Erasing Ethnicity? Propaganda, Nation Building, and Identity in Rwanda

Arthur Blouin

University of Toronto

Sharun W. Mukand

University of Warwick

This paper examines whether propaganda broadcast over radio helped to change interethnic attitudes in postgenocide 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, have increased interethnic trust, and show more willingness to interact face-to-face with members of another ethnic group. Our results suggest that the observed improvement in interethnic behavior is not cosmetic and reflects a deeper change in interethnic 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,

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higher corruption, weaker institutions, and lower economic growth all plague countries with ethnic divisions (see Easterly and Levine 1997; Garcia-Montalvo and Reynal-Querol 2005; Blattman and Miguel 2010). 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 interethnic violence.[1]

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 percent of the minority Tutsi population in 1994 in one of the worst genocides in recorded history. Critics of the government's program of postgenocide 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, the reason is that 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*" (Specia 2017; emphasis added).

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 that of Yanagizawa-Drott (2014) but relies on contemporary variation in Radio Rwanda rather than historical variation in the hate-radio station RTLM.[2] We combine geographic infor-

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[1] See Alesina and Ferrara (2005), Alesina, Michalopoulos, and Papaioannou (2016) for evidence, and Besley and Persson (2011, chap. 7) on the importance of "cohesiveness" of intergroup preferences for development.

[2] Olken (2009) is the first paper to exploit variation of signal transmission for examining economic outcomes. The author examined the impact of watching TV on social capital in Indonesia.

mation systems (GIS) data showing the reach of radio signals with data from a survey and lab-in-the-field experiments on interethnic attitudes conducted with 438 subjects in 52 villages in rural Rwanda.

We examine four main outcomes, each of which sheds light on aspects of interethnic 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 outgroup 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 (known as FARG).[3] FARG is a reparations fund available only to genocide "survivors" (read: Tutsi). Armed with these data, we examine the impact of radio propaganda on interethnic 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.[4] 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).[5] The test provides a simple way to capture whether a subject subconsciously or consciously categorizes others on the basis of their ethnicity: in this case Hutu or Tutsi. The test involves a recall task centered on matching pictures of Hutu and 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 the reason is that ethnicity is less 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 (≈0.39 standard deviations)

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

[4] The pull of national identity has also been shown to influence interethnic attitudes. Gibson and Gouws (2005) show how the pull of national identity weakened intergroup antipathy in South Africa. Also see Sniderman and Carmines (1997) and Eifert, Miguel, and Posner (2010).

[5] 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.

less likely to categorize others on the basis of their ethnicity, as measured by the SIT (mean 86 percent).

The second outcome is a partner selection task. We asked villagers to select, from a pool of 15 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 interethnic trust. Respondents in villages exposed to Radio Rwanda are about a quarter point on a Likert scale (0.35 standard deviations) 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. Interethnic trust offers in the private trust game are 47 percent 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 interethnic 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 interethnic 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 interethnic 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 percent 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 interethnic 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 ra-

dio 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 three nearest 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 politicized media seem to have (at best) modest effects in influencing party vote shares and electoral outcomes in competitive democracies such as the United States (see Gentzkow, Shapiro, and Sinkinson [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 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 do have the potential to improve interethnic attitudes. They examine subjects who are exposed to a reconciliation soap opera on radio, for four segments of 20 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 not only that improvements are 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 interethnic culture by show-

ing 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, Choi, and Strickland 2010; Benjamin, Choi, and Fisher 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 (Voigtländer and Voth 2012; Voors et al. 2012; Shayo and Zussman 2017). 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 the discussion in Fearon and Laitin [2000]).[6]

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 the paper with a short discussion in Section VIII.

II. Ethnicity, Politics, and Nation Building

By the end of the fifteenth 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 of three main ethnic groups: the Hutu and Tutsi, who make up over 97 percent

[6] 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 Gellner [1983] or Smith [1991]) about the political construction of national identity that may overwhelm primordial characteristics (see Cerulo [1997] for a survey).

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.[7]

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.[8] Over a 100-day period between April and July 1994, as many as 1 million people were killed, and more than 70 percent 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.[9]

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 interethnic 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 interethnic trust and 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.[10]

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

[8] For details on postindependence political developments and especially the genocide, see Gourevitch (1998), Hatzfeld (2009), and Meredith (2013).

[9] For an account of the rise of the RPF and that of Kagame, see Kinzer (2008).

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

A. *Legislation, Sanctions, and Interethnic Prejudice*

Given that state capacity is high, the populace genuinely fears being reported to (and punished by) the government for harboring interethnic prejudice. Central to this fear is the adoption of Rwanda Law 18, which defines and criminalizes "genocide ideology."[11] Individuals perceived as harboring interethnic prejudice can be arrested and jailed and sometimes even disappear (Beswick 2010).[12] Strict enforcement and social surveillance by an autocratic government give 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 interethnic prejudice and are not acting in accordance with the dictates 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.[13] This is considered one of the government's top priorities. Its importance can be gleaned from a senior government official's statement (quoted in Thomson [2011b, 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 interethnic 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

[11] 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."

[12] Part of the fear stems from an arbitrary aspect of the law itself. For instance, art. 3 of the law says that 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."

[13] As Kagame asked rhetorically in his speech at the twentieth commemoration of the genocide, "if we succeed in forging a new, more inclusive national identity, would it be a bad thing?"

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 in the same community" (Uwimbabazi and Lawrence 2013, 264). This has been accompanied by a deliberate attempt to rewrite Rwandan history in reeducation camps (*Ingando*) as well as (more recently) in primary school textbooks.[14]

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 of self-censorship, with high levels of reluctance by journalists to write reports criticizing the government, its policies or their implementation" (21).

Radio is by far the "most important form of mass media in Rwanda" (Paluck 2009, 576), 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 (Frére 2009). The reason is not just that Radio Rwanda has the widest geographic coverage, but also that 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" (415).[15] Indeed, Rwandan govern-

[14] For instance, Mgbako (2005, 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* (June 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."

[15] This view is echoed by Frére (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" (169).

ment 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.[16] 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.[17] This simple exercise suggests that treating Radio Rwanda as 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 online figs. A1 and A2). We find that Radio Rwanda is over five times more likely than private radio to discuss national identity positively and accounts for nearly 83 percent of progovernment 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 (fig. 1).[18] 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, the government-initiated fund targeted to Tutsi survivors and for which only Tutsi are eligible. FARG villages were

[16] 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" (*New Times*, February 16, 2015; http://www.newtimes.co.rw/section/article/2015-02-16/186027).

[17] We hired a Kinyarwanda-speaking research assistant to listen to radio over a 4-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. He was instructed to listen to 30-minute segments for between 2 and 3 hours of radio per day, 5 days per week. Given the importance and "reach" of Radio Rwanda, the research assistant spent 47.5 percent 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, and TR Rwanda.

[18] *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.

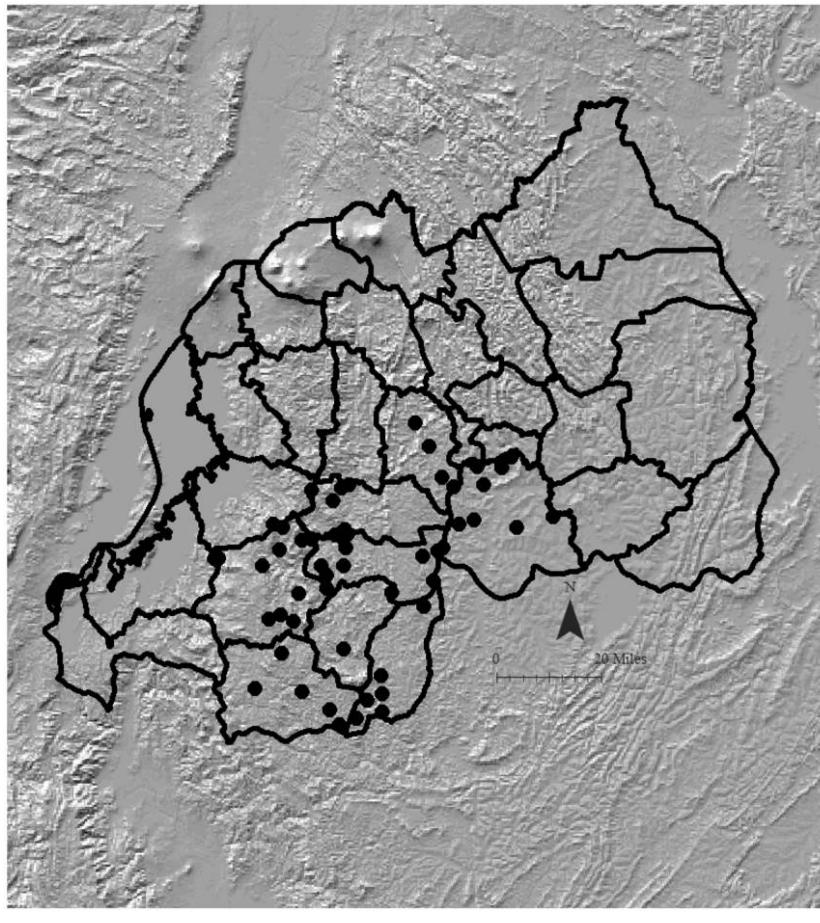


FIG. 1.—Village locations of all subjects in Rwanda. This map shows the village subject locations and administrative boundaries (district level). Subjects each left from their own village (which is what is depicted here) to a pickup location nearby. Our drivers picked them up from this location and drove them to the survey location. The village locations are retrieved from a survey of subjects and then geocoded 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 10 km 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.

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 to the Rwandan government our

survey materials, experimental protocol, schedule, and budget as well as other documents related to the project. These materials included a summary 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 us 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.[19] 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 sport utility vehicles. While our team prepared materials for the experiment and survey, each vehicle picked up subjects from an agreed-on 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.

[19] 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 because of the genocide the ethnic makeup of any village in 1991 may be different from what it is now. While one of the estimates is significant at the 10 percent level, we generally do not find much evidence to suggest that differential selection took place in Radio Rwanda regions.

As subjects arrived at the community hall, the enumerator handed them the consent document that also described the purpose of the study. This document either was read to them by the enumerator or (if they preferred) could be read by themselves.[20] Once they had agreed to participate, all subjects were given an identification 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 1 to 7. The ID 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 ID tags 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 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 interethnic 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.

A. Measurement of Interethnic Attitudes

1. 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.[21] 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.[22]

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

[21] 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, Tooby, and Cosmides (2001).

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

The enumerator randomly arranged eight photographs on a table. The photographs were of eight males and included four Hutu and four Tutsi (see fig. 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.[23] 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 3 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 then had to match it with the appropriate photograph. If subjects were 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 RWF 100 (Rwandan francs).

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 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.[24] 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 subjects process and categorize

[23] As an example of one neutral statement, "This person likes to go for long walks."

[24] Normalizing for the total number of errors is important because a person may make fewer errors because of higher mental ability or as a result of Taylor et al.'s (1978) observation that an individual "may select salient social or physical dimensions . . . for grouping and managing personal information" (779). 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.

TABLE 1
SUMMARY STATISTICS

	Observations	Mean	Standard Deviation	Minimum	Maximum
			A. Variables of Interest		
Radio signal	438	.48	.50	0	1
Public information	438	.45	.50	0	1
			B. Dependent Variables		
Salience of Identity Test	438	.86	.28	0	1
Salience of Identity Test (alternate)	438	1.22	.79	0	5
Salience of Identity Test (binary)	438	.900	.30	0	1
Partner selection	438	.433	.29	0	1
Partner selection (alternate)	438	1.03	.89	0	2.5
Partner selection (IHST count)	438	1.26	.70	0	2.31
Trust out-group (survey question)	438	3.00	.724	1	4
Trust in-group (survey question)	438	3.21	.764	1	4
Trust game offer private (RWF)	242	329	126	100	600
Trust game offer public (RWF)	196	335	129	100	600
			C. Baseline Control Variables		
Tutsi	438	.28	.45	0	1
Gender (% female)	438	40%	.49	0	1
Age	438	43.3	12. 4	19	88
Distance to road (km)	438	1.1	.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	.54	1.08	0	4.25
Distance to nearest station (km)	438	24.1	7.67	9	40
Distance to 2nd-nearest station (km)	438	43.6	14.8	11	78
Distance to 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	.19	.39	0	1
Cell phone usage in village (%)	438	69.3%	.202	.125	1
Raven score	438	5.39	1.34	1	6
			D. Additional Control Variables		
Elevation	438	1,646.1	185.3	1,365	2,141
Genocide prosecutions (/1,000)	438	.518	.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 (US$/year)	438	460.54	877.33	0	9,449
Log(income)	438	5.38	1.26	0	9.15
Log(sector population)	438	10.1	.33	9.46	10.83

TABLE 1 (*Continued*)

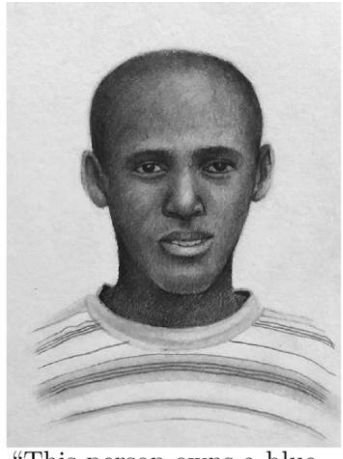
	Observations	Mean	Standard Deviation	Minimum	Maximum
Sector population density	438	1,015	3,618	223	24,623
Facing north	438	.25	.43	0	1
Facing south	438	.25	.43	0	1
Facing east	438	.32	.47	0	1
Facing west	438	.19	.39	0	1
	E. Other Variables of Interest				
Log(land value) (log US$)	438	8.35	2.01	0	14.3
Reception of other stations	438	38.8%	.488	0	1
Forced labor	438	.86	3.86	0	80
Migration (ever in lifetime)	438	22%	.41	0	1
Years in current home	438	39.2	15.41	0	88
Migration since 2004	438	3.7%	.188	0	1
Good signal of Radio Rwanda	154	5.8	1.22	3	9
Regular Radio Rwanda listener	154	42%	.49	0	1
Regular listener of other stations	154	4.5%	.21	0	1

NOTE.—Data presented in this table were collected from 438 subjects in Rwanda (see Secs. III and IV for a description) as well as a follow-up survey of 154 subjects (see Sec. 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 percent. 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 eight-question Raven Cognitive Test.

information without having to question them about their ethnicity.[25] 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

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



"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"

FIG. 2.—Example of SIT exercise. 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.

SIT. A final measure relies on the fact that if individuals were simply choosing photographs randomly, about 42 percent 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 percent of their errors within ethnicity and zero otherwise.

2. Partner Selection Task

At the end of the survey, all subjects engaged in a partner selection task (similar to that in 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 20 individuals at the session, with the caveat that they could not choose anyone they knew or anyone who lived nearby (as denoted by the letter on their ID tag).[26] 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 based on 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.[27]

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

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

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

[26] 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 cooperate well with their choice, they might 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.

[27] 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).

3. 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 interethnic 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 do you trust people from your own community in your village?" where responses were on a 4-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; others still may have interpreted it as an ethnic group. Second, the ethnic makeup of the community can itself be affected by interethnic 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.

4. 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 on which 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 interethnic 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.[28] In every case an enumerator

[28] Despite a large literature on trust games, surveyed in Johnson and Mislin (2011) and Sapienza, Toldra-Simats, and Zingales (2013), 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, Grechenig, and Nikiforakis (2014) show that about 35 percent 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, Cobo-Reyes, and Jiménez (2008) show that the threat of third-party punishment makes trust game offers 60 percent larger, a much larger effect than what we find.

ensured that all partners not only were drawn from different villages but also had never met.[29] 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 RWF 600, and was instructed to transfer as much as she wanted to player 2 and keep the rest. The enumerator matched the amount transferred, and then player 2 chose to keep as much as she 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, Bohnet, and Piankov 2006), the actual trustworthiness of the receiver (rather than perceived trustworthiness), or preferences toward 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.

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 coethnic partner and others with an interethnic 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 the study by 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, Kurzban et al. (2001), shows that when shirt color is made a more salient feature of group membership, race becomes less salient.

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

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 levels of trust toward those of the other ethnicity similar to those toward members of their own group. In other words, we should expect people with lower levels of ethnic salience to make higher offers in interethnic 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 interethnic trust games. Similarly, columns 2 and 4 illustrate the fact that, as expected, ethnic salience is not associated with differences in coethnic behavior in the same way as interethnic 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 coethnic and interethnic offers between people with high and low ethnic salience. Furthermore, people with high ethnic salience seem more sensitive to public information in the interethnic games (see 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.[30] 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

[30] 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 (*a*) were 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.

no. 69/2008 of December 30, 2008).[31] Crucially, eligibility hinges entirely on being a "survivor."[32] 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 as "Hutu" (Thomson 2011b, 111). The implication of this is that those Tutsi still alive are eligible for FARG even if they were not directly affected 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, 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 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

[31] International human rights agencies define someone as a victim of the genocide (and hence family members are genocide survivors) if he or she died in Rwanda as a result of violence between April 7 and July 15, 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, Goetz, and Stephens 2009).

[32] See http://socialprotection.org/programme/genocide-survivors-support-and-assistance-fund-farg, which lists the only eligibility criterion being "that she or he is a genocide survivor."

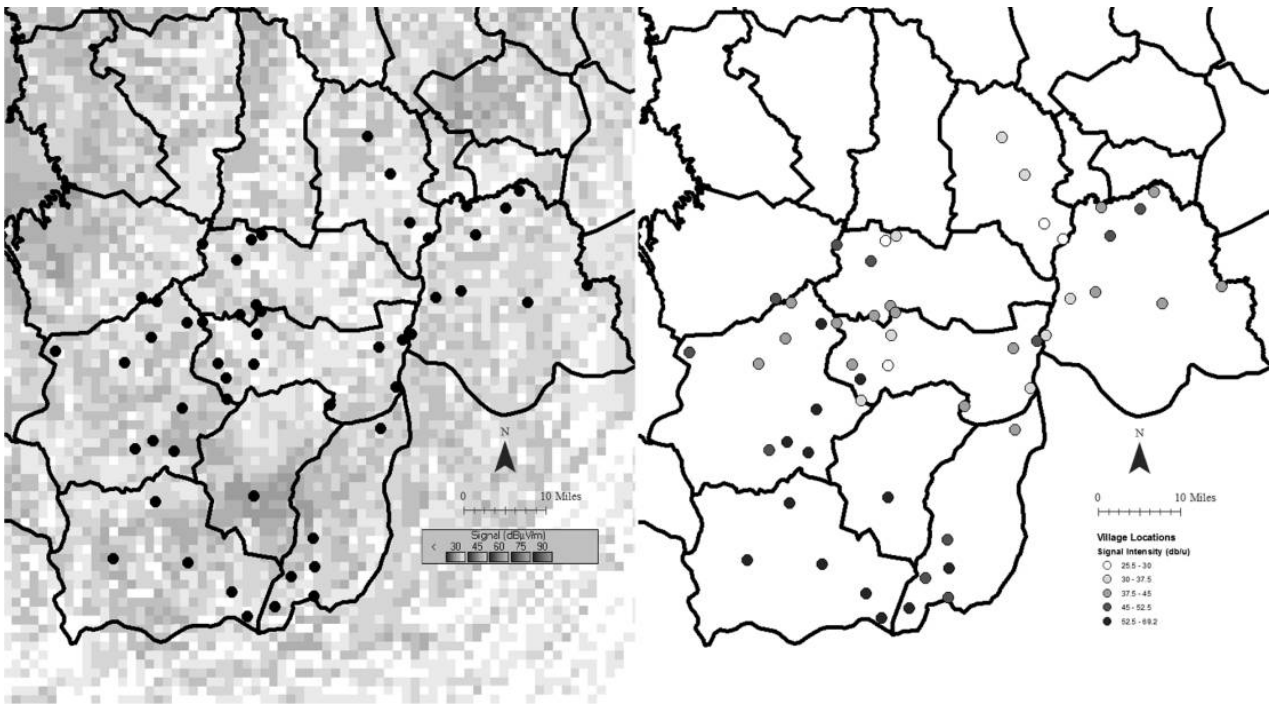


FIG. 3.—Radio Rwanda signal, district boundaries and subjects' village locations. This is a map of Radio Rwanda signal strength in Rwanda. We have overlaid the district administrative boundaries in black and the subject villages, represented by circles. In the map on the right we shade the circles to reflect the strength of the Radio Rwanda signal. The maps on the left and right are identical except 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 eight or nine 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 10 km 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.

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 interethnic 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 Irregular Terrain Model based on radio transmitter location, frequency, and power from http://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 decibels relative to 1 microvolt per meter (dBμ) and compute a binary variable indicating whether a village gets reception if the signal strength in that village is at least 45 dBμ (Figueiras and Frattasi 2010).[33] 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 Demographic and Health Survey (DHS), 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 to 9 in the data), as well as whether the subject regularly listens to Radio Rwanda or any other station.[34] The DHS data include a question on radio ownership and have the advantage of being nationally representative.

We used this survey on radio listenership to evaluate alternative definitions of radio reception at the village level. The resolution of the radio

[33] This is consistent with the methodology described by Hoeg and Lauterbach (2004, 248), who argue that if the coverage in a location is at least 46 dBμ, then we should expect 99 percent of the area in the 1 square kilometer radius to have 30+ signal strength. A detailed description of how we match our village locations to radio signals can be found in app. C.

[34] 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, Bjorkegren (2015) reports that in 2010, 84 percent of mobile phone owners owned a radio while only 63 percent of the general population did. Furthermore, perhaps because of 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 that 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 on the basis of this narrow measure, data from our survey seem somewhat similar. We end up with 154 subjects for our follow-up survey, drawn from 48 villages.

signal data is quite high, so within any village we typically observe radio signals for three to five geographic pixels.[35] Given this, we consider four ways to measure whether or not a village received a signal from a radio station (denoted i, ii, iii, and 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 percent) or (iii) a large majority (e.g., >75 percent) of pixels within the village receive a signal. Finally, we also evaluate (iv) whether the threshold should be >20 percent. Since there are between three and five 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 percent of people in Rwanda primarily listen to radio outside of their home (Stork and Stork 2008). Furthermore, in those data, 28 percent of people report not owning a radio, but 99.9 percent 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, 45 dBμ 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 percent, >50 percent, and >75 percent) the two that seem most relevant are the threshold at 20 percent 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 dBμ thresholds, with the 45 threshold being more precise in each case. For our main results we opt for the former (threshold of >20 percent) 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.

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

V. Empirical Strategy

We are interested in examining the impact on interethnic 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 interethnic 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 interethnic and coethnic 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:

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

and

$$\begin{aligned} \text{Interethnic Trust}_{ivd} &= \lambda_0 + \kappa_d + \lambda_1 \text{RadioRwanda}_{vd} + \Omega' X_{ivd} + \upsilon_{ivd}, \\ \text{Coethnic Trust}_{ivd} &= \rho_0 + \omega_d + \rho_1 \text{RadioRwanda}_{vd} + \Theta' X_{ivd} + \varepsilon_{ivd}, \\ H_0 &: \lambda_1 - \rho_1 = 0. \end{aligned} \tag{3}$$

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. The term 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 45 dBμ, 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.[36]

[36] 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 makes little difference to any of the estimates (table A26).

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;[37] 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 Euclidean distance to the nearest three Radio Rwanda towers are included, as well as variables for the travel time to the nearest three towers.[38] 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 coethnic and interethnic trust as outcomes. Here we have two measures of both interethnic and coethnic 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.[39] 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).[40] 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.[41] Nevertheless, we control for these regional characteristics in case there are nonverbal cues that indicate regional differences.

[37] 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 interethnic encounters; we are agnostic about specifically how. In any event, the results do not differ substantially without this control (table A8).

[38] Travel time is computed by scraping Google Maps using Python. For this we went through the Google Maps application programming interface.

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

[40] 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 interethnic attitudes, rather than exclusively on trust per se, whether the improvement is due to trust or altruism is a secondary concern.

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

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 interethnic trust, public and private trust. When we use the trust game data to estimate equation (3), we further split the sample according to whether the subject played the public or private version of the trust game. Here, ex ante, we thought that increases in both public and private trust were plausible. Both were measured with the aim of observing whether differences in interethnic 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 percent 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.[42]

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 recep-

[42] 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.

tion 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,[43] 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 and 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).[44] 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 percent 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 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 from five within-ethnicity errors out of five total errors,[45] 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 (col. 3).

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

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

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

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 dBμ, and not otherwise. Of course this likely varies from day to day and may also differ on the basis of 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 1 dB intervals.[46] Consistent with table 3, there is a large and negative effect at the 45 dBμ 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 45 dBμ threshold is not a sharp cutoff.[47] Given this consideration, the U-pattern that we observe is what we should expect if 45 dBμ is an appropriate cutoff to consider and if people in 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 interethnic social interaction. Regressions using this measure are examined in table 4. Columns 1 and 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 (col. 3) as well as a simple count of choices from the other group (col. 4). Moreover, it seems unlikely that ethnicity is simply

[46] In the figures we use the baseline set of controls. We present the full set of controls in fig. A3.

[47] 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 45 dBμ 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.

TABLE 2
FIRST STAGE AND BALANCING

Dependent Variable	Mean (1)	Observations (2)	Radio Estimate (3)	Standard Error (4)	Public Estimate (5)	Standard Error (6)	Controls (7)
	A. Constructed Radio Signal and Reported Radio Signal and Habits						
Radio ownership (DHS data)	.585	54,892	.04***	(.015)			District FE
Signal quality of Radio Rwanda (follow-up data)	5.8	154	1.38***	(.36)	−.04	(.17)	Baseline
Regularly listen to Radio Rwanda (follow-up data)	.42	154	.398***	(.148)	.03	(.092)	Baseline
Regularly listen to other stations (follow-up data)	.05	154	−.172***	(.063)	−.034	(.030)	Baseline
	B. Baseline Controls						
Gender (female = 1)	40%	438	−.029	(.063)	−.039	(.047)	Distances & district FE
Age	43.3	438	−.916	(1.783)	−1.23	(1.158)	Distances & district FE
Tutsi	.28	438	−.083	(.071)	−.045	(.035)	Distances & district FE
Distance: road	.1	438	−.009	(.009)	.001	(.003)	Distances (excl. road) & district FE
Distance: Kigali	60	438	5.22**	(2.04)	−.156	(.533)	Distances (excl. Kigali) & district FE
Distance: nearest city	28.7	438	−.223	(.973)	−.027	(.317)	Distances (excl. city) & district FE
Light density at night	.54	438	−.670	(.419)	−.064	(.051)	Distances & district FE
RTLM	.19	438	.394***	(.108)	.058	(.022)	Distances & district FE
Raven score	5.39	438	.162	(.165)	−.006	(.140)	Distances & district FE
Cell phones	69.5%	438	.027	(.060)	−.011	(.013)	Distances & district FE
	C. Additional Controls						
Log(income)	5.37	438	−.145	(.144)	.076	(.100)	Distances & district FE
Genocide	.518	438	.062	(.18)	−.007	(.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	.0004	(.067)	.051	(.046)	Distances & district FE
High school	2.1%	438	.039	(.025)	.019	(.013)	Distances & district FE
College	.2%	438	−.002	(.003)	−.003	(.003)	Distances & district FE
Facing north	.247	438	−.081	(.167)	.107***	(.034)	Distances & district FE
Facing south	.251	438	−.051	(.154)	−.058*	(.031)	Distances & district FE
Facing east	.315	438	.222	(.146)	−.034	(.031)	Distances & district FE
Facing west	.187	438	−.091	(.165)	−.016	(.021)	Distances & district FE
Log(sector population)	10.1	438	.094	(.112)	−.003	(.019)	Distances & district FE
Sector population density	6.17	438	4,779	(3,257)	−219.6	(211.3)	Distances & district FE
	D. Other Variables of Interest						
Log(land value)	8.36	438	.119	(.232)	−.115	(.186)	Distances & district FE
Radio station: Zombo	.290	438	.08*	(.043)	.005	(.013)	Distances & district FE
Radio station: Radio 10	.098	438	.142	(.087)	.010	(.009)	Distances & district FE
Radio station: BBC	.039	438	.029	(.025)	−.019	(.015)	Distances & district FE
Radio station: ContactFM	.039	438	.017	(.087)	−.008	(.009)	Distances & district FE
Radio station: Salus	.078	438	.044	(.027)	−.023	(.015)	Distances & district FE
Forced labor	.86	438	−.558	(.572)	−.232	(.273)	Distances & district FE
Migration (in lifetime)	22%	438	−.058	(.060)	−.022	(.033)	Distances & district FE
Migration (years in current home)	39.2	438	.012	(2.38)	−1.32	(1.34)	Distances & district FE
Migration (since 2004)	3.7%	438	.041	(.034)	.019	(.019)	Distances & district FE

NOTE.—Standard errors clustered by village are in parentheses. 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 cols. 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 A15 (public treatment). Balancing estimates for these two variables are shown in cols. 3 and 5, respectively. Their associated standard errors are presented in cols. 4 and 6. Col. 7 shows the controls included in each regression. All regressions with distances and district fixed effects 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 six are significant at least at the 10 percent level.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE 3
SIT Scores and Radio Rwanda Reception

	Dependent Variable				
	SIT Score (Within Error/Total Error)			SIT Score None (Within Error^2/Total Error)	SIT (Within Error/Total Error > .42)
	(1)	(2)	(3)	(4)	(5)
Radio Rwanda	−.133***	−.0960***	−.106**	−.319**	−.167***
	(.0463)	(.0289)	(.0464)	(.132)	(.0614)
Controls	Baseline	Baseline	Full	Full	Full
Mistake fixed effects	No	Yes	Yes	Yes	Yes
Observations	438	438	438	438	438
R^2	.213	.402	.426	.416	.285
Control group mean of dependent variable	.855	.855	.855	1.243	.916

Note.—Standard errors clustered by village are in parentheses. 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; and cardinal direction the village faces. Mistake fixed effects are a set of five 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 the three variants are each described in Sec. IV. In col. 5, the dependent variable is binary, and this column is estimated using a linear probability model.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

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 1 dB intervals. As in the case with the SIT, here, too, we would expect the largest effects for the signals around 45 dBμ. Once again, the estimates generate the expected inverse-U pattern. The evidence from both table 4 and figure 5 suggests 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. Interethnic Trust (I): Evidence from the Survey

Given the importance of interethnic trust for any form of nation building, we measure interethnic 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.

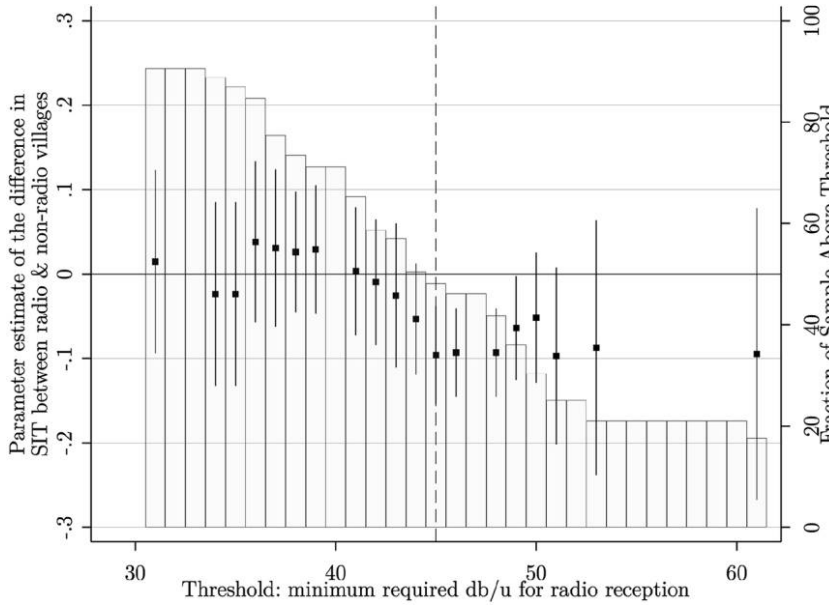


FIG. 4.—SIT and Radio Rwanda reception. The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of 1 dB). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95 percent confidence 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 dBμ).

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 4-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 columns 3 and 4. With the baseline set of controls one is able to reject equality of the coefficients at the 10 percent level ($p =$

TABLE 4
SELECTION OF INTERETHNIC 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	.156***	.171***	.382***	.378***
	(.0274)	(.0360)	(.0955)	(.0924)
Controls	Baseline	Full	Full	Full
Observations	438	438	438	438
R^2	.406	.421	.497	.440
Control group mean of dependent variable	.420	.420	1.01	1.22

NOTE.—Standard errors clustered by village are in parentheses. 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; and cardinal direction the village faces. All three outcomes are a variant of the share of out-of-ethnic group partners chosen. Col. 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.

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

.068), and there is a significant difference with the full set of controls at the 1 percent level ($p = .005$).

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. Interethnic Trust (II): Evidence from the Trust Game

We depict trust game offers by radio status and partner type (interethnic vs. coethnic) in figure 7*A*. Here we observe that overall levels of trust (both coethnic and interethnic) appear lower in radio regions than in nonradio regions. However, this pattern is reversed and interethnic 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 7*B*. In addition, we note that in regions not exposed to Radio Rwanda, average interethnic trust offers are lower than coethnic offers. However, exposure to Radio

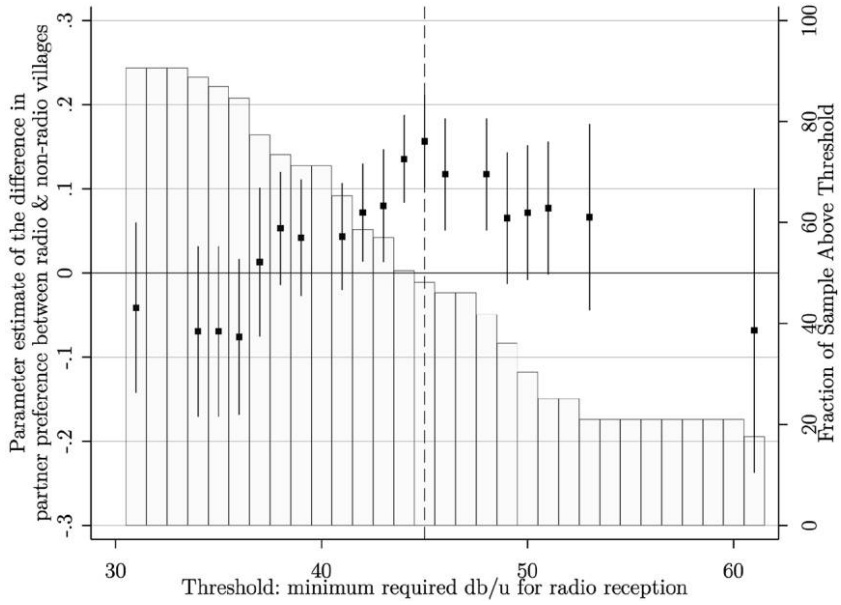


FIG. 5.—Partner selection measure and Radio Rwanda reception. The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of 1 dB). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95 percent 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 dBμ).

Rwanda reverses this, and we observe that interethnic trust offers are marginally higher. Indeed this is unusual and is the only example we know of with higher offers in interethnic relative to coethnic 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 interethnic, private trust game; 150 subjects who played the coethnic, private trust game; 71 individuals who played the interethnic, public trust game; and 125 individuals who played the coethnic, public trust game.

1. The Private Trust Game

We first examine whether subjects in Radio Rwanda villages make higher interethnic trust offers in the private treatment. Panel A, columns 1–6, of table 6 shows the main private trust game results: subjects receiving a Radio Rwanda transmission make higher trust game offers, in the intereth-

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	.250**	.0462	.273**	−.106
	(.105)	(.170)	(.114)	(.191)
Equality of coefficients (p-value)	.068		.005	
Controls	Baseline		Full	
Observations	438	438	438	438
R^2	.159	.127	.174	.141
Control group mean of dependent variable	2.92	3.17	2.92	3.17

NOTE.—Standard errors clustered by village are in parentheses. 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; and 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 cols. 1 and 2 and cols. 3 and 4.

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

nic variant of the game, of over RWF 145 out of a largest possible offer of RWF 600 (cols. 1 and 3), or about 1.13 standard deviations. This corresponds to offers that are about 47 percent higher on average (col. 5).

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

Figure 8 shows our placebo exercise. We find the expected inverted-U pattern.

2. The Public Trust Game

The results from the public trust game are described in panel B of table 6.[48] Observe that public trust game offers are not higher among those exposed

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

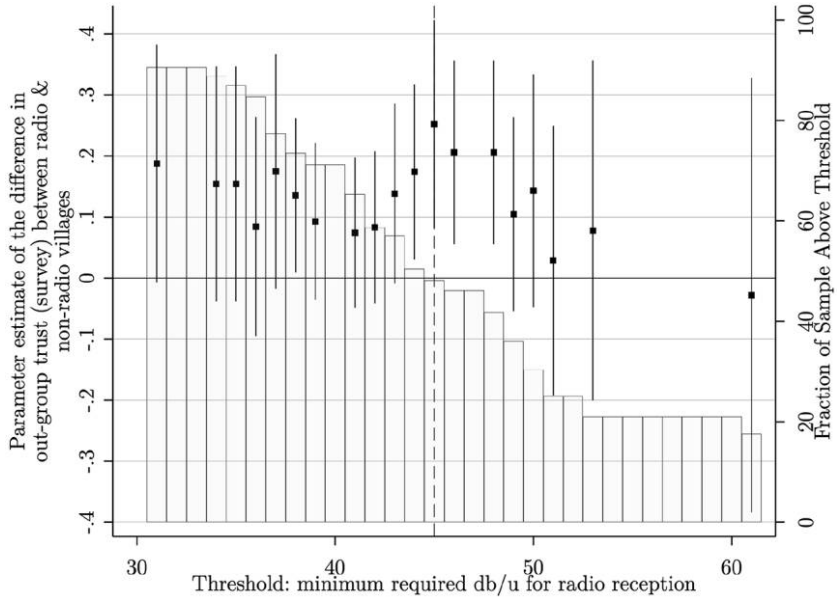


FIG. 6.—Out-group trust (survey) and Radio Rwanda reception. The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of 1 dB). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95 percent 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 dBμ).

to Radio Rwanda, in either the interethnic or coethnic samples. Furthermore, the estimates from the public trust game not only are smaller than their private trust game counterparts but are significantly so (see 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 interethnic trust game offers in public relative to private games (table A17, cols. 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 coethnic 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

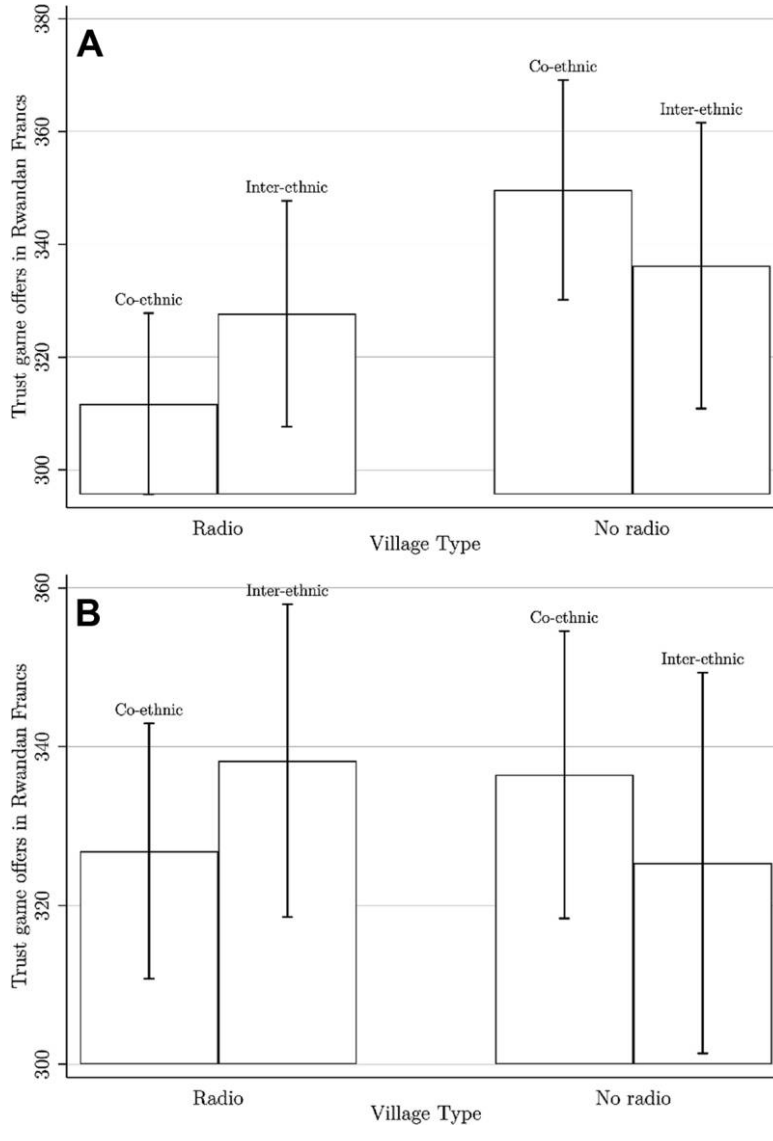


FIG. 7.—Means of trust game outcomes in radio and nonradio regions. The figure shows the mean offers in the private trust game and associated 95 percent confidence intervals. Radio versus nonradio villages are split according to a 45 dBμ cutoff used throughout the paper. Panel A shows unconditional means. 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.

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.[49]

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 interethnic attitudes is an empirical question. Accordingly, we now examine whether the observed change in interethnic 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 because only two villages in our sample (representing fewer than 5 percent 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 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

[49] We also use data from Blouin (2016) on contractual defaults in the agricultural sector to further examine this issue. One question in those 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.

TABLE 6
TRUST GAME OFFERS AND RADIO RWANDA RECEPTION

	Dependent Variable					
	Trust Game Offers				Log(Trust Game Offers)	
	Interethnic Partnership (1)	Coethnic Partnership (2)	Interethnic Partnership (3)	Coethnic Partnership (4)	Interethnic Partnership (5)	Coethnic Partnership (6)
	A. Private Trust Game Offers					
Radio Rwanda reception	148.5***	10.79	168.6***	−53.01	.476**	−.113
	(52.51)	(32.70)	(54.43)	(41.97)	(.228)	(.118)
Equality of coefficients: interethnic − coethnic (p-value)	.004		.0001		.003	
Controls	Baseline		Full		Full	
Trust game–specific controls	Yes		Yes		Yes	
Observations	92	150	92	150	92	150
R^2	.670	.406	.718	.489	.698	.508
Control group mean of dependent variable	325	343	325	343	6.38	6.45

	B. Public Trust Game Offers					
Radio Rwanda reception	−37.60	−1.248	.473	−52.67	.174	−.164
	(59.07)	(55.85)	(77.24)	(65.05)	(.217)	(.195)
Equality of coefficients: interethnic − coethnic (p-value)	.570		.479		.121	
Equality of coefficients: private − public (p-value)	.0001	.799	.002	.994	.090	.716
Controls	Baseline		Full		Full	
Trust game–specific controls	Yes		Yes		Yes	
Observations	71	125	71	125	71	125
R^2	.744	.388	.799	.484	.843	.459
Control group mean of dependent variable	355	358	355	358	6.49	6.47

NOTE.—Standard errors clustered by village are in parentheses. 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; and cardinal direction the village faces. All outcome variables reflect the sender's offer to the receiver in the trust game. The equality of coefficients interethnic − coethnic (p-value) tests for the equality of Radio Rwanda coefficients between cols. 1 and 2 and cols. 3 and 4 and cols. 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.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

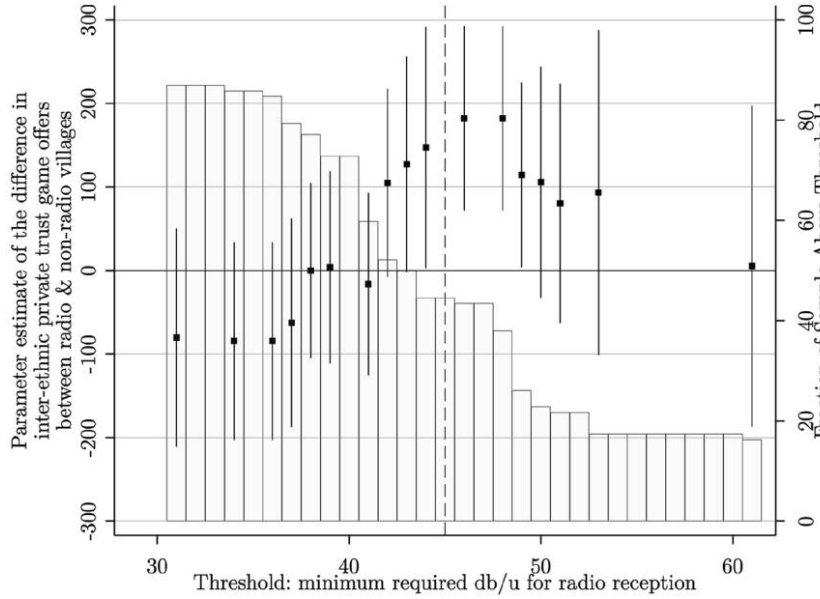


FIG. 8.—Private, interethnic trust game offer and Radio Rwanda reception. The graph plots estimates for each possible choice of the threshold defining whether a village receives a radio signal (at intervals of 1 dB). Point estimates on the Radio Rwanda reception coefficient are denoted by a square, and lines represent 95 percent confidence intervals. The dependent variable in each regression is the interethnic 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 interethnic 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 dBμ).

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 interethnic 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 interethnic or coethnic trust. Furthermore, we cannot rule out (as we could in the Radio Rwanda case) that the effect of other stations is the same for coethnic and interethnic trust ($p = .953$). Columns 3 and 4 show results from a similar exercise that deals with concerns that the Radio Rwanda estimates observed earlier were

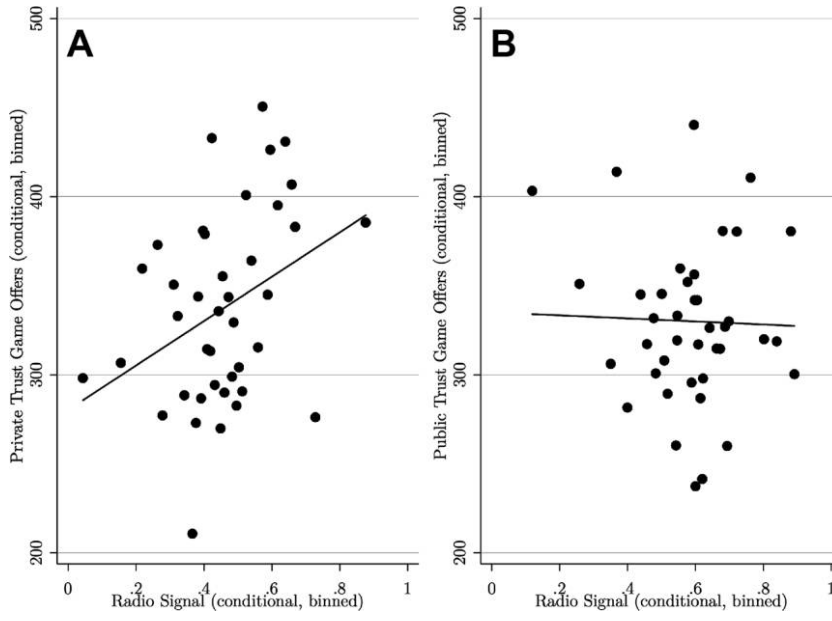


FIG. 9.—Interethnic trust game offers in private (left) and public (right) and Radio Rwanda reception. This figure shows the conditional correlation between Radio Rwanda reception and both private interethnic trust game offers (left) and public interethnic trust game offers (right). In both cases the 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, partner's 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.

simply due to the fact that it has a much larger network than the other stations. Similarly 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 interethnic or coethnic attitudes.

Columns 5–14 repeat this exercise for each of the five individual stations. As the table shows, the only significant interethnic 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 (cols. 6 and 14). In general, we are not overly concerned that the Radio Rwanda

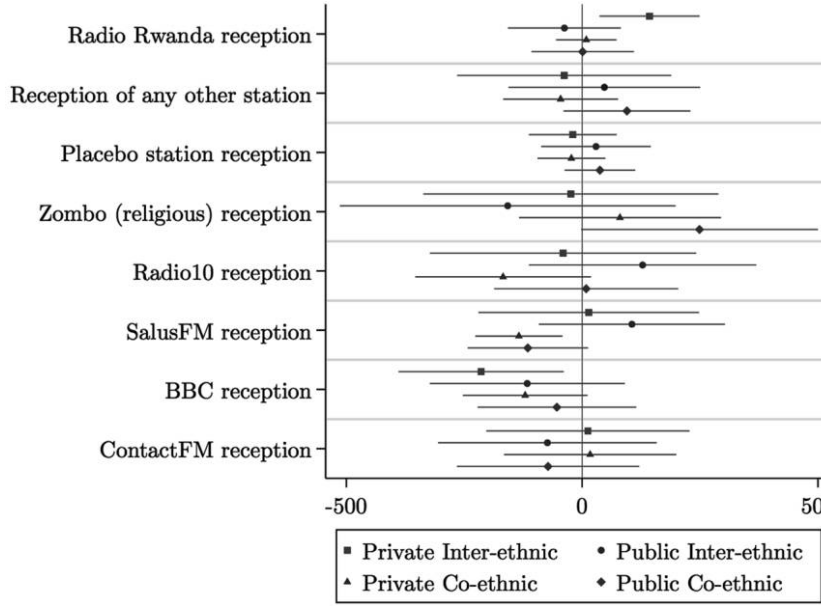


FIG. 10.—Trust game offers and reception of each radio station. This figure includes estimates for all radio stations that service more than one village in the sample. Each estimate and 95 percent confidence interval represents a regression coefficient from a separate regression. The coefficient of interest 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, partner's radio reception, the enumerator in the trust game, the distance between the villages of the sender and the partner, as well as the partner's 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 interethnic Radio Rwanda coefficient at the 10 percent level. For private interethnic games we have three (BBC, any other station, placebo), for private coethnic games we have six (Radio Rwanda, BBC, Radio10, Salus, any other station, placebo), for public interethnic games we have five (Radio Rwanda, BBC, ContactFM, Zombo, placebo), and for public coethnic games we have five (Radio Rwanda, BBC, ContactFM, Salus, placebo). In total, 19 of the 31 estimates other than Radio Rwanda × Private × Interethnic are statistically different from that estimate.

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 interethnic private estimate from table A19, we see a positive estimate in the trust survey (panel D, col. 3) and a negative estimate in the SIT (panel D, col. 1). Both are seemingly inconsistent with a poor interethnic attitudes interpretation that might otherwise be suggested by the trust game esti-

mate. This apparent contradiction undermines the legitimacy of both associations. For each of the other stations, no similarly 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 in which 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 broadcast 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—that 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 interethnic attitudes. This is an important channel to consider because recent evidence suggests that exposure to war violence generates cooperation and prosocial behavior (Bauer et al. 2016). This prosocial 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., because of a labor shortage), this may also foster social cooperation and trust (Rogall and Yanagizawa-Drott 2013). Third, greater intergroup competition may favor prosocial behavior especially among 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 prosocial 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 prosocial interethnic 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 mismeasurement or misspecification of these variables would have to be correlated with both radio signal and interethnic 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 toward zero. If they did, this would be evidence of a spurious correlation. However, table A23 shows that we estimate effects very similar 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 percent 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 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's results using our sample and shows that receipt of an 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 interethnic attitudes. As mentioned above, several studies illustrate that exposure to the genocide could result in greater prosocial behavior. However, most of the evidence (as well as the theory) suggests that this should be reflected in improved coethnic attitudes and trust and not interethnic 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 interethnic 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 interethnic relations. This lack of precision could occur 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 his 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 misattributing 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 mismeasurement 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 mismeasurement is likely to be correlated with radio coverage by examining the nature of coethnic transactions in radio versus nonradio regions. Suppose we allow for heterogeneity among the Hutu such that a subset of them decide to masquerade as Tutsi in radio regions, 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 coethnic matches among both the Tutsi and the Hutu. For example, this would mean that several games in radio regions coded as Tutsi-Tutsi coethnic games should have been coded as interethnic games. This may lower the observed Tutsi coethnic offers in radio regions. Similarly, perhaps any Hutu who do not try to pass as Tutsi are different either be-

cause 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 nonradio regions.

Following this logic, we compare whether the nature of coethnic trust game offers differs between the radio and nonradio 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 (RWF 17 ≈ US$0.01). 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

1. 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 first either 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 interethnic trust game offers, coethnic 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.

2. 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 coethnic pairs being from villages that are closer to each other than interethnic 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 coethnic and interethnic 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.[50]

3. 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 vs. 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 vs. 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, on the basis of 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 who live in a Radio Rwanda village.

4. 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 because of either different attitudes or differences in strate-

[50] 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).

gic behavior used in order to appear to have good interethnic 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, have increased interethnic 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 are 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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