothers. This allows me to use a household fixed effect model to assess differential victimization effects on surviving girls and boys.

To generate plausible exogenous variation in household-level victimization, which is proxied by the number of siblings lost in 1994, I rely on patterns of excess mortality in 1994 compared to previous years (de Walque & Verwimp, 2010). While excess mortality was low and constant for young children in 1994, it increased with age for teenagers and young adults. Finally, it remained high and stable for those over 25–30 years old. This pattern is more pronounced for men relative to women.

The intuition behind the empirical strategy boils down to the following observations. Consider two women who are similar in all dimensions except for the age of their unique brother: the first woman has an older brother by five years, while the second woman has a younger brother by five years. The only situation in which we should expect the first woman to have a higher probability of losing her brother during the genocide is if she is a teenager. On the contrary, having an older or a younger brother by five years should not matter for a woman who was a young child or an adult at the onset of the genocide. This example suggest that the family structure of a household *at the beginning of the genocide* generates plausibly exogenous variations in the likelihood of the household to be targeted.

More formally, I use the number of lost siblings in 1994 as my main measure of household-level victimization, and instrument it by the expected number of siblings lost based on age, sex, and province. For each demographic group (defined by age, sex, and province), I compute a leave-one-out excess mortality rate for 1994. I then construct a Bartik instrument by taking the inner product between the number of siblings in each demographic group and group-specific excess mortality rates. This gives an expected sibling loss variable based on the age and sex profile of the siblings, which strongly predicts the actual number of lost siblings. Crucially, I always control for family structure aspects like the respondent's rank in the family, the number of older siblings, or birth spacing.

The validity of the IV approach hinges on the assumption that mortality shocks affect a woman's education solely through the channel of victimization (Borusyak et al., 2022, 2024). I present three pieces of evidence in support of this assumption. First, victimization does not affect the education of women who were too old for school in 1994, suggesting that mortality shocks operate only through the hypothesized channel. Second, victimization does not impact pre-determined variables (pre-genocide fertility, height for adults in 1994, or age at marriage for women already married by 1994), lending further support to the validity of the IV strategy. Lastly, a placebo test in Burundi a country similar to Rwanda but without a genocide in 1994 — shows the empirical strategy does not apply there. Together, these findings validate the use of the expected number of siblings lost in 1994 as an instrument for household-level victimization.

The main result of the paper is that victimization significantly enhances the human capital of surviving women. Using the instrumental variable strategy, victimization is found to increase years of education substantially. This effect is driven by women under 12 years old in 1994, whose education trajectory could still be improved. Estimates show that for these women, the loss of a sibling results in nearly two additional years of schooling, a 15 p.p. increase in literacy, and a 20 p.p. increase in the probability of finishing high school.

Does victimization affect surviving sisters differently than brothers? Using household structures, I identify brothers of women younger than 18 and unmarried at the interview time. Victimization significantly increases the education of surviving sisters but has a small, insignificant effect on brothers. A household fixed effect model confirms that sisters become significantly more educated than their brothers after victimization, with an 11 p.p. larger increase in the probability of finishing primary school for sisters.

What does victimization mean in this context? The instrument used is strongly correlated with the number of siblings lost in 1994. However, targeting due to siblings' demographic characteristics could lead to other forms of victimization. For example, losing a sibling is correlated with becoming an orphan, though not with mental or physical disabilities. Thus, while victimization is measured by the number of lost siblings, it may also capture effects of orphanhood, a possibility explored in the mechanism section.

Two mechanisms likely explain the findings. First, the results align with the quantity–quality framework of fertility. Parents who lost a child in 1994 chose between having more children (replacement effect) or investing more in surviving children's education. Evidence shows women became more educated after losing a sibling only if no additional sibling was born post-genocide, indicating a tradeoff. Additionally, I argue that the age of the youngest child of a family in 1994 proxies the relative costs of having another child post-1994, and show that lower relative costs led to stronger replacement effects and weaker educational gains.

Second, relief programs may also explain the observed patterns. After 1994, many young genocide survivors received scholarships. If victimization determined scholarship eligibility, the observed results might reflect this. In order to understand the extent to which relief programs explain the increased education of surviving children, I use the fact that those programs are targeting Tutsi survivors. I show that for my results to be solely driven by Tutsi survivors, I would need an effect of scholarships on education that is unrealistically large. I therefore conclude that while relief programs probably play an important role to explain my results — especially among orphans –, they are unlikely to

⁴ Prior studies document correlations between diverse socio-economic outcomes and the loss of household members during the Rwandan genocide — though not necessarily siblings (Justino & Verwimp, 2013; Verpoorten & Berlage, 2007). Specifically, Kraehnert et al. (2019) report a replacement effect when parents lose a child and a negative correlation between sibling loss and post-genocide fertility of surviving children.

⁵ For more detailed analyses of the evolution of education in pre- and post-genocide Rwanda, see for example McLean Hilker (2011) or Pells et al. (2014).

⁶ For qualitative insights into the experiences of survivors of the Rwandan genocide, see for example Hatzfeld (2000) or Dumas (2020).

 $^{^7\,}$ Around 16% of DHS respondents report having lost at least one sibling in 1994.

explain more than half of the size of the effects on education that I am documenting.

The increased education of surviving children mitigated the adverse well-being effects of victimization. While victimization significantly reduced the wealth of surviving children, this negative impact was mainly on those too old to return to school after the genocide. Younger children, who could resume schooling, benefited from increased educational investments, partly offsetting the negative wealth effects.

This paper contributes to several strands of the literature. First, it contributes to the literature on the effects of conflicts on female empowerment. While wars promote women's empowerment by inducing them to enter the job market (Acemoglu et al., 2004; Boehnke & Gay, 2022; Brodeur & Kattan, 2022; Goldin, 1991; Goldin & Olivetti, 2013), creating a power vacuum (Rogall & Zárate-Barrera, 2020), and advancing political participation (Arnsbarger & Ferrara, 2023), they can also hurt women by worsening the sex ratio on the marriage market (Abramitzky et al., 2011; La Mattina, 2017). I contribute to this literature by documenting how victimization at the household level produces changes in intra-household allocation of resources that end up promoting women's empowerment in the long-run.

Second, I contribute to the literature on violence and the demand for education. A large literature documents how exposure to violence affects education (Bertoni et al., 2018; Brück et al., 2019; León, 2012; Shemyakina, 2011). In particular, violence has been shown to affect the demand for education through multiple mechanisms, including poverty (Akresh & de Walque, 2008; Shemyakina, 2011), changes in beliefs (Alfano & Görlach, 2023; Koppensteiner & Menezes, 2021; Prem et al., 2023), changes in preferences (Becker et al., 2020), or reduced mobility (Melnikov et al., 2020; Miaari & Lee, 2021).⁸ Closely related to this paper, La Mattina (2017) and Ogasawara and Komura (2022) highlight how conflict-induced changes in women's bargaining power within their couple affect investments in human capital. I show that civil conflicts profoundly reshape the structure of households and as a result lead to important changes in human capital investments.

Third, this is the first study to document the causal effect that losing a sibling during a civil conflict has on the human capital of surviving children. Even though the death of a relative is so widespread during civil conflicts, surprisingly little is known about how such dramatic events influence children.⁹ Notable exceptions include Dupraz and Ferrara (2023) and Kovac (2017), who focus on the loss of a father during a conflict. Relatedly, Ito et al. (2024) study how the loss of family members, friends, or neighbors influence schooling.

Finally, I contribute to the empirical literature on the quantityquality framework of fertility. While a large body of empirical work tests for the relevance of the trade-off in numerous settings, it remains unclear why the trade-off is more potent in some settings than others. Moreover, evidence supporting the existence of this trade-off in Sub-Saharan Africa remains limited (Alidou & Verpoorten, 2019).¹⁰ I contribute to this literature by providing evidence supporting the key predictions of the quantity–quality tradeoff in a SSA setting, complementing studies such as Burlando and Bbaale (2022) or Bove et al. (2024).

2. Mortality patterns during the 1994 genocide

According to the 1991 census, the last one before the 1994 events, Rwanda had a population of around 8 million individuals, of which 85% was Hutu, and 12% was Tutsi. After independence from Belgium in 1962, political tensions and ethnic violence led to waves of Tutsi refugees, especially to Uganda, where the Rwandan Patriotic Front (RPF) was formed in 1987–8.¹¹ The RPF invaded Rwanda in 1990, marking the beginning of a civil conflict that lasted until the 1993 Arusha peace agreement.

On April 6, 1994, the plane of Juvénal Habyarimana, then President of Rwanda, was shot down in the surroundings of Kigali. On that same day, an extremist Hutu faction took power and executed a plan that had been cautiously prepared during the last few years. During less than four months, from April to July 1994, thousands of Tutsi were systematically killed across the country, together with moderate Hutu and political opponents to the extremist Hutu faction that took power right after the airplane crash. In July 1994, the RPF stopped the genocide and seized power. Around 0.5–0.8 million Tutsi and 6–60 thousand Hutus are believed to have died during the genocide, in addition to soldiers from the RPF and the Rwandan Armed Forces (Prunier, 1996; Verpoorten, 2005, 2020).

The genocide generated three key mortality patterns: a sharp increase in mortality in 1994, higher excess mortality rates for young adults and males, and geographic variations in the intensity of the genocide. The Rwandan Demographic and Health Survey (DHS), covering over ten thousand households per wave, allows to illustrate these patterns since it contains the birth and death history of all female respondents' siblings. Indeed, for each of their ever-born siblings, respondents were asked for their sex, birth date, and date death date (if applicable).

Figure A.1 illustrates the sudden increase in death toll during the genocide. Never before 1994 had Rwanda faced such a stark increase in mortality. While the number of siblings passing away steadily increases from the 1960s until the late 1990s, the number of lost siblings abruptly increases in 1994.¹² After 1994, the yearly number of siblings passing away immediately came back to levels similar to the pre-genocide trend.

Mortality patterns in 1994 differed significantly by age and sex compared to previous years. Fig. 1 shows the yearly mortality rate among siblings for three periods: five years before the genocide (1989-1993), the genocide year (1994), and five years after (1995-1999). Before the genocide, mortality rate was highest for new-borns and quickly stabilizes below 0.02 as children aged. A similar pattern emerges for the five years following the genocide, although infant mortality rate is slightly higher than pre-genocide. However, mortality rate patterns look completely different in 1994: strikingly, it increases with age from adolescence to adulthood. This can be seen even more clearly when plotting the excess mortality rate in 1994 relative to a normal year, as is done in Fig. 2. While the excess probability of dying during the genocide compared to a typical year is constant and below 0.05 for children below 12, it steadily increases with age between 12 and the mid-30s. After that, the excess mortality rate stabilizes itself at high levels. This pattern is also more pronounced for men than for women.¹³

Finally, geographic variations in excess mortality were significant, with mortality rates depending on factors like the pre-genocide Tutsi

⁸ See Justino (2016) for a review of the different ways through which civil conflicts affect the supply and demand for education. See Blattman and Miguel (2010) and Verwimp et al. (2018) for more general reviews of the literature on the consequences of civil conflicts.

⁹ In non-conflict settings, human capital has been shown to be negatively affected by the loss of siblings (Fanos & Nickerson, 1991; Fletcher et al., 2013; Machajewski & Kronk, 2013; Thamarapani et al., 2018).

¹⁰ See Clarke (2018) for a review of this literature.

¹¹ See Des Forges (1999), Mamdani (2001), Prunier (1996), Strauss (2006) for more details about the history of the conflict.

¹² The slight increase in mortality in 1993 contradicts previous accounts of mortality rates in Rwanda in the years leading to the genocide. A possible explanation might be that some respondents could be living in Burundi in 1993 and might have lost siblings at that moment. For that reason, I use 1992 as a baseline year instead of 1993.

¹³ This gender difference in mortality rates is also observed in the 2002 Rwandan census (La Mattina, 2017).



Fig. 1. Yearly mortality rates in Rwanda by age: before, during, and after the genocide.

Notes: These figures represent yearly mortality rates in the five years prior to the genocide (1989–1993), the year of the genocide (1994), and the 5 years following the genocide (1995–1999). The estimates are calculated using the sibling records of the five Rwandan DHS waves. 95% confidence intervals are represented in black.

population, proximity to Kigali, propaganda effectiveness, ease of access for militias, the refugee crisis, and counter-insurgency (Rogall, 2021; Verpoorten, 2012; Yanagizawa-Drott, 2014). Figure A.2 illustrates this point by representing the excess mortality rate by age and sex in the twelve Rwandan provinces. Consistent with the findings of Verpoorten (2010) using Gacaca court records, the figure shows large excess mortality rates in provinces where the genocide was particularly intense.¹⁴

3. Data

The main data source is the 2000, 2005, 2010, 2014–15, and 2019–20 waves of the Rwanda Demographic and Health Survey (DHS). All waves except for the first one are geocoded, which allow me to

associate each respondent to a sector, district, and province. I drop all individuals not born when the genocide started. The analysis primarily focuses on surviving sisters, as DHS provides detailed information on their ever-born siblings. Men are not asked about all siblings' birth and death history, so I can only observe men's siblings in 1994 if matched to an interviewed sister.

Sisters. The main explanatory variable is the number of siblings a woman lost during the 1994 genocide. Interviewed women provide the birth history of all their ever-born siblings and, if deceased, the date of death. This data reveals how many brothers and sisters each woman lost in 1994 and their ages at that time. Additionally, I use this data to capture the sex and age of siblings alive before the genocide, the spacing between their births, and the number of siblings born after 1994.¹⁵

The main outcome variable is the number of years of schooling. I complement this with dummy variables for whether a respondent is literate, finished primary school, or secondary school. The main proxy

¹⁴ The only region for which I find an excess mortality pattern that is not in line with the findings of Verpoorten (2010) is Cyangugu, in the western part of the country. While Verpoorten (2010) finds out that the genocide was relatively intense in Cyangugu, I do not find high excess mortality rates for any age group or sex in that province.

¹⁵ Recall bias is discussed in Section 7.



Fig. 2. Excess Mortality Rate in 1994 vs 1992 (By Sex). Notes: This figure represents the excess mortality rate in 1994 compared to the baseline year (1992) by age and sex.

for well-being is a wealth decile based on DHS wealth index. Finally, I also look at the height-to-age z-score of respondents and their fertility (the number of children born before and after 1994). The DHS surveys also collect information on the disabilities of respondents, which I use to construct indicators for whether they suffer from mental health issues, physical disabilities, or any kind of issues. Lastly, I construct a proxy for orphanhood for women who were younger than 18 at the moment of the interview. I categorize a woman as orphan if she's not the daughter of the household head.

While DHS provides many informative variables about women, it has significant limitations. First, it lacks information on ethnicity. Second, it provides no data on parents, including whether they died during the genocide or their socio-economic status — important predictors of targeting during the genocide. Third, it omits migration data, despite the large-scale displacement caused by the genocide. Finally, it lacks information on women's mental health, likely affected by victimization.

In Table 1, I provide summary statistics for the main variables of interest. The women in my sample were at most 49 years old at the interview time, with DHS waves conducted at least 11 years postgenocide, making the average woman about 14 years old in 1994. Typically, they had slightly more than 2 brothers and 2 sisters before the genocide and lost around 0.18 brothers and 0.11 sisters in 1994. Over 17% of the women lost at least one sibling, suggesting many were Hutu, as the pre-genocide Tutsi population was 10%–12% with a 75% mortality rate among Tutsis. Over 5% lost 2 or more siblings, and around 2% lost 3 or more. On average, women spent 4.4 years in school and married at age 21, with husbands around 25.

Brothers. Enumerators interview all women aged 15–49 in visited households but only interview men in half of the sampled households. When interviewed, men are not asked about the birth and death history of their siblings. To address this, I select male respondents likely living with their sister and use her reported sibling history instead. For each male respondent, I consider as sisters all women who (1) live in the same household, (2) are under 18, and (3) have a relationship to the household head consistent with being a sibling.¹⁶ Moreover, I only keep

male respondents under 18 at the time of the interview (to avoid selfselection due to the marriage market) and who were born before 1994. This procedure matches 4027 men to a sister. For these men, I use the same family characteristics and construct demographic and education variables as for their sisters.

4. Empirical strategy

4.1. OLS specification

The aim of this paper is to examine how household-level victimization, as measured by the loss of siblings during the 1994 genocide, could influence the education of surviving women. To tackle this question, I am interested in estimating the equation

$$y_{i,d} = \beta_1 LostSiblings_{i,d} + \beta_2 X_{i,d} + \delta_d + \epsilon_{i,d}$$
(1)

where $y_{i,d}$ is the education of woman *i* living in district *d*, *LostSiblings*_{*i*,*d*} is the number of siblings lost by woman *i* in 1994, and $X_{i,d}$ is a vector of individual controls including age, age square, altitude, a rural/urban dummy, the number of brothers, the number of sisters, religion, and year of interview fixed effects. I also include district fixed effects δ_d .

The estimate of interest, β_1 , is likely biased for several reasons. First, the specification presented above suffers from reverse causality. Indeed, more educated and wealthier individuals were targeted during the genocide, causing an upward bias. Simultaneously, these individuals had more opportunities to seek help or refuge, leading to a downward bias. Additionally, poorer individuals might have been more impacted by the genocide, also causing a downward bias.

Second, this specification also suffers from an omitted variable bias. The DHS data in Rwanda lack information on ethnicity, likely biasing estimates since Tutsi, who were targeted during the genocide, were generally wealthier and more educated than Hutu, causing an upward bias. Conversely, women with weaker social networks may have lost more siblings and faced poorer economic opportunities, leading to a downward bias. Thus, interpreting β_1 is challenging. To address this, I use an instrumental variable approach.

¹⁶ Relationships to household head are consistent if: both the male respondent and the female respondent are the children of the household head, both are his grandchildren, both are siblings to the head, or one is the household head and the other is his/her sibling.

4.2. Instrumental variable approach

In order to generate plausibly exogenous variations in householdlevel victimization, I exploit three features of the genocide: the specific age and sex patterns of excess mortality, the sharp increase in mortality in 1994, and geographic variations in the intensity of the genocide. Taken together, these three features of the genocide allow me to generate variations in the number of siblings lost by a woman in 1994.

For each woman, I associate an expected number of siblings who would be expected to pass away because of the genocide based on the province she lives in, and the age–sex profile of her siblings.¹⁷ Denoting $P_p(age_s, sex_s)$ the excess probability for sibling *s* of dying in 1994 compared to 1992 in province p_i ¹⁸ and S_i the set of siblings of woman *i*, the expected number of lost siblings for woman *i* is defined by

$$E[LostSiblings]_{i,p} = \sum_{s \in S_i} P_p(age_s, sex_s).$$

Hence, within a given province, the expected number of lost siblings in 1994 is the sum of the probability of losing each sibling based on his age and sex.

To see the intuition behind this instrument variable, consider two comparable women who are both 10 years old in 1994. The first one has a brother who is 5 years old, while the second one has a brother who is 15 years old. We should expect the second woman to have a higher probability of losing a sibling than the first one. Now, consider that same pair of women, but suppose that instead of being 10 years old at the time of the genocide, they are 5 years old. The first one would thus have a new-born brother, while the second one would have a 10 years old brother. In this scenario, we should expect those two women to have similar probabilities to lose a sibling, since excess mortality remains constant for children younger than 12. This is also the case if both women are 40 years old, since excess mortality remains constant for adults. Finally, suppose two identical women who are both 10 years old in 1994 and both have a brother who is 15 years old. However, suppose that only the first woman lives in a province where the genocide had a high intensity. We should here expect the first woman to have a higher probability of losing a sibling than the second one

Therefore, the IV equation I estimate is

$$y_{i,d} = \beta_1 LostSiblings_{i,d} + \beta_2 X_{i,d} + \delta_d + \epsilon_{i,d}$$
⁽²⁾

where *LostSiblings*_{*i,d*} is instrumented by $E[LostSiblings]_{i,p}$.¹⁹ In addition to the set of individual controls that are already included in the OLS regression (Eq. (1)), the set of controls $X_{i,d}$ also consists of family structure controls (the number of older brothers, the number of older sisters, the rank of woman *i* in her family, and the average space in months between the birthdates of children in *i*'s family).

Identification in this context relies on the quasi-exogeneity of excess mortality shocks (Borusyak et al., 2022). Indeed, excess mortality rates should not systematically differ for siblings of children who were on different educational trajectories before 1994. Importantly, the timing of the genocide plays a critical role: had it occurred several years earlier or later, the children most affected would have been different. Although this assumption cannot be directly tested, Section 5.2 and Section 8.5 provide multiple pieces of evidence that excess mortality shocks are indeed exogenous.

Table 1	
Descriptive	Statistics.

	(1) N. Obs	(2) Mean	(3) SD	(4) Min	(5) Max
Individual Characteristics					
Age in 1994	41085	13.71	8.98	0	39
Education (years)	41085	4.65	3.72	0	22
Wealth decile	41085	5.45	2.78	1	10
Height-age z-score	20550	-1.14	1.07	-5.9	5.7
Catholic	41085	0.41	0.49	0	1
Protestant	41085	0.55	0.50	0	1
Geographic Characteristics					
Urban	41085	0.22	0.41	0	1
Altitude	41085	1720.37	292.11	951	3095
Family Characteristics					
N. of sisters before 1994	41085	2.31	1.55	0	10
N. of brothers before 1994	41085	2.25	1.52	0	12
Rank in family	41085	2.45	2.08	0	14
N. of older sisters before 1994	41085	1.25	1.32	0	9
N. of older brothers before 1994	41085	1.20	1.28	0	9
Avg space between births (months)	41085	40.19	22.62	0	488
N. of siblings born after 1994	41085	0.69	1.31	0	10
Mortality in 1994					
N. siblings lost in 1994	41085	0.28	0.80	0	11
N. brothers lost in 1994	41085	0.17	0.55	0	9
N. sisters lost in 1994	41085	0.10	0.41	0	6
Lost at least 1 sibling in 1994	41085	0.17	0.37	0	1
Lost at least 2 siblings in 1994	41085	0.06	0.23	0	1
Lost at least 3 siblings in 1994	41085	0.03	0.16	0	1

Notes: Descriptive statistics of the main outcome, dependent, control, and mortality variables used in the analysis.

4.3. Gender analysis

Are surviving brothers and sisters impacted differently by victimization?²⁰ The DHS surveys include both male and female respondents, but only females are asked about the birth and death history of all siblings, so the main analysis focuses on women. However, both genders report their relationship to the household head, allowing me to identify brothers of women who provided sibling histories. For unmarried female respondents under 18, I check if a likely brother was also interviewed in the same household and use the sibling data from the sister for her brother.

Because I need to restrict the sample to women who are both unmarried and younger than 18 at the time of the interview, I lose a large number of observations. To increase the sample size, I include the 2000 wave of the Rwandan DHS. The main disadvantage of doing so is that the 2000 wave was not geocoded, which means that I cannot include district fixed effects (which I replace by DHS cluster fixed effects), and I cannot use geographic variations when calculating the mortality rates used for the instrument.

To test whether surviving brothers and sisters are impacted differently by victimization, I start by estimating the same IV model with two endogenous variables: the number of lost siblings, and the number of lost siblings interacted with a dummy variable taking a value of 1 if the respondent *i* living in household *h* and DHS cluster *c* is a woman ($Female_{i,h,c}$). The instruments used are the expected number of lost siblings, and the interaction between the female dummy and the expected number of lost siblings. The resulting equation is as follows:

$$y_{i,h,c} = \gamma_1 Lost Siblings_{i,h,c} + \gamma_2 (Lost Siblings_{i,h,c} \times Female_{i,h,c}) + \gamma_3 X_{i,h,c} + \delta_c + \epsilon_{i,h,c}$$
(3)

¹⁷ Although the genocide lead to massive displacement of populations (Verwimp & Van Bavel, 2005), I am not able to take that into account since the Rwandan DHS waves I am using do not contain any information about migration history.

¹⁸ This excess probability at the province level also depends on the sector a woman is from since I use leave-one-out mortality rates. For the sake of clarity, I do not add any subscripts for sectors.

 $^{^{19}}$ The province subscript *p* is included to indicate that excess mortality shocks are calculated at the province level. For simplicity, this subscript is omitted from other variables, as they do not vary at the provincial level.

 $^{^{20}}$ See Buvinić et al. (2014) for a discussion of the factors influencing the gender differential impacts of conflicts on schooling.

To ensure that I compare women to their brothers in particular, I also estimate a household fixed effect model. Denoting $y_{i,h}$ the outcomes of individual *i* in household *h*, I estimate:

$$y_{i,h} = \gamma_1(LostSiblings_{i,h} \times Female_{i,h}) + \gamma_2 X_{i,h} + \delta_h + \epsilon_{i,h}$$
(4)

where *LostSiblings*_{*i*,*h*} is again instrumented by $E[LostSiblings]_{i,p}$. The vector of individual controls $X_{i,h}$ includes the sex, age, and age-square. All characteristics of that are constant within households are absorbed by household fixed effects δ_h .

5. Household-level victimization and human capital

In this section, I start by documenting a strong and positive association between education and the number of lost siblings in 1994, the main proxy in this paper for victimization. I then estimate Eq. (2), where the number of siblings a woman lost in 1994 is instrumented by the expected number of siblings she should lose based on the province she lives in, and the age and sex profile of her siblings. Victimization during the genocide leads women to become significantly more educated, and this effect is solely driven by women who were still sufficiently young to go to school after the genocide. I then examine whether we should fear that the exclusion restriction might be violated, and show that my results are stable when controlling for some key characteristics of family structure.

5.1. OLS results

The first column of Table 2 shows a strong positive correlation between education and the number of siblings a woman lost in 1994, controlling for the number of brothers and sisters. This association remains highly significant when additional individual characteristics are included (column 2) and when controlling for geographic factors like cluster altitude and an urban dummy (column 3). Although the coefficient decreases in size, this is expected since the genocide was more intense in urban areas (de Walque & Verwimp, 2010), where educational levels are typically higher. Finally, the result holds and remains significant when district fixed effects are included (column 4), though the coefficient further decreases. This is unsurprising, as the genocide was particularly severe in more developed regions, such as Kigali. In the final specification, the loss of a sibling in 1994 is associated with a 0.1-year increase in schooling.

As noted in the previous section, the direction of bias in this coefficient is ambiguous. In a survey of households affected by the genocide, André and Platteau (1998) identify two main categories of individuals who died during the 1994 conflict: those with relatively large landholdings (mostly older adults) and land-poor, malnourished individuals (primarily young children).²¹ Since my sample includes only women under 40 in 1994, with the average participant being relatively young, it is likely that many of the siblings who passed away fall into the second category. If this is the case, the coefficients reported in the final column of Table 2 are likely downward biased.

5.2. IV results

To approach a causal interpretation of the relationship between victimization and education, I turn to the IV results, presented in the first column of Table 3. The bottom part of the table reports the first-stage estimates, indicating that the expected number of lost siblings is a strong predictor of the actual number of lost siblings in 1994. The

Table 2
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Dependent variable:	Years of schooling						
	(1)	(2)	(3)	(4)			
N. lost siblings	0.229***	0.270***	0.133***	0.098***			
	(0.034)	(0.035)	(0.026)	(0.027)			
Individual Controls		1	1	1			
Geographic Controls			1	1			
District FE				1			
Mean of Dep. Var.	4.65	4.65	4.65	4.65			
Adj R ²	0.00	0.06	0.16	0.19			
Observations	41085	41085	41085	41085			

Note: This table reports OLS estimates for the relationship between the number of years of education and the number of siblings a woman lost in 1994. The unit of observation is the respondent (all women), and only respondents who were born before the genocide are included in the sample. The dependent variable is the number of years of schooling at the time of the interview, while the explanatory variable of interest is the number of siblings that a woman lost in 1994. All columns include controls for the number of brothers and sisters before the genocide. The additional individual controls used in columns 2–4 are age, age², the year of interview, religions. In column 3 and 4, the additional geographic controls include the altitude and a dummy taking a value of 1 if the respondent lives in an urban area. Column 4 further includes district fixed effects. Standard errors are presented in parenthesis and are clustered at the district level.

coefficient, slightly above one, reflects the use of excess mortality rather than the actual mortality rate in 1994 (where a coefficient of one would be expected).²² The upper part of the table presents the IV estimates, revealing that the loss of one sibling results in slightly less than an additional year of schooling (column 1).

Several observations can be made about this result. First, the positive effect on years of schooling is strikingly large, given that the women in the sample average only 4.5 years of education. Second, the results suggest that the OLS estimate was downward biased. This aligns with the argument in the previous subsection that the relatively young composition of the sample likely gives more weight to poor and malnourished individuals, compared to wealthier, land-owning individuals, leading to a downward bias in the OLS estimate. Third, these findings highlight that victimization, as proxied by sibling loss, has fundamentally different implications for education compared to losing a parent during conflict or the loss of a sibling to non-violent causes.

We could worry that family characteristics driving variations in the instrument might influence education independently of sibling loss in 1994. To address this, I add controls for key family characteristics. Column 2 controls for a woman's rank in her family, as parents might invest more in the education of their first child or receive help from older siblings to finance younger siblings' education (Alidou & Verpoorten, 2019; Kessler, 1991). Column 3 includes the number of older brothers, and column 4 includes the number of older sisters, as these might affect educational outcomes. Column 5 controls for the average spacing between sibling births, reflecting different parental socio-economic characteristics. Column 6 includes all these controls simultaneously. The inclusion of these controls does not significantly affect the IV estimates. In Section 7, I show that the baseline result is robust to additional controls, including interactions between time trends and family characteristics.²³

Given the high mortality rate during the genocide, sample selection could threaten identification. The IV estimate might be upward biased

²¹ In their sample, 32 out of 596 individuals died during the conflict. The first category includes 10 individuals (7 of whom were over 50), while the second category includes 11 individuals (8 of whom were children). The remaining victims were either perceived troublemakers or militia-involved youth. See Table 16 in Andr'e and Platteau (1998) for more details.

²² To assess monotonicity, Figure A.3 shows a semiparametric first stage, confirming a monotonically increasing relationship between the instrument and the number of lost siblings. Figure A.4 further shows that the first-stage coefficient remains stable across different subsamples.

²³ One might be concerned that the presence of older siblings correlates with parental age in 1994. To address this, I test whether the estimates remain stable when controlling directly for the age of each sibling a respondent has. As shown in column 4 of Table A.12, this adjustment does not significantly alter the estimated effect of sibling loss on education.

Table 3			
Impact of	Victimization	on	Education.

1							
Dependent variable:	Years of sch	Years of schooling					
	(1)	(2)	(3)	(4)	(5)	(6)	
IV Estimates							
N. lost siblings	0.807***	0.904***	0.968***	0.796***	0.821***	0.920***	
	(0.171)	(0.205)	(0.211)	(0.176)	(0.170)	(0.212)	
Observations	41085	41085	41085	41085	41085	41085	
Mean of Dep. Var.	4.65	4.65	4.65	4.65	4.65	4.65	
Kleibergen–Paap F-Statistic	195.87	134.07	144.41	163.94	195.24	131.11	
Test of Endogeneity, p-val	0.00	0.00	0.00	0.00	0.00	0.00	
First Stage							
E[N. lost siblings]	1.110***	1.135***	1.109***	1.135***	1.111***	1.127***	
	(0.079)	(0.098)	(0.092)	(0.089)	(0.080)	(0.098)	
Observations	41085	41085	41085	41085	41085	41085	
Adj R ²	0.10	0.10	0.10	0.10	0.10	0.10	
District FE	1	1	1	1	1	1	
Rank in Family		1				~	
N. Older Brothers			1			1	
N. Older Sisters				1		1	
Avg. Space					1	1	

Note: The upper part of the table presents the IV estimates for the effect of losing siblings in 1994 on the education of women, while the bottom part of the table presents the results of the first stage. The dependent variable is the number of years of schooling at the time of the interview. The explanatory variable of interest is the number of siblings a woman lost in 1994, which is instrumented by the expected number of siblings lost in 1994 based on the age and sex profile of a woman's sibling. All the regressions include controls for age, age square, religion, number of brothers and sisters before the genocide, urban/rural area, altitude, year of interview, and district fixed effects. In columns 2-5, I sequentially add family controls (woman's rank in her family, the number of older brothers and sisters she has, and the average space between the births of her siblings) before using all of them in column 6. Standard errors are clustered at the district level.

Table 4

Impact of Victimization on Education (by Age)

Dependent variable:	Years of schooling					
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A - Y	ounger than 1	2 in 1994			
N. lost siblings	1.814***	1.705***	1.972***	1.647***	2.205***	1.993***
	(0.422)	(0.438)	(0.468)	(0.408)	(0.430)	(0.453)
Observations	19171	19171	19171	19171	19171	19171
Mean of Dep. Var.	5.04	5.04	5.04	5.04	5.04	5.04
Kleibergen–Paap F-Statistic	81.85	89.32	76.79	90.54	78.23	82.47
	Panel B - O	lder than 12 i	n 1994			
N. lost siblings	-0.174	-0.347	-0.228	-0.293	-0.174	-0.319
	(0.181)	(0.230)	(0.237)	(0.195)	(0.181)	(0.241)
Observations	21914	21914	21914	21914	21914	21914
Mean of Dep. Var.	4.31	4.31	4.31	4.31	4.31	4.31
Kleibergen–Paap F-Statistic	114.40	55.09	61.83	82.43	114.39	52.01
District FE	1	1	1	1	1	1
Geographic Controls	1	1	1	1	1	1
Individual Controls	1	1	1	1	1	1
Family Controls	No	Rank	Brothers	Sisters	Space	All
H0: $\beta_{PanelA} = \beta_{PanelB}$, <i>p</i> -value	0.00	0.00	0.00	0.00	0.00	0.00

Note: The specifications are the same as in Table 3. Only the IV estimates are presented. Panel A includes all women who were younger than 12 in 1994, while Panel B includes all women who were at least 12 in 1994.

if victimized women survived due to unobservables correlated with education (e.g., ability, political connections, or social networks). As a sanity check, I test if the positive effect on education is driven solely by women younger than 12 in 1994.²⁴ If there were a systematic bias, we would expect a positive effect of losing siblings on the education of women too old to attend school after 1994. Conversely, if effects were driven only by women young enough to attend school in 1994, it would suggest no systematic bias.

Table 4 shows that women younger than 12 in 1994 drive the education results. Panel A indicates that victimization leads to nearly 2 additional years of schooling for these women, while Panel B shows no significant effect for women older than 12. These results are robust

to additional family characteristic controls. For the rest of the paper, I use the specification from column 6 as the baseline.

Strikingly, the loss of a sibling had a profound effect on literacy and on higher education (Table A.1). For women younger than 12 in 1994, victimization does not significantly affect finishing primary school (column 1) but increases the probability of finishing secondary school by 20 percentage points (column 2) and tertiary school by 5.7 percentage points (column 3). This results in a 15 percentage point increase in literacy (column 4). For women older than 12 in 1994, sibling loss does not affect any of these variables.²⁵

I examine whether losing a brother affects education differently than losing a sister. Table A.3 reports estimates with the number of lost brothers (column 1) and lost sisters (column 2) as separate endogenous variables, instrumented by their respective expected losses. While the coefficient for lost sisters in column 2 is slightly larger than

²⁴ I choose 12 as the cutoff since children are supposed to finish primary school at 12 and that only a small minority of women go beyond primary school.

²⁵ Reduced form results are shown in Table A.2.

Table 5

Impact of Victimization on Education (by Age)

Dependent variable:	Years of schooling		Primary (some)	Primary (complete)	Secondary (complete)	
	(1)	(2)	(3)	(4)	(5)	(6)
N. lost siblings	1.127**	0.444				
	(0.511)	(0.642)				
N. lost siblings \times female		0.943**	0.954***	0.110**	0.164**	0.043
		(0.392)	(0.331)	(0.049)	(0.068)	(0.053)
Observations	12426	12426	10857	10857	10857	10857
Mean of Dep. Var.	3.50	3.50	3.34	0.89	0.18	0.10
Kleibergen–Paap F-Statistic	30.41	15.01	53.20	53.20	53.20	53.20
Individual Controls	1	1	1	1	1	1
Family Structure Controls	1	1				
Cluster FE	1	1				
Household FE			1	1	1	1

Note: This table investigates whether the effects of losing siblings on education differ for surviving girls and boys. Column 1 reports estimates for the effect of losing siblings on education using the baseline regression, with district fixed effects being replaced by DHS cluster fixed effects. Column 2 reports estimates when using two endogenous variables (the number of lost siblings, and the interaction between the number of lost siblings and whether the respondent is female or not). The remaining columns report estimates of a household fixed effect model. Standard errors are clustered at the DHS cluster level.

Table 6

Victimization and Wealth.

Dependent variable:	Wealth decile			
Sample	All (1)	Younger than 12 (2)	Older than 12 (3)	
N. lost siblings	-0.284***	-0.006	-0.369***	
Equality test	(0.108)	(0.281) (0.142) p = 0.25		
Observations	41101	19178	21923	
Mean of Dep. Var. Kleibergen–Paan F-Statistic	5.45 133.48	5.53 83.16	5.37 53.80	
District FE	✓	✓	✓	
Geographic Controls	1	1	1	
Individual Controls	1	1	1	
Family Controls	1	1	1	

Note: The dependent variable is the respondent's wealth decile. While the first column includes all respondents, only women younger than 12 in 1994 are included in column 2, and only women older than 12 in 1994 are included in column 3. The specifications are the same as in column 6 of Table 3. For columns 2–4, only women who have been married only once are included.

that for lost brothers in column 1, this difference may partly arise because the effect of losing a brother is partially captured by the sister variable. Column 3 addresses this by including both variables in a single regression, confirming that the effects of losing a brother or a sister are economically similar, with no statistically significant difference. Combined with the higher mortality rates among brothers during the 1994 genocide (Section 2), these findings suggest a redistribution of resources from lost siblings — mainly brothers — to surviving sisters, highlighting the gendered dynamics of resource allocation after such a devastating event.

5.3. Gender analysis

Does victimization impact surviving boys and surviving daughters differently?

Table 5 reports results from two strategies to answer this question. I start with the baseline methodology with two endogenous variables: the number of lost siblings, and the number of lost siblings interacted with the gender of the respondent (Eq. (3)). Next, I use a household fixed effect framework to ensure I compare surviving boys and girls from the same household (Eq. (4)).

Victimization significantly increases years of education (column 1), with the effect largely driven by women (column 2). Specifically, victimization results in 0.9 additional years of schooling for women compared to men. A household fixed effects framework yields similar findings, confirming that women become significantly more educated

than their surviving brothers (column 3). Compared to their brothers, women become also 11% more likely to attend at least some primary school (column 4), 16% more likely to complete primary school (column 5), and 4% more likely to finish secondary school, though the latter result is not statistically significant.

These results confirm that household-level victimization has a strong impact on education. Moreover, this increase in education is especially strong for surviving women, as opposed to surviving men.

5.4. Household-level victimization and well-being

Does the increase in human capital following victimization help women cope with the consequences of their loss? To examine this, I replicate Table 3 using a wealth index (ranging from 1 for the poorest decile to 10 for the richest) as the outcome variable (Table 6). Losing siblings has a significant negative effect on wealth, associated with a 0.3-decile drop in the wealth distribution (column 1). This effect is largely driven by women older than 12 in 1994. For women young enough to return to school after the genocide, the decline in wealth is a non-significant 0.06 deciles (column 2). In contrast, for women older than 12, the drop exceeds 0.4 deciles (column 3).²⁶ These results highlight the severe negative impact of victimization on survivors' wellbeing.²⁷ However, education emerged as an effective coping strategy for some women, mitigating at least part of the material consequences of grief.

6. Mechanisms

Why does household-level victimization, and the loss of siblings in particular, have such a strong effect on education for surviving women? In this section, I discuss two mechanisms that could play a role. First, I argue that the observed increase in education is consistent with a quantity–quality framework of fertility. Parents have the choice between two main coping strategies — replacing the lost children, or increasing their investments in the human capital of the surviving ones –, and their decision depends partly on the cost of education relative

²⁶ Figure A.5 explores the components driving this effect. Significant reductions are observed for access to electricity, ownership of a television, and type of wall material, while only three coefficients are non-significantly positive, indicating a real decrease in wealth.

²⁷ Fig. 3 provides similar regressions using dummy variables for each decile of the wealth distribution. Victimization increases the likelihood of being in the poorest deciles and decreases the likelihood of being in the wealthiest deciles.



Fig. 3. Effect of victimization on wealth distribution.

Notes: This figure represents the results from IV regressions for the effect of sibling loss on wealth. Each row presents the IV coefficient for the number of lost siblings of a separate regression. The dependent variable is a dummy taking a value of one if the respondent is part of the indicated decile in the distribution of the wealth index. 95% confidence intervals are indicated in green. All regressions include individual controls, family characteristic controls, and district fixed effects. Standard errors are clustered at the district level.

to having additional children. Second, I argue that public policies promoting the education of women together with scholarships distributed to genocide survivors after 1994 have also played an important role.

6.1. Quantity-quality tradeoff

Standard models of fertility choice suggest a trade-off between child quality and quantity: higher costs of raising children should lead to greater investments in education (quality) and fewer children (quantity) (Barro & Becker, 1989; Becker & Barro, 1988; Doepke & Tertilt, 2016). Existing research also shows that exposure to civil conflicts can increase fertility (Islam et al., 2016; Rotondi & Rocca, 2022), with child loss in particular triggering a significant replacement effect (Kraehnert et al., 2019). These findings suggest that families adopt one of two coping strategies after losing a child: increasing post-conflict births (replacement effect) or investing more in the education of surviving children. The choice between these strategies depends on the relative cost of raising an additional child. In the following paragraphs, I present evidence supporting this idea.

Table 7 highlights that the increase in education is primarily driven by women without a sibling born after the genocide. Specifically, losing a sibling leads to an additional half-year of education for women without a sibling born after 1994 (column 1), while there is no significant effect on the education of women with a sibling born after 1994 (column 2). The contrast is even starker for women who were under 12 in 1994. In this group, losing a sibling increases schooling by two years for those without a sibling born after 1994, but has no impact for those with at least one (columns 3 and 4). Although the decision to have additional children after 1994 is itself endogenous, these findings support the idea that, in the aftermath of the genocide, families faced a trade-off between replacing lost children and investing more in the education of surviving ones.

Table 7

Tradeoff Betwe	en Education	and Rep	lacement.
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Dependent variable:	Years of schooling				
	All women		Younger the	an 12	
	(1)	(2)	(3)	(4)	
At least 1 sibling born after 1994	No	Yes	No	Yes	
N. lost siblings	0.462*	-0.517	2.152***	-2.000	
	(0.248)	(1.505)	(0.562)	(2.389)	
Equality test	p = 0.52 p		p =	= 0.09	
Observations	28888	12197	9103	10068	
Mean of Dep. Var.	4.53	4.93	5.04	5.04	
Kleibergen–Paap F-Statistic	96.87	27.94	38.94	11.80	
District FE	1	1	1	1	
Geographic Controls	1	1	1	1	
Individual Controls	1	1	1	1	
Family Controls	1	1	1	1	

Note: In this table, I investigate whether the effects of losing a sibling on education depends on the number of siblings born after the genocide. In columns 1 and 3, I restrict the sample to women who do not have any siblings born after 1994. In columns 2 and 4, I focus on women who have at least one sibling born after 1994. Only women who were younger than 12 in 1994 are included in columns 3 and 4. The specifications are the same as in column 6 of Table 3.

The decision to replace lost children or invest more in the education of surviving ones likely depends on the relative cost of having another child. I argue that having an additional child is less disruptive and costly when parents are already caring for young children. In contrast, when the youngest child in the household is older, adding another child entails higher costs and greater lifestyle adjustments for the parents. To capture this dynamic, I use the age of the youngest child in 1994 as a proxy for the cost of having an additional child after that year.

Table 8 shows that women in households where the youngest member was a toddler or preschooler in 1994 are significantly more likely to have a sibling born after 1994 following the loss of a sibling during

Table 8

Effects of Vie	ctimization, b	by Cost fo	r Parents	of Having	an Additional	Child.

Dependent variable:	N. siblings born after 1994			Years of schooling		
	(1)	(2)	(3)	(4)	(5)	(6)
Age of youngest family member	0-2	3-5	6-12	0-2	3-5	6-12
N. lost siblings	1.918***	2.182***	0.726**	0.206	1.895**	3.313**
	(0.537)	(0.457)	(0.339)	(1.024)	(0.917)	(1.317)
Equality test with col 1, p-value		0.70	0.07			
Equality test with col 4, p-value					0.22	0.09
Observations	10389	5894	2895	10385	5892	2894
Mean of Dep. Var.	1.77	0.97	0.23	5.24	4.88	4.64
Kleibergen–Paap F-Statistic	31.07	28.72	12.21	31.06	28.72	12.16
District FE	1	1	1	1	1	1
Geographic Controls	1	1	1	1	✓	1
Individual Controls	1	1	1	1	1	1
Family Controls	1	1	1	1	1	1

Note: The dependent variable are the number of siblings born after 1994 (columns 1–3) and the number of years of education (columns 4–6). I restrict the sample to women whose youngest family member was aged 0–2 years old in 1994 (col 1 and 4), 3–5 years old (columns 2 and 5), and 6–12 years old (columns 3 and 6). Only women who were younger than 12 in 1994 are included in the sample. All the specifications include family and individual controls, as well as district fixed effects. For columns 2 and 3, I report the p-value for the equality test with the coefficient reported in column 1. For columns 5 and 6, I report the p-value for the equality test with the coefficient reported in column 4. Standard errors are clustered at the district level.

the genocide (columns 1 and 2). This effect diminishes as the youngest child's age increases, with the coefficient dropping from over 1.8 to just 0.6 when the youngest child is of school age (column 3). These findings highlight a strong replacement effect across families, with its intensity shaped by the opportunity cost of having additional children after 1994.²⁸

The second part of Table 8 examines whether the increase in education is greater in families where replacing lost children is more costly. Victimization has no significant effect on education in households with a toddler (column 1), but it leads to a nearly 2-year increase in schooling when a preschooler is present (column 2) and over 3 years when the youngest child is of school age (column 3). These findings further support the existence of a quantity–quality tradeoff: when the opportunity cost of education is high (households with a toddler), the replacement effect is stronger, and the educational gains are smaller. Conversely, when the opportunity cost is lower (households without a toddler or preschooler), the increase in education is more pronounced.²⁹

The sample splits in Tables 7 and 8 are endogenous, so definitive conclusions should be avoided. However, the results from these tables provide suggestive evidence consistent with standard models of fertility choice. They support the notion that, following the genocide, parents who lost a child faced two coping strategies: replacing the lost child or investing more in the human capital of the surviving ones. Parents with a lower relative cost of education were more likely to choose the latter, while those with a higher relative cost were more inclined toward the former.

6.2. Relief programs

Relief programs likely played a significant role in the substantial increase in education following the loss of a child. By 1998, the FARG (Fonds d'Assistance aux Rescapés du Génocide), one of the largest relief programs for genocide survivors, was providing secondary school scholarships to 24,000 students—about 40% of (Tutsi) genocide survivors of school age (Guariso & Verpoorten, 2018). If the FARG used victimization as a criterion for scholarship eligibility, the observed increase in education may partly reflect the impact of government aid.³⁰

Despite its importance, the FARG program is nonetheless unlikely to explain alone the large increase in education documented in Section 5. To see this, I suppose that the entire positive effect on education is driven by individuals who received a scholarship, and I estimate the implied effect of receiving a scholarship. In order to do so, I suppose that all the Tutsi who lost at least one sibling received a scholarship should then be equal to the estimated effect on education of losing at least one sibling,³² divided by the probability for an individual to be a Tutsi survivor conditional on having lost at least one sibling.³³ The implied effect of receiving a scholarship would be an additional 28 years of schooling, which is unrealistically large.

While relief programs are likely to play an important role in explaining the main results of the paper on education, it is unlikely that scholarships could explain more than half of the size of the effect of losing a sibling on education that is documented in the previous sections.

6.3. Alternative mechanisms

In this section, I review some alternative mechanisms that could also explain my results, but for which I do not find any supporting evidence.

²⁸ The opportunity cost of childbearing may also depend on labor market opportunities (Bove et al., 2024). Table A.4 supports this by showing that the replacement effect is smaller for wealthier households (columns 1 and 2), likely due to better labor market prospects. To address potential endogeneity in wealth measures, columns 3 and 4 use pre-1994 birth spacing as an alternative proxy, as shorter birth intervals are strongly correlated with lower household wealth (Singh et al., 2024; Tesema et al., 2023). This confirms that the replacement effect is stronger for poorer households.

 $^{^{29}}$ Table A.5 and Table A.6 replicate these analyses while controlling for wealth.

³⁰ For more on the outcomes and limitations of the FARG, as well as broader support for survivors, see Viebach (2023) and Uwamaliya and Smith (2017). ³¹ In practice, it was difficult for Hutu survivors of schooling age to receive help from the FARG (McLean Hilker, 2011).

 $^{^{32}}$ For women who were sufficiently young to go to school in 1994, the loss of at least one sibling leads to an increase of 4.8 years in schooling (Table A.7).

³³ Assuming a mortality rate of 75% among Tutsi during the genocide and a pre-genocide share of the population that was Tutsi equal to 12%, one should expect around 3% of respondents to the DHS survey to be Tutsi survivors. Assuming that all the Tutsi survivors lost at least one sibling in 1994, and given that 17% of the women interviewed for DHS declare having lost at least one sibling in 1994, we should expect 17% of the women who declared having lost at least one sibling to be Tutsi (0.03/0.17 = 0.17).

6.3.1. Other forms of victimization

Victimization can manifest in ways beyond sibling loss. A higher likelihood of family targeting may increase the probability of orphanhood, parental job loss, or disabilities. Table A.10 tests this, showing that victimization raises the likelihood of orphanhood (proxied by relationship to the household head for women under 20 at the time of the interview) but does not significantly increase the probability of mental health issues or physical disabilities. Although victimization encompasses more than sibling loss, these mechanisms — such as orphanhood, parental job loss, or disabilities — are all likely to reduce educational attainment (Beegle et al., 2006, 2010; Di Maio & Nisticò, 2019; Kovac, 2017), and are therefore unlikely to drive the main results of the paper.

6.3.2. Preferences for education and risk aversion

Experiencing a traumatic event, such as the loss of a sibling during civil conflict, could shift preferences away from material possessions toward investments in more mobile forms of capital, such as education (Becker et al., 2020). Similarly, exposure to violence has been associated with increased risk aversion (Brown et al., 2019; Jakiela & Ozier, 2019), potentially driving greater investments in safe, transportable assets like education.

While the Rwandan DHS waves lack questions on preferences or risk aversion to directly test this mechanism, these factors alone cannot fully explain the empirical patterns observed. Specifically, if households prioritized education at the expense of material possessions, one would expect siblings who gained more education due to victimization to be worse off in terms of wealth compared to those whose education was unaffected. However, the opposite pattern emerged: the negative wealth shock diminishes for individuals who were young enough to benefit educationally. Furthermore, delayed benefits from education are unlikely to explain these results, as the wealth effects of victimization do not differ between the 2005–2010 and 2014–2015 DHS waves (Table A.15).

7. Robustness checks

In this section, I show that the results presented in Table 3 are not driven by selective migration, are robust to the use of alternative instruments and controls, pass a series of placebo tests, and that the imprisonment of genocidaires after 1994 is also unlikely to drive the results.

7.1. Selective migration

Selective migration might lead to a violation of the exclusion restriction. To check whether this is likely to be a concern for my empirical analysis, I first investigate to what extent do my results depend on the geographic variations I use in my instrument. I proceed by looking at whether my results are sensitive to dropping areas that welcomed a high share of displaced persons in the aftermath of the conflict.

7.1.1. Excluding likely migrants

Table A.9 provides evidence that selective migration is unlikely to play a role in explaining the main results of the paper. The table shows that excluding sectors in the top-quintile for the share of migrants (column 2) or the share of individuals living in an Umudugudu village (column 3) does not significantly affect the main result of the paper.³⁴ Similarly, excluding sectors that are in the top-quintile for either the share of migrants or the share of individuals living in an umudugudu does not affect the main result (column 4).³⁵

7.1.2. Excluding geographic variation from instrument

In order to understand to what extend my empirical strategy relies on geographic variations, I also replicate the last column of Table 3 using an instrument that ignores geographic variations. While column 1 of Table A.11 reports the baseline estimates, column 2 replicates this result when constructing excess mortality rates at the country level (and not at the province level). As in column 1, the first stage remains strong, and the IV estimates are qualitatively and quantitatively similar to the ones presented in column 1.

7.2. Alternative instruments

The rest of Table A.11 provides additional pieces of evidence that the main results of the paper are robust to changes in the way the instrument works. In column 3, I further exclude any variations in the instrument coming from the gender composition of siblings. I compute excess mortality rates at the country level without taking into account the sex, so that all variations in the instrument come solely from differences in the age of siblings. Again, the main results of the paper remain similar.

The last two columns of the table present the results of a robustness check where the IV does not have the structure of a Bartik instrument. Indeed, the excess mortality patterns suggest a difference-in-differences formulation of the empirical strategy. Controlling for the total number of siblings and age in 1994, having *n* or n + 1 older siblings should not make any difference for young children. However, we should expect teenagers who have more older siblings to lose more siblings in 1994. Therefore, I replicate the main analysis using the interaction between age in 1994 of the respondent and the number of older siblings the respondent had in 1994 as an instrument for the number of lost siblings. Column 4 reports results when using this empirical strategy. Although the estimates are noisier and the F-statistics dive below 10 for both Panel A and Panel B, the main results of the paper still hold. In column 5, I do the same but using only the number of brothers a woman has instead of the number of older siblings. Again, the coefficients are noisy but remain in line with the baseline results presented in column 1.

7.3. Alternative controls

I show that my results are robust to various alternative controls in Table A.12: using 5-year age-group fixed effects instead of a quadratic specification (column 2); including month-of-birth fixed effects (column 3); including 42 variables indicating the number of older siblings a woman has by x years, where x ranges from -10 to 10 (column 4); including the interaction between age in 1994 and the number of siblings, as having many siblings might affect education differently across generations (column 5); controlling for interactions between age in 1994 and other family characteristics such as the number of older brothers (column 6), older sisters (column 7), average birth spacing (column 8), and birth rank (column 9). Finally, including interactions between age in 1994 and district fixed effects confirms the robustness of my results (column 10).

7.4. Recall bias

Since DHS birth and death histories rely on self-reported information, they may be prone to misreporting and recall bias, especially if psychological trauma from victimization in 1994 causes recall bias. To test if recall bias affects the main results, I examine if respondents in heavily impacted sectors under-report siblings at risk of dying in 1994. I regress objective genocide intensity measures on the expected number of lost siblings based on their age and sex in 1994. If women omit siblings who died, we should see a negative effect of genocide intensity on the expected number of lost siblings.

Table A.13 shows the OLS estimates of the effect of sector-level genocide intensity on E[LostSiblings], controlling for individual variables and total siblings. Measures of genocide intensity include the

³⁴ Umudugudu villages were created in the aftermath of the genocide to host refugees and returnees.

³⁵ The share of migrants in a sector is defined as the share of the population living in a sector in 2002 that changed residence between 1990 and 2002 and is obtained from the 2002 Census. The share of the population living in an umudugudu village also comes from the 2002 Census.

share of Tutsi in 1991, the share of suspected perpetrators (measured by *gacaca* trial suspects), and the distance to the nearest mass grave (Verpoorten, 2010, 2012). All regressions include individual controls and district fixed effects, with the only family control being the number of ever-born siblings. None of the objective measures of genocide intensity correlate with the expected number of lost siblings.

These results confirm that women living in areas where the genocide was particularly intense were not more likely to omit mentioning siblings who were more at risk of being targeted during the genocide due to their age and sex. This suggests that the main results of the paper are unlikely to be driven by a recall bias.

7.5. Placebo tests

I now present several placebo checks and I show that my empirical strategy does not work in alternative contexts.

7.5.1. Placebo outcomes

The loss of siblings in 1994 should not affect outcomes determined before the genocide. Table A.14 shows that sibling loss in 1994 does not significantly impact the number of births before 1994 (column 1).³⁶ Column 2 shows no significant effect on the height-to-age z-score for women aged at least 18 in 1994. Column 3 shows no significant effect on the age at marriage for women already married in 1994.³⁷

7.5.2. Placebo country

I now apply my empirical strategy to women in Burundi, a country that was not impacted by the 1994 genocide but share similar social structures and norms with Rwanda. Indeed, Burundi is an ideal placebo due to its shared history, ethnic composition, climate, topography, economy, religion, language, and colonial experiences with Rwanda (Uvin, 1999). Hence, I assign Rwanda's excess mortality rates to the siblings of Burundian women, using the Burundi DHS 2010 and 2016 waves to calculate excess mortality rates.³⁸

Although Burundi also experienced high levels of ethnic conflict between Hutu and Tutsi since independence, unlike Rwanda, where the death toll spiked only from April to July 1994, Burundi saw multiple sharp increases in violence post-independence, notably in 1972 and 1993–2005.³⁹ Thus, we expect noisier and smaller coefficients in the first stage and reduced form compared to Rwanda. In Table A.16, I show that this is indeed the case.⁴⁰

7.5.3. Permutation test

Shift-share designs can over-reject the null hypothesis when regression residuals are correlated across observations with similar initial shares (Adão et al., 2019). To test for this issue, I generate 1000 placebo samples, each of them with the same number of women. The outcomes and controls are identical in all the samples. The placebo samples differ exclusively in the excess mortality rates assigned to each sibling, which are randomly permuted. Figure A.6, shows the empirical distribution of the estimates, centered around zero for both the first stage and the reduced form, with real estimates (red lines) being extreme compared to placebo estimates. Column 1 in Table A.17 shows that the average of the OLS estimates is zero for all outcomes, and column 2 reports the standard deviation of the estimated coefficients. Since the true value of β equals 0 by construction, a correctly behaved test with significance level 5% should have a 5% rejection rate. Column 3 shows that the clustered standard errors estimators do not yield to a higher rejection rate.

8. Conclusion

This paper highlights a crucial mechanism through which civil conflicts and interstate wars can lead to women's empowerment. Conflicts that have a high toll on human lives lead to an important redistribution of resources within households: resources that would have otherwise been spent in the human capital of deceased brothers are instead spent on the human capital of surviving sisters.

In doing so, this paper underscores the critical role of education as a coping mechanism for victims of civil conflicts, leading to two key policy lessons. First, the decision of Rwandan households to invest in the education of surviving children was effective largely because Rwanda swiftly rebuilt its educational infrastructure. Had the country not prioritized the rapid restoration of its schooling system, families might have resorted to alternative coping strategies, potentially limiting the long-term opportunities for conflict survivors.

Second, the Rwandan experience illustrates that even severe forms of victimization do not inevitably result in long-term educational deficits. While the successful reconstruction of the education system was crucial in meeting the heightened demand for education across the population, the extensive scholarship programs provided to genocide survivors played a significant role in propelling the country forward, demonstrating that targeted financial support can be instrumental in mitigating the potential long-term educational costs of traumatic events.

To further illustrate these points, I replicate in an extended version of this paper the Rwandan analysis in to the context of the 1975–79 genocide perpetrated by the Khmer Rouge in Cambodia.⁴¹ Unlike Rwanda, Cambodia's educational system never fully recovered from the four years of violence, during which an estimated 95% of school teachers were killed. As a result, surviving children received minimal support for education, making it particularly challenging for households to invest in their education after the genocide. Applying the same empirical strategy, I find that household-level victimization during the 1975–79 period did not result in significant educational gains for survivors. This inability to invest in the human capital of surviving children had detrimental long-term effects on their well-being, as reflected in adverse marriage market outcomes.

As a final note, one-third of individuals in developing countries lose at least one sibling before turning 25, a proportion that can exceed 50% in countries with violent conflicts (Smith-Greenaway & Weitzman, 2020). The sheer magnitudes and nature of such traumatic events are likely to have a long-term impact on economic development. Despite this, little is known about how sibling loss influences surviving children. This paper is a first step in this direction.

Declaration of competing interest

I hereby submit my manuscript entitled "From missing brothers to educated sisters: The effects of victimization during the Rwandan genocide" for publication in World Development. I declare that I have no known competing financial interests or personal relationships that could have influenced this work.

 $^{^{36}}$ To achieve more precise estimates, I restrict the sample to women who were at least 14 in 1994, the age of the youngest woman who gave birth that year.

³⁷ Since the DHS only provides age at marriage for the last union, I restrict the sample to women married only once.

 $^{^{38}}$ I use the Burundi DHS 2010 and 2016 waves to calculate the excess mortality rates at the country level.

 $^{^{39}}$ Rwanda also witnessed a spike in mortality from 1959 to 1962 is not crucial for my strategy, as less than 3% of the sampled women were born then.

⁴⁰ Using province fixed effects instead of district fixed effects, with standard errors clustered at the province level, I present confidence intervals based on the wild-cluster bootstrap due to the small number of provinces (18) (Cameron et al., 2008).

⁴¹ Results are available upon request.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.worlddev.2025.106945.

Data availability

The authors do not have permission to share data.

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