Preparing for Genocide: Quasi-Experimental Evidence from Rwanda*

Evelina Bonnier,† Jonas Poulsen,‡ Thorsten Rogall,§ Miri Stryjan¶

Abstract

This paper shows how state-controlled community meetings can facilitate large-scale mobilization of civilians into violence. We analyze a Rwandan community program that required citizens to participate in community work and political meetings every Saturday in the years before the 1994 genocide. We exploit cross-sectional variation in meeting intensity induced by exogenous weather fluctuations, and find that a one standard-deviation increase in the number of rainy Saturdays before the genocide decreased civilian violence by 17 percent. We find evidence that the meetings provided an arena for local elites to spread propaganda and bring people together. In research and policy, community meetings are often treated as positive, community building forces. Our results indicate that they can also lead to negative outcomes. This should, however, not suggest that such meetings are inherently destructive. Instead, community meetings should be understood as powerful tools and their effects depend on the political intention of the leaders.

Key Words: Genocide, Conflict, Political Elites, Industrial Organization of Conflict, Collective Action, Community Meetings
JEL classification: D74, I38, N47

*We especially thank Marijke Verpoorten and David Yanagizawa-Drott for kindly sharing their data with us, and Felix Rubogora for valuable assistance with media search. We are grateful to the editor Andrew Foster, and to a number of anonymous referees for constructive comments. This paper has benefited from valuable input from Eli Berman, Martina Björkman Nyqvist, Benjamin Crost, Tom Cunningham, Gordon Dahl, Melissa Dell, Oeindrila Dube, Willa Friedman, Matt Gentzkow, Paola Giuliano, Jonas Hjort, Chang-Tai Hsieh, Saumitra Jha, Juanna Joensen, Magnus Johannesson, Todd Kaplan, Erik Lindqvist, Andreas Madestam, Eva Mörk, Suresh Naidu, Nathan Nunn, Torsten Persson, Cristian Pop-Eleches, Marit Rehavi, Jacob Shapiro, Jesse Shapiro, David Strömberg, Jakob Svensson, Francesco Trebbi, Erik Verhoogen, Miguel Urquiola, and David Yanagizawa-Drott, as well as seminar participants at Harvard, UCSD, Columbia, Tel-Aviv University, ESOC, ASWEDE, SSE, IIES, NEUDC, EIEF, and the Annual Bank Conference on Africa. Financial support from Handelsbanken's Research Foundations is gratefully acknowledged.

†Stockholm School of Economics.

‡Uppsala University.

§Corresponding Author, University of British Columbia, 6000 Iona Drive, Vancouver, BC, V6T1L4, thorsten.rogall@ubc.ca.

¶Ben-Gurion University of the Negev, miristry@bgu.ac.il; miri.stryjan@aalto.fi.

1 Introduction

What makes ordinary citizens participate in mass violence? Most theories seeking to explain this build on a rational choice framework where individuals weigh the expected benefits and costs of participation against each other (see e.g. Finkel and Muller, 1998; Finkel et al., 1989; Muller and Opp, 1986; Oberschall, 1994). According to these models, collective-action problems make large-scale civilian participation in violence unlikely which would explain why mass violence is often committed by government forces or formally organized groups. Yet, some of the worst atrocities in modern history have been carried out by informal groups of seemingly ordinary citizens. One example is the Lynching Era (1878–1939) in the U.S., where entire white communities rounded up African Americans to expunge them. Other examples are the anti-Jewish pogroms in the Russian Empire throughout the 19th century, and the 2007–2008 post-election violence in Kenya.

In this paper, we focus on the infamous case of civilian violence during the Rwandan genocide of 1994, where around 430,000 Hutu civilians joined the army and militia in killing an estimated 800,000 people (predominantly Tutsi) in just 100 days. In particular, we want to understand how a mandatory community program called *Umuganda*, which required citizens to participate in community work and political meetings every Saturday, contributed to the large-scale mobilization of Hutu civilians in the genocide. Studies using qualitative methods have pointed to such a link, but this study provides the first quantitative evidence of a causal relationship between *Umuganda* and participation in genocide violence.

Our results show a negative relationship between canceled *Umuganda* meetings before the genocide and civilian participation in the genocide violence: having fewer meetings just before 1994 is associated with a significantly lower civilian participation rate in violence. Tracing the effects over time suggests that meetings held during the last six months before the genocide were the most important.

To understand the mechanisms we use a framework suggested by Mann (2005) for explaining the determinants of genocide: we show that *Umuganda*, in the years just before the genocide, facilitated both top-down pressure and bottom-up pressure. Top-down pressure by providing an arena for government officials to spread political propaganda, and bottom-up pressure as *Umuganda* involved social interaction which, according to work by e.g. Balliet (2010) and Tanner (2011) helps increase popular support for, and facilitates coordination on extreme standpoints, such as the genocidal agenda in Rwanda. For instance, consistent with a top-down

mechanism at work, we find that the effects of *Umuganda* depend on the political affiliation of the local politician in charge of the meetings. The effects disappear in areas with no local leadership and there is suggestive evidence that the effects reverse in sectors governed by opposition parties.[1]

Alternative channels are addressed and refuted. These include the possibility that economic motives drive our effects, and pure bottom-up channels such as voluntary social interaction during Saturdays altering civilians' perceptions about the Tutsi group.

Identifying the causal effect of *Umuganda* on participation in genocide violence is difficult because of plausible omitted variable bias. On the one hand, area-specific unobservable characteristics that affect both genocide participation and *Umuganda* intensity – such as local leader quality and political views – could produce a spurious positive correlation between the two, thus biasing the estimate upwards. On the other hand, if *Umuganda* meetings were strategically used in areas where genocide participation would otherwise have been low, the estimate would be biased downwards.

To overcome this endogeneity challenge, we use exogenous rainfall variation to estimate the effect of *Umuganda* meetings on participation in civil conflict. The idea is simple: under heavy rain, meeting attendance will be lower and delays and cancellations ensue. Although we lack data on actual meeting attendance, we know that *Umuganda* meetings took place on Saturdays, which enables us to isolate the effect of *Umuganda* from general rainfall effects (working e.g. through income from agriculture). More precisely, we focus on variation in Saturday rainfall, controlling for rainfall on all six other weekdays in the same time period. Our explanatory variable is the number of Saturdays with heavy rainfall during the 3.5 year period leading up to the genocide.[2]

We proxy for genocide violence by the number of persons prosecuted in the local "Gacaca

[1] In opposition-led sectors, it is unlikely that local leaders would push the agenda of the government in *Umuganda* meetings, and it is possible that meetings in such sectors helped overcome animosity between villagers from different ethnic groups, explaining this sign reversal.

[2] We focus on the period from October 1990, to March 1994. After the start of the civil war on October 1, 1990, tensions between the Hutu and Tutsi populations intensified, and the Hutu-dominated government became more aggressive towards the Tutsi minority, eventually culminating in the genocide starting in April 1994. The increasing interethnic hatred was for instance expressed in the government-funded journal *Kangura* (Melvern, 2004).

courts"[3] normalized by sector Hutu population.[4] Importantly, these courts distinguished between civilian perpetrators and organized perpetrators such as members of the militia, the national army or the local police. We focus on civilian perpetrators, for two reasons: First, we are particularly interested in understanding what makes civilians turn to violence, and second, militia and organizers typically did not reside in the villages where they were prosecuted for crimes, and would thus not have been affected by rainy Saturdays in those locations during the pre-genocide period.

The results show that a one standard-deviation increase in the number of rainy Saturdays leads to a 17 percent decrease in the civilian participation rate.[5] We can provide a first placebo check by examining heavy rainfall on all six other weekdays in the same time period, for which we find no effect on civilian violence. This placebo test also allows us to rule out that other factors correlated with rainfall, such as geography, are driving the results as these factors should be correlated with rainfall on any weekday. We nevertheless show that the results are robust to controlling for geographical factors such as sector ruggedness. To control for local characteristics, we include 142 commune fixed effects. Thereby we 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. One concern, is that other events happening on Saturdays such as festivals, or family celebrations, bias the results. We will provide evidence to ease this concern.

The paper contributes to the literature in several ways. It relates to the literature on the roots of individual participation in violent conflict (Blattman and Miguel, 2010). While recent work has focused on government policy, income, foreign aid, epidemics and institutions as determinants of conflict and participation in violence (Dell, 2015; Mitra and Ray, 2014; Nunn and Qian, 2014; Gonzalez-Torres and Esposito, 2017; Dube and Vargas, 2013; Besley and Persson, 2011, respectively), our paper highlights state-sponsored community meetings as an important tool for promoting civilian participation in violence. In particular, we find evidence that the meetings provided a propaganda platform complementary to messages spread by radio,

[3] About 10,000 of these "Gacaca courts" were set up across the country to process the crimes committed during the genocide. Using prosecution rates instead of actual genocide participation rates may introduce some bias. However, the Gacaca data are strongly correlated with measures of genocide violence from various other sources, and we present a number of additional tests using alternative genocide measures to rule out that systematic errors (such as survival bias) are biasing our results.

[4] A sector is the second smallest administrative unit in Rwanda, with an average size of 14 square kilometers and 4,900 inhabitants.

[5] Results are robust to different ways of specifying "heavy rainfall" as is discussed in detail in Section 5.1.

the effect of which has been documented in the Rwandan context by Yanagizawa-Drott (2014) and in other settings by e.g. DellaVigna et al. (2014), Armand et al. (2017) and Adena et al. (2015).

Our finding is especially interesting given the recent popularity of community meetings as a tool in policy implementation by NGOs and development agencies, through "Community Driven Development" initiatives (see e.g. Casey et al. (2012), Heß et al. (2018), Mansuri and Rao (2012), White et al. (2018). There is a widely held belief that such meetings, and an active civil society more generally, foster social capital by providing arenas for people to meet, solve free-rider problems, and create public goods (Guiso et al., 2008; Grootaert and Bastelaer, 2002; Knack and Keefer, 1997; Putnam, 2000). Community-meeting practices similar to the one we study exist in Burundi, and have been proposed in the Democratic Republic of Congo (DRC) and Kenya. Our results suggest that community meetings can be effective vehicles for influencing and controlling a population. Rather than concluding that community meetings are inherently destructive, we claim that their impact depends on the political intentions of the leaders who order and implement the meetings.[6] Our findings can be read as a call for caution against uncritical equation between such meetings and ubiquitously positive community building forces. A closely related recent paper is Linek (2019), who adapted our methodology – using rainfall data from the mid-2000s as a proxy for *Umuganda* meeting attendance – to study how local officials pressure Rwandan villagers to conform to a centrally dictated development agenda.

Two other recent papers close to our work are Acemoglu et al. (2014) and Satyanath et al. (2017). While the former shows that local chiefs in Sierra Leone use civil society organizations (a commonly used proxy for social capital) to entrench their autocratic rule and monitor the society, the latter shows that social clubs in Germany post-World War I facilitated recruitment into the Nazi party. We add to these findings by providing well-identified evidence that local political elites in Rwanda used community meetings to not only make civilians relate with a murderous regime but also take an active part in violence. As such, our findings similarly show socially destructive potential of community meetings, and, in line with Acemoglu et al. (2014), warn against taking the presence of civil society institutions as an indication of social capital. However, different from Acemoglu et al. (2014) and Satyanath et al. (2017) who focus on more

[6] A similar claim can be made for radio messages: while Yanagizawa-Drott (2014) finds that RTLM propaganda was used to foster violence during the genocide, Blouin and Mukand (2019) show that radio broadcasts are used for nation building in post-genocide Rwanda.

long-term effects of social capital, our study suggest that even short-term interventions can have important effects. Furthermore, we provide detailed evidence on the relevant mechanisms – in particular we highlight the crucial interplay of both top-down and bottom-up channels.

We also contribute to the literature on the Rwandan genocide (Friedman, 2013; Verpoorten, 2012b,a; Verwimp, 2006; Yanagizawa-Drott, 2014; Straus, 2006) by providing novel evidence of the mechanisms at work in its preparation. A related study by Rogall (2017), shows how the Rwandan political elite dispersed army and militia to foster civilian participation in violence during the genocide. We complement that work with evidence that civilian violence was affected by propaganda and social interactions during *Umuganda*. In particular, our study shows quantitative and causal evidence of a mechanism that has been put forward in qualitative studies relying on much smaller samples and correlations (Lawrence and Uwimbabazi, 2013; Verwimp, 2006; Straus, 2006).

Lastly, on the methodology side, our results add to the recent discussion on the effects of rainfall on conflict other than through the income channel (Dell et al., 2014; Iyer and Topalova, 2014; Rogall, 2017; Sarsons, 2015).

The rest of the paper is structured as follows. Section 2 provides a brief description of the historical and institutional background of the Rwandan Genocide and *Umuganda*. In Section 3, we lay out the conceptual framework for the study. Section 4 describes the data. In Section 5 we present the empirical strategy. Section 6 presents the main results and Section 7 discusses possible mechanisms, guided by the conceptual framework. Section 8 concludes the paper.

2 Historical and Institutional Background

2.1 The Rwandan Conflict

Tensions between the Hutu and Tutsi populations have been present in Rwanda at least since colonial times. The origins of the two groups and the distinction between them is debated.[7] What seems clear, however, is that Belgian colonizers deepened the division between the two groups, and deliberately favored the Tutsi minority. Under Belgian colonial rule, strong tensions rose between the two groups that culminated in the Rwandan revolution of 1959, when the Tutsi monarchy was replaced by a Hutu republic. During this period, many Tutsi civilians were

[7]The Tutsi minority (with a pre-genocide population share of around 10 percent) is said to have descended from Hamitic migrants from the north of Africa, and the Hutu majority from the Bantu group, who traditionally lived in Rwanda. However, others argue that the two ethnicities have a common ancestry.

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 ceased.

In 1973 – paramount to the introduction of a modern version of *Umuganda* – Juvénal Habyarimana took power in Rwanda through a coup d'état. In 1975, he created a new party called the National Revolutionary Movement for Development (Mouvement Révolutionaire National pour le Développement, or MRND), which would dominate Rwandan political life for more than two decades as the single party in a one-party state. The turning point came in October 1990, when the Rwandan Patriotic Front (RPF), a Tutsi-dominated rebel group that had emerged in exile, invaded Rwanda from Uganda and started the Rwandan civil war. Episodes of fighting between the RPF and the Armed Forces of Rwanda (FAR) continued until August 1993 and the signing of the Arusha Peace Accord.

Parallel to the civil war, rising demands for democracy led to the adoption of a new constitution and the implementation of a multi-party system.[8] The newly established opposition parties became actively engaged in the peace negotiations and formed an alliance with the RPF, which meant that the incumbent MRND was threatened both from within and outside of the country.[9] Mobilization of popular support became a top priority and to help in the recruitment of new members, all parties, including the MRND, created youth wings (which later became militias) (Viret, 2010).

A key role in the local mobilization was played by mayors.[10] They were the lowest level of political representation in Rwanda, heads of the commune, and responsible for *Umuganda*. Traditionally, mayors were appointed by the President on the recommendation of the Minister of Interior (Des Forges, 1999). Hence, all mayors were officially members of the MRND. Following the introduction of multipartyism, however, a number of mayors chose to switch party (voluntarily or as a result of powerful persuasion). Moreover, in the ongoing peace negotiations, it was agreed that a limited form of elections would be held in some pre-selected communes in March 1993. These elections resulted in around 25 percent of the mayors being

[8]New parties were allowed to register from July 1991. The main opposition parties were the Republican Democratic Movement (MDR), the Social Democratic Party (PSD), the Liberal Party (PL), the Christian Democratic Party (PDC) and the Coalition for the Defence of the Republic (CDR). See Guichaoua (2015) for a more detailed list of parties.

[9]The alliance between the opposition parties was, however, not long-lasting. In all parties, groups of hardliners that were not happy with the outcome of the Arusha Accords created "Hutu Power" factions that would later come to support the genocidal agenda.

[10]The local term for mayor is *bourgmestre*.

replaced, with the new mayors entering office in October 1993 (Des Forges, 1999).[11] While the MRND remained the largest party, its dominance was clearly eroding. This led the MRND into taking more extreme measures to stay in power (Straus, 2006), involving open and aggressive promotions of the (Hutu) in-group and harassment and violence against the (Tutsi) out-group (Wagner, 1998).

Finally, on April 6 1994, an airplane carrying President Habyarimana was shot down over Kigali. Soon after the attack, extremists within the Hutu-dominated parties announced a new interim government and ethnic violence erupted across the whole country. Around 800,000 people, mostly Tutsi and politically moderate Hutu, lost their lives. The mass killings stopped in mid-July 1994, when the RPF defeated the FAR and their associated militia groups, most notably the *Interahamwe*.[12]

2.2 History of *Umuganda*

The origin of *Umuganda* can be traced 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). *Umuganda* was initially considered a social obligation (Melvern, 2000). However, this changed during the colonial period, when the Belgian colonizers used *Umuganda* to organize compulsory work. Consistently, the locally employed term for *Umuganda*, in this period, was *uburetwa*, or *forced labor* (IRDP, 2003). All men had to provide 60 days of communal work 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).

When *Umuganda* was re-introduced by the new Habyarimana government in 1974, the program combined a practical motivation – achieving development objectives despite weak state finances – with a strong ideological element. Participation remained compulsory, which was enforced through government coercion, and neighborhood leaders had the right to fine households that failed to participate (Des Forges, 1999). While the mayors were responsible for organizing *Umuganda* in their communes (World Bank, 1987), the central government decided on what projects at least one adult male per family had to work *every* Saturday morning (Uvin,

[11] In these elections, only a group of around 50 people, including village committees, heads of development projects, and heads of local political parties, were allowed to vote.

[12] For more detailed accounts of what happened during this 100-day period, see e.g. Straus (2006), Hatzfeld (2005), Dallaire (2003), Des Forges (1999), and Prunier (1995).

1998). The main tasks included various types of erosion controls, maintenance of roads, and construction of communal buildings.

After the community work, meetings were held where local or higher officials disseminated information to the villagers about the governing principles of the ruling MRND party and messages from the central government (Guichaoua, 1991). Our conceptual framework focuses on these political meetings that were held at the end of *Umuganda*, since these are highlighted as important in previous qualitative work (Lawrence and Uwimbabazi, 2013; Verwimp, 2006; Straus, 2006) that relate *Umuganda* to genocidal violence. We also discuss and test purely economic channels and show that these are unlikely to drive our findings, but the nature of our data does not allow us to rule out that the "work" component of *Umuganda* played a role for our results, for instance through villagers interacting or sharing information while they worked together on public projects.

3 Conceptual Framework

In this section, we describe the political elements of *Umuganda* and then propose and discuss the main channels through which *Umuganda* meetings could have affected civilian participation in genocidal violence.

3.1 Political Elements of *Umuganda*

Together with tax collection, *Umuganda* was the main means available to the mayors for fulfilling their essential duties. According to a World Bank report from 1987, these duties were *"to 'inspire' the local population and officials to implement the ideals of the MRND"* (World Bank, 1987, p. 8). When *Umuganda* was re-introduced in 1974, President Habyarimana turned it into a political doctrine (Mamdani, 2001). Verwimp (2000, p. 344) cites Habyarimana:

> *"The doctrine of our movement [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."*

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 pastoralists.

Universal instructions for *Umuganda* were not specified in legislative or regulatory documents, and thus the exact setup of the work and meetings reflected the local mayors' management style. (World Bank, 1987, p. 63). Despite this, the implementation of *Umuganda*, including the meeting component with information from MRND, was mandated by the national government (Guichaoua, 1991).

Although a causal link has not yet been established between participation in *Umuganda* before the genocide and civilian participation in violence during the genocide, official reports, as well as research from other disciplines point to the importance of *Umuganda* as an instrument for local party and state officials to mobilize the population. A number of accounts from the late 1980s and early 1990s document that a strong political element was tied to its practice (World Bank, 1987; Verwimp, 2011; Guichaoua, 1991). Rwandans from all socio-economic groups, be it farmers or intellectuals, were required to participate in *Umuganda* (Guichaoua, 1991). A patriotic focus of *Umuganda*, promoting the "indigenous" Hutu and marginalizing the "non-indigenous" Tutsi, 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). The meetings were therefore ideal for reaching the entire population.

Moreover, in influential work, Straus (2006) reports that 88 percent of the interviewed genocide perpetrators had regularly participated in *Umuganda* before the genocide. The association between pre-genocide *Umuganda* meetings and genocidal violence is further supported by accounts that during the genocide itself, the notion of *Umuganda* was explicitly associated with the obligation to participate in violence. During this period, *Umuganda* was used together with the new term *gukorn akazi*, or "do the work," which essentially meant "kill the Tutsi" (Verwimp, 2013). Slogans previously related to *Umuganda* such as "clearing bushes and removing bad weeds" now had a completely altered connotation (Lawrence and Uwimbabazi, 2013).[13]

[13]It is important to clarify that during the genocide period itself, regular activities and practices were discontinued, and it is therefore not relevant to investigate Saturdays during the genocide (we do, however, control for rainfall during the genocide period as a robustness check). For instance, Hatzfeld (2005), interviewing several civilian perpetrators after the genocide, notes that social life completely stopped during the genocide. 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."*

3.2 Mechanisms

We argue that *Umuganda* in the early 1990s helped to create the structure necessary for engaging a large number of Hutu civilians in mass violence during the genocide.

According to Mann (2005), most genocides in history primarily have to do with political power relations, exercised in three distinct ways: top-down pressure from the state; side-way pressure from militia; and bottom-up pressure from core constituencies. Following this terminology, *Umuganda* before the genocide may be associated both with top-down pressure, as it provided government officials with an arena for establishing their leadership and disseminating political *propaganda*, and with bottom-up pressure, as it also involved elements of social interaction that could increase *popular support* for the genocidal agenda.

Management (top-down) Since *Umuganda* required local leaders to summon their constituents it formed an opportunity for them to learn to control, organize and lead the population (Verwimp, 2013) – a skill that could facilitate later mobilization into violent activities. Indeed, the United Nations International Criminal Tribunal for Rwanda (ICTR) have found several mayors guilty of direct and public incitement to commit genocide (e.g. Jean-Paul Akayesu in Taba commune, Joseph Kanyabashi in Ngoma commune, and Elie Ndayambaje in Muganza commune), with the charges including incitement of racial hatred, preparation of lists of persons to be killed, and distribution of arms to militias (see e.g. Judgement and Sentence of Case No. ICTR-98-42-T, 2011). At least the first two of these charges could have been aided by having more *Umuganda* meetings in the period just before the outbreak of the genocide.

Political Propaganda (top-down) The public meetings during *Umuganda* provided an arena for the communication of information and propaganda to community members. In line with the official state policy during the last years before the genocide, the rhetoric used during *Umuganda* became more patriotic and focused on marginalizing the Tutsi (Lawrence and Uwimbabazi, 2013). Information about ongoing political developments in the country, as well as propaganda, was also disseminated over the radio and in newspapers.

Popular Support (top-down and bottom-up) The information and propaganda diffused during *Umuganda* could have also complemented bottom-up channels. Individuals' receptiveness to a message can be affected by observing the reaction of fellow villagers to the same message.

The literature on group polarization shows how a discussion on a topic among like-minded individuals often leads to their post-discussion views being more extreme than before the discussions took place (Sunstein, 2002). A well-established finding is that cooperation in social dilemmas is aided by communication, in particular face-to-face communication (Balliet, 2010, provides a meta-analysis of this literature). Following Browning (2004) and Kershaw (1999), Tanner (2011) describes a radicalization process undergone by civilians participating in ethnic violence. In line with these findings, *Umuganda* meetings could have provided a setting in which the idea of violence against the Tutsi population became increasingly accepted, seen as legitimate and even trivialized, slowly altering ordinary citizens' inclination to participate in violence (Thomson, 2009). Hence, the political meetings of *Umuganda* may have increased popular support for and coordination on extreme standpoints and civilians' probability of cooperation, both with each other and with the government, during the genocide.

3.3 Alternative Mechanisms

In this subsection, we describe some alternative mechanisms that we consider less plausible. These alternative mechanisms will be discussed in Section 7.2.

Voluntary and Forced Interaction (bottom-up) Other potential bottom-up scenarios relate *Umuganda* to changes in village cohesion and in the relationship between Hutu and Tutsi villagers caused by their interaction during *Umuganda* meetings (regardless of the local political elites). More specifically, *Umuganda* may have enabled social interaction between local Hutus and Tutsis, and created "bridging" social capital that increased trust (Putnam, 2000). This would imply a negative association between *Umuganda* and later civilian violence, and is not in line with accounts of the hostile environment during *Umuganda* in the early 1990s. Another possibility with opposite implications is that forced interaction between Hutu and Tutsi during *Umuganda*, at a time when ethnic tensions were growing in Rwanda, led to increased animosity towards the Tutsi population (again regardless of the local political elites).

Economic Channels There is a common perception that the likelihood of conflict depends on economic growth (Ray and Esteban, 2017). An alternative explanation of our findings is that our effects are not the result of *Umuganda* per se, but rather driven by farmers' opportunity costs. If a canceled *Umuganda* freed up time for an additional day of work on the fields, this

may have led to higher agricultural yields and incomes, and consequently higher opportunity costs of participating in conflict. Finally, since the communal work during *Umuganda* focused on providing public goods, an additional, potential mechanism is that *Umuganda* indirectly affected civilian violence by improving local infrastructure and economic growth. This might affect violence in either direction, depending on the assumptions made.

4 Data

In this section we provide a description of the main variables of our analysis. Table 1 reports summary statistics.[14] We combine several datasets from various sources to construct our final dataset with a total of 1,433 Rwandan sectors in 142 communes. Sectors are the second smallest administrative level in Rwanda, and the level for which our outcome variable (civilian perpetrators) is available. Details about the matching process are given in Section A.2 of the Appendix.[15]

4.1 Participation Rates

Ideally, we would like to have a direct measure of genocide participation rates. Since such data do not exist, we follow the literature and use prosecution rates for crimes committed during the genocide as a proxy (Friedman, 2013; Heldring, 2014; Rogall, 2017; Yanagizawa-Drott, 2014). Thus, we use a nation-wide sector-level dataset, provided by the government agency "National Service of Gacaca Jurisdiction," which collected the outcomes of the almost 10,000 local Gacaca courts set up throughout the country to prosecute the *genocidaires*. Importantly, people were prosecuted in the sector where they committed their alleged crimes and did not have to be physically present to be prosecuted.

The courts distinguished between different categories of violent perpetrators. Category 1 includes people that were leaders and organizers, or committed particularly brutal crimes. Most of these perpetrators belonged to the army, militia or were local leaders. Approximately 77,000 cases were handled in this category. Category 2 includes accomplices rather than leaders and instigators. People prosecuted in this category were not members of any of the organized

[14] Description of and summary statistics for additional data are shown in Section A.2 and Table A.1 of the Appendix.

[15] Whenever geo-coded data is used, the matching is done in ArcGIS. In all other cases, the data sets are matched by sector names (within communes).

groups mentioned for the first category and are thus considered to be civilians. In our sample, approximately 416,000 cases were recorded in this category.[16] The exact legal definitions are given in Appendix A.2.

Category 2, which we refer to as "civilian perpetrators," is the focus of this study. In the analysis, we use the number of civilian prosecutions normalized by sector Hutu population as our main outcome variable (Figure 1).[17] On average, there were 290 civilians prosecuted in each sector, which amounts to about 7 percent of the local Hutu population. Importantly, violence against the Hutu population was not trialed in the Gacaca courts (Human Rights Watch, 2011; Longman, 2009). Category 1 data is used in our mechanism discussion.

The reliability of the prosecution data is an important issue for our analysis. In particular, if the prosecution process induced systematic errors, our results may be biased. For instance, in sectors with a lot of violence, the killings might have been so widespread that no witnesses were left to accuse the perpetrators, thus resulting in low prosecution rates. Another concern is that *Umuganda* meetings did not lead to increased participation in violence, but rather only made the prosecution process easier because people knew whom to accuse. In Section 6.2 we address this concern using alternative measures of violence that, contrary to the Gacaca data, do not rely on people meeting to point out perpetrators.

To construct our main outcome variable "Participation Rates" we divide the number of perpetrators by the size of the Hutu population in a given sector. Data on population size and ethnic composition are retrieved from Genodynamics and the IPUMS International census database from 1991 (a 10 percent sample of the full census).[18] The population data is available at the sector level. All other census variables are provided at the commune level.

We proxy for ethnicity at the sector level using the share of commune population that are Hutu and Tutsi, respectively. Thus, the size of the Hutu population in a given sector will be that sector's population size multiplied by the share of Hutus at the commune level. Importantly, the Tutsi minority was represented throughout Rwanda with an average population share of about 9 percent (1.3 percent of the population were Tutsi in the commune at the 25^{th} percentile, 12.3 percent in the commune at the 75^{th} percentile).

[16] The Gacaca data also includes a third category: people that committed property offences. As these crimes are less related to violence, we follow the literature and abstract from it in our analysis.

[17] Figure A.1 in the Appendix maps the number of civilian prosecutions without normalization.

[18] No census data was collected between 1991 and the genocide. However, mobility between sectors was highly limited because of governmental restrictions and land market controls (Prunier, 1995). This implies that the population data from 1991 is informative also of the situation at the eve of the genocide.

4.2 Other Violence Data

For our reliability and robustness checks, we use three alternative datasets on violence. The first alternative dataset consists in commune-level estimates on lethality during the genocide, provided by Genodynamics.[19] These casualty estimates are generated using a Bayesian latent variable model on information from five different sources: the Ministry of Education in Rwanda, the Ministry of Local Affairs in Rwanda, Ibuka (the Rwandan survivor organization), African Rights, and the Human Rights Watch (for more details see: https://genodynamics.weebly.com). Data on the location of mass graves, based on satellite maps from the Yale Genocide Studies Program, are retrieved from Verpoorten (2012a).

The second alternative dataset comes from the Uppsala Conflict Data Program (UCDP) database (Sundberg and Melander, 2013) which provides detailed information on the location, actors and death tolls of violent events since 1989. From this data we can identify the location of (pre-)genocide violence and whether the violence was directed primarily against Tutsi (committed by the Rwandan government) or primarily against Hutu (committed by the RPF). The main sources of UCDP are reports by international news agencies (such as Reuters and the BBC) and reports from non-governmental organizations (such as Human Rights Watch).

Our third alternative source of pre-genocide violence data is Viret (2010) who, with somewhat lesser detail, provides information on the location, actors and death tolls of violent events taking place between October 1990 and March 1994.

4.3 Rainfall Data

To construct our measures of rainfall variation, we use rainfall data from the Climate Prediction Center (CPC) Africa Rainfall Climatology Version 2.0 (ARC2), available in the National Oceanic and Atmospheric Administration (NOAA) Data Catalog (used for instance in Rogall (2017)).[20] This database provides daily rainfall estimates for Africa starting in 1983, and uses high-resolution satellite imagery to obtain rainfall estimates at 0.1-degree ($\sim$ 11 kilometers at the equator) latitude-longitude intervals (Novella and Thiaw, 2012).[21] Actual weather station data, whenever available, is used to validate the underlying meteorological model, which means

[19] Genodynamics is a website providing data and information on the Rwandan genocide. The principal investigators are Christian Davenport, University of Michigan, and Allan Stam, University of Virginia.

[20] Rainfall data is available at https://catalog.data.gov/dataset/climate-prediction-center-cpc-africa-rainfall-climatology-version-2-0-arc2-01d37 (accessed November 16, 2018).

[21] While the rainfall data is available from 1983, we use data from 1984 since rainfall information is missing for several days in 1983.

the rainfall data for each grid cell is not simply the result of a linear extrapolation between station data. The data series starts in the early 1980s which is when the first higher resolution (30 to 60 meters) infrared satellites began circling the earth.

This rainfall data has two important advantages. First, since Rwanda is a small country, the high spatial resolution is crucial to obtain 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*. Given Rwanda's hilly terrain, there is considerable local variation in rainfall (micro-climates). Moreover, these sectors criss-cross the various rainfall grids and each sector polygon is likely to overlap with more than one rainfall grid. The overall rainfall in each sector is thus obtained through a weighted average of the grids, where the weights are given by the relative areas of the sector covered by each grid. Average daily rainfall during our period of interest is about 2.44 mm and there are on average 18 Saturdays with heavy rains (rainfall above 10 mm).

Naturally, there will always be some measurement error in the satellite data. However, as long as such error is uncorrelated with the true dependent and independent variables, it should only work against our findings.

4.4 Geographical Data

A sector boundary map is provided by the Center for Geographic Information Systems and Remote Sensing of the National University of Rwanda (CGIS-NUR) in Butare. This map includes information on both recent and old administrative groupings. Since Rwandan sectors have been reorganized several times after 1994 under different higher administrative units, this information allows us to match sectors across datasets such as the 1991 census and the Gacaca records.

Africover provides spatial maps with major cities and roads derived from satellite imagery. The Africover project is part of the Food and Agriculture Organization of the United Nations' (FAO) assistance to the Nile Basin countries in mapping out infrastructure. The maps are used to calculate several distance measures, such as the distance from sector centroids to the nearest main road, the nearest major city, the nearest country border point, and the distance to Kigali (the capital city) and Nyanza (the capital of the old Tutsi Kingdom). The maps are also used to calculate sector area and perimeter.

We control for the average ruggedness of a sector, using the Terrain Ruggedness Index

obtained from Nunn and Puga (2012). Details of the calculation are provided in Appendix A.2.

4.5 Additional Data

Local Political Leaders To understand the mechanisms, we use data on the presence of, and party affiliation of existing mayors in early 1994, from Guichaoua (2015). The parties represented at the commune level were, aside from the dominating MRND, the Republican Democratic Movement (MDR), the Social Democratic Party (PSD), the Liberal Party (PL), the Christian Democratic Party (PDC), and the Coalition for the Defense of the Republic (CDR). In the beginning of 1994, the MRND was still the largest party with mayors in 70% of the communes, while 49 sectors were located in a commune without a mayor in office.

RTLM Data Further regarding mechanisms, Yanagizawa-Drott (2014) provides data on the share of each sector that received the RTLM radio signal together with control variables such as the distance of each sector to the closest out of the two RTLM transmitters in the country, and the mean and variance of sector elevation. The so-called "hate radio-station" RTLM was set up in July 1993 and broadcasted anti-Tutsi propaganda.

5 Empirical Strategy

To identify the effect of *Umuganda* on civilian participation in genocide violence, and because we lack data on the number of people participating in *Umuganda*, we use heavy rainfall as a proxy for low participation or cancellation, and estimate the reduced-form effect of heavy rainfall on civilian genocide participation. Our identification strategy rests on two assumptions. First, sectors with more Saturdays with heavy rainfall 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).

5.1 Relationship between Rainfall and *Umuganda*

Ideally, we would like to directly test the first-stage relationship using data on the number of people participating in *Umuganda* before the genocide. Unfortunately, such data does not exist. There is, however, abundant recent evidence from Rwanda that speaks for a strong first stage.

A number of articles in the Rwandan press report low attendance at *Umuganda* meetings on days with heavy rainfall. For example, the newspaper Kigali Today reported in February 2016 that heavy rainfall stopped an ongoing *Umuganda* in Nyanza district (Kigali Today, 2016). According to our data, total rainfall in the area that day was 8 mm. Rwandan news blog Makuruki.rw writes that during a *Umuganda* meeting in Kigali's Gitega sector, participation was very limited because it started to rain just as *Umuganda* started (Makuruki, 2016). On that day, it rained a total of 11 mm in Gitega. There are also official government reports documenting how rainfall affects participation in *Umuganda*. For example in Remera sector (in the southeast of Rwanda), *Umuganda* was stopped due to rain (Ngoma District Offical Website, 2016). Total rainfall that day was around 8 mm. Similarly, heavy rainfall affected most *Umuganda* meetings in Gicumbi district (in the north of Rwanda) with rainfall that day ranging from 9 to 15 mm (Gicumbi District Official Website, 2015). Additional cases of cancellations of *Umuganda* and other outdoor events due to rainfall are reported in Appendix Figure A.2.

We can also provide supportive evidence of a link between participation in community meetings and heavy rainfall from the neighboring country Burundi. Burundi is very similar to Rwanda along numerous socio-economic and cultural dimensions[22] and in July 2006, Burundi introduced *Ibikorwa rusangi*, a practice similar to *Umuganda* with community meetings held every Saturday. Importantly, the Afrobarometer survey for Burundi contains a question on whether individuals attended community meetings during the last 12 months. Appendix Table **??** shows a statistically significant and negative relationship between heavy rainfall on Saturdays (and no other weekday) and self-reported participation in community meetings.

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 riots after the death of Dr Martin Luther King in the US. More recent examples include Madestam et al. (2013) and Madestam and Yanagizawa-Drott (2011). Similarly, several other studies use rainfall and other weather phenomena to introduce exogenous variation in voter turnout on election days (Eisinga et al., 2012; Green et al., 2012; Fraga and Hersh, 2011; Hansford and Gomez, 2010; Horiuchi and Saito, 2009). In all these cases, rainfall has an effect both on the direct cost of attending the open-air event and the opportunity cost of attending. For example, Lind (2014) finds that voter turnout in Norway increases when it rains on the election day because bad weather reduces the

[22]The two neighboring countries share a common colonial history and have both suffered from ethnic tensions between Hutu and Tutsi.

opportunity cost of going to the polling station.

Still, the true functional form between rainfall and participation in mandatory community work is unknown. To make progress, we assume – consistent with the anecdotal evidence – that the typical *Umuganda* tasks, consisting exclusively of outdoor work, became difficult or impossible to perform once a certain rainfall threshold is reached. Following Harari and La Ferrara (2018), who define an extreme weather outcome for Africa to be two standard deviations from the long-term average, we choose this threshold to be 10 mm.[23] Based on this, our main explanatory variable is the number of Saturdays with rainfall above 10 mm in our chosen period of interest: from October 1990 to March 1994.[24] Another approach would be to use rainfall shocks, i.e. deviations of Saturday rainfall from long-term averages normalized by their long-term standard deviation, as the variable of interest. While we believe that this is less relevant for our study, we show throughout the analysis that our results are robust to using this alternative measure of "heavy rainfall."

Figure 2 shows that there is significant local variation in the number of rainy Saturdays for our period of interest. In Figure A.3 in the Appendix, we also show that there is significant local rainfall variation for all other weekdays. Furthermore, in Figure 3, we plot the number of rainy Saturdays after netting out commune fixed effects and average rainfall controls. This map further confirms that there is substantial local variation and little spatial clustering.

5.2 Exclusion Restriction

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 on all other weekdays, is likely to affect rain-fed production and is therefore correlated with income, which in turn may affect genocide participation. Heavy rainfall may also destroy infrastructure such as roads or households and thereby affect the likelihood of participating in conflict.

[23] 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 taking the average across all sectors and all days from 1984 to 1994. Two standard deviations from the long-term average correspond to 10.24 mm. In Table **??** in the Appendix, we show that our results are also robust to using average daily rainfall on Saturdays and all other weekdays.

[24] Madestam et al. (2013) use a threshold of 0.1 inches (2.5 mm) 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 of 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 Figure **??**, we show that our results are also robust to using this threshold.

To address this problem, and to 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, as well as rainfall on all six other weekdays.[25] The absence of systematic, significant effects for days other than Saturdays serves as a first placebo test. In particular, this placebo test allows us to rule out that any other factors correlated with rainfall, such as geography, are driving the results because these factors should then naturally be correlated with rainfall on all other days as well. Nevertheless, we show that our results are robust to controlling for sector ruggedness. To account for local characteristics, we also add 142 commune fixed effects, and in all regressions we control for sector population size.[26]

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

5.3 Empirical Specification

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

$$\frac{P_{ic}}{H_{ic}} = \alpha + \beta \ \#Saturdays(Rainfall > t \ mm)_{ic} + \mathbf{X}_{ic}\pi + \gamma_c + \varepsilon_{ic}, \tag{1}$$

where P_{ic} is the number of Hutus prosecuted in the second category of perpetrators, i.e. our proxy for civilian participation in genocide violence, and H_{ic} is the Hutu population size in sector 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. The starting date for our period of interest is the date of the outbreak of the Rwandan Civil War, which marks the beginning of a period of increased tensions between the two ethnic groups in Rwanda. 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 sector-specific controls, including the logarithm of sector population size, average daily rainfall from January 1984 to Septem-

[25] To account for possibly non-linear rainfall effects on production, we show in Table **??** in the Appendix that our results are robust to adding a full set of dummies for average rainfall.

[26] In the Appendix, we split the sample into sectors with high Saturday rainfall and low Saturday rainfall (split at the median) and report summary statistics for all of our exogenous variables for both sub-samples. Table **??** confirms that there are no significant differences between these two samples.

ber 1990, average daily rainfall from October 1990 to March 1994, and number of all other weekdays with rainfall above t mm during our period of interest. Finally, γ_c are commune fixed effects, and ε_{ic} is the error term. We allow the error terms to be correlated across sectors within the same commune by clustering the standard errors at the commune level. For the sake of robustness, we also allow error terms to be correlated across sectors within a 25, 50, and 75 km radius (Conley, 1999).[27] Moreover, to account for the fact that the variation in the number of perpetrators is larger in sectors with a smaller Hutu population size, we estimate the above model using weighted least squares (WLS) estimation and weight our observations by sector Hutu population size. Our results do not, however, 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 (we measure participation rates in percent).

6 Results

6.1 Main Effects

Main Reduced-Form Effects The reduced-form relationship between the civilian participation rate and the number of Saturdays with rainfall above 10 mm is strongly negative and statistically significant at the 99 percent confidence level (Regression 1 in Table 2). This relationship holds up when adding 142 commune fixed effects (Regression 2) and the number of all other weekdays with rainfall above 10 mm (Regression 3).[28] Reassuringly, none of the other weekdays is systematically and significantly related to civilian violence, and we fail to reject the null that all these coefficients are equal to zero (p-value 0.9482). The effect is also robust to summing up the number of all rainy weekdays (except Saturdays) and adding the sum to Regression 2, which confirms that it is only Saturday rainfall that matters (Regression 4).[29] The point estimate of -0.300 (standard errors 0.104) in our main specification (Regression 3) suggests that a one-standard deviation increase in the number of rainy Saturdays in October

[27] These results are reported in Appendix Table **??**.

[28] Note that civilian violence was very localized and mostly confined to an individual's home sector (Hatzfeld, 2005). Thus, it is unlikely that traveling civilian perpetrators bias the estimates.

[29] In our main regressions, we linearly control for average sector rainfall, and we allow average rainfall effects to differ for our period of interest (1990–1994) and the time before (1984–1990). In Table **??** in the Appendix, we show that our results are robust to a) pooling the two periods, i.e. controlling for average rainfall for 1984–1994, b) controlling only for average rainfall from 1990–1994, and c) accounting for non-linear rainfall effects (on agricultural production) by including a full set of dummies for average sector rainfall.

1990 to March 1994 reduced the civilian participation rate by 1.3 percentage points.[30]

So far, we focused on days with heavy rain, and controlled for long-term and general rainfall effects by including average daily rainfall before and during our period of interest. An alternative approach is to use rainfall shocks, i.e. deviations of Saturday rainfall from long-term averages normalized by their long-term standard deviation. In Table **??** in the Appendix, we show that our results are robust to using Saturday rainfall shocks. Details on how we calculate these shocks are also given in the Appendix.

Tracing the Effects over Time To gain a better understanding of the effect of *Umuganda*, we trace the effects over time. One way to do this is to split our period of interest into seven equal-length time intervals (6 months each). Splitting the time period in this way allows for comparisons across the years, since the equally long time-intervals will include the same months across the years: October to March, or April to September.[31] Regression 5 in Table 2 shows that the last six months are driving the effect. In fact, the point estimate of -0.756 (standard error 0.231) for the last six-months period just before the genocide suggests that a one standard-deviation increase in the number of rainy Saturdays (1.55 days or about 6 percent of all Saturdays for that period) reduced the average civilian participation rate by about 16 percent (interpreted at the mean civilian participation rate of 7.33 percent). Our main results leverage local sector-level variation within communes. To alleviate concerns that our rainfall data resolution may be too coarse to pick up meaningful effects, we also re-run regression 5 at the more aggregated commune level. The results are robust, as shown in Regression 6 in Table 2.[32]

That the effect of *Umuganda* is driven by the last six months is in line with our expectations: extensive documentation show how political and ethnic tensions increased especially in this period. For example, following the signing of the Arusha Peace Accords in August 1993, there was a serious radicalization among officers of the Rwandan army and political extremists who were unhappy with the terms of the peace agreement (Guichaoua, 2015; Mann, 2005). As a result, the Hutu Power factions of the MRND, CDR and MDR parties became more active in the public sphere, power factions were also created within the PL and PSD parties, and the anti-Tutsi rhetoric went from vague to openly exterministic (Viret, 2010; Mann, 2005). Another

[30] A standard deviation in the number of Saturdays with heavy rainfall in Oct 1990–Mar 1994 is 4.24 mm.

[31] With 42 months in total, 7 is a natural divide and ensures that the intervals are comparable in terms of including Rwanda's two rainy seasons.

[32] Unfortunately, the data does not allows us to say whether the exact timing of the rainfall matters, for instance having more consecutive rainy Saturdays.

accelerating factor was the assassination of the Burundian President Melchior Ndadaye in October 1993, which resulted in around 300,000 (Hutu) Burundians seeking refuge in Rwanda and led to a radicalization of the population in the south of Rwanda.

Magnitudes Since we use variation where only a few *Umuganda* meetings are canceled (there were on average 5 Saturdays with rainfall above 10 mm in Oct 1993–Mar 1994, which corresponds to about 20 percent of all Saturdays in that period), the above coefficient does not allow us to back out the counterfactual number of perpetrators had all meetings been canceled. If we instead consider the hypothetical situation of all sectors having 35 percent of the meetings canceled (which is the maximum in our sample for Oct 1993–Mar 1994), our result suggests that the civilian participation rate could have been around 40 percent lower.[33]

6.2 Reliability of the Gacaca Data

To rule out that our results stem from bias or systematic error in the prosecution data, in this subsection we rerun our main specification using three alternative measures of genocide violence from other data sources: (i) an estimate of the total death toll at commune level[34]; (ii) the location of mass graves (obtained through satellite images) and (iii) geo-coded data from the UCDP at the commune level.

Results are reported in Table 3. Regression 1 shows that, as in our main results, only Saturday rainfall is negatively related with the number of causalities and the point estimate on Saturday rainfall is considerably larger (in absolute terms) than those for other weekdays. One remaining concern is that the meetings might have still facilitated investigations after the genocide. While this could affect the estimated number of deaths, the data on mass graves is based on satellite imagery and thus independent of activities on the ground that could have been affected by *Umuganda*. Consistently, in Regression 2, we show that sectors with many rainy Saturdays are also less likely to have a mass-grave site altogether. [35]

Finally, to corroborate the above findings, we also use geo-coded data from the UCDP. This dataset allows us to distinguish between violence committed by the Rwandan government (and

[33]Note that this exercise assumes a one-to-one relationship between rainy Saturdays and canceled meetings and thus provides a lower bound.

[34]Provided by genocdynamics `https://genodynamics.weebly.com/data-on-violence.html`.

[35]Furthermore, when dropping sectors with at least one mass grave (indicating high death rates), the results are essentially identical to our baseline results (Regression 1 in Appendix Table **??**). This further speaks against the concern that our results are driven by survival bias, or that *Umuganda* made the prosecution process easier.

its allies) and violence committed by the Tutsi rebel forces (RPF). In Regression 3 in Table 3, we show that Saturday rainfall is negatively related to genocide violence initiated by the Rwandan government. In Regression 4, we rerun the regression using violence committed by the RPF during the genocide period. This serves as a placebo test, as we would not expect this violence to be affected by government led *Umuganda* meetings. The insignificant and small point estimate confirms this.[36]

6.3 Robustness Checks

Next, we conduct a number of robustness tests. The results are presented in columns 5-7 in Table 3. A first concern is that our estimates may be picking up effects from rainfall during the genocide rather than the 3.5 years pre-genocide period. To address this concern we control for the number of Saturdays (and all other weekdays) with rainfall above 10 mm during the period of the genocide. The results, presented in Regression 5, are very similar to our baseline results.

In Regression 6, we show that our results are robust to controlling for the following additional controls that may directly affect the civilian participation rate: (i) Sector ruggedness, which could be correlated with sector rainfall and captures micro-climate effects; (ii) closeness to the border, which could have enabled people to flee the country;[37] (iii) distance to major cities – in particular the capital Kigali, since cities have higher economic activity and public goods provision; (iv) distance to Nyanza (the old Tutsi Kingdom capital), since sectors further away from Nyanza still had lower shares of Tutsi inhabitants in 1991; (v) latitude and longitude, which capture general spatial pattern such as southern Rwanda having larger Tutsi population shares than the North; (vi) sector standard deviation in average daily rainfall (from 1984–1994) to ensure that our results are not driven by sectors having a different tolerance to rainfall depending on their average level of rainfall.

Finally, in Regression 7 we report the result when using ordinary least squares (OLS) estimation, which shows that our findings do not depend on the use of weighted least squares estimation. In the Appendix, we present a number of additional checks.[38]

[36] In the Appendix we further show that the results are robust to dropping sectors where pre-genocide violence was taking place as this might have affected the prosecution process (Table **??**).

[37] Results are also robust to including the distance to each of Rwanda's four borders – DRC, Uganda, Burundi, Tanzania – separately.

[38] In particular, we show that the results are robust to varying the rainfall thresholds (Figure **??**), controlling for potential multicollinearity in the weekly rainfall variables (Figure **??**), not driven by outliers (Figures **??** to **??**), controlling for RTLM radio coverage and militia presence (Table **??**), dropping sectors with pre-genocide fighting between the RPF and the FAR, and various transformations of the dependent variable. Additionally, we find no

6.4 Placebo Check

As another placebo check, we rerun the main specification using Saturday rainfall during the 3.5 year pre-genocide calendar period (October 1, YEAR to March 31, YEAR+4), using data from all years in the period 1984–2013. 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.[39] Reassuringly, the coefficient on Saturday rainfall from 1990–1994 is a clear outlier to the left in the distribution of the resulting 20 coefficients, and none of the other point estimates are larger in absolute value. The results are shown in Figure 4.

7 Mechanisms

After having documented a strong, robust effect of Saturday rainfall on civilian participation in genocide violence, we now analyze the mechanisms. However, given that we do not have data on the actual number of *Umuganda* meetings or average attendance at these meetings, this part of the analysis should be regarded as suggestive.

7.1 Main Mechanisms

The two top-down approaches – management and propaganda – discussed in Section 3.2, imply that the effects of *Umuganda* should differ by the party affiliation of the mayors. Mayors affiliated to the ruling party, the MRND, or its close ally, the CDR (from here on referred to as the pro-Hutu parties) are likely to have had stronger career incentives to carefully organize and monitor *Umuganda*, and to have been stronger supporters and thus more inclined to spread the anti-Tutsi rhetoric than mayors affiliated to the opposition parties (the MDR, the PSD and the PL). In communes with no mayor in place, we would expect a smaller or even no effect of Saturday rainfall since mayors played a key role in the implementation of the meetings, and communes with no mayor may not have been able to organize *Umuganda*.[40] To test if there are heterogeneous effects with respect to the party affiliation of the mayors, we interact Saturday rainfall with two different dummy variables.

effects when using the share of category 1 prosecutions as the dependent variable in a placebo check (Table **??**).

[39]Note that we exclude any periods that overlap with our period of interest. Thus, we use three placebo periods before the genocide (starting 1984, 1985 and 1986) and 16 periods after the genocide (starting years 1994 to 2009).

[40]The opposition parties were the Liberal Party (PL), the Social Democratic Party (PSD), and the Republican Democratic Movement (MDR). The pro-Hutu parties were the National Revolutionary Movement for Development (MRND) and the Coalition for the Defense of the Republic (CDR).

The first dummy variable takes on the value of one if the mayor (measured at commune level) was a representative of the MDR, and the second takes on the value of one if the mayor was a representative of the PSD or the PL.[41] The reason for separating out the MDR from the other opposition parties is that the political orientation of the MDR was closer to the MRND than that of the PSD and the PL.[42] The results show that the effect in the pro-Hutu sectors – the coefficient on Saturday rainfall – is negative, highly significant and similar to our baseline result. There appears to be no statistical difference between the pro-Hutu sectors and the MDR. The point estimate on the interaction with MDR mayors is small and statistically insignificant (Regression 1 in Table 4). In the PSD/PL controlled communes, on the other hand, the effect appears to have been the opposite – the point estimate on the interaction term is large and positive – but its statistical insignificance prevents us from drawing any firm conclusions.[43] One concern may be that sectors with mayors from opposition parties have larger Tutsi minorities and that this is driving the results. However, controlling for the Tutsi minority share and its interaction with Saturday rainfall does not change these results (Regression 2).

As an additional check, we test whether the effects differ in sectors with no mayor in office in the year before the genocide, thus shutting down a possible top-down channel. Interestingly, the interaction effect is positive and statistically significant and completely offsets the negative *Umuganda* effect (we cannot reject that the two point estimates sum up to 0). Thus, with no local government in place *Umuganda* appears to have had no effects on civilian participation in violence (also shown in Regressions 1 and 2 of Table 4).

Taken together, the results above provide general evidence for a top-down mechanism. The presence of a government supporting mayor appear crucial for the Saturday rainfall effect to manifest itself. In places with no local leader, the Saturday effect is absent (presumably due to no *Umuganda* meetings in this period, or low ability to organize the meetings) while in places with opposition party mayors in power, the positive point estimate, although insignificant, sug-

[41] Note that mayors, appointed at the commune level and thus one administrative level above our unit of analysis (the sector), were the most powerful local politicians and thus had strong influence on the policy in their sectors. See further discussion on mayors' role in Sections 2.1, 2.2 and 3.1.

[42] For example, a strong Hutu Power faction was created within the MDR before similar splits occurred within the PSD and the PL. Moreover, documents from the ICTR trials point out several MDR mayors (belonging to the MDR Hutu Power faction) as master minds behind the genocide (e.g. Ladislas Nyagasaza, MDR, mayor of Nyakizu in 1993–1994), and mayors from the PSD and PL as opponents to the extremist pro-Hutu ideology (e.g. Narcisse Nyagasaza, PL, mayor of Ntyazo in 1993–1994).

[43] Given that Hutu Power factions were eventually created within all opposition parties, and since we do not have complete information on which mayors belonged to the true opposition, party affiliation is a noisy measure of whether mayors actively supported the extremist ideology or not, which could explain the statistical insignificance of the interaction with PSD/PL mayors.

gest a reverse "Saturday rain effect".[44] In the next step, we will try to distinguish between the two top-down mechanisms described in our conceptual framework in Section 3.2 (management versus propaganda), as well as our third proposed mechanism, namely that receiving political messages in the specific context of meetings may have altered people's acceptance of and willingness to participate in the genocide violence.

Management As discussed in Section 3.2, the local elites could have used *Umuganda* to learn about the mobilization potential of the population and to practice how to organize and lead them. This would then mean that *Umuganda* did not necessarily change Hutu civilians' acceptance of violence against the Tutsi population, but rather helped the local elites to organize the violence. Related to this reasoning, Rogall (2017) shows that militia and soldiers fostered civilian participation in violence during the genocide, relying on the local elites' knowledge of the population. If it was the case that during *Umuganda*, the local elites mostly learned about the mobilization potential of the population, this knowledge should have increased the effectiveness of the army and militiamen in fostering civilian participation – and would thus make *Umuganda* and militia complements in spurring civilian violence. To test for this, we interact the number of rainy Saturdays with the number of militia and organized perpetrators in each village (category 1 from the Gacaca court data). As can be seen in Regression 3 of Table 4, the resulting point estimate on the interaction term is positive and statistically significant, in other words, the effect of militia presence on civilian violence is stronger for places that had few *Umuganda* meetings before the genocide. These results rather suggest that *Umuganda* and militia were substitutes and that *Umuganda* had a direct effect on civilian behavior, rather than the effect working solely through local elites.[45]

Propaganda Second, *Umuganda* may also have affected civilian participation primarily through the diffusion of information and propaganda. If this were the case, we should see a less negative (more muted) effect in areas where the population could receive convincing information through other sources. To test this, we interact Saturday rainfall with the share of the sector with access to propaganda through the "hate-radio" RTLM (Regression 4 in Table 4). Interest-

[44]The large and positive coefficient on the interaction effect between Saturday rainfall and PSD/PL mayors could suggest that opposition party mayors did not push the central government's political agenda in *Umuganda* meetings and that, in such sectors, either the political content or the opportunity to meet provided by *Umuganda* contributed towards overcoming animosity between villagers of different ethnic groups.

[45]Rogall (2017) provides more detailed evidence for such a substitution effect.

ingly, the point estimate on the interaction effect is negative and significant at the 90 percent confidence level. This suggests that the propaganda diffused during *Umuganda* and on the radio were not simple substitutes but rather reinforced each other.[46] One interpretation could be that people needed both propaganda spread via RTLM and during *Umuganda* to be persuaded into violence. This is somewhat contradicted by the fact that the main effect on *Umuganda* is still strongly negative with a p-value of 0.103. Thus, *Umuganda* seemed to have been effective even in the absence of RTLM coverage. Another interpretation is that *Umuganda* functioned as a coordination device, gathering the constituents in one place and allowing them to observe the reaction of fellow villagers to anti-Tutsi propaganda (similar to the one spread on the radio). Below we provide further support for this.

Popular Support – *Umuganda* as a Coordination Device To further investigate whether *Umuganda* was used by local elites to bring people together, we interact Saturday rainfall with local population density. The results, presented in Regression 5 in Table 4, provide support for such a "coordination mechanism". The coefficient on the interaction term is positive and significant at the 99 percent confidence level. Thus, *Umuganda* appears to have been particularly effective in sparsely populated areas where people may otherwise not have met frequently.[47]

Another test of whether *Umuganda* was used to bring people together is to look at the shape of the sector, in particular the area of the sector relative to its perimeter.[48] If this ratio is close to 1, the sector is relatively compact (the ratio is largest for a circle) and thus information spreading and coordination may have been easier even in the absence of *Umuganda*. If the ratio is small, however, the sector is less geographically compact and likely to have had more than one natural meeting point. Regression 6 in Table 4 provides additional support for *Umuganda* functioning as a coordination device; the interaction effect between Saturday rainfall

[46]Recall the result from the preceding paragraph suggesting that *Umuganda* and the militia were substitutes. However, RTLM and *Umuganda* appear to be complementing each other. All three factors increased civilian participation. To reconcile these two different results the timing matters. While *Umuganda* and RTLM prepared civilians *before* the genocide (reinforcing each other), the militia induced civilian violence *during* the genocide. In particular, Rogall (2017) shows that the militia acted as a moral authority to persuade civilians into joining. By this logic, the marginal effect of the militia was smaller in areas where civilians were already radicalized (through *Umuganda*, RTLM access or both), which would explain the substitution effects.

[47]Note that since opportunity costs of meeting attendance are potentially higher in sparsely populated areas, rainfall may have a stronger negative effect on *Umuganda* attendance in such areas. This would be problematic (i.e., affect our reduced-form results) if we assumed that rainfall affects *Umuganda* linearly, but is less problematic given our current specification where we interpret rainfall above a certain threshold as a cancelled meeting. The fact that our results hold up also with higher thresholds, going up to 15mm strengthens the argument that meetings get canceled regardless of whether a sector is highly populated or not.

[48]We use the widely accepted compactness measure proposed by Osserman (1978): $Compact_i = \frac{4\ \pi\ Area_i}{Perimeter_i^2}$.

and compactness is positive and statistically significant at the 90 percent confidence level.[49]

Summary Taken together, the above results suggest that local political leaders used *Umuganda* to spread propaganda and bring people together, and our framework suggests that there are complementarities between the two in facilitating the adaption of extreme standpoints.[50]

7.2 Alternative Mechanisms

In this sub-section, we discuss and rule out a number of alternative mechanisms that may be driving the correlation between Saturday rainfall and civilian participation in the genocide.

Socializing and Voluntary Interaction As discussed in Section 3.3, our main results could simply stem from increased civilian interaction during Saturdays, irrespective of the political elites – thus increasing in-group cohesion among the Hutu. In other words, non rainy Saturdays in general might have enabled social interaction that created "bonding" social capital within the Hutu population. If our findings are driven by social meetings, we would expect similar effects on violence from other events before the genocide that offer opportunities for civilians to meet. Since most Rwandans attend church on Sundays, we first consider Sunday rainfall. We do not observe a significant relationship between Sunday rainfall and participation in violence in our main result Table 2. Our results are also robust to dropping the capital, Kigali, and other major cities from the sample; places where one might expect major outdoor events (such as festivals, sports tournaments and other cultural gatherings) to predominantly take place on Saturdays (Regressions 1 and 2 in Table 5). In a similar vein, heavy rainfall on public holidays, another occasion when communities got together, do not seem to matter. The point estimates on the number of public holidays with rainfall above 10 mm are small and consistently statistically insignificant (Regressions 3–5 in Table 5).[51] These results strengthen the hypothesis that there was something special about the *Umuganda* meetings that are driving our results.[52]

[49] Both high population density and compactness could also affect the ease of finding victims and thus the number of civilian killers. Reassuringly, the interaction effects of Saturday rainfall with population density and compactness are robust to adding interactions with Tutsi population density or Tutsi minority share. The main effects of both variables are also negative.

[50] All the interaction effects are robust to using rainfall shocks instead of high levels of rainfall as the variable of interest. Results using this alternative specification are shown in Appendix Table **??**.

[51] The public holidays include New Year's Day, Commemoration of Democracy, Labor Day, Independence Day, Commemoration of the Second Republic, Assumption Day, Kamarampaka Day, Government Holiday, Justice Day, Christmas Day, and Boxing Day. We do not include holidays that fall on a Saturday.

[52] An alternative explanation for our results could also be forced interaction between Hutu and Tutsi civilians during *Umuganda*, which could have increased tensions between the Hutu and Tutsi populations in an already

Economic Channels A final possibility is that *Umuganda* mattered for purely economic reasons. One may worry that our findings are not the result of *Umuganda* per se, but rather an indirect effect of *Umuganda* being canceled, driven by farmers' opportunity costs. If heavy rainfall canceled *Umuganda* meetings, but farmers still decided to work on their fields, Saturday rainfall could have created a positive income effect that, in turn, could have affected participation in genocide violence. To investigate this, we split the number of rainy Saturdays based on whether they took place during the work season or the lean season.[53] If opportunity costs are driving the result, we should observe larger effects during the work season. However, the results in Regression 6 (Table 5) suggest that this is not the case: both point estimates are negative, and they are not significantly different from each other (p-value 0.85).[54]

Another potential concern is that *Umuganda* affected the provision of public goods, and thereby civilian support for the government. Based on our reading of literature and reports of *Umuganda* from our period of interest, we do not believe this to be an important channel. For example, a World Bank report documented that while *Umuganda* was carefully organized and monitored, *"its effectiveness [for public goods provision is] not always evident"* (World Bank, 1987, p. 67). For example, the equipment used for road maintenance was unsuitable (hoes), projects were typically not adapted to local needs or conditions, and anti-erosion ditches were poorly maintained (World Bank, 1987).[55]

8 Conclusion

In this paper, we estimate the causal effect of a nationwide top-down policy in pre-genocide Rwanda on civilian violence, using an identification strategy that credibly isolates local extensiveness of the policy. Specifically, we show that a mandatory community program called *Umuganda*, which required citizens to participate in community work and political meetings every Saturday, contributed to the large-scale mobilization of Hutu civilians in the ensuing

tense period. This is discussed in Appendix Section **??** and corresponding Appendix table **??**.

[53] According to a seasonal calendar for Rwanda provided by the Famine Early Warning Systems Network (FEWS NET), the lean season includes April, May, October, and November, and the work season include all other months. The calendar can be found at: http://www.fews.net/east-africa/rwanda (accessed Nov. 16, 2018).

[54] For consistency with our main findings table, we repeat this exercise focusing on the six month-period preceding the genocide. Results are consistent with negative and significant point estimates both for the lean season and the work season with point estimates of -1.191 (st. error 0.320) and -0.528 (st. error 0.278), respectively.

[55] In Section **??** in the Appendix and corresponding Tables **??** to **??**, we nevertheless examine if Saturday Rainfall affects public goods, and find no support for this. Additional possibilities for how infrastructure may affect our results are discussed and refuted in Appendix Section **??** and Table **??**.

genocide violence.

We use cross-sectional variation in rainfall intensity on Saturdays to proxy for lower participation in *Umuganda*. Our results strongly suggest that areas with fewer *Umuganda* meetings, in particular during the last six months prior to the genocide, had significantly lower genocide participation rates. Our results further imply that an interplay between top-down and bottom-up mechanisms was at work in the meetings. In particular, we find support for the local political elites using *Umuganda* to bring people together and for spreading propaganda.

Our main results regarding *Umuganda* are in line with qualitative research based on in-depth interviews with perpetrators carried out in the decade following the 1994 genocide. Nevertheless, the potential for the negative consequences of *Umuganda*, and more broadly, policies of mandatory community work and political meetings, is often overlooked in the development discourse and the media (see e.g. New York Times, 2017). Consistent with the popular narrative where "community-driven development" is presented as a solution to a range of accountability problems related to decentralized development policies and weak democracies in developing countries, the current Rwandan government formally reinstalled *Umuganda* in 2008. Similar programs are also in use or discussed in other East African countries, and community driven development practices are broadly promoted by e.g. the World Bank. Our results support the notion that community meetings can be effective vehicles for influencing and controlling a population. However, rather than claiming that community meetings are inherently destructive, our takeaway is that, just as such meetings can be harnessed for good, they can be used for spreading hateful and divisive messages. Thus caution is warranted when establishing these meetings, especially in countries with weak institutional settings and a history of ethnic tensions.

References

Acemoglu, D., T. Reed, and J. A. Robinson (2014). Chiefs: Economic development and elite control of civil society in sierra leone. *Journal of Political Economy 122*(2), 319–368.

Adena, M., R. Enikolopov, M. Petrova, V. Santarosa, and E. Zhuravskaya (2015). Radio and the rise of the nazis in prewar germany. *The Quarterly Journal of Economics 130*(4), 1885–1939.

Armand, A., P. Atwell, J. Gomes, et al. (2017). The reach of radio: Defection messaging and armed group behavior. Technical report, Households in Conflict Network.

Balliet, D. (2010). Communication and cooperation in social dilemmas: A meta-analytic review. *Journal of Conflict Resolution 54*(1), 39–57.

Besley, T. and T. Persson (2011). The logic of political violence. *Quarterly Journal of Economics 126*(3), 1411–1445.

Blattman, C. and E. Miguel (2010). Civil war. *Journal of Economic Literature 48*(1), 3–57.

Blouin, A. and S. W. Mukand (2019). Erasing ethnicity? propaganda, nation building, and identity in rwanda. *Journal of Political Economy 127*(3), 1008–1062.

Browning, C. R. (2004). *The Origins of the Final Solution: The Evolution of the Nazi Jewish Policy, September 1939–March 1942*. Lincoln, Nebraska: University of Nebraska Press.

Casey, K., R. Glennerster, and E. Miguel (2012). Reshaping institutions: Evidence on aid impacts using a preanalysis plan. *The Quarterly Journal of Economics 127*(4), 1755–1812.

Collins, W. J. and R. A. Margo (2007). The economic aftermath of the 1960s riots in American cities: Evidence from property values. *Journal of Economic History 67*(4), 849–883.

Conley, T. G. (1999). GMM estimation with cross sectional dependence. *Journal of Econometrics 92*(1), 1–45.

Dallaire, R. (2003). *Shake Hands with the Devil*. Toronto: Random House Canada.

Dell, M. (2015). Trafficking networks and the Mexican drug war. *American Economic Review 105*(6), 1738–1779.

Dell, M., B. F. Jones, and B. A. Olken (2014). What do we learn from the weather? The new climate-economy literature. *Journal of Economic Literature 52*(3), 740–798.

DellaVigna, S., R. Enikolopov, V. Mironova, M. Petrova, and E. Zhuravskaya (2014). Cross-border media and nationalism: Evidence from serbian radio in croatia. *American Economic Journal: Applied Economics 6*(3), 103–32.

Des Forges, A. (1999). *Leave None to Tell the Story: Genocide in Rwanda*. New York: Human Rights Watch and the International Federation of Human Rights Leagues.

Dube, O. and J. F. Vargas (2013). Commodity price shocks and civil conflict: Evidence from Colombia. *Review of Economic Studies 80*(4), 1384–1421.

Eisinga, R., M. Grotenhuis, and B. Pelzer (2012). Weather conditions and political party vote share in Dutch national parliament election, 1971–2010. *International Journal of Biometeorology 56*(4), 1161–1165.

Finkel, S. E. and E. N. Muller (1998). Rational choice and the dynamics of collective political action: Evaluating alternative models with panel data. *American Political Science Review 92*(1), 37–49.

Finkel, S. E., E. N. Muller, and K.-D. Opp (1989). Personal influence, collective rationality, and mass political action. *American Political Science Review 83*(3), 885–903.

Fraga, B. and E. Hersh (2011). Voting costs and voter turnout in competitive elections. *Quarterly Journal of Political Science 5*(4), 339–356.

Friedman, W. (2013). Local economic conditions and participation in the Rwandan genocide. HiCN Working Paper 160.

Gicumbi District Official Website (2015). Umuganda usoza ukwezi kwa gashyantare. http://www.gicumbi.gov.rw/index.php?id=38&tx_ttnews%5Btt_news%5D=104&cHash=16ab35c781b1fdc2b6c0cc7db450d207 [Last accessed on 2016-10-26].

Gonzalez-Torres, A. and E. Esposito (2017). Epidemics and conflict: Evidence from the ebola outbreak in western africa. Working Paper.

Green, D. P., P. M. Aronow, and M. C. McGrath (2012). Field experiments and the study of voter turnout. *Journal of Elections, Public Opinion & Parties 23*(1), 27–48.

Grootaert, C. and T. Bastelaer (2002). *Understanding and Measuring Social Capital: A Multi-Disciplinary Tool for Practitioners*. Washington, DC: World Bank.

Guichaoua, A. (1991). Les travaux communautaires en Afrique Centrale. *Revue Tiers Monde 32*(127), 551–573.

Guichaoua, A. (2015). *From War to Genocide. Criminal Politics in Rwanda 1990–1994*. Madison, Wisconsin: University of Wisconsin Press.

Guiso, L., S. P., and L. Zingales (2008). Alfred Marshall Lecture: Social capital as good culture. *Journal of the European Economic Association 6*(2–3), 295–320.

Hansford, T. G. and B. T. Gomez (2010). Estimating the electoral effects of voter turnout. *American Political Science Review 104*(2), 268–288.

Harari, M. and E. La Ferrara (2018). Conflict, climate, and cells: A disaggregated analysis. *The Review of Economics and Statistics 100*(4), 594–608.

Hatzfeld, J. (2005). *Machete Season: The Killers in Rwanda Speak*. New York: Picador.

Heldring, L. (2014). State capacity and violence: Evidence form the Rwandan genocide. CSAE Working Paper WPS/2014-08.

Henderson, J. V., A. Storeygard, and D. N. Weil (2012). Measuring economic growth from outer space. *American Economic Review 102*(2), 994–1028.

Heß, S. H., D. Jaimovich, and M. Schündeln (2018). Development projects and economic networks: Lessons from rural gambia. Working Paper.

Hodler, R. and P. A. Raschky (2014). Economic shocks and civil conflict at the regional level. *Economics Letters 124*(2), 530–533.

Horiuchi, Y. and J. Saito (2009). Rain, election, and money: The impact of voter turnout on distributive policy outcomes. Asia Pacific Economic Paper No. 379.

Human Rights Watch (2011). Justice compromised the legacy of Rwanda's community-based Gacaca courts. May.

IRDP (2003). Sustaining peace in Rwanda: Voice of the people. Institut de Recherche et de Dialogue pour la Paix (IRDP).

Iyer, L. and P. Topalova (2014). Poverty and crime: Evidence from rainfall and trade shocks in India. Harvard Business School Working Paper No. 14-067.

Judgement and Sentence of Case No. ICTR-98-42-T (June 24, 2011). The Prosecutor v. P. Nyiramasuhuko, A. S. Ntahobali, S. Nsabimana, A. Nteziryayo, J. Kanyabashi, E. Ndayambaje. International Criminal Tribunal for Rwanda (ICTR). http://unictr.unmict.org/sites/unictr.org/files/case-documents/ictr-98-42/trial-judgements/en/110624.pdf. [Last accessed on 2017-08-31].

Kershaw, I. (1999). *Hitler: 1889–1936: Hubris*. New York: Norton.

Kigali Today (2016). Abaturage bababajwe n'isubikwa ry'uruzinduko rwa Perezida. http://www.kigalitoday.com/amakuru/mu-rwanda/Abaturage-bababajwe-n-isubikwa-ry-uruzinduko-rwa-Perezida. [Last accessed on 2016-10-21].

Knack, S. and P. Keefer (1997). Does social capital have an economic payoff? A cross-country investigation. *Quarterly Journal of Economics 112*(4), 1251–1288.

Lawrence, R. and P. Uwimbabazi (2013). Indigenous practice, power and social control: The paradox of the practice of Umuganda in Rwanda in race, power and indigenous knowledge systems. *Interdisciplinary Journal for the Study of the Arts and Humanities in Southern Africa 20*, 248–272.

Lind, J. T. (2014). Rainy day politics: An instrumental variables approach to the effect of parties on political outcomes. CESifo Working Paper Series No. 4911.

Linek, M. (2019). Dictating development? achieving targets through community work. Working Paper.

Longman, T. (2009). An assessment of Rwanda's Gacaca courts. *Peace Review 21*(3), 304–312.

Madestam, A., D. Shoag, S. Veuger, and D. Yanagizawa-Drott (2013). Do political protests matter? Evidence from the Tea Party Movement. *Quarterly Journal of Economics 128*(4), 1633–1685.

Madestam, A. and D. Yanagizawa-Drott (2011). Shaping the nation: The effect of fourth of July on political preferences and behavior in the United States. HKS Working Paper No. RWP12-034.

Makuruki (2016). Mu mafoto: Uko Umuganda wakozwe mu bice bimwe by'umujyi wa Kigali, henshi haranzwe ubwitabire buke cyane. http://www.makuruki.rw/Politiki/article/MU-MAFOTO-Uko-Umuganda-wakozwe-mu-bice-bimwe-by-Umujyi-wa-Kigali-henshi-haranzwe-ubwitabire-buke-cyane. [Last accessed on 2016-10-21].

Mamdani, M. (2001). *When Victims Become Killers: Colonialism, Nativism, and the Genocide in Rwanda*. Kampala: Fountain.

Mann, M. (2005). *The Dark Side of Democracy: Explaining Ethnic Cleansing*. New York: Cambridge University Press.

Mansuri, G. and V. Rao (2012). *Localizing Development: Does Participation Work?* World Bank Publications.

Melvern, L. (2000). *A People Betrayed: The Role of the West in Rwanda's Genocide*. London: Zed-Books.

Melvern, L. (2004). *Conspiracy to Murder: The Rwandan Genocide*. New York: Verso.

Mitra, A. and D. Ray (2014). Implications of an economic theory of conflict: Hindu-Muslim violence in India. *Journal of Political Economy 122*(4), 719–765.

Mukarubuga, C. (2006). The experience of social forums against poverty: The case of Rwanda. Agency for Co-Operation and Research in Development.

Muller, E. N. and K.-D. Opp (1986). Rational choice and rebellious collective action. *American Political Science Review 80*(2), 471–489.

New York Times (April 25, 2017). How a nation reconciles after genocide killed nearly a million people. https://www.nytimes.com/2017/04/25/world/africa/rwandans-carry-on-side-by-side-two-decades-after-genocide.htm [Last accessed on 2017-08-31].

Ngoma District Offical Website (2016). Abayobozi b'akarere bakoreye Umuganda i Remera. http://www.ngoma.gov.rw/index.php?id=38&tx_ttnews[tt_news]=334&cHash=523c816de256ba76a47cdc002ed6d9b4. [Last accessed on 2016-10-21].

Novella, N. S. and W. M. Thiaw (2012). African Rainfall Climatology Version 2 for Famine Early Warning Systems. *Journal of Applied Meteorology and Climatology 52*, 588–606.

Nunn, N. and D. Puga (2012). Ruggedness: The blessing of bad geography in Africa. *Review of Economics and Statistics 94*(1), 20–36.

Nunn, N. and N. Qian (2014). U.S. food aid on civil conflict. *Review of Economics and Statistics 104*(6), 1630–1666.

Oberschall, A. R. (1994). Rational choice in collective protests. *Rationality and Society 6*(1), 79–100.

Osserman, R. (1978). Isoperimetric inequality. *Bulletin of the American Mathematical Society 84*, 1182–1238.

Pottier, J. (2006). Land reform for peace: Rwanda's 2005 land law in context. *Journal of Agrarian Change 6*(4), 509–537.

Prunier, G. (1995). *The Rwanda Crisis: History of a Genocide*. London: Hurst and Company.

Putnam, R. D. (2000). *Bowling Alone*. New York: Free Press.

Ray, D. and J. Esteban (2017). Conflict and development. *Annual Review of Economics 9*, 263–293.

Rogall, T. (2017). Mobilizing the masses for genocide. Unpublished manuscript.

Sarsons, H. (2015). Rainfall and conflict: A cautionary tale. *Journal of Development Economics 115*(C), 62–79.

Satyanath, S., N. Voigtländer, and H.-J. Voth (2017). Bowling for Fascism: Social capital and the rise of the Nazi Party. *Journal of Political Economy 125*(2), 478–526.

Straus, S. (2006). *The Order of Genocide: Race, Power, And War in Rwanda* (1 edition ed.). New York: Cambridge University Press.

Sundberg, R. and E. Melander (2013). Introducing the UCDP georeferenced event dataset. *Journal of Peace Research 50*(4), 523–532.

Sunstein, C. (2002). The law of group polarization. *Journal of Political Philosophy 10*(2), 175–195.

Tanner, S. (2011). Towards a pattern in mass violence participation? An analysis of Rwandan perpetrators' accounts from the 1994 genocide. *Global Crime 12*(4), 266–289.

Thomson, S. M. (2009). Resisting reconciliation: State power and everyday life in post-genocide Rwanda. PhD dissertation, Halifax, Nova Scotia, Dalhousie University.

US Geological Survey (1996). Sioux Falls, SD: United States Geological Survey Center for Earth Resources Observation and Science (EROS).

Uvin, P. (1998). *Aiding Violence: the Development Enterprise in Rwanda*. West Hartford, Connecticut: Kumarian Press.

Verpoorten, M. (2012a). Detecting hidden violence: The spatial distribution of excess mortality in Rwanda. *Political Geography 31*(1), 44–56.

Verpoorten, M. (2012b). Leave none to claim the land: A Malthusian catastrophe in Rwanda? *Journal of Peace Research 49*(4), 547–563.

Verwimp, P. (2000). Development ideology, the peasantry, and genocide: Rwanda represented in Habyarimana's speeches. *Journal of Genocide Research 2*(3), 325–361.

Verwimp, P. (2006). Machetes and firearms: The organization of massacres in Rwanda. *Journal of Peace Research 43*(1), 5–22.

Verwimp, P. (2011). The 1990–92 massacres in Rwanda: A case of spatial and social engineering? *Journal of Agrarian Change 11*(3), 396–419.

Verwimp, P. (2013). *Peasants in Power: The Political Economy of Development and Genocide in Rwanda*. Heidelberg: Springer.

Viret, E. (2010). Rwanda – A chronology (1867–1994), Online Encyclopedia of Mass Violence, March 1. http://www.massviolence.org/Rwanda-A-Chronology. [Last accessed on 2017-10-27].

Wagner, M. D. (1998). All the bourgmestre's men: Making sense of genocide in Rwanda. *Africa Today 45*, 25–36.

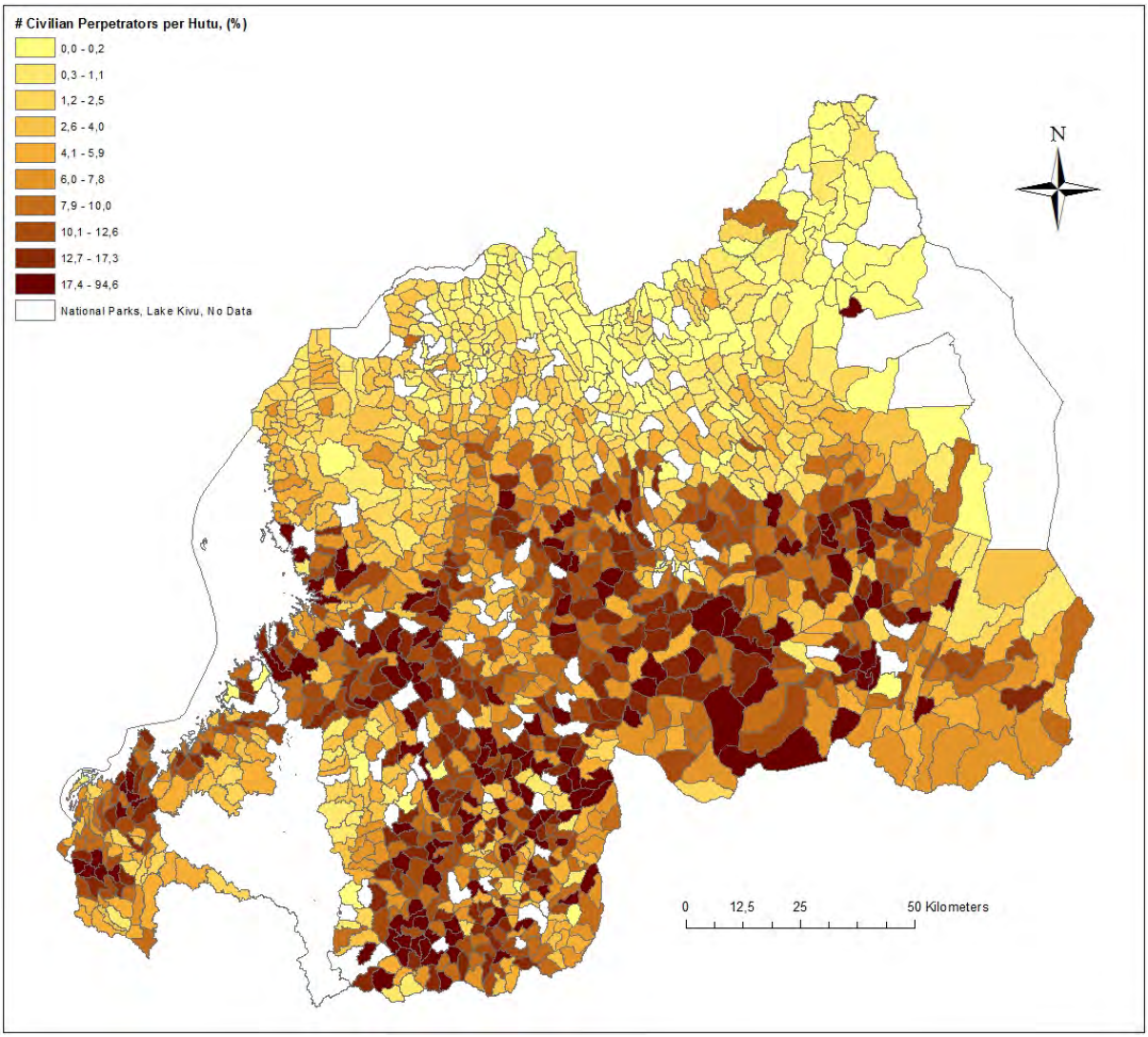
White, H., R. Menon, and H. Waddington (2018). Community-driven development: does it build social cohesion or infrastructure? a mixed-method evidence synthesis. Technical Report.

World Bank (1987). Rwanda – The role of the communes in socio-economic development (english). Washington, DC: World Bank. http://documents.worldbank.org/curated/en/331381468105552313/Rwanda-The-role-of-communes-in-socio-economic-development [Last accessed on 2019-08-31].

Yanagizawa-Drott, D. (2014). Propaganda and conflict: Evidence from the Rwandan genocide. *Quarterly Journal of Economics 129*(4), 1947–1994.

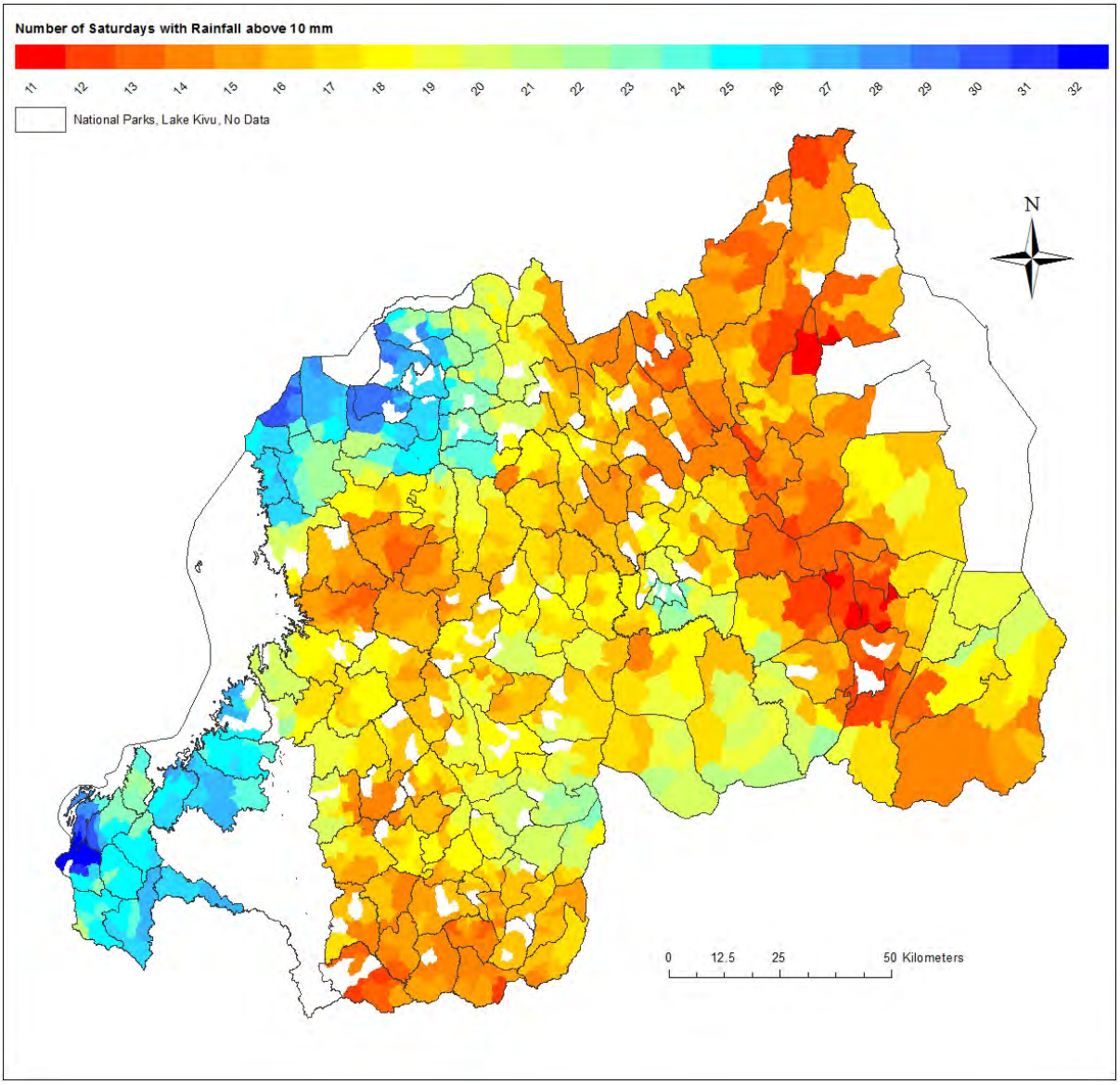
Figures and Tables

Figure 1: Civilian Participation, normalized by Hutu Population



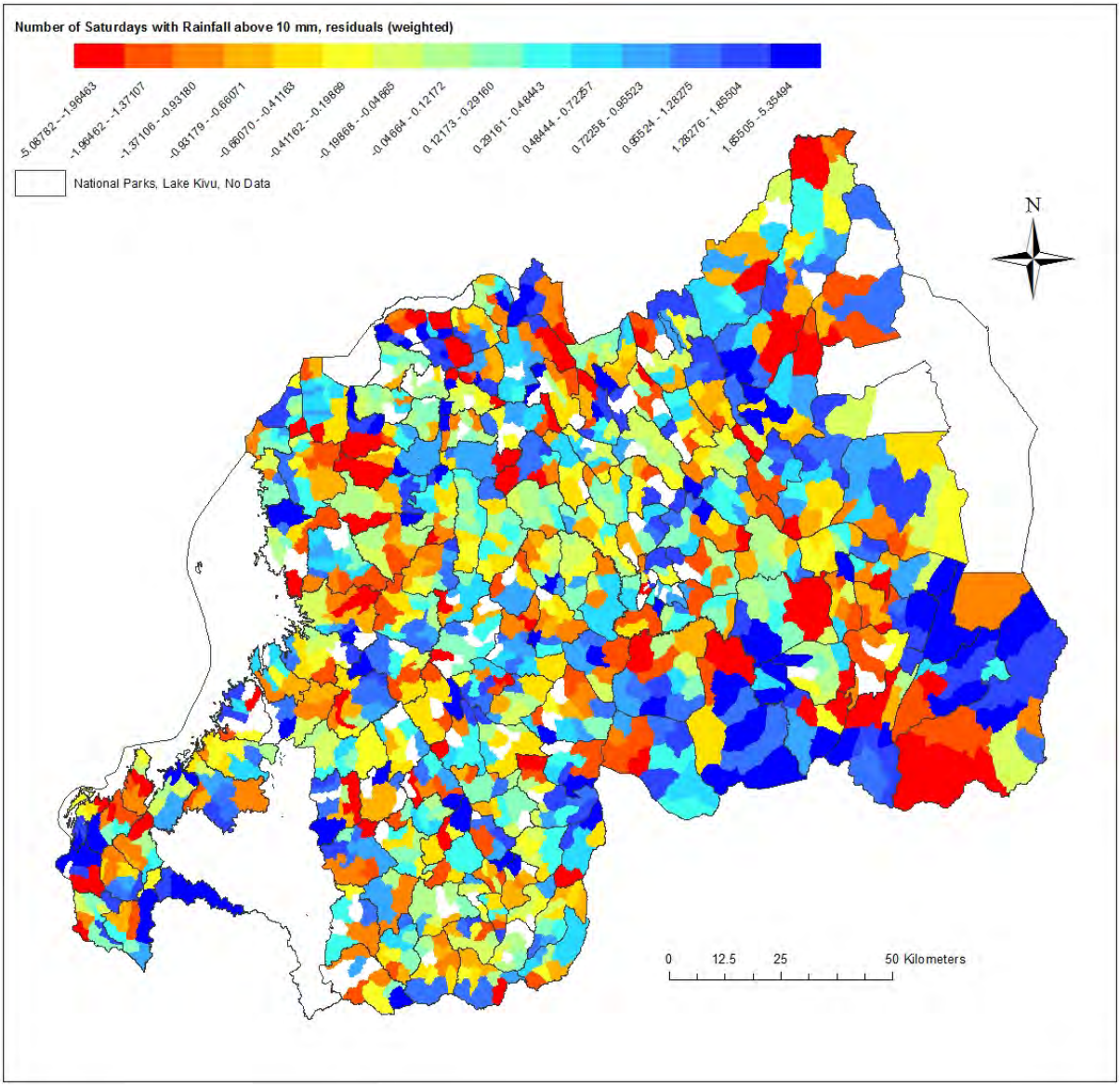
Notes: The map plots the civilian participation rate in each sector, defined as the number of civilian perpetrators divided by Hutu sector population.

Figure 2: Local Variation in Saturday Rainfall



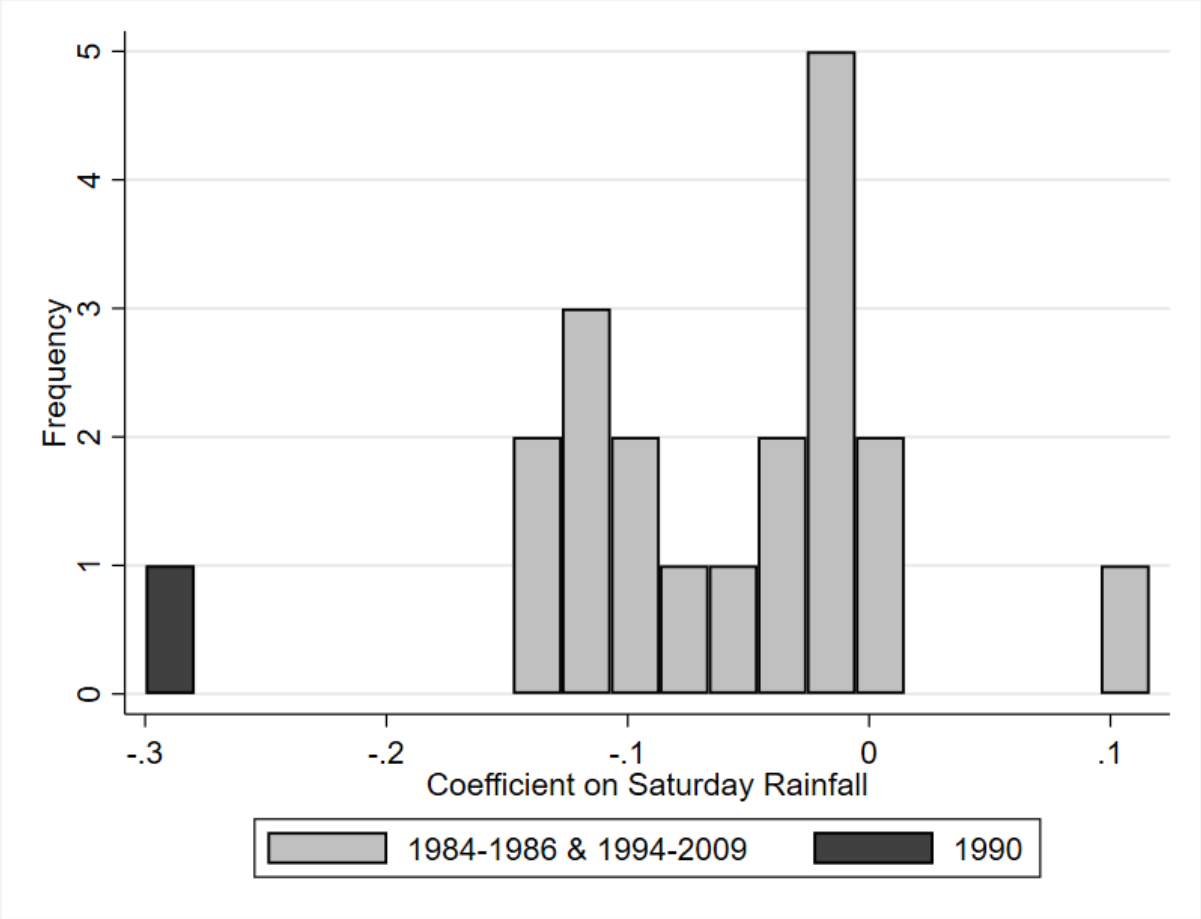
Notes: The map plots the number of Saturdays with rainfall above 10 mm during October 1, 1990 to March 31, 1994 in each sector.

Figure 3: Local Variation in Saturday Rainfall – Residuals



Notes: The map plots the residuals of the number of Saturdays with rainfall above 10 mm after netting out commune fixed effects and our other average rainfall controls.

Figure 4: Placebo Check



Notes: The figure shows the distribution of coefficients on the number of rainy Saturdays when using Saturday rainfall during the 3.5 years of the pre-genocide calendar period (October 1, YEAR to March 31, YEAR+4) from the starting years 1984 to 2009 to construct our variable of interest in Regression 3 in Table 2. Note that we exclude any periods that overlap with our period of interest. Thus, we use three placebo periods before the genocide (starting 1984, 1985 and 1986) and 16 periods after the genocide (starting years 1994 to 2009).

Table 1: Summary Statistics

	Mean	Std.Dev.	Obs.
A. Violence & Population			
# Civilian Perpetrators	290.25	286.43	1433
Civilian Participation Rate (%)	7.33	7.57	1433
Hutu Population, 1991 ('000)	4.46	2.34	1433
Tutsi Minority Share, 1991	0.09	0.09	142
Mass Grave in Sector, dummy	0.05	0.21	1432
B. Rainfall			
# Sundays(Rainfall>10 mm)	15.14	5.19	1433
# Mondays(Rainfall>10 mm)	15.13	4.22	1433
# Tuesdays(Rainfall>10 mm)	18.10	3.52	1433
# Wednesdays(Rainfall>10 mm)	20.51	4.76	1433
# Thursdays(Rainfall>10 mm)	21.53	3.97	1433
# Fridays(Rainfall>10 mm)	17.02	4.75	1433
# Saturdays(Rainfall>10 mm)	18.25	4.24	1433
Average Daily Rainfall, 1980s (mm)	2.75	0.48	1433
Average Daily Rainfall, 1990s (mm)	2.44	0.55	1433
# Public Holidays(Rainfall>10 mm)	1.50	0.99	1433
C. Geographical Variables			
Area, square km	14.04	12.86	1433
Perimeter, km	18.59	7.65	1433
Distance to Kigali, km	62.65	30.00	1433
Distance to Main City, km	22.78	14.69	1433
Distance to the Main Road, km	6.71	5.77	1433
D. Other Variables			
RTLM Coverage Share	0.19	0.22	1057
Vacant Mayor Seat, Oct 1993 (dummy)	0.04	0.18	142
Opposition Mayor, 1994 (dummy)	0.31	0.46	136

Notes: The sample consists of 1433 sectors and 142 communes. The last two variables and the Tutsi Minority Share vary at the commune level, all other variables at the sector level. The exact definitions and data sources of all variables are provided in Section 4.

Table 2: Main Effects

Dependent Variable:	Civilian Participation Rate (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10mm)	−0.371	−0.318	−0.300	−0.321		
	(0.112)***	(0.101)***	(0.104)***	(0.100)***		
# Sat(Rainfall>10mm), Oct90–Mar91					−0.063	−0.072
					(0.229)	(0.330)
# Sat(Rainfall>10mm), Apr91–Sep91					0.105	−0.516
					(0.548)	(0.538)
# Sat(Rainfall>10mm), Oct91–Mar92					−0.378	0.357
					(0.233)	(0.370)
# Sat(Rainfall>10mm), Apr92–Sep92					0.394	−0.249
					(0.351)	(0.629)
# Sat(Rainfall>10mm), Oct92–Mar93					−0.153	0.528
					(0.168)	(0.384)
# Sat(Rainfall>10mm), Apr93–Sep93					−0.463	0.760
					(0.312)	(0.600)
# Sat(Rainfall>10mm), Oct93–Mar94					−0.756	−0.795
					(0.231)***	(0.372)**
#Sun-Fri(Rainfall>10mm)				0.012		
				(0.035)		
# Sun(Rainfall>10mm)			0.026		−0.003	0.117
			(0.088)		(0.097)	(0.098)
# Mon(Rainfall>10mm)			0.060		0.085	−0.165
			(0.093)		(0.091)	(0.149)
# Tue(Rainfall>10mm)			0.021		0.021	0.119
			(0.070)		(0.068)	(0.177)
# Wed(Rainfall>10mm)			0.022		0.051	−0.069
			(0.096)		(0.091)	(0.192)
# Thu(Rainfall>10mm)			−0.046		−0.046	−0.174
			(0.107)		(0.101)	(0.137)
# Fri(Rainfall>10mm)			−0.051		−0.105	−0.076
			(0.090)		(0.092)	(0.187)
Standard Controls	yes	yes	yes	yes	yes	yes
Commune Effects	no	yes	yes	yes	yes	no
R^2	0.17	0.53	0.53	0.53	0.54	0.44
N	1433	1433	1433	1433	1433	142

Notes: In Regressions 1-5, the unit of observation is the sector. In Regression 6 the unit is the (more aggregated) commune level, obtained by collapsing our sector-level data at the commune level. **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm in Oct 1990–Mar 1994 (and similarly for all other weekdays). **# Sat(Rainfall>10 mm), OctXX–MarXX** is the number of Saturdays with rainfall above 10 mm during October 199X to March 199X+1, for all years 1990–1993. **# Sat(Rainfall>10 mm), AprXX–SepXX** is the number of Saturdays with rainfall above 10 mm in Apr 19XX–Sep 19XX, for all years 1991–1994. **# Sun-Fri(Rainfall>10 mm)** is the sum of all weekdays (except Saturday) with rainfall above 10 mm in Oct 1990–Mar 1994. **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. **Standard Controls** include the log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level (robust in regression 6). *p<0.1, **p<0.05, ***p<0.01.

Table 3: Reliability of Gacaca Data and Robustness Checks

Dependent Variable:	Alternative Dep. Var.			Placebo	Civilian Participation Rate (%)		
	# Deaths in Commune (Genodynamics)	Mass Grave in Sector	# Deaths in Commune (UCDP, 2013)	# RPF Deaths in Commune (UCDP, 2013)	Genocide Controls	Additional Controls	OLS, No Weighting
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Sat(Rainfall>10mm)	−801.383	−0.013	−223.051	0.685	−0.260	−0.337	−0.298
	(370.528)*	(0.004)***	(119.030)*	(0.882)	(0.105)**	(0.102)***	(0.143)**
# Sun(Rainfall>10mm)	259.683	0.001	147.904	−2.149	0.010	−0.046	0.029
	(182.301)	(0.004)	(135.415)	(1.305)	(0.087)	(0.094)	(0.112)
# Mon(Rainfall>10mm)	−243.112	−0.002	−169.500	0.780	0.008	0.139	0.101
	(213.905)	(0.004)	(192.540)	(0.910)	(0.092)	(0.090)	(0.108)
# Tue(Rainfall>10mm)	−339.469	0.008	−158.523	1.863	0.010	−0.006	0.049
	(316.845)	(0.004)*	(101.113)	(1.066)	(0.064)	(0.067)	(0.084)
# Wed(Rainfall>10mm)	276.912	0.006	−1.936	−0.105	0.041	−0.016	0.093
	(267.753)	(0.004)	(52.540)	(0.934)	(0.098)	(0.087)	(0.119)
# Thu(Rainfall>10mm)	164.716	−0.003	−14.435	0.483	0.017	−0.030	−0.030
	(301.543)	(0.004)	(69.538)	(0.845)	(0.113)	(0.105)	(0.137)
# Fri(Rainfall>10mm)	141.074	−0.008	−38.099	0.147	−0.056	−0.044	−0.062
	(374.508)	(0.003)**	(256.046)	(0.901)	(0.091)	(0.089)	(0.113)
Standard Controls	yes	yes	yes	yes	yes	yes	yes
Genocide Controls	no	no	no	no	yes	no	no
Additional Controls	no	no	no	no	no	yes	no
Commune Effects	no	yes	no	no	yes	yes	yes
R^2	0.20	0.16	0.07	0.07	0.54	0.54	0.50
N	100	1432	142	142	1433	1433	1433

Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm in Oct 1990–Mar 1994 (and similarly for all other weekdays). **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. Regressions 1-4 are reliability checks. In Regression 1, we use an estimate of the number of genocide deaths per commune from Genodynamics as an alternative dependent variable. In Regression 2, we use a dummy indicating whether at least one mass grave was found in the sector as an alternative dependent variable. In Regression 3, we use use violence committed by government forces (directed primarily against the Tutsi population), taken from the UCDP, as an alternative dependent variable and in Regression 4 we use use violence committed by the RPF, taken from the UCDP, as an alternative dependent variable, providing a placebo check. Regressions 5-7 are robustness checks. In Regression 5, we add controls for Saturday rainfall during the genocide to our main estimation. In Regression 6, we add additional controls (described below) and in Regression 7, we estimate the main model using ordinary least squares (OLS) estimation. **Standard Controls** are log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. **Genocide Controls** are the number of Saturdays with rainfall above 10 mm during the genocide (and similarly all other weekdays). **Additional Controls** are sector ruggedness, sector standard deviation in daily rainfall Jan 1984–Mar 1994, distance to Kigali, Nyanza, border, closest main road and closest main city and town as well as sector latitude and longitude. Regressions 1-6 are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. Note that the geographical resolution for these alternative data sources is the commune level. In Regression 1, we only have 100 observations because the death toll estimates are not available for all communes. **Standard errors** are clustered at the commune level (province level in Regressions 1, 3 and 4). *p<0.1, **p<0.05, ***p<0.01.

Table 4: Main Mechanisms (Interaction Effects)

Dependent Variable:	Civilian Participation Rate (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10mm)	−0.413	−0.399	−0.437	−0.281	−0.367	−0.504
	(0.101)***	(0.124)***	(0.105)***	(0.172)	(0.109)***	(0.129)***
... × MDR Mayor	−0.127	−0.120				
	(0.287)	(0.294)				
... × PSD/PL Mayor	1.029	1.045				
	(0.769)	(0.775)				
... × Mayor Seat Vacant in 1993	0.544	0.536				
	(0.167)***	(0.175)***				
... × Militia Violence			0.148			
			(0.082)*			
... × RTLM Coverage				−0.322		
				(0.188)*		
... × Population Density					0.071	
					(0.025)***	
... × Area/Perimeter						0.354
						(0.190)*
... × Tutsi Minority Share		−0.218				
		(1.680)				
Other Weekday Controls	yes	yes	yes	yes	yes	yes
Standard Controls	yes	yes	yes	yes	yes	yes
RTLM Controls	no	no	no	yes	no	no
Additional Controls	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes
R^2	0.55	0.55	0.63	0.51	0.54	0.54
N	1433	1433	1433	1057	1433	1431

Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm in Oct 1990–Mar 1994. **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. **MDR Mayor** is a dummy equal to 1 if the commune leader (mayor) is from the MDR. **PSD/PL Mayor** is a dummy equal to 1 if the mayor is from the PSD or PL. **Mayor Seat Vacant in 1993** is equal to 1 if the mayoral seat was vacant at least at the end of 1993. **Militia Violence** is the number of prosecution cases in Gacaca category 1. **RTLM coverage** is the fraction of the sector area receiving the RTLM signal. **Area/Perimeter** is our measure of compactness defined in the paper. The **Tutsi Minority Share** is the Tutsi population share in commune multiplied by total sector population. **Standard Controls** include log of sector population, average daily rainfall for Jan 1984–Sep 1990 and Oct 1990–Mar 1994. **Other Weekday Controls** include the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm in Oct 1990–Mar 1994. **Additional Controls** are sector ruggedness, sector standard deviation in daily rainfall Jan 1984–Mar 1994, distance to Kigali, Nyanza, border, closest main road and closest main city and town as well as sector latitude and longitude. **RTLM Controls** are distance to closest RTLM transmitter and the mean and variance of sector elevation. All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Table 5: Alternative Mechanisms

Dependent Variable:	Civilian Participation Rate (%)					
	Without Kigali	Without Major Cities	Public Holidays			Work/Lean Season
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10mm)	−0.350	−0.361		−0.358	−0.337	
	(0.106)***	(0.109)***		(0.097)***	(0.099)***	
# Public Holidays (Rainfall>10mm)			−0.069	−0.011	−0.003	
			(0.347)	(0.334)	(0.377)	
# Sat(Rainfall>10mm), lean season						−0.332
						(0.154)**
# Sat(Rainfall>10mm), work season						−0.342
						(0.156)**
Other Weekday Controls	yes	yes	no	no	yes	yes
Standard Controls	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes
R^2	0.54	0.54	0.54	0.54	0.54	0.54
N	1422	1358	1433	1433	1433	1433

Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm in Oct 1990–Mar 1994. **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. In Regression 1, we drop sectors in the capital Kigali, and in Regression 2, we drop all sectors in and close to major cities. In Regressions 3 to 5, we control for the number of public holidays with rainfall above 10 mm. **#Sat(Rainfall>10mm), work season** is the number of Saturdays with rainfall above 10 mm in Jan-Mar, Jun-Sep, and Dec, **# Sat(Rainfall>10 mm), lean season** is the number of Saturdays with rainfall above 10 mm in Mar-Apr, and Oct-Nov (for the time period Oct 1990-Mar 1994). **Other Weekday Controls** are the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm in Oct 1990–Mar 1994. **Standard Controls** are the log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. **Additional Controls** are sector ruggedness, sector standard deviation in daily rainfall Jan 1984–Mar 1994, distance to Kigali, Nyanza, border, closest main road and closest main city and town as well as sector latitude and longitude. All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Appendix – For Online Publication

A.1 Additional Figures

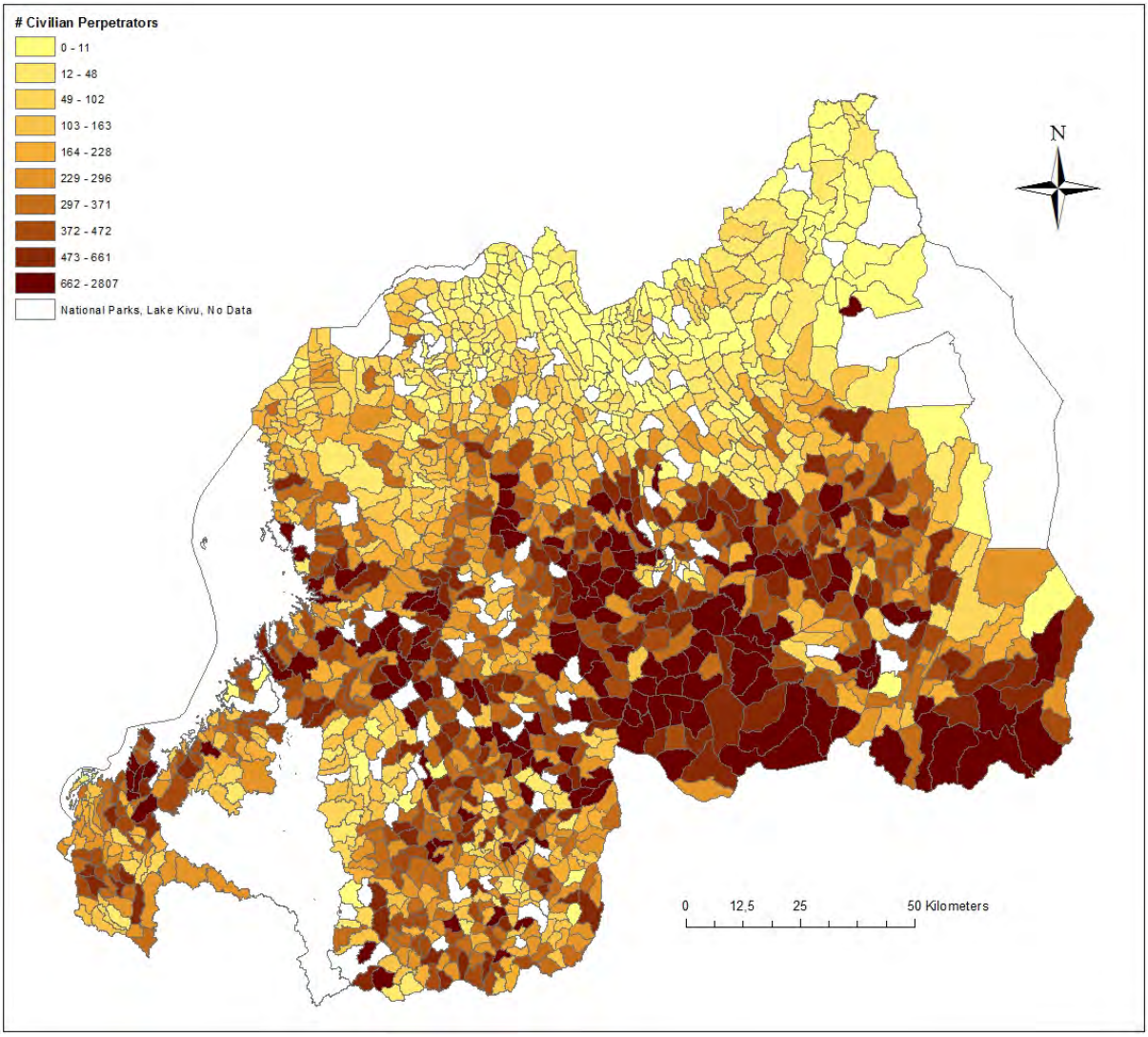
A.2 Extensions to Section 4 – Additional Data Description

A.3 Extensions to Sections 6.2 and 6.3 – Additional Validation and Robustness Checks

A.4 Extensions to Section 7 – Additional Mechanisms

A.1 Additional Figures

Figure A.1: Gacaca Data: Civilian Participation

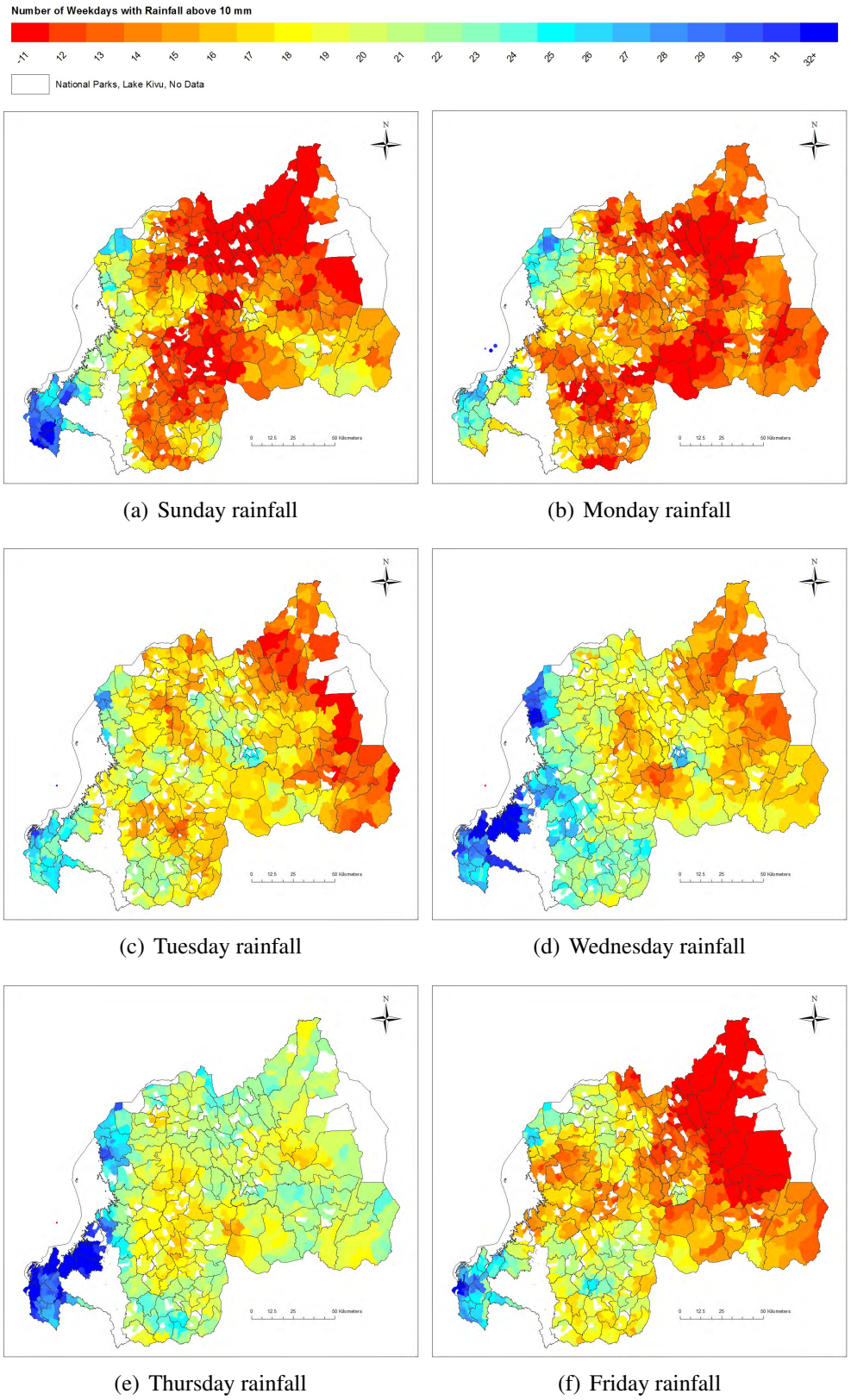


Notes: This maps shows the distribution of the number of civilian perpetrators in each sector. White areas represent lakes, national parks and missing observations.

Figure A.2: Relationship between Outdoor Events and Rainfall

Event type	Date	District	Link	Access Date	Rainfall	Description of event
Umuganda	19-May-2012	Nyagatare	http://www.ibidukikije.com/2012/05/rwanda-nyagatare-imvuraya tumye-umuganda-udasanzwewo-kurwanya-ibiza-udakorwa/	24-Feb-2017	1 mm	In the morning of 19/5/2012, the population of Nyagatare city was ready to go to Umuganda. However, at 7 am it started raining. Those who were on their way to Umuganda went back to their respective homes. Some told the newspaper that even though they brought their tools, they all returned home because rainfall found them on the way. (Translated from Kinyarwanda)
Umuganda	28-Feb-2015	Gicumbi	http://www.gicumbi.gov.rw/index.php?id=38&tx_ttnews%5Btt_news%5D=104&cHash=16ab35c781b1fdc2b6c0cc7db450d20Z	24-Feb-2017	9-15 mm	On 28/02/2015, Umuganda was planned as usual of the end of the month in all sectors of Gicumbi District. However in most sites, Umuganda was disturbed by the rain and participation of the population became very low. (Translated from Kinyarwanda)
Umuganda	30-Apr-2016	Ngoma	http://www.ngoma.gov.rw/index.php?id=38&%20tx_ttnews[tt_news]=334&cHash=523c816de256ba76a47cdc002ed6d9b4	24-Oct-2017	8 mm	In Remera sector Umuganda was stopped because it started to rain. (Translated from Kinyarwanda)
Umuganda	27-Feb-2016	Nyanza	http://www.kigalitoday.com/amakuru/mu-rwanda/Uko-umugandausoza-ukwezi-witabiriwe-AMAFOTO	24-Feb-2017	8 mm	In Nyanza district, while umuganda was going on, it started to rain and Umuganda was stopped. (Translated from Kinyarwanda)
Umuganda	30-Apr-2011	Kigali	http://catholictumbacollege.blogspot.se/2011/06/umugandarusange.html	24-Feb-2017	2 mm	Around 50 students of Tumba College and 30 residents participated in Umuganda and the outcome was good, though they didn't finish the work because Umuganda started with delay because the rain fell early in the morning. (Translated from Kinyarwanda)
Pan-African Dance Festival (FESPAD)	23-Feb-2013	Kigali	http://www.newtimes.co.rw/section/read/106345/	24-Oct-2017	18 mm	A hail and thunderstorm-filled stopped the Pan-African Dance Festival.
Football match	18-Nov-2016	Kigali	http://ruhagoyacu.com/spip.php?article14232	3-Feb-2017	16 mm	The football match on the fifth day of the championship between Kiyovu Sports and Musanze FC was stopped at 78th minute because of a heavy rain. Kiyovu was leading the match with 2-1, but with the heavy rain the playground was full of water and the match could not continue. (Translated from Kinyarwanda)
Football match	30-May-2015	Ngoma	http://www.igihe.com/imikino/volleyball/article/imvura-numwijima-byatumye-umukino	3-Feb-2017	6 mm	The rain in Ngoma District has stopped the football match between Rayons Sport and Inatek twice; both teams were fighting for the 1st place. (Translated from Kinyarwanda)
Commemoration walk	7-Apr-2016	Kigali	http://yeejo.rw/imyidagaduro/amakuru/article/walk-to-rememberntiyaba-kububere-ikibazo-cyivuraariko-abaturage-barihagaye	24-Feb-2017	8 mm	The rain stopped the "walk to remember 2016" that is normally done during the genocide week memorial from CND to Amahoro stadium. (Translated from Kinyarwanda)

Figure A.3: Local Rainfall Variation for all Other Weekdays



(a) Sunday rainfall

(b) Monday rainfall

(c) Tuesday rainfall

(d) Wednesday rainfall

(e) Thursday rainfall

(f) Friday rainfall

Notes: The maps plot the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm during October 1, 1990 to March 31, 1994 in each sector.

A.2 Additional Data Description

In this section we present additional information on the data introduced in the paper or solely used in the Appendix.

A.2.1 Data Matching

We combine several datasets from various sources to construct our final dataset with a total of 1,433 Rwandan sectors in 142 communes. The different datasets are matched by sector names within communes (the administrative unit above the sector). The matching is, however, not perfect: some sectors have different names in different data sources, and in some cases, two or more sectors within the same commune have identical names, which prevents successful matching. Nonetheless, in total, only about five percent of the sectors do not have a unique match across all datasets. Furthermore, these issues are likely idiosyncratic, which means that they will simply result in a lower precision in the estimates than in the case of perfect matching. Whenever geo-coded data was available, the matching was done in ArcGIS.

A.2.2 Gacaca Data – Legal Definitions

The Gacaca court data distinguishes between different categories of violent perpetrators, using the definitions below. Throughout our analysis we focus on Category 2.

Category 1: According to the precise definition taken from the National Service of Gacaca Jurisdiction, this category 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) well-known murderers who distinguished himself because of the zeal that characterized him in the killings or the excessive wickedness with which the killings were carried out; (iv) people who committed rape or acts of sexual torture.

Category 2: According to the precise definition taken from the National Service of Gacaca Jurisdiction, this category concerns: (i) authors, co-authors, accomplices of deliberate homicides, or of serious attacks that caused someone's death; (ii) persons who – with the intention of killing – caused injuries or committed other serious violence, but without actually causing death; (iii) persons who committed criminal acts or became the accomplice of serious attacks, without the intention of causing death. Lastly, category 3 includes people that committed property offences. Around 300,000 people were recorded in this category.

A.2.3 Ruggedness

Using elevation data at 30 arc-seconds ($\sim$ 0.9 kilometers at the equator) latitude-longitude grid cells, ruggedness is calculated in the following way. Let $e_{r,c}$ denote elevation at the point located in row r and column c of a grid of elevation points. The Terrain Ruggedness Index at that point is then given by

$$\sum_{i=r-1}^{i=r+1} \sum_{j=c-1}^{j=c+1} (e_{i,j} - e_{r,c})^2.$$

The elevation data source is GTOPO30 (US Geological Survey, 1996), a global elevation dataset developed through a collaborative international effort led by staff at the US Geological Survey's Center for Earth Resources Observation and Science (EROS).

A.2.4 Nightlight Density Data

Data on nighttime light density, which we use in a robustness check below, is provided by the National Geophysical Data Center (NGDC) on a yearly basis, starting from 1992. We overlay this grid with our sector polygon map to obtain average nighttime lights density in each sector.

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

A.2.5 Burundian Afrobarometer Data

To provide an indirect test of the link between heavy rainfall on Saturdays and participation in community meetings, we use the Afrobarometer survey for Burundi (round 5). Individuals were asked whether they have attended community meetings in the last 12 months. Answers are given on a scale from 0 to 4, where 0 is "no, never" and 4 "yes, often." This dataset is geo-coded, which allows us to match respondents to a commune in Burundi (133 in total) and local variation in rainfall. We use individuals who identify as either Hutu or Tutsi and drop foreigners who might not be subject to the Burundian version of *Umuganda*.[1]

[1]The data can be downloaded at http://afrobarometer.org/countries/burundi-0 (accessed November 16, 2018).

Table A.1: Additional Summary Statistics

	Mean	Std.Dev.	Obs.
A. Violence & Population			
Pre-Genocide Violence (Viret, 2010), dummy	0.19	0.40	1433
Pre-Genocide Violence by FAR (UCDP, 2013), dummy	0.15	0.36	142
Pre-Genocide Violence by RPF (UCDP, 2013), dummy	0.03	0.17	142
Pre-Genocide Violence against Civilians (UCDP, 2013), dummy	0.17	0.38	142
Pre-Genocide Violence by FAR (UCDP, 2013), # Deaths	17.42	90.51	142
Pre-Genocide Violence by RPF (UCDP, 2013), # Deaths	0.52	4.80	142
Genocide Casualties 1994 (Genodynamics), # Deaths	4587.16	6189.56	100
Genocide Violence by FAR (UCDP, 2013), # Deaths	989.33	3853.08	142
Genocide Violence by RPF (UCDP, 2013), # Deaths	7.69	36.18	142
Population in 1991, '000	4.88	2.48	1433
Population Density 1991, per square km	498.53	850.45	1433
Hutu Population Density 1991, per square km	448.62	696.39	1433
B. Rainfall			
# Sun(Rainfall>10 mm), Genocide Period	0.32	0.48	1433
# Mon(Rainfall>10 mm), Genocide Period	1.30	0.46	1433
# Tue(Rainfall>10 mm), Genocide Period	1.42	0.72	1433
# Wed(Rainfall>10 mm), Genocide Period	0.55	0.69	1433
# Thu(Rainfall>10 mm), Genocide Period	0.00	0.07	1433
# Fri(Rainfall>10 mm), Genocide Period	1.17	0.39	1433
# Sat(Rainfall>10 mm), Genocide Period	0.05	0.22	1433
C. Geographical Variables			
Distance to Town, km	16.58	8.83	1433
Distance to Nyanza, km	64.36	30.74	1433
Distance to the Border, km	22.60	13.93	1433
Ruggedness Index	3.45	1.38	1433
Distance to Closest RTLM Transmitter, km	5.18	2.85	1057
Mean Altitude, km	1.71	0.23	1057
Variance in Altitude, km	9.07	10.37	1057
D. Other Variables			
Nightlight Density, 1992	0.54	4.17	1432
Nightlight Density, 1993	0.62	3.95	1432

Notes: The sample consists of 1433 sectors and 142 communes. All the violence variables (except the first one) vary at the commune level, all other variables at the sector level. The exact definitions and data sources of all variables are provided in Sections 4 and A.2.

A.3 Additional Validation and Robustness Checks

In this section we present various additional tests and robustness checks.

Community Meetings in Burundi In Table **??** we examine the relationship between community meetings and rainfall in Burundi as a validation check for our strategy. Importantly, Burundi is very similar to Rwanda along numerous socio-economic and cultural dimensions[2] and in July 2006, Burundi introduced *Ibikorwa rusangi*, a practice similar to *Umuganda* with community meetings held every Saturday.

Regression 1 suggests that Saturday rainfall is significantly negatively related to community meeting attendance and this relationship is robust to adding province fixed effects (Regression

[2]The two neighboring countries share a common colonial history and have both suffered from ethnic tensions between Hutu and Tutsi.

2) and rainfall on all other weekdays (Regression 3). Furthermore, none of the point estimates for the other weekdays is significant. This is further true when adding them one-by-one in Regressions 4 to 9. Finally, since we are using a orderd logit model in Regressions 1 to 9, the results are robust to OLS in Regression 10.

Linear Specification In Table **??** we show that the results are robust to using average daily rainfall in each sector. Regressions 1 to 8 show that there is a strong negative relationship for Saturday rainfall but none of the other weekdays.

Alternative Rainfall Controls In our main regressions, we linearly control for average sector rainfall, and we allow average rainfall effects to differ for our period of interest (1990-1994) and the time before (1984-1990). In Table **??** below, we show that our results are robust to a) pooling the two periods, i.e. controlling for average rainfall for 1984-1994 (Regressions 1, 3 and 4), b) controlling only for average rainfall from 1990-1994 (Regressions 2, 5 and 6), and c) accounting for non-linear rainfall effects (for instance on agricultural production) by including a full set of dummies for average sector rainfall (Regressions 3 to 6).

Balance Test and Spatial Standard Errors In Table **??** we split our sample at the median of average Saturday rainfall and report summary statistics for our exogenous variables. Importantly, there are no significant differences between these two samples.

In Table **??**, we show that our results are robust to adjusting standard errors for spatial correlation within 25, 50, and 75 kilometers.

Rainfall Shocks In the main paper, we focus on days with heavy rain (i.e. rainfall above 10 mm), and control for long-term and general rainfall effects by including average daily rainfall before and during our period of interest. An alternative approach is to use rainfall shocks, i.e. deviations of Saturday rainfall from long-term averages normalized by their long-term standard deviation. We calculate these shocks in two different ways: first, for each sector, we take average Saturday rainfall (in mm) for our period of interest and subtract long-term average Saturday rainfall (measured for the period January 1984 to September 1990). Then we divide that difference by the long-term standard deviation of Saturday rainfall (again measured from January 1984 to September 1990).

Second, again for each sector, we use the number of Saturdays with rainfall above 10 mm for our period of interest and subtract the number of Saturdays with rainfall above 10 mm for the period from January 1984 to September 1990. Then we normalize that difference by the standard deviation in the number of rainy Saturdays, again for the period from January 1984 to September 1990.

The results in Table **??** suggest that our results are robust to using these alternative specifications. While the Saturday shocks are always negatively related to civilian participation, the point estimates for all other weekday shocks are never significant and smaller.

Additional Data Reliability Tests In the main paper we use three alternative violence outcomes to show that our results are not affected by survival bias or other systematic errors in the Gacaca prosecution data. In this section we rule out an additional concern.

Throughout our period of interest from 1990 to 1994, violent acts against the Tutsi population and (Hutu) members of the opposition were already taking place. If perpetrators from the civil war preceding the genocide (October 1990 to August 1993) are included in the Gacaca

data, and there is a relationship between rainfall before the genocide and targeted violence during that period, our estimates would be biased. For example (i) some perpetrators may, in fact, have been accused of participation in massacres and other kinds of violence during the civil war (i.e. not during the genocide), and (ii) individuals who had previously participated in violence during the civil war were easier to recognize and thus prosecute for genocide crimes than individuals who "only" participated in the genocide. In order to mitigate these concerns, we exclude areas with massgraves (Regression 1 in Table **??**) and areas with reported incidents of violence against the Tutsi during the period October 1990 to March 1994. We use two different measures: (i) violence committed by the Rwandan Hutu government forces, identified in UCDP (Sundberg and Melander, 2013), and (ii) violence with civilian casualties committed by any actor except the RPF, identified in Viret (2010). Reassuringly, our results are robust to excluding areas with pre-genocide violence (Regressions 2 and 3 in Table **??**).

RTLM and Militia Violence Two other papers on the Rwandan Genocide stress the importance of the RTLM radio station (Yanagizawa-Drott, 2014) and the army and militia (Rogall, 2017) in mobilization. As an additional robustness check, we show that our estimate is robust to controlling for both the share of the sector with RTLM radio coverage and organized violence carried out by militia and army (Regressions 4 and 6 in Table **??**). Note that when adding the RTLM controls, the significance on Saturday rainfall drops to the 90 percent confidence level. This is because the sample for which we have information on RTLM coverage includes 400 fewer observations, which is shown in Regression 5 where we rerun our main specification with this smaller sub-sample and obtain the same coefficient (and the same 90 percent confidence level).

Pre-Genocide Fighting During our period of interest, there were ongoing confrontations between the RPF and the FAR in Northern Rwanda. To rule out that our results are driven by these areas, we separately exclude the full combat zone of 187 sectors located in the provinces of Ruhengeri and Byumba in Northern Rwanda, and the subset of 22 sectors in this area that remained under the control of the RPF even after the Arusha Talks. Our results are unaffected by excluding these areas (Regressions 1 and 2 in Table **??**).

Transformation of the Dependent Variable We also show that the results are robust to various transformations of our dependent variable (also in Table **??**); in particular, to using total sector population to normalize the number of civilian perpetrators, and to using the logarithm of our dependent variable (to account for observations with no violence we add a 1 to the dependent variable). Importantly, the point estimate of -0.037 in the log-specification (standard error 0.012, Regression 6) suggests that a one standard-deviation increase in the number of rainy Saturdays decreases civilian participation by about 15 percent (similar to the magnitude from our baseline results above). In the spirit of a placebo check, the results for organized perpetrators are insignificant (Regressions 7 and 8). This is not surprising: organized perpetrators mainly consisted of members of the militia or the army, it is therefore unclear if the sector where they committed their genocide crimes (and where they were subsequently prosecuted) is the same as the sector in which they lived – and were exposed to *Umuganda* – before the genocide. Thus, they may not have been exposed to the same number of *Umuganda* meetings as the population of that sector.

Varying the Thresholds In Figure **??**, we show that our results do not depend on the choice of rainfall threshold, by varying the threshold in increments of 1 mm, from 5 to 15 mm. Heavy rainfall on Saturdays is negatively related to civilian participation for all thresholds above 5 mm and significant on, at least, the 90 percent significance level for the 7 mm threshold and all thresholds between 10 and 14 mm (the 8 mm and 9 mm thresholds are just falling short of significance with p-values of around 0.15). However, we cannot reject the null hypothesis that the point estimates for e.g. 9 mm and 11 or 12 mm are equal. Note that for higher thresholds the magnitude of the effects decreases again (in absolute value). This is not surprising since when using these high thresholds, days where *Umuganda* was canceled will wrongly be assigned to the control group and thus create a non-random measurement error.

Multicollinearity in Rainfall We also rerun our main specification using daily rainfall above 10 mm for each weekday in separate regressions, to make sure that multicollinearity between rainfall on the different weekdays are not hiding otherwise significant effects of the other weekdays (Sun–Fri). Figure **??** confirms that only Saturday rainfall is significantly related to civilian participation in genocide violence. The coefficients for rainfall on all other weekdays are much smaller in magnitude and statistically insignificant.

Outliers To test whether our results are affected by outliers, we rerun our main specification and drop one commune at a time. The resulting estimates range from -0.200 to -0.345 and are significantly different from zero at the 99 percent confidence level in all cases (Figure **??**). In addition, Figure **??** graphically shows that the negative relationship between the number of rainy Saturdays and civilian violence is not driven by outliers (and neither is the null result for all other weekdays, Figure **??**).

Table A.2: Burundi – First Stage

Dependent Variable:	Community Meeting Attendance									Community Meeting Attendance, dummy
Regression Method:	Ordered Logit									OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sat(Rainfall > 10 mm)	−0.055	−0.074	−0.076							−0.018
	(0.026)**	(0.035)**	(0.037)**							(0.009)**
Sun(Rainfall > 10 mm)			−0.056	−0.054						−0.015
			(0.044)	(0.042)						(0.011)
Mon(Rainfall > 10 mm)			−0.024		−0.022					−0.007
			(0.041)		(0.040)					(0.011)
Tue(Rainfall > 10 mm)			0.004			−0.021				−0.002
			(0.031)			(0.029)				(0.008)
Wed(Rainfall > 10 mm)			0.009				0.011			0.001
			(0.036)				(0.033)			(0.010)
Thu(Rainfall > 10 mm)			0.015					−0.001		0.001
			(0.031)					(0.031)		(0.008)
Fri(Rainfall > 10 mm)			0.019						0.012	0.001
			(0.034)						(0.031)	(0.009)
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province Effects	no	yes	yes	yes	yes	yes	yes	yes	yes	yes
p-value (wild bootstrap)										0.002
R^2										0.07
N	1072	1072	1072	1072	1072	1072	1072	1072	1072	1072

Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period Jul 2006–May 2010 (and similarly for all other weekdays). The dependent variable **Community Meeting Attendance** is taken from Round 5 of the Afrobarometer Survey for Burundi. Individuals are asked to rank their community meeting attendance on a scale from 0 to 4. In Regression 10, we create a dummy taking on the value of 1 if an individual indicates to have attended several meetings (answers 3 and 4). **Standard Controls** include average daily rainfall for Jul 1996–Jun 2006 and average daily rainfall for Jul 2006–May 2010. There are **133 communes** and **13 provinces** in the sample. **Robust standard errors** are in parentheses. Standard errors in Regression 10 are robust to clustering at the province level (using a wild bootstrap). The p-value refers to Saturday rainfall, all other regressors are insignificant. *p<0.1, **p<0.05, ***p<0.01.

Table A.3: Main Effects – Linear Specification

Dependent Variable:	Civilian Participation Rate (%)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Avg. Rain Sat	−3.222	−3.181						
	(1.317)**	(1.364)**						
Avg. Rain Sun		−0.217	−0.188					
		(1.572)	(1.522)					
Avg. Rain Mon		0.100		1.111				
		(1.376)		(1.611)				
Avg. Rain Tue		0.661			1.113			
		(1.070)			(1.309)			
Avg. Rain Wed		1.014				1.558		
		(1.148)				(1.183)		
Avg. Rain Thu		−1.100					−0.698	
		(0.992)					(1.096)	
Avg. Rain Fri		−0.547						−0.166
		(0.870)						(0.768)
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.53	0.53	0.53	0.53	0.53	0.53	0.53	0.53
N	1433	1433	1433	1433	1433	1433	1433	1433

Notes: **Avg. Rain Sat** is the average daily Saturday rainfall during Oct 1990–Mar 1994 (and similarly for all other weekdays). **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. **Standard Controls** include the log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Table A.4: Additional Robustness Checks – Average Rainfall Controls

Dependent Variable:	Civilian Participation Rate (%)					
	Average Rainfall Controls		Non-Linear Rainfall Controls			
			1984-1994		1990-1994	
	1984-1994	1990-1994	Quintiles	Deciles	Quintiles	Deciles
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10mm)	−0.318	−0.317	−0.291	−0.309	−0.296	−0.300
	(0.107)***	(0.108)***	(0.104)***	(0.105)***	(0.103)***	(0.104)***
# Sun(Rainfall>10mm)	0.012	0.048	0.045	0.032	0.055	0.026
	(0.089)	(0.090)	(0.090)	(0.089)	(0.096)	(0.088)
# Mon(Rainfall>10mm)	0.035	0.050	0.071	0.077	0.066	0.060
	(0.088)	(0.096)	(0.092)	(0.094)	(0.095)	(0.093)
# Tue(Rainfall>10mm)	0.006	0.022	0.030	0.046	0.029	0.021
	(0.070)	(0.073)	(0.071)	(0.072)	(0.071)	(0.070)
# Wed(Rainfall>10mm)	−0.000	0.012	0.040	0.057	0.022	0.022
	(0.097)	(0.096)	(0.103)	(0.102)	(0.099)	(0.096)
# Thu(Rainfall>10mm)	−0.063	−0.038	−0.042	−0.035	−0.036	−0.046
	(0.112)	(0.117)	(0.109)	(0.111)	(0.112)	(0.107)
# Fri(Rainfall>10mm)	−0.082	−0.082	−0.051	−0.039	−0.046	−0.051
	(0.084)	(0.088)	(0.090)	(0.091)	(0.089)	(0.090)
Non-Linear Rain Controls	no	no	yes	yes	yes	yes
Standard Controls	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes
R^2	0.53	0.53	0.54	0.54	0.54	0.53
N	1433	1433	1433	1433	1433	1433

Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm in Oct 1990–Mar 1994 (and similarly for all other weekdays). **Civilian Participation Rate** is defined as the number of civilian perpetrators divided by total sector population. In Regressions 1, 3 and 4, we control for average daily rainfall for Jan 1984–Mar 1994. In Regressions 2, 5 and 6, we only control for average daily rainfall for Oct 1990–Mar 1994. In Regressions 3–6, we non-linearly control for average rainfall by including a set of dummy variables indicating to which quantile or decile average daily rainfall 1984–1994 or 1990–1994 belongs to. **Standard Controls** include log of sector population, average daily rainfall for Jan 1984–Mar 1994 or average daily rainfall for Oct 1990–Mar 1994. All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level (province level in Regression 1). *p<0.1, **p<0.05, ***p<0.01.

Table A.5: Summary Statistics by Rainfall on Saturdays

	Low Rainfall	High Rainfall	
	Mean (Std.Dev.)	Mean (Std.Dev.)	p-Value
Population in Sector 1991, '000	5.030	4.731	0.263
	(2.394)	(2.559)	
Hutu Population in Sector 1991, '000	4.628	4.292	0.275
	(2.118)	(2.539)	
Tutsi Population in Sector 1991, '000	0.380	0.416	0.276
	(0.515)	(0.356)	
Population Density 1991, per square km	0.610	0.387	0.630
	(1.177)	(0.196)	
Tutsi Minority Share in Commune, 1991	0.069	0.109	0.320
	(0.074)	(0.093)	
Sector Area/Perimeter	0.464	0.514	0.063
	(0.126)	(0.117)	
Ruggedness Index	3.617	3.281	0.936
	(1.505)	(1.211)	
Nightlight Density, 1992	0.949	0.140	0.291
	(5.700)	(1.422)	
Nightlight Density, 1993	1.064	0.172	0.545
	(5.456)	(1.003)	
Village Centroid Latitude, km	9781.806	9768.154	0.384
	(39.477)	(45.812)	
Village Centroid Longitude, km	789.015	832.588	0.319
	(40.174)	(37.962)	
Distance to Kigali, km	67.074	58.241	0.451
	(36.333)	(21.036)	
Distance to Main City, km	19.347	26.205	0.939
	(10.652)	(17.165)	
Distance to Town, km	17.453	15.717	0.305
	(8.717)	(8.872)	
Distance to Nyanza, km	68.704	60.022	0.077
	(20.841)	(37.656)	
Distance to the Main Road, km	6.620	6.804	0.466
	(6.286)	(5.208)	
Distance to the Border, km	21.490	23.717	0.249
	(13.759)	(14.022)	
Share of Sector with RTLM Coverage	0.280	0.104	0.484
	(0.267)	(0.134)	
Distance to Closest RTLM Transmitter, km	4.768	5.541	0.190
	(3.380)	(2.242)	
Mean Sector Altitude, km	1.752	1.677	0.356
	(0.266)	(0.191)	
Variance in Sector Altitude, km	11.768	6.756	0.314
	(12.949)	(6.677)	

Notes: The full sample is split at the median value of average Saturday rainfall. The p-value of the test of equality of means is based on standard errors clustered at commune level and after netting out commune indicators. Variables at the commune level are clustered at province level. All variables marked with 1991 are taken from the 1991 census. There are 142 communes in the sample.

Table A.6: Additional Standard Errors

Dependent Variable:	Civilian Participation Rate (%)		
	25 km	50 km	75 km
	(1)	(2)	(3)
# Sat(Rainfall>10mm)	−0.298	−0.298	−0.298
	[0.123]**	[0.128]**	[0.127]**
# Sun(Rainfall>10mm)	0.029	0.029	0.029
	[0.090]	[0.083]	[0.077]
# Mon(Rainfall>10mm)	0.101	0.101	0.101
	[0.090]	[0.100]	[0.100]
# Tue(Rainfall>10mm)	0.049	0.049	0.049
	[0.097]	[0.111]	[0.104]
# Wed(Rainfall>10mm)	0.093	0.093	0.093
	[0.100]	[0.088]	[0.089]
# Thu(Rainfall>10mm)	−0.030	−0.030	−0.030
	[0.115]	[0.125]	[0.138]
# Fri(Rainfall>10mm)	−0.062	−0.062	−0.062
	[0.106]	[0.102]	[0.087]
Standard Controls	yes	yes	yes
Commune Effects	yes	yes	yes
R^2	0.50	0.50	0.50
N	1433	1433	1433

Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm in Oct 1990–Mar 1994 (and similarly for all other weekdays). **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. **Standard Controls** include the log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. **Standard errors** in square brackets, correct for spatial correlation within a radius of 25km, 50km and 75km, Conley (1999). The radius used in each regression is given in the column header. There are **142 communes** in the sample. *p<0.1, **p<0.05, ***p<0.01.

Table A.7: Main Effects – Alternative Specification Using Rainfall Shocks

Dependent Variable:	Civilian Participation Rate (%)			
	Rainfall Shock Average Rainfall		Rainfall Shock Number of Days	
	(1)	(2)	(3)	(4)
Saturday Rainfall Shock	−22.647	−21.951	−21.385	−20.452
	(7.589)***	(9.009)**	(7.412)***	(7.333)***
Sunday Rainfall Shock		1.291		4.724
		(9.590)		(5.936)
Monday Rainfall Shock		−2.381		4.426
		(8.442)		(6.379)
Tuesday Rainfall Shock		8.424		2.345
		(7.195)		(4.847)
Wednesday Rainfall Shock		6.528		2.552
		(6.559)		(6.581)
Thursday Rainfall Shock		−8.081		−2.142
		(6.412)		(8.310)
Friday Rainfall Shock		−4.616		−4.793
		(5.698)		(5.889)
Standard Controls	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes
R^2	0.53	0.53	0.53	0.53
N	1433	1433	1433	1433

Notes: **Saturday Rainfall Shocks** in Regressions 1–2 (Regressions 3–4) are defined as the average Saturday rainfall (number of Saturdays with rainfall above 10 mm) during our period of interest minus the long-term average rainfall (long-term number of Saturdays with rainfall above 10 mm) normalized by its long-term standard deviation (and similarly for all other weekdays). This gives an average threshold of 15 mm. The long-term averages and the standard deviations are for the period Jan 1984–Sep 1990. **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. **Standard Controls** include the log of sector population. All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Table A.8: Additional Reliability and Robustness Checks I

Dependent Variable:	Civilian Participation Rate (%)					
	Without Mass Graves	Excl. Pre-Violence (UCDP, 2013)	(Viret, 2010)	RTLM Controls	RTLM Sample	Militia Violence
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10mm)	−0.373	−0.387	−0.375	−0.272	−0.267	−0.250
	(0.114)***	(0.119)***	(0.124)***	(0.159)*	(0.158)*	(0.095)***
# Sun(Rainfall>10mm)	0.033	0.011	0.054	−0.024	−0.026	0.081
	(0.095)	(0.104)	(0.115)	(0.112)	(0.113)	(0.080)
# Mon(Rainfall>10mm)	0.063	0.161	0.120	0.187	0.182	−0.054
	(0.092)	(0.091)*	(0.113)	(0.132)	(0.130)	(0.081)
# Tue(Rainfall>10mm)	0.062	−0.000	−0.025	−0.050	−0.030	0.072
	(0.075)	(0.078)	(0.085)	(0.097)	(0.095)	(0.070)
# Wed(Rainfall>10mm)	0.006	−0.013	0.011	−0.079	−0.066	0.003
	(0.096)	(0.106)	(0.109)	(0.135)	(0.132)	(0.090)
# Thu(Rainfall>10mm)	−0.025	−0.055	0.103	0.039	0.038	0.040
	(0.105)	(0.108)	(0.133)	(0.135)	(0.136)	(0.094)
# Fri(Rainfall>10mm)	0.018	−0.082	−0.007	−0.113	−0.101	−0.056
	(0.090)	(0.103)	(0.107)	(0.114)	(0.114)	(0.081)
Standard Controls	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes
RTLM Controls	no	no	no	yes	no	no
Militia Controls	no	no	no	no	no	yes
R^2	0.52	0.52	0.51	0.50	0.50	0.62
N	1366	1193	1155	1057	1057	1433

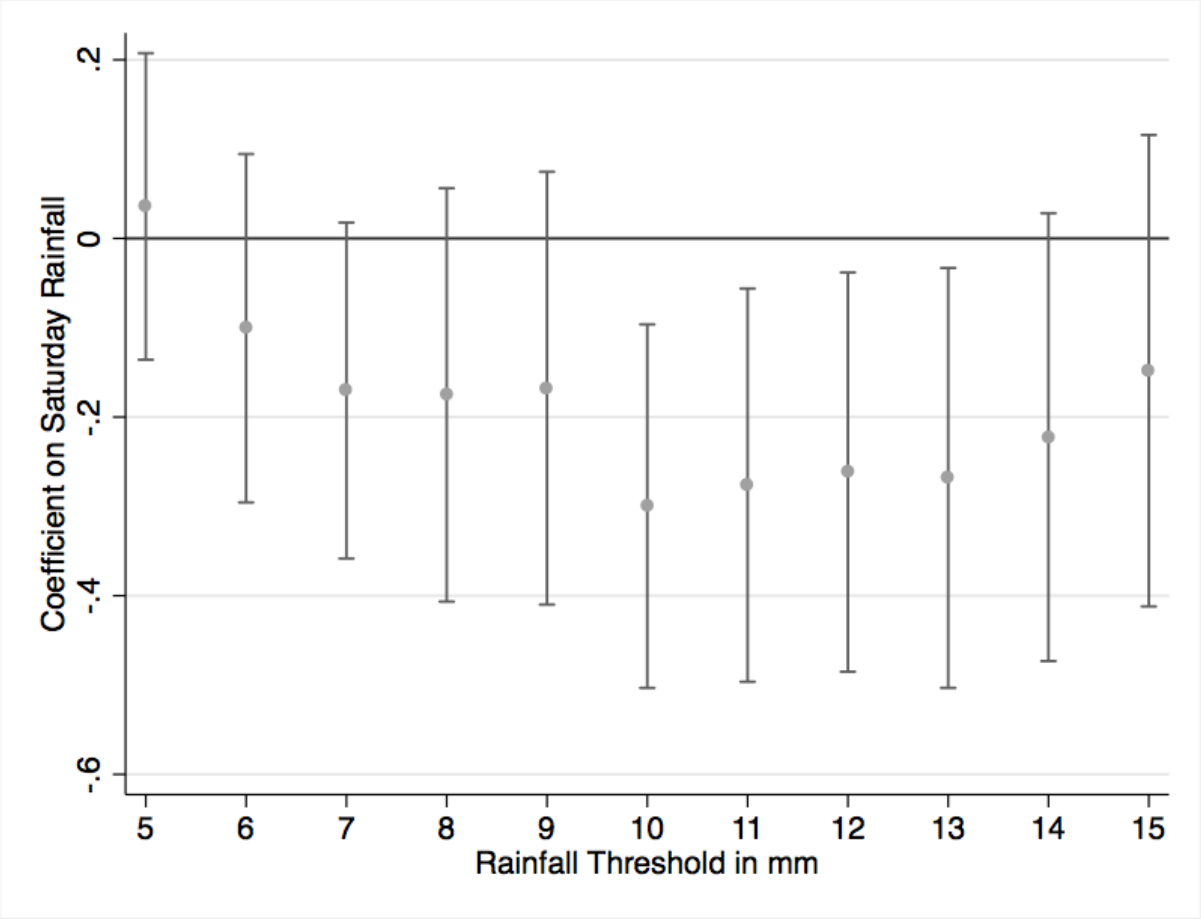
Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during Oct 1990–Mar 1994 (and similarly for all other weekdays). **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. In Regression 1, we drop sectors with at least one mass grave (indicating high death rates). In Regressions 2 and 3, we drop sectors where violence against primarily Tutsi took place before the genocide (using data from UCDP, 2013 and Viret, 2010, respectively). In Regressions 4 and 6, we add various controls. In Regression 5 we reproduce our main results with the smaller RTLM-sample. **Standard Controls** include the log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. **RTLM Controls** are distance to closest RTLM transmitter and the mean and variance of sector elevation. **Militia Controls** are the number of prosecution cases in Gacaca category 1. All regressions are run using weighted least squares (WLS) estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Table A.9: Additional Robustness Checks II

Dependent Variable:	Restricting Sample		Alternative Dependent Variables					
	Civilian Participation Rate (%)		Civilian Participation Rate (%)		Log[Civilian Partcipation Rate]		Organized Participation Rate (%)	
	Excluding Combat Zone	Excluding RPF Sectors	Normalization by Total Population		Transformation of Dep. Var		Alternative Dep. Var	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Sat(Rainfall>10mm)	−0.366	−0.304	−0.291	−0.275	−0.041	−0.037	−0.043	−0.036
	(0.117)***	(0.105)***	(0.092)***	(0.095)***	(0.012)***	(0.012)***	(0.029)	(0.027)
# Sun(Rainfall>10mm)	0.033	0.025		0.029		0.004		−0.039
	(0.093)	(0.088)		(0.081)		(0.011)		(0.029)
# Mon(Rainfall>10mm)	0.075	0.061		0.055		0.007		0.081
	(0.105)	(0.094)		(0.085)		(0.013)		(0.027)***
# Tue(Rainfall>10mm)	0.028	0.024		0.024		−0.008		−0.036
	(0.080)	(0.072)		(0.064)		(0.011)		(0.027)
# Wed(Rainfall>10mm)	0.047	0.023		0.025		0.003		0.014
	(0.109)	(0.097)		(0.088)		(0.013)		(0.026)
# Thu(Rainfall>10mm)	−0.031	−0.033		−0.039		−0.029		−0.062
	(0.124)	(0.112)		(0.097)		(0.018)		(0.035)*
# Fri(Rainfall>10mm)	−0.045	−0.050		−0.043		−0.010		0.004
	(0.105)	(0.092)		(0.083)		(0.011)		(0.024)
Standard Controls	yes	yes	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.47	0.53	0.50	0.50	0.71	0.71	0.37	0.38
N	1246	1411	1433	1433	1433	1433	1433	1433

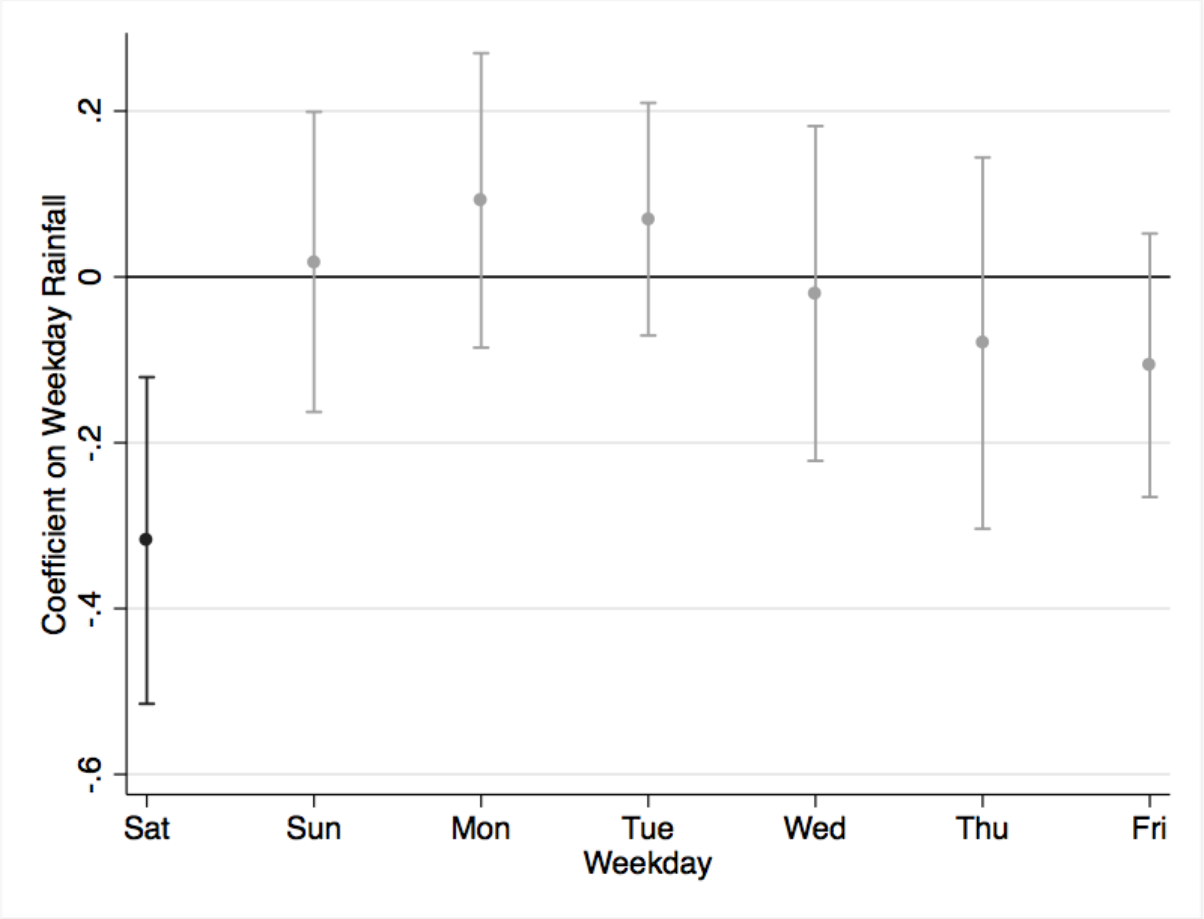
Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm in Oct 1990–Mar 1994 (and similarly for all other weekdays). In Regressions 3–4, the **Civilian Participation Rate** is defined as the number of civilian perpetrators divided by total sector population. In Regressions 5–6, the **Log[Civilian Participation rate]** is a logarithmic transformation of the number of civilian perpetrators divided by sector Hutu population and plus one to deal with civilian participation rates equal to zero. In Regressions 7–8, we use the **Organized Participation Rate**, defined as the number of perpetrators from crime category 1 divided by sector Hutu population, as an alternative outcome variable. **Standard Controls** are log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. All regressions are run using WLS estimation with Hutu population size as weights in all regressions but Regression 3–4, where total sector population size is used. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Figure A.4: Different Thresholds



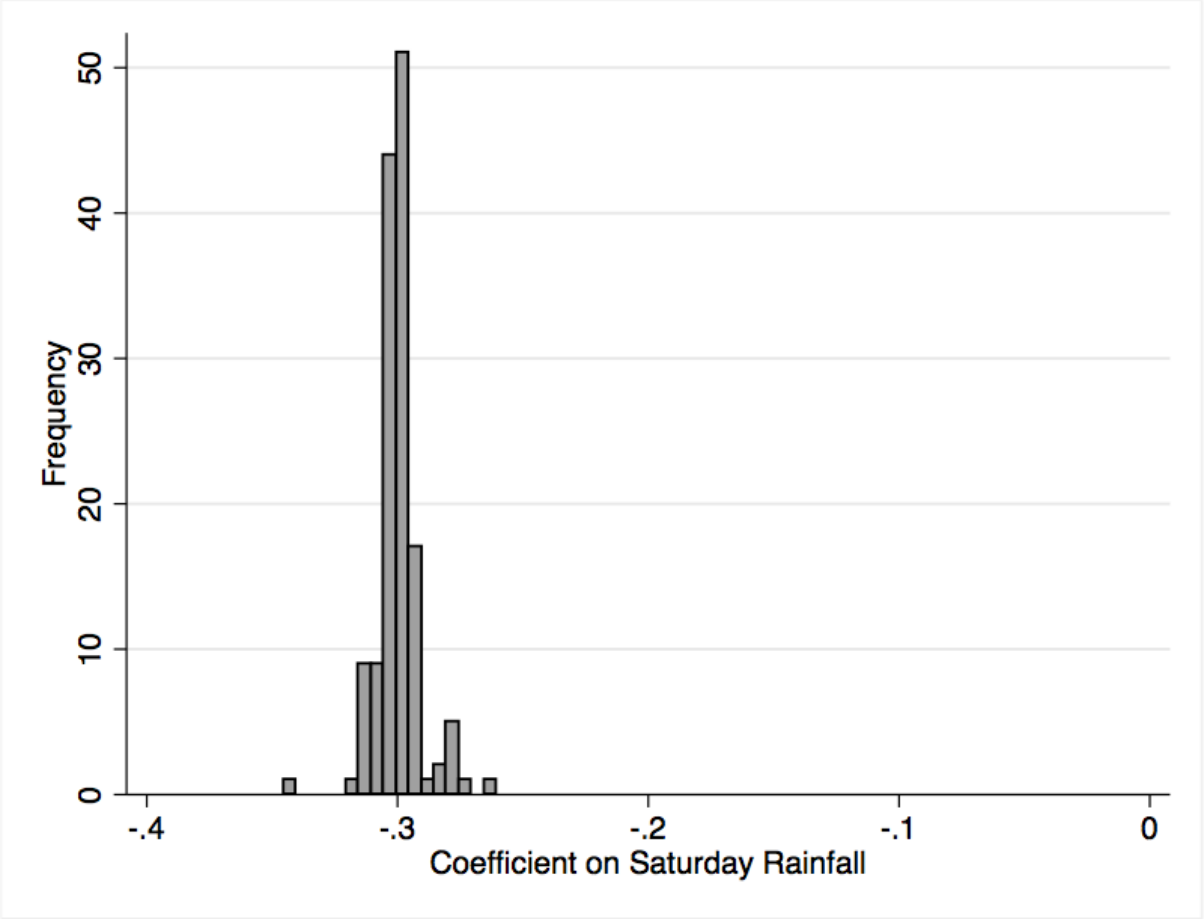
Notes: The figure shows the coefficients on the number of rainy Saturdays (together with 95 percent confidence bounds) when varying the rainfall threshold in our main specification (Regression 3 in Table 2). Each point represents a separate regression.

Figure A.5: Effects of Rainfall by Weekday



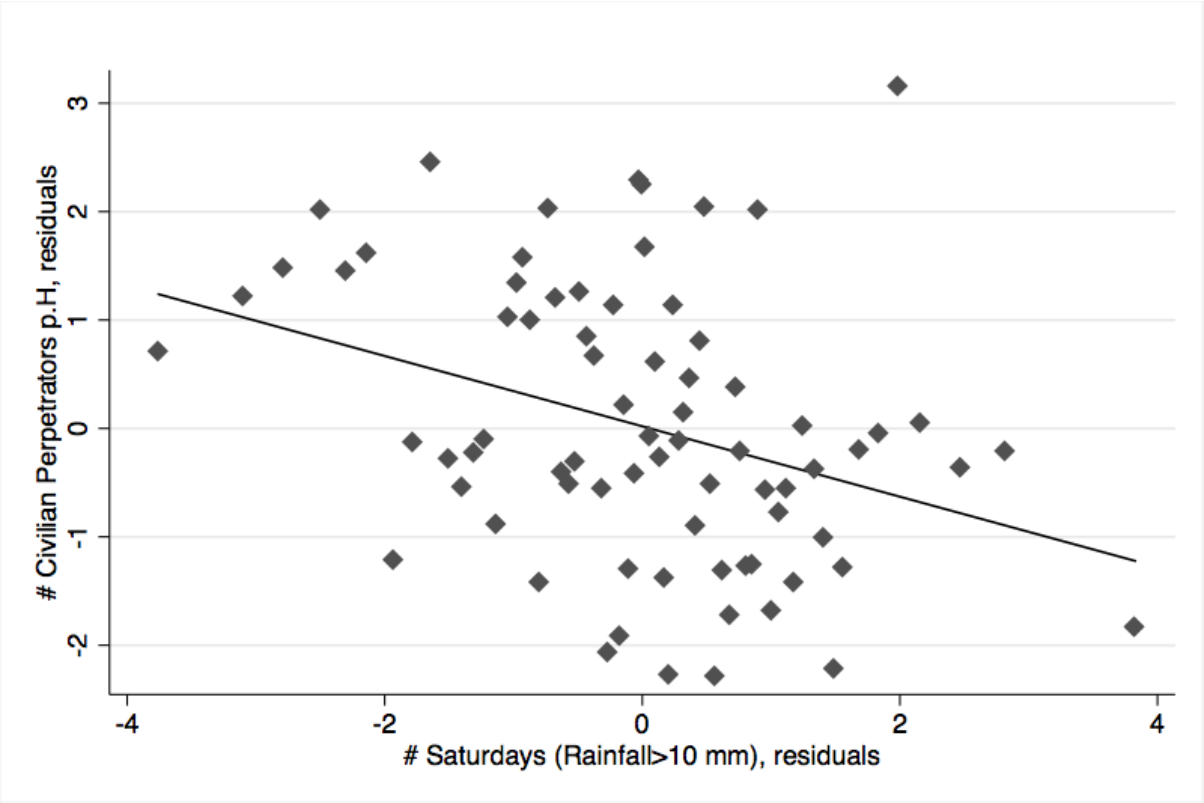
Notes: The figure shows the coefficients on the number of days with rainfall above 10 mm (together with 95 percent confidence bounds) for each weekday entering our main specification separately (Regression 2 in Table 2). Each point represents a separate regression.

Figure A.6: Outliers



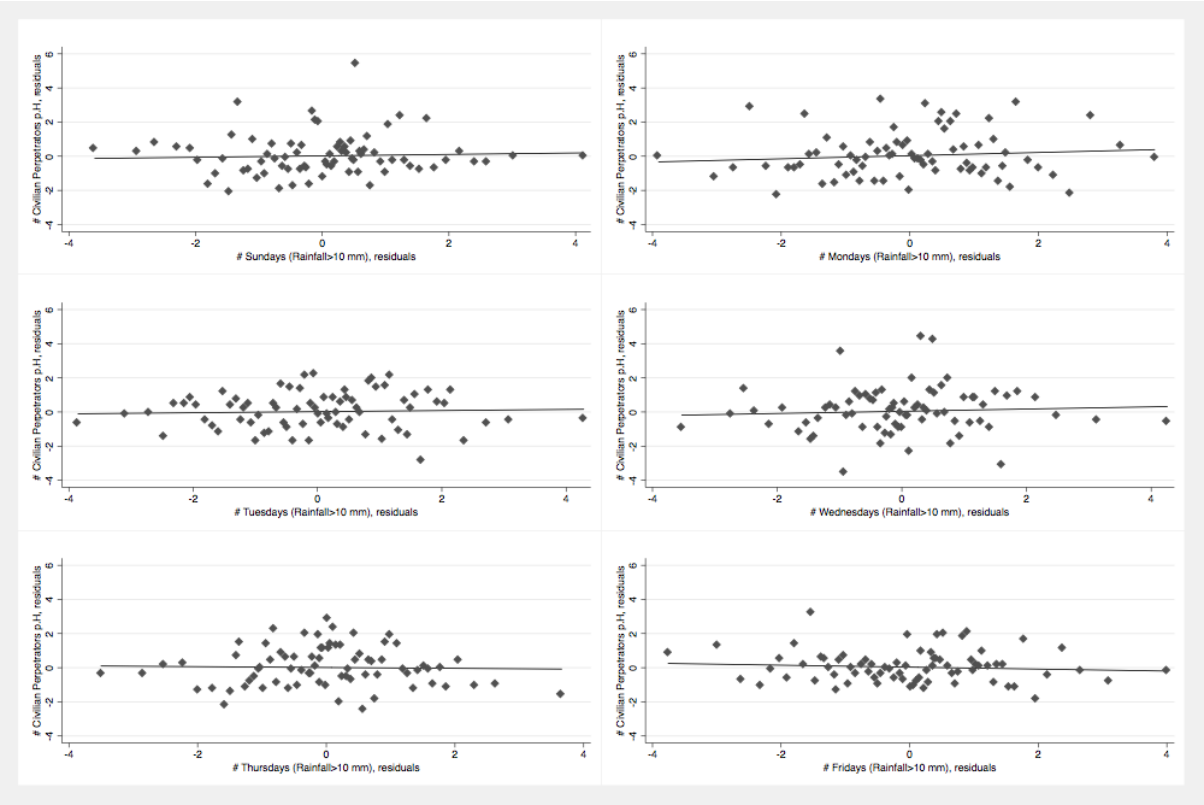
Notes: This figure shows the distribution of point estimates on the number of Saturdays with rainfall above 10 mm during October 1, 1990 to March 31, 1994 when dropping one commune at a time in our baseline specification (Regression 3 in Table 2).

Figure A.7: Saturday-Rainfall-Violence Relationship



Notes: Observations are grouped into 75 equal-sized bins. We use all controls in Regression 3 in Table 2 to construct residuals.

Figure A.8: Relationship between Civilian Participation Rate and Rainfall for all other Weekdays



Notes: Observations are grouped into 75 equal-sized bins. We use all controls in Regression 3 in Table 2 to construct residuals.

A.4 Additional Mechanisms

In this section we discuss and present analysis of a number of additional potential mechanisms behind our results.

Rainfall Shocks First, we show in Table **??** that all of our interaction effects from the paper are robust to using rainfall shocks instead of the number of Saturdays with rainfall above 10 mm.

Forced Interaction In the main paper we discuss and rule out voluntary social interaction as an alternative channel. Another possibility is that forced interaction between Hutu and Tutsi during *Umuganda*, at a time when ethnic tensions were growing in Rwanda, led to increased animosity towards the Tutsi population. If this is an important channel, we would expect *Umuganda* meetings to be particularly successful in stirring hatred against the Tutsi minority in places where initial tensions between the Hutu and Tutsi populations were already high. One indicator of such tensions is the occurrence or intensity of pre-genocide violence by the RPF (Tutsi-dominated rebel group) or the FAR (government forces) in a locality. However, Regressions 1 and 2 in Table **??** provide no support for such a "forced interaction channel." The interaction effects are all positive, suggesting *Umuganda* was less important in places that were already radicalized.[3]

One may also be concerned that military leaders met on Saturdays to discuss the genocide planning. This is unlikely to be affecting our results, as militia and organizers typically did not reside in the villages where they were prosecuted for crimes, and would thus not have been affected by rainy Saturdays in those villages in the pre-genocide period. Moreover, genocide planners were mostly concentrated to the main cities and Kigali, and our results are robust to dropping those. Consistently, we find that the number of rainy Saturdays is unrelated to organized violence which includes militiamen, planners and instigators (results in regressions 7 and 8 in Table **??** above).

Economic Channels One might worry that *Umuganda* affected the provision of public goods. Recall that some of the main tasks during *Umuganda* involved maintaining and building public goods such as various types of erosion controls, maintenance of roads, and construction of communal buildings. These public goods could increase economic well-being and thus affect civilian participation indirectly via income.[4] Moreover, public goods, such as roads and bridges, could affect participation directly by decreasing for instance transport costs – which may have mattered during the genocide itself.[5] In this subsection, we perform a number of tests that indicate that these mechanisms did not play a major role.

First, if *Umuganda* had an effect on civilian violence through such a "public-good channel," we should observe long-term effects of *Umuganda*. Especially larger infrastructure projects require time to build. However, recalling our results in Section 6.1, only the last months before the genocide seemed to matter for mobilization. To further address possible concerns about

[3]While there was militia violence also before the genocide (as detailed in Sundberg and Melander (2013) and Viret (2010)) the widespread civilian violence was a feature of the genocide.

[4]The direction of the effect is a priori unclear. *Umuganda* might increase participation in violence if more public goods increases civilians support for the local government (and thus makes them follow their orders). On the other hand, it could potentially decrease participation if people with more resources are less likely to commit violence (opportunity cost mechanism).

[5]In this case, we should expect *Umuganda* to be positively correlated with participation in violence.

confounders caused by economic effects of *Umuganda*, in this section we present the results from additional tests of the relationship between Saturday rainfall and economic outcomes.[6]

Although we lack disaggregated data on incomes or public goods, a proxy that is available to us is nighttime lights density (Henderson et al., 2012). Regressions 1–4 in Table **??** show that Saturday rainfall is unrelated to nighttime lights density for various time periods before the genocide. To rule out that this insignificant result is not simply due to measurement error, we also show that average daily rainfall in the same time period is indeed positively and significantly related to nighttime lights density – which has previously been found in the literature (see e.g. Hodler and Raschky, 2014). Note that nighttime lights are a rough proxy for wealth that may not be affected by the type of infrastructure projects that were the focus of *Umuganda*. We thus view these results as suggestive.

As an additional test, we use census data to construct a household wealth index at the commune level. The drawback of this data is that it is only available for 1991. The index incorporates information on housing quality, asset ownership and access to electricity and water. Since the census data was collected in July 1991, we re-calculate our main independent variable using Saturday rainfall for the time of October 1990 to July 1991. In Table **??** we report the results. Importantly, Saturday rainfall is uncorrelated with the wealth index: the point estimate is small and insignificant (Regression 1, Panel A). This also holds true when examining Hutu and Tutsi households separately (Panels B and C). Saturday rainfall is also unrelated to the share of households in the sector belonging to the poorest half or poorest quantile of the wealth distribution (Regressions 3 to 6). In Table **??** , we further show that the above results are robust to changing the time period for the independent variable to February 1988 to July 1991 – thus using 3.5 years before the wealth outcomes were measured (a time period of comparable length to the one in our main analysis).

Infrastructure Destruction Our main results show that especially rainfall during the last 6 months before the genocide is driving the effects. This might suggest an alternative channel: if rainfall destroys infrastructure or local roads this might potentially decrease civilian participation. In Table **??**, we address this concern. First, this channel should be particularly strong just before the genocide. Thus in Regression 1 we rerun Regression 5 from Table 2 in the paper but split the time period from October 1993 to March 1994 into monthly intervals. Importantly, the last four weeks before the genocide do not seem to be driving the results. Furthermore, if infrastructure destruction were the main channel, rainfall on all other weekdays should matter too. In Regression 2, we show that this is not the case, the point estimate on rainfall for all other days (again during the month before the genocide) is small and insignificant. Finally, in Regression 3 we show that total rainfall during the genocide does not seem to matter either. This provides further evidence that infrastructure destruction unlikely matters.

[6] All the tests below use income or wealth levels from before the genocide. One concern is that since the last six months of the meetings seem to have mattered the most, these tests might not fully address the public goods channel. However, while there is qualitative evidence that the political content of the meetings changed in the period just before the genocide, we have no reason to suspect systematic changes in the work load during *Umuganda* changed in this period.

Table A.10: Main Mechanisms – Alternative Specification Using Rainfall Shocks

Dependent Variable:	Civilian Participation Rate (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10mm)	−24.825	−22.575	−28.982	−20.644	−20.794	−36.707
	(11.727)**	(11.537)*	(10.837)***	(15.589)	(11.492)*	(15.283)**
... × MDR Mayor	19.722	18.385				
	(25.481)	(25.306)				
... × PSD/PL Mayor	132.313	148.730				
	(119.225)	(121.500)				
... × Mayor Seat Vacant in 1993	24.131	20.676				
	(10.580)**	(10.245)**				
... × Militia Violence			13.010			
			(6.366)**			
... × RTLM Coverage				−0.507		
				(0.196)**		
... × Population Density					9.385	
					(4.991)*	
... × Area/Perimeter						41.983
						(23.407)*
... × Tutsi Minority Share		−2.126				
		(1.273)*				
Other Weekday Controls	yes	yes	yes	yes	yes	yes
Standard Controls	yes	yes	yes	yes	yes	yes
RTLM Controls	no	no	no	yes	no	no
Additional Controls	yes	yes	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes	yes	yes
R^2	0.54	0.54	0.63	0.51	0.54	0.54
N	1433	1433	1433	1057	1433	1431

Notes: **Saturday Rainfall Shocks** are defined as the average Saturday rainfall during our period of interest minus the long-term average rainfall normalized by its long-term standard deviation (and similarly for all other weekdays). The long-term averages and the standard deviations are for the period Jan 1984–Sep 1990. **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. **MDR Mayor** is a dummy equal to 1 if the commune leader (mayor) is from the MDR. **PSD/PL Mayor** is a dummy equal to 1 if the mayor is from the PSD or PL. **Mayor Seat Vacant in 1993** is equal to 1 if the mayoral seat was vacant at least at the end of 1993. **Militia Violence** is the number of prosecution cases in Gacaca category 1. **RTLM coverage** is the fraction of the sector area receiving the RTLM signal. **Area/Perimeter** is our measure of compactness defined in the paper. The **Tutsi Minority Share** is the Tutsi population share in commune multiplied by total sector population. **Standard Controls** include log of sector population, average daily rainfall for Jan 1984–Sep 1990 and Oct 1990–Mar 1994. **Other Weekday Controls** include Sun/Mon/Tue/Wed/Thu/Fri rainfall shocks. **Additional Controls** are sector ruggedness, sector standard deviation in daily rainfall Jan 1984–Mar 1994, distance to Kigali, Nyanza, border, closest main road and closest main city and town as well as sector latitude and longitude. **RTLM Controls** are distance to closest RTLM transmitter and the mean and variance of sector elevation. All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Table A.11: Alternative Mechanism: Forced Interaction

Dependent Variable:	Civilian Participation Rate (%)	
	(1)	(2)
# Sat(Rainfall>10mm)	−0.415	−0.408
	(0.124)***	(0.108)***
... × RPF Pre-Genocide Violence (Presence)	0.200	
	(0.196)	
... × FAR Pre-Genocide Violence (Presence)	0.236	
	(0.217)	
... × RPF Pre-genocide Violence (# Deaths)		0.019
		(0.005)***
... × FAR Pre-genocide Violence (# Deaths)		0.002
		(0.001)**
Other Weekday Controls	yes	yes
Standard Controls	yes	yes
Additional Controls	yes	yes
Commune Effects	yes	yes
R^2	0.54	0.55
N	1433	1433

Notes: **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm in Oct 1990–Mar 1994. **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. **RPF [FAR] Pre-Genocide Violence (Presence)** is a dummy taking the value 1 if any pre-genocide violence against civilians was committed by the RPF [FAR] in the commune in Oct 1990–Mar 1994. **RPF [FAR] Pre-Genocide Violence (# Deaths)** is an estimate of the number of civilian deaths in RPF [FAR] attacks against civilians in Oct 1990–Mar 1994. All pre-genocide violence variables are from UCDP (2013). **Other Weekday Controls** are the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm in Oct 1990–Mar 1994. **Standard Controls** are the log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. **Additional Controls** are sector ruggedness, sector standard deviation in daily rainfall Jan 1984–Mar 1994, distance to Kigali, Nyanza, border, closest main road and closest main city and town as well as sector latitude and longitude. All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Table A.12: Alternative Mechanism: Economic Channels

Dependent Variable:	Nighttime Lights Density, log			
	1992		1993	
	(1)	(2)	(3)	(4)
# Sat(Rainfall>10mm), 90–92	−0.019	−0.017		
	(0.012)	(0.011)		
# Sat(Rainfall>10mm), 90–93			0.002	0.002
			(0.008)	(0.008)
Average Daily Rainfall, 90–92	0.543	0.458		
	(0.208)**	(0.193)**		
Average Daily Rainfall, 90–93			0.657	0.568
			(0.198)***	(0.232)**
Other Weekday Controls	no	yes	no	yes
Standard Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes
Commune Effects	yes	yes	yes	yes
R^2	0.85	0.85	0.88	0.88
N	1432	1432	1432	1432

Notes: **# Sat(Rainfall>10 mm), 90–93** is the number of Saturdays with rainfall above 10 mm during Oct 1990–Dec 1993 (and similarly for 90–92). **Standard Controls** include the log of sector population, average daily rainfall for Jan 1984–Sep 1990. **Other Weekday Controls** include the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm during Oct 1990–Dec XX (where XX can be 1992 or 1993 respectively). **Additional Controls** are sector ruggedness, sector standard deviation in daily rainfall Jan 1984–Mar 1994, distance to Kigali, Nyanza, border, closest main road and closest main city and town as well as sector latitude and longitude. All regressions are run using weighted least squares (WLS) estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.

Table A.13: Alternative Mechanism: Wealth Measure (Census) I

Dependent Variable:	Wealth Index		Wealth Below Median		Poorest Quintile	
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10 mm), Oct90-Jul91	−0.0244	−0.0205	0.0074	0.0073	−0.0042	−0.0035
	(0.0441)	(0.0423)	(0.0163)	(0.0146)	(0.0110)	(0.0101)
Standard Controls	yes	yes	yes	yes	yes	yes
Other Weekday Controls	no	yes	no	yes	no	yes
R^2	0.21	0.32	0.31	0.43	0.31	0.42
N	142	142	142	142	142	142
Panel B: Hutu Population						
# Sat(Rainfall>10 mm), Oct90-Jul91	−0.0214	−0.0194	−0.0021	0.0010	−0.0093	−0.0066
	(0.0306)	(0.0341)	(0.0149)	(0.0155)	(0.0120)	(0.0123)
Standard Controls	yes	yes	yes	yes	yes	yes
Other Weekday Controls	no	yes	no	yes	no	yes
R^2	0.15	0.20	0.27	0.36	0.29	0.38
N	142	142	142	142	142	142
Panel C: Tutsi Population						
# Sat(Rainfall>10 mm), Oct90-Jul91	0.0221	−0.0405	−0.0272	−0.0192	−0.0168	−0.0059
	(0.0459)	(0.0577)	(0.0173)	(0.0156)	(0.0123)	(0.0145)
Standard Controls	yes	yes	yes	yes	yes	yes
Other Weekday Controls	no	yes	no	yes	no	yes
R^2	0.05	0.16	0.16	0.23	0.22	0.28
N	137	137	137	137	137	137

Notes: All regression in this Table are run at the commune level. **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the period October 1990 to July 1991. **Standard Controls** include Hutu population, average daily rainfall for January 1984 to September 1990 and average daily rainfall for October 1990 to July 1991. **Other Weekday Controls** include the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm during the period October 1990 to July 1991. All regressions are run using weighted least squares (WLS) estimation with Hutu population size as weights. **Standard errors** are clustered at the province level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.14: Alternative Mechanism: Wealth Measure (Census) II

Dependent Variable:	Wealth Index		Wealth Below Median		Poorest Quintile	
	(1)	(2)	(3)	(4)	(5)	(6)
# Sat(Rainfall>10 mm), Feb88-Jul91	−0.0317	−0.0219	0.0018	−0.0003	−0.0060	−0.0077
	(0.0244)	(0.0177)	(0.0115)	(0.0108)	(0.0094)	(0.0093)
Standard Controls	yes	yes	yes	yes	yes	yes
Other Weekday Controls	no	yes	no	yes	no	yes
R^2	0.16	0.21	0.29	0.37	0.32	0.38
N	142	142	142	142	142	142
Panel B: Hutu Population						
# Sat(Rainfall>10 mm), Feb88-Jul91	−0.0302	−0.0206	0.0017	−0.0006	−0.0058	−0.0076
	(0.0233)	(0.0169)	(0.0111)	(0.0107)	(0.0092)	(0.0094)
Standard Controls	yes	yes	yes	yes	yes	yes
Other Weekday Controls	no	yes	no	yes	no	yes
R^2	0.18	0.23	0.31	0.38	0.32	0.37
N	142	142	142	142	142	142
Panel C: Tutsi Population						
# Sat(Rainfall>10 mm), Feb88-Jul91	0.0609	0.0662	−0.0169	−0.0161	−0.0156	−0.0148
	(0.0440)	(0.0416)	(0.0127)	(0.0119)	(0.0110)	(0.0089)
Standard Controls	yes	yes	yes	yes	yes	yes
Other Weekday Controls	no	yes	no	yes	no	yes
R^2	0.09	0.12	0.19	0.25	0.26	0.35
N	137	137	137	137	137	137

Notes: All regression in this Table are run at the commune level. **# Sat(Rainfall>10 mm)** is the number of Saturdays with rainfall above 10 mm during the 3.5-year period February 1988 to July 1991. **Standard Controls** include Hutu population, average daily rainfall for January 1984 to September 1990 and average daily rainfall for February 1988 to July 1991. **Other Weekday Controls** include the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm during the period February 1988 to July 1991. All regressions are run using weighted least squares (WLS) estimation with Hutu population size as weights. **Standard errors** are clustered at the province level. *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

Table A.15: Alternative Mechanism: Infrastructure Destruction

Dependent Variable:	Civilian Participation Rate (%)		
	(1)	(2)	(3)
# Sat(Rainfall>10mm), Oct90–Mar91	−0.076	−0.075	−0.076
	(0.223)	(0.232)	(0.232)
# Sat(Rainfall>10mm), Apr91–Sep91	0.037	0.036	0.035
	(0.549)	(0.543)	(0.541)
# Sat(Rainfall>10mm), Oct91–Mar92	−0.398	−0.382	−0.380
	(0.231)*	(0.227)*	(0.227)*
# Sat(Rainfall>10mm), Apr92–Sep92	0.427	0.408	0.412
	(0.358)	(0.362)	(0.367)
# Sat(Rainfall>10mm), Oct92–Mar93	−0.132	−0.127	−0.128
	(0.166)	(0.170)	(0.172)
# Sat(Rainfall>10mm), Apr93–Sep93	−0.357	−0.337	−0.342
	(0.342)	(0.336)	(0.332)
# Sat(Rainfall>10mm), Oct93	−1.084	−1.068	−1.068
	(0.369)***	(0.372)***	(0.372)***
# Sat(Rainfall>10mm), Nov93	−1.201	−1.227	−1.233
	(0.545)**	(0.546)**	(0.549)**
# Sat(Rainfall>10mm), Jan94	−0.243	−0.241	−0.238
	(0.365)	(0.363)	(0.368)
# Sat(Rainfall>10mm), Feb94	−1.547	−1.581	−1.571
	(0.706)**	(0.738)**	(0.728)**
# Sat(Rainfall>10mm), Mar94	−0.623	−0.641	−0.652
	(0.396)	(0.392)	(0.398)
# Sun-Fri(Rainfall>10mm), Mar94		−0.118	−0.117
		(0.224)	(0.226)
# Sat-Fri(Rainfall>10mm), Genocide Period			0.031
			(0.221)
Standard Controls	yes	yes	yes
Other Weekday Controls	yes	yes	yes
Commune Effects	yes	yes	yes
R^2	0.54	0.54	0.54
N	1433	1433	1433

Notes: **# Sat(Rainfall>10 mm), OctXX–MarXX** is the number of Saturdays with rainfall above 10 mm during October 199X to March 199X+1, for all years 1990–1993. **# Sat(Rainfall>10 mm), AprXX–SepXX** is the number of Saturdays with rainfall above 10 mm in Apr 19XX–Sep 19XX, for all years 1991–1994. **# Sat(Rainfall>10 mm), Oct93** is the number of Saturdays with rainfall above 10 mm in October 1993, and similarly for all other months till March 1994. **# Sun-Fri(Rainfall>10 mm), Mar94** is the sum of all weekdays (except Saturday) with rainfall above 10 mm in March 1994. **# Sat-Fri(Rainfall>10mm), Genocide Period** is the sum of all weekdays with rainfall above 10 mm during the genocide period from April to July 1994. **Civilian Participation Rate** is the number of civilian perpetrators divided by the sector Hutu population. **Standard Controls** include the log of sector population, average daily rainfall for Jan 1984–Sep 1990 and average daily rainfall for Oct 1990–Mar 1994. **Other Weekday Controls** include the number of Sun/Mon/Tue/Wed/Thu/Fri with rainfall above 10 mm during Oct 1990–Mar 1994 (note in regressions 2 and 3 we subtract rainfall during March 1994). All regressions are run using WLS estimation with Hutu population size as weights. There are **142 communes** in the sample. **Standard errors** are clustered at the commune level. *p<0.1, **p<0.05, ***p<0.01.