Online Appendix
Mobilizing the Masses for Genocide
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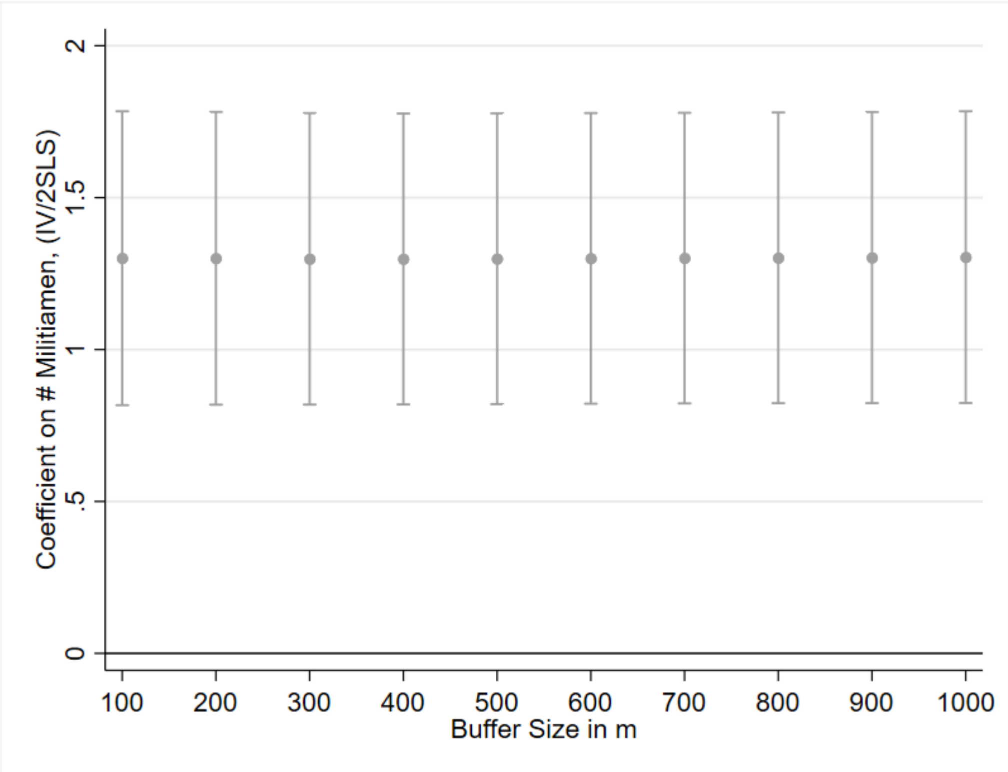
A.1 Additional Tables and Figures

Table A.1: Clustered Standard Errors

Dependent Variable:	# Militiamen, log		# Civilian Perpetrators, log	
	First Stage	Reduced Form	OLS	IV/2SLS
	(1)	(2)	(3)	(4)
Distance × Rainfall along Buffer, 1994	−0.509	−0.661		
Clustered, Commune Level	(0.144)	(0.162)		
Clustered, District Level	(0.144)	(0.162)		
Clustered, Province Level	(0.118)	(0.171)		
Bootstrap, District	0.006	0.002		
Bootstrap, Province	0.002	0.002		
# Militiamen, log			0.626	1.299
			(0.039)	(0.244)
			(0.047)	(0.291)
			(0.069)	(0.236)
			0.000	0.001
			0.000	0.011
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
R^2	0.50	0.58	0.74	.
N	1432	1432	1432	1432

Notes: The first standard errors reported under each coefficient are clustered at the commune level, the second clustered at the district level, the third at the province level. The fourth and fifth entry under each coefficient are p-values using a wild bootstrap to account for the small number of district/province clusters. **Distance × Rainfall along Buffer, 1994** is the instrument (distance to the main road interacted with rainfall along the way 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 calender 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 calender 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, 10 year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the road in the sample and the actual distance to the 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. In each column I also control for main effects and double interactions. There are **142 communes**, **30 districts**, and **11 provinces** in the sample.

Figure A.1: Varying the Buffer Length



Notes: The figure shows the coefficient on the number of militiamen (together with 95 percent confidence intervals) from regression 6 in Panel B in Table 3 when varying the buffer size used for calculating rainfall along the way between village and main road from 100 meters to 1000 meters in increments of 100 meters.

Table A.2: Armed-Group Violence – Age and Gender Composition (Full Sample)

	Female Head		Widow in Household		Fraction Male		Fraction Age: 18-49		Fraction Male Age: 18-49	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance × Rainfall along Buffer, 1994	−0.061	−0.058	−0.053	−0.046	0.024	0.018	0.010	0.010	0.016	0.015
	(0.021)	(0.017)	(0.022)	(0.019)	(0.007)	(0.006)	(0.010)	(0.010)	(0.006)	(0.006)
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	no	yes	no	yes	no	yes	no	yes	no	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.32	0.32	0.29	0.29	0.46	0.46	0.39	0.39	0.18	0.18
Dep. Std.Dev.	0.47	0.47	0.45	0.45	0.23	0.23	0.23	0.23	0.19	0.19
R^2	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.01
N	5255	5255	5255	5255	5255	5255	5255	5255	5255	5255
IV estimate	0.136	0.105	0.118	0.084	−0.053	−0.032	−0.023	−0.018	−0.036	−0.027
	(0.065)	(0.044)	(0.063)	(0.041)	(0.028)	(0.016)	(0.026)	(0.020)	(0.022)	(0.016)

Notes: All fractions correspond to the EICV1 household level, e.g. regressions 9 and 10 use the fraction of household members which are male and between 18 and 49 years old. Female Head and Widow in Household are dummy variables. The **IV estimate** scales the reduced form with the number of Militiamen. **Standard Controls**, **Growing Season Controls**, and **Additional Controls** are defined in Table 3. 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).

Table A.3: Local Violence – Age and Gender Composition (Full Sample)

	Fraction Age: 13-49		Fraction Male Age: 13-49		Female Head		Widow in Household		Fraction Male	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Radio Coverage in Village	0.109	0.112	0.106	0.107	−0.030	−0.051	−0.034	−0.051	0.055	0.061
	(0.033)	(0.033)	(0.053)	(0.049)	(0.145)	(0.126)	(0.116)	(0.097)	(0.060)	(0.051)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	no	yes	no	yes	no	yes	no	yes	no	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.53	0.53	0.24	0.24	0.32	0.32	0.30	0.30	0.46	0.46
Dep. Std.Dev.	0.23	0.23	0.21	0.21	0.47	0.47	0.46	0.46	0.23	0.23
R^2	0.07	0.07	0.05	0.05	0.05	0.06	0.05	0.05	0.04	0.04
N	4039	4039	4039	4039	4039	4039	4039	4039	4039	4039
IV estimate	0.216	0.219	0.210	0.208	−0.060	−0.099	−0.068	−0.100	0.109	0.119
	(0.266)	(0.286)	(0.241)	(0.263)	(0.275)	(0.269)	(0.241)	(0.247)	(0.139)	(0.156)

Notes: All fractions correspond to the EICV1 household level, e.g. regressions 3 and 4 use the fraction of household members which are male and between 13 and 49 years old. Female Head and Widow in Household are dummy variables. The **IV estimate** scales the reduced form with the number of Militiamen. **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 **128 communes** in the sample. **Standard errors** in parentheses are clustered at district level.

Table A.4: Armed-Group Violence – Compliers I

Dependent Variable:	# Militiamen, log							
Sample:	Population Density		Rain-fed Production		Capital City		RPF Presence	
	High	Low	High	Low	Far	Close	Long	Short
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance × Rainfall along Buffer, 1994	−0.314	−0.507	−0.438	−0.714	−0.294	−0.653	−0.485	−0.323
	(0.086)	(0.238)	(0.173)	(0.158)	(0.211)	(0.078)	(0.122)	(0.183)
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	0.56	0.48	0.47	0.56	0.46	0.63	0.63	0.35
N	716	716	716	716	716	716	659	773

Notes: The samples are defined in the column headings. The full sample is split at the median for the variables population density in **regressions 1 and 2**, long-term rainfall during the growing seasons in the village in **regressions 3 and 4**, distance to the capital Kigali in **regressions 5 and 6** and finally number of days with RPF presence in **regressions 7 and 8**. **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 calender 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 calender 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, 10 year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the road in the sample and the actual distance to the 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).

Table A.5: Local Violence – Compliers I

Dependent Variable:	# Militiamen, log							
Sample:	Population Density		Rain-fed Production		Capital City		RPF Presence	
	High	Low	High	Low	Far	Close	Long	Short
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage in Village	0.552	0.623	0.430	0.784	0.578	0.216	0.587	0.321
	(0.278)	(0.608)	(0.204)	(0.469)	(0.213)	(0.411)	(0.428)	(0.181)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.60	0.58	0.51	0.66	0.48	0.69	0.69	0.47
N	528	529	528	529	528	529	511	546

Notes: The samples are defined in the column headings. The full sample is split at the median for the variables population density in **regressions 1 and 2**, long-term rainfall during the growing seasons in the village in **regressions 3 and 4**, distance to the capital Kigali in **regressions 5 and 6** and finally number of days with RPF presence in **regressions 7 and 8**. **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 **128 communes** in the full sample. **Standard errors** in parentheses are clustered at district level.

Table A.6: Armed-Group Violence – Compliers II

Dependent Variable:	# Militiamen, log									
Sample:	Male HH		Fraction 13-49		Fraction Male, 13-49		Fraction 18-49		Fraction Male, 18-49	
	High	Low	High	Low	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance × Rainfall along Buffer, 1994	−0.325	−0.549	−0.284	−0.568	−0.276	−0.672	−0.366	−0.540	−0.267	−0.637
	(0.132)	(0.092)	(0.163)	(0.149)	(0.158)	(0.212)	(0.173)	(0.175)	(0.157)	(0.204)
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
R^2	0.44	0.60	0.43	0.50	0.42	0.52	0.38	0.53	0.38	0.53
N	715	717	711	721	715	717	713	719	708	724

Notes: The samples are defined in the column headings. The full sample is split at the median for the fraction of households with a male head in **regressions 1 and 2**, the average fraction of household members aged 13 to 49 in **regressions 3 and 4**, the average fraction of male household members aged 13 to 49 in **regressions 5 and 6** and finally the same as before with a different age cutoff (18 to 49) in **regressions 7 to 10**. **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 calender 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 calender 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, 10 year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the road in the sample and the actual distance to the 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).

Table A.7: Local Violence – Compliers II

Dependent Variable:	# Militiamen, log									
Sample:	Male HH		Fraction 13-49		Fraction Male, 13-49		Fraction 18-49		Fraction Male, 18-49	
	High	Low	High	Low	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Radio Coverage in Village	0.382	0.942	0.343	0.601	0.380	0.603	0.435	0.555	0.351	0.623
	(0.322)	(0.374)	(0.385)	(0.319)	(0.402)	(0.254)	(0.386)	(0.311)	(0.393)	(0.250)
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	0.56	0.63	0.38	0.58	0.42	0.58	0.39	0.58	0.41	0.59
N	525	532	528	529	520	537	522	535	528	529

Notes: The samples are defined in the column headings. The full sample is split at the median for the fraction of households with a male head in **regressions 1 and 2**, the average fraction of household members aged 13 to 49 in **regressions 3 and 4**, the average fraction of male household members aged 13 to 49 in **regressions 5 and 6** and finally the same as before with a different age cutoff (18 to 49) in **regressions 7 to 10**. **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 **128 communes** in the full sample. **Standard errors** in parentheses are clustered at district level.

Table A.8: EICV Sample – First Stage

Dependent Variable	# Militiamen, log			
Sample	Full	EICV	Full	EICV
	(1)	(2)	(3)	(4)
RTLM Coverage in Village	0.545	0.496		
	(0.229)	(0.880)		
Distance × Rainfall along Buffer, 1994			−0.509	−0.649
			(0.115)	(0.168)
Standard Controls	no	no	yes	yes
Growing Season Controls	no	no	yes	yes
Additional Controls	no	no	yes	yes
Province Effects	no	no	yes	yes
Propagation Controls	yes	yes	no	no
Additional Radio Controls	yes	yes	no	no
Commune Effects	yes	yes	no	no
R^2	0.55	0.73	0.50	0.49
N	1065	326	1432	411

Notes: Regressions 1 and 2 use the RTLM sample, regressions 3 and 4 use my sample. **Distance × Rainfall along Buffer, 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). **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 Radio Controls** include distance to the road, distance to the border, distance to major city, population and population density, and sloping dummies. **Standard Controls**, **Growing Season Controls**, and **Additional Controls** are defined in Table 3 in the paper. All control variables from my sample, except "Number of Days with RPF presence," are in logs. Interactions are first logged and then interacted. There are **11 provinces** and **128 communes** in the sample. In regressions 1 and 2 **standard errors** are clustered at the district level. In all other regressions **standard errors** are corrected for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

A.2 Extensions to Section II. – Data

A.2.1 Data Matching

I combine several datasets from different sources to construct the final dataset, which comprises 1,433 Rwandan villages. The different datasets are matched by village names within communes.[1] Unfortunately, the matching is imperfect, as some villages either have different names in different data sources, or use multiple spellings. However, overall only about 5 percent of the villages do not have a clear match across all sources. Furthermore, these issues are likely idiosyncratic, resulting in less precise estimates.

A.2.2 Participation in Violence

The two key measures are participation in armed-group violence and participation in civilian violence. Since no direct measure of participation is available, I use prosecution numbers for crimes committed during the genocide as a proxy. Importantly, individuals were prosecuted in the village where they committed the crime (individuals did not have to be present in that village to be prosecuted). Depending on the role played by the accused, two categories of criminals are identified by the courts.

Category 1 includes perpetrators that mostly belong to the army and the militia or are members of local armed groups such as policemen, thus I consider this to represent armed-group violence. There were approximately 77,000 prosecution cases in this category (Figure A.2). Note that this number does not necessarily equal the number of people involved. Consistent with organized perpetrators moving around, there are cases where people were prosecuted in multiple locations. Since external militiamen were thus likely prosecuted in absence they could not have simply accused civilians to positively affect their own verdict. The legal definition of category 1 includes: 1) planners, organizers, instigators, supervisors of the genocide; 2) leaders at the national, provincial or district level, within political parties, army, religious denominations or militia; 3) the well-known murderer who distinguished himself because of the zeal that characterized him in the killings or the excessive wickedness with which killings

[1]A commune is an administrative unit above the village. There were 142 communes in total, which were in turn grouped into 11 provinces.

were carried out; and 4) people who committed rape or acts of sexual torture.

The legal definition for category 1 also comprises rapists and torturers who may have been civilians. However, civilians being falsely classified as militiamen in the data, would only work against my findings and bias the estimates downwards.[2] Besides, anecdotal evidence suggests that the especially gruesome and cruel killings (including sexual violence and rape) were in fact committed by militia members. The vast majority of civilian killers did not seem to sadistically enjoy the killings (Hatzfeld, 2005).

People accused in category 2 are not members of any of the organized groups mentioned in category 1 and I therefore label this category civilian violence. Approximately 430,000 prosecution cases were handled in this category (Figure A.3). The legal definition of category 2 includes: 1) authors, co-authors, accomplices of deliberate homicides, or of serious attacks that caused someone's death; 2) the person who – with the intention of killing – caused injuries or committed other serious acts of violence, but without actually causing death; and 3) the person who committed criminal acts or became the accomplice of serious attacks, without the intention of causing death.

The reliability of the prosecution data is a key issue for the analysis. In light of the chaos in the aftermath of the genocide, one might wonder about the general quality of the Gacaca data. Reassuringly, the Gacaca courts have been very thorough in investigating the various prosecution cases, taking about six years to complete their work (the first courts were set up in 2001).[3] Besides, since much of the violence was highly localized people knew the perpetrators well and could easily identify them later on in the prosecution process. Friedman (2010, p. 21) notes that *"reports of those afraid to speak are rare, so this data is likely to be a good proxy for the number of participants in each area."* And even identifying external militiamen was possible because they wore distinctive uniforms indicating their area of origin (Des Forges, 1999). *"A survivor of that massacre identified the party affiliation of the assailants from their distinctive garb, the blue and yellow print boubou of the Interahamwe and the black, yel-*

[2]Reducing the number of militiamen and increasing the number of civilians in the data rotates the first-stage line counterclockwise and the reduced-form line clockwise, which implies larger instrumental-variables estimates. Naturally, villages with many prosecuted militiamen were, if at all, more likely to have some falsely classified civilians.

[3]As an aside, note that the courts' actions (starting in 2001) are therefore unlikely to be correlated with the instrument which uses rainfall from 1994.

low, and red neckerchiefs and hats of the Impuzamugambi. He could tell, too, that they came from several regions." Des Forges (1999, p. 180). Thus after cross-checking these accusations with other villages, the local courts were able to also prosecute external perpetrators.

As a first reliability test of the data, Friedman (2010) shows that the Gacaca data is positively correlated with several other measures of violence from three different sources.[4] Nevertheless, some measurement error may always exist but would only matter as much as it is correlated with the instrument. In particular, random measurement error and allegations that these courts were occasionally misused to settle old scores, resulting in false accusations do not pose any major threat because I am instrumenting for armed-group violence.

However, non-classical measurement error may matter. One potentially important case is the presence of survival bias: in those villages with high participation, the violence might have been so widespread that no witnesses were left to accuse the perpetrators, resulting in low prosecution rates. Another concern is that villages with no reported armed-group violence might have actually received militiamen, but unsuccessful ones. To ease all these concerns that some systematic, non-classical measurement error is biasing the results, I will present several additional tests in the following section.

A.2.3 Reliability of the Gacaca Data

In this section, I perform a number of tests to ease concerns that measurement error leads to any systematic biases (random measurement error does not pose a threat). As noted above, one potentially important case is the presence of survival bias: in those villages with high participation, the violence might have been so widespread that no witnesses were left to accuse the perpetrators, resulting in low prosecution rates. This would be particularly worrisome if it happened in places that were potentially landlocked due to heavy rains and people could not flee.

In general, large transport costs were unlikely to hinder the Tutsi from escaping, since they avoided the main roads as road blocks were set up throughout the country (Hatzfeld, 2005). Thus, their movements should not be correlated

[4]These sources are a database from Davenport and Stam (2009), the PRIO/Uppsala data (Gleditsch et al., 2002), and a report from the Ministry of Higher Education (Kapiteni, 1996).

with the instrument. If anything they should have been more likely to flee if the road had been blocked preventing the militia to arrive – however this would only work against my findings. To nevertheless ease the concern of survival bias, I show that the results are robust to using several alternative measures of genocide violence, in particular actual death data which does not suffer from the same prosecution reporting bias.

Child Mortality As a first alternative genocide violence outcome, I use child mortality data from the Rwandan EICV household survey from 2000/01 introduced above. The dependent variable is the share of children in the household that died, as reported by mothers of age 16 and above at the time of the genocide. Note that there is no age restriction on children – thus, depending on the age of the mother, some of the children are adults themselves.[5] Regression 1 in Table A.9 shows that the instrument indeed negatively maps into child mortality. Reassuringly, child mortality is only affected for individuals that experienced the genocide in their village (regression 2). For individuals that only later moved to their surveyed location the effect is small and insignificant (regression 3). Furthermore, consistent with the armed-group violence targeted especially at males, these negative effects seem to be driven by male mortality (regressions 4 and 5).

Regarding magnitude, the point estimate of -0.063 (standard error 0.008) for males suggests that a village with an average distance to the main road has about 15 percent lower male mortality rates, following a one standard-deviation increase in rainfall between village and main road. This effect is very similar to the ones obtained using the Gacaca prosecution data (for militiamen this number ranges between 20 and 30 percent).[6]

Deaths at Commune Level As another alternative outcome variable for genocide violence, I use data on Tutsi death estimates at the commune level. Unfortunately, this data is not available for my whole sample of villages. Nevertheless, regressions 1 to 3 in Table A.11 show that armed groups' transport costs neg-

[5] Results are robust to varying the mother cutoff age (regressions 1 to 5 in Table A.10).

[6] These results are also robust to controlling for average rainfall (years 1995 to 2001) 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. Recall that the survey data is from 2001, thus rainfall between 1995 and 2001 may be a confounder. Results can be found in regressions 6 to 10 in Table A.10.

atively map into Tutsi deaths (since the data is only available at the commune level I simply assume that each village (within a given commune) receives an equal share of deaths). In regressions 4 to 6, I then rerun the analysis at the commune level. Although the number of observations drop significantly, armed groups' transport costs is still strongly negatively related to the number of Tutsi deaths. The data on the commune-level death estimates is provided by Genodynamics. These estimates are generated using a Bayesian latent variable model on information from five different sources: the Ministry of Education in Rwanda, the Ministry of Local Affairs in Rwanda, Ibuka (the Rwandan survivor organization), African Rights, and Human Rights Watch.[7]

DHS Data Sibling Deaths As yet another outcome variable, I use data from the three latest DHS Rounds (2005, 2010 and 2015). All three rounds contain a question on how many years ago an individual's sibling died (NISR et al., 2016, 2012; INSR et al., 2006). First, I show that 1994 is a clear outlier in the distribution of deaths, around 25 percent of all reported deaths happened in that year. The average for all other years is about 3.7 percent (Figure A.4). Second, I show that my instrument is negatively related to the number of deaths only for 1994 (in all three rounds). For all other years, the point estimate on the instrument oscillates around zero (Figure A.5). Note that the effects for 1994 get slightly weaker over time (i.e. the different DHS rounds), this is unsurprising since migration is likely to create measurement error and bias the point estimate downwards (unfortunately, the DHS data for Rwanda does not allow me to identify whether an individual experienced the genocide in the survey location). The results not only confirm that my instrument has a strong effect on genocide deaths but also show that it does not have an effect on other deaths.

Mass Graves Moreover, recall from the discussion on RTLM violence above that the militia's transport costs also negatively map into whether a mass grave site was found in the village (I re-report the regression in column 6 in Table A.9). The point estimate of -0.035 suggests that a village with an average distance to the main road is 37 percent less likely to have a mass grave site, given a one standard-deviation increase in rainfall between village and main road (recall the magnitudes for armed-group violence from the Gacaca data: 20 to 30 percent).

[7] For more details see: https://genodynamics.weebly.com.

Besides, by dropping those villages with mass grave sites (i.e. villages with high death rates) and rerunning the baseline regression, I provide another test for ruling out the presence of potential survival bias in the prosecution data. Importantly, the instrumental-variables point estimates are virtually identical to the baseline results and similarly significant at the 99 percent confidence level (regression 7). Since especially small Tutsi minorities might have been at risk of being completely wiped out (and thus unable to identify the perpetrators), in the next regression I only drop villages with both a mass grave site and a small Tutsi minority. Again, the results are very similar to the baseline numbers (regression 8). Finally, the results are also robust to dropping villages less than 3.5 kilometers away from a mass grave location, reducing the sample size by about 10 percent (regression 9).

Underreporting Next, potential underreporting of unsuccessful militiamen, something that would bias the OLS estimates upwards, is unlikely to push up the instrumental-variables estimates as well. To see this, I add the average number of militiamen per village in the sample to those villages with zero militiamen reported (only those with a Tutsi minority) and rerun the baseline regression. The point estimate of 1.533 (standard error 0.376, regression 10) is very similar to the baseline results and if anything higher. This is unsurprising, since the reduced form is unaffected by this change and the first-stage coefficient decreases in absolute terms.[8] As a result the instrumental-variables estimates should increase.[9]

Migration Bias Another concern, besides survival bias, is potential migration bias. Towards the end of the genocide several hundreds of thousands Hutu fled Rwanda in fear of the RPF's revenge. If people's decision to flee is correlated

[8]Adding militiamen to low-violence villages, that is villages that were hard to reach, rotates the first-stage regression line counterclockwise.

[9]Besides, it seems puzzling that a genocide planner who wants to maximize civilian participation and the number of Tutsi deaths, would send ineffective militiamen specifically to villages that are hard to reach: not only are the (wasted) costs of getting there higher but the monitoring costs will certainly be higher as well. Moreover, I am not aware of any anecdotal evidence supporting the notion of lazy or unsuccessful militiamen. If anything, the contrary seems to be true: in Hatzfeld (2005, p. 10), a civilian killer reports that the militiamen were the *"young hotheads"* who ragged the others on the killing job. Another one continues (p. 62), *"When the Interahamwe noticed idlers, that could be serious. They would shout, We came a long way to give you a hand, and you're slopping around behind the papyrus!"*

with the instrument this might bias the results. This is particularly true if in places that are harder to reach by the militia and subsequently by the RPF, Hutu civilians are more likely to succeed in escaping. Although individuals were also prosecuted in abstenia it might still be the case that prosecutors who had escaped were not well-known (especially civilians) and thus forgotten.

Note that prosecution in absentia did happen in around 15 percent of the cases (de Brouwer and Ruvebena (2013)). One of the reasons for this was that the Gacaca courts, besides bringing perpetrators to justice, also tried to acknowledge the suffering of the victims. Thus discussing a case in the absence of the accused was part of the post-genocide reconciliation effort (Nyseht Brehm (2014)). In several cases though did perpetrators return and plead guilty (this would often lead to a reduction in their sentence). Detailed migration data from the Rwandan EICV household survey in 2000/01 (representative at national level) suggests that around 81 percent of the refugees returning to Rwanda moved back into their home village (and only 19 percent choose a new location). This is consistent with a number of studies suggesting that even after major conflict episodes, such as those in Sierra Leone or Rwanda, individual tend to move back to their homelands (Glennerster et al., 2013; UN, 1996). On a side note, the large majority – around 90 to 92 percent – of the sampled individuals in 2000/01 experienced the genocide in their surveyed location.

I present a number of results suggesting that migration is unlikely to bias my effects. First, the large majority of Hutu (and Tutsi) fled Rwanda towards the end of the genocide. Thus transport costs at the beginning of the genocide (once I control for costs towards the end) are unlikely to suffer from the same bias. In Table A.15 below, I show that my results are robust to using only rainfall for the first 5 days, first week or first 2 weeks in the excluded instrument while controlling for the remaining genocide days. Second, the RPF from which the Hutu were fleeing took over Rwanda gradually. It is thus unlikely that their movements are correlated with the instrument. In regressions 5 and 6 in Table A.12, I regress the number of days each village was under RPF control on the instrument. The point estimates are small and insignificant.

Finally, I can use detailed migration data from the Rwandan EICV household survey in 2000/01 to shed light on migration patterns.

I first identify refugees who fled Rwanda during the genocide and then returned to their home village afterwards. In regression 1 in Panel A. in Table

A.12, I show that being a refugee is uncorrelated with the instrument. One obvious caveat is that I only observe people who returned (e.g. survived) in my sample. However, once people escaped, the decision to return (after the genocide) is unlikely correlated with the instrument (using rainfall during the genocide). Besides, those refugees escaping from high cost villages were more likely to survive since the RPF could not reach them. Thus, if anything, the true relationship between transport costs and being a refugee should be negative and thus go against my findings.

For completeness, I can also show that the instrument is uncorrelated with whether an individual was internally displaced. Note that is this case I am testing whether low transport costs increased chances that an individual would move into a given village. Again, I do not expect their decisions to escape, facing death, to be the result of a rational transport cost calculation, as was the case for the militia. Consistently, the effect is small and insignificant (regression 2).

Furthermore, the results are robust to splitting the sample by gender and focusing only on adult males (regressions 1 to 6 in Panel B. in Table A.12).

A.2.4 Rainfall Data

I use the recently released National Oceanic and Atmospheric Administration (NOAA, 2010) database of daily rainfall estimates for Africa, which stretches back to 1983, as a source of exogenous weather variation (Novella and Thiaw, 2012).[10] The NOAA data relies on a combination of weather station data as well as satellite information to derive rainfall estimates at 0.1 degree ($\sim$ 11 km at the equator) latitude longitude intervals. Considering the small size of Rwanda, this high spatial resolution data is crucial to obtain reasonable rainfall variation. Furthermore, the high temporal resolution, i.e. daily estimates, allows me to confine variation in rainfall in the instrument to the exact period of the genocide.

To construct the instrument, I compute the amount of rainfall during the period of the genocide over a 500-meter buffer around the distance line between each village centroid and the closest point on the main road (Figure A.6 below illustrates this).[11] Since these buffers crisscross the various rainfall grids and each

[10] ftp://ftp.cpc.ncep.noaa.gov/fews/fewsdata/africa/arc2/bin/.

[11] Taking the village centroid to calculate distances is standard practice and furthermore very reasonable for Rwanda with its very high population density (apart from the few nature reserves – which are excluded – there is no uninhabited space in Rwanda).

distance buffer is thus likely to overlap with more than one rainfall grid, I obtain considerable variation in rainfall along each buffer.[12] Furthermore, Rwanda's hilly terrain ensures sufficient local variation in rainfall (micro-climates), i.e the clouds get stuck somewhere and it rains on one side of the mountain but not on the other. The overall rainfall in each buffer is obtained through a weighted average of the grids, where the weights are given by the relative areas covered by each grid.[13] In a similar fashion, using a village boundary map, I also compute rainfall in each village.

A.2.5 Village Map and Africover Data

The Center for Geographic Information Systems and Remote Sensing of the National University of Rwanda (CGIS-NUR) in Butare provides a village boundary map, importantly with additional information on both recent and old administrative groupings (Verpoorten, 2012). Since Rwandan villages have been regrouped under different higher administrative units a number of times after the genocide, this information allows me to match villages across different datasets (e.g. the 1991 census and the Gacaca records).

Africover (2002a, 2002b) provides maps with the location of major roads and cities derived from satellite imagery. These satellites analyze light and other reflected materials, and any emitted radiation from the surface of the earth. Since simple dirt roads have different radiation signatures than tarred roads or gravel roads, this allows to objectively measure road quality.[14]

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[12] In some cases multiple rainfall buffers fall within the same single rainfall grid. However, this only happens for about 9 percent of the villages. The location of these villages is shown in Figure A.7 below. Naturally, this happens for villages very close to the main roads.

[13] Figure A.8 maps the variation in the difference between rainfall along each buffer during the genocide in 1994 and its long-term average (years 1984-1993) for each village.

[14] Because the satellite pictures are taken a little after the genocide, towards the middle and end of the 1990s, I also cross-check the data with a Rwandan road map from 1994. Except for one road, which runs south of Kigali, all roads match. That missing road, however, was of bad quality and only upgraded sometime after 2000. Consequently, the satellites did not detect it. The results become weaker when including that road which is reasonable given the measurement error it creates.

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[11] **National Institute of Statistics of Rwanda - NISR, Ministry of Health - MOH/Rwanda, and ICF International.** 2012. Rwanda Demographic and Health Survey 2010, Calverton, Maryland, USA: NISR/Rwanda, MOH/Rwanda, and ICF International. https://dhsprogram.com/data/ (accessed January 2018).

[12] **National Institute of Statistics of Rwanda - NISR, Ministry of Finance and Economic Planning/Rwanda, Ministry of Health/Rwanda, and ICF International.** 2016. Rwanda Demographic and Health Survey 2014-15, Kigali, Rwanda: National Institute of Statistics of Rwanda, Ministry of Finance and Economic Planning/Rwanda, Ministry of Health/Rwanda, and ICF International. https://dhsprogram.com/data/ (accessed January 2018).

[13] **National Oceanic and Atmospheric Administration (NOAA)**. 2010. African Rainfall Climatology from the Famine Early Warning System. In cooperation with National Centers for Environmental Prediction (NCEP) and Climate Prediction Center (CPC), ftp://ftp.cpc.ncep.noaa.gov/fews/fewsdata/africa/arc2/bin/

[14] **Novella, Nicholas S., and Wassila M. Thiaw** (2012). African Rainfall Climatology Version 2 for Famine Early Warning Systems. *Journal of Applied Meteorology and Climatology*, 52(3): 588-606.

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Figure A.2: Armed-Group Violence (# Prosecutions)

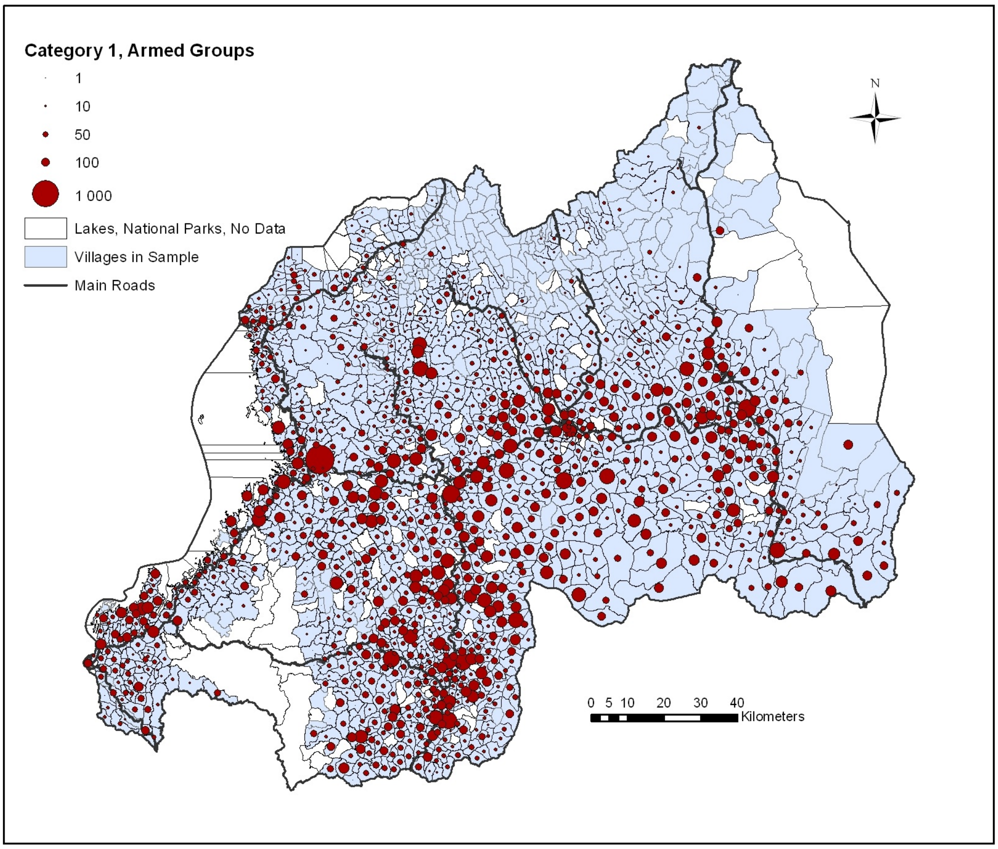


Figure A.3: Civilian Violence (# Prosecutions)

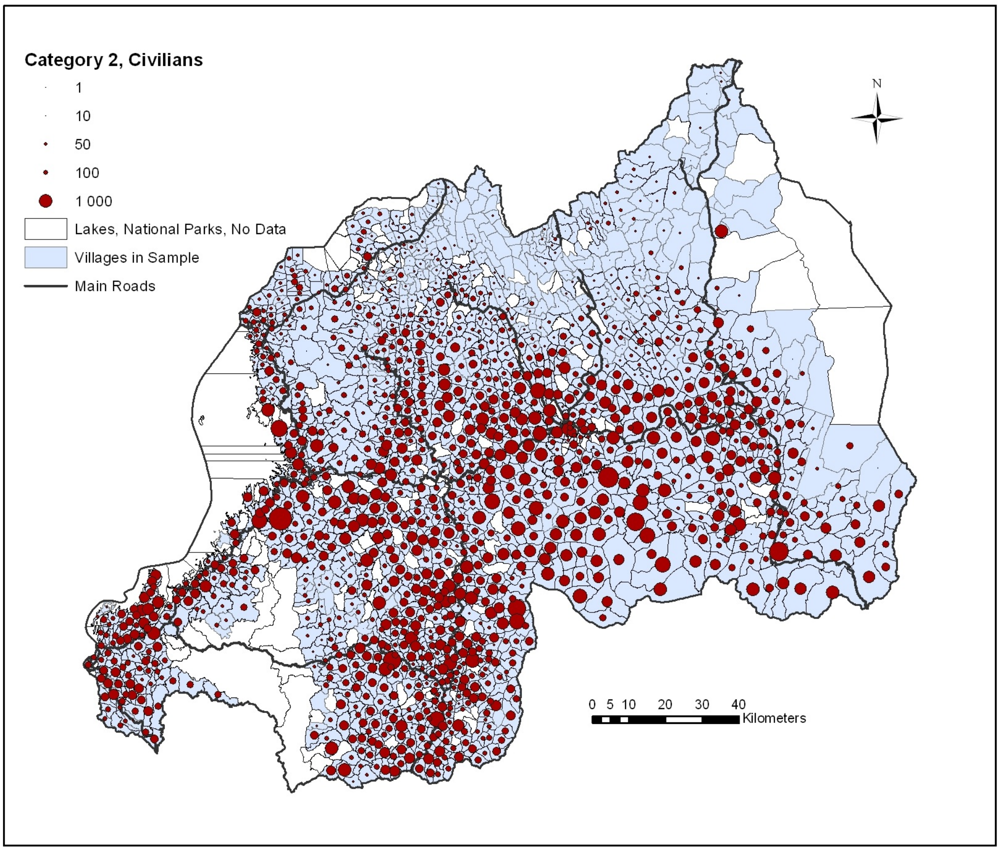


Table A.9: Reliability of the Gacaca Data

Dependent Variable:	Share of Descendents Reported Dead by Mothers					Mass Grave	# Civilian Perpetrators, log			
	Full Sample	In Village 1994	Else where 1994	Males	Females					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance × Rainfall along Buffer, 1994	−0.037	−0.044	0.006	−0.063	−0.019	−0.035				
	(0.010)	(0.013)	(0.022)	(0.008)	(0.019)	(0.012)				
# Militiamen, log							1.284	1.273	1.343	1.533
							(0.268)	(0.256)	(0.276)	(0.376)
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	0.21	0.22	0.18	0.25	0.20	0.05	290.25	286.25	291.60	290.25
Dep. Std.Dev.	0.24	0.24	0.24	0.31	0.30	0.21	286.43	286.18	286.79	286.43
R^2	0.04	0.04	0.08	0.03	0.02	0.05	.	.	.	.
N	3197	2704	501	2428	2467	1432	1366	1387	1270	1432

Notes: The data for the dependent variables in **regressions 1 to 5** is taken from the 2000/01 EICV national household survey (there are 411 villages in the full sample). The unit of observation is a household. The dependent variable is the share of children that died, as reported by mothers of age 16 and above at the time of the genocide. In **regressions 5 to 7** this share is for children of both gender. In **regression 8 (9)** it is the share of male (female) children. In **regression 6 (7)** the sample is restricted to mothers that lived (did not live) in the village at the time of the genocide. In **regression 6** the dependent variable is a dummy taking on the value of 1 if a mass grave was found in the village. In **regression 7** all villages with mass graves are dropped. In **regression 8** all villages with mass graves and small Tutsi minorities are dropped. In **regression 9** all villages at most 3.5 kilometers away from a mass grave site are dropped. In **regression 10** the average number of militiamen is added to villages with 0 reported militiamen. **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 calender 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 calender days of the genocide period (1984-1993), and the latter interacted with distance to the main road. **Growing Season Controls** and **Additional Controls** are defined in Table 3 in the paper. 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).

Table A.10: Gacaca Data Robustness

Dependent Variable:	Share of Descendents Reported Dead by Mothers									
	All Children in All Households						Males		Females	
	Mothers over 14 years	Mothers over 15 years	Mothers over 16 years	Mothers over 17 years	Mothers over 18 years	Mothers over 16 years	All Households	Lived in Village in 1994	All Households	Lived in Village in 1994
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance × Rainfall along Buffer, 1994	−0.028	−0.029	−0.037	−0.039	−0.038	−0.041	−0.053	−0.059	−0.027	−0.016
	(0.010)	(0.008)	(0.010)	(0.010)	(0.009)	(0.011)	(0.008)	(0.012)	(0.022)	(0.020)
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
Future Rainfall Controls	no	no	no	no	no	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.21	0.21	0.21	0.21	0.22	0.21	0.24	0.25	0.20	0.20
Dep. Std.Dev.	0.25	0.25	0.24	0.24	0.24	0.24	0.31	0.31	0.30	0.30
R^2	0.03	0.03	0.04	0.04	0.04	0.04	0.03	0.03	0.02	0.02
N	3375	3301	3197	3085	2968	3197	2868	2428	2874	2467

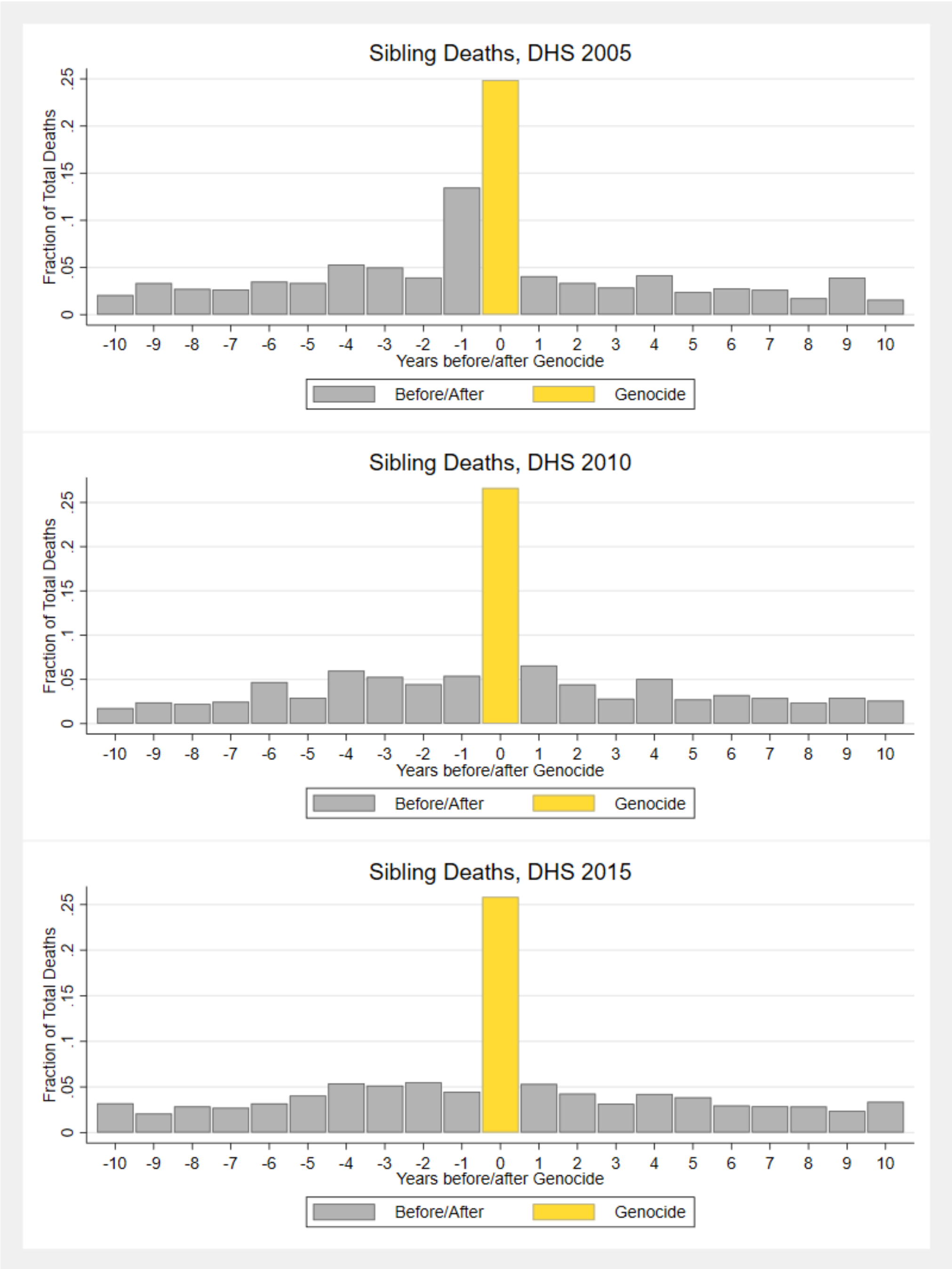
Notes: The data for the dependent variable is taken from the 2000/01 EICV national household survey. The unit of observation is a household. The **dependent variable** is the share of children that died, as reported by mothers of a certain age at the time of the genocide. In **regressions 1 to 5** the age of the reporting mothers varies (shown in the column header). In **regressions 6 to 10** it is mothers who were older than 16 at the time of the genocide. In **regressions 1 to 6** the share is for children of both gender. In **regressions 7 and 8** it is the share of male children, and in **regressions 9 and 10** it is the share of female children. In **regressions 8 and 10** the sample is restricted to households that lived in the village at the time of the genocide. There are 411 villages in the full sample. **Distance × Rainfall along Buffer, 1994** is the instrument (distance to the main road interacted with rainfall along the way 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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. **Future Rainfall Controls** are average rainfall along the way between village and main road during the 100 calendar days of the genocide period for the years 1995 to 2001 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. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Table A.11: Commune Level Deaths

Dependent Variable:	# Deaths per Village, log			# Deaths per Commune, log		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance × Rainfall along Buffer, 1994	−0.513	−0.539	−0.648	−1.439	−1.600	−1.119
	(0.171)	(0.176)	(0.178)	(0.582)	(0.602)	(0.812)
Standard Controls	yes	yes	yes	yes	yes	yes
Growing Season Controls	no	yes	yes	no	yes	yes
Additional Controls	no	no	yes	no	no	yes
Province Effects	yes	yes	yes	yes	yes	yes
R^2	0.59	0.60	0.62	0.65	0.66	0.69
N	1159	1159	1159	118	118	118

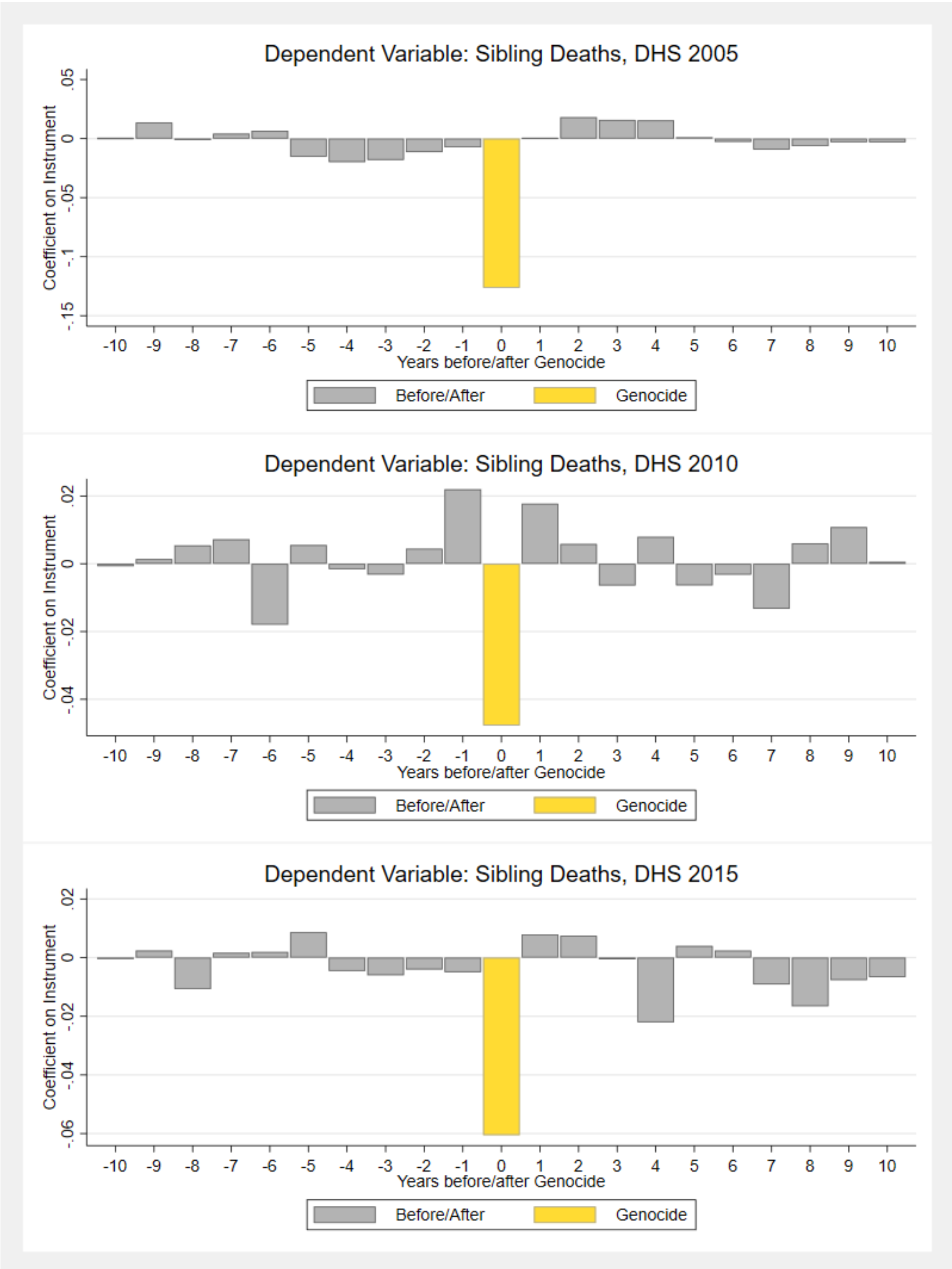
Notes: The commune-level death estimates are provided by Genodynamics. In **regressions 1 to 3** I divide the number of deaths in each commune by the number of villages. In **regressions 4 to 6** I run the regressions at the commune level (all controls are collapsed). **Standard Controls** include village 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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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** and **118 communes** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Figure A.4: Siblings Deaths, taken from DHS



Notes: The figure shows the fraction of yearly sibling deaths for the 10 years before and after the genocide (taken from three rounds of DHS data). The genocide year 1994 is normalized to 0.

Figure A.5: Siblings Deaths Regressions, taken from DHS



Notes: The figures show the distribution of coefficients on the instrument when using the number of sibling deaths for different years (taken from three rounds of DHS data) in my preferred specification (regressions 3 and 6 in Panel A in Table 3 in the paper). The genocide year is normalized to 0. None of the coefficients are larger in absolute value than the one from 1994.

Table A.12: Refugees and Internally Displaced People and RPF

Panel A. Dependent Variable:	Refugee	Internally Displaced	Number of Days Under RPF Control	
	(1)	(2)	(3)	(4)
Distance × Rainfall along Buffer, 1994	−0.001	0.010	−0.114	−0.077
	(0.013)	(0.011)	(0.073)	(0.051)
Standard Controls	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
Harvest Season Controls	no	no	no	yes
Province Effects	yes	yes	yes	yes
R^2	0.27	0.05	0.90	0.90
N	13028	12233	1432	1432

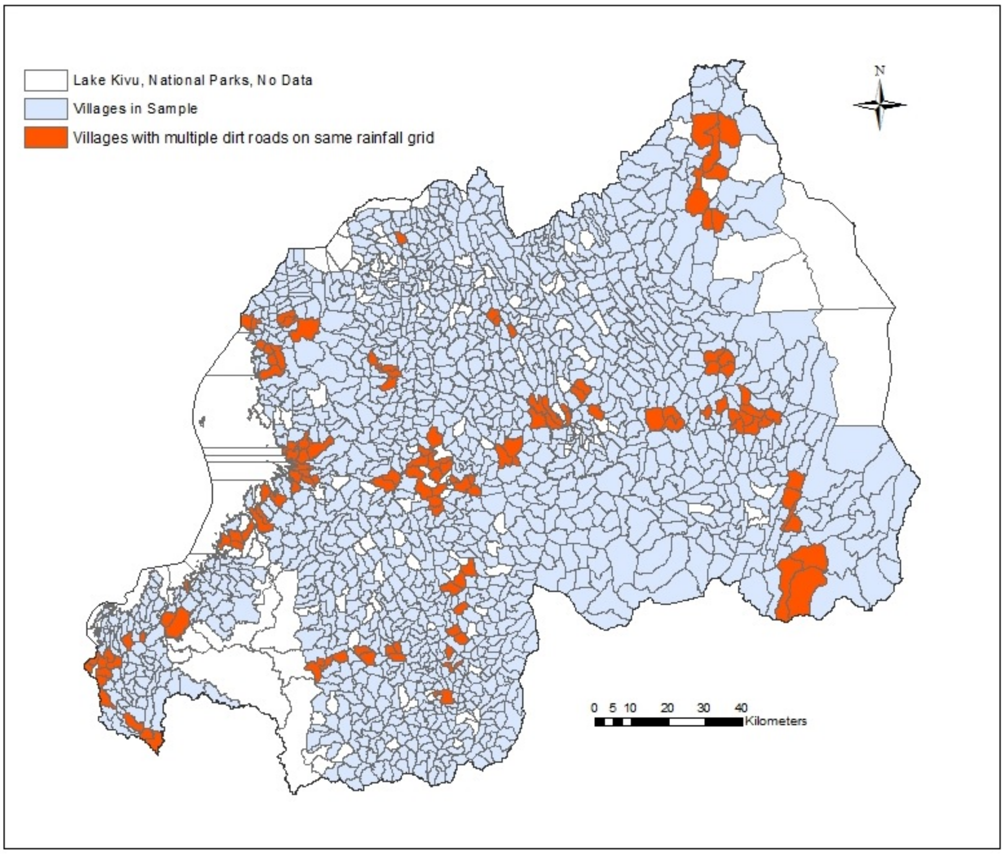
Panel B. Sample:	Females		Males		Adult Males	
Dependent Variable:	Refugee	Internally Displaced	Refugee	Internally Displaced	Refugee	Internally Displaced
	(1)	(2)	(3)	(4)	(5)	(6)
Distance × Rainfall along Buffer, 1994	0.006	0.013	−0.009	0.007	−0.014	0.004
	(0.013)	(0.011)	(0.014)	(0.011)	(0.014)	(0.008)
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
R^2	0.27	0.06	0.27	0.05	0.28	0.06
N	7234	6810	5794	5423	3849	3600

Notes: In **regression 1** the dependent variable is a dummy taking on the value of 1 if an individual was a refugee. In **regression 2** the dependent variable is a dummy taking on the value of 1 if an individual was internally displaced during the genocide. The data is taken from the EICV1 Survey and includes all individuals for which migration data is available (above 16 years old). In Panel B. I split the full sample into different subsets (given in the column header). **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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 (except for regressions 3 and 4 in Panel A.). **Harvest Season Controls** are rainfall along the way between village and main road during the harvest season 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. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Figure A.6: Construction of the Instrument in ArcGIS

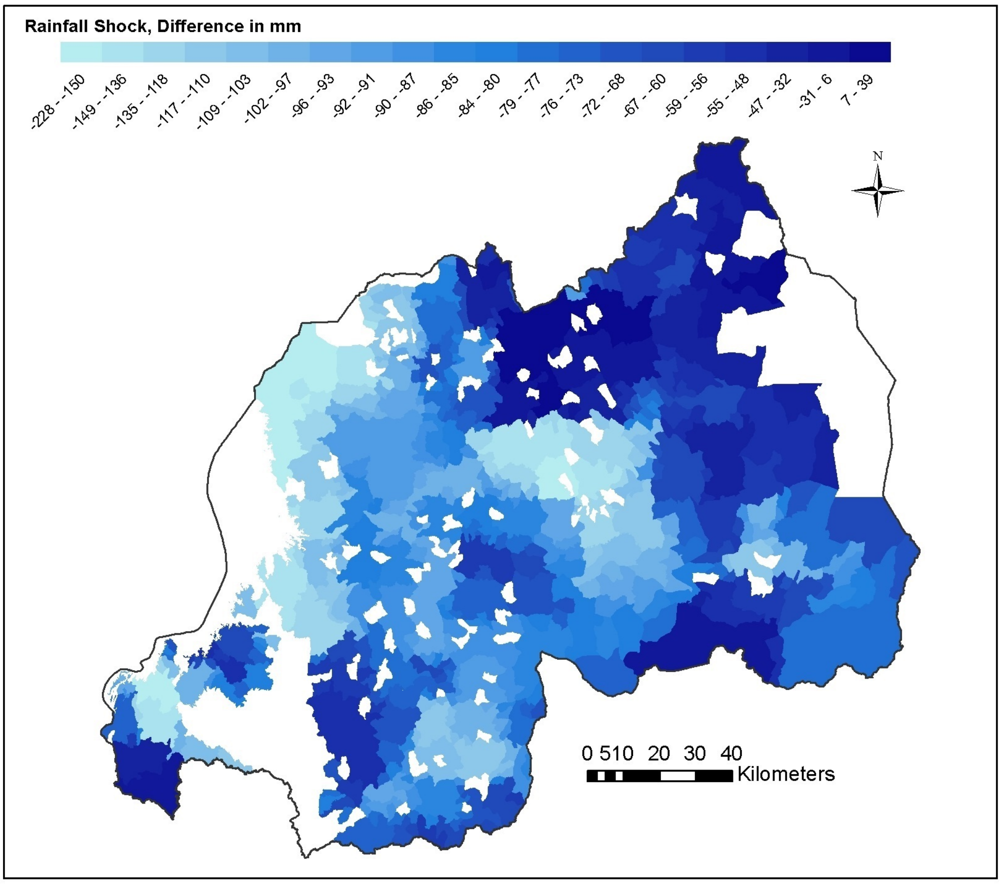
Instrument: Interaction of the length of the red line and amount of rain falling on the area of the blue rectangle during the period of the genocide.

Figure A.7: Perfect Correlation in the Instrument



Notes: The map shows the location of villages with multiple dirt roads (rainfall buffers) falling into the same single rainfall grid.

Figure A.8: Rainfall



Notes: This map shows rainfall along the way between main road and village during the period of the genocide in 1994 for each village, subtracting rainfall between main road and village during the 100 calendar days of the genocide of an average year (years 1984-1993). White areas are either national parks, Lake Kivu, or villages not in the sample.

A.3 Extensions to Section III. – Understanding the First Stage

A.3.1 Understanding the First Stage

In this section I provide some additional results on the mechanisms of the instrument. Recall that I assume that the costs of traveling along the main roads are negligible relative to the costs one has to occur when leaving those main roads, since local roads are usually non-paved dirt roads and heavy rains quickly make them very difficult to penetrate with motorized vehicles. Rain turns dirt roads into slippery mud, usually requiring expensive four wheel drives and forcing drivers to slow down; experts recommend about half the usual speed on wet dirt roads (ASIRT, 2005). Since the genocide planners were under time pressure time was costly. Furthermore, water can collect in potholes and create deep puddles or broken trees might block the road, requiring the driver to stop and clear the road or measure water depth, thus increasing travel time and costs even further.[15]

Unfortunately, my satellite data does not allow me to identify whether an individual dirt road was destroyed by the rain. However, I can provide some suggestive evidence for the channels mentioned above. In particular, rainfall on dirt roads should make traveling harder in hilly areas. In Table A.13, I interact the instrument with the slope along the way between the village and main road.[16] The interaction effect is strongly negative as expected and almost significant (regression 1). In regression 2, I interact the instrument with a dummy taking on the value of one if the slope lies above the median. Again, the interaction effect is negative and this time highly significant. Thus, it seems that rainfall increases transport costs especially in hilly areas. As a robustness check, I show that the main IV results are robust to using both my original instrument and its interaction with slope as instruments (regression 6).

Another important question is how persistent the effects of rainfall are. It

[15] Fallen trees are less of a problem for main roads since there is usually some space between road boundary and the surrounding vegetation.

[16] I show results when using maximum slope along the way between village and main road. Results are robust to using average slope, albeit weaker. This makes sense given that the steepest part should matter most. The data is downloaded from the USGS database (NASA and NGA, 2000).

might be that rainfall before the genocide mattered too if roads got permanently damaged. Note that the instrument only uses rainfall for the exact period of the genocide. To show that this is a reasonable assumption I add distance to the main road interacted with rainfall one week before the genocide to the first stage regression (regression 3). While the point estimate on my instrument (using rainfall during the genocide) is identical to my baseline results the effect for rainfall one week before the genocide is small and insignificant. Besides, when adding the interaction with rainfall two weeks before the genocide in regression 4, the point estimate is even smaller and strongly insignificant (note that the effect for one week before turns significant, however this result is not robust to clustering standard errors). Intuitively, although rainfall can create deep potholes, once the dirt dries it usually becomes passable relatively quickly.

As a final sanity check, in regression 5, I interact the instrument with the standard deviation of daily rainfall between the village and the main road. The point estimate is positive and significant. Thus, the first-stage effects are weaker in areas with high temporal variation in rainfall. This is intuitive since high temporal variation implies that there exist relatively dry time periods during which a village could have easily been reached.

A.3.2 Extension: The Organizer's Time Horizon

Intuitively, if the genocide planners had had unlimited time to complete the genocide, I should not expect to find any effects in the first stage: Places that were hard to reach at the beginning of the genocide were likely easy to reach during the end when the dry season started. However, because the genocide planners did not know if and when the international community would intervene, any day could have been the last. Thus the planners rushed to execute the genocide as quickly as possible (taking into account transport costs). This implies two things: we should observe the bulk of the killings in the first couple of weeks and transport costs likely mattered less towards the end. Figure A.9 plots the estimated total number of killings over time.[17] Importantly, the bulk of the killings were perpetrated within the first couple of weeks. Besides, transport

[17]These estimates are also taken from Genodynamics.com and generated using a Bayesian latent variable model on information from five different sources: the Ministry of Education in Rwanda, the Ministry of Local Affairs in Rwanda, Ibuka (the Rwandan survivor organization), African Rights, and the Human Rights Watch.

costs seemed to have mattered the most at the beginning of the genocide and not at all towards the end. In Figure A.10, I split the genocide period into different intervals (3-week and 5-week intervals) and re-run my first-stage regression. While the point estimates for the first intervals are large and statistically significant at least at the 90 percent level, they are close to zero and insignificant towards the end.

On a side note, I find exactly the same pattern in the reduced form using civilian perpetrators (Figure A.11). This provides some additional evidence that it is really the militiamen driving the effects in the reduced form.

A.3.3 Extension: Main Road Costs

My first-stage analysis makes two assumptions. First, that transport costs on the main roads are negligible relative to the costs for driving on dirt roads and second that armed groups took the shortest route from the main road to each village. I now relax these two assumptions. In particular, I assume that armed groups originated from the nearest main city (which is corroborated by anecdotal evidence) and that they did not necessarily take the shortest route from the main road to the village but rather the one with the lowest costs (accounting for both distance and rainfall).[18]

In Table A.14, I show the results. Since I do not know the relative costs of 1 mm of rainfall on a dirt road versus a tarred road, I present results for various costs. In regression 1, I calculate total travel costs assuming that the costs of traveling on the main road are 50 percent lower than traveling on a dirt road. Recalculating the instrument under these assumptions gives a somewhat weaker first stage. I then re-calculate the instrument assuming that relative costs are 20, 10, and 5 percent. In all cases the results are very similar to my baseline first-stage result from Table 3 in the paper. Besides, I also re-run the second stage with the new instruments (20, 10 and 5 percent) and virtually obtain the same point estimates (regressions 5 to 7). Taken together, these results suggest that using the direct route between village and main road without taking into account main road travel seems to be a reasonable approximation. Note that when main road travel costs are about 20 percent of dirt road costs the first stage

[18] I allow militiamen to leave the main road 10 kilometers up and down the baseline point of departure (i.e. when using the shortest path). I use the ArcGIS network tool to calculate the shortest path between the closest main city and the point where the dirt road begins.

gets equally strong as my baseline first stage. This is somewhat consistent with anecdotal evidence from Ethiopia (like Rwanda a very mountainous country) which suggests that average speed on a dirt road is about 25 to 30 percent of the average speed on a tarred road (Briggs, 2012).[19]

[19]Note that this comparison is likely an upper bound since it also factors in the dry season. In my Rwanda sample, I always observe positive rainfall.

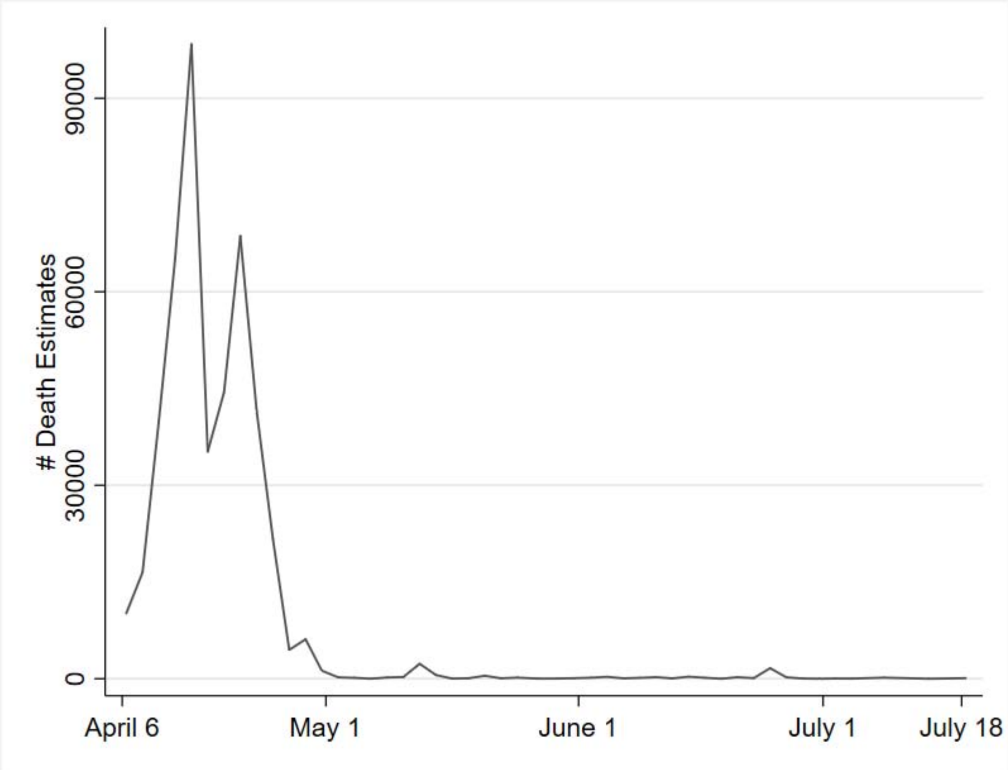
References

[1] **National Aeronautics and Space Administration (NASA) and National Geospatial-Intelligence Agency (NGI).** 2000. Shuttle Radar Topography Mission digital elevation model (SRTM30), provided by United States Geological Survey, https://dds.cr.usgs.gov/srtm/version2_1/SRTM30/e020n40/ (accessed January 2014).

[2] **Association For Safe International Road Travel (ASIRT).** 2005. Road Security Report: South Africa, http://www.wpi.edu/Images/CMS/GPP/South_Africa.pdf.

[3] **Briggs, Philip.** 2012. *Ethiopia – the Bradt Travel Guide*, Guilford, Connecticut: The Global Pequot Press Inc.

Figure A.9: Killings over Time



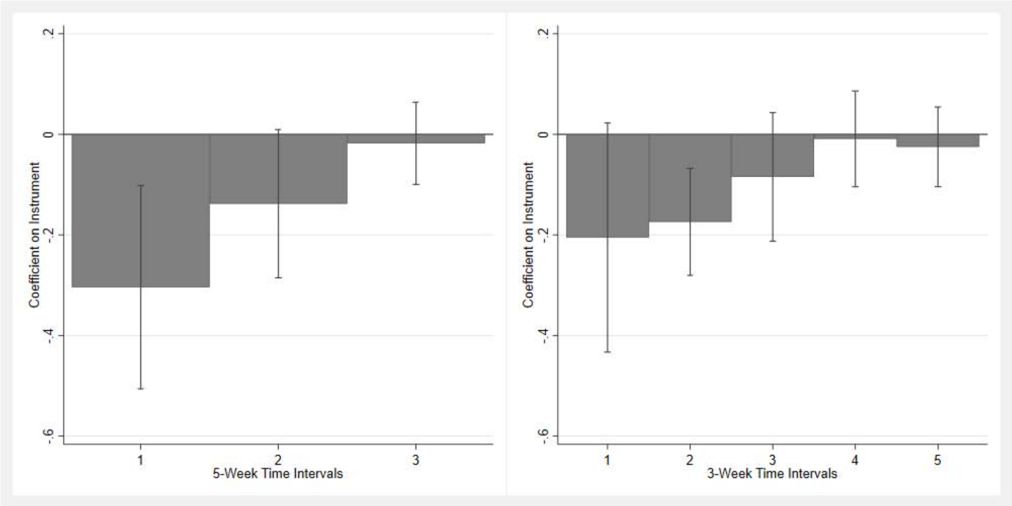
Notes: The figure plots the estimated number of total deaths over time. These estimates are taken from Genodynamics.com and generated using a Bayesian latent variable model on information from five different sources: the Ministry of Education in Rwanda, the Ministry of Local Affairs in Rwanda, Ibuka (the Rwandan survivor organization), African Rights, and the Human Rights Watch.

Table A.13: Understanding the First Stage

Dependent Variable:	# Militiamen, log					# Civilians, log
	Slope along Buffer		Rainfall before Genocide		Temporal Variation	Add. Instrument
	(1)	(2)	(3)	(4)	(5)	(6)
Distance × Rainfall along Buffer, 1994	−0.318	−0.258	−0.443	−0.433	−0.898	
	(0.190)	(0.142)	(0.129)	(0.113)	(0.312)	
... × Slope along Buffer	−3.458					
	(2.175)					
... × High Slope along Buffer, dummy		−0.680				
		(0.162)				
Distance × Rainfall along Buffer, 1 week before			−0.039	−0.042		
			(0.026)	(0.021)		
Distance × Rainfall along Buffer, 2 weeks before				−0.011		
				(0.058)		
... × Temporal Variation in Rainfall along Buffer, 1994					0.212	
					(0.086)	
# Militiamen, log						1.697
						(0.333)
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
R^2	0.51	0.51	0.50	0.51	0.50	.
N	1376	1376	1432	1432	1432	1376

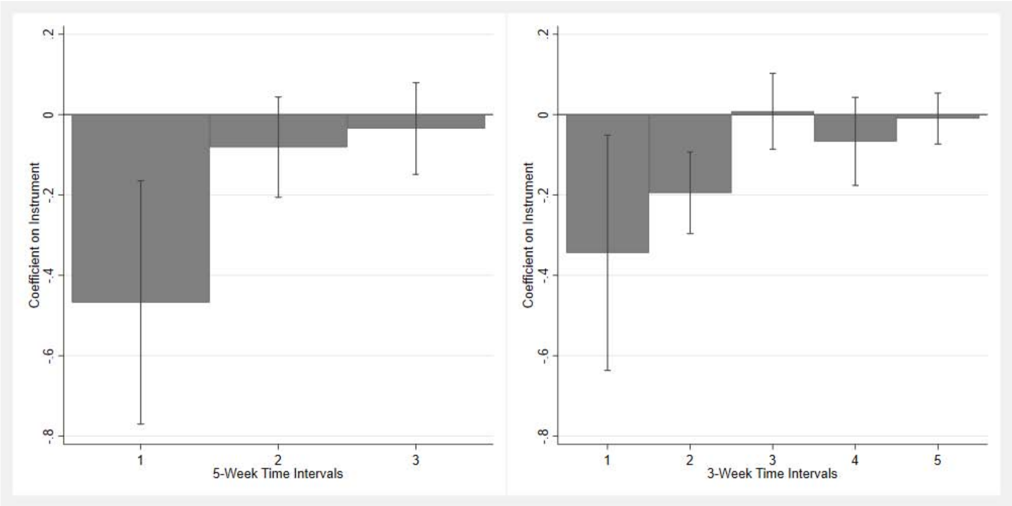
Notes: **Slope along Buffer** is the maximum slope along the buffer between village and main road. **High Slope along Buffer, Dummy** takes on the value of one if the slope is larger than the median. **Temporal Variation in Rainfall** is the standard deviation of daily rainfall along the buffer between village and main during for the time of the genocide. In **regression 5** I use my original instrument interacted with slope as a second instrument. **Standard Controls** include village population, rainfall in the village during the 100 days of the genocide in 1994, long-term rainfall in the village during the 100 days (1984-1993), rainfall along the way between village and road during the 100 days in 1994, long-term average rainfall along the way between village and road during the 100 days (1984-1993), distance to the main road and the latter interacted with the two rainfall-along-the-way measures. **Growing Season Controls** are rainfall during the growing season in 1994 in the village, 10 year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the road in the sample and the actual distance to the 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. I always control for all main effects and double interactions. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Figure A.10: First-Stage Effects over Time



Notes: I replace my instrument in regression 3 in Panel A in Table 3 by interactions of distance to the main road with rainfall along the way for three/five time intervals of equal length. The point estimates together with 95 percent confidence intervals are plotted on the y-axis. To illustrate, the first point estimate on the left figure is on distance to the main road interacted with rainfall along the way during the first five weeks of the genocide.

Figure A.11: Reduced-Form Effects over Time



Notes: I replace my instrument in regression 6 in Panel A in Table 3 by interactions of distance to the main road with rainfall along the way for three/five time intervals of equal length. The point estimates together with 95 percent confidence intervals are plotted on the y-axis. To illustrate, the first point estimate on the left figure is on distance to the main road interacted with rainfall along the way during the first five weeks of the genocide.

Table A.14: Alternative Routes (incl. Main Roads)

Dependent Variable:	# Militiamen, log				# Civilian Perpetrators, log		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Quickest Path, Main Road 50%	−0.386						
	(0.095)						
Quickest Path, Main Road 20%		−0.469					
		(0.112)					
Quickest Path, Main Road 10%			−0.475				
			(0.116)				
Quickest Path, Main Road 5%				−0.481			
				(0.118)			
# Militiamen, log					1.408	1.377	1.358
					(0.303)	(0.307)	(0.303)
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
Main Road Controls	yes	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes
R^2	0.50	0.50	0.50	0.50	.	.	.
N	1432	1432	1432	1432	1432	1432	1432

Notes: The instrument is the total transport costs along the dirt road and along the main road. For example, in **Quickest Path, Main Road 50%** I assume that the travel costs (induced by rainfall) on the main road are 50 percent of those on the dirt road. In regressions 5 to 7, I instrument for the number of militiamen with the instrument from regressions 2 to 4, respectively. **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 calender days of the genocide period (1984-1993), rainfall along the "quickest" way (not necessarily shortest) between village and main road during the 100 days of the genocide in 1994, long-term average rainfall along the "quickest" way between village and main road during the 100 calender days of the genocide period (1984-1993) and the latter interacted with distance to the main road (again not necessarily shortest). **Growing Season Controls** and **Additional Controls** are defined in Table 3 in the paper. **Main Road Controls** are distance to the closest main city (measured along the main road network from the main city to the point where the dirt road begins) and rainfall along the way. 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).

A.4 Extensions to Section III. – Placebo, Exclusion Restriction, and Robustness Checks

A.4.1 Placebo Tests

To rule out systematic measurement error in the rainfall data or that something other than the militia might be driving the results, I rerun both first-stage and reduced-form regressions using rainfall during the 100 calendar days of the genocide from the years 1983 until 2014 in the instrument. Reassuringly, the coefficient on the instrument with rainfall from 1994, the year of the genocide, is an outlier to the left in the distribution of 32 point estimates (Figures A.12 and A.13). Besides, the figures show that the strong negative effect for the genocide period from 1994 is not simply the result of a negative long-term trend: the point estimates for the time periods before 1994 are all oscillating around zero.

Moreover, I also rerun first stage and reduced form using rainfall for various 100-day windows before and after the genocide in the instrument. Again, the coefficient on the instrument with rainfall during the genocide is an outlier in both cases (Figures A.14 and A.15).[20]

Finally, rerunning the two placebo checks from above with child mortality (from the EICV survey) or the number of Tutsi deaths (from Genodynamics) as the dependent variable gives similar results: the coefficient on the instrument with 1994 rainfall during the genocide period is an extreme outlier in all cases (Figures A.16, A.17 and A.18).

A.4.2 Exclusion Restriction Tests

At this point, I still need to argue that civilians were not directly affected by the instrument, i.e. by traveling themselves, or that the instrument somehow affected access to markets, prices and income.

Anecdotal Evidence Starting with anecdotal evidence, several reports and accounts of the genocide indeed support the claim that civilian violence was a very

[20] This also rules out that the results may be driven by some non-linear effects of distance to the main road (this should then be true for other time periods as well).

local affair (Hatzfeld, 2005).[21]

Besides that, few people in Rwanda, let alone ordinary civilians, owned a car or a truck (less than 1 percent according to the 1992 DHS Survey) and the possibilities of moving between villages in motor vehicles, certainly the most affected by rain-slickend roads, were therefore limited for civilians. In addition, moving around along or close to the main roads was risky for ordinary citizens, as roadblocks were set up all over the country and being Hutu did not always ensure safety. Prunier (1995, p. 249) writes that *"To be identified on one's [identity] card [often at a roadblock] as a Tutsi or to pretend to have lost one's paper meant certain death. Yet to have a Hutu ethnic card was not automatically a ticket to safety. (...) And people were often accused of having a false card, (...)."* Amnesty International (1994, p. 6) reports that *"Each individual passing through these roadblocks had to produce an identity card which indicates the ethnic origin of its bearer. Being identified as or mistaken for a Tutsi meant immediate and summary execution."* Des Forges (1999, p. 210) continues, *"During the genocide some persons who were legally Hutu were killed as Tutsi because they looked Tutsi. According to one witness, Hutu relatives of Col. Tharcisse Renzaho, the prefect of the city of Kigali, were killed at a barrier after having been mistaken for Tutsi."*

Furthermore, there were no reasons for Hutu to travel because social life completely stopped (Hatzfeld, 2005). As one civilian killer puts it, *"During the killings, we had not one wedding, not one baptism, not one soccer match, not one religious service like Easter."* (Hatzfeld, 2005, pp. 94-95). Another one continues (p. 133), *"During the killings there was no more school, no more leisure activities, no more ballgames and the like."*

On a more general account, Horowitz (2001, p. 526) notes *"that [civilian] crowds generally stay close to home, attack in locales where they have the tactical advantage, and retreat or relocate the attack when they encounter unexpected resistance."*

Curfew Test Besides the anecdotal evidence, I also present a number of indirect tests. At the beginning of the genocide, a strict nation-wide curfew was implemented, which drastically limited the travel opportunities for civilians –

[21] Hatzfeld (2005) calls it a *Neighborhood Genocide* because only neighbors and co-workers were able to identify Tutsi, as they are similar to the Hutu (Hatzfeld, 2005).

in fact both Hutu and Tutsi were required to stay at home. Des Forges (1999, p. 162) writes that *"At the start, authorities instructed Rwandans to stay at home. The curfew allowed authorities and local political leaders to put in place the barriers and patrols necessary to control the population, multiplying them in communities where they were already functioning and reestablishing them in places where they were no longer in operation. Tutsi as well as Hutu cooperated with these measures at the start, hoping they would ensure their security. The hope was disappointed."* Reassuringly, the instrumental-variables estimates are similar to the baseline results when I restrict the variation in rainfall in the instrument to the first five days, the first week, or the first two weeks of the genocide, while controlling for rainfall along the way between village and main road for the remaining days and its interaction with distance to the main road (regressions 1 to 3 in Table A.15 below).[22] I believe that this is a very powerful test since the curfew essentially ruled out any kind of civilian movement.

Commune Test Next, because of tight population controls, already before 1994, it was practically impossible for civilians to get permission to leave their commune. The authorities carefully monitored and controlled the population. Des Forges (p. 186, 1999) writes *"Rwandans were supposed to be registered in the communes of residence if these differed from their communes of birth. Nyumbakumi, cell heads, and councilors all were involved in making sure that no strangers lived unnoticed in a commune."* Des Forges (pp. 186) continues that *"Authorities also revived an earlier requirement that persons wishing to travel outside their communes receive written authorization to leave (feuilles de route)."* Thus leaving one's commune involved significant costs. And indeed the results are similar, if anything larger, when I restrict the sample to those communes with no main road passing through (regression 4 in Table A.15). This further supports the identification strategy since moving around the commune should have been unrelated to transport costs in these off-the-main-road areas.

The two tests above provide general evidence that the exclusions restriction

[22] To be cautious, I also control for the long-term average rainfall between village and main road for those first couple of days and its interaction with distance to the main road as well as rainfall in the village during the first couple of days and its long-term average. Furthermore, I use different cutoff dates because I do not know when exactly the curfew ended. For the first-five-days and first-week regressions, I lose a few observations, because there was no rainfall during that short time period. However, rerunning the baseline regression with those two reduced samples gives the same results.

is likely to hold. In the following I will work through several possible threats – providing evidence why they should not matter (occasionally drawing on the two tests above).

Civilians Escaping There are two possibilities. High transport costs might hinder civilians from escaping (in case they took the main road). This scenario however would only work against my findings and bias the estimate downwards. On a side note, it seems unlikely since road blocks were set up all over the country and civilians likely avoided those under all circumstances. The other possibility is that civilians could easily flee from places that the militia could not reach (because of high transport costs). A number of results suggest that this is unlikely. First, as noted above at the beginning of the genocide the strict curfew forced people to stay at home and even Tutsi cooperated with these orders. Second, escaping the violence – because of high transport costs – should have been less of an issue in places close to the border since here civilians likely managed to escape much easier anyway. Importantly, in regression 5 in Table A.15 I show that the effects of the militia do not depend on the distance to the border. If anything the effects are slightly larger close to the border. Finally, recall from the discussion above that I do not find any evidence that becoming a refugee or being internally displaced during the genocide is correlated with the instrument.

Civilians Getting Stuck In The Fields Another concern is that civilians who work their fields further from home close to the main dirt road get stuck and cannot return home if transport costs are high. This case is also unlikely for several reasons. First, as noted above Rwandans were only occasionally given permission to leave the commune, it therefore seems highly unlikely that they had fields outside of their home commune – which they would have needed to access every day. Thus the second test from above speaks against this concern. Second, recall again that the curfew prevented civilians from leaving their homes, thus they unlikely worked on their fields. Finally, recall from above that very few civilians owned a car or truck and most of the farmers movements thus happened on foot. However, while strong rainfalls do affect vehicle transportation, they are unlikely to keep individuals from returning home.

Public Transportation The instrument might also affect public transportation for civilians. Again, it seems unlikely that this mattered. First, the curfew requested everyone to stay at home, thus civilians unlikely took a local bus or cab. Besides, public transportation is likely more common in urban areas. Below, I show that the results are robust to dropping all the main cities and the capital Kigali (Table A.23).

Localized Violence Unfortunately, the raw Gacaca data only records the location where people committed their crime(s) and not where they lived. A potential concern is thus that civilians traveled together with the militia and jointly committed the crimes (the curfew does not fully address this issue because civilians may have been granted permission to travel by the authorities). I address this in two ways: first, I did not find any anecdotal evidence that civilians followed the militia on their killing spree (naturally, this does not rule out the counterfactual but it is somewhat comforting). Second and more importantly, the same government agency that provided the Gacaca data ("National Service of Gacaca Jurisdiction") also provided a spreadsheet of a random subset of 20,000 category 2 perpetrators including names, sex, home village and the locations of where the crimes were committed (around 2.5 percent are female). Given a total of about 430,000 category 2 cases, this number is large enough to allow for representativeness.

Importantly, only about 4 percent of the individuals in this sample committed crimes outside of their home village. However, in all these cases the crimes are committed within the home commune. This suggests that it is unlikely that large numbers of civilians joined the militia outside of their home areas but rather stayed within their home village (or in a few cases communes). Note that civilians moving within a commune is unlikely to bias the results. First, the data suggests that this only happened in very few cases (i.e. 4 percent) and second recall from regression 4 in Table A.15 that the results are robust to excluding communes that have a main road passing through. Thus it seems unlikely that civilians' decisions to commit crimes in their neighboring villages are affected by the instrument.

Market Access and Income/Price Effects Transport costs may have also affected civilians' access to markets (often located close to the main road) and thus potentially prices and income which in turn could affect participation in violence. Importantly, the timing of the genocide somewhat alleviates this concern. It partly overlaps with the growing season which ensures enough variation in rainfall to affect transport costs (there is essentially no rainfall during the dry season) but still allows me to control for rainfall during the growing season. Besides, the harvest season and thus the time when villagers take their crops to the market to sell (and hence transport costs should matter) only accounts for a small period during the end of the genocide. I first show that my results are robust to adding harvest season controls to the regressions (rainfall along the buffer between village and main road during the harvest season and its interaction with distance to the road). First stage, reduced form and the main regression are shown in Table A.16. Importantly, transport costs during the harvest season do not affect the first stage or reduced form (regressions 1 and 2) and neither do they affect the main result (regression 3). Besides, the point estimate on harvest costs is small and insignificant.

In the following, I show that rainfall during the genocide period interacted with distance to the main road does not have an effect on wealth and income (once harvest costs are controlled for) for three different outcomes: a) Table A.17 uses nightlight density aggregated at the commune level;[23] b) Table A.18 uses information on household assets from the 1991 census to construct a wealth index (also only available at the commune level);[24] c) Table A.19 uses data on household consumption, assets and agricultural income/output from the EICV household survey from 2000/01. Finally, I can use the EICV household survey to calculate local market prices for the major crops – again, transport costs (during the period of the genocide) have no effects on any of these (Table A.20).

In each case transport costs are constructed using rainfall from the year of the outcome data, i.e. 1991 rainfall for the 1991 census.[25] Importantly, the interaction between rainfall during the genocide period and distance to the main

[23]Several satellites of the US Air Force circle around the earth 14 times a day observing every location on the planet at some instant between 8 and 10 pm local time. Each satellite dataset consists of a grid that reports the average yearly light density with a six-bit digital number (an integer between 0 and 62). The grid comes at a very high resolution, equal to approximately 0.86 square kilometers at the equator. The data is provided by the NOAA (1992/1993).

[24]The index incorporates information on housing such as roofing and floor material, as well as radio ownership and access to electricity and water.

[25]To be cautious, in those cases where the outcome variable is measured after 1994, I also control for distance to the main road interacted with rainfall along the buffer during the 100 genocide days in 1994 and all main effects. Unsurprisingly, the results do not depend on it.

road is never significantly related to any income or wealth measure. Interestingly though, transport costs during the harvest season are often significant and show the right sign, i.e. higher transport costs during the harvest season (and selling season) tend to decrease income and wealth (this is comforting because it shows that I am not simply obtaining noisy estimates). There is no clear relationship for crop prices. However this is unsurprising since there is no clear prediction in this case – whether harvest costs increase or decrease prices depends on local supply and demand. Most importantly, transport costs during the genocide period are unrelated to crop prices. Some of the regressions are run at the commune level. However, my baseline results are robust to collapsing the data at that level (regression 1 in Table A.18).

Coffee Finally, one of Rwanda's most important cash crops is coffee. Coffee production is a very transport-intensive undertaking since it requires the coffee beans to be shipped to special coffee mills for processing before being bagged and possibly exported. Thus if my instrument had effects not only on the militia's transport costs but also on the coffee production process I might obtain very different results in areas with high coffee production or places with a coffee mill.

Thus, to check whether my results differ in coffee production places I use two additional data sets. One is a nation-wide survey on the number of coffee trees in each village. Unfortunately that survey is from 1999 (5 years after the genocide) but it includes information on the age of the tree. I can distinguish between trees planted before 1990 and after ("old" and "new" trees). The second data set contains information on the location of coffee mills that were installed by the 1960s. This data is taken from Guariso and Verpoorten (2018b).[26]

As a first test, I show in Table A.21 that my main effects are not significantly different in places with coffee trees (regressions 1 to 3). The interaction effects of the number of militiamen with the number of coffee trees in each village are small and insignificant. This is true when looking at all trees and old or new trees separately. Besides, the interaction effects with the instrument are further small and insignificant in the first stage (regressions 4 to 6). Finally, the instrument is uncorrelated with the number of coffee trees in each village (regressions 7 to 9).

[26] Although this data is seemingly old, it still predicts for example investment in coffee tree maintenance from 2003 (Guariso and Verpoorten, 2018a). Thus it seems reasonable to assume that many of these mills are either still running or were replaced.

As a second test, I use the location of coffee mills. Results are reported in Table A.22. First, I show that the main results are robust to dropping villages with a coffee mill (regression 1), villages with a coffee mill within 10 kilometers (regression 2) and within 20 kilometers (regression 3). Besides, the results are not significantly different in any of these three subsets of villages (regressions 4 to 6). Interaction effects are insignificant throughout. Finally, the first-stage effects are not significantly different in villages with a coffee mill or coffee mills close-by (regressions 7 to 9).

Gauging the Bias Thus taken together all of the above findings, including the abundance of anecdotal evidence (or sometimes rather absence of anecdotal evidence) suggests that the exclusion restriction is likely to hold.

To nevertheless get a sense of how large the direct effect of transport costs on civilian violence would have to be to render the IV estimate insignificant, I follow Conley et al.'s (2012) plausibly exogenous method. I assume that the (potential) direct effect of the instrument on civilian violence is uniformly distributed on an interval $[\delta, 0]$. By varying δ, I identify the threshold at which the instrumental variable coefficient on (instrumented) militia violence becomes insignificant at the 90 percent level. The results are reported in Figure A.19. As long as the direct effect of my instrument on civilian violence is smaller (in absolute value) than -0.5, I still obtain a significant instrumental variable estimate. In terms of magnitudes this suggest that the direct effect has to be 76 percent of the overall reduced-form effect to render the estimates insignificant. This seems highly implausible given all the evidence from above together with the fact that there is no anecdotal evidence for systematic shipping of civilian perpetrators.

A.4.3 Robustness Checks

General Checks Next, I perform a number of general robustness checks, reported in Table A.23. Besides worrying about transport costs during the harvest season from 1994, which I already discussed in more detail above, one might further worry that yearly long-term rainfall might bias the results. Moreover, ruggedness might be correlated with rainfall and directly affect civilian participation in violence.[27] However, the main effects is unaffected by controlling for a) the yearly long-term average rainfall in the village and along the way between village and main road and the interaction of the latter with distance to the main road, b) the interaction of distance to the main road with both rainfall in the village during the growing season in 1994 and long-term average rainfall in the village during the growing seasons, and c) village terrain ruggedness.

To check whether armed groups might have taken a direct route to each village, possibly affected by rainfall along the way, I also control for rainfall during the genocide along the way between each village and the closest main city and its interaction with distance to the main city. As noted, I do not know exactly where armed groups were stationed, but the vast majority are likely to have started out from the main cities. However, the two additional controls do not affect the main result (regression 3). I provide a more detailed discussion in Section A.3 above. Furthermore, the results are robust to dropping all the main cities and villages close to them (regression 4).

One might also be worried that the UN troops that were stationed in Kigali, although few, were affected by transport costs, thus biasing the estimates. But again, the results are robust to dropping villages in Kigali city (regression 5).

Replacing 11 province fixed effects by 142 commune effects also does not matter (regression 6). Since the rainfall data only comes at a coarse resolution, at least relative to the large number of communes, this significantly reduces the variation in the instrument. Nevertheless, the instrumental-variables point estimate remains similar and equally significant.

Temperature Besides, recent studies found that temperature can also have a direct impact on violence (Burke et al., 2015; Ranson, 2014; Hsiang et al., 2013). Given the correlation across climatic factors and in order to avoid any omitted variables bias, in Table A.24 I add controls for temperature in the village and along the way from the village to the main road as well as the interaction of the latter with distance to the main road. I report IV estimates (regressions 1 to 4), as well as reduced-form estimates (regressions 5 to 8) and the first stage (regressions 9 to 12). Reassuringly, all the results are robust. This is true for

[27] The Terrain Ruggedness Index data is provided by Nunn and Puga (2012b) used in Nunn and Puga (2012a). The average villages ruggedness in my sample is 3.45 with a standard deviation of 1.38.

using both average temperature as well as maximum temperature. The temperature data is provided by the Terrestrial Hydrology Research Group and is also available daily at 0.1 degree resolution (Chaney et al., 2014).[28]

Dependent Variable To show that my findings are not dependent on functional form I report the results for alternative transformations of the dependent variables in Tables A.25 and A.26. Most importantly, I obtain very similar results when using a simple linear regression. The point estimate in regression 1 in Table A.25 suggests that an additional militiaman resulted in 6.8 more civilian perpetrators, very similar to my baseline results from above (recall: 7.2). The results are further robust to using a Poisson IV model (regression 2). The point estimate translates into a marginal effect of around 6 (evaluated at the mean of all the other variables). Moreover, the results are robust to using the number of perpetrators normalized by local village population (regressions 3 and 4).

Finally, I vary the various log transformations. Importantly, the results are robust to using a hyperbolic sine transformation – a common approach when dealing with observations that can take on the value of 0 (regression 1 in Table A.26). The point estimate is virtually identical to my baseline results. In regressions 2 to 9, I report results for various log(X+k) transformations where k varies between 0.0001 and 1000 (and X is either the number of militiamen or civilian perpetrators). The point estimates are only slightly lower for smaller k's. To get a sense of the magnitude, the one for $k = 0.0001$ still implies that an additional militiamen resulted in around 6 more civilian perpetrators.

Outliers To test for outliers, I also dropped one province at a time and the resulting estimates range from 1.153 to 1.527 and are significantly different from zero at the 99 percent confidence level in all cases (Figure A.20).

Sensitivity Analysis Next, in Table A.27 I perform a standard sensitivity analysis on the IV estimate, varying the number of controls. Altonji et al. (2005) derive a measure that determines how strong the unobservables have to be relative to the already included observables to drive the point estimate down to zero. The controls for the unrestricted model (relative to which the other ones

[28]The data is available here: https://hydrology.princeton.edu/data.metdata_africa.php.

are evaluated) are shown in column 1. In columns 2 to 7 I keep adding controls for the restricted models. For instance the number in column 3 suggests that the unobservables have to be 11.71 times as strong as the observables to eliminate the effect (controlling for all standard controls, growing season controls, additional controls and province fixed effects). To put this number into perspective note that the observables in this case include strong predictors of civilian violence such as population and population density, distance to Kigali, province fixed effects, rainfall in the village and all growing season controls (among others). Importantly, the ratio is strongly above 1 for all specifications. Thus it seems unlikely that some other (unobservable) variable might alter the main result.

Robustness Check – Rainfall in Village The 11 kilometer rainfall grids might be too coarse to sufficiently distinguish between rainfall *along* the distance buffer and rainfall *in* the village. Thus, the instrument might pick up differential village rainfall effects rather than transport costs. For instance, rainfall in the village likely affects rain-fed agriculture and this channel is potentially stronger for remote villages that depend on subsistence farming. Thus little rainfall in villages far away from the main road might lower villagers' opportunity costs of violence – violating the exclusion restriction. Alternatively, villages close to the main road that receive a lot of rainfall might be significantly richer not only because they can harvest more but also because they can sell their products on the nearby markets.

First note that these concerns are somewhat eased by the exact timing of the genocide: it only partially overlaps with the growing season (the rainfall that matters the most for plant production and thus the two income channels outlined above). Thus, in my main specifications I always control for rainfall in the village during the 1994 spring growing season and also allow the village growing season effects to vary by distance to the main road. As shown above the results are also robust to controlling for rainfall during the harvest season (the time when transport costs matter most for agricultural income) and its interaction with distance to the road. These controls should pick up most of the two economic effects outlined above.

However, to further alleviate these concerns, I also control for rainfall in the village during the 100 genocide days in 1994 interacted with distance to the main road for various specifications. Results are reported in Panel A of Table A.28. To show that the instrumental variable regressions are really driven by the armed groups' transport costs, I also report the first stage in Panel B. In regression 1, I simply control for my crucial standard and growing season controls. Importantly, the IV results hold up and the interaction with rainfall along the buffer rather than in the village seems to be driving the first stage – while the coefficient on the former is large and almost significant at the 90 percent level, the one on the latter is small and strongly insignificant. In regressions 2 to 4, I add various other controls. For instance, for the sake of interpretation, I also add rainfall *at* the nearest point on the main road[29] and its interaction with distance to the main road. Although this is a very demanding specification, the IV estimates hold up. The first-stage point estimates drop a little. To increase power I drop the province fixed effects in regression 5 and onwards (note that this does not change the effect in my baseline regression).[30] In this case, the point estimates on the instrument increase (in absolute terms) and gain significance at the 90 percent level while both rainfall in the village and at the main road (interacted with distance to the road) are relatively smaller and insignificant. I show several different specifications to ensure that the larger effect for the instrument is not due to some lucky choice of suitable controls.

Taken together, this supports that the results are driven by rainfall *along* the way to the village (neither the interaction of distance to the main road with village rainfall nor with rainfall at the main road matter). Importantly, there is still reasonable variation between rainfall in the village, rainfall at the main road, and rainfall along the buffer even for small distances because main roads, distance buffers, and village boundaries are not perfectly aligned with the rainfall grid but rather crisscross the grid cells in various locations.[31]

[29]Measured two and a half kilometers up and down the road (from the nearest point) over a 500-meter road buffer. Results are robust to varying the 2.5 km cutoff.

[30]Dropping province fixed effects in my baseline regressions (regressions 3 in Panel A and 6 in Panel B in Table 3) gives point estimates of 1.280 in the second stage and -0.798 in the first stage.

[31]For instance, rainfall along the buffer and in the village only fall within the same rainfall grid for about 9 percent of the villages.

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Figure A.12: Placebo Check: Armed-Group Violence

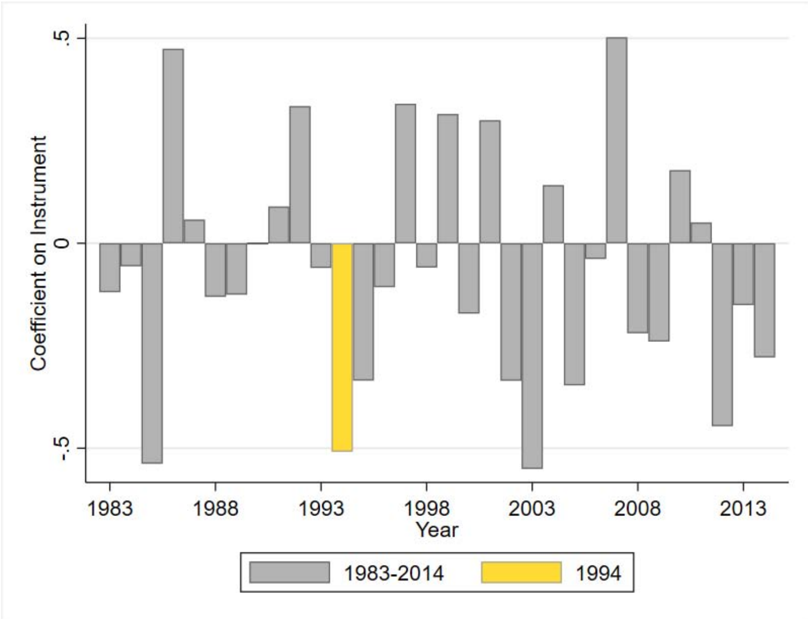
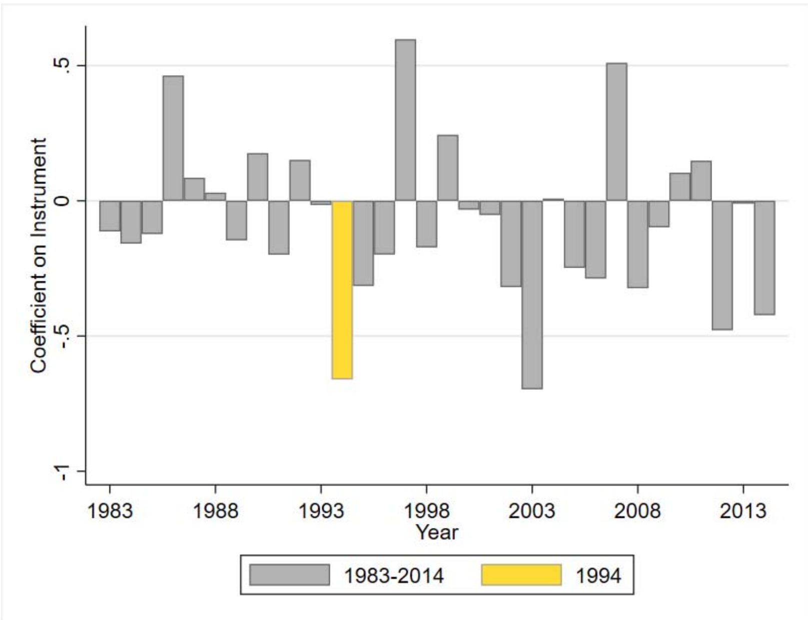


Figure A.13: Placebo Check: Civilian Violence



Notes: The figures show the distribution of coefficients on the instrument in first stage (Figure A.12) and reduced form (Figure A.13) when using rainfall along the way between village and main road during the 100 calender days of the genocide from the years 1983 to 2014 in my preferred specifications (regressions 3 and 6 in Panel A in Table 3 in the paper). For civilian violence only 1 of the 32 coefficients (3 percent) is larger in absolute value than the coefficient from 1994. For armed-group violence it is only 2 of 32 (6 percent).

Figure A.14: Placebo Check: Armed-Group Violence

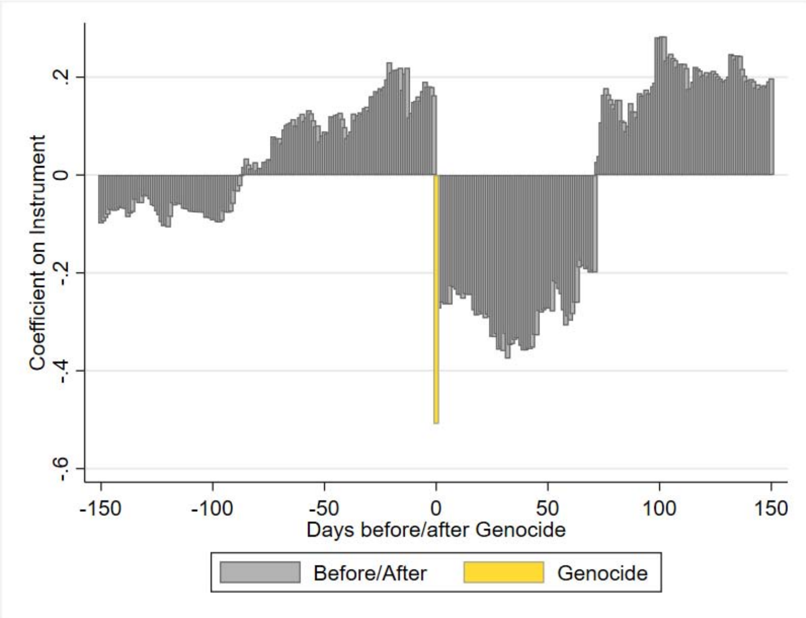
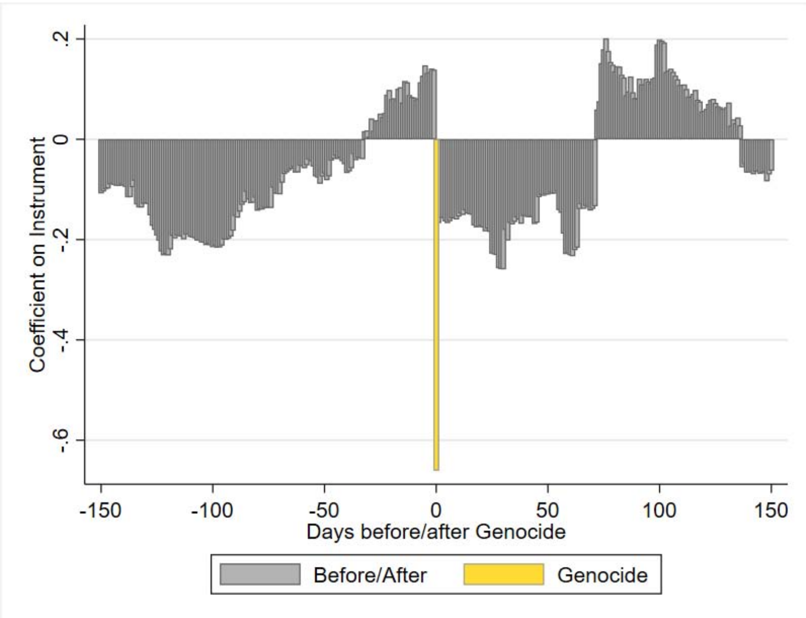


Figure A.15: Placebo Check: Civilian Violence



Notes: The figures plot the coefficients on the instrument in first stage (Figure A.14) and reduced form (Figure A.15) when using rainfall along the way between village and main road during 100-day windows before and after the genocide in my preferred specifications (regressions 3 and 6 in Panel A in Table 3 in the paper). The x-axis gives the start date (end date) of those time windows measured in the number of days after (before) the genocide. The genocide is normalized to 0. To illustrate, 50 on the x-axis means that I used rainfall for 100 days starting 50 days after the genocide. On the other hand, -50 means that I used rainfall for 100 days ending 50 days before the genocide.

Figure A.16: Placebo Check: Child Mortality – Different Years

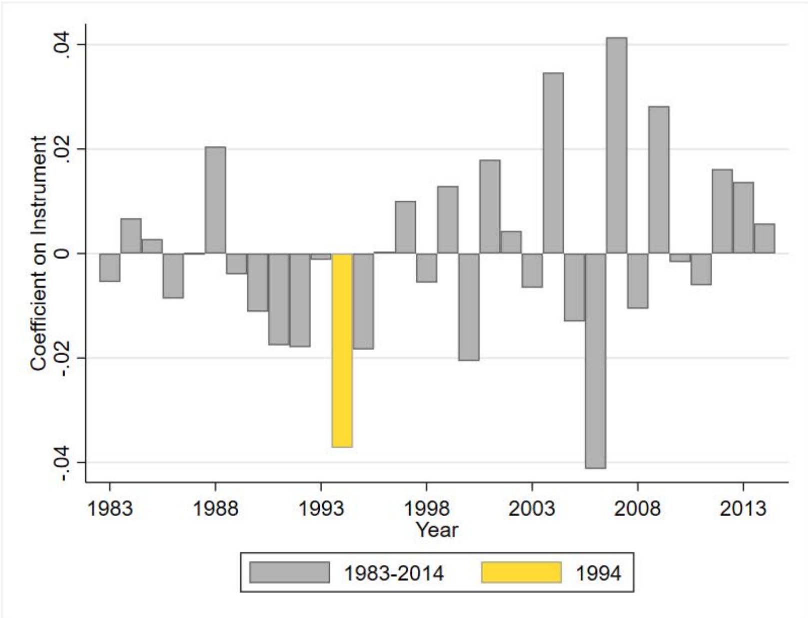
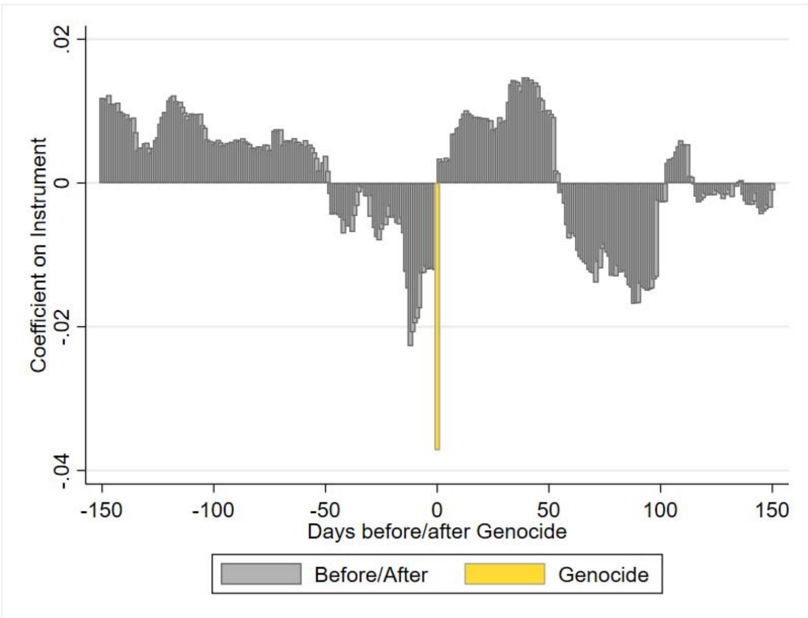
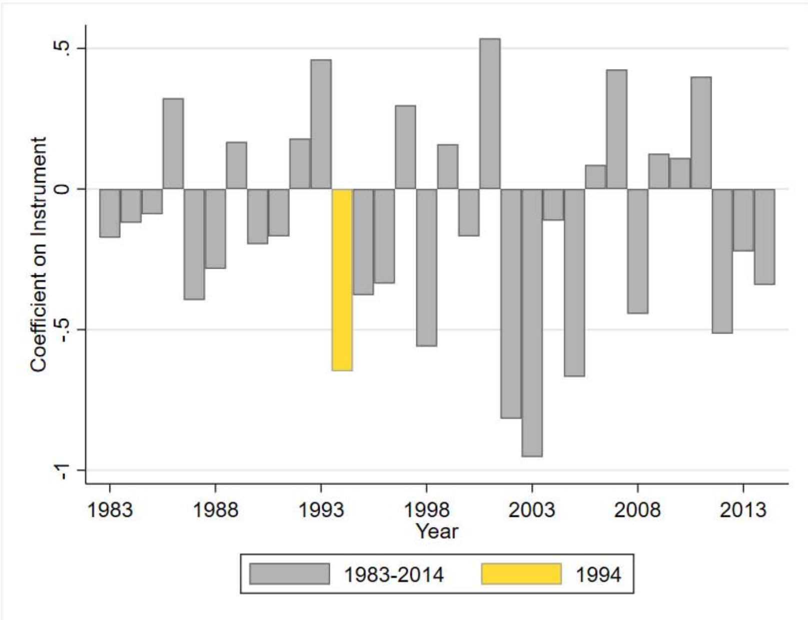


Figure A.17: Placebo Check: Child Mortality – Different Time Windows



Notes: Figure A.16 shows the distribution of coefficients on the instrument when using rainfall along the way between village and main road during the 100 calender days of the genocide from the years 1983 to 2014 in regression 1 in Table A.9 in the paper.
Figure A.17 plots the coefficients on the instrument when using rainfall along the way between village and main road during 100-day windows before and after the genocide in regression 1 in Table A.9 in the paper. The x-axis gives the start date (end date) of those time windows measured in the number of days after (before) the genocide. The genocide is normalized to 0. To illustrate, 50 on the x-axis means that I used rainfall for 100 days starting 50 days after the genocide. On the other hand, -50 means that I used rainfall for 100 days ending 50 days before the genocide.

Figure A.18: Placebo Check: Death Estimates



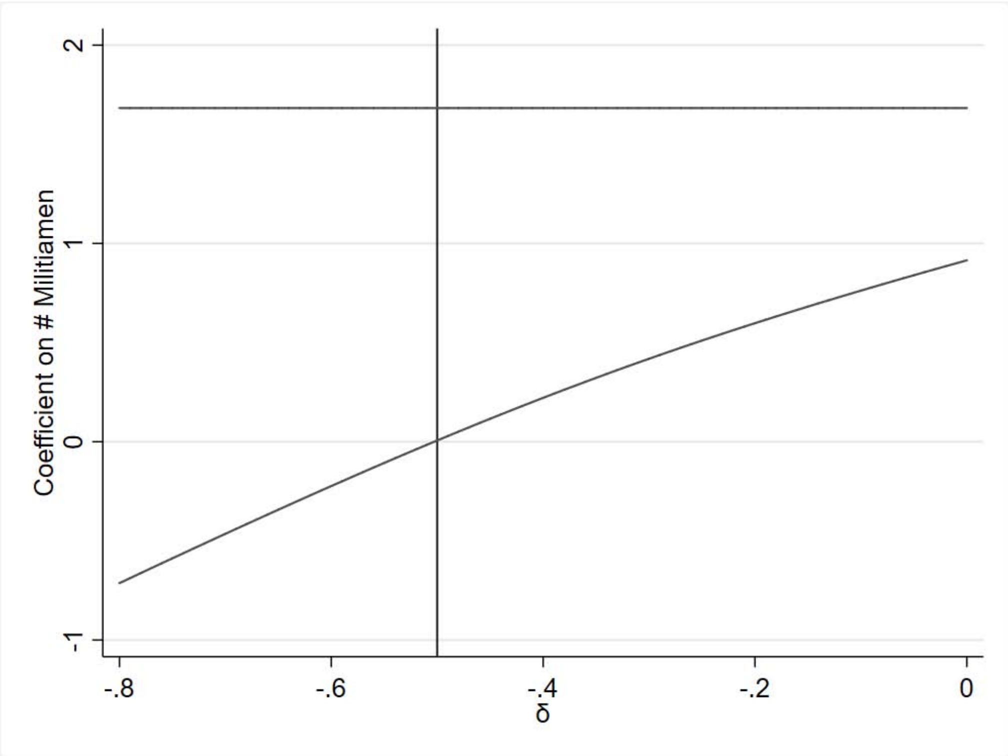
Notes: The figures show the distribution of coefficients on the instrument when using rainfall along the way between village and main road during the 100 calender days of the genocide from the years 1983 to 2014 in my preferred specifications (regressions 3 and 6 in Panel A in Table 3 in the paper) with a number of deaths estimates (at the commune level) as the outcome variable. Only 3 of 32 (9 percent) of the coefficients are larger in absolute value than the one from 1994.

Table A.15: Exclusion Restriction Tests

Dependent Variable:	# Civilian Perpetrators, log				
	First 5 days	First week	First 2 weeks	Communes w/o road	Interaction w/ border
	(1)	(2)	(3)	(4)	(5)
# Militiamen, log	1.332	1.267	1.365	1.696	1.409
	(0.611)	(0.427)	(0.359)	(0.639)	(0.585)
... × Distance to the Border, log					−0.163
					(0.167)
Standard Controls	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
First Days Controls	yes	yes	yes	no	no
Province Effects	yes	yes	yes	yes	yes
N	1399	1406	1432	568	1432

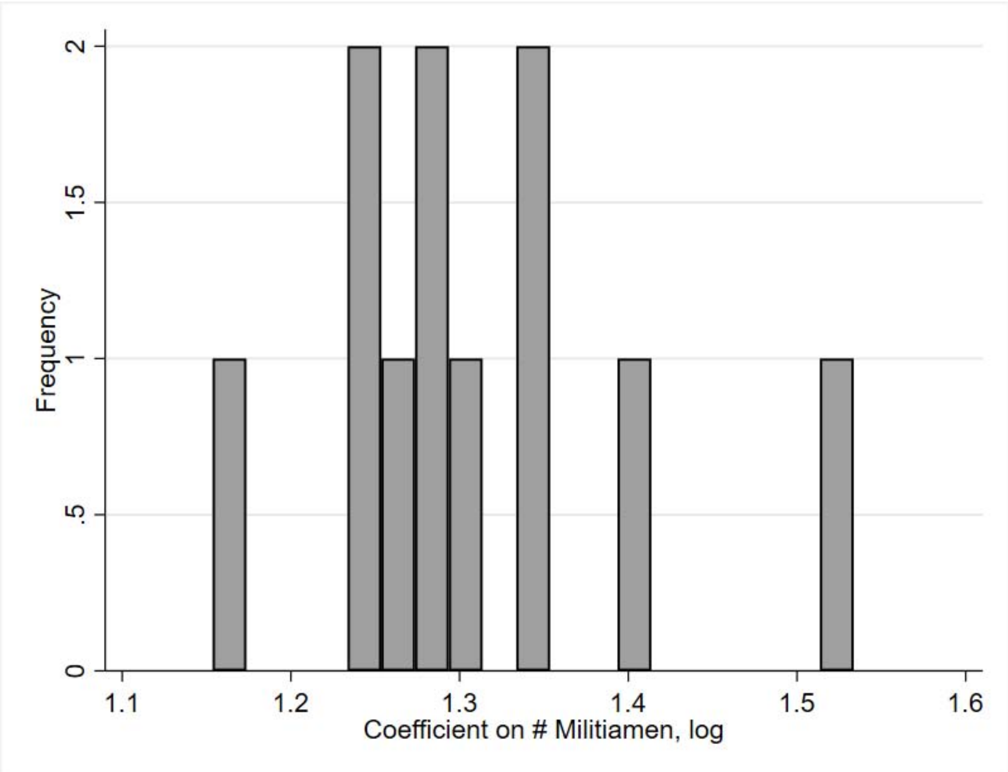
Notes: In **regressions 1 to 3** the instrument is distance to the main road interacted with rainfall along the way between village and main road during the first 5 days/1 week/2 weeks of the genocide. In **regression 4** the sample is restricted to communes without main road passing through. In **regression 5** I interact # Militiamen, log with distance to the border. **Standard Controls (for regressions 1 to 3)** include village population, rainfall in the village during the 100 days of the genocide in 1994, long-term average rainfall in the village during the 100 calender days of the genocide period (1984-1993), rainfall along the way between village and main road during the first 5 days/1 week/2 weeks of genocide in 1994, rainfall along the way between village and main road during the remaining genocide days in 1994, long-term average rainfall along the way between village and main road during the 100 calender days of the genocide period (1984-1993), distance to the main road and its interactions with the two last rainfall-along-the-way measures. **Standard Controls (for regressions 4 and 5)** 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 calender 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 calender days of the genocide period (1984-1993), and the latter interacted with distance to the main road. **Growing Season Controls** and **Additional Controls** are defined in Table 3 in the paper. **First Days Controls** are rainfall in the village during the first 5 days/1 week/2 weeks of the genocide and the ten-year long-term rainfall for those first days, ten-year long-term rainfall along the way between village and main road during the first 5 days/1 week/2 weeks of the genocide period and its interaction with distance to the main road. All control variables, except "Number of Days with RPF presence," are in logs. I always control for all main effects and double interactions. 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).

Figure A.19: Plausibly Exogenous



Notes: The figure shows the upper and lower bound of the 90 percent confidence interval of the second-stage coefficient on militia violence, using my baseline IV specification from column 3 in Table 3 in the paper. Following Conley et al. (2012), I allow for a direct effect of the instrument, assuming that this is uniformly distributed over an interval $[\delta, 0]$, with $\delta < 0$. The interval size δ is plotted on the x-axis. At $\delta = -0.5$, the second -stage coefficient on (instrumented) militia violence becomes insignificant at the 90 percent level.

Figure A.20: Outliers



Notes: This figure shows the distribution of point estimates on the number of militiamen when dropping one province at a time in my baseline specification (regression 6 in Panel B in Table 3 in the paper).

Table A.16: Harvest Season Controls

A. Dependent Variable:	# Militiamen, log	# Civilian Perpetrators, log	
	(1)	(2)	(3)
Distance × Rainfall along Buffer, Genocide Period 1994	−0.499	−0.676	
	(0.146)	(0.170)	
Distance × Rainfall along Buffer, Harvest Period 1994	0.019	0.062	0.036
	(0.093)	(0.054)	(0.087)
# Militiamen, log			1.357
			(0.279)
Standard Controls	yes	yes	yes
Growing Season Controls	yes	yes	yes
Additional Controls	yes	yes	yes
Province Effects	yes	yes	yes
R^2	0.50	0.58	.
N	1432	1432	1432

Notes: **Distance × Rainfall along Buffer, Genocide Period 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, and similarly for the Harvest Period). **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. I always control for all main effects and double interactions. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Table A.17: Night Light Density

Dependent Variable:	Night Light Density							
	1992		1993			1992/1993		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance × Rainfall along Buffer, 1991 Genocide Period	0.028		−0.204			−0.133		
	(0.099)		(0.153)			(0.110)		
Distance × Rainfall along Buffer, 1991 Harvest Period	−0.467		−0.502			−0.506		
	(0.066)		(0.072)			(0.060)		
Distance × Rainfall along Buffer, 1992 Genocide Period		0.129		0.152			0.151	
		(0.177)		(0.152)			(0.165)	
Distance × Rainfall along Buffer, 1992 Harvest Period		−0.230		−0.286			−0.273	
		(0.072)		(0.115)			(0.101)	
Distance × Rainfall along Buffer, 1993 Genocide Period					0.075			
					(0.170)			
Distance × Rainfall along Buffer, 1993 Harvest Period					−0.088			
					(0.031)			
Distance × Rainfall along Buffer, 92/93 Genocide Period								−0.024
								(0.236)
Distance × Rainfall along Buffer, 92/93 Harvest Period								−0.315
								(0.136)
Corresponding Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes
Corresponding 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	0.72	0.73	0.77	0.76	0.76	0.76	0.76	0.74
N	142	142	142	142	140	142	142	142

Notes: The regressions are run at the commune level – all variables are collapsed at that level. **Distance × Rainfall along Buffer, T Genocide Period** is distance to the main road interacted with rainfall along the way between village and main road during the 100 calendar days of the genocide period in year T (and similarly for the Harvest Period). **Corresponding Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 calender days of the genocide period in year T, five-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 calendar days of the genocide in year T, five-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and the latter interacted with distance to the main road. **Corresponding Growing Season Controls** are rainfall during the growing season in year T in the village, five-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road. Year T is defined in each row. **Additional Controls** are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991. All control variables are in logs. I always control for all main effects and double interactions. 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).

Table A.18: Exclusion Restriction – 1991 Wealth, Commune Level

Dependent Variable:	# Civilian Perpetrators, log	Wealth Index		Below Median Wealth	
	(1)	(2)	(3)	(4)	(5)
# Militiamen, log	1.380				
	(0.345)				
Distance x Rainfall along Buffer, 1991 Genocide Period		−0.174	−0.053	0.025	0.005
		(0.074)	(0.072)	(0.028)	(0.028)
Distance x Rainfall along Buffer, 1991 Harvest Period			−0.198		0.030
			(0.041)		(0.018)
1994 Standard Controls	yes	yes	yes	yes	yes
1994 Growing Season Controls	yes	no	no	no	no
1991 Standard Controls	no	yes	yes	yes	yes
1991 Growing Season Controls	no	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes
R^2	.	0.73	0.74	0.62	0.62
N	142	142	142	142	142

Notes: The unit of observation is the commune – all variables are collapsed at that level. **Distance × Rainfall along Buffer, 1991 Genocide Period** is distance to the main road interacted with rainfall along the way during the 100 calender days of the genocide period from 1991 (and similar for the Harvest period). In **regression 1** I rerun regression 6 in Panel B from Table 3 at the commune level. The **Wealth Index** is based on various wealth indicators from the 1991 census. **Below Median Wealth** is dummy variable based on the Wealth Index. **1994 (1991) Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 calender days of the genocide period in 1994 (1991), ten-year (five-year) long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 calender days of the genocide period in 1994 (1991), ten-year (five-year) long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and the latter interacted with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 1994 (1991) in the village, ten-year (five-year) long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. All control variables are in logs. Interactions are first logged and then interacted. Note that in regression 1 I also control for the number of days with RPF presence. I always control for all main effects and double interactions. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Table A.19: Exclusion Restriction – Income, Output and Consumption

	Income, Log		Output, Log		Assets	Consumption
	Livestock & Agriculture	Agriculture	Livestock & Agriculture	Agriculture	Log	Log
	(1)	(2)	(3)	(4)	(5)	(6)
Distance × Rainfall along Buffer, 2000/01 Genocide Period	0.040	0.018	0.045	0.005	−0.005	0.020
	(0.138)	(0.132)	(0.132)	(0.133)	(0.097)	(0.050)
Distance × Rainfall along Buffer, 2000/01 Harvest Period	−0.043	−0.038	−0.039	−0.044	−0.018	0.014
	(0.025)	(0.024)	(0.022)	(0.025)	(0.032)	(0.013)
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
Armed-Groups Transport Costs	yes	yes	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes
R^2	0.13	0.13	0.14	0.14	0.35	0.12
N	4707	4725	4776	4776	5272	5272

Notes: All dependent variables are in logged per capita monetary values. Per capita refers to the consumption/income/output 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 EICV2001/2001 survey. **Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 calender days of the genocide period in 2000/01, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 calender days of the genocide period in 2000/01, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and the latter interacted with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 2001/01 in the village, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. **Armed-Groups Transport Costs** are rainfall along the way between village and main road during the 100 days of the genocide period in 1994 and its interaction with distance to the main road. All control variables, except "Number of Days with RPF presence," are in logs. I always control for all main effects and double interactions. 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).

Table A.20: Exclusion Restriction – Crop Prices

	Corn	Sorghum	Manioc	Sweet Potato	Potato	Soja	Dried Beans	Dried Peas	Cooking Banana	Beer Banana	Beans	Manioc Leaves	Inyabutongo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Distance × Rainfall along Buffer, 20001 Genocide Period	−0.173	−0.016	0.181	0.018	0.045	−0.058	−0.041	0.022	−0.102	−0.106	0.120	−0.008	0.012
	(0.103)	(0.047)	(0.160)	(0.046)	(0.038)	(0.064)	(0.021)	(0.045)	(0.068)	(0.107)	(0.078)	(0.112)	(0.040)
Distance × Rainfall along Buffer, 2000/01 Harvest Period	0.007	0.010	−0.010	−0.005	0.025	−0.021	0.006	0.018	0.036	0.017	0.056	0.066	−0.012
	(0.014)	(0.014)	(0.057)	(0.007)	(0.015)	(0.012)	(0.009)	(0.010)	(0.019)	(0.036)	(0.030)	(0.063)	(0.012)
Standard Controls	yes	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	yes
Additional Controls	yes	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	yes
R^2	0.06	0.13	0.22	0.22	0.31	0.12	0.05	0.13	0.09	0.17	0.19	0.24	0.20
N	2485	2664	748	1543	1159	988	4398	663	1120	983	774	635	1198

Notes: Dependent variables are the corresponding crop prices per kilogram reported by the household. The data is taken from the EICV2001/2001 survey. **Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 calender days of the genocide period in 2000/01, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 calender days of the genocide period in 2000/01, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and the latter interacted with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 2001/01 in the village, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. **Armed-Groups Transport Costs** are rainfall along the way between village and main road during the 100 days of the genocide period in 1994 and its interaction with distance to the main road. All control variables, except “Number of Days with RPF presence,” are in logs. I always control for all main effects and double interactions. 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).

Table A.21: Coffee Trees

Dependent Variable:	# Civilian Perpetrators, log			# Militiamen, log			# All Coffee Trees	# Old Coffee Trees	# New Coffee Trees
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
# Militiamen, log	1.259	1.284	1.318						
	(0.333)	(0.290)	(0.261)						
... x # of All Coffee Trees	0.034								
	(0.137)								
... x # of Old Coffee Trees		0.020							
		(0.108)							
... x # of New Coffee Trees			−0.134						
			(0.220)						
Distance × Rainfall along Buffer, 1994				−0.609	−0.616	−0.533	−0.238	−0.519	0.282
				(0.154)	(0.162)	(0.091)	(0.501)	(0.380)	(0.179)
... x # of All Coffee Trees				0.025					
				(0.035)					
... x # of Old Coffee Trees					0.035				
					(0.043)				
... x # of New Coffee Trees						0.040			
						(0.108)			
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
R^2	.	.	.	0.50	0.50	0.50	0.28	0.26	0.21
N	1432	1432	1432	1432	1432	1432	1432	1432	1432

Notes: The number of coffee trees in each village is taken from the 1999 coffee census. Old coffee trees were planted before 1990, new ones after 1990. **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. I always control for all main effects and double interactions. 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).

Table A.22: Coffee Mills

Dependent Variable:	# Civilian Perpetrators, log						# Militiamen, log		
No Coffee Mill within:	$x = 0\ km$	$x = 10\ km$	$x = 20\ km$	$x = 0\ km$	$x = 10\ km$	$x = 20\ km$	$x = 0\ km$	$x = 10\ km$	$x = 20\ km$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
# Militiamen, log	1.291	1.274	1.422	1.280	1.278	1.322			
	(0.252)	(0.250)	(0.243)	(0.254)	(0.259)	(0.278)			
... x Coffee Mill within x km				−0.413	−0.304	−0.424			
				(0.295)	(0.292)	(0.444)			
Distance × Rainfall along Buffer, 1994							−0.565	−0.554	−0.557
							(0.100)	(0.101)	(0.114)
... x Coffee Mill within x km							−0.134	−0.226	−0.132
							(0.152)	(0.139)	(0.187)
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
R^2	.	.	.	.	.	.	0.50	0.50	0.50
N	1197	1165	968	1431	1431	1431	1431	1431	1431

Notes: The location of coffee mills is taken from Guariso and Verpoorten (2018). In **regressions 1 to 3** I drop all villages within x kilometers of a coffee mill, the cutoff distance is given in the column header (0 means no mill in the village itself). **Regressions 4 to 9** control for various coffee mill interactions. **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. I always control for all main effects and double interactions. 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).

Table A.23: Robustness Checks

Dependent Variable:	# Civilian Perpetrators, log (IV/2SLS)				
	Additional Controls		Without Cities	Without Kigali	Commune Effects
	(1)	(2)	(3)	(4)	(5)
# Militiamen, log	1.327	1.317	1.298	1.305	1.203
	(0.276)	(0.328)	(0.290)	(0.299)	(0.377)
Standard Controls	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Harvest Controls	yes	yes	no	no	no
Other Rainfall Controls	yes	yes	no	no	no
Terrain Ruggedness Index	yes	yes	no	no	no
Main City Transport Controls	no	yes	no	no	no
Province Effects	yes	yes	yes	yes	no
Commune Effects	no	no	no	no	yes
N	1432	1432	1357	1400	1432

Notes: **Regressions 1 to 2** have additional controls. In **regression 4** villages within five kilometers of a main city are dropped. In **regression 5** Kigali province is dropped. In **regression 6** province fixed effects are replaced by commune effects. **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. **Harvest Controls** are rainfall along the way between village and main road during the harvest season and its interaction with distance to the main road. **Other Rainfall Controls** are distance to the main road interacted with a) rainfall during the growing season in 1994 in the village and b) ten-year long-term average rainfall during the growing seasons in the village as well as yearly long-term average rainfall in the village, yearly long-term average rainfall along the way between village and main road and its interaction with distance to the main road. **Terrain Ruggedness Index** is the average village ruggedness index. **Main City Transport Controls** are rainfall along the way between village and the closest main city during the genocide period and its interaction with distance to the main city. All control variables, except "Number of Days with RPF presence," are in logs. Interactions are first logged and then interacted. There are **11 provinces** and **142 communes** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Table A.24: Additional Robustness Checks – Temperature

Dependent Variable:	# Civilian Perpetrators, log								# Militiamen, log			
	IV/2SLS				OLS				OLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
# Militiamen, log	1.426 (0.340)	1.357 (0.354)	1.412 (0.330)	1.376 (0.355)								
Distance × Rainfall along Buffer, 1994					−0.565 (0.127)	−0.469 (0.104)	−0.562 (0.127)	−0.478 (0.104)	−0.396 (0.104)	−0.346 (0.101)	−0.398 (0.105)	−0.348 (0.100)
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
Max. Temperature Controls	yes	yes	no	no	yes	yes	no	no	yes	yes	no	no
Max. Temperature-Road Interactions	no	yes	no	no	no	yes	no	no	no	yes	no	no
Average Temperature Controls	no	no	yes	yes	no	no	yes	yes	no	no	yes	yes
Average Temperature-Road Interactions	no	no	no	yes	no	no	no	yes	no	no	no	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R^2	.	.	.	.	0.59	0.59	0.59	0.59	0.51	0.52	0.51	0.52
N	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432

Notes: **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. **Max. Temperature Controls** are maximum temperature in the village during the 100 days of the genocide in 1994, maximum temperature along the way between village and main road during the 100 days of the genocide in 1994. **Max. Temperature-Road Interactions** is the latter interacted with distance to the main road. **Average Temperature Controls** are average temperature in the village during the 100 days of the genocide in 1994, average temperature along the way between village and main road during the 100 days of the genocide in 1994. **Average Temperature-Road Interactions** is the latter interacted 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. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Table A.25: Additional Robustness Checks – Dependent Variable I

Dependent Variable:	# Civilian Perpetrators		# Civilian Perpetrators, p.c.	
	Linear	Poisson	Linear	log(X+1)
	(1)	(2)	(3)	(4)
# Militiamen	6.768	0.027		
	(2.838)	(0.013)		
# Militiamen, p.c.			6.114	
			(2.221)	
log(# $Militiamen, p.c. + 1$)				1.430
				(0.315)
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
N	1432	1432	1432	1432

Notes: In **regression 2** I run a Poisson IV regression. In **regression 4** the X stands for # Civilian Perpetrators, p.c. **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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). In regression 2, standard errors are clustered at the commune level (142 communes).

Table A.26: Additional Robustness Checks – Dependent Variable II

Dependent Variable:	# Civilian Perpetrators								
	HST	log(X+0.0001)	log(X+0.001)	log(X+0.01)	log(X+0.1)	log(X+1)	log(X+10)	log(X+100)	log(X+1000)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
log(# $Militiamen + \sqrt{\# Militiamen^2 + 1}$)	1.248 (0.246)								
log(# $Militiamen + k$)		1.115 (0.189)	1.131 (0.191)	1.155 (0.195)	1.196 (0.211)	1.299 (0.260)	1.689 (0.403)	3.197 (0.996)	5.690 (2.423)
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
N	1432	1432	1432	1432	1432	1432	1432	1432	1432

Notes: The dependent variable is a function of the # Civilian Perpetrators. **HST in column 1** stands for hyperbolic sine transformation given by $log(X + \sqrt{X^2+1})$. The constant k in regressor 2 is given in the column header. **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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).

Table A.27: Sensitivity Analysis

	Robustness to Controls						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ratio Unobservables to Observables	0.00	14.32	11.71	9.05	16.51	4.02	7.75
Basic Standard Controls	yes	yes	yes	yes	yes	yes	yes
Standard Controls	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	no	yes	yes	yes	yes	yes	yes
Additional Controls	no	no	yes	yes	yes	yes	yes
Main City Transport Controls	no	no	no	yes	yes	yes	yes
Harvest Controls	no	no	no	no	yes	yes	yes
Other Rainfall Controls	no	no	no	no	no	yes	yes
Terrain Ruggedness Index	no	no	no	no	no	no	yes
Province Effects	no	no	yes	yes	yes	yes	yes
N	1433	1433	1432	1432	1432	1432	1432

Notes: The table shows how strong the unobservables have to be relative to the included observables to drive the point estimate down to zero. The controls for the various restricted models are given at the bottom of the table. Note that in the unrestricted model (column 1) I only control for **Basic Standard Controls** which are distance to the main road, rainfall along the way between village and main road during the 100 days of the genocide in 1994, long-term average rainfall along the way between village and main road during the 100 calender days of the genocide period (1984-1993) and the latter interacted with distance to the main road. **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. **Harvest Controls** are rainfall along the way between village and main road during the harvest season and its interaction with distance to the main road. **Other Rainfall Controls** are yearly long-term average rainfall in the village, yearly long-term average rainfall along the way between village and main road and its interaction with distance to the main road. **Terrain Ruggedness Index** is the average village ruggedness index. **Main City Transport Controls** are rainfall along the way between village and the closest main city during the genocide period and its interaction with distance to the main city. 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.

Table A.28: Robustness to Rainfall in Village

A. Dependent Variable:	# Civilian Perpetrators, log										
	IV/2SLS										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
# Militiamen, log	1.034	1.286	1.377	1.583	1.105	1.184	1.193	1.075	1.094	1.161	1.300
	(0.452)	(0.778)	(0.827)	(1.195)	(0.444)	(0.476)	(0.620)	(0.366)	(0.411)	(0.490)	(0.674)
Distance × Rainfall in Village, 1994	−0.095	−0.336	−0.312	−0.286	−0.122	−0.112	−0.103	−0.206	−0.200	−0.196	−0.082
	(0.167)	(0.382)	(0.476)	(0.539)	(0.323)	(0.395)	(0.351)	(0.258)	(0.227)	(0.290)	(0.445)
Distance × Rainfall at Main Road, 1994		0.340	0.331	0.322	−0.007	0.010	−0.015				−0.001
		(0.606)	(0.613)	(0.709)	(0.479)	(0.456)	(0.503)				(0.512)
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	no	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Harvest Controls	no	no	yes	yes	no	yes	no	yes	no	yes	yes
Terrain Ruggedness Index	no	no	no	yes	no	no	yes	no	yes	yes	yes
Province Effects	yes	yes	yes	yes	no	no	no	no	no	no	no
N	1433	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432
B. Dependent Variable:	# Militiamen, log										
	First Stage										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Distance × Rainfall along Buffer, 1994	−0.848	−0.503	−0.494	−0.440	−0.883	−0.841	−0.721	−1.182	−1.112	−0.982	−0.691
	(0.544)	(0.545)	(0.518)	(0.525)	(0.521)	(0.484)	(0.523)	(0.615)	(0.594)	(0.573)	(0.481)
Distance × Rainfall in Village, 1994	0.399	0.410	0.289	0.244	0.533	0.320	0.394	0.383	0.451	0.298	0.217
	(0.540)	(0.575)	(0.625)	(0.601)	(0.619)	(0.638)	(0.580)	(0.625)	(0.606)	(0.599)	(0.598)
Distance × Rainfall at Main Road, 1994		−0.420	−0.298	−0.261	−0.456	−0.297	−0.355				−0.227
		(0.557)	(0.557)	(0.528)	(0.571)	(0.512)	(0.52)				(0.476)
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	no	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Harvest Controls	no	no	yes	yes	no	yes	no	yes	no	yes	yes
Terrain Ruggedness Index	no	no	no	yes	no	no	yes	no	yes	yes	yes
Province Effects	yes	yes	yes	yes	no	no	no	no	no	no	no
R^2	0.48	0.50	0.50	0.50	0.36	0.38	0.37	0.38	0.37	0.39	0.39
N	1433	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432

Notes: **Distance** × **Rainfall along Buffer, 1994** is the instrument (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994) and similarly for Rainfall in the Village and at the Main Road. **Standard Controls**, **Growing Season Controls**, and **Additional Controls** are defined in Table 3 in the paper. **Harvest Controls** are rainfall along the way between village and main road during the harvest season and its interaction with distance to the main road. **Terrain Ruggedness Index** is the average village ruggedness index. All control variables, except "Number of Days with RPF presence," are in logs. I always control for all main effects and double interactions. 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).

A.5 Extensions to Section V. – Network Effects

A.5.1 A Simple Spatial Model

To guide the empirical analysis in the paper, I use a simple game-theoretic model to determine the allocation of militiamen and their effects on civilian participation. The model is based on Acemoglu, Garcia-Jimeno and Robinson (2015).

A.5.2 Network Structure and Utility

Assume that the economy consists of n villages. For village i let $N(i)$ denote the set of villages that are connected to it. In the baseline these will be the direct neighbors to i but I will also present results when using higher-order neighbors. Let $\mathbf{W}$ be an $n \times n$ symmetric, spatial weighting matrix with off-diagonal entries given by

$$w_{ij} = \begin{cases} 0 & \text{if } j \notin N(i) \\ \frac{1}{d_{ij} r_{ij}} & \text{if} j \in N(i), \end{cases} \tag{A.1}$$

where d_{ij} denotes the geodesic distance between the centroids of villages i and j and r_{ij} is the amount of rainfall along the way between villages i and j. Thus possible spillovers between villages depend on the transport costs between the villages. In particular, heavy rainfall makes transportation and thus spillovers harder.

Civilian participation in violence is a function of the number of militiamen in each village and the number of militiamen in neighboring villages

$$c_i = \tau_i m_i + \bar{\eta} t_i m_i + \bar{\phi} m_i \mathbf{W}_i \mathbf{m} + \delta \mathbf{W}_i \mathbf{m} + u_i, \tag{A.2}$$

where c_i is the number of civilian perpetrators in village i and m_i are the number of militiamen in village i. $\mathbf{W}_i$ is the ith row of the spatial weighting matrix $\mathbf{W}$ and $\mathbf{m}$ is the full column vector of militiamen, respectively. Besides, the effect of village i's militia on civilian violence depends on village characteristics in the following way

$$\tau_i = \mathbf{X}_i \bar{\beta} + \bar{\gamma}_i^p + \tilde{\varepsilon}_i, \tag{A.3}$$

where $\mathbf{X}_i$ is a vector of village characteristics such as population, distance to the border or rainfall during the growing season. Note that in the empirical part of the paper these will be standard controls, growing season controls and additional controls. Furthermore, $\bar{\gamma}_i^p$ are a full set of province fixed effects and $\tilde{\varepsilon}_i$ marks some unobserved heterogeneity.

Next, t_i are the transport costs to reach village i (these will be modeled as distance to the main road interacted with rainfall along the way). I expect $\bar{\eta}$ to be negative thus high transport costs reduce the number of civilian perpetrators.[32] I argue in the paper that these affect civilian participation only via the militiamen and will thus be excludable in the second stage.

Finally, $\bar{\phi}$ captures any interaction effects between village i militiamen and neighboring militiamen and δ captures the direct effects of neighboring militiamen on civilian participation.

The error term u_i is also modeled as a linear function of observables

$$u_i = \mathbf{X}_i\tilde{\beta} + \tilde{\gamma}_i^p + e_i, \tag{A.4}$$

where $\mathbf{X}_i$ is the same vector of village characteristics as above, $\tilde{\gamma}_i^p$ are province fixed effects and e_i is a mean zero error term.

A.5.3 Analysis

Each village wants to maximize its own number of civilian perpetrators. In order to do so it can call upon the central government to send army and militia troops. Anecdotal evidence supports this conjecture: Des Forges (1999, p. 184) writes *"Burgomasters (the local leaders) occasionally called in soldiers or National Policemen, particularly if there were many Tutsi to kill."* Furthermore, *"In response to needs identified by the authorities or party heads, the militia leaders displaced their men from one area to another. Leaders dispatched militia from Kigali to Butare town and others from Nyabisindu were ordered to Gatagara in Butare prefecture. They sent militia from other locations to participate in massacres at Kaduha church in Gikongoro, at Rutonde commune in Kibungo, and at Ntongwe commune in Gitarama."* (Des Forges, 1999 p.180). However, calling upon the central government is costly: increasing the number of militia-

[32]For instance, some militiamen might get stuck and not reach the village or the increased travel time might affect their productivity.

men involves a linear transportation cost (modeled above) and a standard convex cost.[33] Each village thus maximizes

$$U_i = c_i - \frac{\zeta}{2} m_i^2. \tag{A.5}$$

Note that villages do not take militiamen or civilians in neighboring villages into account when maximizing their own utility. The first-order conditions are, assuming an interior solution,

$$\tau_i + \bar{\eta} t_i + \bar{\phi} \mathbf{W}_i \mathbf{m} - \zeta m_i = 0. \tag{A.6}$$

Thus, I get

$$m_i = \frac{\bar{\phi}}{\zeta} \mathbf{W}_i \mathbf{m} + \frac{\tau_i}{\zeta} + \frac{\bar{\eta} t_i}{\zeta}. \tag{A.7}$$

Plugging this back into the definition for c_i gives

$$c_i = \zeta m_i^2 + \delta \mathbf{W}_i \mathbf{m} + u_i. \tag{A.8}$$

A.5.4 Empirical Strategy

In order to obtain the final regression equations, I substitute for τ_i and u_i, respectively. The first-stage regression becomes

$$m_i = \frac{\bar{\eta}}{\zeta} t_i + \frac{\bar{\phi}}{\zeta} \mathbf{W}_i \mathbf{m} + \frac{1}{\zeta} \mathbf{X}_i \bar{\beta} + \frac{1}{\zeta} \bar{\gamma}_i^p + \frac{1}{\zeta} \tilde{\varepsilon}_i. \tag{A.9}$$

Since I will not estimate any of the parameters but solely want to give a rationalization for the empirical analysis in the paper, I can re-write

$$m_i = \eta t_i + \phi \mathbf{W}_i \mathbf{m} + \mathbf{X}_i \beta + \gamma_i^p + \varepsilon_i. \tag{A.10}$$

[33] Note that the local leaders likely carefully weighed the benefits and costs of calling external militiamen. While these certainly helped to mobilize civilians and kill Tutsi (a task that the central government required) they also came at the cost of having to share any looted assets. In particular, they often demanded the best pieces. As one of the local perpetrators in Hatzfeld (2005, p. 82) puts it *"On days of large-scale operations, the interahamwe and the soldiers from neighboring communes took priority in the looting. They heaped up new radios, fat cows, comfortable chairs, top-quality sheet metal. We locals shared what they left behind."*

In this first stage t_i is the militia's transport costs measured as the distance of the village centroid from the main road interacted with rainfall along the way from the village to the main road during the time of the genocide. Note that the two main effects, i.e. distance and rainfall uninteracted, are included in $\mathbf{X}_i$. The new term $\mathbf{W}_i\mathbf{m}$ captures any spillover or displacement effects. Depending on the sign of ϕ militiamen arriving in neighboring villages can either increase the number of militiamen in village i (ϕ positive) if militiamen for instance move to neighboring villages once their mission is completed or decrease the number of militiamen if there are displacement effects (ϕ negative).

The second-stage regression becomes

$$c_i = \zeta m_i^2 + \delta \mathbf{W}_i\mathbf{m} + \mathbf{X}_i\tilde{\beta} + \tilde{\gamma}_i^p + e_i. \tag{A.11}$$

Here the new terms $\mathbf{W}_i\mathbf{m}$ capture possible spillover effects. In particular, any spillover effects in equation (A.10) are likely to be mirrored in the second stage (A.11), captured by δ.

Finally note that strictly speaking this model predicts a quadratic relationship between c_i and m_i. In the empirical analysis in the paper I deviate from this as I anyway take the natural logarithm of the two conflict variables.

A.5.5 Identification

Note that because of the recursive structure of the specification $\mathbf{W}_i\mathbf{m}$ is likely to be correlated with the error term and thus endogenous in the first stage. Besides, both m_i and neighboring militiamen $\mathbf{W}_i\mathbf{m}$ are endogenous in the second stage. To obtain causal effects I follow the standard procedure in the spatial econometrics literature and use instruments constructed using the characteristics of neighbors' of neighbors.

The idea is that transport costs (and other characteristics) to second-order neighbors affect the number of militiamen in a given village i only via the number of militiamen going to first-order neighbors. Note that the point estimates on the spillover terms obtained for regressions (A.10) and (A.11), although informative about the direction of potential spillover effects, do not allow me to directly read off marginal effects. I will use the standard formulas to calculate direct, indirect and total marginal effects.[34]

A.5.6 Additional Tables

[34] In general, one can re-write the specifications above as $\mathbf{y} = \mathbf{X}\beta + \mathbf{W}\mathbf{X}\pi + \lambda\mathbf{W}\mathbf{y} + \mathbf{e}$. Solving this equation for $\mathbf{y}$ yields: $\mathbf{y} = (\mathbf{I} - \lambda\mathbf{W})^{-1}(\mathbf{X}\beta + \mathbf{W}\mathbf{X}\pi + \mathbf{e})$. Furthermore, $E(\mathbf{y}|\mathbf{X},\mathbf{W}) = (\mathbf{I} - \lambda\mathbf{W})^{-1}[\mathbf{X}\beta + \mathbf{W}\mathbf{X}\pi]$. The marginal direct effect is $ADE = \frac{1}{n}\sum_{i=1}^{n}\frac{\partial E(y_i|\mathbf{X},\mathbf{W})}{\partial x_i}$ and the indirect effect is given by $AIE = \frac{1}{n}\sum_{i=1}^{n}\sum_{j=1,j\neq i}^{n}\frac{\partial E(y_i|\mathbf{X},\mathbf{W})}{\partial x_i}$. The total effect is the sum of the two.

Table A.29: Spatial Economy of Conflict – Higher-Order Neighbors Robustness

Dependent Variable:	# Militiamen, log							
Spatial Lag:	$X=3$		$X=4$		$X=5$		$X=6$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance × Rainfall along Buffer, 1994	−0.516	−0.510	−0.499	−0.522	−0.493	−0.515	−0.491	−0.504
	(0.133)	(0.136)	(0.131)	(0.132)	(0.131)	(0.132)	(0.130)	(0.130)
Spatial Lag of Order X × # Militiamen, log	0.027	0.022	−0.035	−0.015	−0.057	−0.026	−0.065	−0.050
	(0.067)	(0.067)	(0.062)	(0.056)	(0.059)	(0.049)	(0.057)	(0.116)
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
Marginal Effects of the Instrument								
Direct Effect	−0.516	−0.510	−0.499	−0.522	−0.493	−0.515	−0.491	−0.504
	(0.133)	(0.136)	(0.131)	(0.132)	(0.131)	(0.132)	(0.130)	(0.130)
Indirect Effect	−0.002	−0.002	0.002	0.001	0.002	0.001	0.002	0.001
	(0.005)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Total Effect	−0.518	−0.512	−0.497	−0.521	−0.491	−0.514	−0.489	−0.503
	(0.135)	(0.138)	(0.132)	(0.132)	(0.131)	(0.132)	(0.130)	(0.131)
N	1432	1432	1432	1432	1432	1432	1432	1432

Notes: **Spatial Lag of Order X × # Militiamen, log** is the number of militiamen multiplied by an Xth-Order spatial weighting matrix. The weights for neighbors are defined as $\frac{1}{dist_{ij} \times rain_{ij}}$, the X's are defined in the column headers. **Standard Controls**, **Growing Season Controls**, and **Additional Controls** are defined in Table 3 in the paper. Interactions are first logged and then interacted. There are **11 provinces** in the sample. In all odd regressions spatial auto-correlation spatial auto-correlation is assumed to be captured by the spatial lag of the dependent variable and error terms are heteroskedastic (Conley, 2008). In all even regressions spatial auto-correlation is explicitly modeled in the error term following Drukker et al. (2013). Standard errors for marginal effects are obtained using the delta method.

Table A.30: Spatial Economy of Conflict – Spatial Weights Robustness

Dependent Variable:	# Militiamen, log												# Civilian Perpetrators, log
Spatial Lag:	$X = 1$		$X = 2$		$X = 3$		$X = 4$		$X = 5$		$X = 6$		$X = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Distance × Rainfall along Buffer, 1994	−0.498	−0.497	−0.564	−0.522	−0.567	−0.480	−0.557	−0.536	−0.540	−0.533	−0.527	−0.549	
	(0.132)	(0.138)	(0.134)	(0.148)	(0.134)	(0.152)	(0.134)	(0.145)	(0.134)	(0.139)	(0.134)	(0.134)	
# Militiamen, log													1.291
													(0.074)
Spatial Lag of Order X × # Militiamen, log	0.451	0.446	0.357	0.409	0.247	0.534	0.153	0.133	0.090	0.073	0.048	0.038	0.016
	(0.062)	(0.062)	(0.062)	(0.073)	(0.060)	(0.114)	(0.058)	(0.067)	(0.056)	(0.062)	(0.054)	(0.054)	(0.030)
Standard Controls	yes	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	yes
Additional Controls	yes	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	yes
Marginal Effects													
Direct Effect	−0.508	−0.507	−0.566	−0.524	−0.568	−0.482	−0.557	−0.536	−0.540	−0.533	−0.527	−0.549	1.292
	(0.135)	(0.141)	(0.134)	(0.148)	(0.134)	(0.153)	(0.134)	(0.145)	(0.134)	(0.139)	(0.134)	(0.134)	(0.074)
Indirect Effect	−0.196	−0.192	−0.116	−0.128	−0.053	−0.121	−0.024	−0.020	−0.010	−0.008	−0.004	−0.004	0.010
	(0.067)	(0.067)	(0.041)	(0.050)	(0.022)	(0.054)	(0.013)	(0.013)	(0.008)	(0.008)	(0.006)	(0.005)	(0.019)
Total Effect	−0.703	−0.698	−0.681	−0.652	−0.621	−0.603	−0.581	−0.556	−0.550	−0.542	−0.531	−0.553	1.302
	(0.194)	(0.201)	(0.168)	(0.191)	(0.151)	(0.195)	(0.143)	(0.152)	(0.138)	(0.143)	(0.136)	(0.136)	(0.072)
N	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432	1432

Notes: **Spatial Lag of Order X × # Militiamen, log** is the number of militiamen multiplied by an Xth-Order spatial weighting matrix. The weights for neighbors are defined as $\frac{1}{dist_{ij}}$, the X's are defined in the column headers. **Standard Controls**, **Growing Season Controls**, and **Additional Controls** are defined in Table 3 in the paper. Interactions are first logged and then interacted. There are **11 provinces** in the sample. In all even regressions spatial auto-correlation is explicitly modeled in the error term following Drukker et al. (2013). In regression 13 standard errors are corrected for spatial correlation within a radius of 150km, Conley (1999). In all other regressions spatial auto-correlation is assumed to be captured by the spatial lag of the dependent variable and error terms are heteroskedastic (Conley, 2008).

A.6 Extensions to Section VI. – The Tutsi Minority, Material Incentives, Fear, and RTLM

The results from Section VI. in the paper suggest that the militia mostly acted as a moral authority invoking civilians' obedience. In this Section I consider some additional interaction effects to understand the channels better. Note that, in order to establish causality, I instrument each interaction term with the interaction between the instrument and the variable capturing the heterogeneous effects. Furthermore, I always include all double interactions.

Tutsi Minority First, the militia's effects on civilian participation are larger in villages with Tutsi rebels present at the beginning of the genocide (regressions 1 and 2 in Table A.31). Since the Hutu extremists carefully cultivated the Hutu populations fear of the Tutsi (rebels), mobilizing the population in these villages was likely easier. Additionally, villages with Tutsi rebels had much fewer local militiamen and thus the arriving external militiamen likely had a large impact.[35]

Second, the coefficient on the interaction with the Tutsi minority share, is equally positive (5.161, standard error 13.810, regression 1). However, since variation in the Tutsi minority share only comes at coarse commune level, the effect is insignificant. In regression 2, I thus replace the continuous Tutsi minority share variable by a dummy variable taking on the value of 1 if the Tutsi minority share lies above the median. Once more, the point estimate is similarly positive but, in addition, also significant at the 95 percent level (1.070, standard error 0.531).[36]

A large Tutsi minority likely made mobilization easier as the enemy was easy to point out. In addition, similar to the case for Tutsi rebels, villages with large Tutsi minorities likely had fewer local militiamen, thus increasing the marginal effect of an external militia member.

[35]Note that this explanation assumes that the effects of the militia on civilian participation are concave and that local and external militiamen are substitutes in the mobilization process. I provide evidence for this below.

[36]Unfortunately, because of strong multicollinearity, this specification does not allow me to control for the double interaction of the Tutsi minority share dummy with distance to the main road. To account for the potential omitted variable bias that this creates, I interact the Tutsi minority share dummy with the other controls not involving distance to the main road and include them in the regression.

Material Incentives There is abundant anecdotal evidence that material incentives mattered for civilian participation (Hatzfeld, 2005). In regressions 3 to 5, I test this. Importantly, I differentiate between Hutu wealth and Tutsi wealth. Theory suggests two opposite effects of income on conflict (Dube and Vargas, 2013). Conflict levels should increase if the gains from fighting go up, this is the rapacity channel. On the other hand, conflict prevalence should decrease if income from alternative economic activities goes up, this is the opportunity cost channel. Thus, in this case, since the Hutu attacked the Tutsi, an increase in Tutsi wealth should make mobilization easier and an increase in Hutu wealth should decrease participation. Regressions 3 to 5 confirm these conjectures. The interaction effect with Hutu wealth is negative and statistically significant at the 95 percent level (-0.620, standard error 0.248). For Tutsi wealth the interaction is positive and again statistically significant (regression 4). The results are robust to adding both interactions together (regression 5).

Note that these results are consistent with the result from the main paper that the militia did not coerce civilians but rather invoked obedience. If the militia had coerced civilians this should have happened regardless of the material status of the village. However, acting as a moral authority suggests that there was some room for discretion.

Reassuringly, the above results are robust to including both wealth and Tutsi interactions (regression 6).

Prior Exposure to Violence Finally, I check whether a village's prior exposure to violence affects the results. The Uppsala Conflict Data Program (UCDP) database (Pettersson and Öberg, 2020; UCDP, 2013; Sundberg and Melander, 2013) provides detailed information on the location and actors of violent events since 1989.[37] Results are reported in Table A.32. First, I show that my instrument is uncorrelated with whether a village experienced pre-genocide violence (regression 1). Second, the militia's effects on civilian participation seem to be lower in places with prior violence (regression 2). The point estimate on the interaction term is negative, however strongly insignificant. Because exposure to Hutu-driven and RPF-driven violence may have had different effects, I next split villages into those who experienced any kind of RPF violence and those

[37]The main sources of UCDP are reports by international news agencies (such as Reuters and the BBC) and reports from non-governmental organizations (such as Human Rights Watch).

that only experienced Hutu violence (against Tutsi civilians). The effects in regression 3 suggest that the militia was especially successful in villages with exposure to RPF violence. The point estimate is positive and significant at the 90 percent level (2.853, standard error 1.570). The point estimate on Hutu violence is negative but still highly insignificant. These results are robust to interacting the two violence measures in two separate regressions (regressions 4 and 5). In this case, significance for the RPF violence interaction increases slightly (regression 4).

In terms of interpretation, these results are consistent with anecdotal evidence suggesting that the Hutu government built on the population's fear of the Tutsi and especially the Tutsi rebel army to foster participation (Des Forges, 1999). This fear should have been especially salient in those places that were exposed to pre-genocide Tutsi violence. Once more the findings are inconsistent with the militia coercing civilians into participation. In that case, the effects should have been unrelated to RPF violence exposure.

RTLM As an extension, I look at interaction effects with RTLM coverage in Table A.33. Recall from the main paper that RTLM induced violence by local militia and police forces (and only subsequently civilians). Thus this delivers a test on whether external and local militia were substitutes or complemented each other. I first interact the number of militiamen with a dummy taking on the value of 1 if RTLM coverage lies above the median. The effect is close to zero and insignificant. This is confirmed when I, instead of interacting, split the sample at the median of RTLM (regressions 3 and 4). Both effects are significant and highly similar. Note that the effects are slightly lower than my baseline results from the main paper. This is because the RTLM sample is slightly smaller: in regression 7 I rerun my baseline specification using the RTLM sample and the point estimate is similar to the ones in regressions 3 and 4.

Finally, I split the sample at the 75th percentile. The interaction effect in regression 2 suggests that the effects of the militia are lower in places with high RTLM coverage (above 75th percentile). The effect is almost significant at the 90 percent level (when using clustered standard errors). Note that because of the stark differences between clustered and Conley standard errors in this case, I report both. This negative interaction effect is confirmed when splitting the sample at the 75th percentile. While the effect for villages with low RTLM coverage is close to baseline and highly significant, the effect for high-RTLM villages is low and insignificant (0.243, standard errors 0.366 and 0.289). This suggests that local and external militiamen were substitutes in the mobilization process.

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Table A.31: Tutsi Minority, Material Incentives

Dependent Variable:	# Civilian Perpetrators, log (IV/2SLS)					
	Tutsi		Wealth			All
	(1)	(2)	(3)	(4)	(5)	(6)
# Militiamen, log	0.832	0.987	1.054	1.030	0.827	0.365
	(0.934)	(0.293)	(0.293)	(0.221)	(0.214)	(0.271)
... × Tutsi Rebels	2.178	1.945				1.183
	(1.046)	(0.614)				(0.477)
... × Large Tutsi Minority		1.070				0.918
		(0.531)				(0.341)
... × Tutsi Minority Share	5.161					
	(13.810)					
... × Hutu Wealth Index			−0.620		−1.148	−1.282
			(0.248)		(0.394)	(0.381)
... × Tutsi Wealth Index				0.274	0.361	0.550
				(0.118)	(0.295)	(0.252)
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
Tutsi Controls	no	yes	no	no	no	yes
Province Effects	yes	yes	yes	yes	yes	yes
N	1432	1432	1432	1380	1380	1380

Notes: The **Tutsi Rebels** dummy takes on the value of 1 if Tutsi rebels where in control of the village at the beginning of the genocide. **Tutsi Minority Share** is the fraction of Tutsi divided by the fraction of Hutu. The **Large Tutsi Group** dummy takes on the value of 1 if the Tutsi Minority Share lies above the sample median. **Hutu (Tutsi) Wealth Index** is a 1991 wealth index for the Hutu (Tutsi) village population. **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. **Tutsi Controls** include the interaction of the Large Tutsi Group dummy with all other controls that do not involve 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. In each column, I also control for all main effects and double interactions. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Table A.32: Pre-Genocide Violence and Fear

Dependent Variable:	Total Violence Pre-Genocide	# Civilian Perpetrators, log			
	(1)	(2)	(3)	(4)	(5)
Distance × Rainfall along Buffer, Genocide Period 1994	−0.015				
	(0.023)				
# Militiamen, log		1.330	1.104	1.137	1.308
		(0.316)	(0.244)	(0.224)	(0.451)
... × Total Violence, Pre-Genocide		−0.926			
		(0.668)			
... × Hutu Violence, Pre-Genocide			−0.694		−0.980
			(0.496)		(1.736)
... × RPF Violence, Pre-Genocide			2.853	2.694	
			(1.570)	(1.535)	
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
R^2	0.27	.	.	.	.
N	1432	1432	1432	1432	1432

Notes: The **Total Violence, Pre-Genocide** dummy takes on the value of 1 if a village experienced any violence before the genocide (RPF or Hutu army driven). The **RPF Violence, Pre-Genocide** dummy takes on the value of 1 if a village experienced any RPF violence before the genocide. The **Hutu Violence, Pre-Genocide** dummy takes on the value of 1 if a village experienced solely Hutu-driven violence against the Tutsi before the genocide. **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 calender 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 calender 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, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual 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. In each column, I also control for all main effects and double interactions. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in parentheses, Conley (1999).

Table A.33: RTLM Interaction Effects

	# Civilian Perpetrators, log						
	Full Sample		Radio Coverage above Median	Radio Coverage below Median	Radio Coverage above 75th	Radio Coverage below 75th	Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Militiamen, log	1.037	1.406	1.064	0.925	0.243	1.293	1.007
	(0.741)	(0.682)	(0.429)	(0.460)	(0.366)	(0.455)	(0.303)
	[16.550]	[22.720]	[0.262]	[0.392]	[0.289]	[0.449]	[0.246]
... x RTLM Radio Coverage, dummy (median)	0.021						
	(0.790)						
	[8.419]						
... x RTLM Radio Coverage, dummy (75th)		−0.728					
		(0.469)					
		[11.390]					
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
Propagation Controls	yes	yes	no	no	no	no	no
Additional Radio Controls	yes	yes	no	no	no	no	no
Province Effects	yes	yes	yes	yes	yes	yes	yes
N	1056	1056	529	527	264	792	1056

Notes: **Regressions 1, 2 and 7** use the full RTLM sample matched with my sample. In **regressions 3 and 4** the sample is split at the median value of RTLM coverage. In **regressions 5 and 6** the sample is split at the 75th percentile of RTLM coverage. **Standard Controls**, **Growing Season Controls**, and **Additional Controls** are defined in Table 3 in the paper. **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 Radio Controls** include sloping dummies. All control variables from my sample, except "Number of Days with RPF presence," are in logs. I always control for all main effects and double interactions. Interactions are first logged and then interacted. **Standard errors** correcting for spatial correlation within a radius of 150km are in square parentheses, Conley (1999). **Standard errors** clustered at the commune level are in regular parentheses.

A.7 Extensions to Section VI. – Modeling the Force versus Obedience Channel

To support the anecdotal evidence presented in the paper, in this section I model the two scenarios outlined in the paper (militia as a moral authority versus militia had to use force) and test their theoretical implications. Note upfront that these theoretical results are based on certain assumptions about the militia's fighting technology (which can be backed up by anecdotal evidence). They should nevertheless be taken with a grain of salt.

A.7.1 Obedience Model versus Force Model

Set Up Assume that there are N villages. Each village is inhabited by a Hutu population of size 1, for simplicity, and a Tutsi population of size T. In each village, there might already be local armed groups such as policemen M_l or RPF Tutsi rebels R. Anecdotal evidence suggests that there are fewer local militiamen in villages with a large Tutsi minority or Tutsi rebels, i.e. $\partial M_l / \partial S < 0$ with $S = T, R$.[38] I call T and R the strategic factors S.

Imagine that the N villages can be of two types $j \in \{o, w\}$: those that do not oppose the militia (w) and those that oppose the militia (o).[39] Together with the local armed groups, the external militiamen M_e turn ordinary civilians into civilian killers at a decreasing rate by teaching and organizing them.[40]

$$K^j = K(M^j) \cdot C^j, \tag{A.12}$$

where $M^j = M_e^j + M_l^j$. C^j equals the number of Hutu participating in the training. For simplicity, I assume that all Hutu villagers join in with the training,

[38] In places with large Tutsi minorities, the political leaders were likely to be from opposition parties and thus have their own anti-genocide militia and police force. Furthermore, places under the control of the RPF at the beginning of the genocide were unlikely to have any pro-genocide militia at all. Besides that, the pro-genocide militia in those places with large Tutsi minorities might have been less well prepared and equipped for genocide and thus had lower effects on civilian participation in general.

[39] Opposing is defined in an active way, i.e. fighting the militia (and not just *innerly* opposing the militia).

[40] Anecdotal evidence that armed groups would usually call all Hutu civilians together in one location, and then instruct and organize them, implies decreasing effects of the militia (Hatzfeld, 2005; Gourevitch, 1998).

thus $C^j = 1$.[41] I further assume that $K_M > 0$ and $K_{MM} < 0$.

Non-opposing Villages As mentioned, in non-opposing villages all the Hutu join in with the training, thus $C^w = 1$. The (expected) number of civilian killers is therefore

$$E(K^w) = K(M_e + M_l(S)). \tag{A.13}$$

Opposing Villages Hutu villagers in opposing villages fight the Hutu militia together with the Tutsi civilians T and rebels R, thus the opposing population equals $P = 1 + T + R$. If the militia wins, then all Hutu have to join the militia in the training, i.e. $C^o = 1$, otherwise nobody joins, $C^o = 0$. The militia's winning probability is given by a contest function

$$p = I(\gamma M, P), \tag{A.14}$$

where $\gamma > 1$ measures the militia's superiority, they often carry guns. Furthermore, $I(0,P) = 0$ and p lies between 0 and 1. I make the following assumptions on the derivatives (Skaperdas, 1992)

1. $I_M > 0$ and $I_P < 0$,
2. $I_{MM} \gtreqless 0$ as $\gamma M \lesseqgtr P$,
3. $I_{MP} \lesseqgtr 0$ as $\gamma M \lesseqgtr P$,

which can be backed by anecdotal evidence (Dupuy, 1987).[42]

[41] This assumption does not seem too far fetched in particular since even women and children took part in the killings.

[42] Assumption 1 states that the more militiamen that join in the fight against the Hutu and Tutsi civilians and rebels, the higher the chances of winning ($I_M > 0$), and vice versa ($I_P < 0$).

Furthermore, as long as the number of militiamen is small, each additional militiaman joining the fight has a larger effect on winning than the one before ($I_{MM} > 0$ as $\gamma M < P$). However, once a certain threshold has been crossed, i.e. $\gamma M > P$, the marginal returns to having an additional militiaman joining the fight begin to decrease since the chances of winning are high anyway ($I_{MM} < 0$ as $\gamma M > P$). This seems to be the case for military contexts (Dupuy, 1987).

The third assumption states that when the militiamen are anyway struggling to win, increasing the opponents' strength reduces the effects of an additional man even further ($I_{MP} < 0$ as $\gamma M < P$). On the other hand, if the militia is sufficiently strong, an increase in the opponents' strength will increase the effects of an additional militiaman ($I_{MP} > 0$ as $\gamma M > P$).

An example of a contest function satisfying the assumptions is $I(M,P) = \frac{(\gamma M)^\beta}{(\gamma M)^\beta + P^\beta}$ with $\beta > 1$.

The expected number of civilian killers in opposing villages is thus (there are no local militiamen in opposing villages)

$$E(K^o) = I(\gamma M_e, 1+T+R) \cdot K(M_e). \quad \text{(A.15)}$$

Predictions In the following, I assume that the number of militiamen is relatively small, i.e. $\gamma M \leq P$, which is true for the vast majority of villages included in the data (for reasonable values of γ). This gives the following predictions

Prediction C1. *Expected civilian participation $E(K^j)$ is convex (strictly concave) in the number of external militiamen M_e if Hutu villagers are opposing (not opposing) the genocide: $\partial^2 E(K^o)/\partial M_e^2 \geq 0$ $\left(\partial^2 E(K^w)/\partial M_e^2 < 0\right)$.*

Prediction C2. *The larger the strategic factor S, the smaller (larger) are the effects of the number of external militiamen M_e on expected civilian participation $E(K^j)$ if Hutu villagers are opposing (not opposing): $\partial^2 E(K^o)/\partial M_e \partial S < 0$ $\left(\partial^2 E(K^w)/\partial M_e \partial S > 0\right)$.*

The proofs are presented in Section A.8. Since the first stage provides exogenous variation in the number of external militiamen, all predictions are stated with respect to M_e.

Prediction C1 states that in non-opposing villages, the first militiaman arriving has a larger effect on civilian participation than the second and so on. In opposing villages civilians fight against the militia. Thus, the first man arriving has little effect on civilian participation, but with every additional man this effect increases.

Prediction C2 says that in non-opposing villages, one additional external militiaman has a larger effect on civilian participation when the Tutsi minority is large or Tutsi rebels are present.[43] On the other hand, in opposing villages, as long as the number of militiamen is sufficiently small, a large Tutsi minority or Tutsi rebels decrease the militia's effect on civilian participation because 1) the Tutsi will join the fight against the militia and will thus reduce the militia's chances of winning and 2) the militiamen are anyway struggling to win.

[43] Intuitively, in non-opposing villages with a large Tutsi minority or Tutsi rebels, there are fewer local militiamen thus, given the concavity of the production function, an additional external man has a larger effect.

A.7.2 Results

Prediction C1: Functional Form Supporting the obedience channel, the effects of an additional militiaman seem to be decreasing. The concave relationship between civilian perpetrators and militiamen is presented graphically in Figure A.21, using nonparametric local mean smoothing with an Epanechnikov kernel, conditional on the controls from my preferred specification (regression 6 in Panel B in Table 3 in the paper) and instrumenting for the number of militiamen. Furthermore, when I regress civilian participation (residuals) on a second-order polynomial in the militiamen residuals from Figure A.21, the square term is negative and highly significant at the 99 percent level, again confirming the concave relationship.[44] The coefficient on the square term is graphically depicted in Figure A.22, to the far right of the x-axis, labeled *Full* (sample).[45]

However, the result must be taken with a pinch of salt since the nonlinearities in the second stage might be driven by nonlinearities in the first stage. Reassuringly though, the first-stage relationship looks reasonably linear.[46]

Prediction C1: Interaction Effects Note that the results shown in Section A.6 (Table A.31) clearly support the obedience channel: the effect of the militia is larger in places with Tutsi Rebels and a strong Tutsi minority. Besides, further consistent with the model, the effects seem to be weaker in places with RTLM coverage.[47]

Identifying Opposing Villages The empirical evidence suggests that the militiamen functioned as a moral authority for the whole sample of villages and the bulk of anecdotal evidence supports this view. The same anecdotal evidence,

[44] The concave relationship might simply mirror differences in militia quality. For instance, extremely successful militiamen might have been operating in small groups whereas the badly-trained/badly-equipped militia might have moved around in larger groups (with some well-trained men to guide them). However, since anecdotal evidence suggests that the well-trained/well-equipped troops often led the larger operations which resulted in massive killings, I drop those villages with mass graves and rerun the analysis. Importantly, the effects are still equally concave (Figure A.25 in Section A.7.3 below).

[45] Restricting the sample to those villages where militiamen make up less than 5 percent or less than 2.5 percent of the population, i.e. where $\gamma M \leq P$, still delivers equally concave effects (Figures A.23 and A.24 in Section A.7.3 below).

[46] Figure A.26 in Section A.7.3.

[47] RTLM coverage increases the number of local militia men and thus reduces the marginal effect of an additional external militia member.

however, also suggests that in some villages civilians did oppose the militia. Identifying those, potentially few, villages is not only interesting in itself but also allows me to test the predictions of the force model. In particular, anecdotal evidence suggests that villages with a large proportion of ethnically mixed households were more likely to oppose the militia, since civilians would be more willing to resist when their family members' and friends' lives were at risk (Des Forges, 1999).

Summing up over all Hutu and Hutu-Tutsi households F_c in a commune c (remember that ethnicity data is only available at the commune level), I define intra-household ethnic polarization as[48]

$$IHEP_c = \sum_{i=1}^{F_c} \frac{N_{ic}}{N_c} \cdot h_{ic} \cdot t_{ic}, \tag{A.16}$$

where N_c is the total number of people in all households F_c in commune c, N_{ic} the number of people in household i and h_{ic} is the fraction of household members in household i that are Hutu and t_{ic} the fraction that are Tutsi, respectively. The higher this measure is, the higher the chances that civilians in those villages opposed the militia.

In Figure A.22 I report the coefficients on the square term from regressions of civilian participation (residuals, netting out all controls) on a second-order polynomial in the militiamen residuals from Figure A.21, for different percentiles of intra-household ethnic polarization. Interestingly, for villages with high levels of intra-household ethnic polarization (up to the 91st percentile), i.e. those where one would expect resistance, the effects of an additional militiaman are increasing (the point estimates on the square term are positive and significant), as predicted by the force model (Prediction C2). From the 90th percentile onwards, point estimates turn insignificant and finally negative for the full sample of villages.[49]

Furthermore, anecdotal evidence suggests that these villages with high lev-

[48]Note that I do not include pure-Tutsi households. The reason is that pure-Tutsi households would reduce the polarization measure, since it is symmetric, but they do not reduce the likelihood of opposition.

[49]The convex relationship between civilian perpetrators and militiamen for high levels of intra-household ethnic polarization is also presented graphically in Figure A.27 in Section A.7.3 below. However, sample sizes are small and the results should therefore again be interpreted with caution.

els of intra-household ethnic polarization were opposing the genocide. For example, Des Forges (1999) writes that in Huye commune (97th percentile), both Hutu and Tutsi civilians fended off attackers from outside. Des Forges (1999, p. 350) continues that a witness from the commune of Ngoma (98th percentile) recalls that *"Kanyabashi (the burgomaster) urged the people of Cyarwa to avoid violence and to fight together against attacks."* On a more general note, many of the communes with high intra-household polarization are located in the southwest of Rwanda, where the opposition was overall more pronounced.

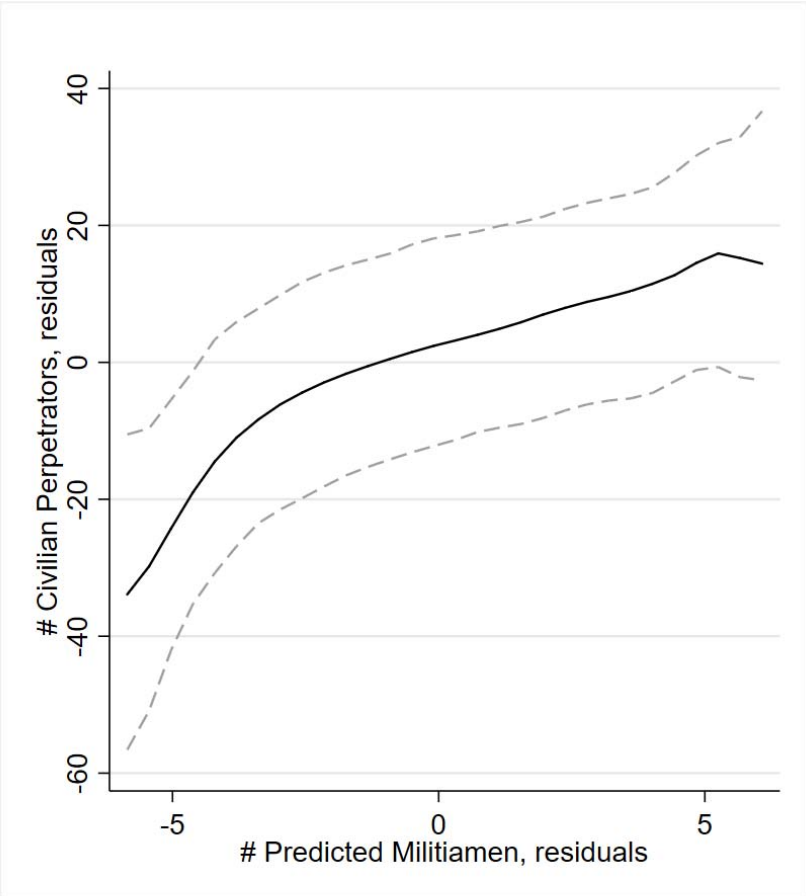
References

[1] **Des Forges, A.** 1999. *Leave None to Tell the Story: Genocide in Rwanda*, New York: Human Rights Watch.

[2] **Dupuy, T. N.** 1987. *Understanding War*, New York: Paragon House.

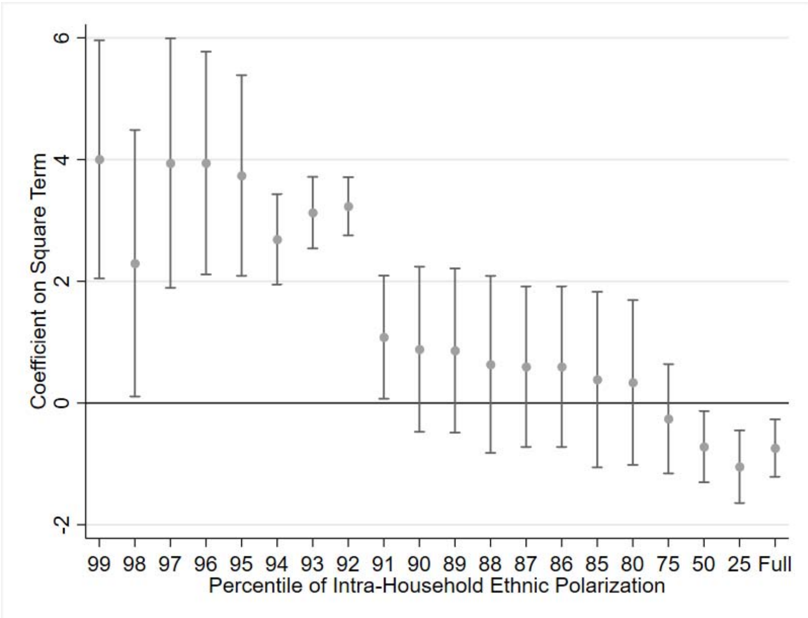
[3] **Skaperdas, S.** 1992. Cooperation, Conflict and Power in the Absence of Property Rights, *American Economic Review*, 82(4): 720-739.

Figure A.21: Functional Form, Obedience Model or Force Model



Notes: Local mean smoothing (Epanechnikov kernel, bandwidth=2.5, observations are grouped into 30 equal-sized bins). 95 percent confidence intervals are bootstrapped. Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Panel B in Table 3 in the paper) are used to construct residuals.

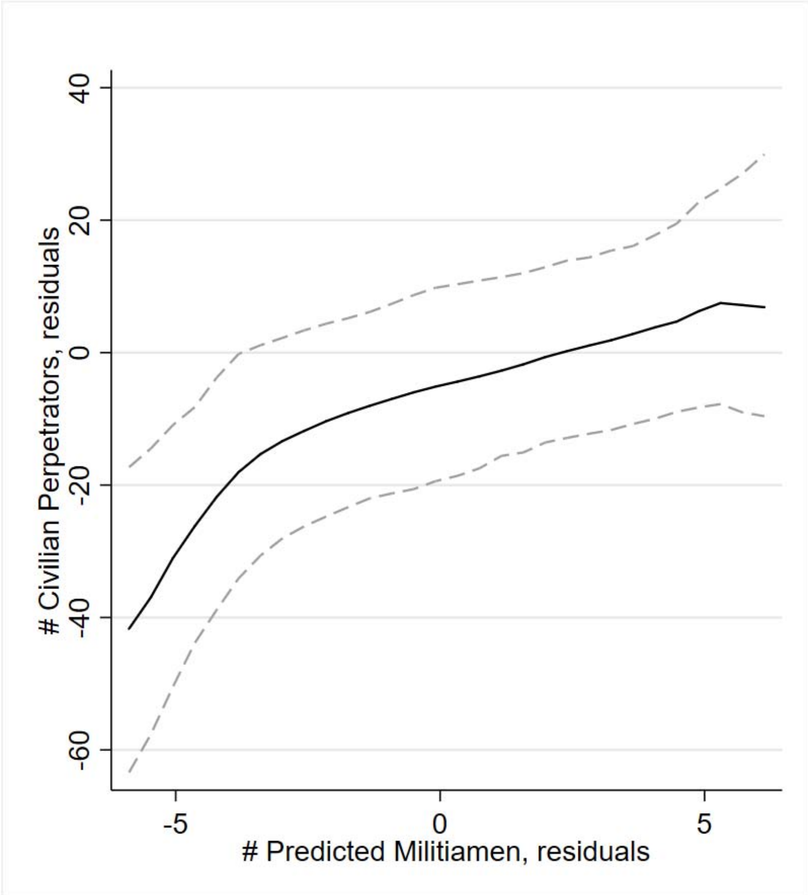
Figure A.22: Identifying Opposing Villages (Convex and Concave Effects)



Notes: I run regressions of the number of civilian perpetrators (residuals) on a second-order polynomial in the residuals of the predicted number of militiamen for different subsamples defined by different percentiles of my intra-household ethnic polarization measure (x-axis). The coefficients on the square terms (indicating the curvature) are reported together with 95 percent confidence intervals on the y-axis. Intra-household ethnic polarization is defined in equation (A.16). Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Panel B in Table 3 in the paper) are used to construct residuals.

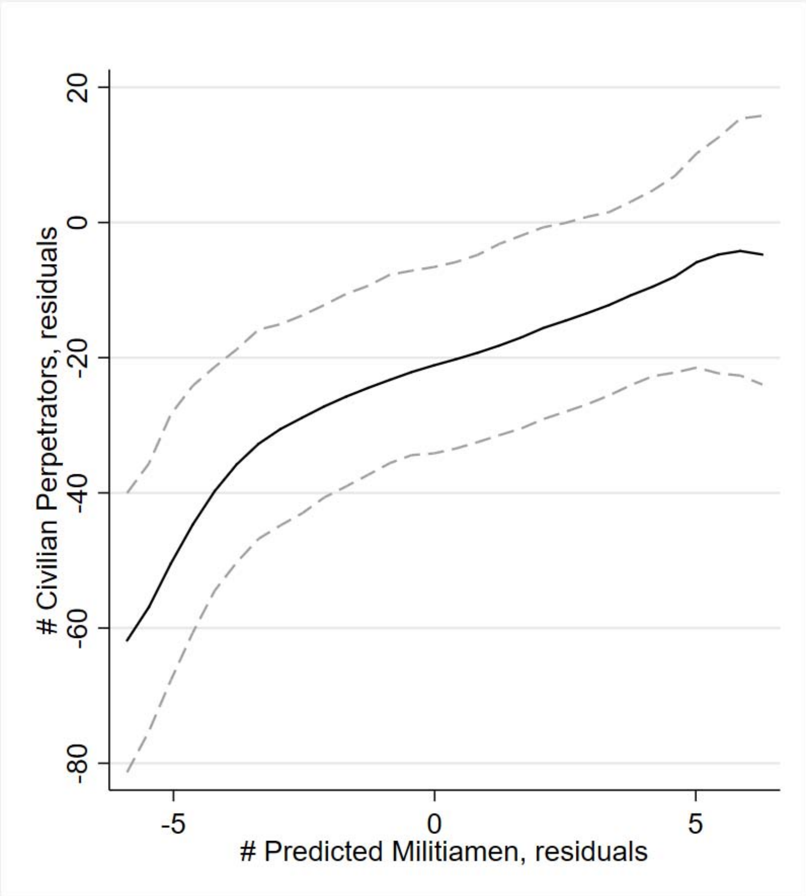
A.7.3 Additional Figures and Tables

Figure A.23: Functional Form (# Militiamen/Population $\leq$ 5 percent)



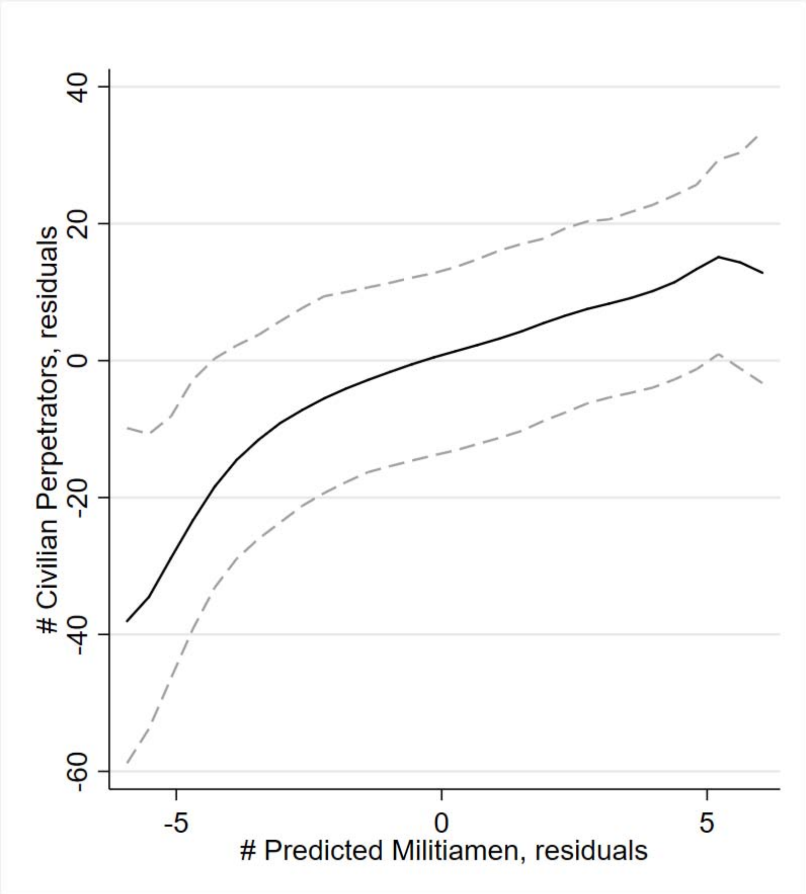
Notes: Local mean smoothing (Epanechnikov kernel, bandwidth=2.5, observations are grouped into 30 equal-sized bins). 95 percent confidence intervals are bootstrapped. Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Panel B in Table 3 in the paper) are used to construct residuals. The sample is restricted to villages where militiamen make up less than 5 percent of the village population.

Figure A.24: Functional Form (# Militiamen/Population $\leq$ 2.5 percent)



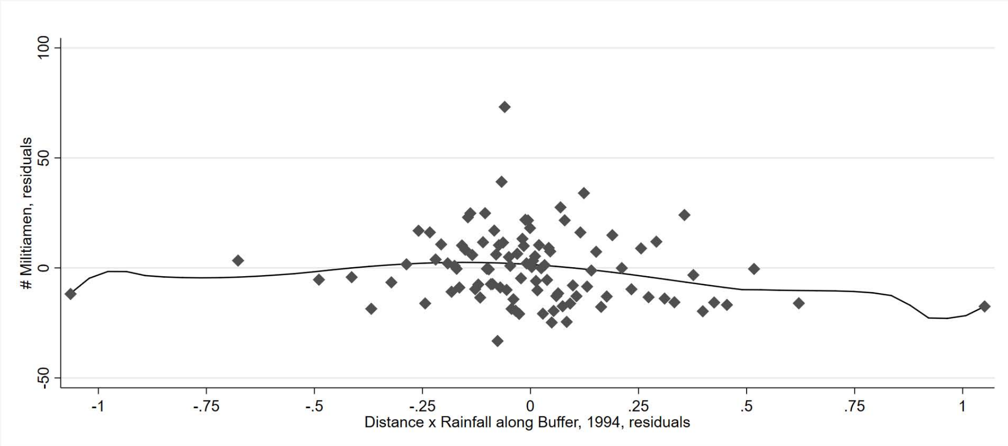
Notes: Local mean smoothing (Epanechnikov kernel, bandwidth=2.5, observations are grouped into 30 equal-sized bins). 95 percent confidence intervals are bootstrapped. Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Panel B in Table 3 in the paper) are used to construct residuals. The sample is restricted to villages where militiamen make up less than 2.5 percent of the village population.

Figure A.25: Functional Form (Excluding Villages with Mass Graves)



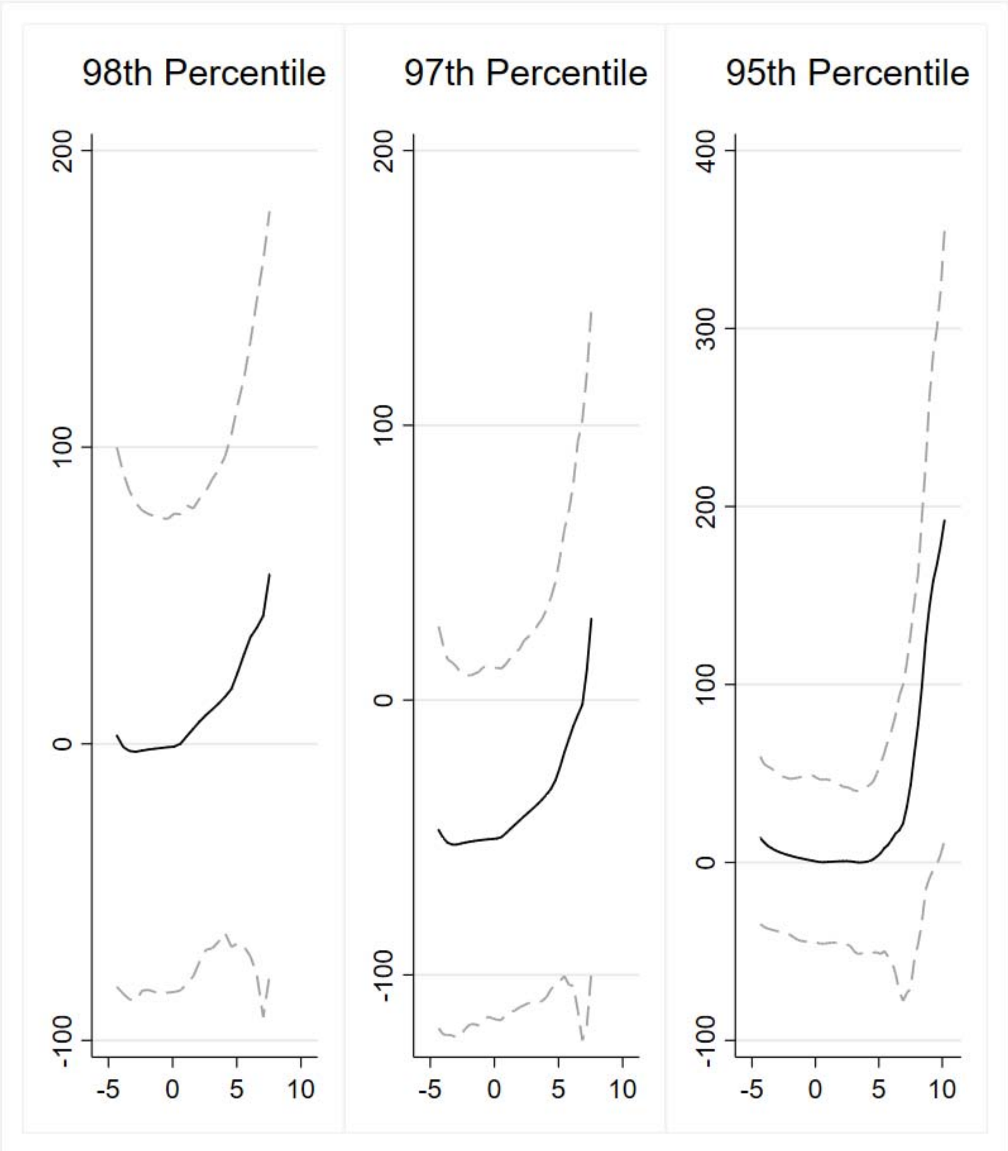
Notes: Local mean smoothing (Epanechnikov kernel, bandwidth=2.5, observations are grouped into 30 equal-sized bins). 95 percent confidence intervals are bootstrapped. Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Panel B in Table 3 in the paper) are used to construct residuals. The sample is restricted to villages where no mass grave was found.

Figure A.26: Linear First-Stage Relationship



Notes: Local nonparametric estimation (Epanechnikov kernel, bandwidth=0.7, observations are grouped into 100 equal-sized bins). Armed Groups' Transport Cost is the instrument (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 3 in Panel A in Table 3 in the paper) are used to construct residuals.

Figure A.27: Opposing Villages (Convex Effects)



Notes: Y-axis: # Civilian Perpetrators, residuals. X-axis: # Predicted Militiamen, residuals. Local mean smoothing (Epanechnikov kernel, bandwidth=3). 95 percent confidence intervals are bootstrapped. Samples restricted to 98th, 97th and 95th percentile of intra-household ethnic polarization. Intra-household ethnic polarization is defined in equation (A.9) in the paper. Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Panel B in Table 3 in the paper) are used to construct residuals.

A.8 Proofs

Prediction C1 – Functional Form

1. **Non-opposing Villages**: The result follows directly from the assumption that $K_{MM} < 0$.

2. **(i) Opposing Villages,** $\gamma M \leq 1+T+R$: Since the second derivative of $H(M,P) = I(M,P) \cdot K(M)$ involves $K_{MM} < 0$, which is negative, the result does not follow directly from differentiation. To show that $H(M,P)$ is convex in M note that convexity of $I(M,P)$ implies that for any two points $M_1 \geq 0$ and $M_2 \geq 0$ and λ between 0 and 1, we have

$$\lambda I(M_1) + (1-\lambda) I(M_2) \geq I(\lambda M_1 + (1-\lambda) M_2). \tag{A.17}$$

Now, set $M_2 = 0$. This gives

$$\lambda I(M_1) \geq I(\lambda M_1). \tag{A.18}$$

Multiply both sides by $K(M_1) \geq 0$ to get

$$\lambda I(M_1) \cdot K(M_1) \geq I(\lambda M_1) \cdot K(M_1). \tag{A.19}$$

Note that since $K(M)$ is strictly increasing

$$\lambda I(M_1) \cdot K(M_1) \geq I(\lambda M_1) \cdot K(M_1) > I(\lambda M_1) \cdot K(\lambda M_1). \tag{A.20}$$

Rearranging gives

$$\lambda H(M_1) > H(\lambda M_1), \tag{A.21}$$

which implies convexity of H.

2. (ii) Opposing Villages, $\gamma M > 1+T+R$: Since both $I(M,P)$ and $K(M)$ are concave functions once $\gamma M > P$, the curvature of the product of the two is ambiguous and depends on functional forms. However, since $I(M,P)$ has to approach 1 and thus $I(M,P) \cdot K(M)$ will approach $K(M)$ the effects eventually will turn concave. To illustrate that the product of two concave functions can either be concave or convex consider $I(M,P) = \frac{M^\alpha}{P}$ (as long as $M^\alpha < P$) with $0 < \alpha < 1$ and $K(M) = M^\beta$ with $0 < \beta < 1$. The resulting product $H(M,P) = \frac{M^{\alpha+\beta}}{P}$ is convex if $\alpha + \beta \geq 1$ but strictly concave otherwise.

Prediction C2 – Interactions

1. Non-opposing Villages: Take the derivative of $E\left(K^{w}\right)$ w.r.t. S and M_e to get

$$\frac{\partial E\left(K^{w}\right)}{\partial S \partial M_e} = K_{MM}(M_e + M_l(S)) \cdot \frac{\partial M_l}{\partial S}. \tag{A.22}$$

The result follows immediately, since both terms in the product are negative.

2. (i) Opposing Villages, $\gamma M \leq 1 + T + R$: Take the derivative of $E\left(K^{o}\right)$ w.r.t. S and $M_e = M$ to get

$$\frac{\partial E\left(K^{o}\right)}{\partial S \partial M} = I_{MP}(M,P) \cdot K(M) + I_P(M,P) \cdot K_M(M). \tag{A.23}$$

The result follows immediately, since the first term in the sum is non-positive and the second term is negative.

2. (ii) Opposing Villages, $\gamma M > 1 + T + R$: Now $I_{MP}(M,P) > 0$, thus $\frac{\partial E(K^{o})}{\partial S \partial M}$ in equation (A.23) is ambiguous.