World Development 200 (2026) 107266

Contents lists available at ScienceDirect

World Development

journal homepage: www.elsevier.com/locate/worlddev

Regular research article

Genocide, women's empowerment, and intergenerational transmission of violent attitudes

Daniel Kammer[a,b,*], Alina Greiner-Filsinger[c]

[a] University of Konstanz, Konstanz, Germany
[b] ISDC - International Security and Development Center, Berlin, Germany
[c] University of Mannheim, Mannheim, Germany

HIGHLIGHTS

- Exposure to the 1994 Rwandan genocide during adolescence shapes women's attitudes and norms.
- Young women from regions more affected by genocide are less likely to justify violence against children (VAC).
- Descendants of these younger women from regions more affected by genocide are similarly less likely to develop violent attitudes.
- The effect is driven by genocide-induced women's empowerment in the post-genocide period.
- Under certain conditions, mass violence can catalyze progressive norm change across generations, consistent with the conflict–prosociality link.

ARTICLE INFO

JEL classification:
J13
D74
D79

Keywords:
Mass violence
Violent attitudes
Intergenerational transmission
Women's empowerment
Rwanda

ABSTRACT

In this paper, we explore how mass violence shapes attitudes on violence against children, and we examine how these attitudes are transmitted across generations in the context of the Rwandan genocide. We exploit spatial variation in genocide intensity from the *Gacaca* records and temporal variation in women's timing of socialization from three rounds of the Demographic and Health Surveys (DHS) in a difference-in-differences framework and find that younger women from regions more affected by genocide hold less violent attitudes compared to their peers from less-affected regions. Using an instrumental variable approach to estimate the transmission effect, we also show that descendants of these younger women from regions more affected by genocide are similarly less likely to develop violent attitudes. We provide evidence that genocide-induced women's empowerment is the underlying mechanism. As such, our findings underscore previous evidence on the conflict–prosociality link by showing that mass violence can catalyze progressive norm change across generations, but also call for a more detailed investigation of the underlying adaptation mechanisms of the second generation.

1. Introduction

Violence against children (VAC) is globally pervasive and occurs most often at home (Hoeffler, 2017).[1] The most common form of VAC is corporal punishment, physical violence used by parents to discipline their children (Cuartas, 2021).[2] Despite severe consequences for children's physical and mental health, development, or educational attainment, acceptance of corporal punishment is widespread (Baker-Henningham et al., 2009; Cuartas, 2021; Erten & Keskin, 2020; Moyo et al., 2025). Violent conflict can further contribute to the normalization and permissibility of violence, transferring violence into the home (Cesur & Sabia, 2016; Saile et al., 2014; Stark et al., 2025; Stojetz & Brück, 2023). At the same time, studies indicate lasting positive effects

* Corresponding author at: University of Konstanz, Konstanz, Germany.
Email addresses: daniel.kammer@uni-konstanz.de (D. Kammer), alina.greiner@uni-mannheim.de (A. Greiner-Filsinger).

[1] The protection of children from all forms of violence is a fundamental human right and a target of the UN's agenda for sustainable development; yet a large share of children live in countries without full legal protection against corporal punishment (UNICEF, 2014). In 130 countries, including Rwanda, corporal punishment is still lawful in the home (World Health Organization, 2025b).

[2] Throughout this paper, we use the abbreviation VAC to refer specifically to corporal punishment, i.e., parental violence against children. While this is the most common type of violence, children are subjected to many different forms (e.g., emotional, physical, sexual) by different perpetrators (e.g., peers, teachers, partners) in different contexts (see e.g., Doerr et al., 2023; Moyo et al., 2025; UNICEF, 2014).

https://doi.org/10.1016/j.worlddev.2025.107266
Accepted 23 November 2025
Available online 5 December 2025

on prosocial behavior and cooperative attitudes years or decades after conflicts have ended, highlighting a conflict-prosociality link (Bauer et al., 2016; Bellows & Miguel, 2009; Blattman, 2009).

In this paper, we ask how exposure to mass violence shapes attitudes on VAC and we examine how these attitudes are transmitted across generations in the context of the 1994 Rwandan genocide. Drastic changes in the aftermath of the Rwandan genocide, both on a micro- and macro-level, led to an unprecedented process of women's mobilization and participation (Berry, 2015, 2018; La Mattina, 2017; Nordenving et al., 2023). For example, areas most affected by genocide violence saw significant shifts in sex ratios as the majority of victims, perpetrators, and prisoners were men (La Mattina, 2017). Women filled the vacuum left by men's absence, both within the home and in society, asserted greater autonomy and influence, and, in doing so, challenged traditional norms (Berry, 2018).[3] Young women, in particular, experienced this phase of genocide-induced empowerment during their formative years, when core beliefs and attitudes are shaped (Nordenving et al., 2023).[4] Building on these findings and insights from socialization theory, we test whether young women from regions more affected by genocide were socialized into environments condemning violence, making them less likely to justify VAC. We then examine whether young women's attitude formation has cascading effects on the next generation. Specifically, we investigate whether descendants of young women from regions more affected by genocide are similarly less likely to justify VAC themselves, suggesting an intergenerational transmission of (non-)violent attitudes.

To empirically test this, we couple spatially fine-grained data on genocide intensity from the *gacaca* court records with three waves of Demographic and Health Survey (DHS). Our population of interest consists of first- and second-generation individuals (mothers and their children) who live within the same household in post-genocide Rwanda. Our identification strategy exploits two sources of variation: spatial variation in genocide intensity across communes where mothers reside in, and temporal variation in mothers' year of birth to identify young women in their formative years during the genocide. We measure genocide-induced empowerment socialization by interacting the two variables and estimate the effect on mothers' VAC attitudes using a factorial difference-in-differences with a continuous treatment (Callaway et al., 2024; Xu et al., 2024). The intuition is that young women ($\leq$ 18 years of age at the time of the genocide) from high genocide regions were socialized under different conditions than older women from the same regions and, importantly, also than their same age cohort counterparts from less-genocide regions (i.e., young women from communes less affected by genocidal violence). In a second stage, we leverage the variation in genocide intensity and socialization period as an instrumental variable for mothers' attitudes to estimate the transmission effect, i.e., the causal relationship between mothers' VAC attitudes and those of their children.

We find strong empirical support for our proposed mechanisms. Women who were 18 years of age or younger at the time of genocide and live in areas with higher levels of genocide violence are significantly less likely to perceive VAC as acceptable compared to older women from the same regions, and to women in the same age cohort from less genocide-affected regions. A one standard deviation increase in local genocide intensity decreases younger women's VAC attitudes by 7.7 percentage points. Furthermore, we show that due to the shift in their mothers' attitudes, children from these younger women in genocide affected regions are also significantly less likely to justify VAC. As such, we find strong support for a causal intergenerational transmission of non-violent attitudes. Our findings are robust across various instrument choices, such as alternative measurements of genocide intensity, and hold for different specifications of our violent attitudes variables. We further show that the effects remain stable in significance and size when we restrict our sample to children born after 1994 which supports our claim that the effect on children is not driven by direct exposure to the Rwandan genocide, but works through mother-level differences in genocide exposure. Additionally, the results are not driven by the age cutoff of 18 years that splits our sample into younger and older women. Instead, our results remain robust across various plausible age cutoff specifications for identifying women in their formative years. We also present evidence in support of our proposed young women's empowerment mechanism. We find positive effects on several proxies for women's empowerment. For instance, young women who live in former high genocide regions are significantly more likely to currently work in formal or informal employment. Importantly, these findings are not driven by selection effects, e.g., these young women could be less likely to live with a partner. Instead, we show that the results remain similar in size and magnitude for partnered and unpartnered women. Furthermore, we provide evidence that younger women in high-genocide regions are less likely to experience intimate partner violence (IPV). This suggests that women choose partners who hold similar attitudes opposing intra-family violence, including VAC. At the same time, we show that women's partners are not driving the results on children. Finally, we can rule out alternative mechanisms operating outside the household (such as foreign aid or selection into partnerships) as well as within families.

This study brings value to several strands of the literature. First, we add to research on legacies of violent shocks (Caruso, 2017; Gay, 2023; Lupu & Peisakhin, 2017). We provide clear evidence that the 1994 Rwandan genocide has a persistent effect on VAC attitudes of survivors surveyed up to 26 years later. Furthermore, we causally identify a transmission effect on VAC attitudes of the second generation. Thus, we add to the conflict-prosociality debate (Annan et al., 2011; Bauer et al., 2016; Blattman, 2009; Cavatorta et al., 2023) by identifying specific conditions under which exposure to mass violence leads to a reduction in the acceptance of violence. By focusing on VAC attitudes, an outcome previously neglected in this literature, we advance our understanding of cycles of violence in post-conflict contexts. That violence can catalyze progressive norm changes across generations is novel and perhaps surprising given the established risk of normalization of intra-family violence post-conflict (Cesur & Sabia, 2016; La Mattina, 2017; Rutayisire & Richters, 2014; Saile et al., 2014; Stojetz & Brück, 2023).

Second, our paper contributes to research on the transmission of attitudes from parents to children (Alan et al., 2017; Black et al., 2005; Campante & Yanagizawa-Drott, 2015; Dohmen et al., 2012; Fernández et al., 2004; Perales et al., 2021; Wilhelm et al., 2008). For instance, Dohmen et al. (2012) show that the transmission of risk and trust preferences from parents to children follows a pro-cyclical pattern and that socialization is an important driver in the transmission process. Similar to these studies, we find a pro-cyclical pattern of VAC attitudes between first and second generation victims of mass violence, which has not been studied in the literature. We go beyond establishing intra-family correlations, instead, we present robust causal evidence that the second generations' VAC attitudes are driven by their mothers' attitudes. Moreover, we causally identify a transmission parameter of VAC attitudes that is caused by an exogenous reduction in mothers' acceptance of VAC (similar to Erten & Keskin (2020)).

Third, we also add to the emerging literature on the nuanced consequences of women's empowerment. Our study builds upon Nordenving et al. (2023) who show that positive post-genocide outcomes for women seem to be driven by younger women. We are the first to demonstrate that conflict-induced women's empowerment leaves persistent effects

[3] Previous research has shown that this phase of women's empowerment led to higher public good provision, less domestic violence against women, and thus set examples for more prosocial and less violent behaviors (Nordenving et al., 2023; Wolbrecht & Campbell, 2007).

[4] We have adapted the term 'war-induced women's empowerment' (Lazarev, 2019; Yadav, 2021) to match our studied context. Several studies have investigated women's resilience and agency in the context of armed conflicts. This does not negate women's hardships, but centers on how women respond to them, and illustrates potentials for post-war gains in women's status, rights, and representation (see e.g., Bakken & Buhaug, 2021; Berry, 2018).

for younger women *and* their descendants. In contrast to previous studies, we look beyond immediate benefits of empowerment for children's health or education (Annan et al., 2021; Duflo, 2012). Instead, we investigate how empowerment is linked to the formation of attitudes in subsequent generations, and analyze the causal relationship between attitudes on VAC of women and their children. Items on the acceptance of corporal punishment are standard in DHS, but we are - to the best of our knowledge - the first to match DHS data across mothers and children to investigate intra-family transmission.

Fourth, while it has been established that children who grow up in violent homes are likely to become perpetrators or victims of domestic violence as adults (Ireland & Smith, 2009; Pollak, 2004), studies on mitigating factors that can help end this violent cycle are rare. Our findings underscore the positive cascading effects of empowering women and strengthening gender equal norms to prevent IPV and VAC (Erten & Keskin, 2020; Stern et al., 2022). We investigate several proxies of women's empowerment and show that younger women in high-genocide regions are less likely to experience IPV. We further find evidence in line with the positive assortative mating argument (Dohmen et al., 2012) that women choose partners who hold similar attitudes opposing intra-family violence, including VAC.

Lastly, we aim to further contribute to understanding the lasting consequences of the Rwandan genocide (Bonnier et al., 2020; Gautier, 2025; Guariso & Verpoorten, 2019; Rogall, 2021; Yanagizawa-Drott, 2014). While persistent effects on child outcomes have been studied, most focus on education (Bundervoet & Fransen, 2018; Chin et al., 2023). Our research offers novel insights into how the second generation's attitudes evolve after violent conflict, suggesting that women's empowerment may be an important mechanism driving anti-violent (or pro-social) attitude formation in the second generation. Thus, we showcase an alternative mechanism through which mass violence can strengthen pro-social norms, namely through the formation of progressive, less violent norms among younger women with important and positive implications for their children and possibly subsequent generations.

The rest of this paper is organized as follows: Section 2 introduces a brief theoretical discussion of how the genocide leads to empowerment which then impacts the second generation. Section 3 provides background information on the history of the Rwandan genocide. Sections 4 and 5 describe the data and empirical strategy respectively before in Section 6, the main results and key mechanism behind our results are presented. Section 7 discusses the results and concludes.

2. Violent conflict and attitudes on VAC

How does violent conflict shape attitudes on violence across generations? We argue that women who were exposed to greater levels of mass violence when they were 18 or younger (i.e., in their formative years when core beliefs and attitudes develop) are more likely to condemn violence within their families as mothers.[5] Our argument builds on two core assumptions. The first is that areas with higher levels of mass violence were marked by greater levels of women's mobilization and participation (Nordenving et al., 2023). The second is that witnessing violence-induced women's empowerment acts as role model mechanism for younger women who are more likely to internalize these changes and form attitudes opposing violence. As such, we expect greater women's empowerment socialization effects in areas that were more affected by genocide violence. Furthermore, we argue that the positive, empowering effect on younger women exposed to higher levels of mass violence has cascading effects on their children. Empowered women who are socialized into condemning violence transmit these attitudes to their children. In sum, we propose that mass violence indirectly reduces the second generations' proclivity for violence through their mothers' socialization into condemning violence.[6]

2.1. Mass violence and women's empowerment

The first part of our argument is that higher levels of mass violence lead to women's empowerment, which leads to role model effects for younger women. Recent research has illustrated how armed conflict can lead to processes that are beneficial to women's rights and status (see e.g., Bakken & Buhaug, 2021; Berry, 2018; Nordenving et al., 2023). While this idea might seem counterintuitive at first, the underlying logic is straightforward. Mass violence is inherently gendered with the majority of perpetrators, victims, and prisoners being men (La Mattina, 2017). Women fill the vacuum resulting from these demographic imbalances by taking on new roles and responsibilities. This leads to a disruption of traditional gender hierarchies, as women gain more economic and political power.

In addition to entering new spaces that have opened up due to men's absence, women also act against collective gendered threats posed by mass violence (Berry, 2015; Kreft, 2019). Through their participation in grassroots movements and campaigns, women bring unprecedented visibility to issues of gender inequality and gender-based violence (Berry, 2015). Only a small proportion of women activists and leaders end up in positions of political power post-conflict, yet those who successfully make it to parliaments or governments act as a powerful symbol of empowerment for girls and women who witness their achievements (Arvate et al., 2021; Campbell & Wolbrecht, 2006; Wolbrecht & Campbell, 2007). In line with our argument, the role model effect has been found to be especially pronounced for adolescents and lessens with age.[7] Even when women's mobilization does not translate into political representation or personal gain, witnessing women organize around issues of gender inequality and act as agents of change – likely for the first time in their lives – can be highly influential during adolescents' formative years. We expect this transformation to be prevalent for younger women in particular as older women are more likely to have already "internalized their social status as persons of lesser value." (Kabeer, 1999, p.440).[8]

Political events experienced during formative years can serve as exogenous catalysts that give issues unprecedented visibility and provide young individuals with extensive information required for attitude formation, hence 'nonattitudes' become 'real attitudes' (Sears & Valentino, 1997). Crucially, this holds true only for attitude objects that are rendered salient by the respective political event (Sears & Valentino, 1997).[9] In the context of the Rwandan genocide, attitudes on gender and violence gained unprecedented salience. As such, we expect women

[5] There exists no clear consensus in the literature on how to define the beginning and end of the *formative-years*, *coming-of-age* or *socialization* period. However, the period of adolescence is widely recognized as a formative stage of human development (see discussion in Dinas, 2014). The World Health Organization (2025a) defines adolescence as "the phase of life between childhood and adulthood, from ages 10 to 19". Similarly, we define adolescents as 18 or younger (i.e., born in 1976 or later) but use different year-of-birth thresholds in a sensitivity analysis.

[6] We want to emphasize that our argument does not rest on the assumption that genocide or any form of mass violence is beneficial for women, or for subsequent generations, but that women's response to mass violence can lead to positive cascading effects. We also acknowledge that not all forms of violence create conditions for women's empowerment. Especially conflicts that involve gender-based violence and one-sided violence – both characteristics of the Rwandan genocide – are thought to lead to more women's mobilization and in turn to more women's empowerment (Savun et al., 2024).

[7] For instance, Wolbrecht & Campbell (2007) show that the political activity of younger women is impacted to a significantly greater degree by the presence of female members of parliament than that of older women.

[8] Experiencing new social or political situations earlier in life has the power to mold attitudes that are in younger age still in development, whereas the same event would be less impactful when experienced later in life when stable attitudes have already been formed (Mannheim, 1952).

socialized in an era characterized by violence-induced women's empowerment to form attitudes rejecting violence as a means to demonstrate authority and enforce discipline.[10]

Therefore, we expect younger women who witnessed women's empowerment to be less likely to accept VAC. Patriarchal norms justify the use of violence, such as the physical discipline of children (Lansford et al., 2020). Patriarchal systems are characterized by a clear hierarchy, with men at the top and women and children beneath them. If subordinates (women or children) 'misbehave', someone higher in the hierarchy (men or parents) must correct their behavior by means of violence (Namy et al., 2017). As such, corporal punishment is rooted in norms of gender inequality (Namy et al., 2017; Stern et al., 2022). Consequently, we argue that younger women who grew up watching older female role models challenge a patriarchal system are socialized into more progressive gender attitudes and are thus more likely to condemn violence within their families. This is supported by evidence that empowered women report less domestic violence (Nordenving et al., 2023) and less use of VAC (Stern et al., 2022), which might set off a virtuous cycle for the next generation.

2.2. Intergenerational transmission of attitudes on violence

In line with the virtuous cycle hypothesis, we expect that the positive, empowering effect on younger women exposed to higher levels of mass violence has cascading effects for their children. As mothers, this generation of women socialized into rejecting violence transmits these attitudes to their children. Our argument here is twofold. On the one hand, women who are less accepting of violence want their children to adopt attitudes identical to theirs. As such, they teach their children the core belief that violence is not acceptable to enforce discipline. On the other hand, following from positive assortative mating, women choose like-minded partners with similar preferences (Dohmen et al., 2012), i.e., who are also more likely to condemn violence within their families. For children, their parents' views and behaviors are the most important point of reference to inform their own attitudes and behaviors. Attitudes on parenting practices, including the use of corporal punishment, develop prior to entering parenthood through the belief system that is taught during childhood or adolescence (Walker et al., 2021). In sum, we expect second generation individuals to be less accepting of VAC if their mothers were in their formative years and exposed to higher levels of violence during the genocide.

Women's empowerment has been linked to immediate positive outcomes for (young) children, e.g., health or education (Annan et al., 2021; Duflo, 2012). In contrast to previous studies, we study the causal effect of mothers' attitudes on self-reported attitudes by their children and investigate if the second generation benefits from genocide-induced attitude formation of their mothers. The vertical social transmission of attitudes and preferences from parents to children is well established in the literature (Schönpflug & Bilz, 2008). Investigating VAC attitudes in the context of intergenerational transmission is particularly relevant. Children who grow up in families that tolerate violence are at risk of adopting the same beliefs, and of becoming violent or ending up with violent partners as adults (Ireland & Smith, 2009). Empowered women can break this violent cycle, with important implications for future generations. If the second generation is more likely to condemn VAC due to their mothers' more progressive VAC attitudes, this might give us an indication of how they will fare as parents. Following the intergenerational transmission argument, the second generation would be less likely to teach their children (i.e., the third generation) that the use of violence is a normal or adequate response.

3. The Rwandan genocide

The origin of the 1994 Genocide in Rwanda against the Tutsi is complex and rooted in decades of colonial history and discrimination.[11] The commonly identified trigger event was the shooting down and fatal crash of the plane of President Habyarimana on April 6, 1994. Within a few hours, members of the military, the administration, and a government-sponsored militia group as well as civilians began to kill Tutsi, but also moderate Hutu and Hutu leaders of the opposition party. While conclusive figures of the death toll among Hutu are not available, many Hutu were killed trying to protect Tutsi, suspected of helping Tutsi, or mistaken for Tutsi (Straus, 2019). The 1994 genocide lasted for 100 days between April–July and around 1 million people were killed, the majority of them men (Republic of Rwanda, 2002).[12]

Approximately one million perpetrators are estimated to have participated in the genocide which corresponds to around 14 % of Rwanda's total population (based on the 1991 census population count, Nyseth Brehm et al., 2014).[13] As the Rwandan justice system was overwhelmed with bringing the massive number of perpetrators to justice, the government tasked local *gacaca* community courts with conducting trials for perpetrators of genocide and crimes against humanity across the country. Perpetrators were classified into three categories: *category 1* if they planned or organized the genocide or committed rape and sexual torture, *category 2* if they committed or were accomplices to torture, murder or desecration of corpses, and *category 3* if they committed property crimes or looting (Nyseth Brehm et al., 2014).[14] The intensity and frequency of genocide events varied across the country (see Fig. 1). According to perpetrator information in the *gacaca* court records, the median age of participants was 34 and around 88 % of all participants (95 % for violent offenses) were men (Nyseth Nzitatira et al., 2023).

[9] Although existing research focuses on socialization induced by non-violent political events, we expect to observe similar socialization patterns for adolescents who come of age during a historical era characterized by violence-induced women's empowerment.

[10] We do not dispute that exposure to mass violence can lead to a normalization of violence. However, we expect that under the conditions that we investigate, a post-war culture of violence can be counteracted by witnessing women's empowerment and the subsequent formation of non-violent (or pro-social) attitudes.

[11] It would be impossible to do the complex history justice within the scope of this paper. Comprehensive and detailed summaries can be found in e.g., Bonnier et al. (2020); Des Forges (1999); Heldring (2021); Hintjens (1999); Prunier (2014); Straus (2006, 2019); Verwimp (2006). It is important to note that an "ethnic conscience" did not exist in the early pre-colonial period in Rwanda, but started to emerge in the 18th century when the heads of the newly established permanent army identified themselves as Tutsi who were to have greater privileges than clan leaders (Ntezimana, 1987; Republic of Rwanda, 2002). As such, the ethnic identities Hutu and Tutsi predate the colonial rule, but the strict distinction between and the salience of belonging to either of the two groups was a product of discriminatory colonial policies which stipulated that the Tutsi were to be the sole group with power to administer.

[12] However, it is worth noting that the Rwandan government speaks about genocide and massacres between October 1990 and December 1994 (Republic of Rwanda, 2002). Verpoorten (2005) identifies the first attacks of the (predominantly Tutsi) rebel group Rwandan Patriotic Front (RPF) in October 1990 as starting point for civil unrest in Rwanda. The RPF's objective was to remove President Habyarimana from power and enable the return of hundreds of thousands of exiled Rwandans. Increasingly threatened by the RPF's success, and faced with decreasing levels of popular support, Habyarimana and his allies launched a campaign designed to incite hatred and fear of the Tutsi, portraying them as collaborators of the RPF. As such, the genocide was not an isolated sudden outbreak of violence, but intertwined with the ongoing civil war between the Rwandan government and the RPF (Des Forges, 1999).

[13] Estimates on participation vary. The official numbers released by the Rwandan government range between 1–2 million participants.

[14] Between 847,233 and 888,307 individuals were found guilty of genocide crimes across all three *gacaca* categories (Nyseth Nzitatira et al., 2023). The *gacaca* courts and records are described in more detail in the next section. See Appendix B for a detailed description of the categories provided in Nyseth Brehm et al. (2014).

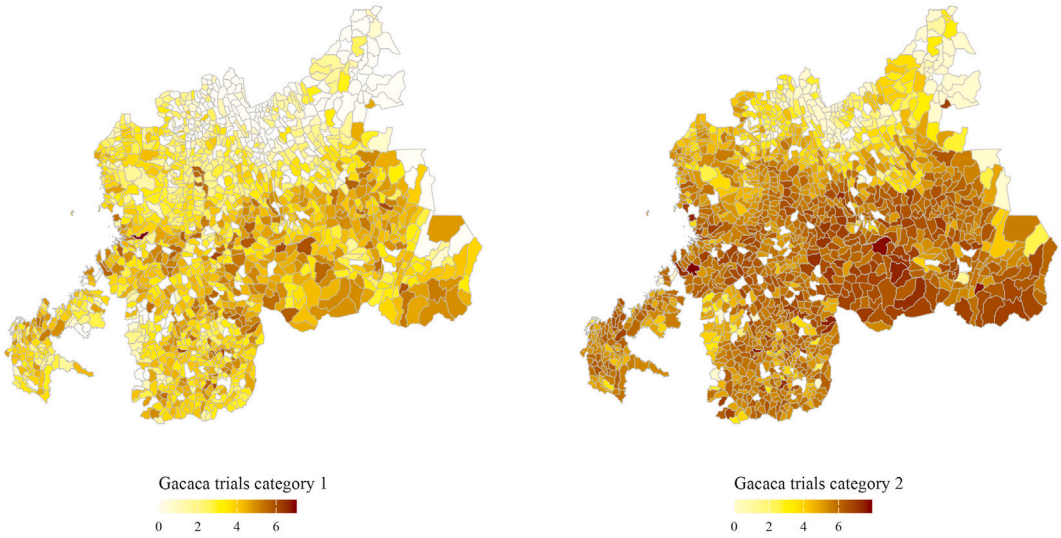

Fig. 1. Distribution of *gacaca* trials across Rwanda. The map displays the number of category 1 (left) and category 2 (right) gacaca trials per sector, scaled by sector population in 1991.

Men made up the majority of victims, perpetrators, and as such the imprisoned population. As a consequence, the genocide drastically altered the sex ratio in Rwanda, particularly in areas with high levels of genocide violence (Eichelsheim et al., 2019; La Mattina, 2017; Rutayisire & Richters, 2014). This, in turn, had strong impacts on gender roles and relations. Historically, Rwandan women faced widespread discrimination and had limited opportunities. They were barred from owning land, received less education compared to men, and were often confined to low-paying jobs, leaving them largely dependent on male authority figures (Kraehnert et al., 2019; Nordenving et al., 2023). However, the 1994 genocide marked a turning point. Women stepped up as heads of families, community leaders, and active participants in political life as a response to the threatening conditions they faced (Berry, 2015). Additionally, the post-genocide government implemented laws to strengthen women's land and inheritance rights, and made it a constitutional right that women occupy 30 % of decision-making positions (Abbott et al., 2018). Young women, in particular, experienced this phase of transition during their formative years, when core beliefs and attitudes were being shaped. They observed and internalized examples of women, including their mothers and other role models, taking on new roles and challenging traditional norms (Nordenving et al., 2023; Wolbrecht & Campbell, 2007).

Important to note is that changes in sex ratios, and as such the extent of women's participation and empowerment (as outlined in the previous section), varied locally with genocide intensity (Nordenving et al., 2023). As such, this context allows us to use the variation in both spatial genocide intensity and temporal variation in the socialization of first generation women to test the transmission of genocide violence. The Rwandan genocide is an ideal case to investigate our research question based on the following factors that we can exploit to empirically test our arguments: i) the strongly gendered participation in genocide, ii) the locally varying intensity of genocide violence, iii) which together led to an unprecedented process of women's mobilization and participation, iv) with temporarily varying implications for women's attitude formation, and v) the availability of fine-grained individual and spatial data to capture all this.

4. Data

We obtain data from several sources - the 2010/2011, 2014/2015 and 2019/2020 Demographic and Health Surveys (DHS), the records of the Rwandan *gacaca* courts and the 1991 Rwandan census.

4.1. Demographic and health surveys (DHS)

Our population of interest are first- and second-generation individuals (mothers and their children) who live within the same household in post-genocide Rwanda. We obtain geo-coded survey data from three waves of Demographic and Health Surveys conducted in Rwanda in 2010/2011, 2014/2015, and 2019/2020 (DHS Rwanda, 2012, 2016, 2021).[15]

The DHS offers several key advantages for our analysis: First, the survey data are geo-coded, allowing us to spatially match them to fine-grained information about local genocide intensity. Second, the DHS interviews all eligible household members which enables us to pursue a two-generational design and allows us to examine identical survey questions across generations.[16] Third, this feature of the DHS also reduces measurement error when studying the transmission of attitudes within households, as all family members are interviewed by the same interviewer. As such, the richness of the DHS survey data combined with its multi-generational structure allows us to measure our outcome of interest – attitudes on VAC – for mothers and their young adult children within households. To the best of our knowledge, we are the first to construct a dataset where each child-level entry is linked to their mothers' information to measure within-family transmission of attitudes.

[15] The DHS are nationally representative, cross-sectional household surveys that are conducted every few years in low- and middle-income countries. In Rwanda, the DHS were implemented by the National Institute of Statistics of Rwanda (NISR). The DHS program is funded by the U.S. Agency for International Development.

[16] The target group of the DHS are primarily women, but also men of reproductive age (defined as 15–49 for women, and 15–59 for men). Eligible household members are regular residents who fall within said age group. The DHS surveys consist of three questionnaires: the household questionnaire, the women's questionnaire, and the men's questionnaire. The household questionnaire includes basic demographic information about all household members (including age, sex, marital status, education, and relationship to the head of the household) and household characteristics. In all sampled households, all eligible women are interviewed using the women's questionnaire. In half of the households, all eligible men are interviewed using the men's questionnaire. In addition, a subsample is interviewed for the domestic violence module. In 2010, 37 % of women and 0 % of men; in 2014, 39 % of women and 34 % of men; and in 2019, 19 % of women and 33 % of men were selected and interviewed for the domestic violence module.

We focus on self-reported VAC attitudes by female genocide survivors and their children for three main reasons. First, VAC attitudes are consistently collected in the DHS across all respondents, regardless of gender, age, or relationship status.[17] This is particularly valuable for obtaining a representative picture of the effect of genocide violence on VAC attitudes, and allows us to investigate intra-family transmission as mothers and their children are asked identical questions. Second, measuring VAC attitudes provides a unique opportunity to examine intergenerational dynamics. Favorable attitudes toward corporal punishment are a strong predictor of perpetrating VAC and individuals form attitudes on parenting before becoming parents (Walker et al., 2021). Gaining insights into the second generation's perspective on violent punishment might foreshadow whether their children will eventually view violence as an adequate response. Lastly, we measure attitudes to mitigate concerns about measurement error. Prior research cautions that experiences of violence are substantially underreported in face-to-face interviews (Barr et al., 2017; Cullen, 2022; Pereira et al., 2020). We assume that disclosing attitudes toward VAC is less stigmatized than reporting violent behavior against children or personal experiences of parental violence.

We measure attitudes on VAC with the following question: "In your opinion, is a parent justified in hitting or beating his/her children for the following reasons: i) if he/she disobeys, ii) if he/she is impolite, iii) if he/she embarrassed the family?".[18] Our main outcome variable *VAC_any* takes the value of 1 if the respondent agrees to any of the three statements, and 0 otherwise. Our alternative outcome variable *VAC_sum* is the sum score of affirmative items, standardized to mean zero and standard deviation one. In Table 1 we present summary statistics for our main variables. We restrict our sample to children (respondents who identify as daughters or sons of the household head) and pair them with their mothers' (partnered women who identify as household head or wife of the household head) data entries to create matched child-mother pairs.[19] We restrict our sample to mothers who are currently in a partnership. We do so to ensure that the family composition is constant across our sample.[20] Given that it is impossible to link children's DHS data to that of their mothers if they do not live within the same household, our data might not perfectly reflect the universe of child-mother-pairs in Rwanda. Instead, we specifically focus on those cases where mothers and children live in the same household. Still, we argue that our sample represents our population of interest (i.e., children and young adults of the second generation) very well. By looking into the full population of respondents in the age group interviewed by the DHS, we show that the overwhelming majority live in a joint household with their parent, which suggests that our sample closely mirrors the broader population. Please see Fig. A.1 in the appendix for more details. Therefore, we argue that the benefits of our approach outweigh the costs: while we lose the small share of interviewed children who do not live in the same household as their parents, we gain the ability to draw inferences about intra-family transmission

Table 1
Summary Statistics: Household Variables from the 2010, 2015, and 2020 DHS.

Variable	Mean	SD	Min	Max	N
Panel A: Child Variables from the DHS (Women's and Men's recode)					
Male	0.23	0.42	0	1	4967
Year of Birth	1996	4.82	1978	2005	4967
Years of Education	6.13	2.82	0	16	4965
Literate	0.86	0.35	0	1	4967
Child states VAC is justified in at least 1 situation	0.78	0.42	0	1	4967
Number of items child states VAC is justified	2.06	1.25	0	3	4967
Child states wife beating (IPV) is justified in at least 1 situation	0.43	0.50	0	1	4965
Number of items child states wife beating (IPV) is justified	1.17	1.67	0	5	4965
Panel B: Mother Variables from the DHS (Women's recode)					
Year of Birth	1971	5.85	1960	1990	4967
Year of Marriage	1992	6.37	1974	2019	4967
Young Mother	0.27	0.45	0	1	4967
Years of Education	4.09	3.54	0	20	4967
Literate	0.59	0.49	0	1	4967
Wealth Index	3.28	1.31	1	5	4967
Woman states VAC is justified in at least 1 situation	0.68	0.47	0	1	4967
Number of items women states VAC is justified	1.76	1.35	0	3	4967
Physical violence by partner in the last 12 months	0.47	0.50	0	1	1215
Number of items women states wife beating (IPV) is justified	1.47	1.83	0	5	4966
Woman states wife beating (IPV) is justified in at least 1 situation	0.49	0.50	0	1	4966
Woman is currently working	0.84	0.36	0	1	4965
Woman receives cash income	0.23	0.42	0	1	4541
Woman is involved in any HH decision	0.52	0.50	0	1	3582
Woman decides on visits	0.21	0.41	0	1	4932
Woman decides on purchases	0.14	0.34	0	1	4930
Woman decides on her healthcare	0.27	0.45	0	1	4932
Woman decides on earnings	0.25	0.43	0	1	2997
Aid Project in 5 km	0.23	0.42	0	1	4967
Gender-targeted aid Project in 5 km	0.08	0.27	0	1	4967
Aid Project in 10 km	0.63	0.48	0	1	4967
Gender-targeted aid Project in 10 km	0.23	0.42	0	1	4967
Panel C: Father Variables from the DHS (Men's recode)					
Year of Birth	1966	7.87	1946	1990	4036
Years of Education	4.28	3.54	0	20	4036
Father states VAC is justified in at least 1 situation	0.59	0.49	0	1	1889
Number of items father states VAC is justified	1.51	1.37	0	3	1889

[17] Unlike the DHS domestic violence module that asks about experiences of physical violence for which only a subset of women, and an even smaller share of men are selected.

[18] These are the only three response alternatives on VAC in the DHS and they are consistently asked across all survey rounds in our sample. We always use all three questions from each survey round for our analysis. Although this is a standard question in both the women's and the men's questionnaire, it was unfortunately not included in the 2019 men's questionnaire. For that reason and because of the DHS strategy to oversample women, daughters are overrepresented in our analyses.

[19] More precisely, we use the DHS individual recode to identify female respondents, and the DHS men's recode to identify male respondents. We then restrict these samples to respondents who identify as daughters or sons of the household head and join them together to construct our children data set. To construct the sample of mothers, we again use the DHS individual recode to identify female respondents, but this time restrict the sample to respondents who identify as either the wife of the household head or the household head and who state that they are currently partnered/married. We then match our children sample with our mothers sample based on household ID and survey wave. We only retain matched observations, i.e., we drop observations of children for whom there is no corresponding mothers' data within the same household.

[20] Our reasoning here includes several aspects. First, the majority of mothers are partnered, i.e., our selection strategy reflects the most common form of family composition. Second, as part of our additional analyses we investigate a subset of our sample of mother-child-pairs where the father has also been interviewed to account for the impact of father's VAC attitudes. If we were to sample unpartnered women in our main analysis, this comparison would not be meaningful. Third, women who leave relationships and women who are widowed might differ in important aspects from partnered women. The former typically have more resources and outside options to be able to leave a relationship, but also often strong reasons to leave a relationship such as abuse and other forms of intra-family violence. The latter often have fewer resources, and might be widowed as a result of the genocide. These specific conditions might correlate with genocide intensity and age, and might thus bias our estimations. Nevertheless, in a robustness test, we also include mothers who do not currently live in a partnership to show that our results are not driven by sample selection (Table A.4).

and can strengthen the precision of our results through the inclusion of child-level and mother-level controls (see Section 5 for a more detailed discussion of the included control variables).

4.2. Gacaca records

We couple the individual-level survey data with fine-grained information on local genocide intensity. These violence measures are spatially merged with the DHS files. As direct measures of individual genocide exposure are not available, we proxy genocide intensity by the prosecution rates for crimes committed during the genocide as in previous studies (Bonnier et al., 2020; Rogall, 2021; Yanagizawa-Drott, 2014). The data come from a nationwide dataset provided by the "National Service of Gacaca Jurisdiction" by the Rwandan government that encompasses the locations of nearly 12,000 local *gacaca* courts established across the country to prosecute individuals accused of perpetrating genocidal violence. *Gacaca* refers to a traditional Rwandan dispute resolution mechanism, in which village elders and community members gather to discuss local disputes.[21] In response to the vast number of prisoners whose cases remained untried in the aftermath of the genocide, the Rwandan government officially launched the *gacaca* court system in 2002 to try and judge those who wished to confess or had been accused of participating in the genocide (Corey & Joireman, 2004). The courts were active until 2012, during which a total of 1.6 million *gacaca* trials were held in 12,000 courts across the country (Nyseth Nzitatira et al., 2023).[22]

Importantly, the courts operated based on the location of the alleged crimes, allowing prosecutions to take place even in the absence of the accused individuals (Bonnier et al., 2020; Nyseth Nzitatira et al., 2023). As in previous studies, we aggregate the *gacaca* data at the 1991 commune level (n = 144) for two main reasons.[23] First, since we merge these data with data from the DHS, in which observations are placed in spatial clusters and coordinates that are randomly shifted by 2 to 5 km (depending on whether the area is urban or rural) to protect respondent confidentiality, finer spatial aggregation would introduce measurement error. The commune level is the smallest administrative unit for which genocide violence can still be accurately identified (Straus, 2006) as category 1 and 2 offenders were tried in courts operating at the sector level in pre-2002 boarders (Nyseth Brehm et al., 2014). Second, as of 1990, residents required government permission to move between communes, making the commune level a stable and suitable unit of analysis (Nyseth Nzitatira et al., 2024).

We proxy for genocide intensity with the number of category 1 and category 2 *gacaca* trials per commune (relative to the respective commune's population). We focus on these categories for two main reasons: first, both encompass violent genocide crimes and are thus more comparable, whereas property destruction and looting (i.e., category 3 crimes) might be seen as a distinct type of offense in our setting. Second, the *gacaca* trials data were found to measure category 3 crimes with severe measurement error, whereas they give an accurate representation of category 1 and 2 guilty verdicts (Nyseth Nzitatira et al., 2024). Nyseth Nzitatira et al. (2023) were able to obtain information on i) the outcome of each verdict, i.e., whether defendants were found guilty or not guilty in the *gacaca* courts, and ii) individual characteristics of the defendants, allowing them to account for repeat participants and duplicate entries. As such, their *gacaca* verdicts data are a more precise approximation of the number of people who stood trial and were found guilty, whereas our data rather reflect the number of trials. However, the verdicts data are only available at today's district-levels and thus lack spatial precision. In Fig. A.3 we show that trials and verdicts are highly correlated. Due to their much higher spatial precision, we rely on the sector-level trials data for category 1 and category 2 crimes and aggregate the data at the 1991 commune level in our main models.[24]

Lastly, for our robustness checks we use two alternative measures of genocide intensity. First, we proxy for genocide exposure with the number of days during which a commune was under RPF (Rwandan Patriotic Front) control. The RPF, primarily composed of Ugandan-based Tutsi living in exile, launched an infiltration into Rwanda from the northeast border with Uganda in April 1994 with the aim of putting an end to the massacres. As they advanced from the northeast, their forces swiftly captured territory, saving countless Tutsis and moderate Hutus from ongoing massacres and finally toppling the Hutu government in July. Their infiltration resulted in a pattern where the genocide lasted longer the farther one got from the Ugandan border. The number of days a commune had been under RPF control correlates with a reduction in genocide violence, as their presence helped to limit further massacres. We use this variation in genocide intensity as a robustness check to estimate our effect of interest. We measure the exact number of days that a commune was under RPF control between the start and the end of the 1994 RPF advancement (a 109 day period from April, 1, 1994 to July, 19, 1994).[25] However, we would like to note this measure is not without limitations. While the RPF saved the lives of numerous Tutsis and moderate Hutus as they advanced toward the south and west, they also killed thousands of militia members and Hutu civilians, especially women and children (Corey & Joireman, 2004; Des Forges, 1999). These killings were seen as retaliation for the ongoing genocide taking place elsewhere in the country and could thus also be considered as a different measure of violence (Rogall, 2021).

Second, we use data on the location of mass graves, based on satellite maps from the Yale Genocide Studies Program to measure the intensity of genocide violence. These data are retrieved from Verpoorten (2012) and aim to measure the proximity to a large-scale massacre. We calculate the distance between the location of the DHS cluster to the nearest location of the 71 mass graves in Rwanda, aggregate this variable at the commune level and scale it to mean 0 and standard deviation of 1. Summary statistics are presented in Table 2.

4.3. Rwandan census 1991

Finally, to assess local pre-genocide conditions we use the second population census that was conducted in August 1991 by the National Institute of Statistics in Rwanda. The census data are available at the commune level in pre-2002 borders. We obtain commune level population data for 1991 as well as literacy and labor force participation

[21] The word itself translates to "justice on the grass" in Kinyarwanda, symbolizing the open-air spaces where the courts were held. See Corey & Joireman (2004) for a detailed description.

[22] Note, however, that not every genocidal crime was tried before *gacaca* courts. Those individuals deemed primarily responsible for the genocide were tried before the International Criminal Tribunal for Rwanda, for instance. Nevertheless, *gacaca* courts constitute the best source of available data on civilians who committed genocidal violence (Nyseth Nzitatira et al., 2023).

[23] Before 2002, Rwanda was organized into 1484 sectors, 145 communes, and 11 prefectures. In 2002, a reform changed the communes into 104 districts, and the prefectures became 12 provinces. In 2006, the country was reorganized again into 30 districts and 5 regions. Today, Rwanda is divided into the following administrative units: provinces (4 + city of Kigali), districts (30), sectors (416), cells (2148), and villages (14,837). In 1991, each commune had an average population of 51,235, and the average size of the area was 153.8 square meters (La Mattina, 2017).

[24] In contrast to Nordenving et al. (2023), we do not differentiate between local and external genocide violence and might therefore pick up a weighted average of the two (similar to La Mattina (2017)). We do not see this as an issue for our proposed channel, but rather understand our approach as more conservative in that it makes it harder to pick up a positive effect by considering both dimensions of genocide violence.

[25] See Verpoorten (2012) for a detailed description. While some communes in the North close to the Ugandan border were under control between 79 and 109 days, other sectors that only came under control by the RPF in July, were only under RPF control between 1 and 19 days.

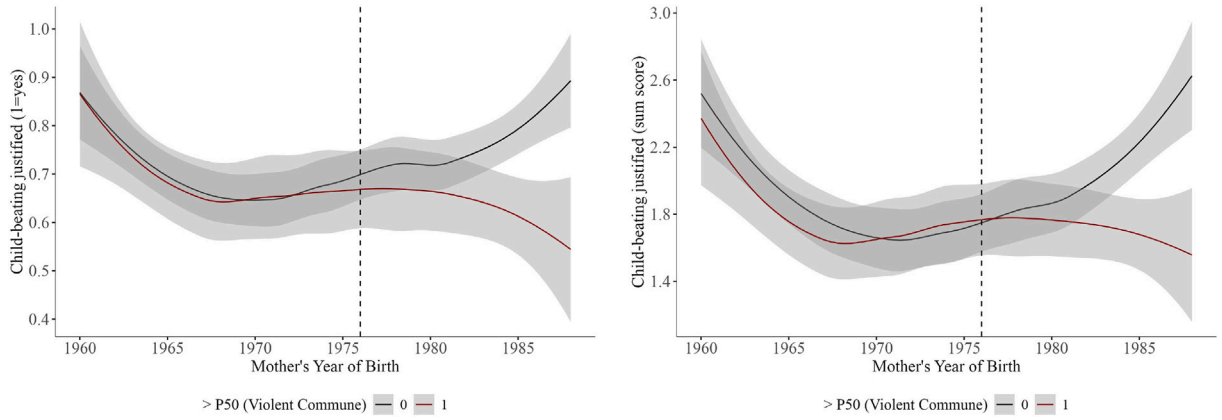

Fig. 2. First stage mechanism. The figure shows local polynomial regressions on the moving average of VAC attitudes across violent and non-violent communes over mothers' year of birth. The binary variable > *P50(Violent Commune)* is equal to 1 if the number of category 1 and category 2 *gacaca* court trials records in a commune exceeds the 50th percentile of the distribution. The vertical bar represents the cutoff by which we divide the group of mothers in those old enough to be socialized in a pre-genocide environment (born 1975 or earlier) and those young enough to be socialized in a (post-)genocide environment (born 1976 or later).

Table 2
Summary Statistics: Genocide and Other Commune-Level Variables.

Variable	Mean	SD	Min	Max	N
Panel A: Data from records of the gacaca courts					
Trials Category 1	3.10	1.19	0.15	5.07	144
Trials Category 2	4.94	1.34	0.62	6.84	144
Distance to mass grave (in km)	11.38	8.87	1.97	56.62	144
Days under RPF control	40.93	41.11	0.00	109.00	144
Panel B: Data from 1991 Census of the Rwandan population					
Literacy Rate Male Population	0.43	0.05	0.31	0.65	144
Literacy Rate Female Population	0.34	0.07	0.19	0.56	144
Labor Force Participation Male Population	0.44	0.04	0.29	0.51	144
Labor Force Participation Female Population	0.46	0.03	0.28	0.52	144

Notes: All variables are aggregated at the commune level. Trials Category 1 and 2 are scaled by commune population size in 1991.

rates separately for the female and male population to account for pre-genocide conditions (Table 2).

5. Identification strategy

This paper examines the intergenerational transmission of violent attitudes from mothers to their descendants. Our identification strategy proceeds in two steps: First, we estimate the causal effect of local genocide violence on mothers' VAC attitudes. Second, we then exploit this exogenous variation in mothers' attitudes as an instrumental variable to estimate the transmission effect, i.e., the effect of mothers' attitudes on those of the second generation.

Our identification strategy relies on the unique historical context following the Rwandan genocide. Due to the gendered nature of the genocide, where men comprised the majority of victims, perpetrators, and prisoners, sex ratios in areas with high levels of violence were significantly altered leading to women's mobilization and participation (Nordenving et al., 2023). Women filled the vacuum left by men's absence, challenging traditional gender roles and norms, and serving as role models for girls in their formative years at that time. As a result, young women in high-genocide regions who witnessed this unprecedented process of 'genocide-induced empowerment' were socialized to condemn violence, making them less likely to justify the use of corporal punishment compared to young women from regions that experienced less violence. Second, we expect young women's attitude formation to have cascading effects for the next generation, i.e., their children. By leveraging the interaction between geographic variation in genocide intensity across 144 communes and women's year of birth, we first measure the effect of genocide socialization on mothers' violent attitudes. Second, as this socialization effect of a mother is exogenous to a child's violent attitudes, it enables us, in a second step, to use the interaction of women's birth year and commune level violence as an instrumental variable to causally identify the effect of mothers' VAC attitudes on those of the second generation.

5.1. First stage: genocide violence and women's violent attitudes

We start by estimating the causal effect of local genocide violence on mothers' VAC attitudes. To do so, we estimate a factorial difference-in-differences with a continuous treatment design (Callaway et al., 2024; Xu et al., 2024) in which we exploit two sources of variation: spatial (women living in communes with varying degrees of genocide intensity) and temporal (women's year of birth). The idea is that young women (≤18 years of age at the time of the genocide) from high genocide regions were socialized under different conditions than older women from the same regions and, importantly, also than their counterparts from less-genocide regions (i.e., young women from communes less affected by genocide violence). Fig. 2 displays the main idea behind our difference-in-differences estimator. For birth cohorts prior to 1994, we observe a common, parallel trend in attitudes toward VAC over time. However, for cohorts that were 18 years of age or younger at the time of the genocide in 1994, we observe a strong divergence in attitudes across communes for both of our outcome measures, the binary *(VAC_any)* as well as the scale *(VAC_sum)* variable. Younger women from above median level genocide communes tend to have less violent attitudes than their counterparts from below median violent communes. This idea can be generalized to a regression framework that exploits greater variation across communes and birth years. We are interested in the causal effect of a marginal increment in genocide intensity for the young mothers, equivalent to the average causal response (Baker et al., 2025), which we estimate by OLS:

$$VACmother_{icgt} = \alpha + \beta_1(Genocide_c \times Young\ mother_g) + \gamma_c + \sigma_g + \tau_t + \delta X_{it} + \delta X_{ct} + \kappa \mathbf{T}_{ct} + \xi_{icgt} \quad (1)$$

Here, the dependent variable *VACmother* indicates whether mother of child i, of mother birth year g, living in commune c, surveyed at the time of survey year t, perceives VAC as justified. The main variable of interest, is given by the interaction between the time-invariant measure of genocide intensity *Genocide_c* in commune c and a binary indicator *Young mother_g* that takes the value of one if the mother is born after

August 1976. This condition ensures that these women were 18 years of age or younger at the end of the genocide in August 1994. For ease of interpretation, all violence measures at the commune level are normalized with a mean of zero and a standard deviation equal to one. β_1 captures the causal effect of a one standard deviation increase in genocide intensity on the probability that a mother who was born after 1976 perceives VAC as justified.

The identifying assumption for a causal interpretation on mothers' VAC attitudes is that β_1 and the error term are not correlated, conditional on control variables. The set of control variables we include is flexible across specifications (see Table 3). First, all specifications include survey year fixed effects (τ_t) to account for overall time trends in outcomes. Second, we add household characteristics, X_{it}, such as the mother's religion, her child's age, her years of schooling and the DHS wealth index as well as pre-genocide commune controls such as population size, male and female labor force participation and male and female literacy rates in 1991.[26] Third, we include a commune (γ_c) and a year of birth fixed effect (σ_g) to account for any cross-sectional differences in outcomes across communes and common differences over birth years. We are careful about including additional variables that control for the repeated cross sectional nature of the DHS at the cluster level such as proxies for economic activity, as these variables might be "bad controls" (Angrist & Pischke, 2009).[27] However, we include province-specific linear time trends (survey year × province), $\mathbf{T}_{jt}$ to account for possible long-run dynamics in socioeconomic and other characteristics across provinces that might affect our outcome of interest.

This difference-in-differences design accounts for time-invariant regional characteristics (such as education or livelihood systems) and trends across cohorts. Importantly, this also includes local overall empowerment conditions that could have affected both groups similarly. In addition, by including mother birth cohort specific controls such as age, and education levels, we capture important time-varying factors that may have affected birth cohorts and VAC differently. Apart from our proposed socialization mechanism, there is little reason to believe that an average woman 18 years old from a high-intensity region is fundamentally different from an average woman with 19 years of age from the same region, and, importantly, also from a 18 year old from lower-intensity genocide region, conditional on the set of controls. Throughout the analysis, we use robust standard errors that are clustered at the commune level to account for potential correlation within spatial clusters.

5.2. Second stage: transmission of attitudes

5.2.1. OLS

We then proceed and estimate the transmission effect, i.e., the relationship between the mother's VAC attitudes and those of her descendant. We start by estimating the following reduced-form specification that is estimated by OLS:

$$VACchild_{icgt} = \alpha + \beta VACmother_{ig} + \gamma_c + \sigma_g + \tau_t + \delta X_{it} + \delta X_{ct} + \kappa' \mathbf{T}_{ct} + \xi_{icgt} \quad (2)$$

Here, the dependent variable *VACchild* indicates whether child i living in commune c, with a mother of birth year cohort g, surveyed at the time of survey year t, perceives VAC as justified. We are interested in the effect of mothers' attitudes on the attitudes of their descendants β. We include the same control variables and fixed effects as in Eq. (1). Importantly, our household control vector X_{it} controls for child age, child education and a dummy on the child's gender. We control for child age and the child's education as children from younger women also tend to be younger or have more education which could bias the estimated effect on VAC. The variable on the child's gender is included in order to account for a potential gendered component in the transmission of attitudes within households, e.g., previous studies have highlighted the importance of same gender role models in the transmission of attitudes (Campante & Yanagizawa-Drott, 2015). All regressions use robust standard errors that are clustered at the commune level.

5.2.2. IV

Although the OLS specification at second stage controls for numerous and the most important confounding variables that jointly affect the mother's and her child's attitudes at the same time, we may not be able to control for all individual factors that jointly affect a child's and their mother's VAC attitudes. Hence, the effect of the OLS estimation might be biased, and we rely on IV estimates to mitigate these concerns. We employ a two-stage least squares (2SLS) approach, using the interaction of genocide violence and our binary indicator on whether the mother's socialization was shaped by the genocide (i.e., younger mothers) from Eq. (1) as an instrumental variable for mothers' VAC attitudes. Apart from the intra-household transmission mechanism we propose, there is no obvious reason why children from younger mothers and from genocide communes should have different attitudes than their counterparts, i.e., children from younger mothers in lower-genocide regions and children from older mothers in the same higher genocide regions, when we control for commune and birth year fixed effects. Thus, under the exclusion restriction that *genocide intensity* × *young mother* only influences the likelihood of the child's VAC attitudes through its effect on mother's attitudes, β_1 captures the transmission parameter of interest. We discuss threats to this exclusion restriction below.

5.2.3. Exclusion restriction

Our identification strategy makes the counterfactual assumption that, absent its effect on mothers' VAC attitudes, genocide violence interacted with our young mother variable has no effect on child's VAC attitudes. This is only true under further precautions. The genocide was one of the most profound and society-altering events, with broad effects on institutions, household dynamics, gender roles, education systems and economic life. Given the scale, one might be concerned that genocide exposure would influence children's attitudes not only through the channel of (young) mothers' attitudes, but through other mechanisms such as the mother's labor force participation or general neighborhood environments, even though these children were not born or were very young at the time of the genocide. For instance, our instrument is composed of commune level genocide violence, which is probably correlated with household characteristics, such as education (Guariso & Verpoorten, 2019). These characteristics likely affect children's VAC attitudes (Erten & Keskin, 2020). Moreover, our instrument is composed of mothers' birth year and children from younger women could have different VAC attitudes, simply by being younger, than children from older mothers, reflecting general trends in the Rwandan society.

However, and this is crucial for our identification strategy, we only use the interaction of genocide violence and young mother as our instrument. In our first stage, we allow both variables (genocide and young mother) to have individual impacts on mother's and child's VAC attitudes that we control for by using commune and birth year fixed effects. Econometrically, genocide violence (commune fixed effect) and young mother (birth year fixed effects) enter our estimation as control variables, while we only use the interaction of both as our instrument. Thus, in our design, older mothers (>18 years) from high genocide regions control for confounding variables stemming from genocide exposure while young women (≤18 years) from less genocide regions control for overall trends across birth cohorts. The identification of the effect of mother's VAC attitudes on those of her child then only stems from variation in the interaction term, which, apart from our proposed intra-household transmission channel, is arguably exogenous to a child's VAC as children

[26] The DHS wealth index is a measure of relative household wealth (based on assets, type of housing, and access to water and sanitation). Each interviewed household is placed into five quintiles of wealth (from poorest to richest).

[27] See a more detailed discussion in Section 6.5.

of young women from high-genocide communes would have followed similar trends in attitudes toward VAC as those from lower-genocide communes in the absence of the genocide. We cannot think of any obvious reason why children of young women from high genocide communes should have different attitudes than their counterparts, i.e., children of young women but lower-genocide regions - except through the channel of mothers' exposure to the genocide - once we control for children of older mothers from the two regions.

In order to add additional credibility to this assumption, we run several robustness checks. First, we control for time-varying factors, such as the general living conditions of individuals, by including the mother's and children's years of schooling and their family wealth as control variables in the second-stage. These variables help us capture important intra-household variation apart from attitude formation. Second, while intra-household transmission is arguably the only reasonable mechanism, the effect might still be driven by other mother-child channels apart from attitude transmission, e.g., a (young) mother's partner selection, observing her with a job, or direct transmission of general resilience. To rule out that the effects are driven by other channels than mothers' VAC attitudes, we use i) the mother's labor force participation and ii) her experiences of domestic violence (both proxies for female empowerment) as endogenous variables in our second stage (see Section 6.5.3).

6. Results

6.1. First stage: genocide violence and mothers' VAC attitudes

Table 3 reports the results from our first stage regression models. In column (1) we estimate the cross-sectional relationship between genocide intensity and mothers' VAC attitudes. The cross-sectional estimate indicates no association between genocide intensity and attitudes on VAC for all mothers when date of birth is not considered: the coefficient is positive, small and statistically insignificant. Column (2) presents the estimate of interest from the most parsimonious model, while column (3) adds community controls, individual controls, and survey-round fixed effects. In both models, we find a negative and statistically significant effect of *genocide intensity* × *young mother* on attitudes toward VAC. Young mothers from high-genocide-intensity regions are 4.5 percentage points less likely to accept VAC compared to young mothers in less-affected regions, controlling for baseline differences in (older mothers') acceptance of VAC across regions. Interestingly, in columns (2) and (3), the baseline effects of *genocide intensity* and *young mother* are positive, yielding two additional findings. First, older mothers in high-intensity regions exhibit higher support for VAC than the reference group (older mothers in low-intensity regions). Second, being a young mother in a low-intensity region is also associated with greater support for VAC—consistent with prior studies showing that younger women in Rwanda report higher acceptance of intimate partner violence (IPV) than older women (Thomson et al., 2015). However, the interaction term indicates a negative treatment effect of genocide violence on young mothers support for VAC. Specifically, the marginal effect of a one–standard deviation increase in genocide intensity is negative for young mothers in high-intensity regions, whereas it is positive for young mothers in low-intensity regions and for older mothers in high-intensity regions. Columns (4) and (5) show that these estimates remain robust in both magnitude and statistical significance when including commune and year-of-birth fixed effects, which capture more variation across communes and cohorts. Finally, the results from our preferred specification (column 5) suggest that a one–standard deviation increase in genocide violence reduces the likelihood that young mothers justify VAC by 7.7 percentage points relative to their counterparts in less violent regions, controlling for older mothers in both regions.

Compared to the overall sample mean of 0.68, this results in a reduction of around 11.3 %. In column (6), we use the same specification and estimate the effect separately for the scaled outcome variable (VAC mother sum) and confirm the negative association. Overall, columns (2)–(6) demonstrate that the instruments are highly significant and relevant with F-statistics above or close to the critical value of 10. To address remaining concerns about weak instruments, we apply weak-instrument robust inference by using robust standard errors clustered at the commune level across all specifications (Hahn & Hausman, 2003).

6.2. Second stage: the intergenerational transmission of violent attitudes

We now turn to our second stage and estimate the transmission effect. Column (1) from Table 4 reports results from a simple linear regression model without controls. Column (2) estimates the same model as a 2SLS model using our instrument (*genocide intensity* × *young mother*) for the endogenous variable of mothers' support of VAC. We also include estimations using a binary instrumental variable (Column 3). For this binary instrument, we interact the young mother dummy with a binary indicator *median violent commune* that equals one if the commune is equal to or above the median number of *gacaca* court trials scaled by population size in 1991. Columns (4)–(6) add our most important control variables and Columns (7) to (9) include the province time trends.

In all models, we find a positive and statistically significant effect of mothers' attitudes on those of their children. In particular, the results point to a strong intergenerational transmission: children are more likely to approve VAC when their mothers do, and—importantly—are substantially less likely to approve when their mothers disapprove. Consistent with our first-stage findings that exposure to genocide significantly lowers young mothers' approval of VAC, this suggests a strong intergenerational reduction in violent attitudes. Across specifications, the 2SLS models return sizable and stable coefficients, indicating that children are about 35–38 percentage points less likely to approve of VAC if their mothers disapprove. Relative to the sample mean of 78 % of children approving VAC, this corresponds to a reduction of roughly 51 %. The estimated coefficients are close to those found in similar studies on intergenerational transmission parameters (Cunningham, 2001; Fernández et al., 2004; Gay, 2023; Wilhelm et al., 2008).[28]

The IV-estimates are larger than the ones estimated by OLS, suggesting that the OLS likely underestimates the transmission effect. This pattern is consistent with previous studies showing that intergenerational transmission parameters can be biased downward by picking up negative correlations between household background factors and attitudes or due to attenuation bias.[29] We attribute the difference between our OLS and IV results to a combination of two main factors: First, the OLS estimates are likely attenuated by noise in our key explanatory variable—mothers' self-reported VAC attitudes—leading to systematic underestimation of the causal effect (Wilhelm et al., 2008). By contrast, the IV approach isolates the variation in mothers' attitudes that is explained solely by the exogenous variation from the instrument, effectively purging the noise and thus recovering a consistent and larger estimate of the true transmission parameter. Second, the IV-estimate identifies a local average treatment effect (LATE), which applies specifically to the "complier" population—the subset of mothers whose attitudes were affected by the instrument. It is not implausible to assume that the causal effect of a mother's attitude on her child's is not uniform across the population and mothers whose attitudes

[28] Fernández et al. (2004), for instance, show that gender norms transmit at a similar rate between first and second generations. They find that the probability of working full time among married women in the US increases by 32 percentage points (from 39 % to 71 %) if their husband's mother worked. Similarly, Wilhelm et al. (2008) report intergenerational transmission parameters of 0.26–0.31 for charitable giving and intergenerational income elasticities of up to 0.32 (interpreted as percentage change in children's outcomes associated with a one percent increase in the corresponding parental variable).

[29] Fernández et al. (2004), for example, show that controlling for several background variables nearly doubles the estimated transmission parameter, and similarly, Wilhelm et al. (2008) show that applying a ballpark correction for attenuation bias can correct the estimated transmission parameters—yielding roughly twice as large estimates, as we also find in our case.

Table 3
The effect of the Rwandan genocide on mothers' VAC attitudes.

	VAC mother any					VAC mother sum
	(1)	(2)	(3)	(4)	(5)	(6)
Genocide Intensity	0.015	0.011	0.030**			
	(0.012)	(0.013)	(0.013)			
Young Mother		0.070***	0.058***			
		(0.020)	(0.020)			
Genocide Intensity × Young Mother		−0.045**	−0.045**	−0.036*	−0.077***	−0.183***
		(0.017)	(0.021)	(0.021)	(0.025)	(0.070)
Observations	4965	4965	4965	4965	4965	4965
R^2	0.047	0.027	0.050	0.114	0.163	0.188
F-test, stat.	13.604	9.6496	13.205	9.5980	8.5849	7.4142
Individual controls	✓		✓	✓	✓	✓
Commune controls	✓		✓			
Survey round FE	✓		✓	✓	✓	✓
Commune FE				✓	✓	✓
Year of birth FE				✓	✓	✓
Province specific linear trend					✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the mother agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother sum*" is the sum score of affirmative items, standardized to mean zero and standard deviation one. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. Commune controls are obtained from the 1991 Census and include population density, literacy rate, and literacy rate for men and women. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

Table 4
The effect of mothers' VAC attitudes on children's VAC attitudes.

	VAC child any								
	OLS	IV/2SLS		OLS	IV/2SLS		OLS	IV/2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VAC mother any	0.197***	0.441***	0.475***	0.170***	0.399***	0.342**	0.151***	0.386***	0.356***
	(0.018)	(0.122)	(0.122)	(0.017)	(0.132)	(0.134)	(0.016)	(0.126)	(0.136)
Observations	4965	4965	4965	4965	4965	4,965	4965	4965	4965
R^2	0.048	−0.026	−0.049	0.091	0.026	0.028	0.130	0.060	0.063
Individual controls				✓	✓	✓	✓	✓	✓
Survey round FE				✓	✓	✓	✓	✓	✓
Commune FE				✓	✓	✓	✓	✓	✓
Year of birth FE				✓	✓	✓	✓	✓	✓
Province time trend							✓	✓	✓
IV-variable		Linear	Binary		Linear	Binary		Linear	Binary

Note: Instrumental variables at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal to or above the median violent commune (binary), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC mother any*" and "*VAC child any*" are measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using OLS/2SLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

were affected by the (post-)genocide socialization may have a stronger, more direct effect on their children's attitudes compared to the average mother in the sample. Therefore, the IV coefficient may be larger for the subgroup of "compliers". High levels of non-compliance, however, can also lead to a weaker instrument. To ensure the validity of our inference, all reported standard errors are weak-instrument robust and clustered at the commune level. Furthermore, we employ two distinct instrumental variables—a continuous measure and a binary indicator—to identify the causal effect. Column (9) presents IV estimates using the binary instrument and the full set of controls. The results remain consistent with those obtained using the continuous instrument. The stability of the IV coefficient across model and instrumental variable specifications further strengthens confidence in the credibility of the LATE we have identified.

We next examine whether the findings hold when alternative outcome and explanatory measures are used. In Table A.1, we replace the binary indicator of child VAC with the child's normalized sum score of affirmative items. The results remain consistent with our baseline findings. Similarly, the effects remain consistent if instead of the mother's binary variable we use the mother's sum of affirmative items as main explanatory variable (Table A.2).

Additionally, as a further robustness check, in Table A.3 we limit our sample to children born in 1995 or later, excluding those old enough to have been directly exposed to the genocide. While we argue that our effect of interest is primarily driven by differences in mother's exposure to genocide-socialization and is largely independent of the child's year of birth, this approach addresses concerns that including children born before 1994 might bias our findings. The effects remain stable in terms

of both significance and size and support our claim that the effect on children is not driven by direct exposure, but works through mother-level differences in genocide exposure (Table A.3).

Moreover, to address potential concerns about the arbitrariness of our "formative years" definition, we show that the observed effects are not driven by the chosen age cutoff. In our main specification, we define formative years as completed by the age of 18, classifying all women 18 or younger as "young mothers", i.e., as those still in their formative years at the time of the genocide. In Fig. A.4, we re-estimate Eq. (1) using alternative age cutoffs. As expected, the effects remain significant at conventional levels for age cutoffs close to 18, yet, and importantly for our argument, the effect gradually diminishes when the cutoff exceeds 18. Additionally, for all cutoffs below 18, the point estimates are negative, suggesting that our main argument still holds even when we consider the formative years to be concluded in younger ages, although some of these estimates lack statistical significance, likely due to different sample compositions.[30] Overall, the effects fade out with younger and older age, which is consistent with our argument of formative years during adolescence and suggests that 18 years is a reasonable choice for defining the age cutoff for formative years.

As other studies on the consequences of armed conflict, we address concerns related to selective migration or survivor bias. Migration patterns in Rwanda are highly complex (Yonekawa, 2020), and we cannot test these patterns directly as respondents in the DHS are not asked about previous places of residence. However, we consider these biases to be less of a concern in our setting as they likely lead us to underestimate the true effect. For instance, genocide violence was generally weaker in villages with higher levels of education or literacy rates (Yanagizawa-Drott, 2014). Highly educated people are also more likely to be able to migrate and are thus less likely to currently live in areas that were strongly affected by genocide violence. Those better-educated individuals in Rwanda are also the ones for whom studies have shown less supportive VAC attitudes (Thomson et al., 2015). Therefore, if anything, we might underestimate the effect in our sample. Yet, to further support this claim, we control for pre-genocide literacy and education levels (at the commune level) as well as individuals' education post-genocide in our preferred specifications.

With respect to survivor bias, it is plausible that women who survived the genocide and had children were exposed to less severe violence than those who did not survive. We therefore interpret our estimates as lower bounds of the true effect. Similarly, we focus on a specific subset of the population, namely (young adult) children who are living together with their mothers, who survived the genocide and are not imprisoned at the time of the surveys (although they might have been in the past). However, Fig. A.1 in the Appendix shows that our sample closely resembles the general population of that age, indicating that we are not selecting an unrepresentative or unusually small subgroup.

6.3. Alternative genocide measures

Our results remain further consistent when employing alternative first-stage specifications. While the *gacaca* trial records serve as a robust and widely recognized measure of genocide violence, aggregating this data at the commune level might introduce potential measurement error, which could bias our results. To address these concerns, we estimate alternative instrumental variable specifications to underscore the robustness of our results to variations in the classification of violence. In Table 5, we estimate the impact of genocide violence on younger mothers' attitudes separately for category 1 (column 1) and category 2 (column 2) trials. The estimated coefficients remain consistent in terms of size and significance across both trial categories, suggesting that our primary findings are not solely attributable to either category of *gacaca* court trials. Instead, our main instrumental variable reflects a broader measure of genocide violence exposure.

Table 5
The effect of genocide on mothers' VAC attitudes (alternative instruments).

	VAC mother any			
	(1)	(2)	(3)	(4)
Category 1 Trials × Young Mother	−0.082***			
	(0.027)			
Category 2 Trials × Young Mother		−0.075***		
		(0.025)		
Dist. to Mass Grave × Young Mother			0.050***	
			(0.016)	
Days under RPF × Young Mother				0.039*
				(0.020)
Observations	4965	4965	4965	4965
R^2	0.164	0.164	0.162	0.162
F-stat	6.49	6.50	6.39	6.32
Individual controls	✓	✓	✓	✓
Survey round FE	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓
Province specific linear trend	✓	✓	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. Genocide variables are measured as a continuous index with mean zero and standard deviation aggregated at the commune level. "Category 1 Trials" and "Category 2 Trials" measure the number of gacaca trials per category and are scaled by commune population size in 1991. "Distance to Mass Grave" measures the average distance to a mass grave per commune and "Days under RPF control" measures the average days a commune has been under RPF control in 1994. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the mother agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

In column (3) we use the mean distance to a mass grave, averaged across all sectors within a commune and standardized to mean 0 and standard deviation of 1, as an alternative instrument. We find that a one standard deviation increase in distance to a mass grave — indicating greater distance from sites of concentrated genocide violence — is associated with an increase in young mothers' likelihood of endorsing VAC attitudes, which is again in line with our main findings.

Similarly, column (4) estimates the effect of the average number of days a commune was under RPF control during the genocide on mothers' VAC attitudes. In line with our theoretical predictions that more days under RPF control - a measure for less genocide violence - should increase attitudes toward violence for younger women, our findings indicate that a one standard deviation increase in days under RPF control corresponds to a higher likelihood of younger mothers endorsing VAC. Again, these findings reinforce our primary conclusion: younger mothers from areas with greater exposure to genocide violence exhibit lower approval of VAC.

We then turn to estimating our second stage using this alternative set of instrumental variables. Table 6 reports the results from our preferred IV estimation using category 1 trials (column 1), category 2 trials (column 2), mass grave distance (column 3), and RPF control (column 4) as alternative measures for genocide intensity in Eq. (1). Across all specifications, the effects remain close to the estimated main effect from Table 4 of 0.38. Thus, we conclude that our primary instrument serves as an appropriate and reliable proxy for measuring genocide violence, and that our main results are robust to alternative measures of genocide violence.

Overall, our results provide strong and robust evidence of a causal, long-term link between exposure to genocide and transmission of violent

[30] Individuals below a given age threshold at the time of the genocide are assigned to the treatment group, while those above are assigned to the control group. As the age threshold is lowered, individuals most likely to be affected the most—those near the original cutoff—are assigned to the control group, potentially reducing the observable differences between groups.

Table 6
The effect of mothers' VAC on children's VAC (alternative first stage).

	VAC child any			
	(1)	(2)	(3)	(4)
VAC mother any	0.396***	0.376***	0.490***	0.459***
	(0.128)	(0.120)	(0.142)	(0.144)
Observations	4,965	4,965	4,965	4,965
R^2	0.055	0.065	0.049	0.037
Individual controls	✓	✓	✓	✓
Survey round FE	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓
Province specific linear trend	✓	✓	✓	✓
IV-variable (genocide measure)	Cat1 Trials	Cat2 Trials	Mass grave dist.	RPF days

Note: Instrumental variables at the first stage are (1) the number of category 1 trials, (2) the number of category 2 trials, (3) distance to a mass grave and (4) the average number of days a commune had been under RPD control in 1994, all interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC mother any*" and "*VAC child any*" are measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using 2SLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

attitudes. We find a strong negative effect of younger mothers' exposure to genocide socialization and her approval of VAC. This norm-changing effect further transmits to the second generation, resulting in lower acceptance of VAC by children who grew up with mothers socialized during the genocide.

6.4. Mechanism: women's empowerment

The mechanism we propose in this paper is genocide-induced empowerment for younger women. In this section, we present evidence to illustrate this channel using various indicators. Given the complex and multifaceted nature of empowerment, we draw on a set of complementary measures that collectively support our proposed mechanism.

6.4.1. Labor force participation

First, we proxy for economic empowerment by the mother's labor force participation. Due to the diminished presence of men, women might be more likely to participate in the labor force as they take on new economic responsibilities and roles traditionally held by men. Prior studies have shown that employment and economic contributions enhance a woman's bargaining power within her household and community and thus serve as a source of empowerment (Duflo, 2012). Moreover, increased economic participation might also strengthen a mother's sense of agency, potentially impacting her attitudes toward violence.

We estimate the impact of genocide on younger mothers' labor force participation using two variables from the DHS: a binary indicator of current employment equal to 1 if the respondent is currently working, and a binary indicator of cash income equal to 1 if the respondent is paid in cash for her work. We use cash income as a proxy because it is associated with formal and thus more stable employment, reflecting a consistent monetary contribution to the household. Table 7 shows that the labor force participation rate is higher among younger mothers in communes with higher genocide intensity. A one standard deviation increase in genocide violence is associated with an increased labor force participation of 2.9 percentage points for mothers aged 18 or younger at the time of genocide. We do not find an effect on cash income. The results remain similar for an alternative sample that includes mothers who are not living in partnership (Panel B).

6.4.2. Bargaining power

Second, we create a bargaining power index using the DHS indicators on women's involvement in the household decision-making process. The idea behind the bargaining index follows the widely accepted notion that empowerment means having the ability to live the life one chooses (Kabeer, 1999; Laszlo et al., 2020). We consider all the questions in the DHS that are related to women being involved in the final say in decisions on i) her own health care, ii) making large household purchases, iii) making household purchases for daily needs, iv) visits to family or relatives, and v) the food to be cooked each day. For example, women having a say over household assets is closely related to their ability to expand their resource base, and thus empowerment (Annan et al., 2021; Duflo, 2012). In order to mitigate concerns about cherrypicking outcome variables, we consider all relevant questions and combine them into a single bargaining power index. Our estimates point toward a positive relationship but we do not find a significant effect.

6.4.3. Domestic violence

Third, we test whether younger mothers are less likely to be victims of physical domestic violence. Previous evidence on the empowerment-domestic violence link is mixed. Some studies find that indicators of empowerment, such as employment, decision-making, and education, are associated with a reduction in intimate partner violence (IPV) as women with greater outside options — due to financial independence or social support — are less likely to experience or remain in violent partnerships (Annan et al., 2021; Duflo, 2012). However, other studies find the opposite, showing that shifts in power dynamics that challenge traditional male dominance can lead to increased violence as a form of male backlash (Laszlo et al., 2020). We test this channel by using the DHS domestic violence module. We measure intimate partner violence as binary variable that takes the value of 1 if the respondent has experienced physical violence by their partner in the past 12 months. Attitudes on IPV are defined as the sum score of affirmative items on the respondent's opinion that a husband is justified in hitting or beating his wife. The results from column (4) of Table 7 suggest that younger mothers from high genocide regions are less likely to experience physical intimate partner violence by 15.9 percentage points. Relative to the overall sample mean of 0.47 this is a meaningful reduction of 33 % compared to their counterparts from non-genocide regions. While earlier studies, such as La Mattina (2017), find that genocide-induced shifts in sex ratios led to an unfavorable marriage market for women—raising the likelihood of ending up with a violent partner, consistent with the marriage market argument of Becker (1993)—our results do not support this mechanism in our sample. This difference can be explained by several reasons: First, La Mattina (2017) studies the effect of marriage timing (before versus after the genocide) whereas we focus on the timing of socialization (young versus older women); second, we use different DHS survey rounds to track mothers and their children; and third, her sample consists of all partnered women, independent of having children.[31]

Importantly in our context, we do not assume that local empowerment disrupts the power balance in young women's relationships. Instead, young women have formed attitudes condemning violence

[31] La Mattina (2017) focuses on partnered women surveyed in 2005 and 2010, whereas our sample includes women-children-pairs across the three most recent DHS rounds (2010, 2015 and 2019). The distinction is not as much in our focus on women who are mothers (which most partnered women in Rwanda are), but rather the additional constraint that we introduce in sampling women whose (young adult) children have been interviewed by the DHS as well. While this could explain why our finding differs – alongside the differing research questions (marriage timing versus socialization) – it needs to be noted that Nordenving et al. (2023) also obtains the opposite result from La Mattina (2017) for their sample of partnered women (using DHS rounds 2005, 2010, and 2015), highlighting that younger women delay marriage rather than making unfavorable matches.

Table 7
Potential Mechanisms: Young Mother's Empowerment.

	Labor Force Participation		Bargaining Power	Intimate Partner Violence	
	Currently Working (1)	Cash Income (2)	(3)	IPV Experience (4)	IPV Attitudes (5)
Panel A: Partnered Women					
Genocide Intensity × Young Mother	0.029*	0.008	0.018	−0.159***	0.008
	(0.015)	(0.022)	(0.025)	(0.061)	(0.025)
Observations	4965	4539	3581	1214	4964
R^2	0.206	0.278	0.193	0.295	0.226
Panel B: Partnered and Unpartnered Women					
Genocide Intensity × Young Mother	0.029**	0.020	−0.0003	−0.115***	0.018
	(0.013)	(0.021)	(0.030)	(0.044)	(0.026)
Observations	7591	6920	4180	1980	7590
R^2	0.178	0.260	0.186	0.247	0.199
Individual Controls	✓	✓	✓	✓	✓
Survey round FE	✓	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓	✓
Year of Birth FE	✓	✓	✓	✓	✓
Province linear trend	✓	✓	✓	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with a mean of zero and a standard deviation of one based on the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "Currently Working" is measured using a binary variable that takes the value of 1 if the mother is currently in formal or informal employment. "Cash Income" is a binary variable taking the value of 1 if the respondent is paid in cash or kind for their work. "Bargaining Power" is an index that measures the sum of affirmative items to questions on the mother's involvement in the final say of the family on i) her own health care, ii) making large household purchases, iii) making household purchases for daily needs, iv) visits to family or relatives and v) the food to be cooked each day. "IPV Experience" is a binary variable that takes the value of 1 if the respondent has experienced physical violence by their partner in the past 12 months. "IPV Attitudes" are measured using the sum score of affirmative items, on the respondent's opinion that a husband is justified in hitting or beating his wife when i) she goes out without telling him, ii) she neglects the children, iii) she argues with him, iv) she refuses to have sex with him or v) she burns the food. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

during their adolescence – most likely prior to entering long-term partnerships. As such, we would expect that women partner with men who share their attitudes, in line with the assortative mating argument (Dohmen et al., 2012).

6.4.4. Assortative mating

To test assortative mating, we analyze a subsample of couples for which information is available on both mothers' and fathers' perceptions of VAC.[32] Fig. A.2 in the Appendix plots the difference between fathers' and mothers' VAC attitudes, aggregated over mothers' birth year and between low- and high-genocide violence communes. Across all birth years, the difference between a father's and a mother's VAC attitudes is close to zero. However, for regions affected by higher genocide violence, this difference becomes negative for younger mother's birth cohorts, indicating that younger women tend to partner with men who have even lower VAC attitudes than themselves in those regions. The opposite pattern is observed in less-violent regions. We thus find evidence of assortative mating in regions with high levels of genocide violence, consistent with young women exercising agency and empowerment in partner choice (Kim, 2023).

The central concern with assortative mating, however, is that women might have partnered with less violent men, in which case children's attitudes could be shaped by their fathers as much as their mothers, particularly for sons (Leight, 2021). We address this concern in two ways. First, in Table A.8, we split the sample of mothers into two groups depending on their partners' VAC attitudes (coded as 1 if the father agrees with any justification of corporal violence, 0 otherwise). If we find an effect independent of mothers VAC levels, it is likely that fathers are driving the results. Instead, the results show that we only observe the effects when considering mothers' VAC. The effects are concentrated in the sample of mothers who have low levels of VAC, making it unlikely that the effect is driven by fathers' VAC. Second, to further rule out that for the mothers with low VAC, their partners are driving the effect, we re-estimate our first stage, the interaction of young mothers and genocide intensity, using fathers' attitudes toward VAC as the dependent variable, both in the full sample and separately for households with sons and daughters. While the coefficients are negative, consistent with the assortative mating channel, they are close to zero and statistically insignificant (Table A.6). Moreover, the effects are smaller for sons and larger for daughters. Thus, although in households where women hold low VAC attitudes violence generally plays a minor role - since these women tend to partner with non-violent men - we find no evidence that fathers' attitudes are driving our main findings.

6.5. Alternative mechanisms

Our findings are consistent with genocide-induced empowerment as the primary channel through which younger mothers develop less violent attitudes and transmit them to the next generation. To strengthen this interpretation, and to ensure the robustness of our findings, we examine other potential mechanisms correlated with genocide violence and VAC that could drive the observed effects apart from women's empowerment.

6.5.1. Foreign aid

A possible reason for endogeneity concerns would be if foreign aid generally, but gender-targeted aid focused on female empowerment or mother and child well-being specifically, has been disproportionately allocated to areas with high past violence. Bridgeland et al. (2009) for instance, documents substantial investment and donor aid to education

[32] Please note that for these data on couples, the DHS only collects data on male-female relationships.

in the years after the genocide. Such aid could affect our results in two ways. On the one hand, if aid is correlated with genocide violence and an independent driver of the attitudes we observe, it could confound our results, making it important to control for foreign aid in the analysis. On the other hand, if aid forms part of the genocide-induced empowerment mechanism, it would act as a mediating variable on the causal pathway and thus represent a "bad control." For example, higher levels of genocide-related violence may have led to greater allocation of (gender-targeted) foreign aid in affected areas, which in turn caused empowerment and reduced violent attitudes. Controlling for aid in this scenario would incorrectly obscure the true causal channel (collider bias).

To address both of these concerns and test the robustness of our main findings, we collect geo-referenced data on foreign aid from the AidData project, specifically covering World Bank and Chinese development projects between 1995 and 2004, the post genocide period.[33] The dataset for Rwanda includes 21 aid projects (19 World Bank, 2 Chinese) across 91 locations (Table A.5). We construct buffers of 5 km, 7.5 km, and 10 km around each project location and merge the aid data with the DHS clusters. The presence of a foreign aid project is coded as 1 if a DHS household falls within the specified buffer range. Following Berlin et al. (2024) we define gender-targeted projects as those whose activity descriptions contain any of the following keywords: "women, female, girl, bride, maternal, gender, mother, genital, or child". Using this definition, we identify a total of three gender-targeted projects. Notably, none of these projects originates from the Chinese aid dataset, and the earliest gender-targeted World Bank projects were not implemented until 2000, a full five years after the genocide (Table A.5).

For the analysis, we first examine the cross-sectional relationship between genocide violence and foreign aid. Specifically, we estimate the effect of former genocide violence on the likelihood that a gender-targeted project has been implemented in the vicinity of a household. As shown in Table A.7, there is no systemic relationship between genocide violence and the allocation of foreign aid; in fact, by adding pre-genocide commune controls, several models even indicate a negative, though not statistically significant, relationship. We thus conclude that aid generally was not higher in genocide regions. While some models suggest positive effects for gender-targeted projects, these effects disappear once the 1991 pre-genocide controls are included. Moreover, since such projects only appear in 2000 - five years after the genocide - there is little reason to interpret them as a direct response to genocide violence.

While these patterns already suggest that aid is unlikely to confound our findings, we take a further step to strengthen the analysis by explicitly controlling for "(gender-targeted) aid" in our first-stage models. We also include the interaction between young mothers and aid to capture potential heterogeneous effects of aid on young mothers. As shown in Table 8, we do not find any significant effect of aid on mothers' attitudes on VAC. In fact, our estimated coefficient of *Genocide_Intensity×Young Mother* remains virtually unchanged (-0.075 to -0.077) relative to our main specification. This provides clear evidence that our results are not driven by foreign aid, and that including aid—whether general or gender-targeted—does not affect the estimated relationship between genocide violence and VAC.

6.5.2. Selection into marriage or partnerships

Another potential concern is that young women from genocide-affected regions may have been subjected to marriage and mating selection, which could bias our estimates. The genocide drastically altered local sex ratios, likely affecting who was able to marry or form stable unions (La Mattina, 2017). In heavily affected areas, women who did marry may have been positively selected for education, social capital, or progressive attitudes—traits also likely correlated with less violent attitudes toward children. Such selection could bias our sample toward younger mothers in treated areas who raise less violence-tolerant children for reasons unrelated to genocide-induced attitude change.

We address this concern in two ways. First, to test selection into partnerships, we re-estimate our main first stage specification now including women not in partnership (see Table A.4). We observe the same effects and thus rule out that selection into relationships (with already more progressive attitudes on VAC) is driving the results. Second, we follow a strategy similar to La Mattina (2017) and compare women depending on whether the mother married before or after the genocide, rather than exploiting variation in mother's age. We find no evidence that marriage timing affects VAC attitudes. The estimated coefficients are close to zero and statistically insignificant, suggesting that marriage or partnership selection is not driving our results (Table A.9).

6.5.3. Alternative intra-household channels

Finally, while we can rule out several mechanisms operating outside the household, including neighborhood environments, foreign aid, and marriage or mating selection, one might still be concerned that genocide affects children's attitudes not solely through the channel of mother's attitudes, but via other plausible mother-child channels, such as the mother's experiences of domestic violence or her labor force participation. These alternative intra-household pathways could violate the exclusion restriction. For example, the effect on children's attitudes could arise not from the mother's attitudes per se, but from observing her in employment or by experiencing lower levels of IPV violence. In Table 7, we showed that both are proxies for female empowerment and potential mechanisms at the first stage. Yet, whether these variables represent plausible alternative channels for the second stage remains unclear.

While we cannot directly test the exclusion restriction, we address this concern by re-estimating our second-stage models using i) the (young) mother's experience of physical IPV and ii) her labor force participation as endogenous variables (Table A.10). Using the same instrumental variable as in our main specification, we find no evidence that mothers' exposure to (less) domestic violence or their labor force participation—though predictive of empowerment in the first stage—directly affects children's attitudes. In other words, although these measures capture aspects of empowerment, they do not explain the intergenerational effect on children's VAC attitudes and they are not themselves the channel through which children's VAC attitudes are formed.

Importantly, this does not imply that the effect is not driven by empowerment; rather, it suggests that changes in children's VAC attitudes operate primarily through the mother's attitudes (norms), rather than through her labor force participation or experiences of IPV. These observable empowerment measures capture some aspects of empowerment but cannot explain the adoption of attitudes and the way children internalize maternal norms. The evidence strongly indicates that mothers' attitudes are internalized by and reproduced in their children. While we cannot pin down the exact pathway from mothers' norms to children's norms, this transmission likely operates via multiple complementary channels, including role modeling, socialization, household decision-making and rules, and the emotional and normative environment mothers create. While we are confident—based on our results—that intergenerational transmission of attitudes is the mechanism at work, studying the precise pathways is limited by the available data, and we therefore highlight this as an important avenue for future research.

[33] The World Bank data is obtained from the AidData project (World Bank IBRD-IDA, Version 1.4.1) and includes all World Bank projects approved from 1995–2004. The data on Chinese aid projects is obtained from the Global Chinese Development Finance Dataset, Version 3.0 from the AidData project. The Chinese data only covers a period from 2000 onward. See Knutsen & Kotsadam (2020) and Dreher et al. (2021) for a detailed description of the respective data collection and methodology.

Table 8
First stage results controlling for (gender-targeted) aid.

	VAC mother any					
	(1)	(2)	(3)	(4)	(5)	(6)
Genocide Intensity × Young Mother	−0.076***	−0.075***	−0.076***	−0.077***	−0.077***	−0.079***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Aid project in 5 km	0.043					
	(0.032)					
Aid project in 5 km × Young Mother	0.00					
	(0.043)					
Aid project in 7.5 km		0.062**				
		(0.026)				
Aid project in 7.5 km × Young Mother		−0.037				
		(0.037)				
Aid project in 10 km			0.017			
			(0.030)			
Aid project in 10 km × Young Mother			−0.056			
			(0.035)			
Gender-targeted aid project in 5 km				0.029		
				(0.061)		
Gender-targeted aid project in 5 km × Young Mother				0.055		
				(0.072)		
Gender-targeted aid project in 7.5 km					0.021	
					(0.056)	
Gender-targeted aid project in 7.5 km × Young Mother					0.007	
					(0.048)	
Gender-targeted aid project in 10 k						−0.032
						(0.052)
Gender-targeted aid project in 10 km × Young Mother						0.017
						(0.043)
Observations	4965	4965	4965	4965	4965	4965
R^2	0.165	0.166	0.164	0.165	0.164	0.164
Individual controls	✓	✓	✓	✓	✓	✓
Survey round FE	✓	✓	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓
Province specific linear trend	✓	✓	✓	✓	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the mother agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother sum*" is the sum score of affirmative items, standardized to mean zero and standard deviation one. "Aid project" is a binary variable indicating whether the surveyed individual resides within a 5–10 km radius of an official aid project site. A project is labeled as gender-targeted if its activity list includes any of the following words: *women, female, girl, bride, maternal, gender, mother, genital, or child*. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

7. Conclusion

This paper examines the long-term effects of mass violence on violent attitudes across generations. We are the first to show that women exposed to high violence and (post)-genocide socialization conditions hold significantly less violent attitudes and transmit these attitudes to their children. Across specifications, a one-standard-deviation increase in genocide intensity reduces VAC attitudes among younger women by 7 percentage points. We also find strong causal evidence of intergenerational transmission, showing that children of women socialized during the genoucde hold significantly less violent attitudes. We estimate transmission parameters of around 38 percentage points, indicating that second-generation attitudes are strongly shaped by their mothers' attitudes. Our results suggest that these effects are driven by an increase in women's empowerment following the 1994 genocide. Young women exposed to the genocide were socialized in environments condemning violence, experienced lower levels of domestic violence themselves, were more likely to participate in the labor force, and partnered with men who held less violent attitudes.

As such, our findings underscore and add to previous evidence on the conflict-prosociality link by demonstrating that exposure to mass violence, under certain institutional and gender conditions, can reduce support for violence. We tie in with previous studies, such as Annan et al. (2011); Bauer et al. (2016), demonstrating that resilience and cooperation tends to be the norm following violent conflict.

The results carry important implications for future research. First, our findings highlight the importance of investigating both local and temporal dynamics of conflicts to understand the dynamics and variation in micro-level outcomes of mass violence victims. Second, by investigating the potential cascading effects of women's empowerment following mass violence, we provide evidence that may help explain how violent cycles can be mitigated across generations. Research that further explores the mechanisms and conditions under which children adopt their parents' beliefs and attitudes—particularly regarding violence—remains critically important. Third, our paper is not without limitations. We rely on self-reported attitudes which can suffer from social desirability bias. Studies on IPV have shown that survey techniques that afford participants more privacy – such as ACASI or list experiments – can help reduce non-response and elicit truthful answers to sensitive questions (Cullen, 2022). Similar studies that focus on reporting (attitudes toward) violence against children have – to the best of our knowledge – not been conducted yet. Investigating how different survey modes might influence disclosure of attitudes on the one hand, and experiences and

perpetration of VAC on the other hand, would be a very important research agenda. Furthermore, the data we use do not allow us to make claims about violent behavior. Future research might benefit from experimentally measuring (proclivity to) violent behavior in experiments with young adults and their parents, who, akin to our design, come from different age groups and have survived an event of mass violence. Finally, while our findings strongly suggest that intergenerational transmission of attitudes is the most plausible mechanism, our data limit the ability to precisely identify the pathways through which children internalize maternal attitudes. Extending the analysis by experimentally investigating the exact transmission channel therefore represents another important avenue for future research.

CRediT authorship contribution statement

Daniel Kammer: Writing – review & editing, Writing – original draft, Visualization, Project administration, Methodology, Conceptualization. **Alina Greiner-Filsinger:** Writing – review & editing, Writing – original draft, Visualization, Project administration, Methodology, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgements

We thank the editor and the three anonymous reviewers for their thoughtful comments. We are grateful to Jule Beck, Tilman Brueck, Sabine Carey, Maximilian Filsinger, Anke Hoeffler, Kelly Kadera, Marie Kammer, Marie-Therese Meye, Guido Schwerdt, Wolfgang Stojetz and participants at the European Political Science Association Annual Conference 2023, Jan Tinbergen European Peace Science Annual Conference 2023, AFK Empirical Peace and Conflict Research Workshop 2023 for their generous feedback on previous iterations of this paper. We thank Dusan Ristic and Jacob Westerholt for their excellent research assistance.

Appendix A. Additional tables and figures

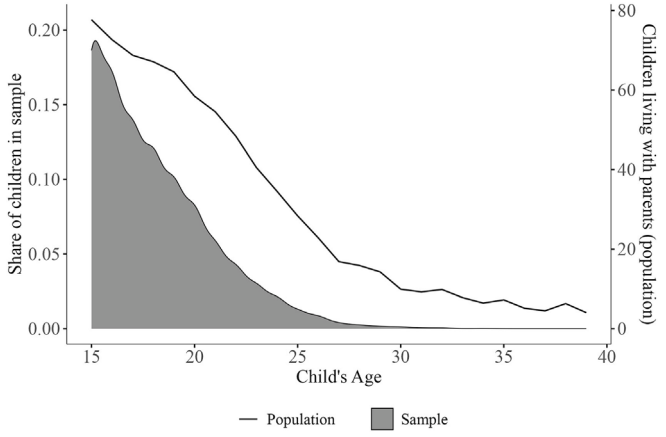

Fig. A.1. The figure shows the share of second generation individuals in our sample, hence (young) adults that live in the same household as their mother, and the share of individuals who do live in the same household as their mother (representative of the general Rwandan population).

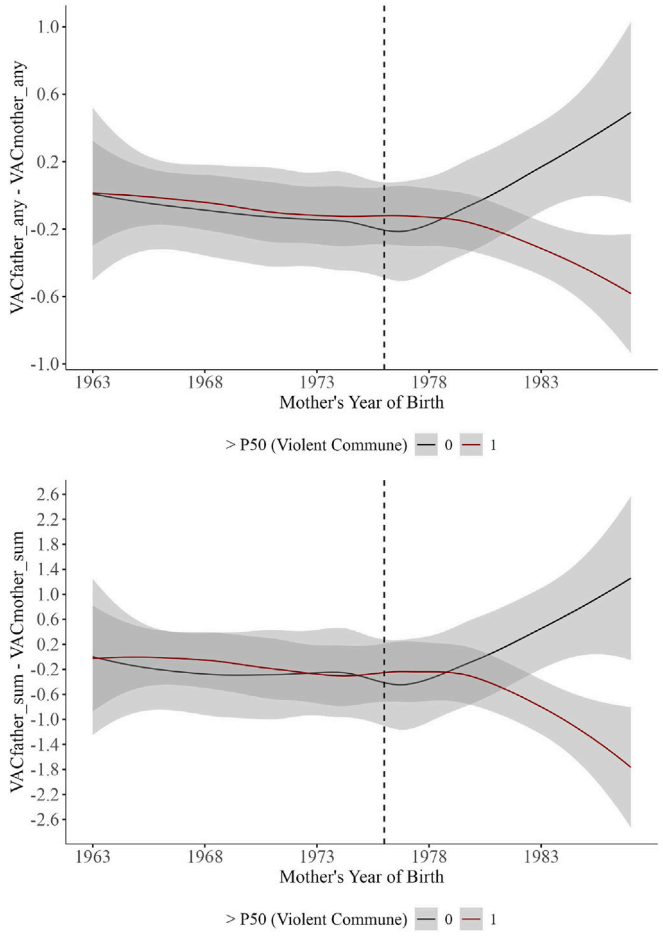

Fig. A.2. Subset of main data restricted to couples. The figures display local polynomial regressions of the difference in VAC attitudes between fathers and mothers across violent and non-violent communes, aggregated over the mother's year of birth. The binary variable > *P50(Violent Commune)* is equal to 1 if the number of category 1 and category 2 *gacaca* court trials records in a commune exceeds the 50th percentile of the distribution. The vertical bar represents the cutoff by which we divide the group of mothers in those old enough to be socialized in a pre-genocide environment (born 1976 or earlier) and those young enough to be socialized in a post-genocide environment (born 1977 or later).

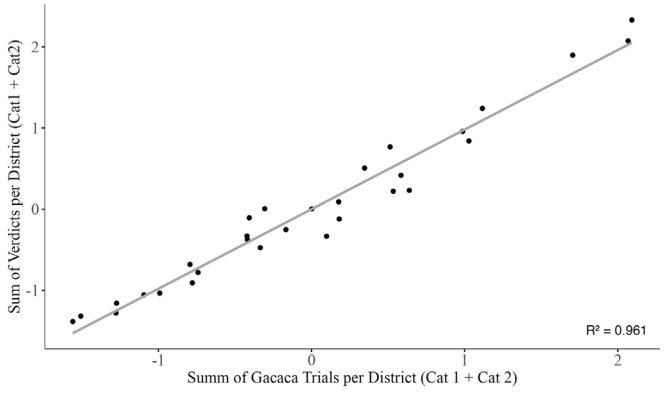

Fig. A.3. Correlation between *gacaca* trials and verdicts, both aggregated at the district level (n = 30).

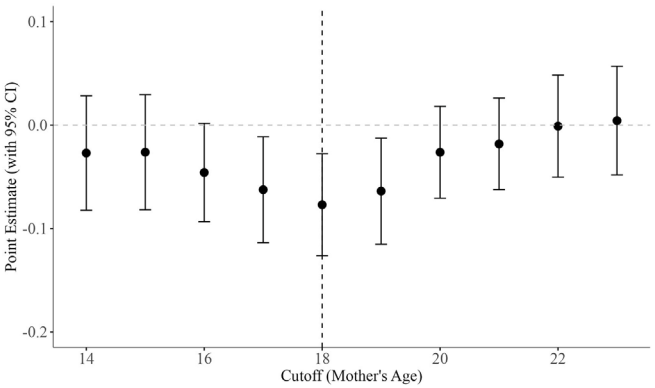

Fig. A.4. The figure shows fully specified first-stage estimations using different age-cutoffs to determine mothers in their formative years (young mother). 95 % confidence intervals are based on clustered standard errors at the commune level. Individuals below a given age threshold at the time of the genocide are assigned to the treatment group, while those above are assigned to the control group. As the age threshold is lowered, individuals most likely to be affected—those near the original cutoff—are assigned to the control group, potentially reducing the observable differences between groups.

Table A.1
The effect of mothers' VAC on children's VAC (child sum).

	VAC child sum								
	OLS	IV		OLS	IV		OLS	IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VAC mother any	0.615***	1.64***	1.74***	0.503***	1.23***	1.19***	0.435***	1.01***	1.03**
	(0.051)	(0.376)	(0.389)	(0.048)	(0.404)	(0.409)	(0.046)	(0.351)	(0.397)
Observations	4,965	4,965	4,965	4,965	4,965	4,965	4,965	4,965	4,965
R^2	0.0528	−0.0955	−0.1241	0.1106	0.0343	0.0419	0.1501	0.1029	0.1000
Individual controls				✓	✓	✓	✓	✓	✓
Survey round FE				✓	✓	✓	✓	✓	✓
Commune FE				✓	✓	✓	✓	✓	✓
Year of birth FE				✓	✓	✓	✓	✓	✓
Province specific linear trend							✓	✓	✓
IV-variable (genocide measure)		Linear	Dummy		Linear	Dummy		Linear	Dummy

Note: Instrumental variable at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal to or above the median violent commune (dummy), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "$VAC\ mother\ any$" is measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. "$VAC\ child\ any$" is measured as the sum of affirmative items. Regressions are estimated using OLS/2SLS with linear and dummy instruments at the first stage respectively. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

Table A.2
The effect of mothers' VAC (mother sum) on children's VAC.

	VAC child any								
	OLS	IV		OLS	IV		OLS	IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VAC_mother_sum	0.068***	0.121***	0.129***	0.057***	0.107*	0.102*	0.051***	0.121**	0.109**
	(0.006)	(0.043)	(0.043)	(0.006)	(0.056)	(0.057)	(0.006)	(0.047)	(0.050)
Observations	4,965	4,965	4,965	4,965	4,965	4,965	4,965	4,965	4,965
R^2	0.0482	0.0194	0.0104	0.0886	0.0549	0.0597	0.1278	0.0744	0.0884
Individual controls				✓	✓	✓	✓	✓	✓
Survey round FE				✓	✓	✓	✓	✓	✓
Commune FE				✓	✓	✓	✓	✓	✓
Year of birth FE				✓	✓	✓	✓	✓	✓
Province specific linear trend							✓	✓	✓
IV-variable (genocide measure)		Linear	Binary		Linear	Binary		Linear	Binary

Note: Instrumental variable at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal to or above the median violent commune (binary), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC child any*" is measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother any*" is measured as the sum of affirmative items. Regressions are estimated using OLS/2SLS with linear and binary instruments at the first stage respectively. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

Table A.3
Transmission of Violent Attitudes (post 1994-born only).

	VAC child any								
	OLS	IV		OLS	IV		OLS	IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VAC_mother_any	0.198***	0.507***	0.545***	0.176***	0.385**	0.395**	0.152***	0.405***	0.396**
	(0.021)	(0.130)	(0.133)	(0.021)	(0.158)	(0.165)	(0.020)	(0.145)	(0.154)
Observations	3293	3293	3293	3293	3293	3293	3293	3293	3293
R^2	0.049	−0.070	−0.102	0.089	0.026	0.021	0.139	0.057	0.062
Individual controls				✓	✓	✓	✓	✓	✓
Survey round FE				✓	✓	✓	✓	✓	✓
Commune FE				✓	✓	✓	✓	✓	✓
Year of birth FE				✓	✓	✓	✓	✓	✓
Province specific linear trend							✓	✓	✓
IV-variable (genocide measure)		Linear	Dummy		Linear	Dummy		Linear	Dummy

Note: Subsample of main sample including only post 1994 born children. Instrumental variable at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal to or above the median violent commune (dummy), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC mother any*" and "*VAC child any*" are measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using OLS/2SLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

Table A.4
Impact of genocide on mothers' VAC attitudes including women not in partnership.

	VAC mother any (1)	VAC mother sum (2)
Genocide Intensity × Young Mother	−0.061***	−0.151**
	(0.023)	(0.064)
Observations	7591	7591
R^2	0.15	0.17
Individual controls	✓	✓
Survey round FE	✓	✓
Commune FE	✓	✓
Year of birth FE	✓	✓
Province specific linear trend	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the mother agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother sum*" is the sum score of affirmative items, standardized to mean zero and standard deviation one. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

Table A.5
Number of aid projects by year in Rwanda.

	World Bank		Chinese Development	
Year	All Projects	Gender-targeted	All Projects	Gender-targeted
1995	1	0	0	0
1996	0	0	0	0
1997	2	0	0	0
1998	1	0	0	0
1999	2	0	0	0
2000	4	1	0	0
2001	3	0	0	0
2002	2	1	1	0
2003	1	1	0	0
2004	3	0	1	0
Total	19	3	2	0

Note: The World Bank data is obtained from the AidData project (World Bank IBRD-IDA, Version 1.4.1) and includes all World Bank projects approved from 1995–2004. The data on Chinese aid projects is obtained from the Global Chinese Development Finance Dataset, Version 3.0 from the AidData project. The Chinese data only covers a period from 2000 onward. See Knutsen & Kotsadam (2020) and Dreher et al. (2021) for a detailed description of the respective data collection and methodology. Projects are labeled as gender-targeted if its activity list includes any of the following words: *women, female, girl, bride, maternal, gender, mother, genital, or child*.

Table A.6
The effect of genocide violence on fathers' VAC.

	VAC father any		
	Full sample	Sons	Daughters
	(1)	(2)	(3)
Genocide Intensity × Young Mother	−0.074	−0.053	−0.099
	(0.056)	(0.075)	(0.074)
Observations	1,932	984	948
R^2	0.47	0.52	0.56
Individual controls	✓	✓	✓
Survey round FE	✓	✓	✓
Commune FE	✓	✓	✓
Year of birth FE	✓	✓	✓
Province specific linear trend	✓	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "*VAC father any*" is measured using a binary variable that takes the value of 1 if the father agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. Column (1) reports results for the full sample of households. Column (2) restricts the sample to households with at least one son, while Column (3) restricts the sample to households with at least one daughter. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.7
The Effect of Genocide Violence on Foreign Aid.

	Aid project in 5 km		Aid project in 7.5 km		Aid project in 10 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Aid Projects						
Genocide Intensity	0.010	−0.03	0.02	−0.005	0.006	−0.01
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Observations	4,965	4,965	4,965	4,965	4,965	4,965
R^2	0.00	0.10	0.00	0.05	0.00	0.05
Panel B: Gender-targeted Projects only						
Genocide Intensity	0.02	0.003	0.05*	0.01	0.08**	0.04
	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
Observations	4,965	4,965	4,965	4,965	4,965	4,965
R^2	0.00	0.13	0.01	0.18	0.02	0.22
Commune controls		✓		✓		✓

Note: "Aid project" is a binary variable indicating whether the surveyed individual resides within a 5–10 km radius of an official aid project site. In Panel A, the effect is estimated using all aid projects, regardless of target group, that were registered by the World Bank and started between 1995 and 2005. In Panel B, the effect is estimated using gender-targeted projects only, where a project is labeled as gender-targeted if its activity list includes any of the following words: *women, female, girl, bride, maternal, gender, mother, genital, or child*. "Genocide intensity" is measured as a continuous index with a mean of zero and a standard deviation of one based on the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. Commune controls are obtained from the 1991 Census and include population density, literacy rate, and literacy rates for men and women. Standard errors in parentheses are clustered at the commune level. Significance levels are indicated as $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.8
Effect of genocide on mothers' VAC attitudes by father's VAC.

VAC father any	VAC mother any		VAC mother sum		VAC mother any		VAC mother sum	
	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Genocide Intensity × Young Mother	−0.112*	0.069	−0.240	0.200				
	(0.062)	(0.076)	(0.162)	(0.225)				
> P50(Genocide Intensity) × Young Mother					−0.401***	−0.007	−0.870**	−0.221
					(0.126)	(0.113)	(0.344)	(0.365)
Observations	784	1150	784	1150	784	1150	784	1150
R^2	0.47	0.40	0.50	0.43	0.48	0.40	0.50	0.43
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓
Survey Round FE	✓	✓	✓	✓	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓	✓	✓	✓	✓
Mother Year of Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-specific linear Trend	✓	✓	✓	✓	✓	✓	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. > P50(Genocide intensity) is equal to 1 if the number 1 and 2 gacaca trails at the commune level are above the median violent commune. "*VAC mother any*" and "*VAC father any*" is measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother sum*" is the sum score of affirmative items, standardized to mean zero and standard deviation one. Regressions are estimated using OLS. Individual controls include mother's and father's education years, father's birth year, a family wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

Table A.9
The effect of year of marriage on mothers' VAC attitudes.

	VAC mother any					VAC mother sum
	(1)	(2)	(3)	(4)	(5)	(6)
Genocide Intensity	0.015	−0.007	0.011			
	(0.012)	(0.013)	(0.016)			
Married after 1995		0.044**	0.057**			
		(0.021)	(0.024)			
Genocide Intensity × Married after 1994		−0.007	−0.0004	0.011	−0.021	−0.076
		(0.021)	(0.021)	(0.022)	(0.028)	(0.071)
Observations	4,965	4967	4965	4965	4965	4965
R^2	0.050	0.028	0.051	0.117	0.162	0.190
Individual controls	✓		✓	✓	✓	✓
Commune controls	✓		✓			
Survey round FE	✓		✓	✓	✓	✓
Commune FE				✓	✓	✓
Year of birth FE				✓	✓	✓
Province specific linear trend					✓	✓

Note: "Married after 1994" is defined as a mother who married after the genocide in 1994. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the mother agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother sum*" is the sum score of affirmative items, standardized to mean zero and standard deviation one. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. Commune controls are obtained from the 1991 Census and include population density, literacy rate, and literacy rate for men and women. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

Table A.10
Alternative channels on children's VAC.

	VAC child any								
	OLS	IV/2SLS		OLS	IV/2SLS		OLS	IV/2SLS	
Domestic Violence (mother)	0.006	0.115	0.141	−0.012	−0.003	−0.004	−0.015	0.055	0.086
	(0.030)	(0.122)	(0.134)	(0.028)	(0.097)	(0.101)	(0.029)	(0.095)	(0.104)
Observations	1215	1215	1215	1111	1111	1,111	1111	1111	1111
R^2	0.01	0.01	0.02	0.09	0.06	0.06	0.19	0.16	0.15
Currently Working (mother)	0.036	0.096	0.343	0.031	−0.068	0.258	0.051**	0.084	0.086
	(0.024)	(0.193)	(0.234)	(0.021)	(0.164)	(0.202)	(0.023)	(0.242)	(0.244)
Observations	4965	4965	4965	4540	4539	4539	4539	4539	4539
R^2	0.00	0.01	0.03	0.05	0.04	0.02	0.11	0.10	0.10
Individual Controls				✓	✓	✓	✓	✓	✓
Survey Round FE				✓	✓	✓	✓	✓	✓
Commune FE				✓	✓	✓	✓	✓	✓
Year of Birth FE				✓	✓	✓	✓	✓	✓
Province time trend							✓	✓	✓
IV-variable		Linear	Binary		Linear	Binary		Linear	Binary

Note: Instrumental variable at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal to or above the median violent commune (binary), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC child any*" are measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. "Currently Working" is measured using a binary variable that takes the value of 1 if the mother is currently in formal or informal employment. "Domestic Violence" is a binary variable that takes the value of 1 if the respondent has experienced physical violence by their partner in the past 12 months. Regressions are estimated using OLS/2SLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

Appendix B. *Gacaca* categories

Please note that this overview is taken from Nyseth Brehm et al. (2014). The initial categorization classified acts of participation into four categories, but was later adapted to the three categories below (Corey & Joireman, 2004; Republic of Rwanda, 1996).

Category 1

1. Any person who committed or was an accomplice in the commission of an offense that places him or her in the category of planners or organizers of the genocide or crimes against humanity;
2. Any person who was at a national leadership level or that of prefecture (state) level— including those serving in public administration, political parties, army, gendarmerie, religious denominations, or a militia—who committed crimes of genocide or crimes against humanity or encouraged others to participate in such crimes, together with his or her accomplice;
3. Any person who committed or was an accomplice in the commission of offense that places him or her among the category of people who incited, supervised, and were ringleaders of genocide or crimes against humanity;
4. Any person who was at the leadership level at the sub-prefecture and commune (municipality)— including those serving in public administration, political parties, army, gendarmerie, communal police, religious denominations, or a militia—who committed any crimes of genocide or other crimes against humanity or encouraged others to commit similar offenses, together with his or her accomplice; and
5. Any person who committed the offense of rape or sexual torture, together with his or her accomplice.

Category 2

1. A notorious murderer who distinguished himself or herself in his or her location or wherever he or she passed due to the zeal and cruelty used, together with his or her accomplice;
2. Any person who tortured another even though such torture did not result into death, together with his or her accomplice;
3. Any person who committed a dehumanizing act on a dead body, together with his or her accomplice;
4. Any person who committed or was an accomplice in the commission of an offense that places him or her on the list of people who killed or attacked others resulting in death, together with his or her accomplice;
5. Any person who injured or attacked another with the intention to kill but such intention was not fulfilled, together with his or her accomplice; and
6. Any person who committed or aided another to commit an offense against another without an intention to kill, together with his or her accomplice

Category 3

1. A person who committed only an offense related to property. However, when the offender and the victim reached a settlement and resolved the matter before authorities or witnesses prior to the commencement of legal proceedings, the offender was not prosecuted.

Appendix C. Supplementary data

Supplementary data to this article can be found online at doi:10.1016/j.worlddev.2025.107266.

Data availability

The authors do not have permission to share data.

References

Abbott, P., Mugisha, R., & Sapsford, R. (2018). Women, land and empowerment in Rwanda. *Journal of International Development*, *30*, 1006–1022.

Alan, S., Baydar, N., Boneva, T., Crossley, T. F., & Ertac, S. (2017). Transmission of risk preferences from mothers to daughters. *Journal of Economic Behavior & Organization*, *134*, 60–77.

Angrist, J. D., & Pischke, J.-S. (2009). Mostly harmless econometrics: an empiricist's companion. Princeton, New Jersey: Princeton University Press.
Annan, J., Blattman, C., Mazurana, D., & Carlson, K. (2011). Civil war, reintegration, and gender in northern Uganda. *Journal of Conflict Resolution*, *55*, 877–908.
Annan, J., Donald, A., Goldstein, M., Gonzalez Martinez, P., & Koolwal, G. (2021). Taking power: women's empowerment and household well-being in sub-Saharan Africa. *World Development*, *140*, 105292.
Arvate, P., Firpo, S., & Pieri, R. (2021). Can women's performance in elections determine the engagement of adolescent girls in politics? *European Journal of Political Economy*, *70*, 102045.
Baker, A., Callaway, B., Cunningham, S., Goodman-Bacon, A., & Sant'Anna, P. H. C. (2025). Difference-in-Differences Designs: A Practitioner's Guide, arXiv preprint arXiv:2503.13323.
Baker-Henningham, H., Meeks-Gardner, J., Chang, S., & Walker, S. (2009). Experiences of violence and deficits in academic achievement among urban primary school children in Jamaica. *Child Abuse & Neglect*, *33*, 296–306.
Bakken, I. V., & Buhaug, H. (2021). Civil war and female empowerment. *Journal of Conflict Resolution*, *65*, 982–1009.
Barr, A. L., Knight, L., França-Junior, I., Allen, E., Naker, D., & Devries, K. M. (2017). Methods to increase reporting of childhood sexual abuse in surveys: the sensitivity and specificity of face-to-face interviews versus a sealed envelope method in Ugandan primary school children. *BMC International Health and Human Rights*, *17*, 4.
Bauer, M., Blattman, C., Chytilová, J., Henrich, J., Miguel, E., & Mitts, T. (2016). Can war foster cooperation? *Journal of Economic Perspectives*, *30*, 249–274.
Becker, G. S. (1993). A treatise on the family: enlarged edition. Harvard University Press.
Bellows, J., & Miguel, E. (2009). War and local collective action in Sierra Leone. *Journal of Public Economics*, *93*, 1144–1157.
Berlin, M. P., Bonnier, E., & Olofsgård, A. (2024). Foreign aid and female empowerment. *The Journal of Development Studies*, *60*, 662–684.
Berry, M. E. (2015). From violence to mobilization: women, war, and threat in Rwanda*. *Mobilization: An International Quarterly*, *20*, 135–156.
Berry, M. E. (2018). War, women, and power: from violence to mobilization in Rwanda and Bosnia-Herzegovina. Cambridge: Cambridge University Press.
Black, S. E., Devereux, P. J., & Salvanes, K. G. (2005). Why the Apple doesn't fall far: understanding intergenerational transmission of human capital. *American Economic Review*, *95*, 437–449.
Blattman, C. (2009). From violence to voting: war and political participation in Uganda. *American Political Science Review*, *103*, 231–247.
Bonnier, E., Poulsen, J., Rogall, T., & Stryjan, M. (2020). Preparing for genocide: quasi-experimental evidence from Rwanda. *Journal of Development Economics*, *147*, 102533.
Bridgeland, J., Wulsin, S., & McNaught, M. (2009). Rebuilding Rwanda: from genocide to prosperity through education. *Civic Enterprises*.
Bundervoet, T., & Fransen, S. (2018). The educational impact of shocks in utero: evidence from Rwanda. *Economics & Human Biology*, *29*, 88–101.
Callaway, B., Goodman-Bacon, A., & Sant'Anna, P. H. C. (2024). Difference-in-differences with a continuous treatment, Working Paper 32117, National Bureau of Economic Research.
Campante, F., & Yanagizawa-Drott, D. (2015). The intergenerational transmission of war. *NBER Working Paper*, w21371.
Campbell, D. E., & Wolbrecht, C. (2006). See Jane Run: women politicians as role models for adolescents. *The Journal of Politics*, *68*, 233–247.
Caruso, G. D. (2017). The legacy of natural disasters: the intergenerational impact of 100 years of disasters in Latin America. *Journal of Development Economics*, *127*, 209–233.
Cavatorta, E., Zizzo, D. J., & Daoud, Y. (2023). Conflict and reciprocity: a study with Palestinian youths. *Journal of Development Economics*, *160*, 102989.
Cesur, R., & Sabia, J. J. (2016). When war comes home: the effect of combat service on domestic violence. *Review of Economics and Statistics*, *98*, 209–225.
Chin, Y.-M., Cunningham, S., & Van, P. H. (2023). The long-term effects of the Rwandan Genocide on child work. *Economic Development and Cultural Change*, *72*, 329–360.
Corey, A., & Joireman, S. F. (2004). Retributive justice: the Gacaca courts in Rwanda. *African Affairs*, *103*, 73–89.
Cuartas, J. (2021). Corporal punishment and early childhood development in 49 low- and middle-income countries. *Child Abuse & Neglect*, *120*, 105205.
Cullen, C. (2022). Method matters: the underreporting of intimate partner violence. *The World Bank Economic Review*, *37*, 49–73.
Cunningham, M. (2001). The influence of parental attitudes and behaviors on children's attitudes toward gender and household labor in early adulthood. *Journal of Marriage and family*, *63*, 111–122.
Des Forges, A. L. (1999). "leave none to tell the story": genocide in Rwanda. New York: Human Rights Watch.
DHS Rwanda. (2012). Rwanda Demographic and Health Survey 2010, Final Report, Calverton, Maryland: National Institute of Statistics of Rwanda, Ministry of Health of Rwanda and ICF International.
DHS Rwanda. (2016). Rwanda Demographic and Health Survey 2014-15, Final Report, Rockville, Maryland: National Institute of Statistics of Rwanda, Ministry of Health of Rwanda and ICF International.
DHS Rwanda. (2021). Rwanda Demographic and Health Survey 2019-20, Final Report, Rockville, Maryland: National Institute of Statistics of Rwanda, Ministry of Health of Rwanda and ICF International.
Dinas, E. (2014). The long shadow of parental political socialization on the development of political orientations. *The Forum*, *12*, 397–416.
Doerr, C. M., Hoeffler, A., Goessmann, K., Olorunlambe, W., & Hecker, T. (2023). Sexual violence affects adolescents' health and prosocial behaviour beyond other violence exposure. *European Journal of Psychotraumatology*, *14*, 2263319.
Dohmen, T., Falk, A., & David, H. (2012). The intergenerational transmission of risk and trust attitudes. *Review of Economic Studies*, *79*, 645–677.
Dreher, A., Fuchs, A., Parks, B., Strange, A., & Tierney, M. J. (2021). Aid, China, and growth: evidence from a new global development finance dataset. *American Economic Journal: Economic Policy*, *13*, 135–174.
Duflo, E. (2012). Women empowerment and economic development. *Journal of Economic Literature*, *50*, 1051–1079.
Eichelsheim, V., Berckmoes, L., Hola, B., Rutayisire, T., & Richters, A. (2019). Before my time? addressing the intergenerational legacies of the Genocide against the Tutsi in Rwanda. *Intervention*, *17*, 31–39.
Erten, B., & Keskin, P. (2020). Breaking the cycle? education and the intergenerational transmission of violence. *The Review of Economics and Statistics*, *102*, 252–268.
Fernández, R., Fogli, A., & Olivetti, C. (2004). Mothers and sons: preference formation and female labor force dynamics. *The Quarterly Journal of Economics*, *119*, 1249–1299.
Gautier, T. (2025). From missing brothers to educated sisters: the effects of victimization during the Rwandan genocide. *World Development*, *191*, 106945.
Gay, V. (2023). The intergenerational transmission of World War i on female labour. *The Economic Journal*, *133*, 2303–2333.
Guariso, A., & Verpoorten, M. (2019). Armed conflict and schooling in Rwanda: digging deeper. *Peace Economics, Peace Science and Public Policy*, *25*, 20180033.
Hahn, J., & Hausman, J. (2003). Weak instruments: diagnosis and cures in empirical econometrics. *American Economic Review*, *93*, 118–125.
Heldring, L. (2021). The origins of violence in Rwanda. *The Review of Economic Studies*, *88*, 730–763.
Hintjens, H. M. (1999). Explaining the 1994 genocide in Rwanda. *The Journal of Modern African Studies*, *37*, 241–286.
Hoeffler, A. (2017). Violence against children: a critical issue for development. *The European Journal of Development Research*, *29*, 945–963.
Ireland, T. O., & Smith, C. A. (2009). Living in partner-violent families: developmental links to antisocial behavior and relationship violence. *Journal of Youth and Adolescence*, *38*, 323–339.
Kabeer, N. (1999). Resources, agency, achievements: reflections on the measurement of women's empowerment. *Development and Change*, *30*, 435–464.
Kim, D. (2023). Women's higher education, assortative mating, and empowerment: long-term evidence from college enrollment expansion in South Korea. *The Journal of Development Studies*, *59*, 430–447.
Knutsen, T., & Kotsadam, A. (2020). The political economy of aid allocation: aid and incumbency at the local level in sub Saharan Africa. *World Development*, *127*, 104729.
Kraehnert, K., Brück, T., Di Maio, M., & Nisticò, R. (2019). The effects of conflict on fertility: evidence from the genocide in Rwanda. *Demography*, *56*, 935–968.
Kreft, A.-K. (2019). Responding to sexual violence: women's mobilization in war. *Journal of Peace Research*, *56*, 220–233.
La Mattina, G. (2017). Civil conflict, domestic violence and intra-household bargaining in post-genocide Rwanda. *Journal of Development Economics*, *124*, 168–198.
Lansford, J. E., Zietz, S., Putnick, D. L., Deater-Deckard, K., Bradley, R. H.Costa, M. …Bornstein, M. H. (2020). Men's and women's views on acceptability of husband-to-wife violence and use of corporal punishment with children in 21 low- and middle-income countries. *Child Abuse & Neglect*, *108*, 104692.
Laszlo, S., Grantham, K., Oskay, E., & Zhang, T. (2020). Grappling with the challenges of measuring women's economic empowerment in intrahousehold settings. *World Development*, *132*, 104959.
Lazarev, E. (2019). Laws in conflict: legacies of war, gender, and legal pluralism in Chechnya. *World Politics*, *71*, 667–709.
Leight, J. (2021). Like father, like son, like mother, like daughter: intergenerational transmission of intrahousehold gender attitudes in Ethiopia. *World Development*, *142*, 105359.
Lupu, N., & Peisakhin, L. (2017). The legacy of political violence across generations. *American Journal of Political Science*, *61*, 836–851.
Mannheim, K. (1952). The problem of generations. In Essays on the sociology of knowledge (pp. 276–322). New York: Oxford University Press.
Moyo, E., Moyo, P., Mangwana, H., Murewanhema, G., & Dzinamarira, T. (2025). Violence against children in sub-Saharan Africa: a call for action. *Frontiers in Public Health*, *13*.
Namy, S., Carlson, C., O'Hara, K., Nakuti, J., Bukuluki, P.Lwanyaaga, J. …Michau, L. (2017). Towards a feminist understanding of intersecting violence against women and children in the family. *Social Science & Medicine*, *184*, 40–48.
Nordenving, S., Rogall, T., & Zarate-Barrera, T. (2023). Yes they can: empowering women, Working paper.
Ntezimana, E. (1987). Histoire, culture, ET conscience nationale: le CAS du Rwanda des origines à 1900. PT. 1. *Études rwandaises*, *1*, 462–497.
Nyseth Brehm, H., Uggen, C., & Gasanabo, J.-D. (2014). Genocide, justice, and rwanda's Gacaca courts. *Journal of Contemporary Criminal Justice*, *30*, 333–352.
Nyseth Nzitatira, H., Billing, T., & Edgerton, J. F. (2024). How radio affects violent conflict: new evidence from Rwanda. *American Sociological Review*.
Nyseth Nzitatira, H., Edgerton, J. F., & Frizzell, L. C. (2023). Analyzing participation in the 1994 genocide in Rwanda. *Journal of Peace Research*, *60*, 291–306.
Perales, F., Hoffmann, H., King, T., Vidal, S., & Baxter, J. (2021). Mothers, fathers and the intergenerational transmission of gender ideology. *Social Science Research*, *99*, 102597.
Pereira, A., Peterman, A., Neijhoft, A. N., Buluma, R., Daban, R. A.Islam, A. …Palermo, T. (2020). Disclosure, reporting and help seeking among child survivors of violence: a cross-country analysis. *BMC Public Health*, *20*, 1051.
Pollak, R. A. (2004). An intergenerational model of domestic violence. *Journal of Population Economics*, *17*, 311–329.
Prunier, G. A. (2014). The Rwanda crisis: history of a genocide. London: Hurst.
Republic of Rwanda. (1996). Organic law no. 08/1996 of 1996 on the organization of prosecutions for offenses constituting the crime of genocide or crimes against humanity committed since 1 October 1990, Official Gazette of the Republic of Rwanda, Year 35, No.17.

Republic of Rwanda. (2002). Denombrement des victimes du Genocide, Final Report, Kigali, Rwanda: Ministry for Local Government, Department for Information and Social Affairs.
Rogall, T. (2021). Mobilizing the masses for genocide. *American Economic Review*, *111*, 41–72.
Rutayisire, T., & Richters, A. (2014). Everyday suffering outside prison walls: a legacy of community justice in post-genocide Rwanda. *Social Science & Medicine*, *120*, 413–420.
Saile, R., Ertl, V., Neuner, F., & Catani, C. (2014). Does war contribute to family violence against children? findings from a two-generational multi-informant study in northern Uganda. *Child Abuse & Neglect*, *38*, 135–146.
Savun, B., Stanton, J. A., Hartzell, C. A., & Reid, L. (2024). Violence beyond the battlefield: civilian targeting, sexual violence, and women's political empowerment. *Journal of Global Security Studies*, *9*.
Schönpflug, U., Bilz, L., & Schönpflug, U. (2008). The transmission process: mechanisms and contexts, ed, by Cultural transmission (pp. 212–239). Cambridge University Press, 1st ed.
Sears, D. O., & Valentino, N. A. (1997). Politics matters: political events as catalysts for preadult socialization. *The American Political Science Review*, *91*, 45–65.
Stark, L., Meinhart, M., Seff, I., Gillespie, A., Roa, A. H., & Villaveces, A. (2025). Associations between conflict violence, community violence, and household violence exposures among females in Colombia. *Child Abuse & Neglect*, *162*, 106341.
Stern, E., Heise, L., Dunkle, K., & Chatterji, S. (2022). How the Indashyikirwa intimate partner violence prevention programme in Rwanda influenced parenting and violence against children. *Journal of Family Violence*, *37*, 195–206.
Stojetz, W., & Brück, T. (2023). Exposure to collective gender-based violence causes intimate partner violence. *Journal of Development Economics*, *164*, 103054.
Straus, S. (2006). The order of Genocide: race, power, and war in Rwanda. Ithaca: Cornell University Press.
Straus, S. (2019). The limits of a genocide lens: violence against Rwandans in the 1990s. *Journal of Genocide Research*, *21*, 504–524.
Thomson, D. R., Bah, A. B., Rubanzana, W. G., & Mutesa, L. (2015). Correlates of intimate partner violence against women during a time of rapid social transition in Rwanda: analysis of the 2005 and 2010 demographic and health surveys. *BMC WOMENS HEALTH*, *15*, 96.
UNICEF. (2014). Hidden in plain sight: a statistical analysis of violence against children. New York, USA: United Nations Children's Fund.
Verpoorten, M. (2005). The death toll of the Rwandan Genocide: a detailed analysis for Gikongoro Province. *Population (English Edition, 2002-)*, *60*, 331–367.
Verpoorten, M. (2012). Detecting hidden violence: the spatial distribution of excess mortality in Rwanda. *Political Geography*, *31*, 44–56.
Verwimp, P. (2006). Machetes and firearms: the organization of massacres in Rwanda. *Journal of Peace Research*, *43*, 5–22.
Walker, C. S., Stearns, M., & McKinney, C. (2021). Effect of parental corporal punishment on endorsement of its use: moderated mediation by parent gender and attitudes toward corporal punishment. *Journal of Interpersonal Violence*, *36*, 6745–6768.
Wilhelm, M. O., Brown, E., Rooney, P. M., & Steinberg, R. (2008). The intergenerational transmission of generosity. *Journal of Public Economics*, *92*, 2146–2156.
Wolbrecht, C., & Campbell, D. E. (2007). Leading by example: female members of parliament as political role models. *American Journal of Political Science*, *51*, 921–939.
World Health Organization (2025a). Adolescent health. https://www.who.int/health-topics/adolescent-health.
World Health Organization (2025b). Ending corporal punishment of children. https://endcorporalpunishment.org/reports-on-every-state-and-territory/rwanda.
Xu, Y., Zhao, A., & Ding, P. (2024). Factorial difference-in-differences, SSRN Working Paper 4896691, Rochester, NY: Social Science Research Network.
Yadav, P. (2021). Can women benefit from war? women's agency in conflict and post-conflict societies. *Journal of Peace Research*, *58*, 449–461.
Yanagizawa-Drott, D. (2014). Propaganda and conflict: evidence from the Rwandan genocide *. *The Quarterly Journal of Economics*, *129*, 1947–1994.
Yonekawa, M. (2020). Post-genocide Rwandan refugees: why they refuse to return 'home': myths and realities. Singapore: Springer Singapore.