# Yes They Can: Empowering Women*

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**Abstract**

We study how women taking on 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. Overall, in women-led villages, women are healthier, better educated, wealthier, less likely to accept and experience domestic violence, and enjoy more sexual and financial autonomy. These villages see more public goods provision and less political violence. Younger women are carrying these changes and gender norms shifted. In men-led villages, we find negative or no effects.

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*Keywords:* Gender Norms, Female Leadership, Household Welfare, Health, Domestic Violence, Political Participation, Public Goods Provision

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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). Finally, female leadership is found to more efficiently avoid conflict between (Caprioli, 2000) and within states (Demeritt et al., 2014). However, all of these studies only provide piecemeal evidence of the positive effects of female leadership.

In this paper, we provide the first empirical evidence that women taking on political and domestic power can lead to persistent, overall female empowerment. We are also able to trace out the timing and pinpoint the channels. 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 which 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, 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 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. Second, 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 using post-genocide household composition data, e.g., female household head, widows, or the fraction of adult men.

Third, using election outcomes from 2011, we show that armed-group violence leads to an increase in the number of female politicians elected into

[1] Note 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.

[2] We summarize his identification strategy, which requires a certain set of controls, below.

the local government. Moreover, elected women are also better educated. In terms of magnitudes, a 10 percent increase in genocide violence leads to a 1 to 2.5 percent increase in political participation.

With women domestically and politically in power, we next estimate the reduced-form effects of external genocide violence on various other welfare outcomes. The results, paired with anecdotal evidence, strongly suggest that women leaders are driving these positive welfare effects. We rule out additional explanations below and also present a strong placebo check using local violence induced by RTLM coverage.

First, we thus estimate the effects on various socio-economic outcomes and gender norms. 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 magnitudes, a 10 percent increase in genocide violence is associated with a 1 to 5 percent increase in these women's outcomes.[3]

Furthermore, women-led villages see an increase in local public goods provision, for instance, in health care, road building, or electricity access (all measured around 2016). A 10 percent increase in violence leads to a 2 to 10 percent increase in public goods.

Finally, using ACLED political violence data, which for instance records police violence against civilians, we show that women-led villages are more peaceful. A 10 percent increase in genocide violence leads to a 12.5 to 18 percent decrease in political violence. Importantly, the ACLED data is available

[3] Scaling 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.

from 1999 until today and allows us to trace out the timing of the effects. More specifically the positive results are visible from around 2016 onwards.

We also provide a few side results that illustrate the timing and likely persistence of our findings. 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 (consistent with the political violence result). 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. Anecdotal 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.

Third, 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. However, men are more likely to develop more gender-equal 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.[4]

[4] On a 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

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)[5] are all unlikely driving the positive results.[6]

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) or institutional effects at the national level such as autocratic genderwashing (Bjarnegård and Zetterberg, 2022)), we also exploit exogenous variation in the village reception of a state-sponsored radio station (RTLM) that called for killings of the Tutsi minority (Yanagizawa-Drott, 2014). 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 participation levels in the killings (we argue below why the exclusion restriction likely holds in this case too). RTLM-induced violence was local, committed with low-technology weapons (e.g., machetes and clubs) targeted at women and children. We thus first document that RTLM violence leads to a male surplus. Consistently, we find negative or no effects on female outcomes or public goods and an increase in political violence.[7]

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 appendix Section A.7.

[5]Villages supporting the ruling RPF might receive favorable treatment from the national government.

[6]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. 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.

[7]We rule out that differences in compliers are driving the different results for local

Our findings potentially carry important policy implications. First, women taking 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.

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 potentially also relevant for post-conflict reconstruction efforts. In particular, a one-size-fits-all reconstruction approach might be misguided.

We add to the literature in several ways. Most importantly, this paper first speaks to a growing literature on the effects of women gaining (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. Furthermore, our paper allows us to shed more light on the short-term timing of empowerment and identifies the young generations as its main carriers. Finally, we show that the positive effects are likely long-lasting.[8]

Related, we also contribute to the scarce literature on gender equality and conflict. Previous research, focusing mostly on cross-country evidence, finds that conflict is less likely with gender equality (Caprioli, 2000; De-

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.

[8]Others have looked at e.g., the effects of female politicians on corruption (Brollo and Troiano, 2016) or the reporting of crimes against women (Iyer et al., 2012).

meritt et al., 2014).[9] However, we are the first to look at female leaders' effects on political violence at a local level using rigorous identification.

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 Khr, 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 channels, for example, via political participation and the timing of empowerment. Especially the effects on political participation is something that has 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).[10]

Finally, 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 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.[11] In recent years, a number of studies have exploited within-

[9] Dube and Harish (2020) provides more nuanced findings from a historical setting suggesting that queens' propensity to go to war depended on their marital status.

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

[11] 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. 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. Besides, our identification strategy allows us to analyze several other outcomes. Another 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. Other 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

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.

## 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,

more mixed results). However, as noted above, our paper allows us to shed more light on the channels, 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.

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 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).

[12] There was significant local variation – which we exploit.

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. Put differently, our findings are unlikely only the result of Rwanda's general women-supporting institutional environment (regardless of whether benevolent or disguised as autocratic genderwashing, i.e., that autocratic rulers promote fabricated gender equality to please the international community (Bjarnegård and Zetterberg, 2022)).

# 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]

The instrument, armed groups' transport costs, is constructed using the National Oceanic and Atmospheric Administration (NOAA) database of daily rainfall estimates for Africa. More specifically, 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

[13]Unfortunately, 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.

main road and interacts it with the distance to the main road.[14] Similarly, a village boundary map allows to compute rainfall in each village. Figure A.1 illustrates the instrument construction.[15]

**Genocide 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

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

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

[16]A 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.

[17]Note 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 RTLM coverage in the number of local militiamen (or policemen). The differential effects on mass graves, shown below, provide empirical evidence for this.

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

from 1999 to 2001. In total, 31,192 individuals in 6,240 households of 486 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 transport cost 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

[19]Note 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.

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

[21]www.nec.gov.rw

its members depends on the population size of the cell. In 2001 Rwanda significantly 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. 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). He further 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.

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]

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

Post-genocide political violence data is obtained from The Armed Conflict Location & Event Data Project (ACLED). The disaggregated and geo-referenced data of violent events covering Rwanda from 1997 up until today is spatially matched to our violence and instrument data. We only include events under local governing power, excluding those where one part is identified as an external or state military force. This leaves us 124 cells that experienced local post-genocide violence.

**RTLM Reception** Our placebo independent variable is predicted RTLM radio coverage at the village level, taken from Yanagizawa-Drott (2014), who uses RTLM 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 RTLM sample is somewhat smaller than the transport cost instrument sample. We match 8,912 households in 465 villages for the socioeconomic 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). Second,

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

**Table 1:** Summary Statistics

| | Mean | Std.Dev. |
|---|---|---|
| A. Endogenous Variables | | |
| # Prosecuted Militiamen | 58.380 | 80.42 |
| Mass Grave in Village | 0.056 | 0.23 |
| B. Exogenous Variables | | |
| 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.

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 socioeconomic 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.

**Specification** To show that higher transport costs caused less violence, we estimate the following first-stage equation

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

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 of the genocide along the way between the main road and each village $j$ in province $p$. Furthermore, $\gamma_p$ are province fixed effects and $\epsilon_{jp}$ is the error

[24] The 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.

term. Given the controls in $\mathbf{X}_{jp}$, explained in detail above, the interaction term captures the armed groups' transport costs. We expect $\beta$ to be negative.

We then run the following reduced-form regressions

$$(2) \qquad post\_y_{ijp} = \alpha' + \beta' \left[ log(Dist_{jp}) \times log\left(Rain_{jp}\right) \right] + \mathbf{X}_{jp}\pi' + \gamma_p + \epsilon_{ijp},$$

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. 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.[25] We also present instrumental variable estimates.[26]

**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.[27]

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

[25] In 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.

[26] Note 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.

[27] Note 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 | # Militiamen, log | | | Mass Graves | |
|---|---|---|---|---|---|
| Sample | Full Sample | DHS Sample | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Distance x Rainfall, 1994 | −0.509 | −0.719 | −0.781 | −0.080 | −0.088 |
| | (0.115) | (0.181) | (0.181) | (0.028) | (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. 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).

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 Women Taking over Power

**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 IBUKA[28]), 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-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

[28]IBUKA, which means "Remember," is a Rwandan genocide survivor organization that works to perpetuate the memory of the genocide.

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.[29] 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.[30] 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 appendix Section A.2.

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

Results are reported in Table 4. Importantly, armed-group violence leads to an increase in the fraction of women elected for office in general (regression 1). Furthermore, not only are more women entering office but the elected ones are also better educated (i.e., finished at least primary school,

[29]Note, 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.

[30]On 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 Females | Fraction Vulnerable | Fraction Children | Fraction Female Children |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Distance × Rainfall, 1994 | 0.025 | 0.060 | 0.045 | 0.048 |
| | (0.011) | (0.013) | (0.017) | (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 | −0.072 | −0.053 | −0.058 |
| | (0.014) | (0.020) | (0.018) | (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).

regression 2); we do not find any effects on the education of elected men (regression 3). 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 women with at least primary education.

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.[31] It thus seems that the above results are not simply a mechanical effect. We provide evidence in appendix Section A.6.

[31] The 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 4:** Women's Political Engagement

| Sample | Women & Men | Women | Men |
|---|---|---|---|
| Dependent Variable | Fraction of Elected Women | Fraction With Primary Education | |
| | (1) | (2) | (3) |
| Distance x Rainfall, 1994 | −0.017 | −0.031 | −0.001 |
| | (0.006) | (0.014) | (0.018) |
| Dep. Mean | 0.53 | 0.44 | 0.45 |
| Dep. Std.Dev. | 0.09 | 0.24 | 0.21 |
| $R^2$ | 0.10 | 0.19 | 0.17 |
| N | 1901 | 1899 | 1898 |
| IV: External Militia Violence | 0.055 | 0.102 | 0.003 |
| | (0.027) | (0.038) | (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).

# 6 Socio-Economic Effects and Norms

## 6.1 Women's Outcomes and Attitudes

We next 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.[32]

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

[32]Note 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.

results for these (Table 5).[33,34] 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.[35,36]

**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 1 in Panel A in Table 5).[37] 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.[38] 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).[39] In terms of magnitudes, a one-standard-deviation increase in external genocide violence leads to about a 0.4 standard-deviation increase

[33] We 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.

[34] Table 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 5 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.

[35] We set the indixes 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.

[36] One 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.

[37] The results are robust to combining this index with the financial autonomy index below.

[38] Note that the wealth index measures household wealth, thus women live in richer families.

[39] The 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).

**Table 5:** Socio-Economic Effects and Norms (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 | −0.317 | −0.301 | −0.090 | −0.294 | |
| | (0.164) | (0.071) | (0.050) | (0.041) | (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 | 0.397 | 0.374 | 0.112 | 0.369 | |
| | (0.136) | (0.066) | (0.097) | (0.059) | (0.103) | |
| **Panel B – IPV** | Domestic Violence Attitudes | Violence Against Kids Attitudes | Domestic Violence | | | |
| | | | All | Severe | Sexual | Less Severe |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Distance × Rainfall, 1994 | 0.281 | 0.437 | 0.305 | 0.253 | 0.077 | 0.051 |
| | (0.167) | (0.138) | (0.278) | (0.097) | (0.071) | (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 | −0.548 | −0.360 | −0.300 | −0.092 | −0.061 |
| | (0.229) | (0.205) | (0.348) | (0.123) | (0.084) | (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).

in the total index.[40]

**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 2, a one-standard-deviation increase in large-scale violence induces an increase in the financial autonomy index of 0.25 standard deviations.

[40]Note 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.

**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 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 3, 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 4 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 4.

**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 contraception[41] 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.[42] The z-score in regression 1 in Panel B 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 accept 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).

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.

We also find a drop in women reporting to be the victim of domestic violence. The DHS data includes nine questions on this topic which we classify as less severe and severe violence.[43] We also separate out sexual violence. In regression 3, we report the z-score on all nine violence outcomes. While

[41]Contraception, while having the right point estimate, is not significant.

[42]The 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.

[43]Whether a woman was *ever* pushed, punched, slapped, kicked, had arm-twisted or hair pulled, was strangled or burnt, or threatened with a knife or gun. The DHS further distinguishes between women being forced into unwanted sex or unwanted sexual acts, and we follow this distinction here.

the IV estimate is negative, it is not significant. However, splitting the outcomes, we find a strong significant drop in severe violence (regression 4). 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 5 and 6). Details in Table A.10.[44]

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 (...)."*

**Robustness Checks** In the interests of space, we provide additional robustness checks in appendix Section A.3. 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.

## 6.2 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

[44] Note 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.

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.[45] 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).

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

### 6.3 Effects Over Time, Intergenerational Transmission and Children's Outcomes

In appendix Section A.4, 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 them, play with them, etc.) in their kids, especially girls. And second, this results in increases in children's cognitive and non-cognitive abilities. Again, details can be found in the appendix.

## 7 Local Public Goods and Political Violence

**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 6, we test this. We use information about the length of the road system, the number of primary schools, the 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.

**Table 6:** Public Goods Provision

| Dependent Variable | Road Length, log | # Health Facilities | Social Housing, log | Access To Electricity | # Primary Schools | # Secondary Schools | Index Using 1-6 |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Distance × Rainfall, 1994 | −0.124 | −0.076 | −0.058 | −0.151 | −0.023 | −0.055 | −0.904 |
| | (0.039) | (0.027) | (0.034) | (0.043) | (0.086) | (0.049) | (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 | 0.212 | 0.160 | 0.418 | 0.065 | 0.152 | 2.507 |
| | (0.167) | (0.086) | (0.093) | (0.216) | (0.233) | (0.123) | (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).

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 likely focused on providing important public goods and set examples for future (female) generations to come (recall that the results are particularly strong for younger women). Again, we relegate more detailed anecdotal evidence to appendix Section A.2.

**Political Violence** Given the documented increase in local public goods provision (opportunity cost mechanism) and the general societal change towards more equal gender norms, we should also observe a drop in political violence (Melander, 2005). The results indeed suggest a significant negative relationship between external genocide violence and post-genocide political violence (Table A.16 in the appendix). In terms of magnitudes, a 10 percent increase in genocide violence leads to a 12.5 percent decrease in overall political violence (from 1999 to 2022, regression 1). However, consistent with the delayed effects already documented above, this result is driven by recent years, from around 2016 onwards (regression 4). We find no difference for earlier years (regressions 2 and 3).

# 8 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 appendix Section A.5 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.

**Results** RTLM-induced violence was different from armed-group violence analyzed above. RTLM 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, RTLM coverage is unrelated to whether a village has a mass grave site or not.[46]

Second, the main targets of RTLM-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. Consistently, women are less likely to be household heads or elected into a local political office. More details and empirical evidence, together with the identification strategy, are shown in appendix Section A.5.

Consistent with men staying in power in these places, we do not find positive effects on women's socioeconomic outcomes and attitudes.[47] Rather, taken at face value, the point estimates on the various indexes suggest that

[46] In Table A.35 in the appendix, we confirm these results using our DHS sample.

[47] Note 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 radio coverage – likely local militiamen.

women are worse off in RTLM-violence villages (Table A.17 and details in Tables A.18 to A.25).[48] Most notably, women are significantly more likely to accept wife beating (regression 1) on all dimensions (details in Table A.24) 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.[49] In the appendix, we rule out an alternative channel, 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.

Finally, we find if at all, negative effects on public goods provision and significant positive effects on post-genocide political violence, with a 10 percent increase in local genocide violence increasing post-genocide violence by almost 40 percent. Again, details are provided in appendix Section A.5.

# 9 Outlook

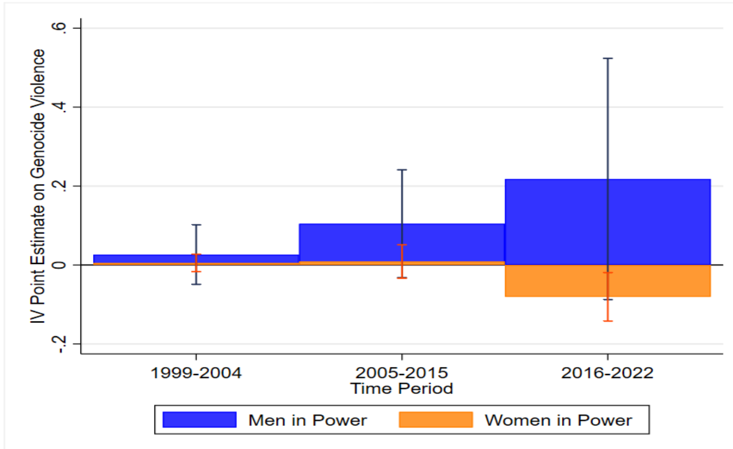
Are the positive results on female outcomes in women-led villages likely to last in the future? Several pieces of evidence suggest that this is likely.

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

[48] Importantly, note that the differential results for RTLM-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.

[49] The 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.35).

**Figure 1:** Effects on Post-Genocide Political Violence




Notes: We run our main IV specifications with political violence as dependent variable for different time periods. The point estimates together with 95 percent confidence intervals are plotted on the y-axis. To illustrate, the first point estimate to the left uses all violent ACLED events between 1999 and 2004. Regressions for "Men in Power" use the RTLM instrument. For "Women in Power" we use the transport costs instrument.

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 appendix Section A.6.

Third, 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).

Finally, recall from above that the significant drop in post-genocide violence in women-led villages is driven especially by recent years, i.e., from 2016 onwards. Since violence begets violence or the other way around, this peaceful development is likely to have long-lasting effects. Furthermore, in Figure 1 we plot the effects of genocide violence on political violence over time for both women-led and men-led villages (recall the RTLM placebo).

The effects clearly diverge. Thus, not only are women-led villages likely more peaceful in the future but men-led villages seem persistently more violent.[50]

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. (...) "*

# 10 Alternative Channels

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.

[50] Note that we also tried using the recently released DHS from 2020 but unfortunately, there is no first stage and consistently we find nothing in the reduced form.

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 appendix Section A.7.

# 11 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.

In terms of channels, our findings suggest that the external militia's strategy to target primarily adult men leads to a power vacuum which women filled with 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, women taking 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 to be persistent once the positive effects materialize. Second, to the best of our knowledge, we are the first to show that different types of violence in the same conflict can affect later outcomes 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 7: Detailed Anecdotal Evidence

## A.3 Extensions to Section 6.1: Robustness Checks

## A.4 Extensions to Section 6.3: Effects over Time and Intergenerational Transmission

## A.5 Extensions to Section 8: Placebo – RTLM Hate Radio

## A.6 Extensions to Section 9: Outlook

## A.7 Extensions to Section 10: Alternative Channels

# A.1 Additional Tables and Figures

**Table A.1:** Summary Statistics – RTLM Sample

| | Mean | Std.Dev. |
|---|---|---|
| A. Endogenous Variables | | |
| # Prosecuted Militiamen | 66.426 | 84.20 |
| Mass Grave in Village | 0.052 | 0.22 |
| B. Exogenous Variables | | |
| RTLM 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. RTLM 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 Attitudes | Violence Against Kids Attitudes | Education, Wealth, Health | Financial Autonomy | Physical Autonomy | Decision Power | Sexual Knowledge, Fertility | Domestic Violence: All | Domestic Violence: Severe | Domestic Violence: Sexual | Domestic Violence: Less Severe |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Local and External Militia Violence | −0.050 | −0.006 | 0.017 | 0.042 | −0.005 | −0.000 | −0.001 | −0.073 | −0.060 | −0.016 | −0.013 |
| | (0.042) | (0.018) | (0.019) | (0.028) | (0.007) | (0.016) | (0.014) | (0.073) | (0.040) | (0.025) | (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).

**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 | −0.125 | −0.135 | −0.058 | −0.184 | −0.097 | −0.108 |
| | (0.164) | (0.038) | (0.069) | (0.023) | (0.071) | (0.051) | (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 | 0.157 | 0.170 | 0.073 | 0.230 | 0.118 | 0.134 |
| | (0.136) | (0.034) | (0.061) | (0.024) | (0.068) | (0.078) | (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).

**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 | −0.112 | −0.057 | −0.083 | −0.056 |
| | (0.071) | (0.078) | (0.040) | (0.038) | (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 | 0.141 | 0.068 | 0.104 | 0.070 |
| | (0.066) | (0.080) | (0.038) | (0.056) | (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).

**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 | Husband Has Other Wives | Age At First 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 | −0.031 | −0.016 | −0.165 | −0.119 | −0.105 | −0.119 | 0.127 | −0.051 | 0.076 | −0.044 | 0.048 |
| | (0.050) | (0.032) | (0.012) | (0.025) | (0.045) | (0.027) | (0.045) | (0.023) | (0.024) | (0.020) | (0.021) | (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 | 0.037 | 0.020 | 0.202 | 0.143 | 0.126 | 0.040 | −0.154 | 0.062 | −0.093 | 0.054 | −0.058 |
| | (0.097) | (0.037) | (0.014) | (0.047) | (0.066) | (0.043) | (0.019) | (0.046) | (0.034) | (0.038) | (0.030) | (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 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).

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

| Dependent Variable | Index | Decides On Husband's Money | Decides On Large Purchases | Decides On Own Healthcare | Needs Permission To Get Medical Help |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Distance × Rainfall, 1994 | −0.090 | −0.045 | 0.038 | −0.064 | 0.053 |
| | (0.041) | (0.032) | (0.028) | (0.032) | (0.031) |
| 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 | 0.04 | 0.10 | 0.21 | 0.97 |
| Dep. Std.Dev. | 1.89 | 0.20 | 0.30 | 0.41 | 0.16 |
| $R^2$ | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |
| N | 24800 | 12318 | 12548 | 12541 | 24795 |
| IV: External Militia Violence | 0.112 | 0.055 | −0.046 | 0.077 | −0.066 |
| | (0.059) | (0.038) | (0.034) | (0.037) | (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).

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

| Dependent Variable | Index | Knowledge of Ovulatory Cycle | Knowledge of Contraception | Knowledge of HIV | Wants More Children | Ideal Number of Children |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Distance × Rainfall, 1994 | −0.294 | −0.098 | −0.034 | 0.053 | 0.051 | 0.073 |
| | (0.044) | (0.034) | (0.027) | (0.021) | (0.039) | (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 | 0.123 | 0.042 | 0.066 | −0.064 | −0.092 |
| | (0.103) | (0.050) | (0.034) | (0.024) | (0.055) | (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).

**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 | 0.137 | 0.078 | 0.099 | 0.072 | 0.056 |
| | (0.167) | (0.043) | (0.044) | (0.040) | (0.033) | (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 | −0.172 | −0.097 | −0.123 | −0.090 | −0.070 |
| | (0.229) | (0.061) | (0.051) | (0.058) | (0.052) | (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).

**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 | 0.106 | 0.098 | 0.078 |
| | (0.138) | (0.047) | (0.044) | (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 | −0.132 | −0.123 | −0.098 |
| | (0.205) | (0.071) | (0.068) | (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).

**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 | 0.073 | −0.010 | 0.088 | −0.022 | −0.014 | 0.018 | 0.022 | 0.049 |
| | (0.029) | (0.038) | (0.042) | (0.033) | (0.055) | (0.056) | (0.047) | (0.033) | (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 | −0.086 | 0.012 | −0.104 | 0.026 | 0.017 | −0.021 | −0.026 | −0.059 |
| | (0.047) | (0.043) | (0.050) | (0.040) | (0.063) | (0.065) | (0.056) | (0.041) | (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).

**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 | −0.019 | −0.050 | 0.025 | −0.179 |
| | (0.184) | (0.040) | (0.064) | (0.034) | (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 | 0.024 | 0.066 | −0.033 | 0.235 |
| | (0.197) | (0.051) | (0.074) | (0.048) | (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).

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

| Dependent Variable | Attitude Index | Wife Can Request Condom if Husband has STI | Wife Can Refuse Sex if Husbands Cheats | Ever Paid Someone For Sex | Paid Someone For Sex Last 12 Months | Children Should be Taught About Condoms |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Distance × Rainfall, 1994 | −0.228 | −0.011 | −0.081 | 0.086 | 0.084 | 0.032 |
| | (0.087) | (0.040) | (0.037) | (0.014) | (0.026) | (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 | 0.015 | 0.105 | −0.113 | −0.111 | −0.042 |
| | (0.089) | (0.050) | (0.059) | (0.029) | (0.031) | (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).

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

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

**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 | −0.259 | −0.020 | −0.137 |
| | (0.081) | (0.041) | (0.072) | (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 | 0.346 | 0.024 | 0.167 |
| | (0.090) | (0.104) | (0.087) | (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).

**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 | Goes Out Without Telling | Neglects Children | Argues With Husband | Refuses To Have Sex | Burns Food | Cheats | Disobeys | Is Impolite | Embarrasses The Family |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Distance × Rainfall, 1994 | 0.109 | 0.100 | 0.018 | 0.007 | 0.032 | 0.026 | −0.040 | −0.029 | −0.020 | 0.010 |
| | (0.292) | (0.027) | (0.043) | (0.033) | (0.061) | (0.023) | (0.037) | (0.056) | (0.068) | (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 | −0.131 | −0.024 | −0.010 | −0.042 | −0.034 | 0.053 | 0.038 | 0.026 | −0.014 |
| | (0.391) | (0.054) | (0.058) | (0.043) | (0.078) | (0.034) | (0.053) | (0.073) | (0.088) | (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).

**Table A.16:** Post-Genocide Political Violence

| Dependent Variable | Cell Experienced Political Violence | | | |
|---|---|---|---|---|
| | 1999-2022 | 1999-2005 | 2006-2015 | 2016-2022 |
| | (1) | (2) | (3) | (4) |
| Distance × Rainfall, 1994 | 0.032 | −0.002 | −0.004 | 0.033 |
| | (0.020) | (0.004) | (0.009) | (0.014) |
| 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 | 0.06 | 0.01 | 0.02 | 0.04 |
| Dep. Std.Dev. | 0.24 | 0.09 | 0.15 | 0.21 |
| $R^2$ | 0.03 | 0.02 | 0.02 | 0.02 |
| N | 1980 | 1980 | 1980 | 1980 |
| IV: External Militia Violence | −0.079 | 0.005 | 0.009 | −0.081 |
| | (0.044) | (0.011) | (0.022) | (0.031) |

Notes: All regressions are run at the cell level. Standard Controls, Growing Season Controls, Additional Controls and Province Effects 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).

**Table A.17:** Local Violence (RTLM) – Socio-Economic Effects and Norms

| Dependent Variable | | | | | | | | Domestic Violence | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Domestic Violence Attitudes | Violence Against Kids 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) |
| RTLM Radio Coverage | 1.524 | 0.545 | −0.413 | 0.109 | −0.460 | 0.011 | −0.250 | 0.043 | 0.154 | 0.015 | −0.111 |
| | (0.813) | (0.326) | (0.667) | (0.201) | (0.248) | (0.134) | (0.129) | (1.099) | (0.370) | (0.310) | (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 | 0.759 | −0.575 | 0.152 | −0.658 | 0.016 | −0.348 | 0.063 | 0.224 | 0.022 | −0.162 |
| | (1.613) | (0.631) | (1.027) | (0.242) | (0.529) | (0.185) | (0.278) | (1.534) | (0.534) | (0.433) | (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.

**Table A.18:** 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 | −0.204 | 0.205 | −0.215 | −0.094 | −0.008 |
| | (0.131) | (0.248) | (0.127) | (0.200) | (0.112) | (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 | −0.285 | 0.287 | −0.299 | −0.140 | −0.011 |
| | (0.224) | (0.417) | (0.252) | (0.376) | (0.161) | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.19:** Local Violence – Women's Financial Autonomy

| Dependent Variable | Works in Skilled Job | Receives Cash Earnings | Owns House | Owns Land |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| RTLM Radio Coverage | 0.027 | −0.121 | −0.013 | −0.026 |
| | (0.129) | (0.163) | (0.066) | (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 | −0.190 | −0.018 | −0.036 |
| | (0.165) | (0.314) | (0.087) | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.20:** Local Violence – Women's Physical Autonomy

| Dependent Variable | Postpartum Abstinence | Woman Decides Contraception | Can Get Condom | Ask Partner To Use Condom | Condom Used Last Sex | Husband 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 | −0.047 | −0.244 | −0.173 | −0.100 | −0.147 | −0.038 | 0.083 | −0.053 | 0.141 | −0.039 |
| | (0.084) | (0.095) | (0.110) | (0.142) | (0.118) | (0.116) | (0.087) | (0.073) | (0.076) | (0.081) | (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 | −0.093 | −0.336 | −0.261 | −0.157 | −0.215 | −0.056 | 0.124 | −0.081 | 0.214 | −0.055 |
| | (0.177) | (0.191) | (0.257) | (0.217) | (0.225) | (0.208) | (0.135) | (0.134) | (0.115) | (0.150) | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.21:** Local Violence – Women's Sexual Knowledge, Fertility Preferences

| Dependent Variable | Knowledge of Ovulatory Cycle | Knowledge of Contraception | Knowledge of HIV | Wants More Children | Ideal Number of Children | Contraception Women Business | Contraception makes Women Promiscuous |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| RTLM Radio Coverage | −0.030 | −0.047 | −0.015 | 0.062 | 0.128 | 0.139 | 0.177 |
| | (0.125) | (0.037) | (0.091) | (0.127) | (0.105) | (0.122) | (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 | −0.065 | −0.021 | 0.093 | 0.179 | 0.197 | 0.252 |
| | (0.178) | (0.063) | (0.125) | (0.185) | (0.184) | (0.224) | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.22:** Local Violence – Women's Decision-Making Power

| Dependent Variable | Decides On Husband's Money | Decides On Large Purchases | Decides On Own Healthcare | Needs Permission To Get Medical Help | Women Should Participate In Large Purchases |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| RTLM Radio Coverage | 0.068 | −0.003 | −0.093 | 0.030 | −0.254 |
| | (0.101) | (0.107) | (0.107) | (0.072) | (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 | −0.004 | −0.136 | 0.042 | −0.517 |
| | (0.149) | (0.150) | (0.141) | (0.102) | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.23:** 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 | 0.156 | 0.231 | 0.160 |
| | (0.326) | (0.117) | (0.107) | (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 | 0.218 | 0.322 | 0.224 |
| | (0.631) | (0.199) | (0.238) | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.24:** 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) |
| RTLM Radio Coverage | 0.270 | 0.291 | 0.312 | 0.286 | 0.372 |
| | (0.164) | (0.145) | (0.182) | (0.164) | (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 | 0.408 | 0.438 | 0.404 | 0.518 |
| | (0.307) | (0.295) | (0.353) | (0.306) | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.25:** 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) |
| RTLM Radio Coverage | 0.031 | 0.114 | 0.025 | −0.010 | −0.012 | −0.130 | −0.073 | 0.074 | 0.030 |
| | (0.111) | (0.197) | (0.200) | (0.160) | (0.171) | (0.173) | (0.220) | (0.156) | (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 | 0.166 | 0.037 | −0.014 | −0.017 | −0.190 | −0.104 | 0.108 | 0.043 |
| | (0.156) | (0.313) | (0.279) | (0.224) | (0.241) | (0.302) | (0.326) | (0.239) | (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.17. Standard errors in parentheses are clustered at district level.

**Figure A.1:** Construction of Transport Costs


Notes: Picture is taken from Rogall (2021).

## A.2 Extensions to Sections 4 and 7: 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, 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 6.1: 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.26 we show that all the result are robust to dropping these controls.

Next, in Table A.27 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.27 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.28). 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.

[2]The 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.26:** Robustness Check I – Socio-Economic Effects and Norms (2010 and 2015) No Additional Controls

| Dependent Variable | Domestic Violence Attitudes | Violence Against Kids Attitudes | Education, Wealth, Health | Financial Autonomy | Physical Autonomy | Decision Power | Sexual Knowledge, Fertility | Domestic Violence: All | Domestic Violence: Severe | Domestic Violence: Sexual | Domestic Violence: Less Severe |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Distance × Rainfall, 1994 | 0.436 | 0.272 | −0.660 | −0.286 | −0.331 | −0.091 | −0.299 | 0.287 | 0.232 | 0.057 | 0.055 |
| | (0.164) | (0.146) | (0.185) | (0.072) | (0.045) | (0.036) | (0.061) | (0.302) | (0.097) | (0.068) | (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 | −0.363 | 0.883 | 0.382 | 0.439 | 0.122 | 0.400 | −0.372 | −0.301 | −0.074 | −0.071 |
| | (0.229) | (0.229) | (0.191) | (0.087) | (0.111) | (0.060) | (0.132) | (0.410) | (0.137) | (0.089) | (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).

**Table A.27:** Robustness Check II – Socio-Economic Effects and Norms (2010 and 2015) Household Controls

| Dependent Variable | | | | | | | | Domestic Violence | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Domestic Violence Attitudes | Violence Against Kids 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) |
| Distance × Rainfall, 1994 | 0.441 | 0.292 | −0.643 | −0.292 | −0.300 | −0.081 | −0.278 | 0.282 | 0.254 | 0.080 | 0.028 |
| | (0.168) | (0.135) | (0.154) | (0.069) | (0.050) | (0.045) | (0.040) | (0.281) | (0.096) | (0.070) | (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 | −0.368 | 0.810 | 0.368 | 0.376 | 0.102 | 0.350 | −0.338 | −0.305 | −0.096 | −0.033 |
| | (0.231) | (0.199) | (0.123) | (0.063) | (0.107) | (0.071) | (0.099) | (0.348) | (0.125) | (0.084) | (0.247) |

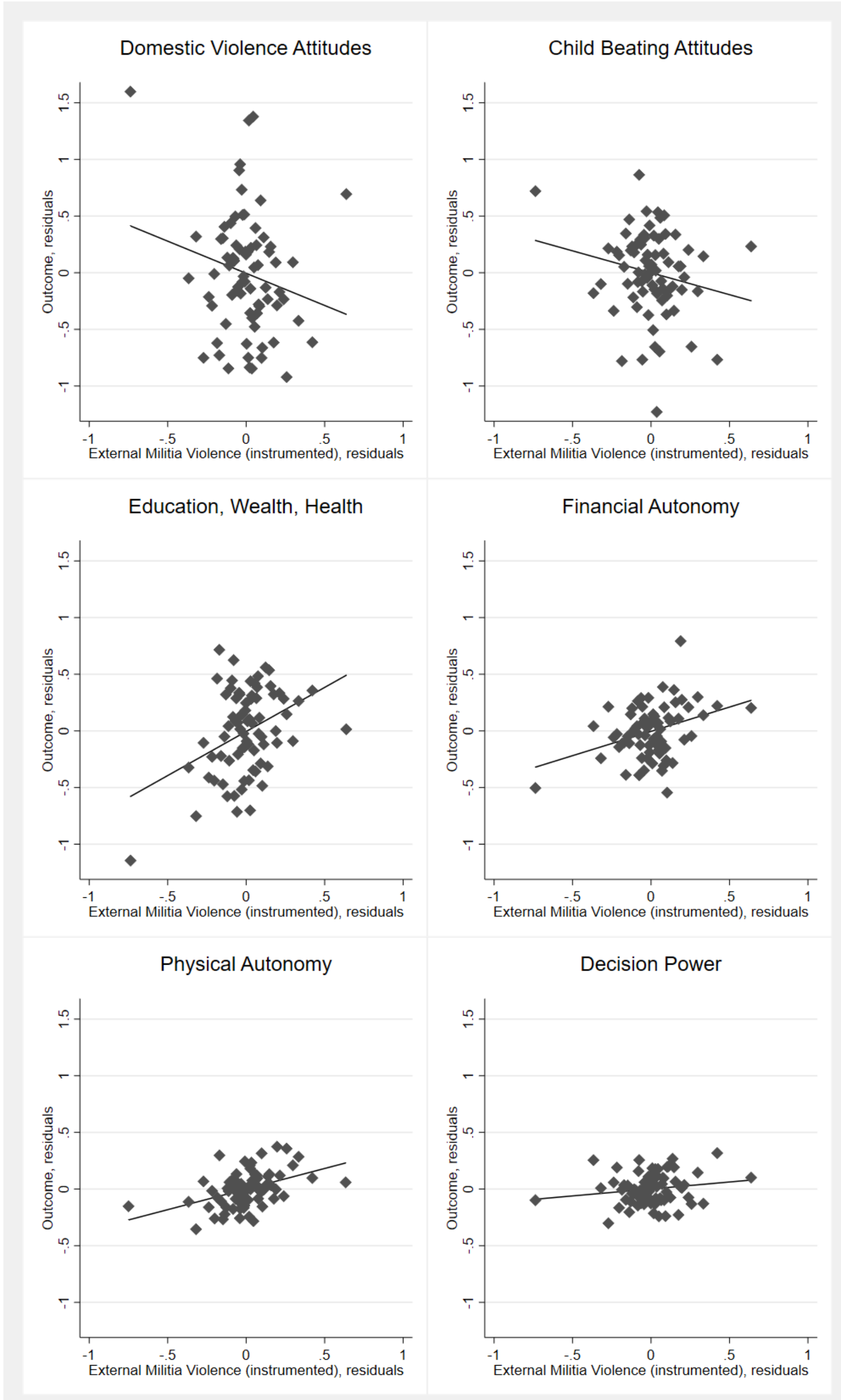
Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. Standard Controls, Growing Season Controls, and Additional Controls are defined in Table 2. All village-level control variables, except "Number of Days with RPF 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).

**Table A.28:** Robustness Check III – Socio-Economic Effects and Norms (2010 and 2015) Additional Rainfall Controls

| Dependent Variable | | | | | | | | Domestic Violence | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Domestic Violence Attitudes | Violence Against Kids 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) |
| Distance × Rainfall, 1994 | 0.478 | 0.303 | −0.561 | −0.319 | −0.278 | −0.104 | −0.275 | 0.454 | 0.364 | 0.163 | 0.089 |
| | (0.189) | (0.167) | (0.190) | (0.068) | (0.054) | (0.041) | (0.054) | (0.232) | (0.102) | (0.070) | (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 | −0.407 | 0.752 | 0.428 | 0.373 | 0.140 | 0.369 | −0.529 | −0.424 | −0.190 | −0.104 |
| | (0.282) | (0.255) | (0.196) | (0.088) | (0.102) | (0.058) | (0.117) | (0.294) | (0.124) | (0.083) | (0.209) |

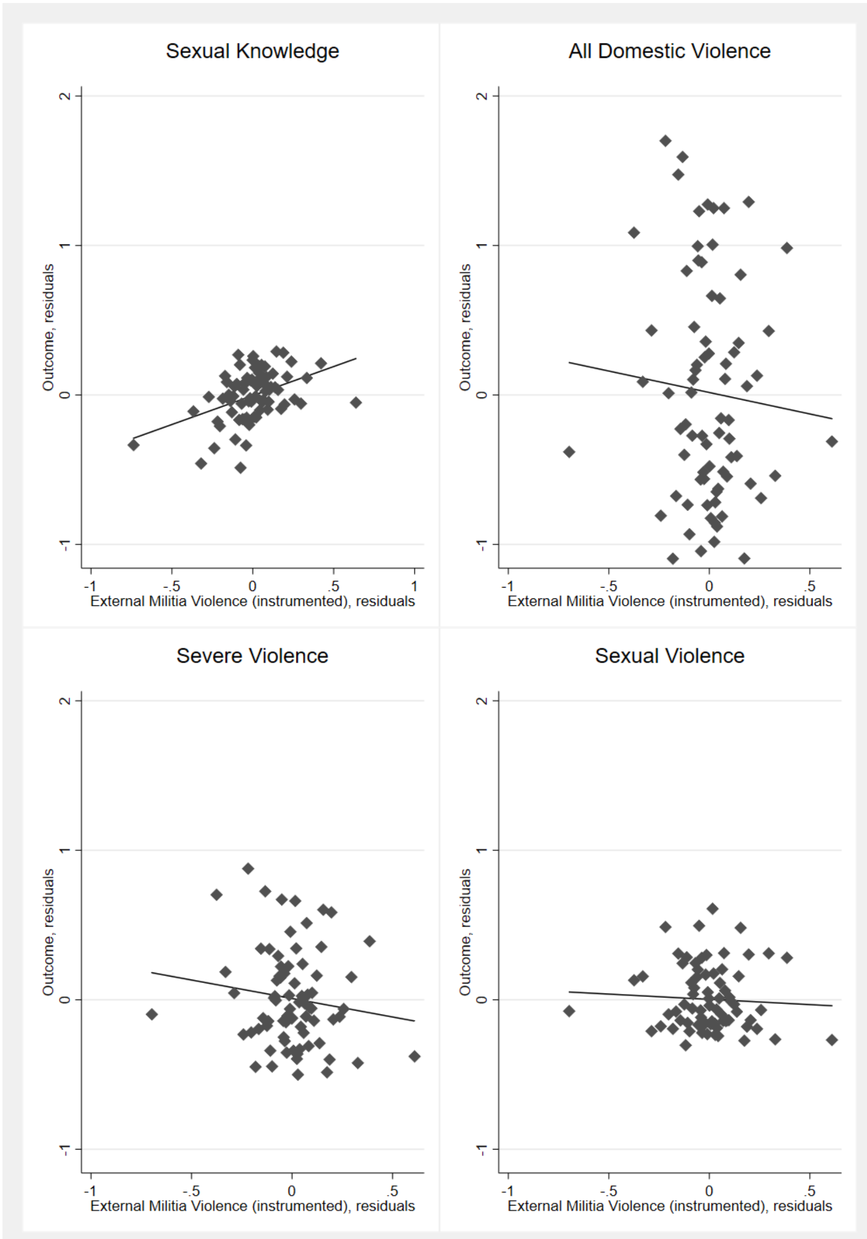
Notes: The sample is restricted to women from DHS rounds 6 and 7. We calculate various indexes, the composition of each index is given in the paper. Standard Controls and Growing Season Controls, and Additional Controls are defined in Table 2. **Additional Rainfall Controls** are average rainfall along 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).

**Figure A.2:** Relationship between External Violence and Socio-Economic 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 Socio-Economic 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 6.3: 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.29 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.30 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.31 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.32, 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-

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

[4] Ideally, 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

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.33 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.34 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.

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

[6]Note 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.

[7]Outcomes 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).

Carrell, S. E. and Hoekstra, M. L. (2010). Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone's Kids. *American Economic Journal: Applied Economics*, 2(1):211-228.

Heckman, J. J., Stixrud, J., and Urzua, S. (2006). The Effects of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior. *Journal of Labor Economics*, 24(3):411-482.

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

**Table A.29:** Armed-Group Violence – Socio-Economic Effects and Norms (2005)

| Dependent Variable | Domestic Violence Attitudes | Education, Wealth, Health | Financial Autonomy | Physical Autonomy | Decision Power | Sexual Knowledge, Fertility | Domestic Violence: All | Domestic Violence: Severe | Domestic Violence: Sexual | Domestic Violence: Less Severe |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Distance × Rainfall, 1994 | −0.386 | −0.054 | 0.005 | −0.064 | 0.097 | 0.146 | 0.046 | −0.131 | 0.052 | 0.178 |
| | (0.119) | (0.185) | (0.072) | (0.086) | (0.071) | (0.130) | (0.405) | (0.140) | (0.064) | (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 | 0.088 | −0.009 | 0.101 | −0.160 | −0.241 | −0.091 | 0.259 | −0.103 | −0.350 |
| | (0.379) | (0.280) | (0.119) | (0.120) | (0.153) | (0.285) | (0.800) | (0.297) | (0.132) | (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 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).

**Table A.30:** First Stage – DHS (2005)

| Dependent Variable | # Militiamen, log | | Mass Graves | |
|---|---|---|---|---|
| | | Add. Controls | | Add. Controls |
| | (1) | (2) | (3) | (4) |
| Distance × Rainfall, 1994 | −0.419 | −0.509 | −0.073 | −0.079 |
| | (0.211) | (0.204) | (0.034) | (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).

**Table A.31:** Armed-Group Violence – Young versus Old

| Dependent Variable | Domestic Violence Attitudes | Violence Against Kids Attitudes | Education, Wealth, Health | Financial Autonomy | Physical Autonomy | Decision Power | Sexual Knowledge, Fertility | Domestic Violence: All | Domestic Violence: Severe | Domestic Violence: Sexual | Domestic Violence: Less Severe |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Panel A – Young Women (< 18 years) | | | | | | | | | | | |
| Distance × Rainfall, 1994 | 0.496 | 0.306 | −0.678 | −0.342 | −0.266 | −0.175 | −0.287 | 0.622 | 0.323 | 0.213 | 0.299 |
| | (0.173) | (0.136) | (0.179) | (0.064) | (0.053) | (0.036) | (0.052) | (0.333) | (0.154) | (0.089) | (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 | −0.388 | 0.862 | 0.435 | 0.336 | 0.223 | 0.365 | −0.831 | −0.431 | −0.284 | −0.400 |
| | (0.221) | (0.209) | (0.142) | (0.083) | (0.093) | (0.076) | (0.104) | (0.457) | (0.215) | (0.127) | (0.254) |
| Panel B – Older Women (> 18 years) | | | | | | | | | | | |
| Distance × Rainfall, 1994 | 0.314 | 0.254 | −0.556 | −0.286 | −0.408 | 0.184 | −0.382 | −0.664 | −0.036 | −0.300 | −0.628 |
| | (0.267) | (0.171) | (0.120) | (0.170) | (0.079) | (0.113) | (0.079) | (0.440) | (0.178) | (0.103) | (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 | −0.302 | 0.660 | 0.339 | 0.483 | −0.219 | 0.454 | 0.628 | 0.034 | 0.284 | 0.594 |
| | (0.341) | (0.218) | (0.156) | (0.142) | (0.147) | (0.153) | (0.160) | (0.383) | (0.169) | (0.118) | (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 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).

**Table A.32:** Armed-Group Violence – Intergenerational Transmission

| Sample | Non-Migrants | | | | At Least Once Away | | | | More Than Once Away | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dependent Variable | Domestic Violence Attitudes | Violence Against Kids Attitudes | Domestic Violence Total | Domestic Violence Severe | Domestic Violence Attitudes | Violence Against Kids Attitudes | Domestic Violence Total | Domestic Violence Severe | Domestic Violence Attitudes | Violence Against Kids Attitudes | Domestic Violence Total | Domestic Violence Severe |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Distance × Rainfall, 1994 | 0.514 | 0.392 | 1.032 | 0.595 | 0.388 | 0.191 | −0.226 | 0.026 | −0.130 | 0.092 | −0.033 | 0.069 |
| | (0.200) | (0.151) | (0.589) | (0.259) | (0.151) | (0.143) | (0.249) | (0.086) | (0.216) | (0.135) | (0.344) | (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 | −0.513 | −1.620 | −0.934 | −0.465 | −0.229 | 0.220 | −0.025 | 0.161 | −0.115 | 0.033 | −0.068 |
| | (0.305) | (0.243) | (0.953) | (0.399) | (0.182) | (0.189) | (0.231) | (0.084) | (0.290) | (0.176) | (0.338) | (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).

**Table A.33:** 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 | 0.035 | −0.068 | −0.095 | −0.151 | −0.039 | 0.026 |
| | (0.308) | (0.043) | (0.051) | (0.031) | (0.047) | (0.049) | (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 | −0.044 | 0.086 | 0.121 | 0.192 | 0.050 | −0.034 |
| | (0.453) | (0.065) | (0.075) | (0.057) | (0.092) | (0.066) | (0.054) |
| Panel B – Boys Age 3-5 | | | | | | | |
| Distance × Rainfall, 1994 | −0.277 | −0.113 | −0.032 | −0.009 | −0.011 | 0.035 | 0.004 |
| | (0.358) | (0.030) | (0.038) | (0.039) | (0.052) | (0.039) | (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 | 0.125 | 0.036 | 0.010 | 0.013 | −0.038 | −0.005 |
| | (0.409) | (0.043) | (0.042) | (0.043) | (0.059) | (0.043) | (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).

**Table A.34:** Armed-Group Violence – Children Outcomes (Girls and Boys)

| Dependent Variable | 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 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Panel A – Girls Age 3-5 | | | | | | | | | | | |
| Distance × Rainfall, 1994 | −1.131 | −0.042 | 0.001 | 0.013 | −0.117 | 0.002 | 0.012 | −0.089 | −0.013 | −0.128 | −0.045 |
| | (0.556) | (0.055) | (0.024) | (0.048) | (0.038) | (0.043) | (0.044) | (0.060) | (0.024) | (0.056) | (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 | 1.437 | 0.053 | −0.001 | −0.016 | 0.150 | −0.002 | −0.015 | 0.112 | 0.016 | 0.162 | 0.057 |
| | (0.474) | (0.062) | (0.031) | (0.063) | (0.044) | (0.054) | (0.059) | (0.075) | (0.027) | (0.115) | (0.073) |
| Panel B – Boys Age 3-5 | | | | | | | | | | | |
| Distance × Rainfall, 1994 | −1.376 | −0.159 | −0.008 | −0.015 | −0.070 | 0.048 | −0.034 | −0.129 | −0.032 | 0.005 | −0.101 |
| | (0.355) | (0.061) | (0.023) | (0.027) | (0.027) | (0.034) | (0.038) | (0.037) | (0.020) | (0.041) | (0.071) |
| 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 | 1.534 | 0.177 | 0.009 | 0.017 | 0.078 | −0.054 | 0.038 | 0.144 | 0.036 | −0.006 | 0.115 |
| | (0.396) | (0.048) | (0.025) | (0.030) | (0.036) | (0.046) | (0.044) | (0.056) | (0.025) | (0.045) | (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).

## A.5 Extensions to Section 8: Placebo – RTLM Hate Radio

### A.5.1 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.5.2 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 RTLM transmitters. This is not the case, however – both RTLM transmitters were destroyed at the end of the genocide, and the broadcasts stopped.

The exclusion restriction would also be violated if the RTLM 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 RTLM'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 RTLM's broadcasts and did not find evidence that RTLM's broadcasts would directly affect economic or female performance.[8]

**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

$$log\left(h_{jc}\right) = \alpha + \beta rtlm_{jc} + \mathbf{X}_{jc}\pi + \gamma_c + \epsilon_{jc}, \tag{A.1}$$

where $h_{jc}$ is the number of organized perpetrators in village $j$ in commune $c$, and $rtlm_{jc}$ the share of the village with RTLM coverage. $\mathbf{X}_{jc}$ is a vector of propagation controls,[9] as well as pre-genocide village characteristics. Furthermore, $\gamma_c$ is a commune fixed effect and $\epsilon_{jc}$ the error term.

[8] We 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.

[9] These 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

$$post\_y_{ijc} = \alpha' + \beta' rtlm_{jc} + \mathbf{X}_{jc}\pi' + \gamma_c + \epsilon_{ijc}, \tag{A.2}$$

where $post\_y_{ijc}$ is the post-genocide outcome of household $i$ (or individual $i$) 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.[10]

### A.5.3 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.35 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.5.4 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

[10] Note 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.36 provides the results. All outcomes above are normalized by the total number of killed siblings. The point estimates confirm that RTLM-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.5.5 RTLM Violence – Political Office, Public Goods, and Political Violence

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.37 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 elected politicians' education levels (regressions 2 and 3). Consistently, the results in Table A.38 suggest that if anything public goods provision is lower. Finally, Table A.39

suggests that men-led cells are significantly more violent. A 10 percent increase in local genocide violence leads to an almost 40 percent increase in political violence (regression 1). Regressions 2 to 4 split political violence into different time intervals. The effects start to become visible from around 2005 onwards.

### A.5.6 Alternative Channels

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.40 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.41 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.[11] 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. Thus, selective migration is unlikely to matter.

Finally, another alternative channel 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.41 rules out this possibility. The effect of RTLM radio coverage on the RPF vote share is small and insignificant.

[11] These 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 RTLM-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.5.7 Tables

**Table A.35:** First Stage – RTLM

| Dependent Variable | # Militiamen, log | | | Mass Graves |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| RTLM Radio Coverage | 0.545 | 1.121 | 1.028 | −0.120 |
| | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.36:** Local Violence – Targeted Killings

| Dependent Variable | Fraction Females | Fraction Vulnerable | Fraction Children | Fraction Female Children |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| RTLM Radio Coverage | 0.052 | 0.115 | 0.131 | 0.074 |
| | (0.060) | (0.068) | (0.067) | (0.039) |
| | (0.057) | (0.055) | (0.059) | (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 | 0.160 | 0.183 | 0.104 |
| | (0.092) | (0.124) | (0.119) | (0.064) |
| | (0.099) | (0.122) | (0.103) | (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.17. Standard errors in parentheses are clustered at district level and commune level.

**Table A.37:** Local Violence – Women's Political Engagement

| Sample | Women & Men | Women | Men |
|---|---|---|---|
| Dependent Variable | Fraction of Elected Women | Fraction With Primary Education | |
| | (1) | (2) | (3) |
| RTLM Radio Coverage | −0.021 | 0.004 | 0.015 |
| | (0.019) | (0.044) | (0.047) |
| Propagation Controls | yes | yes | yes |
| Additional Controls | yes | yes | yes |
| Commune Effects | yes | yes | yes |
| Dep. Mean | 0.54 | 0.44 | 0.44 |
| Dep. Std.Dev. | 0.08 | 0.23 | 0.20 |
| $R^2$ | 0.21 | 0.30 | 0.29 |
| N | 1347 | 1345 | 1345 |
| IV: Local Militia Violence | −0.051 | 0.011 | 0.035 |
| | (0.052) | (0.101) | (0.110) |

Notes: Regressions are run at the cell level. Propagation Controls and Additional Controls are defined in Table A.17. Standard errors in parentheses are clustered at district level.

**Table A.38:** Local Violence – Public Goods Provision

| Dependent Variable | Road Length, log | # Health Facilities | Social Housing, log | Access To Electricity | # Primary Schools | # Secondary Schools |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| RTLM Radio Coverage | 0.143 | 0.069 | −0.041 | −0.193 | −0.299 | −0.092 |
| | (0.136) | (0.083) | (0.168) | (0.112) | (0.168) | (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 | 0.166 | −0.100 | −0.467 | −0.722 | −0.221 |
| | (0.349) | (0.223) | (0.385) | (0.371) | (0.374) | (0.387) |

Notes: Regressions are run at the cell level. Propagation Controls and Additional Controls are defined in Table A.17. Standard errors in parentheses are clustered at district level.

**Table A.39:** Post-Genocide Political Violence

| Dependent Variable | Cell Experienced Political Violence | | | |
|---|---|---|---|---|
| | 1999-2022 | 1999-2005 | 2006-2015 | 2016-2022 |
| | (1) | (2) | (3) | (4) |
| RTLM Radio Coverage | 0.084 | 0.010 | 0.041 | 0.084 |
| | (0.035) | (0.016) | (0.030) | (0.031) |
| Propagation Controls | yes | yes | yes | yes |
| Additional Controls | yes | yes | yes | yes |
| Commune Effects | yes | yes | yes | yes |
| Dep. Mean | 0.06 | 0.01 | 0.02 | 0.04 |
| Dep. Std.Dev. | 0.24 | 0.09 | 0.15 | 0.20 |
| $R^2$ | 0.11 | 0.10 | 0.10 | 0.10 |
| N | 1418 | 1418 | 1418 | 1418 |
| IV: Local Militia Violence | 0.228 | 0.027 | 0.112 | 0.229 |
| | (0.148) | (0.042) | (0.074) | (0.167) |

Notes: Notes: Regressions are run at the cell level. Propagation Controls and Additional Controls are defined in Table A.17. Standard errors in parentheses are clustered at district level.

**Table A.40:** Local Violence – Excluding Potential Victims

| Dependent Variable | Domestic Violence Attitudes | Violence Against Kids Attitudes | Education, Wealth, Health | Financial Autonomy | Physical Autonomy | Decision Power | Sexual Knowledge, Fertility | Domestic Violence: All | Domestic Violence: Severe | Domestic Violence: Sexual | Domestic Violence: Less Severe |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Panel A – Excl. Victims | | | | | | | | | | | |
| RTLM Radio Coverage | 1.507 | 0.482 | −0.284 | 0.014 | −0.423 | 0.031 | −0.338 | −0.799 | −0.111 | −0.037 | −0.688 |
| | (0.804) | (0.404) | (0.673) | (0.219) | (0.272) | (0.149) | (0.174) | (1.104) | (0.417) | (0.352) | (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 | 0.429 | −0.287 | 0.014 | −0.424 | 0.018 | −0.338 | −0.752 | −0.081 | 0.010 | −0.671 |
| | (0.785) | (0.401) | (0.666) | (0.225) | (0.278) | (0.141) | (0.159) | (1.128) | (0.430) | (0.362) | (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.17. Standard errors in parentheses are clustered at district level.

**Table A.41:** 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 |
|---|---|---|---|---|---|---|---|---|---|---|
| Dependent Variable | Consumption | | | | | | Agricultural Income | | | RPF Vote Share |
| | Total | Food | Total | Food | Total | Food | Total | Total | Total | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| RTLM Radio Coverage | 0.736 | 0.248 | 0.704 | 0.276 | 0.954 | 0.630 | 0.483 | 0.492 | 0.745 | −0.050 |
| | (0.237) | (0.116) | (0.244) | (0.125) | (1.150) | (0.889) | (0.204) | (0.283) | (1.299) | (0.042) |
| Propagation Controls | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Additional Controls | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Commune Effects | 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.17. Standard errors in parentheses are clustered at district level.

## A.6 Extensions to Section 9: Outlook

The male replacement is documented in Table A.42. 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.[12] 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.[13]

## References

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

[12] The results are robust to restricting the politician age range from 25 to 85.

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

**Table A.42:** Age and Gender Composition – Over Time

| Dependent Variable | Female Head | | Fraction Adult Male | | Fraction Male Ages 18 to 85 | |
|---|---|---|---|---|---|---|
| Sample | DHS from | | | | | |
| | 2005 | 2010/15 | 2005 | 2010/15 | 2005 | 2010/15 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Distance × Rainfall, 1994 | −0.048 | 0.018 | −0.008 | −0.005 | −0.009 | −0.006 |
| | (0.015) | (0.013) | (0.009) | (0.008) | (0.009) | (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 | −0.023 | 0.014 | 0.006 | 0.017 | 0.008 |
| | (0.045) | (0.019) | (0.017) | (0.010) | (0.018) | (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).

## A.7 Extensions to Section 10: Alternative Channels

We also rule out a number of potential alternative channels.

**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.[14] Regressions 1 to 8 in Table A.43 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).[15]

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

[14] At 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.

[15] The 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.

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 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 channel, 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 6. 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 channel 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.43 rules out this possibility.[16] 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 channel 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.44 shows that there is no correlation between the instrument and years of schooling, a

[16]The 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.

good proxy for adult education. This is true both for the whole sample and only women.[17] 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 channel we should find similar effects in RTLM villages, especially so, since civilian perpetrators in these villages likely participated out of their own initiate and were thus even more prone to violence (the repercussions when refusing to participate when the militia gave the order, were likely worse).

**Selective Migration** A final channel 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.44 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.45 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.46 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.

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

**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.[18]

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

[18]On 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.

sex shortage should force the other sex to marry lower-quality partners (Becker, 1981).[19] Put differently, the male shortage that lasted in high-violence villages for about 20 years should have forced women to marry less attractive men.[20] 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.47 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).[21]

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.48 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 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

[19]Note 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.

[20]Attractiveness can hereby refer to education, occupation, or engagement in domestic violence, etc.

[21]Note 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.

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.43:** 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 | −0.004 | 0.028 | 0.031 | −0.028 | 0.056 | 0.027 | −0.039 | 0.199 | −0.005 |
| | (0.080) | (0.013) | (0.063) | (0.052) | (0.047) | (0.059) | (0.012) | (0.062) | (0.752) | (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 | 0.005 | −0.037 | −0.040 | 0.037 | −0.073 | −0.035 | 0.051 | −0.261 | 0.016 |
| | (0.106) | (0.018) | (0.081) | (0.067) | (0.061) | (0.079) | (0.017) | (0.082) | (0.978) | (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).

**Table A.44:** Armed-Group Violence – Human Capital, Adults

| Dependent Variable | Years of Schooling | | | | | |
|---|---|---|---|---|---|---|
| Sample | Full Sample | | In Village During Genocide | | Moved in After Genocide | |
| | Females | All | Females | All | Females | All |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Distance × Rainfall, 1994 | 0.181 | 0.154 | −0.038 | 0.222 | 0.462 | −0.254 |
| | (0.151) | (0.157) | (0.130) | (0.118) | (0.611) | (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 | −0.224 | 0.064 | −0.344 | −0.400 | 0.250 |
| | (0.250) | (0.253) | (0.216) | (0.223) | (0.548) | (0.474) |

Notes: The regressions use adults, thus older than 24 years. 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. 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).

**Table A.45:** Armed-Group Violence – Consumption/Income

| Dependent Variable | Consumption | | | | | | Agricultural Income | | |
|---|---|---|---|---|---|---|---|---|---|
| Sample | 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 | Food | Total | Total | Total |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Distance × Rainfall, 1994 | 0.138 | 0.125 | 0.192 | 0.124 | 0.114 | 0.195 | 0.265 | 0.240 | 0.419 |
| | (0.119) | (0.082) | (0.098) | (0.080) | (0.214) | (0.103) | (0.142) | (0.127) | (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 | −0.191 | −0.291 | −0.188 | −0.148 | −0.253 | −0.266 | −0.244 | −0.452 |
| | (0.163) | (0.116) | (0.124) | (0.114) | (0.271) | (0.136) | (0.143) | (0.127) | (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).

**Table A.46:** 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. |
|---|---|---|---|---|---|---|---|---|
| Dependent Variable | Migrant With Some Educ. | Migrant With At Least Primary Educ. | Migrant | | Migrant With Some Educ. | Migrant With At Least Primary Educ. | Migrant | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Distance × Rainfall, 1994 | 0.002 | 0.004 | 0.002 | 0.003 | −0.004 | −0.000 | −0.003 | 0.013 |
| | (0.006) | (0.004) | (0.010) | (0.012) | (0.005) | (0.005) | (0.007) | (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 women 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, Conley (1999).

**Table A.47:** 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 | −0.034 | −0.147 | 0.100 | 0.147 | −0.041 | −0.115 | −0.960 | −0.476 | −0.019 | −0.483 |
| | (0.040) | (0.320) | (0.284) | (0.067) | (0.130) | (0.119) | (0.115) | (0.503) | (0.205) | (0.139) | (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 | 0.063 | 0.281 | −0.192 | −0.258 | 0.079 | 0.220 | 1.731 | 0.859 | 0.034 | 0.871 |
| | (0.085) | (0.590) | (0.552) | (0.157) | (0.303) | (0.222) | (0.203) | (1.147) | (0.540) | (0.256) | (0.734) |
| Panel B – DHS 6/7 | | | | | | | | | | | |
| Distance × Rainfall, 1994 | 0.012 | 0.394 | −0.473 | −0.347 | −0.281 | −0.063 | −0.334 | 2.290 | 0.894 | 0.455 | 1.397 |
| | (0.014) | (0.215) | (0.136) | (0.145) | (0.068) | (0.074) | (0.083) | (0.879) | (0.447) | (0.233) | (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 | −0.480 | 0.576 | 0.422 | 0.347 | 0.077 | 0.407 | −3.385 | −1.321 | −0.673 | −2.064 |
| | (0.015) | (0.315) | (0.146) | (0.158) | (0.100) | (0.090) | (0.135) | (1.253) | (0.616) | (0.324) | (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).

**Table A.48:** 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 | −0.190 | 0.023 | −0.001 | −0.072 | 0.026 |
| | (0.546) | (0.436) | (0.291) | (0.017) | (0.034) | (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 | 0.243 | −0.028 | 0.001 | 0.090 | −0.033 |
| | (0.700) | (0.585) | (0.368) | (0.022) | (0.036) | (0.022) |
| Panel B – All Women under 18 | | | | | | |
| Distance × Rainfall, 1994 | −0.304 | −0.157 | −0.442 | −0.021 | −0.101 | 0.003 |
| | (0.303) | (0.181) | (0.270) | (0.006) | (0.034) | (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 | 0.190 | 0.547 | 0.026 | 0.125 | −0.004 |
| | (0.362) | (0.208) | (0.277) | (0.009) | (0.032) | (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).