

PROPAGANDA AND CONFLICT: EVIDENCE FROM THE RWANDAN GENOCIDE*

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This article investigates the role of mass media in times of conflict and state-sponsored mass violence against civilians. We use a unique village-level data set from the Rwandan genocide to estimate the impact of a popular radio station that encouraged violence against the Tutsi minority population. The results show that the broadcasts had a significant effect on participation in killings by both militia groups and ordinary civilians. An estimated 51,000 perpetrators, or approximately 10% of the overall violence, can be attributed to the station. The broadcasts increased militia violence not only directly by influencing behavior in villages with radio reception but also indirectly by increasing participation in neighboring villages. In fact, spillovers are estimated to have caused more militia violence than the direct effects. Thus, the article provides evidence that mass media can affect participation in violence directly due to exposure and indirectly due to social interactions. *JEL Codes:* D7, N4.

I. INTRODUCTION

Since 1945 as many as 22 million noncombatants have been killed in nearly 50 genocides and politicides (Harff 2003). These are political mass killings that are typically sponsored or initiated by elites in control of the government, where those elites have agendas to reduce or eliminate certain groups (ethnic or religious) that are thought to constitute political threats.¹ The tremendous costs of political mass killings in terms of human life warrants a full investigation of how to prevent them; isolating the mechanisms that enable elites to carry them out is key to a fuller understanding.

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1. Political mass killings, that is, genocides and politicides, are considered to be distinctly different phenomena from civil war and revolutions, primarily because of the intent of state authorities to destroy certain groups in society, but also because the violence is large scale and one-sided. That said, multiple definitions exist (e.g., Harff and Gurr 1988; Krain 1997; Harff 2003).

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Joseph Goebbels, Hitler's propaganda minister, called radio "the most important instrument of mass influence that exists anywhere" (Welch 1993). Elites in control of autocratic states have repeatedly used mass media—often under their direct control—with the intention to induce citizen support of and participation in violence against certain groups (Lee 1945; Lasswell 1971). Cross-country evidence indicates that when persecution of certain groups in society is made the official ideology of the elite in power, the likelihood of a conflict transitioning into political mass killings is significantly higher (Harff 2003). Yet it is an open question whether and how propaganda that explicitly encourages violence against a certain group can, in fact, directly induce violence against that group.

This article investigates the role of mass media in the spread of violence by estimating the effects of propaganda disseminated via radio during the 1994 Rwandan genocide. This planned campaign was led by key ethnic Hutu members of the government against the Tutsi ethnic minority. In addition to violence by the military, attacks and massacres conducted by local militia groups and ordinary civilians contributed to a death toll of 0.5–1.0 million deaths (Des Forges 1999; Straus 2004; Verwimp 2006). In a country with low newspaper circulation and few television sets, radio was the dominant medium for the government to deliver messages to the population. The radio station Radio Télévision Libre des Mille Collines (RTLM) led the propaganda efforts by broadcasting inflammatory messages calling for the extermination of the Tutsi minority. Although qualitative evidence suggests this "hate radio" station catalyzed violence (Hatzfeld 2005; Straus 2007), and its cofounders were found guilty of instigating genocide by the International Criminal Tribunal for Rwanda, there is no quantitative evidence establishing whether, how, and to what extent the broadcasts caused more violence.

We hypothesize that mass media could have fueled participation in the violence via two broad mechanisms. First, in line with the literature on persuasive communication, the broadcasts could have had a direct persuasion effect by convincing some listeners that participation in the attacks on Tutsis was preferable to nonparticipation.² This mechanism is plausible given that the

2. For an overview of the persuasion literature, see DellaVigna and Gentzkow (2010); see Glaeser (2005) for a political economy model of propaganda and hatred toward minority groups.

broadcasts contained not only strong anti-Tutsi rhetoric that may have increased pro-violence preferences, but also information about relevant trade-offs: they made it clear that the government would not punish participation in the killing of Tutsi citizens or the appropriation of their property, but instead encouraged or even mandated such behavior. Second, following a long tradition in the social sciences on the role of social interactions in general, and their importance in intermediating mass media effects in particular, a direct persuasion effect could influence the spatial diffusion of violence, even beyond the immediate areas of radio reception. One would expect this to be the case if violence begets violence, leading to contagion, or if information and beliefs spread via social networks.³ Put simply, the broadcasts may have affected overall violence via local spillover effects, in addition to direct effects from exposure.

We build a unique village-level data set from Rwanda to examine these hypotheses. We use information on RTLTM transmitters and radio propagation software to produce a data set on radio coverage at a high spatial resolution, allowing us to calculate the area with reception within each village. To identify causal effects, our empirical strategy exploits variation in radio reception generated by Rwanda's highly varying topography, which is practically random and, therefore, arguably uncorrelated with other determinants of violence.⁴ To measure participation in the violence, we use data on the number of persons prosecuted for violent crimes committed during the genocide in each village. The prosecution data contain two distinct legal categories of crime: the first for members and accomplices of organized forms of violence, primarily from local militias (77,000 persons in total), the second for less organized individual violence carried out by perpetrators who are not members or accomplices of any of the

3. The hypothesis that social interactions provide an intermediate channel for persuasion effects on behavior dates back to Lazarsfeld et al. (1944) and Katz and Lazarsfeld (1955). Formal models of social interactions under complementarities in violence production go back to at least Granovetter (1978), and early information-based models of herd behavior and contagion include Banerjee (1992) and Bikchandani, Hirshleifer and Welch (1992); see Chamley (2004) for an overview. Early theoretical work on the diffusion of crime include Sah (1991), and on the empirical side papers by Case and Katz (1991) and Glaeser, Sacerdote, and Scheinkman (1996). For overviews of the social interactions literature, see Manski (2000), Durlauf (2004), and Jackson and Yariv (2011).

4. Olken (2009) was the first to use a similar, but not identical, strategy to identify media effects.

organized groups in the first category (432,000 persons in total). For simplicity, hereafter we refer to the first category as *militia violence*, the second category as *individual violence*, and the sum of the two as *total violence*.

The results show that the broadcasts led to more violence during the genocide. First, there is a direct effect on participation, with violence increasing in radio coverage in the village. A 1 standard deviation increase in radio coverage is associated with a 12–13% increase in participation in total violence. The effect is similar for militia violence (13–14%) and individual violence (10–11%). A battery of robustness tests show that the effects are unlikely to be spurious due to omitted variables, outliers, or measurement error in violence. Moreover, placebo tests show that another radio station that did not broadcast propaganda instigating genocide had no effects on violence, indicating that radio reception irrespective of content did not influence participation. We also present suggestive evidence that the RTLMM broadcasts were most effective in inducing violence in villages where the population was relatively uneducated and illiterate and where Tutsis made up a relatively small minority.

Second, we find evidence that the broadcasts exhibited positive spillover effects in militia violence. The number of persons engaged in militia violence in a given village was significantly higher when a larger share of the population in neighboring villages had radio coverage, consistent with the hypothesis that social interactions determine the spatial diffusion of violence. There are no spillover effects on individual violence, suggesting that local complementarities or information diffusion among ordinary citizens were weak or nonexistent, at least relative to that among members of organized militias.

Third, we use the regression estimates on the direct effects and indirect effects and perform a simple counterfactual calculation of the countrywide effects on participation, enabling us to also quantify the relative importance of the spillovers. This analysis suggests that 10% of the total participation in the genocide, or approximately 51,000 prosecuted persons, was caused by the radio station. Spillovers had a greater overall effect on militia violence (16,000 additional persons) than did the direct effects (6,000 additional persons). This is consistent with existing qualitative evidence from perpetrator interviews by Hatzfeld (2005) and Straus (2007), which suggest that the broadcasts persuaded a limited number of key agents of the local elite in villages, and

these agents in turn recruited individuals in neighboring villages by engaging in face-to-face mobilization.

The empirical literature on the determinants of conflict has mostly focused on two-sided violence, such as civil war (for a survey, see Blattman and Miguel 2010), with less attention paid to one-sided repression and mass killings (Besley and Persson 2011; Esteban et al. 2014). The standard theoretical approach in this literature is to analyze equilibrium behavior at the group level, assuming away within-group determinants of participation. This article contributes to this literature by giving evidence that mass media played a role in mobilizing individuals within the Hutu ethnic group during the Rwandan genocide—evidence consistent with broadcasts influencing overall violence directly through exposure and indirectly via social interactions. It also contributes to the large literature on collective violence and ethnic riots (Horowitz 2001; Tilly 2003; Varshney 2003), the spatial contagion of civil conflict (Buhaug and Gleditsch 2008; Braithwaite 2010; Black 2013), and the causes of the Rwandan genocide (Andre and Platteau 1998; Verwimp 2005, 2006; Verpoorten 2005). In the media effects literature, the article is most closely related to previous evidence showing effects on ethnic animosity (DellaVigna et al. 2011), political knowledge and beliefs (Gentzkow and Shapiro 2004; Snyder and Stromberg 2010), voting behavior (Gentzkow 2006; DellaVigna and Kaplan 2007; Gerber et al. 2009; Chiang and Knight 2011; Enikolopov et al. 2011), and social capital (Olken 2009; Paluck 2009).

The rest of the article is structured as follows. Section II provides background information on the genocide and mass media in Rwanda, Section III presents a conceptual framework and the main hypotheses, Section IV presents the data, Section V explains the empirical strategy, Section VI presents the results and robustness tests, and Section VII concludes. All appendix material is available in the Online Appendix.

II. BACKGROUND

This section provides a brief historical background of preexisting political tensions in the period leading up to the genocide, as well as the structure and content of RTLm broadcasts.

There are two large ethnic groups in Rwanda: the Hutu majority, and the Tutsi minority (the latter constituting approximately 10% of the population in 1991). Historically, the Tutsi minority had been the ruling elite; however, when the country gained independence from Belgium in 1962, Rwanda became a Hutu-dominated one-party state. Following independence, several episodes of violence between the ethnic groups led to hundreds of thousands of ethnic Tutsis fleeing to neighboring countries (Prunier 1995). A period of relative stability followed, but in 1973 violent conflict resumed as ethnic clashes between Hutus and Tutsis in Burundi spilled over the border into Rwanda.

In October 1990, a Tutsi-led rebel army invaded northern Rwanda from Uganda. The rebels, who called themselves the Rwandan Patriotic Front (RPF), represented Tutsi refugees who had fled during earlier clashes and demanded an end to the ethnically unbalanced policies Rwanda had been practicing.⁵ After a period of negotiations and unrest, the Hutu president Habyarimana and RPF leaders signed a peace agreement in Arusha, Tanzania, in August 1993. With scarce resources and a weak mandate, the United Nations peacekeeping forces were dispatched to facilitate the installation of a transitional government. After bouts of violence, unrest, and delays in the implementation of transitional measures, President Habyarimana was assassinated when his jet was shot down in early April 1994. Within days, extremist factions within Hutu-dominated political parties launched a coup and managed to take over the government. An ethnic cleansing campaign spread throughout the country shortly thereafter.

Violence was highly local; perpetrators and victims often lived in the same village. Broadly speaking, violence occurred in two forms. One was of a highly organized and coordinated kind, in which essentially all branches of government took an active role (Prunier 1995). The bulk of the killing was done by two state-sponsored militias and their paramilitary wings, a coalition that became known as Hutu Power. Militias were organized throughout the country, down to the village level, and members would erect roadblocks, distribute weapons, and

5. The rebel army, numbering about 4,000 well-trained troops, mainly consisted of second-generation Rwandan refugees. They had gained military experience by serving in Uganda's National Resistance Army, which seized power in 1986.

systematically plan and carry out massacres of Tutsis. In addition to the militia violence, there was also large-scale participation by civilians staging attacks that were much less organized and used primitive weapons, mainly machetes and clubs (Straus 2004; Verwimp 2005). That is not to say violence by civilians was independent of that by militias; there are plenty of anecdotes where militia members and civilians attacked in tandem. A study by Straus (2004) found that violence tended to be committed in groups, often of considerable size (with an average size of 116 persons), and that although militias and other armed groups made up a small minority of the perpetrators, they most likely did the lion's share of the killing.

The genocide ended in late July 1994 when the Tutsi RPF rebels defeated the Rwandan army and militia groups. By that point, at least 500,000 Tutsis had been killed, as well as significant number of moderate Hutus. For discussions on the death tolls, see Des Forges (1999) and Verpoorten (2005).

II.A. Media and RTLM

Prior to the start of the genocide, Rwanda had two national radio stations, RTLM and Radio Rwanda. RTLM began broadcasting in July 1993 and quickly became the most popular station in the country (Li 2004). Although the government-owned Radio Rwanda had been broadcasting some anti-Tutsi propaganda before the genocide, RTLM provided the most extreme and inflammatory messages. Alternative print media did exist. The number of independent newspapers at the time of the genocide was between 30 and 60 and included publications from across the political spectrum (Alexis and Mpambara 2003; Higiyo 2007). However, the circulation and readership of these newspapers in rural areas was limited due to relatively low literacy rates. Consequently, radio was the sole source of news for most people (Des Forges 1999).

RTLM was set up by members of Hutu Power, and until his assassination, President Habyarimana had been one its strongest backers (Des Forges 2007). Ferdinand Nahimana, who had previously been the director of the agency responsible for regulating mass media, helped found RTLM and played an active role in determining the content of broadcasts, writing editorials, and giving journalists scripts to read (International Criminal Tribunal for Rwanda [ICTR] 2003). Thus, a connection between

the station and top government officials had evidently been established even before April 6, 1994. After that date, when key members of Hutu Power took over, the station essentially became the voice of the new government. The broadcasts continued throughout the genocide and did not abate until RPF rebels seized power in mid-July 1994.

The radio station called for the extermination of the Tutsi ethnic group and claimed that preemptive violence against it was a response necessary for “self-defense” (ICTR 2003; Frohardt and Temin 2007). In her content analysis of taped RTLM broadcasts, Kimani (2007) reports that the most common inflammatory statements consisted of reports of Tutsi RPF rebel atrocities, allegations that Tutsis in the region were involved in the war or a conspiracy, and allegations that the RPF wanted power and control over the Hutus. Key government officials spoke on air, including Prime Minister Jean Kambanda. The language used in broadcasts was dehumanizing, as Tutsis would often be referred to as *inyenzi* (cockroaches). After the April 1994 coup, messages from the radio station made it clear that the new government had no intention of protecting the Tutsi minority from attacks and that Hutus engaged in killings would not be held accountable. Instead, the propagated message was that the radio station as well as government officials encouraged the killing of Tutsis.

The content of Radio Rwanda was substantially different from RTLM. Listeners viewed RTLM as a credible news provider but were suspicious of Radio Rwanda. The station had provided little information on the progress of the ongoing war with the RPF in the pregenocide years and was largely inactive in the opening weeks of the genocide, paralyzed by internal power struggles (Li 2004). For example, in the morning after the president was killed, Radio Rwanda apparently only played classical music while RTLM gave news about the situation. Furthermore, Radio Rwanda was generally not nearly as extreme in spreading anti-Tutsi messages as RTLM. The ICTR did not prosecute any of the key individuals associated with Radio Rwanda.

In their verdict against RTLM's founders, the ICTR stated that violence by militia groups had been affected by the broadcasts: “The Interahamwe and other militia listened to RTLM and acted on the information that was broadcast by RTLM. RTLM actively encouraged them to kill, relentlessly sending the message that the Tutsi were the enemy and had to be eliminated once

and for all" (ICTR 2003). Furthermore, Straus (2007) provides qualitative evidence based on interviews with perpetrators indicating that RTLM "instigated a limited number of acts of violence, catalyzed some key actors, coordinated elites, and bolstered local messages of violence." Beyond qualitative and anecdotal evidence, however, it is still unclear how much of the violence was, in fact, caused by these broadcasts.

III. CONCEPTUAL FRAMEWORK AND HYPOTHESES

It is clear that a key aim of the RTLM broadcasts was to persuade Hutus to engage in violence against Tutsis. But how do such broadcasts affect behavior and translate into more violence? We present a framework outlining potential channels and hypotheses.

III.A. *Direct Effects*

The theory literature on persuasive communication suggests that mass media exposure can alter behavior by affecting beliefs and/or preferences (for an overview, see DellaVigna and Gentzkow 2010). In belief-based models, receivers typically (but not always) are treated as rational agents applying Bayes's rule when processing information that is credible and relevant for decision making, such as information about the expected costs or benefits of alternative decisions. Applied in the present context, a number of factors would lead one to believe that rational agents would respond to the content of RTLM. Because it was clear that the RTLM broadcasts were endorsed by the government and the armed forces, they arguably carried some credibility and signaled the official policy and agenda of the elite in power, conveying that the government and the armed forces were actively persecuting Tutsis, that civilian participation was strongly encouraged, and that *de facto* property rights and human rights of Tutsis were nonexistent. This is relevant, as there is plenty of qualitative evidence in Hatzfeld (2005) that the individuals were motivated not only by ethnic grievances but also the possibility of looting of Tutsis' assets.⁶ Moreover,

6. For example, one perpetrator explained: "Killing could certainly be thirsty work, draining and often disgusting. Still, it was more productive than raising crops, especially for someone with a meager plot of land or barren soil. During the killings anyone with strong arms brought home as much as a merchant of

and very importantly, RTLTM effectively disseminated the message “kill or be killed,” which referred to the notion of self-defense against an upcoming Tutsi takeover, and also operated as a threat to Hutu citizens who refused to participate in the killings, as they would be considered accomplices to “the enemy” (Des Forges 1999; Kimani 2007). In fact, this threat was credible and the consequences of disobedience were real: thousands of so-called moderate Hutus who opposed the new agenda were killed by militias and the army. Taken together, these factors make it abundantly clear that Hutus listening to RTLTM broadcasts had good reason to fundamentally revise their beliefs about the cost-benefit trade-off of participation and nonparticipation, and ultimately these beliefs could have affected their behavior. Populations in villages without access to RTLTM broadcasts would have been less likely to receive this information.

Moreover, another class of models—preference-based ones—draw on insights from psychology and suggest that noninformative content also may affect behavior, even if agents are not fully rational. People may intrinsically value the act of participating itself. The appeal to emotions and the fostering of hate through methods such as the use of dehumanizing language, describing Tutsis as cockroaches, could thus play an independent or complementary role by influencing intrinsic motivation for violence.

Although RTLTM broadcasts could have increased violence, the existence of such an effect is far from obvious. Participation in violence is potentially very costly, whether in terms of material opportunity costs, physical risks, or intrinsic aversion against harming neighbors and fellow citizens. In the presence of such counteracting forces, the empirical question is then whether RTLTM was sufficiently persuasive to induce greater violence in those villages that were exposed to the broadcasts. This is the primary hypothesis to be tested in this article.

III.B. Social Interactions and Spillover Effects

The framework above describes atomistic behavior among individuals; it is silent on the role of social interactions.

quality. We could no longer count the panels of sheet metal we were piling up. The taxmen ignored us.” Another killer similarly described that “At bottom, we didn’t care about what we accomplished in the marshes, only about what was important to us for our comfort: the stock of sheet metal, the rounded-up cows, the piles of windows and other such goods.”

However, dating back to empirical work by Katz and Lazarsfeld (1955), the political communication literature has emphasized the role of social interactions in general, and the importance of local opinion leaders in particular, in intermediating the effects of mass media. Moreover, ethnic violence is often not atomistic but collective, especially during a genocide. The most obvious case of nonatomistic violence during the Rwandan genocide would be that conducted by militia groups, since it is collective essentially by definition.⁷ Although there were many cases of homicides during the Rwandan genocide where individual citizens killed neighbors and looted property without much assistance, one could argue that some of these acts were triggered precisely because they were so common. In any case, there are multiple plausible ways in which social interactions in theory would matter for how RTL's propaganda translated into more violence among both militias and less organized individuals.⁸

Specifically, when there are social interactions in the violence-production process, decisions and behavior among peers are not disjointed but inherently linked. In this case, exposure to mass media can lead to important spillover effects via peer group influences. Following Durflauf's (2004) review article, such effects can arise via at least three general mechanisms: (i) interdependencies in the constraints that individuals face, so that the costs of a given behavior depend on whether others do the same; (ii) psychological factors, an intrinsic desire to behave like certain others; or (iii) interdependencies in information transmission, so that the behavior of others alters the information on the effects of such behaviors available to a given individual. It is easy

7. Tilly (2003) defines "collective violence" when three criteria of social interactions are fulfilled: they (i) inflict physical damage on persons and/or objects; (ii) involve at least two perpetrators of damage; and (iii) result at least in part from coordination among persons who perform the damaging acts. By this definition, violence by Rwandan militias can be characterized as collective, but arguably also violence by ordinary citizens, as truly independent nongroup violence was very rare (Straus 2004).

8. The standard approach in formal models of conflict, such as the contest success model, is to analyze behavior at the group level and assume away within-group social interactions, including coordination and collective action problems. See Blattman and Miguel (2010) for a review of the literature. The present conceptual framework is stylized in that it restricts the analysis to participation in one-sided mass killings, ignoring strategic behavior among the minority members under attack. This simplification is motivated by the empirical context: there were essentially no coordinated defense efforts among ethnic Tutsis in Rwanda.

to see how such mechanisms could be relevant in the case of the Rwandan genocide. Consider the individual decision to join a militia group that attacks Tutsis, where the relevant peer group is other Hutus in the village as well as in some nearby villages. If the marginal net benefit of joining the militia attacks is increasing in the number of others in the area who join, because of safety in numbers, a desire to conform, or other underlying motives giving rise to strategic complementarities, one would expect positive spillover effects. Put simply, violence may beget violence.⁹ Also, social interactions that influence the diffusion of information and beliefs could lead behavior among non-listeners to be affected, such that levels of violence in a village depend on whether a nearby village has reception.¹⁰ The simplest case would be if the information contained in the broadcast simply travels via word of mouth and persuades nonlisteners to join in the violence; observing some participate in the violence and loot property without being punished by authorities may also lead observers to revise beliefs about the costs of participation, triggering herd behavior. In this case we would, again, expect positive spillover effects.

That said, positive spillovers are not obvious because in theory peer influences may have the opposite effect. If there are strategic substitutes in violence, then a direct persuasion effect that initially makes some listeners take part in the attacks may consequently deter others. Similarly, if participation in massacres is equivalent to voluntary contribution toward a local public good—the elimination of the ethnic minority—then negative spillovers could exist due to free-rider incentives and collective action problems in local violence production.

9. See the Online Appendix for a static model under strategic complementarities where propaganda has a direct persuasion effect and an indirect effect by effectively facilitating coordination. An extension that allows for strategic substitutes is available upon request from the author. The simple model shows that under strategic complementarities there can be increasing returns to scale of propaganda, or a positive “social multiplier” (Glaeser et al. 2003), while under strategic substitutes propaganda exhibits decreasing returns to scale.

10. It is well known that in the absence of experimental variation, it is very difficult to identify which specific mechanism is the underlying driver, and this is beyond the scope of this article. For a structural approach that distinguishes information passing among neighbors from direct influence of neighbors’ participation decisions within the context of the diffusion of microfinance, see Banerjee et al. (2013).

A priori, these positive or negative spillover effects arising from social interactions are all plausible. Ultimately it is an empirical question whether spillovers are important, in which direction they go, and for group versus individual violence. Moreover, from an econometric standpoint it is crucially important to take any spillover effects into account to properly estimate the magnitude of the overall power of RTLM's propaganda—how much of the nationwide violence that can be attributed to the radio station—since ignoring spillovers can easily lead to underestimation or overestimation. Therefore, the existence of spillover effects is a secondary hypothesis to be tested in this article.

The data are available at the village level, and spatial spillovers will therefore be estimated across villages. Additional tests to further shed light on the conditions making the propaganda more or less effective are presented in Section VI.D.

IV. DATA

IV.A. Violence

To measure participation in the violence, we use a nationwide village-level data set on persons prosecuted for violent crimes committed during the Rwandan genocide. The data are taken from the government agency National Service of Gacaca Jurisdictions. The prosecution data for each village comes from local so-called Gacaca courts. This court system was set up in 2001 to process the hundreds of thousands of individuals accused of crimes committed during the genocide.

The data contain two categories of violent crimes. Category 1 includes prosecutions for those accused of having carried out more organized and coordinated attacks, legally defined as planners, organizers, instigators, supervisors of the genocide; and leaders at the national, provincial, or district level, within political parties, the army, religious denominations, and militia. At the village level, this category typically implies crimes committed by local militia members, such as the Interahamwe and Impuzamugambi. For simplicity, these crimes are henceforth referred to as militia violence. In total, approximately 77,000 persons were prosecuted under this category. Category 2 prosecutions concern acts of individual violence committed by ordinary citizens who were not members or accomplices of militia, the army, or other groups that carried out attacks. They are legally

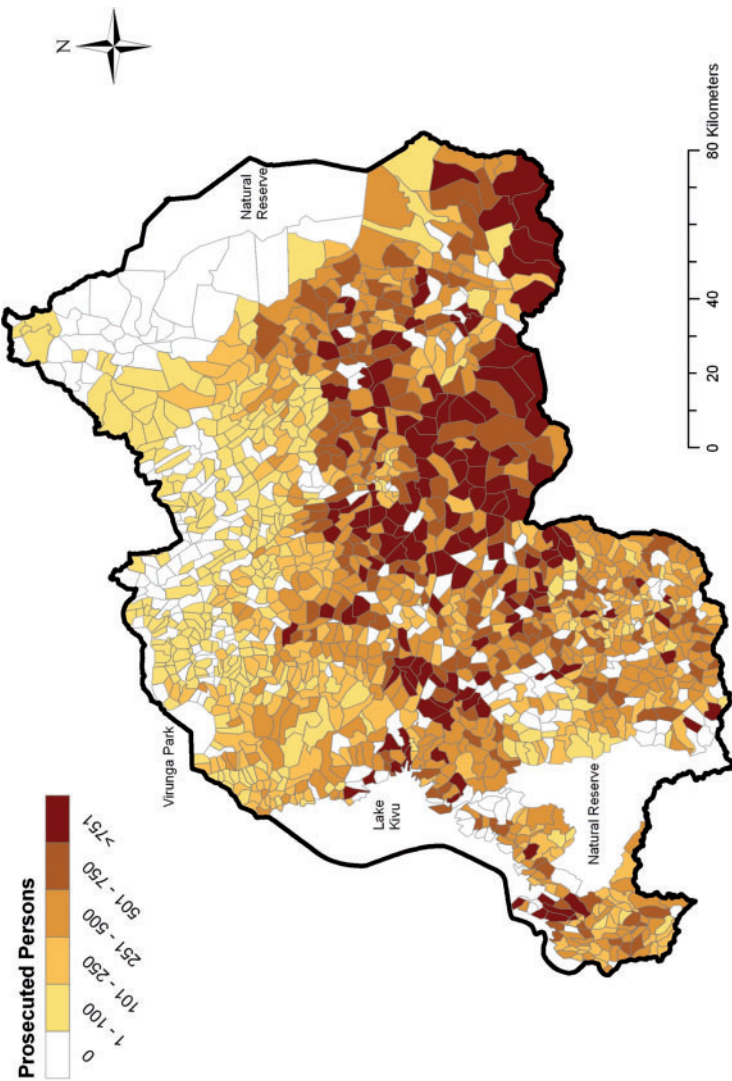


FIGURE I
Genocide Violence in Villages

The categories represent the total number of prosecuted persons in the village (sum of militia and individual violence). White areas are missing data, either because of geography, such as parks and natural reserves, or villages that lack data in the sample.

defined as authors, coauthors, and accomplices of deliberate homicides or of serious attacks that caused someone's death; persons who—with the intention of killing—caused injuries or committed other serious acts of violence without actually causing death; and persons who committed criminal acts or became accomplices of serious attacks without the intention of causing death. Since this category captures unorganized violence by individuals who did not belong to militias or other armed groups, henceforth these crimes are referred to as individual violence. In total, 433,000 persons were prosecuted under this category. Figure I shows a map with total prosecutions across villages.

Henceforth, the number of participants and the number of those prosecuted are used interchangeably. However, because we do not observe actual participation but a proxy, some measurement error is likely. That is, in some villages more individuals were prosecuted relative to the number of individuals who actually committed a given crime, and vice versa. We discuss the possibility of biased estimates due to measurement error and present robustness tests using an alternative proxy for violence based on household survey data.

IV.B. *RTL M Reception*

RTL M had two transmitters. The main transmitter (1000 watts) was located on Mount Muhe, one of the country's highest mountains, and another transmitter (100 watts) in the capital, Kigali. We construct a variable measuring predicted radio coverage of RTL M at the village level. The variable is constructed in several steps. First, it uses data on RTL M transmitter locations and technical specifications, provided by the government agency Rwanda Bureau of Information and Broadcasting (ORINFOR).¹¹ Our data predict radio coverage across the country using digital topographic maps and radio propagation software in ArcGIS. The software uses an extension called Communication System Planning Tool (CSPT) that implements an algorithm called ITM (irregular terrain model)/Longley-Rice, typically used by radio and TV engineers to assess the signal strength of

11. The transmitter specifications include latitude, longitude, altitude of antenna base, antenna height, transmission power, frequency, and polarization. For the Mount Muhe antenna, ORINFOR did not provide data on its exact GPS position. However, since the height above sea level for the antenna was provided, its position on the mountain was possible to pin down with high precision.

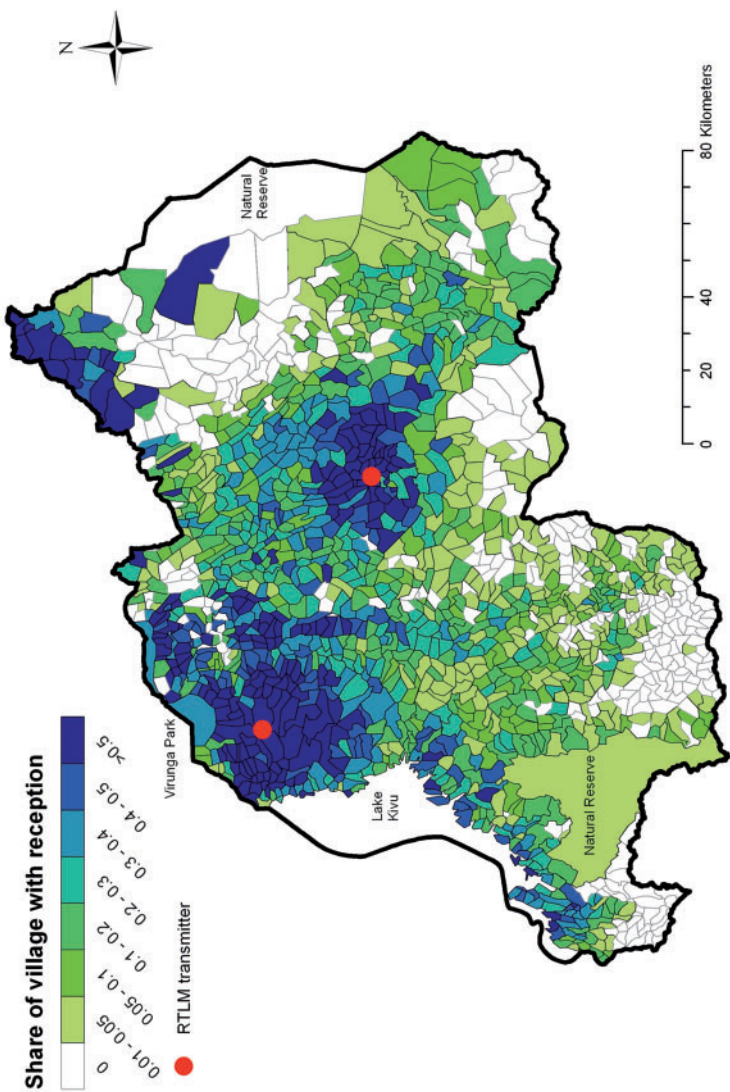


FIGURE II
RTLM Radio Coverage

The figure shows the radio coverage in villages (share of village area with sufficient radio reception) based on the Longley-Rice propagation model. *Source:* Author's calculations in ArcGIS using the Longley-Rice propagation model.

broadcasts.¹² The topographic data were provided by the Shuttle Radar Topography Mission (SRTM). Due to a high resolution of the topographic data, the software can predict radio coverage with high precision. Signal strength is predicted at a 90×90 meter cell resolution, producing a digital map indicating whether each cell has sufficient signal strength for listening using a normal receiver.¹³

Using the digitized map of village boundaries, we can calculate the share of a given village that had RTLTM radio coverage. This is the main independent variable. Figure II shows a map of the radio coverage variable. As the measure uses predicted rather than actual radio coverage, there could be some random measurement errors in the data (although this is unlikely to be significant, given the 90-meter resolution of the topographic data). In that case, one could observe an attenuation bias and an underestimation of the direct effects. We discuss in measurement error in detail in Section VI.¹⁴ As there is no available data set on RTLTM listening rates, the article estimates the reduced-form effect of radio coverage on participation in the violence.¹⁵

IV.C. *Additional Data*

Population and ethnic data were retrieved from the Rwanda 1991 population census provided by IPUMS International and GenoDynamics. The GenoDynamics data are used for the population in each village, but it does not contain any data on ethnicity. The data was matched to village names within communes. Unfortunately, the matching is imperfect, as many villages either have different names in different data sources or use alternate spellings. It is also not uncommon for two or more villages within

12. The author is grateful to Robert DeBolt at the Institute for Telecommunication Sciences for providing the CSPT extension.

13. The reception is measured in field strength dBuV/m, where the signal is deemed as sufficient under normal circumstances if it is at least 50 dBuV/m. This threshold is set by default by the software.

14. The propagation model creates missing data problems for a small section of villages near the border in the north and northeast of the country. Since the predicted radio signal was incorrect for those villages, they were dropped from the sample. This is unlikely to affect the estimations and conclusions, as only a small fraction of the violence (1.9% of all prosecutions) took place in these villages. In fact, all the main results are robust to the inclusion of these villages.

15. According to the 1991 census, the average radio ownership rate in communes in the sample is 34%. Radio ownership data are not available at the village level.

TABLE I
SUMMARY STATISTICS

	Obs.	Mean	Std. dev.
Dependent variables			
Prosecuted persons, total violence	1065	388.5	329.4
Prosecuted persons, militia violence	1065	58.8	72.9
Prosecuted persons, individual violence	1065	329.7	284.6
Share of militia violence, militia/total violence	1045	0.150	0.128
Independent variables			
Radio coverage in village	1065	0.185	0.225
Radio coverage in nearby villages, within 10 km	1065	0.187	0.184
Radio coverage in nearby villages, within 10–20 km	1065	0.197	0.161
Radio coverage in village, Radio Rwanda	1062	0.864	0.170
Mean altitude, km	1065	1.712	0.231
Variance in altitude, m	1065	9074	10401
Distance to transmitter, km	1065	5.194	2.850
Mean distance to major town, km	1065	20.06	12.03
Mean distance to major road, km	1065	5.822	5.283
Mean distance to the border, km	1065	21.79	12.71
North sloping village, dummy	1065	0.239	0.427
East sloping village, dummy	1065	0.246	0.431
West sloping village, dummy	1065	0.262	0.440
South sloping village, dummy	1065	0.253	0.435
Population density in 1991, pop per square km	1065	521.3	875.2
Population in 1991, '000	1065	4.852	2.482
Tutsi minority size, 1991	1065	0.099	0.085
% literate Hutu, 1991	1065	50.30	5.681
% literate Tutsi, 1991	1061	92.17	7.467
% Hutu with primary education, 1991	1065	57.85	6.093
% Tutsis with primary education, 1991	1061	69.26	12.08
Share of Hutu HH with cement floor	1065	0.098	0.551
Share of Tutsi HH with cement floor	1061	0.199	0.162

Notes. All variables are measured at the village level, except Tutsi minority size, primary education, literacy rates, and share of households with cement floor. These data are taken from the 1991 IPUMS census data and are only available at the commune level (128 communes in the sample). Militia violence refers to category 1 prosecutions, and individual violence refers to category 2 (see data section for details). Total violence is the sum of militia and individual violence. Share of militia violence is the number of prosecuted person for militia violence divided by the total number of prosecutions. Radio coverage is the share of the village area that has reception, which refers to RTLTM unless specified as Radio Rwanda. Coverage in nearby villages refers to the population-weighted mean in other villages within a given centroid distance. Altitude, mean is the mean altitude in the village in kilometers. Altitude, variance is the village variance in altitude in meters, distance to transmitter is the distance in km to the nearest RTLTM transmitter. The other distance variables are based on Africover data. Slope dummies refer to the direction of the slope at the village centroid. All distance and slope measures are from the author's calculations in ArcGIS. The % literate Hutu is the percent of Hutu household heads in the commune that are literate. % Hutu with primary education is the percent of Hutu household heads in the commune that have at least some primary education.

a commune to have identical names, which prevents unique identification and successful matching. Hence, the final data set contains 1,065 villages out of the total 1,513 in the country. Because most of these issues are idiosyncratic, the main implication is likely lower precision in the estimates than otherwise would have been the case.

The 1991 census from IPUMS International provide micro data on sociodemographic characteristics allowing us to estimate heterogeneous effects. To test for whether propaganda affects violence differently depending on literacy and primary education, we construct average literacy rates and primary education levels in administrative communes using these data. As an additional control variable for wealth, the fraction of households that have a cement floor (i.e., not a dirt floor) is used as a proxy. The ethnicity of a household is defined by the ethnicity of the household head, allowing us to measure the ethnic minority size in the commune given by the number of Tutsi households per Hutu household.¹⁶

Finally, a set of spatial variables is also included. Using ArcGIS software and the topography data, we calculate the village mean altitude, the village variance in altitude, distance to the border, and population density. Using data from Africover, we also measure the village centroid distance to the nearest major town and the distance to the nearest major road. Summary statistics are presented in Table I.

V. EMPIRICAL STRATEGY

Identifying the causal effects of radio coverage on violence requires variation in radio coverage to be uncorrelated with all other determinants of violence. The main transmitter was placed on Mount Muhe in the northwestern part of the country. This mountain is the second highest in the country. It is also situated in an area where ethnic Hutus constitute a relatively high share of the population, an area that was the political base for key politicians of Hutu Power. The second transmitter was placed in the capital, Kigali. Given where these transmitters were placed and

16. There is no village identifier available in the IPUMS data. The lowest administrative unit identifier available is the commune, which is one level above the village. As all regressions will include commune fixed effects, all commune-level socioeconomic characteristics from the IPUMS dataset will therefore be collinear with the commune dummies. There are 128 communes in the sample.

the political agenda of the station's founders, it seems reasonable to conjecture that placement was primarily driven by the desire to maximize the size of the audience, especially among Hutus who were expected to be persuaded by the broadcasts. The endogeneity concern is that the transmitters were placed in regions more prone to violence against Tutsis for reasons unrelated to the broadcasts.

The following identification strategy addresses the problem in steps. Nicknamed "The Land of the Thousand Hills," Rwanda is a very hilly country without any large, continuously flat regions. A topography map is available in the Online Appendix (Figure A.1). The main strategy of this article is to exploit local variation in radio coverage due to hills lying in the line of sight between radio transmitters and villages.

Radio propagation follows the laws of physics for electromagnetic propagation. Given transmitter height and power, the two main determinants of the signal strength are distance to the transmitter and whether the receiver is in the line of sight of the transmitter. In free space, the power density of the radio signal decreases in the square distance from the transmitter. Since the transmitter may have been placed strategically, the distance to the transmitter is most likely correlated with other determinants of violence.¹⁷ We therefore control for a second-order polynomial in the distance to the transmitter.¹⁸ This will leave variation in signal strength caused by variation in the line of sight between the transmitter and the receiver, which will depend on two factors: the topography of where the receiver is located (the likelihood of reception is increasing in altitude of the receiver) and the topography of the area between the transmitter and the receiver. Since the topography of a village may be correlated with the other unobservable determinants of participation in conflict, we include second-order polynomials in the mean altitude of the village and the altitude variance. This leaves

17. The bias is likely to be negative for at least two reasons. First, radio coverage was better in the northern part of the country. As the Tutsi RPF rebels advanced from the north to stop the genocide, violence against Tutsis was greater in the south. Second, there were fewer Tutsis in the north to begin with, so practically fewer could be attacked and killed.

18. The second-order polynomial in the distance to the transmitter alone explains 44% of the variation in radio coverage. We use second-order polynomials to address the possibility of nonlinear relationships. The results are not sensitive to this specification, however, and simple linear terms give very similar results.

variation in the radio coverage due to the topography between the transmitter and the receiver. Since the two RTLTM transmitters may have been strategically placed in parts of the country with a certain kind of topography, we include commune fixed effects to control for broad regional differences.¹⁹ The variation in radio coverage exploited for identification is thus a highly local variation across villages within communes, arguably uncorrelated with other determinants of conflict, as radio coverage is determined by whether a hilltop randomly happens to be in the line of sight between the transmitter and the village. For additional intuition, Figure A.2. in the Online Appendix graphically shows the variation in four communes.

V.A. *Exogeneity Check*

If the identification strategy is valid, there should be no correlation between the variation in radio coverage and the other determinants of participation in violence. To assess this, we test the validity of the exogeneity assumption by using available observable predetermined village characteristics from different and run the regression

$$(1) \quad y_{ci} = \beta r_{ci} + X'_{ci}\pi + \gamma_c + \varepsilon_{ci},$$

where y_{ci} is a characteristic of village i in commune c ; r_{ci} is the radio coverage of village i in commune c ; X'_{ci} is a vector of village baseline covariates; and γ_c is the commune fixed effects. If the exogeneity assumption is correct, we expect $\beta = 0$.

Table II shows the results. None of the village characteristics are significant, which lends credibility to the identification strategy. In the main regressions, results will be presented both with and without village characteristics. In general, the results are similar with and without the inclusion of these covariates.²⁰

V.B. *Main Specification*

To test whether RTLTM affected participation in the genocide, we estimate the following regression

$$(2) \quad \log(h_{vci}) = \beta_v r_{ci} + X'_{ci}\pi + \gamma_c + \varepsilon_{ci},$$

19. Commune fixed effects alone explain 82% of the variation in village mean altitude and 72% of the variation in radio coverage.

20. Since the IPUMS variables contain commune identifiers but no village identifiers, we cannot test for exogeneity using them as outcomes in Table II.

where h_{vci} is the number of persons prosecuted for violence type v in village i in commune c ; r_{ci} is the RTLTM radio coverage of village i in commune c ; X_{ci} is the vector of village i covariates; and γ_c is the commune fixed effects.²¹ We run separate regressions where h_{vci} is militia violence, individual violence, or total violence. The vectors of baseline covariates to control for radio propagation determinants are latitude, longitude, and second-order polynomials in the distance to the nearest transmitter, the mean altitude in the village, and the variance in altitude within the village. We use second-order polynomials to control for potential nonlinear relationships in distance and altitude. In additional specifications, we also add controls for the slope of the village (north, east, and south dummy variables) at the centroid, and logs of population, population density, distance to the nearest major town, distance to the nearest major road, and distance to the border. Since equation (2) only includes radio coverage of village i , β_v captures the direct (within-village) effect of radio coverage on violence type v . If RTLTM increased participation in the killings, then $\beta_v > 0$.

To account for spatial autocorrelation, we use Conley (1999) standard errors that adjust for spatial dependence.²²

V.C. Spillover Specification

Following the logic outlined in Section III, the broadcasts may have influenced participation in violence through spatial spillovers. We estimate spillovers with the following regression

$$(3) \quad \log(h_{vci}) = \lambda_{vd}\bar{r}_{dci} + \bar{X}'_{dci}\phi_d + \gamma_c + \varepsilon_{ci},$$

where \bar{r}_{dci} is the population-weighted average of radio coverage in *other* villages within distance d from village i in commune c ; and \bar{X}'_{dci} is the population-weighted average of covariates X' of villages within distance d from village i , with distances measured from centroid to centroid. Since \bar{r}_{dci} is population-weighted, it captures the share of the population in neighboring villages

21. Of the 1,065 villages in the sample, 20 had no prosecuted persons. Since the outcome variable is logged, and the log is undefined at zero, we add one prosecution to all observations in the data. The results are robust to dropping the villages with zero prosecutions.

22. The spatial dependence cannot be unlimited. We use a distance cutoff of 50 km. This implies that we assume the errors are uncorrelated across villages at least 50 km apart. The results are also robust to higher cutoffs, for example, a 100-km cutoff, and to using clustered standard errors at the district level (see Online Appendix).

TABLE III
MAIN EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: log(prosecuted persons)								
	Total violence			Militia violence			Individual violence		
Radio coverage in village	0.507** (0.226)	0.526** (0.242)	0.484** (0.235)	0.582** (0.239)	0.559*** (0.216)	0.544*** (0.206)	0.450* (0.233)	0.465* (0.252)	0.418* (0.246)
Population in 1991, log			0.590*** (0.131)			0.589*** (0.171)			0.624*** (0.150)
Population density in 1991, log			-0.014 (0.070)			0.004 (0.101)			-0.015 (0.069)
Distance to major town, log			0.068 (0.150)			-0.233 (0.149)			0.113 (0.152)
Distance to major road, log			-0.196** (0.076)			-0.245*** (0.090)			-0.193** (0.075)
Distance to the border, log			0.171* (0.103)			0.030 (0.126)			0.186* (0.103)
East sloping, dummy			0.017 (0.070)			0.098 (0.092)			0.014 (0.084)
North sloping, dummy			0.065 (0.068)			0.041 (0.092)			0.079 (0.068)
South sloping, dummy			-0.013 (0.074)			-0.028 (0.101)			-0.012 (0.077)
Observations	1065	1065	1065	1065	1065	1065	1065	1065	1065
R-squared	0.63	0.64	0.66	0.52	0.53	0.55	0.62	0.63	0.65
Commune FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Propagation controls	N	Y	Y	N	Y	Y	N	Y	Y

Notes. Militia violence is the number of prosecuted person under category 1 crimes, which are prosecutions against organizers, leaders, army, and militia. Individual violence is crime category 2 prosecutions for homicides, attempted homicides and serious violence. Total violence is the sum of both categories. Radio coverage in village is the share of the village area that has RTLTM reception. The radio propagation controls are latitude, longitude, and second-order polynomials in village mean altitude, village altitude variance, distance to the nearest RTLTM transmitter. Standard errors in parentheses, adjusted for spatial correlation (Conley 1999). Significance levels at *10%, **5%, ***1%.

that have radio coverage. Weighting by population is appropriate since the underlying rationale is that spatial spillovers can arise from social interactions among individuals exposed to the broadcasts. The distance d is either within 10 km, or between 10 and 20 km from village i . In principle, spatial externalities can, of course, work beyond 20 km.²³ For example, the station increases violence in one village, which increases violence in neighboring villages, which in turn affects the violence levels in their respective neighboring villages, and so on. To the extent that such spillovers exist beyond 20 km, the estimated equation will yield a lower bound on the total propaganda effects. However, *a priori* it seems unlikely that spillovers over such distances are present. We also test for this directly by including variables beyond 20 km.²⁴ If there are positive (negative) spillovers for violence of type v within distance d , then $\lambda_{vd} > 0$ ($\lambda_{vd} < 0$).

VI. RESULTS

Table III presents the effects of RTLM radio coverage in a village. Columns (1)–(3) show the effects on total violence. The regression in column (1) uses commune fixed effects, column (2) adds the propagation controls, and column (3) includes additional covariates. The estimated effects of RTLM reception are statistically significant (at the 5% level) and quantitatively important. The estimated coefficients in columns (1)–(3) imply that full radio coverage increased the number of persons prosecuted for any type of violence by approximately 62–69% (0.484–0.526 log points), compared to areas with no radio coverage. A more relevant comparison arises when we scale the coefficient by the variation in radio coverage in the sample. The estimates then imply that a 1 standard deviation increase in radio coverage increased participation in total violence by 12–13%.

Columns (4)–(6) present the results on militia violence. The estimates are significant at the 5% and 1% levels and imply that a 1 standard deviation increase in radio coverage increased

23. For parameters in regression models with spatial spillovers to be identified, the spatial dependence needs to be bounded. For a classic work on spatial econometrics, see Anselin (1988). For a more recent overview of spatial econometric models and their respective limitations, see Elhorst (2010).

24. The results are also robust to the inclusion of variables extending beyond 20 km (results not shown).

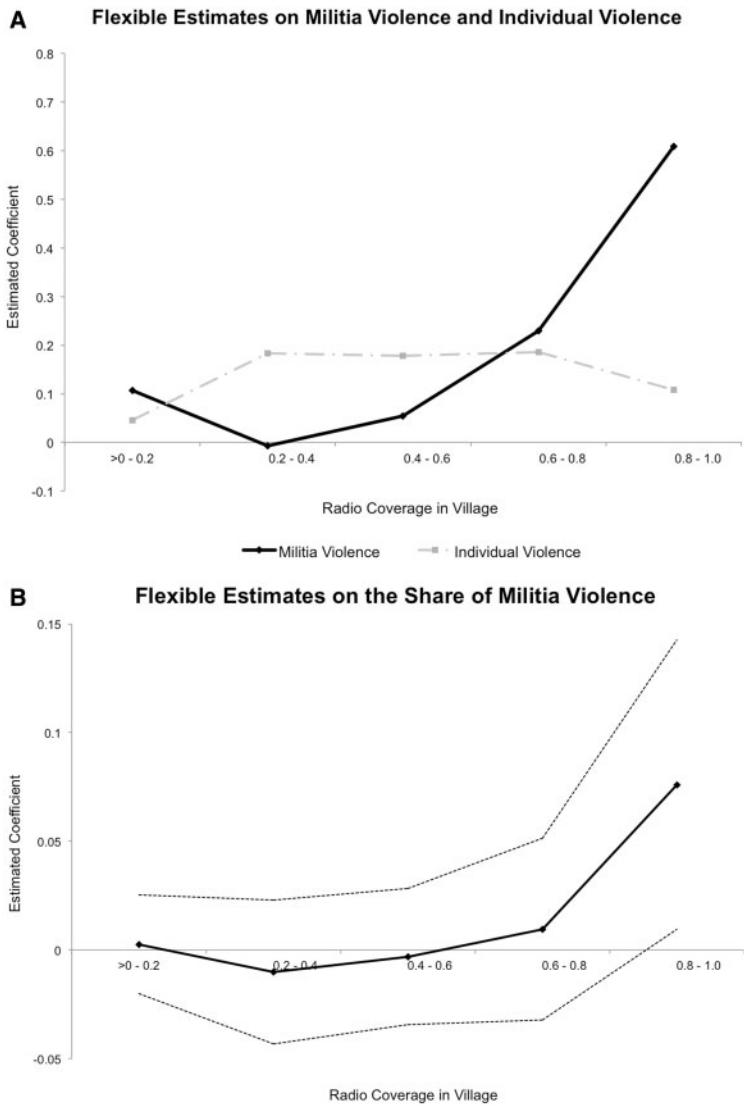


FIGURE III
Flexible Specifications

Panel A plots the estimated coefficients of the radio coverage dummies, for militia and individual violence separately (from Online Appendix Table A1). Panel B plots the estimated effects and confidence intervals on the share of militia violence, and shows that once a sufficiently large share of the village has access to radio, the share of the violence by militias is significantly higher.

participation in militia violence by 13–14%. RTLTM broadcasts were also shown to have increased individual violence. The estimates (significant at the 10% level) imply that a 1 standard deviation increase in the share of the village with radio reception increased individual violence by 10–11%. Compared to the effects on militia violence, the estimates suggest that individual violence was less affected by the broadcasts. However, the effects are not statistically discernible from one another.

These regressions impose a linearity constraint on the coefficient on RTLTM. To probe for a more flexible relationship, Figure III graphically illustrates results using a specification with dummy variables for various levels of RTLTM reception.²⁵ For militia violence (Figure III, Panel A), there is evidence of scale effects. For increases in radio coverage at low levels, the overall pattern indicates that there is no increase in participation, but once a critical level of coverage is reached there is a sharp increase in violence. By contrast, for individual violence there is no similar discernible pattern, and if anything the relationship appears concave, although these differences in point estimates should be interpreted with caution as the confidence intervals are large.²⁶ Figure III, Panel B shows the estimated coefficients when the outcome is the share of militia violence. There is no apparent increase in militia intensity at low levels of radio coverage, but there is a sharp increase in the share of militia violence once a high share of the village has radio coverage. Multiple interpretations of these estimates are possible, not least due to relatively large confidence intervals, but following the logic of Glaeser et al. (2003) the increasing returns to scale are consistent with the presence of a social multiplier in militia violence, possibly because there are reinforcing effects when a large share of the population are simultaneously receiving the same type of inflammatory propaganda.²⁷

25. For coefficients and standard errors, see Table A.1. in the Online Appendix.

26. Unsurprisingly, since the overwhelming fraction of total prosecution cases is for individual crimes, we find little evidence of scale effects when summing collective and individual violence.

27. Such reinforcing effects could arise when radio facilitates coordination, but also if information transmission among neighbors influence how strongly beliefs are updated. It is worth noting that multiple interpretations are possible since the two types of violence may or may not be inherently linked, depending on whether there are strategic interactions between ordinary citizens and militias. For example, the evidence is consistent with ordinary citizens free riding on the high levels of violence caused by the militia, but this mechanism is econometrically not possible to separate from a scenario where ordinary citizens act independently of militias.

VI.A. Measurement Error and Robustness Tests

A potential concern with the results is related to measurement error in the dependent variable. Because we do not observe actual participation but prosecutions, some measurement error is to be expected. That is, in some villages more individuals were prosecuted relative to the number of individuals who actually committed a given crime, and vice versa. Such mismeasurement will of course not lead to biased estimates unless the error is correlated with the variation in radio coverage, conditional on the covariates. Moreover, if there is such a correlation, the sign of the bias will depend on underlying mechanisms driving the error. One worry is that due to the alleged impact RTLTM had on Tutsi deaths, there were fewer Tutsis to act as witnesses after the genocide. Fewer witnesses would arguably decrease the likelihood that someone who committed a given crime was prosecuted for it. In this case, the correlation between the error and radio coverage would be negative, leading to an underestimation of the true effect. By contrast, if RTLTM broadcasts for whatever reason influenced the prosecution process in ways that lead to systematic overreporting of violence, the true effects would be overestimated. For example, such a bias could arise if access to radio broadcasts lead to greater information about victimization or information about the possibility of prosecuting perpetrators. It is important to keep in mind, however, that RTLTM transmitters ceased broadcasting at the end of the genocide. It is therefore not possible for the broadcasts to have directly spread information about the post-genocide prosecution process.²⁸

A related concern is that reception of RTLTM is correlated with reception of other radio stations. In this case, the estimates may not reflect exposure to the type of propaganda content spread by RTLTM, but access to radio per se. This concern is exacerbated by the potential mismeasurement of violence if another radio station was broadcasting content that influenced the prosecution process. More specifically, Radio Rwanda—the only radio station operating in the country in the years after the genocide—was a key media outlet for the Tutsi-led government, and in principle it

28. A third possibility is that RTLTM broadcasts caused a shift in social norms about violence against Tutsis, leading to more prosecutions. Given the inflammatory content targeting Tutsis, this possibility seems rather remote and counter-intuitive because it would require the broadcasts to have made violence against Tutsis less morally acceptable.

TABLE IV
PLACEBO EFFECTS OF RADIO RWANDA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: log(prosecuted persons)								
	Total violence			Militia violence			Individual violence		
Radio coverage, Radio Rwanda	0.192 (0.269)	0.069 (0.281)	-0.004 (0.275)	-0.099 (0.384)	-0.315 (0.376)	-0.403 (0.345)	0.274 (0.283)	0.167 (0.291)	0.104 (0.287)
Radio coverage, RTL			0.560** (0.246)			0.669*** (0.238)			0.482* (0.253)
Observations	1065	1065	1065	1065	1065	1065	1065	1065	1065
R-squared	0.63	0.65	0.65	0.52	0.54	0.55	0.62	0.64	0.65
Commune FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Propagation controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional controls	N	Y	Y	N	Y	Y	N	Y	Y

Notes. Radio coverage is the share of the village area that has reception, for each station respectively. All other variables are defined the same as in Table III. The additional controls are the logs of population, population density, distance to nearest major town, distance to nearest major road, distance to the border; and slope dummies. Standard errors in parentheses, adjusted for spatial correlation (Conley 1999). Significance levels at *10%, **5%, ***1%.

could have influenced the legal process directly by emboldening victims or witnesses.

To address these potential problems and assess the validity of the main results, we perform two tests. First, we include estimates of Radio Rwanda. Doing so adds robustness to the main results. Moreover, we would not expect that Radio Rwanda increased violence since it did not broadcast inflammatory anti-Tutsi propaganda to the same degree as RTLM did, and so unless the station influenced the prosecution process, the effects should be substantially smaller, if not zero. In other words, the station serves as a suitable placebo test of the alternative hypothesis that the inflammatory content did not matter. After the genocide this station was the key media outlet for the Tutsi-led government, which therefore in principle could have influenced the prosecutions process directly by encouraging victims (or relatives of victims) to put forth their cases. In any case, it is clear that this station did not broadcast the same type of anti-Tutsi propaganda as did RTLM. Therefore, if Radio Rwanda were to display similar effects as RTLM, we would be more concerned about whether the latter station's propaganda truly led to more violence.²⁹

The results of this placebo exercise are presented in Table IV. There is no statistically significant effect of Radio Rwanda on prosecution rates across all specifications and outcomes. Columns (3), (6), and (9) show that when both Radio Rwanda and RTLM reception are included in the regression, the effect of RTLM is still significant. The estimate is similar in magnitude to before, consistent with the fact that that reception of the two stations are uncorrelated (Table II, column (12)). Thus, there is no evidence indicating radio access unrelated to the type of propaganda content spread by RTLM led to higher prosecution rates.

Second, to avoid the potential problems associated with prosecution data, we use an alternative proxy for violence based on household surveys. The Integrated Household Living Conditions Survey consists of a nationally representative sample collected in 1999/2000, from approximately 2,400 households in 332 villages. The data contain information on the number of births and deaths

29. Coverage of Radio Rwanda is measured using the same Longley-Rice algorithm and variable definition as for RTLM, applying FM transmitter locations (eight in total) at the time of the genocide. To the best of the author's knowledge, the locations of the station remained the same postgenocide.

within the household, as reported by female heads of households, allowing for measurement of mortality among descendants (i.e., children, including postchildhood adults). The data are therefore not ideal, as they do not exclusively measure mortality due to violence during the genocide.³⁰ Moreover, because only survivors are surveyed, there could be sample selection bias if latent mortality of children among surviving mothers differed from those who were killed during the genocide. That said, the advantage of using this data is that under the null hypothesis of zero RTLM effects on violence we would arguably not expect these biases to arise.³¹ The results of this robustness test are presented in Table A.3. in the Online Appendix. They show that the relationship between RTLM reception and mortality is positive, statistically significant, and stable across specifications. Further robustness tests show that the effects are exclusively driven by households that lived in the village at the time of the genocide, with no evidence of effects among households living elsewhere at the time, and that RTLM primarily affected mortality among male children. The latter result is consistent with the well-known fact that males were disproportionately targeted during the genocide. It also alleviates sample selection bias concerns, since there is arguably no obvious alternative reason households with a relatively high male-to-female latent mortality ratio would be more likely to survive in villages with good RTLM reception. Together, these robustness tests provide evidence against the hypothesis that the results are spurious, and evidence in favor that the anti-Tutsi propaganda spread by RTLM increased violence during the genocide.³²

30. The year of death is not available, but the age of the respondent is. We restrict the sample to female respondents who were at least age 20 at the time of the genocide, since child mortality among very young respondents will primarily reflect disease-related, postgenocide deaths.

31. To see this, note that the first problem is one of measurement error in the dependent variable, as deaths include both genocide related and nongenocide-related deaths. Under zero RTLM effects we would expect the error to be classical, arguably. A similar logic can be applied to the sample selection problem: if RTLM reception did not affect violence and reception is as good as randomly assigned, household members with high and low latent mortality would be equally likely to survive across villages with good and bad reception, on average.

32. We present an additional set of robustness tests in the Online Appendix. Figure IIIA shows the distribution of the estimated coefficients from the main regressions in Tables III and V when districts are dropped one by one. There is no evidence that the results are driven by data from any particular district. Table A.2

TABLE V
SPATIAL SPILLOVER EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Militia violence			Individual violence			Total violence			Share of militia violence		
Radio coverage in nearby villages, within 10 km	2.18*** (0.797)	2.04*** (0.771)	1.76** (0.760)	0.468 (0.570)	0.348 (0.572)	0.682 (0.588)	0.688 (0.613)	0.553 (0.62)	0.790 (0.608)	0.194*** (0.060)	0.184*** (0.059)	0.103 (0.063)
Radio coverage in nearby villages, within 10–20 km	–0.341 (0.739)	–0.277 (0.763)	–0.163 (0.821)	–0.223 (0.675)	–0.167 (0.721)	–0.303 (0.751)	–0.264 (0.629)	–0.202 (0.68)	–0.298 (0.712)	–0.016 (0.091)	–0.012 (0.089)	0.021 (0.092)
Radio coverage in village		0.505*** (0.199)	0.198 (0.443)		0.437* (0.249)	0.801 (0.551)		0.492** (0.240)	0.750 (0.552)		0.036 (0.027)	–0.052 (0.051)
Radio in village * Radio in nearby villages, within 10 km			0.841 (1.029)			–0.999 (0.971)			–0.708 (0.979)			0.240* (0.123)
Observations	1065	1065	1065	1065	1065	1065	1065	1065	1965	1045	1045	1045
R-squared	0.56	0.56	0.56	0.65	0.65	0.65	0.66	0.66	0.66	0.260	0.261	0.263
Commune FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Propagation controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Propagation controls, nearby villages	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes. Radio coverage in village is the share of the village area with RTLTM radio reception. Radio coverage in nearby villages is the population-weighted share of the village areas with RTLTM radio coverage, within a given distance from the village centroid. For nearby villages, the propagation control is the within 10km population weighted average of the standard propagation variables. Additional controls are the logs of population, population density, distance to nearest major town, distance to nearest major road, distance to the border and slope dummies. Standard errors in parenthesis, adjusted for spatial correlation (Conley 1999). Significance levels at *10%, **5%, ***1%.

VI.B. *Spillover Effects*

Table V presents the estimates of equation (3). For militia violence, column (1) shows that cross-village spillovers within 10 km are statistically significant and substantially important. The point estimate (2.18) implies that a 1 standard deviation increase in the share of the population in nearby villages with radio reception (0.18) increases participation in militia violence by 47.6%. The spillover effects are spatially limited, as there is no evidence of radio coverage mattering in villages more than 10 km away. The specification in column (2) includes the direct effect of radio coverage in the village. The estimates on the spillover coefficient are very similar, which alleviates concerns that the spillover effects are spurious due to spatial autocorrelation in radio coverage.

Furthermore, comparing the direct effect in column (2) to the spillover effect, the magnitude of the spillover coefficient is four times the direct effect coefficient (2.04 versus 0.505); however, the two coefficients are not directly comparable, given that a marginal increase in the two variables uses different population scales. The average village population in the sample is 4,850. The population in nearby villages within 10 km is, on average 96,600. Therefore, if we compare a marginal increase in radio coverage within a village to the marginal increase in the population-weighted radio coverage in villages within 10 km, the results imply that the population exposed is approximately 20 times larger in the latter case. Thus, it is not surprising that the spillover coefficient is larger than the direct effect coefficient. If we scale the spillover effect by the relative average population, the spillover effect implies that the marginal effect of an increase in the population having access to the broadcasts in nearby villages (within 10 km) is approximately one fifth of an increase in the share of the population within a given village.

By contrast, columns (4)–(9) show that there is no evidence of positive spillovers for individual violence or, unsurprisingly, for total violence (individual violence constitutes 85% of all prosecutions). The spillover effects consequently affected the composition of violence, as can be seen in columns (10)–(11), increasing the

shows that the results are robust to using district-clustered standard errors. Table A.4 shows that the main specification is not sensitive to outliers when the top 1% violence villages are dropped, while a level specification is more sensitive (this sensitivity analysis is obviously not informative in terms of the true functional form, but it highlights the advantage of the log specification).

share of the violence done by militia members. This is potentially important because militias typically used more advanced weaponry, and therefore were presumably more detrimental in their violence against Tutsis.

To probe for scale effects, column (3) shows that the point estimate on the interaction coefficient between coverage in a village and coverage in nearby villages on militia violence is positive, whereas it is negative for individual violence (column (6)). The confidence intervals are large, though, and we cannot reject a zero effect in each regression. When estimating the impact on militias' share of total violence, in column (12), the point estimate on the interaction is positive and significant at the 10% level, again suggesting a positive social multiplier.

1. Measurement Error. Given the significant spillover effects and the important implications for how propaganda translates into the spread of violence, it is crucial to consider whether measurement error in radio coverage can result in spurious correlations. Specifically, one concern is that the effects of radio coverage in nearby villages could be picking up error in the measurement of the village's own coverage. This would lead one to underestimate the direct effects and potentially overestimate the spillover effects. This concern is particularly valid since the data is based on predicted radio coverage based on software and the irregular terrain model (ITM), rather than true radio coverage. To illustrate this point, we follow the logic of Borjas (1992).³³ Assume that the "true" model is

$$(4) \quad \log(h) = \delta r + \varepsilon,$$

where r is the true radio coverage in a village, with variance σ_r^2 , all variables are in deviations from the mean, and suppressed subscripts. The error term is i.i.d. and independent of radio coverage. In the true model there are thus no spillover effects and the task is to estimate the direct effects, δ , using predicted radio coverage, r' . This variable is an imperfect measure of true radio coverage, so that $r' = r + v_1$, where v_1 is a random i.i.d. noise term independent

33. I am grateful to the editor for suggesting applying Borjas (1992) formalization of the measurement error problem in the context of intergenerational spillovers.

of r , with zero mean and variance σ_1^2 . The OLS estimate in Table III will then asymptotically approach

$$(5) \quad \hat{\delta} = h\delta, \text{ where } h = \frac{\sigma_r^2}{\sigma_r^2 + \sigma_1^2}.$$

Now, suppose there is spatial correlation in radio coverage, so that predicted radio coverage in nearby villages, r'' , provides a second measure of r . Specifically, let $r'' = r + v_2$, where v_2 is i.i.d. with zero mean and variance σ_2^2 , and independent of v_1 . When the second measure is included in the regression model, as in Table V, we have

$$(6) \quad \log(h) = \theta_1 r' + \theta_2 r'' + \epsilon.$$

In this case, OLS implies

$$(7) \quad \text{plim } \hat{\theta}_1 = \frac{\pi h}{1 - h(1 - \pi)} \delta$$

$$(8) \quad \text{plim } \hat{\theta}_2 = \frac{1 - h}{1 - h(1 - \pi)} \delta,$$

where $\pi = \frac{\sigma_2^2}{\sigma_r^2}$ captures the noise-to-signal ratio of the second measure. Equations (7) and (8) imply that the greater the measurement error in a village's own radio coverage is (and the smaller is π), the smaller $\hat{\theta}_1$ and the greater $\hat{\theta}_2$ will be. The first issue to consider, then, is the degree of precision in the measure of radio coverage as predicted by ITM. Thankfully there exists empirical estimates to this end. A validation exercise by Kasampalis et al. (2013) that compared ITM-predicted reception to actual reception indicates that the measurement problem is relatively small, with the implied h being approximately 0.8.³⁴ Since the point estimate of approximately 0.5 when nearby villages' radio coverage is not included in the regression model (as in Table III), this degree of precision suggests that δ may be as high as 0.62. Moreover, equations (5) and (7) tell us that $\frac{\hat{\theta}_1}{\delta} < 1$, so that once radio coverage of nearby villages is included in the model, the point estimate should decrease in magnitude. The results in column (2) of Table IV and

34. Kasampalis et al. (2013) uses the same algorithm, but another software for the calculations (ITM Radio Mobile). They consider the measurement error when predicting reception at a geographic point. Because the measure in this article is for an area we would expect some of the errors to wash out, resulting in potentially even higher precision (greater h).

column (6) of Table III show that there is a decrease, but a very modest one (from 0.544 to 0.505, or $\frac{\hat{\theta}_1}{\delta} = 0.93$). This suggests that nearby radio coverage does little in reducing any potential bias due to measurement error in village radio coverage. Furthermore, by combining the estimated decrease in the estimates across the two models in Tables III and V, together with plausible assumptions about underlying parameter values, this framework allows us to predict what the estimate on nearby radio coverage in column (2) of Table V would be if there were no spillover effects. Specifically, let $\delta = 0.62$ and $h = 0.8$, then equations (5), (7), and (8) imply $\pi = 0.72$, and we would predict the estimate $\hat{\theta}_2$ to be 0.16. This is well below the actual estimate (2.04), corresponding to a mere $\frac{1}{13}$ of its value. Moreover, comparing 0.16 to the lower bound of the 95% confidence interval (0.53) of the regression estimate, we can statistically reject the null hypothesis of no spillover effects in the presence of measurement error. In fact, even if we assume the measurement error problem is twice as severe, with $h = 0.4$, the predicted coefficient (0.42) still lies outside the confidence interval. Thus, this exercise shows that it is unlikely that the estimated spillover effects are simply picking up errors in the measurement of radio coverage.

2. Interpretation. What can jointly explain the direct effects and the spillover effects? As outlined in Section III, a potential explanation is that violence begets violence. That is, RTLTM persuaded some militia members who listened to the radio to join the genocide; as a consequence this led to higher mobilization among militia members in neighboring villages via peer influences. Alternatively, word-of-mouth communication may have spread information and beliefs. To econometrically quantify the contribution of alternative channels of influence is beyond the scope of this article, but useful qualitative evidence from Hatzfeld (2005) and Straus (2007, p. 626) help shed light on how the violence spread in practice. In his survey of over 200 perpetrators, Straus found evidence consistent with RTLTM initially having convinced some key agents to buy into the genocide agenda, and that these agents in turn recruited additional people. These mobilization efforts also spanned across villages. He concluded that

there is evidence that radio broadcasts had a conditional effect of catalyzing some hard-line individuals, but most respondents claim radio was not the

TABLE VI
AGGREGATE EFFECTS, COUNTERFACTUAL ESTIMATES

	Prosecuted persons, counterfactual	Prosecuted persons, actual	Violence caused by RTLM, prosecuted persons	Violence caused by RTLM, (%)
Total violence	459,111 (21,358)	509,826	50,715	9.9
Militia violence, excluding spillover effects	71,311 (2,098)	77,269	5,958	7.7
Militia violence, including spillover effects	54,841 (6,204)	77,269	22,428	29.0
Individual violence	404,240 (15,179)	432,557	28,317	6.5

Notes. The first column reports the mean and standard deviation (in parentheses) of the counterfactual estimates. They are calculated in the following manner: First, the coefficient is drawn from a normal distribution with mean and standard deviation equal to the estimated coefficient and standard error from column (6) of Table V for militia violence, and column (9) of Table III for individual violence. For a given draw, in each village the counterfactual number of persons prosecuted as if radio coverage was 0 is calculated. The aggregate counterfactual is then the sum of village counterfactuals. This procedure is repeated 500 times using random draws to produce the distribution of aggregate counterfactuals. The third and fourth columns report the difference in the actual and the mean of the counterfactuals. The counterfactual for total violence is the sum of militia violence including spillover effects and individual violence.

primary reason that they joined attacks The general pattern of mobilization at the local level reported by respondents is that elites and young toughs formed a core of violence. They then traversed their communities, recruiting a large number of Hutu men to participate in manhunts of Tutsis or to participate in other forms of “self-defense”, such as manning road blocks.

Hatzfeld’s interviews with killers paint a consistent picture of militia spillovers. For example, one perpetrator explained: “We got on fine, except for the days when there was a huge fuss, when Interahamwe reinforcements came in from the surrounding areas in motor vehicles to lead the bigger operations. Because those young hothead ran us ragged on the job.” Another killer further described similar social dynamics: “If many reinforcements from the neighboring hills had turned up, the leaders took advantage of having these attackers along to bring off more profitable hunting expeditions, surround the fugitives on

all sides. It was double work, in a way The outside reinforcements and their enthusiasm—that was the toughest pressure the organization put on us.” These anecdotes provide a rationale for why the RTLM broadcasts had effects beyond the areas the station directly reached. They make it clear that social interactions across villages mattered, and suggest that some key militia leaders that presumably were exposed to the broadcasts functioned as catalysts in the diffusion of violence, whether by spreading information or because their activities directly influenced the participation trade-offs of others. As such, they are broadly consistent with the role of personal influences intermediating media effects as emphasized in the political communication literature.

VI.C. *Aggregate Effects*

The results have shown that RTLM had a direct effect on participation in villages with access to the broadcasts, and indirect spillover effects on militia violence in villages without access. To assess the effect of RTLM broadcasts on aggregate participation, and the relative importance of the spillover effects, we perform a simple counterfactual calculations estimating what the scale of the genocide would have been in the absence of the broadcasts. Table VI presents the results.

The actual number of persons prosecuted for militia violence is approximately 77,000, and for individual violence approximately 433,000. As the results in Table V show that spillovers across villages were important for participation in militia violence, we construct two counterfactual aggregate measures of militia violence using the estimated equation (3). (Using the flexible equation coefficients for the direct effects yields similar results.) First, we estimate a counterfactual allowing for both direct effects and spillovers. The difference between the actual and the counterfactual number of prosecutions gives us the total effect of RTLM broadcasts. Second, we estimate a counterfactual assuming only direct effects while ignoring cross-village spillovers (i.e., restricting the spillover parameter in equation (3) to be 0). Comparing the counterfactuals will then allow us to estimate the contribution of cross-village spillovers.

To do this, for militia violence we use the estimated regression 2 of Table V. Since the coefficient of radio coverage within 10–20 km is small and insignificant, we simply let it be 0. To account for uncertainty in the estimated regression parameters, we

first draw each coefficient (one for the direct effect and one for the spillover effect within 10 km) from a normal distribution with mean equal to the estimated coefficient and standard deviation equal to the standard error. For each observation, we then calculate the counterfactual number of prosecutions. The total number of prosecutions in the sample is then summed. Since the sample does not contain the universe of villages, we rescale the counterfactual number estimated in the sample by the fraction of actual prosecutions in the sample. This gives us the counterfactual number of prosecutions in the country as a whole. This procedure is then repeated 500 times, using a random draw of coefficients each time.³⁵ For individual violence, as we find no evidence of cross-village spillovers, we follow the same procedure as for militia violence, with the difference that the estimated equation (2) is used instead (estimated coefficient and standard error come from column (9) of Table III).

Table VI presents the means and standard deviations of the estimated counterfactuals, and Figure A.4. in the Online Appendix illustrates the distributions graphically. Focusing on the mean, the estimates imply that 9.9% (approximately 51,000 persons) of the total participation in genocidal violence was caused by the propaganda. Looking at the two forms of violence separately, we see that 6.5% of individual violence was caused by the broadcasts, whereas for militia violence the effects are substantially larger. The estimates suggest that 29.0% (approximately 22,000 persons) of the aggregate militia violence was caused by RTLM broadcasts. The evidence also shows that spillovers were important, as only 7.7% of the militia violence is estimated to be due to direct effects. Of the militia violence 22.3% can therefore be attributed to spillover effects.

Conservative estimates suggest that at least 500,000 people were killed in the genocide (Des Forges 1999). However, because there are no reliable nationwide data on deaths available at the village level, one limitation of the data is that it does not allow for direct estimates of how many deaths the broadcasts caused. Additional assumptions are therefore needed to assess the causes

35. It should be noted that the counterfactual analysis obviously estimates the aggregate violence in the absence of RTLM broadcasts without taking into account potential political economy forces at the national level, such as some political entrepreneurs endogenously responding to the vacuum caused by a shutdown of the station.

of deaths. Under the additional assumption that the number of deaths was proportional to the total number of prosecution cases, which is speculative, the estimated effects suggest that RTLM caused approximately 50,000 Tutsi deaths. Due to the lack of data on deaths, however, the degree of uncertainty about this number is high and the estimate should be interpreted with caution.

VI.D. Heterogeneous Effects

Given the detrimental effects of propaganda on the welfare of the Tutsi population, an important question is whether there are factors that can limit the effectiveness of such campaigns that target and encourage violence against minorities. We investigate the role of education. This is an interesting dimension to consider because there are essentially two competing views regarding the relationship between education and political persuasion. One is that public education provided by authoritarian regimes, like the Hutu-controlled government before the genocide, can be used as an indoctrination tool by shaping beliefs or ideology of the population in a desired direction (e.g., Lott 1999; Kremer and Sarychev 2000; Friedman et al. 2011; Cantoni et al. 2014). Indeed, history taught at both primary and secondary levels in Rwanda had propagated a version of the past based largely on ethnic stereotypes and interpretations of Rwandan history, which supported the political ideology and rhetoric of the Hutu regimes in power during the pre-genocide period (McLean-Hilker 2010). Beliefs formed in school may in turn matter for how information by mass media is processed, especially if people favor information that confirms their prior beliefs (e.g., Rabin and Schrag 1999). The implication in this context would be that educated people in Rwanda were potentially more susceptible to the anti-Tutsi broadcasts because the messages resonated with their prior beliefs.

An alternative view is that education leads people to be less susceptible to persuasion from any given media outlet. The underlying rationale behind this view is that education leads to better access to alternative information sources, greater political awareness, and more critical thinking (e.g., Zaller 1992). Literacy is an important dimension of education in developing countries where education levels are low, and particularly relevant in this case because newspapers served as the primary

TABLE VII
HETEROGENEOUS EFFECTS OF RTLM BROADCASTS ON PARTICIPATION IN THE GENOCIDE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: log(prosecuted persons)								
	Total violence			Militia violence			Individual violence		
Radio coverage * % Hutu with primary education	-0.105** (0.048)	-0.100* (0.054)	-0.082* (0.048)	-0.102** (0.046)	-0.081 (0.061)	-0.086 (0.064)	-0.099** (0.049)	-0.102* (0.059)	-0.081 (0.051)
Radio coverage * % Hutu with cement floor	0.043* (0.022)	0.028 (0.037)	0.070* (0.041)	0.049* (0.027)	0.003 (0.043)	-0.009 (0.052)	0.037* (0.021)	0.036 (0.038)	0.085* (0.044)
Radio coverage	0.394 (0.263)	-5.92* (3.42)	-4.46 (3.39)	0.442** (0.207)	-9.81*** (3.31)	-10.2*** (3.40)	0.341 (0.276)	-4.45 (4.13)	-2.69 (4.13)
Radio coverage * Size of Tutsi minority			-0.211** (0.091)			0.059 (0.144)			-0.253** (0.109)
Observations	1065	1061	1061	1065	1061	1061	1065	1061	1061
R-squared	0.66	0.66	0.66	0.55	0.55	0.55	0.65	0.65	0.65
Commune FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Propagation controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional interaction controls	N	Y	Y	N	Y	Y	N	Y	Y

Notes. All variables are defined the same as in previous tables. % with primary education is the percentage of Hutu household heads in the commune that have at least some primary education. The wealth proxy is the fraction Hutu households in the commune that has cement floor in the house. Each additional interaction control consists of the interaction between RTLM radio coverage and: log population density, log population, log distance to the nearest major town, log distance to the nearest major road, log distance to the border, East/West/South dummies, percent literate Tutsi, and percent Tutsi with cement floor, respectively. Standard errors in parentheses, adjusted for spatial correlation (Conley 1999). Significance levels at *10%, **5%, ***1%.

alternative information sources at the time of the genocide. The independent press had grown quickly in the early 1990s, and there were at least 30 independent newspapers that did not align with the government parties (Alexis and Mpambara 2003; Higiyo 2007). Even though each individual newspaper arguably slanted their news toward reader biases, in the aggregate a reader with access to all news sources might get an unbiased perspective (Mullainathan and Shleifer 2005). Anecdotal evidence further indicates that literacy was a key constraint on access to newspapers and demand in rural areas (Des Forges 1999). Thus, an implication of this view is that the effects of the radio station were weaker in villages with greater levels of education in general and higher literacy rates in particular.³⁶

Table VII presents the results. Because education is likely to reflect socioeconomic status or wealth, which may have a direct effect, all regressions control for interactions with a wealth proxy (share of Hutu households in the commune with cement floor from the 1991 census). The results show that the effect of RTL on violence decreases with primary education levels, measured as the share of heads of Hutu households with some primary education. The estimate in column (1) is significant at the 5% level. To further control for various village characteristics that may interact with radio coverage, column (2) adds a set of interaction terms for each of the controls. The estimate is essentially identical in magnitude (the significance level is now 10%) which suggests that any omitted variable bias is limited. The estimate is somewhat smaller in magnitude when we add an interaction term for the relative size of the Tutsi minority population in column (3), but the level of statistical significance remains. The implied magnitude is meaningful, suggesting that the effect of full radio coverage in village is associated with approximately a 50% weaker effect on total violence when primary education levels are 1 standard deviation higher (equal to 6 percentage points in 1991).³⁷ The results for literacy rates are very similar (available in Table A.5 in the Online Appendix), consistent with

36. The two channels suggest education might have heterogeneous effects depending on context and the educational system. For example, Gentzkow and Shapiro (2004) study anti-American beliefs across countries in the Muslim world and find both negative and positive associations.

37. Columns (4)–(9) show results for militia violence and individual violence separately. The coefficients are similar in size, but statistically insignificant in some specifications due to larger residual variation in the outcomes.

the idea that education reduced the effects of the radio station because it enabled access to information provided by less extreme newspapers.

The results further show that the relative size of the Tutsi minority mattered for the effectiveness of RTLM. The coefficient in column (3) of Table VII is negative and statistically significant, implying that the greater share of the population that were Tutsi, the weaker were the effects of RTLM on violence. This would be an unsurprising and rather trivial result if it simply represented a mechanical effect from a smaller audience (relatively fewer Hutus). However, columns (6) and (9) show that the effect only occurs for individual violence, with no evidence of heterogeneous effects for militia violence. An alternative explanation is that ordinary citizens perceived it as more costly to attack Tutsis when their population share was high, perhaps because it seemed more risky. Regardless of the specific mechanism, this result suggests that an ethnic group is particularly vulnerable to inflammatory propaganda against it when it constitutes a relatively small minority of the overall population.

VII. CONCLUDING REMARKS

This article provides evidence that mass media can affect conflict in general, and genocide violence against an ethnic minority in particular. The results show that the main radio station broadcasting anti-Tutsi propaganda during the Rwandan genocide significantly increased participation in the violence against Tutsis. The counterfactual estimates suggest that approximately 10% of overall participation can be attributed to the radio station's broadcasts, and almost one-third of the violence by militias and other armed groups.

The article provides some evidence on the channels by which the broadcasts induced more violence. It also opens some new questions. The data show that the station not only directly influenced violence in a given village if radio reception was possible in that village, but also that such reception lead to greater militia violence in nearby villages. These spillovers are consistent with social interactions being important drivers in the production of militia violence, and although there are multiple ways by which social interactions could give rise to the observed effects, two seem particularly plausible. Violence may beget violence, such

that it endogenously spreads across space. Alternatively, information and beliefs may spread via social interactions among neighbors. The two mechanisms may also work in tandem. This leaves the question of why there are no spillover effects for violence conducted by ordinary citizens, but it may be that the diffusion of information and beliefs is particularly fluid among individuals in militia networks, or that there are stronger complementarities in the production of militia violence. Although it is beyond the scope of this article to identify and quantify the specific underlying mechanisms, what is clear from the evidence is that the spillover effects were quantitatively important. Theoretical and empirical inquiries that identify how ethnic violence spreads via social interactions and networks therefore seem like promising avenues for future research.

The evidence speaks directly to the drivers of the Rwandan genocide. Only so much can be inferred from one historical case, and it is an open question to what extent we should expect inflammatory propaganda against a minority group to display effects in other contexts and countries. The results in this article provide some suggestive evidence on the conditions under which the propaganda is more effective in inducing violence. Specifically, propaganda encouraging violence against an ethnic minority appears to be more capable of inducing participation when the minority is relatively small and defenseless, and when the targeted audience lacks basic education. Apparently, scale matters: militia violence was seemingly disproportionately high when a critical mass of the population could receive the broadcasts. Of course, additional fundamental factors beyond the scope of this investigation are likely to be just as important, such as preexisting ethnic animosity and a history of civil war.

Finally, the results are relevant for the policy debate regarding restrictions on mass media, especially in cases of state-sponsored mass violence. The international debate during the Rwandan genocide is illustrative. The United Nations Force Commander for the peacekeeping intervention, Romeo Dallaire, urged the international community to jam RTLM signals, but his call went unheeded (Dallaire 2007). Arguments against the measure were that it would violate Rwanda's state sovereignty and impinge on the fundamental human right to free speech and a free press. Also, the U.S. Department of Defense estimated that jamming the station would be costly—about \$4 million in total (Chalk 1999). Ultimately, State Department officials concluded

that the United States should not interrupt RTLM broadcasts. The results presented here show that allowing the station to broadcast had substantial human costs, with consequences detrimental for the targeted population. In addition, the violence may have had long-term impact on human capital formation (Akresh and de Walque 2009), social capital, and political stability. In future conflicts where there is evidence of mass media being used as a tool for state-sponsored mass violence, it might therefore be advisable that external actors considering policy options take these possible consequences into account.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournal.org).

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