Stockholm University

The Economics of Genocide and War

Thorsten Rogall

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To my family, especially Margit

Acknowledgments

In 2011, I quit my job as a PhD student at Yale University and moved to Stockholm to join the IIES. The reason was 94 centimeters long and weighed 12 kilograms. Some people said I was making a mistake. Looking back, I believe they were wrong. The IIES turned out to be a great, very inspiring place with many friendly and brilliant people.

One of them is Torsten Persson – my advisor. I first learned about Torsten as an undergraduate. A course on public finance listed his political economy book on the syllabus. Optional – so of course I didn't read it – but I vividly remember thinking, "Oh, funny, that guy has the same name." Later, at the IIES, I quickly realized that Torsten is so much more than just a pretty name. He is not only a brilliant academic who can give pointed advice on virtually anything but also a really nice person. Always generous with his time, he taught me how to push a paper to the limit, how to squeeze every last bit of information out of the data, how to write clearly and concisely, how to design an eye-catching presentation (thanks to his unerring eye for style). I stopped counting the number of hours we talked about my research, the number of e-mails he promptly answered, the number of times he read my papers or scrawled through my presentation. His guidance, his support, his comments and encouragement have considerably shaped this thesis and will surely echo in my future work – thank you so much! In Germany, your advisor is called *Doktorvater*. And in the beginning of my studies, I always thought of that expression as rather silly, overly emotional. But today, after having met Torsten, I must admit, I am absolutely sympathetic to it (I believe this is the biggest compliment I can make, and I gladly make it at this point).

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Stockholm, April 2015
Thorsten Rogall

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Introduction

This thesis consists of four self-contained essays. The first three essays concern the Rwandan Genocide.

In 1994, Hutu perpetrators killed approximately 800,000 people of the Tutsi minority in only about 100 days (Prunier, 1995). This astounding number of deaths could only be achieved because hundreds of thousands of civilians (about 85 percent of the total number of perpetrators) joined the militia and the army in carrying out the killings. The massive civilian participation is probably one of the most puzzling features of genocide. Ordinary farmers killed their neighbors, workers killed their co-workers, teachers killed their students and vice versa, hacking them to death with machetes.

Journalists, policy makers and some international relations scholars popularized the view that civilian participation is, in general, the consequence of an unstoppable outbreak of ancient hatred and there is not much that can be done about it. To illustrate this, one retired US admiral remarks on the subject, referring to the Bosnian War: "Let them fight. They've been fighting for a thousand years." (Rear Admiral JamesW. Nance (ret.) is quoted in Ashbrook (1995)). In contrast, my research on the Rwandan Genocide shows that the massive civilian participation did not follow from suddenly exploding ancient hatred, plunging the country into an unstoppable all-against-all conflict, but rather that civilian participation was carefully fostered by the central leaders in Kigali – rational actors – who used mandatory community meetings in the years before the genocide to prepare the population (essay 1) and sent around their militiamen and army men during the genocide to give the final orders (essay 2). Finally, essay 3 shows that the political Hutu elites' strategy to kill the Tutsi was indeed successful; places with more genocide violence did better economically six years after the genocide. Let me elaborate.

In the second chapter of my thesis, titled **Preparing for Genocide: Community Work in Rwanda** (co-authored with Evelina Bonnier, Jonas Poulsen and Miri Stryjan), we ask if and how political elites prepare the civilian population for participation in violent conflict. As noted above, we empirically investigate this question using village-level data from the Rwandan Genocide in 1994. Every Saturday before 1994, Rwandan villagers had to meet to work on community infrastructure, a practice called *Umuganda*. While *Umuganda* was originally designed as mandatory work meetings to improve village infrastructure, earlier accounts of the genocide suggest that at the beginning of the 1990s, these meetings were abused by the political Hutu elites to spread anti-Tutsi sentiments and prepare the population for genocide (Cook, 2004; Straus, 2006; Thomson, 2009; Verwimp 2013). To estimate the causal effect of these meetings, we exploit cross-sectional variation in meeting intensity induced by exogenous weather fluctuations. The idea is simple: we expect the meetings to be less enjoyable when it rains and, furthermore, to be canceled altogether under heavy rains.

We find that an additional rainy Saturday resulted in a five-percent lower civilian participation rate in genocide violence. Interestingly, this result is entirely driven by places under the control of the pro-genocide Hutu parties. In the few places with the pro-Tutsi opposition parties in power, the effects reverse, suggesting that in those places, these meetings were used to create bonds between the two ethnicities.

Despite the specific focus of this chapter, we argue that examining the possibly negative effect of these community meetings is of more general importance. There is a widely held belief that community meetings foster social capital, by providing arenas for people to meet, exchange ideas, solve free-rider problems and create public goods (Grootaert and van Bastelaer, 2002; Guiso, Sapienza and Zingalez, 2008; Knack and Keefer, 1997; Putnam, 2000). This notion considerably influenced the work of important development agencies, which increasingly focus on community-driven development projects in which deliberative forums and grass root participation play a central role (see Mansuri and Rao (2012) for a recent overview). We show that there is a "dark side" to these community meetings where social capital does not bridge the societal, ethnic divides but rather enforces bonding within the different ethnic groups, i.e. the Hutu population in the Rwandan case. Understanding this process is even more important since *Umuganda* was formally reintroduced in Rwanda in 2008, and similar practices have been set up in Burundi and are discussed in the Democratic Republic of Congo (DRC).

In the third chapter of my thesis, titled **Mobilizing the Masses for Genocide**, I ask whether political elites use armed groups to foster civilian participation in violence. Are these armed groups strategically allocated to maximize civilian participation? And how do they mobilize civilians? Again, I empirically investigate these three questions using village-level data from the Rwandan Genocide in 1994 – focusing on the time *during* the genocide.

To identify the causal effect of these militiamen, I use an instrumental-variables strategy. Specifically, I exploit cross-sectional variation in armed groups' transport costs induced by exogenous weather fluctuations: the shortest distance of each village to the main road interacted with rainfall along the dirt tracks between the main road and the village. The idea is, again, simple: I expect the movements of army and militia, performed by motor vehicles, to be limited by the heavy rains that characterize the first rainy season, which partly overlaps with the genocide, and the more so the further they have to travel.

Guided by a simple model, I come up with the following answers to the three central questions: (1) one additional armed-group member resulted in 7.3 more civilian perpetrators, (2) armed-group leaders responded rationally to exogenous transport costs and dispatched their men strategically to maximize civilian participation and (3) for the majority of villages, armed-group members acted as role models and civilians followed orders, but in villages with high levels of cross-ethnic marriage, civilians had to be forced to join in. I then argue that the results are also relevant for other cases of state-sponsored murder. In particular, I provide both anecdotal and suggestive empirical evidence that the killing of the Jews in Lithuania in the 1940s – organized by the Germans but mostly carried out by local civilians and militias – parallel the Rwandan Genocide in all three ways highlighted in this chapter.

Finally, a back-of-the-envelope calculation suggests that a military intervention targeting the various armed groups – only 10 percent of the perpetrators but responsible for at least 83 percent of the killings – could have stopped the Rwandan Genocide.

In chapter four, titled **The Legacy of Political Mass Killings: Evidence from the Rwandan Genocide** (co-authored with David Yanagizawa-Drott), we study how political mass killings affect later economic performance, again using data from the Rwandan Genocide. To establish causality, we build on Yanagizawa-Drott (2014) and exploit the village-level variation in reception of a state-sponsored radio station (RTLM) that, explicitly, and successfully, incited killings of the ethnic Tutsi minority population.

We find that households in villages that experienced higher levels of violence induced by the broadcasts have higher living standards six years after the genocide. They enjoy higher levels of consumption, own more assets, such as land, livestock and durable goods and output per capita from agricultural production is higher. These results are consistent with the Malthusian hypothesis that mass killings can raise living standards by reducing the population size and redistributing productive assets from the deceased to the remaining population.

However, we also find that the violence affected the age distribution in villages, raised the fertility rates among female survivors and reduced the cognitive skills of children. Together, our results show that political mass killings can have positive effects on living standards among survivors in the short run, but that these effects may disappear in the long run.

In the last chapter, titled **Ethnic Income Inequality and Conflict in Africa** (co-authored with Andrea Guariso), we demonstrate that income inequality between ethnic groups increases the likelihood of ethnic conflict in Africa. Since most countries in Africa rely heavily on rain-fed agricultural production, we exploit the exogenous variation in rainfall over each ethnic group's homeland to identify causal effects.

Our results indicate a strong and positive relationship between rainfall-based between group inequality and ethnic conflict. A one standard-deviation increase in inequality increases the risk of ethnic conflict by about 66 percent. We show that the effects entirely stem from rainfall during the growing season, which is when rainfall is most important for agricultural production and, thus, the economic welfare of individuals living in the region. We find no effects for vertical inequality, that is inequality across individuals, and we do not find any support for the relevance of within ethnic group inequality either.

These results pass several robustness checks and placebo tests. For example, we do not find any effects for non-ethnic conflict. Finally, our results have important policy implications to the extent that global climate change might affect different regions differently and thus increase inequality and conflict.

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Preparing for Genocide: Community Work in Rwanda*

2.1 Introduction

In many civil wars and conflicts, ordinary civilians participate in violence. For example, during the Rwandan Genocide in 1994, around 430,000 Hutu civilians joined the army and militiamen in killing an estimated 800,000 Tutsis and moderate Hutus in only 100 days.[1] Civilian participation in violence often magnifies and escalates a given conflict with disastrous effects on the social fabric and the economy, let alone the human suffering. It is thus crucial to understand the causes of civilian participation in violence. Anecdotal evidence for the Rwandan case suggests that in the years before the genocide, weekly-held community meetings called *Umuganda* were used to sensitize and mobilize the civilian Hutu population. While *Umuganda* was originally designed as mandatory work meetings to improve village infrastructure, earlier accounts of the genocide suggest that at the beginning of the 1990s, these

*This chapter is co-authored with Evelina Bonnier, Jonas Poulsen and Miri Stryjan. We thank Tom Cunningham, Jonas Hjort, Juanna Joensen, Magnus Johannesson, Erik Lindqvist, Eva Mörk, Suresh Naidu, Torsten Persson, Cristian Pop-Eleches, Marit Rehavi, David Strömberg, Jakob Svensson, Erik Verhoogen and Miguel Urquiola, as well as seminar participants at the ASWEDE Conference on Development Economics, SSE and Columbia Development Colloquium for many helpful comments. We also thank Christina Lönnblad for editorial assistance. Financial support from Handelsbanken's Research Foundations is gratefully acknowledged.

[1] In 1990, Rwanda had 7.1 million inhabitants out of which 6 million were Hutus.

meetings were abused by the political elites to spread anti-Tutsi sentiments and prepare the population for genocide (Cook, 2004; Straus, 2006; Thomson, 2009; Verwimp, 2013).

This chapter provides the first empirical analysis of how important these local *Umuganda* meetings might have been in inducing the civilian population to participate in the 1994 genocide. Despite the specific focus of this work, we argue that examining the possibly negative effect of these community meetings is of more general importance. There is a widely held belief that community meetings foster social capital by providing arenas for people to meet, exchange ideas, solve free-rider problems and create public goods (Grootaert and van Bastelaer, 2002; Guiso, Sapienza and Zingalez, 2008; Knack and Keefer, 1997; Putnam, 2000). This notion did considerably influence the work of important development agencies, which increasingly focus on community-driven development projects in which deliberative forums and grass root participation play a central role (see Mansuri and Rao (2012) for a recent overview). We investigate whether there is a "dark side" to these community meetings where social capital does not bridge the societal, ethnic divides but rather enforces bonding within the different ethnic groups, i.e. the Hutu population in the Rwandan case. Understanding this process is even more important since *Umuganda* was formally reintroduced in Rwanda in 2008, and similar practices have been set up in Burundi and are discussed in the Democratic Republic of Congo.

Identifying the causal effect of these meetings on participation in genocide is difficult for two reasons. First, we lack data on the number of people participating in *Umuganda* or the number of meetings taking place in a given locality. Second, even if such data existed, our estimates would likely suffer from an omitted variable bias. On the one hand, village-specific unobservable characteristics that affect both genocide participation and *Umuganda* intensity, for instance local leader quality, could produce a spurious positive correlation between the two, biasing the estimate upwards. On the other hand, if *Umuganda* meetings were strategically used in areas where genocide participation was unobservably low, the estimate would be downward biased.

To overcome these data and endogeneity issues, we use exogenous rainfall variation to estimate the effect of *Umuganda* meetings on participation in civil conflict. The idea is simple: we expect the meetings to be less enjoyable when it rains and, furthermore, to be canceled altogether under heavy rains. The fact that the community-work only took place *on Saturdays* makes it possible to isolate the *Umuganda* effect from general rainfall effects (e.g. rainfall affecting income through agriculture) by only using variation in Saturday rainfall while controlling for average daily rainfall. We use the number of Saturdays with heavy rainfall during the 3.5 year pre-genocide period (from October 1990, the outbreak of the civil war, to March 1994, the eve of the genocide) as our variable of interest. After the start of the civil war in October 1990, the tensions between Hutu and Tutsi intensified and the Hutu-dominated government became more aggressive towards the Tutsi minority, finally culminating in the genocide. To control for local characteristics, we include 142 commune fixed effects. Furthermore, we can provide a first placebo check by controlling for heavy rainfall on all other six weekdays. We thus ensure that identification only stems from local variation in rainfall on Saturdays, which is arguably exogenous and should only affect genocide participation through its effect on *Umuganda* meeting intensity.

There is, however, one major concern regarding the exclusion restriction. In particular, the effect we estimate might not be due to the political element of *Umuganda* per se, but merely a consequence of people getting together in general. We will argue in great detail why this concern is unwarranted.

We proxy for genocide violence by the number of people prosecuted in the Gacaca courts, normalized by village Hutu population.[2] About 10,000 local Gacaca courts were set up all over the country to prosecute the crimes com-

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

mitted during the genocide. Importantly, these courts distinguished between civilian perpetrators and perpetrators belonging to an organized group such as militia gangs, the national army or the local police. Using prosecution instead of actual participation rates might introduce some bias. However, the Gacaca data is strongly correlated with other measures of violence from other various sources and we also directly take potential bias into account in the empirical analysis.

Our reduced-form results indicate a negative relationship between *Umuganda* intensity and civilian participation in genocide: a one standard-deviation increase in the number of rainy Saturdays is associated with a 20 percent decrease in the civilian participation rate. Interestingly, this negative relationship is entirely driven by villages that are ruled by the pro-genocide Hutu parties. In places with pro-Tutsi parties in power, the effects are reversed, suggesting that in these places, these meetings were used to create bonds between the two ethnicities. In terms of mechanisms, several heterogeneous effects suggest that the Hutu elites used *Umuganda* beyond simple propaganda, i.e. to bring people together and practice their mobilization. In contrast, in pro-Tutsi governed places, the data indicates that the elites used these meetings to overcome the various forms of Hutu propaganda and potential opposition in the local Hutu population and thereby reduce Hutu participation in genocide violence.

All effects are similar although statistically weaker for organized participation. This is not surprising since militia and army men should not have been affected by pre-genocide rainfall in the village (they moved around during the genocide and did not necessarily commit their crimes in their hometowns).

Our results have important policy implications and are also relevant for other countries. In 2008, the Rwandan government reintroduced *Umuganda*. Our results show that these meetings can easily be abused and that caution is warranted, in particular since there is still tension between the Tutsi and the

Hutu in Rwanda. Furthermore, similar practices have been set up in Burundi and are being discussed in the Democratic Republic of Congo (DRC). Both Burundi and the DRC have a long history of violent conflict along ethnic lines, which again calls for caution when establishing an institution such as mandatory community meetings.

Our work contributes to the literature in several ways. First of all, it adds to the vast economics literature on conflict. Blattman and Miguel (2010) review this literature, vehemently calling for well-identified studies on the roots of individual participation in violent conflict. This paper adds to the conflict literature by providing novel evidence on the strong effects of local community meetings, controlled by the political elite, on civilian participation in violence. Recent studies on the determinants of conflict and participation in violence and killings consider institutions, government policy, income and foreign aid (Besley and Persson, 2011; Dell, 2012; Dube and Vargas, 2013; Mitra and Ray, 2014; Nunn and Qian, 2014, respectively). Furthermore, our paper complements the literature on the Rwandan Genocide (Rogall, 2014; Straus, 2004; Verpoorten, 2012a-c; Verwimp, 2003, 2005, 2006; Yanagizawa-Drott, 2014) by providing novel evidence on its careful preparation.

On the methodology side, our results add to the recent discussion of the effects of rainfall on conflict other than through the income channel (Iyer and Topalova, 2014; Rogall, 2014; Sarsons, 2011). Prominent studies that use various rainfall measures as instruments for income in Africa include Brückner and Ciccone (2010), Chaney (2013) and Miguel, Satyanath and Sergenti (2004). Our results suggest that rainfall might have negative direct effects on conflict.

Finally, our results are in line with Satyanath, Voigtlaender and Voth (2014) who speak for a "dark side" of social capital, in contrast to several contributions highlighting its positive effects (Grootaert and van Bastelaer, 2002; Guiso, Sapienza and Zingalez, 2008; Knack and Keefer, 1997).

The remainder of the chapter is organized as follows. Section 2.2 provides some background information on the Rwandan Genocide. Section 2.3 presents the data used for the analysis and Section 2.4 lays out our empirical strategy. Section 2.5 presents the main results and assesses their robustness and Section 2.6 discusses mechanisms and channels. Section 2.7 concludes with possible policy implications.

2.2 Background

A History of Conflict The origins of the Hutu and the Tutsi in Rwanda are still today unclear. The Tutsi (with a pre-genocide population share of around 10 percent, the clear minority) are said to have descended from Hamitic migrants from the North of Africa and the Hutu from the Bantu group, who traditionally lived in Rwanda. However, others say that the two ethnicities do have a common ancestry. What seems clear is that Belgian colonizers deepened the differences between the two ethnic groups, and deliberately favored the Tutsi minority. This division created a strong tension between the two groups and culminated in the Rwandan revolution of 1959, where the Tutsi monarchy was replaced by a Hutu republic. During these events, many Tutsi civilians were killed; others fled Rwanda for neighboring countries such as Burundi, Tanzania and, in particular, Uganda. In the 1960s, episodes of political stability alternated with times of violence, but the underlying tensions never stopped.

In 1974 – paramount to the introduction of a modern version of *Umuganda* – Juvénal Habyarimana took power in Rwanda through a coup d'état. His subsequent rule was based on a pro-Hutu ideology ("Hutu power"), further discussed in the next section. In October 1990, the RPF invaded Rwanda from Uganda, starting the Rwandan civil war. The RPF was a Tutsi rebel army, who had emerged in exile, eager to replace the Hutu-led government. Fighting between the Hutu-led government and the Tutsi rebels continued until the Arusha Accords were signed in August 1993.[3] A multi-party system was installed in the early phase of the peace talks, but had little effect on reducing societal tension and conflict. On April 6 1994, the airplane with president Habyarimana on board was shot down over Kigali. Whether the Tutsi or Hutu are responsible for this attack is still unclear today, but quickly after the attack, extremists within the Hutu-dominated parties announced a new interim government and started a 100-day period of ethnic cleansing throughout Rwanda. Around 800,000 people, mostly Tutsi and moderate Hutu lost their lives. The mass killings stopped in mid-July, when the RPF Tutsi rebels defeated the Rwandan Hutu army and the militia groups such as the Interahamwe.

A large number of Hutu civilians participated in the genocide violence, directed by the interim government (Dallaire, 2003). In our sample, there are approximately 416,000 civilian perpetrators.[4]

Umuganda The practice of *Umuganda* dates back to pre-colonial times. During a day of community service, villagers would get together to build houses for the poor, or help each other out in the fields in times of economic hardship (Mukarubuga, 2006). Rather than being mandatory, *Umuganda* was initially considered a social obligation (Melvern, 2000). This changed during the colonial period, when the Belgian colonizers used *Umuganda* for organizing compulsory work. Consistently, the local term for *Umuganda* was now *uburetwa*, or *forced labor* (IRDP, 2003). All men had to provide communal work 60 days per year. Most of the manual labor was hereby carried out by members of the ethnic Hutu majority under the supervision of Tutsi chiefs (Pottier, 2006): a first sign of *Umuganda's* potential to create a division between the two ethnic groups.

[3] The essence of this treaty was a power-sharing government, including representatives from both sides of the conflict.

[4] For more information, see for example Dallaire (2003), Des Forges (1999), Gouveritch (1998), Hatzfeld (2005, 2006), Prunier (1995) and Straus (2006).

During the post-colonial era from 1974 onwards, the meaning of *Umuganda* changed again when the newly elected Hutu president Habyarimana turned it into a political doctrine (Mamdani, 2001). Verwimp (2000, p. 344) cites Habyarimana:

> *"The doctrine of our movement [Movement for Development, MRND] is that Rwanda will only be developed by the sum of the efforts of its people. That is why it has judged the collective work for development a necessary obligation for all inhabitants of the country."*

The program combined a practical motivation – achieving development objectives with weak state finances – with a strong ideological element. Participation was again compulsory through government coercion, and failure to participate usually involved paying a fine.[5] The local leaders of the neighborhood who preceded over a group of ten households were responsible for the weekly *Umugandas* and could decide who were to participate and could demand fines from those failing to participate (Verwimp, 2000). The state chose the projects on which at least one adult male per family had to work *every* Saturday morning (Uvin, 1998). According to a report from 1986: 56 percent of the work performed during *Umuganda* included various types of anti-erosion measures, such as terracing and digging ditches; 15 percent were construction of communal buildings; 21 percent consisted of maintenance work of communal roads; 3 percent were related to construction of water supply systems and another 3 percent were related to agriculture. In this period, *Umuganda* substantially contributed to Rwanda's GDP (Guichaoua, 1991).

Habyarimana's ideology stressed the importance of the cultivator as the true Rwandan (Straus, 2006). This view clearly embraced the Hutu population with their history as cultivators, as opposed to the Tutsi who were

[5] In today's Rwanda, the fine for not participating in *Umuganda* is slightly less than $10.

said to be pastoralists. In fact, during the period leading up to the genocide, *Umuganda* was used to strengthen group cohesion within the "indigenous" ba-Hutu and marginalize the "non-indigenous" ba-Tutsi (Lawrence and Uwimbabazi, 2013). The patriotic focus of *Umuganda* became particularly salient in the early 1990's when *"government propaganda gave no choice to Rwandans other than to attend Umuganda for political mobilization"* (Lawrence and Uwimbabazi 2013, p. 253). Furthermore, *" (...) those who could not attend were regarded as enemies of the country who ran the risk of being brutalised and killed."* (ibid.).

Although little is known about the link between participation in *Umuganda* before the genocide and participation in violence during the genocide – a link which we hope to shed some new light on in this paper – anecdotal evidence speaks to the importance of *Umuganda* as an instrument for local party and state officials to mobilize the peasant population. The fact that all Rwandans of working age, be it farmers or intellectuals, were required to participate in *Umuganda* (Guichaoua, 1991) made it a potential arena for reaching the entire population. Although only a correlation, Straus (2006) shows that 88 percent of the perpetrators he interviewed regularly participated in *Umuganda* before the genocide broke out. Verwimp (2013, p. 40) notes:

> *"Umuganda gave the local party and state officials knowledge and experience in the mobilization and control of the labor of the peasant population. A skill that [would] prove deadly during the genocide."*

Umuganda was also used during the genocide itself, with the new name *gukorn akazi*, or "do the work", which meant the killing of Tutsis (Verwimp, 2013). Other slogans related to *Umuganda* used before the genocide such as "clearing bushes and removing bad weeds" now had a completely altered connotation (Lawrence and Uwimbabazi, 2013). By equating the participation in genocide violence with participation in *Umuganda*, the Hutu elite could signal that participation in genocide violence, just like participation in *Umuganda*, was a social obligation for all 'true' Rwandans.

In 2008, the Tutsi-led government re-introduced *Umuganda* in Rwanda with the general aim of promoting development and reducing poverty in the aftermath of the genocide (Uwimbabazi, 2012). Participation once more becomes mandatory for all able-bodied individuals between 18 and 65 years of age, and typical tasks include cleaning streets, cutting grass and trimming bushes along roads, repairing public facilities or building houses for vulnerable individuals on the last Saturday of every month.

2.3 Data

We combine several datasets from various sources to construct our final dataset with a total of 1,433 Rwandan villages. Villages are the second smallest administrative level, and the level for which the outcome data on the perpetrators is available. Table 2.1 reports the summary statistics for our variables.

Participation Rates Ideally we would like to have a direct measure of participation rates. Since such data does not exist, we follow the literature and use prosecution rates for crimes committed during the genocide as a proxy (Friedman, 2013; Heldring, 2014; Rogall, 2014; Yanagizawa-Drott, 2014). We thus use a nation-wide village-level dataset, provided by the government agency "National Service of Gacaca Jurisdiction", which collects the outcome of the almost 10,000 local Gacaca courts set up throughout the country. Two categories of perpetrators are identified.

The first category which we refer to as "organized participants" concerns: (i) planners, organizers, instigators, supervisors of the genocide; (ii) leaders at the national, provincial or district level, within political parties, army, religious denominations or militia; (iii) the well-known murderer who distinguished himself because of the zeal that characterized him in the killings or the excessive wickedness with which killings were carried out; (iv) people who committed rape or acts of sexual torture. These perpetrators mostly belonged to army and militia or were local leaders. Approximately 77,000 people were prosecuted in this category.[6]

The second category which we refer to as "civilian participants" concerns: (i) authors, co-authors, accomplices of deliberate homicides, or of serious attacks that caused someone's death; (ii) the person who – with the intention of killing – caused injuries or committed other serious violence, but without actually causing death; (iii) the person who committed criminal acts or became the accomplice of serious attacks, without the intention of causing death. People accused in this category are not members of any of the organized groups mentioned for the first category and are thus considered to be civilians. Approximately 430,000 people were prosecuted in this category. As mentioned, the second category is our main outcome variable since civilian participation in the killings is more likely to have been affected by *Umuganda* than organized participation.

The reliability of the prosecution data is an important issue for our analysis. One major concern is survival bias: in places with low prosecution rates participation might have actually been high because the violence might have been so widespread that no witnesses were left, or the ones remaining were too scared to accuse the perpetrators. This concern is, however, unlikely to be warranted: the Gacaca data is positively correlated with several other measures of genocide violence from different sources (Friedman, 2013).[7] Furthermore, Friedman (2013, pp. 19-20) states that *"the Gacaca courts have been very thorough in investigating, and reports of those afraid to speak are*

[6] Since we lose some observations for category 1 and category 2 in the matching process, our sample consists of 415,935 category 2 perpetrators and 74,168 category 1 perpetrators.

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

rare, so this data is likely to be a good proxy for the number of participants in each area." Nevertheless, to be cautious, we will show that our results are similar when dropping those villages with mass graves (an alternative indication of high death rates) and also robust to using the presence of a mass grave directly as a dependent variable.

Another concern is that some of those people prosecuted in the Gacaca courts might not have committed their crimes during the genocide, but rather during the civil war preceding the genocide (October 1990 until August 1993). In particular, we cannot rule out that (a) some perpetrators may, in fact, have been accused of participation in massacres and other kinds of violence during the civil war (and not during the genocide), and (b) that individuals who had previously participated in violence during the civil war were more likely to have been recognized and trialled for genocide crimes than individuals who participated "only" in the genocide. In order to mitigate this concern, we exclude communes with violence against the Tutsi during the period October 1990 to March 1994 (Viret, 2010). Importantly, violence against the Hutus was not trialled in the Gacaca courts (Human Rights Watch, 2011; Longman, 2009).

Rainfall Data The National Oceanic and Atmospheric Administration (NOAA) database of daily rainfall estimates, which stretches back to 1983, provides rainfall data, our source of exogenous weather variation. The NOAA data combines actual weather station data and satellite information on cloud cover to obtain rainfall estimates at 0.1-degree ($\sim$ 11 kilometers at the equator) latitude-longitude intervals. This data has two important advantages. First, since Rwanda is a very small country, the high spatial resolution is crucial for obtaining reasonable variation in rainfall. Second, the high temporal resolution, i.e. daily estimates, allows us to confine the variation in rainfall to the exact days of *Umuganda*. Since Rwanda is a very hilly country, there is a considerable local variation in rainfall. Moreover, these villages criss cross the various rainfall grids and each village polygon is thus likely to overlap with more than one rainfall grid. The overall rainfall in each village is thus obtained through a weighted average of the grids, where the weights are given by the relative areas covered by each grid.

Village Boundary, Road and City Data A village boundary map is provided by the Center for Geographic Information Systems and Remote Sensing of the National University of Rwanda (CGIS-NUR) in Butare. Importantly, the map comes with information on both recent and old administrative groupings. Since Rwandan villages have been reorganized under different higher administrative units several times after 1994, this information allows us to match villages across the datasets, e.g. the 1991 census and the Gacaca records.

Africover provides spatial maps with major cities and roads derived from satellite imagery. The maps are used to calculate several distance measures, such as the distance of the village to the nearest main road, to the nearest city, to the country borders and to Kigali and Nyanza, the recent capital and the old Tutsi Kingdom capital, respectively and to calculate the village area.

Additional Data The remaining data is taken from Genodynamics and the IPUMS International census data base: population, ethnicity and radio ownership from 1991.[8] Except for population, all these variables are only available at a higher administrative level, the commune level. We define Ethnicity as the share of people that are Hutu or Tutsi, respectively. Importantly, the Tutsi minority is spread out across the whole of Rwanda with an average population share of about 10 percent. We calculate the Tutsi minority share used in the following analysis as the share of Tutsi normalized by the share of Hutu.

[8]This data is only available for 1991. However, mobility was highly limited because of governmental restrictions and land market controls (Andre and Platteau, 1998; Prunier, 1995).

Verpoorten (2012c) provides data on the location of mass graves based on satellite maps from the Yale Genocide Studies Program. Guichaoua (1991) provides information on the party affiliation of the commune leaders (called burgomasters) at the eve of the genocide.

Matching of data and summary statistics The different datasets are matched by village names within their communes. A commune (142 in total) is an administrative unit above the village. Unfortunately, the matching is not perfect, since some villages have different names in different data sources. Besides, in some cases two or more villages within the same commune have identical names, preventing successful matching. However, in total, only about five percent of the villages do not have a unique match across all datasets. Furthermore, these issues are likely idiosyncratic, thus simply resulting in a lower precision in the estimates than what would otherwise have been the case.

2.4 Empirical Strategy

To identify the effect of *Umuganda* meetings on participation in genocide violence, we use local variation in rainfall as a proxy. Since we lack data on the number of people participating in *Umuganda*, we focus on the reduced-form effect. Our identification strategy thus rests on two assumptions. First, villages with heavier rainfall on Saturdays experienced fewer or less intensive *Umuganda* meetings (first stage). Second, conditional on our control variables, rainfall on Saturdays does not have any direct effect on genocide violence other than through the *Umuganda* meetings (exclusion restriction).

First Stage Ideally, we would like to directly test the first-stage relationship using data on the number of people participating in *Umuganda* before the genocide. Since such data does not exist, we instead provide indirect evidence for expecting a strong first stage.

Several other studies have documented and exploited negative relationships between rainfall and participation in open-air events. One of the first examples is Collins and Margo (2007) who use rainfall in April 1968 as an instrument for participation in the US riots after the death of Martin Luther King. More recent examples include Madestam et al. (2013) and Madestam and Yanagizawa-Drott (2011). Similarly, several other studies use rainfall and other weather phenomena for an exogenous variation in voter turnout on election days (Eisinga et al., 2012; Fraga and Hersh, 2011; Gomez et al., 2012; Hansford and Gomez, 2010; Horiuchi and Saito, 2009).

However, in all these cases, rain both has an effect on the direct cost of attending the open-air event and the opportunity cost of attending. For example, Lind (2014) finds that the voter turnout in Norway increases when it rains on the election day because bad weather reduces the opportunity cost of going to the polling station. Since *Umuganda* was mandatory, the opportunity cost mechanism is unlikely to play a role in our case, however. Instead, rainfall was to make the meetings and the work less productive, or even lead to cancellations. Still, the true functional form between rainfall and participation in mandatory community work is unknown. To make progress, we reasonably assume that the typical *Umuganda* tasks, exclusively outdoor work, became difficult or impossible to perform once a certain rainfall threshold had been reached.[9] Following Harari and La Ferrara (2013) who define an extreme weather shock as two standard deviations from the long-term average, we choose this threshold to be 10 mm.[10] Thus, we will use the number of Saturdays from October 1990 to March 1994 when each village received

[9]The typical *Umuganda* tasks took place outside and, as mentioned above, included landscaping, road maintenance, construction and agriculture (Guichaoua, 1991).

[10]The long-term average daily rainfall in Rwanda from 1984 to 1994 was 2.6 mm with a standard deviation of 3.8 mm. We calculate this number taking the average across all villages and all days from 1984 to 1994. Two standard deviations from the long-term average correspond to 10.24 mm.

more than 10 mm of rainfall as our main explanatory variable.[11] Furthermore, in Table 2.7, we show that our results are also robust to using average daily rainfall on Saturdays and all other weekdays as our main explanatory variables.

To better understand whether rainfall affected the extensive or the intensive margin of *Umuganda* meetings, we can vary these thresholds. More specifically, we will also use thresholds of 6 mm, 8 mm, 9 mm and 12 mm, respectively.[12] If we see effects already at low thresholds, it speaks for less enjoyable meetings or an effect at the intensive margin. If the effect is only set at higher levels, cancellations are more likely to be driving the results, i.e. an effect at the extensive margin. Average daily rainfall in Rwanda is low, however (see Table 2.1), which means that for very high thresholds, the variation will be too small to detect any effects.

Exclusion Restriction Once more, our empirical strategy relies on the counterfactual assumption that, absent the *Umuganda* meetings, rainfall on Saturdays had no effect on genocide violence. This is unlikely the case without further precautions. Rainfall on Saturdays, like all other weekdays, is likely to affect rain-fed production and is therefore correlated with income. Income, in return, potentially affects genocide participation since the reasons for participating were often driven by material incentives and genocide perpetrators were allowed to loot the property of the victims, or could pay bribes to avoid participation (Hatzfeld, 2005). Besides affecting agricultural out-

[11] Madestam et al. (2013) use a threshold of 0.1 inches (2.5 millimeter) of rainfall, a light drizzle, to predict participation in the Tea Party Tax Day rally in the US. While a 2.5 mm threshold may be appropriate to capture participation in a voluntary rally in the US, we believe that our case, mandatory meetings, requires a higher threshold. Madestam et al. (2013) also use 0.35 inches ($\approx$ 9 mm) as a robustness check for a higher threshold of rainfall. In Table 2.3, we show that our results are also robust to using this threshold.

[12] The 8 mm and 12 mm thresholds correspond to the average of the 95th and the 99th percentile of daily rainfall in Rwanda over the period from 1984 to 1994. Here we follow Dyson (2009) who, in order to understand the characteristics of rainfall in South Africa, defines heavy and very heavy rainfall as the average of the 95th and 99th percentile of daily rainfall, respectively.

comes, heavy rainfall might destroy infrastructure such as roads or housing, which is also likely to affect households' economic well-being and, therefore, participation in conflict.

To address this problem, and to solely isolate the Saturday rainfall effect, we control for average daily rainfall from January 1984 to September 1990 and our period of interest from October 1990 to March 1994. Furthermore, we control for rainfall on all other six weekdays. The absence of systematic, significant effects for days other than Saturdays serves as a first placebo test. To account for local characteristics, we also add 142 commune fixed effects.

At this point, we still need to argue that no other events potentially happening parallel with *Umuganda* on Saturdays could be driving our results. In particular, one might be concerned that people meeting and interacting in general might affect participation in genocide violence. Although we cannot directly test for this, we will provide several indirect tests alleviating this concern.

Specifications We run the following reduced-form regression to estimate the effect of *Umuganda* meetings on participation in genocide violence

$$\frac{G_{ic}}{H_{ic}} = \alpha + \beta \ \#Saturdays(Rainfall > t \ mm)_{ic} + \mathbf{X}_{ic}\pi + \gamma_c + \epsilon_{ic}, \qquad (2.1)$$

where G_{ic} is the number of Hutu prosecuted in either category 1 or category 2, i.e. our proxy for genocide violence and H_{ic} is the Hutu population in village i in commune c. $\#Saturdays(Rainfall > t \ mm)_{ic}$ is our explanatory variable of interest: the number of Saturdays from October 1990 to March 1994 with rainfall above t mm. Our main specification uses 10 mm as a measure of heavy rainfall, but our results are robust to using other rainfall thresholds. $\mathbf{X}_{ic}$ is a vector of village-specific controls, including average daily rainfall from January 1984 to September 1990, average daily rainfall from October 1990 to March 1994 and the number of all other weekdays with rainfall above t mm during our period of interest, October 1990 to March 1994. Finally, γ_c

are commune fixed effects, and ϵ_{ic} is the error term. We allow error terms to be correlated across villages within the same commune by clustering the standard errors on the commune level. For the sake of robustness, we also allow error terms to be correlated across villages within a 25, 50 and 75 km radius (Conley, 1999).[13] Moreover, since the prosecution rates are heavily skewed to the right, we weight our observations by total village population size, but our results do not rely on this weighting scheme. The coefficient of interest, β, captures the percentage point change in genocide participation following an additional Saturday with rainfall above t mm.

2.5 Results

Main Effects The reduced-form relationship between the number of civilian perpetrators per Hutu and the number of Saturdays with rainfall above 10 mm is strongly negative and statistically significant at the 99 percent significance level (regression 1 in Table 2.2) and this relationship holds up when adding 142 commune fixed effects (regression 2) and the number of other weekdays with rainfall above 10 mm (regression 3). Regarding magnitude, the point estimate of -0.409 (standard error 0.128, regression 3 with all controls) suggests that a one standard-deviation increase in the number of rainy Saturdays reduces the civilian participation rate by 1.73 percentage points (note that the civilian participation rate is measured in percent). If we assume a one-to-one relationship between the number of rainy Saturdays and the number of canceled *Umuganda* meetings, then a one standard-deviation increase in the number of canceled meetings reduces the average civilian participation rate by about 20 percent (interpreted at the mean of civilian perpetrators per Hutu, which is 7.7 percent). One possible explanation for this huge effect is the presence of nonlinearities. Since we use variation only at high numbers of *Umuganda* meetings, i.e. on average there are only 18

[13] The results are reported in Table 2.8.

rainy Saturdays or 18 canceled meetings, and if the effects at the lower end are small (the learning effect might only set in after a while), then the effect we measure with our data might be larger than the overall effect. Reassuringly, none of the other weekdays is systematically and significantly related to civilian violence (we cannot reject the null that all coefficients are equal to zero, p-value 0.937).

The results for organized perpetrators are statistically weaker, at the 90 percent level (regressions 4 to 6). This is not surprising: since organized perpetrators mostly consist of members of the militia, it is unclear that the village where they committed their genocide crimes (and were subsequently prosecuted in) is the same as the one where they lived before the genocide (October 1990 to March 1994). Thus, they will not have been exposed to the same number of *Umugandas* as the inhabitants of that village. If this is the case, our data is likely to suffer from measurement error increasing standard errors. Since the main focus in this paper is to examine if *Umuganda* can explain civilian violence, we will exclude organized violence from our main analysis. We will, however, report all the corresponding results for organized perpetrators in Tables 2.9 to 2.12.

To understand whether rainfall led to cancellations, or rather made the *Umuganda* meetings less enjoyable, we vary the threshold in increments of 2 mm: from 6 mm to 12 mm.[14] Table 2.3 reports the results. Heavy rainfall on Saturdays is negatively related to civilian participation for all thresholds and significant at least at the 90 percent confidence level for all thresholds above 6 mm. Importantly, we find the strongest effects for thresholds above 9mm, thus suggesting that it was rather cancellations that led to a decrease in violence. Once more, we find no significant effects for other weekdays and, consistently, we cannot reject the null hypothesis that the non-Saturday coefficients are jointly equal to zero (p-values range from 0.34 to 0.97). In addition, Figure

[14] To be consistent with Madestam et al. (2013), we also use 0.35 inches (which corresponds to 9 mm) as a threshold.

2.1 graphically illustrates these results for thresholds from 5 mm to 15 mm.

Robustness Checks Next, we perform a number of robustness checks and placebo tests, reported in Table 2.4. The potential survival bias in the prosecution data is unlikely to play a role: the reduced-form point estimates are essentially identical to our baseline results and similarly significant at the 99 percent confidence level when dropping villages with at least one mass grave (indicating high death rates, regression 1). Furthermore, we can also use the mass grave dummy as a dependent variable. Consistently, regressions 7 and 8 show that villages with many rainy Saturdays are less likely to have a mass grave site altogether. The point estimate of -0.013 (standard error 0.004, regression 8), significant again at the 99 percent level, suggests that a village is 26 percent less likely to have a mass grave site, given an additional rainy Saturday.

One might also be concerned that the UN troops which were stationed in Kigali, although few, affected the *Umuganda* meetings, thus driving our estimates. But again, the results are robust to dropping Kigali city (regression 2). Furthermore, the results are robust to excluding all the main cities and close-by villages (regression 3).

The results are also unaffected by adding a number of additional controls that potentially affect civilian participation in violence (regression 4). These include distance to the border, distance to major cities, distance to Kigali and distance to Nyanza as well as population density. To illustrate this, being close to the border potentially made it easier for the Tutsi or for moderate Hutu to leave the country. Distance to cities, in particular the capital Kigali, is likely to be correlated with economic activity and public goods provision. Nyanza was the old Tutsi Kingdom capital. Population density is meant to capture social pressure as well as food pressure, both important reasons for the genocide (Boudreaux 2009; Diamond, 2005; Verpoorten, 2012b).[15]

[15]The food pressure argument assumes a Malthusian model: a fixed amount of agricul-

As a first placebo check, we re-estimate the reduced-form regressions, instead using the number of Saturdays (and other weekdays) with high rainfall during the period October 1994 to March 1998 (from here on denoted the post-genocide period). To account for possible seasonality in the rainfall data, we chose the same calendar period as our period of interest, i.e. October 1, 1994 to March 31, 1998. Reassuringly, the coefficient for high rainfall on Saturdays in the post-genocide period is small and insignificant (-0.012, standard error 0.106, regression 5) and the same is true for the coefficients on all other weekdays of the post-genocide period (except Monday). These small and insignificant point estimates are further unchanged when adding rainfall by weekday during our period of interest, October 1990 to March 1994 (regression 6).

As another placebo check, we rerun the main specification for both organized and civilian violence using Saturday rainfall during the 3.5 year pre-genocide period (October 1, YEAR to March 31, YEAR+4) from the year 1983 until 2013. To illustrate this, we begin with the period from October 1, 1984 until March 31, 1988 and end with the period from October 1, 2009 until March 31, 2013. As expected, the two distributions of the resulting 20 coefficients are both somewhat centered around 0 and, reassuringly, the coefficient on Saturday rainfall from 1990 to 1994, the actual pre-genocide period, is an extreme outlier to the left in both cases: None of the other point estimates is larger in absolute value (the results are shown in Figures 2.2 and 2.3).

Exclusion Restriction After demonstrating a strong and robust effect of high Saturday rainfall on civilian participation in genocide, we still have to argue that this effect results from people *participating in Umuganda* together.

Most importantly, since major outdoor events, such as music festivals or soccer games, usually take place on weekends, potentially affected by rain-

tural land feeds a growing population (fertilizers were hardly used in Rwanda (Percival and Homer-Dixon, 2001)).

fall, one might be concerned that people meeting and interacting in general could affect participation in genocide violence. However, recalling our main result in Table 2.2, we find no significant effect for Sunday rainfall. Since people traditionally attend church on Sundays, this is the first piece of evidence speaking against the effects being driven by people meeting in general. Besides, as seen above, our results are robust to dropping the capital Kigali and other major cities in the sample; places where one might expect these major outdoor events to predominantly take place.

In a similar vein, heavy rainfall on public holidays, another occasion for people to meet, does not seem to matter: the point estimate on the number of public holidays with rainfall above 10 mm is statistically insignificant and small, when expressed in standard deviations (regression 1 in Table 2.5).[16] The same is true when adding religious and non-religious holidays separately to the regression (regression 2).[17]

Throughout our period of interest from 1990 to 1994, violent acts against Tutsi and moderate Hutu were already taking place. If these pre-genocide perpetrators are included in the Gacaca data, and there is a relationship between rainfall before the genocide and targeted violence during that period, for instance through transport costs, our estimates might be biased. To rule out this possibility, we drop communes where violence against the Tutsi took place before the genocide (Viret, 2010). Reassuringly, our results for civilian participation are robust (regression 3).

To provide further evidence that the effects we measure above result from the political elites abusing *Umuganda* meetings, we split the sample of villages into places with local pro-genocide Hutu party leaders and pro-Tutsi opposition party leaders. Interestingly, the negative relationship from above seems to be entirely driven by the pro-genocide Hutu-governed villages. The

[16] Note that we exclude holidays that fall on a Saturday since these might still have been subject to *Umuganda*.

[17] Religious holidays are, for instance, Easter and Christmas, non-religious holidays in Rwanda are, for instance, Independence Day and Labor Day.

point estimate on Saturday rainfall is -0.466 (standard error 0.123, regression 5), slightly larger than our main effect and again highly significant at the 99 percent confidence level. The opposite is true in pro-Tutsi villages: the point estimate on Saturday rainfall is large and positive, albeit given the small sample of only 161 villages, it is insignificant (0.706, standard error 0.896, in regression 6 and 0.399, standard error 0.796, in regression 7 with all other weekday controls). The numbers suggest that in these villages, the meetings were used to create bonds between the two ethnicities.

2.6 Channels

In this section, we try to better understand the channels and mechanisms through which *Umuganda* worked. Since the mechanisms in Hutu-governed villages and pro-Tutsi-governed villages are likely to differ, we always analyze the two sub-samples separately. All results are reported in Table 2.6.

Interaction Effects Starting with the Hutu-run villages, a natural first question is whether the political Hutu elites mostly spread propaganda and informed civilians about the views of the Hutu government – something a radio reporter might have done just as well – or whether the local elites rather brought civilians together, practicing mobilization, something that would certainly have required physical presence in the village. Importantly, there existed two radio stations in Rwanda (Radio Rwanda and Radio RTLM, the former had national coverage), which informed listeners about the pro-genocide view of the Hutu government. Thus, if the *Umuganda* meetings mostly worked through information, then the effect of the *Umuganda* meetings, i.e. Saturday rainfall, should be less negative (i.e. more muted) in villages that were already informed, through high levels of radio ownership. Thus, we should observe a positive interaction effect of Saturday rainfall with radio ownership among Hutu in the data. The point estimate on the interaction term is indeed positive (0.659, regression 1); but, with a standard error of 0.786, it is clearly insignificant. Furthermore, when we replace the radio ownership variable by a dummy taking the value of 1 if radio ownership lies above the median, the interaction effect is essentially zero (the result is not shown). Thus, it seems to be the case that *Umuganda* worked beyond information and propaganda.

Rather, consistent with the local elites using *Umuganda* to bring people together, the interaction effect of Saturday rainfall with population density is positive and highly significant at the 99 percent confidence level. The point estimate of 0.134 (standard error 0.023, regression 2) suggests that a one standard-deviation increase in population density reduces the effects of *Umuganda* by about 28 percent. Thus, *Umuganda* has been particularly effective in less densely populated areas – bringing people together.

The effectiveness of *Umuganda* might also depend on the size of the Tutsi minority. Large Tutsi minorities might boycott or hinder the meetings. However, the data suggests that this is not the case. The point estimate on the interaction effect of Saturday rainfall and the Tutsi size is insignificant and, if anything, negative (regression 3). This is once more not surprising: since the Tutsi were the clear minority in Rwanda, never holding the majority in any village, the Hutu elites did not have to worry to any considerable extent about their presence. In fact, taken at face value, the negative point estimate of -1.090 (standard error 1.526) suggests that the meetings were more successful in villages with larger Tutsi minorities. The perceived Tutsi threat might have been more salient in these villages and the enemy easier to point out. All results are robust to adding all three heterogeneous effects at once (regression 4).

The opposite is true in villages run by pro-Tutsi party elites. In these villages, it seems that the local elites had to use the *Umuganda* meetings to compensate for the anti-Tutsi propaganda spread on the radio. The interaction effect of Saturday rainfall with radio ownership among the Hutu is negative and significant at the 90 percent confidence level. The point estimate of -12.925 (standard error 6.542) suggests that the positive effect of *Umuganda* is about 26 percent lower in places with a radio ownership level of one standard deviation as compared to places with no radio ownership at all (regression 5).

Furthermore, the local pro-Tutsi elites seemed to have been more effective in villages with fewer Hutu. The interaction effect of Saturday rainfall with the size of the Tutsi minority is positive and almost statistically significant (p-value 0.124) in regression 7. This is consistent with the Tutsi elites having to overcome a potential pro-genocide bias in the Hutu population. Population density, however, did not seem to matter in these villages. The interaction effect of Saturday rainfall with population density in regression 6 is insignificant and, if anything, positive (0.355, standard error 0.494). Thus, contrary to the Hutu-run villages, *Umuganda* in pro-Tutsi villages was more successful in highly populated areas. The above results are once more robust to controlling for all three heterogeneous effects at once (regression 8).

2.7 Discussion and Conclusion

Our results show that the local Hutu elites used mandatory community meetings to mobilize the civilian population for genocide. Using exogenous variation in heavy rainfall on the day of the mandatory community-work meetings, *Umuganda*, we find that one additional rainy community day decreased the share of civilian perpetrators in the Rwandan Genocide by around 5 percent. Interestingly, this negative effect turns positive in villages run by anti-genocide pro-Tutsi parties. Thus, in these villages, the meetings were used to compensate for the various forms of Hutu propaganda on the radio and bridge the differences between the two ethnic groups. Our findings are important for several reasons.

First, a large number of civilians participated in the killings during the

Rwandan Genocide. While it is a common understanding that the genocide was centrally planned and organized, little is known about the link between the planning and the wide acceptance of the genocide among the civilian population. Our paper suggests that weekly held community meetings played a major role in this preparation and mobilization process.

Second, people getting together during community meetings is commonly said to foster a sense of belonging and create social capital, generally viewed as positive for development and community building (see, for example, Knack and Keefer, 1997; Grootaert and van Bastelaer, 2002; Guiso, Sapienza and Zingalez, 2008). As emphasized by Putnam (2000), social capital can bridge the divides in a society. However, we show that there is a "dark side" to these community meetings. More specifically, our results show that when placed in the wrong hands, the effects can become disastrous. However, somewhat comfortingly, our paper also shows the bright side of *Umuganda*; in particular when placed in the right hands, it can (partly) work against propaganda and overcome hatred.

The more optimistic view of this institution might explain why the current Rwandan government reinstalled *Umuganda* in 2008. Indeed, official statements about *Umuganda* emphasize values such as "solidarity" and "reconciliation", and the practice is said to foster a sense of community. These mandatory work days are now held monthly, on the last Saturday of every month. A similar practice is also present in Burundi and is being discussed in the Democratic Republic of Congo. Our analysis clearly shows that these meetings are powerful instruments and caution is warranted, especially in countries with long histories of ethnic tension.

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Tables and Figures

Table 2.1: Summary Statistics

	Mean	Std.dev.	Obs.
A. Violence & Population			
# Militiamen	290.25	286.43	1433
# Civilian Perpetrators	51.76	70.51	1433
# Civilian Perpetrators per Hutu (p.H.)	7.66	7.93	1433
# Militiamen per Hutu (p.H.)	1.40	2.09	1433
Pre-Genocide Violence against Tutsi, dummy	0.15	0.36	1433
Mass Grave found in Sector, dummy	0.05	0.21	1432
Political Opposition, dummy	0.11	0.32	1433
Population in Sector, '000	4.88	2.48	1433
Hutu Population in Sector, '000	4.26	2.17	1433
Population Density	0.50	0.85	1433
B. Rainfall			
# Sat(Rainfall>10mm)	18.25	4.24	1433
# Sun(Rainfall>10mm)	15.14	5.19	1433
# Mon(Rainfall>10mm)	15.13	4.22	1433
# Tue(Rainfall>10mm)	18.10	3.52	1433
# Wed(Rainfall>10mm)	20.51	4.76	1433
# Thu(Rainfall>10mm)	21.53	3.97	1433
# Fri(Rainfall>10mm)	17.02	4.75	1433
Average Daily Rainfall, 1980s	2.58	0.48	1433
Average Daily Rainfall, 1990s	2.44	0.55	1433
# Pub. Holidays(Rainfall>10mm)	0.85	0.20	1433
# Non-Rel. Holidays(Rainfall>10mm)	1.56	0.21	1433
# Rel. Holidays(Rainfall>10mm)	1.00	0.11	1433
C. Other Variables			
Fraction of Hutu with Radio	0.33	0.09	1433
Tutsi Minority Share	0.10	0.13	1433
Distance to Kigali	3.99	0.64	1433
Distance to Main City	2.91	0.71	1433
Distance to Nyanza	4.00	0.66	1433
Distance to the Main Road	1.41	1.23	1433
Distance to the Border	2.82	0.91	1433

Note: The # prosecuted militiamen is crime category 1: prosecutions against organizers, leaders, army and militia; # prosecuted civilians is crime category 2: prosecutions against civilians. The per Hutu (p.H.) variables are expressed in percent. Pre-Genocide Violence against Tutsi is a dummy taking on the value of 1 if the village experienced violence against Tutsi in the pre-genocide period. The two average daily rainfall variables are measured in millimeters. The distance variables are measured in kilometers. Population is the population number in the village and Population Density is population per square kilometer, from the 1991 census. Radio ownership and ethnicity data is taken from the 1991 census, available only at the commune level. There are 142 communes in the sample. The Tutsi Minority Share is defined as the fraction of Tutsi normalized by the fraction of Hutu.

Table 2.2: Main Effects

Dependent Variable:	# Civilian Perpetrators, p.H.			# Militiamen, p.H.		
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10mm)	−0.580	−0.425	−0.409	−0.115	−0.065	−0.057
	(0.118)***	(0.125)***	(0.128)***	(0.033)***	(0.033)*	(0.030)*
# Sun(Rainfall>10mm)			0.041			−0.037
			(0.102)			(0.031)
# Mon(Rainfall>10mm)			0.080			0.100
			(0.112)			(0.031)***
# Tue(Rainfall>10mm)			0.023			−0.046
			(0.084)			(0.030)
# Wed(Rainfall>10mm)			0.031			0.007
			(0.111)			(0.028)
# Thu(Rainfall>10mm)			−0.007			−0.064
			(0.134)			(0.041)
# Fri(Rainfall>10mm)			−0.057			0.006
			(0.099)			(0.027)
Standard Controls	yes	yes	yes	yes	yes	yes
Commune Effects	no	yes	yes	no	yes	yes
R^2	0.15	0.52	0.52	0.07	0.36	0.37
N	1433	1433	1433	1433	1433	1433

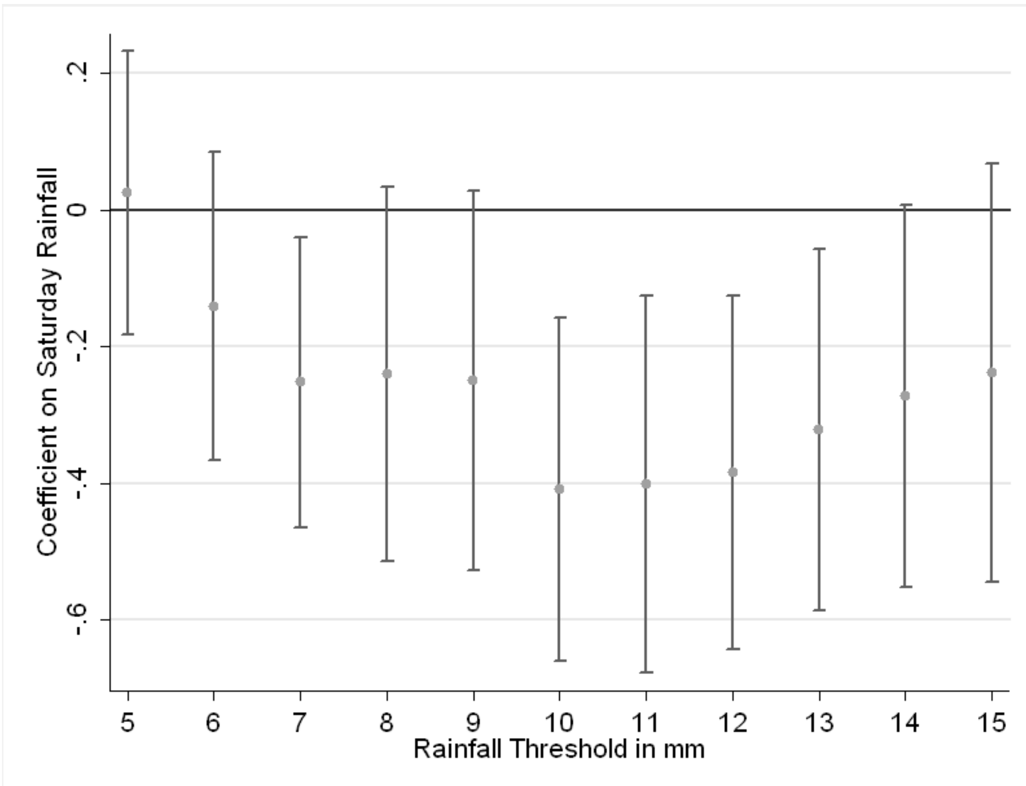
Note: **# of Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to March 1994 (and similarly for all other weekdays). **# Civilian Perpetrators per Hutu (p.H) and # Militiamen per Hutu** are measured in percent. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 2.3: Different Rainfall Thresholds

Dependent variable:	# Civilian Perpetrators, p.H.				
Rainfall Threshold x:	6 mm	8 mm	9 mm	10 mm	12 mm
	(1)	(2)	(3)	(4)	(5)
# Sat(Rainfall > x mm)	−0.142	−0.241	−0.250	−0.409	−0.385
	(0.115)	(0.140)*	(0.141)*	(0.128)***	(0.132)***
# Sun(Rainfall > x mm)	0.080	0.068	0.073	0.041	−0.043
	(0.084)	(0.124)	(0.117)	(0.102)	(0.137)
# Mon(Rainfall > x mm)	0.069	0.009	0.079	0.080	−0.053
	(0.088)	(0.118)	(0.117)	(0.112)	(0.120)
# Tue(Rainfall > x mm)	−0.020	0.000	0.069	0.023	0.135
	(0.128)	(0.123)	(0.099)	(0.084)	(0.123)
# Wed(Rainfall > x mm)	0.003	0.043	−0.065	0.031	−0.058
	(0.093)	(0.111)	(0.106)	(0.111)	(0.118)
# Thu(Rainfall > x mm)	0.129	0.004	0.140	−0.007	−0.233
	(0.096)	(0.107)	(0.123)	(0.134)	(0.107)**
# Fri(Rainfall > x mm)	−0.048	0.106	−0.079	−0.057	−0.216
	(0.086)	(0.094)	(0.086)	(0.099)	(0.137)
Standard Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
R^2	0.51	0.51	0.51	0.52	0.52
N	1433	1433	1433	1433	1433

Note: **# of Sat(Rainfall>x mm)** is the number of Saturdays with rainfall above x mm during the period October 1990 to March 1994 (and similarly for all other weekdays). The value of x is given in the column header. **# Civilian Perpetrators per Hutu (p.H)** is measured in percent. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure 2.1: Different Rainfall Thresholds



Note: We rerun our main specification for civilian participation with different thresholds. The coefficients on Saturday rainfall are reported together with 95 percent confidence intervals on the y-axis.

Table 2.4: Robustness and Placebo Tests

Dependent variable:	# Civilian Perpetrators, p.H.						Massgrave in Village	
	Without Mass Graves	Without Kigali	Without Major Cities	Additional Controls	Future Rainfall		Alternative Dep. Var.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Sat(Rainfall>10mm)	−0.403	−0.426	−0.425	−0.368		−0.452	−0.015	−0.013
	(0.129)***	(0.131)***	(0.133)***	(0.125)***		(0.126)***	(0.004)***	(0.004)***
# Sun(Rainfall>10mm)	0.054	0.048	0.065	0.033		0.051		0.002
	(0.103)	(0.106)	(0.108)	(0.107)		(0.110)		(0.004)
# Mon(Rainfall>10mm)	0.056	0.076	0.063	0.113		0.108		−0.003
	(0.109)	(0.113)	(0.115)	(0.114)		(0.101)		(0.004)
# Tue(Rainfall>10mm)	0.074	0.028	0.049	0.021		0.062		0.008
	(0.086)	(0.089)	(0.089)	(0.080)		(0.086)		(0.004)*
# Wed(Rainfall>10mm)	0.029	0.015	0.035	0.033		0.055		0.006
	(0.107)	(0.121)	(0.127)	(0.105)		(0.133)		(0.004)
# Thu(Rainfall>10mm)	−0.011	0.021	0.018	0.019		0.059		−0.003
	(0.128)	(0.136)	(0.140)	(0.126)		(0.144)		(0.004)
# Fri(Rainfall>10mm)	−0.014	−0.054	−0.034	−0.006		−0.034		−0.009
	(0.097)	(0.099)	(0.104)	(0.098)		(0.103)		(0.003)**
# Sat(Rainfall>10mm), 94-98					−0.012	0.008		
					(0.106)	(0.111)		
# Sun(Rainfall>10mm), 94-98					0.130	0.111		
					(0.114)	(0.112)		
# Mon(Rainfall>10mm), 94-98					−0.279	−0.323		
					(0.118)**	(0.139)**		
# Tue(Rainfall>10mm), 94-98					−0.153	−0.099		
					(0.109)	(0.105)		
# Wed(Rainfall>10mm), 94-98					−0.168	−0.231		
					(0.152)	(0.155)		
# Thu(Rainfall>10mm), 94-98					−0.123	−0.113		
					(0.128)	(0.131)		
# Fri(Rainfall>10mm), 94-98					0.124	0.200		
					(0.110)	(0.103)*		
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	no	no	no	yes	no	no	no	no
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.51	0.51	0.51	0.52	0.51	0.52	0.16	0.16
N	1367	1422	1358	1433	1433	1433	1432	1432

Note: **# of Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to March 1994 (and similarly for all other weekdays). **# Civilian Perpetrators per Hutu (p.H)** is measured in percent. In **regression 1** we drop villages with mass graves (indicating high death rates). In **regression 2** we drop villages in the capital Kigali and in **regression 3** we drop all villages in and close to the main cities. In **regression 4** we add additional controls. These are population density, distance to Kigali, Nyanza, the border, the closest main road and the closest main city. In **regressions 5 and 6** we also control for future rainfall. In **regressions 7 and 8** we use a dummy indicating whether a mass grave was found in the village as an alternative dependent variable. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure 2.2: Placebo Check: Civilian Partcipation

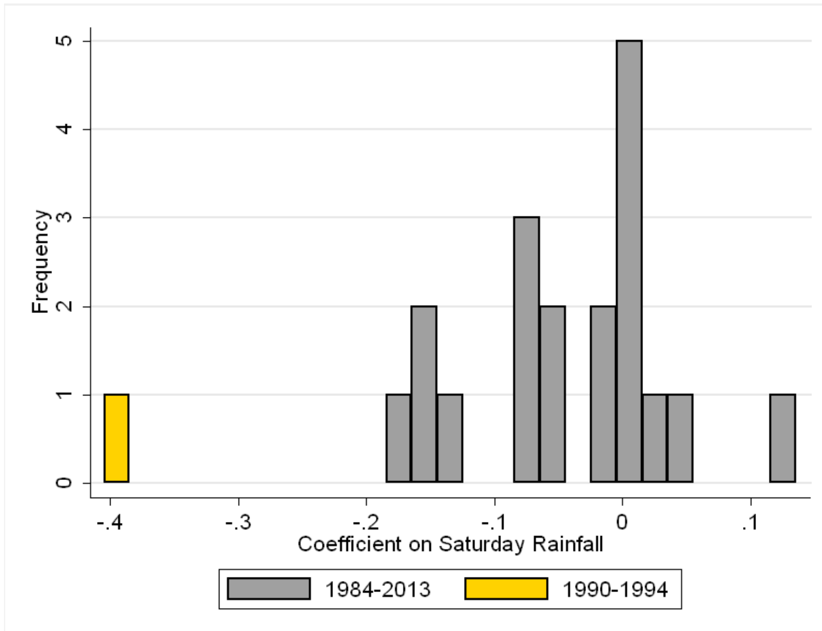
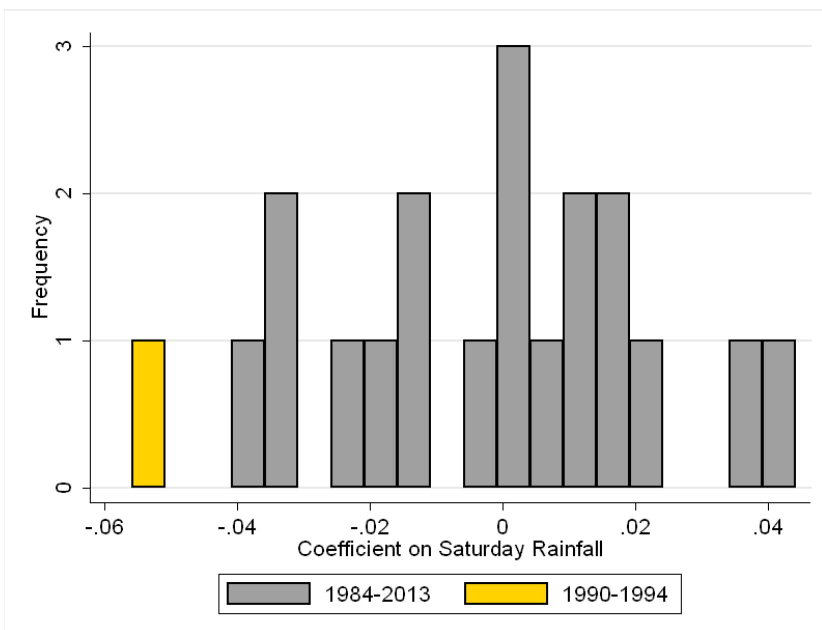


Figure 2.3: Placebo Check: Organized Participation



The figures show the distribution of coefficients on Saturday Rainfall for civilian violence (Figure 2.2) and organized violence (Figure 2.3) when using Saturday rainfall during the 3.5 years of the pre-genocide period (October 1, YEAR to March 31, YEAR+4) from the years 1984 to 2013 in our main specification.

Table 2.5: Exclusion Restriction

Dependent variable:	# Civilian Perpetrators, p.H.						
	Public Holidays		Excl. Pre-Violence	Local Hutu Leaders		Local Tutsi Leaders	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Sat(Rainfall>10mm)	−0.407	−0.393	−0.483	−0.481	−0.466	0.706	0.399
	(0.124)***	(0.122)***	(0.149)***	(0.120)***	(0.123)***	(0.896)	(0.796)
# Sun(Rainfall>10mm)	0.045	0.049	0.053		0.043		0.583
	(0.103)	(0.106)	(0.121)		(0.098)		(0.896)
# Mon(Rainfall>10mm)	0.082	0.074	0.116		0.045		0.087
	(0.110)	(0.111)	(0.131)		(0.110)		(0.387)
# Tue(Rainfall>10mm)	0.031	0.040	0.002		0.037		0.002
	(0.095)	(0.094)	(0.095)		(0.082)		(0.424)
# Wed(Rainfall>10mm)	0.030	0.028	−0.025		−0.002		0.673
	(0.113)	(0.110)	(0.116)		(0.116)		(0.349)*
# Thu(Rainfall>10mm)	−0.006	−0.000	0.126		−0.072		0.814
	(0.134)	(0.135)	(0.151)		(0.127)		(0.681)
# Fri(Rainfall>10mm)	−0.050	0.005	−0.016		0.010		−0.193
	(0.117)	(0.132)	(0.118)		(0.092)		(0.400)
# Pub. Holidays(Rainfall>10mm)	−0.597						
	(2.369)						
# Non-Rel. Holidays(Rainfall>10mm)		−1.439					
		(1.759)					
# Rel. Holidays(Rainfall>10mm)		−5.424					
		(3.871)					
Standard Controls	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes
R^2	0.52	0.52	0.49	0.54	0.55	0.32	0.35
N	1433	1433	1213	1272	1272	161	161

Note: **# of Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to March 1994 (and similarly for all other weekdays). **# Civilian Perpetrators per Hutu (p.H)** is measured in percent. In **regressions 1 and 2** we also control for the number of public holidays (separated into religious and non-religious holidays in regression 2) with rainfall above 10 mm. In **regression 3** we drop villages where violence against Tutsi took place before the genocide. In **regressions 4 and 5** the sample is restricted to villages with pro-genocide parties ruling the commune. In **regressions 6 and 7** the sample is restricted to villages with anti-genocide parties ruling the commune. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 2.6: Channels – Interaction Effects

Dependent variable:	# Civilian Perpetrators, p.H.							
	Local Hutu Leaders				Local Tutsi Leaders			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Sat(Rainfall>10mm)	−0.695	−0.533	−0.378	−0.541	4.292	0.295	−1.463	1.820
	(0.294)**	(0.124)***	(0.140)***	(0.309)*	(2.396)*	(0.801)	(0.870)	(1.470)
# Sat(Rainfall>10mm) x Radio Ownership	0.659			0.414	−12.925			−11.722
	(0.786)			(0.895)	(6.543)*			(6.572)*
# Sat(Rainfall>10mm) x Population Density		0.134		0.125		0.355		0.279
		(0.023)***		(0.032)***		(0.494)		(0.540)
# Sat(Rainfall>10mm) x Tutsi Minority Share			−1.090	−1.505			10.016	10.834
			(1.526)	(1.533)			(6.208)	(6.133)*
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes
Other Weekday Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.55	0.55	0.55	0.55	0.36	0.36	0.36	0.38
N	1272	1272	1272	1272	161	161	161	161

Note: **# of Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to March 1994. **# Civilian Perpetrators per Hutu (p.H)** is measured in percent. **Radio Ownership** is the fraction of Hutu owning a radio. **Tutsi Minority Share** is the fraction of Tutsi divided by the fraction of Hutu. In **regressions 1 to 4** the sample is restricted to villages with pro-genocide Hutu parties ruling the commune. In **regressions 5 to 8** the sample is restricted to villages with anti-genocide pro-Tutsi parties ruling the commune. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. **Other Weekday Controls** include the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm during the period October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 2.7: Main Effects – Linear Specification

Dependent Variable:	# Civilian Perpetrators, p.H.	# Militiamen, p.H.
	(1)	(2)
Average Rainfall Sat	−4.093	−0.569
	(1.635)**	(0.355)
Average Rainfall Sun	0.541	−0.175
	(1.783)	(0.480)
Average Rainfall Mon	0.306	0.507
	(1.543)	(0.432)
Average Rainfall Tue	1.378	0.287
	(1.351)	(0.422)
Average Rainfall Wed	1.703	−0.107
	(1.454)	(0.303)
Average Rainfall Thu	−0.082	−0.057
	(1.113)	(0.298)
Average Rainfall Fri	0.262	−0.001
	(0.983)	(0.235)
Standard Controls	yes	yes
Commune Effects	yes	yes
R^2	0.51	0.36
N	1433	1433

Note: **Average Rainfall Sat** is the average daily Saturday rainfall during the period from October 1990 to March 1994 (and similarly for all other weekdays). **# Civilian Perpetrators per Hutu (p.H)** is measured in percent. **Standard Controls** include average daily rainfall for January 1984 to September 1990. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 2.8: Conley Standard Errors

Dependent Variable:	# Civilian Perpetrators, p.H.			# Militiamen, p.H.		
	25 km	50 km	75 km	25 km	50 km	75 km
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10mm)	−0.391	−0.391	−0.391	−0.053	−0.053	−0.053
	[0.129]***	[0.134]***	[0.132]***	[0.035]	[0.029]*	[0.026]**
Number Sun>10	0.054	0.054	0.054	−0.031	−0.031	−0.031
	[0.097]	[0.088]	[0.078]	[0.032]	[0.034]	[0.032]
Number Mon>10	0.129	0.129	0.129	0.121	0.121	0.121
	[0.096]	[0.108]	[0.108]	[0.036]***	[0.038]***	[0.041]***
Number Tue>10	0.062	0.062	0.062	−0.049	−0.049	−0.049
	[0.104]	[0.119]	[0.116]	[0.029]*	[0.027]*	[0.026]*
Number Wed>10	0.071	0.071	0.071	0.010	0.010	0.010
	[0.105]	[0.09]	[0.087]	[0.028]	[0.024]	[0.021]
Number Thu>10	0.025	0.025	0.025	−0.046	−0.046	−0.046
	[0.128]	[0.14]	[0.155]	[0.036]	[0.036]	[0.035]
Number Fri>10	−0.057	−0.057	−0.057	0.003	0.003	0.003
	[0.11]	[0.103]	[0.09]	[0.027]	[0.03]	[0.03]
Standard Controls	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes
R^2	0.48	0.48	0.48	0.37	0.37	0.37
N	1433	1433	1433	1433	1433	1433

Note: **# of Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to March 1994 (and similarly for all other weekdays). **# Civilian Perpetrators per Hutu (p.H) and # Militiamen per Hutu** are measured in percent. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. There are **142 communes** in the sample. **Standard errors** correcting for spatial correlation within a radius of 25km, 50km and 75km are in square brackets (Conley, 1999). The radius used in each regression is given in the column header. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 2.9: Different Rainfall Thresholds

Dependent variable:	# Militiamen, p.H.				
Rainfall Threshold x:	6 mm	8 mm	9 mm	10 mm	12 mm
	(1)	(2)	(3)	(4)	(5)
# Sat(Rainfall > x mm)	0.020	−0.037	−0.041	−0.057	−0.072
	(0.030)	(0.032)	(0.030)	(0.030)*	(0.034)**
# Sun(Rainfall > x mm)	−0.022	0.005	−0.009	−0.037	−0.014
	(0.027)	(0.034)	(0.033)	(0.031)	(0.038)
# Mon(Rainfall > x mm)	0.021	0.024	0.051	0.100	0.060
	(0.039)	(0.034)	(0.027)*	(0.031)***	(0.031)*
# Tue(Rainfall > x mm)	0.020	0.022	0.011	−0.046	0.022
	(0.029)	(0.031)	(0.026)	(0.030)	(0.037)
# Wed(Rainfall > x mm)	−0.001	−0.027	−0.021	0.007	−0.016
	(0.027)	(0.037)	(0.035)	(0.028)	(0.036)
# Thu(Rainfall > x mm)	−0.002	−0.015	−0.014	−0.064	−0.045
	(0.024)	(0.036)	(0.039)	(0.041)	(0.042)
# Fri(Rainfall > x mm)	0.023	0.049	−0.008	0.006	−0.026
	(0.031)	(0.035)	(0.030)	(0.027)	(0.035)
Standard Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
R^2	0.36	0.36	0.36	0.37	0.36
N	1433	1433	1433	1433	1433

Note: **# of Sat(Rainfall>x mm)** is the number of Saturdays with rainfall above x mm during the period October 1990 to March 1994 (and similarly for all other weekdays). The value of x is given in the column header. **# Militiamen per Hutu (p.H)** is measured in percent. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 2.10: Robustness and Placebo Tests

Dependent variable:	# Militiamen, p.H.						Massgrave in Village	
	Without Mass Graves	Without Kigali	Without Major Cities	Additional Controls	Future Rainfall		Alternative Dep. Var.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Sat(Rainfall>10mm)	−0.044	−0.061	−0.069	−0.052		−0.069	−0.015	−0.013
	(0.028)	(0.031)**	(0.031)**	(0.030)*		(0.030)**	(0.004)***	(0.004)***
# Sun(Rainfall>10mm)	−0.026	−0.037	−0.030	−0.036		−0.034		0.002
	(0.031)	(0.032)	(0.031)	(0.032)		(0.030)		(0.004)
# Mon(Rainfall>10mm)	0.089	0.098	0.097	0.112		0.104		−0.003
	(0.029)***	(0.031)***	(0.031)***	(0.031)***		(0.032)***		(0.004)
# Tue(Rainfall>10mm)	−0.035	−0.048	−0.041	−0.057		−0.038		0.008
	(0.030)	(0.031)	(0.031)	(0.029)*		(0.029)		(0.004)*
# Wed(Rainfall>10mm)	0.012	0.001	0.012	0.002		0.009		0.006
	(0.027)	(0.030)	(0.031)	(0.028)		(0.027)		(0.004)
# Thu(Rainfall>10mm)	−0.062	−0.062	−0.073	−0.052		−0.054		−0.003
	(0.041)	(0.042)	(0.043)*	(0.036)		(0.041)		(0.004)
# Fri(Rainfall>10mm)	0.016	0.004	0.004	0.015		0.017		−0.009
	(0.026)	(0.027)	(0.029)	(0.025)		(0.026)		(0.003)**
# Sat(Rainfall>10mm), 94-98					0.002	0.014		
					(0.028)	(0.028)		
# Sun(Rainfall>10mm), 94-98					0.078	0.075		
					(0.033)**	(0.031)**		
# Mon(Rainfall>10mm), 94-98					−0.063	−0.060		
					(0.032)*	(0.030)**		
# Tue(Rainfall>10mm), 94-98					0.039	0.043		
					(0.039)	(0.036)		
# Wed(Rainfall>10mm), 94-98					−0.022	−0.033		
					(0.037)	(0.033)		
# Thu(Rainfall>10mm), 94-98					−0.021	−0.025		
					(0.035)	(0.035)		
# Fri(Rainfall>10mm), 94-98					0.039	0.050		
					(0.027)	(0.025)**		
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	no	no	no	yes	no	no	no	no
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.37	0.37	0.38	0.38	0.36	0.38	0.16	0.16
N	1367	1422	1358	1433	1433	1433	1432	1432

Note: **# of Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to March 1994 (and similarly for all other weekdays). **# Militiamen per Hutu (p.H)** is measured in percent. In **regression 1** we drop villages with mass graves (indicating high death rates). In **regression 2** we drop villages in the capital Kigali and in **regression 3** we drop all villages in and close to the main cities. In **regression 4** we add additional controls. These are population density, distance to Kigali, Nyanza, the border, the closest main road and the closest main city. In **regressions 5 and 6** we also control for future rainfall. In **regressions 7 and 8** we use a dummy indicating whether a mass grave was found in the village as an alternative dependent variable. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 2.11: Exclusion Restriction

Dependent variable:	# Militiamen, p.H.						
	Public Holidays		Excl. Pre-Violence	Local Hutu Leaders		Local Tutsi Leaders	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Sat(Rainfall>10mm)	−0.052	−0.054	−0.048	−0.092	−0.083	0.386	0.214
	(0.029)*	(0.029)*	(0.032)	(0.031)***	(0.028)***	(0.223)	(0.186)
# Sun(Rainfall>10mm)	−0.030	−0.033	−0.019		−0.034		−0.017
	(0.030)	(0.030)	(0.038)		(0.031)		(0.180)
# Mon(Rainfall>10mm)	0.104	0.104	0.107		0.066		0.419
	(0.031)***	(0.031)***	(0.033)***		(0.028)**		(0.140)***
# Tue(Rainfall>10mm)	−0.032	−0.033	−0.057		−0.035		−0.214
	(0.029)	(0.031)	(0.030)*		(0.031)		(0.120)*
# Wed(Rainfall>10mm)	0.004	0.005	0.005		0.007		−0.101
	(0.030)	(0.029)	(0.027)		(0.029)		(0.166)
# Thu(Rainfall>10mm)	−0.061	−0.060	−0.030		−0.080		0.045
	(0.042)	(0.041)	(0.040)		(0.041)*		(0.173)
# Fri(Rainfall>10mm)	0.018	0.026	0.033		0.010		0.064
	(0.028)	(0.030)	(0.027)		(0.028)		(0.153)
# Pub. Holidays(Rainfall>10mm)	−1.055						
	(0.557)*						
# Non-Rel. Holidays(Rainfall>10mm)		−0.777					
		(0.359)**					
# Rel. Holidays(Rainfall>10mm)		−0.837					
		(0.866)					
Standard Controls	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes
R^2	0.37	0.37	0.36	0.36	0.37	0.33	0.37
N	1433	1433	1213	1272	1272	161	161

Note: **# of Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to March 1994 (and similarly for all other weekdays). **# Militiamen per Hutu (p.H)** is measured in percent. In **regressions 1 and 2** we also control for the number of public holidays (separated into religious and non-religious holidays in regression 2) with rainfall above 10 mm. In **regression 3** we drop villages **where** violence against Tutsi took place before the genocide. In **regressions 4 and 5** the sample is restricted to villages with pro-genocide parties ruling the commune. In **regressions 6 and 7** the sample is restricted to villages with anti-genocide parties ruling the commune. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 2.12: Channels – Interaction Effects

Dependent variable:	# Militiamen, p.H.							
	Local Hutu Leaders				Local Tutsi Leaders			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Sat(Rainfall>10mm)	−0.185	−0.105	−0.045	−0.089	−0.281	0.076	0.011	−0.958
	(0.079)**	(0.029)***	(0.038)	(0.083)	(1.055)	(0.179)	(0.406)	(1.479)
# Sat(Rainfall>10mm) x Radio Ownership	0.292			0.107	1.646			3.081
	(0.216)			(0.265)	(3.661)			(4.640)
# Sat(Rainfall>10mm) x Population Density		0.034		0.040		0.326		0.307
		(0.006)***		(0.008)***		(0.114)**		(0.123)**
# Sat(Rainfall>10mm) x Tutsi Minority Share			−0.447	−0.647			1.093	0.664
			(0.535)	(0.606)			(1.870)	(1.605)
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes
Other Weekday Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.37	0.37	0.37	0.37	0.37	0.39	0.37	0.40
N	1272	1272	1272	1272	161	161	161	161

Note: **# of Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to March 1994. **# Militiamen per Hutu (p.H)** is measured in percent. **Radio Ownership** is the fraction of Hutu owning a radio. **Tutsi Minority Share** is the fraction of Tutsi divided by the fraction of Hutu. In **regressions 1 to 4** the sample is restricted to villages with pro-genocide Hutu parties ruling the commune. In **regressions 5 to 8** the sample is restricted to villages with anti-genocide pro-Tutsi parties ruling the commune. **Standard Controls** include average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to March 1994. **Other Weekday Controls** include the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm during the period October 1990 to March 1994. All regressions are run using weighted least squares (WLS) estimation with population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Mobilizing the Masses for Genocide*

3.1 Introduction

In many genocides and civil wars, ordinary civilians with no military affiliation or military training whatsoever turn into killers. To illustrate this, during the Rwandan Genocide in 1994, Hutu perpetrators killed approximately 800,000 people belonging to the Tutsi minority in only about 100 days (Prunier, 1995). This astounding number of deaths could only be achieved because hundreds of thousands of civilians (about 85 percent of the total number of perpetrators) joined the militia and the army in carrying out the killings. In light of the immense human suffering caused and the often disastrous effects on the social fabric and the economy (Rohner et al., 2013), it is crucial – especially for international policy makers contemplating an

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intervention – to understand the factors that trigger civilian participation.

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

This paper provides the first empirical analysis of how important elite-controlled armed groups might be in inducing civilians to participate in killings. It answers three questions: How much do armed groups affect civilian participation? Do armed-group leaders allocate their men strategically in order to maximize civilian participation? How are civilians mobilized? In answering these questions, I focus on the Rwandan Genocide – to my knowledge the only conflict where data on civilian and armed-group violence is separately available at a local-village level.[2]

The main difficulty in estimating the effects of armed groups on civilian

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

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

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, thus 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.[3]

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

To ensure that the instrument solely picks up armed groups' transport

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

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

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

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

The OLS results indicate a positive relationship between armed-group and civilian violence: a 1 percent increase in the number of militiamen is as-

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[4]The genocide lasting only 100 days is another advantage for the identification strategy as this rules out the presence of time confounding factors. Technically, the genocide lasted about 104 days. However, I will always refer to the "100 days" as the genocide period.

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

sociated with a 0.63 percent increase in civilian participation. In contrast, the instrumental-variables estimates are about twice as large: 1.3. The numbers imply that, on average, one additional militiaman resulted in 7.3 more civilian perpetrators and, under a linearity assumption, in 13 additional deaths. Henceforth, I will use the two expressions armed groups and militiamen interchangeably.

The local average treatment effect I identify has a straightforward and policy-relevant interpretation: I measure the effect of *external* militiamen, those men sent around by the genocide planners and thus affected by transport costs, excluding the effect of the various *local* militiamen such as policemen, already present in the village. Since these external militiamen, around 50,000 men strong, were initially stationed in Kigali and other big cities and only afterwards spread around the entire country, a quick military intervention could potentially have stopped them, not least because they were often badly equipped. Furthermore, the instrumental-variables estimates imply that stopping those 50,000 men would have cut the number of perpetrators by about 83 percent. The number of deaths would probably have gone down even more since external militiamen arguably had higher killing rates than civilians or local militiamen (if I assume that external militiamen killed five times as many people, the number of deaths would have fallen by almost 90 percent).

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

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

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

Finally, in a first extension, I show that the militia's physical presence in each village was necessary to mobilize civilians. This is especially important from a policy perspective because it implies that a genocide planner cannot simply compensate for the absence of his men – for instance, by stirring up radio propaganda. In a second extension, I show that a subset of villages with high levels of cross-ethnic marriage, about 9 percent of the sample, seemed to have opposed the militia: I can link some of these villages to anecdotal evidence of Hutu opposition against the genocide and I present suggestive empirical evidence that the predictions of the force model are fulfilled for those villages.

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

My paper contributes to the literature in several ways. First of all, it adds to the vast conflict literature. Blattman and Miguel (2010) give an excellent review of this research, vehemently calling for well-identified and theoretically grounded studies on the roots of individual participation in violent conflict and the strategic use of violence. This paper starts filling the gap by providing novel evidence on the strong effects of armed groups on civilian participation, the strategic use of armed-group violence and on some recruiting mechanisms. Recent studies on the determinants of conflict and participation in violence consider institutions, government policy, income, ethnic composition and foreign aid (Besley and Persson, 2011; Dell, 2012; Dube and Vargas, 2013; Mitra and Ray, 2014; Novta, 2014; Nunn and Qian, 2014). Several other studies have analyzed the recruitment of civilians. Although very informative, these studies are mostly descriptive, drawing on self-reported survey data (Arjona and Kalyvas, 2008; Humphreys and Weinstein, 2004, 2008; Pugel, 2007; Weinstein, 2007). Furthermore, my paper complements the literature on the Rwandan Genocide (Friedman, 2010; Straus, 2004; Verpoorten, 2012a-c; Verwimp, 2003, 2005, 2006; Yanagizawa-Drott, 2014) by providing novel evidence on the way it was organized and carried out.

Regarding the importance of transport costs, my paper contrasts with recent contributions by Banerjee et al. (2012) and Donaldson (forthcoming) that highlight the positive economic effects of low transport costs. My findings loosely echo those in Nunn and Puga (2012) which shows that high transport costs in Africa – in that case caused by rugged terrain – have positive effects on people's welfare today because they hindered slave traders. My paper is also related to a literature in economics stressing the importance of political elites and their effects on income, institutions and conflict (Jones and Olken, 2005, 2009).

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

My paper also speaks to a wider literature on the psychology of violence. In particular, my results are consistent with Milgram's seminal work (1963, 1967, 1974) that obedience to authority can explain ordinary peoples' willingness to inflict harm on others.

The remainder of the chapter is organized as follows. Section 3.2 provides some background information on the Rwandan Genocide. Section 3.3 presents the data used for the analysis. Sections 3.4 to 3.6 each answer one of the three central questions of the paper. Section 3.7 discusses the external validity of the results and Section 3.8 concludes with possible policy implications.

3.2 Institutional Background

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

A History of Conflict The distinction between the Hutu and the Tutsi in Rwanda is strongly debated. Some argue that the Tutsi (with a pre-genocide

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

population share of around 10 percent, clearly the minority) descended from Hamitic migrants from Egypt or Ethiopia and that the Hutu belong to the Bantu group, who have 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 – by giving them exclusive access to administrative posts and higher education.

When the country gained independence in 1962, the Hutu managed to take over power, establishing a one-party state. The ethnic violence that accompanied the event led several hundreds of thousands of Tutsi to flee the country. In the following decade, periods of relative political stability and peace alternated with episodes of unrest and violence, but the tensions never ceased. In 1975, following a military coup, Habyarimana created the Hutu-dominated National Revolutionary Development Movement (MRND), the only political party legally authorized in the country, and in 1978 he officially became the new president of Rwanda.

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

The 1994 Genocide In January 1994, Romeo Dallaire – the Brigadier General of the UN peacekeeping force for Rwanda – reported to his superiors in New York that an informant had revealed that 1,700 men had been trained in military camps right outside Kigali: *"The 1,700 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 1,000 Tutsi."* (Frontline, 1999). Three months later, the informant was proven right. During the night of the airplane crash, the Presidential Guard went around Kigali, targeting moderate politicians, journalists and civil rights activists, with the moderate prime minister Agathe Uwilingiyimana and her 10 Belgian bodyguards being among the first victims. The new interim government immediately declared a nation-wide curfew and the various army and militiamen under its control, around 45,000 to 50,000 men strong, set up road blocks, killing everyone presumed to be Tutsi. Local leaders enforced the curfew, the necessary infrastructure was already in place, and started organizing the killings in their communities. In the end, about 430,000 civilians participated in the genocide, hacking their Tutsi neighbors to death with machetes.

The militia gangs played an important role in the killings. The two infamous ones were the Interahamwe ("those who work together"), associated with the MRND party, and the Impuzamugambi ("those with a single aim"), associated with the CDR (Coalition for the Defense of the Republic), another even more extremist Hutu party. At the beginning of the 1990s, these groups – their members mostly recruited from the pool of unemployed and disaffected youth in the big cities – started receiving military training from the Presidential Guard and the army. The groups were turned into outright militia, indoctrinated in ethnic hatred and taught how to implement mass murder (Physicians for Human Rights, 1994).

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

Besides army and militia, the central government also used radio propaganda to spur the killings. Radio RTLM, established in June 1993 by Hutu extremists, continuously called on the Hutu to kill the Tutsi. But also Radio Rwanda, although less inflammatory, provided information 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 the operations. Additional pressure came from the possibility of an international intervention, which was highly feared, but never took place. In fact, false reports of an impending Western intervention were sometimes used by the Hutu elite to motivate fellow Hutu to quickly complete the killings (Kuperman, 2000).

3.3 Data

I combine several datasets from different sources to construct the final dataset, which comprises 1,433 Rwandan villages. The different datasets are matched by village names within communes. A commune is an administrative unit above the village. There were 142 communes in total, which were in turn grouped into 11 provinces. Unfortunately, the matching is imperfect, as many villages either have different names in different data sources, or use multiple spellings. It is also not uncommon for two or more villages within a commune to have identical names, which prevents successful matching. However, overall only about 5 percent of the villages do not have a clear match across all sources. Furthermore, as these issues are idiosyncratic, the main implication is likely a lower precision in the estimates than would otherwise have been the case. Villages have an average size of 14 square kilometers, with around 4,900 inhabitants. Table 3.1 reports the summary statistics for the variables.

Participation in Violence The two key measures are participation in armed-group violence and participation in civilian violence. Since no direct measure of participation is available, I use prosecution numbers for crimes committed during the genocide as a proxy (Friedman, 2010; Yanagizawa-Drott, 2014). This data is 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 the severity of the crime, two different categories of criminals are identified.

The legal definition of category 1 includes: 1) planners, organizers, instigators, supervisors of the genocide; 2) leaders at the national, provincial or district level, within political parties, army, religious denominations or militia; 3) the well-known murderer who distinguished himself because of the zeal that characterized him in the killings or the excessive wickedness with which killings were carried out; and 4) people who committed rape or acts of sexual torture. Since these perpetrators mostly belong to the army and the militia or are members of local armed groups such as policemen, I consider this to represent armed-group violence. There were approximately 77,000 prosecution cases in this category.[7]

The legal definition of category 2 includes: 1) authors, co-authors, accomplices of deliberate homicides, or of serious attacks that caused someone's death; 2) the person who – with the intention of killing – caused injuries or committed other serious acts of violence, but without actually causing death; and 3) the person who committed criminal acts or became the accomplice of serious attacks, without the intention of causing death. People accused in this category are not members of any of the organized groups mentioned in category 1 and I therefore label this type of violence civilian violence. Approximately 430,000 prosecution cases were handled in this category. Figures 3.1 and 3.2 show the distribution of violence throughout Rwanda for armed-group and civilian violence.

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

[7]Importantly, this number does not necessarily equal the number of people involved, since the same person might have committed a crime in multiple locations. This is especially true for organized perpetrators who moved around. Since army and militiamen wore distinctive uniforms, they were easily identified later on in the prosecution process, *"A survivor of that massacre identified the party affiliation of the assailants from their distinctive garb, the blue and yellow print boubou of the Interahamwe and the black, yellow, and red neckerchiefs and hats of the Impuzamugambi. He could tell, too, that they came from several regions."* Des Forges (1999, p. 180).

remaining were 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 is positively correlated with several other measures of violence from three different sources.[8] Furthermore, Friedman (2010, p. 21) notes that *"the Gacaca courts have been very thorough in investigating, and reports of those afraid to speak are rare, so this data is likely to be a good proxy for the number of participants in each area."*[9] Nevertheless, to be cautious, in the following analysis, I show that the results are robust to dropping those villages with mass graves or near mass graves (indicating very high death rates).

Another concern is that villages with no reported armed-group violence might have actually received militiamen, but unsuccessful ones. I deal with this concern in Section 3.4.4.

Finally, random measurement error and allegations that these courts were occasionally misused to settle old scores, resulting in false accusations do not pose any major threat because I am instrumenting for armed-group violence. In fact, the instrumental-variables approach will correct for potential attenuation biases arising from random mismeasurement.

Rainfall Data I use the recently released National Oceanic and Atmospheric Administration (NOAA) database of daily rainfall estimates for Africa, which stretches back to 1983, as a source of exogenous weather variation. The NOAA data relies on a combination of actual weather station gauge measures

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

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

as well as satellite information on the density of cloud cover to derive rainfall estimates at 0.1 degree ($\sim$ 11 km at the equator) latitude longitude intervals. Considering the small size of Rwanda, this high spatial resolution data, to my knowledge the only one available, is crucial to obtain reasonable rainfall variation.[10] Furthermore, the high temporal resolution, i.e. daily estimates, allows me to confine variation in rainfall in the instrument to the exact period of the genocide. To construct the instrument, I compute the amount of 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, I obtain considerable variation in rainfall along each buffer. Furthermore, Rwanda's very hilly terrain ensures sufficient local variation in rainfall. The overall rainfall in each buffer is obtained through a weighted average of the grids, where the weights are given by the relative areas covered by each grid (Figure 3.3 maps the variation in the difference between rainfall along each buffer during the genocide in 1994 and its long-term average (years 1984-1993) for each village). In a similar fashion, using a village boundary map, I also compute rainfall in each village. Figure 3.4 illustrates how the instrument is constructed.

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

[10] About 220 rainfall grids cover the whole of Rwanda. To compare, with 0.5 degree grid cell data, only about 9 grids would have covered Rwanda.

Africover provides maps with the location of major roads and cities derived from satellite imagery. These satellites analyze light and other reflected materials, and any emitted radiation from the surface of the earth. Since simple dirt roads have very different radiation signatures than tarred roads or gravel roads, this allows to objectively measure road quality.[11]

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

Additional Data The remaining data is drawn from Genodynamics and the IPUMS International census data base. This data includes population, ethnicity and radio and cement floor ownership from 1991.[12] Except for population, all these variables are only available at the commune level. Ethnicity is defined as the fraction of people that are Hutu or Tutsi, respectively. About 10 percent of the population are Tutsi. Importantly, the Tutsi minority is spread out across the entire country. I calculate the Tutsi minority share used in the analysis as the fraction of Tutsi normalized by the fraction of Hutu.

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

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

[12]This data is only available for 1991. Mobility, however, was extremely limited because of governmental restrictions and land markets were also strongly controlled (Andre and Platteau, 1998; Prunier, 1995).

3.4 How Much Do Armed Groups Affect Civilian Violence?

3.4.1 OLS Specification

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

$$log\,(K_{ip}) = \alpha^O + \beta^O log\,(M_{ip}) + \mathbf{X}_{ip}\pi^O + \gamma_p + \epsilon_{ip}, \tag{3.1}$$

where K_{ip} is the number of Hutu prosecuted in category 2, my proxy for civilian violence, and M_{ip} the number of Hutu prosecuted in category 1, my proxy for armed-group violence in village i in province p. $\mathbf{X}_{ip}$ is a vector of village-specific control variables, which I will explain below, γ_p are province fixed effects and ϵ_{ip} is the error term. I allow error terms to be correlated across villages within a 150 kilometer radius (Conley, 1999).[13] Armed groups were sent around the entire country, so I expect errors to be correlated over long distances. In particular, the cutoff of 150 kilometers coincides with the maximum distance to Kigali – the center of the country and the genocidal plan – in my sample of villages. The prosecution numbers are heavily skewed to the right and I therefore logaritmize them.[14] The coefficient β^O thus captures the percentage increase in civilian participation associated with an increase of one percent in the number of militiamen.

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

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

3.4.2 OLS Results

The number of militiamen in each village is positively correlated with civilian participation at the 99 percent confidence level with a point estimate of 0.688 (standard error 0.077, regression 1 in Table 3.2). And this relationship holds up when controlling for a number of other factors that potentially affect civilian participation (regression 2). I call them "additional controls".

These include distance to the border, distance to major cities, distance to Kigali, distance to Nyanza as well as village population, population density and the number of days the RPF was present in each village. To illustrate this, 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 cities, in particular the capital Kigali, is likely to be correlated with urbanization and public goods provision (economic activity). Nyanza was the old Tutsi Kingdom capital and villages further away from it still exhibit lower Tutsi shares, on average. Population density eventually captures social pressure as well as food pressure, both said to be important reasons for the genocide (Boudreaux 2009; Diamond, 2005; Verpoorten, 2012b).[15] Finally, RPF presence in a village, as they moved through Rwanda, was likely to have affected civilian participation.

Nevertheless, even after including a large set of controls, the OLS estimates might still be biased. For instance, I lack a good control for leader quality in the villages and it might be that in villages with peaceful leaders, civilians are less likely to commit violent acts. If army and militia were strategically sent into those villages to spur the killings, I would underestimate the true effect. Measurement error would also contribute to a downward bias. Alternatively, it might be that there are some unobserved village-specific reasons for tensions that promote both civilian and armed-group violence,

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

thus biasing the estimates upwards. Furthermore, the OLS estimates are less informative from a policy perspective because they also pick up the effects of local armed groups which would have been difficult to target with an international intervention.

3.4.3 Instrumental-Variables Strategy

To overcome the issues raised above, I use an instrument for armed-group violence. The instrument is distance to the closest main road interacted with the total amount of rain falling during the period of the genocide along the dirt tracks between main road and village (technically, along a 500-meter buffer around the line between village centroid and the closest point on the main road).[16]

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

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

[16]Results are robust to varying the size of the buffer, i.e. using 250-meter or 750-meter buffers. The genocide started on April 6 1994 and ended on July 18 1994. To account for rainfall before the starting date, I add an additional day to construct the instrument.

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 to Butare city and others from Nyabisindu were ordered to Gatagara in Butare prefecture. They sent militia from other locations to participate in massacres at Kaduha church in Gikongoro, [and so on]. A survivor of that massacre identified the party affiliation of the assailants from their distinctive garb, (...). He could tell, too, that they came from several regions."*

Most of these movements were made by motorized vehicles, for instance Hatzfeld (2005) cites civilian killers describing how they moved on foot while the militia used cars. Unfortunately, I do not have any data on the exact locations of the Hutu army and militia. However, anecdotal evidence suggests that they were stationed around the cities (Frontline, 1999; Waller, 2002), 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 that point, likely to have used the main road system which is generally paved. I assume that the costs of traveling along these main roads are negligible relative to the costs one has to incur when leaving those main roads, since local roads are usually non-paved dirt roads and heavy rains quickly make them very difficult to penetrate with motorized vehicles.

Rain turns dirt roads into slippery mud, usually requiring expensive four-wheel drives and forcing drivers to slow down; experts recommend about half the usual speed on wet dirt roads (ASIRT, 2005). Since the genocide planners were under time pressure, time was costly. Furthermore, water can collect in potholes and create deep puddles or broken trees might block the road, requiring the driver to stop and clear the road or measure water depth, thus increasing travel time and costs even further.[17] For example, a recent survey in Uganda, a direct neighbor to Rwanda in the north, shows that during the rainy seasons public transport prices almost double (East African Business Week, 2013). Thus, the instrument should capture transport costs sufficiently well and my model, outlined in Section 3.5.1, suggests that higher transport costs should translate into fewer militiamen.

Exclusion Restriction Once more, the instrumental-variables strategy makes the counterfactual assumption that, absent armed-group violence, distance to the main road interacted with rainfall along the way between village and main road during the period of the genocide has no effect on civilian violence. This is unlikely to be true without further precautions. The instrument, composed of distance to the main road and a rainfall measure, is probably correlated with factors such as education, health, access to markets, rain-fed production and, therefore, with income. These characteristics are, in turn, likely to affect civilian participation, as reasons for joining in with 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, I control for distance to the main road interacted with long-term average rainfall (years 1984 to 1993) during the 100 calendar days of the genocide period along the way between village and main road as well as all main effects.[18] Therefore, I only exploit seasonal weather variation in the year of the genocide. Furthermore, I control for rainfall in the village during the 100 genocide days in 1994 and its long-term average. These variables take into account the possibility that rainfall in the village directly

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

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

affects civilian participation, for example through malaria prevalence or civilians' transport costs within the village. Finally, I always control for village population. In the following analysis, I will call these "standard controls". To control for broad geographic characteristics, I include 11 province fixed effects. Identification then 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.

The genocide partially overlaps with the rainy season which potentially affects (expected) rural income. I doubt this to lead to a serious bias because looting was mostly directed towards building materials, household assets and livestock (Hatzfeld, 2005), thus high rainfall during the growing season should not have affected the perpetrators. Moreover, several country-wide indicators for Rwanda show that agricultural production completely collapsed, suggesting that rainfall should not have affected the plot owners either. Nevertheless, to be cautious and to ensure that the instrument is not picking up any income effects but solely transport costs, I also include in the set of controls the total amount of rainfall in the village during the 1994 growing season and its long-term average as well as the interaction of the two with the difference between the maximum distance to the main road in the sample and the actual distance from the main road to each village.[19] The last interaction term takes into account the possible heterogeneous effect of rainfall because of market accessibility. The intuition here is that high agricultural output (and hence rainfall) is more valuable the shorter the distance to the main road. I call these "growing season controls".

At this point, I still need to argue that civilians were not directly affected by the instrument, i.e. by traveling themselves. Starting with anecdotal evidence, several reports and accounts of the genocide indeed support the claim that civilian violence was a very local affair. Hatzfeld (2005) calls it a *Neigh-*

[19]The first growing season, overlapping with the genocide period, lasts from mid-February to mid-May. The second growing season, used together with the first one for calculating long-term averages, lasts from the end of September to the end of November.

borhood Genocide because only neighbors and co-workers were able to identify Tutsi, as they are very similar to the Hutu, speaking the same language and also looking similar (Hatzfeld, 2005).

Besides that, few people in Rwanda, let alone civilians, owned a car or a truck (less than 1 percent according to the 1992 DHS Survey) and the possibilities of moving between villages in motor vehicles, certainly the most affected by rain-slickend roads, were therefore limited for civilians. In addition, moving around along or close to the main roads was risky for ordinary citizens, as roadblocks were set up all over the country and being Hutu did not always ensure safety.[20] On a more general account, Horowitz (2001, p. 526) notes *"that [civilian] crowds generally stay close to home, attack in locales where they have the tactical advantage, and retreat or relocate the attack when they encounter unexpected resistance."* Furthermore, there were no reasons for Hutu to travel because social life completely stopped. As one civilian killer puts it, *"During the killings, we had not one wedding, not one baptism, not one soccer match, not one religious service like Easter."* (Hatzfeld, 2005, pp. 94-95). Another one continues (p. 133), *"During the killings there was no more school, no more leisure activities, no more ballgames and the like."* Besides this anecdotal evidence, in Section 3.4.4, I also present three indirect tests which all strongly support the identification assumption.

Finally, as a first robustness check, adding the additional controls, introduced in Section 3.4.2, should not alter the results.

[20] Amnesty International (1994, p. 6) reports that *"Each individual passing through these roadblocks had to produce an identity card which indicates the ethnic origin of its bearer. Being identified as or mistaken for a Tutsi meant immediate and summary execution."* Similarly, Prunier (1995, p. 249) writes 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."* Des Forges (1999, p. 210) continues, *"During the genocide some persons who were legally Hutu were killed as Tutsi because they looked Tutsi. According to one witness, Hutu relatives of Col. Tharcisse Renzaho, the prefect of the city of Kigali, were killed at a barrier after having been mistaken for Tutsi."* Moreover, Tutsi tended to avoid the roads but rather hide in the bushes (Hatzfeld, 2005).

IV Specification I run the following first-stage regression

$$log\,(M_{ip}) = \alpha + \beta\,[log(Dist_{ip}) \times log\,(Rain_{ip})] + \mathbf{X}_{ip}\pi + \gamma_p + \epsilon_{ip}, \qquad (3.2)$$

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

The second-stage equation becomes

$$log\,(K_{ip}) = \alpha' + \beta' log\left(\widehat{M_{ip}}\right) + \mathbf{X}_{ip}\pi' + \gamma_p + \epsilon_{ip}, \qquad (3.3)$$

where $log(\widehat{M_{ip}})$ is instrumented as per (3.2). The coefficient β' captures the causal effect of armed-group violence on civilian violence for those armed groups affected by transport costs.

3.4.4 Instrumental-Variables Results

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

First Stage and Reduced Form The first-stage relationship between transport costs and armed-group violence is strongly negative at the 99 percent confidence level (regression 1 in Panel A in Table 3.3), and this relationship holds, or becomes somewhat stronger, when including growing season controls (regression 2) and additional controls (regression 3). The F-statistic on the excluded instrument in my preferred specification (regression 3) reaches 19.54.

Regarding magnitude, the point estimate of -0.509 (standard error 0.115) suggests that a village with an average distance to the main road receives 16 fewer militiamen, about 30 percent of the mean (51.76), following a one standard-deviation increase in rainfall between village and main road. I provide a theoretical foundation for this result in Section 3.5.

Importantly, higher transport costs are also associated with fewer civilian perpetrators in the reduced form (regressions 4 to 6 in Panel A in Table 3.3), with a point estimate of -0.661 (standard error 0.141) in my preferred specification (regression 6). The results are robust across all three specifications and significant throughout at the 99 percent confidence level. This is a first indication that villages that were harder or more costly to reach had fewer civilian killers.

Main Effects The instrumental-variables point estimates are about twice as large as the analogous OLS estimates: a 1 percent increase in the number of militiamen leads to a 1.299 percent (standard error 0.258) increase in the number of civilian perpetrators (regression 6 in Panel B in Table 3.3, with all controls; the OLS result with the same set of controls is reported in column 3). The results are once more very robust across all three specifications and significant throughout at the 99 percent confidence level.[21] The size of the estimated impact of armed-group violence on civilian violence is huge: when I focus on my preferred specification, these numbers imply that

[21]Note that this positive relationship is not trivial since armed groups and civilians might have been substitutes in the killing process, which would imply a negative relationship. Furthermore, I cannot replicate this result when using only distance to the main road or only rainfall between the village and the main road or both but uninteracted as instruments, providing further evidence that transport costs are at work.

one additional external militiaman resulted in (430,000 $\div$ 77,000) $\times$ 1.299 = 7.3 more civilian perpetrators or 13 additional deaths.[22] 430,000 is the total number of prosecuted civilians and 77,000 the total number of militia and army men, respectively. Put differently, the average number of external militiamen, around 33,[23] arriving at a village increases the number of civilian participants by about 240 which is around 5 percent of the average population in the village.

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 664,000 Tutsi deaths, which is about 83 percent of the total number of deaths (again under a linearity assumption that the number of perpetrators is proportional to the number of estimated victims, and equally so for civilians and militiamen). If I reasonably assume that external militia and army men had a higher killing rate than ordinary civilians or local militiamen, this number will be larger, since the direct effects of an additional external militiaman increase.

The large instrumental-variables coefficients, compared to the analogous OLS estimates, suggest that militia and army were strategically sent into those villages with originally little civilian participation.[24] Additionally, the instrumental-variables strategy might be correcting for measurement error in the endogenous variable. Furthermore, I measure the local average treatment effect (LATE) induced by changes in armed-group violence due to the instrument. External army and militiamen, for instance well-trained and highly mo-

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

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

[24]If there were an unobserved factor S^{un} that would lower civilian participation, i.e. $\beta^{S^{un}} < 0$, then the genocide planners should send more militiamen into areas where S^{un} is high, thus $cov(M, S^{un}) > 0$. Combining the two conditions gives a downward bias.

tivated national troops, from further away, thus affected by transport costs, might have been particularly ruthless and ambitious, resulting in a high local average treatment effect. In particular, when compared to the average treatment effect (ATE) which also includes the effect of local and maybe less effective or well-trained armed groups, for instance local policemen. However, since a military intervention would have focused on stopping precisely those external army and militiamen, these were initially concentrated around the big cities, the local average treatment effect I identify is more informative than the average treatment effect, certainly from a policy perspective.

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

Finally, from a theoretical perspective, transport costs should matter less for villages that the militia urgently wants to reach, i.e. in which it has large effects on civilian participation. I show this in Section 3.5. Thus if anything, the estimate above would give me a lower bound.

Exclusion Restriction Tests Traveling civilians, potentially affected by the instrument, who spread information about the genocide or started killing outside of their home village are unlikely to pose a threat to the exclusion restriction. At the beginning of the genocide, a strict nation-wide curfew was implemented, which drastically limited the travel opportunities for civilians.[25] Barriers, erected on roads and at the entrances to towns, enforced these regulations (Kirschke, 1996; Physicians for Human Rights, 1994). Des Forges (1999, p. 162) writes that *"Tutsi as well as Hutu cooperated with these measures at the start, hoping they would ensure their security."*

Reassuringly, the instrumental-variables estimates are very similar to the baseline results and equally statistically significant when I restrict the variation in rainfall in the instrument to the first five days, the first week or the first two weeks of the genocide, while controlling for rainfall along the way between village and main road for the remaining days and its interaction with distance to the main road (regressions 1 to 3 in Table 3.4).[26] The point estimate of the specification using only the first five days is 1.332 (standard error 0.608), almost identical to the ones from the baseline results, thus supporting the identification assumption. Importantly, this result does not imply that only the first couple of days are sufficient to identify the main effect. In fact, the first-stage point estimates drop significantly as compared to the baseline first-stage result, and the main effect thus only remains 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 3.4). First-stage and reduced-form point estimates moving together proportionally provide another indication that armed groups alone are driving these results.

[25] 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 was already in place and had been extensively used. In 1990, stringent limitations on the right to freedom of movement were introduced under the State of Emergency.

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

Furthermore, because of tight population controls, already before the genocide in 1994, it was practically impossible for civilians to get permission to leave their commune. And indeed the results are similar, if anything larger, when I restrict the sample to those communes with no main road passing through (regression 4 in Table 3.4), once more supporting the identification strategy. Moreover, since traveling civilians 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 Section 3.6.3 below I show that this is not the case.

Note that Tutsi civilians escaping the violence are unlikely to bias the results, since they avoided the main roads, and instead rather hid in the bushes (Hatzfeld, 2005). Furthermore, their decision to escape, facing death, was unlikely to be the result of a rational transport cost calculation, as was the case for the militia (I show this in Section 3.5). Thus, their movements should not be correlated with the instrument. For the same reason, those hundreds of thousands of Hutu fleeing the country in fear of the RPF's revenge towards the end of the genocide are also unlikely to bias the results. And reassuringly, using detailed migration data from a Rwandan household survey in 2000, I find that individuals who lived in villages with low transport costs were not more or less likely to move, either within Rwanda or abroad, during the genocide: the point estimate on the instrument is close to zero and highly insignificant (0.008, standard error 0.015, result not shown).[27]

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

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

from a mass grave location, reducing the sample size by about 10 percent (regression 2). Furthermore, I can also use the presence of a mass grave directly as a dependent variable. Consistently, regression 10 shows that villages with high transport costs are less likely to have a mass grave site altogether. The point estimate of -0.035 (standard error 0.012) suggests that a village with an average distance to the main road is 37 percent less likely to have a mass grave site, given a one standard-deviation increase in rainfall between village and main road.[28]

Potential underreporting of unsuccessful militiamen, something that would certainly bias the OLS estimates upwards, is unlikely to push up the IV estimates as well. To see this, I add the average number of militiamen per village in the sample to those villages with zero militiamen reported and rerun the baseline regression. The point estimate of 1.489 (standard error 0.305, regression 3) is very similar to the baseline results and if anything higher. This is unsurprising, since the reduced form is unaffected by this change and the first-stage coefficient decreases in absolute terms.[29] As a result the instrumental-variables estimates should increase. Besides, it seems puzzling that a genocide planner who, as we will see, wants to maximize civilian participation, would send ineffective militiamen specifically to villages that are hard to reach: not only are the (wasted) costs of getting there higher but the monitoring costs will certainly be higher as well. Finally, I am not aware of any anecdotal evidence supporting the notion of lazy or unsuccessful militiamen. If anything, the contrary seems to be true: in Hatzfeld (2005, p. 10), a civilian killer reports that the militiamen were the *"young hotheads"* who ragged the others on the killing job. Another 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*

[28]Furthermore, villages with high transport costs are also more likely to be further away from a mass grave location (results not shown).

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

papyrus!"

One might also worry that rainfall between each village and the main 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 main road decrease the transport costs to markets. In practice, this is once more unlikely to matter. As mentioned earlier, agricultural production and market activity completely collapsed. And indeed, the results are robust to controlling for rainfall along the way between village and main road during the 1994 harvest season and its interaction with distance to the main road (regression 4).

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

To check whether armed groups might have taken a direct route to each village, possibly affected by rainfall along the way, I also control for rainfall along the way between each village and the closest main city during the genocide and its interaction with distance to the main city. As noted, I do not know exactly where armed groups were stationed, but the vast majority are likely to have started out from the main cities. However, the two additional controls are small and insignificant in the first stage (results not shown) and they do not affect the main result (regression 6).

Replacing 11 province fixed effects by 142 commune effects also does not matter (regression 7). Since the rainfall data only comes at a coarse resolution, at least relative to the large number of communes, this significantly reduces the variation in the instrument. Nevertheless, the instrumental-variables point estimate remains similar and equally significant.

One might also be worried that the UN troops which were stationed in Kigali, although few, were affected by transport costs, thus biasing the estimates. But again, the results are robust to dropping villages in Kigali city (regression 8). Furthermore, the results are robust to dropping all the main cities and villages close to them (regression 9).

To test for outliers, I also dropped one province at a time and the resulting estimates range from 1.153 to 1.527 and are significantly different from zero at the 99 percent confidence level in all cases (results not shown).

Finally, as a placebo check, I rerun both first-stage and reduced-form regressions using rainfall during the 100 calendar days of the genocide from the years 1983 until 2014 in the instrument. As expected, the two distributions of the resulting 32 coefficients are both somewhat centered around 0 and, reassuringly, the coefficient on the instrument with rainfall from 1994, the year of the genocide, is an outlier to the left in both cases (results shown in Figures AO.1 and AO.2 in the online appendix). In the reduced-form regressions only 1 of the 32 coefficients is smaller (and larger in absolute value) than the actual coefficient from 1994 and in the first-stage regressions only 2 of the 32 coefficients are smaller (and larger in absolute value) than the actual coefficient from 1994. Furthermore, the difference between the two actual 1994 coefficients and the few coefficients lying to the left is very small.

3.5 Are Armed Groups Used Strategically?

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

3.5.1 Model

Consider a central genocide planner who wants to maximize civilian participation in the killings but faces a fixed budget B, that is only owns a limited number of trucks and buses to drive his external militiamen M_e to each village i to promote the killings (there are N villages in total).[30] There is anecdotal evidence that the central genocide planners wanted every Hutu to join in with the killings. *"If all were guilty, none could be absolved later should the political winds turn."* (Fujii, 2009, p. 174).

Each village is inhabited by a Hutu population of size 1, for simplicity, and a Tutsi population of size T. In each village, there might already be local armed groups such as policemen M_l or RPF Tutsi rebels R. Anecdotal evidence suggests that there are fewer local militiamen in villages with a large Tutsi minority or Tutsi rebels, i.e. $\partial M_l / \partial S < 0$ with $S = T, R$.[31] I call T and R the strategic factors S. Together with the local armed groups, the external militiamen turn ordinary civilians into civilian killers at a decreasing rate by teaching and organizing them.[32] To make progress, I let the militia's technology to turn civilians C into civilian killers K take the following form

$$K = A(M_e + M_l)^{\alpha} C, \tag{3.4}$$

with $A > 0$, $0 < \alpha < 1$ and where C equals the number of Hutu participating in the training. For simplicity, I assume that all Hutu villagers join in with the training, thus $C = 1$.[33]

[30] Since the genocide planners were under time pressure, B might also capture their limited amount of time.

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

[32] Anecdotal evidence that armed groups would usually call all Hutu civilians together in one location, and then instruct and organize them, implies decreasing effects of the militia (Gourevitch, 1998; Hatzfeld, 2005). I will provide empirical evidence for this in Section 3.6.

[33] This assumption does not seem too far fetched in particular since even women and children took part in the killings. As expressed by one UNAMIR officer, *"I had seen war before, but I had never seen a woman carrying a baby on her back kill another woman with*

The planner faces the following problem (assuming perfect information about the Tutsi minority share, transport costs and local militiamen)[34]

$$\begin{aligned} \max_{\{M_{ei}\}} \quad & U = \sum_{i=1}^{N} A\left(M_{ei} + M_{li}(S_i)\right)^{\alpha} \\ \text{s.t.} \quad & B = \sum_{i=1}^{N} M_{ei} r_i, \end{aligned} \tag{3.5}$$

where r_i are the exogenous transport costs for reaching each village. Solving this maximization problem for the number of external militiamen M_e gives the following predictions

Prediction S 1. *The number of total militiamen $M_e + M_l = M$ is strictly decreasing in the transport cost r: $\partial M/\partial r < 0$.*

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

Prediction S 3.

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

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

The proofs are presented at the end of the chapter. Intuitively, high transport costs lead to fewer militiamen because these can be used more efficiently in low-cost villages (Prediction S1, local militiamen do not respond to transport cost changes). Furthermore, because external militiamen have larger

a baby on her back.” (Des Forges, 1999, p. 197).

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

marginal effects on civilian participation when the Tutsi minority is large or Tutsi rebels are present (I show this empirically in Section 3.6), the central planner will prefer to send more militiamen into those villages with many Tutsi or Tutsi rebels (Prediction S3 (i)) and thus, transport costs should matter less for these villages (Prediction S2). Note that I cannot directly test Prediction S3 (i), since I do not separately observe local and external militiamen in the data. However, I will be able to determine which of the two effects ($\partial M_e/\partial S > 0$ or $\partial M_l/\partial S < 0$) dominates for each strategic factor (Prediction S3 (ii)).

3.5.2 Results

The results suggest that armed groups were strategically allocated among villages: both Predictions S1 and S2 are confirmed in the data. Furthermore, the influx of external militiamen compensated for the fewer local militiamen in villages with large Tutsi minority shares (Prediction S3). All results are reported in Table 3.6.

Prediction S1: Transport Costs To test Prediction S1 that an increase in transport costs reduces the number of militiamen, I rerun the first-stage regression but drop villages with high rainfall between village and main road, above the 90th percentile. This is to show that the negative relationship from the first stage does not simply reflect that some villages are impossible to reach, but rather that driving to a low-transport-cost village instead of a high-transport-cost village was a strategic choice.[35] The point estimate of -0.632 (standard error 0.177) is even slightly larger than the baseline result in Table 3.3 and still strongly significant at the 99 percent confidence level (regression 1). This provides the theoretical foundation for my instrumental-

[35]On average villages are around seven kilometers away from the main road. This relatively short distance – possibly a walking distance – also suggests that strategic cost considerations were at play rather than villages being impossible to reach.

variables strategy.

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

Prediction S3: Strategic Factors Finally, villages with a larger Tutsi minority share received more militiamen. The point estimates are robust and highly significant at the 99 percent level across all specifications, ranging between 2.097 (standard error 0.572, regression 4) and 2.178 (standard error 0.555, regression 2); they suggest that a one standard-deviation increase in the Tutsi minority share increases the number of militiamen by about 20 percent.[36] Thus, the influx of external militiamen compensated for the fewer local militiamen, once more confirming the strategic importance of these villages. The opposite is true for villages with Tutsi rebels: coefficients are negative throughout and again highly significant. Thus, in this case, the initial lack of local militiamen was not compensated for by an influx of external men. As mentioned above, the Tutsi rebels quickly defeated the Hutu army in those areas, rendering further Hutu efforts useless.

[36]Note that both rainfall along the way between village and main road and distance to the main road in the instrument are demeaned.

Since Tutsi were on average richer than Hutu, these effects might be picking up wealth effects. However, all the above results are robust when controlling for the fraction of people with a cement floor (my best proxy for wealth (Yanagizawa-Drott, 2014)) and its interaction with transport costs, suggesting that wealth is not driving these effects (regression 4).

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

3.6 How Do Armed Groups Mobilize Civilians?

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

First, armed-group members might have acted as role models, ordering civilians to participate, informing about the genocide, teaching civilians and organizing them. Hatzfeld (2005) reports that often militiamen took a lead in the killings and showed civilians how best to kill. One of the civilian killers he interviews highlights this point (Hatzfeld, 2005, p. 36):

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

Second, militiamen might have physically forced civilians to join in with the killings. Anecdotal evidence of survivors and perpetrators confirms that civilian villagers sometimes fought off external aggressors. Des Forges (1999, p. 156) writes: *"Both in Kigali and elsewhere, Hutu [occasionally] cooperated with Tutsi in fighting off militia attacks (...)."*[37]

As I do not have any data to directly distinguish between those two cases, I model the two scenarios in the following section and will then test their theoretical implications.[38]

3.6.1 Role Model versus Force Model

Set Up Imagine that the N villages, introduced in Section 3.5.1, can be of two types $j \in \{o, w\}$: those that do not oppose the militia (w) and those that oppose the militia (o).[39] As noted, the militia turns ordinary civilians into killers (I now assume a very general functional form)

$$K^j = K(M^j) \cdot C^j, \tag{3.6}$$

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

Non-opposing Villages As mentioned in Section 3.5.1, in non-opposing villages all the Hutu join in with the training, thus $C^w = 1$. The (expected)

[37]The militia might also have promised civilians security from the Tutsi rebels in return for their participation, instead of only teaching them how to kill and organizing or supervising the killings. In Section 3.6.3 I will argue that this is unlikely to matter. Furthermore, since Tutsi never held the majority in a village, it is also unlikely that the militia simply changed the balance of power. Moreover, there is no anecdotal evidence of coordinated defense operations by Rwandan Tutsi.

[38]Note that in the preceding section, I anticipated the result that the militiamen acted as role models. For completeness, I discuss the maximization problem of the genocide planners if faced with opposing villages at the end of the chapter.

[39]Opposing is defined in an active way, i.e. fighting the militia. One might, however, also imagine that people were coerced into participating, thus *innerly* opposing the militia. That is, they were not necessarily welcoming the militia but still too afraid to actively resist them. This phenomenon might best be described in Bertholt Brecht's parable *Actions against violence* (Brecht, 2001/1930). Unfortunately, I lack data to address this possibility directly.

number of civilian killers is therefore (remember that the number of local militiamen depends on the strategic factors S)

$$E(K^w) = K(M_e + M_l(S)). \tag{3.7}$$

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

$$p = I(\gamma M, P), \tag{3.8}$$

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

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

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

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

($I_{MM} < 0$ as $\gamma M > P$). There is anecdotal evidence that this is the case for military contexts (Dupey, 1987; Hirshleifer, 1989).[40]

The third assumption states that when the number of militiamen is relatively small (large), increasing the opponents' strength decreases (increases) the marginal effects of an additional militiaman. Put differently, when the militiamen are anyway struggling to win, increasing the opponents' strength reduces the effects of an additional man even further ($I_{MP} < 0$ as $\gamma M < P$). On the other hand, if the militia is sufficiently strong, an increase in the opponents' strength will increase the effects of an additional militiaman ($I_{MP} > 0$ as $\gamma M > P$).[41]

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

$$E(K^o) = I(\gamma M_e, 1 + T + R) \cdot K(M_e). \tag{3.9}$$

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

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

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

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

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

The proofs are presented at the end of the chapter. Since the first stage, derived in the preceding sections, provides exogenous variation in the number of external militiamen, all predictions are stated with respect to M_e.

Prediction C1 says that in non-opposing villages, one additional external militiaman has a larger effect on civilian participation when the Tutsi minority is large or Tutsi rebels are present. Intuitively, in non-opposing villages with a large Tutsi minority or Tutsi rebels, there are fewer local militiamen thus, given the concavity of the production function, an additional external man has a larger effect. On the other hand, in opposing villages, as long as the number of militiamen is sufficiently small, a large Tutsi minority or Tutsi rebels decrease the militia's effect on civilian participation because 1) the Tutsi will join the fight against the militia and will thus reduce the militia's chances of winning and 2) the militiamen are anyway struggling to win.

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

3.6.2 Results

Prediction C1 implies that the interaction effect of the number of militiamen with the two strategic factors should be positive if the militiamen acted as role models and negative if the militiamen had to use force against opposing villagers. Prediction C2 implies that the militia should exhibit decreasing marginal effects under the role model channel and increasing effects if force was necessary. All results for Prediction C1 are reported in Table 3.7.

Prediction C1: Interaction Effects The first test between the force or role model channel supports the fact that armed groups acted as role models: the interaction effect of the number of militiamen with a dummy variable indicating whether Tutsi rebels were controlling a village at the beginning of the genocide is positive and significant at the 95 percent confidence level (2.178, standard error 1.067, regression 1). Furthermore, the coefficient on the interaction with the other strategic factor, the Tutsi minority share, is equally positive (5.161, standard error 14.210, regression 1). However, since variation in the Tutsi minority share only comes at coarse commune level, the effect is insignificant. Note that, in order to establish causality, I instrument each interaction term with the interaction between the instrument and the variable capturing the heterogeneous effects. Furthermore, I always include all double interactions.

In regression 2, I replace the continuous Tutsi minority share variable by a dummy variable taking on the value of 1 if the Tutsi minority share lies above the median. Once more, the point estimate is similarly positive but, in addition, also significant at the 95 percent level (1.125, standard error 0.564). Unfortunately, because of strong multicollinearity, this specification does not allow me to control for the double interaction of the Tutsi share dummy with distance to the main road. To account for the potential omitted variable bias that this creates, I interact the Tutsi share dummy with the other controls not involving distance to the main road and include them in the regression.[42]

Since Tutsi were on average richer than Hutu, these effects might be picking up wealth effects. However, all the above results are robust when controlling for the fraction of people with a cement floor (my best proxy

[42] Since the force model predicts that I should observe negative interaction effects, especially for low levels of militiamen, i.e. $\gamma M \leq P$, I also restrict the sample to those villages where militiamen make up less than 4 percent or less than 2 percent of the population. Recalling the model, this implies that one militiaman is equivalent to 25 and 50 civilian Tutsi and Hutu fighters, respectively, with the true value probably being somewhere in between ($= \gamma$, the militia's fighting superiority parameter from the contest function). However, interaction effects are equally positive (results not shown).

for wealth (Yanagizawa-Drott, 2014)) and its interaction with the number of militiamen, suggesting that wealth is not driving these effects (regression 3).

Prediction C2: Functional Form Again consistent with the role model channel, the effects of an additional militiaman seem to be decreasing. The concave relationship between civilian perpetrators and militiamen is presented graphically in Figure 3.5, using nonparametric local mean smoothing with an Epanechnikov kernel, conditional on the controls from my preferred specification (regression 6 in Table 3.3) and instrumenting for the number of militiamen. Furthermore, when I regress civilian participation (residuals) on a second-order polynomial in the militiamen residuals from Figure 3.5, the square term is negative and highly significant at the 99 percent level, again confirming the concave relationship.[43] The coefficient on the square term is graphically depicted in Figure 3.6, to the far right of the x-axis, labeled *Full* (sample).[44]

This result must be taken with a pinch of salt since the nonlinearities in the second stage might be driven by nonlinearities in the first stage. Reassuringly though, when I repeat the analysis above but also use a second-order term in transport costs as an excluded instrument, the results in the second stage look similarly concave (results not shown).

3.6.3 Extensions

Information To better understand the role model channel, I ask whether the militia mostly informed civilians about the ongoing genocide – something a radio reporter might have done just as well – or whether the militia

[43]This concave relationship also rules out that the militia simply changed the balance of power in a village, helping Hutu civilians to fight and kill the Tutsi. If that had been the case the militia's effects should have been convex.

[44]Once more, restricting the sample to those villages where militiamen make up less than 4 percent or less than 2 percent of the population, i.e. where $\gamma M \leq P$, still delivers equally concave effects (results not shown).

rather taught and organized civilians, something that certainly would have required physical presence in the village. Importantly, there were two radio stations in Rwanda (Radio Rwanda and Radio RTLM, the former having national coverage), which relentlessly informed listeners about the ongoing genocide. This hints at a way of testing the initial question: if the militia mostly worked through information, then the effect of the militia should be smaller in villages that were already informed, i.e. exhibited high levels of radio ownership. Thus, I should observe a negative interaction effect of the number of militiamen with radio ownership among Hutus in the data. However, if anything, the opposite seems true: the interaction effect of the number of militiamen with a Hutu radio ownership dummy variable, taking on the value of 1 if the fraction of Hutu owning a radio lies above the median, is positive although insignificant (0.716, standard error 0.844, regression 1 in Table 3.8). This result is potentially important for policy since it implies that a genocide planner could not simply substitute for an eventual absence of armed groups by enhancing radio propaganda.[45]

Because one might be concerned that radio ownership does not solely reflect information but also wealth, I further control for the fraction of Hutu with a cement floor (as noted, a good proxy for wealth (Yanagizawa-Drott, 2014)) and its interaction with the number of militiamen (regression 2 in Table 3.8) and the results are robust. (As mentioned above, the insignificant coefficient on the radio ownership interaction effect also rules out that traveling civilians, who spread information, have a direct effect on civilian participation, since the militia's effect, including the potential direct effect of the instrument, should then be larger for villages with no outside information, i.e. radio access.)

In line with the militia's physical presence in the village being crucial for civilian mobilization, I also find that, once I fix the number of militiamen in each village, militiamen in neighboring villages, within a certain radius,

[45]This result also rules out that the militia solely functioned as a coordination device.

have no effect on civilian participation, which should have been the case, if information spillovers had been of importance (militiamen in neighboring villages are instrumented with the average transport costs for those neighboring villages). All results are reported in Table 3.8. The coefficient on the average number of militiamen in villages within a 10 kilometer radius is -0.507 (standard error 1.573, regression 3), insignificant and, if anything, negative. The same is true for the coefficient on the average number of militiamen in villages within 10 to 20 kilometers (-0.411, standard error 1.810, regression 5). Furthermore, all results are robust to controlling for the within-10-km and 10-to-20-km, respectively, average of the standard controls (regressions 4 and 6) which ensures a causal interpretation.[46]

Identifying Opposing Villages The empirical evidence suggests that the militiamen functioned as role models for the whole sample of villages and the bulk of anecdotal evidence supports this view. The same anecdotal evidence, however, also suggests that in some villages civilians did oppose the militia. Identifying those, potentially few, villages is not only interesting in itself but also allows me to test the predictions of the force model. In particular, anecdotal evidence suggests that villages with a large proportion of ethnically mixed households were more likely to oppose the militia, since civilians would be more willing to resist when their family members' and friends' lives were at risk. Des Forges (1999, p. 381) writes, *"In the southern part of Ngoma commune, a man of some standing in the community at first took in many relatives from his wife's Tutsi family as well as his Tutsi godson and his*

[46]Since each village has on average 23 neighboring villages within a 10 kilometer radius and 60 neighboring villages between 10 and 20 kilometers away, the estimated spillover coefficients from above further need to be normalized by 23 and 60, respectively, to be directly comparable to the main effect. This insignificant result for neighboring villages also rules out that promising Hutu civilians safety from the Tutsi rebels in exchange for their participation was a major channel, since those promises should become more credible the more militiamen that arrive in neighboring villages. Furthermore, the militia's effects on civilian participation are not significantly larger in villages close to the RPF fighting front (results not shown).

family."

Summing up over all Hutu and Hutu-Tutsi households F_c in a commune c (remember that ethnicity data is only available at the commune level), I define intra-household ethnic polarization as[47]

$$IHEP_c = \sum_{i=1}^{F_c} \frac{N_{ic}}{N_c} \cdot h_{ic} \cdot t_{ic}, \tag{3.10}$$

where N_c is the total number of people in all households F_c in commune c, N_{ic} the number of people in household i and h_{ic} is the fraction of household members in household i that are Hutu and t_{ic} the fraction that are Tutsi, respectively. The higher this measure is, the higher the chances that civilians in those villages opposed the militia.[48]

In Figure 3.6 I report the coefficients on the square term from regressions of civilian participation (residuals, netting out all controls) on a second-order polynomial in the militiamen residuals from Figure 3.5, for different percentiles of intra-household ethnic polarization. Interestingly, for villages with high levels of intra-household ethnic polarization (up to the 91st percentile), i.e. those where one would expect resistance, the effects of an additional militiaman are increasing (the point estimates on the square term are positive and significant), as predicted by the force model (Prediction C2). From the 90th percentile onwards, point estimates turn insignificant and finally negative for the full sample of villages. The convex relationship between civilian perpetrators and militiamen for high levels of intra-household ethnic polar-

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

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

ization is also presented graphically in Figure 3.7. However, sample sizes are small and the results should therefore again be interpreted with caution.

Nevertheless, to provide further support for the argument that these villages with high levels of intra-household ethnic polarization were opposing the genocide, I can also link some of them to anecdotal evidence. For example, Des Forges (1999) writes that in Huye commune (97th percentile), both Hutu and Tutsi civilians fended off attackers from outside. Des Forges (1999, p. 350) continues that a witness from the commune of Ngoma (98th percentile) recalls that *"Kanyabashi (the burgomaster) urged the people of Cyarwa to avoid violence and to fight together against attacks."* On a more general note, many of the communes with high intra-household polarization are located in the south-west of Rwanda, where the opposition was overall more pronounced. Butare province, for instance, had a Tutsi leader who actively opposed the genocide.[49]

The Psychology of Participation Overall, my results suggest that civilians did not spontaneously start killing people nor were the majority of them actively opposing the genocide but rather: villagers followed the militia's orders. This finding is consistent with extensive literature in psychology linking participation in violence to obedience to authority.

Furthermore, anecdotal evidence from interviews with Rwandan perpetrators supports this view (Hatzfeld, 2005; Lyons and Straus, 2006). An interviewee, asked why he participated, answers (Lyons and Straus, 2006, p. 79), *"It was a law. Whenever a leader gives you a command, you do it."* Another one puts it more generally (Lyons and Straus, 2006, p. 96), *"I had to respect the law of those who were higher than me. (...) Rwandans obey*

[49]Novta (2014) demonstrates the importance of ethnic composition for conflict using data from the Bosnian civil war. Similar in flavor to the Rwandan case, conflict breaks out in ethnically homogeneous areas first and only afterwards spreads into ethnically diverse areas. However, the channels differ: Whereas in her case conflict is deferred in ethnically diverse areas because each groups chance of winning is relatively low, in the Rwandan case ethnically mixed areas oppose conflict because of family ties.

authorities."[50]

Milgram (1974, p. 133) explains this behavior with the *agentic state*, in which a person *"sees himself as an agent for carrying out another person's wishes."*[51] Waller (2002, p. 111) continues that there seems to be *"a mystical shift from one self to another that enables a person to commit extraordinary evil."* One interviewee in Hatzfeld (2005, p. 48) vividly describes this phenomenon, admitting his obedience but failing to take full responsibility:

> *"I offer you an explanation: it is as if I had let another individual take on my living appearance and the habits of my heart, without a single pang in my soul. (...) I admit and recognize my obedience at that time, my victims, my fault, but I fail to recognize the wickedness of the one who raced through the marches on my legs, carrying my machete. (...) Therefore I do not recognize myself in that man. But perhaps someone outside this situation, like you, cannot have an inkling of that strangeness of mind."*

3.7 External Validity

In this section, I argue that the massive civilian participation during the Rwandan Genocide, however horrible and grim, is not unique, but that similar events have occurred throughout 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

[50] Many other examples can be found. For instance, asked, why he did not refuse, another perpetrator answers (Lyons and Straus, 2006, p. 88), *"I could not. They were the authorities. I respected them. If you come and order me, can I refuse? I did not know there were consequences."* The same person, asked, why he could kill people he knew and had good relations with, *"It was not my will. It was because of the authorities who asked me to do it."*

[51] *"The most far-reaching consequence of the agentic shift is that a man feels responsible to the authority directing him but feels no responsibility for the content of the actions that the authority prescribes."* (Milgram, 1974, pp. 145-146).

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 vast 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 mostly acted 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 is reminiscent of the Rwandan Genocide: usually, a few German officers would arrive at a village, ordering local Lithuanians, civilians as well as militia, to round up the Jews and kill them. The Germans supervised these massacres and instructed local perpetrators how best to kill.

To substantiate the argument that the Germans had an impact on Lithuanian participation in the killing of the Jews, I also present suggestive empirical evidence. To this end, I collected 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 Jewish State Museum and the Austrian Verein Gedenkdienst. I match this massacre data to an administrative map of Lithuania to get the number of Nazi

(Lithuanian) massacres per municipality, the first unit of observation. Since unfortunately I lack data on the number of perpetrators, I assume that they are proportional to the number of massacre victims.[52]

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 the 99 percent confidence level (0.683, standard error 0.151 in regression 1 in Table 3.9) and this relationship holds up when I add 10 county fixed effects and various geographic controls, such as distance to the border, distance to the western border (from where the Germans invaded), distance to the capital Vilnius, distance to the closest major road or railway track and distance to the closest city, to proxy for population density (regression 2).[53]

Since there are only 48 municipalities, I further divide Lithuania into 1,033 grids of equal size (0.1 degree x 0.1 degree) which I once again match to the massacre data. This refined analysis allows me to control for 48 municipality fixed effects (or 133 artificial grid effects). Moreover, it confirms the positive relationship: point estimates increase to 0.898 (standard error 0.029) in the specification with all controls and municipality effects (regression 4) and to 0.907 (standard error 0.026) in regression 5 with 133 grid effects of size 0.3 by 0.3 degree.

At this point, one could potentially use a similar instrumental-variables strategy as for the Rwandan case to identify causal effects, but this is beyond the scope of this paper. Thus, although I cannot claim that these effects are causal, the results are consistent, in particular since I am likely to estimate a lower bound, measurement error as well as the potentially strategic use of Nazi perpetrators are likely to push the OLS estimates down. Furthermore, the "Holocaust Atlas of Lithuania" provides narrative background

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

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

information on each of the massacres which occasionally contains the exact number of perpetrators on both sides. Consistently, the few cases where this information is available confirm the huge multiplier effect: the number of Lithuanian perpetrators is always very much larger than the number of German perpetrators. The anecdotal evidence further suggests that the majority of Lithuanians did not actively oppose the Germans, again mirroring the Rwandan Genocide.[54]

Finally, I provide some suggestive evidence that transport costs seemed to be of importance for the allocation of Nazis, thus resembling the first stage for the Rwandan case: the number of Nazi perpetrators is strongly negatively related to the distance to the nearest major road or railway. Point estimates are very robust across the three specifications using 1,033 grids, and significant throughout at the 99 percent confidence level (Table 3.10).

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

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

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

> *ing."*

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:

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

3.8 Conclusion

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

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

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

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

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

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

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

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Tables and Figures

Table 3.1: Summary Statistics

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

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

Table 3.2: OLS Estimates of Main Effect

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

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

Figure 3.1: Armed-Group Violence (# Prosecutions)

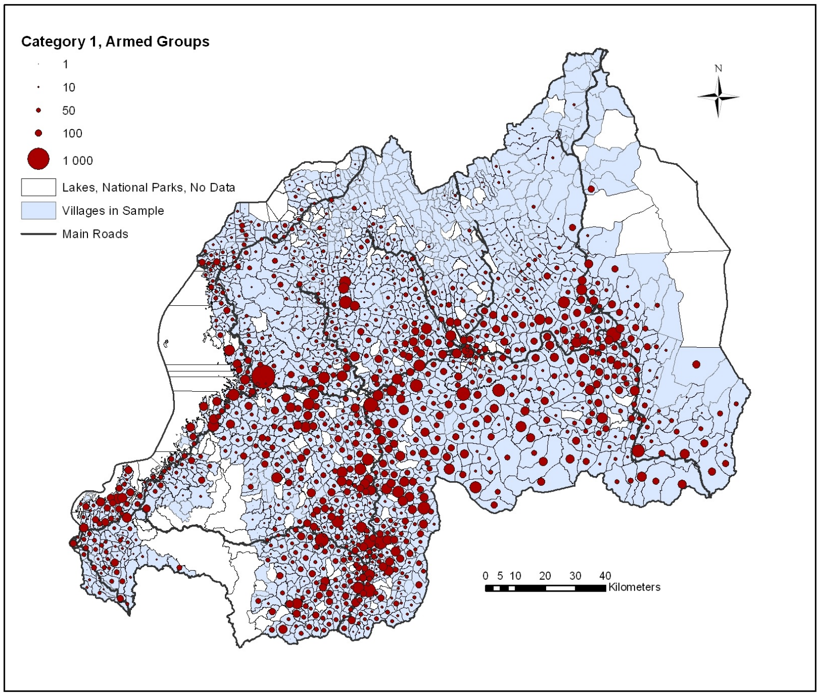


Figure 3.2: Civilian Violence (# Prosecutions)

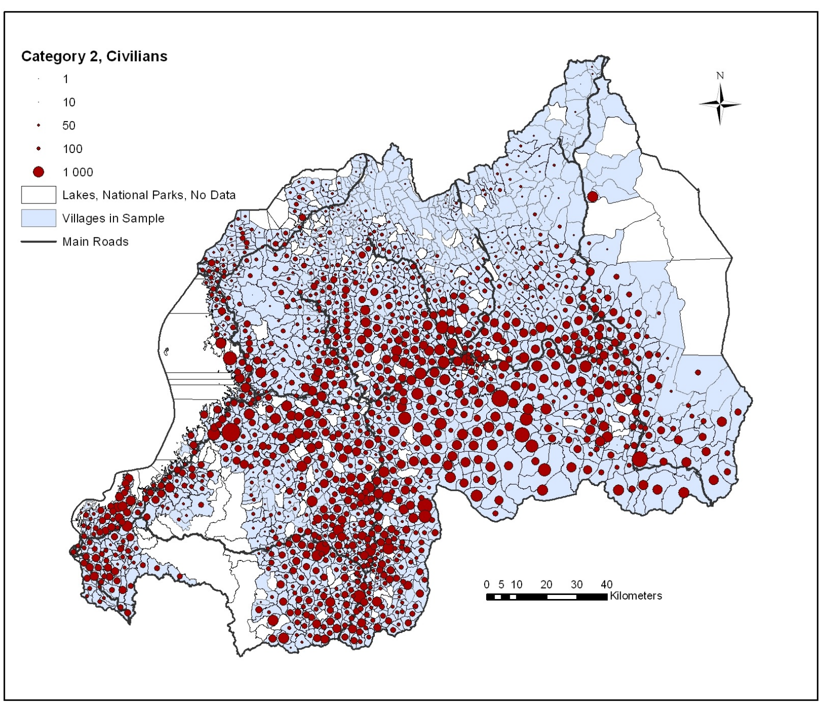
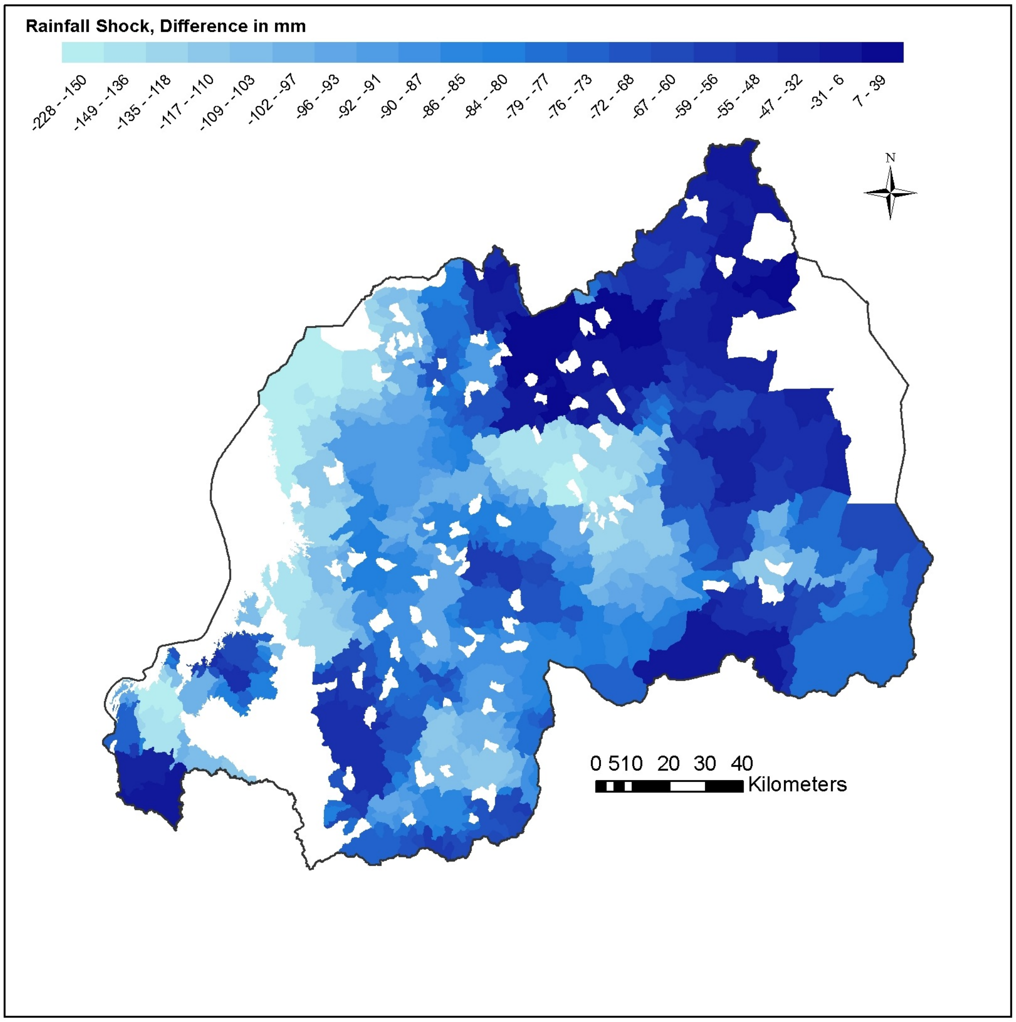


Figure 3.3: Rainfall



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

Figure 3.4: Construction of the Instrument in ArcGIS

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

Table 3.3: Main Effects

A. Dependent Variable:	# Militiamen, log			# Civilian Perpetrators, log		
	First Stage			Reduced Form		
	(1)	(2)	(3)	(4)	(5)	(6)
Armed Groups' Transport Cost	−0.357	−0.460	−0.509	−0.480	−0.573	−0.661
	[0.116]***	[0.117]***	[0.115]***	[0.126]***	[0.125]***	[0.141]***
Standard Controls	yes	yes	yes	yes	yes	yes
Growing Season Controls	no	yes	yes	no	yes	yes
Additional Controls	no	no	yes	no	no	yes
Province Effects	yes	yes	yes	yes	yes	yes
F-stat	9.50	15.54	19.54	14.45	20.93	21.91
R^2	0.46	0.48	0.50	0.53	0.54	0.58
N	1433	1433	1432	1433	1433	1432

B. Dependent Variable:	# Civilian Perpetrators, log					
	OLS			IV/2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
# Militiamen, log	0.649	0.647	0.626	1.345	1.245	1.299
	[0.065]***	[0.066]***	[0.051]***	[0.369]***	[0.241]***	[0.258]***
Standard Controls	yes	yes	yes	yes	yes	yes
Growing Season Controls	no	yes	yes	no	yes	yes
Additional Controls	no	no	yes	no	no	yes
Province Effects	yes	yes	yes	yes	yes	yes
R^2	0.72	0.72	0.74	0.50	0.56	0.54
N	1433	1433	1432	1433	1433	1432

Note: **Armed Groups' Transport Cost** is the instrument (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). **Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 days of the genocide in 1994, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and its interaction with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 1994 in the village, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road. **Additional Controls** are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and the number of days with RPF presence. All control variables, except "Number of Days with RPF presence", are in logs. Interactions are first logged and then interacted. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150 km 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 3.4: Exclusion Restriction Tests

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

Note: In **regressions 1 to 3** the instrument is distance to the main road interacted with rainfall along the way between village and main road during the first 5 days/1 week/2 weeks of the genocide. In **regression 4** the sample is restricted to communes without main road passing through. **Standard Controls (for regressions 1 to 3)** include village population, rainfall in the village during the 100 days of the genocide in 1994, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the first 5 days/1 week/2 weeks of genocide in 1994, rainfall along the way between village and main road during the remaining genocide days in 1994, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period, distance to the main road and its interactions with the two last rainfall-along-the-way measures. **Standard Controls (for regression 4)** include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 days of the genocide in 1994, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and its interaction with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 1994 in the village, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road. **Additional Controls** are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and the number of days with RPF presence. **First Days Controls** are rainfall in the village during the first 5 days/1 week/2 weeks of the genocide and the ten-year long-term rainfall for those first days, ten-year long-term rainfall along the way between village and main road during the first 5 days/1 week/2 weeks of the genocide period and its interaction with distance to the main road. All control variables, except "Number of Days with RPF presence", are in logs. Interactions are first logged and then interacted. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150 km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 3.5: Robustness Checks

Dependent Variable:	# Civilian Perpetrators, log (IV/2SLS)									Mass Grave
	Measurement Error in Gacaca Data			Additional Controls			Commune Effects	Without Kigali	Without Cities	Alternative Dep. Var.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# Militiamen, log	1.284 [0.268]***	1.351 [0.285]***	1.489 [0.305]***	1.357 [0.277]***	1.246 [0.300]***	1.266 [0.252]***	1.203 [0.387]***	1.305 [0.298]***	1.298 [0.292]***	
Armed Groups' Transport Cost										−0.035 [0.012]***
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Harvest Controls	no	no	no	yes	yes	no	no	no	no	no
Other Rainfall Controls	no	no	no	no	yes	no	no	no	no	no
Main City Transport Controls	no	no	no	no	no	yes	no	no	no	no
Province Effects	yes	yes	yes	yes	yes	yes	no	yes	yes	yes
Commune Effects	no	no	no	no	no	no	yes	no	no	no
R^2	0.55	0.54	0.34	0.51	0.57	0.56	0.66	0.54	0.55	0.05
N	1366	1279	1432	1432	1432	1432	1432	1400	1357	1432

Note: In **regression 1** all villages with mass graves are dropped. In **regression 2** all villages at most 3.5 kilometers away from a mass grave site are dropped. In **regression 3** the average number of militiamen is added to villages with 0 reported militiamen. **Regressions 4 to 7** use different controls. In **regression 8** Kigali province is dropped and in **regression 9** villages within five kilometers of a main city are dropped. In **regression 10** the dependent variable is a dummy taking on the value of 1 if a mass grave was found in the village. **Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 days of the genocide in 1994, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and its interaction with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 1994 in the village, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road. **Additional Controls** are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and the number of days with RPF presence. **Harvest Controls** are rainfall along the way between village and main road during the harvest season and its interaction with distance to the main road. **Other Rainfall Controls** are distance to the main road interacted with a) rainfall during the growing season in 1994 in the village and b) ten-year long-term average rainfall during the growing seasons in the village as well as yearly long-term average rainfall in the village, yearly long-term average rainfall along the way between village and main road and its interaction with distance to the main road. **Main City Transport Controls** are rainfall along the way between village and the closest main city during the genocide period and its interaction with distance to the main city. All control variables, except "Number of Days with RPF presence", are in logs. Interactions are first logged and then interacted. There are **11 provinces** and **142 communes** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150 km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 3.6: Strategic Use of Armed Groups

Dependent Variable:	# Militiamen, log			
	(1)	(2)	(3)	(4)
Armed Groups' Transport Cost	−0.632	−0.690	−0.659	−0.659
	[0.177]***	[0.152]***	[0.143]***	[0.158]***
AGTC x Tutsi Minority Share		2.458	1.975	1.930
		[0.875]***	[0.649]***	[0.754]**
AGTC x Tutsi Rebels		0.214	−0.111	−0.059
		[0.322]	[0.371]	[0.373]
AGTC x Cement Floor				0.936
				[0.831]
Tutsi Minority Share		2.178	2.134	2.097
		[0.555]***	[0.490]***	[0.572]***
Tutsi Rebels		−1.159	−1.001	−0.999
		[0.128]***	[0.175]***	[0.171]***
Cement Floor				0.138
				[0.747]
Standard Controls	yes	yes	yes	yes
Growing Season Controls	yes	yes	yes	yes
Additional Controls	yes	no	yes	yes
Province Effects	yes	yes	yes	yes
R^2	0.47	0.50	0.51	0.51
N	1286	1433	1432	1432

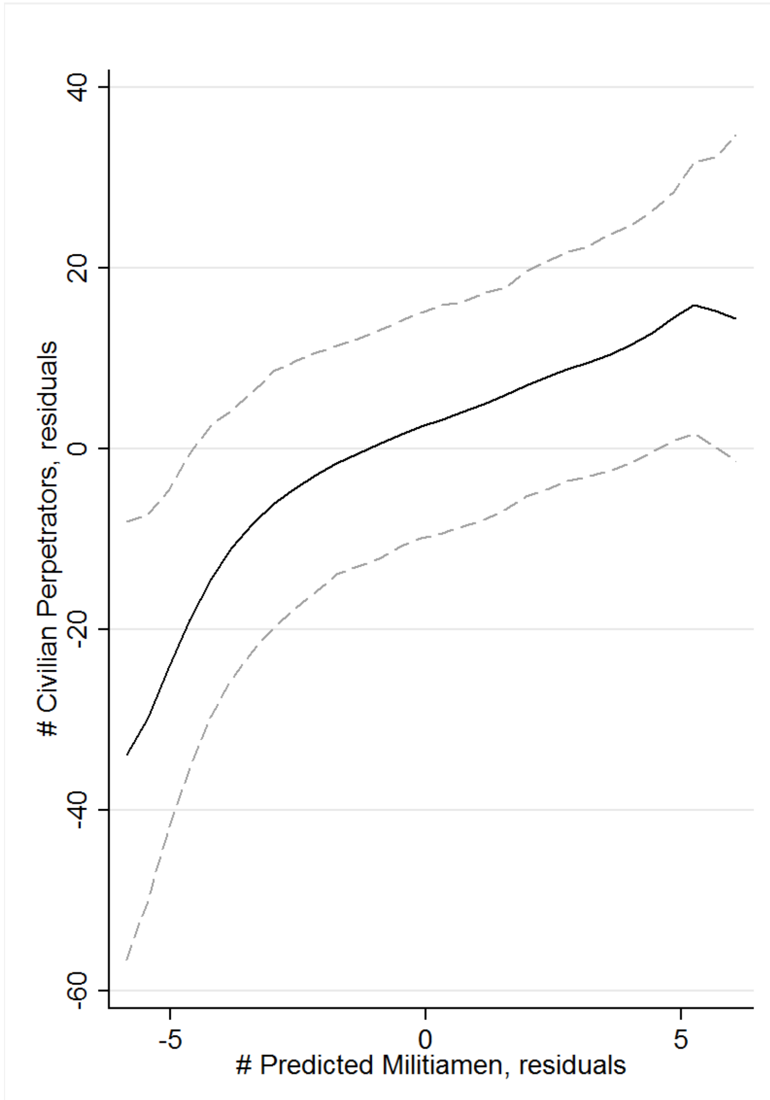
Note: **Armed Groups' Transport Cost (AGTC)** is the instrument (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). **Tutsi Minority Share** is the fraction of Tutsi divided by the fraction of Hutu. The **Tutsi Rebels** dummy takes on the value of 1 if Tutsi rebels where in control of the village at the beginning of the genocide. **Cement Floor** is the fraction of villagers with a cement floor. **Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 days of the genocide in 1994, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and its interaction with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 1994 in the village, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road. **Additional Controls** are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and the number of days with RPF presence. All control variables, except "Number of Days with RPF presence", are in logs. Interactions are first logged and then interacted. In each column, I also control for all main effects and double interactions. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150 km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 3.7: Interaction Effects, Role Model or Force Model

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

Note: The **Tutsi Rebels** dummy takes on the value of 1 if Tutsi rebels where in control of the village at the beginning of the genocide. **Tutsi Minority Share** is the fraction of Tutsi divided by the fraction of Hutu. The **Large Tutsi Group** dummy takes on the value of 1 if the Tutsi Minority Share lies above the sample median. **Cement Floor** is the fraction of villagers with a cement floor. **Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 days of the genocide in 1994, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and its interaction with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 1994 in the village, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road. **Additional Controls** are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and the number of days with RPF presence. **Tutsi Interactions** include the interaction of the Large Tutsi Group dummy with all other controls that do not involve distance to the main road. All control variables, except "Number of Days with RPF presence", are in logs. Interactions are first logged and then interacted. In each column, I also control for all main effects and double interactions. Note, in regressions 2 and 3 I do not control for the Large Tutsi Group dummy interacted with distance to the main road. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150 km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure 3.5: Functional Form, Role Model or Force Model



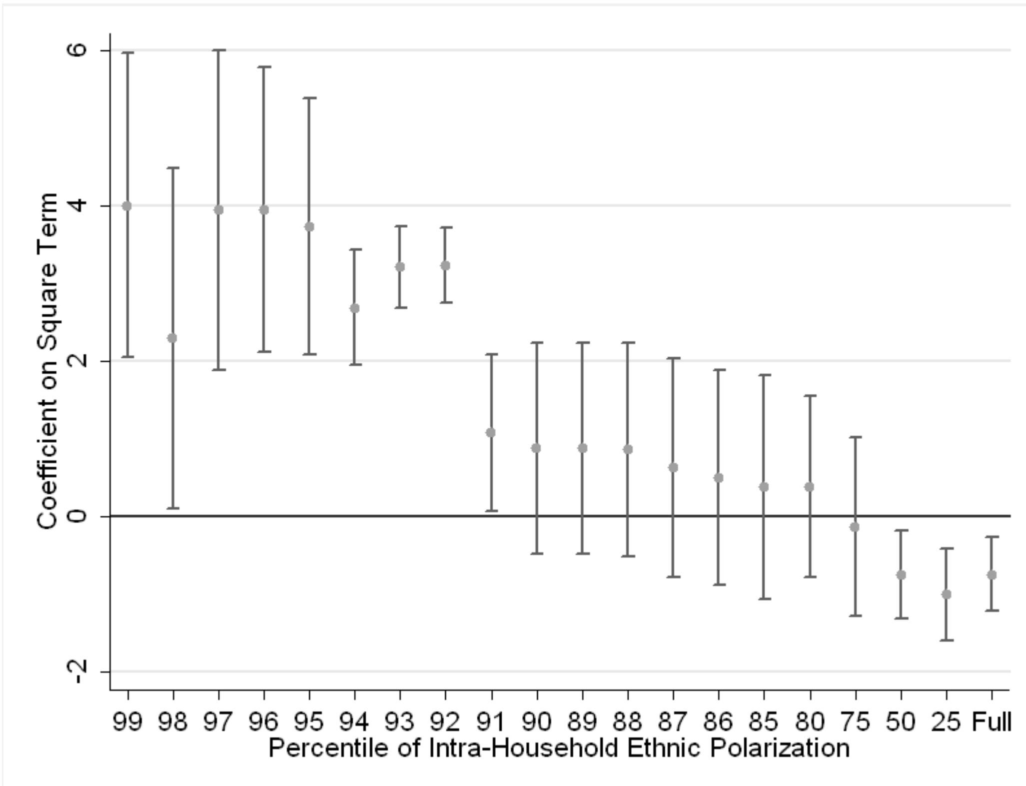
Note: Local mean smoothing (Epanechnikov kernel, bandwidth=2.5, observations are grouped into 30 equal-sized bins). 95 percent confidence intervals are bootstrapped. Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Table 3.3) are used to construct residuals.

Table 3.8: Extension: Information

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

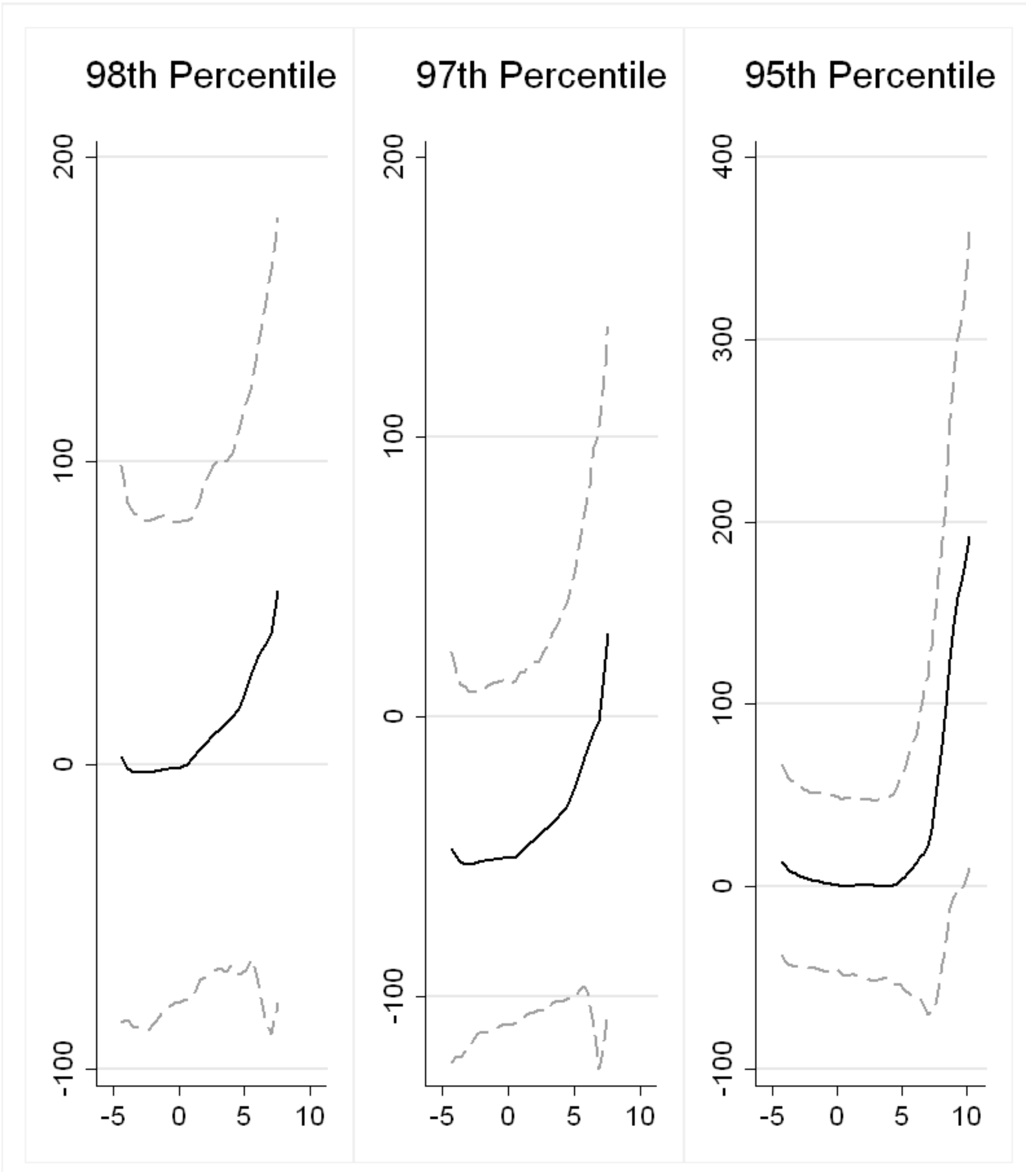
Note: **Hutu Radio Ownership** is a dummy taking on the value of 1 if the fraction of Hutu villagers that own a radio lies above the median. **Hutu Cement Floor** is the fraction of Hutu villagers with a cement floor. **# Militiamen within 10 km (10-20 km)** is the average log number of militiamen in neighboring villages (radius with 10 km or 10-20 km). **Standard Controls** include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, ten-year long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 days of the genocide in 1994, ten-year long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and its interaction with distance to the main road. **Growing Season Controls** are rainfall during the growing season in 1994 in the village, ten-year long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road. **Additional Controls** are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and the number of days with RPF presence. **Standard Controls, Neighbors** are the averages of all standard controls for neighboring villages. In regressions 3 and 5 I also control for the average of distance to the main road and rainfall along the way between village and main road during the 100 days of the genocide in 1994 for neighboring villages. All control variables, except "Number of Days with RPF presence", are in logs. Interactions are first logged and then interacted. In each column, I also control for all main effects and double interactions. There are **11 provinces** in the sample. **Standard errors** correcting for spatial correlation within a radius of 150 km are in square brackets, Conley (1999). *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure 3.6: Extension: Identifying Opposing Villages (Convex and Concave Effects)



Note: I run regressions of the number of civilian perpetrators (residuals) on a second-order polynomial in the residuals of the predicted number of militiamen for different subsamples defined by different percentiles of my intra-household ethnic polarization measure (x-axis). The coefficients on the square terms (indicating the curvature) are reported together with 95 percent confidence intervals on the y-axis. Intra-household ethnic polarization is defined in equation (3.10). Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Table 3.3) are used to construct residuals.

Figure 3.7: Extension: Opposing Villages (Convex Effects)



Note: Y-axis: # Civilian Perpetrators, residuals; X-axis: # Predicted Militiamen, residuals. Local mean smoothing (Epanechnikov kernel, bandwidth=3). 95 percent confidence intervals are bootstrapped. Samples restricted to 98th, 97th and 95th percentile of intra-household ethnic polarization. Intra-household ethnic polarization is defined in equation (3.10). Militiamen are instrumented with transport costs (distance to the main road interacted with rainfall along the way between village and main road during the 100 days of the genocide in 1994). All controls from my preferred specification (regression 6 in Table 3.3) are used to construct residuals.

Table 3.9: The Case of Lithuania: Main Effects

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

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

Table 3.10: The Case of Lithuania: "First Stage"

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

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

Proofs

Predictions S1, S2 and S3 – Central Planner's Problem (Role Model)

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

$$M_{ei} = \frac{1}{1+\rho_i}\left(\frac{B}{r_i} - \rho_i \cdot M_{li}(S_i) + \sum_{j\neq i}^{N} \frac{r_j}{r_i}(M_{lj}(S_j))\right), \tag{3.11}$$

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

Prediction C1 – Interactions

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

$$\frac{\partial E\left(K^w\right)}{\partial S \partial M_e} = K_{MM}(M_e + M_l(S)) \cdot \frac{\partial M_l}{\partial S}. \tag{3.12}$$

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

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

$$\frac{\partial E\left(K^o\right)}{\partial S \partial M} = I_{MP}(M,P) \cdot K(M) + I_P(M,P) \cdot K_M(M). \tag{3.13}$$

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

2. (ii) Opposing Villages, $\gamma M > 1 + T + R$: Now $I_{MP}(M,P) > 0$, thus $\frac{\partial E(K^o)}{\partial S \partial M}$ in equation (3.13) is ambiguous.

Prediction C2 – Functional Form

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

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

$$\lambda I(M_1) + (1-\lambda) I(M_2) \geq I(\lambda M_1 + (1-\lambda) M_2). \tag{3.14}$$

Now, set $M_2 = 0$. This gives

$$\lambda I(M_1) \geq I(\lambda M_1). \tag{3.15}$$

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

$$\lambda I(M_1) \cdot K(M_1) \geq I(\lambda M_1) \cdot K(M_1). \tag{3.16}$$

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

$$\lambda I(M_1) \cdot K(M_1) \geq I(\lambda M_1) \cdot K(M_1) > I(\lambda M_1) \cdot K(\lambda M_1). \tag{3.17}$$

Rearranging gives

$$\lambda H(M_1) > H(\lambda M_1), \tag{3.18}$$

which implies convexity of H.

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

Central Planner's Problem: Militia in Opposing Villages (Force Model)

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

$$\begin{aligned} \max_{\{M_{ei}\}} \quad & U = \sum_{i=1}^{N} I(\gamma M_{ei}, P_i) \cdot A\,(M_{ei})^\alpha \\ \text{s.t.} \quad & B = \sum_{i=1}^{N} M_{ei} r_i, \end{aligned} \tag{3.19}$$

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

Prediction S 4. *The number of militiamen M_e is zero in villages with a large transport cost r and large strategic factors S (if B is not too large).*

Intuitively, since the militia's effects are increasing up to some cutoff (i.e. when $\gamma M = P$ and potentially a little beyond),[55] the planner will start sending militiamen to villages that are easy to reach and easy to fight until the budget has been used up, thus places with high transport costs and high levels of the strategic factors will not get any militiamen. This is only true if the budget B is not too large, because otherwise all villages will receive militia. Loosely speaking, villages will either receive a lot of militiamen or none at all. To be more precise:

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

[55] Eventually the effects have to turn concave because the contest function approaches 1.

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

However, I do not find evidence for Prediction S4, i.e. that villages with both high transport costs and high levels of the strategic factors receive no militiamen as would be the case if the majority of villages had opposed the militia. Furthermore, the total number of militiamen in the sample is too low to suggest that the budget constraint was not binding.

The Legacy of Political Mass Killings: Evidence from the Rwandan Genocide*

4.1 Introduction

Do political mass murders affect later economic performance? Since 1945, there have been nearly 50 political mass murders (genocides and politicides) where an estimated 12 million combatants and 22 million noncombatants have been killed; more than all victims of internal and international wars during the same time period (Harff, 2003). These tragic events and the associated loss of lives thus evidently have immense, direct, negative implications for the welfare of societies. Beyond the immediate impact, however, political mass murders may also result in longer term impacts on the remaining population. But, if so, what exactly are these impacts?

This question is of particular interest since the consequences are *a priori* unclear, at least with respect to economic aspects of welfare such as per capita consumption, income and assets. On the one hand, political mass murders are typically associated with civil conflict and war, which may destroy physical and human capital. In addition, deeper determinants of economic

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development, such as social capital, institutions and norms conducive to the efficiency of markets, may be adversely affected. These mechanisms would tend to decrease income, assets and consumption.[1] On the other hand, since, by definition, political mass murders imply the loss of human lives, they are intrinsically linked to reductions in population size. Consequently, factors of production that are fixed, such as land and other natural resources, may increase on a per capita basis. More broadly, the capital intensity among the remaining population may increase, as assets are effectively redistributed from the deceased to the living. This is in essence the Malthusian view of the role of conflict. It is also consistent with the central assumption in the rationalist branch of conflict theory, i.e. that a key motivation for conducting mass killings is looting and rents-capture for the group (e.g., ethnic) conducting the killings (Esteban et al., forthcoming; Jackson and Morelli, 2011).[2] The key implication is that such mechanisms would tend to increase per capita income, assets and consumption for the remaining population. The (net) effects are thus theoretically ambiguous and, as such, empirical evidence is necessary.

Yet, robust evidence on the legacy of political mass killings on economic performance is scarce. This is not least due to the fundamental challenge of establishing causality, since economic shocks are likely to jointly determine both violence and future economic performance (Easterly et al., 2006; Miguel et al., 2004).[3]

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[1] The effects under this mechanism would depend on the time horizon. Under a neoclassical production function and perfectly competitive markets, for example, negative effects in the short and medium run may in the long run lead the economy back to the steady-state growth rate (Miguel and Roland, 2011).

[2] If ethnic or religious diversity hampers economic performance, as is shown by evidence (Hjort, 2014; Montalvo and Reynal-Querol, 2005), an additional mechanism of deliberate and systematic destruction of an ethnic or religious group, in whole or in part, may be that it is conducive to economic performance by decreasing diversity.

[3] The direction of the bias is *a priori* unclear. If conflict predominantly breaks out in poorer areas, as people's opportunity costs of fighting are lower, then a simple bivariate estimate would be downward biased. If richer areas are more prone to fighting, as the stakes are higher, the estimate would be upward biased. The literature on conflict and

We approach these issues by investigating the economic effects of violence conducted against the ethnic Tutsi minority population during the 1994 Rwandan Genocide. In the history of political mass murders, this is certainly a prominent case. During a period of approximately 100 days, the government – lead by extremists of the ethnic Hutu majority – conducted an extermination campaign against the Tutsi population that resulted in an estimated 0.5 to 1 million deaths. Rich household survey data allows us to give a detailed picture of the socio-economic situation in Rwandan villages six years after the genocide. To address the issue of causality, we build on Yanagizawa-Drott (2014) and exploit local variation in the reception of the radio station RTLM (Radio Television Libre des Mille Collines). Backed by the Hutu extremist government and setup shortly before the genocide, the radio station explicitly called upon the Hutu majority population to exterminate the Tutsi minority population. Using the local variation in reception induced by Rwanda's hilly terrain to identify causal effects and prosecution data to measure violence, Yanagizawa-Drott (2014) finds that villages with good reception experienced significantly higher levels of violence and participation in the killings. Importantly, as the station's transmitters were destroyed with the end of the genocide, the temporary shock in exposure to radio that induced violence against the ethnic minority presents us with a rare opportunity to examine the economic effects of genocidal violence. We estimate the reduced-form impact of RTLM reception on later economic outcomes in villages, and, under the arguably plausible assumption that the reception affected later outcomes only through violence, we also present scaled

war has tried to solve this problem by using various difference-in-difference techniques and instrumental variables. Instruments include distances to various borders (Akresh and de Walque, 2010; Miguel and Roland, 2011; Pellillo, 2012) or rebel headquarters (Arcand and Wouabe, 2009).

instrumental-variable estimates.[4]

Our results show that households living in villages that experienced greater levels of violence induced by RTLM reception have *higher* living standards six years after the genocide. Specifically, they have higher levels of consumption and own more land assets, livestock, durable goods and total assets per capita. Furthermore, we find that per capita income and output from agricultural production are significantly higher in villages that exogenously experienced more violence. These effects are also quantitatively meaningful. Our estimates indicate that a 10 percent increase in violence in a village during the 1994 genocide is associated with approximately a 10-15 percent increase in per capita consumption and income among households six years afterwards.

These results are thus consistent with the Malthusian view that mass murders can reduce the population, which raises capital intensity and redistributes productive assets, such as land, from the deceased to the remaining population.[5] Thus, to the extent that the violence during the Rwandan Genocide destroyed physical and human capital, or decreased the efficiency of markets more broadly, these effects seem to have been muted and dominated by the Malthusian mechanism. Importantly, the results cannot simply be explained by selective killings based on pre-genocide wealth or human capital. First, Tutsis were generally wealthier and more educated than Hutus. A pure selection mechanism where relatively poor individuals were killed, leaving survivors that generally had more assets to begin with, is therefore unlikely. Second, we find no evidence that the completed years of schooling or cognitive skills of surviving adults are significantly different in villages that

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[4] The broadcasts essentially contained no content that would directly affect productivity or markets, such as information about agricultural technologies or health education. Instead, the content was primarily music mixed with ethnically charged propaganda and direct encouragement to participate in the killings of Tutsis (Kimani, 2007).

[5] This is also consistent with qualitative evidence showing that looting of the property of killed Tutsis was common (Hatzfeld, 2005).

experienced more violence.[6]

It is worth noting a limitation of our study: the estimates capture short-/medium-term effects, as the data only allows us to investigate outcomes measured six years after the killings took place. Our results do therefore not directly speak to whether these effects will last in the long term. However, we do provide some evidence on mechanisms that are informative about the potential long-term impacts. First, we find that the violence affected the age distribution of the surviving population. Villages that experienced higher levels of genocidal violence have a higher fraction of the surviving population of working age (13-49). This is informative, not only because it sheds some additional light on the mechanisms driving the positive effects on output and income, but also because this suggests that the positive effects may be temporary, as the short-term effects would tend to disappear as these cohorts become older and less productive. Second, we find evidence of higher fertility rates among young women. Thus, if an important driver of the positive effects is the increase in capital intensity when a significant portion of the population is killed, the effects may be transitory as the deceased population is rapidly replaced over time. Third, we find that the violence reduced the human capital among surviving children of primary school age at the time of the genocide. Specifically, there is a decrease in cognitive skills, such as the ability to read, write and do simple math. This suggests that these cohorts of children will be relatively less productive as adults, with negative implications for future income that may counteract the positive effects estimated in the short term. Together, these results provide suggestive evidence that the estimated increases in assets, income and consumption may be transitory and not persist in the long run.

We add to the literature in several ways. To our knowledge, this paper is the first to demonstrate that conflict in general, and political mass mur-

[6] Unfortunately, the data does not allow us to directly test to what extent other channels such as social capital, local institutions and norms are affected.

der in particular, can have positive effects on economic performance.[7] More specifically, it first contributes to the literature on the effects of the genocide in Rwanda on later outcomes (Akresh and de Walque, 2010; Schindler and Brueck, 2012; Serneels and Verpoorten, 2012) by producing novel evidence on the positive effects on living standards. Second, the paper is related to the literature on civil war and ethnic conflict. In recent years, a number of studies have exploited within-country variation to estimate the economic effects of conflict (Abadie and Gardeazabal, 2003; Brakman et al., 2004; Davis and Weinstein, 2002; Miguel and Roland, 2011), with a special focus on human capital (Alderman et al., 2006; Chamarbagwala and Moran, 2011; Shemyakina, 2011).[8]

Our work is also related to a small literature on the effects of ethnic cleansing (Acemoglu et al., 2011; Chaney and Hornbeck, 2012). While Acemoglu et al. (2011) document negative economic effects of the killing of the Jews during the Holocaust in Russia, Chaney and Hornbeck (2012) find that the expulsion of the Moriscos in Spain in 1609 increased the economic performance for the remaining population. However, besides considering a more recent setting, our paper establishes that similarly positive effects (although for another time period and horizon) can prevail even in a conflict environment where the ethnic cleansing consists of outright mass killings. As far as the genocide resulted in a population decrease, our paper echoes the findings by Young (2005) and Farmer (1991). The former finds that the large number of HIV deaths in South Africa have positive effects on the surviving population; the latter documents similar effects as a result of the Black

[7] The increase in standards of living for survivors by no means implies that violence increases welfare. After all, mass killings imply immense losses of lives. Assessing social welfare is also a daunting task with significant philosophical challenges, such as how to value a human life, or how to take into account distributional aspects.

[8] Starting in the late 1970s with Organski and Kugler (1977, 1980) there are also numerous cross-country studies that have looked into the effects of civil conflict on economic recovery and growth (Cerra and Saxena, 2008; Chen et al., 2008; Collier, 1999). The approach taken in this paper is to exploit village-level variation and therefore, it is a limitation that we are unable to estimate the aggregate economic effects of the genocide.

Death in Europe, ringing in the Golden Age of the Laborer. Our results also speak to a wider, interdisciplinary literature on resource scarcity and conflict (Homer-Dixon, 1999) and complement the strand of the literature that views the Rwandan Genocide as a Malthusian check (Andre and Platteau, 1998; Diamond, 2005; Verpoorten, 2012).

The remainder of the chapter is organized as follows. Section 4.2 provides some background information on the Rwandan genocide and the media in Rwanda. Section 4.3 presents a conceptual framework to guide our empirical analysis. Section 4.4 describes the data and Section 4.5 lays out our empirical strategy. Section 4.6 reports the results and the conclusion summarizes our findings and discusses potential policy implications.

4.2 Institutional Background

Rwanda's history is strongly influenced by the tensions between Hutus and Tutsis, the two largest ethnicities in the country. The nature of the distinction between the two groups is heavily debated. While some argue that the Tutsi are originally Hamitic migrants from Egypt or Ethiopia and that the Hutu are Bantu, others say that the two groups are actually much closer related. Undoubtedly, Belgian colonizers, who governed Rwanda after World War I, enforced the differences between the two groups, by favoring the Tutsi. For example, they received access to administrative positions and higher education. With the country's independence in 1962, the Hutu managed to gain power, reversing the political situation and creating a one-party state. The resulting violence forced several hundreds of thousands of Tutsi to escape Rwanda into neighboring countries such as Uganda. In 1973, following new episodes of violence fueled by unrest in the neighboring Burundi, the Hutu military leader Habyarimana seized power through a military coup, becoming officially elected president in 1978.

By 1990, Habyarimana was still president and the country was still facing tensions between the political Hutu elite and the economic Tutsi elite. The situation worsened towards the end of 1990: The Rwandan Patriotic Front (RPF) – a Tutsi rebel army willing to replace the Hutu government – started attacking in the north of the country from their base in Uganda. A conflict began between the RPF and the national army (the Forces Armees Rwandaises – FAR). Two years of conflicts led to the formation of a multi-party government and, one year later, a peace agreement under the supervision of the United Nations was signed in Arusha, Tanzania. The power sharing agreement that followed failed at dissipating the tension within the country, which started again when the airplane carrying president Habyarimana was shot down on April 6 1994. Within only a few days, extremists within the Hutu-dominated parties, known as the Akazu, managed to take over important government positions and initiate a 100 day lasting period of genocide, reaching every corner of the country. Leaders at various administrative levels took an active role in the killing supported by the Presidential Guard and the regular Rwandan Hutu Army FAR. Militia gangs such as the Interahamwe and the Impuzamugambi, equipped and trained by the FAR, agitated at local levels. Together, these two groups would become known as Hutu Power. Furthermore, several hundreds of thousands of civilians joined in the killings. The killings were highly localized; 80 to 90 percent of them were committed within the individuals' own village using low technology weapons such as clubs and machetes. There were almost no coordinated defense efforts by the Rwandan Tutsi.

The mass killings ended in mid July, when the RPF rebels conquered the capital Kigali, defeating the Rwandan army and the various militia groups. Estimates reveal that approximately 800,000 people, mostly belonging to the Tutsi minority, were killed in those 100 days. There was no foreign intervention. More detailed accounts can be found in Dallaire (2003), Des Forges (1999), Hatzfeld (2005, 2006), Gourevitch (1998) and Straus (2006).

4.2.1 Media and RTLM

Before the genocide, Rwanda had two national radio stations: RTLM and Radio Rwanda. RTLM started broadcasting in July 1993, using two transmitters. One 100 Watt transmitter was placed in the capital, Kigali, and another 1000 Watt transmitter was placed on Mount Muhe, one of the country's highest mountains. The government-owned Radio Rwanda had been broadcasting some propaganda before the genocide, but RTLM's broadcasts were by far the most extreme and inflammatory. RTLM was founded by members of Hutu Power and backed by President Habyarimana (Des Forges, 2007). One of the station's founders, Ferdinand Nahimana, was also the director of the Rwanda Bureau of Information and Broadcasting (ORINFOR), responsible for regulating mass media. Thus, connections between the station and top government officials existed even before 1994. With the start of the genocide, RTLM became the voice of the new interim government. The broadcasts continued throughout the genocide and only ended when RPF rebels seized power in mid-July 1994.

RTLM called for the extermination of the Tutsi and claimed that preemptive strikes were a necessary response for "self-defense" (Frohardt and Temin, 2007; ICTR, 2003). Analyzing taped RTLM broadcasts, Kimani (2007) reports that the most common inflammatory messages consisted of reports of Tutsi RPF rebel atrocities (33 percent); allegations that Tutsis in the region were involved in the war or a conspiracy (24 percent); and allegations that the RPF wanted power and control over the Hutus (16 percent). Key government officials appeared on air, for instance Prime Minister Jean Kambanda. After April 6 1994, the radio station made it clear that the government would not protect the Tutsi minority from attacks, and that Hutus would not be held accountable for the killings. Instead, the radio station as well as government officials encouraged the killing of Tutsis.[9]

[9] The station was very popular and there was strong demand for its broadcasts. For example, Des Forges described the high demand for RTLM as follows: "people listened to

Alternative print media also existed. There were some 30 to 60 independent newspapers at the time of the genocide, including political opposition publications (Alexis and Mpambara, 2003; Higiro, 2007). However, the circulation and readership of these newspapers was limited, especially in rural areas, because of relatively low literacy rates. Consequently, for most people radio was the only source of information (Des Forges, 1999). Consistently, Yanagizawa-Drott (2014) finds that RTLM had a significant effect on Hutu participation in violence against the Tutsi and that the RTLM broadcasts account for approximately 10 percent of the Tutsi deaths.

4.3 Conceptual Framework

To guide our empirical analysis, we use a version of the standard Solow model. Consider a country that has two ethnic groups, majority group H and minority group T. Each village in the country functions as an independent economy, with a total population of L_t at time t, of which L_t^T belong to the minority and L_t^H to the majority. Each village is further equipped with a constant returns to scale Cobb-Douglas production function

$$Y_t = A(K_t^T + K_t^H)^\alpha (L_t^T + L_t^H)^{1-\alpha}, \tag{4.1}$$

where Y_t is village output and K_t^T and K_t^H are the stock of capital of group T and H, respectively. Output can be expressed in per capita terms, with $y_t = Y_t/(L_t^T + L_t^H)$ and $k_t = (K_t^T + K_t^H)/(L_t^T + L_t^H)$

$$y_t = Ak_t^\alpha. \tag{4.2}$$

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the radio all the time, and people who didn't have radios went to someone else's house to listen to the radio. I remember one witness describing how in part of Rwanda, it was difficult to receive RTLM, and so he had to climb up on the roof of his house in order to get a clear signal, and he would stand up there on the roof of his house with his radio to his ear listening to it." Interview with Alison des Forges, available (January 30 2011) at <www.carleton.ca/jmc/mediagenocide>.

As in the original Solow model, a constant fraction s of output is saved, the capital stock depreciates at the rate δ and population grows exogenously at the rate n. Equating investment and savings gives

$$(1+n)k_{t+1} = k_t(1-\delta) + sAk_t^{\alpha}. \tag{4.3}$$

This equation determines the steady-state level of capital per capita k^*. Assume that all the villages are still far away from their steady-state level of capital intensity, thus they experience transitional growth.

Now assume that the central government is ruled by members of group H, and it initiates a genocide against group T at time $\tilde{t}$. To mobilize group H members in villages, the government sends out radio broadcasts encouraging people to engage in the killings by stating that the government has initiated a genocide, implying that group H members will not be punished (alternatively, that non-participation will be heavily punished) if they kill group T members and acquire their capital. Let that signal be sufficiently persuasive for some group H members who are at the margin of participating, then the fraction of group H members that participate, h, will be an increasing function in the fraction of members that receive the broadcasts in a village, r. Furthermore, assume that the number of group T survivors L_g^T is decreasing in h and that group H acquires the property of the killed group T members. To capture that conflict is costly and inefficient, let some fraction l of total capital be destroyed. Consumption per capita some s years after the genocide is therefore

$$c_{g,\tilde{t}+s} = (1-s)A\left(\frac{(1-l(r))K_{g,\tilde{t}+s}^{T} + K_{g,\tilde{t}+s}^{H}}{L_{g,\tilde{t}+s}^{T}(r) + L_{g,\tilde{t}+s}^{H}}\right)^{\alpha}. \tag{4.4}$$

From this very simple framework, it is clear that the resulting short- to medium-term effects of killings of group T (induced by radio reception) on consumption per capita and capital intensity are *a priori* unclear. On the one hand, if the capital destruction is sufficiently large and outweighs the number of group T deaths, then capital intensity and consumption per capita will decline, bringing the village further away from its steady-state level. Naturally, other mechanisms outside the model could also negatively affect output and consumption, for example if violence erodes trust which adversely affects the allocation of capital. Moreover, poverty trap models even suggest that in the worst case, capital destruction might be so large that villages are permanently condemned to low consumption.[10] On the other hand, if the capital stock of group T gets redistributed to group H and a large number of the minority group T dies (or permanently leaves the village), and the costs of conflict are low, then consumption per capita can ultimately increase after the genocide. [11]

4.4 Data

We combine several sources of data to construct a household/village-level dataset. The final dataset consists of 4,278 households in 332 villages.

RTLM Reception Our main independent variable is predicted RTLM radio coverage at the village level, taken from Yanagizawa-Drott (2014), who uses RTLM transmitter locations and a high precision topographical map of Rwanda (SRTM) to construct the data in ArcGIS. As the country is littered with hills and valleys, there is a substantial local variation in topography. Based on technical parameters of the two transmitters, such as geographic position, antenna height, transmitter power, etc., the software uses a Longley-

[10] If we assume that there is a minimum level below which consumption cannot fall and the savings rate will adjust accordingly, the above model would also allow for a poverty trap (Miguel and Roland, 2011).

[11] Outside this stylized model, per capita output and consumption might be affected if killing is selective and based on important individual characteristics that influence labor productivity, such as human capital. We investigate various mechanisms outside this framework in the empirical section.

Rice algorithm with a 90x90 meter cell precision to calculate the fraction of the village that can receive the radio signal at sufficiently high levels for normal radio sets.[12] Figure 4.1 shows a map of the radio coverage variable.[13]

Violence To show that RTLM coverage is positively correlated with genocide intensity, we use participation in violence. Since no direct measure of participation rates is available, we follow Yanagizawa-Drott (2014) and use prosecution rates for crimes committed during the genocide as a proxy. The data is taken from a nationwide village-level dataset, provided from the government agency "National Service of Gacaca Jurisdiction", which gives the outcome of the almost 10,000 Gacaca courts set up all over the country to prosecute the genocide criminals. The legal definition consists of: 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. At the village level, this mostly consists of crimes undertaken by the Interahamwe and Impuzamugambi militias.

Household Data Socio-economic household data is taken from the first wave of the Integrated Household Living Conditions Survey (EICV1),[14] conducted in 1999, 2000, and 2001 and representative at the national level. 31,192 individuals in 6,240 households in 486 villages were surveyed on various socio-economic factors regarding consumption, agricultural production, education and fertility. This data is matched by village names within communes to the RTLM reception data. Unfortunately, the matching in the RTLM data

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

[13]White areas on the map indicate an absence of data. This is either because of the presence of national parks and Lake Kivu, or because of difficulties in matching villages across datasets (see below).

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

is imperfect, as several villages either have different names in different data sources, or use alternate spelling. Moreover, sometimes two or more villages within a commune have identical names, which prevents matching. Because of these data-matching issues, the final RTLM dataset contains 1,065 or about 70 percent of the total 1,513 villages in the country. Consistently, we thus match about 70 percent of the villages in the EICV survey (332 of the total 486 villages). As most of these issues are idiosyncratic, the main implication is likely only a lower precision in our estimates.

Additional Data Population data was retrieved from the Rwanda 1991 population census provided by IPUMS International and GenoDynamics. In addition, the SRTM topography data and ArcGIS software maps allow us to calculate the village mean altitude, the village variance in altitude, distance to the border, and population density. Using satellite information from Africover, we can also measure the village centroid distance to the nearest major town and the distance to the nearest major road.

4.5 Empirical Strategy

Our identification strategy builds on two assumptions. First, villages with a high RTLM coverage experienced higher genocide violence. This is the result of Yanagizawa-Drott (2014) who used local variation in radio coverage to establish causality. Below, we reproduce these results, and provide additional empirical evidence using the household data. Second, RTLM coverage does not have a direct effect on any of the socio-economic outcomes but rather only works through genocide violence. Even though this assumption cannot be directly tested, we can provide some indirect evidence. Specifically, we test whether RTLM coverage is correlated with time-invariant or predetermined outcomes (village population in 1991, population density, village size, distance to major town, distance to major road and distance to the border) using the following specification

$$pre\_y_{jc} = \delta^0 + \delta^1 rtlm_{jc} + X_{jc}\delta^2 + \gamma_c + \epsilon_{jc}, \tag{4.5}$$

where pre_y_{jc} is a pre-genocide characteristic of village j in commune c, $rtlm_{jc}$ the share of the village with RTLM coverage, γ_c is a commune fixed effect, and ϵ_{jc} is the error term. RTLM had two transmitters in Rwanda, one located in the capital Kigali and the other on a mountain top in the northwestern part of the country. As the transmitters might have been geographically placed in a strategic manner, we include a vector of controls, X_{jc}, for second-order polynomials in distance to the transmitter, mean altitude of a village, altitude variance, latitude and longitude.[15] The identification therefore stems from highly local differences between villages within communes induced through exogenous hills in the line-of-sight of the transmitter and the village. If our RTLM coverage measure is as good as randomly assigned, we expect $\delta^1 = 0$. Reassuringly, none of the pre-genocide village characteristics is significantly correlated with RTLM coverage, given our controls (regressions 1 to 9 in Table 4.1).[16]

Two concerns still remain, however. The exclusion restriction would be violated if some other radio station, whose broadcasts possibly affect economic well-being, were to use the RTLM transmitters after the genocide or had a similar outreach to that of RTLM. This is not the case, however. First, both RTLM transmitters were destroyed at the end of the genocide, and the broadcasts stopped. Furthermore, until 2004, only Radio Rwanda was broadcasting. Radio Rwanda, also destroyed during the genocide, was reinstalled during the years 1997 to 2000 and essentially obtained national coverage, whereas RTLM's coverage was limited to the areas around the two transmitters. Thus, we should not expect their outreach to be correlated. Only in

[15] The exact technical reasons for these propagation control variables can be found in Yanagizawa-Drott (2014).

[16] The pre-genocide census from 1991 does not include any data on other socio-economic characteristics, such as income and education, at the village level.

2004 and thus *after* our sampling period do the first private radio stations go on air.

The exclusion restriction would also be violated if the RTLM broadcasts in 1994 provided information about economic issues such as fertilizer use, optimal crop circulation, health education, etc. This concern is also likely to be unwarranted. First, anecdotal evidence suggests that RTLM's broadcasts mainly involved stirring up hatred against the Tutsi minority and playing modern music. Second, to directly assess content relevant for socio-economic outcomes, we obtained and analyzed a 10 percent sample of RTLM's broadcasts and did not find any evidence that RTLM was broadcasting content that could directly affect economic performance.[17]

4.5.1 Specification

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

$$h_{jc} = \alpha + \beta rtlm_{jc} + X_{jc}\pi + \gamma_c + \epsilon_{jc}, \tag{4.6}$$

where h_{jc} is the genocide participation rate of village j in commune c, and $rtlm_{jc}$ the share of the village with RTLM coverage. X_{jc} is a vector of propagation controls, listed above, as well as the pre-genocide village characteristics used in the exogeneity check. γ_c is a commune fixed effect and ϵ_{jc} the error term.

[17]The radio tapes are retrieved online from Jake Freyer's homepage, who downloaded them from the International Criminal Tribunal for Rwanda (ICTR). The ICTR received the tapes from various sources; thus, we believe this to be a random sample. The ICTR translated about 20 percent of these tapes from Kinyarwanda into English (another 20 percent were originally in French). As the ICTR was mainly interested in finding evidence for genocidal behavior we expect, if at all, the untranslated Kinyarwanda tapes to contain broadcasts about economic or social advice to the listeners. We look for keywords such as school, income, fertilizer, education.

We then run the following reduced-form regressions

$$post\_y_{ijc} = \alpha' + \beta' rtlm_{j,c} + X_{ijc}\pi' + \gamma_c + \epsilon_{ijc}, \quad (4.7)$$

where $post\_y_{ijc}$ is the post-genocide per capita outcome of household i in village j in commune c, $rtlm_{jc}$ the share of the village with RTLM coverage, and the other independent variables are the same as before. Standard errors are clustered at the district level.

Throughout, we present the reduced-form estimates. In addition, as the station broadcasted no direct socio-economic content and the transmitters were destroyed with the end of the genocide, it is reasonable to assume that the temporary shock in exposure to radio affected later economic outcome only by inducing more violence during the genocide. Under this assumption, we also present scaled instrumental-variable estimates. To achieve the best precision, we follow Angrist and Krueger (1992) and use the full sample of 1059 villages to estimate the first-stage relationship, and a two-sample instrumental variable (TSIV) approach using the 326 villages with household data, to estimate the effect of violence on later economic outcomes.

Throughout our analysis, we will exclude villages in the capital Kigali from our sample, since these experienced extreme amounts of in and out migration. Furthermore, we always include all surveyed households in each village. However, the results are almost identical when restricting the sample to those households that experienced the genocide at their surveyed location.

4.6 Results

4.6.1 First Stage

The first-stage relationship between radio coverage and genocide violence is strongly positive at the 95 percent confidence interval (regression 1 in Table 4.2), and this relationship holds when dropping villages in the capital Kigali (regression 3) and restricting the sample to those villages surveyed in EICV1 (regressions 2 and 4), although we lose significance here because of the large reduction in sample size, from 1065 villages to 332. Regarding magnitude, the point estimate of 0.484 log points (standard error 0.232) in our preferred specification, used in our two-sample instrumental variable estimation, suggests that a village with full radio coverage has about 62 percent more perpetrators than a village with no perception or, put differently, that a one-standard deviation increase in radio coverage increases the violence by 10 percent.

Reassuringly, radio coverage is also positively and significantly related to child mortality in the household survey (regressions 5 and 6). The regressions use mothers that were present in the village during the genocide and are older than 25 years in the sample (and thus older than 19 years during the genocide).[18] Child mortality is defined as the number of dead children over the total number of children born to each mother in the regression sample. In terms of magnitude, full radio coverage increases child mortality by about 0.083 (standard error 0.035) in our specification with additional controls; given a sample mean of 0.23, this amounts to 36 percent. Furthermore, dividing the sample into girls and boys reveals that this result is driven by the boys. The point estimate for boys nearly doubles to 0.158 (standard error 0.059, regression 8), full radio coverage thus increases boy mortality by about 61 percent. The point estimates for female mortality rates are close

[18]Below, Table 4.17, we show that the results are not dependent on the exact cutoff age. In particular, the effect becomes stronger the older the women were during the genocide which seems reasonable given that younger women are more likely to get more children after the genocide and thus reduce their child mortality measure. Furthermore, we should not see any positive effects on child mortality when we only include mothers that were younger than a certain age in the regressions. Consistent with this, the effects turn insignificant and if anything negative when we restrict mothers in the sample to be younger than 15, 16, 17, 18 or 19 years at the time of the genocide (Table 4.18). The negative coefficients, although insignificant due to the small sample sizes, rather suggest that child mortality is lower in high violence villages *after* the genocide, which is consistent with our main result that households are better off.

to zero and insignificant. Given that the perpetrators mainly targeted males, this finding is consistent with the genocide producing this high mortality. Unfortunately, we do not observe adult mortality in the data.

4.6.2 Main Effects

Consumption First, and perhaps most importantly, we find that consumption is positively affected. The reduced-form effects are highly significant at the 99 percent confidence level (0.736 log points, standard error 0.237, regression 2 in Table 4.3), and the TSIV estimates imply that a 10 percent increase in violence in 1994 increased consumption per capita six years later by 13-15 percent, or approximately USD 100.[19] When breaking down the consumption goods into food, non-food and durable goods, all coefficients are positive. The overall effects are seemingly not primarily driven by food expenditures (regressions 3 and 4), but rather (and consistent with Engel's law) non-food expenditures (regressions 5 to 8): both durable goods consumption and small non-food consumption (this does, for instance, include expenses for hygiene, medicine, leisure). Other expenditures such as net-transfers to other households, schooling or festival expenses are also strongly positively related to genocide violence (regressions 9 and 10).

4.6.3 Possible Mechanisms

Next we discuss the possible mechanisms explaining the positive effect on consumption per capita.

Assets First, RTLM reception is positively and significantly related to total per capita household assets with a point estimate of 0.354 (s.e.=0.184) in our

[19]It is worth noting that when using OLS to estimate the effect of violence on the outcomes, the estimates are generally close to zero and insignificant, implying that the OLS estimates are negatively biased and suggesting that greater levels of poverty increased participation in the killings of Tutsis.

preferred specification with all controls (regression 2 in Table 4.4). Household assets are the sum of farm land, livestock assets, and durable goods. Livestock value is the sum of households' ownership of cattle, sheep, goat, pigs, rabbits and chicken, each multiplied by its price. Durable goods include assets such as radio, bicycles, cars, refrigerators or furniture. Individual regressions of the various wealth measures on radio coverage confirm the positive results: the point estimates are 0.373 for land assets (s.e.=0.163), 0.780 for livestock assets (s.e.=0.403) and 1.004 for durable goods (s.e.=0.472).

The estimates are quantitatively meaningful. Using a two-sample instrumental variable approach, we estimate the relationship between violence and total assets per capita; thus, a 10 percent increase in violence increases total assets per capita by about 7 percent, which is USD 306 of the mean. Similarly, we can estimate the corresponding point estimates for land assets (0.762), livestock (1.594) and durable goods (2.052), all reported at the bottom of Table 4.4. Thus, our results show that households living in villages that experienced higher levels of violence induced by RTLM reception own more assets six years after the genocide.

Agricultural Income Table 4.5 shows that the RTLM reception is also positively and again significantly related to farming incomes. The point estimates in our preferred specifications with all controls range from 0.526 (standard error 0.212) for total farm income (regression 2 in Table 4.5), the sum of agricultural output and livestock output minus running capital costs, such as expenses for fertilizers, transportation, fuel or fencing and external wage payments, to 0.483 (s.e.=0.204) for only agricultural income (regression 4) and are throughout significant at the 95 percent level.

The results are similar and still significant at the 90 percent level when we consider output, i.e. thus do not subtract running costs. Once more, TSIV estimates are reported at the bottom.

The estimates are also quantitatively meaningful and similar to the effects on assets. The scaled TSIV estimates imply that a 10 percent increase in violence increases the per capita income six years after the genocide by approximately 10-11 percent, corresponding to about 140 USD in the sample and output by about 8 percent.

Interpretation Our results show that households living in villages that experienced higher levels of violence induced by RTLM reception have higher living standards six years after the genocide. They own more assets per capita, such as land, livestock, and durable goods.[20] This is consistent with qualitative evidence showing that looting of the property of killed Tutsis was common (Hatzfeld, 2005). Furthermore, we find that per capita output and income from agricultural production, as well as consumption, are significantly higher in villages that exogenously experienced more violence. The effects are also quantitatively meaningful. Our estimates indicate that a 10 percent increase in violence in a village during the 1994 genocide is associated with approximately a 10-15 percent increase in per capita income and consumption among households six years afterwards.

These results are thus consistent with the Malthusian view that mass murders that reduce the population can raise the capital intensity by effectively redistributing assets, such as land, from the deceased to the survivors. This raises the living standards of the remaining population. Since the underlying process is a conflict between ethnic groups, the results are also consistent with rationalist explanations of why genocides may occur since the remaining population will tend to consist of members of the attacking group. Moreover, to the extent that the violence during the Rwandan Genocide destroyed phys-

[20]Note that these positive effects are not driven by nominal price effects (something one might be concerned about since we are considering monetary values): using Kling et al.'s (2007) method to calculate average effects, we show that RTLM reception is unrelated to prices for the six major Rwandan crops but, in contrast, positively and strong significantly associated with the corresponding crop quantities; thus, we can document a real effect (regressions 1 to 4 in Table 4.12). Similarly, radio coverage is unrelated to average livestock and durable goods prices (regressions 5 to 8). Item by item regressions confirm the average effects (Tables 4.13 to 4.16).

ical and human capital, or decreased the efficiency of markets more broadly, these effects seem to have been muted and dominated by the Malthusian mechanism.[21]

A limitation of the study is that since the genocide occurred in 1994 and the outcomes are measured six years afterwards, the estimates capture short/medium term effects. Therefore, our results do not directly speak to whether these effects will last in the very long term. However, we can use the existing data on socio-economic outcomes to further investigate the mechanisms by which the violence resulted in the effects we detect, which may also be informative about potential persistencies.

4.6.4 Additional Results and Mechanisms

Technology Adaption A natural question is whether the positive effects did not only arise because of the direct effects on productive assets, but also whether they are due to productivity increases resulting from technology adaption. One reason why we might expect such a mechanism is due to poverty trap models with credit constraints and a non-convex production function. That is, if the first-order effect of genocide is an increase in assets for the survivors, this capital injection may, in turn, facilitate investments in technologies with high fixed costs.

In Table 4.6, we investigate whether violence induced by the RTLM is associated with a higher use of irrigation, fertilizers, fuel use or transport and storage. The endogenous variables take on the value of one if the household accrued any expenses for these items. Transport and storage as well as fuel use potentially proxy for having taken a fixed cost of vehicles or mechanization. Except for irrigation which is significant at 90 percent in one specification

[21] It is important to note that the positive effects on standards of living for survivors by no means imply that violence increases welfare. After all, mass killings imply (gross) losses in welfare due to lost lives. Assessing social welfare is also a daunting task with significant philosophical challenges in what welfare criterion to use, whether one can value a human life and what the value would then be, and how to discount future income.

(regression 2), there is no evidence that technology adaption was affected. Point estimates are close to zero throughout. Furthermore, irrigation cannot solely explain our main effects since less than 1 percent of the households (mean=0.0039) have irrigation systems.

Age and Gender Composition An additional channel through which violence could have affected output per capita is that the surviving population is more productive due to age differences and, to the extent that there is a differential labor supply and productivity across gender, i.e. gender composition. Table 4.7 shows that there is no evidence that the gender composition among adults was affected by the violence. Thus, the differential mortality rates among male children shown in Table 4.2 are not mirrored among adults. The point estimates in regressions 1 to 4 do point towards relatively fewer females in high violence villages, but they are far from being significant.

Turning to the age composition, we find evidence that the violence increased the working age population share (age 13 to 49) when measured six years after the genocide. The TSIV estimates in regressions 7 to 8 imply that a 10 percent increase in genocide violence increases the working age population share by 2.2 to 2.3 percentage points.[22] This suggests that the most vulnerable, children and the elderly, were more likely to suffer deaths from the violence. Importantly, the results indicate that an explanation for the positive effects on output per capita, in addition to the increase in per capita assets, is that a higher share of the population is of working age and thus, increased the productive capacity of the typical person.

Human Capital Violence may affect human capital in at least two ways. First, the killings during the genocide may have been overrepresented among more educated adults, and relatively highly educated individuals may have migrated to the high violence villages after the genocide. Second, the violence

[22]Children aged 13 have usually finished schooling (very few go on to secondary school) and life expectancy in Rwanda at the time (and before) was around 49.

may have been disruptive for children of school age during the genocide, for example because experience of violence in the villages hampers the learning process, or because the schooling supply decreases (schools are destroyed, teachers are killed), or because the opportunity costs of education increase (e.g., the returns to child labor are higher because households own more land assets). We proxy for human capital by years of schooling, as is standard in the literature. For children, we also have measures of cognitive skills based on survey test results of reading, writing and simple math. Furthermore, we consider the effects on three distinct age groups: children that were below primary school age during the genocide, children of primary school age and children of secondary school age.

We find negative effects on cognitive skills among children. First, there is some evidence that children of primary school age during the genocide living in high violence villages have fewer years of schooling, as the coefficient is negative with a p-value of 0.105 (regression 1 in Table 4.8). Regressions 2 to 4 show highly significant estimates and display that cognitive skills are adversely affected, as these children are less likely to be able to read, write and do simple math. The effects are quantitatively substantial, as the TSIV estimates imply that a 10 percent increase in violence decreases the likelihood of being able to read by 3.5 percentage points, the ability to write by 4.3 percentage points, and the ability to do simple math by 4.9 percentage points. Secondary schooling cohorts seem unaffected, point estimates are all insignificant and close to zero. Finally, we find no differences in human capital for young cohorts that were below primary school age at the time of the genocide, ruling out that the genocide had persistent effects on schooling supply, for example through missing teachers or destroyed schools.

However, the loss in human capital for the primary school cohort seems to be persistent: the point estimates are very similar to those we obtain above when we restrict the sample to those children who are currently out of school (regressions 5-8 in Table 4.8). Thus, it seems unlikely that these children (young adults) will catch up. Since the schooling supply side does not seem to be driving this result, one alternative explanation, consistent with our findings for wealth and income above, is that the genocide temporarily prevented children from going to school; however, once the killings were over, it became more attractive to work in agriculture rather than return to school since land and capital intensity had increased. However, we find no evidence that human capital among adults is affected (regressions 1 to 4 in Table 4.9). The point estimates are small and highly insignificant.

The negative effects on human capital among children suggest that affected cohorts will be relatively less productive as adults, with negative implications for future income. Although speculative, this mechanism may counteract the positive effects estimated in the short term, as the effects on income and consumption may be transitory and not persist in the longer run.

Population and Fertility Finally, since a first-order effect of political mass killings is the reduction in population size, it is natural to investigate how migration and fertility are affected. First, since there is a demonstrated positive effect on land assets, if the fixed costs of migration are low, one might expect individuals from low violence areas to move into high violence areas because the returns to labor are higher. We use a special community survey attached to the EICV1 survey which includes a question about whether communities saw their population growing after the genocide to investigate this possibility. There is no strong evidence pointing towards this mechanism. The point estimates are positive (regressions 1 and 2 in Table 4.10), but statistically insignificant. This suggests that fixed costs of migration may be non-trivial.

On the other hand, we find strong evidence for fertility increases: radio coverage is positively and statistically significant at the 95 percent level, associated with the total number of children per young woman (age 13 to 29). We only measure births conceived after the genocide in order to exclude involuntary births through rape. The point estimates imply that a 10 percent increase in violence increases fertility by approximately 11 percent. We do not find any significant effects for the two older cohorts, women between 30 and 39 or between 40 and 49, respectively (regressions 3 and 4).

It is interesting to note that we can rule out that young mothers in high violence villages are simply "replacing" those children lost during the genocide. The point estimates are robust or even somewhat larger when we restrict the sample of women to those who did not suffer from any child death (regressions 5 and 6). The point estimates for women between 30 and 39 also increase in magnitude. Furthermore, differences in school attendance among young women are also unlikely to drive the results: point estimates become even stronger when we drop those women who are currently enrolled in school (regressions 7 and 8).

These results are interesting in their own right, but also informative about the potential persistence of the positive effects on per capita assets, income and consumption. If a key reason for the short-term effects was an increase in capital intensity, then higher fertility rates would tend to suppress these effects over time. Together with the effects on the age distribution and lower human capital among children, it seems likely that the positive effects are likely to be muted over time, potentially disappear and, in the extreme case, turn negative.

Post-Conflict Reconstruction Furthermore, we can also rule out that post-conflict reconstruction or assistance to survivors by the central government or some NGO is driving the positive results. First, communities with high levels of violence are not more likely to report government funded infrastructure construction after the genocide, such as schools, clinics, roads, bridges, water sources or social housing (regressions 1 to 12 in Table 4.11). The insignificant result for the last point, social housing or Imidugudu, might seem surprising since that program was explicitly designed as an emergency housing project to help those adversely affected by the genocide. However, by the time of its implementation, it was redefined as a general development program benefiting all households. Moreover, the consensus seems to be that it was *"implemented hastily and in a rather disorderly manner."* (Isaksson, 2011, p. 5) and that there was no systematic selection of Imidugudu sites or dwellers (RISD, 1999).

Second, we can also identify those children in the sample who were supported by a scholarship. Since these scholarships were often given to genocide survivors, e.g. FARG (Fonds d'Assistance aux Rescapes du Genocide) supported some 24,000 students after the genocide, they might bias our results. However, in our sample, only about 1 percent of the households have at least one child with a scholarship and all our results are unaffected by dropping those households (or individuals in the human capital regressions). Furthermore, we do not find any evidence that whether a child obtains a scholarship is related to radio coverage (results not shown).

Selective Killings or Migration In principle, neither selective killings nor selective migration should bias our main results. To see this, consider a village with one rich villager owning assets R and one poor villager owning assets P. If the rich villager is killed or leaves, then the poor villager will own assets P+R. If the poor villager is killed or leaves, the rich villager will now own assets P+R. Thus, from a per capita perspective, both cases look the same. However, more realistically it might be that non-transferable assets such as human capital are of importance.

To shed some light on this, recall our result from above, that years of schooling among adults are unrelated to radio coverage (regressions 1 to 4 in Table 4.9). This suggests that selective killings or migration did not happen. Note that adults had finished their education by the time of the genocide and any differences in schooling will therefore reflect selection effects, rather than direct effects on human capital as is the case for children. Furthermore, as noted above, we find very similar results when restricting the sample to households who resided in a given village during the time of the genocide. This suggests that selective migration into villages from, say Uganda, did not matter and furthermore that selective out migration did not matter. Furthermore, for selective out migration to bias our results, we would need poorer households to leave high violence villages. However, most empirical work on migration usually finds that richer households are the first to leave since they can afford it.

4.7 Discussion and Conclusion

To the best of our knowledge, this paper is the first to demonstrate that conflict in general, and political mass murder in particular, can have positive effects on economic performance. Our results show that households living in villages that experienced higher levels of violence induced by RTLM reception have higher living standards six years after the genocide. Specifically, they own more land assets, livestock, durable goods and total assets per capita. Furthermore, we find that per capita output and income from agricultural production, as well as consumption, are significantly higher in villages that exogenously experienced more violence. These effects are also quantitatively meaningful. Our estimates indicate that a 10 percent increase in violence in a village during the 1994 genocide is associated with approximately a 10-15 percent increase in per capita income and consumption among households six years afterwards. Although our main results show that political mass murders can have positive effects on economic performance in the short to medium run, we find additional evidence on age distribution, human capital and fertility, which indicates that these effects are likely to be temporary and perhaps disappear in the long run.

In light of these findings, one should be cautious when generalizing the effects. Important heterogeneities are expected. For example, like many developing countries, the Rwandan economy is overwhelmingly agrarian, where land assets play a particularly prominent role. The positive effects on output are therefore less surprising in this context. By contrast, Acemoglu et al. (2011) find that the persecution of the Jewish population in Russia during the Holocaust had long-lasting negative effects on economic performance. In this case, the loss in human capital is likely to dominate any effect of a population decrease. In an agricultural environment such as Rwanda, human capital might not be as important as other factors of production, and the differences in human capital between Tutsi and the general population in Rwanda were arguably of an order of magnitude smaller than the difference between the Jewish and the general population in Russia. These two contrasting cases highlight that there are no good reasons to believe that the effects of political mass killings must be homogeneous and that the living standards are expected to increase in all contexts. Further theoretical and empirical research that can shed some light on the conditions determining the economic effects of political mass murders would be useful, not to mention the potentially negative effects on mental and physical health, and the social fabric of societies.

The reader may wonder whether the muddy-roads instrument (the militia's transport costs to reach each village) used in Rogall (2014) could be used as an alternative source of exogenous variation in the amount of violence. This is indeed a natural idea that we intend to pursue in future research. Preliminary results, relying on this alternative instrument, corroborate the positive estimates reported in this version of the paper.

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Tables and Figures

Table 4.1: Exogeneity check

	Population in 1991, log	Population Density in 1991, log	Distance to Major Town, log	Distance to Major Road, log	Distance to the Border, log	North Sloping	East Sloping	South Sloping	West Sloping
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Radio Coverage in Village	−0.194	−0.115	0.024	−0.247	0.068	0.088	−0.118	−0.034	0.065
	(0.277)	(0.577)	(0.127)	(0.341)	(0.356)	(0.408)	(0.270)	(0.326)	(0.366)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.66	0.60	0.96	0.80	0.95	0.41	0.39	0.37	0.43
N	332	332	332	332	332	332	332	332	332

Note: Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Figure 4.1: Rwandan Villages, Radio Coverage.

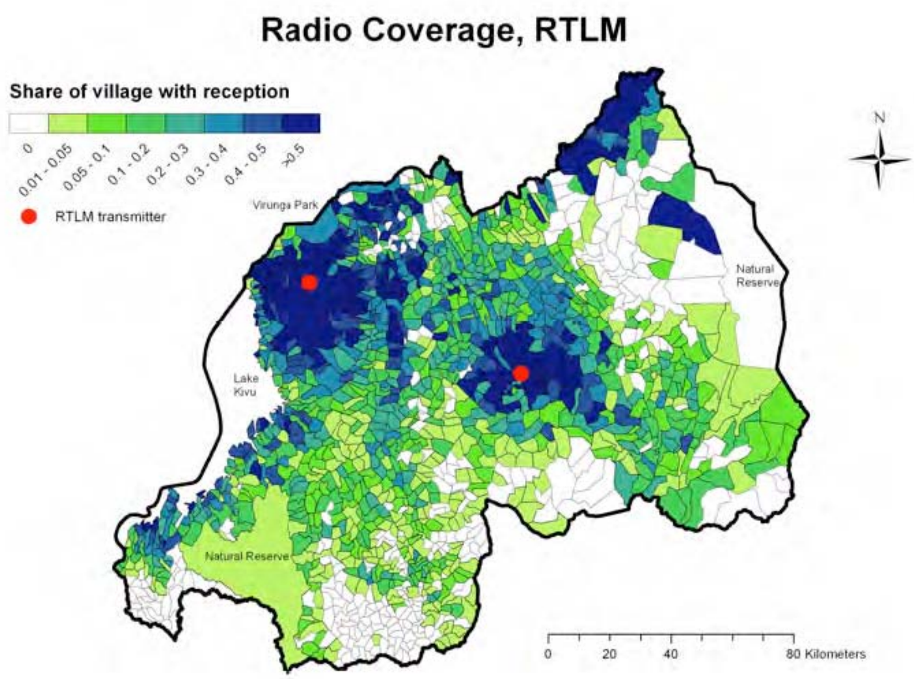

Table 4.2: First Stage

	Full sample		No Kigali		In village during Genocide					
	Genocide Violence				Mortality		Male Mortality		Female Mortality	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Radio Coverage in Village	0.545	0.708	0.484	0.496	0.088	0.083	0.144	0.158	−0.016	−0.034
	(0.229)**	(0.830)	(0.232)**	(0.880)	(0.039)**	(0.035)**	(0.053)**	(0.059)**	(0.058)	(0.064)
Population 1991, log	0.589	0.468	0.591	0.449		−0.006		0.028		−0.048
	(0.148)***	(0.284)	(0.150)***	(0.293)		(0.019)		(0.028)		(0.020)**
Population density 1991, log	0.004	0.227	0.010	0.268		0.003		0.006		0.011
	(0.113)	(0.167)	(0.115)	(0.182)		(0.011)		(0.013)		(0.015)
Distance to Major Town, log	−0.233	−0.367	−0.218	−0.285		−0.016		−0.050		0.007
	(0.161)	(0.519)	(0.160)	(0.556)		(0.032)		(0.042)		(0.054)
Distance to Major Road, log	−0.245	−0.273	−0.237	−0.239		0.032		0.053		0.026
	(0.100)**	(0.158)*	(0.100)**	(0.161)		(0.015)**		(0.022)**		(0.016)
Distance to Border, log	0.030	−0.360	0.030	−0.349		0.051		0.052		0.041
	(0.122)	(0.330)	(0.121)	(0.320)		(0.025)*		(0.028)*		(0.045)
North Sloping, dummy	0.041	0.143	0.035	0.113		−0.005		0.015		−0.020
	(0.106)	(0.239)	(0.106)	(0.245)		(0.026)		(0.031)		(0.032)
East Sloping, dummy	0.098	0.058	0.102	0.054		−0.015		−0.011		−0.010
	(0.076)	(0.210)	(0.077)	(0.213)		(0.017)		(0.028)		(0.020)
South Sloping, dummy	−0.028	0.067	−0.025	0.078		−0.035		−0.033		−0.036
	(0.122)	(0.227)	(0.122)	(0.227)		(0.022)		(0.028)		(0.025)
Rural Household, dummy					0.067	0.044	0.105	0.083	0.032	0.001
					(0.035)*	(0.027)	(0.055)*	(0.050)	(0.068)	(0.054)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean					0.23	0.23	0.26	0.26	0.21	0.21
R^2	0.55	0.72	0.55	0.73	0.09	0.09	0.09	0.09	0.09	0.09
N	1065	332	1059	326	2009	2009	1844	1844	1848	1848

Note: Genocide Violence is measured as the number of people prosecuted for genocide violence in the Gacaca courts, category 1 normalized by village population. Mortality is measured as the number of dead children (boys/girls) over the number of total children per mother (boys/girls). Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Regression 1 uses the full sample of villages from Yanagizawa-Drott (2014). Regression 2 uses that subset which overlaps with the EICV1 Survey. Regressions 3 and 4 exclude Kigali. Regressions 5-10 use women (from EICV1 Survey) who are older than 25 and who resided in the surveyed village during the genocide. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.3: Consumption

	Total Log		Food Log		Non-Food Log		Durable Goods Log		Other Expenses Log	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Radio Coverage in Village	0.643	0.736	0.196	0.248	0.663	0.905	0.763	0.920	0.552	0.755
	(0.242)**	(0.237)***	(0.157)	(0.116)**	(0.647)	(0.612)	(0.245)***	(0.275)***	(0.308)*	(0.343)**
Distance to Major Town, log		−0.331		−0.210		−0.695		−0.363		−0.444
		(0.410)		(0.201)		(0.630)		(0.496)		(0.684)
Distance to Major Road, log		−0.206		−0.155		−0.337		−0.197		0.103
		(0.070)***		(0.056)**		(0.134)**		(0.101)*		(0.103)
Distance to Border, log		−0.235		−0.235		−0.473		−0.171		0.171
		(0.093)**		(0.100)**		(0.236)*		(0.141)		(0.156)
Population 1991, log		0.010		−0.009		0.065		0.067		0.352
		(0.096)		(0.069)		(0.188)		(0.111)		(0.155)**
Population density 1991, log		−0.032		−0.041		−0.052		−0.046		0.002
		(0.088)		(0.054)		(0.124)		(0.112)		(0.122)
North Sloping, dummy		0.152		0.051		0.361		0.119		0.217
		(0.090)		(0.049)		(0.126)***		(0.152)		(0.120)*
East Sloping, dummy		0.250		0.099		0.661		0.162		0.284
		(0.072)***		(0.040)**		(0.130)***		(0.127)		(0.147)*
South Sloping, dummy		0.166		0.049		0.482		0.168		0.238
		(0.103)		(0.055)		(0.170)***		(0.134)		(0.092)**
Rural Household, dummy	−0.805	−0.585	−0.571	−0.428	−1.755	−1.355	−0.643	−0.417	−0.278	−0.131
	(0.184)***	(0.167)***	(0.132)***	(0.122)***	(0.289)***	(0.271)***	(0.276)**	(0.235)*	(0.283)	(0.305)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	630.63	630.63	121.82	121.82	216.11	216.11	346.80	346.80	227.72	227.72
R^2	0.21	0.22	0.18	0.19	0.18	0.20	0.16	0.17	0.13	0.14
N	4039	4039	4039	4039	3872	3872	3513	3513	2783	2783
TSIV estimate	1.310	1.504	0.399	0.507	1.350	1.849	1.554	1.880	1.123	1.544
	(0.492)**	(0.485)***	(0.321)	(0.237)**	(1.319)	(1.251)	(0.499)***	(0.563)***	(0.628)*	(0.701)**

Note: All dependent variables are in logged per capita monetary values. Per capita refers to the consumption of the household, divided by the number of persons living in the household. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.4: Assets

	Total Assets Log		Land Assets Log		Livestock Log		Durable Goods, Log	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage in Village	0.479	0.354	0.471	0.373	0.793	0.780	0.703	1.004
	(0.265)*	(0.184)*	(0.199)**	(0.163)**	(0.362)**	(0.403)*	(0.536)	(0.472)**
Distance to Major Town, log		−0.001		0.201		0.065		−0.894
		(0.221)		(0.180)		(0.292)		(0.505)*
Distance to Major Road, log		0.073		0.003		−0.037		−0.472
		(0.076)		(0.067)		(0.085)		(0.129)***
Distance to Border, log		0.258		0.226		0.267		−0.049
		(0.097)**		(0.086)**		(0.213)		(0.225)
Population 1991, log		−0.073		−0.073		−0.104		0.282
		(0.109)		(0.099)		(0.148)		(0.162)*
Population density 1991, log		−0.110		−0.062		0.041		0.064
		(0.060)*		(0.049)		(0.101)		(0.129)
North Sloping, dummy		0.049		0.083		0.038		0.165
		(0.094)		(0.087)		(0.175)		(0.143)
East Sloping, dummy		0.062		0.062		−0.026		0.172
		(0.088)		(0.085)		(0.106)		(0.169)
South Sloping, dummy		−0.020		0.041		0.020		0.180
		(0.102)		(0.094)		(0.127)		(0.149)
Rural Household, dummy	0.219	0.079	−0.328	−0.427	−0.135	−0.105	−2.179	−1.457
	(0.188)	(0.225)	(0.190)*	(0.221)*	(0.310)	(0.293)	(0.478)***	(0.427)***
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	4224.00	4224.00	3828.50	3828.50	698.42	698.42	157.80	157.80
R^2	0.12	0.13	0.13	0.14	0.13	0.13	0.23	0.24
N	4027	4027	3847	3847	2451	2451	3612	3612
TSIV estimate	0.976	0.724	0.960	0.762	1.614	1.594	1.432	2.052
	(0.539)*	(0.376)*	(0.404)**	(0.333)**	(0.737)**	(0.824)*	(1.093)	(0.964)**

Note: All dependent variables are in logged per capita monetary values. Per capita refers to the assets of the household, divided by the number of persons living in the household. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.5: Agricultural Income and Output

	Income, Log				Output, Log			
	Livestock & Agriculture		Agriculture		Livestock & Agriculture		Agriculture	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage in Village	0.546	0.526	0.500	0.483	0.408	0.397	0.380	0.374
	(0.197)**	(0.212)**	(0.193)**	(0.204)**	(0.198)*	(0.212)*	(0.188)*	(0.202)*
Distance to Major Town, log		0.242		0.204		0.160		0.149
		(0.203)		(0.203)		(0.196)		(0.197)
Distance to Major Road, log		−0.040		−0.025		−0.034		−0.030
		(0.085)		(0.088)		(0.079)		(0.083)
Distance to Border, log		−0.238		−0.227		−0.154		−0.159
		(0.123)*		(0.121)*		(0.112)		(0.109)
Population 1991, log		−0.183		−0.177		−0.144		−0.152
		(0.137)		(0.136)		(0.136)		(0.148)
Population density 1991, log		0.078		0.070		0.068		0.079
		(0.070)		(0.067)		(0.071)		(0.078)
North Sloping, dummy		0.122		0.113		0.101		0.099
		(0.112)		(0.112)		(0.106)		(0.105)
East Sloping, dummy		0.099		0.097		0.085		0.075
		(0.097)		(0.099)		(0.088)		(0.089)
South Sloping, dummy		0.138		0.131		0.151		0.146
		(0.120)		(0.122)		(0.119)		(0.120)
Rural Household, dummy	0.223	0.181	0.262	0.217	0.193	0.170	0.250	0.229
	(0.391)	(0.438)	(0.383)	(0.423)	(0.354)	(0.403)	(0.354)	(0.398)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	1388.75	1388.75	1363.71	1363.71	1467.62	1467.62	1417.70	1417.70
R^2	0.23	0.23	0.24	0.24	0.24	0.24	0.26	0.26
N	3775	3775	3783	3783	3833	3833	3833	3833
TSIV estimate	1.112	1.076	1.018	0.988	0.830	0.812	0.775	0.764
	(0.400)**	(0.433)**	(0.394)**	(0.416)**	(0.403)*	(0.434)*	(0.383)*	(0.412)*

Note: All dependent variables are in logged per capita monetary values. Per capita refers to the output or income of the household, divided by the number of persons living in the household. Income is defined as output minus running costs. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.6: Technology Adaption

	Irrigation, dummy		Fertilizer Use, dummy		Diesel & Oil		Transport & Storage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage in Village	0.015	0.016	−0.068	−0.080	−0.001	−0.002	0.095	0.119
	(0.009)	(0.009)*	(0.075)	(0.083)	(0.001)	(0.002)	(0.082)	(0.078)
Distance to Major Town, log		0.002		0.054		0.006		−0.052
		(0.009)		(0.111)		(0.005)		(0.034)
Distance to Major Road, log		0.002		−0.035		−0.001		−0.013
		(0.005)		(0.019)*		(0.001)		(0.014)
Distance to Border, log		0.004		−0.003		0.001		−0.027
		(0.004)		(0.046)		(0.001)		(0.031)
Population 1991, log		−0.005		−0.018		−0.002		0.048
		(0.004)		(0.033)		(0.002)		(0.020)**
Population density 1991, log		0.006		0.007		−0.000		−0.002
		(0.003)*		(0.022)		(0.000)		(0.013)
North Sloping, dummy		−0.004		0.001		−0.000		−0.007
		(0.003)		(0.030)		(0.000)		(0.018)
East Sloping, dummy		0.001		0.008		0.001		0.002
		(0.002)		(0.031)		(0.001)		(0.016)
South Sloping, dummy		0.002		−0.002		−0.001		0.013
		(0.002)		(0.028)		(0.001)		(0.017)
Rural Household, dummy	0.001	0.001	0.034	0.045	0.001	0.001	0.011	0.042
	(0.002)	(0.003)	(0.046)	(0.059)	(0.001)	(0.001)	(0.043)	(0.040)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.0039	0.0039	0.1244	0.1244	0.0003	0.0003	0.0556	0.0556
R^2	0.09	0.09	0.14	0.14	0.02	0.03	0.06	0.06
N	3833	3833	3833	3833	3833	3833	3833	3833
TSIV estimate	0.031	0.033	−0.141	−0.165	−0.001	−0.005	0.197	0.246
	(0.019)	(0.019)*	(0.156)	(0.171)	(0.002)	(0.005)	(0.169)	(0.160)

Note: All dependent variables are dummies indicating whether the household accrued the corresponding costs (e.g. irrigation costs in regressions 1 and 2). Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.7: Age and Gender Composition

	Fraction Male		Female Head		Age of Head		Fraction Age: 13-49		Fraction Male Age: 13-49	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Radio Coverage in Village	0.055	0.061	−0.030	−0.051	−3.851	−4.869	0.109	0.112	0.106	0.107
	(0.060)	(0.051)	(0.145)	(0.126)	(2.480)	(2.628)*	(0.033)***	(0.033)***	(0.053)*	(0.049)**
Distance to Major Town, log		0.009		0.029		2.622		−0.040		0.000
		(0.019)		(0.051)		(1.644)		(0.031)		(0.023)
Distance to Major Road, log		−0.011		0.008		−1.106		−0.007		−0.011
		(0.013)		(0.019)		(0.822)		(0.009)		(0.011)
Distance to Border, log		−0.037		0.033		1.085		−0.012		−0.014
		(0.014)**		(0.039)		(1.387)		(0.024)		(0.022)
Population 1991, log		−0.008		−0.036		−0.911		0.002		−0.003
		(0.012)		(0.032)		(0.841)		(0.010)		(0.011)
Population density 1991, log		0.010		0.014		0.161		−0.008		−0.001
		(0.010)		(0.021)		(0.603)		(0.007)		(0.008)
North Sloping, dummy		−0.001		0.032		−0.096		0.013		0.002
		(0.017)		(0.024)		(0.935)		(0.011)		(0.012)
East Sloping, dummy		0.001		−0.034		−1.144		0.027		0.011
		(0.015)		(0.028)		(0.948)		(0.014)*		(0.011)
South Sloping, dummy		0.009		−0.000		−0.335		−0.002		0.006
		(0.014)		(0.026)		(0.813)		(0.011)		(0.011)
Rural Household, dummy	−0.048	−0.040	0.088	0.069	3.567	3.565	−0.064	−0.053	−0.061	−0.054
	(0.027)*	(0.026)	(0.060)	(0.044)	(1.207)***	(1.731)*	(0.023)***	(0.025)**	(0.025)**	(0.022)**
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.46	0.46	0.32	0.32	44.46	44.46	0.53	0.53	0.24	0.24
R^2	0.04	0.04	0.05	0.06	0.05	0.05	0.07	0.07	0.05	0.05
N	4039	4039	4039	4039	4039	4039	4039	4039	4039	4039
TSIV estimate	0.113	0.127	−0.062	−0.105	−7.957	−10.060	0.225	0.232	0.219	0.220
	(0.124)	(0.106)	(0.300)	(0.259)	(5.123)	(5.430)*	(0.068)***	(0.068)***	(0.109)*	(0.101)**

Note: All fractions correspond to the household level, e.g. regressions 9 and 10 use the fraction of household members who are male and between 13 and 49 years old. Female head is a dummy variable. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.8: Human Capital, All School Age Children

	All Children				Children out of School			
	Years of Schooling	Ability to Read	Ability to Write	Ability to do Maths	Years of Schooling	Ability to Read	Ability to Write	Ability to do Maths
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage x Under Primary School Age	0.136	0.004	−0.002	−0.030	−0.254	−0.132	−0.095	−0.121
	(0.289)	(0.099)	(0.098)	(0.066)	(0.482)	(0.084)	(0.068)	(0.075)
Radio Coverage x Primary School Age	−0.734	−0.172	−0.209	−0.237	−0.777	−0.288	−0.307	−0.297
	(0.438)	(0.067)**	(0.068)***	(0.080)***	(0.533)	(0.094)***	(0.095)***	(0.104)***
Radio Coverage x Secondary School Age	−0.011	−0.077	−0.022	−0.080	0.590	−0.037	0.006	−0.042
	(0.853)	(0.122)	(0.118)	(0.132)	(0.578)	(0.104)	(0.108)	(0.112)
Age	0.316	0.054	0.053	0.053	0.258	0.033	0.033	0.030
	(0.018)***	(0.004)***	(0.004)***	(0.004)***	(0.025)***	(0.006)***	(0.006)***	(0.006)***
Father's Schooling	0.148	0.015	0.014	0.014	0.150	0.019	0.018	0.018
	(0.010)***	(0.002)***	(0.003)***	(0.003)***	(0.016)***	(0.003)***	(0.003)***	(0.003)***
Mother's Schooling	0.113	0.008	0.010	0.010	0.125	0.012	0.014	0.014
	(0.011)***	(0.003)**	(0.003)***	(0.003)***	(0.019)***	(0.004)**	(0.004)***	(0.005)***
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune x Age Group Effects	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	3.15	0.51	0.48	0.53	2.93	0.51	0.48	0.51
Dep. Mean: Primary School	3.66	0.65	0.62	0.66	2.80	0.51	0.49	0.53
R^2	0.46	0.28	0.29	0.26	0.46	0.35	0.35	0.36
N	8190	8190	8190	8190	4180	4180	4180	4180
TSIV estimates								
Genocide Violence x Under Primary School Age	0.281	0.007	−0.005	−0.061	−0.525	−0.272	−0.196	−0.251
	(0.596)	(0.205)	(0.202)	(0.136)	(0.997)	(0.174)	(0.140)	(0.155)
Genocide Violence x Primary School Age	−1.517	−0.356	−0.432	−0.489	−1.605	−0.596	−0.634	−0.614
	(0.906)	(0.139)**	(0.140)***	(0.165)***	(1.101)	(0.194)***	(0.196)***	(0.214)***
Genocide Violence x Secondary School Age	−0.023	−0.159	−0.045	−0.166	1.220	−0.077	0.013	−0.086
	(1.762)	(0.252)	(0.245)	(0.274)	(1.194)	(0.215)	(0.223)	(0.231)

Note: The school age of the interaction terms refers to the age during the 1994 Rwandan Genocide. The cognitive skills outcomes are dummy variables. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Additional Controls include distance to the road, distance to the border, distance to major city, population and population density, and sloping dummies as well as a dummy for rural areas. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.9: Human Capital, Adults

	Years of Schooling	Ability to Read	Ability to Write	Ability to do Maths
	(1)	(2)	(3)	(4)
Radio Coverage in Village	0.298	−0.004	−0.011	−0.030
	(0.576)	(0.077)	(0.091)	(0.085)
Age	−0.058***	−0.009***	−0.008***	−0.009***
	(0.003)	(0.001)	(0.001)	(0.001)
Father's Schooling	0.359***	0.022***	0.026***	0.024***
	(0.028)	(0.005)	(0.005)	(0.005)
Mother's Schooling	0.168***	−0.006	−0.006	−0.005
	(0.033)	(0.005)	(0.005)	(0.005)
Propagation Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes
Dep. Mean	2.90	0.47	0.44	0.46
R^2	0.35	0.13	0.13	0.13
N	5710	5710	5710	5710
TSIV estimate	0.616	−0.009	−0.023	−0.061
	(1.190)	(0.160)	(0.188)	(0.176)

Note: The cognitive skills outcomes are dummy variables. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Additional Controls include distance to the road, distance to the border, distance to major city, population and population density, and sloping dummies as well as a dummy for rural areas. The regressions use adults, thus older than 24 years. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.10: Population and Fertility

	Growing Pop., dummy		Dependent variable: Number of Children (born at least 1 year after the genocide)					
					Women who never lost a child			
			All women		All women		Out of School	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage in Village	0.213	0.181						
	(0.370)	(0.355)						
Radio Coverage x Women (13-29)			0.181	0.184	0.204	0.203	0.269	0.269
			(0.076)**	(0.069)**	(0.066)***	(0.062)***	(0.085)***	(0.077)***
Radio Coverage x Women (30-39)			0.011	0.001	0.343	0.305	0.394	0.343
			(0.231)	(0.214)	(0.182)*	(0.184)	(0.201)*	(0.209)
Radio Coverage x Women (40-49)			−0.101	−0.101	−0.018	−0.062	−0.013	−0.064
			(0.155)	(0.171)	(0.587)	(0.588)	(0.594)	(0.597)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	no	yes	no	yes	no	yes	no	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
Commune x Age Effects	no	no	yes	yes	yes	yes	yes	yes
Dep. Mean: Women (13-29)			0.33	0.33	0.28	0.28	0.37	0.37
Dep. Mean	0.37	0.37	0.47	0.47	0.39	0.39	0.47	0.47
R^2	0.52	0.54	0.38	0.38	0.43	0.43	0.40	0.41
N	310	310	5825	5825	4286	4286	3510	3510
TSIV estimates								
Genocide Violence	0.440	0.374						
	(0.764)	(0.734)						
Genocide Violence x Women (13-29)			0.373	0.381	0.422	0.419	0.557	0.556
			(0.156)**	(0.143)**	(0.137)***	(0.127)***	(0.175)***	(0.160)***
Genocide Violence x Women (30-39)			0.022	0.003	0.708	0.631	0.815	0.708
			(0.477)	(0.442)	(0.375)*	(0.381)	(0.415)*	(0.432)
Genocide Violence x Women (40-49)			−0.209	−0.210	−0.038	−0.127	−0.026	−0.133
			(0.321)	(0.354)	(1.214)	(1.216)	(1.228)	(1.234)

Note: The dependent variable in regressions 1 and 2 is a dummy indicating whether community population increased after the genocide. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Additional Controls include distance to the road, distance to the border, distance to major city, population and population density, and sloping dummies. Age and a dummy for rural areas are also controlled for. Women's age in the interaction term refer to the age during the sample period. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.11: Public Goods, constructed after Genocide

	Schools		Clinics		Bridges		Roads		Water Sources		Imidugudu	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Radio Coverage in Village	−0.107	−0.108	−0.006	−0.004	0.007	0.029	−0.036	−0.046	0.122	0.114	−0.073	−0.041
	(0.245)	(0.230)	(0.034)	(0.036)	(0.087)	(0.068)	(0.059)	(0.066)	(0.187)	(0.194)	(0.158)	(0.182)
Distance to Major Town, log		0.151		−0.004		0.003		−0.027		−0.023		−0.299
		(0.159)		(0.023)		(0.065)		(0.064)		(0.124)		(0.153)*
Distance to Major Road, log		0.058		−0.003		−0.021		−0.013		−0.013		−0.001
		(0.057)		(0.013)		(0.032)		(0.028)		(0.053)		(0.069)
Distance to Border, log		0.113		−0.008		0.020		−0.001		−0.155		0.032
		(0.083)		(0.026)		(0.048)		(0.022)		(0.124)		(0.120)
Population 1991, log		0.004		0.022		0.100		0.012		−0.024		0.108
		(0.084)		(0.022)		(0.075)		(0.030)		(0.103)		(0.062)*
Population density 1991, log		0.005		−0.011		0.003		−0.031		0.014		−0.050
		(0.040)		(0.011)		(0.021)		(0.022)		(0.075)		(0.044)
North Sloping, dummy		−0.018		0.029		0.012		−0.028		0.027		0.012
		(0.064)		(0.021)		(0.039)		(0.027)		(0.068)		(0.104)
East Sloping, dummy		0.024		−0.003		0.054		0.013		0.036		−0.065
		(0.082)		(0.010)		(0.027)*		(0.050)		(0.064)		(0.092)
South Sloping, dummy		0.071		0.012		0.020		−0.023		0.072		−0.144
		(0.096)		(0.020)		(0.019)		(0.033)		(0.094)		(0.070)*
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	0.10	0.10	0.01	0.01	0.02	0.02	0.02	0.02	0.09	0.09	0.10	0.10
R^2	0.44	0.45	0.33	0.34	0.44	0.49	0.38	0.40	0.57	0.58	0.52	0.56
N	310	310	310	310	310	310	310	310	310	310	310	310
TSIV estimate	−0.222	−0.224	−0.012	−0.008	0.015	0.061	−0.075	−0.096	0.253	0.236	−0.151	−0.086
	(0.506)	(0.475)	(0.070)	(0.074)	(0.180)	(0.140)	(0.121)	(0.137)	(0.387)	(0.400)	(0.327)	(0.375)

Note: All dependent variables are dummies indicating whether the corresponding public good (e.g. schools in regressions 1 and 2) was built in the community after the genocide (funded by the government or NGOs). Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.12: Prices and Quantities, Average Effects

	Crop Quantities, log		Crop Prices, log		Livestock Prices, log		Durable Goods Prices, log	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage in Village	0.448	0.477	−0.304	−0.272	0.226	0.242	−0.416	−0.547
	(0.183)**	(0.172)***	(0.272)	(0.210)	(0.186)	(0.185)	(0.475)	(0.560)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	no	yes	no	yes	no	yes	no	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
N	15397	15397	9357	9357	2411	2411	1136	1136

Note: The dependent variable in regressions 1 and 2 is measured in log kilograms per household and includes the six major crops in Rwanda. Livestock prices are from cattle, pigs, sheep, goats, chicken and rabbits. Durable goods prices are from radios, beds, chairs and lamps. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Additional Controls include distance to the road, distance to the border, distance to major city, population and population density, and sloping dummies as well as a dummy for rural areas. The coefficients represent the average effects of radio coverage on various prices and crop quantities, respectively (Kling et al., 2007). Standard errors in parentheses, clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.13: Livestock Prices

	Cattle	Sheep	Goat	Pig	Rabbit	Chicken	Livestock Value pc. (1,3,5,6)	Livestock Value pc. (2,4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage in Village	0.061	0.292	0.058	0.420	−0.061	0.015	0.935	0.348
	(0.696)	(0.169)*	(0.237)	(0.478)	(0.381)	(0.167)	(0.434)**	(0.408)
Distance to Major Town, log	−0.046	0.384	−0.103	0.157	0.201	0.074	0.269	0.075
	(0.232)	(0.231)	(0.086)	(0.302)	(0.302)	(0.100)	(0.329)	(0.224)
Distance to Major Road, log	0.080	0.078	0.009	−0.069	0.003	−0.052	−0.030	−0.085
	(0.063)	(0.079)	(0.031)	(0.179)	(0.116)	(0.034)	(0.082)	(0.140)
Distance to Border, log	−0.155	−0.499	−0.005	0.106	−0.354	−0.011	0.370	−0.294
	(0.226)	(0.145)***	(0.067)	(0.268)	(0.288)	(0.083)	(0.224)	(0.258)
Population 1991, log	0.062	−0.035	−0.000	−0.138	−0.215	0.036	−0.144	0.084
	(0.138)	(0.122)	(0.045)	(0.138)	(0.179)	(0.061)	(0.139)	(0.205)
Population density 1991, log	0.199	0.188	−0.030	0.102	0.215	−0.005	0.075	0.060
	(0.047)***	(0.061)***	(0.030)	(0.144)	(0.143)	(0.022)	(0.079)	(0.117)
North Sloping, dummy	−0.063	0.014	−0.013	0.065	0.316	−0.023	0.032	0.174
	(0.202)	(0.140)	(0.049)	(0.157)	(0.136)**	(0.053)	(0.179)	(0.189)
East Sloping, dummy	0.010	0.057	0.087	−0.170	0.434	0.028	0.066	0.173
	(0.111)	(0.119)	(0.047)*	(0.200)	(0.132)***	(0.057)	(0.125)	(0.146)
South Sloping, dummy	0.031	0.075	0.017	−0.063	0.145	−0.015	0.070	0.134
	(0.111)	(0.091)	(0.044)	(0.104)	(0.126)	(0.049)	(0.129)	(0.107)
Rural Household, dummy	−0.128	0.182	−0.265	−0.216	−0.059	−0.245	−0.247	0.071
	(0.149)	(0.237)	(0.136)*	(0.262)	(0.390)	(0.111)**	(0.328)	(0.338)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	140.77	13.97	15.25	19.57	1.19	2.07	27.64	7.66
R^2	0.20	0.44	0.21	0.28	0.40	0.31	0.14	0.24
N	791	413	1374	577	383	1051	2209	866
TSIV estimate	0.126	0.603	0.120	0.867	−0.126	0.032	1.932	0.720
	(1.438)	(0.350)*	(0.490)	(0.987)	(0.788)	(0.346)	(0.897)**	(0.844)

Note: Dependent variables in regressions 1 to 6 are the corresponding livestock prices reported by the household. The dependent variable in regression 7 (8) is the per capita monetary value of households' ownership of cattle, goat, chicken and rabbit (sheep and pig). Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.14: Durable Goods Prices

	Bed	Chair	Lamp	Radio	Durable Goods Value pc. (1,2,3)		Durable Goods Value pc. (Rest)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Coverage in Village	−0.521	−0.955	−0.517	0.097	0.822	0.985	−0.298	0.173
	(1.233)	(0.843)	(0.731)	(0.619)	(0.330)**	(0.303)***	(0.694)	(0.785)
Distance to Major Town, log	−0.656	−0.261	−0.627	−1.172		−0.648		−1.123
	(0.722)	(0.435)	(0.635)	(0.394)***		(0.388)		(0.567)*
Distance to Major Road, log	0.113	−0.002	−0.107	−0.008		−0.357		0.119
	(0.228)	(0.211)	(0.157)	(0.141)		(0.101)***		(0.162)
Distance to Border, log	1.160	0.144	0.228	0.082		−0.042		0.152
	(0.521)**	(0.250)	(0.260)	(0.289)		(0.214)		(0.175)
Population 1991, log	−0.171	0.117	0.094	0.186		0.133		0.303
	(0.460)	(0.287)	(0.180)	(0.239)		(0.128)		(0.227)
Population density 1991, log	0.321	−0.110	−0.132	−0.215		0.002		−0.032
	(0.166)*	(0.142)	(0.175)	(0.122)*		(0.092)		(0.185)
North Sloping, dummy	−0.224	0.140	0.142	−0.119		0.126		−0.199
	(0.305)	(0.246)	(0.187)	(0.176)		(0.128)		(0.192)
East Sloping, dummy	−0.304	0.267	0.070	−0.067		0.001		−0.239
	(0.445)	(0.230)	(0.213)	(0.217)		(0.151)		(0.151)
South Sloping, dummy	−0.396	−0.033	0.173	−0.180		0.114		−0.020
	(0.195)*	(0.204)	(0.129)	(0.124)		(0.132)		(0.204)
Rural Household, dummy	−1.311	−1.346	−1.046	−0.497	−1.038	−0.559	−0.596	−0.387
	(0.431)***	(0.303)***	(0.321)***	(0.245)*	(0.349)***	(0.341)	(0.381)	(0.390)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	13.55	5.16	3.06	8.60	36.69	36.69	285.56	285.56
R^2	0.67	0.64	0.49	0.37	0.16	0.17	0.21	0.22
N	296	274	568	459	3533	3533	1542	1542
TSIV estimate	−1.077	−1.973	−1.069	0.199	1.698	2.035	−0.616	0.357
	(2.548)	(1.742)	(1.509)	(1.278)	(0.682)**	(0.626)***	(1.434)	(1.621)

Note: Dependent variables in regressions 1 to 4 are the corresponding durable goods prices reported by the household. The dependent variable in regressions 5 and 6 (7 and 8) is the per capita monetary value of households' ownership of bed, chair and lamp (other durable goods). Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.15: Crop Prices

	Sorghum		Cassava		Sweet Potato		Dry Beans		Cooking Banana		Beer Banana	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Radio Coverage in Village	0.029	0.001	−0.032	−0.032	−0.277	−0.216	−0.035	−0.029	−0.467	−0.184	−0.362	−0.492
	(0.248)	(0.218)	(0.631)	(0.448)	(0.171)	(0.153)	(0.099)	(0.103)	(0.212)**	(0.297)	(0.293)	(0.308)
Distance to Major Town, log		−0.031		0.522		0.251		0.058		−0.458		0.317
		(0.108)		(0.226)**		(0.115)**		(0.073)		(0.170)**		(0.210)
Distance to Major Road, log		−0.064		0.048		0.045		0.038		−0.022		−0.013
		(0.031)**		(0.098)		(0.078)		(0.022)*		(0.072)		(0.084)
Distance to Border, log		−0.138		−0.101		−0.118		0.009		−0.031		0.117
		(0.062)**		(0.238)		(0.108)		(0.036)		(0.166)		(0.090)
Population 1991, log		−0.036		−0.179		−0.206		0.055		−0.028		−0.218
		(0.040)		(0.092)*		(0.064)***		(0.038)		(0.103)		(0.074)***
Population density 1991, log		0.029		0.168		0.189		−0.010		0.099		0.050
		(0.025)		(0.124)		(0.046)***		(0.023)		(0.066)		(0.057)
North Sloping, dummy		−0.033		0.360		0.102		−0.002		0.073		0.074
		(0.047)		(0.143)**		(0.073)		(0.037)		(0.087)		(0.197)
East Sloping, dummy		−0.045		0.278		0.035		−0.032		0.174		0.183
		(0.046)		(0.126)**		(0.070)		(0.038)		(0.064)**		(0.081)**
South Sloping, dummy		−0.019		0.092		−0.028		−0.020		0.079		−0.106
		(0.031)		(0.064)		(0.074)		(0.031)		(0.067)		(0.087)
Rural Household, dummy	0.529	0.558	0.524	0.540	0.629	0.617	0.768	0.738	0.495	0.571	0.365	0.317
	(0.091)***	(0.074)***	(0.231)**	(0.187)***	(0.183)***	(0.156)***	(0.088)***	(0.086)***	(0.182)**	(0.179)***	(0.231)	(0.208)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	5.29	5.29	5.12	5.12	2.31	2.31	7.38	7.38	2.88	2.88	1.50	1.50
R^2	0.26	0.26	0.51	0.54	0.41	0.43	0.25	0.25	0.27	0.29	0.37	0.39
N	2136	2136	667	667	1250	1250	3512	3512	901	901	785	785
TSIV estimate	0.060	0.002	−0.065	−0.066	−0.572	−0.446	−0.072	−0.060	−0.965	−0.381	−0.747	−1.016
	(0.512)	(0.451)	(1.304)	(0.926)	(0.354)	(0.317)	(0.205)	(0.213)	(0.438)**	(0.614)	(0.605)	(0.637)

Note: Dependent variables are the corresponding crop prices per kilogram reported by the household. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.16: Crop Quantities

	Sorghum		Cassava		Sweet Potato		Dry Beans		Cooking Banana		Beer Banana	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Radio Coverage in Village	0.612	0.642	0.377	0.546	0.936	0.892	0.680	0.750	0.399	0.408	0.325	0.297
	(0.490)	(0.442)	(0.409)	(0.416)	(0.181)***	(0.164)***	(0.258)**	(0.248)***	(0.442)	(0.403)	(0.606)	(0.512)
Distance to Major Town, log		0.083		−0.037		0.146		−0.140		−0.256		−0.318
		(0.299)		(0.338)		(0.202)		(0.193)		(0.195)		(0.309)
Distance to Major Road, log		0.072		−0.014		−0.043		−0.014		−0.155		−0.193
		(0.077)		(0.092)		(0.081)		(0.055)		(0.095)		(0.072)**
Distance to Border, log		0.086		0.342		0.022		−0.091		−0.131		−0.157
		(0.222)		(0.206)		(0.165)		(0.127)		(0.222)		(0.259)
Population 1991, log		−0.154		0.097		−0.085		−0.074		−0.106		−0.296
		(0.097)		(0.126)		(0.156)		(0.096)		(0.232)		(0.117)**
Population density 1991, log		0.135		−0.030		−0.044		0.133		0.045		0.221
		(0.055)**		(0.065)		(0.070)		(0.055)**		(0.104)		(0.060)***
North Sloping, dummy		0.321		−0.004		0.024		0.186		0.160		−0.049
		(0.068)***		(0.133)		(0.109)		(0.078)**		(0.121)		(0.160)
East Sloping, dummy		0.212		0.054		0.191		0.299		0.047		−0.042
		(0.064)***		(0.106)		(0.113)		(0.081)***		(0.168)		(0.101)
South Sloping, dummy		0.178		0.012		0.075		0.211		0.163		0.136
		(0.081)**		(0.138)		(0.125)		(0.087)**		(0.129)		(0.142)
Rural Household, dummy	0.120	0.062	0.284	0.296	0.622	0.586	−0.159	−0.073	−0.857	−0.734	−0.471	−0.299
	(0.396)	(0.410)	(0.464)	(0.507)	(0.420)	(0.432)	(0.276)	(0.260)	(0.274)***	(0.230)***	(0.401)	(0.470)
Propagation Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Dep. Mean	113.31	113.31	423.80	423.80	795.46	795.46	103.58	103.58	304.56	304.56	698.79	698.79
R^2	0.27	0.28	0.23	0.23	0.27	0.27	0.26	0.27	0.38	0.38	0.22	0.23
N	2148	2148	2473	2473	3220	3220	3546	3546	1669	1669	2341	2341
Radio Coverage in Village	1.265	1.327	0.778	1.129	1.934	1.843	1.405	1.549	0.823	0.843	0.671	0.614
	(1.013)	(0.913)	(0.846)	(0.859)	(0.375)***	(0.339)***	(0.534)**	(0.513)***	(0.914)	(0.833)	(1.253)	(1.059)

Note: Dependent variables are measured in log kilograms per household of the corresponding crop. Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.17: First Stage Robustness Check

	Women (> 15 years)	Women (> 17 years)	Women (> 19 years)	Women (> 21 years)	Women (> 23 years)
	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Child Mortality				
Radio Coverage in Village	0.035	0.079	0.083	0.093	0.105
	(0.037)	(0.040)*	(0.035)**	(0.038)**	(0.055)*
Propagation Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
Dep. Mean	0.22	0.22	0.23	0.23	0.24
R^2	0.09	0.09	0.09	0.11	0.12
N	2324	2180	2009	1831	1695
	Dependent Variable: Boy Mortality				
Radio Coverage in Village	0.138	0.171	0.158	0.154	0.166
	(0.055)**	(0.052)***	(0.059)**	(0.060)**	(0.061)**
Propagation Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
Dep. Mean	0.24	0.25	0.26	0.26	0.26
R^2	0.09	0.09	0.09	0.10	0.11
N	2067	1970	1844	1704	1592
	Dependent Variable: Girl Mortality				
Radio Coverage in Village	−0.105	−0.071	−0.034	−0.011	0.024
	(0.062)	(0.067)	(0.064)	(0.058)	(0.066)
Propagation Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
Dep. Mean	0.20	0.20	0.21	0.21	0.22
R^2	0.08	0.08	0.09	0.10	0.11
N	2068	1971	1848	1701	1586

Note: Mortality is measured as the number of dead children (boys/girls) over the number of total children (boys/girls). Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Each regression uses a different subsample of women, defined in the header, e.g. regression 1 women above age 15. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 4.18: First Stage Robustness Check II

	Women (< 15 years)	Women (< 16 years)	Women (< 17 years)	Women (< 18 years)	Women (< 19 years)
	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Child Mortality				
Radio Coverage in Village	−0.076	−0.346	−0.490	−0.264	−0.153
	(0.815)	(0.547)	(0.242)*	(0.239)	(0.162)
Propagation Controls	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes
Dep. Mean	0.14	0.12	0.14	0.14	0.14
R^2	0.78	0.65	0.50	0.38	0.31
N	97	149	215	293	366

Note: Mortality is measured as the number of dead children (boys/girls) over the number of total children (boys/girls). Propagation controls are: latitude, longitude, a second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Each regression uses a different subsample of women, defined in the header, e.g. regression 1 women below age 15. Standard errors in parentheses are clustered at the district level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Ethnic Income Inequality and Conflict in Africa*

5.1 Introduction

Since World War II, more than a third of all UN member states have experienced at least one civil conflict with death tolls about three times as high as those of interstate conflicts (Fearon and Laitin, 2003). But what are the determinants of civil conflict? For decades, academics as well as policy makers have tried to answer this question. While the vast empirical literature appears to broadly agree on the importance of a few key determinants, such as low income levels, large populations and political instability, it is still very much divided on many of the others.[1] Economic inequality, in particular, has become one of the most debated determinants in this literature in recent years. From a theoretical perspective economic inequality has been proposed as a central driver of civil conflict since the time of Karl Marx, but most of the early cross-country empirical studies failed to identify a significant relationship between economic inequality and conflict (Fearon and Laitin, 2003;

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[1] Performing a sensitivity analysis of empirical results on civil war onset, Hegre and Sambanis (2006) identify 88 different variables, grouped in 18 different "concept categories", which had been proposed in the previous empirical literature, often with conflicting results.

Hegre, Gissinger and Gleditsch, 2003; Collier and Hoeffler, 2004). However, as civil conflicts are typically fought between different groups, for instance defined along ethnic or religious lines, it is inequality between these groups (i.e. "horizontal inequality") rather than among individuals (i.e. "vertical inequality") that should matter the most. Recent empirical studies suggest that inequality along ethnic lines, both within and across groups, might indeed matter, although again, the results are mixed (Huber and Mayoral, 2014; Lindquist, 2012; Østby, 2008).

Methodological issues are likely to explain these weak and inconclusive results. Given the lack of disaggregated income data, most of these studies construct inequality measures from survey data which is typically very noisy, however (see, for instance, de Nicola and Gine, 2014). Moreover, survey data is not annually available, and aggregation and/or extrapolations usually add to the measurement error, which is likely to bias the estimates downwards, possibly explaining the weak or insignificant results. Furthermore, high conflict prone areas might be difficult to survey and therefore underrepresented in the data, again biasing the results. In general, the existing literature does not adequately address the endogeneity of economic variables to civil war, thus failing to establish a causal relationship.

This paper addresses these issues by relying on exogenous variation in rainfall to identify the causal effects of between ethnic inequality on civil conflict in 46 African countries, using annual data for the period from 1958 to 2002. Thus, we link the literature on inequality and civil conflict to the recent and fast-growing literature on climate and conflicts (see Burke et al. (2014) for a comprehensive review). More specifically, since the seminal paper by Miguel et al. (2004), rainfall has increasingly been used as a proxy for income, especially for developing countries, where crop yields crucially depend on the amount of rainfall during the growing season. To the best of our knowledge, this is the first paper to exploit the proven link between rainfall and income to construct measures of inequality.

More specifically, we combine rich rainfall data from the European Centre for Medium-Term Weather Forecasting (ECMWF) with Murdock's 1959 spatial map of ethnic group boundaries in Africa to calculate the amount of rainfall for each ethnic group's homeland. In the absence of income data at the ethnic group level, we focus on the reduced-form effect of ethnic rainfall inequality on conflict (for a small sub-sample covering the years 1992 to 1999, we show that ethnic rainfall inequality indeed maps into ethnic income inequality, as measured by night light density).[2] Given that our inequality measure captures inequality across *ethnic* groups, we focus on *ethnic* conflicts, identified in the Ethnic Power Relations (EPR) dataset (Cederman et al., 2009).

Our main measure of interest is *between* ethnic group rainfall inequality, but we also consider vertical rainfall inequality and *within* ethnic group rainfall inequality. The latter allows us to test the hypothesis that conflict should be particularly salient when inequality within an ethnic group is high, as the rich can finance the poor who will then fight (Esteban and Ray, 2008).

The high frequency of our data allows us to include year- and country-specific fixed effects as well as country-specific linear time trends in our regression, thus addressing concerns that any time-specific shock or time-invariant (or linearly-variant) country-specific characteristics could be confounding our analysis.

Our results indicate a strong and positive relationship between rainfall-based between group inequality and ethnic conflict. A one standard-deviation increase in inequality increases the risk of ethnic conflict by about 60 percent. Importantly, the effects entirely stem from rainfall during the growing season, which is when rainfall is most important for agricultural production and, thus, income. We find no effects for vertical inequality – in line with most of the existing empirical literature. Neither we do find any support for the

[2]We only focus on the years before 2000, since the relationship between rainfall and income is said to become significantly weaker after 2000 (Miguel and Satyanath, 2011).

relevance of within ethnic group inequality.

We then discuss in great detail a number of potential threats to our identification strategy. For instance, recent studies have shown that rainfall affects transport costs (Rogall, 2014) and malaria incidence (Kudamatsu et al., 2014) which, in turn, are likely to affect conflict prevalence. However, our results are robust to controlling for rainfall-related transport costs and malaria incidence as well as a rich set of additional controls used in the literature. Furthermore, in line with our mechanism, the effects disappear when we consider non-ethnic conflicts.

This paper adds to the literature in several ways. First of all, it adds to the vast conflict literature – surveyed in Blattman and Miguel (2010) – by providing novel evidence on the strong effects of inequality on ethnic conflict. Recent studies on the determinants of conflict and participation in violence look at for instance institutions, income and foreign aid (Besley and Persson, 2011; Dube and Vargas, 2013; Mitra and Ray, 2014; and Nunn and Qian, 2014, respectively).

More specifically, our paper complements the literature on the effects of ethnic income inequality on civil conflicts. The concept of between group inequality was introduced by Stewart (2000), and is based on the notion of relative deprivation (Gurr, 1993). In a series of case studies, Stewart (2002) provides empirical evidence for the connection between ethnic inequality and conflict. One of the first studies to systematically consider the effects of ethnic inequality on conflict is Østby (2008). Using DHS Survey data for 36 developing countries over the period 1986 to 2004, she finds that ethnic income inequality does not affect conflict. However, relying on survey data might be problematic, as mentioned above. A different approach is taken by Cederman et al. (2011) who rely on the G-Econ data developed by Nordhaus, which provides data on local economic activity disaggregated at a 1 degree resolution, to construct measures of ethnic inequality. This data set has a global coverage, but is only available for a single year (1990), which is also likely to induce measurement error as conflicts up to 13 years later are considered in the analysis. Recently, Alesina et al. (2014), have used night light density data to construct a measure of ethnic inequality. However, the authors only consider three points in time (1990, 2000 and 2012) and their analysis focuses on the link between inequality and development, without touching upon armed conflicts.

This paper also contributes to the vast literature on climate and conflicts, nicely reviewed by Burke et al. (2014). Particularly relevant for our paper is the work by Harari and La Ferrara (2014), which uses disaggregated rainfall data to study the link between climate shocks during the growing season and civil conflict incidence in Africa, finding a strong and persistent link. Different from our study, they consider a rainfall grid-cell as the unit of analysis and their data allows them to only consider the period 1997-2011. Most importantly, their paper does not touch upon either the issue of ethnicity or of inequality, which are the focus of our work.

The remainder of the chapter is organized as follows. Section 5.2 introduces the theoretical background to this research. Section 5.3 presents the various data sources used in the empirical analysis. Section 5.4 then explains the empirical framework, while Section 5.5 presents and discusses the results. Finally, Section 5.6 concludes the paper.

5.2 Theoretical Background

Inequality and Conflict A large and diverse literature tries to explain, both theoretically and empirically, the effects of economic inequality on political violence. Starting with a paper by Russett in 1964, numerous studies have looked into the relationship between vertical inequality – that is, inequality between individuals – and conflict, with up to now very mixed results.[3]

[3] Examples include Alesina and Perotti (1996), Auvinen and Nafziger (1999), Collier and Hoeffler (2004), Fearon and Laitin (2003), Hegre, Gissinger and Gleditsch (2003), Muller and Seligson (1987), Nagel (1974), Parvin (1973), Sigelman and Simpson (1977)

Most of the traditional papers are based on the theory of relative deprivation by Gurr (1993): while absolute poverty might lead to resignation, comparing oneself to others who do better will lead to conflict. Recently, the literature has emphasized the greed versus grievance theory. Greed – which is often proxied by variables such as the availability of natural resources, education levels and income per capita – hereby captures the opportunities to start a conflict following a rational calculation (Tilly, 1978), whereas grievance – typically captured by measures of inequality or ethnic and religious hatred – subsumes the motives for groups to change their situation (Gurr, 1970). Collier and Hoeffler (2004) discuss several different factors from both notions and largely dismiss inequality, a grievance factor, as a cause of conflicts. Other prominent studies such as Fearon and Laitin (2003) and Hegre, Gissinger and Gleditsch (2003) also point towards a zero effect.

However, these studies are confined to vertical inequality, typically measured by the Gini coefficient, even though the conflicts they set out to explain are usually fought along the lines of certain groups, be it ethnic, religious or economic, i.e. the poor versus the rich (Duclos, Esteban and Ray, 2004; Montalvo and Reynal-Querol, 2003; and Esteban and Ray, 2005). Especially ethnicity is conflict-prone as it is typically founded on a combination of fundamental factors such as language, race or religion (Esteban and Ray, 2008; Horowitz, 1985; Østby, 2008). In particular in Africa – the region of interest in this study – the identification with ethnicities often trumps national identity, due to the relatively short national histories and to the often arbitrary national boundaries of many African countries (Michalopoulos and Papaioannou, 2010).

To account for these group effects, the literature has developed two additional broad concepts of inequality. One is the notion of between group inequality, the other is within group inequality. Between group inequality is a grievance factor which is highest when some ethnic groups in the country

and Weede (1981).

are rich and others are poor. A number of studies propose between-group inequality as a key determinant for civil conflicts (Cederman et al., 2011; Stewart 2000, 20002; Østby, 2008), although, as mentioned in the introduction, empirical tests of this hypothesis have been affected by methodological limitations and have, so far, led to mixed and inconclusive results. Within group inequality, instead, measures differences across individuals within one ethnic group. Esteban and Ray (2008) suggest that conflict should be particularly salient when this inequality is high because the rich elite can finance the war and the poor population (with low opportunity costs) can fight it.

Climate and Income Our approach circumvents the lack of disaggregated income data, by constructing inequality measures based on rainfall data. Following the seminal paper by Miguel et al. (2004), a large and fast-growing literature – both at a micro (e.g. Jia, 2014; Miguel, 2005) and at a macro (e.g. Burke, 2012; Kim, forthcoming) level – has relied on climatic variables as proxies (or, in some cases, as instruments) for income in developing settings. Given the absence of disaggregated income measures, we focus on the reduced-form effect of rainfall on ethnic conflicts. However, the validity of this instrumental-variable approach has recently been questioned by a growing literature showing that climate affects a large number of socio-economic outcomes (see Dell et al. (2014) for a comprehensive review). Therefore, we present our reduced-form results as the net effect of rainfall inequality on ethnic conflict, operating through a number of potential channels. Nevertheless, we show that rainfall inequality maps into night light inequality, which is the only disaggregated measure of economic development available.

Inequality Measures We use rainfall data to construct three different measures and to investigate the relationship between inequality and ethnic conflicts. The first and most important measure is BGRI (Between Group Rainfall Inequality), which captures rainfall inequality between ethnic groups in a country. BGRI is computed for every country and year in our sample and is defined as (we drop time and country indices)

$$BGRI = \frac{1}{2\mu} \sum_{i=1}^{E} \sum_{j=1}^{E} n_i n_j \mid r_i - r_j \mid, \tag{5.1}$$

where E indicates the number of ethnic groups whose homeland is located within the country, n_i is the relative size of ethnic group i and r_i is the amount of rain that fell over ethnic group i's homeland.[4] The measure is normalized by the average amount of rainfall in each country and year, μ, so as to allow for comparisons across countries. In a similar fashion, but replacing rainfall by night light density, we also construct a $BGNI$ (Between Group Night light Inequality) measure, to show that rainfall inequality directly relates to economic inequality over the period 1992-1999.

Our second measure is WGI (Within Group Inequality), constructed in two steps. First, we use variation in rainfall across the rainfall grids that cover the homeland of ethnic group i to construct a group-specific measure of inequality

$$WGI_i = \frac{1}{2\mu_i} \sum_{k=1}^{G_i} \sum_{l=1}^{G_i} \frac{1}{G_i^2} \mid r_{i,k} - r_{i,l} \mid, \tag{5.2}$$

where μ_i indicates the average amount of rain that fell over the homeland of ethnic group i in a given year, G_i is the number of rainfall grids that cover group i's territory, and $r_{i,k}$ and $r_{i,l}$ indicate the amount of rain falling on grid l and k, respectively. To obtain the overall within group inequality measure for the whole country, we then weight the above by a group i's relative group size and relative group rainfall in the country

$$WGI = \frac{1}{2\mu_i} \sum_{i=1}^{E} n_i \frac{r_i}{\sum_{i=1}^{E} r_i} WGI_i. \tag{5.3}$$

Finally, we also calculate a standard measure of VI (Vertical Inequality) in

[4]Since population data might be endogenous each ethnic group gets equal weights.

each country and year, by considering differences in rainfall across all grids covering the country, irrespective of the ethnic groups

$$VI = \frac{1}{2\mu} \sum_{k=1}^{G} \sum_{l=1}^{G} \frac{1}{G^2} \mid r_k - r_l \mid, \tag{5.4}$$

where G is the total number of rainfall grids covering the country.

5.3 Data and Measurement

We combine several datasets from different sources to construct our final dataset, which comprises 46 African countries and covers 45 years, from 1958 to 2002.[5] Table 5.1 reports the full list of countries included in the study, while summary statistics for all our variables are given in Table 5.2. Figure 5.1 illustrates how we combine the three key datasets on conflict, ethnicity and rainfall.

Conflict Data To construct our dependent variable, we combine two data sources. We take information on conflicts from the Armed Conflict Data database developed by the International Peace Research Institute of Oslo, Norway, and Uppsala University, Sweden (Gleditsch et al., 2002). This database records all conflicts with a threshold of 25 battle deaths per year. An armed conflict is defined as a contested incompatibility which concerns government and/or territory where the use of an armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths. Since our study focuses on how ethnic inequalities relate to civil conflicts, we exclude interstate conflicts from the analysis.

We merge the conflict data with the Ethnic Power Relations (EPR) dataset, recently developed by Cederman et al. (2009) to determine whether

[5] We exclude small islands and territories from our main analysis, because most of the data sources we use to generate our control variables do not cover these countries.

the civil conflicts in question are fought along ethnic lines. For a civil conflict to be classified as ethnic, the armed groups have to follow ethnonationalist aims, motivations, and interests and recruit fighters and forge alliances based on ethnic affiliations. Our final dependent variable is an indicator variable for the presence of an ethnic conflict in a country in a given year. Table 5.1 provides details for each country on the number of years of ethnic and non-ethnic conflicts over our sample period from 1958 to 2002.

Ethnicity The cornerstone of the analysis is a map by Murdock (1959) that reports the spatial distribution of 843 ethnic groups around Africa in the mid/late 19th century. Compared to alternative, more recent maps that have been proposed in the literature, such as the *Soviet Atlas Narodov Mira* or the *Ethnologue* (see, for instance, Alesina et al., 2014), the Murdock map has the advantage of providing pre-determined ethnic boundaries which are unaffected by the ethnic conflicts that we investigate.[6] However, one might wonder how the ethnic distribution changed over time and how well the Murdock map still reflects more recent ethnic diversity in Africa. Comfortingly, using individual data from the Afrobarometer, Nunn and Wantchekon (2011) show a strong relationship between the location of respondents in 2005 and the historical homeland of their ethnicity as indicated by Murdock's map. This evidence is furthermore supported by a number of case studies: Glennerster, Miguel and Rothenburg (2013), for instance, show that even after the huge displacement following the civil war in Sierra Leone, there has been a systematic movement of people back to their ethnic homelands. Similarly, refugees leaving Rwanda in the aftermath of the 1994 genocide mostly returned to their old locations.

Rainfall Rainfall data is provided by the European Centre for Medium-Term Weather Forecasting (ECMWF) which relies on historical data from

[6]The *Soviet Atlas* refers to the early 1960s, while the *Ethnologues* to the mid-1990s.

a variety of sources: weather stations, ships, aircraft, weather balloons, radiosondes and, most importantly, satellites orbiting the earth. Data is available at a 1.25 degree spatial resolution and a six-hour frequency since September 1957 which we aggregate to obtain yearly rainfall.

To compute rainfall during the growing season, we follow the approach in Kudamatsu et al. (2014). More specifically, we use the Normalized Difference Vegetation Index (NDVI) dataset provided by Tucker et al. (2005). This data contains bi-weekly measures of plant growth since 1982 and comes at a 8×8 km resolution.[7] We use a program called TIMESAT, to process the NDVI data, remove the noise and extract seasonality information, which allows us to determine the growing season (see Jonsson and Eklundh (2004) for more details). We exploit the full NDVI data (from 1982 to 2006) to estimate the average growing season for each NDVI pixel. Then, we aggregate that fine-gridded data at our original 1.25×1.25 degrees grid resolution to obtain the average growing season in each rainfall grid-cell.

The average yearly rainfall (or rainfall during the growing season) for each ethnic group and each country is obtained by overlaying the rainfall grids with ethnicity and administrative maps (the latter obtained from the Global Administrative Unit Layers data set). Since several rainfall grids typically cover an ethnic homeland or country, the overall rainfall is obtained through a weighted average of the grids, where the weights are given by the relative areas covered by each grid.

How reliable is our measure of rainfall during the growing season? Kudamatsu et al. (2014) provide the best validation check, showing that rainfall during the growing season, estimated as outlined above, significantly affects local crop prices in Sub-Saharan Africa, as measured by the USAID Famine Early Warning Systems Network (FEWS NET).

[7]The original dataset is generated using satellite images that record red and infrared radiances and reflectances, which are highly correlated with photosynthetically active biomass, chlorophyll abundance, and energy absorption, thus allowing us to estimate plant growth on the hearth surface. See Tucker et al. (2005) for more details.

Night Light Density Data on night light density is provided by the National Geophysical Data Center (NGDC). Several satellites of the US Air Force circle around the earth 14 times a day, observing every location on the planet at some instant between 8 and 10 pm local time. The data is available for the years 1992 to 2009. Each satellite year dataset consists of a grid which reports the light density with a six-bit digital number (an integer between 0 and 62). The grid comes at a very high spatial resolution (every grid at the equator covers approximately 0.86 square kilometers). We match the night light data with the spatial data on ethnic groups. As several night light density grids cover the ethnic homelands, the overall night light density per ethnicity is obtained through a weighted average of the night light density grids where the weights are the areas covered by each night light density grid.

Temperature The ECMWF data also includes high-frequency measures of temperature. Therefore, we follow the same approach as described above to generate a temperature-specific inequality measure, both specific to the growing season and based on the yearly data.

Malaria Prevalence Using the monthly rainfall and temperature data we generate an indicator variable for malaria prevalence in each rainfall grid. We follow Kudamatsu et al. (2014) and set this variable equal to 1 if the following four conditions are met: 1) average monthly rainfall in the previous 3 months is at least 60 mm; 2) rainfall in at least one of these months is above 80 mm; 3) no month in the previous 12 has an average temperature below 5 C; 4) the average temperature in the previous 3 months exceeds the sum of 19.5 C and the standard deviation of monthly average temperature in the past 12 months (see Kudamatsu et al. (2014) for more details). Our final variable, at a yearly level, is the share of months with malaria.

Transport Costs A digitalized map of the road system is taken from the African Development Bank and OpenStreetMap, which records the location of major African roads in 2009 and 2014. Unfortunately, to the best of our knowledge, there is no database recording the evolution of the road system through Africa over time and therefore, we rely on this data as the best available proxy for our sample period.[8] To calculate rainfall-induced transport costs, we first generate a 10 meter buffer around each road and then compute the amount of rainfall over each buffer (per year and country).

Additional Controls Data on population, GDP per capita (at 2005 constant prices) and openness to trade is taken from Penn World Table 7.1 (Henston et al., 2012). Data on natural disasters is taken from the Emergency Events Database (EM-DAT) kept by the Centre for Research on the Epidemiology of Disasters (CRED). The EM-DAT dataset records all natural disasters since 1900 that fulfill at least one of four criteria: 1) ten or more people reported killed; 2) hundred or more people reported affected; 3) declaration of a state of emergency; 4) call for international assistance. For each disaster, the database combines information from different sources, including UN agencies, non-governmental organizations, insurance companies, research institutes and press agencies and reports. We use the total number of individuals affected by a natural disaster, obtained by summing up the number of individuals that died (which includes missing individuals), were injured, lost their house and/or required basic survival needs (such as food, water, shelter, sanitation or immediate medical assistance).

Data on institutional quality is taken from the widely used Polity IV database (see, for instance, Fearon and Laitin (2003) and Hegre and Sambanis (2006)) provided by the Center for Systemic Peace.[9]

[8]The road system is, in any case, generally only changing slowly over time. In a robustness check, we show that our results are robust to using only more recent years, when road infrastructure is even more likely to have remained relatively constant.

[9]The standard polity IV index is constructed combining two comprehensive variables:

5.4 Empirical Framework

We estimate the impact of between ethnic group income inequality on the incidence of ethnic conflicts in Africa. Given the lack of a reliable disaggregated measure of income, we estimate the reduced-form relationship between rainfall-based inequality and conflict. However, there are good reasons to believe that rainfall inequality directly maps into economic inequality. Most economies in Africa rely heavily on agriculture as their primary income. In 2004, some 55 percent of the people in Africa were employed in agriculture and many more indirectly depended on it (Frenken, 2005). Moreover, irrigation systems are severely under-developed, especially in Sub-Saharan Africa, where only 6.4 percent of cultivated land were irrigated in 2004 (Frenken, 2005) and the vast majority of farmers thus depended on rainfed crops (Harari and La Ferrara, 2014). More rigorously, Kudamatsu et al. (2014) show that rainfall during the growing season significantly affects local crop prices in Sub-Saharan Africa. Unsurprisingly, several studies in the literature have exploited the relationship between rainfall and income in Africa (Miguel et al., 2004). In the next section we show that our specific rainfall inequality measures positively map into economic inequality, measured by night light density. Albeit imperfect, this is to the best of our knowledge the only alternative measure of economic inequality available in disaggregated form (and used, for instance, by Alesina et al., 2014).

Our approach makes the counterfactual assumption that absent income inequality, rainfall inequality has no direct effect on civil conflict. However,

1) the democracy indicator, which is an additive eleven-point scale (0-10) variable derived from codings of the competitiveness of political participation, the openness and competitiveness of executive recruitment and constraints on the chief executive. 2) the autocracy indicator, which is another additive eleven-point scale (0-10) variable derived from codings of the competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive and constraints on the chief executive. The final index variable is computed by subtracting the autocracy score from the democracy score; the resulting unified polity scale therefore ranges from +10 (strongly democratic) to -10 (strongly autocratic). See Marshall et al. (2012) for further details.

a number of recent studies suggest that this might not be the case. For instance, rainfall affects the road system and might reduce the chances that troops meet each other in combat (Rogall, 2014), or rainfall might negatively affect the health of individuals (and therefore fighting ability) by increasing malaria prevalence (Kudamatsu et al., 2014). To address these concerns we first exploit the fact that the timing of the rainfall is of crucial importance. In particular, rain is most "productive" when it falls in the growing seasons. Hence, it should be inequality in rainfall during the growing season that should determine economic inequality, while rainfall during the rest of the year should play no (or only a minor) role. Second, besides adding a battery of fixed effects, we directly control for rainfall-related transport costs and malaria incidence.

Specification Our main specification captures the reduced-form relationship between rainfall inequality and the prevalence of ethnic conflict. The corresponding empirical model is therefore

$$EthnicConflict_{c,t} = \alpha + \beta BGRI_{c,t} + X_{c,t}\Psi + \theta_c + \gamma_t + \tau_c t + \omega_{c,t}, \qquad (5.5)$$

where the dependent variable is a dummy taking on the value of 1 if country c experienced ethnic conflict in year t and $X_{c,t}$ collects various controls, described in the next section. We allow for country-fixed effects θ_c to capture time-invariant characteristics, such as history or topography. Several studies confirm that, for instance, very hilly terrain has an effect on the likelihood of conflict (Buhaug and Rød, 2006; Miguel et al., 2004). Other important time-invariant characteristics are e.g. distance to the capital (or other major) cities or country borders and, maybe most importantly, the colonial past, culture and institutions. We further control for time-specific common shocks across the African continent γ_t (e.g. global economic shocks, or the signing of a new global agreement), as well as for country-specific factors that change linearly over time $\tau_c t$ (e.g. years since independence). Finally, $\omega_{c,t}$ is the error term.

Whenever the dependent variable is binary, like in our case, the literature typically uses logit or probit estimators. However, in line with Harari and La Ferrara (2014), we prefer fitting an unrestricted linear probability model, because it allows us to address the special error correlation in our data. More specifically, we report errors clustered by country and year, to account for the possibility that observations are correlated over time for the same country and across countries for the same year.[10] The coefficient of interest is β.

To test the reliability of our measure as a proxy for economic inequality, we replace the dependent variable with our measure of between group night light density inequality, as follows

$$BGNI_{c,t} = \alpha' + \beta' BGRI_{c,t} + \phi_c + \pi_t + \vartheta_c t + \epsilon_{c,t}, \tag{5.6}$$

where ϕ_c and π_t are again indicate country- and year-fixed effects, respectively, and $\vartheta_c t$ captures country-specific linear time trends. The coefficient of interest is β', capturing how well rainfall inequality maps into BGNI, i.e. our best proxy for economic inequality.

5.5 Results

Main Results We start with the simple reduced-form relationship between ethnic conflict and between group rainfall inequality (BGRI). Table 5.3 reports the results. Regression 1 shows a strong and significant relationship between our BGRI measure and ethnic conflict, with a point estimate of 0.487 (standard error 0.214). When we add the full set of country- and year-fixed effects as well as country-specific linear time trends, the relationship remains positive, but significantly weakens both in terms of magnitude (0.184,

[10]The estimates are generated in STATA 12, using the cluster2 command written by Mitchell A. Petersen. The command is based on Cameron, Gelbach and Miller (2006) and can be downloaded from the author's website. There are 46 countries and 45 years in our final sample.

standard error 0.122) and significance (the p-value drops to 0.133, regression 2). However, when we restrict the focus to rainfall during the growing season, the most precise determinant of income, the BGRI coefficient once more becomes large (0.301, standard error 0.151) and statistically significant at conventional levels (regression 3). Regression 4 confirms that the positive relationship between inequality and ethnic conflicts is entirely driven by rainfall during the growing season. In all our regressions, we include a control for (the logarithm of) yearly rainfall over the whole country, to take into account broad country-wise variations in rainfall.

The estimated effects are huge: the point estimate of 0.333 (standard error 0.163, regression 4) suggests that a one standard-deviation increase in BGRI increases the likelihood of ethnic conflict by 66 percent. Furthermore, the effects show some time persistence. The coefficient of lagged inequality is relatively large up to two years back in time, although it is only significant at a conventional level for the first lag (regression 5). Overall, these results provide strong support for the idea that between group (or "horizontal") inequality has a strong and significant impact on the prevalence of armed conflicts, as proposed by Stewart (2002).

Interestingly, we find no significant relationships between ethnic conflict and within group inequality or total vertical inequality (regressions 6 and 7). The former result contrasts with the theory proposed by Esteban and Ray (2008). The latter is instead consistent with what most of the empirical literature has previously found, based on income measures derived from national statistics or various survey rounds (Collier and Hoeffler, 2004; Fearon and Laitin, 2003; Hegre, Gissinger and Gleditsch, 2003). In regression 8 we add all three measures at once. This further confirms these findings: the coefficient on BGRI remains large and highly significant, while within and vertical inequality continue to play no role.

A large part of the conflict literature has focused on conflict onset, rather than the incidence of conflict. Therefore, we check whether our main result holds when we replace the dependent variable with an indicator for conflict onset. We construct the variable following standard practice in the literature, coding the consecutive years of conflict as missing, given that countries in conflict are not at risk of having a new onset (see, for instance, Buhaug and Rød, 2005). Our results are clearly confirmed: BGRI is strongly associated with the onset of ethnic conflict (regression 9).

Placebo Tests We next perform a number of placebo tests, reported in Table 5.4. Since our measure focuses on ethnic inequality, it should not be able to predict non-ethnic conflict. Regression 1 confirms this conjecture. When we replace the dependent variable with an indicator for civil conflicts that are not classified as ethnic in the EPR dataset and rerun the main regression, the point estimates become very small, insignificant and, if anything, negative. The same is true when we use the onset of non-ethnic conflict (regression 2). As a last validation check we add rainfall inequality using future rainfall to the regression. As expected the point estimate is small and insignificant (regression 3).

Robustness Checks – Additional Controls We now test the robustness of our finding to a large set of checks. The results are reported in Table 5.5.

First, conflicts tend to be persistent over time. Many empirical studies on conflict thus control for a lagged dependent variable. Regression 1 shows that the lagged ethnic conflict variable is indeed a powerful predictor for current ethnic conflict, but the coefficient on our BGRI measure is virtually unaffected.

Recent empirical studies have shown that rainfall may have direct effects on conflict through transport costs (Rogall, 2014) and malaria incidence (Kudamatsu et al., 2014). In regression 2, we address these concerns and control for a malaria prevalence index and yearly rainfall along the main roads within a country and the results are robust.

In their comprehensive review of empirical studies on climate and conflicts, Burke et al. (2014) point out that temperature and rainfall are often highly correlated and recommend including both of them in the regressions to avoid omitted variables bias. In regression 3, we therefore include between group inequality measures constructed using temperature rather than rainfall data in the regressions. The coefficient of interest on rainfall inequality once again remains very stable and significant.

We next consider a number of additional potential determinants of conflict that have been proposed in the literature.[11] Hegre and Sambanis (2006) define GDP per capita, population size and years of peace as the three core variables for civil conflict models. GDP per capita is meant to capture the economic conditions of the country and the opportunity cost of conflict (Collier and Hoeffler, 2004; Fearon and Laitin, 2003). Moreover, large populations in general increase the chances that a conflict enters the database since the required fatality thresholds are easier reached, ceteris paribus. Finally, the number of years of peace captures the accumulation of peace-specific capital.[12] We also include a control for openness to trade, as some scholars have suggested a link between trade and civil conflicts (e.g. Elbadawi and Sambanis, 2002; Hegre et al., 2003). We also include a control for natural disasters in the country, as it might be correlated with both extreme weather situations and with the likelihood of conflicts by, for instance, affecting the opportunity cost of conflict. Our variable captures the (logarithm of) the total number of individuals affected by the disaster. Finally, we also include a control for institutional quality, whose role as a determinant of civil conflicts has been highly debated in the literature, with some studies finding a negative association (e.g. Gurr, 2000) and others finding no significant relationship (e.g.

[11] Due to the limited coverage of some of the sources used to generate these additional controls, we lose about 15 percent of the observations in our dataset.

[12] We start counting the years of peace since 1958 – i.e. the beginning of our sample – and define the variables as $2^{-yearsinpeace/8}$, following the approach used by Hegre and Sambanis (2006).

Collier and Hoeffler, 2004). The results in regression 4 show that, the inclusion of these controls makes our main finding even stronger, as the inequality coefficient increases.

Besides a country's own characteristics, the situation in neighboring countries might be important. Although the literature is divided on this point – with some studies finding that civil conflicts tend to spill over to neighboring countries (e.g. Sambanis, 2001), while others do not find any significant effect (e.g. Fearon and Laitin, 2003) – we are cautious and include two controls, for the presence of an ethnic conflict in any of the neighboring countries and for the average institutional quality of the neighboring countries (which is supposed to proxy for "bad" neighbors). We find that the quality of the neighbors indeed plays a significant role in determining the likelihood of an ethnic conflict, but again, our coefficient of interest remains virtually unaffected as compared to our baseline specification (regression 5).

In regression 6, we include all of the above controls together. Not only does the magnitude of the coefficient of interest remain very stable, but the precision of the estimates increases thanks to the additional controls.

Since our controls might have delayed effects on conflict or are themselves outcomes of rainfall we also include lagged variables of all our controls (except for the variable capturing the number of years of peace). Once more, the results are unchanged (regression 7).

Other Checks Next, we test the robustness of our results to changes in the sample and to the definition of our key variables.

Regression 1 in Table 5.6 shows that our results are robust to restricting the sample to sub-Saharan Africa (i.e. dropping Egypt, Tunisia, Morocco, Libya and Algeria). The coefficients on ethnic inequality become even larger, which is in line with the intuition that Sub-Saharan Africa is the region most heavily dependent on rain-fed agriculture, and for which rainfall inequality should therefore be a better proxy for income inequality.

Empirical studies on civil conflicts that use rainfall data mostly focus on recent decades, when data quality is likely to be higher.[13] In regression 2, we show that our results are robust to restricting the sample to the post-1980 period.

Finally, one might be concerned about the accuracy of the Murdock map that we use to define ethnic boundaries. One might, for instance, wonder whether alternative geographical divisions, such as for instance sub-country administrative units, might be more salient than ethnic boundaries for defining "horizontal" inequality and thus, for predicting conflicts. One issue here is that administrative boundaries are often redesigned over time and, to the best of our knowledge, there is no systematic database recording their evolution over the African continent. Moreover, administrative boundaries are likely to be endogenous to conflicts. Keeping these caveats in mind, we compute inequality in rainfall between groups living in different administrative units within the same country. In doing so, we refer to a map of level 1 administrative units (i.e. the largest level subnational administrative division, typically corresponding to regions or districts) for the African continent. In regression 3, we repeat our main regression, using these alternative measures. The coefficient of rainfall inequality measured during the growing season is still positive and close to significant at the 90 percent level (the p-value is 0.107), which is what we would expect if administrative division were indeed partly aligned with ethnic division. However, when we include the measures constructed using ethnic boundaries and administrative boundaries in the same regression, we see that only the former remain large and statistically significant (regression 4).

"First Stage" So far, we have shown that rainfall inequality between ethnic groups is a strong and robust predictor of ethnic conflict incidence. We have also shown that the results are driven by rainfall during the growing

[13] For instance, the seminal paper by Miguel et al. (2004) considers the period 1981-1999, while Harari and La Ferrara look at the period 1997-2011.

season – the time when rainfall is most important for agricultural output and thus, the economic welfare of individuals living in the region.

One might, however, still wonder how reliable rainfall inequality is as a proxy for economic inequality. Unfortunately, there is no database recording disaggregated measures of economic welfare in a systematic way over time. However, there is a large recent literature that relies on satellite image data on night light density as a proxy for income (Chen and Nordhaus, 2011; Henderson, Storeygard, and Weil, 2012; Hodler and Raschky, 2014; Michalopoulos and Papaioannou, 2013, 2014; Pinkovskiy, 2013; Pinkovskiy and Sala-i-Martin, 2014). Particularly relevant for our study is the work by Alesina et al. (2014), which uses night light density to construct ethnic inequality measures.

We adopt a similar approach and construct inequality measures based on night light density, to check how it relates to the corresponding rainfall inequality by estimating equation 5.6, described above. Unfortunately, night light density data is only available from 1992 onwards which limits the number of observations. Nevertheless, BGRI is positively related to BGNI at the 99 percent confidence level with a point estimate of 0.159 (standard error 0.054, regression 1 in Table 5.7). The relationship holds up when adding a lagged dependent variable (regression 2) and, consistent with our previous findings, it is entirely driven by rainfall during the growing season (regression 3). The relationship is also confirmed when past and future rainfall inequality are included in the regression (regression 4). Importantly, neither vertical inequality nor within group inequality explain between group night light density (regressions 5 and 6, respectively). When all inequality measures are simultaneously added to the regression, the significance is lost, although the coefficient on between group inequality remains the only one close to significance (the p-value is 0.121, regression 5). However, when we use the whole sample of African countries – i.e. also include smaller countries and islands, which are excluded from the main analysis – the coefficient is once more significant at conventional levels (regression 6).

5.6 Discussion and Conclusion

Our findings suggest that between ethnic group income inequality – based on rainfall – has strong effects both on the onset and incidence of ethnic conflict in Africa. We do not find any evidence that vertical inequality, thus inequality across individuals, matters. Neither do we find evidence that inequality within ethnic groups matters.

Our results have important policy implications. Global warming and the corresponding climate change are said to increase average temperatures and lead to more extreme rainfall patterns, generally bad for agricultural production. If these climate and weather changes affect different regions in Africa differently then they will likely lead to more conflict. Thus to possibly prevent civil conflict in the future it is indispensable to understand how, when and especially where these changes will take place.

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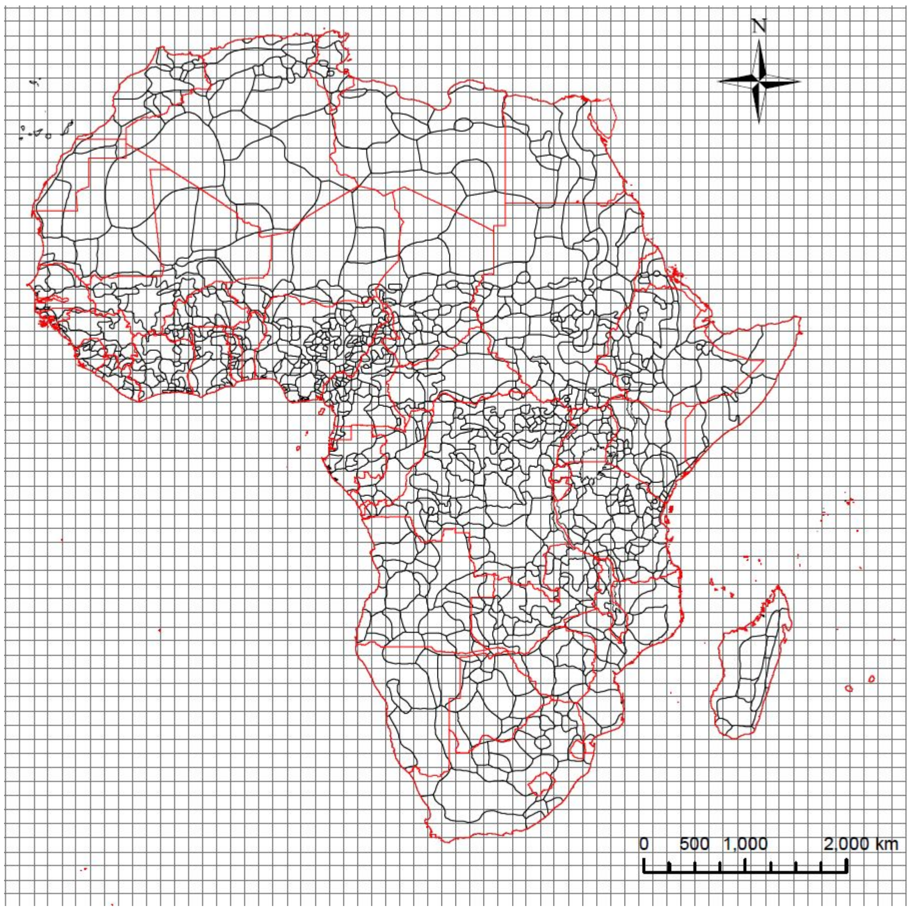
Tables and Figures

Figure 5.1: Dataset construction



Notes: The figure shows how the dataset has been constructed, by spatially merging three key maps: a rainfall grid of 1.25 × 1.25 degree cells, 1959 Murdoch's map of ethnic boundaries (black lines), and African country borders (red lines).

Table 5.1: List of Countries

	Total Years	Years of Non-Ethnic Conflict	Years of Ethnic Conflict
ALGERIA	45	12	0
ANGOLA	45	7	21
BENIN	45	0	0
BOTSWANA	45	0	0
BURKINA FASO	45	1	0
BURUNDI	45	0	12
CAMEROON	45	0	1
CENTRAL AFRICAN REPUBLIC	45	1	1
CHAD	45	2	34
CONGO	45	4	2
CôTE D'IVOIRE	45	0	1
DEMOCRATIC REPUBLIC OF THE CONGO	45	9	5
EGYPT	45	0	0
ERITREA	45	0	2
ETHIOPIA	45	14	24
GABON	45	1	0
GAMBIA	45	1	0
GHANA	45	3	0
GUINEA	45	3	0
GUINEA-BISSAU	45	2	0
KENYA	45	1	0
LESOTHO	45	1	0
LIBERIA	45	3	8
LIBYA	45	0	0
MADAGASCAR	45	1	0
MALAWI	45	0	0
MALI	45	0	2
MAURITANIA	45	0	0
MOROCCO	45	11	5
MOZAMBIQUE	45	2	14
NAMIBIA	45	0	0
NIGER	45	0	4
NIGERIA	45	0	5
RWANDA	45	4	7
SENEGAL	45	0	9
SIERRA LEONE	45	10	0
SOMALIA	45	19	0
SOUTH AFRICA	45	15	8
SUDAN	45	1	30
SWAZILAND	45	0	0
TOGO	45	0	2
TUNISIA	45	1	0
UGANDA	45	23	2
TANZANIA	45	0	0
ZAMBIA	45	0	0
ZIMBABWE	45	0	8

Table 5.2: Summary Statistics

	Mean	Std.dev.	Obs.
A. Conflict			
Ethnic Conflict	0.10	0.30	2070
Ethnic Conflict Onset	0.02	0.15	1909
Non-Ethnic Conflict	0.07	0.26	2070
Non-Ethnic Conflict Onset	0.02	0.15	1961
B. Inequality Measures			
Between Group Rainfall Inequality, BGRI	0.34	0.20	2070
Within Group Inequality, WGI	0.11	0.13	2070
Vertical Inequality	0.36	0.23	2070
BGRI, Growing Season	0.39	0.19	2070
WGI, Growing Season	0.10	0.14	2070
Vertical Inequality, Growing Season	0.43	0.21	2070
BGRI, Adm. Units	0.29	0.19	2070
BGRI, Growing Season, Adm. Units	0.34	0.18	2070
BGI, Temperature	0.25	0.17	2070
BGI, Growing Season, Temperature	0.22	0.19	2070
Between Group Night Light Inequality, BGNI	0.48	0.25	368
C. Other Variables			
Average Yearly Rainfall	34.23	27.83	2070
Average Yearly Temperature	23.63	3.19	2070
Malaria Prevalence Index	0.58	0.32	2070
Avg. Yearly Rainfall along Main Roads	33.97	28.52	2070
GDP per capita (constant 2005 price), '000	1.74	2.38	1895
Population, '000,000	10.99	15.46	2070
Openness, share	57.63	32.20	1895
Individuals affected by natural disasters, thousands	128.79	848.28	2070
Institutional quality	0.31	0.27	1832
Years without Ethnic Conflict	16.88	13.32	2070
Ethnic Conflict in Neighboring Countries	0.53	0.74	2070
Avg. Instit. Quality in Neighboring Countries	0.30	0.20	2025

Notes: All inequality measures, as well as the Institutional quality variable, have been normalized by taking $(X - X_{min}) / (X_{max} - X_{min})$. Consecutive years of conflict are coded as missing for the Conflict Onset variables. Missing observations for some variables in group C are due to the fact that some data sources do not fully cover the sample of countries and years included in our analysis.

Table 5.3: Main Effects

Dependent Variable:	Ethnic Conflict								Ethnic Onset
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BGRI	0.487	0.184		−0.038				−0.221	−0.121
	(0.213)**	(0.122)		(0.114)				(0.204)	(0.075)
BGRI, Growing Season			0.301	0.333	0.207			0.446	0.316
			(0.151)**	(0.163)**	(0.118)*			(0.213)**	(0.103)***
BGRI, Growing Season, t-1					0.156				
					(0.080)**				
BGRI, Growing Season, t-2					0.122				
					(0.081)				
BGRI, Growing Season, t-3					−0.003				
					(0.064)				
WGI						0.164		0.131	
						(0.281)		(0.326)	
WGI, Growing Season						0.412		0.378	
						(0.363)		(0.353)	
Vertical Inequality (VI)							0.035	0.235	
							(0.143)	(0.221)	
VI, Growing Season							0.275	−0.327	
							(0.214)	(0.285)	
Yearly Rainfall (log)	0.051	0.014	0.020	0.020	0.027	0.022	0.020	0.029	0.026
	(0.019)***	(0.026)	(0.027)	(0.028)	(0.030)	(0.027)	(0.029)	(0.031)	(0.013)**
Country Effects	no	yes	yes	yes	yes	yes	yes	yes	yes
Year Effects	no	yes	yes	yes	yes	yes	yes	yes	yes
Country-Specific Time Trends	no	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.06	0.47	0.47	0.47	0.49	0.48	0.47	0.48	0.16
N	2070	2070	2070	2070	1978	2070	2070	2070	1909

Note: All inequality measures have been normalized by taking (X - X_{min}) / (X_{max} - X_{min}). **Ethnic Conflict** is a dummy variable taking on the value of 1 if a country experienced ethnic war in a given year. **Ethnic Onset** is a dummy variable taking on the value 1 of in the first year of an *ethnic* conflict and is missing for the consecutive years of conflict. **Standard errors** are clustered at both the country and the year level. There are 46 countries and 45 years in the sample. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 5.4: Placebo Tests

Dependent variable:	Non-Ethnic Conflict	Non-Ethnic Onset	Ethnic Conflict
	(1)	(2)	(3)
BGRI, Growing Season	−0.042	0.040	0.305
	(0.103)	(0.073)	(0.148)**
BGRI	−0.092	−0.039	−0.007
	(0.125)	(0.082)	(0.123)
BGRI, Growing Season, t+1			0.066
			(0.075)
Yearly Rainfall (log)	−0.039	−0.002	0.021
	(0.028)	(0.012)	(0.028)
Country Effects	yes	yes	yes
Year Effects	yes	yes	yes
Country-Specific Time Trends	yes	yes	yes
R^2	0.43	0.13	0.47
N	2070	1961	2024

Note: All inequality measures have been normalized by taking (X - X_{min}) / (X_{max} - X_{min}). **Non-Ethnic Conflict** is a dummy variable taking on the value of 1 if a country experienced a *non-ethnic* conflict in a given year. **Non-Ethnic Onset** is a dummy variable taking on the value of 1 in the first year of a *non-ethnic* conflict and is missing for the consecutive years of conflict. **Ethnic Conflict** is a dummy variable taking on the value of 1 if a country experienced *ethnic* conflict in a given year. **Standard errors** are clustered at both the country and the year level. There are 46 countries and 45 years in the sample. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 5.5: Robustness Checks I - Additional Controls

Dependent Variable:	Ethnic Conflict						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BGRI, Growing Season	0.268	0.356	0.344	0.408	0.325	0.376	0.366
	(0.112)**	(0.169)**	(0.157)**	(0.163)**	(0.167)*	(0.143)***	(0.140)***
BGRI	−0.081	−0.056	−0.065	−0.117	−0.035	−0.111	−0.099
	(0.073)	(0.117)	(0.124)	(0.132)	(0.113)	(0.117)	(0.116)
Ethnic Conflict, t-1	0.581					0.496	0.487
	(0.071)***					(0.107)***	(0.109)***
Malaria prevalence		−0.100				0.071	0.145
		(0.146)				(0.140)	(0.141)
Rainfall along main roads		−0.091				−0.096	−0.113
		(0.069)				(0.059)	(0.065)*
BGRI, Growing Season, Temperature			−0.444			−0.154	−0.245
			(0.568)			(0.378)	(0.413)
BGRI, Temperature			0.616			0.385	0.268
			(0.330)*			(0.292)	(0.267)
Yearly Temperature (log)			−0.507			−0.423	−0.481
			(0.442)			(0.247)*	(0.199)**
GDP per capita (log)				−0.129		−0.088	−0.135
				(0.068)*		(0.040)**	(0.096)
Population (log)				−0.053		0.042	−0.164
				(0.255)		(0.188)	(0.623)
Openness				−0.000		−0.000	−0.001
				(0.001)		(0.000)	(0.001)
Natural disasters				0.003		0.003	0.003
				(0.001)**		(0.001)**	(0.001)**
Institutional quality				0.000		0.014	−0.085
				(0.060)		(0.044)	(0.050)*
Years without ethnic conflicts				0.409		−0.033	−0.045
				(0.098)***		(0.087)	(0.090)
Ethnic Conflict in neighboring countries					0.002	−0.006	−0.009
					(0.022)	(0.011)	(0.012)
Inst quality in neighboring countries					−0.325	−0.120	−0.095
					(0.128)**	(0.054)**	(0.116)
Yearly Rainfall (log)	0.023	0.107	−0.001	0.048	0.023	0.115	0.129
	(0.015)	(0.075)	(0.034)	(0.026)*	(0.028)	(0.065)*	(0.070)*
Country Effects	yes	yes	yes	yes	yes	yes	yes
Year Effects	yes	yes	yes	yes	yes	yes	yes
Country-Specific Time Trends	yes	yes	yes	yes	yes	yes	yes
Lags included	no	no	no	no	no	no	yes
R^2	0.65	0.48	0.48	0.60	0.48	0.67	0.67
N	2070	2070	2070	1765	2025	1753	1708

Note: All inequality measures, as well as the Institutional quality variable, have been normalized by taking $(X - X_{min}) / (X_{max} - X_{min})$. **Ethnic Conflict** is a dummy variable taking on the value of 1 if a country experienced *ethnic* conflict in a given year. We refer to the main text for a detailed definition of the other variables. In **regression 7** we include lags $t-1$ for all control variables that have been added in regressions 2 to 5, with the only exception of *Years without ethnic conflicts*. **Standard errors** are clustered at both the country and the year level. There are 46 countries and 45 years in the sample. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 5.6: Robustness Checks II - Additional Checks

Dependent Variable:	Ethnic Conflict			
	Only SSA	Only from 1980s	Administrative Borders	
	(1)	(2)	(3)	(4)
BGRI, Growing Season	0.400	0.410		0.444
	(0.187)**	(0.180)**		(0.217)**
BGRI	−0.004	−0.099		−0.324
	(0.124)	(0.188)		(0.225)
BGRI, Growing Season, Adm. Units			0.249	−0.085
			(0.154)	(0.174)
BGRI, Adm. Units			0.077	0.331
			(0.103)	(0.219)
Yearly Rainfall (log)	0.019	0.063	0.018	0.021
	(0.036)	(0.028)**	(0.028)	(0.028)
Country Effects	yes	yes	yes	yes
Year Effects	yes	yes	yes	yes
Country-Specific Time Trends	yes	yes	yes	yes
R^2	0.48	0.66	0.47	0.48
N	1845	1058	2070	2070

Note: All inequality measures, as well as the Institutional quality variable, have been normalized by taking $(X - X_{min}) / (X_{max} - X_{min})$. **Ethnic Conflict** is a dummy variable taking on the value of 1 if a country experienced *ethnic* conflict in a given year. In **regression 1** we restrict the sample to SSA. In **regression 2** we only consider the years since 1980. In **regressions 3** and **4** we include inequality measures constructed using administrative borders (level 1). **Standard errors** are clustered at both the country and the year level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table 5.7: Night-light density, 1992-1999

Dependent variable:	BGNI (Night Light Density)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BGRI, Growing Season	0.159	0.192	0.292	0.240			0.115	0.147
	(0.054)***	(0.094)**	(0.173)*	(0.113)**			(0.074)	(0.065)**
BGRI			−0.107					
			(0.206)					
BGNI, Night Light, t-1		−0.123	−0.122	−0.129				
		(0.175)	(0.177)	(0.179)				
BGRI, Growing Season, t-1				0.099				
				(0.091)				
BGRI, Growing Season, t+1				0.039				
				(0.094)				
WGI, Growing Season					0.116		0.285	−0.031
					(0.458)		(0.410)	(0.155)
WGI					0.526			
					(0.427)			
VI, Growing Season						0.261	0.041	0.082
						(0.192)	(0.173)	(0.145)
VI						−0.074		
						(0.171)		
Yearly Rainfall (log)	0.020	0.013	0.012	0.019	0.018	0.019	0.020	0.004
	(0.023)	(0.023)	(0.023)	(0.025)	(0.024)	(0.023)	(0.023)	(0.010)
Country Effects	yes	yes	yes	yes	yes	yes	yes	yes
Year Effects	yes	yes	yes	yes	yes	yes	yes	yes
Country-Specific Time Trends	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.99
N	368	322	322	322	368	368	368	448

Note: All inequality measures, as well as the Institutional quality variable, have been normalized by taking (X - X_{min}) / (X_{max} - X_{min}). **BGNI** measures between ethnic group inequality using night light density data instead of rainfall. There are 46 countries in the sample, except for **regression 8**, where the sample is extended to 56 countries. **Standard errors** are clustered at both the country and the year level. There are 8 years in the sample. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Sammanfattning

Denna avhandling består av fyra fristående artiklar. De tre första artiklarna handlar om folkmordet i Rwanda.

1994 dödade hutugärningsmän ungefär 800 000 människor från tutsiminoriteteten under en så kort period som 100 dagar. Detta förbluffande antal döda kunde enbart åstadkommas genom att hundratusentals i civilbefolkningen (ungefär ca 85 procent av det totala antalet gärningsmän) anslöt sig till milisen och armén i dödandet. Det massiva deltagandet från civilbefolkningen är sannolikt ett av de mest förbryllande kännetecknen för folkmord. Vanliga lantbrukare dödade sina grannar, arbetare dödade sina kollegor, lärare dödade sina studenter och vice versa, genom att hacka dem till döds med machetes.

Journalister, makthavare och vissa forskare som sysslar med internationella relationer har gjort det allmänt känt att civilbefolkningens deltagande, i allmänhet, är en följd av ett utbrott av urgammalt hat som inte går att hejda. För att illustrera detta, så kommenterar en pensionerad amerikansk general (James W. Nance) ämnet, med hänvisning till kriget i Bosnien: ”Låt dem slåss. De har slagits i tusentals år.” (egen översättning) (citeras i Ashbrook T. 1995: US Weighs Solo Role, Multilateral Efforts, Boston Globe, May 3). Min forskning om folkmordet i Rwanda visar däremot att det enorma deltagandet bland civilbefolkningen inte var en följd av en plötslig explosion av urgammalt hat, som kastade in landet i en konflikt som inte gick att stoppa där alla slogs mot alla, utan att det civila deltagandet noggrant främjats av de centrala ledarna i Kigali – rationella aktörer – som använde sig av obligatoriska samhällsmöten under åren före folkmordet för att förbereda befolkningen (artikel 1) och skickade runt sina milismän och armén medan folkmordet pågick för ge slutliga order (artikel 2). Slutligen visar artikel 3 att den politiska hutuelitens strategi att döda tutsier faktiskt var framgångsrik; sex år efter folkmordet var den ekonomiska situationen bättre på platser där det förekommit mer folkmordsvåld. Låt mig utveckla detta.

I det andra kapitlet i min avhandling med titeln Förberedelse för folkmord – samhällsarbete i Rwanda (Preparing for Genocide: Community Work in Rwanda) (samförfattat med Evelina Bonnier, Jonas Poulsen och Miri Stryjan), frågar vi om och hur den politiska eliten förbereder civilbefolkningen för deltagande i en våldsam konflikt. Som noterats ovan så studerar vi denna fråga empiriskt genom att använda data på bynivå från folkmordet i Rwanda 1994. Varje lördag före 1994 så var byborna i Rwanda tvungna att träffas för att arbeta med samhällets infrastruktur, en praxis som kallas Umuganda. Även om Umuganda ursprungligen utformades som obligatoriska arbetsmöten för att förbättra byarnas infrastruktur så tyder tidigare rapporter om folkmordet på att vid 1990-talets början så missbrukades dessa möten av den politiska hutueliten för att sprida tutsiefientliga känslor och förbereda befolkningen för folkmord. För att beräkna den kausala effekten av dessa möten så använder vi oss av den tvärsnittsvariation i mötesintensiteten som föranleds av exogena vädervariationer. Tanken är enkel: vi förväntar oss att mötena ska vara mindre trivsamma när det regnar och vidare att de ställs in helt och hållet vid kraftigt regn.

Vi finner att en ytterligare regnig lördag resulterade i ett fem procent lägre deltagande i folkmordsvåldet bland civilbefolkningen. Intressant nog så drivs detta resultat helt av platser som kontrolleras av hutupartier som är positivt inställda till folkmord. På de få platser där tutsiminoriteter innehar makten så blir effekterna de motsatta, vilket tyder på att på dessa platser så användes dessa möten för att skapa band mellan de två etniciteterna.

I det tredje kapitlet i min avhandling, med titeln Mobilisering av massorna för folkmord (Mobilizing the Masses for Genocide), så ställer jag frågan om den politiska eliten använder beväpnade trupper för att främja civilbefolkningens deltagande i våldet. Allokeras dessa beväpnade styrkor för att maximera civilbefolkningens deltagande? Och hur mobiliserar de civilbefolkning? Jag studerar ännu en gång empiriskt dessa tre frågor genom att använda data på bynivå från folkmordet i Rwanda 1994 – med fokus på tiden för folkmordet.

För att identifiera den kausala effekten av dessa milismän använder jag en instrumentvariabelstrategi. Specifikt så utnyttjar jag en tvärsnittsvariation i de beväpnade gruppernas transportkostnader till följd av exogena väderfluktuationer: det kortaste avståndet i varje by till huvudvägen i kombination med regn längs de jordvägar som går mellan huvudvägen och byn. Tanken är återigen enkel: Jag förväntar mig att armens och milisens förflyttningar, vilka sker med motorfordon, är begränsade av det kraftiga regn som karaktäriserar den första regnperioden, vilken delvis överlappar med folkmordet, och detta ju mer desto längre de måste resa.

Med vägledning av en enkel modell så hittar jag följande svar på de tre centrala frågorna: (1) en ytterligare beväpnad gruppmedlem gav 7,3 fler civila gärningsmän, (2) beväpnade gruppledare reagerade rationellt på exogena transportkostnader och skickade strategiskt sina män för att maximera det civila deltagandet och (3) i majoriteten av byarna så agerade beväpnade gruppmedlemmar som förebilder och civilbefolkningen följde order, men i byar med en hög nivå av äktenskap mellan etniska grupper, fick man tvinga civilbefolkningen att deltaga.

Slutligen så tyder en överslagsberäkning på att ett militärt ingripande med de olika beväpnade grupperna som mål – enbart 10 procent av gärningsmännen men ansvariga för 83 procent av morden – skulle ha kunna stoppat folkmordet i Rwanda.

I kapitel fyra, med titeln Arvet från de politiska massmorden – bevis från folkmordet i Rwanda (The Legacy of Mass Killings: Evidence from The Rwandan Genocide) (samförfattat med David Yanagizawa-Drott), studerar vi hur politiska massmord påverkar senare ekonomisk prestation, än en gång med hjälp av data från folkmordet i Rwanda. Vi finner att hushåll i byar som hade högre nivåer av våld som förorsakats av radioutsändningar har en högre levnadsstandard sex år efter folkmordet. De åtnjuter en högre konsumtionsnivå, äger fler tillgångar, så som mark, boskap och varaktiga konsumtionsvaror, och produktionen per capita från jordbruket är högre.

Dessa resultat överensstämmer med den malthusianska hypotesen att massmord kan öka levnadsstandarden genom att minska befolkningsstorleken och omfördela produktionstillgångar från de avlidna till den kvarvarande befolkningen. Vi finner emellertid även att våldet påverkade åldersfördelningen i byarna, ökade fertilitetsgraden bland kvinnliga överlevande och minskade barnens kognitiva förmåga. Sammantaget visar våra resultat att massmord kan ha positiva effekter på levnadsstandarden bland de överlevande på kort sikt, men att dessa effekter kan försvinna på lång sikt.

I det sista kapitlet med titeln Etnisk inkomst ojämlikhet och konflikt i Afrika (Ethnic Income Inequality and Conflict in Africa) (samförfattat med Andrea Guariso), visar vi att inkomstojämlikheten mellan olika etniska grupper ökar sannolikheten för etnisk konflikt i Afrika. Då de flesta länder i Afrika är starkt beroende av jordbruksproduktion som bevattnas genom nederbörd, utnyttjar vi den exogena variationen i nederbörd i form av regn i varje etnisk grupps "hemland" (homeland) för att identifiera kausala effekter. Vi finner att en ökning uppgående till en standardavvikelse i den etniska inkomstojämlikheten ökar sannolikheten för en etnisk konflikt med ca 66 procent.

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