Armed Groups, Civilians and Muddy Roads: The Conduct of Political Mass Murder *

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Abstract

Do political elites use armed groups to foster civilian participation in violence or are civilian killers driven by unstoppable ancient hatred? If armed groups matter, are they allocated strategically to maximize civilian participation? How do they mobilize civilians? I empirically investigate these three questions using village-level data from the Rwandan Genocide in 1994. To establish causality, I use an instrumental-variables strategy. Specifically, I exploit cross-sectional variation in armed groups' transport costs induced by exogenous weather fluctuations: the shortest distance of each village to the main road interacted with rainfall along the dirt tracks between main road and village. Guided by a simple model, I come up with the following answers to the three central questions: (1) one additional armed-group member resulted in 7.3 more civilian perpetrators, (2) armed-group leaders rationally responded to exogenous transport costs and dispatched their men strategically to maximize civilian participation, (3) for the majority of villages, armed groups acted as role models and civilians followed orders, but in villages with high levels of cross-ethnic marriages civilians had to be forced to join. These results pass a number of indirect tests regarding the exclusion restriction as well as other robustness checks. I argue that the results are also relevant for other cases of state-sponsored murder, such as the killings of the Jews in Lithuania in the 1940s. Finally, a back-of-the-envelope calculation suggests that a military intervention targeting the various armed groups - only 10 percent of the perpetrators but responsible for at least 83 percent of the killings - could have stopped the Rwandan Genocide.

JEL classification: D74, N47
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1 Introduction

In many genocides and civil wars, ordinary civilians with no military affiliation or military training whatsoever turn into killers. To illustrate, during the Rwandan Genocide in 1994 Hutu perpetrators killed approximately 800,000 people of the Tutsi minority in just about 100 days (Prunier, 1995). This astounding number of deaths could only be achieved because hundreds of thousands of civilians (about 85 percent of the total number of perpetrators) joined the militia and the army in the killings. In light of the immense human suffering, it is crucially important - especially for international policy makers contemplating an intervention - to understand the factors that trigger civilian participation.

Two competing views coexist. In one view, civilian participation is often interpreted as an unstoppable outbreak of ancient hatred, usually fought along ethnic lines, ruling out a successful foreign intervention.[1] Journalism, policy makers, and some international relations scholars popularized this view (Friedman, 1995; Kaldor, 1999; Kaplan, 1994). There was no foreign intervention in Rwanda. Promoting the other view, some observers argue that political elites strategically use their armed groups to trigger civilian participation (Brown, 1996). Armed groups are naturally of much smaller size and thus potentially easier to stop. For example, Brigadier General Romeo Dallaire - the Canadian commander of the UN force in Rwanda - insisted that with 5,000 to 8,000 well-equipped troops he could have prevented the Rwandan genocide, by stopping the various militia and army groups in the capital Kigali and other big cities from spreading into the country.

This paper provides the first empirical analysis of how important elite-level-controlled armed groups might be in inducing civilians to participate in killings. It answers three questions: How much do armed groups affect civilian participation? Do armed-group leaders allocate their men strategically in order to maximize civilian participation? How are civilians mobilized? In answering these questions, I focus on the Rwandan Genocide, to my knowledge the only conflict where data on civilian and armed-groups violence is separately available at a local-village level.

The main difficulty in estimating the effects of armed groups on civilian participation arises from joint determination and reverse causality. Furthermore the direction of the bias is *a priori* unclear. On the one hand, village-specific unobservable characteristics that affect both civilian and armed-group violence, for instance local leader quality, could produce a spurious positive correlation between the two, biasing the estimate upwards. On the other hand, if army and militia were strategically sent into areas where civilian participation was

[1] To illustrate, one retired US admiral remarks on the subject, referring to the Bosnian War: *"Let them fight. They've been fighting for a thousand years."* (Rear Admiral James W. Nance (ret.) is quoted in Tom Ashbrook (1995)). Similarly, Mueller (2000, pp. 65-66) explains the rationale behind the inactiveness of the international community *"First, they [the international community] assumed that the wars were essentially inexplicable Kaplanesque all-against-all conflicts, rooted in old hatreds that could hardly be ameliorated by well-meaning, but innocent and naïve, outsiders."*

unobservably low, the estimate would be downward biased.[2]

To overcome these endogeneity issues, I use an instrumental-variables strategy based on an exogenous measure of transport costs to estimate the effect of armed groups on civilian participation in civil conflict. More specifically, I exploit two sources of variation. First, I exploit variation in distance to the main road. There is abundant anecdotal evidence that army and militia troops were sent around the entire country to promote the killings. Because the few main roads crossing the country in 1994 were the only ones in reasonable condition, I expect areas further away from these main roads to be more costly to reach by army and militia. However, distance to the road is certainly correlated with other, possibly unobservable, determinants of civilian violence such as education, health or income. Therefore, I further exploit variation in rainfall during the period of the genocide, introducing a novel, high resolution rainfall data set. In particular, my instrument is the distance to the main road interacted with rainfall during the period of the genocide along the dirt tracks between each village and the closest point on the main road (technically, rainfall will be measured along a 500 meter buffer around the shortest distance line).[3] The idea is simple: I expect the movements of army and militia, performed by motor vehicles, to be limited by the heavy rains that characterize the first rainy season, which partly overlaps with the genocide, and the more so the further they have to travel.

To ensure that the instrument solely picks up armed groups' transport costs I first control for the main effects of the instrument components, in particular distance to the road. Second I control for distance to the road interacted with rainfall between village and road during the 100 calendar days of the genocide of an *average year* (taken over the 10-year period 1984 to 1993). This way, I only exploit the seasonal weather variation in the year of the genocide.[4] Finally I control for rainfall during the 100 genocide days in 1994 and its long-term average in each village, that is at the armed group's destination. By doing so, I ensure that identification only stems from short-term variation in rainfall *along the distance* measure, which is arguably exogenous and should only affect armed groups' transport costs.[5]

A remaining concern regarding the excludability of the instrument is that villages that were difficult to reach by armed groups might have also been difficult to reach by traveling civilian killers or informants. However, civilian violence was very localized - neighbors killing neighbors - and I will argue in great detail why this concern is unwarranted.

[2]In addition, measurement error might bias the OLS estimate downwards.

[3]A village corresponds to the Rwandan administrative unit of a sector with an average size of 14 square kilometers and 4,900 inhabitants.

[4]The genocide lasting only 100 days is another advantage for the identification strategy as this rules out the presence of time confounding factors. Technically the genocide lasted a little more than 100 days. However, I will always refer to the "100 days" as the genocide period.

[5]Rainfall in each village might be correlated with malaria prevalence or civilians' transport costs within the village, both of which are likely to directly affect civilian participation. All long-term averages are based on the 10-year period 1984 to 1993, for which data is available.

I proxy for armed-group and civilian violence by the number of people prosecuted for armed-group genocide violence and civilian violence in the Gacaca courts. About 10,000 of these local courts were set up all over the country to prosecute the crimes committed during the genocide. Using prosecution numbers instead of actual participation might introduce some bias. However, I show that the Gacaca data is strongly correlated with other measures of violence from various different sources. I also directly take potential bias into account in the empirical analysis.

The OLS results indicate a positive relationship between armed-group and civilian violence: a 1 percent increase in the number of militiamen is associated with a 0.63 percent increase in civilian participation. In contrast, the instrumental-variable estimates are about twice as large: 1.3. The numbers imply that, on average, one additional militiaman resulted in 7.3 more civilian perpetrators and, under a linearity assumption, in 12 more deaths. Henceforth, I will use the two expressions armed groups and militiamen interchangeably.

The local average treatment effect I identify has a straightforward and policy-relevant interpretation: I measure the effect of *external* militiamen, those men sent around by the genocide planners and thus affected by transport costs, excluding the effect of the various *local* militiamen such as policemen, already present in the village. Since these external militiamen, around 50,000 men strong, were initially stationed in Kigali and other big cities and only afterwards spread around the entire country a quick military intervention could have potentially stopped them, let alone that they were often badly equipped. Furthermore, the instrumental-variable estimates imply that stopping those 50,000 men would have cut the number of perpetrators by about 83 percent. The number of deaths would probably have gone down even more since external militiamen arguably had higher killing rates than civilians (if I assume that militiamen killed five times as many people as civilian perpetrators, the number of deaths would have fallen by almost 90 percent).

Although many scholars and policy makers recently argue that a military intervention in Rwanda could have been successful, this view is not uncontested. In particular, critics of a foreign intervention in Rwanda usually argue that an intervention would have not been quick enough to reach every corner of the country (Kuperman, 2000). My results suggest that a full-blown intervention, i.e., also targeting the rural areas, would have not been necessary and that a quick military intervention targeting the various militia and army groups could have stopped the genocide.

In the second main part of the paper, I find that the central planners in Kigali can be seen as rational actors who allocated their armed groups strategically. I model a genocide planner who wants to maximize civilian participation but faces a transport constraint and find strong empirical support for the predictions of the model. Importantly, one of the predictions is the first-stage relationship, providing the theoretical foundation for my instrumental-variables strategy.

In the last main part of the paper, I examine different recruiting channels through which armed groups might have spurred civilian participation. A natural question is whether the militia needed to force opposing civilians to join in the killings or whether they rather organized the killings and taught civilians how to kill. Unfortunately I do not have data to directly distinguish between these two possibilities. Instead, I test the theoretical implications of the force versus role model scenarios. The results suggest that, at least on average, villagers were not actively opposing the militia but that the militia rather functioned as a role model.

Finally, in a first extension I show that the militia's physical presence in each village was necessary to mobilize civilians. This is important especially from a policy perspective because it implies that a genocide planner cannot simply compensate for an absence of his men for instance by stirring up radio propaganda. In a second extension, I show that a subset of villages with high levels of cross-ethnic marriages, about 8 percent of the sample, seemed to have opposed the militia: I can link some of these villages to anecdotal evidence of Hutu opposition against the genocide and I present suggestive empirical evidence that the predictions of the force model are fulfilled for those villages.

To alleviate concerns that the Rwandan Genocide might be a very special case, I also briefly discuss other cases of state-sponsored murder. In particular, I provide both anecdotal and suggestive empirical evidence that the killings of the Jews in Lithuania in the 1940s, organized by the Germans but mostly carried out by local civilians and militias, parallels the Rwandan Genocide in all three ways stressed in this paper. Other examples where elite groups fostered civilian participation in violence include the Cultural Revolution in China in the 1960s, the long-lasting civil conflict in Guatemala (1950s onwards) or recent examples such as the 2007 post-election violence in Kenya.

My paper contributes to the literature in several ways. First of all, it adds to the vast conflict literature. Blattman and Miguel (2010) give an excellent review of this research, vehemently calling for well-identified and theoretically grounded studies on the roots of individual participation in violent conflict and the strategic use of violence. This paper starts filling the gap by providing novel evidence on the strategic use of armed-group violence, the strong effects that armed groups have on civilian participation and on some recruiting mechanisms. Recent studies on the determinants of conflict and participation in violence consider institutions, government policy, income, foreign aid and propaganda (Besley and Persson, 2011; Dell, 2012; Dube and Vargas, 2013; Mitra and Ray, forthcoming; Nunn and Qian, 2014; Yanagizawa-Drott, forthcoming, respectively). Several other studies have analyzed the recruitment of civilians. Although very informative, these studies are mostly descriptive drawing on self-reported survey data (Arjona and Kalyvas, 2008; Humphrey and Weinstein, 2004, 2008; Pugel, 2007; Weinstein, 2007). Furthermore, my paper complements the literature on the Rwandan Genocide (Friedman, 2010; Straus, 2004; Verpoorten,

2012a-c; Verwimp, 2003, 2005, 2006; Yanagizawa-Drott, forthcoming) by providing novel evidence on its conduct and organization.

Regarding the importance of transport costs my paper contrasts with recent contributions by Banerjee et al. (2012) and Donaldson (forthcoming) that highlight the positive economic effects of low transport costs. My findings loosely echo those in Nunn and Puga (2012), who show that high transport costs in Africa, in their case caused by rugged terrain, have positive effects on today's welfare because they hindered slave traders.

On the methodology side, my findings speak to the recent discussion on the effects of rainfall on conflict other than through the income channel (Iyer and Topalova, 2014; Sarsons, 2011). Prominent studies that use rainfall as an instrument for income in Africa include Brückner and Ciccone (2010), Chaney (2013) and Miguel, Satyanath and Sergenti (2004). My results suggest that especially in areas with poor infrastructure, such as Africa, rainfall might have negative direct effects on conflict through transport costs.

The remainder of the paper is organized as follows. Section 2 provides some background information on the Rwandan Genocide. Section 3 presents the data used for the analysis. Sections 4 to 6 each answer one of the three central questions of the paper. Section 7 discusses the external validity of the results and Section 8 concludes with possible policy implications.

2 Institutional Background

The history of Rwanda is marked by the conflict between Hutu and Tutsi, the two major ethnic groups living in the country. This section summarizes the key moments in their history, before describing in more detail the 1994 Genocide.[6]

A History of Conflict The distinction between the Hutu and Tutsi in Rwanda is strongly debated. Some argue that the Tutsi, with a population share of around 10 percent clearly the minority, descended from Hamitic migrants from Egypt or Ethiopia and that the Hutu belong to the Bantu group, who lived in Rwanda for much longer; others say that the two groups, in fact, share a common ancestry. What goes undisputed is that Belgian colonizers, who took over Rwanda after World War I, radicalized the differences between the two groups, establishing an official register to record the ethnicity of each citizen and explicitly favoring the Tutsi minority - believed to be the superior ethnic group - through reserving them access to administrative posts and higher education.

When the country gained independence in 1962 the Hutu managed to take over power, reversing the political situation and establishing a one-party state. The ethnic violence that

[6]Refer to Dallaire (2003), Des Forges (1999), Gourevitch (1998), Hatzfeld (2005, 2006), Physicians for Human Rights (1994) and Straus (2006) for further details.

accompanied the event led several hundreds of thousands of Tutsi to flee the country. In the following decade periods of relative political stability and peace altered with episodes of unrest and violence, but tensions never ceased. In 1975, following a military coup, Habyarimana created the Hutu-dominated National Revolutionary Development Movement (MRND), the only political party legally authorized in the country, and in 1978 he officially became the new president of Rwanda.

By 1990 the country was still under Habyarimana leadership and was still facing an uneasy coexistence between the political and administrative Hutu elite and the economic Tutsi elite. The situation degenerated towards the end of the year, when the Rwandan Patriotic Front (RPF) - a rebel army mostly composed of Tutsi exiles eager to replace the Hutu-led government - started launching attacks in the North of the country, from Uganda. Two years of conflict, between the RPF and the national army FAR (Forces Armes Rwandaises), led the Habyarimana regime to initiate some liberal reforms, which included the formation of a multi-party government. The power sharing agreement, however, failed in dissipating the tension in the country. On the 6th of April 1994 the airplane carrying president Habyarimana was shot down. Responsibility for the attack is still today disputed, but within only a few hours after the attack, extremists within the Hutu-dominated parties managed to take over key positions of government and initiated a 100 day lasting period of ethnic cleansing throughout Rwanda. Estimates suggest that around 800,000 people, mostly Tutsi and moderate Hutu, believed to stand on the side of Tutsi, were killed. The mass killings ended in mid July, when the RPF rebels, who in the meantime renewed the civil war, defeated the Rwandan Hutu army and the various militia groups.

The 1994 Genocide In January 1994 Romeo Dallaire - the Brigadier General of the United Nations peacekeeping force for Rwanda - reported to his superiors in New York that an informant had revealed that 1,700 men were trained in military camps right outside Kigali: *"The 1700 are scattered in groups of 40 throughout Kigali... Since UNAMIR mandate he [the informant] has been ordered to register all Tutsi in Kigali. He suspects it is for their extermination. Example he gave was that in 20 minutes his personnel could kill up to 1000 Tutsi"* (Frontline, 1999). Three months later the informant was proven right. In the night of the air plane crash, the Presidential Guard went around Kigali, targeting moderate politicians, journalists, and civil rights activists, with the moderate prime minister Agathe Uwilingiyimana and her ten Belgian bodyguards being among the first victims. The new interim government immediately declared a nation-wide curfew and the various army and militia men under its control, around 45,000 to 50,000 men strong, set up road blocks, killing everyone presumed Tutsi. Local leaders enforced the curfew, the necessary infrastructure was already in place, and started organizing the killings in their communities. In the end, about 430,000 civilians participated in the genocide, hacking their Tutsi neighbors to death with machetes.

The militia gangs played an important role in the killings. The two infamous ones were the Interahamwe ("those who work together"), associated with the MRND party, and the Impuzamugambi ("those with a single aim"), associated with the CDR (Coalition for the Defense of the Republic), another even more extremist Hutu party. At the beginning of the nineties these groups, their members mostly recruited from the pool of unemployed and disaffected youth in the big cities, started receiving military training from the Presidential Guard and the army. The groups were turned into outright militia, indoctrinated in ethnic hatred and taught how to implement mass murder (Physicians for Human Rights, 1994).

There is today ample evidence that the genocide had been centrally planned. Already the very first operations in Kigali had been ordered and directed by the new de facto authorities in Kigali, centered around the *Akuza*, a group of Hutu hard-liners. Among them was Colonel Theoneste Bagosora, who led virtually all of Rwanda's elite military units during the genocide. Furthermore, Jean Kambanda, the Prime Minister of Rwanda during the genocide, admitted issuing a directive on June 8th 1994 by which the government openly assumed responsibility for the action of the militia, encouraging and reinforcing their activity (OAU, 2000). A striking example of how quickly changes in the central directives were implemented at the local level, is the killing of women towards the end of the genocide. As reported by Des Forges (1999, p. 227) *"The number of attacks against women, all at about the same time, indicates that a decision to kill women had been made at the national level and was being implemented in local communities"*.

Besides army and militia, the central government also used radio propaganda to spur the killings. Radio RTLM, established in June 1993 by hard-line Hutu extremists, continuously called on the Hutu to kill the Tutsi. But also Radio Rwanda, although less inflammatory, informed about the ongoing genocide.

From the start, the genocide planners in Kigali were under time pressure. The RPF Tutsi rebels, initially constrained by the Arusha treaty to a small part of northern Rwanda, advanced through Rwanda's eastern flank towards the capital Kigali, forcing the Hutu elite to speed up operations. Additional pressure came from the possibility of an external international intervention, which was highly feared, but never took place. In fact, false reports of an impending Western intervention were sometimes used by the Hutu elite to motivate fellow Hutu to quickly complete the killings (Kuperman, 2000).

3 Data

I combine several data sets from different sources to construct the final dataset, which comprises 1,433 Rwandan villages. The different data sets are matched by village names within communes. A commune is an administrative unit above the village. There were 142 communes in total which were again grouped into 11 provinces. Unfortunately, the matching

is imperfect, as many villages either have different names in different data sources, or use alternate spelling. It is also not uncommon for two or more villages within a commune to have identical names, which prevents successful matching. However, overall only about 5 percent of the villages do not have a clear match across all sources. Furthermore, as these issues are idiosyncratic, the main implication is likely lower precision in the estimates than otherwise would have been the case. Villages have an average size of 14 square kilometers, with around 4,900 inhabitants. Table 1 reports the summary statistics for the variables.

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 (Friedman, 2010; Yanagizawa-Drott, forthcoming). These data are taken from a nation-wide village-level dataset, provided by the government agency "National Service of Gacaca Jurisdiction", which records the outcome of the almost 10,000 Gacaca courts set up all over the country. Depending on the role played by the accused and on the severity of the crime, two different categories of criminals are identified.

The legal definition for 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 were carried out; 4) people who committed rape or acts of sexual torture. Since these perpetrators mostly belong to army and militia or are local armed groups such as policemen, I consider this as representing armed-group violence. There were approximately 77,000 prosecution cases in this category.[7]

The legal definition for 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 intention of killing - caused injuries or committed other serious violence, but without actually causing death; 3) the person who committed criminal acts or became accomplice of serious attacks, without the intention of causing death. People accused in this category are not members of any of the organized groups mentioned in category 1 and I therefore label this type of violence as civilian violence. Approximately 430,000 prosecution cases were handled in this category. Figures 1 and 2 show the distribution of violence throughout Rwanda for armed-group and civilian violence.

The reliability of the prosecution data is a key issue for the analysis. One concern when using prosecution data instead of actual participation is the presence of survival bias: in those villages with high participation, the violence might have been so widespread that no

[7]Importantly, this number does not necessarily equal the number of people involved, since the same person might have committed a crime in multiple locations. This is especially true for organized perpetrators who moved around.

witnesses were left or the few remaining too scared to identify and accuse the perpetrators resulting in low prosecution rates. This concern is, however, likely to be unwarranted: Friedman (2010) shows that the Gacaca data are positively correlated with several other measures of violence from three different sources.[8] Furthermore Friedman notes that *"the Gacaca courts have been very thorough in investigating, and 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"*. To provide further evidence for the accuracy of the Gacaca data, I also show that the prosecution numbers are strongly negatively correlated with the distance to the nearest mass grave (indicating very high death rates).[9] I thus believe this data to be a good proxy for the number of participants in each village. Nevertheless, to be cautious, in the following analysis I will show that the result are robust to dropping those villages with mass graves or near mass graves.

Another concern is that villages with no reported armed-group violence might have actually received militiamen, but unsuccessful ones. I deal with this concern in Section 4.4.

Finally, random measurement error and allegations that these courts were occasionally misused to settle old scores resulting in false accusations, do not pose a major threat because I am instrumenting for armed-group violence. In fact, the instrumental-variable approach will correct for potential attenuation biases arising from random mismeasurement. Henceforth, the number of participants and the number of those prosecuted will be used interchangeably.

Rainfall Data I use the recently released National Oceanic and Atmospheric Administration (NOAA) database of daily rainfall estimates, which stretches back to 1983, as a source of exogenous weather variation. The NOAA data rely on a combination of actual weather station gauge measures as well as satellite information on the density of cloud cover 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, to my knowledge the only one available, is crucial to obtain reasonable rainfall variation. Furthermore the high temporal resolution, i.e. daily estimates, allow 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 rain that fell 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. Since

[8]These sources are a 1996 report from the Ministry of Higher Education, Scientific Research, and Culture (Kapiteni, 1996); the PRIO/Uppsala data on violent conflicts (Gleditsch et al, 2002); and a database of timing and lethality of conflict from Davenport and Stam (2009).

[9]Moreover, using data from a Rwandan household survey in 2000, Rogall and Yanagizawa-Drott (2013) find that the Gacaca prosecution data is strongly positively associated with mortality: a 10 percent increase in the number of people prosecuted increases child mortality by 1.7 percentage points which is about 8 percent of the average in the sample (they have to rely on child mortality because adult mortality is not observed in the household survey).

these buffers crisscross the various rainfall grids and each distance buffer is thus likely to overlap with more than one rainfall grid, I obtain considerable variation in rainfall along each buffer. 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. In a similar fashion, using a village boundary map, I also compute rainfall in each village. Figure 3 illustrates how the instrument is constructed.

Village Boundary, Road and Town Data The Centre for Geographic Information System 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. 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 data sets (e.g. the 1991 census and the Gacaca records).

Africover provides maps with the location of major roads and towns derived from satellite imagery. These satellites analyze light and other reflected materials, and any emitted radiation from the earth's surface. Since simple dirt roads have very different radiation signatures than tarred roads or gravel roads, this allows to objectively measure road quality.[10]

I use these maps to calculate various distance measures, such as the distance of the village centroid to the closest main road, to the closest town, to the borders of the country and to Kigali and Nyanza, the recent capital and the old Tutsi Kingdom capital, respectively and to calculate the village area.

Additional Data The remaining data are drawn from Genodynamics and the IPUMS International census data base. This data includes population, ethnicity, and radio and cement floor ownership from 1991. Except for population, all these variables are only available at the commune level. Ethnicity is defined as the fraction of people that are Hutu or Tutsi respectively. About 10 percent of the population are Tutsi. I calculate the Tutsi minority share used in the analysis as the fraction of Tutsi normalized by the fraction of Hutu.

Verpoorten (2012c) provides data on the number of days the RPF Tutsi Rebels were present in each village and the location of mass graves which she constructs using satellite maps from the Yale Genocide Studies Program. A dummy variable on whether the RPF Tutsi rebels controlled a village at the beginning of the genocide is taken from Straus (2006).

[10]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 of the 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.

4 How Much Do Armed Groups Affect Civilian Violence?

4.1 OLS Specification

The simplest way to look at the effect of armed-group violence on civilian violence is to run the following OLS regression:

$$log\,(K_{ip}) = \alpha^O + \beta^O log\,(M_{ip}) + \mathbf{X}_{ip}\pi^O + \gamma_p + \varepsilon_{ip} \tag{1}$$

where K_{ip} is the number of Hutu prosecuted in category 2, i.e. my proxy for civilian violence and M_{ip} the number of Hutu prosecuted in category 1, i.e. my proxy for armed-group violence in village i in province p. $\mathbf{X}_{ip}$ is a vector of village-specific control variables, which I will explain below, γ_p are province fixed effects and ε_{ip} is the error term. I allow error terms to be correlated across villages within a 150 kilometer radius (Conley, 1999).[11] Armed groups were sent around the entire country, so I expect errors to be correlated over long distances. In particular, the cutoff of 150 kilometer coincides with the maximum distance to Kigali - the center of the country and the genocidal plan - in my sample of villages. Prosecution rates are heavily skewed to the right and I therefore logaritmize them.[12] The coefficient β^O thus captures the percentage increase in civilian participation associated with an increase of one percent in the number of militiamen.

4.2 OLS Results

The number of militiamen in each village is positively correlated with civilian participation at 99 percent confidence level with a point estimate of 0.688 (standard error 0.077, regression 1 in Table 2). And this relationship holds up when controlling for a number of other factors that potentially affect civilian participation. I call them ”additional controls”.

These include distance to the border, distance to major towns, distance to Kigali, distance to Nyanza as well as village population, population density and the number of days the RPF was present in each village. To illustrate, being close to the border potentially made it easier for the Tutsi or for those Hutu unwilling to participate in the killings to leave the country. Distance to towns, in particular the capital Kigali, is likely to be correlated with urbanization and public goods provision (economic activity). Nyanza was the old Tutsi Kingdom capital and villages further away from it still exhibit lower Tutsi shares, on average. Population density eventually captures social pressure as well as food pressure, both said to be important

[11] Results are robust to clustering at commune or province level. Clustered standard errors for all main results are reported in Table OA.1 in the online appendix.

[12] To deal with 0 observations I add 1 to the number of prosecution cases. I also experiment with the inverse hyperbolic sine transformation defined by $ln(X + \sqrt{(1+X^2)})$ as suggested in Burbidge et al. (1988) and the results are robust.

reasons behind the genocide (Verpoorten, 2012b; Diamond, 2005; and Boudreaux 2009).[13] Finally, RPF presence in a village, as they moved through Rwanda, likely affected civilian participation.

Nevertheless even after including a large set of controls, the OLS estimates might still be biased. For instance, I lack a good control for leader quality in the villages and it might be that in villages with peaceful leaders civilians are less likely to commit violent acts. If army and militia were strategically sent into those villages to spur the killings, I would underestimate the true effect. Measurement error would also contribute to a downward bias. Alternatively, it might be that there are some unobserved village-specific reasons for tensions that promote both civilian and armed-group violence, thus biasing the estimates upwards. Furthermore, the OLS estimates are less informative from a policy perspective because they also pick up the effects of local armed groups who would have been difficult to target with an international intervention.

4.3 Instrumental-Variables Strategy

To overcome the issues raised above, I use an instrument for armed-group violence. The instrument is distance to the closest main road interacted with the total amount of rain falling during the period of the genocide along the dirt tracks between road and village (technically, along a 500 meter buffer around the line between village centroid and closest point on the main road).[14]

My identification strategy rests on two assumptions. First, villages with heavier rainfalls along the shortest way between main road and village experienced lower armed-group violence and the more so the further they were from the main roads (first stage). Second, conditional on the control variables (explained in detail below), distance to the main road interacted with rainfall along the way does not have a direct effect on civilian violence other than through armed-group violence (exclusion restriction).

First Stage Although I can directly test the first-stage assumption, at this point I want to give some intuition as to why I should expect to find this negative relationship between transport costs and the number of militiamen in the data. There is plenty of anecdotal evidence showing that the genocide had been carefully planned and centrally administered by the authorities, which directed the movements of army and militia all over the country. Des Forges (1999, p. 180) writes,

[13]The food pressure argument essentially assumes a Malthusian type of model: a fixed amount of land to grow crops (fertilizers were seldom used in Rwanda (Percival and Homer-Dixon, 2001)) feeds a growing population.

[14]The genocide started on the 6th of April 1994 and ended on the 17th of July 1994. To account for rainfall before the starting date I add an additional day to construct the instrument.

"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, [and so on]. A survivor of that massacre identified the party affiliation of the assailants from their distinctive garb, (...). He could tell, too, that they came from several regions."

Most of these movements were done by motorized vehicles, for instance Hatzfeld (2005) cites civilian killers describing how they moved on foot while the militia used cars. Unfortunately, I do not have data on the exact locations of Hutu army and militia. However, anecdotal evidence suggests, that they were stationed around the cities (Frontline, 1999), which are all connected by the main roads. In particular, the great majority of them were in Kigali, trained by the Presidential Guards, and spread out into the entire country from there, likely to have used the main road system which is generally paved. I assume that the costs of traveling along these 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] For example, a recent survey in Uganda, a direct neighbor to Rwanda in the North, shows that during the rainy seasons public transport prices almost double (East African Business Week, 2013). Thus the instrument should capture transport costs sufficiently well and the model, outlined in the next section, suggests that higher transport costs should translate into fewer militiamen.

Exclusion Restriction Again, the instrumental-variables strategy makes the counterfactual assumption that, absent armed-group violence, distance to the main road interacted with rainfall along the way has no effect on civilian violence. This is unlikely to be true without further precautions. The instrument, composed of distance to the main road and a rainfall measure, is probably correlated with factors such as education, health, access to markets, rain-fed production and, therefore, with income. These characteristics are in turn likely to affect civilian participation, as reasons for joining the killings were often driven by

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

material incentives and killers were given the opportunity to loot the property of the victims or people could bribe themselves out of participation (Hatzfeld, 2005).

To address this problem, taking into account the general living conditions of individuals in each village, I control for distance to the road interacted with long-term average rainfall (years 1984 to 1993) during the 100 calendar days of the genocide period along the way between village and road as well as all main effects.[16] I therefore only exploit seasonal weather variation in the year of the genocide. Furthermore, I control for rainfall in the village during the 100 genocide days in 1994 and its long-term average. These variables take into account the possibility that rainfall in the village directly affects civilian participation, for example through malaria prevalence or civilians' transport costs within the village. Finally, I always control for village population. In the following analysis I will call these "standard controls". To control for broad geographic characteristics I include 11 province fixed effects. Identification then stems only from short-term variation in rainfall along the distance measure, which is arguably exogenous and should only affect the militia's transport costs.

The genocide partially overlaps with the rainfall season which potentially affects (expected) rural income. I doubt this to lead to a serious bias because looting was mostly directed towards building materials, household assets and livestock (Hatzfeld, 2005), thus high rainfall during the growing season should not have affected the perpetrators. Moreover several country-wide indicators for Rwanda show that agricultural production completely collapsed, suggesting that rainfall should not have affected the plot owners either. Nevertheless, to be cautious and to ensure that the instrument is not picking up any income effects but solely transport costs, I also include in the set of controls the total amount of rainfall in the village during the 1994 growing season and its long-term average as well as the interaction of the two with the difference between the maximum distance to the road in the sample and the actual distance to the road to each village. The last interaction term takes into account possible heterogeneous effect because of market accessibility. The intuition here is that *high* agricultural output (and hence rainfall) is more valuable the *shorter* the distance to the road. I call these "growing season controls".

At this point, I still need to argue that civilians were not directly affected by the instrument, i.e. by traveling themselves. Starting with anecdotal evidence, several reports and accounts of the genocide indeed support the claim that civilian violence was a very local affair. Hatzfeld (2005) calls it a *Neighborhood Genocide* because only neighbors and coworkers were able to identify Tutsi, as they lack big differences to the Hutu in terms of language or look (Hatzfeld, 2005). Besides that, few people in Rwanda, let alone civilians, owned a car or a truck (less than 1 per cent according to the 1992 DHS Survey) and the possibilities of moving across villages by motor vehicles, certainly the most affected by

[16]These are distance to the road, 100 day rainfall along the way between village and road in 1994, and its long-term average.

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 was not always ensuring safety.[17] 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"*. Besides this anecdotal evidence, in Section 4.4 I also present three indirect tests which all strongly support the identification assumption.

Finally, as a first robustness check, adding the additional controls, introduced in Section 4.2 should not alter the results.

IV Specification I run the following first-stage estimation

$$log\left(M_{ip}\right) = \alpha + \beta\left[log(Dist_{ip}) \times log\left(Rain_{ip}\right)\right] + \mathbf{X}_{ip}\pi + \gamma_p + \varepsilon_{ip} \tag{2}$$

where M_{ip} is, as before, my measure of armed-group violence, $Dist_{ip}$ is the distance to the nearest main road and $Rain_{ip}$ is the amount of rain falling during the period of the genocide along the way to village i in province p. Furthermore, γ_p are province fixed effects and ε_{ip} is the error term. Given the controls in $\mathbf{X}_{ip}$, explained in detail above, the interaction term captures the armed groups' transport costs. As a reminder, I include in $\mathbf{X}_{ip}$ village population, the interaction of distance to the road with rainfall along the way during the 100 calender days of the genocide period of an *average year* and all main effects as well as village rainfall and growing season controls. I expect β to be negative.

The second stage equation becomes

$$log\left(K_{ip}\right) = \alpha' + \beta' log\widehat{(M_{ip})} + \mathbf{X}_{ip}\pi' + \gamma_p + \varepsilon_{ip} \tag{3}$$

where $log\widehat{(M_{ip})}$ is instrumented as per (2). The coefficient β' captures the causal effect of armed-group violence on civilian violence for those armed groups affected by transport costs.

[17] Amnesty International (1994, p. 6) report 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."*. Similarly, Prunier (1995, p. 249) reports that *"To be identified on one's card 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, especially if they were tall and with a straight nose and thin lips."* Finally Des Forges (1999, p. 210) writes that *"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."* Also Tutsi tended to avoid the roads but rather hide in the bushes (Hatzfeld, 2005).

4.4 Instrumental-Variable Results

This section presents the main results. I answer the first question posed in the introduction: How much do armed groups affect civilian participation in violence?

First Stage and Reduced Form The first-stage relationship between transport costs and armed-group violence is strongly negative at 99 percent confidence (regression 1 in Panel A in Table 3), and this relationship holds, or gets somewhat stronger, when including growing season controls (regression 2) and additional controls (regression 3). The F-statistic on the excluded instrument in my preferred specification (regression 3) reaches 19.54.

Regarding magnitude, the point estimate of -0.509 (standard error 0.115) suggests that a village with an average distance to the road receives 16 fewer militiamen, about 31 percent of the mean (51.76), following a one standard-deviation increase in rainfall between village and road. I provide a theoretical foundation for this result in the next section.

Importantly, higher transport costs are also associated with fewer civilian perpetrators in the reduced form (regressions 4 to 6 in Panel A in Table 3), with a point estimate of -0.661 (standard error 0.141) in my preferred specification. The results are robust across all three specifications and throughout significant at 99 percent confidence. This is a first indication that villages that were harder or more costly to reach had fewer civilian killers.

Main Effects The instrumental-variable point estimates are about twice as large as the analogous OLS estimates: a 1 percent increase in the number of militiamen leads to a 1.299 percent (standard error 0.258) increase in the number of civilian perpetrators (regression 6 in Panel B in Table 3, with all controls; the OLS result with the same set of controls is reported in column 3).[18] The results are again very robust across all three specifications and throughout significant at 99 percent confidence level.[19] The size of the estimated impact of armed-group violence on civilian violence is huge: when I focus on my preferred specification, these numbers imply that one additional external militiaman resulted in $(430{,}000 \div 77{,}000) \times 1.299 = 7.3$ more civilian perpetrators or 12 additional deaths.[20] 430,000 is the total number of prosecuted civilians and 77,000 the total number of militia and army men, respectively. Put differently, the average number of external militiamen, around 33,[21] arriving at a village increase the number of civilian participants by about 240 which is around 5.6 percent of the average Hutu population in the village.

[18] Note that this positive relationship is not trivial since armed groups and civilians might have been substitutes in the killing process, which would imply a negative relationship.

[19] I cannot replicate this result when using only distance to the road or only rainfall along the road or both but uninteracted as instruments, providing further evidence that transport costs are at work.

[20] Under the linearity assumptions that the number of prosecuted, 507,000, is proportional to the number of perpetrators and the number of estimated victims, 800,000.

[21] Since the 1,433 villages do not comprise the universe of villages, 5 percent are missing, I calculate this number in the following way: $\frac{50{,}000}{1{,}433 \times \frac{100}{95}}$.

Note that the estimated multiplier effect only applies for external militiamen, since these are the ones affected by the instrument. A simple back-of-the-envelope calculation suggests that these 50,000 external army and militia men, around 10 percent of the total number of perpetrators, were directly and indirectly responsible for at least 664,000 Tutsi deaths, which is about 83 percent of the total number of deaths (again under a linearity assumption that the number of perpetrators is proportional to the number of estimated victims, and equally so for civilians and militiamen). If I reasonably assume that militia and army men have a higher killing rate than ordinary civilians this number will be larger, since the direct effects of an additional militiaman increase.

The large instrumental-variable coefficients, compared to the analogous OLS estimates, suggest that militia and army were strategically sent into those villages with originally little civilian participation.[22] Additionally, the instrumental-variable strategy might be correcting for measurement error in the endogenous variable. Furthermore, I measure the local average treatment effect induced by changes in armed-group violence due to the instrument. External army and militia men, for instance well-trained and highly motivated national troops, coming from further away, thus affected by transport costs, might have been particularly ruthless and ambitious, resulting in a high local average treatment effect (LATE). In particular when compared to the average treatment effect (ATE) which also includes the effect of local and maybe less effective or well-trained armed groups, for instance local policemen. However, since a military intervention would have focused on stopping precisely those external army and militia men, these were initially concentrated around the big cities, the local average treatment effect I identify is more informative than the average treatment effect (ATE), certainly from a policy perspective.

Besides understanding how the instrument affects the type of militiamen it is also important to know for which type of villages high transport costs induced fewer militiamen. This is particularly important when generalizing the effect estimated above for the whole universe of villages. Although I cannot directly observe the set of compliers, I can provide some evidence that higher transport costs induced fewer militiamen for various different sub-populations (above and below the median). In particular, higher transport costs lead to fewer militiamen in villages with high and low population densities, with high and low levels of long-term rainfall during the growing seasons, potentially affecting rain-fed production, far from and close to the capital Kigali and a long and short period of time with Tutsi rebels present (results are reported in Table OA.2 in the online appendix). Furthermore, Figure 4 shows that the number of militiamen is monotonically decreasing in transport costs. Finally, from a theoretical perspective, transport costs should matter less for villages that the militia

[22] Recall the model, if there were an unobserved strategic factor (S_i^{un}) that would lower civilian participation, i.e. $\beta^{unS} < 0$, then the model would predict that $cov(M_{ei}, S_i^{un}) > 0$. Combining the two conditions gives a downward bias.

urgently wants to reach, i.e. in which it has large effects on civilian participation. Thus if anything, the estimate above would give me a lower bound. I show this in the next section.

Exclusion Restriction Traveling civilians, potentially affected by the instrument, who spread information about the genocide or start killing outside of their home village are unlikely to pose a threat to the exclusion restriction. In the beginning of the genocide a strict nation-wide curfew was implemented, which drastically limited the travel opportunities for civilians.[23] Barriers, erected on roads and at the entrances to towns, enforced these regulations (Kirschke, 1996; Physicians for Human Rights, 1994). Des Forges (1999, p. 162) writes that *"Tutsi as well as Hutu cooperated with these measures at the start, hoping they would ensure their security."*

Reassuringly, the instrumental-variable estimates are very similar to the baseline results and equally statistically significant when I restrict the variation in rainfall in the instrument to the first 5 days, the first week or the first two weeks of the genocide while controlling for rainfall along the way between village and road for the remaining days and its interaction with distance to the road (regressions 1 to 3 in Table 4).[24] The point estimate of the specification using only the first 5 days is 1.332 (standard error 0.608), almost identical to the ones from the baseline results, thus supporting the identification assumption. Importantly, this result does not imply that only the first couple of days are sufficient to identify the main effect. In fact, the first-stage point estimates drop significantly as compared to the baseline first-stage result, and the main effect thus only stays constant because interestingly the reduced-form effects drop as well, but proportionally so (first-stage and reduced-form coefficients are all reported at the bottom of Table 4). First-stage and reduced-form point estimates moving together proportionally provides another indication that armed groups alone are driving these results.[25]

Furthermore, because of tight population controls, already before the genocide in 1994 it was practically impossible for civilians to get the permission to leave their commune. 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 4), again supporting the identification strategy. Also, since traveling civilians were most likely to pass on information

[23]Radio Rwanda, the nation-wide radio station informed people that the interim government had announced a nation-wide curfew, following the president's plane crash. Importantly, the infrastructure to control and monitor the population were already in place and had been extensively used. In 1990 stringent limitations on the right to freedom of movement were introduced under the State of Emergency.

[24]To be cautious, I also control for the long-term average rainfall along the buffer for those first couple of days and its interaction with distance to the 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.

[25]For the first-five-days and first-week regressions I lose a few observations, simply because there was no rainfall during that short time period. However, rerunning the baseline regression with those two reduced samples give very similar results.

about the genocide, a potential upward bias should be larger for villages with no outside information available, i.e. with little radio ownership. In Section 6.3 below I show that this is not the case.

Note that Tutsi civilians escaping the violence are unlikely to bias the results, since they avoided the main roads, and instead rather hid in the bushes (Hatzfeld, 2005). Furthermore, their decision to escape, facing death, was unlikely the result of a rational transport cost calculation, as it was the case for the militia (as I will show below). Thus their movements should not be correlated with the instrument. For the same reason, those hundreds of thousands of Hutu fleeing the country in fear of the RPF's revenge towards the end of the genocide, are also unlikely to bias the results. And reassuringly, using detailed migration data from a Rwandan household survey in 2000, I find that individuals who lived in villages with low transport costs were not more or less likely to move, either within Rwanda or abroad, during the genocide: the point estimate on the instrument is close to zero and highly insignificant (0.007, standard error 0.015, result not shown).[26]

Robustness Checks I next perform a number of robustness checks, all reported in Table 5. Potential survival bias in the prosecution data is unlikely to matter: the instrumental-variable point estimates are virtually identical to the baseline results and similarly significant at 99 percent confidence when dropping villages with at least one mass grave (indicating high death rates, regression 1) or dropping villages less than 3.5 kilometers away from a mass grave location, reducing the sample size by about 10 percent (regression 2).

Potential underreporting of unsuccessful militiamen, something that would certainly 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 and rerun the baseline regression. The point estimate of 1.489 (standard error 0.305, regression 3) 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,[27] as a result the instrumental-variables estimates should increase as well. Besides, it seems puzzling that a genocide planner, who, as we will see, wants to maximize civilian participation, would send ineffective militiamen precisely to villages that are hard to reach: not only are the (wasted) costs of getting there higher but monitoring costs will certainly be higher as well. Finally, 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

[26]The EICV1 Household Survey contains detailed migration history data for almost 15,000 individuals and is representative at the national level.

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

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!"*

One might also worry that rainfall between each village and the road during the harvest season (towards the end of the genocide), might have a direct effect on civilian participation because it could be correlated with people's income from selling their harvest as low rainfalls along the way to the road decrease transport costs to markets. In practice, this is again unlikely to matter. As mentioned earlier, agricultural production and market activity completely collapsed. And indeed the results are robust to controlling for rainfall along the way between village and road during the 1994 harvest season and its interaction with distance to the road (regression 4).

The estimates are also unaffected by adding the interaction of distance to the road with both rainfall in the village during the growing season in 1994 and with long-term average rainfall during the growing seasons as well as controlling for the yearly long-term average rainfall in the village and along the way between village and road and the interaction of the latter with distance to the road (regression 5).

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 along the way between 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 big majority is likely to have started out from the main cities. However, the two additional controls are small and insignificant in the first stage (not shown) and they do not affect the main result (regression 6).

Replacing 11 province fixed effects by 142 commune effects also does not matter (regression 7). 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.

One might also be worried that the UN troops which 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 8).

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 99 percent confidence in all cases (results not shown).

Finally, as a placebo check, I rerun both first-stage and reduced-form regressions using rainfall from the years after 1994 until 2013 in the instrument. As expected the distributions of the resulting 20 coefficients are both somewhat centered around 0 and reassuringly, the coefficient on the instrument with rainfall from 1994, the year of the genocide, lies to the far left of the distribution in both cases (results shown in Figures AO.1 and AO.2 in the online

appendix).

5 The Strategic Use of Armed Groups

After showing that armed groups have strong effects on civilian participation I now ask whether they were used strategically to maximize civilian participation.

5.1 Model

I model a central genocide planner who wants to maximize civilian participation in the killings but faces a fixed budget B, that is owns only a limited number of trucks that can transport his external militiamen M_{ei} to each village i to promote the killings (there are N villages in total).[28] There is anecdotal evidence that the central genocide planners wanted every Hutu to join in the killings. *"If all were guilty, none could be absolved later should the political winds turn."* (Fujii, 2009, p. 174).

Each village is inhabited by a Hutu population of size 1 and a Tutsi population of size T_i. In each village there might already be local armed groups such as policemen (M_{li}) or RPF Tutsi rebels (R_i). Anecdotal evidence suggests, that there are fewer local militiamen in villages with a large Tutsi minority or Tutsi rebels, i.e. $\partial M_{li}/\partial S_i < 0$ with $S_i = T_i, R_i$.[29] I call T and R the strategic factors (S). Together with the local armed groups, the external militiamen turn ordinary civilians into civilian killers at a decreasing rate by teaching and organizing them.[30] To make progress, I let the militia's technology to turn civilians (C) into killers (K) take on the following form

$$K = A(M_e + M_l)^{\alpha} C \tag{4}$$

with $A > 0$, $0 < \alpha < 1$ and C equals the number of Hutu joining in the training. For simplicity, I assume that all the Hutu villagers join in the training, thus $C = 1$.[31]

The planner faces the following problem (assuming perfect information about the Tutsi

[28] An additional interpretation might be that the planners were under time pressure. And thus B captures the limited amount of time they had.

[29] 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.

[30] Anecdotal evidence that armed groups would usually call all Hutu civilians together in one location, and then instruct and organize them, imply decreasing effects of the militia (Gourevitch, 1998; Hatzfeld, 2005). I will provide empirical evidence for this in the next section.

[31] This assumption does not seem too far fetched in particular since even women and children joined in the killings. As one UNAMIR officer puts it *"I had seen war before, but I had never seen a woman carrying a baby on her back kill another woman with a baby on her back."* (Des Forges, 1999, p.197).

minority share, transport costs and local militiamen)[32]

$$\begin{aligned} \max_{\{M_{ei}\}} \quad & U = \sum_{i=1}^{N} A\left(M_{ei} + M_{li}(S_i)\right)^{\alpha} \\ \text{s.t.} \quad & B = \sum_{i=1}^{N} M_{ei} r_i \end{aligned} \tag{5}$$

where r_i are the exogenous transport costs to reach each village. Solving this maximization problem for the number of external militiamen M_e gives the following predictions (S for Strategic use of violence):

Prediction S1. *The number of total militiamen $M_e + M_l = M$ is strictly decreasing in the transport price r: $\partial M / \partial r < 0$.*

Prediction S2. *But this effect is smaller in strategically important villages: $\partial^2 M / \partial r \partial S > 0$*

Prediction S3.

(i) The number of external militiamen M_e is strictly increasing in the strategic factors S: $\partial M_e / \partial S > 0$.

(ii) The total number of militiamen M is strictly increasing in the strategic factors S if effect (i) dominates the negative effect of S on M_l: $\partial M / \partial S > 0$ (and decreasing vice versa: $\partial M / \partial S < 0$).

The proofs are presented in the appendix. Intuitively, high transport costs lead to fewer militiamen because these can be used more effectively in low cost villages (Prediction S1, local militiamen do not respond to transport price changes). Furthermore because armed groups have larger marginal effects on civilian participation when the Tutsi minority is large or Tutsi rebels are present, the central planner will prefer sending more militiamen into those villages with many Tutsi or Tutsi rebels, thus transport costs should matter less for these villages (Prediction S2). Note that I cannot directly test Prediction S3 (i), since I do not separately observe local and external militiamen in the data. I will however be able to determine which of the two effects ($\partial M_e / \partial S > 0$ or $\partial M_l / \partial S < 0$) dominates for each strategic factor (Prediction S3 (ii)).

5.2 Results

The results suggest that armed groups were strategically allocated among villages: both Predictions S1 and S2 are confirmed in the data. Furthermore, the inflow of external militiamen compensated for the fewer local militiamen in villages with large Tutsi minority shares (Prediction S3). All results are reported in Table 6.

[32] Since Rwanda was a very organized and centralized state, this assumption is not unreasonable.

Prediction S1: Transport Costs To test Prediction S1, i.e. that an increase in transport costs reduces the number of militiamen, I rerun the first-stage regression but drop villages with high rainfall between village and road, above the 90th percentile. This is to show that the negative relationship from the first stage is not simply reflecting that some villages are impossible to reach, but rather that driving to a low-transport-cost village instead of a high-transport-cost village was a strategic choice. The point estimate of -0.632 (standard error 0.177) is even slightly larger than the baseline result in Table 3 and still strongly significant at 99 percent confidence (regression 1 in Table 6). This provides the theoretical foundation for my instrumental-variables strategy.

Prediction S2: Interaction Effects Also in line with the strategic use of armed groups (Prediction S2), I find a positive and statistically significant interaction effect between transport costs and the Tutsi minority share with a point estimate of 1.975 (standard error 0.649) in my preferred specification, i.e. a one standard-deviation increase in the Tutsi minority share reduces the negative effects of transport costs by about 40 percent (regression 3), confirming that transport costs mattered less for strategically important villages. Unfortunately the coefficients on the interaction with the second strategic factor, the Tutsi rebels, do not deliver a clear picture as they move around across specifications. One explanation for why I do not observe clear positive effects, as predicted by the model, is that since the Tutsi rebels quickly defeated the Hutu army in those areas further effort was deemed useless.

Prediction S3: Strategic Factors Finally, villages with a larger Tutsi minority share received more militiamen. Point estimates are robust and highly significant at 99 percent level across all four specifications, ranging between 2.050 (standard error 0.558) and 2.119 (standard error 0.544); they suggest that a one standard-deviation increase in the Tutsi minority share increases the number of militiamen by about 20 percent.[33] Thus the inflow of external militiamen compensates for the fewer local militiamen (Prediction S3), again confirming the strategic importance of these villages. The opposite is true for villages with Tutsi rebels: coefficients are throughout negative and again highly significant. Thus in this case the initial lack of local militiamen was not compensated for by an inflow of external men. As mentioned above, the Tutsi rebels quickly defeated the Hutu army in those areas, rendering further Hutu efforts useless.

Since Tutsi were on average richer than Hutu, these effects might be picking up wealth effects. However, all of the results above are robust when controlling for the fraction of people with a cement floor (my best proxy for wealth (Yanagizawa-Drott (forthcoming)), suggesting that wealth is not driving these effects (regression 4).

[33] Note that both rainfall along the way and distance to the road are demeaned.

Note that we should not observe the above effects if militiamen were just randomly roaming the country. In particular the evidence for Prediction S2 confirms the centralized organization of the genocide, since common external militiamen were unlikely to know the distribution of Tutsi in the country, especially when coming from further away. The results are also consistent with the bulk of anecdotal evidence suggesting that the genocide was centrally managed.

6 How do Armed Groups mobilize Civilians?

In the last main section, I discuss the two potential channels through which armed groups might affect civilian participation and present how to test them in the data.

First, armed groups might have acted as a role model. Hatzfeld (2005) reports that often militiamen took a lead in the killings and showed civilians how to kill best. One of the civilian killers he interviews brings it to the point (Hatzfeld, 2005, p. 36)

> *"Many people did not know how to kill, but that was not a disadvantage, because there were Interahamwe to guide them in the first steps. [...] They were more skilled, more impassive. They were certainly more specialized. They gave advice on what paths to take and which blows to use, which techniques."*

Second, militiamen might have physically forced civilians to join in the killings. Anecdotal evidence of survivors and perpetrators confirms that civilian villagers sometimes fought off external aggressors. Des Forges (1999, p. 156) writes: *"Both in Kigali and elsewhere, Hutu [occasionally] cooperated with Tutsi in fighting off militia attacks (...)."*.

As I do not have data to directly distinguish between those two cases, I model the two scenarios in the following section and will then test their theoretical implications.[34]

6.1 Role Model versus Force Model

Set Up Imagine that the N villages, introduced in Section 5.1, can be of two types $j \in \{o, w\}$: those that do not oppose the militia (w) and those that oppose the militia (o).[35] As noted the militia turns ordinary civilians into killers (I now assume a very general functional

[34]One might also imagine that the militia promised civilians security from the Tutsi rebels in return for their participation, instead of only teaching them how to kill and organizing or supervising the killings. In Section 6.3 I will argue that this is unlikely to matter.

[35]Note that opposing is defined in an active way, i.e. fighting the militia. One might however also imagine, that people were coerced to participate, thus *innerly* opposing the militia. That is they were not necessarily welcoming the militia but still too afraid to actively resist. This phenomenon is maybe best described in Bertholt Brecht's parable *Actions against violence* (Brecht, 2001). Unfortunately, I lack data to directly address this possibility.

form):

$$K^j = K(M^j) \cdot C^j \tag{6}$$

where $M^j = M_e^j + M_l^j$. I assume that $K_M > 0$ and $K_{MM} < 0$.

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

$$E(K^w) = K(M_e + M_l(S)) \tag{7}$$

Opposing Villages Hutu villagers in opposing villages fight the Hutu militia together with the Tutsi civilians T and rebels R. For simplicity I assume that everybody joins the fight, 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, $C^o = 0$. As is standard in the conflict literature, the militia's winning probability is given by a contest function

$$p = I(\gamma M, P) \tag{8}$$

where $\gamma > 1$ measures the militia's superiority, they often carry guns. 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 $\gamma M \lesseqgtr P$

Furthermore $I(0,P) = 0$ and p lies between 0 and 1.[36] Assumption 1 states that the more militiamen join in the fight against the Hutu and Tutsi civilians and rebels the higher are 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 is crossed, i.e. $\gamma M > P$, the marginal returns to having an additional militiaman joining the fight begin to decrease since chances of winning are anyway high ($I_{MM} < 0$ as $\gamma M > P$). There is anecdotal evidence that this is the case for military contexts (Dupey, 1987; Hirshleifer, 1989).[37]

[36] An example of a contest function satisfying those conditions is $I(M,P) = \frac{M^\beta}{M^\beta + P^\beta}$ with $\beta > 1$.

[37] To back up this anecdotal evidence, consider the simple example of a soldier who has to fight against civilians. As long as he can fire his gun no one will approach him. He has two bullets and needs to reload once, during which he his defenseless and can be attacked. Thus he will eventually fire one bullet. Now consider a second soldier joining him, while the first one reloads the second can fire and vice versa, thus together they fire four shots - implying increasing returns.

The third assumption states that when the number of militiamen is relatively small (large) increasing the opponents strength decreases (increases) the marginal effects of an additional militiaman. Put differently, 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 $\gamma M > P$).

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

$$E(K^o) = I(\gamma M_e, 1 + T + R) \cdot K(M_e) \tag{9}$$

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 big majority of villages in the data. This gives the following predictions (C for Channels):

Prediction C 1. *The larger the strategic factor S, the smaller (larger) are the effects of the number of external militiamen M_e on civilian participation $E(K^j)$ if Hutu villages are opposing (not opposing) the genocide: $\partial^2 E(K^o)/\partial M_e \partial S < 0$ $\left(\partial^2 E(K^w)/\partial M_e \partial S > 0\right)$.*

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

The proofs are presented in the appendix. Since the first stage, derived in the preceding sections, provides exogenous variation in the number of external militiamen all predictions are stated with respect to M_e.[38] Prediction C1 says that, in non-opposing villages, one additional external militiaman has a larger effect on civilian participation when the Tutsi minority is large. 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. On the other hand, in opposing villages, as long as the number of militiamen is sufficiently small, a large Tutsi minority decreases the militia's effect on civilian participation because 1) the Tutsi civilians 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.

Finally, Prediction C2 states that in non-opposing villages the first militiaman arriving has a larger effect on civilian participation than the second and so on. The opposite is true in opposing villages, since civilians fight against the militia the first man arriving has very low effects on civilian participation, but with every additional man these effects increase.

[38] For completeness, I discuss the maximization problem of the genocide planners if faced with opposing villages in the appendix.

6.2 Results

Prediction C1 implies that the interaction effect of the number of militiamen with the two strategic factors should be positive if the militia acted as a role model and negative if the militia had to use force against opposing villagers. Prediction C2 implies that the militia should exhibit decreasing marginal effects under the role model channel and increasing effects if force was necessary.

Prediction C1: Interaction Effects The first test between the force or role model channel, supports that armed groups acted as role models: the interaction effect of armed-groups violence with a dummy indicating whether Tutsi rebels were controlling a village at the beginning of the genocide is positive and significant at 95 percent confidence level (2.415, standard error 1.095, regression 1 in Table 7). Furthermore, the coefficient on the interaction with the other strategic factor, the Tutsi minority share, is equally positive however, since variation only comes at coarse commune level, insignificant (1.483, standard error 2.311, regression 1). 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.

In regression 2 I replace the continuous Tutsi minority share variable by a dummy taking on the value 1 if the Tutsi share lies above the median. Again, the point estimate is similarly positive but in addition also significant at 95 percent level (1.198, standard error 0.593). Unfortunately, because of strong multicollinearity, this specification does not allow me to control for the double interaction large Tutsi share times distance to the road. To account for the potential omitted variable bias this creates I interact the Tutsi dummy with the other controls not involving distance to the road and include them in the regression.[39]

Since Tutsi were on average richer than Hutu, these effects might be picking up wealth effects. However, all of the results above are robust when controlling for the fraction of people with a cement floor (my best proxy for wealth (Yanagizawa-Drott (forthcoming)), suggesting that wealth is not driving these effects (regression 3). Interestingly, as a side result, the effects of an additional militiaman are lower in richer villages. A one standard-deviation increase in the fraction of villagers with a cement floor decreases the effect of armed groups by about 15 percent (-3.047, standard error 1.135, regression 4). This result is consistent with higher wealth raising the opportunity costs for participation, and rich civilians showing less motivation to participate (Dube and Vargas, 2013; Verwimp, 2003,

[39]Since the force model predicts that I should observe negative interaction effects especially for low levels of militiamen, i.e. $\gamma M < P$, I also restrict the sample to those villages where militiamen make up less than 4 percent or less than 2.5 percent of the population. Recalling the model, this implies that one militiamen is equivalent to 25, respectively 50 civilian Tutsi and Hutu fighters with the true value probably somewhere in between ($= \gamma$, the militia's fighting superiority parameter from the contest function). However, interactions effects are equally positive (not shown).

2005).

Prediction C2: Functional Form Consistent with the findings above, the effects of an additional militiaman seem to be decreasing. The concave relationship between civilian perpetrators and militiamen is presented graphically in Figure 5, using nonparametric local mean smoothing with an Epanechnikov kernel, conditional on the controls from my preferred specification (regression 6 in Table 3) 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 5, the square term is negative and highly significant at 99 percent level, again confirming the concave relationship. The coefficient on the square term is depicted graphically in Figure 6, to the far right of the x-axis, labeled *Full* (sample).[40]

This result has to be taken with a grain of salt since the nonlinearities in the second stage might be driven by nonlinearities in the first stage. Reassuring though, when I repeat the analysis above but also use second order terms in transport costs as excluded instruments the results in the second stage look similarly concave (results not shown).

6.3 Extensions

Information To better understand the role model channel I ask whether the militia mostly informed civilians about the ongoing genocide, something a radio reporter might have done just as well, or whether they rather taught and organized civilians, something that certainly required physical presence in the village. Importantly, there existed two radio stations in Rwanda (Radio Rwanda and Radio RTLM, the former having national coverage) who relentlessly informed listeners about the ongoing genocide. This hints at a way to test the initial question: if the militia mostly worked through information then the effect of the militia should be smaller in villages that were already informed, i.e. exhibited high levels of radio ownership. Thus I should observe a negative interaction effect of the number of militiamen with radio ownership among Hutus in the data. However, if anything the opposite seems true: the interaction effect of armed-groups violence with a Hutu radio ownership dummy, taking on the value 1 if the fraction of Hutu owning a radio lies above the median, is positive although insignificant (0.716, standard error 0.875, regression 1 in Table 8). This result is potentially important for policy since it implies that a genocide planner could not simply substitute for an eventual absence of armed groups by enhancing radio propaganda.

Because one might worry that radio ownership does not solely reflect information but also wealth, I further control for the fraction of Hutu with a cement floor (as noted, a good proxy for wealth (Yanagizawa-Drott, forthcoming)) and its interaction with the number of

[40]Restricting the sample again to those villages where militiamen make up less than 4 percent or less than 2.5 percent of the population, i.e. where $\gamma M < P$, still delivers equally concave effects (results not shown).

militiamen (regression 2) and the results are robust. (As mentioned above, the insignificant coefficient on radio ownership also rules out that traveling civilians, who spread information, have a direct effect on civilian participation, since the militia's effect should be larger for villages with no outside information, i.e. radio access.)

In line with the militia's physical presence in the village being crucial for civilian mobilization, I also find that, once I fix the number of militiamen in each village, militiamen in neighboring villages, within a certain radius, have no effect on civilian participation, which should have been the case, if information spillovers had mattered. The coefficient on the average participation in villages within a 10 kilometer radius is -0.507 (standard error 1.573, regression 3), insignificant and if anything negative. The same is true for the coefficient for villages within 10 to 20 kilometers (-0.411, standard error 1.810, regression 5). Furthermore all results are robust to controlling for the within 10km, respectively 10 to 20km, average of the standard controls (regressions 4 and 6) which ensures a causal interpretation.[41]

Identifying Opposing Villages The empirical evidence suggests that the militia functioned as a role model for the whole sample of villages and the bulk of anecdotal evidence supports this view. The same anecdotal evidence, 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 to test the predictions of the force model. In particular, anecdotal evidence suggests that villages with a high fraction 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, p. 381) writes *"In the southern part of Ngoma commune, a man of some standing in the community at first took in many relatives from his wifes Tutsi family as well as his Tutsi godson and his family."*

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[42]

$$IHEP_c = \sum_{i=1}^{F_c} \frac{N_{ic}}{N_c} \times h_{ic} \times t_{ic} \tag{10}$$

where N_c is the total number of people in the households 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, the higher are

[41] Since each village on average has 23 neighboring villages within a 10 kilometer radius and 60 neighboring villages between 10 and 20 kilometers away, the estimated spillover coefficients form above further need to be normalized by 23, respectively 60 to be directly comparable to the main effect. This result also rules out that promising Hutu civilians safety from the Tutsi rebels in exchange for their participation was a major channel, since those promises should get more credible the more militiamen arrive in neighboring villages.

[42] 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 likeliness of opposition.

the chances that civilians in those villages opposed the militia.[43]

In Figure 6 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 5, 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 one additional militiaman are increasing (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. The convex relationship between civilian perpetrators and militiamen for high levels of intra-household ethnic polarization is also presented graphically in Figure 7. However, sample sizes are very small and the results should therefore again be interpreted with caution.

Nevertheless, to provide further support for the argument that these villages with high levels of intra-household ethnic polarization were opposing the genocide I can also link some of them to anecdotal evidence. For example Des Forges (1999, p. 345) writes, that in Huye commune (97th percentile) both Hutu and Tutsi civilians fended off attackers from outside. Des Forges (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 south-west of Rwanda, where opposition was overall more pronounced, Butare province for instance had a Tutsi leader who actively opposed the genocide.

7 External Validity

In this section I argue that the massive civilian participation during the Rwandan Genocide, however horrible and grim, is not unique, but that similar events can be found throughout in history.

The Case of Lithuania In the summer of 1941 Nazi Germany invaded the Soviet Union. In Lithuania the Germans were welcomed as liberators and quickly began to organize the murder of the Jewish population. By the end of World War II 196,000 Jews or about 95 percent of Lithuania's Jewish population had died, the big majority shot dead in pits near

[43]Note that this measure is highest, i.e. equal to 0.25, when Hutu and Tutsi shares are both one half. To illustrate the numbers, consider the two cases of Cyimbogo commune in Cyangugu province and Rwamiko commune in the neighboring Gikongoro province, both in the southwestern part of the country: in both communes Hutu account for about 72 percent of the total population, however in Cyimbogo one out of four marriages is mixed (ethnic polarization measure of about 0.049, the highest in the sample), in Rwamiko, on the other hand, only every 20th marriage; thus bringing the measure down to 0.018.

their hometowns. The Lithuanian Holocaust parallels the Rwandan Genocide in many ways. Although the Germans *"must be seen as the prime organizing force in these killings, the majority of the murders was actually performed by Lithuanians."* (MacQueen, 1998, p. 1). Similarly, for SS Brigadeführer Franz Walter Stahlecker (1941) the Germans acted mostly as catalysts

> *"Basing [oneself] on the consideration that the population of the Baltic countries had suffered most severely under the rule of Bolshevism and Jewry while they were incorporated into the U.S.S.R., it was to be expected that after liberation from this foreign rule they would themselves to a large extent eliminate those of the enemy left behind after the retreat of the Red Army. It was the task of the Security Police to set these self-cleansing movements going and to direct them into the right channels in order to achieve the aim of this cleansing as rapidly as possible."*

Furthermore, the organization of these massacres reminds of the Rwandan genocide: usually a few German officers would arrive at a village, ordering local Lithuanians, civilians as well as militia, to round up the Jews and kill them. The Germans supervised these massacres and instructed local perpetrators how to kill best.

To substantiate the argument that the Germans had an impact on Lithuanian participation in the killings of the Jews, I also present suggestive empirical evidence. To this end, I collect data on the precise location of every massacre in Lithuania as well as whether Germans or local Lithuanians or both were involved in the killings. This data is taken from the "Holocaust Atlas of Lithuania", a data project initiated in 2010 by the Vilna Gaon State Jewish Museum and the Austrian Verein Gedenkdienst. I match this massacre data to an administrative map of Lithuania to get the number of Nazi (Lithuanian) massacres per municipality, the first unit of observation. Since I unfortunately lack data on the number of perpetrators, I assume that they are proportional to the number of massacre victims.[44] In line with the findings for the Rwandan genocide, the number of Nazi perpetrators is strongly positively related to the number of Lithuanian perpetrators at 99 percent confidence level (regression 1 in Table 9) and this relationship holds up when I add 10 county fixed effects and various geographic controls, such as distance to the border, distance to the western border (from where the Germans invaded), distance to the capital Vilnius, distance to the closest main road or railway track and distance to closest town (regression 2).[45]

Since there are only 48 municipalities I further divide Lithuania into 1,033 grids of equal size (0.1 degree x 0.1 degree) which I again match to the massacre data. This refined

[44]Data on the identity of the perpetrators is occasionally missing and I drop those massacres from the analysis. However, these massacres are small and only account for 1.6 percent of the total number of victims.

[45]All of these controls are calculated in ArcGIS. To calculate distances to major roads and railways, I digitize an old Lithuanian map from 1940 in ArcGIS, obtained from www.maps4u.lt.

analysis, allows me to control for 48 municipality fixed effects (or 133 artificial grid effects). Moreover, it confirms the positive relationship: point estimates increase to 0.898 (standard error 0.029) in the specification with all controls and municipality effects (regression 4) and to 0.907 (standard error 0.026) in regression 5 with 133 grid effects of size 0.3 by 0.3 degree.

At this point one could potentially use a similar instrumental-variables strategy as for the Rwandan case to identify causal effects, but this is beyond the scope of this paper. Thus, although I cannot claim that these effects are causal, the results are consistent, in particular since I am likely to estimate a lower bound, measurement error as well as the potentially strategic use of Nazi perpetrators are likely to push the OLS estimates down. Furthermore, the "Holocaust Atlas of Lithuania" provides narrative background information on each of the massacres which occasionally contains the exact number of perpetrators on both sides. Consistently, the few cases where this information is available confirm the huge multiplier effect, the number of Lithuanian perpetrators is always very much larger than the number of German perpetrators. The anecdotal evidence further suggests that the majority of Lithuanians did not actively oppose the Germans, again mirroring the Rwandan Genocide.[46]

Finally, I provide some suggestive evidence that transport costs seem to matter for the allocation of Nazis, thus resembling the first stage for the Rwandan case: the number of Nazi perpetrators is strongly negatively related to the distance to the nearest road or railway. Point estimates are very robust across the three specifications using 1,033 grids, and throughout significant at 99 percent confidence level (Table 10).

Other Cases Another example is the collective killings during China's Cultural Revolution in the 1960s. Although fought along class-membership rather than ethnic lines, this shares many of the horrible features of the Rwandan Genocide. These state-sponsored killings were mostly performed by ordinary civilians who hacked and bludgeoned their fellow village colleagues and neighbors to death using simple farming tools. Su (2011, p. 4) writes,

> *"Together, the primitiveness and intimacy [of these killings] underscore the fact that the killers were ordinary civilians rather than institutional state agents, such as soldiers, police, or professional executioners. (...) A village or a township was turned into a willing community during these extraordinary days of terror in the Cultural Revolution, for the killers inflicted the atrocities in the name of their community, with other citizens tacitly observing."*

Su mentions other closely related examples, such as the Bosnian War or the case of Jedwabne, a village in Poland where in 1941 half of the village population killed the other half because they were Jews.

[46]This is not to deny that, again similar to the Rwandan case, occasionally individuals risked their lives to help potential victims.

Yet another case of state-sponsored killings performed by civilians is Guatemala's civil conflict in the second half of the 20th century. Ball et. al. (1999, p. 100) state,

> *"One of the most destructive aspects of state terror in Guatemala was the State's widespread use of civilians to attack other civilians. (...) The army claimed that the [civilian] patrols sprang from the spontaneous desires of peasants to protect themselves from the guerrillas (Americas Watch 1989: 7). Still, almost no village resisted the army order."*

But also recent examples can be found such as the fighting between Muslim and Buddhist civilians in Rakhine State in Burma which has cost several lives and was at least partly elite-triggered (Asia Times, 2012) or the 2007 post-election violence in Kenya, where *"Communities turned on each other with crude weapons as they were encouraged, and even paid, by power-hungry politicians."* (BBC, 2010). Wenger and Mason (2008) even suggest that the *civilianization* of armed conflict, as they call it, will become more and more common.

8 Conclusion

My results show that the massive civilian participation during the genocide in Rwanda did not follow from suddenly exploding ancient hatred, plunging the country into an unstoppable all-against-all conflict, but rather that in midst the seemingly senseless killings there was method. Civilian participation was carefully fostered by the central leaders in Kigali - rational actors - who allocated their armed groups strategically.

The 50,000 external army and militia men under the control of the genocide planners in Kigali did not only kill themselves but also incited civilians to do so. The large multiplier effect of 7.3 estimated above implies that those 50,000 men, around 10 percent of the total number of perpetrators, were directly and indirectly responsible for at least 83 percent of the Tutsi deaths. In particular, this number increases if we reasonably assume that militiamen had higher killing rates than civilians (almost 90 percent if one militiamen killed five times as many people as a civilian perpetrators).

The results have important policy implications: if international troops had stopped that small group of perpetrators, the bulk of the killings could have been prevented. Furthermore since these men were initially stationed in the big cities, in particular the capital Kigali, a military intervention would have likely been successful. This is important since critics of a foreign intervention in Rwanda usually argue that an intervention would have not been quick enough to reach every corner of the country (Kuperman, 2000). My results show that a full-blown intervention, i.e., also targeting the rural areas, would have not been necessary. The results also suggest, somewhat comforting, that once taken out a genocide planner

could not have simply compensated for the absence of his armed troops by stirring up radio propaganda.

To illustrate, if I assume that the number of militiamen in each main city is proportional to the city size then focusing only on Kigali alone, which should have been relatively easy, would have cut the number of deaths by a half, saving 400,000 people. A more ambitious intervention would have likely saved even more.

Returning to the general question posed in the introduction whether a) political elites use armed groups to foster civilian participation in violence or b) civilian killers are driven by unstoppable ancient hatred, this paper clearly points to answer a). In Rwanda the national army and various militia groups incited civilians to participate in the genocide, in Lithuania the Germans incited local Lithuanians to kill the Jews.

Policy recommendations however might differ. In light of my results for the Rwandan Genocide I believe that General Romeo Dallaire - the Canadian commander of the UN force in Kigali at the time - was right when he insisted that with 5,000 to 8,000 troops he could have stopped the genocide, possibly saving hundreds of thousands of lives. However, whereas the various armed groups in Rwanda were relatively weak and badly equipped and thus potentially easy to stop with a military intervention, stopping the Germans in Lithuania was undoubtedly far more difficult.

While I am keenly aware that the results are based on a single case study of the Rwandan Genocide and some suggestive evidence for the Lithuanian Holocaust, anecdotal evidence for other cases of state-sponsored murder indicate that the findings are likely to be relevant for other countries as well.

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Figure 1: Armed Group Violence.

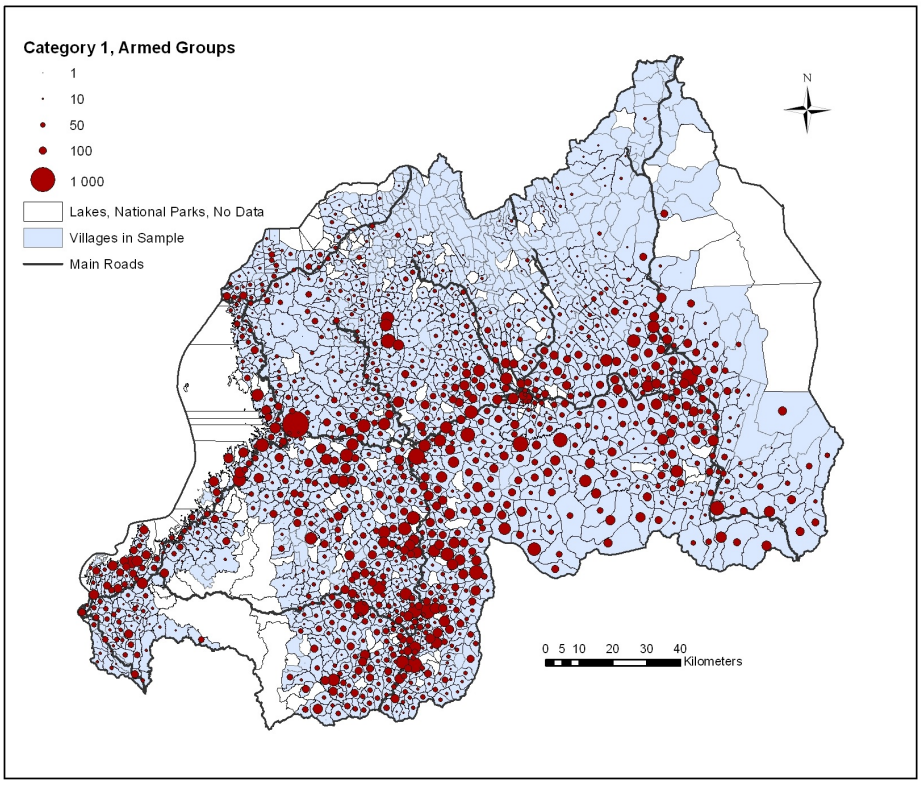


Figure 2: Civilian Violence.

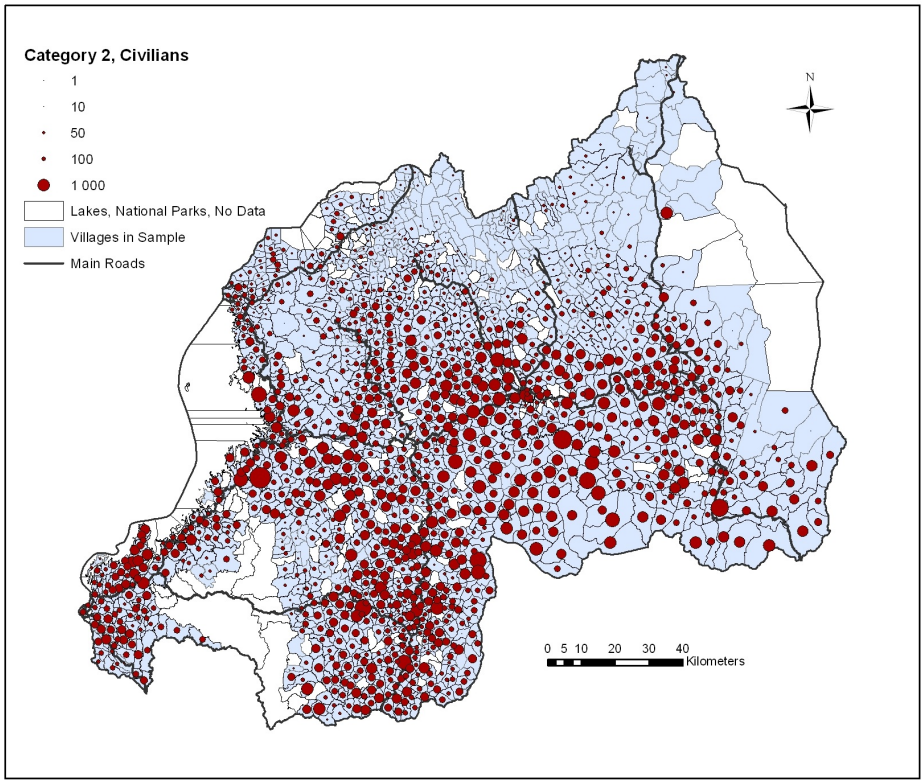


Figure 3: Construction of the Instrument in ArcGIS.

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

Table 1: Summary Statistics

	Mean	Std.dev.	Obs.
A. Endogenous Variables			
# Prosecuted Militiamen	51.757	70.51	1433
# Prosecuted Civilians	290.255	286.43	1433
B. Exogenous Variables			
Rainfall between Village and Road, genocide period, 1994	122.701	35.94	1433
Rainfall between Village and Road, genocide period, 10-year average	206.181	37.78	1433
Rainfall between Village and Road, whole year, 10-year average	962.759	180.15	1433
Rainfall between Village and Road, harvest season, 1994	22.418	10.15	1433
Rainfall between Village and Main City, genocide period, 1994	123.256	33.82	1433
Rainfall in Village, genocide period, 1994	122.677	35.62	1433
Rainfall in Village, genocide period, 10-year average	204.989	38.86	1433
Rainfall in Village, growing season, 1994	243.895	69.61	1433
Rainfall in Village, growing season, 10-year average	621.095	117.51	1433
Rainfall in Village, whole year, 10-year average	960.677	182.70	1433
Distance to the Road	6.712	5.77	1433
Distance to Kigali	62.654	30.00	1433
Distance to Nyanza	64.360	30.74	1433
Distance to Main City	22.778	14.69	1433
Distance to the Border	22.604	13.93	1433
1991 Population, '000	4.882	2.48	1433
1991 Population Density	494.710	850.75	1433
Number of Days with RPF Presence	42.471	43.12	1432
Mass grave in Village	0.046	0.21	1432
Fraction of Hutu with Radio	0.325	0.09	1433
Fraction of Hutu with Cement Floor	0.086	0.08	1433
Fraction of Villagers with Cement Floor	0.093	0.09	1433
Tutsi Minority Share	0.105	0.13	1433
Tutsi Rebels (RPF)	0.054	0.23	1433

The # prosecuted militiamen is crime category 1 prosecutions against organizers, leaders, army and militia; # prosecuted civilians is crime category 2 prosecutions against civilians. The rain variables are measured in millimeter. The 10-year average is for the years 1984 to 1993. The distance variables are measured in kilometers. Radio and cement floor ownership and ethnicity data are taken from the 1991 census, available only at the commune level. There are 142 communes in the sample. The Tutsi Minority Share is defined as the fraction of Tutsi normalized by the fraction of Hutu. Population is the population number in the village and Population Density is population per square kilometers, also from the 1991 census. Days with RPF Presence gives the number of days the Tutsi Rebels were present in each village. Tutsi Rebels (RPF) is a dummy indicating whether RPF Tutsi rebels were controlling a village at the beginning of the genocide.

Table 2: OLS Estimates of Main Effect

Dependent Variable:	# Civilian Perpetrators, log	
	(1)	(2)
# Militiamen, log	0.688	0.639
	[0.077]***	[0.051]***
Additional Controls	no	yes
Province Effects	yes	yes
R^2	0.71	0.73
N	1433	1432

Note: **Additional controls** are distance to Kigali, main town, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and village area and the number of days with RPF presence. All control variables, except "Number of Days RPF present", are in logs. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 3: Main Effects

A. Dependent Variable:	# Militiamen, log			# Civilian Perpetrators, log		
	First Stage			Reduced Form		
	(1)	(2)	(3)	(4)	(5)	(6)
Armed Groups' Transport Cost	−0.357	−0.460	−0.509	−0.480	−0.573	−0.661
	[0.116]***	[0.117]***	[0.115]***	[0.126]***	[0.125]***	[0.141]***
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
F-stat	9.50	15.54	19.54	14.45	20.93	21.91
R^2	0.46	0.48	0.50	0.53	0.54	0.58
N	1433	1433	1432	1433	1433	1432
B. Dependent Variable:	# Civilian Perpetrators, log					
	OLS			IV/2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
# Militiamen, log	0.649	0.647	0.626	1.345	1.245	1.299
	[0.065]***	[0.066]***	[0.051]***	[0.369]***	[0.241]***	[0.258]***
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	0.72	0.72	0.74	0.50	0.56	0.54
N	1433	1433	1432	1433	1433	1432

Note: Armed Groups' Transport Cost is the instrument (distance to the main road interacted with rainfall between village and main road). **Standard controls** include village population, rainfall in the village during the 100 days of the genocide in 1994, 10-year long-term rainfall in the village during the 100 days, rainfall along the way between village and road during the 100 days in 1994, 10-year long-term rainfall along the way between village and road during the 100 days, distance to the road and its interactions 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 town, 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 RPF present", 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 square brackets, Conley (1999). The **F-Statistic** refers to the excluded instrument. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure 4: First Stage

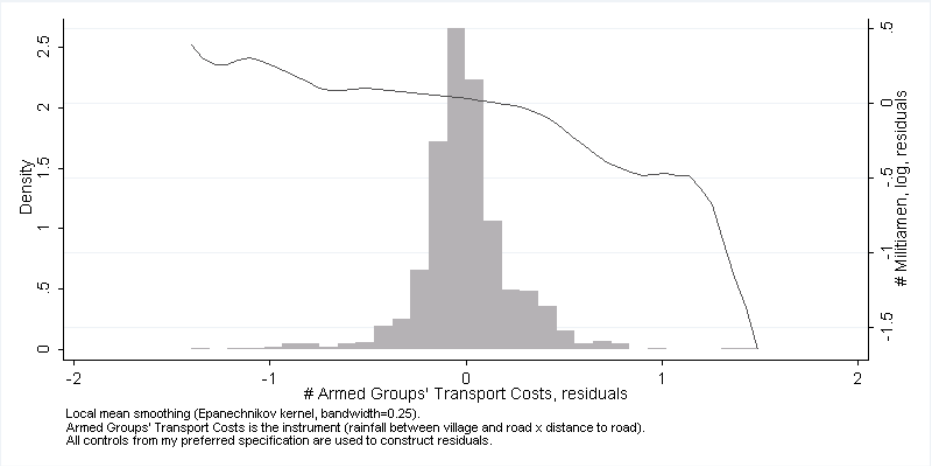


Table 4: Exclusion Restriction Tests

Dependent Variable:	# Civilian Perpetrators, log (IV/2SLS)			
	First 5 days	First week	First 2 weeks	Communes w/o road
	(1)	(2)	(3)	(4)
# Militiamen, log	1.332	1.267	1.366	1.688
	[0.608]**	[0.424]***	[0.353]***	[0.627]***
Standard Controls	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
First Days Controls	yes	yes	yes	no
Province Effects	yes	yes	yes	yes
R^2	0.52	0.57	0.50	0.32
N	1399	1406	1432	568
Coefficients on Excluded Instrument				
First Stage	−0.084	−0.129	−0.270	−0.855
	[0.041]**	[0.038]***	[0.071]***	[0.266]***
Reduced Form	−0.112	−0.164	−0.369	−1.442
	[0.061]*	[0.061]***	[0.097]***	[0.425]***

Note: In **regressions 1 to 3** the instrument is distance to the road interacted with rainfall 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. **Standard controls** include village population, rainfall in the village during the 100 days of the genocide in 1994, 10-year long-term rainfall in the village during the 100 days, rainfall along the way between village and road during the last 95 days/93 days/86 days in 1994, 10-year long-term rainfall along the way between village and road during the 100 days, distance to the road and its interactions 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. **First day controls** are rainfall in the village during the first 5 days/1 week/2 week and the 10-year long-term rainfall, rainfall along the way between village and road during the first 5 days / 1 week /2 weeks in 1994, 10-year long-term rainfall along the way between village and road during the first days, and its interaction with distance to the road. All control variables, except "Number of Days RPF present", 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 square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 5: Robustness Checks

Dependent Variable:	# Civilian Perpetrators, log (IV/2SLS)							
	Measurement Error in Gacaca Data			Additional Controls			Commune Effects	Dropping Kigali
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Militiamen, log	1.284	1.351	1.489	1.357	1.246	1.266	1.203	1.305
	[0.266]***	[0.286]***	[0.305]***	[0.278]***	[0.306]***	[0.250]***	[0.404]***	[0.300]***
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
Harvest Controls	no	no	no	yes	yes	no	no	no
Other Rainfall Controls	no	no	no	no	yes	no	no	no
Main City Transport Controls	no	no	no	no	no	yes	no	no
Province Effects	yes	yes	yes	yes	yes	yes	no	yes
Commune Effects	no	no	no	no	no	no	yes	no
R^2	0.55	0.54	0.34	0.51	0.57	0.56	0.66	0.54
N	1366	1279	1432	1432	1432	1432	1432	1400

Note: In **regression 1** all villages with mass graves are dropped. In **regression 2** all villages at most 3.5 kilometers away from a mass grave site are dropped. In **regression 3** the average number of militiamen is added to villages with 0 reported militiamen. **Regressions 4 to 7** use different controls and in **regression 8** Kigali province is dropped. **Standard controls** include village population, rainfall in the village during the 100 days of the genocide in 1994, 10 year long-term rainfall in the village during the 100 days, rainfall along the way between village and road during the 100 days in 1994, 10 year long-term rainfall along the way between village and road during the 100 days, distance to the road and its interactions 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 town, 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 path between village and road during the harvest/transportation season and its interaction with distance to the road. **Other Rainfall controls** are distance to the road interacted with a) rainfall during the growing season in 1994 in the village and b) 10 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 path between village and road and its interaction with distance to the road. **Main City Transport controls** are rainfall along a 500 buffer between village centroid and the closest main city and its interaction with distance to the main city. All control variables, except "Number of Days RPF present", 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 square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 6: Strategic Use of Armed Groups

Dependent Variable:	# Militiamen, log			
	(1)	(2)	(3)	(4)
Armed Groups' Transport Cost	−0.632	−0.690	−0.659	−0.659
	[0.177]***	[0.152]***	[0.143]***	[0.158]***
AGTC x Tutsi Minority Share		2.458	1.975	1.930
		[0.875]***	[0.649]***	[0.754]**
AGTC x Tutsi Rebels		0.214	−0.111	−0.059
		[0.322]	[0.371]	[0.373]
AGTC x Cement Floor				0.936
				[0.831]
Tutsi Minority Share		2.178	2.134	2.097
		[0.555]***	[0.49]***	[0.572]***
Tutsi Rebels		−1.159	−1.001	−0.999
		[0.128]***	[0.175]***	[0.171]***
Cement Floor				0.138
				[0.747]
Standard Controls	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes
Additional Controls	yes	no	yes	yes
Province Effects	yes	yes	yes	yes
R^2	0.47	0.50	0.51	0.51
N	1286	1433	1432	1432

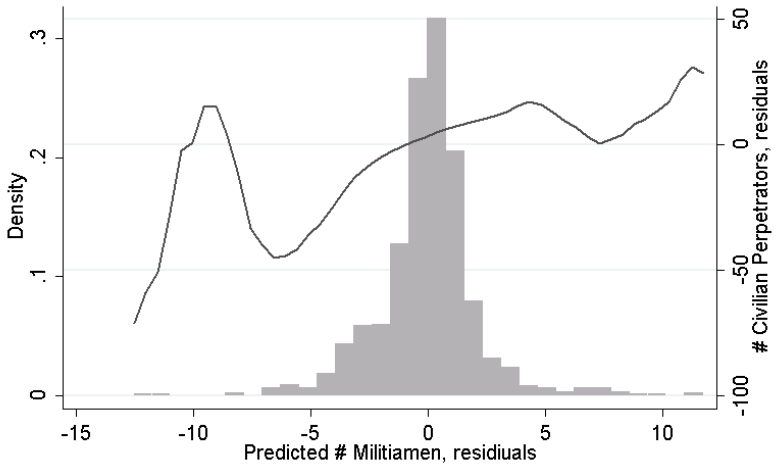
Note: Armed Groups' Transport Cost (AGTC) is the instrument (distance to the main road interacted with rainfall between village and main road). **Standard controls** include village population, rainfall in the village during the 100 days of the genocide in 1994, 10 year long-term rainfall in the village during the 100 days, rainfall along the way between village and road during the 100 days in 1994, 10 year long-term rainfall along the way between village and road during the 100 days, distance to the road and its interactions 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 RPF present", 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 square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 7: Interaction Effects, Role Model or Force Model

Dependent Variable:	# Civilian Perpetrators, log (IV/2SLS)		
	(1)	(2)	(3)
# Militiamen, log	0.832	1.024	1.013
	[0.950]	[0.333]***	[0.285]***
# Militiamen, log x Tutsi Rebels	2.178	1.907	1.900
	[1.067]**	[0.591]***	[0.508]***
# Militiamen, log x Tutsi Minority Share	5.161		
	[14.210]		
# Militiamen, log x Large Tutsi Group		1.125	0.999
		[0.564]**	[0.487]**
# Militiamen, log x Cement Floor			−2.021
			[0.823]**
Standard Controls	yes	yes	yes
Growing Season Controls	yes	yes	yes
Additional Controls	yes	yes	yes
Tutsi Interactions	no	yes	yes
Province Effects	yes	yes	yes
R^2	0.26	0.15	0.37
N	1432	1432	1432

Note: **Standard controls** include village population, rainfall in the village during the 100 days of the genocide in 1994, 10 year long-term rainfall in the village during the 100 days, rainfall along the way between village and road during the 100 days in 1994, 10 year long-term rainfall along the way between village and road during the 100 days, distance to the road and its interactions 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 RPF present", are in logs. Interactions are first logged and then interacted. In each column I also control for the main effect and double interactions. The Large Tutsi Group Dummy takes on the value 1 if the Tutsi Minority Share lies above the sample median. The Tutsi rebels dummy takes on the value 1 if Tutsi rebels where in control of the village at the beginning of the genocide. **Tutsi interactions** include the interaction of the Large Tutsi Group dummy with all other controls that do not involve distance to the road. Note in regressions 2 to 4 I do not control for Large Tutsi Group dummy interacted with distance to the road. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure 5: Functional Form, Role Model or Force Model



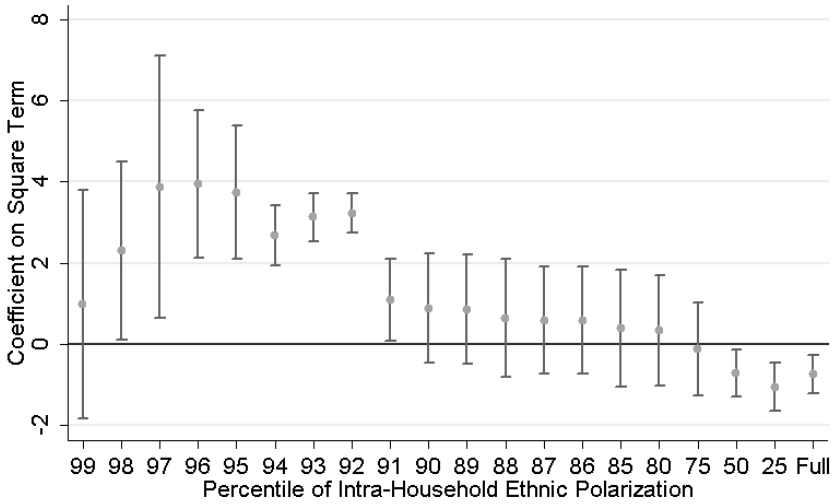
Local mean smoothing (Epanechnikov kernel, bandwidth=2).
Militiamen residuals are instrumented with transport costs (rainfall between village and road x distance to the road).
All controls from my preferred specification (regression 6,Table 3) are used to construct residuals.

Table 8: Extension: Information

Dependent Variable:	# Civilian Perpetrators, log (IV/2SLS)					
	Radio		Information Spillovers			
	(1)	(2)	(3)	(4)	(5)	(6)
# Militiamen, log	0.977	1.140	1.629	1.544	1.654	1.508
	[0.286]***	[0.244]***	[0.845]*	[0.532]***	[0.464]***	[0.973]
# Militiamen, log x Hutu Radio Ownership	0.716	0.717				
	[0.844]	[0.809]				
# Militiamen, log x Hutu Cement Floor		−2.692				
		[1.289]**				
# Militiamen, log, within 10km			−0.507	−0.174	−0.473	0.021
			[1.573]	[0.917]	[1.408]	[1.504]
# Militiamen, log, within 10-20km					−0.411	−0.428
					[1.810]	[1.812]
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
Standard Controls, Neighbors	no	no	no	yes	no	yes
R^2	0.42	0.48	0.32	0.39	0.30	0.42
N	1432	1432	1432	1432	1432	1432

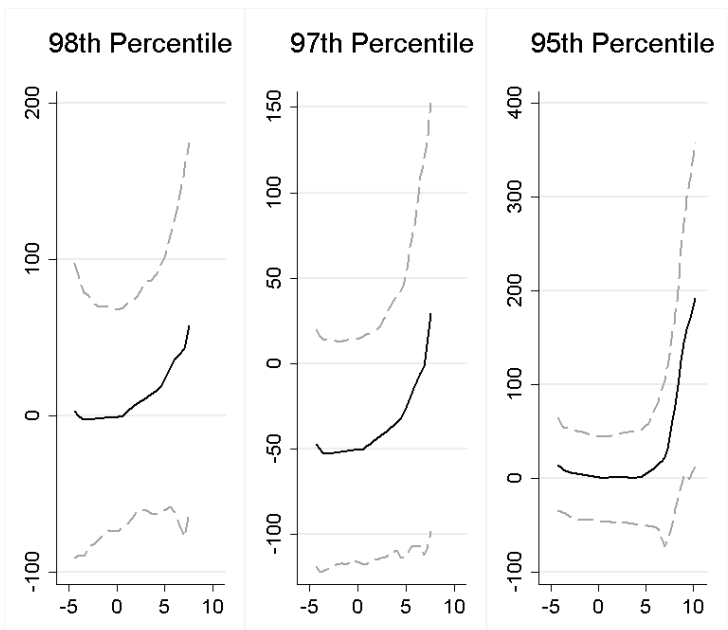
Note: # Militiamen within 10km (10-20km) is the average number of militiamen in neighboring villages (radius with 10km or 10-20km). **Standard controls** include village population, rainfall in the village during the 100 days of the genocide in 1994, 10 year long-term rainfall in the village during the 100 days, rainfall along the way between village and road during the 100 days in 1994, 10 year long-term rainfall along the way between village and road during the 100 days, distance to the road and its interactions 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. **Standard Controls for Neighbors** are the averages of all standard controls for neighboring villages. All control variables, except "Number of Days RPF present", are in logs. Interactions are first logged and then interacted. Hutu Radio Ownership is a dummy taking on the value 1 if the fraction of Hutu that owns a radio lies above the median. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure 6: Extension: Identifying Opposing Villages (Convex and Concave Effects)



I run regressions of the number of civilian perpetrators (residuals) on a second order polynomial in the residuals from a regression of the predicted number of militiamen (instrumenting by transport costs) on all controls (from regression 6, Table 3). The coefficients on the square terms (indicating the curvature of the function) are reported together with 95 percent confidence intervals.

Figure 7: Extension: Opposing Villages (Convex Effects)



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 different percentiles of intra-household ethnic polarization.
The number of militiamen is instrumented with transport costs.
All controls from my preferred specification are used to construct residuals (regression 6 in Table 3).

Table 9: The Case of Lithuania: Main Effects

	# Lithuanian Perpetrators, log				
	Municipalities		Artificial Grids (0.1 Degree)		
	(1)	(2)	(3)	(4)	(5)
# Nazi Perpetrators, log	0.683	0.623	0.885	0.898	0.907
	[0.151]***	[0.147]***	[0.030]***	[0.029]***	[0.026]***
Controls	no	yes	no	yes	yes
Municipality Effects	no	no	no	yes	no
County Effects	no	yes	no	yes	no
Grid Effects	no	no	no	no	yes
R^2	0.68	0.82	0.74	0.76	0.77
N	48	48	1033	1033	1033

Note: **Controls** include distance to the border, distance to the capital Vilnius, distance to major city and distance to the western border as well as distance to major road or railway. All control variables are in logs. Interactions are first logged and then interacted. There are **10 counties** and **133 grid effects** (0.3 degree). **Standard errors** correcting for spatial correlation within a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 10: The Case of Lithuania: "First Stage"

	# Nazi Perpetrators, log		
	(1)	(2)	(3)
Distance to Major Road or Railway, log	−0.390	−0.342	−0.369
	[0.068]***	[0.067]***	[0.083]***
Controls	no	yes	yes
Municipality Effects	no	yes	no
Grid Effects	no	no	yes
R^2	0.06	0.11	0.17
N	1033	1033	1033

Note: **Controls** include distance to the border, distance to the capital Vilnius, distance to major city and distance to the western border. All control variables are in logs. Interactions are first logged and then interacted. There are **48 municipalities** and **133 grid effects** (0.3 degree). **Standard errors** correcting for spatial correlation within a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Appendix

Predictions S1, S2 and S3 - Central Planners Problem (Role Model)

Solving the planner's maximization problem gives the following equilibrium level of external militia in village i

$$M_{ei} = \frac{1}{1+\rho_i}\left(\frac{B}{r_i} - \rho_i \cdot M_{li}(S_i) + \sum_{j\neq i}^{N} \frac{r_j}{r_i}(M_{lj}(S_j))\right) \tag{11}$$

where $\rho_i = \sum_{j\neq i}^{N}\left(\frac{r_j}{r_i}\right)^{\alpha/(\alpha-1)}$. Note that $\frac{\partial M_{li}}{\partial S_i} < 0$ and $\frac{\partial \rho_i}{\partial r_i} > 0$, therefore $\frac{\partial \frac{\rho_i}{1+\rho_i}}{\partial r_i} > 0$. The three results follow directly.

Prediction C1 - Interactions

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

$$\frac{\partial E\left(K^w\right)}{\partial S \partial M_e} = K_{MM}(M_e + M_l(S)) \cdot \frac{\partial M_l}{\partial S} \tag{12}$$

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

$$\frac{\partial E\left(K^o\right)}{\partial S \partial M} = I_{MP}(M,P) \cdot K(M) + I_P(M,P) \cdot K_M(M) \tag{13}$$

The result follows immediately, since both terms in the sum are 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 (13) is ambiguous.

Prediction C2 - Nonlinearity

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

$$\lambda I(M_1) + (1-\lambda) I(M_2) \geq I(\lambda M_1 + (1-\lambda) M_2) \tag{14}$$

Now, set $M_2 = 0$. This gives

$$\lambda I(M_1) \geq I(\lambda M_1) \tag{15}$$

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

$$\lambda I(M_1) \cdot K(M_1) \geq I(\lambda M_1) \cdot K(M_1) \tag{16}$$

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

$$\lambda I(M_1) \cdot K(M_1) \geq I(\lambda M_1) \cdot K(M_1) > I(\lambda M_1) \cdot K(\lambda M_1) \tag{17}$$

Rearranging gives

$$\lambda H(M_1) > H(\lambda M_1) \tag{18}$$

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 $I(M,P) \times K(M)$ will approach $K(M)$ eventually the effects will turn concave. To illustrate that the product of two concave functions can be either 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 concave otherwise.

Central Planners problem: Militia in Opposing Villages

When the militia faces opposing villages, the genocide planner's objective function changes, as he now has to take into account that civilians will fight. Thus the planner faces the following problem

$$\begin{aligned} \max_{\{M_{ei}\}} \quad & U = \sum_{i=1}^{N} I(\gamma M_{ei}, P_i) \times A\left(M_{ei}\right)^{\alpha} \\ \text{s.t.} \quad & B = \sum_{i=1}^{N} M_{ei} r_i \end{aligned} \tag{19}$$

where r_i are the again exogenous transport costs to reach each village and $I(M,P)$ is the contest function which maps the militia's and civilians' strength into a winning probability.

Prediction S4. *The number of militiamen M_e is zero in villages with large transport price r and large strategic factors S (if B is not too big).*

Intuitively, since the militia's effects are increasing up to some cutoff (i.e. when $\gamma M = P$ and potentially a little beyond)[47], the planner will start sending militiamen to villages that are easy to reach and easy to fight until the budget is used up, thus places with high transport prices and high levels of the strategic factors will not get any militiamen. This is only true if the budget B is not too big, because otherwise all villages will receive militia. Loosely speaking, villages will either receive a lot of militiamen or none at all. To be more precise:

Assume for illustration purposes that only transport prices r_i differ and that the contest function were convex everywhere. Then naturally the genocide planner should pick the village with the lowest transport cost and send all his men there, since the marginal effects are ever increasing. Now go back to the original assumption that the contest function is convex up to some cutoff ($\gamma M = P$, and a little beyond). Now the genocide planner will still send his first men to the village with the lowest transport cost. However, since marginal returns are decreasing for that first village after the cutoff, at some point the genocide planner will start sending his men to the second cheapest village and so on until the budget is used up. This implies that villages with very high transport costs do not receive any militiamen (unless the budget is so large that every village receives militia). Adding heterogeneous Tutsi minority shares implies that villages that are both costly to reach and have large numbers of Tutsi (since this reduces the chances of winning) will not receive any militiamen.

Note that I cannot say anything about the direct effects of the Tutsi minority share or the Tutsi rebels, because on the one hand villages with large Tutsi minority shares are less likely to be targeted by the militia, because the marginal effects are lower but on the other hand, if the planner does decide to target a village he is likely to send more militiamen into those villages because the cutoff is larger (i.e. $\gamma M = P = 1 + T + R$). Places that are harder to reach however, get unambiguously fewer militiamen. However, since this is also true for role model villages it does not allow me to distinguish between the two cases.

However, I do not find evidence for Prediction P4, i.e. that villages with both high transport costs and high levels of the strategic factors receive no militiamen as would be the case if the majority of villages had opposed the militia. Furthermore, the total number of militiamen in the sample is too low to suggest that the budget constraint was not binding.

[47] Eventually the effects have to turn concave because the contest functions approaches 1.