

Yes They Can: Empowering Women*

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Abstract

We study how giving women political and domestic responsibility can lead to persistent female empowerment and overall welfare improvements. Using Rwandan post-genocide data, we exploit local variation in gender imbalances that caused a power vacuum which women filled as household heads and local politicians. In office, they provide more public goods. Overall, in female-led villages, women are healthier, better educated, wealthier, less likely to accept and experience domestic violence, and enjoy more sexual and financial autonomy. Importantly, younger women are carrying these changes and gender norms changed. In villages where men stayed in power, we find negative or no effects.

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1 Introduction

Gender equality is considered a pressing issue of our time. It was explicitly spelled out as one of the top three Millennium Development Goals, yet many countries fail to achieve it. Women across the globe are still paid less, have lower education, are more likely to be victims of violence, and have less freedom of choice in general (World Bank, 2022). While some scholars attribute these differences, for example in earnings, to inherent psychological and behavioral gender differences (e.g., women are more agreeable or less competitive) (Bertrand, 2011; Reuben et al., 2015), others point to deeply enshrined male dominance (Dahl et al., 2020; Dhar et al., 2022). If only women were given the chance to take on more responsibility in business and politics (e.g., via quotas), this would endogenously lead to more female empowerment and eventually make the quota obsolete. A few studies consistently confirm that handing women political power can change voter attitudes and pave the way for future generations of women leaders (Beaman et al., 2009). Others show that female leaders invest more in public goods that benefit women (Chattopadhyay and Duflo, 2004), for example, health (Bhalotra and Clots-Figueras, 2014) and education (Clots-Figueras, 2012). However, all of these studies only provide piecemeal evidence of the positive effects of empowering women.

In this paper, we provide the first empirical evidence that handing women political and domestic power can lead to persistent, overall female empowerment. We are also able to trace out the exact timing and mechanism. To uncover the effects, we use post-genocide data from Rwanda. During approximately 100 days in 1994, the Rwandan government – lead by extremists of the ethnic Hutu majority – conducted an extermination campaign against the Tutsi minority that resulted in an estimated 800,000 deaths. After the Rwandan Genocide, 70 percent of the population was female. Today, Rwanda ranks 6th on gender equality in the World Economic Forum, just behind the Scandinavian countries, and the parliament has the highest percentage of women worldwide (61.3 percent, (IPU and UN-Women, 2020)).

We exploit local variation in gender imbalances that caused a power vacuum women filled as household heads and local politicians.

Since genocide killings were unlikely to happen randomly, we exploit exogenous variation in armed groups' transport costs (Rogall, 2021). During the genocide, the extremist Hutu government sent around the army and militiamen to promote and execute the killings. These men were affected by the rain that fell along the dirt roads to each village.¹ Exploiting only seasonal weather variation during the 100 days of the genocide, Rogall (2021) finds that those villages further away from the (tarred) main road and with more rainfall along the way from the village to the main road received fewer militiamen (the instrument is essentially the interaction of the two).² We argue below why the exclusion restriction is likely to hold in our setting as well.

Our main argument works in several steps. First, we show that the instrument indeed induced violence by external perpetrators who – for strategic reasons laid out below – targeted particularly adult males and thus opened up a power vacuum. These externals were often better trained and equipped and headed large-scale operations. Reassuringly, we find that transport costs are significantly negatively related to the existence of a mass grave site in a village. Furthermore, transport-cost-induced violence leads to a decrease in the adult male population. Using DHS data on sibling deaths, we find that women in high-violence villages are more likely to have lost an adult brother than a sister or younger brother during the genocide. All the results are confirmed in Rogall (2021) using post-genocide household composition data, e.g., female household head, widows, or the fraction of adult men.

Next, using election outcomes from 2011, we show that armed-group violence leads to an increase in the number of female politicians elected into the local government. Moreover, elected women are also better educated.

¹Note that a village refers to the administrative unit of a *sector* in Rwanda – with an average size of almost 17 square kilometers and 5,500 inhabitants.

²We summarize his identification strategy, which requires a certain set of controls, below. More details can be found in Rogall (2021).

With women leaders in office, these villages see an increase in public goods provision, for instance, in health care, road building, or electricity access (all measured around 2016). In terms of magnitude, a 10 percent increase in genocide violence leads to a 1 to 2.5 percent increase in political participation and a 2 to 10 percent increase in public goods provision.

In the last main step, we estimate the impact of transport costs (and thus army and militiamen) on later female socio-economic outcomes. Rich Demographic and Health Survey data allows us to give a detailed picture of female empowerment some 15 to 20 years after the genocide. In particular, we find evidence that women in high armed-group violence villages are healthier, better educated, wealthier, have more decision-making power within the household, and are less likely to accept and experience domestic violence. Furthermore, they are more likely to work in high-skilled jobs and enjoy more sexual and financial autonomy. Finally, they are less likely to support the corporal punishment of children.

In terms of magnitude, a 10 percent increase in genocide violence is associated with a 1 to 5 percent increase in our women's outcomes.³

We also provide a few side results. First, unpacking the three rounds of DHS data (2005, 2010, and 2015), we show that the effects are strongest for the two most recent periods. Moreover, a further analysis points to a reversal of fortune. The immediate short-term consequences of men's mass killing are likely negative, leaving households in poverty: families lost their former male household heads, and the militia looted their assets (Brück and Schindler, 2009). We show that six years after the genocide, households that experienced violence indeed produce lower agricultural output and, consequently, consume less. However, eventually, the situation reverses.

Second, consistent with the delayed positive effects, we show that especially the younger generations of women are carrying the success. Anecdotes

³Scaling these reduced-form effects by the effects on political participation gives an elasticity of around 1 to 2, thus a 10 percent increase in the fraction of female politicians leads to a 10 to 20 percent increase in women's outcomes. Naturally, this exercise, somewhat unrealistically, assumes that genocide violence only affects women's outcomes via the political participation channel.

tal evidence suggests that older generations were skeptical of the changes, whereas younger women watched their mothers (and other female role models) struggle (for instance, after losing their husbands) and succeed. To corroborate this cultural, intergenerational transmission mechanism, we show that the effects, especially for domestic violence, are mostly driven by non-migrant women. Furthermore, we show that parents invest more in their pre-school daughters, thereby increasing their cognitive and non-cognitive abilities.

Finally, by 2010/15, the time we observe the positive socio-economic and political participation effects above, men have already replaced women as household heads again. Furthermore, men are also more likely to develop more feminist views (although this finding is weaker). Thus, it seems that the initial imbalance in gender ratios led to a change in gender norms, which prevail even after gender ratios have normalized again. Taken together, the effects are likely to be persistent.

We can also rule out a number of possible alternative explanations: strong reconstruction efforts right after the genocide, selective killings based on human capital, selective migration (or incarceration), direct effects of the Gacaca courts (details below), or local support for the president's party (RPF)⁴ are unlikely driving the positive results.⁵

To corroborate the importance of the initial gender imbalance and the resulting power vacuum (and rule out more general "progressive" effects of conflict documented for instance in [Bellows and Miguel \(2009\)](#)), we also exploit exogenous variation in the village reception of a state-sponsored radio station (RTL) that called for killings of the Tutsi minority ([Yanagizawa-Drott, 2014](#)). Using the local variation in reception induced by Rwanda's

⁴Villages supporting the ruling RPF might receive favorable treatment from the national government.

⁵Another alternative explanation could be that the initial gender imbalances changed women's returns to labor and education, thus increasing incentives to invest more in human capital and take on high-skilled jobs. Moreover, given the male scarcity, women might have felt inclined to invest in education and obtain a high-paying job to attract a future husband. Although we cannot fully rule out these two possibilities, we will argue that they are unlikely the main drivers.

hilly terrain to identify causal effects, [Yanagizawa-Drott \(2014\)](#) finds that villages with good reception experienced significantly higher participation levels in the killings (we argue below why the exclusion restriction likely holds in this case too). RTLTM-induced violence was local, committed with low-technology weapons (e.g., machetes and clubs) targeted at women and children. We thus first document that RTLTM violence leads to a male surplus. Consistently, we find negative or no effects on female outcomes.⁶

Our findings potentially carry important policy implications. First, giving women the opportunity to take over (political) responsibility seems to increase welfare along numerous dimensions. Our results also suggest that once the positive effects materialize, they are likely to be persistent.

On a final side note, our results do not support the classic marriage market argument put forward by [Becker \(1981\)](#) – where a male shortage should lead to worse marriages and outcomes for women and, thus, for instance, more domestic violence. One of the reasons for this is that it likely became socially acceptable to stay single due to the substantial reduction in men. We find evidence that women delay their marriage, thereby avoiding initial bad matches. Thus, on a more general note, our results highlight the limitations of the Becker model. The equilibria in the marriage market seem to depend strongly on social norms, and women’s identities and solely focusing on supply and demand may be misleading. In the interest of space, we discuss this in more detail in the appendix.

Finally, to the best of our knowledge, this paper is the first to show that different types of violence in the same conflict can affect later outcomes very differently. Thus, the findings are potentially also relevant for post-conflict reconstruction efforts. In particular, a one-size-fits-all reconstruction approach might be misguided.

⁶We rule out that differences in compliers are driving the different results for local and external violence. Note further that although this exercise essentially rules out more general effects of conflict as the main driving force, the two types of violence may have nonetheless differed beyond their effects on gender-ratios. For instance, women in local-violence villages may fare worse not because of gender-ratios but rather because the perpetrators were locals who often stayed in the village after the genocide, forcing women to potentially see or even meet their abusers regularly. We rule this out below.

We add to the literature in several ways. Most importantly, this paper first speaks to a growing literature on the effects of handing women (political) power. For instance, [Chattopadhyay and Duflo \(2004\)](#) show that female leaders invest more in public goods that benefit women, for example, health ([Bhalotra and Clots-Figueras, 2014](#)) and education ([Clots-Figueras, 2012](#)). Furthermore, [Beaman et al. \(2009\)](#) find that women in politics can change voter attitudes and pave the way for future generations of women leaders. We add to this literature by showing that putting women in power increases not only public goods provision but female welfare along several other dimensions, including domestic violence and various gender norms. Additionally, our paper also allows us to shed more light on the short-term timing of empowerment and identifies the young generations as its main carriers.⁷

The paper further contributes to the literature on how various shocks to the male-female ratio affect females' labor market outcomes and attitudes ([Grosjean and Khattar, 2018](#); [Baranov et al., 2019](#); [Teso, 2018](#); [Abramitzky et al., 2011](#); [Boehnke and Gay, 2020](#)). However, this paper goes beyond labor market outcomes and provides a more detailed picture of female empowerment, including domestic violence and various gender norms. Additionally, our paper also allows us to shed more light on the mechanisms, for example, via political participation and the timing of empowerment. Especially the effects on political participation are something that have not been previously shown in this literature. Furthermore, different from most of the work cited above, we focus on a developing country, where gender inequality is often one of the most pressing issues ([Jayachandran, 2015](#)).⁸

Third, the paper is related to the literature on the effects of civil war and ethnic conflict. To our knowledge, this paper is the first to show how different types of violence in the same conflict can affect socioeconomic outcomes differently. Moreover, [Justino et al. \(2012\)](#) and [Buvinic et al. \(2013\)](#) survey

⁷Others have looked at e.g., the effects of female politicians on corruption ([Brollo and Troiano, 2016](#)), the reporting of crimes against women ([Iyer et al., 2012](#)) and conflict ([Dube and Harish, 2020](#)).

⁸[Teso \(2018\)](#) is a notable exception but stresses mostly labor market outcomes.

this literature through a gendered lens, vehemently calling for more well-identified studies on the effects of conflict on women. This paper starts filling the gap. In recent years, a number of studies have exploited within-country variation to estimate the effects of conflict on various outcomes such as economic performance (Miguel and Roland, 2011; Davis and Weinstein, 2002), political engagement (Bellows and Miguel, 2009), social cohesion (Voors et al., 2012), with a special focus on human capital and health (Alderman et al., 2006; Shemyakina, 2011).

Few papers have looked at how the effects of violence vary by gender. One exception, related to our work, is García-Ponce (2017), who finds that exposure to violence during Peru's Shining Path insurgency leads to more female political participation.⁹ However, García-Ponce (2017) does not have information on election outcomes (only candidates) and further emphasizes behavioral responses as a result of experiencing violence rather than gender imbalances as the main mechanism.¹⁰ Besides, our identification strategy allows us to analyze several other outcomes.¹¹

The remainder of the paper is organized as follows. Section 2 provides some background information on the Rwandan Genocide and its aftermath. Section 3 presents the data used for the analysis, and Section 4 outlines the empirical strategy. Sections 5 to 9 discuss the results. Section 10 concludes with possible policy implications.

⁹Another paper close to ours is La Mattina (2017), who finds that women in high-violence areas experience *more* domestic violence in post-genocide Rwanda. However, La Mattina does not distinguish between external and local violence and might, therefore, be picking up a weighted average of the two. Thus, while seemingly contradicting at first, our paper highlights the importance of rigorous identification.

¹⁰Bargain et al. (2019) also emphasize a behavioral mechanism by showing that female participation in the Arab Spring demonstrations positively change women's perceptions about their rights.

¹¹Other recent papers that look at the effects of conflict-induced gender ratios are Alix-Garcia et al. (2020) and Boggiano (2020) (both analyze the Paraguayan war). Their outcomes also include, for instance, domestic violence and gender norms (with more mixed results). However, as noted above, our paper allows us to shed more light on the mechanisms, for example, via political participation and public goods provision, and the short-term timing of empowerment. In fact, we believe that women taking over as politicians is the main driver of Rwanda's female empowerment. Besides, our RTLM placebo test allows us to rule out other direct effects of conflict itself.

2 Institutional Background

Armed Groups and Genocide Rwanda's history is strongly influenced by the tensions between Hutus and Tutsis, the two largest ethnicities in the country. These tensions culminated in the genocide of 1994.

After president Habyarimana's airplane was shot down on April 6, 1994, extremists within the Hutu-dominated parties, known as the Akazu, managed to take over important government positions and initiate a 100-day lasting period of genocide. The various militia groups and the Hutu army, around 45,000 to 50,000 men, were sent around the country to help with the killings.

The mass killings ended in mid-July, when the RPF rebels (a Tutsi rebel group that had formed in the north) conquered the capital Kigali, defeating the Rwandan Hutu army and the various militia groups. Estimates reveal that around 800,000 people, mostly belonging to the Tutsi minority, lost their lives in those 100 days. There was no foreign intervention. More detailed accounts can be found in [Prunier \(1995\)](#).

The Aftermath Traditionally, Rwandan women were constrained in their choices and heavily discriminated against. They were not allowed to own land, obtained less education than men, were forced to work worse jobs, and were generally at the mercy of their husbands or other men (see [Schindler \(2010\)](#) for more details).

This started to change after the genocide. With most genocide victims being men, the killings created large gender imbalances. At the aggregated national level, the share of women increased by 40 percent to around 70 percent of the population in 1994.¹² Thus, women were forced to take on responsibilities in their families and local communities as well as leading roles in the political sphere. In 1996, the first women's councils were established. These councils gave women experience in voting and campaigning before the first post-genocide elections in 2003, where women secured 48

¹²There was significant local variation – which we exploit.

percent of the seats in parliament.

The women councils were part of a bigger decentralization effort that shifted political power from the national level to the grassroots. Since women did not historically have political experience, this shift allowed them to gain experience at the local levels and then move up the ranks. In leading positions, they quickly gained influence in promoting and enhancing the protection of women's and children's rights. For instance, the first step towards a more sweeping prohibition of sexual violence was child rape's criminalization in 2001 (Hunt, 2017). This was followed by a general gender-based violence law that finally criminalized all forms of sexual violence, including domestic violence, such as marital rape. The progress was spurred by a gender quota system that set a 30 percent for women in elected positions in 2003.

Our paper also helps to shed light on a recurring discussion in Rwanda, namely that while all these laws did shift *de jure* power to women, and Rwanda is today one of the world's most gender-equal countries at the national level, everyday practice often lagged behind (Berry, 2015). For instance, gender-based violence is still prevalent, and although reporting rates for sexual violence have increased, there is still significant under-reporting due to stigma, retaliation, or women's economic dependence on the perpetrator (Hunt, 2017).

Our results suggest that increased female influence at the national level is insufficient to empower women overall. Rather, local variation in women's *de facto* power can be explained by local variation in different exposure to the genocide and the resulting difference in gender ratios.

An important follow-up question is whether Rwanda's women-supporting institutional environment is itself a result of the genocide and the gender imbalances or rather a *sui generis* enabling condition (exogenous to Rwanda and potentially driven by President Kagame) which allowed women to thrive at least in those places with fewer men. While the cross-sectional data at hand does not allow us to answer this question directly, anecdotal evidence points to the former as the likely answer: the genocide left Rwanda's institu-

tional organization in ruins. With most former government officials dead or out of the country, the new RPF administration, themselves with little government experience, had to draw on capable, trusted women (Hunt, 2014). Men, not only among the majority of victims but in lower supply, were furthermore associated with the genocide’s horrors and, therefore, less trusted to rebuild the nation. Women, on the other hand, were seen as “powerful symbols of healing and rebirth” (Herndon and Randell, 2013, p. 75) and thus reconciliation.

3 Data

We combine several sources of data to construct an individual/village-level dataset. Instead of providing a large summary statistics table for all our outcome variables, we report means and standard deviations for the various dependent variables under each regression. Summary statistics for our explanatory variables are reported in Table 1.

Transport Cost Data The transport cost data set is taken from Rogall (2021) and comprises 1,433 or 90 percent of the total 1,575 Rwandan villages (officially called sectors).¹³

To construct the instrument, armed groups’ transport costs, Rogall (2021) uses the National Oceanic and Atmospheric Administration (NOAA) database of daily rainfall estimates for Africa. More specifically, he computes the amount of rainfall during the 100 days of the genocide over a 500-meter buffer around the distance line between each village centroid and the closest point on the main road and interacts it with the distance to the main

¹³Unfortunately, the matching in that data is imperfect, as several villages either have different names in different data sources (such as the Gacaca violence data), or use alternate spelling. Moreover, sometimes two or more villages within a commune have identical names, which prevents matching. As these issues are idiosyncratic, the main implication is likely only a lower precision in our estimates. A commune (142 in total) is an old administrative unit above the village.

road.¹⁴ Similarly, using a village boundary map, he also computes rainfall in each village. Figure A.1 illustrates how the instrument is constructed.¹⁵

Violence To show that our instrument is correlated with genocide intensity and scale the reduced-form effects, we use participation in violence. Since no direct measure of participation rates is available, we follow the literature (e.g., Yanagizawa-Drott (2014)) and use prosecution rates for crimes committed during the genocide as a proxy. The data is taken from a nationwide village-level dataset provided by the government agency “National Service of Gacaca Jurisdictions,” which gives the outcome of the almost 10,000 Gacaca courts set up all over the country to prosecute the genocide criminals.¹⁶ The data includes perpetrators that mostly belong to the army and the militia (e.g., the infamous *Interahamwe*) or are members of local armed groups such as policemen, thus it captures both external and local armed-group violence.¹⁷ We will occasionally refer to all these simply as *militiamen*.

Household Survey Data We use household survey data from two different sources. Together they cover the time period from 1999 to 2015.

The first socio-economic household data is taken from the first wave of the Integrated Household Living Conditions Survey (EICV1)¹⁸ conducted from 1999 to 2001. In total, 31,192 individuals in 6,240 households of 486

¹⁴Importantly, the road data is taken from the time of the genocide and different from the road network data described below.

¹⁵For more details, especially discussion on data quality and ruling out systematic biases, see Rogall (2021).

¹⁶A natural concern when using prosecution instead of actual participation data is survival bias or other systematic biases. For instance, in places with many killings, there might have been no witnesses left, thus resulting in low prosecution rates. Using data from numerous other sources, Rogall (2021) shows that these concerns are unwarranted.

¹⁷Note that in this data we only observe the sum of the two. However, our instruments likely shifted different parts of that sum, i.e., the transport-cost instrument provides variation in the number of external militiamen (in a sense by construction) and RTL coverage in the number of local militiamen (or policemen). The differential effects on mass graves, shown below, provide empirical evidence for this.

¹⁸EICV stands for Enquete Integrale sur les Conditions de Vie des menages.

villages were surveyed on various socio-economic and demographic factors regarding consumption, agricultural production, and education. The data is representative at the national level. This data is matched by village names within communes to the transport cost data. Recall from above that the Rogall (2021) dataset uses about 90 percent of the total number of villages. Consequentially, we match about 90 percent of the villages in the EICV1 survey (444 of the total 486 villages). As these issues are idiosyncratic, the main implication is likely only a lower precision in our estimates.

The second set of socio-economic household data is taken from the last two waves of the DHS, conducted in 2010 and 2015, again representative at the national level. We use individuals in 11,674 households in 660 villages who were surveyed on various socioeconomic factors such as education and health. This DHS data comes with GPS locations and is spatially merged with the transport cost data.¹⁹ We also complement this data with the 2005 wave of the DHS data to track changes over time; for this data, we match 346 villages.²⁰

Given the matching issues outlined above, we match about 90 percent of the total number of surveyed villages for all three DHS data sets.

Election Data Local election data from 2011 is obtained from the National Electoral Commission (NEC).²¹ The data covers elections held at the cell level, which is the second-lowest administrative unit in Rwanda and one unit below the village level. The members of the cell council – all citizens from the cell above 18 years – directly elect the cell executive committee members.

The cell executive committee is elected for five years, and the number of its members depends on the population size of the cell. In 2001 Rwanda sig-

¹⁹Note that the GPS locations are randomly replaced to ensure respondent confidentiality. However, this random measurement error should only affect the precision of our estimates and potentially bias the reduced-form towards zero, thus working against our findings.

²⁰Note that different villages are interviewed during each wave, thus we obtain a repeated cross-section.

²¹www.nec.gov.rw

nificantly decentralized its administration and shifted power to local governments. Thus, local governments hold executive power for various public goods, e.g., building regulations, social protection, health services, water and electricity, rural road construction, education, and transport. Within these areas, the local cell committee is responsible for *“identifying and prioritizing local needs, designing development plans, mobilizing development resources, and finally implementing the plans.”* (CLGF, 2017, p. 177).

In 2003, the Rwandan government changed the constitution, introducing a uniform gender quota that requires every administrative body to have at least 30 percent women. In our data, the average share of women is 53 percent, and all council committees consistently have at least 30 percent women.

We also have information on whether a local committee seat is won by a member of the National Women’s Council (NWC). This council, supported by the Rwandan Ministry of Gender and Family Promotion, is a national organ to explicitly promotes gender equality. NWC members win about 18 percent of the cell seats. Besides gender, the data also includes information on politicians’ education.

The data covers all 2,068 Rwandan cells, which we spatially match to the violence and instrument data (the Rwanda Geoportal provides a cell boundary map). Again, due to the matching issues described above, we are able to match 1,901 cells – 90 percent of the total.

Additional Data Population data for 1991 as well as distance from the village centroid to the nearest major town, the nearest major road, the capital Kigali, and the former Tutsi kingdom capital Nyanza are taken from Rogall (2021).

Public goods data (all from around 2016) is provided by the Rwanda Geoportal and contains a recent map of the road network, which we use to measure road length, and maps with the locations of social housing projects, schools, and health facilities as well as access to electricity. To be consistent with the election data, we spatially aggregate this data at the cell level and

then match it to the transport costs data. Again, we recover 1,901 cells.²²

Finally, Rogall (2021) provides data on how many days the RPF Tutsi rebels were present in each village and the precise location of mass grave sites based on satellite maps from the Yale Genocide Studies Program.

RTLTM Reception Our placebo independent variable is predicted RTLTM radio coverage at the village level, taken from Yanagizawa-Drott (2014), who uses RTLTM transmitter locations together with a high-precision topographical map (SRTM) of Rwanda to construct the data. Importantly, as the whole country is littered with hills and valleys, there is substantial local variation in topography.²³

Yanagizawa-Drott (2014) also provides data on essential control variables such as the distance of each village to the nearest radio transmitter, village altitude, as well as dummy variables indicating whether a village's mountains are sloping north, south, east or west. The appendix shows summary statistics for all these variables in Table A.1.

The RTLTM sample is somewhat smaller than the transport cost instrument sample. We match 8,912 households in 465 villages for the socio-economic DHS data (rounds 6 and 7) and 1,347 cells for the local politician data.

4 Empirical Strategy

Identification Our identification strategy rests on two assumptions. First, villages with more rainfall along the shortest path between the main road and the village experienced lower levels of armed-group violence, and the more so, the further they were from the main roads (first stage). This is the result of Rogall (2021). Second, conditional on a set of control variables (explained in detail below), distance to the main road interacted with rainfall along the way to the village does not have any direct effects on socioe-

²²Note that these are the only public goods that we found data for.

²³For further details about the data, see Yanagizawa-Drott (2014).

Table 1: Summary Statistics

	Mean	Std.Dev.
<u>A. Endogenous Variables</u>		
# Prosecuted Militiamen	58.380	80.42
Mass Grave in Village	0.056	0.23
<u>B. Exogenous Variables</u>		
1991 Population, '000	5.511	2.97
Rainfall between Village and Main Road, genocide period, 1994	121.292	34.11
Rainfall between Village and Main Road, genocide period, 10-year average	206.665	37.82
Rainfall in Village, genocide period, 1994	121.002	33.45
Rainfall in Village, genocide period, 10-year average	205.372	38.68
Rainfall in Village, growing season, 1994	242.246	64.65
Rainfall in Village, growing season, 10-year average	614.899	117.00
Distance to the Main Road	6.743	6.10
Distance to the Capital Kigali	60.014	30.63
Distance to the Border	23.075	13.94
Distance to old Tutsi Kingdom Capital Nyanza	65.429	30.60
Distance to Main City	23.905	16.53
Village Area	16.896	17.24
Number of Days with RPF Presence	45.180	42.72

Notes: There are 660 observations for each variable. All variables are measured at the village level. The # Prosecuted Militiamen are prosecutions against organizers, leaders, army and militia, local police. Population is the population number in the village from the 1991 census. The rain variables are measured in millimeters. The ten-year average is for the years 1984 to 1993. The distance variables are measured in kilometers. Village Area is measured in square kilometers. Days with RPF Presence gives the number of days the Tutsi rebels were present in each village.

conomic outcomes other than via armed-group violence (exclusion restriction).

The exclusion restriction is unlikely to hold without further precautions. The instrument, composed of distance to the main road and rainfall, is probably correlated with access to markets, health centers, education, rain-fed production, and therefore, income.

To address this problem, we follow the identification strategy in [Rogall \(2021\)](#). To summarize, in order to take into account the general living conditions of individuals in each village, we control for the distance to the main road interacted with long-term average rainfall (years 1984 to 1993) during the 100 calendar days of the genocide period along the way between the

village and main road as well as all main effects.²⁴ This way, we only exploit seasonal weather variation in the year of the genocide. Furthermore, we always control for rainfall in the village during the 100 genocide days in 1994 and its long-term average. Finally, we control for the village population size. In the following analysis, we call all these our “standard controls.” To control for broad geographic and political characteristics, we include 11 province-fixed effects.

The genocide partially overlaps with the growing season, potentially affecting expected agricultural income. Thus, we also control for the total amount of rainfall in the village during the 1994 growing season and its long-term average as well as the interaction of the two with the distance to the main road (called “growing season controls”).

Rogall (2021) provides a battery of tests to show that, given these controls, the exclusion restriction holds in his context, i.e., that the instrument unlikely affects civilian participation. However, to do so, he needs to rule out direct effects on wealth or any other socioeconomic outcomes that might be driving civilian participation. For instance, the instrument is uncorrelated with numerous wealth and income measures as well as agricultural crop prices, and the results do not differ in coffee-producing places or areas with coffee mills (coffee production is a transportation-intensive activity). Thus, the exclusion restriction is likely to hold in our context as well.²⁵

Specification To show that higher transport costs caused less violence in our DHS sample, and reproduce the main result in Rogall (2021), we estimate the following first-stage equation

$$(1) \quad \log(h_{jp}) = \alpha + \beta [\log(Dist_{jp}) \times \log(Rain_{jp})] + \mathbf{X}_{jp}\pi + \gamma_p + \epsilon_{jp},$$

where h_{jp} is our measure of armed-group violence, $Dist_{jp}$ is the distance to the nearest main road and $Rain_{jp}$ is the amount of rainfall during the period

²⁴The main effects are distance to the main road, rainfall along the way between village and main road during the 100 days of the genocide in 1994 and its long-term average.

²⁵Several additional tests and placebo checks are provided in Rogall (2021).

of the genocide along the way between the main road and each village j in province p . Furthermore, γ_p are province fixed effects and ϵ_{jp} is the error term. Given the controls in \mathbf{X}_{jp} , explained in detail above, the interaction term captures the armed groups' transport costs. We expect β to be negative.

We then run the following reduced-form regressions

$$(2) \quad post_y_{ijp} = \alpha' + \beta' [\log(Dist_{jp}) \times \log(Rain_{jp})] + \mathbf{X}_{jp}\pi' + \gamma_p + \epsilon_{ijp},$$

where $post_y_{ijp}$ is the post-genocide outcome of household i (or individual i) in village j in province p and the other variables are as before. Following Rogall (2021), we allow error terms to be correlated across villages within a 150 kilometer radius (Conley, 1999). Armed groups were sent around the entire country, so we expect errors to be correlated over long distances.²⁶ We also present instrumental variable estimates.²⁷

First Stage The first-stage relationship between transport costs and genocide violence is strongly negative at the 99 percent confidence level (regression 1 in Table 2), and this relationship holds when restricting the sample to those villages surveyed in the DHS (regressions 2 and 3). Although losing more than half of the observations compared to the full sample, the F-statistic on the excluded instrument still reaches 18.60 in regression 3.²⁸

Regarding magnitude, the point estimate of -0.781 (standard error 0.181) in regression 3 suggests that a village with an average distance to the main road received 25 fewer militiamen, about 40 percent of the mean (58.38),

²⁶In particular, the suggested cutoff of 150 kilometers coincides with the maximum distance to Kigali – the center of the country and the genocidal plan – in the sample of villages.

²⁷Note that we scale the reduced-form effects using the total number of militiamen (both local and external). The IV estimate thus gives the effect of those militiamen affected by transport costs – likely external militiamen.

²⁸Note that we also control for several important additional factors that potentially have direct effects on genocide violence: distance to the border, distance to main cities, distance to the capital Kigali (the organizational center of the genocide) and distance to Nyanza as well as population density and the number of days the RPF was present in each village. Nyanza was the old Tutsi Kingdom capital and villages further away from it exhibited lower Tutsi shares, on average.

Table 2: First Stage – DHS (2010 and 2015)

Dependent Variable Sample	# Militiamen, log			Mass Graves	
	Full Sample	DHS Sample			
	(1)	(2)	(3)	(4)	(5)
Distance x Rainfall, 1994	-0.509 [0.115]***	-0.719 [0.181]***	-0.781 [0.181]***	-0.080 [0.028]***	-0.088 [0.029]***
Standard Controls	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes
Additional Controls	yes	no	yes	no	yes
Province Effects	yes	yes	yes	yes	yes
F-stat	19.54	15.79	18.60	8.03	9.47
R ²	0.50	0.48	0.50	0.07	0.08
N	1432	660	660	660	660

Notes: Mass Graves is a dummy taking on the value of 1 if a mass grave was found in a village. Regression 1 uses the full sample of villages from Rogall (2021). The sample in regressions 2 to 5 is restricted to the villages from DHS rounds 6 and 7. **Distance × Rainfall, 1994** is the instrument (distance to the main road interacted with rainfall along the way (a 500m buffer) between village and main road during the 100 days of the genocide in 1994). **Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, long-term average rainfall in the village during the 100 calendar days of the genocide period (average for 1984-1993), rainfall along the buffer during the 100 days of the genocide in 1994, long-term average rainfall along the buffer during the 100 calendar days of the genocide period (1984-1993), and the latter interacted with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 1994 in the village, long-term average rainfall during the growing seasons in the village and both of these interacted with distance to the main road. **Additional Controls** are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and the number of days with RPF presence. All control variables except “Number of Days with RPF presence” are in logs. Interactions are first logged and then interacted. There are **11 provinces** in the sample. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

following a one-standard-deviation increase in rainfall between a village and main road.

To interpret the main effects below it is essential to understand the type of violence that our instrument induced. By the very nature of the transport-cost instrument, it induced violence by perpetrators coming from *outside* of the village: army and militiamen. These army and militiamen were often better trained and equipped (firearms and grenades) and headed large-scale operations. Consistently, regressions 4 and 5 in Table 2 show that the

transport-cost instrument maps negatively into whether the village has a mass grave site.

5 Targeting Adult Men

First, we need to show that our instrument provides variation in the male population. Above we already argued that the instrument induced violence by the external army and militiamen. Next, we show that these men targeted a special population sub-group.

In particular, [Verwimp \(2006\)](#) argues that armed-group violence seemed to have targeted primarily adult males. Using detailed information on the deaths of 59,050 victims from Kibuye Province in the west of the country (collected by IBUKA²⁹), [Verwimp \(2006\)](#) shows that adult men (of working age) were especially more likely to die by firearms in large-scale massacres. The most vulnerable – women, young children, and the elderly – were more likely to die from a machete or club. Since the genocide was strategically planned to kill as many Tutsi as possible, and bullets were in short supply, [Verwimp \(2006\)](#) argues that bullets were used to kill those who were more likely to escape or resist, i.e., adult men. Because external armed groups mostly owned firearms, whereas the local population used machetes and clubs for killing, our instrument (picking up armed-group violence) should thus have produced a female surplus. We provide two pieces of evidence for this.

First, we directly analyze deaths. The DHS data contains information on individuals' siblings, for example, their gender and if they died, their age at death and year of death. This information allows us to back out the number of women, elderly men as well as children that were killed during the genocide. [Table 3](#) provides the results. All outcomes above are normalized by the total number of killed siblings. The point estimates confirm that external violence did not target the most vulnerable. A 10 percent increase in large-

²⁹IBUKA, which means “Remember,” is a Rwandan genocide survivor organization that works to perpetuate the memory of the genocide.

scale violence leads to a 1 percent decrease in the fraction of killed women, a 1.5 percent decrease in the fraction of killed vulnerable (i.e., women and elderly men as well as boys), a 2 percent decrease in the fraction of killed children and a 5 percent decrease in the fraction of killed girls. Note that random measurement error in the dependent variable (e.g., the DHS data does not specify whether a sibling was killed during the genocide or happened to die that year for other reasons) is likely going to increase standard errors.³⁰ Nonetheless, most point estimates are significant.

Second, we can look at the age and gender distribution of the survivors. Using data from the EICV1 survey conducted some 6 years after the genocide, Rogall (2021) shows that armed-group violence significantly increases the probability of having a female household head. A 10 percent increase in genocide violence leaves households with a 3.5 percent higher probability of having a female head.³¹ Additionally, he finds evidence that households are more likely to report having a widow and fewer men in general. In line with the predictions, he also finds that while it did not affect the fraction of adults in general, it did significantly decrease the fraction of adult males.

Thus, the drop in the number of males likely forced women to take over more responsibilities within their households and local communities – with positive effects on (women’s) welfare. Besides the empirical evidence above, we provide more detailed anecdotal evidence in the appendix.

6 Political Office and Public Goods Provision

Local Politicians Besides taking over as household heads, this section provides evidence that women also take on government positions in their local cell committees.

³⁰Note, however, that 1994 is a clear outlier in the distribution of deaths, around 25 percent of all reported deaths happened in that year. For all other years (we use 10 years before and 10 years after 1994), the death rate is low and close to the average of 3.75 percent.

³¹On average, about one-third of all households were headed by women in 2000, an increase of about 40 percent from pre-genocide levels (similar to the general 40 percent increase in the share of women, pre-genocide data from 1991 census).

Table 3: Targeted Killings

Dependent Variable	Fraction	Fraction	Fraction	Fraction
	Females	Vulnerable	Children	Female Children
	(1)	(2)	(3)	(4)
Distance × Rainfall, 1994	0.025 [0.011]**	0.060 [0.013]***	0.045 [0.017]***	0.048 [0.013]***
Dep. Mean	0.38	0.50	0.27	0.12
Dep. Std.Dev.	0.43	0.45	0.42	0.31
R ²	0.01	0.01	0.02	0.01
N	6897	6897	6897	6897
IV: External Militia Violence	-0.030 [0.014]**	-0.072 [0.020]***	-0.053 [0.018]***	-0.058 [0.017]***

Notes: The data is taken from individuals of all three rounds of DHS data. All dependent variables are fractions of an individual's sibling deaths. For example, Fraction Vulnerable is the number of vulnerable male (the elderly and children) and all female sibling deaths normalized by the total number of sibling deaths. All deaths are restricted to siblings that died during the genocide. **We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2.** Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Results are reported in Table 4. First, armed-group violence leads to an increase in the fraction of women elected for office in general (regression 1). Second, not only are more women elected, but the ones elected are also more likely to be members of the National Women's Council – a government organization that explicitly promotes gender equality (regression 2). As a placebo check, we show that armed-group violence is unrelated to whether a politician is a member of the Council for Disabled individuals (regression 3). Finally, not only are more women entering office but the elected ones are also better educated (i.e., finished at least primary school, regression 4); we do not find any effects on the education of elected men (regression 5). In terms of magnitudes, a 10 percent increase in armed-group violence leads to 1 percent increase in the fraction of elected females and a 2.5 percent increase in the fraction of seats won by National Women's Council members and the fraction of women with at least primary education.

Table 4: Women’s Political Engagement

Sample	Women & Men			Women	Men
	Fraction of Elected Women	Fraction of National Council Women	Fraction of Disabled	Fraction With Primary Education	
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Distance x Rainfall, 1994	-0.017 [0.006]***	-0.013 [0.005]***	0.000 [0.002]	-0.031 [0.014]**	-0.001 [0.018]
Dep. Mean	0.53	0.18	0.17	0.44	0.45
Dep. Std.Dev.	0.09	0.07	0.07	0.24	0.21
R ²	0.10	0.05	0.06	0.19	0.17
N	1901	1901	1901	1899	1898
IV: External Militia Violence	0.055 [0.027]**	0.042 [0.021]**	-0.002 [0.008]	0.102 [0.038]***	0.003 [0.056]

Notes: All regressions are run at the cell level. **We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2.** All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Given the findings on gender imbalances above, one concern is that these results may merely reflect that the male candidates’ pool is lower in high-violence villages. However, the DHS data suggests that by the time of the elections, 2011, gender imbalances – both in general and for the subset of potential candidates – have normalized.³² It thus seems that the above results are not simply a mechanical effect. We provide evidence further below.

Local Public Goods Several studies suggest that women care more about public goods such as health care and education ([Duflo, 2003](#)). Thus, the increase in the number of female politicians and thereby access to executive power should also translate into more local public goods spending (all variables measured around 2016). In [Table 5](#), we test this. We use information about the length of the road system, the number of primary schools, the

³²The subset of potential candidates covers adults between 18 and 85 years old (since few politicians are very young, we also use 25 years as the lower cutoff age).

Table 5: Public Goods Provision

Dependent Variable	Road	# Health	Social	Access To	# Primary	# Secondary	Index
	Length, log	Facilities	Housing, log	Electricity	Schools	Schools	Using 1-6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Distance × Rainfall, 1994	-0.124 [0.039]***	-0.076 [0.027]***	-0.058 [0.034]*	-0.151 [0.043]***	-0.023 [0.086]	-0.055 [0.049]	-0.904 [0.226]***
Dep. Mean	17.60	0.20	1.75	0.41	1.13	0.20	0.00
Dep. Std.Dev.	11.02	0.43	2.63	0.49	0.98	0.57	3.15
R ²	0.40	0.02	0.27	0.20	0.09	0.06	0.16
N	1901	1901	1901	1901	1901	1901	1901
IV: External Militia Violence	0.344 [0.167]**	0.212 [0.086]**	0.160 [0.093]*	0.418 [0.216]*	0.065 [0.233]	0.152 [0.123]	2.507 [0.911]***

Notes: All regressions are run at the cell level. We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

number of higher educational facilities, health care centers, social housing projects, and whether a cell has access to electricity. The results suggest a positive relationship between genocide violence and public goods provision. All IV point estimates are positive and, except for two, significant (regressions 2 to 7). In terms of magnitudes, regressions 1 to 4 suggest that a 10 percent increase in genocide violence leads to a 2 to 10 percent increase in public goods provision. Again, to account for multiple hypothesis testing, the results are robust to using a public goods index in regression 7.

The documented drop in the number of males and, in particular, adult males not only forced women to take over more responsibility in their families but also left a political vacuum which, again, women filled. Consistent with the existing literature cited above, in these powerful positions, women focused on providing important public goods and set examples for future (female) generations to come (we show below that the results are particularly strong for younger women). Again, we relegate more detailed anecdotal evidence to the appendix.

7 Socio-Economic Effects

7.1 Women’s Outcomes and Attitudes

We first show that in villages with high levels of external armed-group killings, women do better on various dimensions. The DHS data contains numerous questions on women’s financial autonomy, sexual freedom, domestic violence, decision-making power within households, education, occupation, and health. It also includes a survey on men’s outcomes and attitudes.³³

Because of the many outcome variables available in the DHS, we need to account for multiple hypothesis testing and thus calculate various z-score measures, grouping outcomes into 11 different categories, and present the results for these (Table 6).^{34,35} In the appendix, we also report all individual regressions (Tables A.3 to A.10). Note that some of the DHS outcomes are positively associated with female empowerment (e.g., “can get a condom”), others negatively (e.g., “needs permission to get medical help”). We account for that when calculating the indexes.^{36,37}

³³Note that we find no significant differences between the 2010 and 2015 DHS waves and thus pool the two cross-sections here to increase statistical power. Note further that all the results are robust to adding wave fixed effects or wave-province fixed effects.

³⁴We also experiment with the Romano-Wolf procedure (Romano and Wolf, 2005) and find that the corrected p-values are similar to the corresponding uncorrected ones.

³⁵Table A.2 in the appendix shows that the OLS results for all 11 outcomes are small and insignificant throughout. It is informative to compare the OLS results to the analogous IV estimates in Table 6 which are all larger in absolute value. Two reasons could explain the differences. First, random measurement error in the violence variable could bias the OLS estimates towards zero. Second, the IV estimates quantify the local average treatment effect (LATE) induced by transport costs and thus external militiamen. The OLS estimate (ATE) on the other hand, also includes the effects of local militiamen. These two opposing effects (external vs. local violence) likely offset each other, again pushing the OLS estimate towards zero.

³⁶We set the indexes to missing only if all outcomes are missing. However, the results are the same if we do so when at least one outcome is missing.

³⁷One concern when using self-reported data is social desirability bias. We consider this less of a problem in our setting since female empowerment is officially promoted by the national government throughout Rwanda. Thus, social desirability bias, if at all, is likely affecting all villages.

Education, Wealth and Health To start, women in high-violence villages have higher socioeconomic status, thus are more likely to be better educated, wealthier, and healthier (regression 3 in Table 6).³⁸ Both reduced-form and IV point estimates are highly significant at the 99 percent confidence level. The z-score includes a dummy variable for literacy and reading the news, years of schooling as well as a wealth index.³⁹ As access to health and health status proxies, we use whether an individual was *ever* tested for HIV and results from the Rohrer index, respectively (details in Table A.3).⁴⁰ In terms of magnitudes, a one-standard-deviation increase in external genocide violence leads to about a 0.4 standard-deviation increase in the total index.⁴¹

Financial Autonomy When it comes to financial autonomy, women are more likely to work in skilled occupations, receive their earnings in cash (as opposed to in-kind), and own a house or land by themselves (details in Table A.4). Based on the estimates from regression 4, a one-standard-deviation increase in large-scale violence induces an increase in the financial autonomy index of 0.25 standard deviations.

Physical Autonomy Our results further suggest that women's physical autonomy increases: they have better access to contraception, can request their partners to use a condom, and can refuse sexual intercourse if the partner cheats. Furthermore, their husbands are less likely to have other wives, with potentially positive effects on women's mental and physical health (Shepard, 2012) as well as lower HIV transmission (Bertocchi and

³⁸The results are robust to combining this index with the financial autonomy index below.

³⁹Note that the wealth index measures household wealth, thus women live in richer families.

⁴⁰The Rohrer's index is similar to the body mass index – normalizing an individual's weight by the third power of height (instead of the square).

⁴¹Note that the Tutsi were generally believed to have had higher socioeconomic status than the Hutu. However, this would only go against our findings. Moreover, recent work, for instance, by Verwimp (2013) challenges this view, suggesting that there did not exist profound differences between the two groups.

Table 6: Main Effects (2010 and 2015)

Panel A – Socio-Economic Indicators	Education, Wealth, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility	
	(1)	(2)	(3)	(4)	(5)	
Distance × Rainfall, 1994	−0.664 [0.164]***	−0.317 [0.071]***	−0.301 [0.050]***	−0.090 [0.041]**	−0.294 [0.044]***	
Dep. Mean	0.00	0.00	−0.00	0.00	0.00	
Dep. Std.Dev.	3.00	2.38	1.83	1.89	2.21	
R ²	0.11	0.04	0.02	0.01	0.03	
N	24802	24802	23301	24800	24802	
IV: External Militia Violence	0.831 [0.136]***	0.397 [0.066]***	0.374 [0.097]***	0.112 [0.059]*	0.369 [0.103]***	

Panel B – IPV	Domestic Violence Attitudes	Violence Against Kids Attitudes	All	Domestic Violence		
	(1)	(2)	(3)	Severe	Sexual	Less Severe
Distance × Rainfall, 1994	0.281 [0.167]***	0.437 [0.138]**	0.305 [0.278]	0.253 [0.097]***	0.077 [0.071]	0.051 [0.208]
Dep. Mean	−0.00	−0.00	0.00	0.00	0.00	0.00
Dep. Std.Dev.	2.69	3.99	6.17	2.80	1.77	4.05
R ²	0.04	0.05	0.01	0.01	0.01	0.01
N	24797	24782	4951	4951	4950	4951
IV: External Militia Violence	−0.353 [0.229]**	−0.548 [0.205]*	−0.360 [0.348]	−0.300 [0.123]**	−0.092 [0.084]	−0.061 [0.249]

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. **We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2.** All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Dimico, 2019). We also find that women are, on average older at first cohabitation. This effect seems to be driven by a substantial drop in the number of girls underage at first cohabitation – a positive development outcome (Leeson and Suarez, 2017). Consistently, they are also significantly older at first birth, driven especially by a drop in teenage pregnancies, with again likely positive long-term effects for these young women (Adda et al., 2017). In terms of magnitudes, according to the estimates from regression 5, a one-standard-deviation increase in external militia violence leads to an increase equal to approximately 0.3 standard deviations in the physical autonomy index (details in Table A.5).

Decision-Making Power Their decision-making power within the household also seems to improve, although this effect is weaker. The results in regression 6 imply that a one-standard-deviation increase in external violence leads to a rise in the decision-making power index equal to approximately 0.1 standard deviations. Here, decision-making power is understood as to whether women are involved in deciding on large household expenditures, how to spend their husbands' money and their own medical insurance, and whether they need their husbands' permission to get medical help. Details in Table A.6 suggest that especially health-related decisions are driving the positive effects in regression 6.

Sexual Knowledge and Fertility Furthermore, a one-standard-deviation increase in large-scale violence increases our sexual knowledge index by about 0.25 standard deviations. This implies that women are more likely to know about HIV, the ovulatory cycle, and contraception⁴² in high-violence villages (see Table A.7). Moreover, women in these villages are more likely to want fewer children, another good development indicator (Adda et al., 2017).

Domestic Violence Finally, women are less likely to accept and experience domestic violence.⁴³ The z-score in regression 1 includes whether women find beating by their husbands justified if the wife goes out without informing the husband, neglects the children, refuses to have sex or burns the food. The negative effect on accepting violence in regression 1 is driven by a strong negative effect on all four variables (details in Table A.8).

Furthermore, women in high-violence villages are also less likely to ac-

⁴²Contraception, while having the right point estimate, is not significant.

⁴³The husband of an interviewee (or other adult males) was present during less than 0.5 percent of the interviews. Results are robust to dropping those. However, women may still want to generally under-report domestic violence. This would be worrisome if the under-reporting were correlated with, say, violence or wealth. The results below for the RTLM case in which we find if anything increases in domestic violence somewhat suggest that the former is unlikely to matter. Regarding the latter, the results are robust to controlling for household wealth.

cept violence against children (both boys and girls, regression 2). Here, possible reasons for violence include the child disobeying, being impolite, or embarrassing the family (details in Table A.9). This result is not only indicative of a general drop in the acceptance of violence but also suggests that these positive effects are likely to be persistent. A large amount literature in psychology and public health now documents a strong relationship between exposure to violence as a child (both as a victim and a witness) and violent behavior as an adult (Lindert and Levav (2015) give an overview). Thus the observed drop in domestic violence and corporal punishment may set off a virtuous cycle.

The magnitudes of the two effects are similar, with a one-standard-deviation increase in genocide violence reducing domestic violence attitudes by about 0.2 standard deviations.

Besides affecting attitudes, we also find a drop in women reporting to be the victim of domestic violence. The DHS data includes nine questions on this topic: whether a woman was *ever* pushed, punched, slapped, kicked, had arm-twisted or hair pulled, was strangled or burnt, threatened with a knife or gun, forced into unwanted sex, or forced into unwanted sexual acts.⁴⁴ We classify the first five as less severe violence while the last four are severe. We also separate out sexual violence. In regression 8, we report the z-score on all nine violence outcomes. While the IV estimate is negative, it is not significant. However, splitting the outcomes, we find a strong significant drop in severe violence (regression 9). The IV point estimate suggests a 0.15 standard-deviation decrease in severe domestic violence following a one-standard-deviation increase in genocide violence. We do not find strong effects for sexual violence or less severe violence (regressions 10 and 11). Details in Table A.10.⁴⁵

⁴⁴The DHS distinguishes between women being forced into unwanted sex or unwanted sexual acts, and we follow this distinction here.

⁴⁵Note that these findings may simply be reflective of a general drop in the acceptance of post-conflict violence, i.e., people may support a “never again” approach. However, the non-negative or even positive effects on domestic violence (and attitudes) in the RTLM case below are inconsistent with this view.

Anecdotal evidence suggests that women in power were able to raise awareness of the issue and lobby for better laws against domestic violence. [Hunt \(2017, p. 185\)](#) notes: “(...) women around the world have been terrorized by sexual assault. (...) the women of Rwanda found a way to make that violence an occasion (...) of transformation. (...) To generate that shift, women had to elevate society’s awareness of the severity of the aggression (...).”

7.2 Men’s Outcomes and Attitudes

Fortunately, the DHS data also includes several questions on men’s attitudes towards women. The results for these are presented in [Tables A.11 to A.15](#) in the appendix. To summarize, the results are somewhat weaker than for female outcomes. However, for most cases, the point estimates imply a positive effect on men’s gender attitudes. Besides, whenever the results turn significant, the point estimates always have the right sign (except for one outcome: “ideal number of children”). For instance, the positive effects on household wealth for women are mirrored in men’s results, thus men also tend to live in richer families (regression 5 in [Table A.11](#)).

Furthermore, men’s sexual attitudes are positively affected: a one-standard-deviation increase in genocide violence leads to a 0.2 standard-deviation increase in the men’s sexual attitude index. This includes, for instance, that men are more likely to agree that women can refuse sex if their husband cheats on them (regression 3 in [Table A.12](#)) and men are less likely to ever having paid someone for sex (regression 4). The results are more mixed in terms of sexual knowledge and fertility preferences. In particular, men favor more children rendering the overall index insignificant (regressions 4 and 5 in [Table A.13](#)).

Another strong result is shown in [Table A.14](#). Men are more likely to agree that a woman should have a say on large household expenditures and the husband’s money (regressions 2 and 4). In terms of magnitudes, a one standard-deviation increase in violence leads to a 0.3 standard-deviation increase in the decision-making power index.

Finally, results for domestic violence attitudes are shown in Table A.15. The questionnaire asks whether beating one's wife is justified when the wife goes out without telling, neglects the children, argues with her husband, refuses to have sex, burns the food, or cheats.⁴⁶ The IV point estimates for all cases (except cheats) are negative, suggesting that men are less willing to beat their wives; however, it is only significant for the first case (regression 2).

7.3 Robustness Checks

In the interests of space, we provide additional robustness checks in the appendix. To summarize, the results are robust to dropping the additional controls in our baseline specification, adding additional individual and household controls (e.g., number of household members, religion fixed effects), and adding various post-genocide rainfall controls. Finally, we show the relationship between external armed-group violence and all our main outcomes graphically. Importantly, none of the effects seems to be driven by outliers.

7.4 Effects Over Time and Intergenerational Transmission

In the appendix, we further show that the positive effects need time to materialize and are not yet visible for the 2005 DHS. Importantly, the null results for 2005 are not driven by a weak first stage. Consistent with this time lag, we next show that the effects are driven by younger women (the sample is split at women of age 18). This is in line with the anecdotal evidence suggesting that older women were reluctant to change. To further strengthen the argument that intergenerational transmissions matter, we show that the results are stronger for non-migrant women. Besides, we observe strong effects on children's welfare. First, parents are more likely to invest (read to

⁴⁶Note that only men are given the question whether beating is justified if their wives cheat.

them, play with them, etc.) in their kids, especially girls. And second, this results in increases in children's cognitive and non-cognitive abilities.

7.5 Outlook

Are the positive results on female outcomes likely to last in the future? Several pieces of evidence suggest that this is likely.

First, not only do we document positive effects on women's outcomes and attitudes, but men's attitudes towards women seem to be positively affected as well. Second, by the time we measure the positive effects above (i.e., 2010 and 2015), men have already replaced women as household heads again, and gender ratios have returned to normal. Put differently, although women are potentially losing their dominating roles in the households (and their majority in society in general), the positive socio-economic effects remain. We provide more detailed evidence for this in the appendix.

Finally, the documented drop in domestic violence and child beating and the increase in pre-school children's abilities may also have long-lasting positive effects and set off a virtuous cycle. Several studies show that children who experience or observe violent behavior are more likely to become violent adults themselves (Lindert and Levav, 2015).

To offer an interpretation, it seems that the initial imbalance in gender ratios led to a change in gender norms which prevail even after gender ratios return to normal. This is supported by anecdotal evidence, for instance Hunt (2017, p. 308) interviews entrepreneur Janet Nkubana: *"Today, married women are more respected by their husbands (...) because of the change of attitude toward women (...). You're contributing equally, so you have equal rights. That means if a man beats you, you have to report it. (...) Women used to think they could not go to a construction site or drive trucks. 'Don't underestimate yourself!' That was the message."*

8 Alternative Mechanisms

First, using data on infrastructure construction *six years after* the genocide, we rule out post-conflict reconstruction by the central government or some NGO as the main channel. In fact, anecdotal evidence points to a general inefficiency of the post-genocide government efforts. Next, local-level data from the 2013 parliamentary elections, allows us to rule out that high-violence villages are more likely to support the ruling Tutsi party (RPF), thus receiving favorable treatment by the national government. Or alternatively, that female politicians who rise up the ranks directly support their home constituencies, since national politics is strictly dominated by the RPF party. Third, using the EICV1 survey data from 2000/01, we rule out selective killings based on human capital or selective migration explaining the positive effects. Fourth, we argue that it's unlikely that changed economic incentives (i.e., changed returns to labor and education or changed incentives to attract a husband) are the main drivers of the positive effects. Fifth, the insignificant OLS results speak against the Gacaca courts themselves having direct positive effects on female empowerment, for instance, via reconciliation, trust-building, or women's roles as judges and witnesses. Finally, since the army and militiamen also incited civilians to join in the killings (Rogall, 2021), we also rule out that incarcerating these violence-prone men and thus taking them off the marriage market is driving the positive effects. More details and empirical results are given in the appendix.

9 Placebo – RTLM Hate Radio

Background To corroborate the importance of the initial gender imbalance, we finally exploit local variation in the reception of the radio station RTLM (Radio Television Libre des Mille Collines) (Yanagizawa-Drott, 2014). Unfortunately, we have to relegate large parts of the discussion to the appendix in the interest of space.

The RTLM radio station, established in July 1993 by Hutu extremists,

explicitly called upon the Hutu majority population to kill the Tutsi minority. Using the local variation in reception induced by Rwanda’s hilly terrain to identify causal effects, [Yanagizawa-Drott \(2014\)](#) finds that villages with good reception experienced significantly higher levels of local participation in the killings.

RTLTM-induced violence was different from armed-group violence analyzed above. RTLTM reception led to local, small-scale violence with perpetrators from the village who were often less experienced in killing and who used low-technology weapons such as clubs and machetes. Thus different from armed-group violence, RTLTM coverage is unrelated to whether a village has a mass grave site or not.⁴⁷

Second, the main targets of RTLTM-induced violence seemed to have differed. While armed-group violence targeted men primarily, local violence targeted women, children, and the elderly and thus produced a male surplus ([Rogall, 2021](#)). Consistently, women are less likely to be in households or elected into a local political office. We find if at all, negative effects on public goods provision. More details and empirical evidence, together with the identification strategy, are shown in appendix Section [A.7](#).

Main Effects Consistent with men staying in power in these places, we do not find positive effects on women’s socioeconomic outcomes and attitudes.⁴⁸ Rather, taken at face value, the point estimates on the various indexes suggest that women are worse off in RTLTM-violence villages ([Table A.16](#) and details in [Tables A.17](#) to [A.24](#)).⁴⁹ Most notably, women are

⁴⁷Results for the full sample are shown in [Rogall \(2021\)](#). In [Table A.41](#) in the appendix, we confirm these results using our DHS sample.

⁴⁸Note that we scale the reduced-form effects using the total number of militiamen (both local and external). The IV estimate thus gives the effect of those militiamen affected by RTLTM radio coverage – likely local militiamen.

⁴⁹Importantly, note that the differential results for RTLTM-induced violence and transport-cost-induced violence are unlikely driven by differences in compliers. Although we cannot directly observe the set of compliers, [Rogall \(2021\)](#) provides evidence that both first stages work for various different sub-populations, for instance, places with high and low population densities, high and low rain-fed production, near and far from the capital, high and low fractions of males, or fractions of adults.

significantly more likely to accept wife beating (regression 1) on all dimensions (details in Table A.23) as well as violence against children (the index in regression 2 is almost significant at the 90 percent confidence level), they enjoy less physical autonomy (regression 5) and have less sexual knowledge (regression 7). However, these results, significant in the reduced form, are not significant in the second stage.⁵⁰ In the appendix, we rule out an alternative mechanism, namely that women in local-violence villages fare worse not because of gender ratios but rather because the perpetrators were locals who often stayed in the village after the genocide, forcing women to potentially see or even meet their abusers regularly.

10 Conclusion

Our results show that women living in villages that experienced large-scale violence by external army and militia groups have higher living standards some 15 to 20 years after the Rwandan genocide. On the contrary, women that experienced higher levels of local violence induced by RTLM radio reception do not have higher (if anything lower) living standards around 20 years after the genocide.

In terms of mechanisms, our findings suggest that the external militia's strategy to target primarily adult men leads to a power vacuum allowing women to take on key positions in the household and government. In these positions, they provided more local public goods and set examples for younger generations. On the other hand, local violence targeted women, and the resulting male surplus did not deliver female empowerment.

These findings have important policy implications: first, giving women the opportunity to take over (local) political responsibility seems to have positive welfare effects for both genders. Besides, these changes are mostly driven by younger women and take some time to develop – in Rwanda, about 15 to 20 years. However, our results also suggest that they are likely

⁵⁰The insignificant second stages are less surprising here since the first-stage sample for the RTLM case is significantly smaller and thus delivers less precise estimates (Table A.41).

to be persistent once the positive effects materialize. Specifically, men's attitudes are (weakly) positively affected; domestic violence against women and children decreases which can offset virtuous cycles; by the time we measure the positive effects on gender outcomes and political participation, male-female ratios have returned back to normal.

Second, to the best of our knowledge, this paper is the first to show that different types of violence in the same conflict can affect later outcomes very differently. Thus, the findings are also relevant for post-conflict reconstruction efforts. In particular, one should be cautious when generalizing the effects of conflict, and following a one-size-fits-all reconstruction, the approach might be misguided.

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Appendix – Not For Publication

A.1 Additional Tables and Figures

A.2 Extensions to Sections 5 and 6: Detailed Anecdotal Evidence

A.3 Extensions to Section 7.3: Robustness Checks

A.4 Extensions to Section 7.4: Effects over Time and Intergenerational Transmission

A.5 Extensions to Section 7.5: Outlook

A.6 Extensions to Section 8: Alternative Mechanisms

A.7 Extensions to Section 9: Placebo – RTLH Hate Radio

A.1 Additional Tables and Figures

Table A.1: Summary Statistics – RTLTM Sample

	Mean	Std.Dev.
<u>A. Endogenous Variables</u>		
# Prosecuted Militiamen	66.426	84.20
Mass Grave in Village	0.052	0.22
<u>B. Exogenous Variables</u>		
RTLTM Radio Coverage	0.206	0.25
1991 Population, '000	5.470	2.74
1991 Population Density	507.386	672.41
Mean Altitude	1.690	0.24
Variance in Altitude	9.397	11.33
Distance to Transmitter	4.912	2.82
Distance to the Border	22.862	12.69
Distance to the Main Road	7.085	5.81
Distance to Main City	22.475	14.01
Fraction of Villages facing East	0.245	0.43
Fraction of Villages facing North	0.237	0.43
Fraction of Villages facing South	0.269	0.44

Notes: There are 465 observations for each variable. The # Prosecuted Militiamen are prosecutions against organizers, leaders, army and militia, local police. RTLTM Radio Coverage is the fraction of the village that received the radio signal. Population is the population number in the village from the 1991 census. Population Density is measured per square kilometers. The distance and altitude variables are measured in kilometers. A village facing North/East/South is determined by the direction of the average slope of the village.

Table A.2: OLS – Indexes (2010 and 2015)

Dependent Variable	Domestic Violence							Domestic Violence			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Domestic Violence Attitudes	Violence Against Kids Attitudes	Education, Wealth, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility	All	Severe	Sexual	Less Severe
Local and External Militia Violence	-0.050 [0.042]	-0.006 [0.018]	0.017 [0.019]	0.042 [0.028]	-0.005 [0.007]	-0.000 [0.016]	-0.001 [0.014]	-0.073 [0.073]	-0.060 [0.040]	-0.016 [0.025]	-0.013 [0.035]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R ²	0.05	0.04	0.11	0.03	0.02	0.01	0.03	0.01	0.01	0.01	0.01
N	24782	24797	24802	24802	23301	24800	24802	4951	4951	4950	4951

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. Standard Controls, Growing Season Controls and Additional Controls are defined in Table 2. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.3: Armed-Group Violence – Woman’s Education, Wealth and Health

Dependent Variable	Index	Literacy	Schooling	Reads News	Wealth Index	Rohrer’s Index	Tested for HIV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Distance × Rainfall, 1994	-0.664 [0.164]***	-0.125 [0.038]***	-0.135 [0.069]**	-0.058 [0.023]**	-0.184 [0.071]**	-0.097 [0.051]*	-0.108 [0.022]***
Standard Controls	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.00	0.79	4.84	0.05	3.08	0.62	0.81
Dep. Std.Dev.	3.00	0.41	3.51	0.21	1.43	0.49	0.39
R ²	0.11	0.03	0.09	0.01	0.21	0.02	0.01
N	24802	24775	24791	24774	24802	12468	24760
IV: External Militia Violence	0.831 [0.136]***	0.157 [0.034]***	0.170 [0.061]***	0.073 [0.024]***	0.230 [0.068]***	0.118 [0.078]	0.134 [0.043]***

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.4: Armed-Group Violence – Women’s Financial Autonomy

Dependent Variable	Index	Works in Skilled Occupation	Receives Cash Earnings	Owns House	Owns Land
	(1)	(2)	(3)	(4)	(5)
Distance × Rainfall, 1994	−0.317 [0.071]***	−0.112 [0.078]	−0.057 [0.040]	−0.083 [0.038]**	−0.056 [0.034]*
Standard Controls	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes
Dep. Mean	0.00	0.13	0.66	0.08	0.10
Dep. Std.Dev.	2.38	0.33	0.47	0.28	0.31
R ²	0.04	0.09	0.05	0.01	0.01
N	24802	24773	20962	24793	24794
IV: External Militia Violence	0.397 [0.066]***	0.141 [0.080]*	0.068 [0.038]*	0.104 [0.056]*	0.070 [0.051]

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.5: Armed-Group Violence – Women’s Physical Autonomy

Dependent Variable	Autonomy Index		Postpartum Abstinence		Woman Decides Contraception		Can Get Condom		Ask Partner To Use Condom		Condom Used Last Sex		Can Refuse Sex If Husband Cheats		Has Other Wives		Age At Cohabitation		Underage at First Cohabitation		Age At First Birth		Teen Pregnancy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)												
Distance × Rainfall, 1994	-0.301 [0.050]***	-0.031 [0.032]	-0.016 [0.012]	-0.165 [0.025]***	-0.119 [0.045]***	-0.105 [0.027]***	-0.119 [0.045]***	0.127 [0.023]***	-0.051 [0.024]**	0.076 [0.020]***	-0.044 [0.021]**	0.048 [0.018]***												
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes												
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes												
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes												
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes												
Dep. Mean	-0.00	3.56	0.08	0.71	0.83	0.10	0.82	0.08	20.76	0.28	21.61	0.54												
Dep. Std.Dev.	1.83	9.35	0.28	0.45	0.37	0.30	0.38	0.26	3.69	0.45	3.55	0.50												
R ²	0.02	0.01	0.01	0.02	0.03	0.02	0.03	0.01	0.06	0.03	0.04	0.03												
N	23301	11210	6625	21960	12248	14228	12248	12516	15239	15239	15799	15799												
IV: External Militia Violence	0.374 [0.097]***	0.037 [0.037]	0.020 [0.014]	0.202 [0.047]***	0.143 [0.066]**	0.126 [0.043]***	0.040 [0.019]**	-0.154 [0.046]***	0.062 [0.034]*	-0.093 [0.038]**	0.054 [0.030]*	-0.058 [0.026]**												

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate the index in regression 1 using the outcomes from 2-9 and 11 in this table (10 and 12 are reformulations of 9 and 11). All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RFP presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses. Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.6: Armed-Group Violence – Women’s Decision-Making Power

Dependent Variable	Decides On		Decides On		Decides On		Needs Permission To Get Medical Help
	Index	Husband’s Money	Large Purchases	Own Healthcare	Own Healthcare	Get Medical Help	
	(1)	(2)	(3)	(4)	(4)	(5)	(5)
Distance × Rainfall, 1994	-0.090 [0.041]**	-0.045 [0.032]	0.038 [0.028]	-0.064 [0.032]**		0.053 [0.031]*	
Standard Controls	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.00	0.04	0.10	0.21	0.21	0.97	0.97
Dep. Std.Dev.	1.89	0.20	0.30	0.41	0.41	0.16	0.16
R ²	0.01	0.01	0.01	0.01	0.01	0.01	0.01
N	24800	12318	12548	12541	12541	24795	24795
IV: External Militia Violence	0.112 [0.059]*	0.055 [0.038]	-0.046 [0.034]	0.077 [0.037]**		-0.066 [0.046]	

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.7: Armed-Group Violence – Women’s Sexual Knowledge, Fertility Preferences

Dependent Variable	Index	Knowledge of	Knowledge of	Knowledge of	Wants More	Ideal Number
	(1)	Ovulatory Cycle	Contraception	HIV	Children	of Children
	(2)	(3)	(4)	(5)	(6)	(6)
Distance × Rainfall, 1994	-0.294 [0.044]***	-0.098 [0.034]***	-0.034 [0.027]	0.053 [0.021]**	0.051 [0.039]	0.073 [0.031]**
Standard Controls	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes
Dep. Mean	0.00	0.16	0.99	0.90	0.56	3.32
Dep. Std.Dev.	2.21	0.37	0.07	0.30	0.50	1.45
R ²	0.03	0.02	0.00	0.02	0.01	0.03
N	24802	24779	24802	24501	20802	24561
IV: External Militia Violence	0.369 [0.103]***	0.123 [0.050]**	0.042 [0.034]	0.066 [0.024]***	-0.064 [0.055]	-0.092 [0.047]*

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.8: Armed-Group Violence – Domestic Violence Attitudes

Dependent Variable	Beating of Wife Justified If Wife					
	Index	Goes Out Without Telling	Neglects Children	Argues With Husband	Refuses To Have Sex	Burns Food
	(1)	(2)	(3)	(4)	(5)	(6)
Distance × Rainfall, 1994	0.437 [0.167]***	0.137 [0.043]***	0.078 [0.044]*	0.099 [0.040]**	0.072 [0.033]**	0.056 [0.034]*
Standard Controls	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes
Dep. Mean	-0.00	0.29	0.37	0.27	0.31	0.14
Dep. Std.Dev.	3.99	0.45	0.48	0.44	0.46	0.35
R ²	0.05	0.04	0.04	0.03	0.05	0.02
N	24782	24673	24701	24614	24311	24699
IV: External Militia Violence	-0.548 [0.229]**	-0.172 [0.061]***	-0.097 [0.051]*	-0.123 [0.058]**	-0.090 [0.052]*	-0.070 [0.041]*

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.9: Armed-Group Violence – Violence Against Children Attitudes

Dependent Variable	Beating of Child Justified If Child			
	Index	Disobeys	Is Impolite	Embarrasses The Family
	(1)	(2)	(3)	(4)
Distance × Rainfall, 1994	0.281 [0.138]**	0.106 [0.047]**	0.098 [0.044]**	0.078 [0.052]
Standard Controls	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes
Dep. Mean	0.00	0.64	0.61	0.60
Dep. Std.Dev.	2.69	0.48	0.49	0.49
R ²	0.04	0.02	0.03	0.03
N	24797	24786	24787	24771
IV: External Militia Violence	-0.353 [0.205]*	-0.132 [0.071]*	-0.123 [0.068]*	-0.098 [0.071]

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.10: Armed-Group Violence – Domestic Violence Experience

Dependent Variable	Experienced the Following by Your Husband/Partner								
	Severe Violence				Less Severe Violence				
	Strangled or Burnt	Threatened w/ Knife or Gun	Forced Into Unwanted Sex	Forced Into Unwanted Sexual Acts	Pushed or Shook	Slapped	Punched or Hit	Kicked or Dragged	Arm Twisted Hair Pulled
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
Distance × Rainfall, 1994	0.103 [0.029]***	0.073 [0.038]*	-0.010 [0.042]	0.088 [0.033]***	-0.022 [0.055]	-0.014 [0.056]	0.018 [0.047]	0.022 [0.033]	0.049 [0.036]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.02	0.01	0.01	0.01	0.02	0.02	0.01	0.03	0.01
Dep. Std.Dev.	0.13	0.08	0.12	0.09	0.13	0.14	0.10	0.18	0.10
R ²	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
N	4949	4948	4946	4948	4944	4948	4941	4946	4947
IV: External Militia Violence	-0.122 [0.047]***	-0.086 [0.043]**	0.012 [0.050]	-0.104 [0.040]**	0.026 [0.063]	0.017 [0.065]	-0.021 [0.056]	-0.026 [0.041]	-0.059 [0.045]

Notes: The sample is restricted to women from DHS rounds 6 and 7. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.11: Armed-Group Violence – Men’s Education, Wealth and Health

Dependent Variable	Index	Literacy	Schooling	Reads News	Wealth Index
	(1)	(2)	(3)	(4)	(5)
Distance × Rainfall, 1994	−0.223 [0.184]	−0.019 [0.040]	−0.050 [0.064]	0.025 [0.034]	−0.179 [0.086]**
Standard Controls	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes
Dep. Mean	−0.00	0.81	5.04	0.11	3.26
Dep. Std.Dev.	2.80	0.39	3.66	0.31	1.39
R ²	0.14	0.02	0.07	0.07	0.19
N	11451	11436	11451	11446	11451
IV: External Militia Violence	0.293 [0.197]	0.024 [0.051]	0.066 [0.074]	−0.033 [0.048]	0.235 [0.082]***

Notes: The sample is restricted to men from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.12: Armed-Group Violence – Men’s Sexual Attitudes

Dependent Variable	Wife Can		Wife Can Refuse		Ever Paid		Paid Someone		Children Should	
	Attitude Index	Request Condom if Husband has STI	Sex if Husbands Cheats	Someone For Sex	Someone For Sex	For Sex Last 12 Months	For Sex Last 12 Months	be Taught About Condoms		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance × Rainfall, 1994	-0.228 [0.087]***	-0.011 [0.040]	-0.081 [0.037]**	0.086 [0.014]***	0.084 [0.026]***	0.032 [0.035]				
Standard Controls	yes	yes	yes	yes	yes	yes				
Growing Season Controls	yes	yes	yes	yes	yes	yes				
Additional Controls	yes	yes	yes	yes	yes	yes				
Province Effects	yes	yes	yes	yes	yes	yes				
Dep. Mean	0.00	0.98	0.87	0.08	0.03	0.90				
Dep. Std.Dev.	2.29	0.13	0.33	0.27	0.14	0.30				
R ²	0.02	0.00	0.01	0.02	0.01	0.01				
N	11343	11341	11329	11448	11448	11343				
IV: External Militia Violence	0.299 [0.089]***	0.015 [0.050]	0.105 [0.059]*	-0.113 [0.029]***	-0.111 [0.031]***	-0.042 [0.052]				

Notes: The sample is restricted to men from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.13: Armed-Group Violence – Men’s Sexual Knowledge, Fertility Preferences

Dependent Variable	Knowledge of		Wants More		Ideal Number		Contraception		Contraception Makes	
	Index	Ovulatory Cycle	Knowledge of Contraception	Children	Children	of Children	Is Womens Business	Women Promiscuous	Contraception	Women Promiscuous
	(1)	(2)	(3)	(4)	(5)	(6)	(7)			
Distance × Rainfall, 1994	-0.060 [0.063]	-0.107 [0.046]**	-0.007 [0.014]	-0.009 [0.068]	-0.079 [0.028]**	0.024 [0.027]	0.010 [0.037]			
Standard Controls	yes	yes	yes	yes	yes	yes	yes			yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes			yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes			yes
Province Effects	yes	yes	yes	yes	yes	yes	yes			yes
Dep. Mean	0.00	0.19	1.00	0.40	2.91	0.18	0.27			0.27
Dep. Std.Dev.	2.77	0.39	0.06	0.49	1.06	0.87	1.21			1.21
R ²	0.02	0.05	0.01	0.01	0.02	0.02	0.01			0.01
N	11451	11446	11451	5983	11440	11423	11422			11422
IV: External Militia Violence	0.079 [0.079]	0.141 [0.065]**	0.009 [0.017]	0.012 [0.084]	0.104 [0.043]**	-0.031 [0.032]	-0.013 [0.047]			

Notes: The sample is restricted to men from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RFF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.14: Armed-Group Violence – Men’s Responses to Women’s Decision-Making Power

Dependent Variable	Woman Can Have A Say On			
	Index	Husband’s Money	Husband’s Healthcare	Large HH Purchases
	(1)	(2)	(3)	(4)
Distance × Rainfall, 1994	−0.375 [0.081]***	−0.259 [0.041]***	−0.020 [0.072]	−0.137 [0.047]***
Standard Controls	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes
Dep. Mean	0.00	0.79	0.56	0.67
Dep. Std.Dev.	2.08	0.41	0.50	0.47
R ²	0.04	0.04	0.04	0.03
N	6461	5016	6458	6461
IV: External Militia Violence	0.456 [0.090]***	0.346 [0.104]***	0.024 [0.087]	0.167 [0.038]***

Notes: The sample is restricted to men from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.15: Armed-Group Violence – Men’s Domestic Violence Attitudes

Dependent Variable	Beating of Wife Justified If Wife					Beating of Son Justified If Son				
	Index (1)	Goes Out Without Telling (2)	Neglects Children (3)	Argues With Husband (4)	Refuses To Have Sex (5)	Burns Food (6)	Cheats (7)	Disobeys (8)	Is Impolite (9)	Embarrasses The Family (10)
Distance × Rainfall, 1994	0.109 [0.292]	0.100 [0.027]***	0.018 [0.043]	0.007 [0.033]	0.032 [0.061]	0.026 [0.023]	-0.040 [0.037]	-0.029 [0.056]	-0.020 [0.068]	0.010 [0.061]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.00	0.09	0.15	0.08	0.09	0.03	0.38	0.58	0.55	0.53
Dep. Std.Dev.	5.57	0.29	0.36	0.35	0.29	0.21	0.49	0.49	0.50	0.50
R ²	0.04	0.02	0.03	0.01	0.02	0.01	0.03	0.03	0.03	0.02
N	11450	11420	11426	11410	11371	11425	11265	11445	11444	11399
IV: External Militia Violence	-0.144 [0.391]	-0.131 [0.054]**	-0.024 [0.058]	-0.010 [0.043]	-0.042 [0.078]	-0.034 [0.034]	0.053 [0.053]	0.038 [0.073]	0.026 [0.088]	-0.014 [0.082]

Notes: The sample is restricted to men from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.16: Local Violence (RTLTM) – Main Effects

Dependent Variable	Domestic Violence				Domestic Violence						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Domestic Violence Attitudes	Violence Against Kids Attitudes	Education, Wealth, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility	All	Severe	Sexual	Less Severe
RTLTM Radio Coverage	1.524 (0.813)*	0.545 (0.326)	-0.413 (0.667)	0.109 (0.201)	-0.460 (0.248)*	0.011 (0.134)	-0.250 (0.129)*	0.043 (1.099)	0.154 (0.370)	0.015 (0.310)	-0.111 (0.781)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	-0.00	0.00	0.00	-0.00	-0.00	-0.00	0.00	-0.00	-0.00	-0.00	-0.00
Dep. Std.Dev.	3.98	2.69	3.00	2.27	1.83	1.90	2.21	6.16	2.76	1.75	4.05
R ²	0.09	0.07	0.14	0.08	0.04	0.02	0.04	0.04	0.04	0.04	0.05
N	17877	17889	17892	17892	16822	17890	17892	3525	3525	3525	3525
IV: Local Militia Violence	2.131 (1.613)	0.759 (0.631)	-0.575 (1.027)	0.152 (0.242)	-0.658 (0.529)	0.016 (0.185)	-0.348 (0.278)	0.063 (1.534)	0.224 (0.534)	0.022 (0.433)	-0.162 (1.120)

Notes: The sample is restricted to women from DHS rounds 6 and 7. **Propagation controls** are: latitude, longitude, a second order polynomial in village mean altitude, village altitude variance, and a second order polynomial in the distance to the nearest transmitter. **Additional Controls** include distance to the road, distance to the border, distance to major city, population and population density, and sloping dummies. There are 122 communes in the sample. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.17: Local Violence – Women’s Education, Wealth and Health

Dependent Variable	Literacy		Schooling		Reads News		Wealth Index		Rohrer’s Index		Tested for HIV	
	(1)	(2)	(3)	(4)	(5)	(6)						
RTLM Radio Coverage	-0.134 (0.131)	-0.204 (0.248)	0.205 (0.127)	-0.215 (0.200)	-0.094 (0.112)	-0.008 (0.096)						
Propagation Controls	yes	yes	yes	yes	yes	yes						
Additional Controls	yes	yes	yes	yes	yes	yes						
Commune Effects	yes	yes	yes	yes	yes	yes						
Dep. Mean	0.79	4.85	0.05	3.05	0.63	0.81						
Dep. Std.Dev.	0.41	3.47	0.22	1.43	0.48	0.39						
R ²	0.04	0.12	0.03	0.25	0.04	0.02						
N	17872	17884	17874	17892	8997	17859						
IV: Local Militia Violence	-0.187 (0.224)	-0.285 (0.417)	0.287 (0.252)	-0.299 (0.376)	-0.140 (0.161)	-0.011 (0.130)						

Notes: The sample is restricted to women from DHS rounds 6 and 7. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.18: Local Violence – Women’s Financial Autonomy

Dependent Variable	Works in Skilled Job	Receives Cash Earnings	Owens House	Owens Land
	(1)	(2)	(3)	(4)
RTLTM Radio Coverage	0.027 (0.129)	−0.121 (0.163)	−0.013 (0.066)	−0.026 (0.046)
Propagation Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes
Dep. Mean	0.12	0.66	0.08	0.10
Dep. Std.Dev.	0.33	0.47	0.27	0.31
R ²	0.11	0.11	0.03	0.03
N	17865	15095	17885	17887
IV: Local Militia Violence	0.037 (0.165)	−0.190 (0.314)	−0.018 (0.087)	−0.036 (0.067)

Notes: The sample is restricted to women from DHS rounds 6 and 7. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.19: Local Violence – Women’s Physical Autonomy

Dependent Variable	Woman Decides Contraception		Can Get Condom		Ask Partner To Use Condom		Condom Used Last Sex		Has Other Wives		Age At First Cohabitation		Underage at First Cohabitation		Age At First Birth		Teen Pregnancy		Can Refuse Sex If Husband Cheats	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)									
RTLM Radio Coverage	-0.097 (0.084)	-0.047 (0.095)	-0.244 (0.110)**	-0.173 (0.142)	-0.100 (0.118)	-0.147 (0.116)	-0.038 (0.087)	0.083 (0.073)	-0.053 (0.076)	0.141 (0.081)*	-0.039 (0.075)									
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes									
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes									
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes									
Dep. Mean	3.66	0.97	0.70	0.83	0.10	0.07	20.92	0.26	21.75	0.27	0.88									
Dep. Std.Dev.	9.39	0.16	0.46	0.38	0.30	0.26	3.71	0.44	3.59	0.45	0.33									
R ²	0.02	0.04	0.04	0.07	0.03	0.02	0.07	0.04	0.05	0.03	0.04									
N	8088	4750	15877	8767	10232	8973	10941	10941	11405	11405	8301									
IV: Local Militia Violence	-0.165 (0.177)	-0.093 (0.191)	-0.336 (0.257)	-0.261 (0.217)	-0.157 (0.225)	-0.215 (0.208)	-0.056 (0.135)	0.124 (0.134)	-0.081 (0.115)	0.214 (0.150)	-0.055 (0.093)									

Notes: The sample is restricted to women from DHS rounds 6 and 7. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.20: Local Violence – Women’s Sexual Knowledge, Fertility Preferences

Dependent Variable	Knowledge of Ovulatory Cycle		Knowledge of HIV		Wants More Children		Ideal Number of Children		Contraception Business		Contraception makes Women Promiscuous	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)					
RTLTM Radio Coverage	-0.030 (0.125)	-0.047 (0.037)	-0.015 (0.091)	0.062 (0.127)	0.128 (0.105)	0.139 (0.122)	0.177 (0.081)**					
Propagation Controls	yes	yes	yes	yes	yes	yes	yes					
Additional Controls	yes	yes	yes	yes	yes	yes	yes					
Commune Effects	yes	yes	yes	yes	yes	yes	yes					
Dep. Mean	0.17	0.99	0.90	0.55	3.27	0.18	0.28					
Dep. Std.Dev.	0.38	0.07	0.30	0.50	1.41	0.89	1.22					
R ²	0.04	0.02	0.04	0.02	0.04	0.05	0.04					
N	17871	17892	17670	15000	17704	8371	8370					
IV: Local Militia Violence	-0.042 (0.178)	-0.065 (0.063)	-0.021 (0.125)	0.093 (0.185)	0.179 (0.184)	0.197 (0.224)	0.252 (0.213)					

Notes: The sample is restricted to women (men) from DHS rounds 6 and 7 in regressions 1 to 5 (regressions 6 and 7). All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.21: Local Violence – Women’s Decision-Making Power

Dependent Variable	Decides On	Decides On	Decides On	Needs Permission To	Women Should Participate
	Husband’s Money	Large Purchases	Own Healthcare	Get Medical Help	In Large Purchases
	(1)	(2)	(3)	(4)	(5)
RTLM Radio Coverage	0.068 (0.101)	-0.003 (0.107)	-0.093 (0.107)	0.030 (0.072)	-0.254 (0.125)*
Propagation Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
Dep. Mean	0.05	0.10	0.21	0.98	0.66
Dep. Std.Dev.	0.21	0.30	0.41	0.16	0.47
R ²	0.03	0.02	0.03	0.02	0.07
N	8832	9002	8998	17885	4642
IV: Local Militia Violence	0.105 (0.149)	-0.004 (0.150)	-0.136 (0.141)	0.042 (0.102)	-0.517 (0.449)

Notes: The sample is restricted to women from DHS rounds 6 and 7. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.22: Local Violence – Violence Against Children – Attitudes

Dependent Variable	Beating of Child Justified If Child			
	Index	Disobeys	Is Impolite	Embarrasses The Family
	(1)	(2)	(3)	(4)
RTLM Radio Coverage	0.545 (0.326)	0.156 (0.117)	0.231 (0.107)**	0.160 (0.117)
Propagation Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes
Dep. Mean	0.00	0.64	0.61	0.60
Dep. Std.Dev.	2.69	0.48	0.49	0.49
R ²	0.07	0.06	0.06	0.07
N	17889	17878	17882	17868
IV: Local Militia Violence	0.759 (0.631)	0.218 (0.199)	0.322 (0.238)	0.224 (0.215)

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate the index in regression 1 using all the other outcomes in this table. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.23: Local Violence – Domestic Violence Attitudes

Dependent Variable	Beating of Wife Justified If Wife				
	Goes Out Without Telling	Neglects Children	Argues With Husband	Refuses To Have Sex	Burns Food
	(1)	(2)	(3)	(4)	(5)
RTLTM Radio Coverage	0.270 (0.164)	0.291 (0.145)*	0.312 (0.182)*	0.286 (0.164)*	0.372 (0.195)*
Propagation Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
Dep. Mean	0.29	0.37	0.27	0.31	0.14
Dep. Std.Dev.	0.46	0.48	0.44	0.46	0.35
R ²	0.06	0.07	0.07	0.09	0.07
N	17790	17812	17746	17527	17817
IV: Local Militia Violence	0.381 (0.307)	0.408 (0.295)	0.438 (0.353)	0.404 (0.306)	0.518 (0.401)

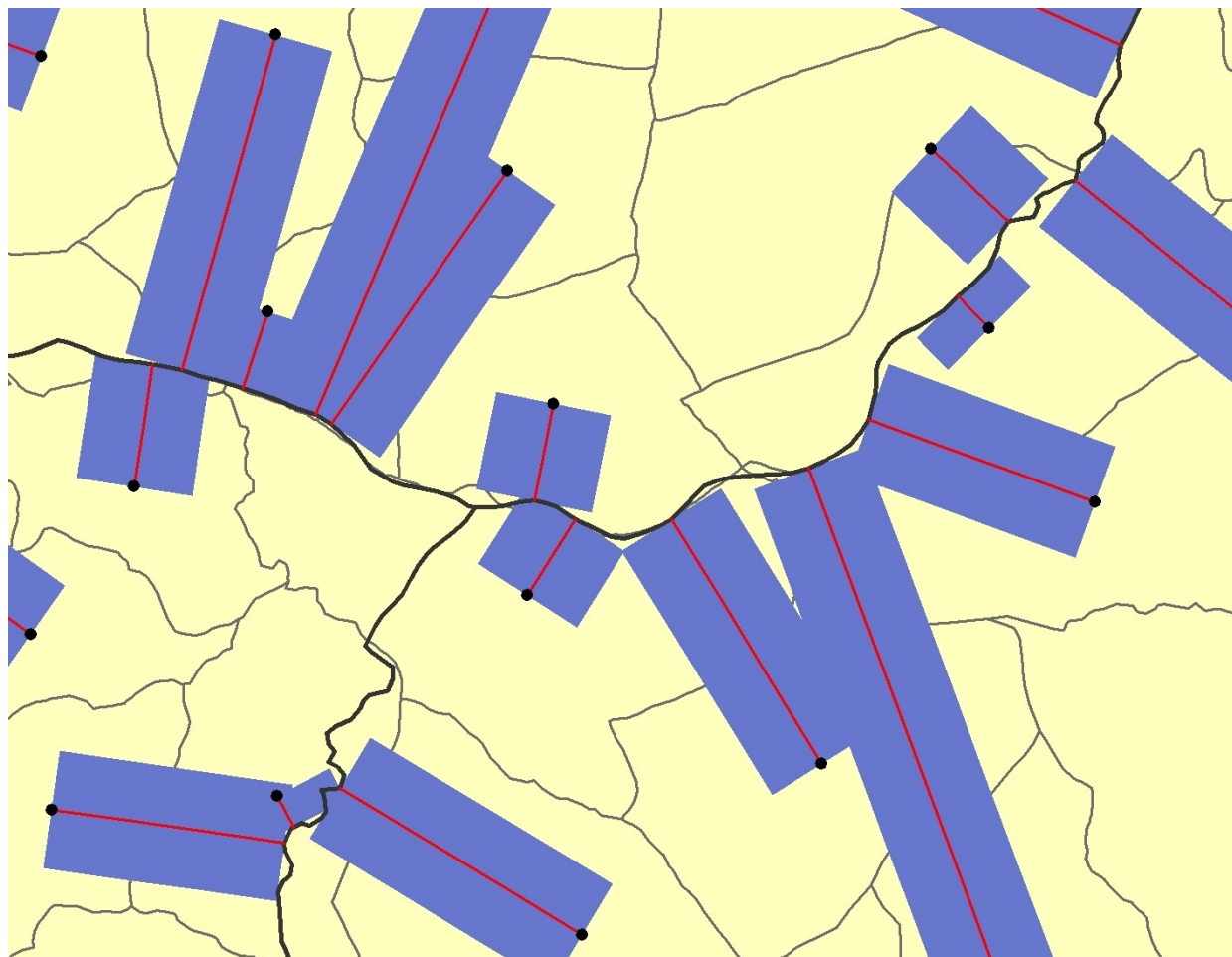
Notes: The sample is restricted to women from DHS rounds 6 and 7. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.24: Local Violence – Domestic Violence Experience

Dependent Variable	Experienced the Following by Your Husband/Partner								
	Severe Violence				Less Severe Violence				
	Strangled or Burnt	Threatened w/ Knife or Gun	Forced Into Unwanted Sex	Forced Into Unwanted Sexual Acts	Pushed or Shook	Slapped	Punched or Hit	Kicked or Dragged	Arm Twisted Hair Pulled
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
RTL M Radio Coverage	0.031 (0.111)	0.114 (0.197)	0.025 (0.200)	-0.010 (0.160)	-0.012 (0.171)	-0.130 (0.173)	-0.073 (0.220)	0.074 (0.156)	0.030 (0.205)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.02	0.01	0.01	0.01	0.02	0.02	0.01	0.03	0.01
Dep. Std.Dev.	0.13	0.09	0.12	0.09	0.13	0.14	0.10	0.18	0.10
R ²	0.05	0.03	0.04	0.04	0.05	0.06	0.05	0.03	0.05
N	3524	3524	3523	3524	3521	3524	3517	3522	3524
IV: Local Militia Violence	0.045 (0.156)	0.166 (0.313)	0.037 (0.279)	-0.014 (0.224)	-0.017 (0.241)	-0.190 (0.302)	-0.104 (0.326)	0.108 (0.239)	0.043 (0.284)

Notes: The sample is restricted to women from DHS rounds 6 and 7. All outcome variables are standardized. The reported means and standard deviations refer to the unstandardized outcomes. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure A.1: Construction of Transport Costs



- Village centroids
- Main roads
- Shortest distance between main road and village centroid
- 500m rainfall buffers
- Village polygons

Notes: Picture is taken from Rogall (2021).

A.2 Extensions to Sections 5 and 6: Detailed Anecdotal Evidence

Responsibility as Household Heads In this section we provide more detailed anecdotal evidence backing our claim above that women were forced to take on more responsibility within their households and local communities – with positive effects on (women’s) welfare.

First, Hunt (2017, p.72) notes *“At the grassroots level, too, women’s leadership started out organically. They (...) converted proficiencies into newly required skills: (...) cleaning expanded to construction. Traditionally, women never helped build houses. Now, they were on site, pushing wheelbarrows, hauling bricks.”*

A Rwandan woman interviewed by Behnke (2019) notes *“They [women] had no choice (...). This is what happened to our society. We can’t wait for men to tell us what to do – they are not here.”* In a similar vein, another interviewee, Nadine Umutoni Gatsinzi, the secretary of Rwanda’s Ministry of Gender and Family Promotion, adds *“Women realized they were now the head of the household, they have to take care of their children, they have to work.”*

Responsibility as Politicians and Public Goods Provision The documented drop in the number of males and in particular adult males not only forced women to take over more responsibility in their families but also left a political vacuum which again women filled. Consistent with the existing literature cited above, in these powerful positions women focused on providing important public goods and set examples for future (female) generations to come (we show below that the results are particularly strong for younger women).

To give some more anecdotal evidence, Alphonsine M., a Rwandan politician working her way up from the grass roots level, interviewed in Hunt (2017, p.153-154) remembers: *“We [female politicians] kept in touch with women from the grass roots. (...) we went to visit them and said, ‘(...) You should participate in the elections – and not just as voters but as candidates!’ (...) That gave them confidence to imagine what their future could be.”*

Furthermore, when interviewed on the credibility and responsiveness of female politicians, Governor Inyumba replied Hunt (2017, p.338): *“I think that’s why we women are more responsive as leaders, (...). It’s our daily life. I understand issues of children, of health, even legal reform and access to land – we’re the ones tilling the land.”* The last quote hints at why we should not only expect improvements in education and public health (the more traditional women’s interests). Women now held all the responsibilities, e.g., they were gathering firewood, they were taking their goods to the market. Thus they would also benefit from investments in roads or electricity access.

References

Behnke, C. (2019). *Women in Rwanda Redefine Gender Roles through Agribusiness*.

Hunt, S. (2017). *Rwandan Women Rising*. Duke University Press, Durham and London.

A.3 Extensions to Section 7.3: Robustness Checks

We conduct a number of robustness checks. Our preferred specification includes controls for distance to the border, distance to main cities, distance to the capital Kigali and distance to Nyanza (the old Tutsi kingdom capital) as well as population density and the number of days the RPF was present in each village. In Table A.25 we show that all the results are robust to dropping these controls.

Next, in Table A.26 we add a number of individual and household controls. Note that we only show these specifications as a robustness check and not as our baseline specification since all of these are potentially bad controls, i.e., affected by genocide violence themselves. Nevertheless, in Table A.26 we add controls for age, age squared, gender and age of the household head, number of household members, number of children under 5 in the household, and religion fixed effects. All our results are essentially identical.

The results are robust to controlling for average post-genocide rainfall (years 1995 to 2010/15) during the 100 calendar days of the genocide period along the way between village and main road and its interaction with distance to the main road (Table A.27). This is unsurprising since Rogall (2021) shows that transport costs during the genocide period do not affect socioeconomic outcomes directly.²

Finally, Figures A.2 and A.3 show the relationship between external armed-group violence and all our main outcomes graphically. Importantly, none of the effects seems to be driven by outliers.

References

Rogall, T. (2021). Mobilizing the Masses for Genocide. *American Economic Review*, 111(1):41-72.

²The results are further robust to controlling for average post-genocide rainfall in the village during the growing seasons and its interaction with distance to the road, as well as average post-genocide rainfall in the village for the genocide calendar period.

Table A.25: Robustness Check I – Main Effects (2010 and 2015) No Additional Controls

Dependent Variable	Domestic Violence										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	All	(9)	(10)	(11)
	Domestic Violence Attitudes	Violence Against Kids Attitudes	Education, Wealth, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility		Severe	Sexual	Less Severe
Distance × Rainfall, 1994	0.436 [0.164]***	0.272 [0.146]*	-0.660 [0.185]***	-0.286 [0.072]***	-0.331 [0.045]***	-0.091 [0.036]**	-0.299 [0.061]***	0.287 [0.302]	0.232 [0.097]**	0.057 [0.068]	0.055 [0.227]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R ²	0.05	0.03	0.11	0.03	0.02	0.01	0.02	0.01	0.01	0.01	0.01
N	24812	24827	24817	24832	23307	24816	24823	4961	4961	4960	4961
IV: External Militia Violence	-0.583 [0.229]**	-0.363 [0.229]	0.883 [0.191]***	0.382 [0.087]***	0.439 [0.111]***	0.122 [0.060]**	0.400 [0.132]***	-0.372 [0.410]	-0.301 [0.137]**	-0.074 [0.089]	-0.071 [0.298]

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. Standard Controls and Growing Season Controls are defined in Table 2. All control variables are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.26: Robustness Check II – Main Effects (2010 and 2015) Household Controls

Dependent Variable	Domestic Violence										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Domestic Violence Attitudes	Violence Against Kids Attitudes	Education, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility	All	Severe	Sexual	Less Severe
Distance × Rainfall, 1994	0.441 [0.168]***	0.292 [0.135]**	-0.643 [0.154]***	-0.292 [0.069]***	-0.300 [0.050]***	-0.081 [0.045]*	-0.278 [0.040]***	0.282 [0.281]	0.254 [0.096]***	0.080 [0.070]	0.028 [0.214]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Household Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R ²	0.06	0.05	0.16	0.17	0.04	0.03	0.07	0.01	0.01	0.01	0.01
N	24635	24650	24655	24655	23162	24653	24655	4920	4920	4919	4920
IV: External Militia Violence	-0.556 [0.231]**	-0.368 [0.199]*	0.810 [0.123]***	0.368 [0.063]***	0.376 [0.107]***	0.102 [0.071]	0.350 [0.099]***	-0.338 [0.348]	-0.305 [0.125]**	-0.096 [0.084]	-0.033 [0.247]

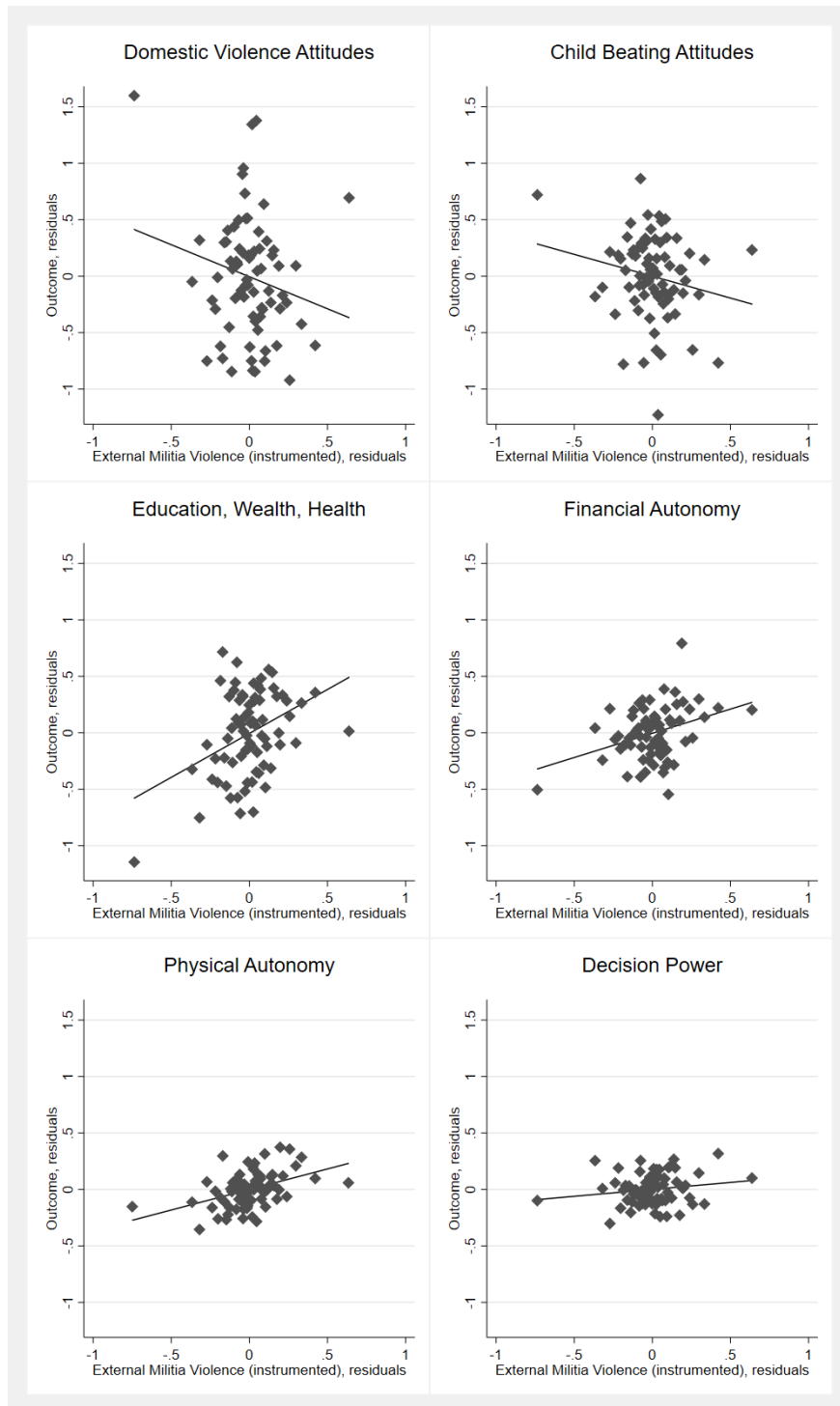
Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All village-level control variables, except “Number of Days with RFP presence,” are in logs. Interactions are first logged and then interacted. Household Controls are age, age squared, gender and age of the household head, household size, number of children under 5, and religion fixed effects. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.27: Robustness Check III – Main Effects (2010 and 2015) Additional Rainfall Controls

Dependent Variable	Domestic Violence							Domestic Violence			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	All	Severe	Sexual	Less Severe
	Domestic Violence Attitudes	Violence Against Kids Attitudes	Education, Wealth, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility				
Distance × Rainfall, 1994	0.478 [0.189]**	0.303 [0.167]*	-0.561 [0.190]***	-0.319 [0.068]***	-0.278 [0.054]***	-0.104 [0.041]**	-0.275 [0.054]***	0.454 [0.232]*	0.364 [0.102]***	0.163 [0.070]**	0.089 [0.174]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Rainfall Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R ²	0.05	0.04	0.11	0.04	0.02	0.01	0.03	0.01	0.01	0.01	0.01
N	24782	24797	24802	24802	23301	24800	24802	4951	4951	4950	4951
IV: External Militia Violence	-0.642 [0.282]**	-0.407 [0.255]	0.752 [0.196]***	0.428 [0.088]***	0.373 [0.102]***	0.140 [0.058]**	0.369 [0.117]***	-0.529 [0.294]*	-0.424 [0.124]***	-0.190 [0.083]**	-0.104 [0.209]

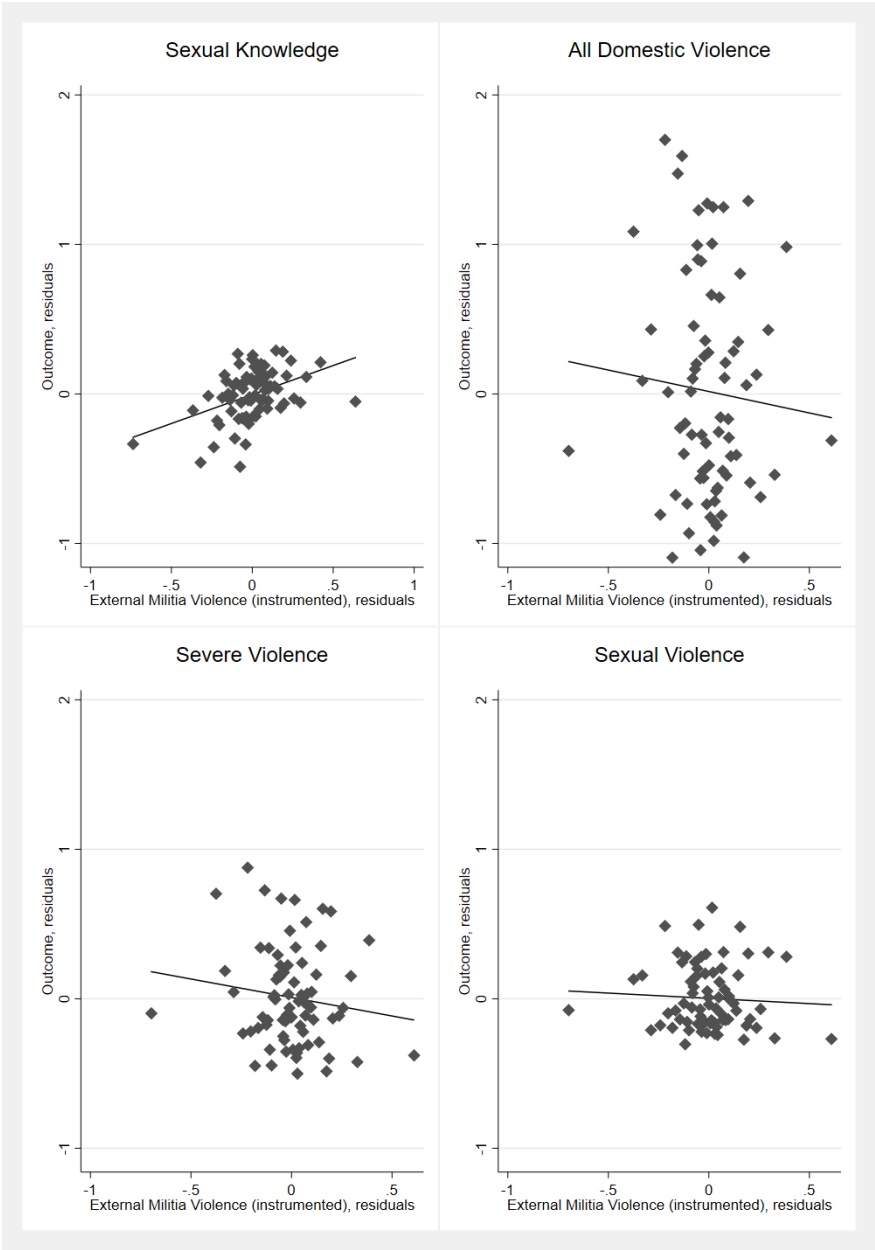
Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. Standard Controls and Growing Season Controls, and Additional Controls are defined in Table 2. **Additional Rainfall Controls** are average rainfall along the buffer between village and main road for 1995 to the survey collection year and its interaction with distance to the main road. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure A.2: Relationship between External Violence and Main Outcomes I



Notes: Observations are grouped into 75 equal-sized bins. We use all baseline controls and province effects to construct residuals.

Figure A.3: Relationship between External Violence and Main Outcomes II



Notes: Observations are grouped into 75 equal-sized bins. We use all baseline controls and province effects to construct residuals.

A.4 Extensions to Section 7.4: Effects over Time and Intergenerational Transmission

Effects Over Time To better understand the effects on female outcomes, we also track them over time. Above, we have documented strong positive effects of armed-group violence for some 15 to 20 years after the genocide. We now re-run our main results using data from the 5th DHS round from 2005 – ten years after the genocide. The results reported in Table A.28 suggest that the positive effects of armed-group violence are not yet present in 2005: the point estimates are all close to zero and insignificant (except for domestic violence attitudes which already point towards a positive development). Importantly, these null effects are unlikely simply the result of a weak first stage. Table A.29 shows that the instrument is still strong for the subset of 2005 DHS villages (regressions 1 and 2) with a point estimate identical to the one using the full sample of villages. It further maps negatively into whether a village has a mass grave site (regressions 3 and 4). Thus it seems to have taken about 15 to 20 years for the changes to become visible.

Intergenerational Transmission Further consistent with this time lag, the effects seem to be driven by younger generations of women. In Table A.30 we split the 2010/2015 DHS sample into women younger and older than 18 at the time of the genocide. The results suggest that the effects are particularly strong for younger women, especially for the results on domestic violence. In contrast to the full sample, the point estimates are larger in absolute value and all highly significant (regressions 1 to 11).³ This is consistent with anecdotal evidence suggesting that older women were often reluctant to change. Fatuma interviewed in Hunt (2017, p.223): *“We also had resistance, especially from old women, who were saying, ‘Why don’t we stick to tradition?’ (...).”* On the other hand, young women especially seem to have been inspired by their mothers and political leaders. Mutamba, interviewed in Hunt (2017, p.304), notes: *“We have this generation of leaders, like those of my age, who grew up seeing our mothers effectively cope (...). Managing in a crisis, being resourceful, keeping the children well, giving children our values, and maintaining our dignity. And we have male leaders who appreciate that.”* Nadine Niyetigeka, a student interviewee, continues (p.334): *“While I was growing up, we had some great women leaders, (...). They showed how powerful they were, (...). I believe that I will become a great leader...”*

To strengthen the argument that intergenerational transmissions matter, we show that the results are stronger for non-migrant women. In Table A.31, we split the sample into women that never left their home village (regressions 1 to 4), left at least once (regressions 5 to 8), and more than once (regressions 9 to 12) in the last 12 months.⁴ The findings sug-

³Note however that the results are robust to dropping women born after the genocide, thus not simply driven by the post-genocide generation.

⁴Ideally, we’d like to have data on migrants or native borns. Unfortunately, while these questions are asked in the DHS questionnaire they are not publicly distributed for Rwanda. While we believe that the last 12 months are indicative of an individual’s general migration behavior (since people in Rwanda traditionally travel little and about half of the sample did not leave their home village), we do acknowledge that

Table A.28: Armed-Group Violence – Main Effects (2005)

Dependent Variable	Domestic Violence					Domestic Violence				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance × Rainfall, 1994	-0.386 [0.119]***	-0.054 [0.185]	0.005 [0.072]	-0.064 [0.086]	0.097 [0.071]	0.146 [0.130]	0.046 [0.405]	-0.131 [0.140]	0.052 [0.064]	0.178 [0.292]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Dep. Std.Dev.	3.32	3.14	1.50	1.46	2.03	2.47	5.31	2.15	1.00	3.93
R ²	0.10	0.17	0.20	0.04	0.01	0.06	0.01	0.02	0.03	0.02
N	9961	10023	10020	7239	10022	10023	2269	2269	2268	2269
IV: External Militia Violence	0.634 [0.379]*	0.088 [0.280]	-0.009 [0.119]	0.101 [0.120]	-0.160 [0.153]	-0.241 [0.285]	-0.091 [0.800]	0.259 [0.297]	-0.103 [0.132]	-0.350 [0.603]

Notes: The sample is restricted to women from DHS round 5. We calculate various indexes, the composition of each index is given in the paper. Note that the violence-against-kids-attitude questions are not available for the 5th DHS round. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except "Number of Days with RFF presence," are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.29: First Stage – DHS (2005)

Dependent Variable	# Militiamen, log		Mass Graves	
		Add. Controls		Add. Controls
	(1)	(2)	(3)	(4)
Distance × Rainfall, 1994	−0.419 [0.211]**	−0.509 [0.204]**	−0.073 [0.034]**	−0.079 [0.035]**
Standard Controls	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes
Additional Controls	no	yes	no	yes
Province Effects	yes	yes	yes	yes
F-stat	3.94	6.23	4.61	5.09
R ²	0.45	0.48	0.10	0.11
N	346	346	346	346

Notes: Mass Graves is a dummy taking on the value of 1 if a mass grave was found in a village. The sample is restricted to villages from DHS round 5. **Distance × Rainfall, 1994** is the instrument (distance to the main road interacted with rainfall along the way (a 500m buffer) between village and main road during the 100 days of the genocide in 1994). Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

gest that especially the effects on domestic violence (both attitude and experiences) seem to be driven by the non-travelers.⁵ That intergenerational transmission matters especially for domestic violence seems reasonable since it has particularly lasting and harmful impacts on children (Carrell and Hoekstra, 2010; Aizer, 2011).

Finally, we can directly look at parents' early childhood investments. The DHS data contains information on whether children between 3 and 5 years old have someone who reads to them, sings with them, tells them stories, takes them outside, plays with them, or counts and draws with them. All of these are important for a child's development of both cognitive and non-cognitive skills (Heckman et al., 2006; Attanasio et al., 2020). In Table A.32 we show the results for each outcome together with an index. Importantly, parents are significantly more likely to invest in girls: a one standard-deviation increase in genocide violence leads to a 0.25 standard-deviations increase in our early childhood investment index (Panel A, regression 1). For boys the total effect is smaller and insignificant, however still positive (Panel B, regression 1).⁶

Consistently, we observe a significant increase in cognitive and non-cognitive skills for these children. In Table A.33 we report the results for various outcomes and a total index.⁷ A one standard-deviation increase in genocide violence is associated with a 0.5 standard-deviation increase in the skill index for both boys and girls (regression 1 in Panels A and B). One reduced-form result worth mentioning is that girls are significantly more likely to act out (i.e., kick or hit). Kicking or hitting is usually regarded as boys' behavior and the strong increase for girls is thus likely indicative of a more equal upbringing and not necessarily a bad outcome (regression 10). Schindler (2010, p. 9) notes that traditionally "Rwandan girls are brought up to be modest, reserved, silent, obedient, and with a submissive attitude."

References

Aizer, A. (2011). Poverty, Violence, and Health: The Impact of Domestic Violence During Pregnancy on Newborn Health. *Journal of Human Resources*, 46(3):518-538.

Attanasio, O., Cattan, S., Fitzsimons, E., Meghir, C., and Rubio-Codina, M. (2020). Estimating the production function for human capital: Results from a randomized controlled trial in Colombia. *American Economic Review*, 110(1):48-85.

this exercise should be seen as suggestive.

⁵We only report the results on domestic violence. For all other indexes we find no significant differences between the three sub-samples.

⁶Note that we cannot formally reject the null hypothesis that parents' investments in boys and girls differ. The p-value, based on the overall index, equals 0.644.

⁷Outcomes are for instance whether a child can identify letters, reads simple words, knows the numbers from 1 to 10, follows simple instructions, can concentrate on a task, gets along with other children, is allowed to "act out", and can pick up small objects with two fingers (fine motor skills).

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Table A.30: Armed-Group Violence – Young versus Old

Dependent Variable	Domestic Violence							Domestic Violence			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Domestic Violence Attitudes	Violence Against Kids Attitudes	Education, Wealth, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility	All	Severe	Sexual	Less Severe
Panel A – Young Women (< 18 years)											
Distance × Rainfall, 1994	0.496 [0.173]***	0.306 [0.136]**	-0.678 [0.179]***	-0.342 [0.064]***	-0.266 [0.053]***	-0.175 [0.036]***	-0.287 [0.052]***	0.622 [0.333]*	0.323 [0.154]**	0.213 [0.089]**	0.299 [0.187]
R ²	0.05	0.04	0.11	0.05	0.02	0.01	0.03	0.01	0.01	0.01	0.01
N	18863	18879	18883	18883	17595	18881	18883	3235	3235	3235	3235
IV: External Militia Violence											
	-0.631 [0.221]***	-0.388 [0.209]*	0.862 [0.142]***	0.435 [0.083]***	0.336 [0.093]***	0.223 [0.076]***	0.365 [0.104]***	-0.831 [0.457]*	-0.431 [0.215]**	-0.284 [0.127]**	-0.400 [0.254]
Panel B – Older Women (> 18 years)											
Distance × Rainfall, 1994	0.314 [0.267]	0.254 [0.171]	-0.556 [0.120]***	-0.286 [0.170]*	-0.408 [0.079]***	0.184 [0.113]	-0.382 [0.079]***	-0.664 [0.440]	-0.036 [0.178]	-0.300 [0.103]***	-0.628 [0.397]
R ²	0.05	0.03	0.14	0.04	0.02	0.01	0.04	0.02	0.02	0.02	0.02
N	5919	5918	5919	5919	5706	5919	5919	1716	1716	1715	1716
IV: External Militia Violence											
	-0.373 [0.341]	-0.302 [0.218]	0.660 [0.156]***	0.339 [0.142]**	0.483 [0.147]***	-0.219 [0.153]	0.454 [0.160]***	0.628 [0.388]	0.034 [0.169]	0.284 [0.118]**	0.594 [0.332]*

Notes: The sample is restricted to DHS rounds 6 and 7. In Panel A, we use women younger than 18 during the time of the genocide. In Panel B, we restrict the sample to women older than 18 during the genocide. We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2. All control variables, except 'Number of Days with RPT presence', are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.31: Armed-Group Violence – Intergenerational Transmission

Sample	Non-Migrants			At Least Once Away			More Than Once Away					
	Domestic Violence Attitudes	Violence Against Kids Attitudes	Domestic Violence Total	Domestic Violence Attitudes	Violence Against Kids Attitudes	Domestic Violence Total	Domestic Violence Attitudes	Violence Against Kids Attitudes	Domestic Violence Total			
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Distance × Rainfall, 1994	0.514 [0.200]**	0.392 [0.151]***	1.032 [0.589]*	0.595 [0.259]**	0.388 [0.151]**	0.191 [0.143]	-0.226 [0.249]	0.026 [0.086]	-0.130 [0.216]	0.092 [0.135]	-0.033 [0.344]	0.069 [0.131]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R ²	0.06	0.05	0.02	0.01	0.05	0.03	0.02	0.02	0.05	0.03	0.03	0.03
N	13089	13101	2898	2898	11669	11672	2053	2053	5592	5593	846	846
IV: External Militia Violence	-0.676 [0.305]**	-0.513 [0.243]**	-1.620 [0.953]*	-0.934 [0.399]**	-0.465 [0.182]**	-0.229 [0.189]	0.220 [0.231]	-0.025 [0.084]	0.161 [0.290]	-0.115 [0.176]	0.033 [0.338]	-0.068 [0.131]

Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.32: Armed-Group Violence – Parents’ Investments (Girls and Boys)

Dependent Variable	Someone in Household ... to/with Child						
	Index	Read Books	Told Stories	Sang Songs	Went Outside	Played	Named, Counted, Drew
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A – Girls Age 3-5							
Distance × Rainfall, 1994	-0.589 [0.308]*	0.035 [0.043]	-0.068 [0.051]	-0.095 [0.031]***	-0.151 [0.047]***	-0.039 [0.049]	0.026 [0.041]
Dep. Mean	0.00	0.26	0.41	0.54	0.58	0.72	0.55
Dep. Std.Dev.	4.32	0.44	0.49	0.50	0.49	0.45	0.50
R ²	0.12	0.05	0.06	0.08	0.08	0.12	0.10
N	1174	1174	1173	1174	1173	1173	1174
IV: External Militia Violence	0.753 [0.453]*	-0.044 [0.065]	0.086 [0.075]	0.121 [0.057]**	0.192 [0.092]**	0.050 [0.066]	-0.034 [0.054]
Panel B – Boys Age 3-5							
Distance × Rainfall, 1994	-0.277 [0.358]	-0.113 [0.03]***	-0.032 [0.038]	-0.009 [0.039]	-0.011 [0.052]	0.035 [0.039]	0.004 [0.056]
Dep. Mean	-0.00	0.25	0.41	0.51	0.61	0.72	0.55
Dep. Std.Dev.	4.36	0.43	0.49	0.50	0.49	0.45	0.50
R ²	0.09	0.06	0.05	0.07	0.09	0.12	0.08
N	1242	1242	1242	1242	1239	1239	1242
IV: External Militia Violence	0.308 [0.409]	0.125 [0.043]***	0.036 [0.042]	0.010 [0.043]	0.013 [0.059]	-0.038 [0.043]	-0.005 [0.063]

Notes: The sample is restricted to DHS rounds 6 and 7. In Panel A, we use girls between 3 and 5 years old. In Panel B, we restrict the sample to boys between 3 and 5 years old. We calculate the index in regression 1 using all the other outcomes in this table. **We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2.** All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.33: Armed-Group Violence – Children Outcomes (Girls and Boys)

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Index	Can Identify At Least 10 Letters	Can Read 4 Simple Words	Knows All Numbers 1 to 10	Can Pick Up Small Objects	Is Healthy Enough To Play	Follows Simple Instructions	Does Things Without Help	Gets Along With Children	Acts Out (i.e., Kicks or Hits)	Is Not Easily Distracted
Panel A – Girls Age 3-5											
Distance × Rainfall, 1994	-1.131 [0.556]**	-0.042 [0.055]	0.001 [0.024]	0.013 [0.048]	-0.117 [0.038]***	0.002 [0.043]	0.012 [0.044]	-0.089 [0.060]	-0.013 [0.024]	-0.128 [0.056]**	-0.045 [0.056]
Dep. Mean	0.00	0.24	0.03	0.16	0.89	0.63	0.81	0.78	0.97	0.17	0.27
Dep. Std.Dev.	4.02	0.43	0.16	0.36	0.31	0.48	0.39	0.42	0.16	0.37	0.45
R ²	0.08	0.10	0.03	0.07	0.09	0.06	0.05	0.04	0.03	0.15	0.10
N	1170	1165	1168	1167	1159	1167	1167	1168	1169	1170	1164
IV: External Militia Violence											
Distance × Rainfall, 1994	1.437 [0.474]***	0.053 [0.062]	-0.001 [0.031]	-0.016 [0.063]	0.150 [0.044]***	-0.002 [0.054]	-0.015 [0.059]	0.112 [0.075]	0.016 [0.027]	0.162 [0.115]	0.057 [0.073]
Dep. Mean	-1.376	-0.159	-0.008	-0.015	-0.070	0.048	-0.034	-0.129	-0.032	0.005	-0.101
Dep. Std.Dev.	3.87	0.42	0.03	0.13	0.89	0.61	0.79	0.75	0.97	0.21	0.29
R ²	0.06	0.08	0.04	0.06	0.08	0.06	0.05	0.03	0.04	0.17	0.10
N	1237	1236	1236	1235	1233	1236	1235	1236	1236	1237	1228
Panel B – Boys Age 3-5											
Distance × Rainfall, 1994	1.534 [0.396]***	0.177 [0.048]***	0.009 [0.025]	0.017 [0.030]	0.078 [0.036]**	-0.054 [0.046]	0.038 [0.044]	0.144 [0.056]**	0.036 [0.025]	-0.006 [0.045]	0.115 [0.072]
Dep. Mean	0.00	0.24	0.03	0.13	0.89	0.61	0.79	0.75	0.97	0.21	0.29
Dep. Std.Dev.	3.87	0.42	0.16	0.34	0.31	0.49	0.41	0.43	0.16	0.41	0.45
R ²	0.06	0.08	0.04	0.06	0.08	0.06	0.05	0.03	0.04	0.17	0.10
N	1237	1236	1236	1235	1233	1236	1235	1236	1236	1237	1228
IV: External Militia Violence											
Distance × Rainfall, 1994	1.534 [0.396]***	0.177 [0.048]***	0.009 [0.025]	0.017 [0.030]	0.078 [0.036]**	-0.054 [0.046]	0.038 [0.044]	0.144 [0.056]**	0.036 [0.025]	-0.006 [0.045]	0.115 [0.072]

Notes: The sample is restricted to DHS rounds 6 and 7. In Panel A, we use girls between 3 and 5 years old. In Panel B, we restrict the sample to boys between 3 and 5 years old. We calculate the index in regression 1 using all the other outcomes in this table. We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2. All control variables, except "Number of Days with RPF presence," are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

A.5 Extensions to Section 7.5: Outlook

The male replacement is documented in Table A.34. The reduced-form effect of armed groups' transport costs on whether a household has a female head for 2005 is still negative and significant at the 99 percent confidence level (regression 1). The magnitude implied by the IV estimate, around 3 percent, is similar to the one using the EICV1 data from 2000/01 found in Rogall (2021). Thus, in 2005, households in high-violence villages are still more likely to have a female head. However, the effect disappears for later DHS rounds from 2010 and 2015 (regression 2). The same is true for the fraction of adult males (older than 18) in general. Here, the differences are small and insignificant for both 2005 and 2010/15 (regressions 3 and 4).

On a side note, to show that gender ratios have also returned to normal within the age group of potential politicians, we rerun regressions 3 and 4, but restrict the sample to adults between 18 and 85 years of age (regressions 5 to 6). Again, we find no significant differences.⁸ Thus, the positive effect of genocide violence on female politicians from above is not simply a mechanical result of an increase in supply but seems to follow from a longer-lasting change in norms.⁹

References

Rogall, T. (2021). Mobilizing the Masses for Genocide. *American Economic Review*, 111(1):41-72.

⁸The results are robust to restricting the politician age range from 25 to 85.

⁹We also experimented with WVS data but unfortunately there is not enough spatial variation to deliver meaningful results.

Table A.34: Age and Gender Composition – Over Time

Dependent Variable	Female Head		Fraction Adult Male		Fraction Male Ages 18 to 85	
	DHS from					
	2005	2010/15	2005	2010/15	2005	2010/15
Sample	(1)	(2)	(3)	(4)	(5)	(6)
Distance × Rainfall, 1994	−0.048 [0.015]***	0.018 [0.013]	−0.008 [0.009]	−0.005 [0.008]	−0.009 [0.009]	−0.006 [0.008]
Standard Controls	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes
Dep. Mean	0.33	0.32	0.24	0.26	0.24	0.26
Dep. Std.Dev.	0.47	0.47	0.24	0.25	0.24	0.25
R ²	0.01	0.01	0.03	0.03	0.03	0.03
N	9073	23145	9073	23145	9073	23145
IV: External Militia Violence	0.088 [0.045]**	−0.023 [0.019]	0.014 [0.017]	0.006 [0.010]	0.017 [0.018]	0.008 [0.010]

Notes: All fractions correspond to the household level, e.g., regressions 3 and 4 use the fraction of household members that are adult males (older than 18). Female Head is a dummy variable. The sample uses households from the three DHS rounds 5, 6 and 7. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, [Conley \(1999\)](#). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

A.6 Extensions to Section 8: Alternative Mechanisms

We also rule out a number of potential alternative mechanisms.

Post-Conflict Reconstruction First, the positive socioeconomic effects estimated above might simply result from post-conflict reconstruction or assistance to survivors by the central government or some NGO. To rule out this possibility, we show that communities with high levels of armed-group violence are not more likely to report infrastructure construction *six years after* the genocide, including schools, clinics, roads, bridges, mosques, churches, markets, or social housing.¹⁰ Regressions 1 to 8 in Table A.35 show that coefficients are insignificant throughout, except for markets, and in most cases, even negative. The point estimates for markets are significant at the 95 percent confidence level in both reduced form and second stage. However, the IV estimate is negative and furthermore, only about 1 percent of all communities report market construction. To account for multiple hypothesis testing, we also construct an index using the outcomes from regression 1 to 8. The IV point estimate is highly insignificant and even negative (regression 9).¹¹

The insignificant result for the last case, *Imidugudu* in regression 8, is particularly interesting since that government program was explicitly designed as an emergency housing project to help those adversely affected by the genocide. However, by the time of its implementation, it was redefined as a general development program benefiting all households. Moreover, the consensus seems to be that it was “*implemented hastily and in a rather disorderly manner.*” (Isaksson, 2013, p.5) and that there was no systematic selection of *Imidugudu* sites or dwellers (RSID, 1999). The inefficiency of the post-genocide government efforts are also showcased in a second large government fund to support genocide survivors, called FARG (Fonds d’Assistance aux Rescapes du Genocide). Although well-endowed and with ambitious objectives, it apparently did not live up to its expectations either. As the independent RNA (2009) puts it, “*However, years down the road, officials with the Fund have very little to show, as conditions of survivors have remained unchanged.*” One of the reasons might be the serious corruption that is plaguing the fund. Moreover, there are several thousand cases where FARG was falsely supporting non-victims.

Other large government programs like *Girinka* which distributes livestock to rural households (one cow per family) is concerned about poverty in general and designed to benefit all poor households, thus it is unlikely that their outreach is correlated with our instrument. Finally, more localized NGO-run programs are likely too small to bias our results. Moreover, NGOs (or government programs) are unlikely using transport costs

¹⁰At first glance, the genocide likely resulted in little physical capital destruction (given the strong focus on low-technology weapons). At second glance, however, this does not hold true. Especially armed groups were equipped with machine guns or grenades and destroyed schools, hospitals, and churches – places where Tutsi would gather for presumed safety. Furthermore, looting was common, even among lower ranked perpetrators who “only” killed with a machete. Thus, Rwanda required extensive rebuilding after 1994.

¹¹The data for this test is taken from the EICV1 survey, which includes a community survey that asks about public goods provisions for the time period between the genocide and the survey.

from 1994 to determine their beneficiaries but rather localized genocide violence data, e.g., from the Gacaca courts. Thus, if post-conflict reconstruction were the main mechanism, we should see strong positive effects in the OLS regressions. However, recall from Table A.2 that this is not the case. The insignificant OLS results also speak against the Gacaca courts themselves having direct positive effects on female empowerment, for instance, via reconciliation, trust building, or women's roles as judges and witnesses.

Note that these null effects for public goods provision do not contradict the positive results on public goods from Table 5. The timing is important. While the former measures investments some six years after the genocide by the national government and local NGOs and is, therefore, a measure of post-conflict reconstruction, the latter measures local public goods provision around 20 years after the genocide and is thus more likely the result of good (female) governance.

Local RPF Support Another alternative mechanism could be that high-violence villages are more likely to support the ruling Tutsi party (RPF), thus receiving favorable treatment by the national government. Using cell-level data from the 2013 parliamentary elections, regression 10 in Table A.35 rules out this possibility.¹² Both reduced-form and IV estimates are small and highly insignificant. Note that these insignificant results also somewhat rule out that female politicians who rise up the ranks directly support their home constituencies, since national politics is strictly dominated by the RPF party.

Selective Killings and Incarceration A third alternative mechanism is that for some reason in high-violence villages, all the productive, more educated women and men survived – explaining the positive effects. Again, using the EICV1 survey data from 2000/01, Table A.36 shows that there is no correlation between the instrument and years of schooling, a good proxy for adult education. This is true both for the whole sample and only women.¹³ Furthermore, the results do not seem to differ by whether a woman/individual experienced the genocide in their village or moved in afterwards.

Related, since army and militiamen incited civilians to join in the killings (Rogall, 2021), one may worry that those individuals most prone to violence likely participated in the genocide. These men were then potentially incarcerated after the genocide, thus leaving the marriage market – leading to female empowerment. However, if this were the main mechanism we should find similar effects in RTL M villages, especially so, since civilian perpetrators in these villages likely participated out of their own initiative and were thus even more prone to violence (the repercussions when refusing to participate when the militia gave the order, were likely worse).

¹²The data is provided by the National Electoral Commission and available at the cell level. Importantly, there is significant local variation, with RPF votes shares falling as low as 8 percent in certain areas.

¹³Note that we include only adults above 24 years who have already finished their education and where the effects of conflict are thus purely selection.

Selective Migration A final mechanism could be selective migration. The positive effects might simply result from more successful and educated women migrating into high violence villages after the genocide. A number of findings suggest that this is unlikely. First, note from the preceding paragraph that highly educated women were not more likely to move into high-violence villages; if anything, regression 5 in Table A.36 suggests that lower educated women moved into these places.

This is not surprising: using again EICV1 survey data from 2000/01 – we next show that high violence villages did worse in terms of economic prospects than other villages some six years after the genocide; thus, they were unlikely traveling destinations. Results in Table A.37 suggest that, if anything, high violence villages experience a drop in agricultural income and consequently, consumption (total consumption and food consumption). Besides, using detailed migration data, we further show that the effects of armed-group violence on income and consumption are unaffected by whether the sample is restricted to those households who experienced the genocide in their village or using the full sample, including migrants. It is thus unlikely that migrants significantly differed from locals.

Finally, we directly rule out that educated women are more (or less) likely to migrate. The results in Table A.38 suggest that the instrument is unrelated to whether an individual with at least primary education (alternatively at least one year of schooling) migrated after the genocide. The results are identical for educated men.

Economic Incentives Another alternative explanation could be that the initial gender imbalances changed women's returns to labor and education, thus increasing incentives to invest more in human capital and take on high-skilled jobs. This could ultimately drive the positive effects. While we cannot fully rule out this channel (in fact, it is likely to play a role), it is unlikely to play the sole or major role for two reasons: 1) the 15 to 20 year delay of the positive effects is perfectly consistent with slowly changing norms but somewhat inconsistent with pure economic channels, which should materialize much earlier; and 2) the majority of Rwandan women are still highly uneducated, with most of them working on their subsistence farms. The positive effects still remain when we restrict the sample to these unskilled women where the economic incentives outlined should not have mattered.

Related, the male scarcity might have forced women to invest more in education and obtain a high-paying job in order to attract a future husband. However, first, anecdotal evidence renders this channel highly implausible. In particular, education, a high-skilled occupation and financial independence was nothing that men considered valuable in early post-genocide Rwanda. Rather, Rwandan women were traditionally seen as childbearers, focused on domestic tasks, and shut off from education. Any deviant behavior was sanctioned (Schindler, 2010). Second, again, the 15 to 20 year delay of the positive effects is somewhat inconsistent with this channel. Thus, it seems unlikely that women changed their behavior to attract men, but rather men ultimately changed their

views about women.¹⁴

On a side note, recall from the last section that there did not seem to be selective killings or migration of men (based on human capital). Thus it also seems unlikely that more progressive men, sympathetic to women's rights, survived the genocide and are driving the positive results (for instance, by being better husbands).

Marriage Market So far, we have argued that women did not change their behavior, i.e., invest in human capital, to attract a husband. Alternatively, men, faced with an oversupply of potential partners, could have reduced their marriage efforts. More general, a common understanding in the economics of marriage literature is that a particular sex shortage should force the other sex to marry lower-quality partners (Becker, 1981).¹⁵ Put differently, the male shortage that lasted in high-violence villages for about 20 years should have forced women to marry less attractive men.¹⁶ This goes against our positive findings from above; for instance, women do not end up with more violent partners. To reconcile, we look closer at the subset of women between the ages of 12 and 18, who would be of marrying age by the end of the genocide. For these women, the shortage of men should have been particularly pressing. Table A.39 reports the point estimates on our main outcomes for DHS rounds 5 and 6/7 separately. Importantly, the results mirror the ones for the full sample. Thus, we do not observe strong effects for 2005 (shown in Panel A) but positive effects on female outcomes throughout for 2010 and 2015 (Panel B, regressions 2 to 12).¹⁷

Regression 1 delivers a possible explanation: women are delaying their marriage. In 2005, women are significantly less likely to be married (at least in the reduced form). At face value, the point estimate of 0.13 suggests that a 10 percent increase in genocide violence increases the likelihood of being unmarried by about 2 percent. By 2010/15, this difference in marriage rates disappears. Finally, since large numbers of women in that generation do not get married, likely it becomes socially acceptable. For instance, in 2005, 25 percent of the women between 12 and 18 during the genocide were not married. This number goes down to 5 percent for those older than 18 during the genocide.

These results indirectly show that women of the most pressing marrying age do not fare worse. We can also present some more direct evidence that they do not end up with lower quality men. Panel A in Table A.40 suggests that the husbands of women (again from age 12 to 18 at the time of the genocide) in 2010/2015 are neither significantly older

¹⁴On a side note, the positive effects on education (usually determined pre-marriage) are the same for married and unmarried women. This also speaks against the results on domestic violence being driven, for instance, by selective sorting into marriage.

¹⁵Note that the Becker model and its extensions deliver various predictions and channels as to how women's outcomes are affected, such as changes in outside options and bargaining power, marriage investments, etc., we follow here a more reduced-form approach. Also while some of our findings are consistent with the Becker model, for instance, increases in age at first birth or cohabitation, generally they are not.

¹⁶Attractiveness can hereby refer to education, occupation, or engagement in domestic violence, etc.

¹⁷Note that the implicit assumption of localized marriage markets seems valid here. The EICV1 survey data suggests that only 7 percent of all married individuals moved in from another village.

nor do they have significantly different years of schooling (regressions 1 to 4). If at all husbands of women in high-violence villages are more often employed in higher-skilled occupations (compared to husbands in low-violence villages), although again not more often than their wives (regressions 5 and 6).

The findings are similar when we also include women that were younger than 12 years during the genocide. These women do not marry lower quality husbands (results in Panel B). We find no significant age differences (regressions 1 and 2). We do find that wives seem to have slightly more education than their husbands (regression 4). However, this seems reflective of the overall positive effects for women rather than lower husband quality, since husbands in high-violence villages are still better educated than their counterparts in low-violence villages (regression 3). Finally, husbands are more likely to work in a skilled occupation (regression 5), however not more likely than their spouses (regression 6).

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Table A.35: Armed-Group Violence – Public Goods (constructed between 1994 and 2001) and 2013 Elections

Dependent Variable	Schools	Clinics	Bridges	Roads	Mosques	Churches	Markets	Imidugudu	Index using 1-8	RPF Vote Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance × Rainfall, 1994	-0.054 [0.080]	-0.004 [0.013]	0.028 [0.063]	0.031 [0.052]	-0.028 [0.047]	0.056 [0.059]	0.027 [0.012]**	-0.039 [0.062]	0.199 [0.752]	-0.005 [0.009]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.15	0.01	0.08	0.12	0.03	0.39	0.01	0.31	0.00	0.75
Dep. Std.Dev.	0.36	0.12	0.27	0.33	0.17	0.49	0.09	0.46	0.36	0.11
R ²	0.10	0.05	0.16	0.17	0.06	0.18	0.07	0.27	0.19	0.04
N	401	401	401	401	401	401	401	401	401	1450
IV: External Militia Violence	0.071 [0.106]	0.005 [0.018]	-0.037 [0.081]	-0.040 [0.067]	0.037 [0.061]	-0.073 [0.079]	-0.035 [0.017]**	0.051 [0.082]	-0.261 [0.978]	0.016 [0.028]

Notes: The dependent variables in regressions 1 to 8 are dummy variables indicating whether the corresponding public good (e.g., schools in regression 1) was built in the village after the genocide (data taken from a EICV1 community survey from 2000/01). The dependent variable in regression 9 is an index variable created using the outcomes from regressions 1 to 8. The dependent variable in regression 10 is the RPF vote share from the 2013 national elections, measured at the cell level and provided by the National Electoral Commission. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.36: Armed-Group Violence – Human Capital, Adults

Dependent Variable	Years of Schooling					
	Full Sample		In Village During Genocide		Moved in After Genocide	
	Females (1)	All (2)	Females (3)	All (4)	Females (5)	All (6)
Distance × Rainfall, 1994	0.181 [0.151]	0.154 [0.157]	-0.038 [0.130]	0.222 [0.118]*	0.462 [0.611]	-0.254 [0.500]
Standard Controls	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes
Additional Rainfall Controls	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes
Dep. Mean	2.39	2.86	2.20	2.58	3.03	3.74
Dep. Std.Dev.	3.11	3.28	2.92	3.01	3.59	3.90
R ²	0.36	0.29	0.33	0.25	0.45	0.37
N	4253	7520	3267	5729	986	1791
IV: External Militia Violence	-0.265 [0.250]	-0.224 [0.253]	0.064 [0.216]	-0.344 [0.223]	-0.400 [0.548]	0.250 [0.474]

Notes: The regressions use adults, thus older than 24 years. The data is taken from the EICVI Survey conducted in 2000/2001. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Additional Rainfall Controls are average rainfall (from 1995 to survey collection year) along the buffer between village and main road and its interaction with distance to the main road. Individual Controls are age, and father’s and mother’s years of schooling. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.37: Armed-Group Violence – Consumption/Income

Dependent Variable Sample	Consumption					Agricultural Income			
	Full Sample		In Village During Genocide		Moved in After Genocide	Full Sample		In Village During Genocide	Moved in After Genocide
	Total	Food	Total	Food	Total	Total	Total	Total	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
Distance × Rainfall, 1994	0.138 [0.119]	0.125 [0.082]	0.192 [0.098]*	0.124 [0.080]	0.114 [0.214]	0.195 [0.103]*	0.265 [0.142]*	0.240 [0.127]*	0.419 [0.332]
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Rainfall Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	610.48	123.67	507.32	114.66	965.16	154.67	1373.86	1264.79	1796.35
Dep. Std.Dev.	1943.19	117.61	1413.69	100.38	3119.61	159.76	2338.65	2207.41	2750.03
R ²	0.12	0.07	0.10	0.05	0.23	0.16	0.12	0.11	0.20
N	5255	5255	4071	4071	1184	1184	4903	3897	1006
IV: External Militia Violence	-0.212 [0.163]	-0.191 [0.116]	-0.291 [0.124]**	-0.188 [0.114]*	-0.148 [0.271]	-0.253 [0.136]*	-0.266 [0.143]*	-0.244 [0.127]*	-0.452 [0.434]

Notes: All dependent variables are in logged per capita monetary values. Per capita refers to the consumption/income of the household, divided by the number of persons living in the household. Income is defined as output minus running costs. The data is taken from the EICV1 Survey conducted in 2000/2001. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except “Number of Days with RPF presence,” are in logs. Interactions are first logged and then interacted. Additional Rainfall Controls are average rainfall (from 1995 to survey collection year) along the buffer between village and main road and its interaction with distance to the main road. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.38: Armed-Group Violence – Selective Migration

Sample	All Women		Women With Some Educ.		Women With At Least Primary Educ.		All Men		Men With Some Educ.		Men With At Least Primary Educ.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)				
Dependent Variable												
Distance × Rainfall, 1994	0.002 [0.006]	0.004 [0.004]	0.002 [0.010]	0.003 [0.012]	-0.004 [0.005]	-0.000 [0.005]	-0.003 [0.007]	0.013 [0.014]				
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes				
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes				
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes				
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes				
R ²	0.05	0.05	0.06	0.07	0.05	0.04	0.05	0.07				
N	7439	7439	4765	2194	5942	5942	4490	2004				

Notes: All dependent variables are dummy variables. For instance, in regression 1 the dependent variable takes on the value of 1 if a woman is a migrant and has at least one year of schooling. In regression 3 the dependent variable takes on the value of 1 if a woman is a migrant, however in this case we restrict the sample to women with at least one year of schooling. The data is taken from the EICV1 Survey conducted in 2000/2001. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All control variables, except Number of Days with RPF presence, are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Comley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.39: Armed-Group Violence – Marriage Market (Women from 12 to 18)

Dependent Variable	Domestic Violence										
	Ever Married	Domestic Violence Attitudes	Education, Wealth, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility	All	Severe	Sexual	Less Severe
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A – DHS 5											
Distance × Rainfall, 1994	0.068 [0.040]*	-0.034 [0.320]	-0.147 [0.284]	0.100 [0.067]	0.147 [0.130]	-0.041 [0.119]	-0.115 [0.115]	-0.960 [0.503]*	-0.476 [0.205]**	-0.019 [0.139]	-0.483 [0.370]
R ²	0.08	0.09	0.21	0.27	0.07	0.04	0.09	0.05	0.06	0.09	0.05
N	2035	2032	2035	2034	1781	2035	2035	648	648	648	648
IV: External Militia Violence											
	-0.130 [0.085]	0.063 [0.590]	0.281 [0.552]	-0.192 [0.157]	-0.258 [0.303]	0.079 [0.222]	0.220 [0.203]	1.731 [1.147]	0.859 [0.540]	0.034 [0.256]	0.871 [0.734]
Panel B – DHS 6/7											
Distance × Rainfall, 1994	0.012 [0.014]	0.394 [0.215]*	-0.473 [0.136]**	-0.347 [0.145]**	-0.281 [0.068]**	-0.063 [0.074]	-0.334 [0.083]**	2.290 [0.879]**	0.894 [0.447]**	0.455 [0.233]*	1.397 [0.445]**
R ²	0.02	0.06	0.12	0.07	0.03	0.02	0.04	0.04	0.04	0.04	0.03
N	4147	4145	4147	4147	4092	4147	4147	1306	1306	1306	1306
IV: External Militia Violence											
	-0.014 [0.015]	-0.480 [0.315]	0.576 [0.146]**	0.422 [0.158]**	0.347 [0.100]**	0.077 [0.090]	0.407 [0.135]**	-3.385 [1.253]**	-1.321 [0.616]**	-0.673 [0.324]**	-2.064 [0.669]**

Notes: The sample is restricted to women between the ages of 12 and 18 years at the time of the genocide. Panel A uses data from DHS round 5. Panel B uses rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2. All control variables, except "Number of Days with RPF presence," are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.40: Armed-Group Violence – Partner Match

Dependent Variable	Husband's Age	Absolute Age Difference	Husband's Schooling	Wife Has More Schooling	Husband in Skilled Occupation	Wife in Better Occupation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Women between 12 and 18						
Distance × Rainfall, 1994	-0.023 [0.546]	-0.190 [0.436]	0.023 [0.291]	-0.001 [0.017]	-0.072 [0.034]**	0.026 [0.013]**
Dep. Mean	37.10	5.34	4.90	0.35	0.27	0.03
Dep. Std.Dev.	7.49	6.16	3.84	0.48	0.45	0.18
R ²	0.02	0.02	0.09	0.01	0.17	0.01
N	3269	3269	3717	3715	3747	3745
IV: External Militia Violence						
	0.029 [0.700]	0.243 [0.585]	-0.028 [0.368]	0.001 [0.022]	0.090 [0.036]**	-0.033 [0.022]
Panel B – All Women under 18						
Distance × Rainfall, 1994	-0.304 [0.303]	-0.157 [0.181]	-0.442 [0.270]	-0.021 [0.006]***	-0.101 [0.034]***	0.003 [0.009]
Dep. Mean	32.90	5.04	4.72	0.35	0.30	0.04
Dep. Std.Dev.	7.52	5.58	3.63	0.48	0.46	0.18
R ²	0.01	0.01	0.09	0.01	0.17	0.01
N	8483	8483	9487	9482	9571	9566
IV: External Militia Violence						
	0.367 [0.362]	0.190 [0.208]	0.547 [0.277]**	0.026 [0.009]***	0.125 [0.032]***	-0.004 [0.011]

Notes: The sample is restricted to DHS rounds 6 and 7. Panel A uses women between 12 and 18 years during the genocide. Panel B uses women younger than 18 years during the genocide (this includes unborn). Age Difference is defined as the absolute difference between the wife's age and the husband's age. Wife Has More Schooling/Wife in Better Occupation are dummy variables. A skilled occupation is work in clerical jobs, sales, professional/technical/managerial, services and skilled manual labor, the control group are unemployed, agricultural subsistence farming, agricultural employees, household and domestic services, and unskilled manual labor. **We control for Standard Controls, Growing Season Controls, Additional Controls and Province Effects in all specifications, defined in Table 2.** All control variables, except "Number of Days with RPF presence," are in logs. Interactions are first logged and then interacted. Standard errors correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

A.7 Extensions to Section 9: Placebo – RTLM Hate Radio

A.7.1 Media and RTLM – Institutional Background

Before the genocide, RTLM and Radio Rwanda were the only radio stations in Rwanda. RTLM was founded in July 1993, and broadcasted using two transmitters: one in the capital Kigali, and the other one on Mount Muhe, one of Rwanda’s highest mountains. The government-owned Radio Rwanda had been broadcasting some propaganda before the genocide, but RTLM’s broadcasts were by far the most extreme (Yanagizawa-Drott, 2014). Members of the Hutu Power had founded RTLM with President Habyarimana backing it (Des Forges, 2007). With the start of the genocide, RTLM became the voice of the new Hutu interim government. The broadcasts lasted throughout the genocide and only ended when RPF rebels took power in mid-July 1994 (International Criminal Tribunal for Rwanda ICTR, 2003).

RTLM openly called for the annihilation of the Tutsi and claimed that preemptive strikes were a necessary response for “self-defense.” In order to degrade and dehumanize the Tutsi (a common tactic in genocides) they were often referred to as ‘cockroaches’ during the broadcasts (Frohardt, 2007; International Criminal Tribunal for Rwanda ICTR, 2003).

Alternative print media also existed. There were 30 to 60 independent newspapers at the time of the genocide, including political opposition publications (Higiro, 2007; Alexis, 2003). However, these newspapers’ circulation and readership were limited, especially in rural areas, because of relatively low literacy rates; for most people, radio was the only source of information (Des Forges, 2007). Besides, RTLM often mixed its anti-Tutsi propaganda with contemporary music selections and was thus very popular among the younger generations especially (who later made up the majority of the local militia members). Consistently, Yanagizawa-Drott (2014) finds that RTLM had a significant effect on local Hutu participation in violence against the Tutsi.

A.7.2 Identification Strategy

The RTLM identification strategy builds on two assumptions. First, villages with better RTLM coverage experienced higher genocide violence. This is the result of Yanagizawa-Drott (2014), who uses local variation in village topography to establish causality. Below, we reproduce these results. Second, RTLM coverage does not have a direct effect on

any of the socioeconomic (gender) outcomes, but rather only works via local genocide violence. Even though this assumption cannot be directly tested, we can provide some indirect evidence.

There are two main concerns. The exclusion restriction would be violated if some other radio station, whose broadcasts possibly affect economic well-being or female empowerment, were to use the RTLTM transmitters. This is not the case, however – both RTLTM transmitters were destroyed at the end of the genocide, and the broadcasts stopped.

The exclusion restriction would also be violated if the RTLTM broadcasts in 1994 provided information about female empowerment issues such as domestic violence or sexual education. This concern is also likely to be unwarranted. First, anecdotal evidence suggests that RTLTM’s broadcasts mainly involved stirring up hatred and playing popular music (Kimani, 2007). Second, to directly assess content relevant for socioeconomic (gender) outcomes, we obtained and analyzed a 10 percent sample of RTLTM’s broadcasts and did not find evidence that RTLTM’s broadcasts would directly affect economic or female performance.¹⁸

Specification To show that the broadcasts caused more violence, and reproduce the main result in Yanagizawa-Drott (2014), we estimate the following (first-stage) equation

$$(A.1) \quad \log(h_{jc}) = \alpha + \beta rtm_{jc} + \mathbf{X}_{jc}\pi + \gamma_c + \epsilon_{jc},$$

where h_{jc} is the number of organized perpetrators in village j in commune c , and rtm_{jc} the share of the village with RTLTM coverage. \mathbf{X}_{jc} is a vector of propagation controls,¹⁹ as well as pre-genocide village characteristics. Furthermore, γ_c is a commune fixed effect and ϵ_{jc} the error term.

¹⁸We retrieved numerous radio tapes online from Jake Freyer’s homepage, who downloaded them from the International Criminal Tribunal for Rwanda (ICTR). The ICTR received the tapes from various sources; thus, we reasonably believe this to be a random sample. The ICTR had about 20 percent of these tapes translated from Kinyarwanda into English, and another 20 percent were originally in French. As the ICTR was mainly looking for evidence for genocidal behavior we expect, if at all, the untranslated Kinyarwanda broadcasts to contain economic or social advice to the listeners. We search for keywords such as school, income, domestic violence, education, and etc.

¹⁹These include distance to the transmitter and village altitude as well as the variance in altitude, all of which are likely confounders. For more details on the identification strategy as well as additional robustness and identification tests, see Yanagizawa-Drott (2014).

We then run the following reduced-form regressions

$$(A.2) \quad post_y_{ijc} = \alpha' + \beta' rtm_{jc} + \mathbf{X}_{jc} \pi' + \gamma_c + \epsilon_{ijc},$$

where $post_y_{ijc}$ is the post-genocide outcome of household i (or women i) in village j in commune c and the other independent variables are the same as before. Standard errors are clustered at the district level. To gauge the magnitudes we also report instrumental variable estimates.²⁰

A.7.3 RTLM Violence – First Stage and Mass Graves

To show that the null results from above are not simply the result of a missing first stage, we report the results in Table A.41 below. The first-stage relationship between radio coverage and genocide violence is strongly positive at the 95 percent confidence level (regression 1), and this relationship holds again when restricting the sample to those villages surveyed in DHS (regressions 2 and 3). Regarding magnitude, the point estimate of 1.028 log points (standard error 0.626) in our preferred specification, suggests that a village with full radio coverage has about 1.6 times more perpetrators than a village with no reception. Put differently, a one-standard-deviation increase in radio coverage increases the violence by around 30 percent.

To differentiate local RTLM-induced violence from large-scale armed-group violence above, in regression 4 we show that RTLM coverage is unrelated to whether a village has a mass grave site (if anything villages are less likely to have a mass grave).

A.7.4 Targets of RTLM Violence

Contrary to external violence above, Rogall (2021) argues that local RTLM-induced violence, mostly using low-technology weapons such as machetes and clubs, targeted especially women, children, and the elderly. As shown by Verwimp (2006), women, young children, and the elderly were more likely to die from a machete or club – weapons used predominantly by local perpetrators.

Again, Rogall (2021) provides the first piece of evidence for this: when measured six

²⁰Note that we scale the reduced-form effects using the total number of militiamen (both local and external). The IV estimate thus gives the effect of those militiamen affected by RTLM coverage – likely local militiamen.

years after the genocide, local genocide violence increased the working-age population share (age 13 to 49). His IV estimates imply that a 10 percent increase in genocide violence increases the working-age population share by 2.2 to 2.3 percentage points. This suggests that the most vulnerable, young children and the elderly were more likely to suffer violence. He finds that it is especially the fraction of working-age males that increased; thus, women were killed. Although insignificant, the point estimates further suggest that households with high levels of local violence are less likely to have a female household head, a female widow in the household and a larger fraction of males in general.

Besides looking at the age and gender distribution of the survivors, we can also directly analyze deaths again. The DHS data contains information on individuals' siblings, for example, their gender and if they died – their age at death and year of death. This information allows us to back out the number of women, older men, and children killed during the genocide. Table A.42 provides the results. All outcomes above are normalized by the total number of killed siblings. The point estimates confirm that RTLTM-induced local violence seemed to have targeted especially the vulnerable. A 10 percent increase in genocide violence leads to a 2 percent increase in the fraction of killed woman, a 3 percent increase in the fraction of killed vulnerable (i.e., woman and older men as well as boys), a 7 percent increase in the fraction of killed children and a 9 percent increase in the fraction of killed girls. Note that random measurement error in the dependent variable (e.g., the DHS data does not specify whether a sibling was killed during the genocide or happened to die that year for other reasons) is likely going to increase standard errors. Nonetheless, most point estimates are significant (we report standard errors clustered both at the district and commune level).

A.7.5 RTLTM Violence – Political Office and Public Goods

Recall from above that local violence targeted especially women, children, and the elderly, leading to a working-age male surplus. Accordingly, women do not take over key positions in the family and government. When it comes to local politicians, Table A.43 shows that if anything, fewer women are elected into a public office although the point estimates are insignificant (regression 1). Furthermore, there are no significant effects on being a National Women's Council member (regression 2), or elected politicians' education levels (regressions 4 and 5). Consistently, the results in Table A.44 suggest that if anything public goods provision is lower.

A.7.6 Alternative Mechanisms

The main effects in local-violence villages could, alternatively, differ from those in external violence villages because in the former, the perpetrators may have stayed in the village. This might force women to regularly see or even meet their abusers with potentially negative effects on their (economic) well-being and empowerment. In the latter, the perpetrators generally left the village after the genocide. To rule out this possibility, we show that the negative/non-positive gender effects in local-violence villages remain even when we drop genocide victims. Since the DHS data does not allow us to identify a respondent's ethnicity, we proxy for being a genocide victim by whether a woman reports having any siblings that died during the genocide. Additionally, we drop potential rape victims, i.e., women who gave birth around nine months after the genocide. Table A.45 below suggests that exposure to one's abuser is unlikely driving the results since the negative effects remain even for non-victims.

Furthermore, note that Table A.46 points to a reversal of fortune: villages with high levels of local violence initially did better economically (measured six years after the genocide). They experienced higher consumption and higher income.²¹ Thus, if continued exposure to local genocide perpetrators was driving the negative results later on, we should also observe negative effects right after the genocide. Note further that these positive results are robust to using all households, excluding migrants or using only migrants. Again, selective migration is unlikely to matter.

Finally, another alternative mechanism could be that high-violence villages are more likely to oppose the ruling Tutsi party (RPF), thus receiving less favorable treatment by the national government. Using cell-level data from the 2013 parliamentary elections, regression 10 in Table A.46 rules out this possibility. The effect of RTLTM radio coverage on the RPF vote share is small and insignificant.

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²¹These results are consistent with the Malthusian view that mass murder can increase living standards by redistributing productive assets, e.g., livestock, from the deceased to the remaining population (looting was common). In particular so, since RTLTM-induced, local violence was committed with low-technology weapons such as machetes or clubs, and thus likely resulted in little physical capital destruction.

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A.7.7 Tables

Table A.41: First Stage – RTL

Dependent Variable	# Militiamen, log			Mass Graves
	(1)	(2)	(3)	(4)
RTL	0.545	1.121	1.028	-0.120
Radio Coverage	(0.229)**	(0.592)*	(0.626)	(0.083)
Propagation Controls	yes	yes	yes	yes
Additional Radio Controls	yes	no	yes	yes
Commune Effects	no	yes	yes	yes
R ²	0.55	0.64	0.66	0.25
N	1065	465	465	464

Notes: The dependent variable Mass Graves is a dummy taking on the value of 1 if a mass grave site is present in the village. The sample in regressions 2 to 4 is restricted to the villages from DHS rounds 6 and 7. In regression 1 we use the full sample provided by Yanagizawa-Drott (2014). Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.42: Local Violence – Targeted Killings

Dependent Variable	Fraction	Fraction	Fraction	Fraction
	Females	Vulnerable	Children	Female Children
	(1)	(2)	(3)	(4)
RTLTM Radio Coverage	0.052 (0.060) (0.057)	0.115 (0.068) (0.055)**	0.131 (0.067)* (0.059)**	0.074 (0.039)* (0.035)**
Propagation Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes
Dep. Mean	0.39	0.50	0.26	0.12
Dep. Std.Dev.	0.43	0.45	0.42	0.30
R ²	0.04	0.04	0.06	0.05
N	5060	5060	5060	5060
IV: Local Militia Violence	0.072 (0.092) (0.099)	0.160 (0.124) (0.122)	0.183 (0.119) (0.103)*	0.104 (0.064) (0.062)*

Notes: The data is taken from all three rounds of DHS data. All dependent variables are fractions of an individual's sibling deaths. For example, Fraction Vulnerable is the number of vulnerable male (the elderly and children) and all female sibling deaths normalized by the total number of sibling deaths. Deaths are restricted to siblings that died during the genocide. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level and commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.43: Local Violence – Women’s Political Engagement

Sample Dependent Variable	Women & Men			Women	Men
	Fraction of Elected Women	Fraction of National Council Women	Fraction of National Council Disabled	Fraction With Primary Education	
	(1)	(2)	(3)	(4)	(5)
RTLTM Radio Coverage	–0.021 (0.019)	0.014 (0.024)	–0.005 (0.014)	0.004 (0.044)	0.015 (0.047)
Propagation Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
Dep. Mean	0.54	0.18	0.18	0.44	0.44
Dep. Std.Dev.	0.08	0.07	0.06	0.23	0.20
R ²	0.21	0.17	0.17	0.30	0.29
N	1347	1347	1347	1345	1345
IV: Local Militia Violence	–0.051 (0.052)	0.034 (0.056)	–0.012 (0.033)	0.011 (0.101)	0.035 (0.110)

Notes: Regressions are run at the cell level. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.44: Local Violence – Public Goods Provision

Dependent Variable	Road	# Health	Social	Access To	# Primary	# Secondary
	Length, log	Facilities	Housing, log	Electricity	Schools	Schools
	(1)	(2)	(3)	(4)	(5)	(6)
RTLMM Radio Coverage	0.143 (0.136)	0.069 (0.083)	−0.041 (0.168)	−0.193 (0.112)*	−0.299 (0.168)*	−0.092 (0.178)
Propagation Controls	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes
Dep. Mean	17.64	0.20	1.97	0.42	1.15	0.20
Dep. Std.Dev.	10.32	0.44	2.80	0.49	0.99	0.60
R ²	0.51	0.09	0.48	0.35	0.24	0.13
N	1347	1347	1347	1347	1347	1347
IV: Local Militia Violence	0.346 (0.349)	0.166 (0.223)	−0.100 (0.385)	−0.467 (0.371)	−0.722 (0.374)*	−0.221 (0.387)

Notes: Regressions are run at the cell level. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.45: Local Violence – Excluding Potential Victims

Dependent Variable	Domestic Violence											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	
	Domestic Violence Attitudes	Violence Against Kids Attitudes	Education, Wealth, Health	Financial Autonomy	Physical Autonomy	Decision Power	Sexual Knowledge, Fertility	All	Severe	Sexual	Less Severe	
Panel A – Excl. Victims												
RTLM Radio Coverage	1.507 (0.804)*	0.482 (0.404)	-0.284 (0.673)	0.014 (0.219)	-0.423 (0.272)	0.031 (0.149)	-0.338 (0.174)*	-0.799 (-1.104)	-0.111 (0.417)	-0.037 (0.352)	-0.688 (0.768)	
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
R ²	0.09	0.07	0.14	0.09	0.05	0.02	0.04	0.05	0.04	0.05	0.06	
N	14886	14896	14899	14899	13962	14897	14899	2822	2822	2822	2822	
Panel B – Excl. Rape Victims												
RTLM Radio Coverage	1.478 (0.785)*	0.429 (0.401)	-0.287 (0.666)	0.014 (0.225)	-0.424 (0.278)	0.018 (0.141)	-0.338 (0.159)**	-0.752 (-1.128)	-0.081 (0.430)	0.010 (0.362)	-0.671 (0.778)	
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
R ²	0.09	0.07	0.14	0.09	0.05	0.02	0.04	0.05	0.04	0.04	0.06	
N	14584	14594	14597	14597	13671	14595	14597	2746	2746	2746	2746	

Notes: The sample is restricted to women from DHS rounds 6 and 7. In Panel A, we restrict the sample to women who do not report any sibling deaths during the time of the genocide. In Panel B, we further drop women who gave birth nine months after the genocide (potential rape victims). We calculate various indexes, the composition of each index is given in the paper. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.46: Local Violence – Consumption/Income

Sample	Full Sample		In Village During Genocide		Moved in After Genocide		Full Sample		In Village During Genocide		Moved in After Genocide		Full Sample	
	Total	Food	Total	Food	Total	Food	Total	Food	Total	Food	Total	Food	Total	Food
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
RTLM Radio Coverage	0.736 (0.237)***	0.248 (0.116)**	0.704 (0.244)**	0.276 (0.125)**	0.954 (1.150)**	0.630 (0.889)	0.483 (0.204)**	0.492 (0.283)*	0.745 (1.299)	-0.050 (0.042)				
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R ²	0.22	0.19	0.18	0.16	0.46	0.36	0.24	0.24	0.41	0.18				
N	4039	4039	3257	3257	782	782	3783	3122	661	1109				

Notes: All dependent variables in regressions 1 to 9 are in logged per capita monetary values. Per capita refers to the consumption/income of the household, divided by the number of persons living in the household. Income is defined as output minus running costs. The data is taken from the EICV1 Survey conducted in 2000/2001. The dependent variable in regression 10 is the RPF vote share from the 2013 national elections, measured at the cell level and provided by the National Electoral Commission. Propagation Controls and Additional Controls are defined in Table A.16. Standard errors in parentheses are clustered at district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.