The Escalation of Violence: Armed Groups and Civilian Perpetrators[*]

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Abstract

This paper demonstrates that civilian participation in violence during civil conflicts can largely be explained by the presence of elite-level armed forces. Guided by a simple model, we empirically examine how the central Rwandan Hutu government strategically allocated armed groups like the Interahamwe militia or the National Hutu army to maximize civilian participation in the Rwandan Genocide. To establish causality, we exploit cross-sectional variation in transport costs - the interaction of the shortest distance to the main road with rainfall along the way - at the village level. Our instrumental variables estimates reveal a huge multiplier effect: one additional militia man resulted in 7.2 more civilian perpetrators. High levels of Hutu radio ownership, a good proxy for information, do not mitigate the militia's effects, indicating that it was the *physical presence* of armed groups which was crucial for mobilizing the population. Furthermore, building on the model predictions, we rule out that civilians were, at least the majority, forced into joining the killings. Instead, the data suggests that armed groups acted as catalysts and civilians followed orders.

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1 Introduction

In many genocides and civil wars ordinary civilians with no military affiliation or military training whatsoever turn into perpetrators. 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 high death rate 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 views coexist. On the one hand, civilian participation is often interpreted as an unstoppable outbreak of ancient hatred, usually fought along ethnic lines, thus ruling out a successful foreign intervention. One retired US admiral remarks on the subject, referring to the Bosnian War: *"Let them fight. They've been fighting for a thousand years."*[2] There was no foreign intervention in Rwanda. On the other hand, anecdotal evidence suggests that strategically used elite-level armed groups trigger civilian participation. Brown (1996, p.23) writes, *"Elite decisions and actions are usually the catalysts that turn potentially volatile situations into violent confrontations."*[3] Elite groups are naturally of much smaller size and thus potentially easier to stop. For example, General Romeo Dallaire - the Canadian commander of the UN force in Rwanda - insisted that with 5000 to 8000 well-equipped troops he could have stopped the genocide, hindering the various militia and army groups in Kigali and other big cities from spreading into the country.

This paper provides the first empirical analysis of how important elite-level armed groups might be in inducing civilians to participate in killings. It answers three questions: First, how much do armed groups affect civilian participation? Second, what are the channels? Third, do armed-group leaders allocate their militiamen strategically? In answering these questions, we will focus on the Rwandan Genocide, to our 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 unobservably low, the estimate would be downward biased.[4]

To overcome these endogeneity issues, we use an exogenous measure of transport costs for estimating the effect of armed groups on civilian participation in civil conflict. More specifically, we exploit two sources of variation. First, we 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

[2] Rear Admiral James W. Nance (ret.) is quoted in Tom Ashbrook (1995).

[3] Of the 34 major internal conflicts he considers 26 were elite-triggered (Brown, 1996).

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

reasonable condition, we expect areas further away from these main roads to be harder to reach by the militia. However, because distance to the road is certainly correlated with other, possibly unobservable, determinants of civilian violence such as education, health or income, we further exploit variation in rainfall during the period of the genocide, introducing a novel, high resolution rainfall data set.[5] Specifically, the instrument is the distance to the main road interacted with rainfall along the way between each village and the closest point on the main road during the period of the genocide. The idea is simple: We expect the movements of army and militia, mostly performed by motor vehicles, to be limited by the heavy rains that characterize the first rainy season, which partly overlaps with the genocide.

Using this interaction has several advantages; first, it allows us to control for the main effects of our instrument, in particular distance to the road. Second by controlling for distance to the road interacted with rainfall between village and road during the 100 genocide days of an *average year* we only exploit the seasonal weather variation in the year of the genocide. Finally by controlling for rainfall during the 100 days in 1994 and its long-term average in each village, that is at the armed group's destination, we ensure that identification only stems from short-term variation in rainfall *along the distance* measure, which is arguably exogenous and should only affect the militia's transport costs.[6] We find that higher transport costs are in fact strongly negatively related with the number of militiamen arriving in a village (first stage). Following a one standard-deviation increase in rainfall, a village with an average distance to the road receives 18 fewer militiamen (35 percent of the mean).

There is one concern regarding the excludability of our instrument: villages that were difficult to reach by armed groups might have also been difficult to reach by civilian killers or informants. However, civilian violence was very localized and we will devote an ample amount of care to corroborate that this concern is unwarranted.

We 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, normalized by village Hutu population. There were about 10,000 of these local (grassroots) courts set up all over the country to prosecute the crimes committed during the genocide.[7] Using prosecution instead of actual participation rates might introduce some bias. However, first we show that the Gacaca data is strongly correlated with other measures of violence from various different sources. Second, we 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 per Hutu is associated with a 0.631 percent increase in the civilian participation rate. In contrast, the instrumental variable estimates are about twice as large. The numbers imply that, on average, one additional militia man resulted in 7.2 more civilian perpetrators. Put differently, the 50,000 militia and army men roaming the country, about 10 percent

[5]The genocide lasting only 100 days is another advantage for our identification strategy as this limits the presence of time confounding factors.

[6]Rainfall in each village might be correlated with malaria prevalence or civilian's transport costs within the village, both of which are likely to directly affect civilian participation.

[7]Gacaca roughly translated means short, clean cut grass.

of the total number of perpetrators, were directly and indirectly responsible for at least 82 percent of the Tutsi deaths.[8] The results are robust to the inclusion of various geographic controls, they pass several indirect tests concerning the exclusion restriction and are also relevant for other cases of state-sponsored murder, in particular the killings of the Jews in Lithuania in the 1940s.

In the second main part of the paper we examine different channels through which armed groups might have spurred civilian participation. First, we show that high levels of Hutu radio ownership in a village, a good proxy for information, do not mitigate the militia's effect on civilians. This result is consistent with the militia providing more than simple information about the ongoing genocide or put differently that their physical presence was crucial for mobilizing the population. A natural next 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 we do not have data to directly distinguish between these two possibilities. So instead, we test the theoretical implications of these two scenarios.

Our model suggests that the militia's effects are increasing if force is needed and decreasing otherwise, a result based on the reasonable assumption that forcing Hutu civilians to participate gets more effective the more militia members arrive but that only a few militiamen are needed to provide an example. Furthermore the model predicts different signs for the interaction effect with the Tutsi minority share. In the data, we find decreasing effects of the militia as well as a positive interaction effect with the Tutsi minority share. Both these findings suggest that the militia did not force people to join in the killings but rather functioned as a role model.

In the last main part of the paper, we find that the central planners in Kigali allocated their armed groups strategically. We model a genocide planner who wants to maximize civilian participation (under the role model) but faces a transport constraint. Both predictions of the model are confirmed in the data: villages which are more costly to reach receive fewer militiamen (this is essentially the first-stage result; however, since that result might be driven by villages which were, due to very bad weather, simply impossible to reach, we also drop those villages with high rainfall and the negative relationship remains). Furthermore, transport costs matter less for villages with large Tutsi group shares.

Our paper contributes to the literature in several ways. First of all, it adds to the vast conflict literature, of which Blattman and Miguel (2010) give an excellent review, vehemently calling for well-identified studies on the roots of individual participation in violent conflict and the strategic use of violence. This paper starts filling the gap, adding to the literature on the determinants of conflict by providing novel evidence on the strong effects that armed groups have on civilian participation and on the strategic use of armed groups. Recent studies consider government policy, foreign aid, propaganda, income, and institutions (Dell, 2012; Nunn and Qian, forthcoming; Yanagizawa-Drott, 2012; Dube and Vargas, 2012; Besley and Persson, 2011, respectively). Furthermore, our paper complements the literature on the causes of the Rwandan Genocide (Verwimp, 2003, 2005, 2006; Straus, 2004; Friedman, 2010; Verpoorten, 2012b).

[8]See the results section for the necessary assumptions for this back-of-the-envelope calculation.

Our findings also speak to the discussion on the effects of rainfall on conflict other than through the income channel (Sarsons, 2011). Our results suggest that especially in areas with poor infrastructure, such as Africa, rainfall might have negative direct effects on conflict through transport costs. Regarding the importance of transport costs our paper contrasts recent contributions by Donaldson (forthcoming), Faber (2013), and Banerjee et al. (2012) that highlight the positive economic effects of low transport costs. Our findings loosely echo the ones in Nunn and Puga (2012) who show that high transport costs in Africa, i.e. rugged terrain, have positive effects on todays GDP because they hindered slave traders.

The remainder of the paper is organized as follows. Section 2 provides some background information on the Rwandan Genocide. Section 3 sets up a simple model to highlight three channels through which armed groups might have affected civilian killings and to distinguish between these channels in the data. It also models the strategic use of armed groups from a central genocide planner perspective. Section 4 presents the data used for the analysis and section 5 lays out our empirical strategy. Section 6 presents the results and accesses their robustness. Section 7 discusses the external validity of our results. 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 begins with a brief overview of the key moments in their tormented history, before describing in more detail the 1994 Genocide.[9]

2.1 A History of Conflict

The nature of the distinction between the two major ethnic groups living in Rwanda is strongly debated. Some argue that the Tutsi, with a population share of around 10 percent clearly the minority, are descendants of 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 its independence in 1962 the Hutu managed to take over power, reversing the situation and establishing a one-party state. Local Tutsi rulers got replaced by *burgomaster* predominantly of Hutu origin and Gregoire Kayibanda became the first elected (Hutu) president of Rwanda. The ethnic violence that accompanied the event led several hundreds of thousands of Tutsi to flee the country. The following decade recorded an alternation of periods of relative political stability

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

and peace with episodes of unrest and violence, but tensions never sedated. In 1973, following new episodes of violence fueled by unrest in the neighboring Burundi, the Hutu military leader Habyarimana overthrew Kayibanda in a military coup. In 1975 Habyarimana created the Hutu-dominated National Revolutionary Development Movement (MRND), which became the only political party legally authorized in the country, and in 1978 he officially become the new elected 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 willing 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. One year later a peace treaty was signed in Arusha, Tanzania under the supervision of the United Nations. The power sharing agreement that followed failed in dissipating the tension in the country, which began again more violent than before when the airplane carrying president Habyarimana was shot down on the 6th of April 1994. 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 the whole of Rwanda. Estimates suggest that around 800,000 people, mostly Tutsi and moderate Hutu, who were believed to stand on the side of Tutsi, were killed during those 100 days. The mass killings ended in mid July, when the RPF rebels, which in the meantime renewed the civil war, conquered the capital city Kigali, defeating the Rwandan Hutu army and the various militia groups.

2.2 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 1700 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 same 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 militia gangs under its control set up road blocks, killing everyone who was presumed Tutsi.

As a result of the highly centralized state apparatus, the order to kill quickly reached every corner of the country. Local leaders enforced the curfew, the necessary infrastructure was already in place, and started organizing the killings in their communities. The various militia groups and the Hutu army, around 45,000 to 50,000 men, were driven into areas where more resources were needed or the death toll appeared too low. When army and militia arrived at the villages, they actively contributed in fueling civilian violence, by providing leadership, weapons and coordinating the massacres (Hatzfeld, 2005). Civilians refusing to participate in the violence were often considered accomplices of the enemy, and treated as such (Des Forges, 1999). In fact the genocide was planned in a way to not only kill as many Tutsi as possible but in doing so also involve all Hutu civilians throughout the country. This collective involvement should create a totally implicated society. In the end, about 430,000 civilians participated in the genocide, hacking their Tutsi neighbors to death with machetes.

As noted, the militia gangs played a predominant role in the killings. They originally represented the youth groups attached to the various political parties. The two infamous ones were the Interahamwe ("those who work together"), associated with the MRND, 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. During these training weeks the groups were turned into outright militia, indoctrinated in ethnic hatred and taught how to implement mass murder (Physicians for Human Rights, 1994). They were usually equipped with AK-47 assault rifles, grenades and slashing knives or machetes (Prunier, 1995) and responsible for many of the most gruesome killings.

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 8 1994 by which the government openly assumed responsibility for the action of the militia, encouraging and reinforcing their activity (OAU, May 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 only in June 1993 by hard-line Hutu extremists, continuously called on the Hutu to kill the Tutsi. In these RTLM messages the Tutsi were dehumanized, labeled as "cockroaches", and the slogan "Hutu power" was repeatedly proposed to strengthen the feeling of unity among the Hutu population. 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 (the inaction of the international community is still today object of discussion). 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 Theoretical Channels

In this section we discuss three potential channels through which armed-group violence might affect civilian participation and present how to test them in the data.

First of all, armed groups might have simply informed civilians about the implementation of the genocide, telling them that participation would remain unpunished. We test this information channel by considering the interaction effect of armed groups and Hutu radio ownership which we take as a proxy for information. As noted, Radio Rwanda and Radio RTLM, the former having national coverage, relentlessly informed about the ongoing genocide.

Empirical Prediction 1. *If armed groups mostly work through information we should observe a negative interaction effect with Hutu radio ownership in the data.*

Second, 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.[10] Third, 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). As we do not have data to directly distinguish between the two last cases, we model the two scenarios in the following section and will then test their theoretical implications.[11]

3.1 Role Model vs. Force: A Simple Model

3.1.1 Set Up

Consider a village inhabited by a continuum of civilian villagers of size 1 that belong to the ethnic majority (Hutu) and of size T that belong to the ethnic minority (Tutsi). Assume the central Hutu government initiates an ethnic-cleansing campaign against the Tutsi minority. During this genocide a number of external Hutu militiamen M_e reaches the village in order to promote the killings.

Villagers own one machete each and need to be instructed in how to kill best and how to organize the killings. Imagine two types of villages $j \in \{o, w\}$: those that welcome the militia (w) and those that

[10]Strictly speaking, armed groups might have still only *informed* civilians, for instance about how to kill, something a radio reporter might have done just as well. However the difference to the information channel is that their *physical presence* was necessary.

[11]One might also think about the militia promising civilians security from the Tutsi rebels in return for their participation or allowing them to loot the property of their victims or changing the social norms in the village, instead of only teaching them how to kill and organizing or supervising the killings. Unfortunately neither the data nor the model allow us to say anything about this and we subsume it all under the role model channel. What we can say however, is the following: was the militias physical presence necessary and given so did they have to force civilians into the killings?

oppose the militia (o).[12] In some villages there might be Tutsi rebels stationed (R=1). Furthermore, local armed groups such as policemen $M_l^w(T,R)$ are already in welcoming villages but not in opposing villages ($M_l^o = 0$). Anecdotal evidence suggests, that there are fewer local militiamen in villages with a large Tutsi minority or Tutsi rebels, i.e. $\partial M_l^w / \partial S < 0$ with $S = T, R$. We call T and R the strategic factors. The militia turns ordinary Hutu civilians C^j in village j into civilian killers K^j at a decreasing rate, for example by training and organizing them,

$$K^j = K(M^j) \cdot C^j \tag{1}$$

where $M^j = M_e^j + \phi M_l^j$. We assume that $K_M > 0$ and $K_{MM} < 0$ and ϕ, between 0 and 1, measures the superiority of external militiamen. The militia wants to ensure that everyone of the Hutu majority joins in the killings, both to increase the killing rate and to promote collective guilt.

Hutu villagers in the opposing villages do not want to join in the killings, but resist or fight the militia together with the Tutsi civilians T and rebels R. Des Forges (1999, p. 156) writes: *"Both in Kigali and elsewhere, Hutu [occasionally] cooperated with Tutsi in fighting off militia attacks (...)."*. As is standard in the conflict literature, the militia's winning probability is given by a contest function

$$p = I(\gamma M, P) \tag{2}$$

where $\gamma > 1$ measures the militia's superiority, they often carry guns, and P is the number of Hutu and Tutsi civilians and Tutsi rebels opposing the militia. We 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 $I(0,P) = 0$. Thus the more militiamen join in the fight against the Hutu and Tutsi civilians the higher are the chances of winning and vice versa. Furthermore the first militia man joining the fight has a larger effect on winning than the second but only for small numbers of militiamen (Assumption 2.). There is anecdotal evidence that this is the case for military contexts (Dupey, 1987; Hirshleifer, 1989). The third assumption states that the effects of an additional militia man are larger when facing a small civilian opposition, which seems natural given Assumption 2.

3.1.2 Predictions

Prediction 1. *Expected civilian participation $E(K^j)$ is strictly increasing in the number of total militiamen M: $\partial E(K^j)/\partial M > 0$.*

[12] Note that opposing is defined in an active way: fighting or other how resisting the militia. One might however also imagine, that people were 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).

Prediction 2.

(i) $\gamma M \leq 1+T+\Gamma R$: *Expected civilian participation* $E(K^j)$ *is convex (strictly concave) in the number of militiamen M if Hutu villagers are opposing (welcoming) the genocide:* $\partial^2 E(K^o)/\partial M^2 \geq 0$ $\left(\partial^2 E(K^w)/\partial M^2 < 0\right)$.

(ii) $\gamma M > 1+T+\Gamma R$: *same as (i) for welcoming villages and ambiguous second derivative for opposing villages.*

Prediction 3.

(i) $\gamma M \leq 1+T+\Gamma R$: *The larger the strategic factor S, the smaller (larger) are the effects of the number of militiamen M on civilian participation* $E(K^j)$ *if Hutu villages are opposing (welcoming) the genocide:* $\partial^2 E(K^o)/\partial M \partial S < 0$ $\left(\partial^2 E(K^w)/\partial M \partial S > 0\right)$.

(ii) $\gamma M > 1+T+\Gamma R$: *same as (i) for welcoming villages and ambiguous effects for opposing villages.*

The proofs are presented in the appendix. Intuitively, Prediction (2) states that many militiamen are needed to successfully force civilians to join the killings but that only a few are needed to provide an example. This is only the case for low levels of militiamen. Once a certain threshold is reached additional militiamen arriving in opposing villages do not matter as much. Prediction (3) states that, in welcoming villages, one additional external militiaman has a larger effect on civilian participation when the Tutsi minority is large and vice versa in opposing villages. Intuitively, in welcoming villages with a large Tutsi minority or Tutsi rebels there are fewer local militiamen thus an additional man has a larger effect. In opposing villages a large Tutsi minority decreases the militia's effect on civilian participation because the Tutsi civilians will fight against the militia and will thus reduce the militia's chances of winning.

3.1.3 Robustness

The initial convexity of the contest function is crucial for Prediction 2. and although military evidence suggests that this convexity for low levels of militia seems reasonable, we now drop that assumption and use the following widely applied concave contest function instead

$$I(M,P) = \frac{\gamma M}{\gamma M + P} \tag{3}$$

Prediction 1. is unaffected by this change but Prediction 2. changes: The second derivative for the force model is now ambiguous also for case (i) and depends on functional forms. Prediction 3., however, is robust to this change, so fortunately we can still distinguish between the two channels. Proofs are again the appendix.[13]

[13] One might also imagine that teaching civilians how to kill requires a minimum number of militiamen. Although unlikely, this would imply convex effects also for the role model, in particular we should observe no effects for very low levels of militiamen. However, since we observe concave effects everywhere in the data, especially at the lower end, this possibility is irrelevant.

3.2 Model of Strategic Use of Armed Groups

Consider, a central genocide planner who wants to maximize civilian participation in the killings. The planner faces a fixed budget B, that is owns only a limited number of trucks that can transport her militiamen M^w_{ei} to each village i. To simplify, we let $K(M) = AM^{\alpha}$, with $A > 0$ and $0 < \alpha < 1$. Thus the planner faces the following problem[14]

$$\max_{\{M^e_i\}} \quad U = \sum_{i=1}^{N} A\left(M^w_{ei} + \phi M^w_{li}(S_i)\right)^{\alpha}$$
$$\text{s.t.} \quad B = \sum_{i=1}^{N} M^w_{ei} r_i \tag{4}$$

where r_i are the exogenous transport costs to reach each village.

3.2.1 Predictions

Prediction 4. *The number of militiamen $M^w_{ei} + M^w_{li} = M^w_i$ is strictly decreasing in the transport price r_i: $\partial M^w_i / \partial r_i < 0$.*

Prediction 5. *But this effect is smaller in strategically important villages: $\partial^2 M^w_i / \partial r_i \partial S_i > 0$*

Prediction 6.

(i) The number of external militiamen M^w_{ei} is strictly increasing in the strategic factors S_i: $\partial M^w_{ei} / \partial S_i > 0$.

(ii) The number of local militiamen M^w_{li} is strictly decreasing in the strategic factors S_i: $\partial M^w_{li} / \partial S_i < 0$.

(iii) The total number of militiamen M^w_i is strictly increasing (decreasing) in the strategic factors S_i if effect i (ii) dominates ii (i) : $\partial M^w_i / \partial S_i > 0$ ($\partial M^w_i / \partial S_i < 0$).

The proofs are presented in the appendix. Intuitively, high transport costs lead to fewer militiamen since these can be used more effectively in low cost villages. Furthermore because armed groups have larger marginal effects on civilian participation when the Tutsi minority is large, the central planner will prefer sending more militiamen into those villages with many Tutsi, thus transport costs should matter less. Note that we cannot directly test predictions 6(i) and 6(ii) in the data, since we do not separately observe local and external militiamen.

4 Data

We combine several data sets from different sources to construct our final dataset, which comprises 1433 Rwandan villages. The different data sets are matched by village names within communes.

[14]Note that we assume that the role model is prevailing, which seems to be the case in the data.

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 cannot be correctly matched 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. Table 1 reports the summary statistics for our variables.

Participation Rates Our two key measures are participation in armed-group violence and participation in civilian violence. Since no direct measure of participation rates is available, we use prosecution rates for crimes committed during the genocide as a proxy (Yanagizawa-Drott, 2012; Friedman, 2011). 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.

Category 1 concerns: 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 which 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 leaders, we consider this as representing armed group violence. There were approximately 77.000 people prosecuted in this category.

Category 2 concerns: 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 listed in category 1 and we therefore label this type of violence as civilian violence. Approximately 430.000 people were prosecuted in this category.

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 rates, the violence might have been so widespread that no 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.[15] 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*

[15] These sources are a 1996 report from the Ministry of Higher Education, Scientific Research, and Culture (Kapiteni, 2006); the PRIO/Uppsala data on violent conflicts (Gleditsch et al, 2002); and a database of timing and lethality of conflict from Davenport and Stam (2007).

of participants in each area". To provide further evidence for the accuracy of the Gacaca data, we also show that the prosecution numbers are strongly positively correlated with the distance to the nearest mass grave (indicating very high death rates). 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 child 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. We 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 we will show that our result are robust to dropping those villages with mass graves.[16]

Another concern is that villages with no reported armed group violence might have actually received militiamen, but unsuccessful ones. We deal with this concern in section 6.

Finally, random measurement error, there were allegations that these courts were occasionally misused to settle old scores resulting in false accusations, does not pose a major threat because we are instrumenting for armed-group violence. In fact, our instrumental variable approach will correct for potential attenuation biases arising from random mismeasurement.

Rainfall Data We 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 our knowledge the only one available, is crucial to obtain reasonable rainfall variation. Furthermore the high temporal resolution, i.e. daily estimates, allow us to confine variation in rainfall in our instrument to the exact period of the genocide. To construct our instrument, we 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 these buffers crisscross the various rainfall grids and each distance buffer is thus likely to overlap with more than one rainfall grid, we obtain considerable variation in rainfall along each buffer. The overall rainfall in each buffer is then 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 we also compute rainfall in each village. Figure 1 illustrates how the instrument is constructed.

Road, Town and Village Boundary Data A village boundary map is obtained from the Centre for Geographic Information System and Remote Sensing of the National University of Rwanda (CGIS-NUR) in Butare. Africover provides maps with the location of major roads and towns derived from satellite imagery. We 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.

[16]In another check we also drop those villages closer than 3.5 kilometers to a mass grave (10 percent of the sample).

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.[17] Ethnicity is defined as the fraction of people that are Hutu or Tutsi respectively. Except for population, all these variables are only available at the commune level. Verpoorten (2012c) provides data on the number of days each village was under the control of the RPF 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 were stationed in a village at the beginning of the genocide is taken from Straus (2006).

5 Empirical Strategy

5.1 Specifications

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

$$log\left(\frac{K_{ip}}{H_{ip}}\right) = \alpha^O + \beta^O log\left(\frac{M_{ip}}{H_{ip}}\right) + \mathbf{X}_{ip}\pi^O + \gamma_p + \varepsilon_{ip} \tag{5}$$

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

The two violence variables are, however, likely to be spuriously correlated and jointly determined by unobservable characteristics. Thus, even after including a large set of controls, our OLS estimates might still be biased. For instance, we lack a good control for leader quality in the villages and it might be the case 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, we would be underestimating the effect of armed group violence on civilian violence. 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 our

[17] Population data is only available for 1991. Mobility, however, was highly limited because of governmental restrictions and land markets were also strongly controlled (Prunier, 2005; Andre and Platteau, 1996).

estimates upwards.

We therefore use an instrument for armed-group violence. Our instrument is distance to the closest main road interacted with the total amount of rain that fell during the period of the genocide along the way between road and village. Given the controls in **X**, which we will explain below, this instrument captures the armed group's transport costs. We run the following first-stage estimation:

$$log\left(\frac{M_{ip}}{H_{ip}}\right) = \alpha + \beta\left[log(Dist_{ip}) \times log\left(Rain_{ip}\right)\right] + \mathbf{X}_{ip}\pi + \gamma_p + \varepsilon_{ip} \tag{6}$$

where M_{ip}/H_{ip} is, as before, our measure of armed group violence, $Dist_{ip}$ is the distance to the nearest main road and $Rain_{ip}$ is the amount of rain that fell during the period of the genocide along the way to village i in province p. As before, $\mathbf{X}_{ip}$ is a vector of controls, γ_p are province fixed effects and ε_{ip} is the error term. We expect β to be negative.

The second stage equation becomes

$$log\left(\frac{K_{ip}}{H_{ip}}\right) = \alpha' + \beta' \widehat{log\left(\frac{M_{ip}}{H_{ip}}\right)} + \mathbf{X}_{ip}\pi' + \gamma_p + \varepsilon_{ip} \tag{7}$$

where $log(\widehat{M_{ip}/H_{ip}})$ is instrumented as per (6). The coefficient β' captures the causal effect of armed-group violence on civilian violence.[18]

5.2 Identifying assumptions

Our 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 our control variables, 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 we can directly test the first-stage assumption, at this point we want to give some intuition as to why we should expect to find this negative relationship between transport costs and the number of militiamen in the data. It is today well documented 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,

> *"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*

[18] As usual, IV estimation gives the local average treatment effect (LATE). In the presence of heterogeneous treatment effects, our coefficient thus captures the effect of armed-group violence induced by low transport costs. However, identifying and measuring this LATE has valuable policy implications, which are discussed in our conclusions. In fact, we believe that in our case the LATE is more useful than the ATE, at least from a policy perspective.

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."

A USIP report (1995) also documents that more experienced killers were sent out into those areas where too few killings were taking place. 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, we do not have data on the exact locations of 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. We 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)). Water can collect in potholes and create deep puddles of water 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.[19] 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 our instrument should capture transport costs sufficiently well.

Exclusion Restriction Again, our IV 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. Our instrument, composed of distance to the main road, is probably correlated with factors such as education, health, access to markets and, therefore, with income. These characteristics are in turn likely to affect civilian participation, as reasons for joining the killings were often driven by 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, we control for distance to the road interacted with long-term average rainfall in the 100 days of the genocide along the way between village and road as well as all main effects.[20] Furthermore, we control for rainfall in the village during the 100 days

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

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

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. In the following analysis we will call these our "standard controls". To control for broad geographic characteristics we 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. We 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. Hatzfeld (2005) cites Hutu civilians (p.60), *"No one was going to the fields anymore. Why dig in the dirt when we were harvesting without working, eating our fill without growing a thing."* Nevertheless, to be cautious and to ensure that our instrument is not picking up any income effects but solely transport costs, we also include in our 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 purpose of the last interaction is to take 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. We call these "growing season controls".

At this point, we still need to argue that civilians were not directly affected by our 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 co-workers 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 slickend roads, were therefore limited for civilians. In addition, moving around along 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.[21] 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*

[21] Amnesty International (1999, 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).

tactical advantage, and retreat or relocate the attack when they encounter unexpected resistance". Besides this anecdotal evidence, in Section 6 we also present four indirect tests which all strongly support our identification assumption.

As a first robustness check, we also control for various other factors that potentially affect civilian participation, call them "additional controls". These include distance to the border, distance to major towns, distance to Kigali, distance to Nyanza as well as population density, village area and the number of days each village was under RPF control. 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 and village area eventually capture social pressure as well as food pressure. Many scholars believe that food pressure and limited cropland were important reasons behind the genocide (Verpoorten, 2012b; Diamond, 2005; and Boudreaux 2009).[22] Finally, falling under the control of the RPF, as they moved through Rwanda, likely affected civilian participation. Note that the movements of the RPF are unlikely to be correlated with our instrument, since the RPF, starting out in the North, *gradually* advanced through the entire country.

6 Results

OLS Estimates The number of militiamen in each village is positively correlated with the number of civilian perpetrators in OLS specifications with only standard controls (regression 1 in panel B in table 2), growing season controls (regression 2) as well as additional controls (regression 3). The point estimates, ranging from 0.631 (standard error 0.048) in our preferred specification (regression 3) to 0.653 (standard error 0.060), are very robust and all strongly statistically significant at 99 percent confidence. Nevertheless, as discussed above, unobservable characteristics might bias these estimates.

First Stage 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 2), 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 our preferred specification (regression 3) reaches 22.57.

Regarding magnitude, the point estimate of -0.541 (standard error 0.114) suggests that a village with an average distance to the road receives 18 fewer militiamen, which is about 35 percent of the mean (51.76), following a one standard-deviation increase in rainfall.

Interestingly, higher transport costs are also associated with fewer civilian perpetrators in the reduced form (regressions 4 to 6 in table 2), with a point estimate of -0.694 (standard error 0.131)

[22] 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.

in our preferred specification. The results are robust across all three specifications and throughout significant at 99 percent confidence. This is a first indication that villages which 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 armed group violence leads to a 1.281 percent (standard error 0.246) increase in the number of civilian perpetrators (regression 6 in table 2, with all controls). Our results are again very robust across all three specifications and throughout significant at 99 percent confidence level. The size of the estimated impact of armed-group violence on civilian violence is huge: when we focus on our preferred specification as our benchmark, these numbers imply that one additional external militia man resulted in $(430{,}000 \div 77{,}000) \times 1.281 \approx 7.2$ more civilian perpetrators or 11 additional deaths (under a linearity assumption that the number of perpetrators, 507,000, is proportional to the number of estimated victims, 800,000). 430,000 is the total number of prosecuted civilians and 77,000 the total number of militia and army men, respectively.

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 militiamen, around 10 percent of the total number of perpetrators, were directly and indirectly responsible for at least 656,000 Tutsi deaths which is about 82 percent (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 we reasonably assume that militia and army men have a higher killing rate than ordinary civilians this number is likely to be larger, since the direct effects of one additional militiaman increase.

The large IV coefficients, compared to the analogous OLS estimates, suggest that militia and army were strategically sent into those villages with originally little civilian participation.[23] Additionally, our instrumental variable strategy might be correcting for measurement error in the endogenous variable. Furthermore, we measure the local average treatment effect induced by changes in armed-group violence due to the instrument. Army and militiamen coming from further away, thus affected by transport costs, might have been particularly ruthless and ambitious, resulting in a high LATE as compared to the ATE which also includes the effect of local and maybe less effective or well trained armed groups, for instance local policemen.

Exclusion Restriction Traveling civilians, potentially affected by our 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.[24] Barriers, erected on roads

[23]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.

[24]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

and at the entrances to towns, enforced these regulations (Kirschke, 1996; Physicians for Human Rights, 1994). Reassuringly, 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."* Our IV estimates are very similar to our baseline results and equally statistically significant when we restrict the variation in rainfall in our instrument to the first 5 days, the first week or the first two weeks 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 3). The point estimate of the specification using only the first 5 days is 1.292 (standard error 0.548), almost identical to the ones from our baseline results, thus supporting our identification assumption. Importantly, this result does not imply that only the first couple of days are necessary to identify the main effect. In fact, the first-stage point estimates drop significantly as compared to our 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 3). First-stage and reduced-form point estimates moving together proportionally provides another indication that armed groups alone are driving these results.

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.[25] And reassuringly the results are similar, if anything larger, when we restrict the sample to those communes with no main road passing through (regression 4 in table 3), again supporting our identification strategy. Also, since civilians who traveled were most likely to pass on information about the genocide, a potential upward bias should be larger for villages with no outside information available, i.e. with little radio ownership. In the channels discussion below we show that this is not the case.

Note that Tutsi civilians escaping the violence are unlikely to matter, 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 cold transport cost calculation such as it was the case for the militia (as we will see below). Thus it should not be correlated with the instrument. For the same reason, those hundreds of thousands of Hutu who fled the country in fear of the RPF's revenge towards the end of the genocide, are also unlikely to bias our results. And reassuringly, using detailed migration data from a Rwandan household survey in 2000, we 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 our instrument is close to zero and highly insignificant (0.007, standard error 0.024, result not shown).[26]

Robustness Checks We next perform a couple of robustness checks, all reported in table 4. Potential survival bias in our prosecution data is unlikely to matter: the IV point estimates are virtually identical to our baseline results and similarly significant at 99 percent confidence when dropping

were introduced under the State of Emergency.

[25] An administrative unit above the village. There were 142.

[26] This EICV1 Household Survey from 2000 contains detailed migration history data for almost 15,000 individuals and is representative at the national level.

those 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 our sample size by about 10 percent (regression 2).

Potential underreporting of unsuccessful militiamen, something which would certainly bias our simple OLS estimates upwards, is unlikely to push up our IV estimates as well. To see this, we add the average number of militiamen per village in the sample to those villages with zero militiamen reported and rerun our baseline regression. The point estimate of 1.525 (standard error 0.354, regression 3) is very similar to our 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 IV 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 which are hard to reach: not only are the (wasted) costs of getting there higher but monitoring costs will certainly be higher as well. Finally, we are not aware of any anecdotal evidence supporting the notion of lazy or unsuccessful militiamen. If anything the contrary seems to be true: in Hatzfeld (2005, p.10) a civilian killer reports that the militiamen were the *"young hotheads"* who ragged the others on the killing job. Another one continues (p.62), *"When the Interahamwe noticed idlers, that could be serious. They would shout, We came a long way to give you a hand, and you're slopping around behind the papyrus!"*

One might also be worried 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 our 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).

Our estimates are also unaffected by adding the interaction of distance to the road with rainfall in the village during the growing season in 1994 and with the long-term average as well as 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).

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

Finally, one might be worried that the UN troops which were stationed in Kigali, although few, were affected by transport costs, thus biasing our estimates. But again, our results are robust to dropping villages in Kigali city (regression 7).

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

Channels We now examine the channels through which the militia might have affected civilian participation by testing Predictions 1., 2. and 3.

First we find that the presence of armed groups in each village was essential for mobilizing the population, that is the militia provided more than simple information about the ongoing genocide, and a genocide planner could not simply substitute for an eventual absence of armed groups by enhancing radio propaganda: the interaction effect of armed groups violence with a Hutu radio ownership dummy is insignificant and if anything positive (regression 1 in table 5, empirical prediction 1.). This result is also robust to controlling for the fraction of people with a cement floor (a good proxy for wealth) and its interaction with armed-group violence (regression 2). Interestingly, the effects of an additional militiaman are lower in richer villages. A one standard-deviation increase in the fraction of people with cement floor decreases the effect of armed groups by about 25 percent. (Note that, 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.)

Furthermore, the data shows that armed groups acted as role models, teaching civilians how to kill and organizing the genocide, but without exerting force. The interaction effects of armed groups violence with the two strategic factors (the Tutsi minority share and whether Tutsi rebels were stationed in the village) are both positive and the latter significant at 95 percent confidence level (regression 3), both findings clearly speak to the role model (Prediction 2.). Note that, in order to establish causality, we instrument each interaction term with the interaction between our instrument and the variable capturing the heterogeneous effects. Furthermore, we always include all double interactions.

In regression 4 we replace our continuous Tutsi minority share variable by a dummy taking on the value 1 if the Tutsi share lies above the median. Again, the point estimates are similar but in addition the interaction term with the Tutsi share is now also significant at 95 percent level. Unfortunately, because of strong multicollinearity, this specification does not allow us to control for the double interaction large Tutsi share times distance to the road. To account for the potential omitted variable bias this creates we interact the Tutsi dummy with our other controls not involving distance to the road and include them in the regression. The results are also robust to including the other three interaction terms from above (Tutsi rebels, fraction of people with cement floor and radio ownership; regressions 5, 6 and 7).

Consistent with the findings above, the effects of an additional militiaman seem to be decreasing which provides further support for the role model (Prediction 3.). Comparing our baseline regression in table 2 to a linear specification (regression 1 in table 6): the point estimates are similar in magnitude (6.776, standard error 2.905), but the fit deteriorates which suggests a non-linear relationship. Squaring the number of militiamen gives negative and significant point estimates pointing towards decreasing effects of the militia (regressions 2 and 3). Since the model predicts that we should observe convex effects especially for low levels of militiamen we restrict the sample to those villages where militiamen make up less than 5 percent (regression 2) or less than 1 percent (regression 3) of the population. Recalling the model, this implies that one militiamen is equivalent to 20, respectively 100 civilian Tutsi and Hutu fighters with the true value probably somewhere in between (=γ, the fighting superiority parameter from the contest function).

Strategic Use of Violence In the last main result we show that armed groups were strategically allocated among villages: both Predictions 4. and 5. are confirmed in the data. To test Prediction 4., i.e. that an increase in the transport costs reduces the number of militiamen, we rerun our first-stage regression but drop villages with high rainfall. 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 strategic cost considerations are at play. The point estimate of -0.709 (standard error 0.192) is even slightly larger than our baseline result in table 2 and still strongly significant at 99 percent confidence (regression 1 in table 7).

Also in line with the strategic use of armed groups (Prediction 5.), we find a positive and statistically significant interaction effect between transport costs and the Tutsi minority share with a point estimate of 2.103 (standard error 0.650) in our 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 4). Note that both the Tutsi minority variable and the number of militiamen are normalized by the size of the Hutu population in the village, thus these interactions effects are unlikely to simply reflect that larger village populations require more militiamen. Unfortunately we do not have enough statistical power to draw any conclusions from the interaction with our second strategic factor, the Tutsi rebels.

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 2.888 (standard error 0.570) and 3.048 (standard error 0.584); they suggest that a one standard-deviation increase in the Tutsi minority share increases the number of militiamen per Hutu by about 28 percent. Thus the inflow of external militiamen compensates for the fewer local militiamen (Prediction 6.). The opposite is true for villages with Tutsi rebels: coefficients are throughout negative and again highly significant.

All results are again robust when controlling for the fraction of people with a cement floor (regression 5), suggesting that wealth is not driving these effects.

7 External Validity

In this section we will argue that 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 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 occasionally instructed local perpetrators how to kill best, often they also photographed the gruesome scenes. The voluntary involvement of local Lithuanians in the almost total destruction of the Jews is still today subject to discussion and one of the defining features of the Lithuanian Holocaust.

To substantiate our argument that the Germans had an impact on Lithuanian participation in the killings of the Jews, we also present suggestive empirical evidence. To this end, we 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. We match this massacre data to an administrative map of Lithuania to get the number of Nazi (Lithuanian) massacres per municipality, our first unit of observation. Since we unfortunately lack data on the number of perpetrators, we assume that they are proportional to the number of massacre victims.[28] Very much 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 8) and this relationship holds up when we 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).[29]

Since we only have 48 municipalities we further divide Lithuania into 1033 grids of equal size (0.1 degree x 0.1 degree) which we again match to our massacre data. This refined analysis, allows us to control for 48 municipality fixed effects (and 133 artificial grid effects). Moreover, it confirms the positive relationship: point estimates increase to 0.898 (standard error 0.029) in our specification with

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

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

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 we cannot claim that these effects are causal, the results are comforting, in particular since we are 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. Reassuringly, 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 forcing Lithuanians to participate was unnecessary, again mirroring the Rwandan Genocide.[30]

Finally, we can also provide some suggestive evidence that transport costs seem to matter for the allocation of Nazis, thus resembling our 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 our three specifications using 1033 grids, and throughout significant at 99 percent confidence level (table 9).

Other Cases Another example can be found in 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 low-technology weapons such as 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.

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,

[30] This is not to say that, again similar to the Rwandan case, occasionally brave people risked their lives to help potential victims.

"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 violence in Kenya in 2007 following election disputes, 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 Discussion and Conclusion

Our analysis suggests that the mere physical presence of armed groups is crucial for mobilizing the civilian population for genocide. A simple back-of-the-envelope calculation further suggests that only 10 percent of the perpetrators, i.e. the mobile armed groups, were directly and indirectly responsible for at least 82 percent of the Tutsi deaths. The implications are potentially important for policy: if only a small group of perpetrators, in Rwanda around 50,000 men, are directly and indirectly responsible for the bulk of the killings, it should be possible for a peace-making force by explicitly targeting and taking out that very group of perpetrators to keep the conflict and the number of killings at bay. And, somewhat comforting, our results further suggest, that once the key group has been taken out a genocide planner cannot simply compensate for the absence of her armed troops by stirring up propaganda. Finally, our first stage hints how the spead of violence could be stopped, namely by blocking and securing the main transportation system, in Africa usually the road system.

In the light of our results we believe that General Romeo Dallaire - the Canadian commander of the UN force in Kigali at the time - was right when he insisted that with 5000 to 8000 troops he could have stopped the genocide, possibly saving hundreds of thousands of lives.

While we are keenly aware that the results are based on a single case study and that further research will be needed to corroborate them, the findings may be relevant for other countries. As Mueller (1999, p.42) puts it *"often (...) "ethnic war" is substantially a condition in which a mass of essentially mild, ordinary people can unwillingly and in considerable bewilderment come under the vicious and arbitrary control of small groups of armed thugs."*

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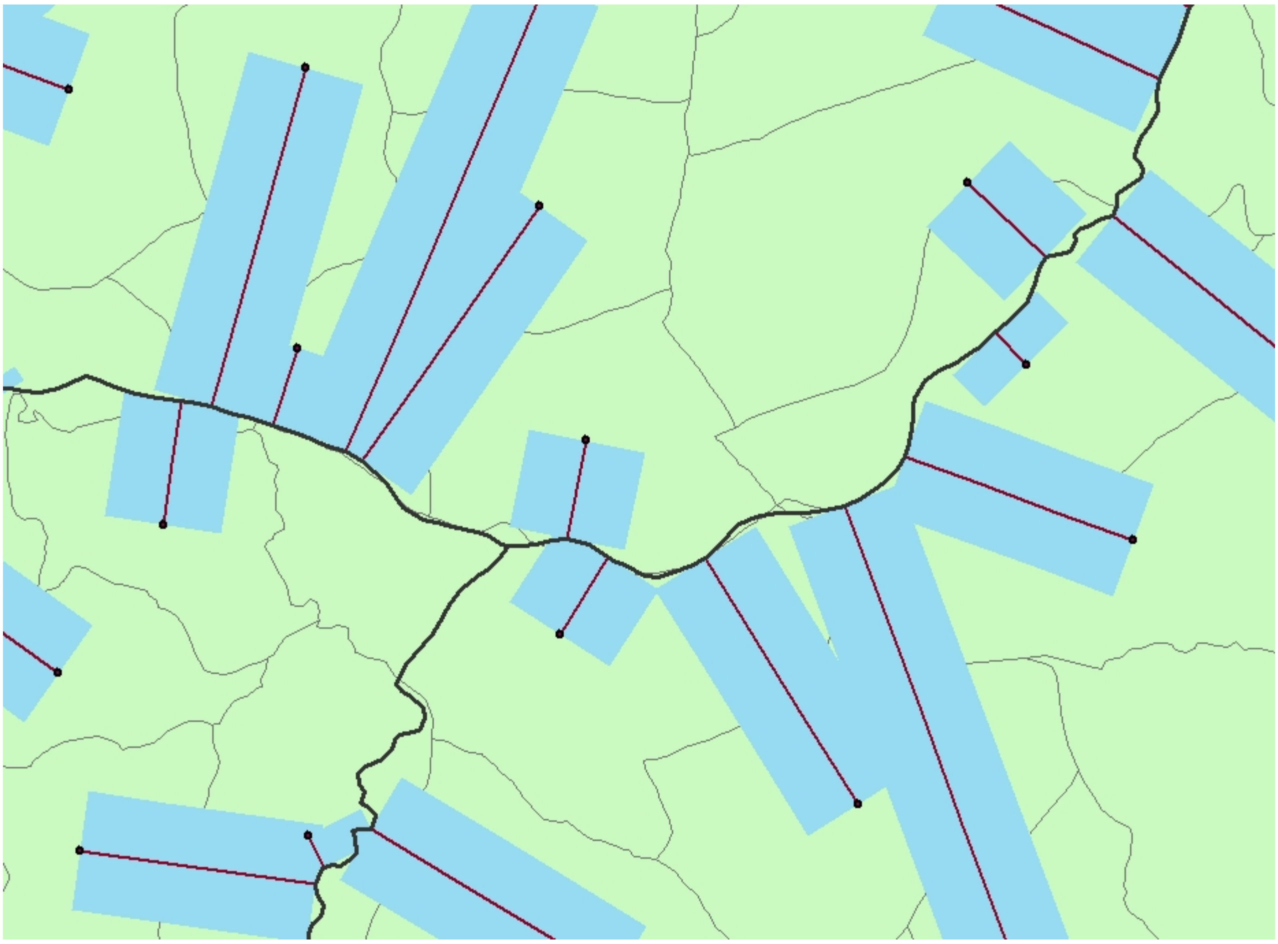
Figure 1: Construction of the Instrument in ArcGIS.



Light gray lines: village boundaries
Black lines: main roads
Red lines: shortest distance between road and village centroid (black dots)
Blue rectangles: 500m rainfall buffers
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	51.76	70.51	1433
# Prosecuted Civilians	290.25	286.43	1433
B. Exogenous Variables			
Total Rainfall between Sector and Road, 100 days, 1994	122.70	35.94	1433
Total Rainfall during 100 days, 1994	122.66	35.61	1433
Total Rainfall during Growing Season, 1994	243.87	69.63	1433
Total Rainfall between Sector and Road, 100 days, 10 year average	206.18	37.78	1433
Total Rainfall during 100 days, 10 year average	205.00	38.86	1433
Total Rainfall during Growing Seasons, 10 year average	621.12	117.52	1433
Total Rainfall during Whole Year, 10 year average	962.76	180.15	1433
Total Rainfall between Sector and Road, Whole Year, 10 year average	960.73	182.72	1433
Total Rainfall between Sector and Road, Harvest Season, 1994	22.42	10.15	1433
Distance to the Road	6.71	5.77	1433
Distance to Kigali	62.65	30.00	1433
Distance to Nyanza	64.36	30.74	1433
Distance to Main City	22.78	14.69	1433
Distance to the Border	22.61	13.93	1433
1991 Population, ’000	4.26	2.17	1433
1991 Population Density	498.60	850.55	1433
Number of days under RPF control	42.47	43.12	1432
Massgrave in Sector, Dummy	0.05	0.21	1432
Fraction with Radio	0.33	0.09	1433
Fraction with Cement Floor	0.09	0.09	1433
Tutsi Minority Size	0.10	0.13	1433
Military Opposition, dummy	0.05	0.23	1433

The violence categories are: # prosecuted militiamen is crime category 1 prosecutions against organizers, leaders, army and militia; # prosecuted civilians is crime category 2 prosecutions for homicides, attempted homicides and serious violence. The rain variables are measured in millimeter. The 10 year average is for the years 1984-1993. 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. Population is the population number in the village and Population Density is population per square kilometers, also from the 1991 Census. Days under RPF control gives the number of days each village was under the control of the Tutsi Rebels (RPF). Military opposition is a dummy indicating whether RPF Tutsi rebels were stationed in the village at the beginning of the genocide.

Table 2: Main Effects

A. Dependent Variable:	# Militiamen p.H.			# Civilian Perpetrators p.H.		
	First Stage			Reduced Form		
	(1)	(2)	(3)	(4)	(5)	(6)
Armed Group's Transportation Cost	−0.414	−0.514	−0.541	−0.529	−0.620	−0.694
	[0.115]***	[0.114]***	[0.114]***	[0.115]***	[0.113]***	[0.131]***
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	12.89	20.48	22.57	21.22	29.84	27.91
R^2	0.46	0.47	0.50	0.53	0.53	0.57
N	1433	1433	1432	1433	1433	1432

B. Dependent Variable:	# Civilian Perpetrators p.H.					
	OLS			IV/2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
#Militiamen p.H.	0.653	0.652	0.631	1.276	1.205	1.281
	[0.06]***	[0.061]***	[0.048]***	[0.315]***	[0.217]***	[0.246]***
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	0.54	0.58	0.55
N	1433	1433	1432	1433	1433	1432

Note: Unless otherwise stated, variables are in logs. Interaction terms are first individually logged and then interacted. Standard controls include 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 village area and the number of days under RPF control. There are 11 provinces in the sample. Standard errors correcting for spatial correlation with 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.

Table 3: Exclusion Restriction Tests

	Dependent Variable: # Civilian Perpetrators p.H., log (IV/2SLS)			
	First 5 days	First week	First 2 weeks	Communes w/o road
	(1)	(2)	(3)	(4)
# Militiamen p.H.	1.292	1.240	1.337	1.588
	[0.548]**	[0.391]***	[0.316]***	[0.546]***
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	0.55	0.58	0.52	0.41
N	1399	1406	1432	568
Coefficients on Excluded Instrument				
First Stage	−0.096	−0.143	−0.293	−1.003
	[0.046]**	[0.043]***	[0.074]***	[0.301]***
Reduced Form	−0.124	−0.178	−0.392	−1.593
	[0.06]**	[0.059]***	[0.091]***	[0.401]***

Note: Unless otherwise stated, variables are in logs. Interaction terms are first individually logged and then interacted. Standard controls include 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 village area and the number of days under RPF control. There are 11 provinces in the sample. Standard errors correcting for spatial correlation with a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4: Robustness Checks

	Dependent Variable: # Civilian Perpetrators p.H., log (IV/2SLS)						
	Measurement Error in Gacaca Data			Additional Controls		Commune Effects	Dropping Kigali
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Militiamen, p.H.	1.267	1.327	1.525	1.337	1.233	1.210	1.284
	[0.251]***	[0.27]***	[0.354]***	[0.26]***	[0.271]***	[0.36]***	[0.277]***
Standard Controls	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes
Harvest Controls	no	no	no	yes	yes	no	no
Other Rainfall Controls	no	no	no	no	yes	no	no
Province Effects	yes	yes	yes	yes	yes	no	yes
Commune Effects	no	no	no	no	no	yes	no
R^2	0.56	0.54	0.18	0.52	0.58	0.66	0.55
N	1366	1279	1432	1432	1432	1432	1400

Note: Unless otherwise stated, variables are in logs. Interaction terms are first individually logged and then interacted. Standard controls include 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 village area and the number of days under RPF control. 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. There are 11 provinces and 142 communes in the sample. Standard errors correcting for spatial correlation with a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 5: Channels, Interaction Effects

	Dependent Variable: # Civilian Perpetrators p.H., log (IV/2SLS)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Militiamen, p.H.	1.234	1.172	1.293	0.971	0.953	0.763	0.823
	[0.252]***	[0.282]***	[0.357]***	[0.393]**	[0.327]***	[0.285]***	[0.355]**
# MM, p.H. x Radio Ownership	0.474	0.464					0.180
	[0.45]	[0.445]					[0.305]
# MM, p.H. x Military Opposition			2.412		2.536	2.599	2.883
			[1.046]**		[0.977]***	[0.642]***	[1.113]***
# MM, p.H. x Tutsi Share			1.482				
			[2.303]				
# MM, p.H. x Large Tutsi Group				1.199	0.980	0.960	0.848
				[0.604]**	[0.463]**	[0.385]**	[0.365]**
# MM, p.H. x Cement Floor		−3.681				−2.983	−2.388
		[1.487]**				[1.197]**	[0.904]***
Standard Controls	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes
Tutsi Interactions	no	no	no	yes	yes	yes	yes
Province Effects	yes	yes	yes	yes	yes	yes	yes
R^2	0.52	0.55	0.43	0.18	0.23	0.35	0.35
N	1432	1432	1432	1432	1432	1432	1432

Note: Unless otherwise stated, variables are in logs. Interaction terms are first individually logged and then interacted. Standard controls include 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 village area and the number of days under RPF control. In each column we also control for the main effect and double interactions, e.g. in column (4) for a dummy indicating whether a village has a Tutsi minority size above the median. All interaction dummies take on the value 1 if the variable in question lies above the sample median. Tutsi interactions include the interaction of the Tutsi Share dummy with all other controls that do not involve distance to the road. Note in column (4) to (7) we do not control for Tutsi Share dummy interacted with distance to the road. There are 11 provinces in the sample. Standard errors correcting for spatial correlation with a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 6: Channels, Nonlinearity

	Dependent Variable: # Civilian Perpetrators, linear		
	Full sample	$\gamma = 20$	$\gamma = 100$
	(1)	(2)	(3)
# Militiamen, linear	6.776	13.372	20.533
	[2.905]**	[7.99]*	[6.113]***
# Militiamen, squared		−0.053	−0.235
		[0.029]*	[0.111]**
Standard Controls	yes	yes	yes
Growing Season Controls	yes	yes	yes
Additional Controls	yes	yes	yes
Province Effects	yes	yes	yes
R^2	.	.	0.07
N	1432	1384	904

Note: Standard controls include 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 village area and the number of days under RPF control. There are 11 provinces in the sample. The γ refers to the militia's superiority parameter, thus in regression 2 the sample is restricted to villages where militiamen make up less than 5 percent of the population and in regression 3 restricted to villages with less than 1 percent. Standard errors correcting for spatial correlation with a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 7: Strategic Use of Armed Groups

	Dependent Variable: # Militiamen p.H., log				
	(1)	(2)	(3)	(4)	(5)
Armed Group's Transportation Cost	−0.709	−0.452	−0.550	−0.526	−0.528
	[0.192]***	[0.127]***	[0.133]***	[0.134]***	[0.157]***
AGTC x Tutsi Share		2.514	2.414	2.130	2.053
		[0.807]***	[0.885]***	[0.65]***	[0.757]***
AGTC x Military Opposition		0.052	0.401	−0.100	−0.056
		[0.379]	[0.326]	[0.366]	[0.368]
AGTC x Cement Floor					0.885
					[0.825]
Tutsi Share		3.048	2.930	2.942	2.888
		[0.584]***	[0.547]***	[0.495]***	[0.570]***
Military Opposition		−1.296	−1.146	−0.962	−0.960
		[0.179]***	[0.156]***	[0.194]***	[0.192]***
Cement Floor					0.373
					[0.755]
Standard Controls	yes	yes	yes	yes	yes
Growing Season Controls	yes	no	yes	yes	yes
Additional Controls	yes	no	no	yes	yes
Province Effects	yes	yes	yes	yes	yes
R^2	0.48	0.49	0.50	0.52	0.52
N	1336	1433	1433	1432	1432

Note: Unless otherwise stated, variables are in logs. Interaction terms are first individually logged and then interacted. Standard controls include 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 village area and the number of days under RPF control. In each column we also control for all main effects and double interactions. There are 11 provinces in the sample. Standard errors correcting for spatial correlation with a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 8: The Case of Lithuania: Main Effects

	# Lithuanian Perpetrators				
	Municipalities		Artificial Grids (0.1 Degree)		
	(1)	(2)	(3)	(4)	(5)
# Nazi Perpetrators	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. There are 10 counties and 133 grid effects (0.3 degree). Standard errors correcting for spatial correlation with a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 9: The Case of Lithuania: "First Stage"

	# Nazi Perpetrators		
	(1)	(2)	(3)
Distance to Major Road or Railway	−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. There are 48 municipalities and 133 grid effects (0.3 degree). Standard errors correcting for spatial correlation with a radius of 150km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Appendix

Prediction 1. - Increasing Effects

1. Welcoming Villages: If Hutu villagers welcome the militiamen, then, for simplicity, all of them will surely join the militia, thus $C^w = 1$. This implies that civilian participation reads

$$E(K^w) = K(M) \tag{8}$$

The result follows immediately, since $K_M > 0$.

2. Opposing Villages: If the militia wins the fight against the Hutu and Tutsi villagers then, for again simplicity, every villager has to join in the killings, thus $C^o = 1$. If the militia however loses, then none of the villagers join and participation C^o is 0. Taken together this implies that the expected number of civilian killers is

$$E(K^o) = I(M,P) \cdot K(M) \tag{9}$$

where we assume, again for simplicity, that all of the civilians in the village and Tutsi rebels fight against the militia, thus $P = 1 + T + \Gamma R$ and $\Gamma > 0$. The first derivative is given by

$$\frac{\partial E(K^o)}{\partial M} = I_M(M,P) \cdot K(M) + I(M,P) \cdot K_M(M) \tag{10}$$

Since all of these terms are positive, the result follows immediately.

Prediction 2. - Nonlinearity

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

2. **(i) Opposing Villages**: 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

$$\lambda I(M_1) + (1-\lambda) I(M_2) \geq I(\lambda M_1 + (1-\lambda) M_2) \tag{11}$$

Now, set $M_2 = 0$. This gives

$$\lambda I(M_1) \geq I(\lambda M_1) \tag{12}$$

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

$$\lambda I(M_1) \cdot K(M_1) \geq I(\lambda M_1) \cdot K(M_1) \tag{13}$$

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

$$\lambda I(M_1) \cdot K(M_1) \geq I(\lambda M_1) \cdot K(M_1) > I(\lambda M_1) \cdot K(\lambda M_1) \tag{14}$$

Rearranging gives

$$\lambda H(M_1) > H(\lambda M_1) \tag{15}$$

which implies convexity of H.

2. (ii) Opposing Villages: The effects are ambiguous and depend on functional forms. For example, take $I(M,P) = \frac{M^\alpha}{P}$ (as long as $M^\alpha < P$) with $0 < \alpha < 1$ and $K(M) = M^\beta$ with $0 < \beta < 1$. The resulting product $H(M,P) = \frac{M^{\alpha+\beta}}{P} \cdot$ is convex if $\alpha + \beta \geq 1$ but concave otherwise.

Prediction 3. - Interactions

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

$$\frac{\partial E(K^w)}{\partial S \partial M} = K_{MM}(M_e + M_l(S)) \cdot \frac{\partial M_l}{\partial S} \tag{16}$$

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

2. (i) Opposing Villages: Take the derivative of $E\left(K^O\right)$ w.r.t. S and M to get

$$\frac{\partial E(K^o)}{\partial S \partial M} = I_{MP}(M,P) \cdot K(M) + I_P(M,P) \cdot K_M(M) \tag{17}$$

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

2. (ii) Opposing Villages: Now $I_{MP}(M,P) > 0$, thus $\frac{\partial E(K^o)}{\partial S \partial M}$ in equation (17) is ambiguous.

Robustness - Tullock Contest Function

Note that Predictions 1a to 3a only consider opposing villages since welcoming villages are unaffected by the contest function.

Prediction 1a. - Increasing Effects

We use the following Tullock contest function

$$I(M,P) = \frac{\gamma M}{\gamma M + P} \tag{18}$$

clearly, $I_M > 0$ and $I(M,P) \geq 0$. Thus equation (10) still holds.

Prediction 2a. - Nonlinearity

Assume, that $K(M) = M^{\alpha}$. The second derivative of $I(M,P) \cdot M^{\alpha}$ w.r.t M is positive only if the following inequality holds

$$\frac{M(M+2P)}{P^2} < \frac{\alpha}{(1-\alpha)} \tag{19}$$

which is not necessarily true, in particular for small values of α.

Prediction 3a. - Interactions

Recall the contest function

$$I(M,P) = \frac{\gamma M}{\gamma M + P} \tag{20}$$

Clearly, $I_P < 0$ and

$$I_{MP} = \frac{\gamma M - P}{(\gamma M + P)^3} \tag{21}$$

thus $I_{MP} \leq 0$ if $\gamma M \leq P$. Thus equation (17) still holds.

Prediction 4. and 5. - Central Planners Problem

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

$$M_{ei} = \frac{1}{1+\rho_i}\left(\frac{B}{r_i} - \rho_i \cdot \phi M_{li}(S_i) + \sum_{j \neq i}^{N} \frac{r_j}{r_i}(\phi M_{lj}(S_j))\right) \tag{22}$$

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.