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Arthur Blouin and Sharun W. Mukand

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Erasing Ethnicity?

Propaganda, Nation Building and Identity in Rwanda

Arthur Blouin

University of Toronto

Sharun W. Mukand

University of Warwick*

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Abstract

This paper examines whether propaganda broadcast over radio helped to change inter-ethnic attitudes in post-genocide Rwanda. We exploit variation in exposure to the government's radio propaganda due to the mountainous topography of Rwanda. Results of lab-in-the-field experiments show that individuals exposed to government propaganda have lower salience of ethnicity, increased inter-ethnic trust and show more willingness to interact face-to-face with members of another ethnic group. Our results suggest that the observed improvement in inter-ethnic behavior is not cosmetic, and reflects a deeper change in inter-ethnic attitudes. The findings provide some of the first quantitative evidence that the salience of ethnic identity can be manipulated by governments.

I. Introduction

Can a government in an ethnically divided, conflict-ridden society help bridge the ethnic divide? A large literature argues that more conflict, higher corruption, weaker institutions and lower economic growth all plague countries with ethnic divisions.[1] In this context, nation-building - in the form of increased ethnic trust, cooperation, and reduced ethnic salience - may have the potential to undercut the roots of inter-ethnic violence.[2]

This paper examines the role of propaganda as a tool of nation-building in Rwanda - a country in which Hutu extremists massacred more than 70% of the minority Tutsi population in 1994 in one of the worst genocides in recorded history. Critics of the government's program of post-genocide nation-building (e.g. Thomson (2011a)), have noted how difficult it is to assess whether progress in ethnic reconciliation is cosmetic or real. In large part, this is because, under President Kagame, Rwanda is a quasi-autocracy that controls the media and tries to manage the narrative on reconciliation. In fact, according to a recent report on Rwanda in the *New York Times*: "Mr. Kagame has created a nation that is orderly but repressive...Against this backdrop, it is *difficult to gauge sentiment about the effectiveness of reconciliation efforts*."[3]

Accordingly, we evaluate the Rwandan government's efforts to reshape ethnic attitudes through radio propaganda, by implementing a series of lab-in-the-field experiments that measure ethnic attitudes. Our design exploits village-level variation in reception of government-owned-and-operated Radio Rwanda - the result of the mountainous topography of Rwanda (colloquially known as the *land of a thousand hills*). Our empirical strategy is similar in spirit to Yanagizawa-Drott (2014) but relies on contemporary variation in Radio Rwanda rather than historical variation in the hate-radio station RTLM.[4] We combine GIS data

[1]See Easterly and Levine (1997); Garcia-Montalvo and Reynal-Querol (2004), Blattman and Miguel (2010).

[2]See Alesina and Ferrara (2005), Alesina et al. (2016) for evidence, and Besley and Persson (2011) (Chapter 7) on the importance of "cohesiveness" of inter-group preferences for development.

[3]This is a from "How a Nation Reconciles After Genocide Killed Nearly a Million People" by Megan Specia in April 25, 2017. (emphasis added)

[4]The first paper to exploit variation of signal transmission for examining economic outcomes is Olken

showing the reach of radio signals with data from a survey, and lab-in-the-field experiments on inter-ethnic attitudes conducted with 438 subjects in 52 villages in rural Rwanda.

We examine four main outcomes, each of which sheds light on aspects of inter-ethnic attitudes. The first aims to measure the salience of ethnic identity and the second measures a subject's willingness to engage in a cooperative task with a partner from another ethnic group. We also collected data on both a private and a public version of the trust game and supplemented this with simple survey questions on in-group and out-group trust. Of course, the key to this exercise relies on the ability to identify the ethnic identity of all subjects. This is particularly challenging since the Rwandan government typically forbids researchers from directly asking subjects if they are Hutu or Tutsi. Navigating this challenge was facilitated by our discovery of a proxy for Tutsi ethnicity: whether or not the subject was *eligible* to receive funds from the Fund for the Assistance of Survivors of the Genocide[5] (known as F.A.R.G.). F.A.R.G is a reparations fund available only to genocide "survivors" (read: Tutsi). Armed with these data, we examine the impact of radio propaganda on inter-ethnic behavior and ethnic salience.

Our first outcome is a measure of ethnic salience. Several studies suggest that ethnic salience may be weak when individuals do not expect ethnicity to matter for the allocation of economic or political rewards.[6] Finding a measure of ethnic salience that is both plausible and easy to measure in a country such as Rwanda presents a research challenge. To address this challenge, in this paper, we introduce a new methodological tool to economics - the *salience of identity test* (SIT).[7] The test provides a simple way to capture whether a subject subconsciously or consciously categorizes others on the basis of his or her ethnicity: in this case Hutu or Tutsi. The test involves a recall task centered on matching pictures of Hutu and

(2009) who examined impact of watching TV on social capital in Indonesia.

[5]This is translated from French. The original name of the fund in French is: "Fonds d'Assistance aux Rescapés du Génocide"

[6]The pull of national identity has also been shown to influence inter-ethnic attitudes. Gibson and Gouws (2005) show how the pull of national identity weakened inter-group antipathy in South Africa. Also see Sniderman and Carmines (1997) and Eifert et al. (2010).

[7]Versions of this test have been used in cognitive psychology and the classic reference is Taylor et al. (1978) - we discuss this much more systematically later in the paper.

Tutsi men to associated generic (neither positive nor negative) statements. If we find that an individual is systematically more likely to mistakenly attribute a statement that corresponds to one Hutu to another Hutu rather than a Tutsi, we say that this is because ethnicity is salient for that subject. We find that ethnicity is *less* salient in regions exposed to the government's nation building rhetoric. Individuals in exposed regions are 10-13 percentage points ($\approx$ 0.39 standard deviations) less likely to categorize others on the basis of their ethnicity, as measured by the SIT (mean 86%).

The second outcome is a partner selection task. We asked villagers to select, from a pool of fifteen other subjects, the five that they would most like to be partnered with for a cooperative face-to-face interaction. Residents of villages with exposure to Radio Rwanda were 15.6 percentage points, or about 0.54 standard deviations, more likely to choose someone of the other ethnicity with whom to engage in a cooperative social task.

The third outcome is a survey measure of inter-ethnic trust. Respondents in villages exposed to Radio Rwanda are about a quarter point on a Likert scale (0.35 s.d.) more likely to respond that they trust members of the other community, but they do not report being more or less trusting of members of their own community.

The fourth outcome comes from behavior in a trust game. Each subject played either a private or a public variant of the trust game. The private trust game results are consistent with the other three outcomes. Inter-ethnic trust offers in the private trust game are 47% higher, or 1.13 standard deviations, in areas that receive a radio transmission from Radio Rwanda. Further, radio transmission does *not* seem to influence trust between members of the same ethnic group. Our results suggest that radio propaganda seems to have been successful at targeting inter-ethnic relationships rather than levels of generalized or overall trust.

One of the key criticisms of nation building and reconciliation in Rwanda is that there has been no *real* change in inter-ethnic attitudes. Thomson (2011a) has argued that observed changes in behavior are solely due to "ritualized dissimulation and strategic compliance."

Similarly, Ingelaere (2010) argues that any observed improvement in inter-ethnic relationships is merely cosmetic, since the populace masks its true feelings about ethnic relations, and pretends to get along with those of the other ethnicity to avoid attracting government attention. The results from the public version of the trust game suggest that government propaganda may have aligned public and private behavior. Trust offers in the public game are over 25% higher than private trust offers (0.58 standard deviations) in regions *not* exposed to Radio Rwanda. In contrast, trust offers in public and private games are very similar in radio regions.

Taken together the evidence suggests that exposure to government radio leads to higher inter-ethnic trust and cooperation as well as lower ethnic salience.

We examine whether alternative mechanisms or confounds could explain the above empirical patterns. For instance, given that radio transmission (most specifically the radio station RTLM) was used to incite violence during the 1994 genocide (Yanagizawa-Drott 2014), in all our empirical specifications we directly control for coverage by RTLM as well as for genocide prosecutions, even though no evidence suggests that radio towers were built strategically to target villages that were ultimately affected by the genocide.

We can also rule out that reception of stations other than Radio Rwanda affects the outcomes that we study. Moreover, the main results are robust to including controls both for the reception of other signals (including RTLM), and for distance to the nearest three broadcast towers.

Furthermore, we can rule out, to various degrees, that dimensions of trust in Radio Rwanda regions could be an accidental by-product of economic development; that measurement error in the ethnicity proxy is affecting estimates; as well as that confounding effects, such as migration or strategic radio tower placement, are a factor.

This paper contributes to three areas of social science research. First, consider the literature on politics and the media (Gentzkow and Shapiro 2008). Much of this literature suggests that a politicized media seems to have (at best) modest effects in influencing party

vote shares and electoral outcomes in competitive democracies such as the United States (see Gentzkow et al. (2014) and for a survey Strömberg (2015)). The situation may be quite different in countries with autocratic or nascent democratic political institutions. For instance, a few studies that suggest that media and propaganda can help drive a wedge between groups and exacerbate conflict as in Yanagizawa-Drott (2014), which was the first paper to exploit the mountainous topography of Rwanda to identify the impact of radio (the now defunct RTLM) on the genocide. Similarly, we see that exposure to Serbian radio catalyzed anti-Serbian sentiments in Croatia (DellaVigna et al. 2014). We see similar effects of radio on Nazi party popularity and anti-Semitic actions in Nazi Germany (Adena et al. 2015). By contrast, we have fewer studies that examine how media can bring groups together and erase ethnic cleavages. In the Rwandan context, Paluck (2009) and Paluck and Green (2009) demonstrate that media does have the *potential* to improve inter-ethnic attitudes. They examine subjects who are exposed to a reconciliation soap opera on radio, for four segments of twenty minutes each, once a month for a year. They find that radio programming can improve self-reported attitudes, leading to greater tolerance of dissent and increased openness to ethnic intermarriage. Our study builds on this work by showing that not only are improvements possible but that the Rwandan government has actually realized these achievements at a countrywide level (Thomson 2011b).

Second, our paper directly contributes to the literature on nation building. Many country-specific studies have focused on the role of the state in catalyzing nation building, including France (Weber 1976), Italy (Duggan 2007), and Singapore (Ortmann 2009). Our results on the erasure of ethnic identity suggest that social identity is malleable, even in a relatively short time frame. Miguel (2004) emphasized the importance of nation building in changing a country's inter-ethnic culture by showing that nation building in Tanzania allowed ethnically diverse communities to raise more resources for local public goods than in neighboring Kenya. Alesina and Reich (2013) provide a theoretical analysis of a government's incentives to engage in nation building. They point out that autocratic governments representing a minority

group may undertake some of the more aggressive forms of nation building - a finding that is consistent with what we observe in Rwanda.

Third, this paper relates to the literature on the economics of identity. The seminal work on the economics of identity by Akerlof and Kranton (2000) examines how identity affects economic outcomes. As Sen (2007) has pointed out, individuals can choose from a multiplicity of identities - be it gender, race, class or any ascriptive marker. Indeed, evidence suggests that social identity and preferences are malleable in a laboratory environment (Chen and Li 2009; Benjamin et al. 2010, 2013); and that they respond to real-world exogenous events, such as bombings in Israel (Shayo and Zussman 2011). These influences can be quite persistent (Voors et al. 2012; Voigtländer and Voth 2012; Shayo and Zussman 2016). However, our results suggest that the salience of identity is a choice variable that is manipulated by political entrepreneurs - with rich implications for how to model identity politics in political economy (see discussion in Fearon and Laitin (2000)).[8]

The outline of the rest of the paper is as follows. In section II we provide the historical background and the contemporaneous political context in Rwanda. The experimental protocol is described in section III, while section IV describes our experimental measures and data. Section V describes the empirical strategy while section VI presents all our results. We examine alternative mechanisms and examine the robustness of our results in section VII and conclude with a short discussion in section VIII.

II. Ethnicity, Politics and Nation Building

By the end of the 15th century, the Hutu and Tutsi settled the African Great Lakes region. Political power was gradually consolidated into two dynasties whose geographical ambit approximately corresponds with present-day Rwanda and Burundi. Rwanda is now composed

[8]Esteban and Ray (2008), Shayo (2009), Caselli and Coleman (2013) and Mukand and Rodrik (2018) explore some of the theoretical underpinnings of the politics of identity salience. More generally, our findings provide evidence in favor of the social constructivists (such as Smith (1991) or Gellner (1983)) about the political construction of national identity that may overwhelm primordial characteristics (see Cerulo (1997) for a survey).

of three main ethnic groups: the Hutu and Tutsi who make-up over 97% of the population, and the Twa, who represent the remainder. Germany laid claim to the region during the colonial scramble for Africa, and this claim was recognized by other European powers in 1891. The First World War resulted in a transfer of administration of the kingdoms of Ruanda-Urundi to Belgium, which administered the region starting in 1916. In 1962 Ruanda-Urundi was given independence, and Rwanda and Burundi were formed.[9]

During the colonial period, a combination of direct and indirect rule fostered the idea of Tutsi superiority over the Hutu and hardened resentment of each ethnic group for the other. By the time of independence in 1962, these two ethnic categories were politically salient with both groups represented by ethnic political parties - the Hutu by PARMEHUTU and the Tutsi by UNAR. With independence, Belgium switched its political support from the minority Tutsi to the majority Hutu, and the PARMEHUTU party assumed power. Over the next three decades the underlying political dynamic was one of continuous, low-intensity Hutu-Tutsi conflict (with the Hutu in power) that boiled into the Rwandan Genocide in April 1994.[10] Over a 100-day period between April and July 1994, as many as 1 million people were killed, and more than 70% of the Tutsi population was slaughtered. In terms of percentage of the population, it is one of the worst genocides in recorded history. The Tutsi-controlled Rwanda Patriotic Front (RPF) ended the civil war when it took control of Kigali in July of that year. Gradually, over the next few years the RPF asserted control and after the nominally free elections of 2003, Paul Kagame officially took over the Presidency.[11]

After taking over as head of the government, President Kagame and the RPF made reconciliation a top priority. The ostensible reason for the nation-building exercise was that a minimal degree of inter-ethnic rapport was essential not only for economic development, but also to prevent genocide in the future. Accordingly, the National Unity and Reconciliation Commission (NURC) was established and tasked with building inter-ethnic trust and

[9]For a historical overview of Rwanda and Burundi see Newbury (2001) and Mamdani (2012).

[10]For details on post-independence political developments and especially the genocide see Meredith (2013), Gourevitch (1998), Hatzfeld (2009)

[11]For an account of the rise of the RPF and that of Kagame see Stephen (2008).

forging a new Rwandan identity. The NURC mandate is quite broad, and the commission implemented a substantial package of policies nationally, with the aim of promoting a new Rwandan identity. We describe some of these policies below.[12]

A. Legislation, Sanctions and Inter-Ethnic Prejudice

Given that state capacity is high, the populace genuinely fears being reported to (and punished by) the government for harboring inter-ethnic prejudice. Central to this fear is the adoption of *Rwanda Law 18* which defines and criminalizes "genocide ideology."[13] Individuals perceived as harboring inter-ethnic prejudice can be arrested and jailed and sometimes even disappear (Beswick 2010).[14] Strict enforcement and social surveillance by an autocratic government gives the law its teeth. The RPF has offices at the sector level and informants at the village level. If individuals are perceived to be harboring inter-ethnic prejudice and are not acting in accordance with the diktats of the office of the NURC, then anything from blocked access to local cooperatives and government services to harassment and arrest is possible.

B. Persuasion, Propaganda and Indoctrination

A primary objective of the Rwandan government has been to create a new inclusive Rwandan identity and to erase the hold of ethnic identity.[15] This is considered one of the government's top priorities. Its importance can be gleaned from a senior government official's statement

[12]This discussion draws on Thomson (2011b), who provides an excellent overview of the variety of measures adopted by the government as part of this ostensible nation building exercise.

[13]*Article 2* defines genocide ideology as "an aggregate of thoughts characterized by conduct, speeches, documents and other acts aiming at exterminating or inciting others to exterminate people basing on ethnic group, origin, nationality, region, color, physical appearance, sex, language, religion or political opinion, committed in normal periods or during war."

[14]Part of the fear stems from an arbitrary aspect of the law itself. For instance (Article 3) of the law says an individual can be accused of the crime of "genocide ideology" for " (1). threatening, intimidating, degrading through defamatory speeches, documents or actions that aim at propounding wickedness or inciting hatred; (2). marginalizing, laughing at one's misfortune, defaming, mocking boasting, despising, degrading, creating confusion aiming at negating the genocide which occurred, stirring up ill feelings, taking revenge, altering testimony or evidence for the genocide which occurred."

[15]As Kagame asked rhetorically in his speech at the 20th Commemoration of the Genocide "...if we succeed in forging a new, more inclusive national identity, would it be a bad thing?"

(quoted in Thomson (2011b), p. 114) that "We are no longer Hutu, Tutsi or Twa – we are Rwandans." The regime plays a crucial role in emphasizing "the unifying aspects of Rwandan history, such as our shared culture and language and de-emphasizing divisive ones in all activities in the public sphere," the same official said. Kagame's government has attempted to change inter-ethnic preferences through a package of measures including direct indoctrination through the media, the rewriting of ethnic, colonial and genocide history in school textbooks, as well as the enforced social interaction and solidarity building through programs such as *Itorero* (civic education) or *Umuganda* (community building projects). Consistent with this package of policies, the government has made the mere mention of ethnicity illegal in public discourse and has discontinued the usage of ethnic terms in the census.

Umuganda employs an unusual, compulsory rule: individuals from all ethnicities are required to get together once a month to collectively work on a project for the public good. The explicit objective has less to do with simply completing a project and more to do with "bringing together people living together in the same community" (Lawrence and Uwimbabazi 2013). This has been accompanied by a deliberate attempt to rewrite Rwandan history in re-education camps (*Ingando*) as well as (more recently) in primary school textbooks.[16]

Reporters Without Borders ranks Rwanda as one of the worst countries in the world in regard to freedom of the media, and the World Press Freedom Index ranks it 161st out of 179 countries. Any criticism of the government (especially with regard to Hutu-Tutsi relationships) has been dealt with severely, with reporters and newspapers such as *Umuseso* and *Umuco* being prosecuted under the Rwandan ethnic divisionism law. According to the 2010 *Commonwealth Observer Report*, "the media environment is characterized...by a culture

[16]For instance, Mgbako (2005) (p. 218) reports an interview with a former *Ingando* student who argued that "the colonialists brought these ideas so that they could strengthen their politics...What difference does it make whether you have a thin nose or a flat nose? After *Ingando* I identify only as Rwandan." Similarly, the Financial Times (06/14/2014) reported that recently textbooks have been rewritten to reflect the view that "the Hutu/Tutsi distinction is considered an invention of Belgian colonial rule."

of self-censorship, with high levels of reluctance by journalists to write reports criticizing the government, its policies or their implementation."

Radio is by far the "most important form of mass media in Rwanda" (Paluck 2009), and in rural areas, it often offers the *only* vehicle for news and information. However, radio broadcasting in Rwanda is limited in terms of its reach, programming, and point of view. Despite the presence of many transmission towers, the country's hilly topography means that radio coverage is quite patchy. Furthermore, despite the proliferation of radio stations in Rwanda over the past decade, the broadcast of news and information is largely confined to Radio Rwanda - the official radio station (Frère 2009). This is not just because Radio Rwanda has the widest geographic coverage, but also because widespread self-censorship has resulted in the other private radio stations focusing on entertainment programming.

Radio Rwanda is regarded as an instrument of state policy. For example, Waldorf (2007) quotes the Rwandan minister for information as saying, "the public radio and television are there for relaying the action of the government. The private media, rather, should be interested in other things, like music and entertainment."[17] Indeed, Rwandan government officials believe that radio has been so successful in promoting national unity and reconciliation that they now advocate using it to promote other aims, such as economic development.[18] Independent research has also confirmed the effectiveness of specific programs broadcast (e.g. evidence of the effectiveness of *Musekewya* (New Dawn)) on Radio Rwanda (Paluck 2009; Paluck and Green 2009).

We also collected independent information about the content broadcast by Radio Rwanda as well as other stations.[19] This simple exercise suggests that treating Radio Rwanda as

[17]This view is echoed by Frère (2013), who reports that "several radio monitoring projects (around the 2003, 2008 and 2010 elections) have demonstrated that the national radio station was blatantly imbalanced in its coverage of the RPF and its candidates."

[18]For instance, the director of Radio Rwanda said that radio has become "the real mass media for the people of Rwanda," and that "the ability of radio to unite people has to be leveraged to improve their well-being." Source: The New Times http://www.newtimes.co.rw/section/article/2015-02-16/186027

[19]We hired a Kinyarwanda-speaking research assistant to listen to radio over a four-week period, and to code over 50 hours of radio broadcasts in Rwanda. The research assistant, a Tutsi, had some flexibility to choose the times during which he listened to any given station. However, the research assistant was informed only that the project was about the radio sector in Rwanda, and, thus was unaware of the project's purpose.

Kagame's station is justified since it broadcasts news that is overwhelmingly in favor of the government, and helps to emphasize a new Rwandan identity instead of a tribal/ethnic identity (see figures A1 and A2). We find that Radio Rwanda is over 5 times more likely than private radio to discuss national identity positively, and accounts for nearly 83 percent of pro-government content even though it is only 61 percent of the content we sampled.

III. Experimental Protocol

In 2013-14 we collected and processed data from lab-in-the-field experiments and an associated survey that included 438 farmers from 52 collines (villages) in rural Rwanda (figure 1).[20] A team of eight Rwandan enumerators and a field manager conducted the survey and experiments in Rwanda. The enumerators were informed that the experiments were part of a study examining working conditions, cooperation and contracts in the agricultural sector (Blouin 2016). We chose villages on the basis of two criteria. The first criterion was the geographic suitability of the land for coffee production, which is a proxy for whether the village had a history of forced labor (Blouin 2016). The second criterion was whether the village was eligible for FARG, a government-initiated fund targeted to Tutsi survivors, and for which only Tutsi are eligible. FARG villages were selected in order to be able to differentiate between Hutu and Tutsi in the sample, without violating any government rules about directly asking individuals about their ethnicity.

The project was reviewed by both the University of Warwick Ethics Committee and the Rwandan government. Before we engaged in any pilots related to the project, we submitted our survey materials, experimental protocol, schedule and budget as well as other documents related to the project to the Rwandan government. These materials included a summary

He was instructed to listen to 30-minute segments for between two and three hours of radio per day, five days per week. Given the importance and 'reach' of Radio Rwanda, the research assistant spent 47.5% of his time listening to Radio Rwanda, and the rest of the time he listened to private radio stations. These include the following: FlashFM, Ikondera Info, Imbaraga FM, Ingando Star, Itahuka Radio, KT Radio, MagicFM, Radio 10, TR Rwanda

[20] Colline is the administrative term used to refer to a village in Rwanda. We also conducted data collection in Burundi. We do not use these data in the body of the paper but discuss their collection in the appendix.

of the proposed research project referred to above. The Rwandan Government gave us approval and provided our field team with a document granting us permission to conduct surveys in the districts that we had outlined. On completion of the research project, the government requested that we supply it a copy of our research findings (we did this in late 2013 and provided them with a preliminary version of Blouin (2016)). Prior to starting our work in a district our field manager typically had to review the document with a political or administrative representative from the district. They received a broad overview of the project (i.e. that we were studying attitudes and cooperation in the agricultural sector in rural Rwanda) and also informed that the project had been reviewed and approved by the Rwandan Government.

Prior to our arrival at each village, our field manager selected subjects randomly from a list of potential subjects made available by the political representative of the village.[21] All individuals who agreed to participate in the experiment were promised compensation equal to about half a day's average wage, and after being compensated for the various incentive-compatible tasks they typically earned more than a day's average wage. Not surprisingly, in the vast majority of villages we surveyed, everybody participated. In the few villages where some opted not to take part in the experiment, almost 90 percent of those selected agreed to participate. The survey and experiments were conducted in a community hall located in the district, and all subjects were provided free transportation to get them to and from the survey location. The total time taken for a subject to complete the experiments and survey was less than half a day.

In each case, our team of enumerators arrived at the survey site in four SUVs. While our team prepared materials for the experiment and survey, each vehicle picked up subjects

[21] One concern may be that exposure to Radio Rwanda, influenced the village representative's selection of subjects directly. We check for this in table A1, which looks at the effect of radio on the difference between characteristics of our sample and the average of those characteristics based on the last census. For ethnicity, we use the 1991 census. This exercise comes with the caveat that due to the genocide the ethnic makeup of any village in 1991 may be different than it is now. While one of the estimates is significant at the 10% level, we generally do not find much evidence to suggest that differential selection took place in Radio Rwanda regions.

from an agreed-upon location from four different regions of the district, and they were driven to a community hall in one of the villages to conduct the experiment. In some cases, we also selected subjects who lived in the same village where this community hall was located. Therefore, in any given data-collection session we gathered information from participants from four or five different villages.

As subjects arrived at the community hall, the enumerator handed them the consent document that also described the purpose of the study. This document was either read to them by the enumerator or (if they preferred) could be read by themselves.[22] Once they had agreed to participate, all subjects were given an I.D. tag, consisting of a letter and a number. The letter denoted the region that the subject was picked up from (ranging from A to E if subjects from five regions were at the session), and the number was a unique within-region identifier ranging from one to seven. The I.D. tags, which were assigned to each subject and pinned to his or her shirt, were randomly picked out of a bag by the enumerator. These I.D. cards were the basis for assignment of partnerships and treatments in the various lab experiments. People of the same letter were never matched together, but otherwise, ID tags were randomly matched to form partnerships.

We describe additional details of the protocol in the supplementary online appendix.

IV. Data

Once subjects entered the community hall they participated in a battery of lab-in-the-field experiments and an associated survey. For each subject we collected inter-ethnic attitudes using a measure of ethnic salience, a partner selection measure, two survey questions on trust, and data from the trust game, which we match to GIS data on radio signal. Summary statistics for each of the outcomes appear in table 1, panel B. Unconditional correlations between the main measures used throughout the analysis can be seen in table A2. We will discuss each in turn.

[22] All subjects were informed that the study was interested in assessing cooperation and work in rural Rwanda in the agricultural sector.

A. Measurement of Inter-Ethnic Attitudes

i) Salience of Identity Test (SIT): We are interested in measuring how individuals mentally categorize others. Taylor et al. (1978) and Taylor and Fiske (1978) argued that such categorization is a fundamental aspect of social cognition in complex environments.[23] They developed a test to assess whether individuals categorize on a dimension of interest, such as race or gender. We adapted their test for the Rwandan context.[24]

The enumerator randomly arranged eight photographs on a table. The photographs were of eight males and included four Hutu and four Tutsi (see figure 2 for an illustration). In the first phase of the experiment, all subjects were asked to pay attention while the enumerator picked up a particular photograph and read a neutral statement about the individual depicted in that photograph.[25] After reading the statement, the enumerator placed the photograph back on the table and picked up the next photograph and again, read the associated paired statement. This process was repeated for all eight photographs. This first phase of the experiment took approximately three minutes. After a break of a few minutes, the enumerator implemented the second phase of the experiment. In this phase, each subject was informed that there was a surprise recall task. In particular, the enumerator informed subjects that one of the statements that had been read in the first phase would be read back to them. The subject had then to match it with the appropriate photograph. If the subject was unsure about the photograph associated with a statement, they were asked to take their best guess. The subjects were informed that each correct pairing of the statement to a photograph would be rewarded at the rate of 100RWF.

Typically, subjects matched some statements and photographs correctly, and also made errors. The key issue for our research stemmed from the errors. In particular, we examined

[23]The original Taylor et al. (1978) experiment was used to study whether individuals encode race. The importance of such categorization for social cognition has been further explored by Stangor et al. (1992) and Kurzban et al. (2001).

[24]Any such categorization presumes that there are discernible physical/genetic differences between the Hutu and the Tutsi. We discuss at some length in Section IV.C.

[25]As an example of one of a neutral statement: "This person likes to go for long walks."

whether or not subjects were more likely to confuse one ethnicity for another. For example, suppose an individual in the recall phase of the experiment matched a statement that was paired with the photo of a Tutsi to the wrong photograph. If a subject more frequently misattributed the statements associated with a Tutsi to another Tutsi, then according to our measure, ethnicity was more salient for that subject. That is, plausibly, more within-ethnicity errors are correlated with the salience of ethnicity. We measure ethnic salience for a given subject by dividing the subject's total number of within-ethnicity errors by her total number of errors, thus normalizing for variation across subjects in the overall error rate.[26] In practice, all subjects made at least one error, so this measure was always well-defined.

This particular measure of ethnic salience has several features that are especially valuable in our context. One is that it is an unobtrusive measure that captures in a simple way how a subject processes and categorizes information without having to question them about their ethnicity.[27] Another is that instructions are easy for the enumerator to convey. The experiment does not require subjects to be literate or comfortable with tablets or computers. A final noteworthy feature is that the elicitation is incentive-compatible.

Note that not all within-ethnicity errors suggest that ethnicity is salient. These errors arise even if the individual answered randomly. To account for this we define two alternative measures of ethnic salience. The first attempts to account for the fact that ethnic salience is more reliably inferred when individuals make a larger number of within-ethnicity errors. So it may be argued that such individuals should be given more weight. Accordingly, we also define a quadratic measure of ethnic salience given by the ratio of the square of the number of within-ethnicity errors to the total number of errors, which we refer to as the alternative SIT. A final measure relies on the fact that if individuals were simply choosing

[26]Normalizing for the total number of errors is important because a person may make fewer errors because of higher mental ability, or due to Taylor et al. (1978)'s observation that an individual "may select salient social or physical dimensions...for grouping and managing personal information." In other words, while a subject for whom ethnicity is more salient is more likely to confuse one Tutsi for another, it is possible that he will make fewer overall errors because he categorizes by ethnicity.

[27]This is in contrast to the Implicit Association Test (Greenwald and Banaji 1995), which highlights ethnic categories.

photographs randomly, about 42% of their errors would be within-ethnicity. Accordingly, we also examine a binary variable that is equal to one if people make more than 42% of their errors within-ethnicity and zero otherwise.

ii) Partner Selection Task: At the end of the survey, all subjects engaged in a partner selection task (similar to Rao (2015)). All subjects were informed that they had to select five individuals with whom they would prefer to engage in a cooperative task that required them to spend time with the selected person. They chose from the set of twenty individuals at the session, with the caveat that they could not choose anyone they knew or anyone that lived nearby (as denoted by the letter on their ID tag).[28] Subjects chose prospective partners on the basis of looking around the room and at the ID tags of others at the session. So this selection may have been on the basis of observable characteristics such as (among other things) gender, clothing, height, age, and ethnicity. We ensured that these partner selections were incentive compatible by informing subjects that two of them (from each session of 20) would be chosen to be partnered with one of their choices in a separate unrelated task.[29]

We construct three measures of willingness to socially interact with those of a different ethnicity. Our main measure is given by:

$$\text{Preference for inter-ethnic partner} = \frac{\text{number of choices from other ethnic group}}{min\{5, \text{total other ethnic group}\}}. \quad (1)$$

Here the numerator is simply a count of the number of choices that are from the other ethnic group. The denominator accounts for the fact that in some sessions, for example, a Hutu has the option of choosing five Tutsi, while in some other sessions fewer Tutsi may be available. We also experiment with an alternative measure that replaces the numerator in equation (1)

[28]The survey question outlined to participants that in a specific task (they were told the last one of the day, so they knew which task it was) some participants would be chosen to partner with someone on their list, and that the nature of the task was such that if they could not co-operate well with their choice that they may earn less money. They did not know that their choices themselves were part of the data collection effort, from their perspective the choices were an intermediary step for the completion of another task.

[29]The other task was used to collect data for an unrelated project. Controlling for whether subjects were selected to be matched to their choices in this unrelated task did not affect any of our results (table A3).

with its square and a third measure that is a function of only the numerator.

iii) Trust Survey Questions As part of the survey, subjects answered two questions about trust. Given government-imposed restrictions, we were unable to directly ask about inter-ethnic trust. As a substitute we asked individuals about trust of people from *their own community* and about trust of people from *the other community.* Specifically the questions ask: "How much do you trust people from other communities in your village" and "How much you trust people from your own community in your village?" where responses were on a four-point scale (i.e. Not at all/Just a little/Somewhat/A lot).

We stress that this variable should be interpreted with caution for several reasons. First, we do not know how subjects interpreted the word community. Some may have considered it to mean family and friends; others as colleagues or other members of their cooperative; and others still, may have interpreted it as an ethnic group. Second, the ethnic make-up of the community can itself be affected by inter-ethnic attitudes in the first place. Finally, even if subjects, for the most part, did interpret the question as a veiled question about ethnic preferences, we still have to consider the possibility that people in Radio Rwanda-receiving villages just know what they are supposed to say. In other words, one possibility is that people in Radio Rwanda's broadcast regions are more sensitive to appearing as though they favor equality, and respond accordingly when asked about it directly.

iv) Trust Game We also had all subjects play a face-to-face, one-shot trust game. An enumerator used a dice roll to randomly assign all subjects to play either the private or the public version of the trust game. The primary difference between the two treatments was that in the public version of the game a decision made in the game would be written on a poster board where other subjects at the session could see how they played. One of the enumerators was tasked with ensuring this took place throughout the day. The aim of the public version was to examine whether fear of being identified as having low trust may influence behavior (especially in inter-ethnic relationships). For instance, if a subject fears

that choosing an action may be disapproved of by her community or the local government (which is plausible given the presence of informants), then she may choose to cooperate and make a high-trust offer.[30] In every case an enumerator ensured that all partners were not only drawn from different villages, but also had never met.[31] In the private information treatment, the enumerator informed the two subjects that decisions made by the two of them in the game would be kept confidential and not publicly announced by being written on the poster board as in some of the other games.

One participant was randomly designated as player 1 (the sender) and the other was player 2 (the receiver). The sequence of moves was described to both of them, prior to the implementation of the game. Player 1 was given an endowment of approximately a day's wage, or 600RWF and was instructed to transfer as much as (s)he wanted to player 2 and keep the rest. The enumerator matched the amount transferred and then Player 2 chose to keep as much as (s)he wanted and transferred the rest to Player 1. We examine whether or not subjects in villages receiving a Radio Rwanda signal make higher trust game offers in the private trust game when interacting with a person of another ethnicity.

One important caveat is that the trust game may capture things other than trust. For example, the trust game may measure other-regarding preferences (Cox 2004; Ashraf et al. 2006); the actual trustworthiness of the receiver (rather than perceived trustworthiness); or preferences towards risk (Karlan 2005; Schechter 2007). We attempt to address these confounds in various ways, such as by examining return offers as a proxy for other-regarding preferences and by including controls for risk preference in our analysis. We stress, however, that if these factors are affected by receipt of Radio Rwanda, they may explain a portion of the effects we attribute to trust.

[30]Despite a large literature on trust games, surveyed in Sapienza et al. (2013) and Johnson and Mislin (2011), we are not aware of any literature on the public version of the trust game as described by us here. However, a literature that explores third-party punishment closely relates to our work. For example, Balafoutas et al. (2014) show that about 35% of third parties in a one-shot trust game are willing to punish low trust offers despite having no strategic incentive to do so. Charness et al. (2008) show that the threat of third party punishment makes trust game offers 60% larger, a much larger effect than what we find.

[31]They were both asked independently if they had ever met their partner prior to being designated as partners.

At the time that subjects were randomly matched to others at their session to play the trust game, we had not yet inferred their ethnicities. Therefore, some of these subjects randomly found themselves playing the trust game with a co-ethnic partner and others with an inter-ethnic partner.

B. Validity of the SIT

The SIT measure, while new to the economics literature, has been extensively used in cognitive psychology. The first application is from Taylor and Fiske (1978) who show that some features about an individual's recall are facilitated by the encoding of features that are salient. This idea was further explored by Stangor et al. (1992) who examined conditions under which race or sex was salient. A more recent application is Kurzban et al. (2001) who show that when shirt colour is made a more salient feature of group membership, race becomes less salient.

We want to ensure that our measure of ethnic salience is associated with measures of actual behavior. For instance, individuals with lower ethnic salience should be expected to display similar levels of trust toward those of the other ethnicity as they do toward members of their own group. In other words, we should expect people with lower levels of ethnic salience to make higher offers in inter-ethnic matches of the private trust game. This is indeed what table A4, panel A reveals. Columns 1 and 3 show that individuals for whom ethnicity is salient (i.e. high SIT scores) are those that make lower offers in private information inter-ethnic trust games. Similarly, columns 2 and 4 illustrate the fact that, as expected, ethnic salience is not associated with differences in co-ethnic behavior in the same way as inter-ethnic behavior.

The dynamic is different in public game trust offers (table A4, panel B). Here we observe that there is no clear difference between the co-ethnic and inter-ethnic offers between people with high and low ethnic salience. Furthermore, people with high ethnic salience seem more sensitive to public information in the inter-ethnic games (see the Equality of Coefficients:

Private - Public (p-value) in panel B).

C. Ethnicity and the Collection of Data

In Rwanda, it is not feasible (or permissible) to directly ask individuals their ethnicity. The key to resolving this challenge was our discovery that information on ethnicity could be indirectly inferred by using income surveys that ask subjects their sources of income. In particular, individuals in Rwanda may receive funds from the government under a variety of different programs, including the FARG program. However, the FARG program is exclusively available to survivors (as defined by the government) of the 1994 genocide - a category that coincides almost perfectly with the Tutsi wherever FARG gives money.[32] Not only is the Rwandan government very strict about the criteria for FARG eligibility, it has also defined the term *survivors* in a political way such that it coincides with the Tutsi ethnicity and excludes any Hutu who died during the genocide.

For example, article 3 of the law establishing FARG outlines *Definitions of terms*, and specifically defines *survivors* as "survivors of the Genocide against the Tutsi" while *Fund* is specifically defined as "Fund for the support and assistance to the Tutsi survivors of the genocide" (Law No. 69/2008 of 30/12/2008).[33] Crucially, eligibility hinges entirely on being a "survivor."[34] This singular eligibility criterion (given locality) is especially important because the word "survivor" is both officially interpreted and commonly understood as "Tutsi". Similarly the word "génocidaire" is officially interpreted and commonly understood

[32]Accordingly, we asked participants in Rwanda their sources of income from various aid programs (including FARG). For each aid program they were asked whether they were (a) eligible for it, and (b) received money from it. In order to make the question on FARG eligibility less salient, the section about FARG eligibility was part of a subsection about income sources from government funds, which in turn was part of a larger income module.

[33]International human rights agencies define someone as a victim of the genocide (and hence family members are genocide *survivors*) if they died in Rwanda due to violence between April 7th and July 15th, 1994, regardless of ethnicity. However, under the Rwandan government definition, only Tutsi can be considered genocide victims. The genocide has been relabeled as the "genocide against the Tutsi" (Ferstman et al. 2009).

[34]see: http://socialprotection.org/programme/genocide-survivors-support-and-assistance-fund-farg (last accessed November 6th 2017), which lists the only eligibility criterion being "...that she or he is a genocide survivor."

as "Hutu" (Thomson 2011b, p. 111). The implication of this is that those Tutsi still alive are *eligible* for FARG even if they were not directly impacted by the genocide.

Our inability to even mention or otherwise identify an individual's ethnicity raises the question of whether the subjects in our experiments know each other's ethnicity by physical appearance alone - particularly given that the people are drawn from different villages and have never met each other.

As documented by a number of genetic studies, there are clear genetic differences between the Hutu and the Tutsi, with the latter having many more Nilo-Saharan markers (Luis et al. 2004; Shepard and Herrera 2006). Of course, it is possible that genetic differences may not manifest themselves into discernible physical characteristics. Nevertheless, as described by Gourevitch (1998) (p.50) "nobody can dispute the physical archetypes: for Hutu, stocky and round-faced, dark-skinned and flat nosed, thick lipped and square-jawed; for Tutsi, lanky and long-faced, not so dark-skinned, narrow-nosed, thin-lipped and narrow-chinned." While these stereotypical physical characteristics do exist, there are also (as pointed out by Gourevitch (1998) and others) likely to be many exceptions of individuals who are not easy to classify neatly into these ethnic categories. This suggests that misattribution of someone's ethnicity is possible. In section VII.B, we further investigate this issue when we examine the robustness of the results.

D. Radio Signal and the Radio Listenership Survey

Given Rwanda's mountainous topography, we exploit local variation in the reception of government radio broadcasts (as in Yanagizawa-Drott (2014)). Indeed, this mountainous topography is such that despite the presence of 27 radio transmission towers (see table A5), there exist many pockets of rural Rwanda that do not receive a good signal - as illustrated in figure 3. Our main identification strategy compares regions within districts of Rwanda that do receive a good quality Radio Rwanda signal, with those that do not. This heterogeneity is such that even relatively small administrative units (such as districts) usually contain

villages both with and without a good radio signal. Indeed, the empirical exercise relies on differences in inter-ethnic behavior between precisely these villages.

Using radio tower location and topographical information, we calculate whether or not a village received a radio signal. The signal strength data are calculated using the ITM (Irregular Terrain Model) based on radio transmitter location, frequency, and power from FMScan.org. We match signal strength in each village to the villages of each subject, which are acquired from the survey. We compute signal strength in db/μ and compute a binary variable indicating whether a village gets reception if the signal strength in that village is at least 45db/μ (Figueiras and Frattasi 2010).[35] This threshold is based on audible radio reception using a low-quality receiver as would be typically found in Rwanda. We will examine robustness to alternative thresholds.

We corroborate our radio signal measure with actual radio ownership and listenership patterns across regions exposed to Radio Rwanda broadcasts and not exposed. These data come from two sources: the first is based on a follow-up survey of a subset of our subjects, completed by April 2017; and the second is from the Rwandan DHS survey, conducted in 2014. The follow-up survey provides us with a measure of signal strength at a subject's home (the measure ranges from 3-9 in the data); as well as whether the subject regularly listens to Radio Rwanda, or any other station.[36] The DHS data include a question on radio ownership and has the advantage of being nationally representative.

[35]This is consistent with the methodology described in Hoeg and Lauterbach (2004) who argue that if the coverage in a location is at least 46db/u then we should expect 99% of the area in the $1km^2$ radius to have 30+ signal strength (p. 248). A detailed description of how we match our village locations to radio signals can be found in Appendix C.

[36]The follow-up survey was conducted over the phone. Hence it automatically constrained our sample to the subset of participants who had an assigned cell phone number in 2013 - which need not be representative of the broader population. For example, in 2010 Bjorkegren (2015) reports that 84% of mobile phone owners owned a radio while only 63% of the general population did. Furthermore, perhaps due to changes in the media landscape as well as the entry of new cell phone providers, there has been attrition in our original sample of cell phone users. MTN is the dominant company, but one major player lost its license just before we arrived, and Tigo, a newer entrant has gained substantial market share since we were last there. On the other hand, Stork and Stork (2008) completed a representative survey of Rwanda and report the mean number of hours per day listening to the radio in rural areas is 4.37 while in our data it is 4.16. At least based on this narrow measure, data from our survey seems somewhat similar. We end up with 154 subjects for our follow-up survey, drawn from 48 villages.

We used this survey on radio listenership to evaluate alternative definitions of radio reception at the village level. The resolution of the radio signal data is quite high, so within any village we typically observe radio signals for three to five geographic pixels.[37] Given this, we consider four ways to measure whether or not a village received a signal from a radio station (denoted i, ii, iii, iv). A first option (i) is to take the fraction of pixels that we believe receives a signal. This would provide us with the share of coverage within a village, so that the higher the share, the more likely it is for anyone in the village to be able to listen from home. A second approach is to construct a binary measure that equals one if a certain threshold of pixels within the village receives a signal. Here there are three distinct thresholds that we evaluate. These include (ii) whether a simple majority (i.e. $> 50\%$) or (iii) a large majority (e.g. $> 75\%$) of pixels within the village receive a signal. Finally, we also evaluate (iv) whether the threshold should be $> 20\%$. Since there are between 3 and 5 pixels per village this captures whether any one pixel receives a signal. This latter definition is attractive since it might capture informational spillovers in communities with thick social networks. This is a crucial consideration, especially since it is quite common for people to seek out a place to listen, say at the homes of friends or family. As many as 20% of people in Rwanda primarily listen to radio outside of their home (Stork and Stork 2008). Furthermore, in that data, 28% of people report not owning a radio, but 99.9% report listening to radio regularly.

We check each of these four definitions of village level reception, using various different radio thresholds to see which is most relevant in terms of actual listening habits. First, we find that across all of our methods of assigning a signal to a village, 45db/μ is the most relevant threshold. Table A6 shows that this threshold provides the largest estimate when we regress listening regularity on village reception, regardless of how reception is defined (i.e. in each of the four panels). Of the four methods of assigning reception to a village (fraction of village receiving a signal; thresholds at $> 20\%$, $> 50\%$, $> 75\%$) the two that seem most

[37] Our data provides us with coordinates for the centre of each village, and around each, we draw a circle with radius of approximately 2km to mark approximate village boundaries.

relevant are the threshold at 20% and the fraction of the village receiving a signal. In both of these cases there is a significant effect of reception on listening at both the 45 and 50 db/μ thresholds, with the 45 threshold being more precise in each case. For our main results we opt for the former (threshold of $> 20\%$) because of the reported prevalence of Rwandans seeking-out a signal away from home. However, our results are also robust to using the fraction of pixels in a village receiving a signal, as we show in table A7.

V. Empirical Strategy

We are interested in examining the impact on inter-ethnic trust and ethnic salience of living in a region that is exposed to propaganda from Radio Rwanda. By the end of 2013, there were 27 radio transmission towers (mostly built recently) that broadcast Radio Rwanda. Given the topography, this resulted in radio coverage of about 48 percent of our sample (table 1, panel A).

A. Main Specification

We employ two main strategies to investigate differences in inter-ethnic attitudes between those living in villages that do and do not receive Radio Rwanda. For the first strategy, which we use for the SIT and partner selection measures, we simply regress these outcomes on a dummy variable indicating whether the subject's village is able to receive a Radio Rwanda signal. For the second strategy, which we use for the trust measures, we separately regress inter-ethnic and co-ethnic trust on an indicator for receipt of Radio Rwanda, and test the equality of the coefficients between the two trust outcomes. More formally, we implement the following two main tests:

$$\phi_{ivd} = \beta_0 + \alpha_d + \beta_1 RadioRwanda_{vd} + \Gamma' X_{ivd} + \epsilon_{ivd} \tag{2}$$

and

$$\textit{Inter-Ethnic Trust}_{ivd} = \lambda_0 + \kappa_d + \lambda_1 RadioRwanda_{vd} + \Omega' X_{ivd} + \upsilon_{ivd} \tag{3}$$

$$\textit{Co-Ethnic Trust}_{ivd} = \rho_0 + \omega_d + \rho_1 RadioRwanda_{vd} + \Theta' X_{ivd} + \varepsilon_{ivd}$$

$$H_0 : \lambda_1 - \rho_1 = 0$$

Here subscripts i, v and d denote an individual, village and district respectively. In equation (2) ϕ_{ivd} is a generic outcome that can be either the SIT score of the individual or the partner-selection variable. α_d denotes district fixed-effects; we have data on individuals from eight different districts and 52 different villages. $RadioRwanda_{vd}$ is a binary variable that indicates that the subject's village receives a signal of greater than 45db/μ and X_{ivd} is a vector of controls.

We have controls that vary at either the individual or the village level. At the individual level, there is information on gender, age, our proxy for ethnicity, an aptitude test score (the Raven test), education, income (which we include the logarithm of), and enumerator fixed-effects.[38] At the village level, controls are included for whether the village historically received an RTLM signal (the signal for the hate radio station that incited genocide violence); the light density in the village at night;[39] the distance to Kigali, to the nearest road, and to the nearest major city; the level of genocide violence; the elevation of the village; the variance in elevation of the village's sector; the ethnic composition; and indicators for whether the village faced north, south, east, or west. Additionally, since identification should rely only on the variation of the topography of Rwanda, variables for the Euclidian distance to the nearest three Radio Rwanda towers are included, as well as variables for the travel-time to

[38] One concern could be whether the identity of the enumerators affects how subjects behave in the various experiments. To deal with this concern, enumerator fixed effects appear in all of the empirical specifications. However, adding these enumerator fixed effects make little difference to any of the estimates (table A26).

[39] This may capture local income but may also capture proximity to a nearby city. Both may indicate economic activity which may be associated with increased inter-ethnic encounters, we are agnostic about specifically how. In any event, the results do not differ substantially without this control (table A8).

the nearest three towers.[40] The conditional correlations between all of our controls and all of our outcomes in villages with and without Radio Rwanda can be seen in table A9.

We use the same set of controls in equation (3), which examines co-ethnic and inter-ethnic trust as outcomes. Here we have two measures of both inter-ethnic as well as co-ethnic trust. The first measure draws on responses to the trust survey questions, and the second measure is given by offers made in the trust game. For the trust game specifications, we include an additional set of controls for the experimental conditions. These controls include the gender of the assigned partner, an indicator for whether the partner also lives in a village that receives Radio Rwanda, as well as the distance between villages of the sender and receiver in the trust game. We also include the trust-game enumerator fixed effects.[41] In addition, we had a risk preference measure to control for the fact that risk preferences can confound the trust-based interpretation of trust game offers (Sapienza et al. 2013).[42] In principle, none of our subjects should know where other participants in the experiment came from, since we disallowed communication between subjects from different villages.[43] Nevertheless, we control for these regional characteristics in case there are non-verbal cues that indicate regional differences.

Our hypothesis is that in equation (2) $\beta_1 < 0$ for the SIT measure and $\beta_1 > 0$ for the partner-selection task measure. In equation (3) we expect that $\lambda_1 > 0$ but that $\rho_1 \approx 0$, so we test for the equality of λ_1 and ρ_1.

In the trust game, we have two types of inter-ethnic trust, public and private trust. When we use the trust-game data to estimate equation (3) we further split the sample according

[40] Travel time is computed by scraping Google Maps using Python. For this we went through the Google Maps API.

[41] We do not have enumerator fixed effects for the partner selection task and the SIT, since everyone had the same enumerator for these tasks.

[42] Sapienza et al. (2013) point out that other regarding preferences can confound trust game estimates. In our context, however, we do not see that as inconsistent with the main hypothesis, which is that Hutu-Tutsi relations are better in villages that receive Radio Rwanda. Given our broader focus on nation building and inter-ethnic attitudes, rather than exclusively on trust per se, whether the improvement is due to trust or altruism is a secondary concern.

[43] Participants from the same region arrived together in the same SUV, but these people were not allowed to partner with each other.

to whether the subject played the public or private version of the trust game. Here, ex-ante we thought both an increase in public and private trust were plausible. Both were measured with the aim of observing whether differences in inter-ethnic behavior were due to differences in fear of having one's mistrust exposed (public trust game), or due to actual differences in attitudes, beliefs or preferences (private trust game). Of course, from the subject's perspective, even in the private game there is some risk that the information will be made public. We assume that the perceived probability of the trust offer being made public is higher in the public trust game.

B. Identifying Assumptions

An important assumption for our analysis is that those living in villages receiving a Radio Rwanda signal are more likely to listen to Radio Rwanda. Here the data on radio ownership and the follow-up survey on listenership patterns provide a consistent picture. We present the results in table 2, panel A. We find that individuals located in the catchment area of Radio Rwanda are more likely to own a radio, much more likely to receive a high-quality radio signal, and 40% more likely to listen to Radio Rwanda regularly. Our follow-up survey also reveals that all but one of our 154 subjects consider Radio Rwanda a credible source of news and information.

A second assumption is that access to Radio Rwanda is not correlated, conditional on our controls, with other determinants of the outcomes that we study. Our identification strategy relies on idiosyncratic variation in access to radio transmission due to the topography. Given that radio transmission is affected by (poorly understood) fluctuations in temperature, air pressure, rainfall and humidity (Luomala and Hakala 2015), any targeting would likely occur at a broader level than the village. This is why we control for district fixed effects throughout our analysis.[44]

[44] We also show that our main results are robust to a model with fewer controls but with sector fixed effects instead of district fixed effects. While we lose degrees of freedom in the model with full-controls and sector fixed-effects, the sector-level model implicitly controls for most of the geographic variation due to the fact that sectors in Rwanda are so small. For these results, see table A11.

Our main balancing test includes district fixed effects as well as distance to the nearest big city, road, and Kigali. These results - for both Radio Rwanda reception and assignment to the public treatment - are found in table 2, panels B-D. In each row, we test whether radio reception or the public treatment explains various different variables that may have the potential to be relevant for the results. Panel B examines the baseline control variables,[45] while panel C examines the full controls, and panel D shows balancing on other variables of interest. In panels B and C we find that reception of RTLM and the religious radio station *Maria-Nyina wa Zombo*, as well as distance to Kigali predict Radio Rwanda signal, while RTLM, north facing and south facing villages seem to predict assignment to the public treatment.

In panel D we investigate other stations and colonial history (because they are used in our robustness exercises), and migration (because it is a potential confound).[46] None of these estimates are statistically distinguishable from zero either for Radio Rwanda or for assignment to the public trust game. Overall, we test balancing with 66 different estimates and find only six that are statistically different from zero at the 10% level (three each for Radio Rwanda and the public treatment).

VI. Results

A. Erasing Ethnicity: The Salience of Identity Test

We begin by examining the relationship between Radio Rwanda and the measure for the salience of ethnic identity (SIT). Differences in the salience of ethnicity by Radio Rwanda reception can be seen in table 3 and figure 4.

Columns 1-3 of table 3 show that SIT scores are lower in Radio Rwanda villages, under different specifications. Column 1 presents estimates from a regression using the baseline

[45] All baseline control variables are included with the exception of the distance to tower variables, which are mechanically correlated with signal strength.

[46] Note that Radio Rwanda only began focusing on the new-Rwanda identity when Kagame took political control just over a decade ago. Since that time, only 3.7% of the subjects in our sample have migrated (table 1).

specification, which includes all of the controls listed in table 1 panel C. It suggests that people from villages that receive a Radio Rwanda signal, score about 13 percentage points lower on average than villages without reception, which translates to a difference of about 0.46 standard deviations. This implies that for subjects living in Radio Rwanda villages, ethnicity is less salient. To further account for the fact that one within-ethnicity error out of one total error is likely much different than five within-ethnicity errors out of five total errors,[47] in column 2, we add fixed effects for the number of mistakes made, and the estimate is robust. With additional controls, the estimate again remains similar, at just over 10 percentage points (column 3).

Each of these regressions is based on an assumption that the radio signal is audible if the village receives a signal strength of more than 45 db/μ, and not otherwise. Of course this likely varies from day to day, and may also differ based on the quality of the radio owned by an individual. Indeed, in our follow-up survey, many subjects in regions without Radio Rwanda indicate that they sometimes receive a radio signal, even if it is inconsistent and difficult to hear through the static. Since it seems reasonable that a binary measure may not perfectly capture the heterogeneity in radio reception, we examine robustness of our results to alternative thresholds of signal strength. These are depicted in figure 4, which plots estimates for many different thresholds of receiving a radio signal at 1db intervals.[48] Consistent with table 3, there is a large and negative effect at the 45db/μ threshold, which is denoted with a dashed line. There are negative and significant estimates, albeit smaller in absolute magnitude, surrounding our main estimate, consistent with the fact that the 45db/μ threshold is not a sharp cut-off.[49] Given this consideration, the U-pattern that we observe is what we should expect if 45db/μ is an appropriate cut-off to consider, and if people in

[47]For example, see table A12 which suggests that individuals in Radio Rwanda villages may make more mistakes.

[48]In the figures we use the baseline set of controls. We present the full set of controls in the online appendix, figure A3.

[49]Instead some people are willing to listen even if reception is poor, while others are probably able to offset part of the effect of poor reception with higher quality radios. Furthermore, the 45db/μ threshold is meant to capture a location that receives a very consistent signal. Many villages with a lower reception will still receive Radio Rwanda when the weather is good, for example.

villages that receive Radio Rwanda do in fact have lower salience of ethnicity. Table A13 also presents estimates showing the robustness (using each of the four main outcomes) to a continuous measure or radio signal strength.

Columns 4 and 5 present robustness to using our two alternative measures of the salience of ethnic identity.

B. Ethnicity and the Selection of Partners: the Partner-Selection Task

The partner-selection measure allows us to examine whether living in villages exposed to radio programming encourages face-to-face inter-ethnic social interaction. Regressions using this measure are examined in table 4. Columns 1-2 show that subjects in villages that receive Radio Rwanda are about 15-17 percentage points, or 0.51-0.58 standard deviations, more likely to request partnering with someone from another ethnic group. The result is robust to alternative specifications and constructions of the outcome measure. In particular, the effect is similar using a quadratic measure that gives more weight to people that chose more partners from the other group (column 3) as well as a simple count of choices from the other group (column 4). Moreover, it seems unlikely that ethnicity is simply proxying for income - since in table A14 we do not observe any greater preference for those with higher incomes. Figure 5 also shows results from the main placebo exercise. The figure plots a wide range of signal thresholds at 1db intervals. As in the case with the SIT, here, too, we would expect the largest effects for the signals around 45db/μ. Once again, the estimates generate the expected inverse-U pattern. The evidence from both table 4 and figure 5 suggest that living in villages exposed to Radio Rwanda may have played some role in making people more willing to engage with members of the other ethnic group.

C. Inter-Ethnic Trust (I): Evidence from the Survey

Given the importance of inter-ethnic trust for any form of nation building, we measure inter-ethnic trust in different ways. First we report results from a survey question that asked

subjects whether they trusted members of the "other community" in their village. We also asked about members of a subject's own community, with the hypothesis that *only* out-group trust might be higher in villages that receive a Radio Rwanda signal.

Estimates based on these two survey measures can be seen in table 5. The model with the baseline set of controls can be seen in columns 1 and 2, and the full set in columns 3 and 4. Columns 1 and 3 show out-community trust, while columns 2 and 4 show in-community trust. The hypothesized out-group effect seems plausible, as the out-group trust estimates are both much larger than their in-group counterparts, and are the only estimates that are statistically different from zero. The estimate on out-group trust is about 0.25 of a point on a four-point Likert scale, or about 0.35 standard deviations. This essentially closes the gap in trust between the in-group and out-group (control group means are 3.17 and 2.92 respectively for a difference of 0.25). However, perhaps more relevant is whether the out-group estimate is statistically different from the in-group estimate. Accordingly, the table also reports the p-value that tests for the equality of the coefficients in columns 1 and 2, as well as the equality of the estimates in 3 and 4. With the baseline set of controls one is able to reject equality of the coefficients at the ten percent level ($p = 0.068$) and there is a significant difference with the full set of controls at the 1 percent level ($p = 0.005$).

However, observe that the placebo tests do not provide as much confidence regarding this measure as they did for the previous two measures. Figure 6, shows estimates from regressions defining radio reception at different signal strength thresholds. Observe that several of the thresholds that are unlikely to be capturing differences in Radio Rwanda reception, nevertheless produce fairly large estimates.

D. Inter-Ethnic Trust (II): Evidence from the Trust Game

We depict trust-game offers by radio status and partner-type (inter-ethnic versus co-ethnic) in figure 8 panel A. Here we observe that overall levels of trust (both co-ethnic and inter-ethnic) appear lower in radio regions than non-radio regions. However, this pattern is re-

versed and inter-ethnic offers are higher in radio regions, if we condition on a basic set of controls, be it the baseline set or the full-set or even district fixed effects and distance to various locations (as in table 2). This graph is pictured in figure 8 panel B. In addition, we note that in regions not exposed to Radio Rwanda, average inter-ethnic trust offers are lower than co-ethnic offers. However, exposure to Radio Rwanda reverses this and we observe that inter-ethnic trust offers are marginally higher. Indeed this is unusual and is the only example we know of with higher offers in inter-ethnic relative to co-ethnic trust games.

We now discuss the private version of the trust game before we turn to the public treatment. Given that we also randomly assigned subjects to play the private- and public-information treatments, we had a sample of 92 subjects who played the inter-ethnic, private trust game; 150 subjects who played the co-ethnic, private trust game; 71 individuals who played the inter-ethnic, public trust game; and 125 individuals who played the co-ethnic, public trust game.

i) The Private Trust Game We first examine whether subjects in Radio Rwanda villages make higher inter-ethnic trust offers in the private treatment. Panel A columns 1-6 of table 6 shows the main private trust game result: subjects receiving a Radio Rwanda transmission make higher trust game offers, in the inter-ethnic variant of the game, of over 145RWF out of a largest possible offer of 600RWF (columns 1 & 3), or about 1.13 standard deviations. This corresponds to offers that are about 47% higher on average (column 5).

It is worth emphasizing that living in villages that receive radio seems to have a very different impact on inter-ethnic trust game offers than on trust game offers between members of the same ethnic group - see columns 2, 4 & 6. Indeed, the equality of the inter-ethnic and co-ethnic coefficients can be rejected at the 1% level for each specification. Consistent with our hypothesis, estimates of differences in inter-ethnic offers are much larger than differences in co-ethnic offers.

Figure 7 shows our placebo exercise. We find the expected inverted U pattern.

ii) The Public Trust Game The results from the public trust game are described in panel B of table 6.[50] Observe that public trust game offers are *not* higher among those exposed to Radio Rwanda, in either the inter-ethnic or co-ethnic samples. Furthermore, the estimates from the public trust game are not only smaller than their private trust game counterparts, but they are significantly so (see the Equality of Coefficients: Private - Public (p-value) in panel B of table 6). One possible reason for this may be that government programs other than Radio Rwanda already induce subjects to change their behavior in public, so that the additional effect of Radio Rwanda is minimal. Indeed, an examination of inter-ethnic trust game offers in public relative to private games (table A17, columns 1-6) suggests that public treatment offers may be different from private ones only in villages *not* receiving a Radio Rwanda signal (though the effect is not always significant). Furthermore, the public treatment appears to have had little impact on co-ethnic offers in either type of village (panel B, table A17).

A sharper way of illustrating the impact of information on private and public trust offers is depicted in figure 9. Here the x-axis in each panel measures the strength of the radio signal (conditional on other village observables) and the y-axis measures trust game offers (also conditional). Note the difference in the slope of the line between the two panels.

One concern however, is that the perceived probability that the private game is actually private varies between regions that receive a Radio Rwanda signal and those that do not. In this case the private game would not be able to differentiate between differences in the attitudes of subjects, and their perceived risk that people will report their behavior to others. Note that the SIT measure seems free of that concern, since the subjects likely believed the task was a test of memory and not ethnic salience.[51]

[50] We again split the sample and examine p-values between estimates. However, in the appendix we also provide the pooled sample, see table A15

[51] We also use data from Blouin (2016) on contractual defaults in the agricultural sector to further examine this issue. One question in that data was how individuals responded to a contractual default, and whether they report complaints to the local authorities and/or others in the village community. However we find no difference between villages that receive a good Radio Rwanda signal and those that do not in this propensity to report their contractual grievances - as can be seen in table A16.

E. Other Radio Stations

Under President Kagame, the Rwandan government controls the entire media landscape. The threat of sanctions and self-censorship affects programming in almost all of the private radio stations. However, whether these stations influence inter-ethnic attitudes is an empirical question. Accordingly, we now examine whether the observed change in inter-ethnic attitudes is driven by radio programs broadcast from any of the other radio stations.

Before examining this issue, we should point out that subjects live in areas that are exposed to programming from only a subset of stations - see table A18. At the time that we conducted our experiments, there were 24 stations other than Radio Rwanda that broadcast in the country. Most of these were localized community-level private stations, and, indeed, transmissions from only five radio stations (other than Radio Rwanda) reach two or more of our 52 villages. At least one of these (the BBC) seems irrelevant in this context. This is because only two villages in our sample (representing fewer than 5% of our subjects), receive broadcasts from the BBC. Moreover, the BBC broadcasts from this radio transmission tower were in English, a language that very few (if any) of the subjects speak. This was further corroborated by our follow-up survey on radio-listening habits, where none of of the subjects reported regularly listening to the BBC.

Nevertheless, we individually analyze reception of each of these five stations. We also examine differences in attitudes based on whether a subject receives *any* other signal. As a first step, we simply replace Radio Rwanda with each of the other stations in the main empirical specifications. This helps illustrate whether the effect is limited to Radio Rwanda, or is simply due to the presence of any radio signal. Estimates from each of these regressions are plotted in figure 10, which provides an overview of these results. The figure offers support for the hypothesis that the estimates from Radio Rwanda are somewhat distinct, with 19 of the 31 estimates from other stations being distinguishable statistically from the Radio Rwanda inter-ethnic private game estimate.

The specific estimates for the impact of other stations on trust-game offers are presented

in panel A of table A19. The estimates when all stations are combined together are presented in columns 1 and 2, and do not predict differences in inter-ethnic or co-ethnic trust. Furthermore, we cannot rule out (as we could in the Radio Rwanda case) that the effect of other stations is the same for co-ethnic and inter-ethnic trust ($p = 0.953$). Columns 3-4 show results from a similar exercise that deals with concerns that the Radio Rwanda estimates observed earlier were simply due to the fact that it has a much larger network than the other stations. Similar to Bjorkegren (2015), we therefore construct a placebo network that looks at a hypothetical signal of a station broadcasting from the top 10 peaks in Rwanda that currently do not have a tower. Results there are similar to columns 1 and 2, and suggest no impact on either inter-ethnic or co-ethnic attitudes.

Columns 5-14 repeat this exercise for each of the five individual stations. As the table shows, the only significant inter-ethnic estimate stems, oddly, from the BBC, for which the estimate on ethnic attitudes runs in the opposite direction of the Radio Rwanda estimate. Panel B then reports the analogous results for trust game offers in the public treatment. Two cases show a significant effect on public trust-game behavior (columns 6 and 14). In general we are not overly concerned that the Radio Rwanda estimate is due to a correlated signal with another station, as none of the other stations show any similar pattern across trust game treatments.

Further emphasizing this point is the association between the other three main outcomes and each of the other stations (see table A20). For example, with the BBC, in contrast to the negative and significant inter-ethnic private estimate from table A19, we see a positive estimate in the trust survey (panel D column 3) and a negative estimate in the SIT (panel D column 1). Both are seemingly inconsistent with a poor inter-ethnic attitudes interpretation that might otherwise be suggested by the trust game estimate. This apparent contradiction undermines the legitimacy of both associations. For each of the other stations, similarly no consistent patterns emerge using the other outcomes.

To further investigate the few significant results we see for the effects of other stations,

we compute Bonferroni-corrected p-values that account for the fact that we are making multiple comparisons over several stations (table A21). When we account for this fact, the Radio Rwanda estimates remain significant in all cases where that was previously the case. For the other stations, however that is not the case. Specifically, every significant estimate in table A19 (trust game results) is no longer statistically significant after the Bonferroni correction, while only the BBC effect remains statistically significant when we look at the other measures. The BBC effect appears to be an anomaly. Recall that only two villages received the BBC - only one of those is driving the significant effect that remains, it just so happens that one of the two villages that receives BBC was the one that scored the lowest mean SIT of all of the villages in the sample. Given the context, we attribute the remaining significant effect associated with the BBC to sampling error.

In another robustness check relating to the signal from other stations, dummy variables are included one at a time, for each of the towers that broadcasts Radio Rwanda to one of the villages in the sample (table A22). If any individual tower were solely responsible for the differences observed that might indicate that it is actually something else about the tower - for instance broadcast of a different station - could have been responsible. However, table A22 shows that the results are quite robust to this as well.

VII. Robustness

A. Genocide

We now evaluate the role of the genocide in possibly driving the empirical results. While the genocide undoubtedly had an impact on the whole country, there may be concern that differences in localized violence are associated with local differences in inter-ethnic ethnic attitudes. This is an important channel to consider because recent evidence suggests that exposure to war violence generates cooperation and pro-social behavior (Bauer et al. 2016). This pro-social behavior may be driven by a number of alternative channels. For instance, the destruction due to the genocide may provide an incentive to invest in social capital due

to the need for insurance. Second, if there is an increase in wages and economic growth as a consequence of the genocide (e.g. due to labor shortage), this may also foster social cooperation and trust (Rogall and Yanagizawa-Drott 2013). Third, greater inter-group competition may favor pro-social behavior especially amongst in-groups (Henrich and Boyd 2001; Choi and Bowles 2007). Finally, the shock and trauma of witnessing the genocide may give rise to a "never-again" sentiment, and may transform preferences in a pro-social direction.

There are several reasons why we do not think that accounting for the genocide alters our results. First, it seems unlikely that genocide is a confound because we control for it directly in our full set of controls. Furthermore, while radio played a role in the genocide, the radio broadcasts that explicitly encouraged the genocide were from a different radio station: RTLM. However, since RTLM shared a transmission tower with Radio Rwanda, we want to be careful to avoid attributing greater pro-social inter-ethnic cooperation to Radio Rwanda, when in fact it could in theory have been a by-product of the (RTLM-catalyzed) genocide. Therefore, in each specification we directly control for RTLM broadcasts in 1994, while the full set of controls includes genocide prosecutions. For there to be a confound of these estimates, residual errors arising from mis-measurement or mis-specification of these variables would have to be correlated with both radio signal *and* inter-ethnic attitudes. To be certain, we run our main results separately for the subsample that did and did not receive RTLM. We want to see that both estimates do not go towards zero. If they did, this would be evidence of a spurious correlation. However, table A23 shows that we estimate very similar effects to the main estimates in both the RTLM and non-RTLM villages. While we lose a little precision because we split the sample even further than we had already done, we still recover estimates precise at a 15% threshold in every case except the trust survey.

Similarly, for genocide to be a driver of the observed results, it must be correlated with both the Radio Rwanda signal and with the main outcomes. Given that Yanagizawa-Drott (2014) has shown that radio had an impact on the genocide, we first look at the relationship between Radio Rwanda transmission and genocide in table A24. Column 1 replicates

Yanagizawa-Drott (2014) using our sample, and shows that receipt of a RTLM signal is positively associated with genocide. We get less precision than the original estimates because all villages in our sample experienced some genocide. Also consistent with his results, column 2 confirms that there is no relationship between Radio Rwanda and genocide.

Now consider the differential impact of exposure to the genocide on the four measures of inter-ethnic attitudes. As mentioned above, several studies illustrate that exposure to the genocide could result in greater pro-social behavior. However, most of the evidence (as well as the theory) suggests that this should be reflected in improved co-ethnic attitudes and trust and *not* inter-ethnic attitudes. Nevertheless, we directly test the impact of RTLM and genocide on our four main outcomes in columns 3-6 of table A24. We find that exposure to RTLM is not associated with *any* of the four measures - be it the SIT, the trust game, the trust survey or partner selection. Similarly, when looking at genocide violence the table suggests that it is negatively associated with responses to the survey question on out-group trust, but actually positively associated with inter-ethnic offers in the private trust game. Both seem to have increased ethnic salience, however not significantly so. While our estimates are not precise, their general pattern suggests that genocide may have hurt inter-ethnic relations. This lack of precision could be because there is heterogeneity in the genocide effect by ethnicity, or because our sample is restricted to villages directly affected by the genocide. We leave an analysis of these considerations to future work.

B. Measurement Error and Ethnicity

One potential concern with the trust estimates is the impact of measurement error in the ethnicity variable. Because we were unable to directly ask individuals about their ethnicity, we relied on a survey response to a question that proxies for whether an individual is Tutsi: an affirmative answer to the question about their eligibility for a government fund set-up for Tutsi survivors of the genocide.

It should be pointed out that this proxy could result in measurement error due to mis-

attributing ethnicity. For example, this may arise if some Hutu deliberately masquerade as Tutsi in order to receive FARG money, or if some Tutsi do not know that they are eligible for receiving money from the fund. This seems unlikely not only because Rwanda is run by an autocratic Tutsi leader, but also because of the country's (unusual for Africa) high state capacity. Indeed in our sample, all participants were aware of the fund. However, even if mis-measurement of ethnicity occurred, it would not be able to explain the results unless it systematically correlated with the variation in radio coverage. Accordingly, we further examine whether this is a potential concern. Of course, we should emphasize that neither the SIT results nor the trust survey results would be affected regardless because they do not rely on knowledge of the subject's ethnicity.

We try to assess whether any potential mis-measurement is likely to be correlated with radio coverage by examining the nature of co-ethnic transactions in radio versus non-radio regions. Suppose we allow for heterogeneity amongst the Hutu such that a subset of them decides to masquerade as Tutsi in radio regions. This may be either because these Hutu think they can get away with it, or because they look different, or feel less loyalty to the Hutu community. However, if these Hutu are masquerading as Tutsi in radio regions in order to receive FARG money, this will change the nature of co-ethnic matches both amongst the Tutsi as well as the Hutu. For example, this would mean that several games in radio regions coded as Tutsi-Tutsi co-ethnic games should have been coded as inter-ethnic games. This may lower the observed Tutsi co-ethnic offers in radio regions. Similarly, perhaps any Hutu who do not try to pass as Tutsi are different either because they look stereotypically Hutu, or because they have stronger own-group loyalty. In this case, Hutu-Hutu trust offers in radio regions should be higher than in non-radio regions.

Following this logic, we compare whether the nature of co-ethnic trust game offers differ between the radio and non-radio regions in table A25, where we split the sample into Hutu-Hutu and Tutsi-Tutsi games. In both cases the effect is not significant. Although the Tutsi-Tutsi games show a large negative effect, we attribute this to the extremely small sample.

Indeed, the Hutu-Hutu have a more reasonable sample to work with, and the estimate there is essentially zero ($17RWF \approx 0.01USD$). We interpret this as suggestive evidence that any measurement error in our ethnicity variable is not associated with radio broadcasts.

C. Experimental Protocol and Implementation

i) Order of Experiments: Another concern may be that since each subject was involved in more than one experiment, outcomes in one experiment could influence the others. Specifically, each subject played the trust game twice and was randomly assigned to play either first as a sender or as a receiver. We can therefore check whether playing as a sender first had an impact on the offers. These results are described in table A27 and they are reassuring; we observe that there is no correlation between playing first as a sender with either the inter-ethnic trust game offers, co-ethnic trust game offers or SIT scores. Since the survey and the partner-selection task took place *before* the trust game, the sender-receiver order is not relevant for these measures.

ii) Geographical Characteristics of Trust-Game Partners: A reasonable concern regards how partners in the trust game were matched. While individuals were matched randomly, if ethnic groups are geographically clustered, this could mechanically result in co-ethnic pairs being from villages that are closer to each other than inter-ethnic pairs. In this case, perhaps what the subjects noticed was not an ethnic difference but instead a regional difference. However, for this effect to confound our results this would also have to vary by reception of Radio Rwanda. Accordingly, we examine this more systematically.

Table A28 examines the distance between the villages of two partners in the trust game, to see whether this is explained by the trust game sender's reception of Radio Rwanda. As expected, Radio Rwanda does not explain this distance for any of the games, and in no case is there a difference between estimates using the co-ethnic and inter-ethnic samples. Relatedly, it could be that subjects from Radio Rwanda villages are more likely to be paired with

someone else from a Radio Rwanda village. In this case their attitudes may not actually be different, they may just expect a higher return because they expect their partner's attitude to be different. We disallowed verbal communication between partners, so it seems unlikely that they could know the home village of any partner. However we can also show this formally. Table A29 shows that offers are not higher when the partner is from a village that receives Radio Rwanda.[52]

iii) SIT on Other Photo Characteristics: For the SIT to be a plausible measure of ethnic salience, the key difference between the photographs should be ethnically distinctive facial features. The SIT results would be difficult to interpret if there was some aspect of the photographs that was correlated with ethnicity - be it age, gender, class, clothing or any other aspect of their appearance. Accordingly, we selected individuals for the photos who were either Hutu or Tutsi, but of the same gender and approximately the same age. Still, there were some differences in the photos that were unavoidable, so it seems prudent to make sure that these are not the differences that are being noticed by the subjects. Table A30 presents results from testing for as many differences as we can notice between the pictures. Column 1 examines shirt color (light versus dark); column 2 looks at the background of the photo - some photos were taken near our office and others near a market where you can see the city in the background; column 3 investigates the type of shirt (collar versus no-collar); in column 4 we look at facial hair while column 5 checks whether subjects categorized on whether the person in the photo was wearing a jacket. For each of these photo characteristics an SIT score was recomputed, based on that characteristic, and that new SIT score serves as the outcome using the main model. However, none of these characteristics seems differentially salient among people that live in a Radio Rwanda village.

[52]Return offers are higher when the sender is in a radio village because on average there is about a one-for-one return of any money shared in the trust game (table A31)

iv) SIT and Public Information: One last consideration is the effect of the public treatment on the SIT score. The SIT is one of the main ways to identify that subjects' attitudes are different in Radio Rwanda regions, since most of the other outcomes could differ either because of different attitudes or differences in strategic behavior used in order to *appear* to have good inter-ethnic attitudes. Here we check whether being exposed to the public information treatment in the trust game influenced SIT scores. The idea is that perhaps priming subjects about the possibility of any of their results being made public might make them more cautious of their behavior in the other exercises. There is no evidence that this priming effect had any influence on SIT scores (table A32).

VIII. Discussion

In this paper we empirically analyzed nation building in the shadow of a genocide in Rwanda. We used variation in exposure to government radio propaganda due to the mountainous topography of Rwanda to investigate the impact of propaganda on nation building using a series of lab-in-the field experiments. Our results show that individuals exposed to Radio Rwanda have lower salience of ethnicity, increased inter-ethnic trust and show more willingness to interact face-to-face with members of another ethnic group. These findings provide some of the first quantitative evidence suggesting that the salience of ethnic identity can be manipulated by governments.

We should emphasize that these results should be treated with considerable caution. This is especially the case since we do not have a measure of the resilience or reversibility of the observed improvement in Hutu and Tutsi relations in Rwanda. Any observed progress in nation building may be temporary and vulnerable to the possibly shifting priorities of President Kagame (and the RPF) or any unexpected political transition in the future. Furthermore, its very distinctive political context makes Rwanda a difficult case study to serve as a barometer of nation building efforts elsewhere. Indoctrinating and changing attitudes is perhaps much easier in an autocratic country with no media freedom (such as Rwanda)

than in a country with real media choices. We leave the study of nation building in other political contexts for future work.

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Main figures

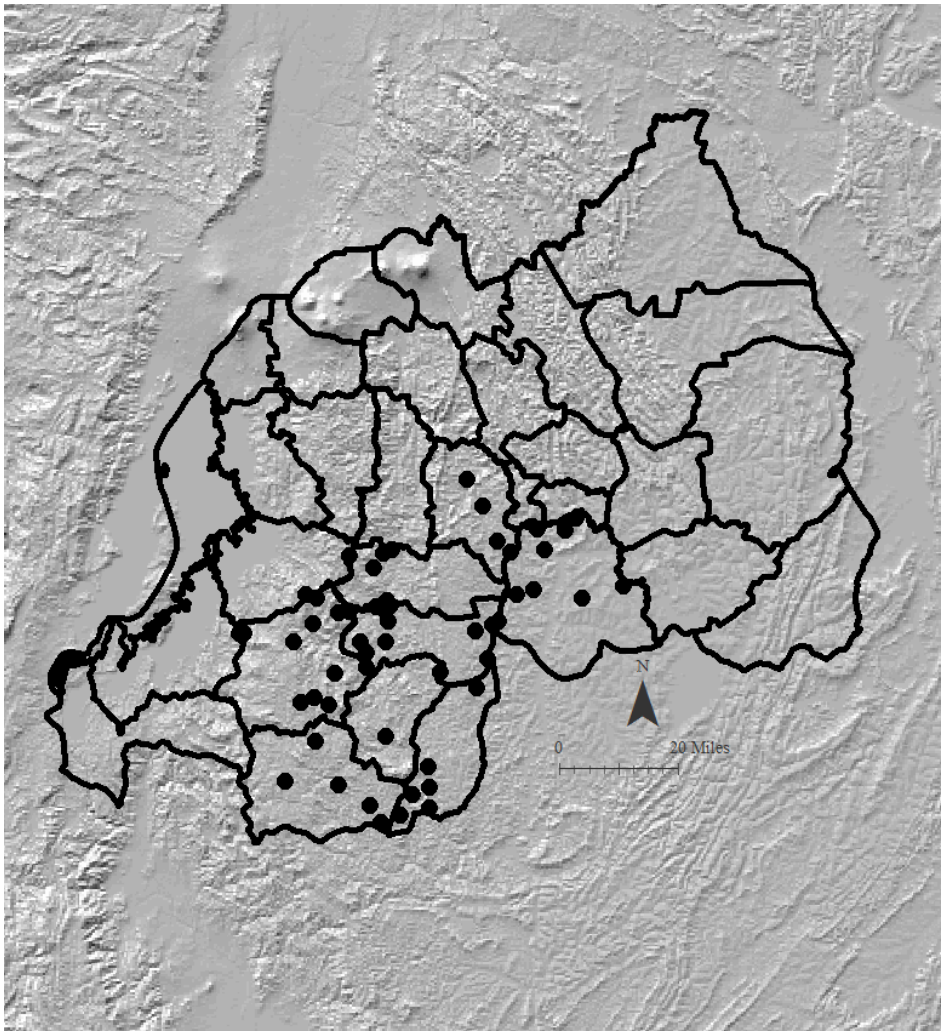
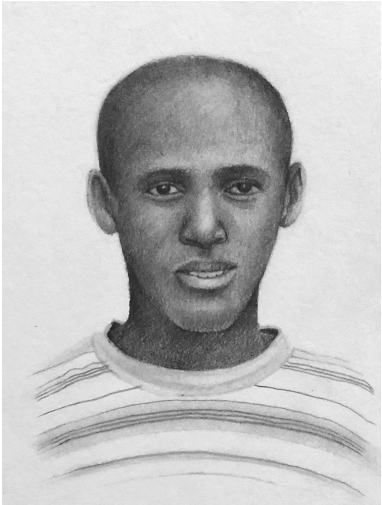
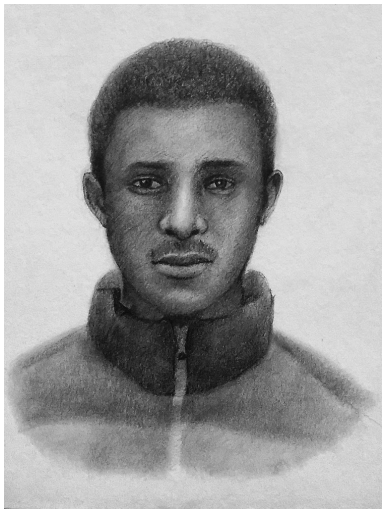


Figure 1: Village locations of all subjects in Rwanda

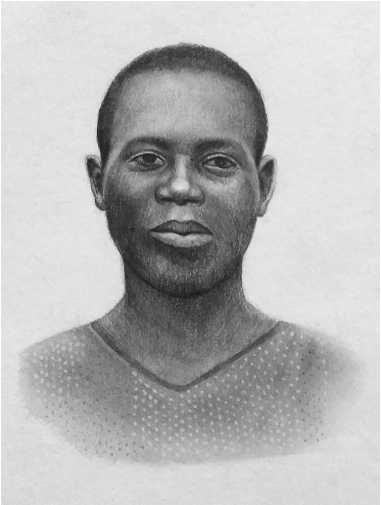
Note: This map shows the village subject locations and administrative boundaries (District level). Each subject left from their own village (which is what is depicted here) to a pick-up location nearby. Our drivers picked them up from this location and drove them to the survey location. The village locations are retrieved from a survey of subjects and then geo-coded to the centroid of the village. Villages are often extremely small, so to protect the anonymity of the subjects we have altered, randomly, the locations of each village by up to about 10km for the purpose of the map. Precisely, the random perturbation is less than or equal to 0.1 decimal degrees from the actual village location.



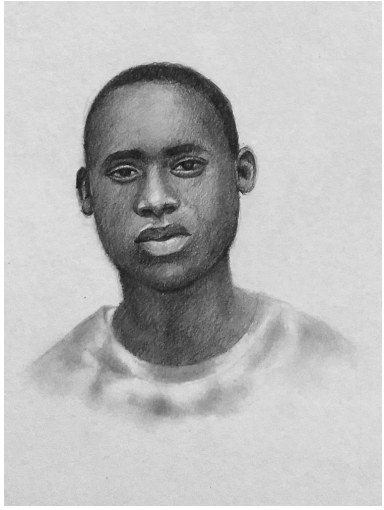
"This person owns a blue bicycle and two red motorbikes"



"This person really likes bananas but dislikes guava"



"This person has four children: two boys and two girls"



"This person has two brothers"

Figure 2: Example of SIT Exercise

Notes: This figure is an example of the tool used for the SIT exercise where we have replaced the original colored photographs with pencil sketches. Subjects were shown the photographs and read the statement displayed beside it. In the recall portion of the task, the photographs were laid out on a table and the statements were read back to the subject. The subject was tasked with identifying the photograph associated with the statement. We chose subjects with a view that they were representative of typical Tutsi (top two photos) and Hutu (bottom two photos) appearance. However, in order to protect anonymity of those photographed, in her portraits, the artist made minor modifications to facial features and erased any identifying background.

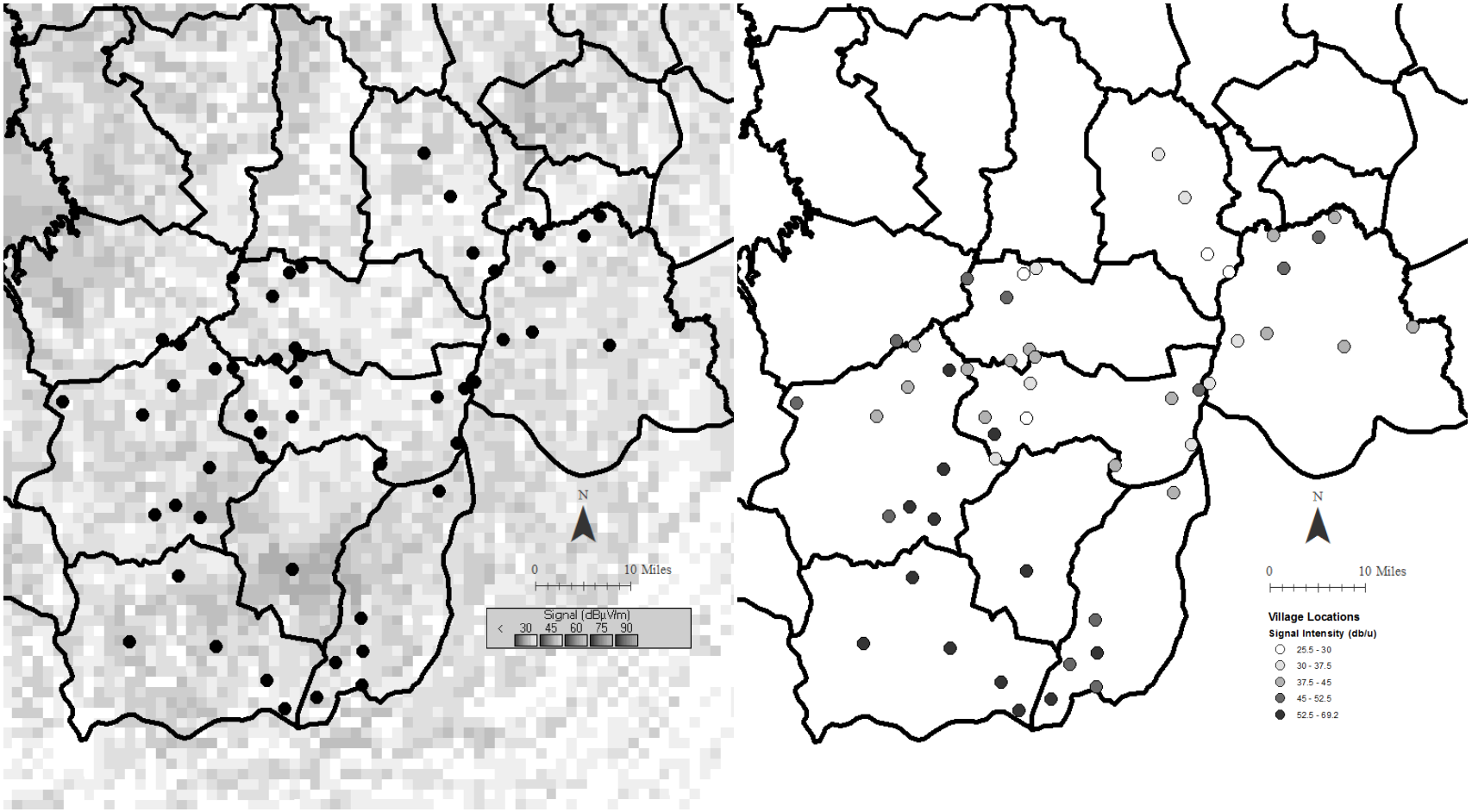


Figure 3: Radio Rwanda signal, district boundaries and subjects' village locations

Notes: This is a map of Radio Rwanda signal strength in Rwanda. We have overlaid the district administrative boundaries in black and the subject villages, represented by circles. In the map on the right we shade the circles to reflect the strength of the Radio Rwanda signal. The maps on the left and right are identical except for the map on the left shows the Radio Rwanda signal for the whole region, while the map on the right shows signal strength in a particular village in the sample. Because of the sampling procedure, each dot represents about 8 or 9 subjects. Villages are often extremely small, so to protect the anonymity of the subjects we have altered, randomly, the locations of each village by up to about 10km for the purpose of the map. Precisely, the random perturbation is less than or equal to 0.1 decimal degrees from the actual village location. The assignment of radio signal is based on the true location of the village rather than the altered location of the village.

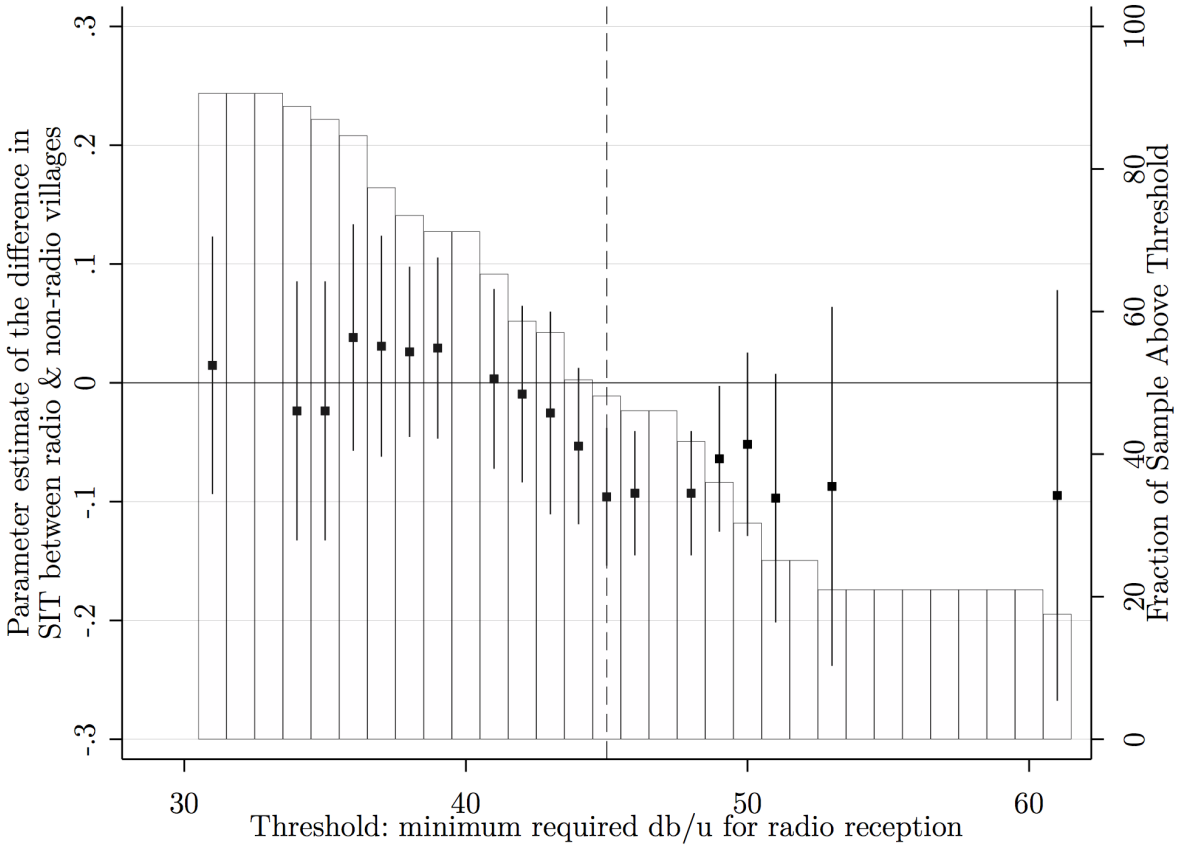


Figure 4: SIT and Radio Rwanda reception

Note: The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of $1db$). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95% confidence intervals. Each point and confidence interval is from a separate regression. Each regression includes the same set of controls, which is the baseline set of controls from the tables. This includes all variables in table 1 panel B. The regressions in this figure are analogous to column 2 of table 3. The SIT measure we use is the main measure, which is used in columns 1-3 in table 3 and defined in section V.A. Confidence intervals are constructed using standard errors clustered at the village level. The dashed vertical line indicates the threshold used in the main results (45 db/μ).

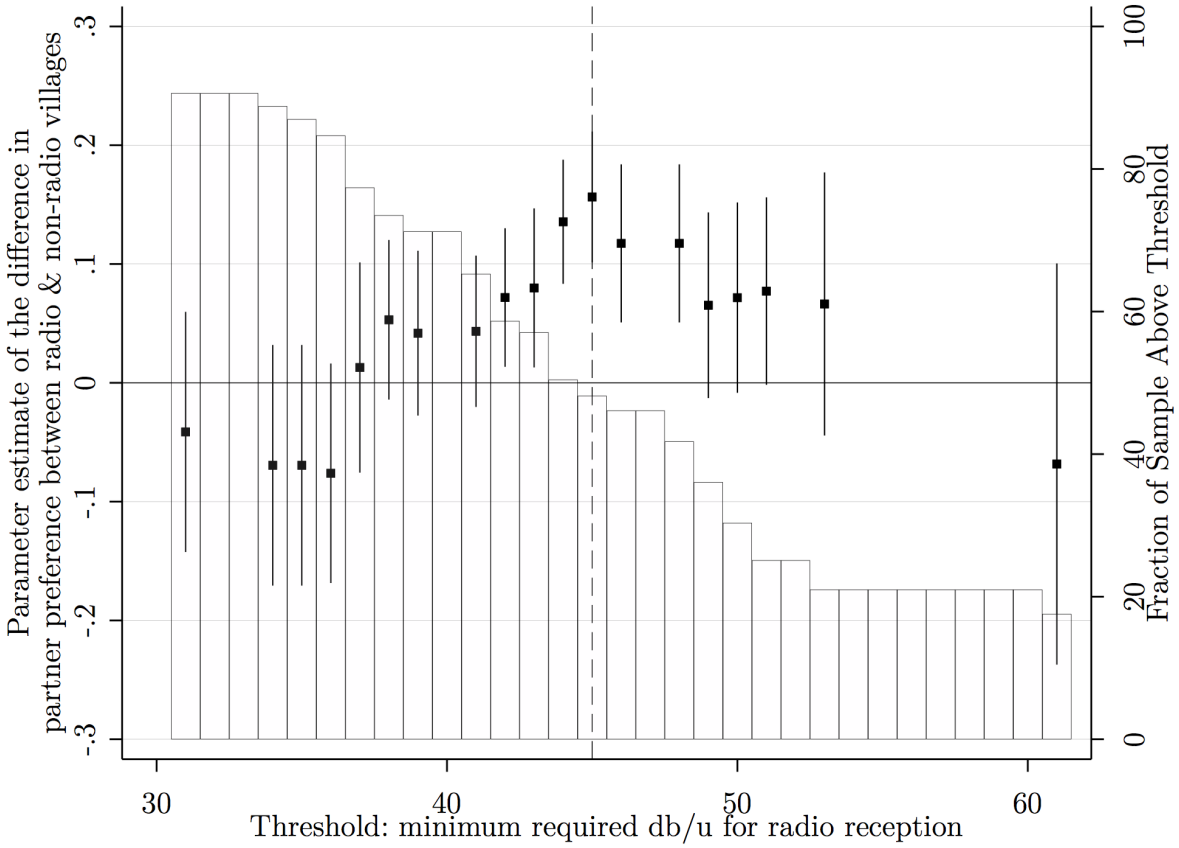


Figure 5: Partner selection measure and Radio Rwanda reception

Note: The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of $1db$). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95% confidence intervals. Each point and confidence interval is from a separate regression. Each regression includes the same set of controls, which is the baseline set of controls from the tables. This includes all variables in table 1 panel B. The partner selection measure we use is the main measure which is also used in columns 1-3 of table 4 and defined in equation (1). Confidence intervals are constructed using standard errors clustered at the village level. The dashed vertical line indicates the threshold used in the main results (45 db/μ).

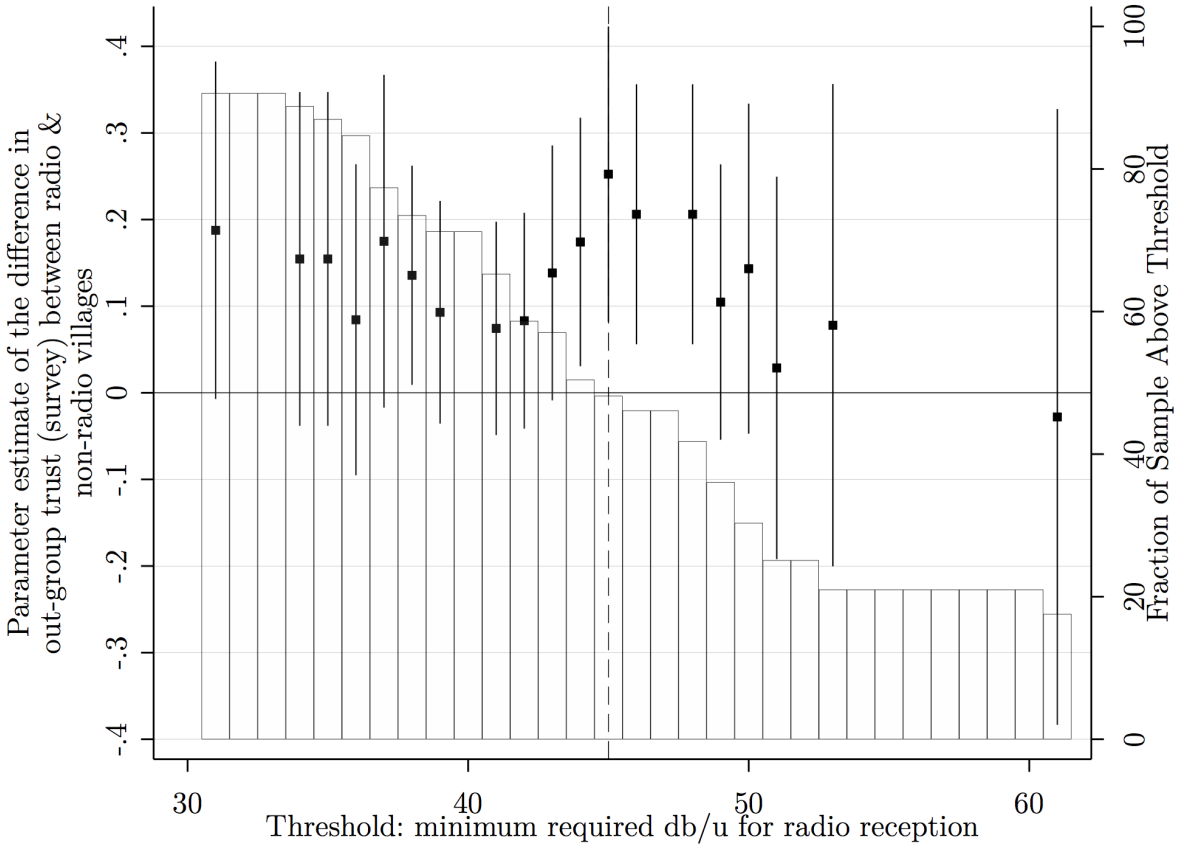


Figure 6: Out-group trust (survey) and Radio Rwanda reception

Note: The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of $1db$). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95% confidence intervals. Each point and confidence interval is from a separate regression. Each regression includes the same set of controls, which is the baseline set of controls from the tables. This includes all variables in table 1 panel B. The trust measure we use is from responses to the out-group trust survey question, which asks subjects how much they trust other communities. This is the same measure used in table 5, columns 1 and 3. Confidence intervals are constructed using standard errors clustered at the village level. The dashed vertical line indicates the threshold used in the main results (45 db/μ).

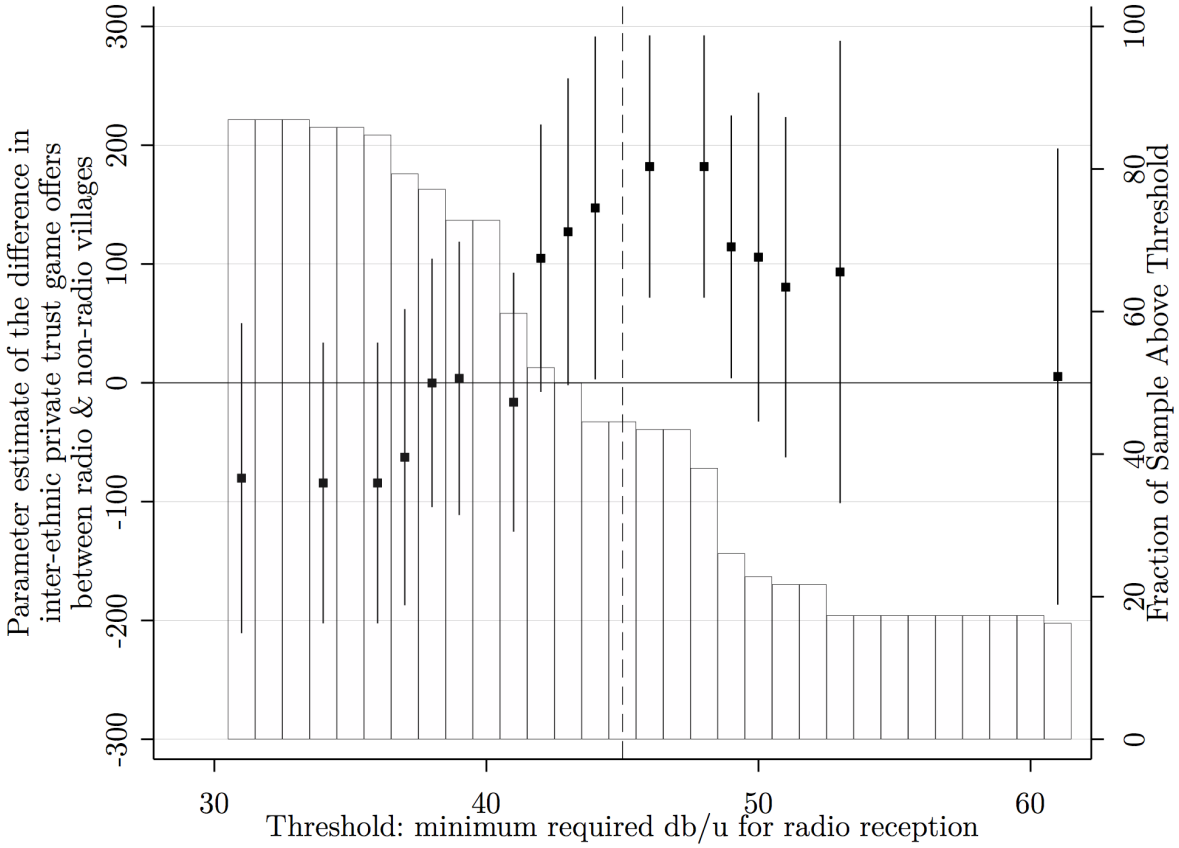
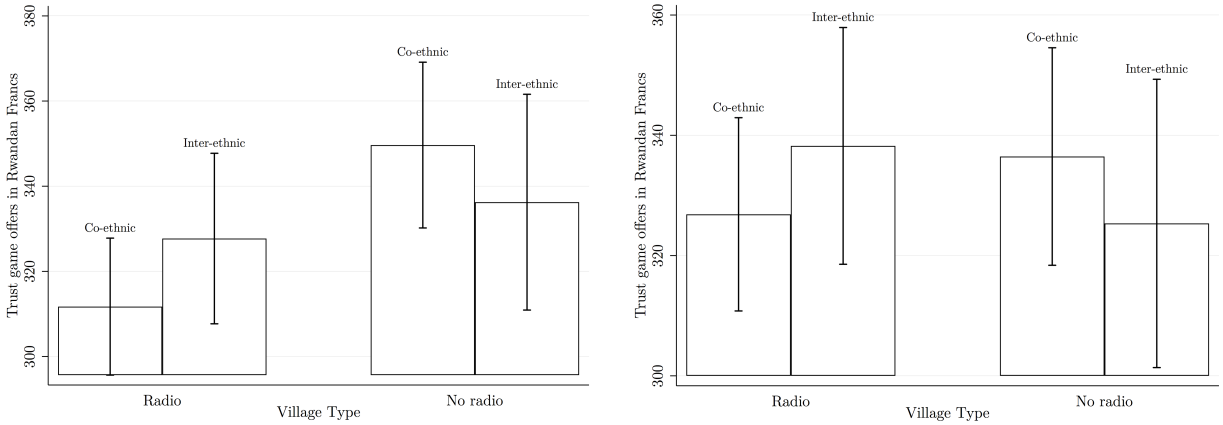


Figure 7: Private, inter-ethnic trust game offer and Radio Rwanda reception

Note: The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of $1db$). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95% confidence intervals. The dependent variable in each regression is the inter-ethnic trust game offers from the private trust games. Each point and confidence interval is from a separate regression. Each regression includes the same set of controls, which is the baseline set of controls from the tables. This includes all variables in table 1 panel B. The trust game measure we use is the sender's offer in the inter-ethnic private trust game. This is the same outcome and sample as in table 6, panel A, columns 1 and 3. Confidence intervals are constructed using standard errors clustered at the village level. The dashed vertical line indicates the threshold used in the main results (45 db/μ).



Panel A: unconditional means

Panel B: conditional means

Figure 8: Means of Trust Game Outcomes in Radio and Non-Radio Regions

Note: The figure shows the mean offers in the private trust game and associated 95% confidence intervals. Radio versus non-Radio villages are split according to a $45db/\mu$ cut-off used throughout the paper. Panel A shows unconditional means. Panel B shows means conditional on district, distance to city, road and Kigali, and distance to the three nearest radio towers. This is constructed by adding the mean of the trust game offers to the residuals from a regression of trust game offers on the variables listed above.

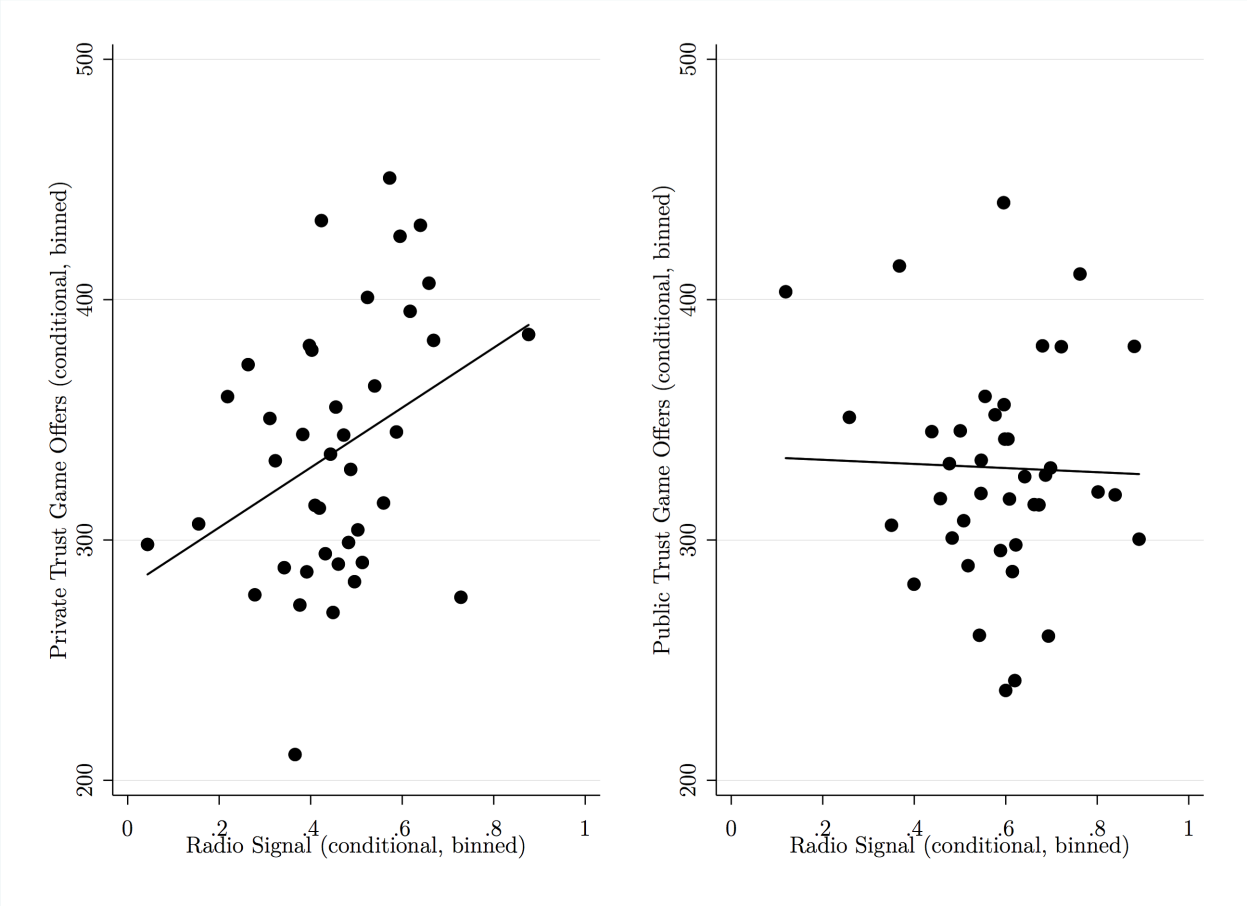


Figure 9: Inter-ethnic trust game offers in private (left) and public (right) and Radio Rwanda reception

Note: This figure shows the conditional correlation between Radio Rwanda reception and both private inter-ethnic trust game offers (left) and public inter-ethnic trust game offers (right). In both cases the plot is created by regressing the trust game offer and the radio variable on the baseline set of controls, which can be found in table 1, panel B. Additionally we include the trust-game specific controls in each regression, which include gender of partner, partners radio reception, the enumerator in the trust game, the distance between the villages of the sender and the partner, as well as their score on a risk measure. We take the residuals from these regressions and add them to the means of the relevant variables for the relevant sample (used in the relevant regression) so that we can compare values across graphs. We then plot the re-meaned residuals against each other. To make the graph easier to read, each dot in each graph represents the mean of an x-axis bin, where there are 40 equidistant bins in each graph.

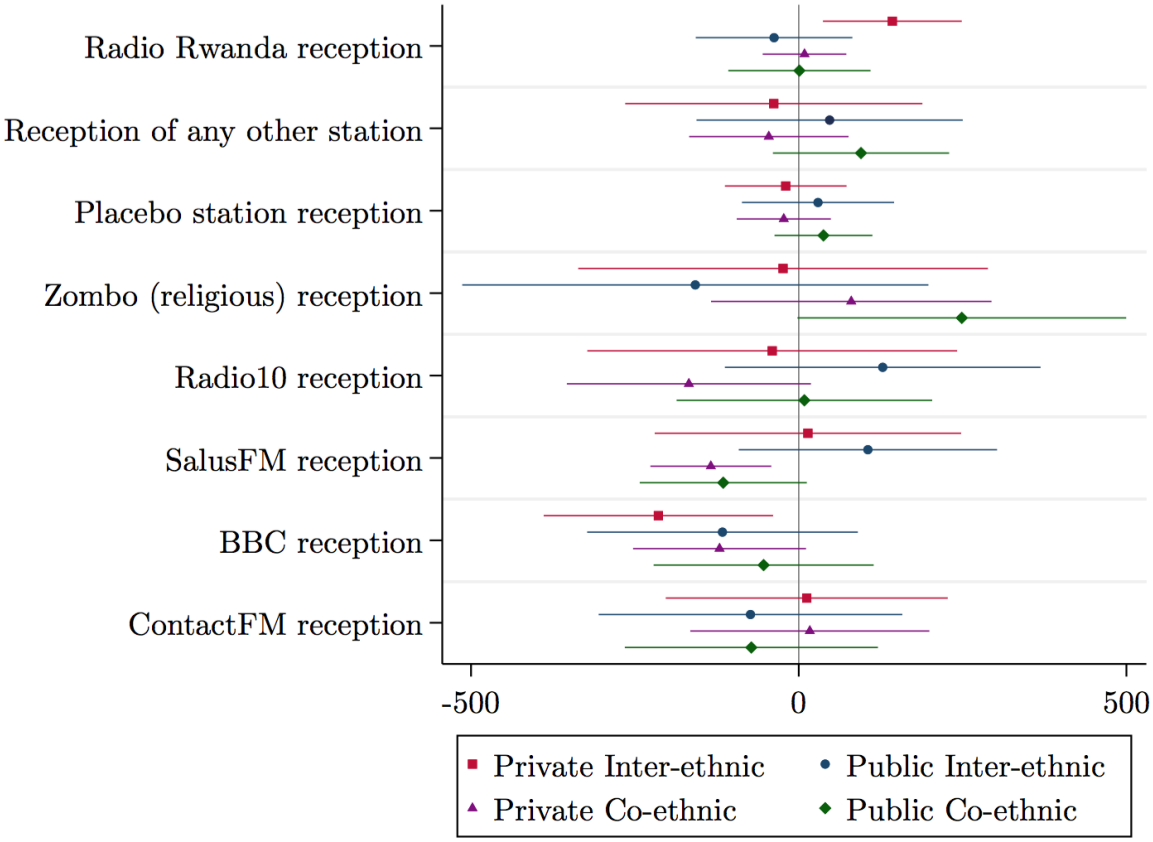


Figure 10: Trust game offers and reception of each radio station

Note: This figure includes estimates for all radio stations that service more than 1 village in the sample. Each estimate and 95% confidence interval represents a regression coefficient from a separate regression. The coefficient of interest relates to the station denoted on the vertical axis, while the horizontal axis denotes the magnitude of the estimate. Each regression includes a Radio Rwanda variable in addition to the same baseline set of controls, and trust-game specific controls which include gender of partner, partners radio reception, the enumerator in the trust game, the distance between the villages of the sender and the partner, as well as the partners score on a risk measure. The list of baseline controls can be seen in table 1, panel C. Confidence intervals are constructed using standard errors clustered at the village level. We tested for the number of point estimates that are not equal to our private inter-ethnic Radio Rwanda coefficient at the 10% level. For private inter-ethnic games we have 3 (BBC, any other station, placebo), for private co-ethnic games we have 6 (Radio Rwanda, BBC, Radio10, Salus, any other station, placebo), for public inter-ethnic games we have 5 (Radio Rwanda, BBC, ContactFM, Zombo, placebo), and 5 for public co-ethnic games (Radio Rwanda, BBC, ContactFM, Salus, placebo). In total, 19 of the 31 estimates other than Radio Rwanda x Private x Inter-ethnic are statistically different from that estimate.

Main tables

Table 1: Summary statistics

	Obs	Mean	Std. Dev.	Min	Max
	Panel A: Variables of Interest				
Radio Signal	438	0.48	0.50	0	1
Public Information	438	0.45	0.50	0	1
	Panel B: Dependent Variables				
Salience of Identity Test	438	0.86	0.28	0	1
Salience of Identity Test (Alternate)	438	1.22	0.79	0	5
Salience of Identity Test (Binary)	438	0.900	0.30	0	1
Partner Selection	438	0.433	0.29	0	1
Partner Selection (Alternate)	438	1.03	0.89	0	2.5
Partner Selection (IHST Count)	438	1.26	0.70	0	2.31
Trust Out-group (Survey question)	438	3.00	0.724	1	4
Trust In-group (Survey question)	438	3.21	0.764	1	4
Trust Game Offer Private (RWF)	242	329	126	100	600
Trust Game Offer Public (RWF)	196	335	129	100	600
	Panel C: Baseline Control Variables				
Tutsi	438	0.28	0.45	0	1
Gender (% Female)	438	40%	0.49	0	1
Age	438	43.3	12. 4	19	88
Distance to Road (km)	438	1.1	0.063	1	1.21
Distance to Capital (km)	438	59.9	26.1	10	105
Distance to Major City (km)	438	28.6	11.4	3.3	45.9
Light Density at Night	438	0.54	1.08	0	4.25
Dist. Nearest Station (km)	438	24.1	7.67	9	40
Dist. 2nd Nearest Station (km)	438	43.6	14.8	11	78
Dist. 3rd Nearest Station (km)	438	51.9	12.6	35	91
Travel Time to Nearest Station (sec)	438	5,121	2,617	1,233	11,588
Travel Time to 2nd Nearest Station (sec)	438	7,507	2,098	2,326	11,588
Travel Time to 3rd Nearest Station (sec)	438	8,238	1,726	5,071	11,588
Radio RTLM	438	0.19	0.39	0	1
Cell Phone Usage in Village (%)	438	69.3%	0.202	0.125	1
Raven Score	438	5.39	1.34	1	6
	Panel D: Additional Control Variables				
Elevation	438	1,646.1	185.3	1,365	2,141
Genocide Prosecutions (prosecutions per 1000)	438	0.518	0.451	0	2.217
Variance in Elevation	438	26,187	88,449	0	357,286
Education years	438	5.8	2.56	0	19
Income (USD/yr)	438	460.54	877.33	0	9449
log (income)	438	5.38	1.26	0	9.15
log(sector pop.)	438	10.1	0.33	9.46	10.83
Sector Pop. Dens.	438	1,015	3,618	223	24,623
Facing North	438	0.25	0.43	0	1
Facing South	438	0.25	0.43	0	1
Facing East	438	0.32	0.47	0	1
Facing West	438	0.19	0.39	0	1
	Panel E: Other Variables of Interest				
log (land value) (log(USD))	438	8.35	2.01	0	14.3
Reception of Other Stations	438	38.8%	0.488	0	1
Forced Labor	438	0.86	3.86	0	80
Migration (ever in lifetime)	438	22%	0.41	0	1
Years in Current Home	438	39.2	15.41	0	88
Migration Since 2004	438	3.7%	0.188	0	1
Good Signal of Radio Rwanda	154	5.8	1.22	3	9
Regular Radio Rwanda Listener	154	42%	0.49	0	1
Regular Listener of Other Stations	154	4.5%	0.21	0	1

Note: Data presented in this table was collected from 438 subjects in Rwanda (see sections III and IV for a description) as well as a follow-up survey of 154 subjects (see section V). The Salience of Identity Test is computed using the share of within-ethnicity mistakes. The Salience of Identity Test (Alternate) is the number of within-ethnicity mistakes squared over the total number of mistakes. The Salience of Identity Test (Binary) is a binary variable equal to one if the share of within-ethnicity errors is over 42%. The Partner Selection variable is computed as the share of selections in the partner-selection task from the other ethnicity. The Partner Selection (Alternate) variable is the number of choices from the other ethnicity squared over the total available choices from the other ethnicity. The Partner Selection (IHST count) is the inverse hyperbolic sine transformation applied to the count of choices from the other ethnic group, regardless of the number of available choices there were. All of the travel time variables are measured in seconds. All of the distance variables are measured in kilometers. Raven Score is the number of correct answers on an 8 question Raven Cognitive Test.

Table 2: First stage & balancing

Dependent variable	Mean (1)	N (2)	Radio Estimate (3)	Standard Error (4)	Public Estimate (5)	Standard Error (6)	Controls (7)
	Panel A: Constructed Radio Signal and Reported Radio Signal and Habits						
Radio Ownership (DHS data)	0.585	54,892	0.04***	(0.015)	.	.	District FE
Signal Quality of RR (follow-up data)	5.8	154	1.38***	(0.36)	-0.04	(0.17)	Baseline
Regularly Listen to RR (follow-up data)	0.42	154	0.398***	(0.148)	0.03	(0.092)	Baseline
Regularly Listen to Other Stations (follow-up data)	0.05	154	-0.172***	(0.063)	-0.034	(0.030)	Baseline
	Panel B: Baseline Controls						
Gender (female = 1)	40%	438	-0.029	(0.063)	-0.039	(0.047)	distances & District FE
Age	43.3	438	-0.916	(1.783)	-1.23	(1.158)	distances & District FE
Tutsi	0.28	438	-0.083	(0.071)	-0.045	(0.035)	distances & District FE
Distance: road	0.1	438	-0.009	(0.009)	0.001	(0.003)	distances (excl. road) & District FE
Distance: Kigali	60	438	5.22**	(2.04)	-0.156	(0.533)	distances (excl. Kigali) & District FE
Distance: nearest city	28.7	438	-0.223	(0.973)	-0.027	(0.317)	distances (excl. city) & District FE
Light Density at Night	0.54	438	-0.670	(0.419)	-0.064	(0.051)	distances & District FE
RTLM	0.19	438	0.394***	(0.108)	0.058**	(0.022)	distances & District FE
Raven Score	5.39	438	0.162	(0.165)	-0.006	(0.140)	distances & District FE
Cell phones	69.5%	438	0.027	(0.060)	-0.011	(0.013)	distances & District FE
	Panel C: Additional Controls						
log(income)	5.37	438	-0.145	(0.144)	0.076	(0.100)	distances & District FE
Genocide	0.518	438	0.062	(0.18)	-0.007	(0.024)	distances & District FE
Elevation	1,646	438	-41.3	(28.9)	1.19	(7.37)	distances & District FE
Elevation Variance	26,187	438	4,309	(6,724)	-1,174	(4,655)	distances & District FE
Primary School	63%	438	0.0004	(0.067)	0.051	(0.046)	distances & District FE
High School	2.1%	438	0.039	(0.025)	0.019	(0.013)	distances & District FE
College	0.2%	438	-0.002	(0.003)	-0.003	(0.003)	distances & District FE
Facing North	0.247	438	-0.081	(0.167)	0.107***	(0.034)	distances & District FE
Facing South	0.251	438	-0.051	(0.154)	-0.058*	(0.031)	distances & District FE
Facing East	0.315	438	0.222	(0.146)	-0.034	(0.031)	distances & District FE
Facing West	0.187	438	-0.091	(0.165)	-0.016	(0.021)	distances & District FE
log(sector population)	10.1	438	0.094	(0.112)	-0.003	(0.019)	distances & District FE
Sector Pop. Dens.	6.17	438	4,779	(3,257)	-219.6	(211.3)	distances & District FE
	Panel D: Other Variables of Interest						
log(Land Value)	8.36	438	0.119	(0.232)	-0.115	(0.186)	distances & District FE
Radio Station: Zombo	0.290	438	0.08*	(0.043)	0.005	(0.013)	distances & District FE
Radio Station: Radio 10	0.098	438	0.142	(0.087)	0.010	(0.009)	distances & District FE
Radio Station: BBC	0.039	438	0.029	(0.025)	-0.019	(0.015)	distances & District FE
Radio Station: ContactFM	0.039	438	0.017	(0.087)	-0.008	(0.009)	distances & District FE
Radio Station: Salus	0.078	438	0.044	(0.027)	-0.023	(0.015)	distances & District FE
Forced Labour	0.86	438	-0.558	(0.572)	-0.232	(0.273)	distances & District FE
Migration (in lifetime)	22%	438	-0.058	(0.060)	-0.022	(0.033)	distances & District FE
Migration (years in current home)	39.2	438	0.012	(2.38)	-1.32	(1.34)	distances & District FE
Migration (since 2004)	3.7%	438	0.041	(0.034)	0.019	(0.019)	distances & District FE

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each *row* represents a different regression. The variable in the left-most column is the dependent variable. The mean and number of observations of the dependent variable are listed in columns 1 and 2 respectively. Each regression includes a variable for Radio Rwanda and assignment to the public trust game (except for the DHS data, wherein none of the subjects played the trust game), which are the two regressors of interest in our results. See tables 3, 4, 5, 6 (Radio Rwanda) and table A15 (Public treatment). Balancing estimates for these two variables are shown in columns 3 and 5 respectively. Their associated standard errors are presented in columns 4 and 6. Column 7 shows the controls included in each regression. All regressions with distances and district FE were run with: district fixed effects and distance (km) to nearest road, nearest city, and Kigali. The table shows all of the baseline controls in panel B, except the distance and time to the towers, which are mechanically related to radio signal. Panel C includes all of the additional controls in the full specification, while panel D includes land value to double check whether wealth is balanced, as well as other stations and forced labor since those variables are used in robustness exercises. We also examine migration here as migration could confound our estimates. We have 66 total balancing estimates and 6 are significant at least at the 10% level.

Table 3: SIT scores and Radio Rwanda reception

Dependent Variable:	**SIT score** ($\frac{WithinError}{TotalError}$)			**SIT score** ($\frac{WithinError^2}{TotalError}$)	**SIT** ($\frac{WithinError}{TotalError} > 0.42$)
	(1)	(2)	(3)	(4)	(5)
Radio Rwanda	-0.133***	-0.0960***	-0.106**	-0.319**	-0.167***
	(0.0463)	(0.0289)	(0.0464)	(0.132)	(0.0614)
Controls	Baseline	Baseline	Full	Full	Full
Mistake FE	No	Yes	Yes	Yes	Yes
Observations	438	438	438	438	438
R^2	0.213	0.402	0.426	0.416	0.285
Control Group Mean of Dependent Variable	0.855	0.855	0.855	1.243	0.916

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column represents a different regression. All regressions with the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time (seconds) by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions with the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village that is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction the village faces. Mistake fixed effects are a set of 5 dummy variables that denote whether a person made, 1, 2, 3, etc. mistakes in total on the SIT, regardless of the nature of the mistake. All outcome variables are a variant of the SIT scores and each of the three variants are described in section IV. In column 6-9 the dependent variable is binary, and these three columns are estimated using a linear probability model.

Table 4: Selection of Inter-ethnic Partners and Radio Rwanda reception

Dependent Variable:	**# other ethnicity chosen / min{total, 5}**		**(# other ethnicity chosen)^2 / min{total, 5}**	**log(# other ethnicity chosen)**
	(1)	(2)	(3)	(4)
Radio Rwanda	0.156***	0.171***	0.382***	0.378***
	(0.0274)	(0.0360)	(0.0955)	(0.0924)
Controls	Baseline	Full	Full	Full
Observations	438	438	438	438
R^2	0.406	0.421	0.497	0.440
Control Group Mean of Dependent Variable	0.420	0.420	1.01	1.22

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column represents a different regression. All regressions with the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time (seconds) by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions with the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village that is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction the village faces. All three outcomes are a variant of the share of out of ethnic group partners chosen. Column 4 uses the log of a simple count variable. To account for the possibility of zeros in this variable, the inverse hyperbolic sine transformation is used to construct the log.

Table 5: Trust survey responses and Radio Rwanda reception

Dependent Variable:	**Out-group trust** (1)	**In-group trust** (2)	**Out-group trust** (3)	**In-group trust** (4)
Radio Rwanda Reception	0.250**	0.0462	0.273**	-0.106
	(0.105)	(0.170)	(0.114)	(0.191)
Equality of Coefficients (p-value)	0.068		0.005	
Controls	Baseline		Full	
Observations	438	438	438	438
R^2	0.159	0.127	0.174	0.141
Control Group Mean of Dependent Variable	2.92	3.17	2.92	3.17

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column represents a different regression. All regressions with the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time (seconds) by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions with the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village that is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction the village faces. Outcome variables reflect answers to the following question: How much do you trust people from other (your own) community in your village? [Not a lot / Just a little / Somewhat / A lot]. The *Equality of Coefficients (p-value)* tests for the equality of Radio Rwanda coefficients between columns 1 and 2 and columns 3 and 4.

Table 6: Trust game offers and Radio Rwanda reception

Dependent Variable:	Trust Game Offers				log(Trust Game Offers)	
Partnership Type:	Inter-Ethnic (1)	Co-Ethnic (2)	Inter-Ethnic (3)	Co-Ethnic (4)	Inter-Ethnic (5)	Co-Ethnic (6)
	Panel A: Private Trust Game Offers					
Radio Rwanda Reception	148.5***	10.79	168.6***	-53.01	0.476**	-0.113
	(52.51)	(32.70)	(54.43)	(41.97)	(0.228)	(0.118)
Equality of Coefficients: Inter-ethnic - Co-ethnic (p-value)	0.004		0.0001		0.003	
Controls	Baseline		Full		Full	
Trust game-specific controls	Yes		Yes		Yes	
Observations	92	150	92	150	92	150
R^2	0.670	0.406	0.718	0.489	0.698	0.508
Control Group Mean of Dependent Variable	325	343	325	343	6.38	6.45
	Panel B: Public Trust Game Offers					
Radio Rwanda Reception	-37.60	-1.248	0.473	-52.67	0.174	-0.164
	(59.07)	(55.85)	(77.24)	(65.05)	(0.217)	(0.195)
Equality of Coefficients: Inter-ethnic - Co-ethnic (p-value)	0.570		0.479		0.121	
Equality of Coefficients: Private - Public (p-value)	0.0001	0.799	0.002	0.994	0.090	0.716
Controls	Baseline		Full		Full	
Trust game-specific controls	Yes		Yes		Yes	
Observations	71	125	71	125	71	125
R^2	0.744	0.388	0.799	0.484	0.843	0.459
Control Group Mean of Dependent Variable	355	358	355	358	6.49	6.47

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column represents a different regression. All regressions with the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time (seconds) by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions also include trust-game specific controls: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. All regressions with the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village that is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction the village faces. All outcome variables reflect the sender's offer to the receiver in the trust game. The *Equality of Coefficients Inter-ethnic - Co-ethnic (p-value)* tests for the equality of Radio Rwanda coefficients between columns 1 and 2 and columns 3 and 4 and columns 5 and 6, always within the same panel. The *Equality of Coefficients: Private - Public (p-value)* tests for the equality of Radio Rwanda coefficients between panel A and panel B, always within the same column.

Supplemental Appendix (For Online Publication)

Appendix A. Additional Figures and Tables

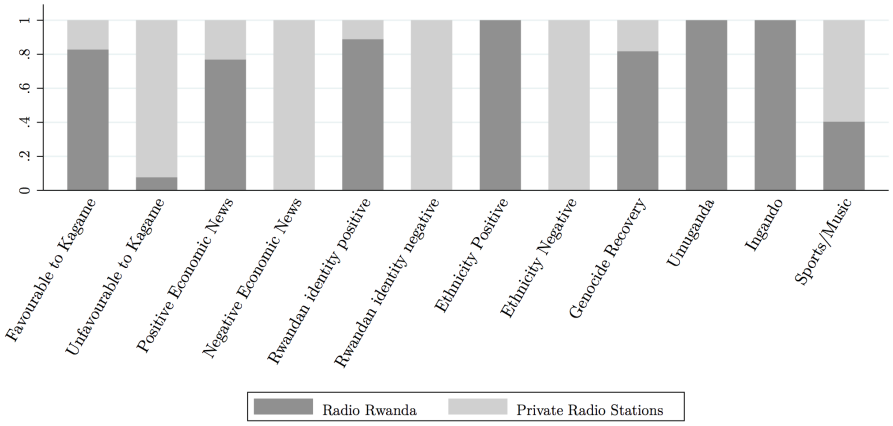


Figure A1: Share of discussion topics that appear on Radio Rwanda

Notes: The figure was constructed using data collected by a Rwandan research assistant who listened to over 50 hours of radio broadcasts in Rwanda in 30 minute segments. For each segment he wrote down whether any of a number of pre-specified topics were mentioned. For the ethnicity based variables this requires some reading between the lines, as ethnicity is almost never explicitly mentioned over the radio. In this graph, for each topic we calculate the share of mentions that appeared on a Radio Rwanda relative to any other station operating in the country. The x-axis lists the topics that were tracked. The y-axis shows the percentage of mentions of that topic appearing on Radio Rwanda relative to any other station.

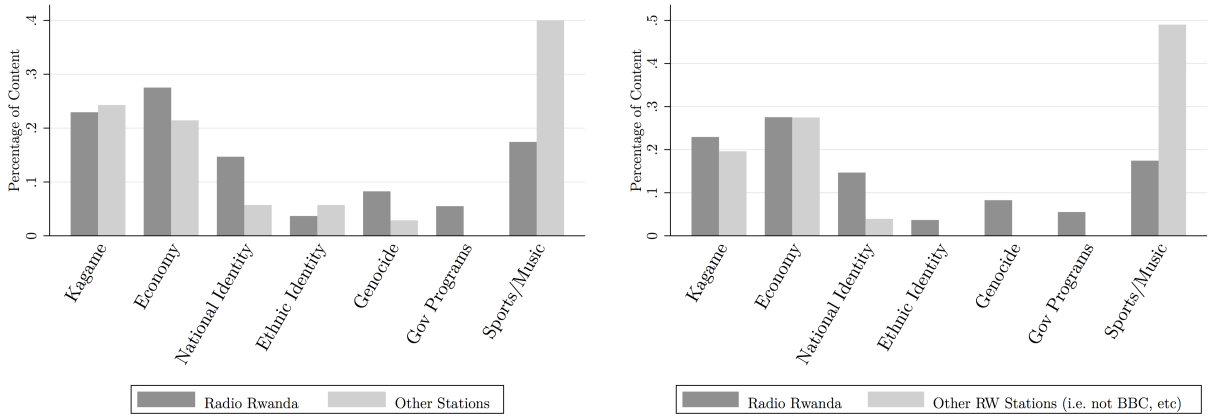


Figure A2: Share of programming dedicated to each topic

Notes: The figure was constructed using data collected by a Rwandan research assistant who listened to over 50 hours of radio broadcasts in Rwanda in 30 minute segments. For each segment he wrote down whether any of a number of pre-specified topics were mentioned. For the ethnicity based variables this requires some reading between the lines, as ethnicity is almost never explicitly mentioned over the radio. In this graph, for each topic we calculate the share of mentions that appeared on a Radio Rwanda relative to any other station operating in the country. The x-axis lists the topics that were tracked. The y-axis shows the percentage of content on Radio Rwanda as well as the other stations that is devoted to each topic. The panel on the left includes all stations while the panel on the right ignores the international radio stations like BBC and Voice of America, which may self-censor less than the stations operating from within Rwanda.

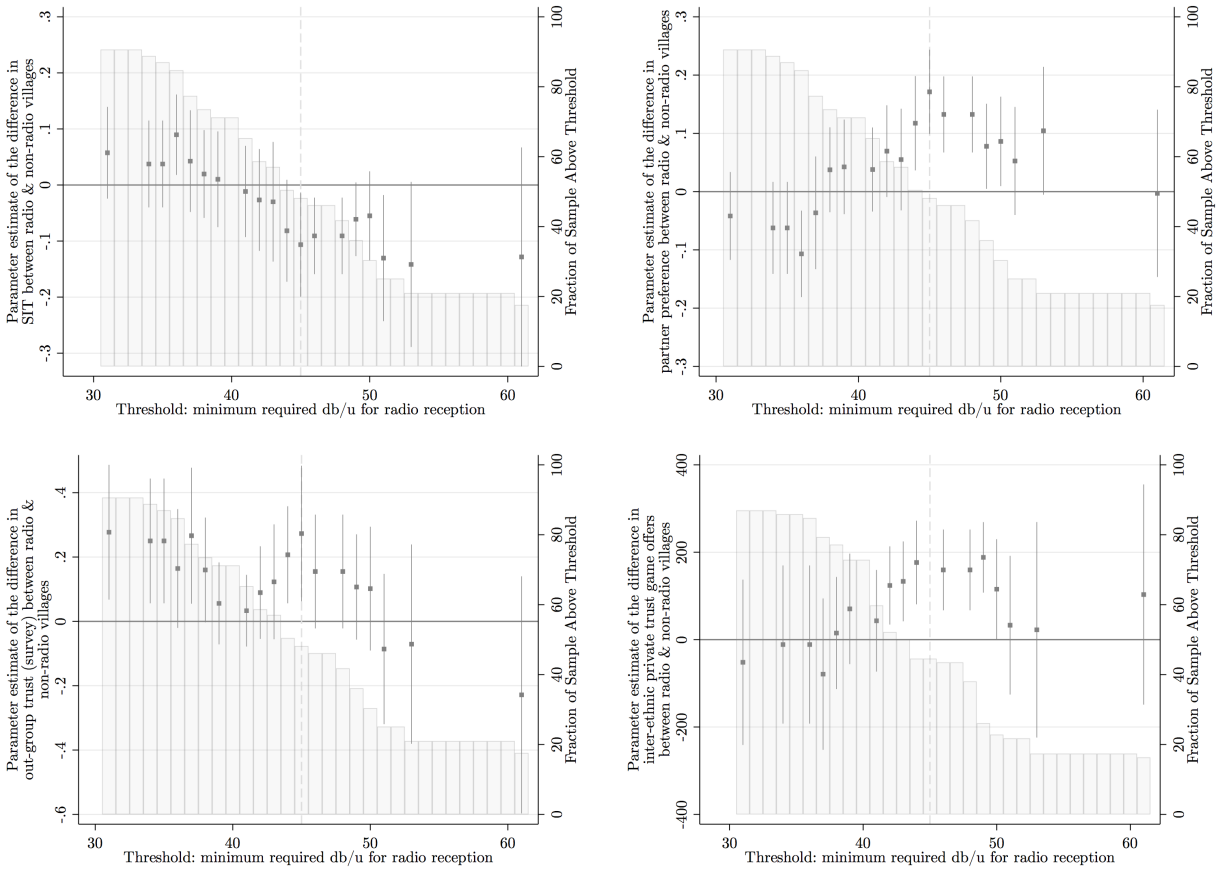


Figure A3: Four Main Outcomes And Radio Rwanda Reception Using Full Controls

Note: The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of $1db$). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95% confidence intervals. Each point and confidence interval is from a separate regression. Each regression includes the same set of controls, which is the full set of controls from the tables. This includes all variables in table 1 panel B and C. The outcome measures we use the same measure used in tables 3-6. Confidence intervals are constructed using standard errors clustered at the village level. The dashed vertical line indicates the threshold used in the main results (45 db/μ).

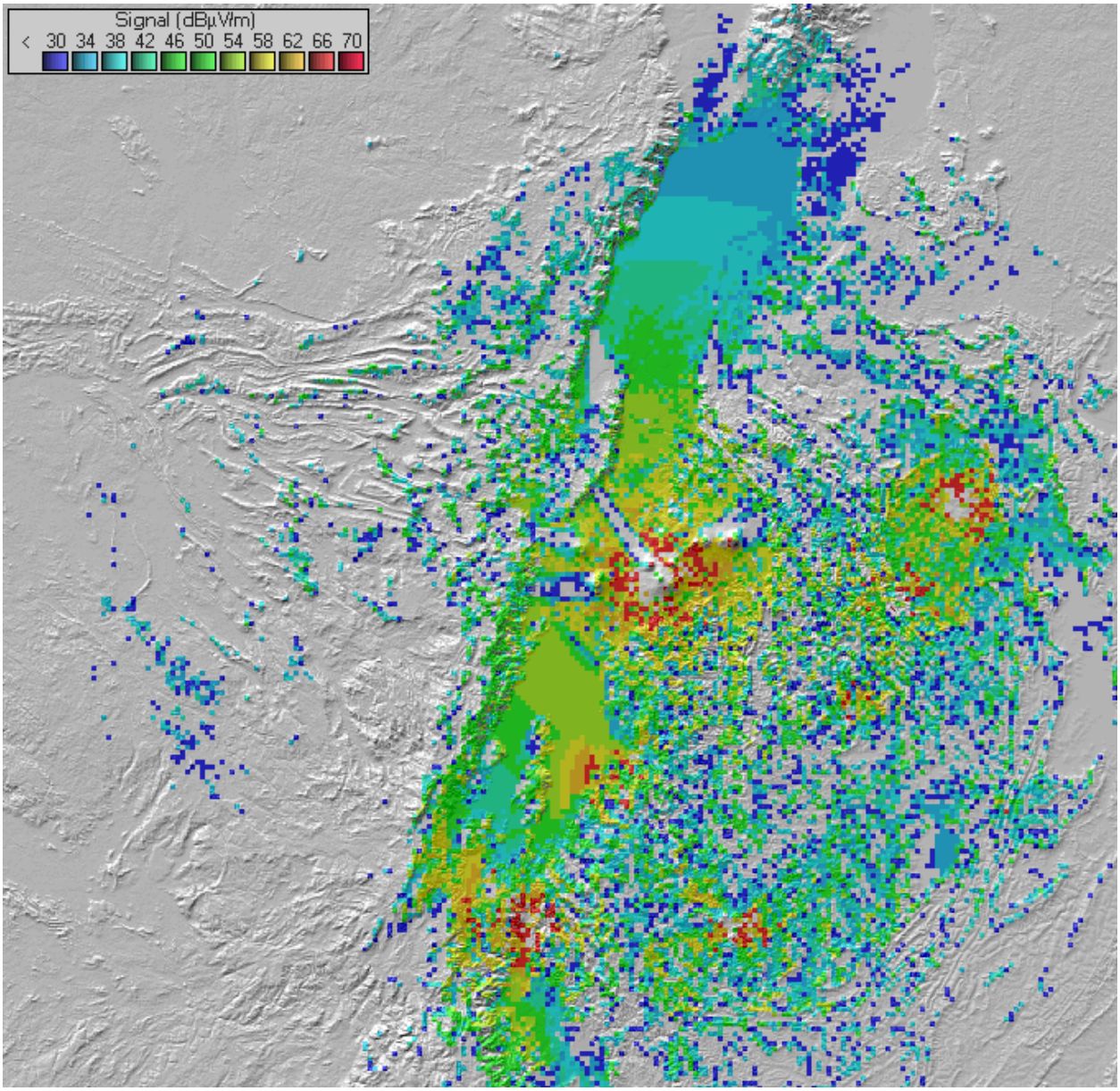


Figure A4: Raw signal data

Note: The figure shows the signal strength of Radio Rwanda, computed by the authors. Details of how the signal strength variable was constructed can be found in Appendix C.

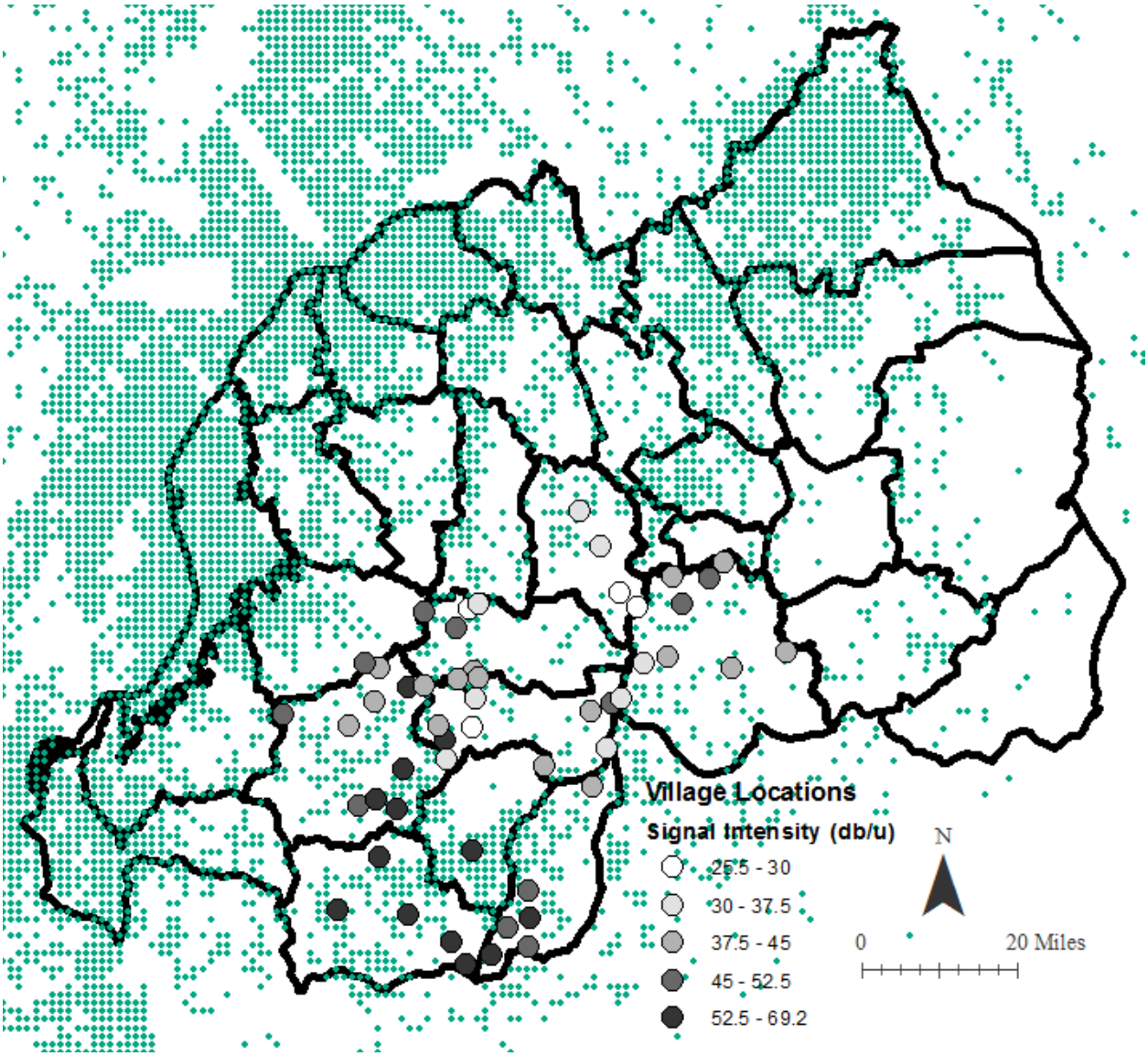


Figure A5: Radio Signal Data represented as points at the centroid of each pixel

Notes: This is a map of Radio Rwanda signal strength in Rwanda. We have overlaid the district administrative boundaries and the subject locations. Subject villages colored in teal are villages that receive a Radio Rwanda signal of over $45db/\mu$ while those colored in pink receive a signal less than $45db/\mu$. Because of the sampling procedure (see appendix D for details), each star represents about 8 or 9 subjects. The figure shows the conversion of the raw radio signal data shown in figure A4, but as points at the centroid of each cell. The resolution is of the radio signal data is 0.015 decimal degrees. Every dot represents a cell that receives a radio signal (defined using a $45db/\mu$ cutoff), conversely cells that do not receive a signal are blank. Villages are often extremely small, so to protect the anonymity of the subjects we have altered, randomly, the locations of each village by up to about 10km for the purpose of the map. Precisely, the random perturbation is less than or equal to 0.1 decimal degrees from the actual village location. The assignment of radio signal to a village is based on the true location of the village rather than the altered location of the village.

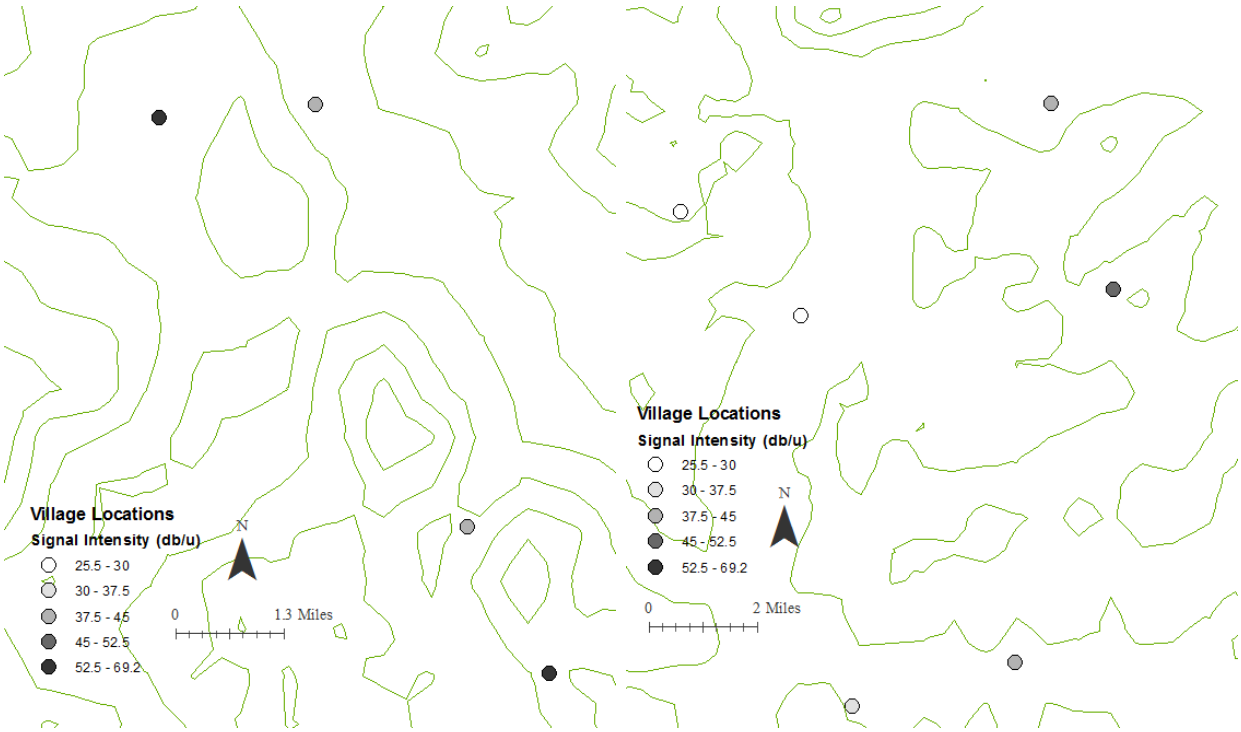


Figure A6: Zoomed image showing how pixels were matched to village locations

Note: This figure shows a very up-close view of the villages in the sample in order to get a sense of where the identification comes from. We have overlaid a contour plot on the subject locations. Each line on the contour plot denotes a constant elevation so that continuous ring separating two locations implies that the villages are separated by either a mountain or a valley. The contour interval in both maps are set to 100 meters so that a new line is drawn for every 100 meter change in elevation. Circles represent the village centroid of the subjects. Each star represents between 8 and 9 subjects. Circles are shaded to represent the signal strength of Radio Rwanda.

Table A1: Subject participation and Radio Rwanda reception

Dependent Variables:	**Difference between subject characteristic and sector-level averages from the census**			
Variables	**Hutu**	**Male**	**Age**	**Education (years)**
	(1)	(2)	(3)	(4)
Radio Rwanda Signal	0.142*	-0.0371	-0.444	0.0348
	(0.0788)	(0.0695)	(0.361)	(0.0517)
Public Information Treatment	-0.0122	0.00504	-0.0309	0.00157
	(0.00951)	(0.0126)	(0.0416)	(0.00882)
Controls	Baseline	Baseline	Baseline	Baseline
Observations	438	438	438	438
R^2	0.454	0.483	0.702	0.455
Control group mean of dependent variable	0.454	0.483	0.702	0.455

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions were estimated with the baseline set of controls which includes: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. Each outcome variable is the difference between the subjects characteristic and the average characteristic from their sector from the last census. For the ethnicity variable we match to the 1991 census. For example, in column 1 on average we received 14% fewer Hutu than were listed in the census from the same sector in Radio Rwanda villages.

Table A2: Unconditional correlation between 4 main outcomes

	SIT	Partner Selection	Out-group Trust (survey)	Inter-ethnic Trust (trust game)
SIT (N=438)	1			
Partner Selection (N=438)	-0.0045	1		
Out-Group Trust (survey) (N=438)	0.044	-0.032	1	
Inter-ethnic Trust (trust game) (N=92)	-0.027	0.167	0.034	1

Note: The table reports unconditional correlations between the main four outcomes in the paper. We use the full sample of 438 respondents for the correlations between the SIT, the Partner Selection measure and the out-group trust surveys. We use the sample of those that played the inter-ethnic private trust game when we report the correlations between the inter-ethnic trust game offers and the SIT, partner selection measure and the out-group trust survey.

Table A3: Main results with a control for being assigned to be matched to a chosen partner

Dependent Variable:	SIT (1)	Partner Selection (2)	Out-group Trust (survey) (3)	Inter-ethnic Trust Game Offer (4)
Radio Rwanda reception	-0.144***	0.152***	0.265**	149.8***
	(0.0425)	(0.0277)	(0.106)	(52.34)
Baseline controls	Yes	Yes	Yes	Yes
Match control	Yes	Yes	Yes	Yes
Observations	438	438	438	92
R^2	0.221	0.407	0.161	0.677

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column reports estimates from separate regressions. All regressions estimated with the baseline set of controls include the following variables: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. The match control examined here is a control for whether the respondent was one of the individuals from each session chosen to be matched with one of their five preferred partners.

Table A4: SIT score and trust game offer

Dependent Variable:	Trust Game Offers				log(Trust Game Offers)	
Partnership Type:	Inter-Ethnic (1)	Co-Ethnic (2)	Inter-Ethnic (3)	Co-Ethnic (4)	Inter-Ethnic (5)	Co-Ethnic (6)
	Panel A - Dependent Variable: Private Trust Game Offers					
SIT	-146.8***	6.35	-140.4***	12.87	-0.492***	-0.0265
	(45.02)	(39.91)	(47.73)	(39.30)	(0.133)	(0.108)
Equality of Coefficients: Inter-ethnic - Co-ethnic (p-value)	0.001		0.001		0.0002	
Controls	Baseline + Village FE		Full + Village FE		Full + Village FE	
Trust game-specific controls	Yes		Yes		Yes	
Observations	92	150	92	150	92	150
R^2	0.884	0.634	0.893	0.656	0.876	0.639
Control Group Mean of Dependent Variable	391	371	391	371	6.62	6.56
	Panel B - Dependent Variable: Public Trust Game Offers					
SIT	34.76	-17.21	19.02	-34.15	0.0073	-0.172
	(110.43)	(88.89)	(117.17)	(85.23)	(0.342)	(0.254)
Equality of Coefficients: Inter-ethnic - Co-ethnic (p-value)	0.663		0.608		0.526	
Equality of Coefficients: Private - Public (p-value)	0.079	0.773	0.100	0.547	0.062	0.533
Controls	Baseline + Village FE		Full + Village FE		Full + Village FE	
Trust game-specific controls	Yes		Yes		Yes	
Observations	71	125	71	125	71	125
R^2	0.847	0.590	0.849	0.617	0.878	0.574
Control Group Mean of Dependent Variable	300	354	300	355	6.37	6.52

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column in each panel reports estimates from separate regressions. All regressions estimated with the baseline set of controls include the following variables: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions estimated with the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces. The SIT variable we use here is a binary variable indicating that the subject made more than 42% of their errors within-ethnicity in the SIT recall task. Results are similar using the continuous version of the variable. All outcome variables reflect the sender's offer to the receiver in the trust game. The *Equality of Coefficients Inter-ethnic - Co-ethnic (p-value)* tests for the equality of Radio Rwanda coefficients between columns 1 and 2 and columns 3 and 4 and columns 5 and 6, always within the same panel. The *Equality of Coefficients: Private - Public (p-value)* tests for the equality of Radio Rwanda coefficients between panel A and panel B, always within the same column.

Table A5: Radio towers in Rwanda

Tower	Latitude	Longitude	Directional	Power	Height+Elevation (meters)	Stations
Mount Karongi	-2.147	29.376	N	2.5	2,386.8	RR and others
Kigali/Mount Jari	-1.886	30.01	N	0.5	1,861.1	RR and others
Butare	-2.6	29.733	N	1	1,719.5	Others only
Kibagabaga	-1.933	30.117	N	1	1,472.8	Others only
Kigali	-1.944	30.058	N	2	1,503.8	RR and others
Kicukiro	-1.996	30.1	N	0.5	1,604.8	Others only
Mount Karisimbi	-1.506	29.45	D	5	4,468.7	RR and others
Kigali/Mount Rebero	-1.993	30.074	N	1	1,733.7	Others only
Kibungo	-2.197	30.529	N	10	1,495.9	RR and others
Mount Huye	-2.564	29.684	N	1	2,074.3	RR and others
Gitarama	-2.0667	29.75	N	2	1,814.7	Others only
Cyangugu	-2.483	28.9	N	0.3	1,590.2	Others only
Butare/NUR	-2.613	29.739	N	1	1,720.4	Others only
Nyagatare	-1.3	30.317	N	1	1,346.6	Others only
Mutara	-1.515	30.466	N	1	1,394.9	Others only
Gisenyi	-1.699	29.269	N	1	1,610.3	Others only
Kinanira	-2.541	28.976	N	1	2,040.6	RR and others
Rusizi	-2.65	29.017	N	1	995.3	Others only
Nyarupfubire	-1.27	30.4	N	5	1,551.9	RR only
Mount Mugogo	-1.586	29.56	N	0.3	2,397.7	RR only
Rushaki	-1.455	30.103	N	5	2,097.2	RR only
Musanze	-1.4833	29.617	N	1	1,920.4	RR only
Nyabitimbo	-2.578	29.066	N	1	1,727.7	RR only
Kibuye	-2.056	29.35	N	1	1,466	RR only
Gihundwe	-2.476	28.918	N	1	1,648	RR only
Buyumba	-1.58	30.07	N	1	2,190	RR only
Bwisige	-1.593	30.187	N	0.3	1,968.5	RR only

Note: The information in this table is used to construct the radio networks in ArcGIS. The source for the tower data comes from FMScan.org. See Appendix 3 for details on how to reconstruct the radio signal data.

Table A6: Selection of Radio Reception Definition

Dependent Variable:	**Reported Reception of Radio Rwanda**				
Threshold Used:	35db/μ	40db/μ	45db/μ	50db/μ	55db/μ
	(1)	(2)	(3)	(4)	(5)
	Panel A: > 20% of pixels receive a signal				
Signal Received	0.141	-0.142	1.134***	1.181***	0.531
	(0.238)	(0.352)	(0.269)	(0.393)	(0.469)
Observations	154	154	154	154	154
	Panel B: Fraction of pixels with a signal				
Signal Received	0.452	-0.873	2.314***	1.241*	0.553
	(0.444)	(0.755)	(0.733)	(0.669)	(0.622)
Observations	154	154	154	154	154
	Panel C: > 50% of pixels receive a signal				
Signal Received	0.0713	-0.249	0.779	0.293	0.208
	(0.388)	(0.570)	(0.575)	(0.564)	(0.351)
Observations	154	154	154	154	154
	Panel D: > 75% of pixels receive a signal				
Signal Received	0.154	0.123	0.298	0.208	0.228
	(0.311)	(0.680)	(0.565)	(0.457)	(0.429)
Observations	154	154	154	154	154
Baseline Controls (all panels)	Yes	Yes	Yes	Yes	Yes

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column in each panel reports estimates from separate regressions. All regressions estimated with the baseline set of controls include the following variables: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. The dependent variable in each regression is a subjects score of the quality of the signal their village receives of Radio Rwanda. The resolution of the radio data is high enough that we typically observe 3-5 pixels within a village. We construct and test four different village level reception definitions against listening habits. They are (panel A) whether > 20% of the village receives a signal; (panel B) a continuous variable measuring the fraction of pixels within the village that receives a signal; (panel C) whether a simple majority (> 50%) of pixels within the village receive a signal; (panel D) whether a large majority (> 75%) of pixels within the village receive a signal.

Table A7: Robustness to using the share of locations receiving a signal

Dependent Variable	SIT	Partner Selection	Out-group Trust (survey)	Inter-ethnic Trust (trust game)
	(1)	(2)	(3)	(4)
Share of Locations Receiving a Signal	-0.281***	0.140**	0.165	162.04*
	(0.0950)	(0.0665)	(0.188)	(96.19)
Baseline Controls	Yes	Yes	Yes	Yes
Trust Game Specific Controls	No	No	No	Yes
Observations	438	438	438	92
R^2	0.218	0.392	0.153	0.651

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column in each panel reports estimates from separate regressions. The table examines robustness to an alternate definition of Radio Rwanda reception that table A6 suggests may be valid. Instead of examining whether any location in a village receives a Radio Rwanda signal, here we examine whether a greater share of locations receiving a signal is similarly associated with each of our main outcomes. All regressions are estimated with the baseline set of controls, which includes the following variables: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. Column 4 also includes trust-game specific controls for: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance.

Table A8: Main results without controlling for light density at night

Dependent Variable:	SIT	Partner Selection	Out-group Trust (survey)	Inter-ethnic Trust Game Offer
	(1)	(2)	(3)	(4)
Radio Rwanda reception	-0.132***	0.157***	0.250**	147.5**
	(0.0489)	(0.0254)	(0.106)	(55.63)
Baseline controls (excl. light density)	Yes	Yes	Yes	Yes
Trust Game Specific Controls	No	No	No	Yes
Observations	438	438	438	92
R^2	0.149	0.403	0.159	0.659

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column in each panel reports estimates from separate regressions. All regressions estimated with the baseline set of controls - except for the light density control - and include the following variables: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score.

Table A9: All covariates and outcomes by Radio Rwanda reception

Dependent Variable:	SIT		Partner Selection		Out-Group Trust (survey)		Inter-Ethnic Trust Game Offer	
Village Type:	Radio Rwanda (1)	No-Radio Rwanda (2)	Radio Rwanda (3)	No-Radio Rwanda (4)	Radio Rwanda (5)	No-Radio Rwanda (6)	Radio Rwanda (7)	No-Radio Rwanda (8)
Gender	-0.0925**	-0.0192	-0.0289	0.0405	-0.00943	0.107	16.34	-8.535
	(0.0390)	(0.0434)	(0.0369)	(0.0379)	(0.113)	(0.112)	(17.32)	(20.67)
Tutsi	0.00960	-0.0279	0.390***	0.386***	0.0113	-0.192*	-15.80	1.767
	(0.0455)	(0.0439)	(0.0431)	(0.0384)	(0.132)	(0.114)	(20.23)	(20.95)
Age	-0.000296	-0.00160	-0.00222	-0.00149	0.00628	0.00233	0.773	0.161
	(0.00161)	(0.00169)	(0.00153)	(0.00148)	(0.00467)	(0.00438)	(0.717)	(0.808)
Distance to nearest Tower	0.00244	0.0444	0.0536	-0.0613	0.483	0.0120	31.72	-53.88
	(0.116)	(0.0943)	(0.110)	(0.0824)	(0.336)	(0.244)	(51.70)	(44.95)
Distance to 2nd nearest Tower	0.0402	-1.745	-0.0262	-2.157	0.155	6.399	20.74	-501.0
	(0.0714)	(2.227)	(0.0676)	(1.947)	(0.206)	(5.760)	(31.71)	(1,062)
Distance to 3rd nearest Tower	-0.0551	1.041	-0.0522	1.369	-0.345	-3.948	-43.91	319.6
	(0.104)	(1.408)	(0.0981)	(1.231)	(0.299)	(3.641)	(46.02)	(671.3)
Driving time to nearest tower	-3.65e-05	0.00798	-5.42e-05	0.00961	-0.000592	-0.0285	0.00251	2.243
	(0.000146)	(0.00996)	(0.000139)	(0.00871)	(0.000423)	(0.0258)	(0.0650)	(4.750)
Driving time to 2nd nearest tower	0.000109	-0.00278	6.78e-06	-0.00250	0.000333	0.00868	-0.00575	-0.346
	(0.000293)	(0.00295)	(0.000278)	(0.00257)	(0.000848)	(0.00762)	(0.130)	(1.404)
Driving time to 3rd nearest tower	0.000198	-0.00555	3.59e-05	-0.00708	0.00226	0.0213	0.0547	-1.566
	(0.000981)	(0.00728)	(0.000929)	(0.00636)	(0.00284)	(0.0188)	(0.436)	(3.471)
Distance to major city	0.00222	0.388	-0.0539	0.648	-0.451*	-1.703	-18.82	189.0
	(0.0907)	(0.654)	(0.0859)	(0.572)	(0.262)	(1.691)	(40.31)	(311.7)
Distance to nearest road		-73.28		-82.61		258.2		-17,798
		(88.99)		(77.80)		(230.2)		(42,431)
Distance to Kigali	0.00120	0.635	0.0188	0.736	-0.0346	-2.281	3.617	143.4
	(0.0242)	(0.790)	(0.0229)	(0.691)	(0.0700)	(2.044)	(10.75)	(376.9)
RTLM	-0.0792	2.709	0.0938	2.676	-3.347	-8.156	-112.4	825.6
	(0.839)	(3.132)	(0.795)	(2.739)	(2.426)	(8.101)	(372.9)	(1,494)
Light Density	0.0820	0.530	0.0247	0.793	1.309	-1.984	112.3	200.4
	(0.277)	(0.761)	(0.263)	(0.665)	(0.802)	(1.968)	(123.2)	(362.8)
Raven Score	0.0149	0.0339**	0.0127	0.00925	-0.0384	-0.0654*	-2.408	0.675
	(0.0137)	(0.0153)	(0.0129)	(0.0133)	(0.0395)	(0.0394)	(6.068)	(7.272)
Elevation	-0.000527	-0.0128	-0.000620	-0.0225	0.00500	0.0581	-0.0331	-5.846
	(0.00145)	(0.0224)	(0.00138)	(0.0196)	(0.00420)	(0.0581)	(0.645)	(10.70)
Variance in Elevation	-2.20e-06	0.00318	3.06e-07	0.00400	-4.44e-06	-0.0113	-0.000367	1.173
	(1.89e-06)	(0.00415)	(1.79e-06)	(0.00363)	(5.46e-06)	(0.0107)	(0.000839)	(1.977)
Cell use in village	0.211	-4.925	-0.0238	-6.028	2.087	18.64	84.04	-1,365
	(0.462)	(6.115)	(0.438)	(5.346)	(1.336)	(15.82)	(205.3)	(2,916)
Genocide violence	0.162	10.60	-0.124	11.90	-1.273	-37.07	30.91	2,516
	(0.390)	(12.54)	(0.370)	(10.96)	(1.129)	(32.43)	(173.5)	(5,978)
log(income)	-0.0235	-0.00115	0.00520	-0.00228	-0.0124	-0.0134	8.228	4.102
	(0.0164)	(0.0165)	(0.0155)	(0.0144)	(0.0474)	(0.0426)	(7.288)	(7.847)
Share of Village that is Tutsi	0.196	30.89	0.283	37.46	1.562	-108.3	74.72	9,464
	(0.919)	(38.70)	(0.870)	(33.83)	(2.656)	(100.1)	(408.2)	(18,451)
Education	0.00884	0.00201	-0.00623	0.00378	-0.0197	0.00362	2.199	10.90***
	(0.00726)	(0.00842)	(0.00688)	(0.00736)	(0.0210)	(0.0218)	(3.226)	(4.013)
log(sector population)	1.215	15.40	-0.0160	14.17	1.569	-50.14	338.8	2,151
	(1.113)	(16.27)	(1.054)	(14.22)	(3.218)	(42.08)	(494.6)	(7,758)
Population Density	-2.31e-05	-0.0168	-1.25e-06	-0.00354	-0.000149	0.0454	-0.00963	5.179
	(7.20e-05)	(0.0194)	(6.82e-05)	(0.0170)	(0.000208)	(0.0502)	(0.0320)	(9.252)
North Facing Village	0.257	-6.284	0.00212	-7.345	1.337	22.37	112.4	-1,657
	(0.282)	(7.676)	(0.267)	(6.711)	(0.816)	(19.85)	(125.3)	(3,660)
South Facing Village	0.151	-7.747	-0.00629	-8.291	-0.0806	26.02	100.9*	-1,847
	(0.130)	(8.788)	(0.123)	(7.683)	(0.376)	(22.73)	(57.86)	(4,190)
East Facing Village	0.182	-3.877	0.146	-5.001	0.435	14.32	110.3	-1,282
	(0.250)	(5.138)	(0.237)	(4.492)	(0.724)	(13.29)	(111.3)	(2,450)
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enumerator Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	211	227	211	227	211	227	211	227
R^2	0.398	0.259	0.430	0.492	0.208	0.244	0.199	0.319
Group Mean of Dependent Variable	0.859	0.855	0.447	0.420	3.08	2.93	318	345

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column represents a separate regression. The distance variables are all in km, while the driving times are in seconds. The elevation data is in meters. Raven Score is the number of correct answers, with a maximum of eight.

Table A11: Main results (sector fixed-effects)

Dependent Variable:	**SIT**	**Partner Selection**	**Out-Group Trust (survey)**	**Inter-Ethnic Trust Game Offer**
	(1)	(2)	(3)	(4)
Radio Rwanda Signal	-0.178***	0.0671**	0.0958	76.88***
	(0.0562)	(0.0283)	(0.125)	(22.32)
Sector FE	Yes	Yes	Yes	Yes
Distance to Nearest towers	Yes	Yes	Yes	Yes
Radio RTLM	Yes	Yes	Yes	Yes
Trust-game specific controls	No	No	No	No
Observations	438	438	438	92
R^2	0.146	0.101	0.103	0.457
Control Group Mean of Dependent Variable	0.916	0.420	2.93	325

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Sector (rather than the typical district) fixed effects are included along with controls for both distance (km) and travel time by car to first, second, and third nearest radio towers were included. A control for RTLM was also included. We do not use the full control set because with the inclusion of the sector fixed-effects we begin to lose statistical power.

Table A12: SIT Errors

Dependent Variable:	**# SIT Errors**	
	(1)	(2)
Radio Rwanda	0.331	0.539*
	(0.225)	(0.276)
Controls	Baseline	Full
Observations	438	438
R^2	0.229	0.273
Control Group Mean of Dependent Variable	1.76	1.76

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. The regression using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; Radio RTLM dummy variables; light density; distance (km) to nearest road, nearest city, and Kigali; Prevalence of cell phone usage at the village level; and Raven Cognitive Test score. The regression using the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces.

Table A10: Regression coefficients for estimates of each covariate on Radio Rwanda

Panel A

Dependent Variable	Gender (1)	Tutsi (2)	Age (3)	RTLM (4)	Lights (5)	Distance City (6)	Distance Roads (7)	Distance Capital (8)	Raven (9)	Cell (10)	Genocide (11)
Radio Rwanda Signal	0.0294	-0.0301	1.095	0.0235	-0.402***	-9.099***	-0.0395***	28.79***	-0.0993	-0.0525***	-0.102**
	(0.0470)	(0.0428)	(1.183)	(0.0381)	(0.101)	(1.001)	(0.00582)	(2.086)	(0.128)	(0.0191)	(0.0429)
Observations	438	438	438	438	438	438	438	438	438	438	438
R^2	0.001	0.001	0.002	0.001	0.035	0.159	0.095	0.304	0.001	0.017	0.013
Control Group Mean of Dependent Variable	1.39	0.291	42.8	0.185	0.736	33.03	1.12	45.9	5.44	0.718	0.566

Panel B

Dependent Variable	log(income) (12)	Share Tutsi (13)	Education (14)	Elevation (15)	Variance Elevation (16)	log(sector pop.) (17)	Pop. Dens. (18)	North (19)	South (20)	East (21)	West (22)
Radio Rwanda Signal	-0.361***	-0.00523	-0.351	122.9***	52,018***	-0.0950***	1,254***	-0.101**	0.128***	0.0962**	-0.123***
	(0.120)	(0.0117)	(0.245)	(16.73)	(8,093)	(0.0315)	(341.1)	(0.0410)	(0.0411)	(0.0443)	(0.0369)
Observations	438	438	438	438	438	438	438	438	438	438	438
R^2	0.020	0.000	0.005	0.110	0.087	0.020	0.030	0.014	0.022	0.011	0.025
Control Group Mean of Dependent Variable	5.55	0.278	5.93	1,587	1,128	10.2	411	0.295	0.189	0.269	0.246

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column represents a separate regression. Panel A and panel B present regressions of Radio Rwanda on the 22 main covariates used in this paper. The two panels do not differ in methodology, and were only broken apart for legibility purposes. There are no controls included any any of the regressions other than a constant.

Table A13: Robustness of Measures to a Continuous Measure of Radio Reception

Dependent Variable:	**SIT**	**Partner Selection**	**Out-Group Trust (survey)**	**Inter-Ethnic Trust Game Offer**
	(1)	(2)	(3)	(4)
Radio Rwanda Signal (Cont.)	-0.00952**	0.00421*	0.00933*	3.060
	(0.00396)	(0.00237)	(0.00487)	(3.361)
Controls	Full	Full	Full	Full
Trust-game specific controls	No	No	No	Yes
Observations	438	438	438	92
R^2	0.191	0.411	0.171	0.702

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the full set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; Raven Cognitive Test score; intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces. Column 4 also includes trust-game specific controls for: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance.

Table A14: Partner Selections for Income and Radio Rwanda

Dependent Variable:	**Avg. log(income) of choices**	
	(1)	(2)
Radio Rwanda	0.143	0.271
	(0.315)	(0.307)
Controls	Baseline	Full
Observations	438	438
R^2	0.089	0.111
Control Group Mean of Dependent Variable	11.63	11.63

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. The regression using the baseline set of controls includes: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; Radio RTLM dummy variables; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. The regression using the full set of controls include all of the baseline controls as well as: intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces. The dependent variable is the average income of the 5 choices the subject made in the partner selection task.

Table A15: Pooled Trust Game Regressions

Dependent Variable:	**Trust Game Offers**							
Sample:	**inter-ethnic**				**co-ethnic**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio	61.63	110.3**	57.09	125.6***	-4.349	1.736	7.040	13.35
	(37.08)	(43.49)	(37.63)	(42.02)	(31.10)	(30.13)	(30.69)	(29.41)
Public	6.660	54.66*	32.92	77.81	16.33	23.99	65.95	70.41
	(18.88)	(27.65)	(67.28)	(71.45)	(17.33)	(27.61)	(64.56)	(67.33)
Radio x Public		-89.24***		-122.8***		-16.70		-17.07
		(32.36)		(38.26)		(33.62)		(32.55)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trust Game Specific Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions Between Trust Game and Public	No	No	Yes	Yes	No	No	Yes	Yes
Observations	163	163	163	163	275	275	275	275
R^2	0.514	0.537	0.543	0.578	0.226	0.227	0.258	0.259

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column reports estimates from a separate regression. All regressions estimated with the baseline set of controls include the following variables: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. Each column also includes trust-game enumerator fixed effects as well as controls for: gender of partner; whether partner receives Radio Rwanda; distance between partners' villages; and risk tolerance. We also in two columns include interactions between whether the game was public or private and the following (trust game specific) controls: gender of partner; whether partner receives Radio Rwanda; distance between partners' villages; and risk tolerance. The dependent variable in each case is the offer made by the sender to the receiver in the first stage of the trust game.

Table A16: Contract dispute resolution and Radio Rwanda reception

Dependent Variable:	**External Punishment**		**Internal Punishment**		**No Punishment**	
	(1)	(2)	(3)	(4)	(5)	(6)
Radio Rwanda Signal	0.0292	-0.0316	0.0921	0.000172	-0.0957*	-0.0751
	(0.0720)	(0.0814)	(0.0671)	(0.0989)	(0.0490)	(0.0523)
Controls	Baseline	Full	Baseline	Full	Baseline	Full
Observations	438	438	438	438	438	438
R^2	0.230	0.261	0.349	0.362	0.220	0.238

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the baseline set of controls include: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; Radio RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. The full set of controls includes all the baseline controls as well as: intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces. The dependent variable relies on data from Blouin (2016), which asks subjects about how they punish people that have wronged them in a contract dispute. External punishment is defined as either going to government or spreading rumours in response to broken contracts. Internal punishment is defined as either restricting future business with an individual who breaks a contract, or specifying fines in advance.

Table A17: Assignment to public trust game treatment and trust game offers

Dependent Variable:	Trust Game Offers				log(Trust Game Offers)	
Village Type:	Radio Rwanda	No-Radio Rwanda	Radio Rwanda	No-Radio Rwanda	Radio Rwanda	No-Radio Rwanda
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Inter-ethnic Trust Game Offers					
Assignment to Public Trust Game	-31.32	54.96	-30.07	68.22**	-0.0724	0.254
	(23.37)	(33.32)	(26.09)	(32.43)	(0.0784)	(0.152)
Equality of Coefficients: RR - No RR (p-value)	0.006		0.0007		0.0049	
Controls	Baseline		Full		Full	
Trust game-specific controls	Yes		Yes		Yes	
Observations	83	80	83	80	83	80
R^2	0.676	0.709	0.726	0.773	0.787	0.730
Control Group Mean of Dependent Variable	346	325	346	325	6.48	6.37
	Panel B: Co-ethnic Trust Game Offers					
Assignment to Public Trust Game	-0.505	20.92	-2.359	0.589	0.00264	-0.0257
	(23.65)	(28.50)	(27.36)	(30.14)	(0.0910)	(0.0951)
Equality of Coefficients: RR - No RR (p-value)	0.473		0.919		0.795	
Equality of Coefficients: Inter-ethnic - Co-ethnic (p-value)	0.231	0.323	0.271	0.017	0.323	0.012
Controls	Baseline		Full		Full	
Trust game-specific controls	Yes		Yes		Yes	
Observations	128	147	128	147	128	147
R^2	0.417	0.328	0.447	0.469	0.466	0.445
Control Group Mean of Dependent Variable	306	343	306	343	6.35	6.45

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. Each column represents a different regression. All regressions with the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time (seconds) by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions also include trust-game specific controls: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. All regressions with the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village that is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction the village faces. All outcome variables reflect the sender's offer to the receiver in the trust game. The *Equality of Coefficients: RR - No RR (p-value)* tests for the equality of Public Trust Game coefficients between columns 1 and 2 and columns 3 and 4 and columns 5 and 6, always within the same panel. The *Equality of Coefficients: Inter-ethnic - Co-ethnic (p-value)* tests for the equality of Public Trust Game coefficients between panel A and panel B, always within the same column.

Table A18: Radio Stations and Broadcast Towers in Rwanda

Station	Number of Towers	Tower 1	Tower 2	Tower 3	Tower 4	Tower 5	Total Land Coverage (% RW)
BBC Africa	3	Mount Karongi	Kigali/Mount Jari	Butare			5.3%
Choice FM	1	Kibagabaga					1.0%
City Radio	1	Kigali					0.1%
Contact FM	1	Kicukiro					2.5%
Deutsche Welle	1	Kigali					1.0%
Flash FM	1	Kigali/Mount Jari					1.0%
Magic FM	1	Mount Karisimbi					36.7%
Radio 1	1	Mount Karisimbi					36.7%
Radio 10 FM	2	Kigali/Mount Jari	Kigali/Mount Rbro				3.5%
Radio Ijwi Ry'ibyiringiro	1	Kigali					1.0%
Radio Izuba	1	Kibungo					0.5%
Radio Maria-Nyina wa Zombo	5	Mount Huye	Gitarama	Kigali/Mount Jari	Cyangugu	Mount Karongi	31.2%
Radio Salus	2	Butare/NUR	Butare				1.8%
Radio Umucyo	1	Kigali					1.0%
RC Huye	2	Gitarama	Butare				1.5%
RC Musanze	1	Mount Karisimbi					36.7%
RC Nyagatare	2	Nyagatare	Mutara				1.4%
R.Communitaire de Butare	1	Butare					0.8%
R.Communitaire de Cyangugu	1	Cyangugu					1.2%
RC Rabavu	2	Mount Karongi	Gisenyi				4.6%
RC Rusizi	3	Kinanira	Mount Karongi	Rusizi			7.8%
RFI Afrique	1	Kigali					1.0%
Sana Radio/Restore FM	1	Kigali					1.0%
VOA Africa	1	Kigali					1.0%
Radio Rwanda	17	See table titled: Radio Towers in Rwanda					44.1%
Potential Coverage (All towers)	27	See table titled: Radio Towers in Rwanda					44.1%

Note: The information tower and station information in this table is used to construct the radio networks in ArcGIS. The source for the tower data comes from FMScan.org. See Appendix 3 for details on how to reconstruct the radio signal data. The national coverage of each station is author calculation.

Table A19: Trust-game offers and reception of stations other than Radio Rwanda

Dependent Variable:	Trust Game Offers													
Station Signal Used	**Any other station**		**Placebo**		**Zombo**		**Radio 10**		**BBC**		**ContactFM**		**Salus**	
Sample:	Inter-Ethnic (1)	Co-Ethnic (2)	Inter-Ethnic (3)	Co-Ethnic (4)	Inter-Ethnic (5)	Co-Ethnic (6)	Inter-Ethnic (7)	Co-Ethnic (8)	Inter-Ethnic (9)	Co-Ethnic (10)	Inter-Ethnic (11)	Co-Ethnic (12)	Inter-Ethnic (13)	Co-Ethnic (14)
	Panel A - Dependent Variable: Private Trust Game Offers													
Reception in village of station	-71.93	-42.92	31.76	-15.34	13.59	78.75	-111.5	-159.4*	-208.4**	-114.8*	-66.30	15.69	-54.65	-134.3***
	(102.3)	(59.56)	(48.89)	(28.10)	(168.0)	(104.4)	(132.8)	(90.65)	(95.51)	(64.56)	(121.6)	(89.73)	(123.3)	(43.89)
Equality of Coefficients: Inter-ethnic - Co-ethnic (p-value)	0.953		0.447		0.987		0.747		0.285		0.596		0.751	
1 Controls	Baseline		Baseline		Baseline		Baseline		Baseline		Baseline		Baseline	
Trust-game specific controls	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Observations	92	150	92	150	92	150	92	150	92	150	92	150	92	150
R^2	0.633	0.408	0.633	0.407	0.631	0.408	0.636	0.417	0.650	0.412	0.633	0.406	0.632	0.418
Control Group Mean of Dependent Variable	336	341	331	338	338	336	333	328	338	329	333	327	340	331
	Panel B - Dependent Variable: Public Trust Game Offers													
Reception in village of station	46.34	92.19	19.79	36.18	-78.29	256.6**	93.54	-7.324	-110.5	-64.49	-61.25	-74.61	108.1	-118.6*
	(103.7)	(69.14)	(57.10)	(31.86)	(174.4)	(123.4)	(117.2)	(99.94)	(101.0)	(79.46)	(110.5)	(100.0)	(96.35)	(64.82)
Equality of Coefficients: Inter-ethnic - Co-ethnic (p-value)	0.766		0.542		0.028		0.332		0.986		0.881		0.006	
Equality of Coefficients: Public - Private (p-value)	0.164	0.063	0.805	0.182	0.319	0.144	0.049	0.195	0.225	0.744	0.852	0.509	0.089	0.685
Controls	Baseline		Baseline		Baseline		Baseline		Baseline		Baseline		Baseline	
Trust-game specific controls	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Observations	71	125	71	125	71	125	71	125	71	125	71	125	71	125
R^2	0.742	0.395	0.742	0.393	0.742	0.407	0.745	0.388	0.745	0.389	0.742	0.390	0.749	0.396
Control Group Mean of Dependent Variable	330	355	364	342	322	353	333	338	328	342	326	338	327	346

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions included controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions also include trust-game specific controls: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. All outcome variables reflect the sender's offer to the receiver in the trust game. The *Equality of Coefficients Inter-ethnic - Co-ethnic (p-value)* tests for the equality of Radio Rwanda coefficients between columns 1 and 2 and columns 3 and 4 and columns 5 and 6, always within the same panel. The *Equality of Coefficients: Private - Public (p-value)* tests for the equality of Radio Rwanda coefficients between panel A and panel B, always within the same column. The variable *Any Other Station* is a binary variable equal to one if any of the other stations (not including the Placebo station) receives a signal strength in the village of greater than $45db/\mu$. The Placebo station is a variable capturing the hypothetical signal from the top 10 peaks in Rwanda that do not currently have a radio tower on them. It, as well as each of the other stations, is a binary variable equal to one if signal strength of that station in the village is greater than $45db/\mu$.

Table A20: Inter-ethnic bahavior and reception of stations other than Radio Rwanda

Dependent Variable:	**SIT**	**Partner Selection**	**Out-Group Trust (survey)**
	(1)	(2)	(3)
	Panel A - Station considered: Any other station		
Reception in village of station	-0.111	0.0370	-0.328
	(0.0808)	(0.0485)	(0.201)
Observations	438	438	438
R^2	0.405	0.406	0.163
	Panel B - Station considered: Placebo		
Reception in village of station	0.118***	-0.037	-0.069
	(0.0363)	(0.0366)	(0.0843)
Observations	438	438	438
R^2	0.415	0.407	0.159
	Panel C - Station considered: Zombo		
Reception in village of station	0.0182	0.106	-0.137
	(0.124)	(0.0835)	(0.301)
Observations	438	438	438
R^2	0.402	0.406	0.159
	Panel D - Station considered: BBC		
Reception in village of station	-0.462***	0.0352	0.341**
	(0.0668)	(0.0899)	(0.155)
Observations	438	438	438
R^2	0.427	0.406	0.161
	Panel E - Station considered: ContactFM		
Reception in village of station	-0.192**	-0.132	-0.146
	(0.078)	(0.0824)	(0.191)
Observations	438	438	438
R^2	0.406	0.408	0.159
	Panel F - Station considered: Salus		
Reception in village of station	-0.142	-0.177**	-0.273
	(0.102)	(0.0662)	(0.200)
Observations	438	438	438
R^2	0.405	0.411	0.161
Control Group Mean of Dependent Variable	0.851	0.423	2.92
Controls (All Panels)	Baseline	Baseline	Baseline
Mistake FE	Yes	No	No

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions included controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; Raven Cognitive Test score and Radio Rwanda. The variable *Any Other Station* is a binary variable equal to one if any of the other stations (not including the Placebo station) receives a signal strength in the village of greater than $45db/\mu$. The Placebo station is a variable capturing the hypothetical signal from the top 10 peaks in Rwanda that do not currently have a radio tower on them. It, as well as each of the other stations, is a binary variable equal to one if signal strength of that station in the village is greater than $45db/\mu$.

Table A21: Bonferroni Corrected P-Values for Each Station

				Trust Game			
				Private		Public	
Dependent Variable:	**SIT**	**Partner Selection**	**Trust Survey**	**inter-ethnic**	**co-ethnic**	**inter-ethnic**	**co-ethnic**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Radio Rwanda	-0.181***	0.173***	0.300**	162.7*	26.86	-102.8	-5.589
	[0.002]	[0.00]	[0.047]	[0.060]	[1.00]	[1.00]	[1.00]
Placebo Station	0.0964	-0.0456	-0.111	-26.92	-37.19	1.745	26.64
	[0.170]	[0.708]	[0.859]	[1.00]	[1.00]	[1.00]	[1.00]
Zombo	-0.0665	0.138	-0.195	-41.20	65.23	-154.1	276.2
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[0.317]
Radio 10	0.130	0.0313	-0.606	12.86	-171.3	229.2	66.63
	[1.00]	[1.00]	[0.176]	[1.00]	[0.577]	[1.00]	[1.00]
BBC	-0.426***	0.0258	0.454**	-216.8	-108.6	-125.7	-57.71
	[0.000]	[1.00]	[0.028]	[0.346]	[0.748]	[1.00]	[1.00]
Contact FM	-0.0886	-0.183	0.00640	0.208	22.13	-205.7	-149.5
	[1.00]	[0.299]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Radio Salus	-0.186	-0.121	-0.172	-0.584	-94.27	16.51	-38.77
	[0.519]	[0.606]	[1.00]	[1.00]	[0.994]	[1.00]	[1.00]
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trust Game Specific Controls	No	No	No	Yes	Yes	Yes	Yes
Observations	438	438	438	92	150	71	125
R^2	0.253	0.415	0.170	0.699	0.441	0.791	0.419

Note: p-values derived from standard errors clustered by village, and corrected for multiple comparisons using the Bonferroni correction are in square parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions included controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. Trust-game specific controls include: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. Each column represents a single regression.

Table A22: Inter-ethnic behavior and Radio Rwanda reception (controlling for reception from specific towers)

Dependent Variable:	**SIT**	**Partner Selection**	**Out-Group Trust (survey)**	**Inter-Ethnic Trust Game Offer**
	(1)	(2)	(3)	(4)
			Panel A: Tower 1	
Radio Rwanda Reception	-0.210***	0.168***	0.264**	162.7***
	(0.0751)	(0.0369)	(0.114)	(51.08)
Controls	Full	Full	Full	Full
Trust-game specific controls	No	No	No	Yes
Tower Control	Tower 1	Tower 1	Tower 1	Tower 1
Observations	438	438	438	92
R^2	0.195	0.421	0.174	0.719
Control Group Mean of Dependent Variable	0.916	0.420	2.93	345
			Panel B: Tower 2	
Radio Rwanda Reception	-0.209***	0.175***	0.286***	169.6***
	(0.0702)	(0.0286)	(0.103)	(51.82)
Controls	Full	Full	Full	Full
Trust-game specific controls	No	No	No	Yes
Tower Control	Tower 2	Tower 2	Tower 2	Tower 2
Observations	438	438	438	92
R^2	0.196	0.424	0.178	0.723
Control Group Mean of Dependent Variable	0.916	0.420	2.93	345
			Panel C: Tower 3	
Radio Rwanda Reception	-0.204**	0.140***	0.335***	202.8***
	(0.0886)	(0.0420)	(0.118)	(63.9)
Controls	Full	Full	Full	Full
Trust-game specific controls	No	No	No	Yes
Tower Control	Tower 3	Tower 3	Tower 3	Tower 3
Observations	438	438	438	92
R^2	0.193	0.423	0.175	0.725
Control Group Mean of Dependent Variable	0.916	0.420	2.93	345

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. The variable *Tower Control* is simply a dummy variable indicating whether a given village receives its best a radio reception from a given Tower. All regressions include controls, including: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; Raven Cognitive Test score; intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces. Column 4 also includes trust-game-specific controls: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. The dependent variable in column 4 is the private, inter-ethnic trust game offer.

Table A23: Main Effect Broken Down by RTLM Status

Dependent Variable	**SIT**		**Partner Selection**		**Trust Survey (out-group)**		**Trust Game (inter-ethnic)**	
RTLM Village Status	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Rwanda Village	-0.0774†	-0.454***	0.115***	0.152**	0.114	-0.131	107.7†	.
	(0.0490)	(0.108)	(0.0370)	(0.0609)	(0.108)	(0.158)	(68.90)	.
Subset of baseline controls (see notes)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trust game-specific controls (excl. enumerator FE)	No	No	No	No	No	No	Yes	Yes
Observations	352	86	352	86	352	86	75	17
R^2	0.230	0.271	0.389	0.634	0.150	0.335	0.404	1.000

Note: Standard errors clustered by village are in parentheses. † *, **, and *** represent significance at the 15%, 10%, 5%, and 1% levels respectively. All regressions control for the baseline controls with a few control variables dropped to avoid collinearity of the Radio Rwanda variable in the smaller sample (which is an issue for all outcomes with the entire set of baseline controls). The variables we exclude are the distance and time to the third nearest radio tower; gender; age. This leaves us with the following controls: Tutsi indicator; enumerator fixed effects (for the survey); district fixed effects; time and distance to the two nearest radio towers; distance to the capital, nearest big city and road; village level cell phone availability; and Raven score. RTLM reception, which is also in the baseline set of controls is not included because the sample is conditioned on its (historical) reception. The trust game regression includes the full set of trust-game specific controls used throughout with the exception of the trust game enumerator fixed-effects. It therefore includes: gender and radio status of partner; distance between the villages of the partners; risk preference are all included.

Table A24: Genocide

Dependent Variable:	**Genocide**		**SIT**	**Partner Selection**	**Out-Group Trust (survey)**	**Inter-ethnic Trust Game Offer**
	(1)	(2)	(3)	(4)	(5)	(6)
Radio Rwanda Reception		0.0954	-0.176***	0.171***	0.273**	168.6***
		(0.164)	(0.0607)	(0.0360)	(0.114)	(54.43)
Radio RTLM Reception	0.201	0.152	0.0849	-0.0677	-0.153	-120.9
	(0.142)	(0.196)	(0.0722)	(0.0456)	(0.141)	(96.41)
Genocide Violence			0.0701	0.0363	-0.177	103.5
			(0.0672)	(0.0554)	(0.117)	(87.50)
Controls	Full (excl. genocide)	Full (excl. genocide)	Full	Full	Full	Full
Trust-game specific controls	No	No	No	No	No	Yes
Observations	438	438	438	438	438	92
R^2	0.840	0.842	0.242	0.421	0.174	0.726
Control Group Mean of Dependent Variable	0.567	0.567	0.855	0.420	2.93	325

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the full set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; Raven Cognitive Test score; intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces. Column 6 also includes controls used in trust game analysis: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. The dependent variable in column 6 is the private, inter-ethnic trust game offer.

Table A25: Measurement Error

Dependent Variable	**Trust Game Offer**	
Sample	**Tutsi-Tutsi**	**Hutu-Hutu**
	(1)	(2)
Radio Rwanda	-154.5	17.67
	(274.1)	(39.31)
Controls	Baseline	Baseline
Observations	38	237
R^2	0.670	0.198
Control Group Mean of Dependent Variable	345	350

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; level of cell phone usage at the village level; and Raven Cognitive Test score. We note that our small sample size of Tutsi co-ethnic pairs means estimates should be interpreted with caution.

Table A26: Enumerator Effects

Dependent Variable:	**SIT**		**Partner Selection**		**Out-Group Trust (survey)**		**Inter-Ethnic Trust Game Offer**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Radio Rwanda Reception	-0.158**	-0.176***	0.175***	0.171***	0.267**	0.273**	124.9**	166.6***
	(0.0614)	(0.0607)	(0.0335)	(0.0360)	(0.126)	(0.114)	(63.55)	(54.42)
Equality of Coefficients (p-value)	0.376		0.687		0.825		0.392	
Controls	Full	Full	Full	Full	Full	Full	Full	Full
Trust-game specific controls	No	No	No	No	No	No	Yes	Yes
Includes Enum. FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	438	438	438	438	438	438	92	92
R^2	0.117	0.242	0.406	0.421	0.112	0.174	0.424	0.718
Control Group Mean of Dependent Variable	0.855	0.855	0.420	0.420	2.93	2.93	326	326

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the full set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; Raven Cognitive Test score; intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces. Where indicated, enumerator fixed effects were dropped from this set. Columns 7 and 8 also include standard trust game controls for: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. Trust game offers used to estimate columns 7 and 8 are for private, inter-ethnic games.

Table A27: Difference in offers based on playing first as a sender vs receiver

Dependent Variable:	**Trust Game Offer**		**SIT score** ($\frac{WithinError}{TotalError}$)
Sample:	**Inter-ethnic** (1)	**Co-ethnic** (2)	**All** (3)
First Trust Game Played as Receiver	-26.53	1.236	0.0306
	(33.02)	(20.13)	(0.0462)
Controls	Baseline	Baseline	Baseline
Trust game specific controls	Yes	Yes	Yes
Observations	163	275	438
R^2	0.507	0.225	0.214
Control Group Mean of Dependent Variable	321	324	0.863

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. We include trust-game specific controls for: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. All regressions use the baseline set of controls, and include: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; Radio RTLM dummy variables; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. The variable *First Trust Game Played as Receiver* is a binary variable equal to one if the first trust game that the subject played was as a receiver. We test whether this influenced their decisions in subsequent games. We omit the partner selection task outcome and the trust survey outcome from this test since both were universally conducted prior to the trust game, so there is no way playing first as a sender or receiver could have possibly influenced behavior in those exercises.

Table A28: Distance Between Partner Villages by Radio Rwanda reception

Dependent Variable:	**Distance between the Villages of the Partners in Private Games**			
Sample (game-type):	Inter-Ethnic (1)	Co-Ethnic (2)	Inter-Ethnic (3)	Co-Ethnic (4)
Radio Rwanda Reception (of sender)	-2.487	1.197	7.818	7.422
	(7.369)	(5.505)	(7.130)	(6.209)
Equality of Coefficients (p-value)	0.582		0.948	
Controls	Baseline		Full	
Trust-game specific controls	Yes		Yes	
Observations	92	150	92	150
R^2	0.700	0.534	0.840	0.648
Control Group Mean of Dependent Variable	14.7	19.7	14.7	19.7

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions include controls for: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; and risk tolerance. All regressions using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions using the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces. The dependent variable in each regression is the Euclidian distance between the village centroids of the two partners in kilometers. *Equality of Coefficients (p-value)* tests the equality of the estimates reported in column 1 and 2 as well as 3 and 4.

Table A29: Opponent Radio and Return Offers

Dependent Variable:	**Amount of Offer Returned**				**Trust Game Offer**			
Sample:	Inter-ethnic (1)	Co-ethnic (2)	Inter-ethnic (3)	Co-ethnic (4)	Inter-ethnic (5)	Co-ethnic (6)	Inter-ethnic (7)	Co-ethnic (8)
	Panel A: Private Games							
Radio Rwanda Reception	215.7**	9.799	224.2*	-65.53	148.5***	10.79	168.6***	-53.02
	(94.02)	(62.99)	(116.9)	(85.68)	(53.31)	(32.69)	(54.42)	(41.96)
Partner Radio Reception	-13.36	-14.74	-89.23	-1.377	18.13	-16.26	7.175	-8.665
	(52.70)	(36.29)	(78.36)	(38.09)	(28.61)	(27.46)	(49.29)	(27.69)
Controls	Baseline	Baseline	Full	Full	Baseline	Baseline	Full	Full
Trust-game specific controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	92	150	92	150	92	150	92	150
R^2	0.563	0.343	0.636	0.390	0.676	0.406	0.726	0.490
Control Group Mean of Dependent Variable	335	344	335	344	325	342	325	342
	Panel B: Public Games							
Radio Rwanda Reception	-55.29	23.17	40.74	-26.51	-37.60	1.248	0.473	-52.67
	(125.5)	(75.72)	(162.9)	(104.8)	(59.07)	(55.85)	(77.24)	(65.05)
Opponent Radio Reception	60.01	-6.694	111.9	-23.72	73.92	-62.11	75.48	-46.88
	(69.02)	(62.47)	(114.1)	(71.21)	(51.82)	(39.69)	(79.08)	(62.69)
Controls	Baseline	Baseline	Full	Full	Baseline	Baseline	Full	Full
Trust-game specific controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	71	125	71	125	71	125	71	125
R^2	0.598	0.436	0.695	0.502	0.744	0.389	0.799	0.486
Control Group Mean of Dependent Variable	390	426	390	426	355	358	355	358

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions include controls for: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; and risk tolerance. All regressions using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions using the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces.

Table A30: Effect of Radio Rwanda on Salience on Other Photo Observables

Dependent Variable:	**SIT score** ($\frac{WithinError}{TotalError}$)				
	Shirt Colour (light versus dark) (1)	**Background** (city versus local) (2)	**Shirt Type** (collar versus no-collar) (3)	**Facial Hair** (moustache versus none) (4)	**Jacket** (Jacket versus no-Jacket) (5)
Radio Rwanda	-0.000852	-0.0659	0.00636	0.0152	0.0216
	(0.0626)	(0.0435)	(0.0432)	(0.0402)	(0.0532)
Controls	Baseline	Baseline	Baseline	Baseline	Baseline
Observations	438	438	438	438	438
R^2	0.189	0.174	0.166	0.136	0.168
Control Group Mean of Dependent Variable	0.807	0.926	0.795	0.807	0.820

Note: *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. For each case the dependent variable is reconstructed using the characteristic under consideration. For example, in column 1 the within-error component of the SIT is recomputed to see if subjects are more likely to choose someone wearing a light shirt if the person for the correct answer was wearing a light shirt. The denominator is the same for each variable.

Table A31: Trust game offers and trust game return offers, by game type

Dependent Variable:	**Return Offers**							
	Private Games				**Public Games**			
	Inter-Ethnic (1)	Co-Ethnic (2)	Inter-Ethnic (3)	Co-Ethnic (4)	Inter-Ethnic (5)	Co-Ethnic (6)	Inter-Ethnic (7)	Co-Ethnic (8)
Trust Game Offer	1.138***	1.006***	1.131***	1.057***	1.030***	1.113***	1.094***	1.103***
	(0.212)	(0.126)	(0.183)	(0.137)	(0.265)	(0.108)	(0.267)	(0.141)
Wald Test for Coefficient equal one (p-value)	0.529	0.755	0.551	0.476	0.991	0.265	0.817	0.474
Equality of Coefficients (p-value)	0.725		0.931		0.623		0.886	
Controls	Baseline		Full		Baseline		Full	
Trust-game specific controls	Yes		Yes		Yes		Yes	
Observations	241	241	241	241	196	196	196	196
R^2	0.750	0.646	0.797	0.675	0.736	0.741	0.819	0.757
Control Group Mean of Dependent Variable	335	344	335	344	390	426	390	426

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions include controls for: gender of partner; whether partner receives Radio Rwanda; enumerator of trust game fixed effects; distance between partners' villages; and risk tolerance. All regressions using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions using the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces.

Table A32: Public Treatment and SIT

Dependent Variable:	**SIT score** ($\frac{WithinError}{TotalError}$)			**SIT score** ($\frac{WithinError^2}{TotalError}$)			**SIT** ($\frac{WithinError}{TotalError} > 0.42$)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Public Information	0.0265	0.0181	0.0108	-0.0398	-0.0285	-0.0123	0.00484	-0.00409	-0.00226
	(0.0246)	(0.0266)	(0.0242)	(0.0715)	(0.0704)	(0.0606)	(0.0258)	(0.0267)	(0.0269)
Controls	Baseline	Full	Full	Baseline	Full	Full	Baseline	Full	Full
Mistake FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	438	438	438	438	438	438	438	438	438
R^2	0.201	0.228	0.421	0.133	0.163	0.410	0.134	0.176	0.274
Control Group Mean of Dependent Variable	0.855	0.855	0.855	1.243	1.243	1.243	0.916	0.916	0.916

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. All regressions using the full set of controls include all the baseline controls as well as: intensity of genocide; log income; share of village which is Tutsi; years of education; village elevation; variance in sector's elevation; log sector population; sector population density; cardinal direction village faces.

Appendix B. Heterogeneity

We now examine the heterogeneity in the impact of living in villages that receive Radio Rwanda on the reduction in the salience of ethnic identity as well as changes in inter-ethnic trust. We examine several aspects of the underlying heterogeneity: age, gender, Hutu or Tutsi ethnicity as well as pre-existing levels of ethnic salience.

B.1. Distributional Effects:

It would be useful to know whether the reduction in the salience of ethnicity was across-the-board or whether it was concentrated amongst those for whom salience was (for example) high to begin with. We examine this using a quantile regression with results presented in table B1 The table (and associated graph) suggest that while the effect of radio does not work only on the people who already have the best inter-ethnic attitudes, among those with room for improvement, there does not seem to be much heterogeneity in the effect. This can be seen from the relatively constant estimates we get in the 30%-90% range of the distribution. This in its own right is somewhat interesting as it suggests that there is not a large fraction of people that select out of listening to Radio Rwanda. This is perhaps because it is so frequently the only source of news and entertainment in rural Rwanda.

Table B1: Quantile Regression of Radio Rwanda on SIT

Dependent Variable:	**Salience of Identity Test**					
	15th percentile (1)	**30th percentile** (2)	**45th percentile** (3)	**60th percentile** (4)	**75th percentile** (5)	**90th percentile** (6)
Radio Rwanda Reception	-0.010	-0.0589**	-0.0544***	-0.0316***	-0.0355***	-0.0419*
	(0.0767)	(0.0229)	(0.0159)	(0.0157)	(0.0131)	(0.0216)
Controls	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Observations	438	438	438	438	438	438

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score.

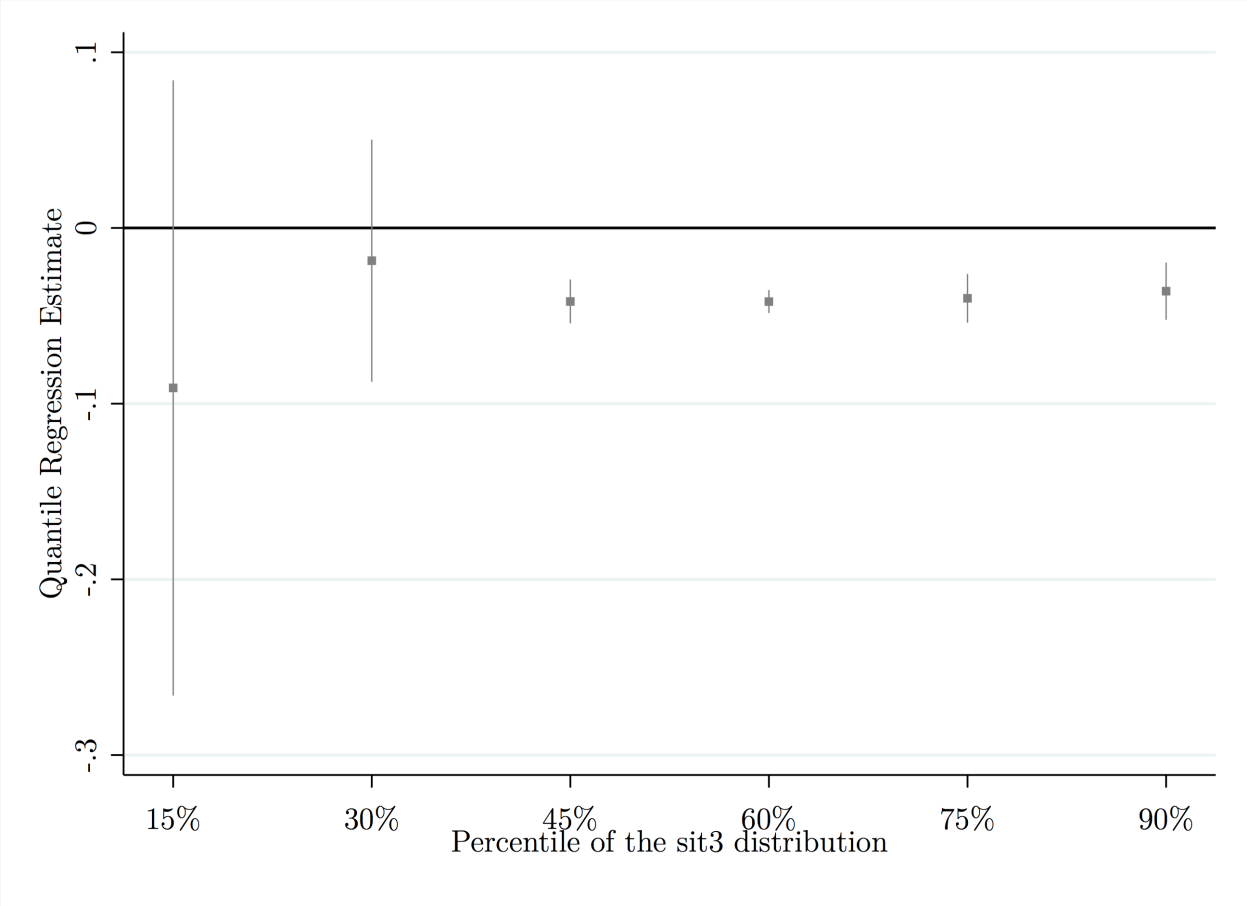


Figure B1: Point Estimates and 95% Confidence intervals from a quantile regression of % within ethnicity SIT errors on reception of Radio Rwanda

B.2. Heterogeneity by Ethnicity:

In panel A of table B2 we look at whether the Hutu or Tutsi are more influenced by the radio. There may be some reason for this to go both ways. The Hutu were the perpetrators of the genocide and persuading this group might be most beneficial to Kagame (both for staying in power and preventing another violent outbreak). However on the other hand Radio Rwanda is known to be a government-run (and mostly Tutsi-run) station, so Tutsi may be more likely to listen, and may be more likely to trust the information being broadcast. We actually find similar estimates for both groups. The SIT is influenced by radio slightly more for Tutsi, but the partner selection task variable seems more driven by Hutu. For both of the trust estimates the Tutsi estimate is slightly larger, but both are large, and we can not statistically distinguish effects from each other.

B.3. Heterogeneity by Age:

If we look at heterogeneity by age (panel B) we see a similar pattern. Both the old and young seem influenced by the radio. We might have expected the attitudes of the older generation to be more entrenched, especially since they experienced the genocide first hand. While this

does seem somewhat true - most estimates are larger and more precise for the young group - we again can not statistically distinguish any differential effect between the two groups.

B.4. Heterogeneity by Gender:

In panel C we examine heterogeneity by gender. The trend of little evidence of a heterogeneous effect persists here as well. Both men and women are impacted in very similar ways to Radio Rwanda broadcasts.

B.5. Colonial History of Forced Labor:

Panel D presents heterogeneity by colonial history. Here the heterogeneity in the effect seems more clear, and is consistent with the distributional analysis. Most of the impact of Radio Rwanda is felt in regions with a history of inter-ethnic conflict. Regions where Tutsi forced Hutu to work on coffee plantations without pay, are still the regions with the most ethnic distrust (Blouin, 2017). However, they also appear to be the regions that respond more to reconciliation efforts. In particular, we see fairly stark contrasts in the effect of radio on partner selection and the trust survey question. In both cases there are significantly larger positive estimates in the historically troubled villages.

Table B2: Heterogeneity (by ethnicity, age, gender and colonial history) in the effect of of living in a village that receives a signal from Radio Rwanda on % within-ethnicity SIT errors; selection for a partner of the other ethnicity; answer to a survey question on out-group trust; and private inter-ethnic trust game offers

Dependent Variable:	SIT		Partner Selection		Out-Group Trust (survey)		Inter-Ethnic Trust Game Offer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A - Heterogeneity by Ethnicity							
	Tutsi	Hutu	Tutsi	Hutu	Tutsi	Hutu	Tutsi	Hutu
Radio Rwanda Signal	-0.202	-0.187**	0.0721	0.201***	0.275	0.221*	-569.6	265.4
	(0.193)	(0.0759)	(0.117)	(0.0550)	(0.328)	(0.128)	(1,496)	(197.9)
Observations	121	317	121	317	121	317	48	44
R^2	0.415	0.227	0.416	0.217	0.527	0.188	0.856	0.779
Control Group Mean of Dependent Variable	0.909	0.919	0.688	0.311	2.80	2.98	340	305
	Panel B - Heterogeneity by Age							
	Young	Old	Young	Old	Young	Old	Young	Old
Radio Rwanda Signal	-0.176**	-0.296***	0.242***	0.115**	0.161	0.157	366.1	378.0*
	(0.0793)	(0.101)	(0.0651)	(0.0573)	(0.218)	(0.174)	(0)	(192.5)
Observations	177	261	177	261	177	261	35	57
R^2	0.313	0.353	0.427	0.499	0.301	0.207	1.000	0.658
Control Group Mean of Dependent Variable	0.937	0.901	0.404	0.432	2.87	2.96	330	322
	Panel C - Heterogeneity by Gender							
	Male	Female	Male	Female	Male	Female	Male	Female
Radio Rwanda Signal	-0.214***	-0.0821	0.241***	0.0140	0.364*	0.280	313.7	268.9**
	(0.0639)	(0.0861)	(0.0459)	(0.0368)	(0.200)	(0.168)	(191.6)	(97.41)
Observations	262	176	262	176	262	176	44	48
R^2	0.237	0.220	0.452	0.506	0.214	0.317	0.780	0.694
Control Group Mean of Dependent Variable	0.936	0.902	0.403	0.432	2.87	2.96	330	323
	Panel D - Heterogeneity by Colonial History							
	Corvée	No Corvée	Corvée	No Corvée	Corvée	No Corvée	Corvée	No Corvée
Radio Rwanda Signal	-0.173**	-0.117	0.185***	0.00932	0.399**	-0.0267	116.1	255.0
	(0.0699)	(0.0897)	(0.0392)	(0.0780)	(0.173)	(0.179)	(89.01)	(518.5)
Controls	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Observations	301	137	301	137	301	137	60	32
R^2	0.161	0.267	0.417	0.536	0.150	0.357	0.531	0.937
Control Group Mean of Dependent Variable	0.909	0.931	0.406	0.450	2.93	2.92	294	375

Note: Standard errors clustered by village are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels respectively. All regressions using the baseline set of controls include controls for: district fixed effects; enumerator fixed effects; gender; ethnicity; age; both distance (km) and travel time by car to first, second, and third nearest radio towers; RTLM; light density; distance (km) to nearest road, nearest city, and Kigali; prevalence of cell phone usage at the village level; and Raven Cognitive Test score. In each specification we split the sample along different dimensions of heterogeneity. The binary classifications are determined as follows: panel A - Hutu/Tutsi; panel B - above/below median age; panel C - male/female; panel D - coffee was / was not the most profitable crop in the colonial era (see Blouin, 2017).

Appendix C. Steps to Replicate Radio Signal

In ArcGIS:

III. Import village location csv file

III. Export the file to get ID numbers (otherwise buffer will not work)

III. Construct a buffer around the points using the "Buffer" tool in "Analysis Tools"

- Set buffer size to radius 0.017dd (*just* larger than the resolution of the radio data)

In the program RadioMobile:

III. Click Unit Properties and add all of the towers, and their info as listed in the paper

III. Add each network according to the network and tower information in the paper

III. For each:

a choose mode of variability: broadcast

b Surface refractivity: 310

c Ground conductivity: 0.005

d Relative Ground Permittivity: 15

e Polarization: Vertical

f Climate: Equatorial

g Variability % of time: 60

h Variability % of location: 60

i Variability % of situations: 60

j Topology: Visible and Voice-net (Command/Subordinate/Rebroadcast)

III. Click "Combined Cartesian Radio Coverage"

a Add the relevant towers and the relevant network.

b Set a mobile unit set to 2m off the ground

c Set maximum range to 200km

d Link direction: MobileRX

e Mode: All picture

f Signal range to draw: dbuV/m

g Draw size (pixels): 3

h From ($\geq$): 0

i To (<): 200

j Make sure "save raster data to file (txt)" is checked off.

k Click draw (it can take a long time)

III. When it finishes drawing save the raster file it produces to a folder.

III. Before you import into ArcGIS you need to get rid of the preamble that RadioMobile produces. So for each output you have to open the raster file with a text editor and delete everything above the variable names, and overwrite the file.

In ArcGIS (again):

III. Import the raster file from RadioMobile into ArgGIS

III. Spatial join to the buffer file constructed in step 3 of the (first) ArcGIS section.

a Choose the option where you match to a point that falls inside the shapefile and choose the 'max' option among the summary stats. This gives the interpretation that if anyone in the village gets a signal, the village is counted as getting a signal.

III. Export the new matched file to a txt file, and insheet into Stata.

Appendix D. Protocol Appendix

Enumerators were hired from a local country-specific pool used by the firm that we hired to help us manage the data collection. There were two nearly identical data collections efforts, one in Rwanda and another in Burundi. The main difference was that subjects were directly asked about their ethnicity in Burundi, while this was not permitted in Rwanda. In addition, all instructions (written or oral) were in Kinyarwanda in Rwanda and in Kirundi in Burundi. Data was collected for three projects in mind. The first is Blouin (2016) the second is the project described in this paper and the third is a yet unwritten project.

On a given day there was a morning and an afternoon data collection session. Typically the same villages were used for the morning and afternoon sessions. In any given session we typically have 4-5 people from any given village and 4-5 villages present (20 people total), but overall, in the data we have 8-10 subjects from each village (4-5 from the morning and 4-5 from the afternoon).

Data collection sessions took place in a town hall. There was a survey portion and an experimental portion to the session. The surveys took place first, and the experiments took place second. Surveys were completed sitting down at a table in private with a subject. In the experiment portion of the session there 4 were experimental stations and a waiting area. Subjects were in one of these two locations throughout the experiments. In the waiting area there was a large poster board that listed the partnerships for the trust game. The poster

board was updated with the offers of the trust game throughout the day if the trust game was assigned to the public treatment.

In each data collection session, there were well-defined roles for our 8 enumerators. They were specialists in that, for example, the person who was in the Enumerator 1 role was in that role for every session. It will help to label these roles as follows, and we'll refer to them as E1-E4:

- Enumerator 1 (1 person)
  - IV. responsible for greeting subjects as they arrived and handling consent.
  - IV. responsible for matching trust game partners as well as roles (sender / receiver) for the trust game
  - IV. responsible for collecting partner preferences and briefing subjects on how the experiments would run.
  - IV. responsible for managing the flow of experiments, ensuring subjects knew where to go, etc.
  - IV. responsible for payment at the end of the day
- Enumerator 2 (3 people)
  - IV. responsible for completing surveys with subjects.
  - IV. responsible for assisting E1 with task 4 above (i.e. logistics / flow of experiments).
- Enumerator 3 (3 people)
  - IV. responsible for completing surveys with subjects
  - IV. responsible for completing the trust game with subjects
  - IV. responsible for completing a separate task (for another paper) with subjects
- Enumerator 4 (1 person)
  - IV. responsible for completing surveys with subjects
  - IV. responsible for completing the SIT with subjects

The timeline of events for data collection was as follows:

- Between 8:00 and 8:30am the enumeration team travelled from the hotel in 4 SUVs to the town hall where the data collection session took place.
- The team unloaded materials and started arranging tables, chairs, posters, and other materials needed for the survey and experiments.
- While set-up was taking place each driver drove an SUV to met subjects near their own villages at a pre-determined meeting location, and drove them to the town hall.

- As subjects arrived (typically 4-5 at a time) they were greeted by E1 who described in general terms the survey, experiments and the purpose of the study before distributing and reading the consent agreement.

- After collecting the consent agreements, each subject was given an ID card that they pinned to their shirt which listed a letter that corresponded to the region they live in (a geographic cluster close to the pick-up location), and a number identifying each individual from that region. ID tags were in bags corresponding to the region (i.e. letter) and were dolled out randomly upon subject arrival (conditional on letter).

- As they entered the town hall, each subject was paired up with any one of seven enumerators (i.e. E2-E4) to complete the survey. These enumerators simply lined up near the entrance of the town hall and paired-up with subjects as they entered.

- As subjects were completing the surveys with one of the other enumerators, E1 matched each subject to another for the trust game and determined roles (who was to play as sender / receiver). This matching was done using numbers from the ID tags, and was done without any consideration (or knowledge of) the ethnic identity of the subjects. At this stage the only consideration when matching two subjects was logistical - to ensure that subjects who were matched to each other were not from the same village. Once E1 knew the exact composition of the session (i.e. how many subjects from each village) she ensured that, for instance, whoever had ID tag A2 would be partnered with whoever got B5. An 'A' (i.e. from region A) was never partnered with another 'A', and likewise for every other letter. So as a first step, we assumed that if two people were not from the same village it was unlikely they would know each other. This matching process typically took no more than 15 minutes.

  - The only reason this was not completely trivial (and hence why it typically took more than 30 seconds) is each subject could only be partnered with another individual once, to prevent outcomes from one game influencing another. For example, subjects played the trust game twice, once as a sender and once as a receiver. E1 always made sure that that any partnership only ever took place once.

- As subjects finished the surveys, they were sent back to E1. She briefed each subject (sequentially and one at a time) on logistics and also elicited information in the following order:

  - Partner-selection task: at this point the partner-preferences were taken for each subject. Each subject was asked to look around at the ID tags of the people at the session, and list the top 5 individuals that they would like to be partnered with to take part in a cooperative task. They were told that a few people would be matched with a partner of their choice for the last game of the day (which was the task under E3 item 3 in the list of responsibilities above)
  - Payment Protocol. Each subject was informed that the monopoly money he/she received, represented real money. Every dollar of monopoly money represented

a Rwandan or Burundian Franc. At the end of each experiment an enumerator would write down on a piece of paper how much money was earned in the experiment, signed the back of the piece of paper, and put the piece of paper in a sac that the subjects were responsible for. At the end of all experiments, subjects would reach into the sac and pull out one piece of paper, and would be paid in cash, the amount listed. It was stressed that total money earned would depend on outcomes of the various lab exercises, but that they would earn money from only one specific exercise, chosen at random.

  - Question on previous acquaintance: As noted above, subjects were assigned trust-game partners randomly. Since the partner pairings were done while surveys were being completed, at this point subjects were informed of who their partner would be. The enumerator asked subjects (individually) if they had ever met their partner before. If either had, the protocol dictated that new partners be found.

- Subjects were then taken to an area of the town hall that had seats for them to wait until they were called to participate in an experiment. They often had to briefly wait for an assigned partner to complete the survey.

- As subjects became available, E1 called out the ID tags of subjects and brought each subject them over to E3 or E4, at one of the experiment stations depending on who was free.

- When a subject arrived at E4's work station, he implemented the SIT for the subject (see Appendix F).

- In the case of the trust-game, as subjects arrived and sat down at the trust-game station, E1 (or E2) told E3 which subject was player 1 and which was player 2.[53] This is to to ensure that each player got to play once as 'sender' and the other time as 'receiver'.

  – E3 assigned the pair to either the public or private treatment. Before explaining the game E3 rolled a six-sided dice. If a 1, 2, or 3 was rolled the pair was assigned to the public treatment. If a 4, 5, or 6 was rolled the pair was assigned to the private treatment. The game was the same, but the script that E3 read to them differed (see Appendix E).

  – E3 explained the experiment to both subjects and gave out the Monopoly money to player 1. There was a script that they read, and then they asked both subjects if they understood both the rules and the implications of the decisions. The enumerator was free to explain the trust game in their own words, if they felt that subjects did not understand the game after being read the script.

  – After playing the game, E1 (or E2) was notified whether the game had been assigned to the public or private treatment, and if it was assigned to the public treatment the poster board was updated by E2.

[53] Recollect that after the survey was completed, all the E2's had only one responsibility - namely to help E1 with logistics.

- After playing the trust game, the subject was sent back to the waiting area until they could participate in another experimental exercise.
- Part of the job of E1 and the three E2s was to keep an eye out for idle experimental stations and subjects that had not yet participated in those stations. E.g. if E4 was idle and a participant in the waiting area had not completed the SIT, E1 or E2 would be responsible for making sure that E4 completed the SIT with the subject at that time.
- Once a subject finished all the experiments, they were called from the waiting area, and they pulled from their sac a piece of paper that listed their payment. They were given this payment plus a participation fee plus a case of soap. They returned the ID tag and were free to leave.
- As they exited the town hall, the drivers were waiting for them to take them back to their village. When drivers dropped off one set of subjects another set was waiting at the same location for the afternoon session.
- While drivers dropped off and picked up subjects, enumerators typically went into town to have lunch. In the evening session when drivers dropped off subjects, the enumerators organized all of the surveys and experimental materials. Drivers returned, the SUV was packed up, and the team typically went for dinner near the hotel.

Appendix E. Original Experiment Instructions:

T1) Player 1 ID:_____

T2) Player 2 ID:_____

Enumerator instructions: Role the dice.

T3) What did you roll: _____

If a 1, 2, 3 is rolled read the following paragraph:
Remember that this is for real money. The choices that you make will be seen by everyone when we post them on the wall throughout the day

If a 4, 5, 6 is rolled read the following paragraph:
Remember that this is for real money. The choices that you make will not be seen by anyone. We will post some other people's choices on the wall throughout the day, but the choices you make will only be seen by us.

For all respondents, read the following:

Here's how the exercise works: Player 1 receives 600RWF. You can put some money onto the table, and whatever is placed on the table will be doubled. Then Player 2 gets to decide how divide all of the money on the table between the two of you.

Enumerator instructions:

- Make sure they understand the game.
- Explain in your own words if necessary.
- Give the 6 bills of Monopoly money to player 1.

T4) Player 1: How much of your 600RWF would you like to share? *Circle one*

a) 0 RWF
b) 100 RWF
c) 200RWF
d) 300RWF
e) 400RWF
f) 500RWF
g) 600RWF

Instruction to Enumerator: Now double what was given by player 1 and give the money to player 2.

Player 2: I have taken the money shared by player 1 and have doubled it. You can now decide how to divide this money between the two of you. How much would you like to keep and how much would you like player 1 to have, in addition to what (s)he has already kept?

T5) Player 2 keeps:_____

T6) Player 2 shares with player 1: _____

Enumerator Instructions: Now write down on a 'payment stub' for each person, how much that person earned in the experiment. Sign or initial the back of the stub and place it in the sac that they have to hold the stubs.

Appendix F. Original Experiment Instructions - SIT

Respondent ID:_____

Now we're going to read a few statements, using pictures of people so you can keep track of who the statement was about. Here are the people I will refer to.

Enumerator Instructions: Lay-out the numbered photos on the table so that they're right side up for the participant. Pick up each picture in random order, showing the photo to the participant, and read the statements in the order listed. Please record the order of the photos you pick up as you pick them up in the space provided (SIT1). Lay the picture back down on the other side of the table. Once you have read the statement for each photo, ask them to wait in the waiting area for five minutes.

After five minutes call them back and read the following:

Now I'm going to read back some of these statements and I want you to point to the picture associated with the statement. For each one that you can match to the correct picture I'll give you 100RWF.

Enumerator instruction: write down the picture number of the picture they point to in the space provided.

SIT1) Order of Photos: ____ ____ ____ ____ ____ ____ ____ ____

SIT2) Point to the picture of the person that owns a red moto:_____

SIT3) Point to the picture of the person that loves basketball:_____

SIT4) Point to the picture of the person with brothers:_____

SIT5) Point to the picture of the person with two boys:_____

SIT6) Point to the picture of the person who likes bananas:_____

SIT7) Point to the picture of the person that likes cooking:_____

Enumerator Instructions: Now write down on a 'payment stub' how much they earned in the experiment. Sign or initial the back of the stub and place it in the sac that they have to hold the stubs.

FULL LIST OF PHOTOS AND STATEMENTS

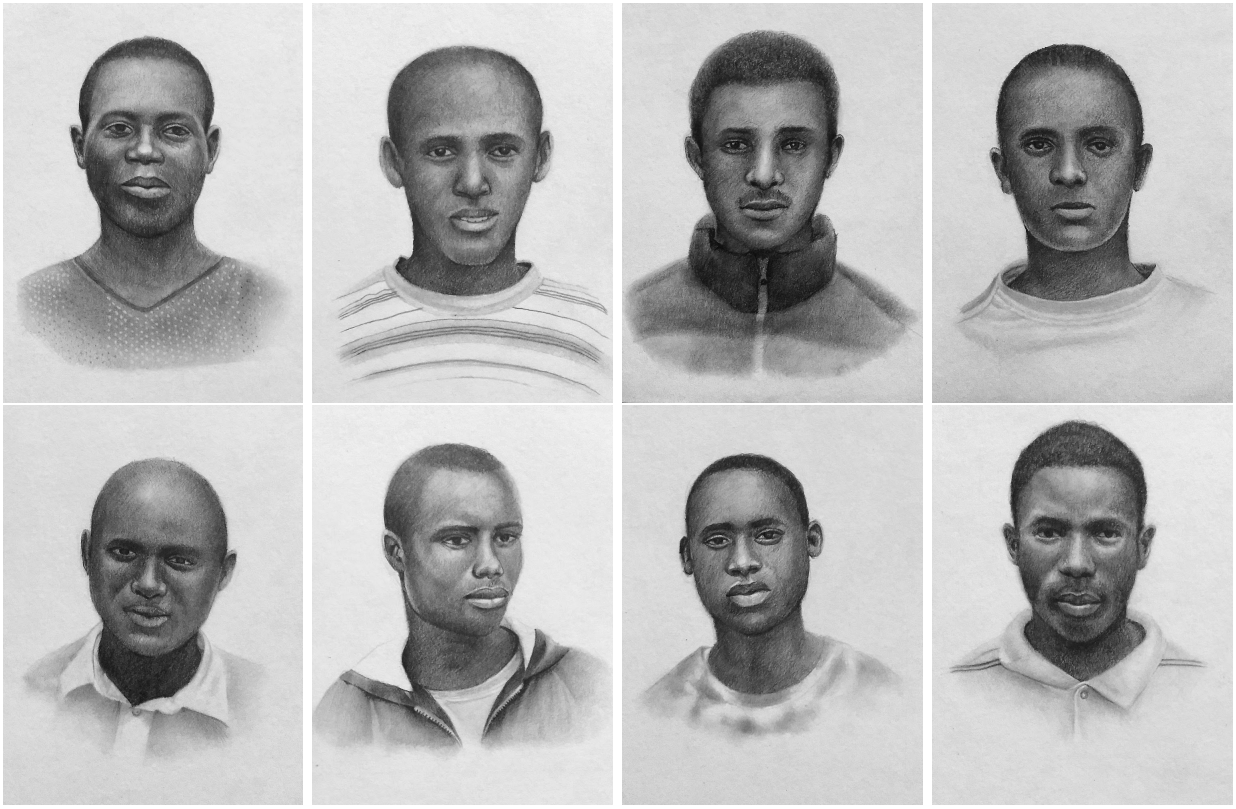


Figure F1: Full set of SIT pictures

- This person likes to go for long walks
- This person owns a blue bicycle and 2 red motos
- This person has 4 children, two boys and two girls
- This person has never seen a football match but loves to watch people play basketball
- This person really likes bananas and but dislikes guava
- This person can run really fast
- This person likes to cook
- This person has 2 brothers