

## The Origins of Violence in Rwanda

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**Abstract:** This paper shows that the intensity of violence in Rwanda's recent past can be traced back to the initial establishment of its precolonial state. Villages that were brought under centralized rule one century earlier experience a doubling of violence during the state-organized 1994 genocide. Instrumental variable estimates exploiting differences in proximity to Nyanza – an early capital – suggest these effects are causal. In other periods, when the state faced rebel attacks, with longer state presence, violence is lower. Using data from several sources, including a lab-in-the-field experiment across an abandoned historical boundary, I show that the effect of the historical state is primarily sustained by culturally transmitted norms of obedience. The persistent effect of the precolonial state interacts with government policy: Where the state developed earlier, there is more violence when the Rwandan government mobilized for mass killing and less violence when the government pursued peace.

**Keywords:** Violence, States, Rwanda

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Several recent studies examine the long-run impact of precolonial states in Africa (Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013). The authors find that achieving greater bureaucratic complexity before colonization has a significant positive effect on contemporary economic development despite most precolonial states being absorbed into larger countries during the Scramble for Africa (Michalopoulos and Papaioannou, 2016), despite European colonial powers radically changing political institutions (Acemoglu et al., 2001) and despite post-independence governments frequently failing to represent the interests of inhabitants of these former states (Easterly and Levine, 1997). Although the effect of precolonial states on development is arguably causal, previous studies do not focus on identifying exact causal mechanisms.

In this paper, I use village level data on violence, individual level data on rule following behavior from a lab-in-the-field experiment, data on attitudes towards rule following from surveys, and extensive observational data on local government to study the effects of exposure to state institutions. I test the hypothesis that one of the effects of the establishment of a centralized state is the development of norms of obedience to political authority to develop (Putnam et al., 1994; Guiso et al., 2014). Because the precolonial Rwandan state was a 'feudal' patron-client state, exposure to the state changed social norms about loyalty and obedience towards patrons and - ultimately - the state (Vansina, 2004; Des Forges, 2011). If such norms persist over time, historical differences in their presence or 'strength' may be measurable today.<sup>1</sup>

Rwanda is an attractive laboratory for studying the impact of the state for three main reasons. First, I can exploit the fact that the precolonial Rwandan state - the Nyiginya kingdom - expanded slowly between 1700 and colonization in 1897, introducing local variation in exposure to the state.<sup>2</sup> Second, Rwanda is the only African country whose current borders are largely coincidental with the borders of its (only) precolonial state. This fact allows me to study the Rwandan precolonial state in isolation. Third, Rwanda's recent history provides an elegant natural experiment to test the idea that the historical state affects norms of obedience to authority today. Between 1990 and 1993, the Rwandan government fought

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<sup>1</sup>I understand a 'norm' in this paper as a belief about the 'right' course of action. Norms may optimally arise as a heuristic for decision making when information processing is costly (see the evidence summarized in Nunn (2012)).

<sup>2</sup>The focus in this paper is therefore on the intensive margin of the presence of the state, comparing villages in precolonial districts that were incorporated earlier to those incorporated later. Because the authority of the state at a local level takes time to form, I expect the effect of state on rule following behavior to be stronger with longer exposure to state institutions. This idea is consistent with notions of the accumulation of social, civic, and democratic capital discussed in Putnam et al. (1994); Guiso et al. (2014); Persson and Tabellini (2009) and Guiso et al. (2010). In a recent model of the incentives to cooperate by Tabellini (2008) the establishment of a government has a slowly diffusing effect on civil society due to parent-child transmission of cooperative values. Because the incentives for parents to inculcate cooperative values depend on the number of cooperators in society, it may take several generations for stronger norms of cooperation to develop (Tabellini, 2008). Similar mechanisms predict that when obedience is enforced by a community, enforcement norms are built up slowly (Acemoglu and Jackson, 2016).

to maintain territorial control in the face of a rebellion. In 1994, it instead organized the Rwandan genocide, in which it managed to mobilize hundreds of thousands ordinary Rwandan Hutu to exterminate the Tutsi minority. After the genocide, a new government fought former genocide perpetrators to re-establish territorial control. The fact that the policies of the Rwandan government changed so quickly is the most interesting reason to study this period and time because it provides a natural experiment to study the nature of persistence of historical institutions. To be precise, should the effect of the historical state indeed work through the likelihood of individual Rwandans heeding the demands of the government, I expect that when the Rwandan government was facing attacks, Rwandans living where the state established earlier were less likely to rebel and these places should therefore be less violent than places where the state established later. When the government instead sanctioned violence and mobilized the population for genocide, I expect more violence where the state established earlier. In other words, the effect of the historical state should be conditioned by modern policy, if the data support the hypothesis of this paper.<sup>3</sup> I exploit these predictions by separately estimating the effect of the development of the state on violence in periods of rebel attacks and during the genocide.

To test the hypothesis of this paper, I first combine village level data on violence with a detailed reconstruction of the expansion of the state. I find that villages where the state was established earlier experience more violence during the genocide. This result is consistent with individuals being more likely to comply with the state's call to mobilize for violence in areas with longer state presence. When I separately study individuals involved in the organization of the violence and participation of ordinary civilians, I find that the effect is stronger for mobilization of ordinary civilians. In contrast, in the years just prior to and just after the genocide, and again consistent with the hypothesis of this paper, I find that violence is lower where the state developed earlier. Figure 1 provides a timeline of the main events studied in this paper and a graphical summary of these results.

A competing explanation for this result is that places where the state developed earlier were more 'obedient' to begin with, and therefore easier to conquer. Alternatively, places where the state developed earlier may simply have been more developed. To establish the causality of my main finding I pursue three strategies. First, I estimate the effect of the historical state when the contemporary government was facing rebel attacks and when it was mobilizing for genocide. If there were omitted time-invariant his-

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<sup>3</sup>Anecdotally, historians observers have noted the impact of Rwanda's long tradition of centralized government in each of these periods. Studying the genocide, Gérard Prunier writes: "Rwandese political tradition, going back to the Banyiginya Kingdom ... is one of systematic, centralised and unconditional obedience to authority" (Prunier, 1995, p. 141). Studying post-genocide rebuilding, Philip Reyntjens writes: "An ancient state tradition played an undeniable role here: a mere two years after the extreme human and material destruction of 1994, the state was rebuilt. Rwanda was again administered from top to bottom" (Reyntjens, 2013, p. 25).

torical or geographical factors that explained the main result, I would expect to find a constant effect of the state on violence across these periods. Since I find that the effect of the state changes with the demands that are made on the people, such confounders are unlikely to drive my results.<sup>4</sup> Second, I directly control for variables that are candidates for contemporary omitted variables, such as the fraction of Tutsi and household income in 1990. I also control for several measures of the impact of the colonial period. Controlling for these covariates has little impact on the effect of the state.<sup>5</sup> Third, I re-estimate the relationship between state presence and violence using distance to Nyanza, an early capital village, as an instrumental variable for the timing of expansion of the state. The logic behind the use of this distance is that expansion of the precolonial state proceeded from Nyanza but today Nyanza is not particularly important for the state, not least because it was replaced as the capital in the early colonial period by Kigali - Rwanda's current capital city. It may still be the case that Nyanza was significantly more productive or otherwise attractive before the founding of the state, and was therefore selected as a capital village. I verify, however, that Nyanza was not unusual before the founding of the state using archeological data to capture population density, as well as terrain characteristics such as elevation, and soil suitability to capture productivity differences. These tests suggest that it is unlikely that distance to Nyanza influences violence, other than through its impact on the expansion of the state. Using this strategy, I again find a stable and significant effect of the establishment of the state on violence. A one century increase in the presence of the precolonial state at a local level is associated with an eight percentage point increase in violence during the genocide (relative to a mean of 8%) and a 37% decrease in the number of violent events before and after the genocide.

Besides providing support for the hypothesis of this paper, the main result speaks to the broader literature on the long-run impact of the state. Several prominent studies find that precolonial states positively impact long-run development in Africa (Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013; Depetris-Chauvin, 2013). Other studies find that factors usually considered conducive to development can have adverse consequences for development as well (Satyanath et al., 2013; Acemoglu et al., 2014). My results suggest that these findings may be reconciled by the interaction of persistent factors and government policy. This finding has two broader implications: Historical institutions do not just persist and exert a constant effect on economic development today (Nunn, 2009), but their persistent effects are potentially non-linear and interact with policy. Conversely, the effect of a policy change likely interacts

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<sup>4</sup>Naturally, it may be the case that confounders have time-varying effects that coincide with the periods I study. I report instrumental variable results and several robustness checks to alleviate this concern.

<sup>5</sup>Ethnic composition and household income may be outcomes of the formation of the precolonial state. I therefore separately regress these measures on state presence, and find that there is no systematic relationship.

with society in a way that can be understood by mapping out persistent historical variation. The main result furthermore naturally speaks to the literature trying to understand the causes of the massive death toll in the genocide (André and Platteau, 1998; Yanagizawa-Drott, 2014). The present results suggest that a long history of centralized rule affected popular participation in the killings.

Before turning to mechanisms, I use data on radio ownership to provide further insight into the successful mobilization of ordinary Rwandans. The Rwandan government used the radio to sanction violence and mobilize its citizens (see Glaeser (2005) for background and a model of such efforts), and I expect Rwandans in parts of Rwanda where the state established earlier to follow new rules more zealously.<sup>6</sup> I test this hypothesis using data on radio ownership in 1991, comparing the effect of the state in high radio ownership districts where orders are more easily communicated, to low radio ownership districts. During the genocide the positive effect of state presence is stronger in places with high radio ownership. Consistent with the changing objectives of the government, the negative effect of state presence before and after the genocide is stronger in areas with high radio ownership as well.

The results for violence provide evidence consistent the hypothesis that the historical state affects the way citizens interact with the state today. In the rest of the paper, I directly test this hypothesis as well as several competing hypotheses. My focus is on distinguishing between two main mechanisms: Persistent differences in citizen's norms and beliefs about how to respond to government demands and persistent differences in the strength or presence of government institutions. I find that the establishment of the state affects a broad set of norms and beliefs about the desirability of obedience to authority, rather than the capacity of the state (at a local level). To establish stronger - culturally transmitted - norms of obedience as the primary causal mechanism, I employ three strategies. First, I directly measure the tendency of Rwandans to follow unenforced rules in an anonymous lab-in-the-field experiment with 420 participants in rural Rwanda. Anonymity and random selection of participations allows me in principle to separate rule following from the effects of the modern state, social networks or other external influences. I nevertheless directly test for their effects in a series of robustness checks. In the experiment, which is a modified version of the resource allocation game pioneered by Hruschka et al. (2014), I ask people to follow a simple rule. Crucially, it is costly to the participant to follow the rule and beneficial to disregard the rule. I expect that individuals living where the state established earlier are more likely to follow the experimental rule, even when unmonitored. I introduce variation in historical state presence by comparing participants that

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<sup>6</sup>There is a subtle distinction between following rules and following orders. However, because in the native language of Rwandans a law and an order are referred to by the same word (in *Kinyarwanda* both are *igeteko*), I do not pursue this distinction.

live close to, but on either side of, an abandoned outer border of the historical state in the eighteenth century. Across this border, the presence of the state discretely changes by about one century and I compare average rule following behavior in the two populations separated by this boundary. I find that individuals that are close to the boundary but just happen to be on the side that has longer state presence exhibit significantly greater rule following behavior.<sup>7</sup>

Second, I provide evidence on the source of transmission of the effect of the state from over a century ago to the present day. It may for instance be the case that although there are differences in norms of rule following behavior, these are sustained by differences in local government. To further distinguish between internalized norms and factors that are external to the individual, such as the effects of local government, I compute a second treatment within the experiment. I record birthplace, rather than living place, of each participant. The idea behind this measure is that when participants move, they take their norms and values with them into a new institutional environment. If the main source of persistence is cultural transmission, then the place where participants grew up should be more important than where they live. If local differences in institutions are the primary source of persistence their location should be more important than the place of birth. My estimates show cultural transmission of norms of obedience is the main source of persistence.

Third, I use extensive data from household surveys and government reports to further understand the effects of the historical state. The advantage of using these data is that they cover all of Rwanda and that they therefore provide evidence that the results in the lab generalize beyond the experimental context. I first use data from the World Values Survey. Individuals that live in places with longer state presence are less likely to find rule breaking acceptable, and are more likely to think obedience to authorities is central to democracy. I then turn to participation in village level meetings between citizens and government representatives. Individuals who today live in areas with longer state presence are equally likely to attend community meetings, but are less likely to speak up in these same meetings and the government is more likely to be the only party speaking. Moving beyond attitudes, I then assess contemporary household violence. The 2003 Rwandan constitution criminalized violence against women and I use data from the 2014 Demographical and Health Survey to study violence against women in the household. I find that women in villages with longer state presence are less likely to experience verbal abuse, physical violence or sexual violence at the hands of their partners. I then use government data on local provision of pub-

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<sup>7</sup>In a series of balance and robustness tests I show that the presence of the local government, corruption, and several other measures that are likely candidates for omitted variables balance across the boundary.

lic goods and government spending to test whether public goods provision or government expenditure can explain my results. Consistent with the results in the experimental sample, I find precisely estimated zero effects of historical state presence on modern state capacity. Using data on public good provision during the colonial period, I verify that there are no differences in public good provision for this period either. This zero effect is likely driven by the fact that all Rwanda's physical government infrastructure - its buildings, roads etc. - as well as its tax bureaucracy were built up in the (post)colonial period. The Belgians explicitly aimed at creating a uniform bureaucratic infrastructure across Rwanda, for example (Dorsey, 1989).

I also verify that several other hypotheses about the intensity of violence in Rwanda are not the primary drivers of the effect of the precolonial state. I show, for example, that neither population growth, which has been hypothesized to lead to Malthusian pressures and violence (André and Platteau, 1998), nor the government hate radio whose propaganda affected violence during the genocide (Yanagizawa-Drott, 2014) can explain the effect of the historical state away.

The results in this paper provide support for its hypothesis: Exposure to state institutions historically affects cultural norms of obedience today. Its ultimate effect on development, however, is determined by the interaction of this channel with government policy. The effect of the precolonial state is conducive to development when the modern state pursues 'pro-development' policies. If the state organizes violence, the effect of the precolonial state is to facilitate its organization. It is plausible that such effects are not confined to the Rwandan context. For instance, the obedience of the German population to Nazi demands is often associated with the successful execution of the Holocaust (Goldhagen, 1996). When the German government does not pursue genocide, rule following behavior likely facilitates economic exchange and positively affects prosperity (Tabellini, 2010).

The results in this paper complement a large literature that identifies a positive reduced form effect of historical states on modern development outcomes (Acemoglu et al., 2015; Bockstette et al., 2002; Dell et al., 2015; Depetris-Chauvin, 2013; Dincecco and Katz, 2014; Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013). I contribute to this literature by providing a mechanism that drives these effects. By providing evidence for society's response to the establishment of the state as a mechanism carrying the effect of the state, this paper contributes to the literature finding a positive relationship between historical states and civil society (Putnam et al., 1994; Guiso et al., 2014) and to a growing empirical literature in economics that finds more mixed results. For instance, Dell et al. (2015) show that when the

state exercises its capacity through a strong civil society, it is less open to foreigners, Acemoglu et al. (2014) show that chiefs in Sierra Leone use civil society institutions to cement their own authority and Satyanath et al. (2013) show that higher social capital led to increased Nazi party membership in Germany.<sup>8</sup> By showing that rule following is affected by the historical Rwandan state, I contribute to a literature that studies the effects of history on modern preferences, attitudes, and beliefs (Alesina and Fuchs-Schündeln, 2007; Di Tella et al., 2007; Nunn and Wantchekon, 2011; Giuliano and Spilimbergo, 2014; Becker et al., 2015), and to a small but growing literature that uses experimental methods to study long-run economic development (Lowes et al., 2017; Blouin, 2014). This strand of literature is part of a broader effort to understand institutions, culture, and their interactions (Alesina and Giuliano, 2015). By providing evidence that state presence and compliance are important determinants of within-country violence, I contribute to the study of the determinants of violence and conflict (Blattman and Miguel, 2010; Besley and Persson, 2009; Dube and Vargas, 2013; Collier and Rohner, 2008; Esteban et al., 2012), as well as to the study of individual motivations to join conflict (Humphreys and Weinstein, 2008). By pointing out the important role of compliance of ordinary Rwandans as a driver behind genocidal violence, I contribute to the literature in economics that tries to understand drivers of violence during the genocide, from the role of government propaganda (Yanagizawa-Drott, 2014), to the role of youth militias (Rogall, 2014), to community meetings (Bonnier et al., 2015) and food scarcity (André and Platteau, 1998). Finally, this paper is related to a large and heterogeneous literature on socialization by nation states, parents and modern governments, see e.g. Anderson (1983) and Cantoni et al. (2014). The papers that are closest to this study are Yanagizawa-Drott (2014) who provides empirical evidence that the government radio was instrumental in organizing the genocide and Lowes et al. (2017) who study the effects of the historical Kuba kingdom on rule following today. The latter authors use a version of the resource allocation game that is similar to the one I use. They, however, reach the opposite conclusion: Citizens of the Kuba kingdom are less likely to follow unenforced rules today. The difference in conclusion likely lies in the nature of the precolonial state studied. Whereas the Kuba kingdom had laws and courts among other hallmarks of a bureaucratic state and, the authors show, these institutions crowded out social norms, the Nyiginya state lacked formal institutions and enforced its demands through strengthened social norms. Section 1 elaborates further on this distinction.

The rest of this paper proceeds as follows. Section 1 gives an overview of the relevant episodes of Rwandan history. Section 2 presents the estimation framework used in this paper. Section 3 presents the

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<sup>8</sup>In political science, there is a large literature on the interaction between the state and civil society, see for instance Mann (2005), Valentino (2013) and Bates (2008).



main results of this paper, relating state presence to violence. Section 4 studies mechanisms, and section 5 concludes. An appendix provides further relevant historical background and additional results.

## 1 Historical background

This section presents the relevant historical and institutional background to this paper. Before discussing the recent history of conflict in Rwanda, I outline the expansion and the organization of Rwanda's pre-colonial state, the Nyiginya kingdom. This history forms the background to the more abstract hypothesis that the state affects obedience to authority.

### 1.1 The expansion and organization of the Nyiginya kingdom

The Nyiginya kingdom was Rwanda's first state, replacing the extended family group - or 'kinship group' (*umulyango* in Kinyarwanda) - as the highest level of political organization. It was founded in the late sixteenth century and was initially confined to a small area in central Rwanda called *Nduga* (Vansina, 2004). From 1700 to 1897, the small kingdom expanded to Rwanda's current borders (see figure 1 for the expansion and figure A1 in the appendix for *Nduga*). In 1897 Rwanda became part of German East Africa and in 1916 it became a Belgian colony. Although there were pressures towards consolidation of the kinship groups, there was no polity that managed to centralize power and project its authority beyond the limits of the extended kinship group. The Nyiginya kingdom was the first polity to overcome these constraints by forming a standing army and organizing a rudimentary bureaucracy. Appendix sections 1.1 and 1.2 provide the relevant historical detail behind the emergence of these institutional innovations. Note that Rwanda throughout its history remained untouched by the slave trades.

We can get a sense of the degree of organization of the Nyiginya state by comparing it to other pre-colonial African states. The Murdock ethnographic atlas, which attempts to record systematic information on precolonial African ethnic groups classifies Rwanda as having three levels of bureaucracy above the village level (Murdock, 1967). The Nyiginya bureaucracy was therefore comparable in sophistication to the Buganda state in Uganda and the Asante Empire in Ghana.

Relative to these states, however, the Nyiginya kingdom was organized as a patron client state and lacked many features of modern states: It provided no public goods aside from external security, had no money or writing and innovated no formal rules or checks and balances for conducting government. It in-

stead sustained its army and its bureaucracy through 'feudal' patron-client networks in which protection was exchanged for labor services. To give an example, rather than setting up a tax collection bureaucracy, the state coopted kinship group chiefs as clients into the networks that existed within the state bureaucracy and requisitioned taxes from them. A dramatic change, however, was that the weight of the state and its military was now behind the authority of the chiefs, who demanded higher taxes and obedience to the authority of the state. These chiefs, in turn, were able to raise taxes from their group through social norms prescribing the payment of tribute by their kinsmen (Vansina, 2004). This meant that expansion of the state boiled down to breaking local resistance and subsequently embedding local social relationships in the broader patron-client networks of the state (Des Forges, 2011). The appendix provides further detail on the incentives that sustained the patron-client bureaucracy.

In this context Rwandans had a relationship with the central state, which requisitioned food and labor services. Sustained by threats of violence by the army for non-compliance, Rwandans became used to following the demands of this new central state (Des Forges, 2011), so much so that by the 1870 the Rwandan population had effectively become enserfed and society had become highly militarized (Vansina, 2004). There are many examples from the early colonial period that detail how Rwandans in areas of the country that were incorporated into the precolonial state only shortly before colonization were disobedient and rebellious, since they were not yet subjected to the discipline of the military and the patron-client bureaucracy. In these areas, the German and Belgian colonial armies had to intervene regularly to stop tax protests and outright rebellion against Nyiginya authority (see the case studies in Des Forges (1986, 2011), Louis (1963) and Newbury (1987), as well as Botte (1985a,b) for a timeline of expeditions).

Interwoven with the history of the state is the history of the Hutu and Tutsi, the two main social groups in Rwanda. Initially Hutu and Tutsi social classifications of rich, cattle-owning, elites (Tutsi) and poor, farming, masses (Hutu). The Belgian colonizers turned this fluid economic distinction – upwardly mobile Hutu could become Tutsi and Tutsi could become Hutu – into a rigid racial distinction by classifying every Rwandan as either Hutu or Tutsi (or Twa, a marginal ethnic group accounting for about one percent of Rwanda's population). Although estimates vary, about 85% of Rwandans were Hutu and about 14% were Tutsi. The Belgians favored Tutsi as their agents of indirect rule and restricted access to education and lucrative government positions for Hutus.

During the colonial period, modern bureaucratic institutions were built up, consisting of a trained bureaucracy, physical infrastructure such as roads and government offices, and (irregular) censuses. In

this period, authority based on social relationships was starting to be replaced by authority based on the rule of law (Des Forges, 2011). In 1963 Rwanda gained its independence. The drive for independence resulted in part from a large Hutu rebellion in 1959 which is seen as a culmination of the accumulated grievances held by Hutu (Lemarchand, 1970).

## 1.2 Violence between 1990 and 2000

During the 1959 rebellion a large number of Tutsi fled to Uganda and many of their children joined the Ugandan army. In 1990, they formed the Rwandan Patriotic Front (RPF) and invaded Rwanda. Between 1990 and 1993, the Rwandan army fought the RPF. It also used the fight against the RPF as a pretext to assassinate political opponents and, as was later revealed, to organize practice killings for the genocide. In 1993, the invasion was halted and peace negotiations held in Arusha, Tanzania, resulted in the establishment of a multiparty democracy. Several political parties were formed, some openly sympathetic to Tutsi interests. Although multiparty democracy was supposed to lead to a peaceful integration of Hutu and Tutsi in society and repatriation of exiled Tutsi, hardliners within the Rwandan government organized the killing of all Tutsi which, they believed, would break the RPF's power base and would keep them in power (Mamdani, 2002).

On April 6th, 1994, an airplane carrying Rwanda's president, Juvénal Habyarimana, was shot down on its approach to Kigali. Within an hour, members of the political opposition were assassinated in their homes and roadblocks were set up all across the country. After this initial wave of killings, which targeted mostly senior government officials, the mass killing of all Tutsi began. In the hundred days that followed, between 500,000 and 1,000,000 Tutsi and politically moderate Hutu were killed by the army, youth militias (the *interahamwe*) or by machete blows from their next door neighbors, colleagues or even family (Des Forges, 1999). It is estimated that about 75% of the number of Tutsi identified in the 1991 census were killed (Straus, 2006). Mobilization was extraordinarily high, standing at 14-17 % of the adult male population (Ibid.). The genocide ended when the RPF captured Kigali.<sup>9</sup>

As the RPF set out to rebuild, many Hutu genocide perpetrators fled to the Democratic Republic of the Congo (DRC) and, to a lesser extent, to Tanzania. From a post-genocide population of about 6.9 million, 2.1 million fled to the DRC. Roughly 700,000 diaspora Tutsi emigrated back, almost fully replacing the

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<sup>9</sup>There is debate about the question when the government started planning for the genocide. It is clear that practice killings were being carried out in 1992 and 1993, which leads some commentators to point to the RPF invasion as the inception of the idea of genocide. For overviews, see Des Forges (1999) and Guichaoua (2010).

Tutsi population killed in the genocide. In total, about 3.6 million Rwandans, or about 45% of the pre-genocide population, lived in their pre-genocide villages (Prunier, 2008). Between 1995 and 2000 the Rwandan government fought former members of the youth militias. It also persecuted Hutu rebelling against the new government and those who were - in their view - likely to help the rebels.<sup>10</sup> Around 2000, the RPF-dominated government had re-established territorial control and had started a large-scale transitional justice effort, spearheaded by a system of 8000 local courts, called *Gacaca* courts (see below).

### **1.3 The role of the government during the genocide**

It is widely acknowledged that a group of hardliners with the ruling Hutu party - called the *akazu* - and the army planned the genocide in order to remove the power base - the Tutsi in Rwanda - of the invading RPF army (Straus, 2006). The mobilization of the population and the targeting of prominent Tutsi was very well prepared. Machetes were imported and stockpiled throughout the country, detailed lists of prominent Tutsi targets were drawn up, and a targeted propaganda effort was started to turn public opinion against the Tutsi (Des Forges, 1999). Mobilization of the population was left to local bureaucrats, and I describe the daily rhythm of mobilization and killings below.

### **1.4 The impact of the expansion of the Nyiginya state**

I hypothesize that the development of the Nyiginya state led to the development of a 'culture of obedience'. Its informal organization meant that, rather than being punished for non-compliance through, for instance, a legal system, Rwandans were confronted with the state through new patron-client relationships or the intensification of the relationship between an individual and their kinship group chiefs. Backed by the army, the state made significantly higher demands on the population, both in terms of time demanded in labor services as well as in taxes in kind than the chief did. In this context, longer exposure to the state may have built norms that prescribe how to interact with the state, or to be obedient. In the rest of the paper, I test whether the development of the state is linked to an observable outcome of obedience to the state, mobilization during the genocide. In later sections, I provide experimental and observational support for obedience to government authorities as a channel driving this result.

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<sup>10</sup>These killings targeted families of former genocide perpetrators, educated citizens, members of the former Hutu political parties, and people who did not act 'right' (Prunier, 2008).

## 2 Estimation framework

The identification strategy of this paper exploits the fact that the Nyiginya expansion proceeded by conquering territory adjacent to its existing domain. Since armies and bureaucrats travelled from the administrative center of the kingdom to conquer and administer new territories, places close to *Nduga*, its historical heartland, are more likely to be incorporated early. By the early twentieth century expansion of the state was halted by colonization and the colonizers moved the capital to Kigali, still Rwanda's capital today, near the geographical center of the country. Located in southern Rwanda, Nduga is not especially prominent for government today.

To use these facts as the basis for the identification strategy of this paper I follow three steps. First, I compute several measures of distance to *Nyanza*, an early capital village which can be located in southern Nduga.<sup>11</sup> Today, Nyanza is close to a modern town also called Nyanza, which is a local administrative center responsible for administering one of Rwanda's thirty districts (*akerere*). I will discuss how I empirically deal with the potential effect of being close to a local administrative center in detail below. Second, I show proximity to Nyanza is uncorrelated with measures of pre-Nyiginya population density and measures of agricultural productivity. Establishing these 'exogeneity' results shows that Nyanza did not look particularly attractive for settlement in a way that may correlate with violence today. Third, I show that proximity to Nyanza is correlated with state presence. Before discussing the data sources used in this paper at the end of this section, I discuss the estimating equations of this paper.

### 2.1 Constructing proximity to Nyanza

The main measure of proximity I use in this paper is straight line distance. In addition, I compute three more measures which capture not just the distance aspect of proximity but also take into account the costs terrain imposes on travel time. I compute these measures between Nyanza and the centroid of each pre-colonial district. I describe the reconstruction of these districts below and in more detail in the appendix.

**Distance to Nyanza.** The first instrument I use is the straight line distance in kilometers to Nyanza.

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<sup>11</sup>There were other capital villages such as *Nyamagana* and *Nyundo*. These may even have been established before Nyanza, but lost their importance early on whereas Nyanza was prominent throughout the expansion of Nyiginya kingdom. By the early nineteenth century Nyanza had become the main capital (Newbury, 1991, p. 100), and Nyanza became Rwanda's sole capital before colonization (Lugan, 1997). More pragmatically, Nyanza is also the only capital which can be precisely located. For Nyamagana and Nyundo, only approximate locations are known. Nyamagabe is said to have been in Southern Nduga (Vansina, 2004, p. 49), and Nyundo is said to have been in Southern Nduga as well, close to a place called Bunyogombe (Vansina, 2004, p. 241). It is nevertheless possible to use modern village names to get a sense of where these capitals were. Using similar villages names near the approximate historical locations suggests that Nyamagana and Nyundo were 13 and 15 kilometers away from Nyanza.

Distance is frequently used as a source of exogenous variation, such as in Becker et al. (2009) who use distance to Wittenberg as an instrument for the spread of Protestantism, Dittmar (2011) who uses distance to Mainz as an instrument for the spread of the printing press and Nunn and Wantchekon (2011) who use distance to the coast as an instrument for the intensity of the slave trade.

**Cost distance to Nyanza (days).** The second instrument I use considers elevation variability as an impediment to travel. I compute optimal walking routes to Nyanza and I use the average travel time along these routes to Nyanza (and back) as my second measure of proximity.<sup>12</sup> I measure time in days and assume that one day equals twelve hours of walking.<sup>13</sup> Figure 3 visualizes the process of generating this measure. Figure 3a maps elevation in Rwanda. Figure 3b plots contour lines of points that are six hours marching distance away from Nyanza. Figure 3c maps the optimal paths from Nyanza to each district. Finding the optimal routes involves a straightforward application of Dijkstra's algorithm (Dijkstra, 1959), see Dell (2015) for another recent application.

**Cost distance to Nyanza - Özak (days).** Özak (2010, 2013) has proposed a Human Mobility Index, which computes travel time taking into account not only elevation, but also weather patterns and soil conditions. As a third instrument, I use travel time in days to Nyanza computed according to his procedure.

**Cost distance to nearest Nyiginya army location (days).** I finally consider the expansion of the Nyiginya kingdom using the location of Nyiginya armies as the origin of expansion, rather than Nyanza. Although the Nyiginya bureaucracy was administered from the capital villages in the center of the kingdom, the King positioned armies throughout the kingdom to defend against invasions and launch new expansions (Vansina, 2004). I use data from Kagame (1963) who reports the location of each army battalion recruitment as well as its year of establishment. I compute travel time to the nearest army that was established before the annexation of a district as my fourth measure of proximity.

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<sup>12</sup>For gentle slopes, walking downhill goes faster than going up. For steep slopes, both downhill and uphill travel is significantly slower. Because of this asymmetry, I average over a return journey.

<sup>13</sup>I use Tobler's function (Tobler, 1993) to convert elevation changes into travel speed. This function is calibrated on data collected by Imhof (1950). Travel time  $t(s)$  between two points that are one meter apart is a function of the increase in elevation over that distance,  $s$ , measured in degrees:  $t(s) = \frac{18}{5} \exp(-3.5 |(\tan(s \frac{\pi}{180})) + 0.05|)$ .  $t(0) = 3.02205 \text{ km/h}$ . To compute marching speed over paved terrain, multiply by  $5/3$  (Tobler, 1993).

## 2.2 Estimating equations

In the next section I estimate the relationship between state presence and violence. The unit of observation for the main results is a sector, a small administrative unit which, at the time of the genocide, typically coincided with a large village or several smaller villages. The main treatment variable, the time a sector was exposed to the precolonial state, varies at the level of the precolonial district (see below). My baseline estimating equation is:

$$Y_{sdp} = \beta_0 + \beta_1 StatePresence_d + \mathbf{X}'_d \beta_2 + \mathbf{Q}'_s \beta_3 + r_p + \varepsilon_{sdp} \quad (1)$$

where  $s$  indexes sectors ( $n=1449$ ),  $d$  indexes precolonial districts ( $n=50$ ) and  $p$  indexes modern provinces ( $n=5$ ).  $Y_{sdp}$  is an outcome of interest, such violence in the 1994 genocide.  $StatePresence_d$  is the number of years between the annexation of precolonial district  $d$  and colonization in 1897.

$\mathbf{X}_d$  is a vector of travel distances along the 1988 road network. In addition to the distance to Nyanza, this vector includes distance to Kigali, Rwanda's current capital.  $\mathbf{Q}_s$  is a vector of sector level covariates, including straight line distance to the national border. The distance controls capture the location of several armed groups, such as the Rwandan army, the RPF rebels or militias who were active killers in the genocide. Distance from Kigali, for instance, is important because militias, who were important killers in the genocide, travelled from Kigali (Rogall, 2014). Similarly, distance to the border captures the presence of the RPF who invaded from Uganda in the North. During the genocide, for example, violence was concentrated in the South and West whereas before the genocide, violence was concentrated in the North. Fixed effects and distance controls ensure that I compare local differences in violence. Finally, driving distance to Nyanza captures any direct effects that may result from the ease of reaching modern Nyanza. I therefore focus on the historical relevant distance, which is straight line distance (all results are nearly identical without controlling for driving distance to Nyanza). These controls constitute a 'baseline' set of controls. This vector also includes several other geographical and historical covariates, as well as household income and the fraction of the population that was recorded as Tutsi in the 1991 census. I discuss these further as they are introduced in the empirics.<sup>14</sup>  $r_p$  is a vector of province fixed effects, dividing the country into the Northern, Eastern, Southern and Western provinces, as well as the area around and including Kigali.<sup>15</sup>  $\varepsilon_{sdp}$  is an error term.

<sup>14</sup>The census data vary at the commune level which is one administrative level above the sector but below the precolonial district.

<sup>15</sup>I use modern, post 2006 administrative reform, provinces since these provinces divide the country into four regions - north, east, south and west - plus Kigali and these are natural units for fixed effects. Eastern Rwanda has historically interacted with the Democratic Republic of the Congo, for instance (Newbury, 1988). More practically, if I use fixed effects at a lower level of geographical aggregation, I lose statistical power due to the fact that state presence varies at the level of the precolonial district

When estimating the effect of state presence on violence in the genocide, I estimate equation (1) using OLS. Because the number of violent incidents in other periods is measured as count data and exhibits overdispersion, I estimate negative binomial regressions using maximum likelihood when using these data.

**IV estimates.** I test whether proximity to Nyanza is correlated with the expansion of the Nyiginya kingdom by estimating the following first stage relationship:

$$StatePresence_d = \gamma_0 + \gamma_1 Distance_d + \mathbf{X}'_d \boldsymbol{\gamma}_2 + \mathbf{Q}'_s \boldsymbol{\gamma}_3 + r_p + \nu_{sdp} \quad (2)$$

where  $Distance_d$  is either the straight line distance to Nyanza, or one of the cost distance measures introduced in the previous section.  $StatePresence_d$  and  $Distance_d$  both vary at the precolonial district level. I therefore report the first stage both at this level as well as the sector level.

I then use predicted state presence from the first stage in the second stage, which I estimate using two stage least squares when using data on violence during the genocide:

$$Y_{sdp} = \beta_0 + \beta_1 StatePresence_d + \mathbf{X}'_d \boldsymbol{\beta}_2 + \mathbf{Q}'_s \boldsymbol{\beta}_3 + r_p + \varepsilon_{sdp} \quad (3)$$

$Y_{sdp}$  is an outcome of interest, such as violence or mobilization during the genocide.  $\hat{\beta}_1$  is the coefficient of interest. Because state presence varies at a higher level than my outcome data, I report clustered standard errors at the precolonial district level (n=50) in all specifications. I also report Conley (1999) standard errors that account for spatial correlation. For most results, the Conley standard errors are smaller than the clustered standard errors. When using count data on violent events in the years surrounding the genocide, I include the predicted residuals from the linear first stage and estimate a negative binomial (NB2) model using maximum likelihood in a control function approach. This approach necessitates using bootstrapped standard errors (Hilbe, 2011).

This instrumental variables strategy requires several assumptions to be met to be a valid approach for causal inference. First, although geographical proximity to Nyanza may be exogenous, Nyanza may have been chosen because it was uniquely attractive for settlement. This would result in a violation of the exclusion restriction to the extent that the factors that make Nyanza attractive correlate with violence.

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(n=50).



Second, the correlation between proximity to Nyanza and state presence has to be sufficiently strong. To understand the attractiveness of Nyanza, table 1 reports regressions of several characteristics of Rwandan villages on the four instruments introduced above. Because the outcome variable, state presence, varies at the level of the precolonial district (n=50) I report regressions at this level.<sup>16</sup>

The first characteristic I consider is pre-Nyiginya population density. I use data on locations of archeological finds indicating population settlement before the foundation of the Nyiginya kingdom from Prioul and Sirven (1981). I compute population density as the number of archeological finds normalized by the area of the district. If prehistorical population density is correlated with proximity to Nyanza, the exclusion restriction may be violated because population density may persist and correlate with  $\varepsilon_{sdp}$  in equation 1. I furthermore consider elevation and average slope (as a measure of ruggedness or inhospitability (Nunn and Puga, 2012)), as well as data on the suitability for growing banana, Rwanda's main staple crop, from the Food and Agricultural Organization, each proxying suitability of the landscape to growing crops and therefore plausibly capturing initial productivity differences. Each panel reports results for a different instrument. All effect sizes are standardized (i.e. measured in standard deviations). All instruments are uncorrelated with all pre-characteristics, which lends credence to the use of proximity to Nyanza (there is one correlation that is marginally significant). In appendix table A1, I furthermore show that proximity to Nyanza is uncorrelated with soil suitability for sorghum (the second most important staple crop in Rwanda), suitability for coffee (Rwanda's main export crop) and with the effect of the RTLTM hate radio studied by Yanagizawa-Drott (2014). Proximity to Nyanza is uncorrelated with both the distance to the nearest RTLTM transmitter as well as share of the population in a district that can receive the hate radio signal.

Aside from the exclusion restriction being met, the first stage correlation between the instruments and state presence needs to be sufficiently strong. If this correlation is weak, the second stage results become difficult to interpret (Staiger and Stock, 1997). Table 2 reports estimations of the first stage equation (equation 2) using state presence as the dependent variable and the four measures of proximity to Nyanza as independent variables. Even-numbered columns report univariate regressions using only the instruments as independent variables. Odd-numbered columns add the 'baseline' controls proxying for the location of armed groups.

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<sup>16</sup>Re-estimating these regressions using data at the sector level produces very similar results (provided standard errors are clustered at the precolonial district level).

Columns (1) and (2) report results using the distance to Nyanza in kilometers as the variable of interest. In line with the narrative in the introduction to this section, I find a negative correlation: Districts that are further away from Nyanza were annexed to the Nyiginya kingdom later. This effect remains strong and significant when introducing controls in column (2). I find similarly strong results using the cost distance measures to Nyanza, in columns (3)-(4) and (5)-(6). Using cost distance to the nearest army as the variable of interest does not produce robust results when introducing controls. The partial F-statistics of the excluded instrument confirm these patterns, and show that, aside from cost distance to the nearest army, the first stages are sufficiently strong.<sup>17</sup>

For the remainder of the analysis, I use the distance to Nyanza in kilometers as the main instrument. This instrument is precisely measured and parsimonious, since it does not rely on specific assumptions regarding movement speed of soldiers nor on assumptions regarding the impediments posed by traversing rugged terrain. In the appendix I report all IV results using the other instruments as well.<sup>18</sup>

## 2.3 Data

This section introduces the main variables used in the empirical analysis. Summary statistics for all variables used in this paper, as well as detailed data sources, are reported in the appendix.

**Violence and mobilization in the genocide.** Data on violence in the genocide is provided by the National Service of Gacaca Jurisdictions court proceedings. A Gacaca court is a form of traditional local justice, revolving around a village meeting in which individuals accused of genocidal crimes confess or deny crimes and are sentenced or acquitted. Starting in 2001, over 8000 of these courts were instituted in Rwanda to reduce the enormous backlog in the regular court procedures (for a detailed description of the data as well as the Gacaca procedure, see Verpoorten (2011)). The data consist of three categories of crimes:

1. Planners, organizers and supervisors of the genocide. This includes organizers at the local bureaucratic level and within political parties as well as the youth militias.<sup>19</sup>
2. Murder, manslaughter, and non-lethal violence with the intention of killing.
3. Property theft or damage, if not amicably settled.

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<sup>17</sup>In the appendix I report two robustness exercises. In table A5 I report estimates of equation (2) at the precolonial district level, the level at which state presence varies. The estimated effects are very similar. Following a recent contribution by Ashraf and Galor (2013) I then verify that the results in table 2 are not driven by the distance to Addis Abeba in table A6.

<sup>18</sup>I do not report results using the cost distance to the nearest army since the correlation in column (8) of table 2 is insignificant.

<sup>19</sup>This category also includes individuals prosecuted for rape and sexual violence.

In my sample there are 816,325 prosecuted individuals; 76,572 in category 1, 431,265 in category 2 and 308,488 in category 3. In order to measure the intensity of genocidal violence and popular mobilization, I construct three village level variables: The first is the sum of categories 1 and 2 divided by total population from the 1991 census (Ministère du Plan, 1992). This measure captures the overall intensity of the genocide in a village. Figure 4 contains a map of this variable. Second, I use the number of prosecuted individuals as organizers (category 1 individuals), normalized by the Hutu population, to understand the role Hutu organizers played. I finally use the number of prosecuted individuals in category 2, normalized by the Hutu population to measure civilian mobilization.

**Reliance on Gacaca data.** It is important to note the possibility that differences in state presence affect the presence of Gacaca courts or the accuracy of Gacaca data. Below I report robustness checks, including one using a different data source for violence, that show that these concerns do not affect the conclusions in this paper.

**Violence before and after the genocide.** I take pre- and post-genocide violence from the Uppsala Conflict Data Program database (UCDP) (Sundberg et al., 2010; Sundberg and Melander, 2013), which records the location, actors and death tolls of violent events since 1989. Its main sources are reports by international news agencies (such as Reuters and the BBC) as well as reports from NGOs (such as Human Rights Watch). In my sample there are 139 events in 1990-1993, 161 in 1994, and 147 in 1995-2000.<sup>20</sup> The UCDP data have been used extensively in economics, see e.g. Michalopoulos and Papaioannou (2016).

The UCDP database records three types of violence. First, state based armed conflict, which is defined as “contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in one calendar year”. Before the genocide, these conflicts occur mostly between the Rwandan government and the invading RPF army. After the genocide, these conflicts occur between the Rwandan government and Hutu rebels. Before 1994, 82 out of a 139 recorded conflicts are state based. After 1994, 59 out of 147 recorded events are state based. Second, one-sided violence, which is defined as the “use of armed force by the government of a state or by a formally organized group against civilians which

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<sup>20</sup>The UCDP data for 1994 are a gross underestimate. The total death toll for 1994 in this data is 155,217 which is far below the death toll estimated after the genocide as well as below the data introduced above (see Straus (2006) for a discussion). The discrepancy is in all likelihood driven by the fact that the UCDP data are based on crawling media reports. The local media all but disappeared during the genocide and foreign media were unable to report in any detail. In addition, the localization of the victims for those media report that exist is highly imprecise. The UCDP data in some cases assign more victims to a village than there were pre-genocide inhabitants. I therefore use the Gacaca court data for 1994. Most results in the rest of this paper, however, are qualitatively similar when using the UCDP data, although I lose precision on some. These results are available upon request.

results in at least 25 deaths in a year". All remaining events are of this type. The final category is conflicts "between two organized armed groups, neither of which is the government of a state, which results in at least 25 battle-related deaths in a year."

I use the total number of violent events between 1990-1993 and 1995-2000 as the main measure of violence in the years surrounding the genocide. This variable is mapped in figure 5. Figure A3 in the appendix splits this map up pre- and post-genocide. In addition, I use the count of state based violent events and the count of one-sided violent events as variables of interest. These capture, in turn, conflict between the government and organized rebels and conflict between the government and individuals Rwandans. In the appendix, I report results using the count of violent events for different periods separately as well as normalized by pre-genocide population from the 1991 census.

**State presence.** I measure state presence as the number of years the Nyiginya state was present in a district before colonization in 1897. Identifying when a district was annexed to the Nyiginya kingdom requires reconstructing the administrative divisions of the precolonial kingdom and the identification of the years of the annexation for each district. Full details of the procedure, as well as the raw data, are in the appendix.

### 3 Results

This section reports the main results in this paper, the effect of state presence on violence before, during and after the genocide. I first report results from OLS and negative binomial regressions of violence on state presence. After reviewing a large set of robustness checks, and finding that the main result is robust and stable, I introduce the IV estimations. I find a positive, significant and robust effect of state presence on violence in the genocide. Using the UCDP data for 1990-1993 and 1995-2000, I find a negative effect of state presence during these periods. Finally, the IV results are in agreement with the OLS results, providing evidence for a causal interpretation of the effect of state presence on violence.

#### 3.1 Results for violence and mobilization in the genocide

Table 3 reports estimates of equation (1) using measures of violence and mobilization from the Gacaca records as dependent variables. Columns (1) and (2) use the fraction of population prosecuted for violence as the dependent variable, and columns (3) and (4) look at mobilization of the Hutu population as

organizers or as participants. Each regression includes the 'baseline' set of controls, which capture the location of the different groups in the genocide as well as province level fixed effects. The estimates in the first row show a positive and significant effect of state presence on violence and mobilization. The mobilization effect is strongest in prosecutions for murder, rather than organization, which is consistent with the hypothesis of this paper that the effect of the historical state is transmitted through rule following behavior and popular participation in genocide activities rather than a larger number of (government) organizers. Furthermore, since organizers either lived in administrative centers (politicians) or travelled around (militias) I expect a weaker effect of differences in state presence at a local level. Note that Conley (1999) standard errors are smaller than the clustered standard errors in all regressions, suggesting that spatial correlation is not very important for these results. Since the Tutsi were the targets of the genocide, a natural concern when interpreting these effects is their spatial distribution. Column (2) includes the fraction Tutsi in the 1991 census as a covariate. Note that the fraction Tutsi may well be an outcome of state presence. The results in column (2) can therefore not be interpreted as causal. I consider ethnic distribution as a channel below. The estimated effect of state presence is virtually identical to column (1) and remains positive and highly significant. Overall, the results in this table suggest that state presence is positively correlated with violence. These effects appear predominantly driven by larger bottom up participation of the population, rather than by more top down organizers.

**Economic impact.** The estimated coefficients in table 3 are not only statistically significant, they are economically meaningful as well. Consider the point estimate in column (1), 0.0307 (clustered s.e. 0.00953). A one century increase in state presence (or about its mean) is associated with an increase in violence of about 3 percentage points. This increase is equal to about 40 percent of the mean incidence of violence. A different way of benchmarking the effect size is to consider moving through the interquartile range of state presence. The interquartile range of state presence is 132 years. Taking the point estimate from column (1) again, this range is associated with a 4 percent increase in violence, which is about 40 percent of its interquartile range. Finally, subtracting the mean of dependent and independent variables and rescaling by their standard deviations produces standardized estimates (mean zero, unit standard deviation). A one standard deviation increase in state presence results in a one-quarter standard deviation increase in violence.

In the years surrounding the genocide the Rwandan government was fighting rebels, rather than organizing mass killing. In this period, it did not mobilize its population for violence. Rather, violence or supporting rebels was illegal, and the government actively fought the invading RPF and local rebels

(Des Forges, 1999). The hypothesis of this paper predicts that if the government is facing unsanctioned attacks, violence should be lower in areas where the precolonial state was established earlier because the local population is arguably less likely to support the rebels, become rebels, or harbor rebels in their homes. To understand if the effect of state presence without government sanctioned violence, I now report results for violence in 1990-1993 and 1995-2000.

Table 4 reports estimates of negative binomial regressions of violence on state presence. In columns (1) and (2), the dependent variable is a count variable of the number of violent events between 1990 and 2000, excluding 1994. In columns (3) and (4) I break down this result by looking at state based and one-sided violence, capturing violence resulting of government and organized rebels - mostly the RPF - and government and rebelling individuals. As before, column (2) includes the fraction Tutsi as a covariate. In the pre-genocide period, the Tutsi were starting to get systematically excluded by the Hutu government and may have been more likely to rebel. It is therefore important to control for their presence. The estimates in the first row show a negative and significant effect of state presence on total violence and on both state based and one-sided violence. In places with longer state presence, there are fewer violent events in the years surrounding the genocide, both overall and when broken down into different types.

Before moving on the robustness and the instrumental variables estimates, I first discuss three issues pertaining to the interpretation of the effects found in this section. First, compared to the measures of popular participation in the genocide, the UCDP data measure popular participation in violence less directly. Although the definitions of the UCDP violence categories precludes the possibility that all data are simply about violence between organized groups, there is clearly a complicated process that leads citizens to become rebels that I do not measure. In essence, I will have to assume that the data capturing violence between government and rebels reflects to some extent popular participation in violence. In section 4 I provide evidence on household violence which more directly measures individual violence, and which supports this assumption. The results of that exercise are in agreement with the results in this section.

Second, there are qualitative differences between 1990-1993 and 1995-2000. In 1990-1993 and during the genocide, the same group of Hutu was in charge. If obedience to a government goes along ethnic lines, Hutu civilians are more easily mobilized by a Hutu government, the effects for this period should be stronger. Similarly, as discussed above, the genocide displaced about half of Rwanda's pre-genocide population. I would therefore again expect the effect to be weaker after the genocide. In appendix table A16, I break up the pre- and post-genocide period in these two sub periods, and find that the effect for

1990-1993 is indeed stronger than for the post-genocide period.

Finally, it is important to recognize that - in principle - the effect of the establishment of a state may have long-run effects not just through the establishment of the state per se, but also through the endogenous reaction of unconquered Rwandans to the establishment of the state. They may withdraw from commerce, revert to growing hard-to-tax crops, or may even move to inhospitable terrain, all in an effort to become less attractive for conquest (Scott, 2009). They may, alternatively, start to centralize governance and expand military efforts to resist conquest (Tilly, 1992). Rwanda's historiography is clear that there were no states that effectively competed with the Nyiginya state within the borders of what would become modern Rwanda (see e.g. Vansina (2004). While it is likely true that some Rwandans withdrew from society in anticipation of being conquered (Des Forges (2011) describes a resistance movement in the early colonial period operating from a swamp, for example), the consensus among historians is that the Rwandan state casts a long shadow, rather than historical subversion efforts (see the overviews in Straus (2006) and Prunier (1995)). This anecdotal evidence is supported by empirical results using the location of precolonial armies. If - during precolonial rule - newly incorporated areas would have been more unruly they should have required more military presence *after* conquest to enforce new rules and demands. The alternative hypothesis comes from the historical description we have of different armies and says that the armies were used to defend against foreign attacks, mostly from Burundi and Southern Uganda (Kagame, 1963). I can test this idea by using data on the location of precolonial armies from Kagame (1963). When I estimate the main regression equation (equation (1)) using the number of precolonial armies at a local level as the dependent variable, I find a small and insignificant effect of state presence (standardized coefficient: 0.013, s.e. 0.041). Although not conclusive, this result suggests that the effects found in tables 3 and 4 are due to the presence of the state rather than the differential endogenous response to the establishment of the state across Rwandan communities.

In the rest of this section, I assess the robustness of these results by including several candidates for omitted variables in the main regressions. I then directly estimate the causal effect of state presence using distance to Nyanza as an instrumental variable.

### **3.2 Robustness**

I undertake a series of robustness exercises, which I will only briefly discuss here. I assess two broad categories of robustness: Measurement and the impact of the colonial period. Overall, the main results are

highly robust to using alternative data sources, alternative ways of measuring the main outcomes and to including a large number of historical controls. A sensitivity exercise shows that it is furthermore unlikely that the main results are driven by unobservables. The appendix discusses each robustness check in more detail.

**Violence during the genocide.** I first assess the robustness of the results for violence during the genocide. I re-estimate equation (1) at the precolonial district level, the level at which state presence varies. Second, I further address the concern that the spatial distribution of violence during the genocide and in the surrounding years reveals that most violence during the genocide happened in southern Rwanda whereas most violence in surrounding years happened in northern Rwanda. This is potentially problematic since the historical heartland of the state and the highest concentration of Tutsi are both in the South. I vary the measurement of distance to Kigali and the national border by including polynomials in these distances and interactions with province fixed effects, allowing for the effect of distance to the border to be different for each province. Third, normalizing by the number of Gacaca courts or using alternative data sources for genocide violence results in qualitatively similar and statistically significant estimates. The main objective of this exercise is to show that the main result is not driven by the idea that places with longer state presence are better at prosecuting people, resulting in a mechanical correlation between state presence and the number of prosecutions. Across all these exercises, the effect of state presence remains stable and significant.

I then use equivalent household income in 1990 as a summary measure of differences in development before the genocide. When using income as the dependent variable, I find no systematic relationship with state presence. I then include income as a covariate. Because income data is only available for a subset of villages, the estimated effects become noisier but the effect of state presence is stable. Ideally, I would be able to measure income differences before the inception of the Nyiginya state. No such data exist but I can use data from Prioul and Sirven (1981) that record archeological sites in Rwanda where remains have been found that predate the Nyiginya kingdom. In addition to pre-existing differences, the German and Belgian colonization may have had differential impacts across Rwanda. In order to understand the impact of the colonial period, I use data from the Government of Belgium (1960) and the Parliament of Belgium (1935) on the number of missionary stations in 1935 to account for missionary presence and the number of cattle in 1960. In absence of income data, cattle is the best measure of prosperity in the colonial period. I report results using these variables as covariates in appendix table A11. Including these variables reduces the magnitude of the estimated effects from about 0.031 to about 0.0295.



This reduction in estimated effects can be further examined using a ‘selection on observables’ exercise following Altonji et al. (2005) and Bellows and Miguel (2009). Let  $\hat{\delta}$  be the reduction in the estimated effect resulting from including observable covariates between two corresponding regressions in tables 3 and A8, and  $\hat{\beta}$  be the estimated effect in table 3. Then  $\frac{\hat{\beta}}{\hat{\delta}}$  can be interpreted as a measure of how much unobservables would have to reduce the estimated effects to explain them away, expressed in terms of the reduction in the same effect resulting from including observables. This ratio, reported in every column in table A11, is around 25 for violence and mobilization of civilians, and equal to 12.45 for organizers suggesting that, in order to explain away the effect, selection on unobservables would have to be at least twelve times as strong as selection on unobservables.<sup>21</sup>

**Violence before and after the genocide.** In appendix table A14 I show that the results in table 4 are robust to including the historical covariates introduced above: The number of archeological sites, missionary stations and cattle. I also show that instead of using the number of violent events as the dependent variable, I can normalize the count of events by 1991 population. I can also split violent events up by pre- and post-genocide and by whether the Rwandan government in one of the parties involved in the violence (there is also violence of rebels against civilians, in which the government is not involved). The main results do not change.

### 3.3 The impact of state presence on violence

Table 5 reports estimates of equations (2) and (3), using distance to Nyanza as the excluded instrument. Panel I reports two stage least squares estimates and panel II reports the corresponding first stages. As before, all regressions include the baseline set of controls and fixed effects. As before, columns (1) and (2) measure total violence and columns (3) and (4) use mobilization outcomes. Panel II confirms the results in table 2: Distance to Nyanza is an informative instrument, and is strong enough (partial F-statistics are between 14 and 16) to be confident about the second stage estimates. Column (1) presents the main result: A positive and significant relationship between state presence and violence. As before, this effect is stronger for Hutu civilian mobilization.

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<sup>21</sup>Note that since the R-squared between table 3 and the corresponding regressions in table A11 does not increase by much from including these extra covariates, there is still a substantial amount of variation ‘left’ to be explained by unobservables (Oster, 2016). This limited increase is in part due to the fact that the regressions in table 3 already include several covariates as well as fixed effects. A univariate regression of violence on state presence yields a R-squared of about seven percent, and including fixed effects and the covariates in table A11 raises the R-squared to twenty one percent. The instrumental variables approach in the next section provides further insight in the role of unobservables.

**Economic impact.** The estimated effect in column (1), 0.0807 (clustered s.e. 0.0270), implies that increasing state presence by its mean increases violence by about 8 percent, or its mean. Similarly, increasing state presence by its interquartile range increases violence by about its interquartile range. The estimated effects of mobilization are similar. Note that these effects are larger than the corresponding OLS estimates in table 3. There are a number of reasons for why OLS estimates are typically smaller than IV estimates. State presence could be measured with error, for instance, due to the fact that I reconstructed it from maps and archival sources. The IV estimates may pick up the Local Average Treatment Effect only for those sectors affected by the instrument, the OLS estimates may be biased downwards due omitted variables or the exclusion restriction could be severely violated. The results in table 1 and accompanying appendix results provide evidence that suggests that there is no direct link between proximity to Nyanza and violence today, and it is therefore unlikely that severe violations of the exclusion restriction drive the differences in estimated effects. It seems more likely that the OLS estimates are biased downwards due to the measurement error and the presence of omitted variables. For example, if more fertile areas are likely to be incorporated earlier and individuals living in fertile areas are richer and therefore more likely to be targeted in the genocide, the effect of the state will be attenuated. Similarly, areas that are more remote may be more difficult to conquer, and it may be easier for the local population to hide there during the genocide (such as the *Musanze* caves where Tutsi hid). More generally, any unobservable that correlates with both the timing of the expansion of the state and violence attenuates the effect of the OLS estimates.

**Violence before and after the genocide.** In order to understand whether the results for violence surrounding the genocide admit a causal interpretation, I present instrumental variable estimates in table 6. The columns present negative binomial regressions in panel I and linear, first stage, regressions in panel II. The estimated effects in the first row show a negative effect of state presence on both state based and one-sided violence and therefore provide a causal interpretation of the results in table 4.<sup>22</sup>

**Economic impact.** Because I estimate the relationship between state presence and violence in this section using negative binomial regressions, I report marginal effects (at the mean). Consider the point estimate in column (1) of table 6, -0.00371 (clustered s.e. 0.00116). This estimate implies that a one century increase in state presence, at its mean, is associated with a 37.1% decrease in violence. Taking the point estimates in columns (3) and (4), a one century increase in state presence results in a 14.2% reduction in state based violence and a 18.6% reduction in one-sided violence.

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<sup>22</sup>The control function approach admits an informal test of the endogeneity of state presence. The estimated effect of the predicted residuals from the first stage is significantly different from zero which suggests that state presence is endogenous.

**Additional robustness.** In the appendix, I use the alternative cost distance instruments introduced above. The estimated effects of state presence are virtually identical for both the genocide, as well as the years surrounding the genocide. I finally verify - for the years surrounding the genocide, that results estimated using two stage least squares rather than the IV-control function approach give similar results.

The results in this section show that state presence has a large causal effect on contemporary violence. This effect, however, is not constant. During the genocide I find a strong positive effect and in the years immediately surrounding the genocide, this effect turns negative.<sup>23</sup> These results are in line with the hypothesis that the historical state affects rule following. Before moving on to testing the causal mechanism behind these results, I exploit features of the organization of the genocide to test for heterogeneous effects of state presence.

### 3.4 Heterogeneous effects of state presence

In this section I take a different approach to testing the hypothesis of this paper by exploiting the fact that changes in policy need to be communicated and legitimized (Glaeser, 2005). The hypothesis tested in this paper suggests that in places where the government was better able to communicate its policies I expect the effect of the state to be stronger. Using two different sources of heterogeneous effects I find support for this idea.

The Rwandan government, as many governments do, used the media to communicate its new policies. Before the genocide the Rwandan government started on a campaign that aimed at formally sanctioning violence against Tutsi. The publication of the 'Hutu Ten Commandments' in the December 1990 edition of *Kangura*, a government sponsored magazine, is seen as the start of this campaign (Des Forges, 1999). The commandments forbade social interactions between Hutu and Tutsi and encouraged Hutu to 'stop having mercy' on the Tutsi. More important than magazines was the use of the radio (Yanagizawa-Drott, 2014).

Yanagizawa-Drott (2014) has shown that the government radio station, the RTLM ('radio television

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<sup>23</sup>A natural next exercise is to exploit the time variation in violence data and estimate a continuous treatment differences in differences model, using state presence as the continuous treatment and using a dummy for 1994 as the policy shift. Although this appears attractive, creating the sample for this exercise involves combining the Gacaca data with the UCDP data. An alternative strategy is to use the UCDP for 1994 as well, which has problems of its own (see above). Table A20 nevertheless implements this idea. The interaction between a 1994 dummy and state presence is positive and significant and the magnitude is comparable to the result in table 3.

libres des milles collines'), was instrumental in mobilizing the population. He finds that places that happened to have radio coverage due to varying geography see more violence. Although the radio was not used to mobilize the population before and after the genocide, the government did use the radio to instruct Rwandans to dissociate themselves from the rebels before the genocide. I therefore also use violence before the genocide as a dependent variable. I use data on radio ownership from the 1991 census, and split radio ownership at its median (the median of radio ownership is equal to 33% of the 1991 population). I expect the effect of the state to be stronger in places where radio ownership is higher. If the effect of the precolonial state primarily works through a complacent population, being able to reach the population better should result in more violence, all else equal.<sup>24</sup>

At a local level, the *burgomasters*, or mayors, of the 154 communes in Rwanda were instrumental in mobilizing the population. Not all mayors, however, were members of the ruling MRND party. Following the introduction of multi-party government in Rwanda in 1992, a number of new political parties attracted a significant following among local mayors.<sup>25</sup> When the genocide started, many mayors recognized that the orders that were given were coming from the MRND party, and some opposed the genocide (although we do not know the exact number of mayors who opposed, see Straus (2006, p. 79) for a concrete example and Des Forges (1999, ch. 4) for detailed description of the interaction between local and central government, and the role of mayors in Gikongoro and Butare provinces). These case studies suggest that the effect of state presence may be less strong in communes with opposition mayors. I test this hypothesis by investigating heterogeneous effects by party affiliation of the local mayor using data from Guichaoua (2010), coding an indicator variable equal to one if the local mayor was member of an opposition party and estimating heterogeneous effects by splitting the sample by this indicator variable. I expect the effect of the historical state to be stronger in places where the mayor was ideologically aligned with the central government.

Table 7 explores these hypotheses using the count of violent events in 1990-1993 and 1995-2000 as the dependent variable in columns (1) and (2). In columns (3) and (4) I use the fraction of Hutu prosecuted for

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<sup>24</sup>The data of Yanagizawa-Drott (2014) only provide coverage for the RTLM radio station which operated from July 8th, 1993 to July 31st, 1994. Using radio ownership allows me to study violence in the entire sample period.

<sup>25</sup>In 1993, 96 out of 145 communes had a mayor that was affiliated with the ruling MRND party. 24 mayors were affiliated with the Hutu MDR party, and 16 were affiliated with either the PL or the PSD, parties that were sympathetic to Tutsi interests. 6 positions were vacant, one mayor was unaffiliated, and for two communes the affiliation of the mayor is not known (Guichaoua, 2010). The MRND, the *Mouvement Revolutionnaire National pour le Developpement*, was the party of the president, Habyarimana, and was a Hutu party. The radical arm of this party, the *Akazu*, has been identified as the organizing force behind the genocide. The MDR, the *Mouvement Democratique Republicain*, was the main Hutu opposition party and consisted mainly of supporters of the previous president, Gregoire Kayibanda. Geographically, the MRND had its support among Hutu in North-West Rwanda, Habyarimana's home region, and the MDR among Hutu living in central Rwanda. The PSD, *Parti Social Democrate*, and the PL, *Parti Liberal* were opposition parties based that were sympathetic to the Tutsi (PSD) or had many Tutsi as members (PL).

murder as a measure of civilian mobilization during the genocide. In line with the hypothesis tested in this paper, I find that that effect of state presence on mobilization during the genocide is positive irrespective of radio ownership, but that it is stronger and more precisely estimate in those places with higher radio ownership (Chow tests show that the difference between coefficients in these subsamples is statistically significant). This finding is consistent with the idea that there is more violence in places where people are more likely to hear - and obey - the orders for mobilization. Columns (5) and (6) use mobilization as their dependent variables as well, but split the sample by political affiliation of the mayor. As hypothesized the effect of state presence is concentrated in areas with mayors that are aligned with the government. Note that due to the small sample in column (5) the sub sample effects are not statistically distinguishable. It is of course very well possible that radio ownership and political affiliation by themselves are correlated with other potential channels of transmission of the effect of the historical state. The rest of this paper is devoted to distinguishing causal mechanisms.

## 4 Mechanisms

The previous sections have quantified the causal effect of state presence on genocide intensity and mobilization of the Hutu population, as well as on violence in the years surrounding the genocide. In this section, I explore the mechanisms through which a longer history of centralized government may affect violence. I focus on testing the link between the development of the state and two broad classes of mechanisms: Norms and beliefs that dictate how to respond to state demands on the one hand and the strength and presence of government institutions on the other hand. To test for the effect of norms, I pursue several strategies. First, I report results from a lab-in-the-field experiment that measures rule following behavior. I find that individuals that today in places where the state developed earlier are more likely to follow an unenforced rule. Second, I use detailed information on the place where participants in the experiment grew up, rather than where they live today, to establish that these effects are likely transmitted culturally. Third, I report results from opinion surveys that show that Rwandans living in places where the state developed earlier hold stronger views about the desirability of rule following and obedience to government, are less likely to challenge the government in community meetings, and are more likely to follow the laws governing household violence.<sup>26</sup> I then show that the infrastructural aspect of the Rwandan government - its offices, public places etc. - do not correlate with the presence of government institutions at a local level. The rest of this section contains two more analyses. First, I test for several further poten-

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<sup>26</sup>It would seem natural to test furthermore for tax compliance and voter turnout. Unfortunately, tax compliance data is not available at a village level and Rwanda consistently has voter turnout numbers close to one hundred percent around the country.

tial mechanisms suggested in the literature. Finally, I re-estimate the main result of this paper, the effect of the state on violence, within the experimental sample and find a positive relationship. In sum, this section complements the results for violence in the previous section and provides direct support for the hypothesis tested in this paper.

#### 4.1 Rule following as a mechanism

This section provides motivating case study evidence for obedience to political authority, which I'll operationalize as 'rule following' in the empirical part of this section, as a mechanism driving the effect of the precolonial state.

The idea that the extraordinary high degree of obedience of ordinary Rwandans to the state was central to successful mobilization is widespread in case studies of the Rwandan genocide, (Prunier, 1995; Hintjens, 1999; Des Forges, 1999; Newbury and Newbury, 2000; Straus, 2006). Despite the prevalence of these ideas, there is little systematic evidence on their empirical validity. One exception is a survey of 209 incarcerated genocide perpetrators, in which Scott Straus finds that 45% of perpetrators was motivated by 'Intra-Hutu coercion and/or obedience'. He describes (2006, p. 137):

“Here respondents said that they joined attacks because doing so was ‘the law’ (*igeteko*). Others said that they went with murderous groups or killed because they were ‘obeying’ what they had been told to do. Still other said that they participated because they had been ‘authorized’ to kill Tutsis. In these accounts, respondents stressed that ‘the state’ or ‘the authorities’ had mandated participation for all able-bodied Hutu men. Killing was ‘the law’.”<sup>27</sup>

Based on Straus' estimates, the lower bound of popular Hutu participation in the genocidal killings was between 14 and 17% of adult Hutu males (Straus, 2006). This level of participation is extraordinarily high, and is comparable to the highest conscription rates in modern European wars (Onorato et al., 2014).

What would 'being obedient' look like in the genocide? The key to answering this question lies in the details of the organization of the killings. The central government had prepared the genocide by coopting local government employees, as well as ordinary citizens who could lead the organizational efforts. These 'intimidators', as the civilian perpetrators called them, were in charge of morale and order. Usually, intimidators would be shopkeepers, teachers and other local notables. When the intimidators could not motivate the civilians or when they encountered resistance, the authorities would call on the national

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<sup>27</sup>Note that in *Kinyarwanda* the word for law and for an order given between individuals are the same (*igeteko*).

police or the youth militias, to break the resistance or lead the killings.<sup>28</sup>

During the genocide, the local bureaucrats together with the intimidators would call all men throughout Rwanda to a central location, usually between 6AM and 9AM. At these morning meetings, targets for that day would be announced, usually local Tutsi but also disobedient Hutu. After a day of 'work', as the killings became known, the authorities would usually blow a whistle to announce the end of the day's work. Shirkers were reminded of their duty, and charged a fine for missing a day of work. This process was repeated every day, but stayed local. Local execution ensured that intimidators and administrators knew the perpetrators and the perpetrators knew who was Tutsi and where the loot was. Only the national police and the youth militias travelled. It is easy to see how in this context obedience will ensure higher participation rates and may ultimately higher deaths tolls.

Scholars have mostly investigated obedience in the context of the genocide. It is however plausible that obedience is also important outside periods of extreme violence, especially for the government of what was at the time one of the world's poorest countries. One telling quote comes from a refugee in the Democratic Republic of the Congo. Although free the coercive power of the government that organized the genocide, he/she says: "...we do not know what to think because our leaders are not around just now. We are waiting for a new burgomaster to give us our orders." (Prunier, 2008, p. 25). It is of course hard to disentangle the effect of obedience from expected punishment for non-compliance, social pressure and the prospect of material gain (this is the conclusion Straus reaches, see p. 140).<sup>29</sup> Furthermore, arguments about obedience are always made comparing Rwanda to other countries. There is no empirical evidence that provides guidance on whether there are systematic differences in rule following behavior within Rwanda. To further isolate rule following that is driven by the historical state, I report results from a fieldwork exercise which, as part of an experiment, holds environmental effects constant. I find a greater propensity to follow rules in villages with a longer state presence. After presenting these results, I present supporting evidence from several household surveys.

## 4.2 Mechanisms: Experimental evidence

Rwandans decided to participate in the genocide when the expected benefit of doing so outweighed the expected cost. These costs and benefits are not only material, but also psychological (Humphreys and

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<sup>28</sup>This paragraph is based on the detailed reconstructions of the genocide in several districts of Rwanda by Des Forges (1999), and on interviews with genocide perpetrators (Straus, 2006; Hatzfeld, 2005).

<sup>29</sup>See also the debate on compliance of ordinary Germans with Nazi demands (especially Browning (1993) and Goldhagen (1996)), and Stanley Milgram's experiments (Milgram, 1963).

Weinstein, 2008). As the case study evidence cited in the previous section suggests, some Rwandans participated because they simply thought that this was the 'right' thing to do when the government demands it (for further case study evidence that reaches the same conclusion, see Hatzfeld (2005)). It is nevertheless a challenge to isolate the internal norm that prescribes that following orders to kill is the right thing to do from simple coercion or a conscious calculation of expected social punishment or material gain. In this section I present results from a lab-in-the-field experiment which I implemented in fall 2014 in Southern Rwanda. The experiment is designed to isolate unenforced rule following from environmental factors, such as the influence of the local government, and I relate individuals to the historical exposure to the state of their ancestors through the villages they live in.

#### **4.2.1 An experimental approach to measuring rule following**

The experiment, which is based on the resource allocation game pioneered by Hruschka et al. (2014), asks participants to repeatedly follow a rule. To separate the intrinsic motivation to comply with the rule from the extrinsic (perceived) influence of the government, the experimenter, family or anything else that may affect compliance, I implement the experiment guaranteeing full anonymity for the participants and providing plausible deniability for any action taking in the game. In other words, no actions can ever be traced back to an individual participant and any action I observe could be consistent with full compliance with the rule.

To be more specific, the experiment proceeds as follows: I provide an endowment equal to 6000 Rwandan Francs (about equal to wage compensation for one week of work) in an experimental lab in which the participant will be alone for the duration of the experiment. The experimental lab, which I describe in more detail below - is prepared with thirty stacks of two coins of 100 Rwandan Francs (RWF). Participants are also provided with two unmarked envelopes. They are asked to associate - in their head - one envelope with the government and one with themselves. The other player in the game is therefore the Rwandan government. Finally, the participants are provided with an extra coin, which acts as a randomization device. They are asked to flip this coin, which determines the split of each pile of two coins between the participant and the government. The rule that participants need to follow is: If the coin comes up heads, the participant deposits one coin, or 100RWF, into the envelope associated with the government. The other coin is deposited in the envelope associated with the participant. If the coin comes up tails, both coins are to be deposited in the envelope for the participant. When the participant has completed thirty flips for each pile, the game ends. The participant then keeps the money that he/she allocated to him/herself and the envelope for the government is handed back to an enumerator, who has



left the lab for the duration of the game.<sup>30</sup>

I adapted the game to the local Rwandan context in one main way. In the instructions to the participants, deposits in the envelope for the government are framed as taxes, so as to increase the salience of the government as the counterparty in the game. The reason that for Rwandans paying taxes over and above regular income and VAT taxes is inoffensive is that Rwanda operates a sovereign wealth fund, the *Agaciro* fund, which regularly receives donations from citizens. Over half of the participants in my sample report to have contributed to this fund themselves, and I use this fund to transfer the allocated money to the government.<sup>31</sup> This adaptation is the main difference between the use of this game in both Hruschka et al. (2014) and Lowes et al. (2017).<sup>32</sup>

In addition to the anonymous environment, this setup also guarantees plausible deniability for any rule breaking on the part of the participant. For example, the participant could have seen a series of coin flips that allocated all money to him or herself.

I measure rule following by comparing average contribution to the government to what should have been given to the government had everyone followed the fair coin. This ‘full compliance benchmark’ is equal to 25% of the endowment since the coin dictates paying 50RWF with probability one half in every round. I express individual allocations to the government as a percentage of the benchmark and compare the mean difference in this measure between two subgroups of respondents, whose ancestors were differentially exposed to the historical state.

#### 4.2.2 Historical background, sample selection and fieldwork

Figure 6 highlights the boundary between two precolonial districts, Mayaga and Bugusera (see figure A1 in the appendix for a map with precolonial district names). Mayaga became part of the Nyiginya kingdom around 1700 and Bugusera in 1799. I sampled individuals close to the boundary between these district

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<sup>30</sup>Even though participants hand back money to the enumerator, it is impossible for me to link these funds to an individual participant because I used number to identify participants. At the start of the fieldwork each participant was given a sheet of paper with a participant ID on it. They were asked to write their name on this sheet and participate in the fieldwork only using their number. This sheet is the only key between name and participant ID. I can link survey responses and experimental behavior using this ID, but its use provides participants with an additional guarantee that I can never trace back behavior to individual participants. Participants signed informed consent with their name, but not their ID.

<sup>31</sup>In addition to explaining that the tax would go to the government, the exact wording of the instructions is: “I will provide RWF 200 to you, as well as a coin. You will flip the coin. If you get heads, you have to pay RWF 100 in tax, which leaves RWF 100 for you to take home. If you get tails, you keep the RWF 200. Let me repeat that, if you get heads, you have to pay tax. If you get tails, you do not have to pay tax.” Virtually everyone in Rwanda pays either income tax, VAT (18%) or one of a variety of other taxes.

<sup>32</sup>Lowes et al. (2017) study a sample of Congolese citizens. Some have a background - either growing up, or ancestry - in the Kuba kingdom and other are from areas surrounding the kingdom. All now live in Kananga, a nearby city. Those from the Kuba kingdom are less likely to exhibit pro-social behavior.

(the 'study boundary') and I compare average rule following behavior in the subsamples created by this boundary.

It is important to understand the historical origins of the establishment of this border. If the border was consciously created because individuals were different on one side before the expansion of the state, this may confound any findings. To answer this question I need to understand why expansion stopped where it did, and why the boundary was crossed. Expansion likely stopped at the boundary in 1700 because the boundary coincides with a small river, the Akanyaru, which is a small tributary river to Rwanda's main river, the Nyabarongo. I chose this boundary on purpose because the advantage of having a natural boundary is that I can pinpoint its location today very precisely and sample participants close to the boundary, whereas borders crossing land are measured with error (in the appendix I detail the reconstruction of these boundaries). The disadvantage is of course that a natural boundary may be an impediment to the movements of goods and people that would make the side of the study boundary that was conquered later a valid counterfactual for the side that was conquered earlier. The natural way to assess whether the sides of the study boundary were plausibly similar before the establishment of the study boundary as an outer border of the state is to study balance on observable characteristics of the villages border the boundary. I do this below and find balance on plausible proxies for productivity and settlement differences providing evidence that the sides of the study boundary looked similar before the establishment of the state. Why was the study boundary crossed in 1799? Another advantage of choosing this particular boundary is that there is detailed historical evidence on its crossing. The boundary was crossed by farmers in 1799 after the polity occupying Bugusera was attacked by Burundi in the south. After Bugusera had been settled by the farmers, the King formally annexed it. Jan Vansina, the leading historian of precolonial Rwanda - notes about this annexation: "The .. conquest .. illustrates the role of serendipity and private initiative in the process" (Vansina, 2004, p. 155). This history, which is more fully detailed in the appendix, suggests that villages on either side of the boundary are similarly attractive for incorporation.

The study boundary is bordered by four administrative districts (mapped in figure 6). Within these districts, I randomly sampled villages to participate in a fieldwork exercise and within these villages, I randomly sampled participants. Each village was visited twice, once to identify participants, and once for to carry out the fieldwork. In total, there are 21 villages and 420 participants in the sample. An important identifying assumption for the empirical part of this section is that these participants are randomly selected. I provide balance tests below to shed light on the extent to which this assumption is met.

The game is implemented in an experimental 'lab' for the day, usually a small room in a local hotel or bar, which are common in Rwandan villages. This location had to meet two criteria: 1) it can not be owned by the government to assuage suspicions that the government was monitoring the experiment and 2) it should guarantee anonymity of choices the participant makes as part of the experiment.<sup>33</sup> The appendix contains a photo of one of these labs.

The fieldwork proceeded itself in several steps. The participants were first asked to participate in a survey, which was rewarded with a sitting fee of RWF1500, in three bills of RWF500. RWF1500 is roughly equal to wages for two days of unskilled labor. This sitting fee could then be shared in a dictator game. Second, the participants participated in the main experiment, described above. Upon completion of the fieldwork, there was a second survey as well as a survey of village characteristics. The village survey mainly recorded the presence of government institutions.

The dictator game proceeds as follows. Each participant, the sender, is told that they can share part of their sitting fee with an anonymous receiver. When the sender has decided how much to send, the game ends. The receivers were randomly selected and every participant was asked to share with only one receiver. The receiver could be an anonymous member of his/her village, an anonymous citizen of the capital, an anonymous representative of the local government or an anonymous representative of the central government.

#### **4.2.3 Balance**

In appendix table A21 I report results for two different balance tests. I study balance on village level characteristics to understand whether the side of the study boundary looked similar before the establishment of the boundary. Second, I study individual level characteristics across treatment status (that is, for individuals sampled on either side of the study boundary) to understand if random selection of participants was successful. Panel I has results for individual characteristics. Columns (1) and (2) report the effect of longer state presence, now measured as an indicator variable equal to one for those individuals living on the side of the Akanyaru river with longer state presence, on migrant status (measured by a dummy equal to one if the respondent has migrated during his or her lifetime), accounting for potential differential migration rates across the study boundary. Columns (3) and (4) report results for two measures from a dictator game, accounting for potential differences in altruism towards employees of the local and central

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<sup>33</sup>All results are robust to dropping those participants that indicated in post-experiment interviews that they felt observed. About one percent of participants indicated to have felt observed.

government.

Columns (5) and (6) assess effects on trust and social capital, accounting for the confounding effect greater generalized trust and denser social networks may have. Columns (7) and (8) use survey questions on the expectation of gains from taxation. Specifically, I asked individuals whether they thought taxation benefited themselves or the country and coded two indicators equal to one if participants agreed with this statement. These questions aim to understand whether participants that have higher expectations of future government transfers or investment are concentrated on either side of the study boundary. Naturally, higher expectations of future transfers may lead a participant to allocate more money to the government today. In panel II, I report results using two dependent variables that measures underlying differences in geography between places across the study boundary; elevation and terrain suitability for growing banana, Rwanda's main food crop. Pairs of columns restrict the sample to villages closer to the study boundary and columns (7) and (8) report effects within the experimental sample.

Table A21 reports results for individual level characteristics in the experimental sample, as well as results for village level characteristics. In panel I, all individual observables balance. In panel II, elevation and banana suitability start balancing as I restrict to villages closer to the study boundary. These results suggest that villages and participants look similar in terms of observables before the start of the fieldwork.

A final concern is that the boundary is administratively relevant today, so that government policy may be different today. Up to the administrative reorganization of 2002, the study boundary also coincided with the border between three administrative districts in Bugusera and four communes in Mayaga. After the 2002 redrawing of boundaries, Bugusera became one district and Mayaga is subdivided into three districts. This means that the effect that I observe may be driven by post-2002 district level confounders. This is however unlikely since district are very similar across Rwanda in terms of duties, and employ the same number of employees everywhere. Table A22 shows a large number of balance tests that show that the becoming part of the state earlier is uncorrelated with the presence or accountability of the modern government. The experimental main results is furthermore robust to including fixed effects for the pre-2002 communes (results available upon request).

#### **4.2.4 Experimental results on rule following**

This section uses OLS to estimate the relationship between state presence and rule following. I estimate the following relationship:

$$y_{iv} = \alpha + \beta_D \cdot D_{iv} + \mathbf{x}'_{iv} \cdot \boldsymbol{\beta}_X + f(\text{location}) + \varepsilon_{iv} \quad (4)$$

$y_{iv}$  is the amount allocated to the government in the experiment for individual  $i$  in village  $v$ , divided by the full compliance benchmark amount of RWF1500.  $D_v$  is an indicator variable equal to one if village  $v$  is in Mayaga, rather than in Bugusera such that  $\hat{\beta}_D$  is the coefficient of interest, the measured effect of being on the side of the river with a deeper history on rule following.  $f(\text{location})$  is a linear function of distance to the study boundary interacted with  $D_v$ . Since experimental villages are on average further away and more dispersed on the early state side, I allow for a differential effects of proximity to the Akanyaru on either side of the boundary.  $\mathbf{X}_{iv}$  includes age, age squared and a gender dummy, as well as equivalent daily income in Rwandan Francs (RWF) and education in years. Following Young (2016) I report p-values from a permutation test to assess statistical significance.

Table 8 reports results. Column (1) reports the effect of state presence using just interacted distance to the study boundary as a covariate. Column (2) includes demographic covariates, and column (3) adds equivalent income and education as covariates to account for the effect these variables may have on rule following. For example, if education correlates with cognitive ability it may proxy for differential understanding of either the experiment or the implications of (dis)obedience. In all specifications, I find a positive and significant effect of state presence on rule following. To take the estimated effect in column (1), a hundred years longer state presence (which is about the mean of state presence), captured by the state presence dummy, leads to an increase in compliance with a taxation demand in the experimental fieldwork of 8.916% (permutation p-value 0.040). This effect is equal to about 13% of the control group mean of compliance.<sup>34</sup>

**Cultural norms as the main transmission mechanism.** I naturally face the question what transmits the effect of the precolonial state, which ceased to exist over a century ago, to the present. The literature has identified institutions – i.e. government enforced rules – and culture – norms that are internal to the individual – as two important sources of persistence (see e.g. Acemoglu et al. (2001) and Nunn and Wantchekon (2011)). In the context of the experimental results, I can use detailed information on the history of the migration of participants to disentangle the effect of external – institutional – factors from cultural norms. I do so by constructing a second treatment dummy which is based on the presence of the

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<sup>34</sup>Because participants observe the coin flip privately and follow the rule based on this coin flip, it may be the case that individuals are equally compliant with a rule but individuals in places with a longer state presence are less likely to lie about the result of the coin flip. Since participants are not required to disclose the result of the coin flip to anyone but themselves, the only motivation to falsely report coin flip is the anticipation of wanting to break the rule.

state in the village where a respondent grew up, rather than where he or she lives today. For those individuals who were born near to where they live today, the two measures will be equal. However, for those who were born in a part of Rwanda where the state expanded earlier (later), and subsequently moved to the side of the boundary where the state established later (earlier), the two measures will differ. Since internalized values and norms move with a participant while institutions are tied to the place where he/she lives, comparing a location treatment (the treatment used before) to a birthplace treatment (the new treatment based on place of birth) is informative about the transmission mechanism of the long-run effect of the state. In particular, if the effect of the state works primarily through internalized norms of rule following, the effect of the birthplace treatment should overwhelm the effect of the location-based treatment.

Following this logic, 10.5% of my sample is assigned a new treatment status in the birthplace treatment.<sup>35</sup> Since the two treatment measures are the same for the rest of the sample, this subsample – those who moved across the treatment boundary – is the source of identification. Since this is a smaller group of people than the full sample, estimates are likely to become more noisy.<sup>36</sup>

Column (4) of table 8 includes both treatment measures in equation (4). As expected, both location treatment and the birthplace treatment positively affect compliance, suggesting that the effect of the historical state works both through the location of a respondent – through for instance enforcement of compliance by peers – as well as through cultural norms. The birthplace treatment, however, is larger and more precisely estimated. This result suggests that cultural norms are the main transmission mechanism of the effect of the state.<sup>37</sup>

**Robustness.** The balance tests reported in this section show that there are no systematic differences in trust, social capital, altruism towards the government or beliefs about the usefulness of paying taxes. In the appendix I perform a series of robustness checks. Most importantly I have gathered extensive data on the presence, corruption and accountability of the local government in the study villages. Following Blouin (2014) I also account for the long-run effects that forced coffee cultivation in the colonial period may have had on norms today. Villages in the sample are balanced with respect to observable measures of modern local government as well as colonial coffee cultivation.

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<sup>35</sup>Overall migration in my sample is higher. Almost half of all participants have moved, but frequently they moved very locally, often to marry someone in a nearby village.

<sup>36</sup>Due to the smaller sample, it is difficult to assess the reasons for moving. One likely explanation however is marriage. Those who moved are significantly more likely to be married, and focus groups reveal frequent relocation for family reasons.

<sup>37</sup>The R-squareds of the regressions in table 8 are low, as is often the case in experimental work. A Shapley decomposition of these R-squareds shows that the effect of the state presence accounts for 70% of the explained variation in column (1), 26% in column (2) and 23% in column (3). In column (4), location based state presence accounts for 24% and birthplace based state presence for 67%.

**The effect of the state on genocidal violence in the experimental sample** In the appendix I also test for the effect of the precolonial state on genocidal violence within the sample of sectors (the unit of observation at which the genocidal violence varies) that contain the experimental villages. I find a positive and significant effect of the state presence, in line with the results above.

The results in this section support the main hypothesis of this paper: Longer exposure state institutions affects rule following behavior. The attractiveness of measuring rule following experimentally is that the lab setting keeps the environment constant and random selection, the results in this section suggest, induced balance on individual observables. Nevertheless, results from a lab setting may not generalize. The next section therefore turns to evidence from household surveys as a next test of the hypothesis of this paper. In this section, too, I distinguish between norms of compliance, using data on beliefs for instance, and government institutions as sources of persistence.

### 4.3 Mechanisms: Survey evidence

In this section, I present further evidence for the hypothesis that longer exposure to centralized government is systematically linked to rule following today. Whereas the previous section directly measures rule following, in this section I focus on beliefs about rule following and obedience from the World Values Survey waves for 2007 and 2012. I also reports on civic engagement from the Rwanda Threshold survey and evidence on domestic violence from the Demographic and Health Survey for 2014. I find effects consistent with the hypothesis advanced in the previous section: individuals who live in parts of Rwanda with longer state presence are less likely to find rule breaking acceptable, are less likely to speak up in community meetings and are less likely to engage in domestic violence. These results should, however, be interpreted with caution. Since these surveys do not sample every sector in Rwanda, the first stage is underpowered and I therefore report OLS estimates.

I start with the World Values Survey (WVS). In the 2007 and 2012 survey waves, participants were asked a series of questions about rule following. Specifically, questions V198-V202 asked participants to indicate on a scale from 1 to 10 how justifiable it is to: 'avoid a fare on public transport', 'steal property', 'cheat on taxes if you have a chance' and how justifiable it is for 'someone to accept a bribe in the course of their duties'.<sup>38</sup> A score of 1 is 'never justifiable' and 10 'always justifiable'. Results for these questions

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<sup>38</sup>I do not consider a question that asks about cheating on social security, since most Rwandans are not familiar with such services.

are reported in Table 9A. Since the World Values Survey has limited geographical coverage, resulting in 22 clusters, I report p-values from a permutation test in addition to heteroskedasticity robust standard errors. Note that, besides a question on the justifiability of cheating on social security, which is unavailable to most Rwandans, I include all questions in the 'justifiability' category of the WVS in an effort to avoid multiple hypothesis testing issues.

I also consider question v138 from the 2012 wave which asks about essential aspects of democracy. It asks: "Many things may be desirable, but not all of them are essential characteristics of democracy. Please tell me for each of the following things how essential you think it is as a characteristic of democracy. Use this scale where 1 means "not at all an essential characteristic of democracy" and 10 means it definitely is "an essential characteristic of democracy". I consider the statement: "People obey their rulers". I then consider several variables from the Rwanda Threshold survey which asks question about participation in civil society. I focus on community meetings in which the government announces new plans and citizens have an opportunity to speak up to challenge the government. The survey asks whether individuals recently went to a 'budget meeting' in which the budget of the local government was discussed or to any other government meeting. I code an indicator equal to one if the respondent says that he/she went to either meeting. The survey then asks whether other people spoke up in the meeting and whether the participant spoke up. From the first question I code an indicator if the government was the only party speaking at the meeting, and from the second question I code an indicator equal to one if the respondent speaks up. The results from these questions are in table 9B.

I then consider several questions from the 2014 wave of the Demographic and Health Survey (DHS), asking about violence against women. Before the genocide, women could not legally enter into business relations. Since the genocide a series of laws gave women the right to own property, start business and inherit property (Burnet, 2008). The 2003 Rwandan constitution formally codifies the equality of men and women and affirms its commitment to this principle by 'ensuring that women are granted at least thirty per cent of posts in decision making organs' (Republic of Rwanda, 2003, p. 3). Because men and women are equal before the law, violence against women is a crime. I take absence of violence against women as auxiliary evidence of rule following behavior. The DHS asks women whether they have experienced verbal violence, physical violence or sexual violence from their husband. I code indicators for each. I also assess how justified men find it to beat their wives using question V208 in the 2012 wave of the World Values Survey. I report results in table 9C.



When estimating the effect of state presence on these outcomes, I match the respondents based on their place of residence to precolonial districts, and control for age, age squared, a gender dummy as well as survey year fixed effects. When reporting WVS results, I report heteroskedasticity robust standard errors and p-values from a permutation test because the WVS has limited geographical coverage which results in a small number of clusters. The other surveys have wider geographical coverage, and I report clustered standard errors for these. To ensure comparability of results across regressions I report standardized coefficients. I expect that respondents who live in places where the state established earlier are more likely to report that they find rule following important, that they are less likely to challenge the authorities and that they are less violent at home.

Table 9A-C reports results. Consider table 9A. In places with longer state presence, respondents are less likely to consider rule breaking justifiable across all categories. The results in table 9B suggest that respondents in areas with longer state presence are more likely to think that obedience to authorities is a central aspect of democracy. Although they are equally likely to attend community meetings, they are less likely to speak up in these meetings and it is more likely that the government is the only party speaking. Finally, the results in table 9C suggest that men find it marginally less acceptable to beat their wives,<sup>39</sup> and violence against women is lower in areas with longer state presence. Overall, although the estimated effects in table 9 are not large, they are supportive of the hypothesis that the historical state in Rwanda has a persistent effect on rule following today.<sup>40</sup>

#### 4.4 The local state

Formal government bureaucracy was set up by the Belgian colonizers and continued by the post-independence Hutu governments. In this paragraph I test whether longer state presence is correlated with the presence of the local state. If so, the effects I find on civil society outcomes (obedience, participation etc.) may in fact work through a stronger local government. This section tests this idea and finds no support for the local state being an important source of persistence. I also discuss the consensus among historians why this is the case.

Table 10 uses data from the Rwandan government on a number of public investments and other as-

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<sup>39</sup>The p-value from the permutation test suggests that there is a marginally significant negative effect of state presence.

<sup>40</sup>I also use data from the World Values Survey to verify that there are no systematic differences in several psychological outcomes variables. Using survey questions, I check that risk taking, individualism, locus of control (the extent to which respondents think they are in control of their lives), trust and confidence in the national government are not systematically correlated with state presence. I describe the variable definitions in the appendix. The results in Table A26 suggest that areas of Rwanda that became part of the Nyiginya state earlier look similar in terms of psychological outcomes.

pects of local government to test for a relationship between state presence and local government presence or capacity. In column (1) I use the number of primary schools in 1980 as the dependent variable, in column 2 I use the number of hospital beds. Column (3) uses the number of post offices, and column (4) uses the number of government administered markets. Column (5) uses total local government revenue and column (6) uses total government expenditure, in per capita terms. Row 1 reports IV estimates of the effect of state presence on these variables. I find a consistently small and insignificant effect of state presence on these outcomes.<sup>41</sup>

Additionally, I look at a subset of these outcomes over time. Results are reported in table A28. I use the number of hospitals in 1935, 1960 and the number of hospital beds in 1980. I also use the number of missionary stations in 1924 and 1935 as well as schools in 1960 to measure the supply of education (before the second world war, missionaries were the primary educators in Rwanda). As before, I estimate precise zeroes on all these outcomes, but for the number of schools in 1960, for which I estimate a small and marginally significant effect.

Why would there be no effect on the local state? In the historical section and appendix I outlined the consensus among historians of the development of the Nyiginya state which was unique in the sense that it never innovated modern institutions of government. Instead, it relied on a standing army and networks of socially enforced patron client relations. It is therefore not surprising that - given that the modern bureaucracy was set up after colonization - exposure to the precolonial state is not correlated with bureaucratic quality. It is also possible that it was easier for the colonial government to set up institutions where the precolonial state was stronger? The historical evidence suggests however that the colonizers aimed at creating a uniform bureaucracy throughout Rwanda. The Belgian minister of colonies remarked in 1921:

"We will practice in Rwanda and Urundi a colonial protectorate policy. The basis of this policy is the maintenance of indigenous institutions...this is perfectly realizeable in these coun-

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<sup>41</sup>In the appendix I test for an effect of state presence using a wider set of outcomes, as well as principal components of groups of outcomes. These results are reported in table A27. I first look at local public good provision. In column (1) I use the first principal component of number of energy centers, energy transformers, water installations and water pumps in 1980. In column (2) I use the first principal component of the number of primary, secondary and secondary technical schools in 1980. In column (3) I use the first principal component of the number of hospitals, maternity clinics, health clinics and other clinics in 1980. I then look at semi-public institutions. In column (4) I use the first principal component of the number of social centers, reading centers, nutritional centers, and orphanages in 1980. In column (5) I use the first principal component of the number of post offices, grain silos, gas stations and banks in 1980. In column (6) I use the first principal component of the number of agricultural cooperatives, markets, trade centers and commercial centers in 1980. I finally look at public revenue and expenditure. In column (7) I use the first principal component of total tax received, trade tax received and other tax received, all in per capita terms, in 1980. In column (8) I use the first principal component of total public expenditure, administrative expenditure, social expenditure and economic expenditure, in per capita terms, in 1980. The results suggest that there is no systematic relationship between state presence and the presence or capacity of the local government: All standardized estimated effects are smaller than 0.02 and statistically insignificant.

tries where the organization is ancient and remarkable and whose ruling class shows evident political talents...Our administration will maintain royal authority and reinforce it."

Minister Franck, 1921, cited in Straus, 2006, p. 209.

#### 4.5 Other explanations for violence in the genocide

There is an extensive literature that explains the intensity of the violence in Rwanda from other angles than the state. In an early contribution, André and Platteau (1998) argue that Rwanda's rapid growing population caused agricultural plots to be subdivided up to the point that farming an average plot did not meet subsistence needs. The genocide, they argue, was a struggle over land as a scarce resource, or a Malthusian 'positive check'. I use data on population growth between the 1978 and 1991 census as well as a measure of the extent to which Rwandan communes could provide the calories to feed the local population in 1980 from the Rwandan government to test this idea within my sample. Another theory of genocide intensity views the genocide as an outburst of ethnic hatred between Hutu and Tutsi (see Prunier (1995) for a statement of this view). Hatred towards other groups is hard to measure, and I will show results on the composition and fractionalization of the local population.<sup>42</sup> In places where Tutsi are more numerous, the Hutu population may, for instance, feel more competition over resources or propaganda initiatives by the government may be more effective. Finally, Yanagizawa-Drott (2014) shows how coverage of the RTLM radio station affected violence during the genocide. I use his data on fraction of each village that had radio coverage to test if state presence affects radio coverage. I assess the relationship between state presence and these hypotheses in appendix table A28. Column (1) uses the percent increase in population between the 1978 and 1991 censuses as the dependent variables. Column (2) uses as the dependent variable an indicator variable equal to one if a sector was unable to meet the caloric needs of the local population in 1980. I find a negative and insignificant effect of state presence on population growth and a negative and significant effect of state presence on the inability to provide sufficient calories. These results find no support for the Malthusian hypothesis. I find no systematic relationship between state presence and the presence of Tutsi and on the fractionalization of Hutu-Tutsi, nor on radio coverage. Table A30 in the appendix assesses the robustness of the correlation between state presence and violence and mobilization to including these variables as covariates. The effect of state presence remains positive, stable and significant.<sup>43</sup>

<sup>42</sup>For Hutu,  $H$  and Tutsi,  $T$ , let  $\pi_g$  be the in the fraction of the local population belonging to group  $g$ ,  $g \in H, T$ . I disregard the marginal Twa ethnic group. Fractionalization is then defined as  $\pi_H(1 - \pi_H) + \pi_T(1 - \pi_T)$ . This measure is maximized at  $\pi_H = \pi_T = 0.5$  and can be interpreted as the probability that two randomly selected individuals will belong to different groups.

<sup>43</sup>Radio coverage is only available for two thirds of the village in my sample. To preserve power, I therefore test its effect by including it separately of the other covariates in column (3).

## 5 Conclusion

A growing literature has tested the idea that more successful states historically affect prosperity even when these states have disappeared or after several leadership transitions. In order to leverage these results for policy purposes, it is important to understand what drives these effects. This paper has tested the hypothesis that longer state presence affects the way individuals respond to government demands in the future, in the context of violence in Rwanda.

The results in this paper show that a longer history of centralized rule impacts violence today. Supporting the hypothesis of this paper, this effect is different depending on the rules that government agents enforce. When the Rwandan government pursued mass mobilization and violence during the 1994 Rwandan genocide, violence is higher in those parts of Rwanda where the state formed earlier. When the Rwandan government pursued territorial control, violence is lower in these areas. Instrumental variable estimates exploiting the proximity to Nyanza – an early capital – provide evidence that these results have a causal interpretation. To further test the hypothesis that state presence affects rule following, I report heterogeneous effects by radio ownership. In places where radio ownership was higher, and rules were more easily communicated, the positive effect of state presence on violence during the genocide is stronger than in places with low radio ownership. Before and after the genocide, I observe the opposite. The effect of state presence is more strongly negative in areas where radio ownership is higher.

I then turn to directly testing the effect of state presence on rule following. I show, using data from a lab in the field experiment, that in villages with longer state presence, individuals display a greater propensity to comply with unenforced government demands. While this experiment directly measures rule following, lab results may not generalize. I provide supporting evidence from household surveys. I find that individuals that live in areas of Rwanda where the state formed earlier are less likely to find rule breaking acceptable, are more likely to think that obedience to rulers is central to democracy, are less likely to speak up in community meetings and are less likely to experience household violence.

The results in this paper speak more broadly to the persistence of historical institutions. While there is now a large body of evidence showing that history matters (Nunn, 2009), there is less evidence on whether persistent effects are constant over time. This paper has documented that a deep historical factor, the length of exposure to centralized rule, can have rapidly changing effects on economic development because the channel of transmission interacts with public policy.

The results in this paper furthermore suggest that policies aimed at promoting state capacity and policies aimed at promoting civil society need to be cautious. Policies enacted by a strong state – such as the genocide – interact with historically determined norms to potentially produce socially undesirable outcomes. In anticipation of the abuse of state capacity, capacity building may be delayed (see also Besley and Persson (2011) on this point). If civil society reform increases ‘civic mindedness’ or otherwise increases rule following, individuals are easier to mobilize for socially unproductive ends as well. Policies aimed at building civic or social capital should therefore be similarly cautious.

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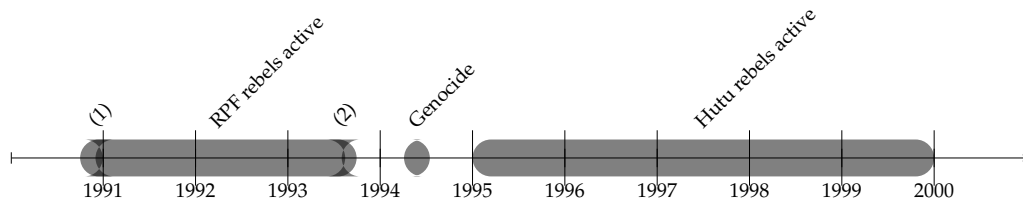
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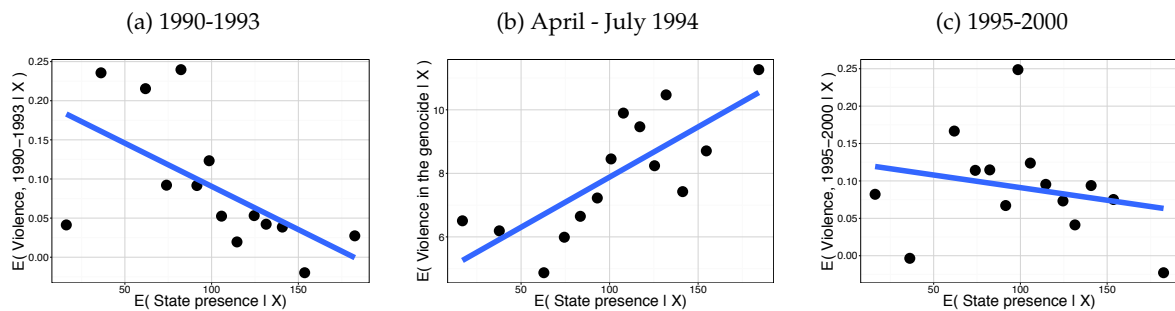
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Figure 1: TIMELINE OF MAIN EVENTS IN RWANDA, 1990-2000, AND SCATTER PLOTS OF MAIN ESTIMATED RELATIONSHIPS

Panel I: Timeline of main events in Rwanda, 1990-2000

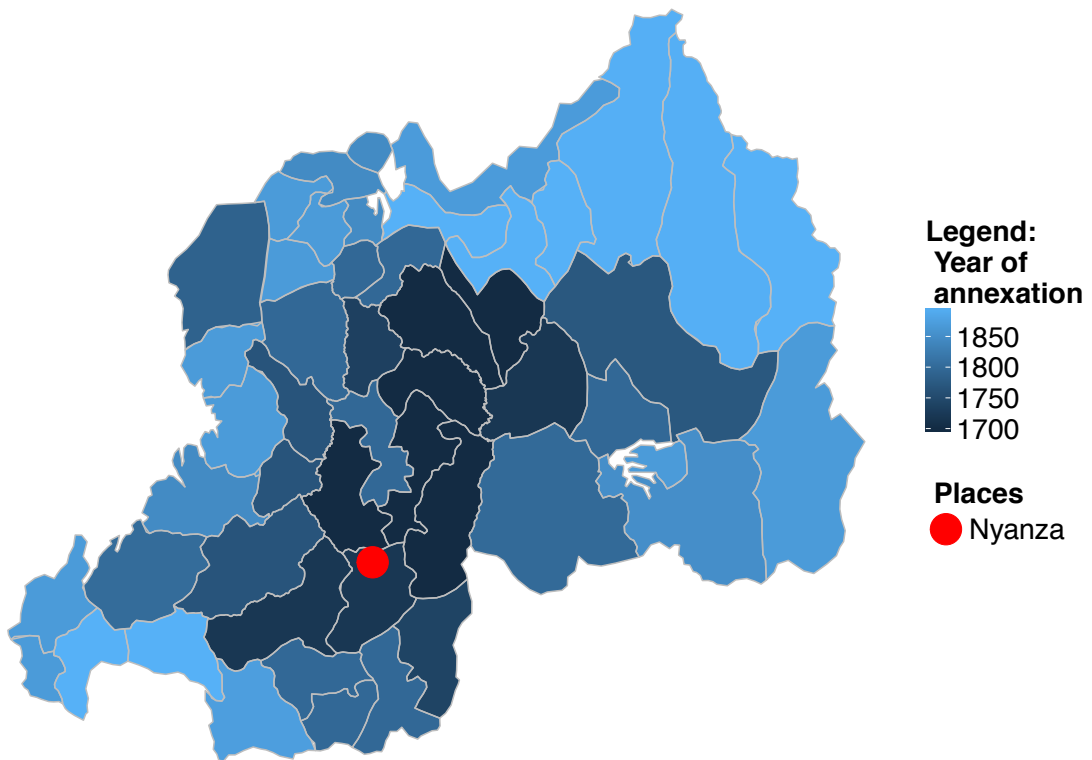


Panel II: Binned scatterplots of the relationship between state presence and violence in different periods



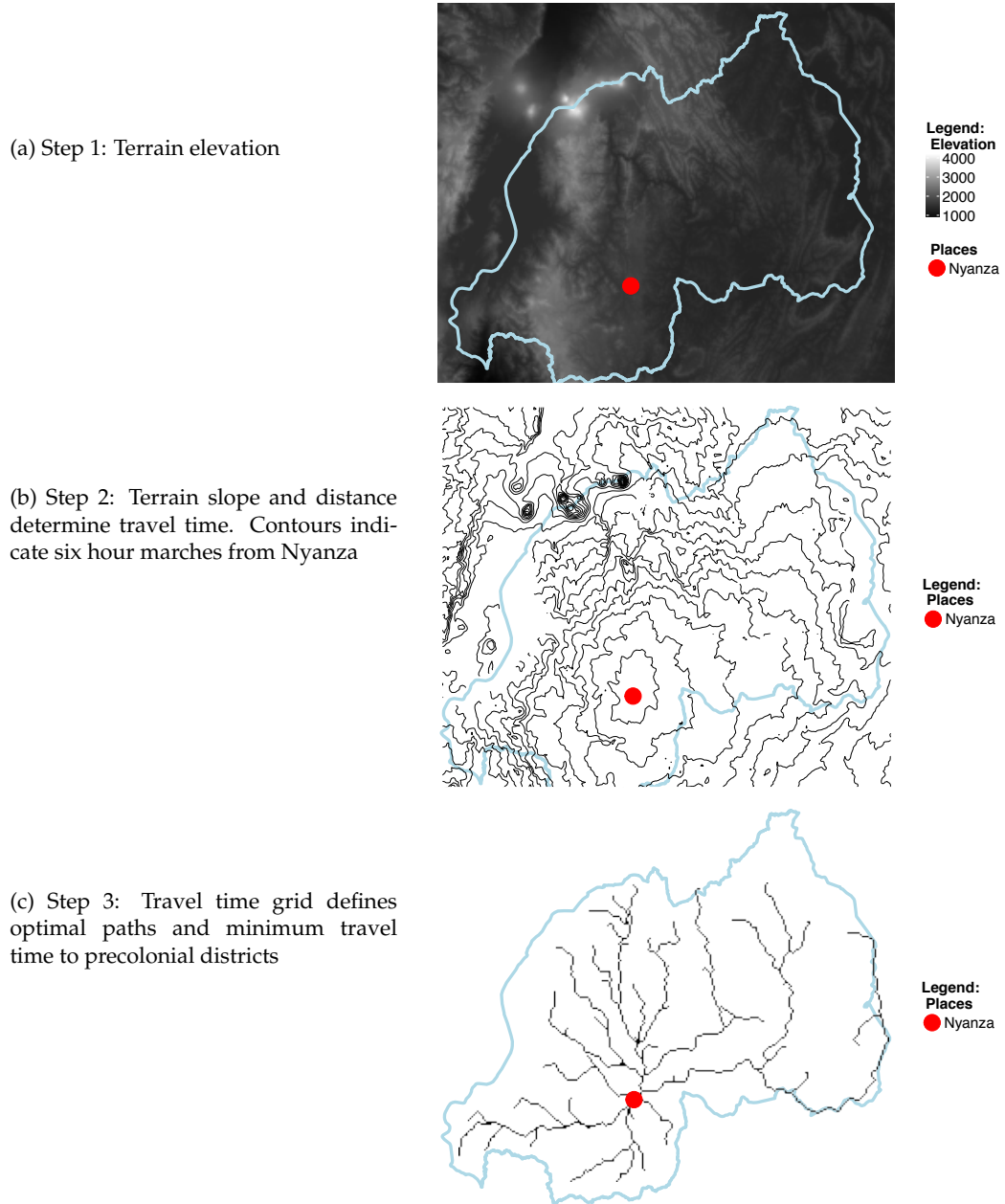
Notes: Panel I depicts a timeline of the events that are studied in this paper. On October 1st, 1990, the RPF rebels invade Rwanda from Uganda, starting a period of rebel activity in Rwanda. In December 1990, the Hutu 10 commandments are published, which started a campaign that placed Tutsi outside the law (indicated as point (1) on the timeline). Point (2) indicates the signing of the Arusha accords, on August 4th, 1993. The genocide starts on April 8th, 1994 and ends mid-July 1994. From 1995 and 2000, the Rwandan government faced rebel activity from Hutu rebels, many of whom were genocide perpetrators, and operated from within Rwanda as well as from the Democratic Republic of the Congo. Subfigures (a)-(c) are binned scatterplots of the relationship between violence and the length of the presence of the centralized state across sectors in Rwanda ( $n=1449$ ). The depicted relationships are conditioned on the distance to Kigali, Rwanda's capital, the national border, and province fixed effects. Subfigure (a) contains a scatter of the count of violent events recorded in the UCDP violence data (Sundberg and Melander, 2013). I restrict the sample to events that occurred before the Rwandan genocide, between January 1990 and December 1993. This relationship is estimated in table A16 in the appendix. Subfigure (b) contains a scatter of the fraction of individuals prosecuted for genocide violence as a fraction of 1991 population in the Gacaca data (see text for details on this data source), against state presence in years. This relationship is estimated in table 3. These data only cover the Rwandan genocide. Subfigure (c) contains a scatter of the count of violent events recorded in the UCDP violence data (Sundberg and Melander, 2013). I restrict the sample to events that occurred after the Rwandan genocide, between January 1995 and December 2000. This relationship is estimated in table A16 in the appendix.

Figure 2: MAP OF THE NYIGINYA EXPANSION



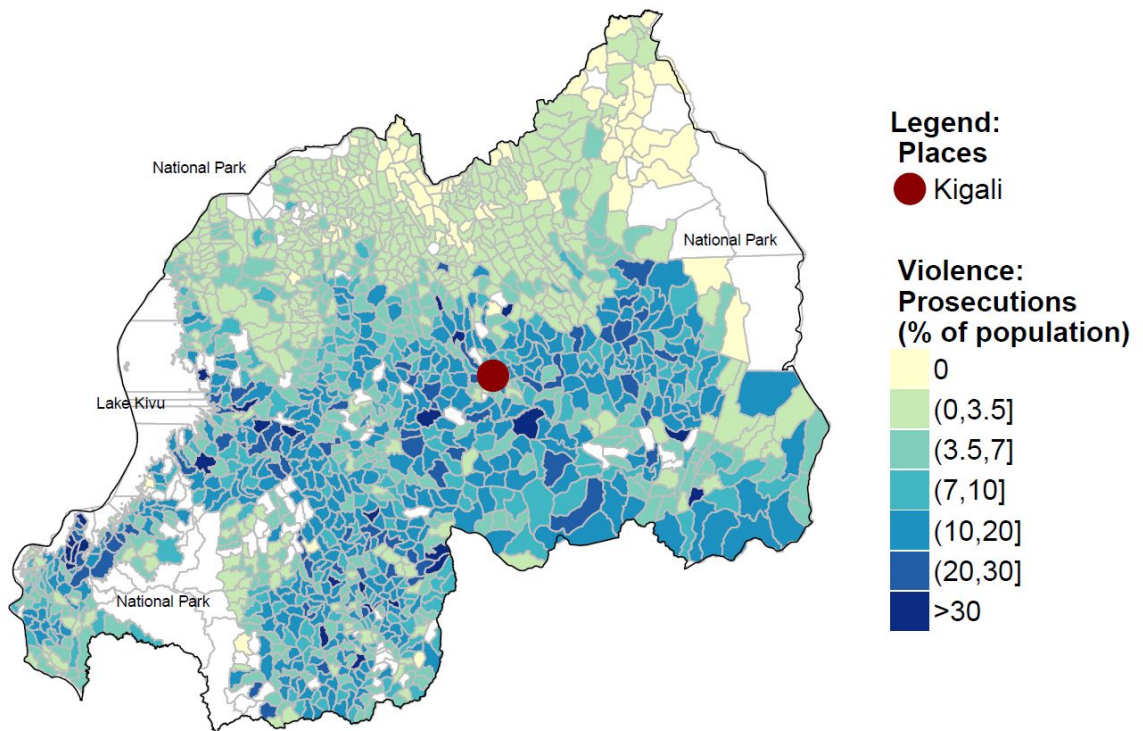
Map of districts in precolonial Rwanda. A darker shade indicates districts that have a longer history of centralized rule as part of the Nyiginya kingdom. The raw data are reported in the appendix. Nyanza is also indicated.

Figure 3: THE CONSTRUCTION OF COST DISTANCE INSTRUMENTS



Notes: Subfigures (a)-(c) describe different parts of the construction of the cost distance instruments. The full procedure is described in the main text. This process starts with a grid that defines the cost of traversing a grid cell, such as elevation in (a). Using Tobler's hiking function (Tobler, 1993) I convert the elevation into a travel time grid. Subfigure (b) visualizes this step by plotting travel time contour lines onto the elevation surface in subfigure (a). Using Dijkstra's algorithm, I then find the least time-consuming path to travel from Nyanza to each precolonial district's centroid (Dijkstra, 1959). These paths are visualized in subfigure (c). The Human Mobility Index proposed by by Özak (2010, 2013) proceeds similarly using a coarser grid. For details, see Özak (2010, 2013).

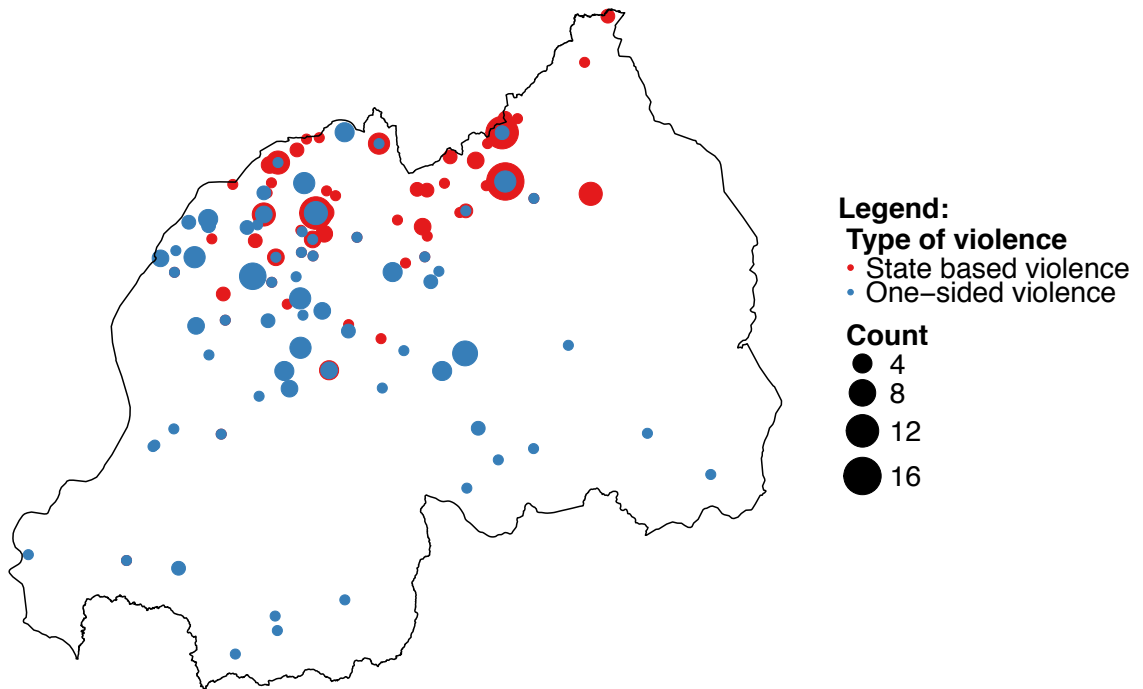
Figure 4: MAP OF VIOLENCE DURING THE GENOCIDE



Notes: Map of administrative sectors. Sectors are shaded by the number of individuals prosecuted for being an organizer of genocide violence or for murder in the Gacaca courts divided by total population in the 1991 census.

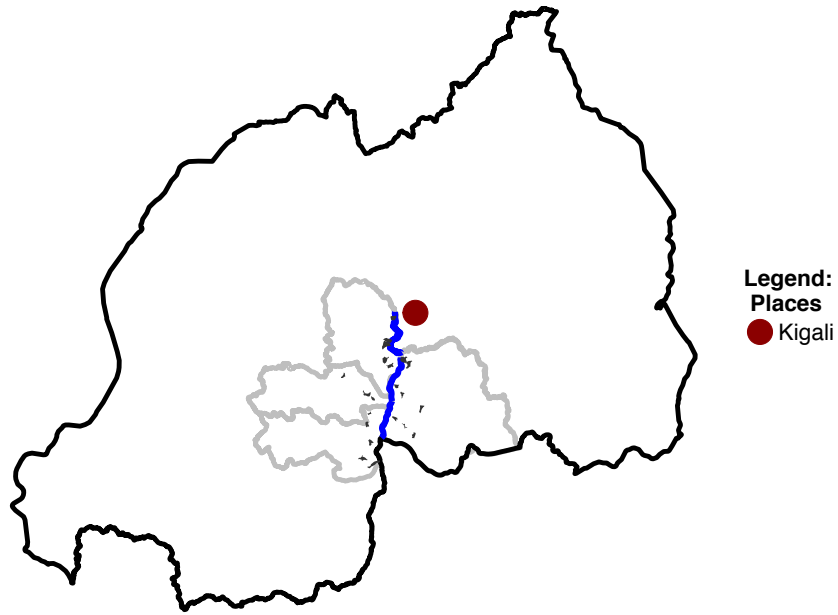


Figure 5: MAP OF VIOLENT EVENTS BEFORE AND AFTER THE GENOCIDE

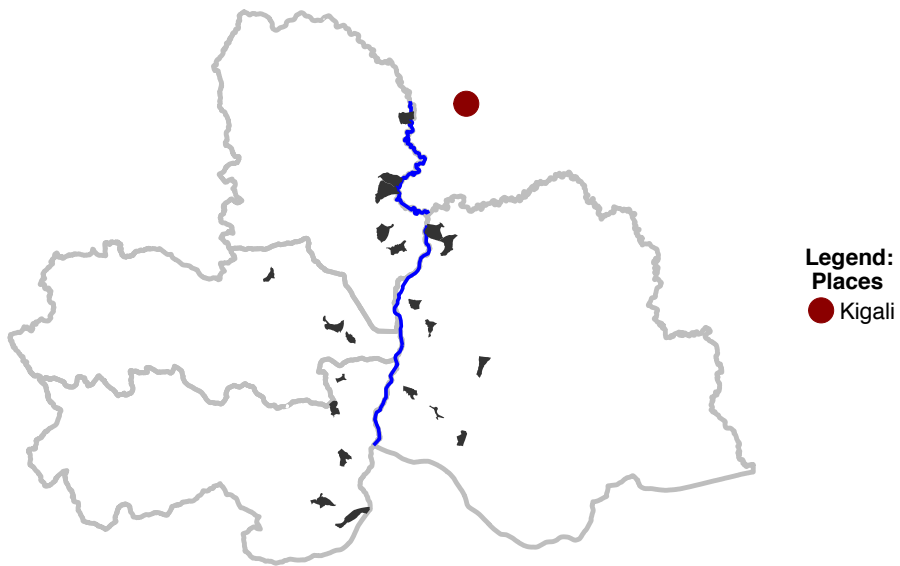


Notes: Map of Rwanda. Violent events are indicated by dots, which are scaled by the number of violent events occurring in that sector between 1990-1993 and 1995-2000. Colors indicate types of violence, state based or one-sided.

Figure 6: THE LOCATION OF PARTICIPANTS IN THE RULE FOLLOWING EXPERIMENT



(a)



(b)

Notes: Subfigures (a) and (b) map the villages that are in the fieldwork sample. In subfigure (a), the black line demarcates Rwanda's national boundaries and the districts within which participating villages are randomly selected are depicted in grey. Villages are depicted in black. The boundary separating the villages by their exposure to the Nyiginya kingdom is indicated in blue. Subfigure (b) depicts districts in grey, villages in black, the boundary separating the villages by their exposure to the Nyiginya kingdom is indicated in blue. Kigali, Rwanda's capital city, is also indicated.

Table 1: CORRELATION OF PREDETERMINED CHARACTERISTICS AND DISTANCE TO NYANZA

		Pre-Nyiginya settlement	Elevation	Slope	Banana Suitability
		(1)	(2)	(3)	(4)
Panel I					
Distance to Nyanza		-0.0732	0.0754	0.118	-0.112
	<i>White s.e.</i>	(0.226)	(0.163)	(0.145)	(0.230)
	<i>Conley s.e.</i>	[0.116]	[0.124]	[0.101]	[0.117]
$R^2$		0.214	0.547	0.364	0.455
Panel II					
Cost distance to Nyanza		-0.107	0.191	0.0522	-0.0835
	<i>White s.e.</i>	(0.249)	(0.197)	(0.190)	(0.171)
	<i>Conley s.e.</i>	[0.141]	[0.151]	[0.118]	[0.100]
$R^2$		0.217	0.560	0.361	0.454
Panel III					
Cost distance to Nyanza - Özak		-0.115	0.222	-0.0513	-0.113
	<i>White s.e.</i>	(0.228)	(0.177)	(0.219)	(0.218)
	<i>Conley s.e.</i>	[0.104]	[0.104]	[0.148]	[0.095]
$R^2$		0.216	0.559	0.361	0.455
Panel IV					
Cost distance to nearest Nyiginya army location		0.162	-0.108	0.287*	-0.0845
	<i>White s.e.</i>	(0.168)	(0.137)	(0.165)	(0.144)
	<i>Conley s.e.</i>	[0.056]	[0.079]	[0.053]	[0.084]
$R^2$		0.230	0.553	0.416	0.456
Observations		50	50	50	50

Notes: All regressions are estimated using OLS. The unit of observation is a precolonial district. All point estimates are standardized. All regressions include fixed effects at the modern province level. Parentheses report heteroskedasticity robust (White) standard errors. Square brackets report Conley (1999) standard errors, correcting for two-dimensional spatial correlation (assuming a 4 decimal degree cutoff). \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level, computed using White standard errors.

Table 2: OLS ESTIMATES OF THE DETERMINANTS OF PRESENCE OF THE NYIGINYA STATE

<i>Dependent variable: State Presence (years)</i>	Distance to Nyanza		Cost distance to Nyanza (days)		Cost distance to Nyanza - Özak (days)		Cost distance to nearest Nyiginya army location (days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	estimated coefficient	-1.356***	-1.675***	-26.41***	-14.80**	-108.3***	-134.7***	-12.06***
<i>Clustered s.e.</i>	(0.194)	(0.435)	(4.234)	(7.157)	(15.52)	(34.50)	(3.798)	(4.302)
<i>Conley s.e.</i>	[0.062]	[0.220]	[1.731]	[3.038]	[7.085]	[14.781]	[2.739]	[3.113]
Travel distance to Nyanza along 1988 road network	N	Y	N	Y	N	Y	N	N
Travel distance to Kigali along 1988 road network	N	Y	N	Y	N	Y	N	Y
Distance to country border	N	Y	N	Y	N	Y	N	Y
Province fixed effects	N	Y	N	Y	N	Y	N	Y
F-stat excluded instrument	48.81	14.83	38.90	4.28	48.70	15.25	10.08	1.77
Number of clusters	50	50	50	50	50	50	50	50
Observations	1553	1553	1553	1553	1553	1553	1553	1553
$R^2$	0.404	0.622	0.443	0.594	0.356	0.629	0.116	0.564

Notes: All regressions are estimated using OLS. The unit of observation is an administrative sector in 1994. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. Distance to Nyanza is the distance (in kilometers) between the centroid of a precolonial district and Nyanza. Cost distance to Nyanza (days) is the travel distance between the centroid of a precolonial district and Nyanza for a person on foot, measured in hours. Cost distance to Nyanza - Özak (days) is the travel distance between the centroid of a precolonial district and Nyanza for a person on foot, measured by the Human Mobility Index proposed by Özak (2010, 2013). Cost distance to nearest Nyiginya army location is the travel distance (in days) between the centroid of a precolonial district and the nearest Nyiginya army battalion, measured in hours. Travel distance is defined as the distance (in kilometers) between the centroid of a precolonial district and an endpoint along the Rwandan road network in 1988. Regressions include fixed effects as indicated in the table. Parentheses report clustered standard errors at the level of the precolonial district. Square brackets report Conley (1999) standard errors, correcting for two-dimensional spatial correlation (assuming a 4 decimal degree cutoff). \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level, computed using standard errors clustered at the precolonial district level.

Table 3: DETERMINANTS OF VIOLENCE AND MOBILIZATION IN THE RWANDAN GENOCIDE

<i>Dependent variable:</i>	Fraction of population prosecuted for:		Fraction of Hutu prosecuted for:	
	Violence (1)	Violence (2)	Organization (3)	Murder (4)
<b>State Presence (Years)</b>	<b>0.0307***</b>	<b>0.0287***</b>	<b>0.00314</b>	<b>0.0287***</b>
<i>Clustered s.e.</i>	(0.00953)	(0.00763)	(0.00243)	(0.00972)
<i>Conley s.e.</i>	[0.00508]	[0.00692]	[0.00148]	[0.00364]
Fraction of population Tutsi 1991		0.250*** (0.0611)		
Travel distance to Kigali along 1988 road network	Y	Y	Y	Y
Distance to country border	Y	Y	Y	Y
Mean of the dependent variable	7.95	7.95	1.40	7.75
Number of clusters	50	50	50	50
Observations	1449	1449	1449	1449
$R^2$	0.191	0.234	0.113	0.184

Notes: All regressions are estimated using OLS. The unit of observation is an administrative sector in 1994. Fraction of population prosecuted for violence is the number of individuals prosecuted for being an organizer of genocide violence or for murder in the Gacaca courts divided by total population in the 1991 census. Fraction of Hutu population prosecuted for organization (murder) is the number of individuals prosecuted for organizing genocide violence (murder) in the Gacaca courts divided by total Hutu population in the 1991 census. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. Fraction of population Tutsi 1991 is the total number of Tutsi divided by total population in the 1991 census. Travel distance is defined as the distance (in kilometers) between the centroid of a precolonial district and an endpoint along the Rwandan road network in 1988. All regressions include fixed effects at the modern province level. Parentheses report clustered standard errors at the level of the precolonial district. Square brackets report Conley (1999) standard errors, correcting for two-dimensional spatial correlation (assuming a 4 decimal degree cutoff). \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level, computed using standard errors clustered at the precolonial district level.

Table 4: DETERMINANTS OF VIOLENCE BEFORE AND AFTER THE RWANDAN GENOCIDE

<i>Dependent variable:</i>	Number of violent events 1990-1993, 1995-2000 (count)		State based violence (against rebels) 1990-1993, 1995-2000 (count)	One-sided violence (against civilians) 1990-1993, 1995-2000 (count)
	(1)	(2)	(3)	(4)
<b>State Presence</b> (Years)	<b>-0.00152***</b> (0.00045)	<b>-0.00132***</b> (0.00049)	<b>-0.00059***</b> (0.00018)	<b>-0.00046*</b> (0.00026)
Fraction of population Tutsi 1991		-0.00362 (0.00339)		
Travel distance to Kigali along 1988 road network	Y	Y	Y	Y
Distance to country border	Y	Y	Y	Y
Population 1991	Y	Y	Y	Y
Mean of the dependent variable	0.18	0.18	0.09	0.09
Log-likelihood	-507.08	-505.79	-309.33	-354.92
Number of clusters	50	50	50	50
Observations	1553	1553	1553	1553

Notes: All regressions are negative binomial regressions, estimated using maximum likelihood. Estimated coefficients are marginal effects (at the mean). The unit of observation is an administrative sector in 1994. Number of violent events 1990-1993, 1995-2000 (count) is a count variable measuring the number of violent events resulting in more than twenty five casualties that are classified as either state based violence or one-sided violence in the UCDP dataset occurring between January 1990 and December 1993, and between January 1995 and December 2000. State based violence (against rebels) 1990-1993, 1995-2000 (count) is a count variable measuring the number of violent events resulting in more than twenty five casualties that are classified as state based violence in the UCDP dataset occurring between January 1990 and December 1993, and between January 1995 and December 2000. One-sided violence (against civilians) 1990-1993, 1995-2000 (count) is a count variable measuring the number of violent events resulting in more than twenty five casualties that are classified as one-sided violence in the UCDP dataset occurring between January 1990 and December 1993, and between January 1995 and December 2000. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. Fraction of population Tutsi 1991 is the total number of Tutsi divided by total population in the 1991 census. Travel distance is defined as the distance (in kilometers) between the centroid of a precolonial district and an endpoint along the Rwandan road network in 1988. 1991 Population is total population in the 1991 census. All regressions include fixed effects at the modern province level. Parentheses report clustered standard errors at the level of the precolonial district. \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.

Table 5: IV ESTIMATES OF THE EFFECT OF STATE PRESENCE ON VIOLENCE AND MOBILIZATION IN THE RWANDAN GENOCIDE

<i>Dependent variable:</i>	Fraction of population prosecuted for:		Fraction of Hutu prosecuted for:	
	Violence (1)	Violence (2)	Organization (3)	Murder (4)
	<i>Panel I: Second stage estimates</i>			
<b>State Presence (Years)</b>	<b>0.0807***</b> (0.0270)	<b>0.0664***</b> (0.0211)	<b>0.00957*</b> (0.00527)	<b>0.0799***</b> (0.0268)
Mean of the dependent variable	7.95	7.95	1.40	7.75
	<i>Panel II: First stage estimates</i>			
Distance to Nyanza	-1.675*** (0.435)	-1.720*** (0.430)	-1.675*** (0.435)	-1.675*** (0.435)
F-stat of excluded instrument	14.59	15.98	14.59	14.59
R <sup>2</sup>	0.622	0.623	0.622	0.622
Fraction of population Tutsi 1991	N	Y	N	N
Travel distance to Nyanza along 1988 road	Y	Y	Y	Y
Travel distance to Kigali along 1988 road	Y	Y	Y	Y
Distance to country border	Y	Y	Y	Y
Number of clusters	50	50	50	50
Observations	1449	1449	1449	1449

Notes: All regressions in Panel I are estimated using two stage least squares using distance to Nyanza as the instrument. All regressions in panel II are estimated using OLS. The unit of observation is an administrative sector in 1994. Fraction of population prosecuted for violence is the number of individuals prosecuted for being an organizer of genocide violence or for murder in the Gacaca courts divided by total population in the 1991 census. Fraction of Hutu population prosecuted for organization (murder) is the number of individuals prosecuted for organizing genocide violence (murder) in the Gacaca courts divided by total Hutu population in the 1991 census. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. Distance to Nyanza is the distance (in kilometers) between the centroid of a precolonial district and Nyanza. Fraction of population Tutsi 1991 is the total number of Tutsi divided by total population in the 1991 census. Travel distance is defined as the distance (in kilometers) between the centroid of a precolonial district and an endpoint along the Rwandan road network in 1988. All regressions include fixed effects at the modern province level. Parentheses report clustered standard errors at the level of the precolonial district. \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.

Table 6: IV ESTIMATES OF THE EFFECT OF STATE PRESENCE ON VIOLENCE BEFORE AND AFTER THE RWANDAN GENOCIDE

<i>Dependent variable:</i>	Number of violent events 1990-1993, 1995-2000 (count)		State based violence (against rebels) 1990-1993, 1995-2000 (count)	One-sided violence (against civilians) 1990-1993, 1995-2000 (count)
	(1)	(2)	(3)	(4)
<i>Panel I: Second stage estimates</i>				
<b>State Presence (Years)</b>	<b>-0.00371***</b> (0.00116)	<b>-0.00357***</b> (0.00112)	<b>-0.00142**</b> (0.000456)	<b>-0.00186***</b> (0.000710)
Mean of the dependent variable	0.18	0.18	0.09	0.09
<i>Panel II: First stage estimates</i>				
Distance to Nyanza	-1.767*** (0.463)	-1.822*** (0.457)	-1.767*** (0.463)	-1.767*** (0.463)
$R^2$	0.622	0.623	0.622	0.622
Fraction of population Tutsi 1991	N	Y	N	Y
Travel distance to Nyanza along 1988 road	Y	Y	Y	Y
Travel distance to Kigali along 1988 road	Y	Y	Y	Y
Distance to country border	Y	Y	Y	Y
Population 1991	Y	Y	Y	Y
Number of clusters	50	50	50	50
Observations	1553	1553	1553	1553

Notes: All regressions in Panel I are negative binomial regressions, reporting marginal effects (at the mean) estimated using a control function - maximum likelihood approach with distance to Nyanza as the instrument. All regressions in panel II are estimated using OLS. The unit of observation is an administrative sector in 1994. Number of violent events 1990-1993, 1995-2000 (count) is a count variable measuring the number of violent events resulting in more than twenty five casualties that are classified as either state based violence or one-sided violence in the UCDP dataset occurring between January 1990 and December 1993, and between January 1995 and December 2000. State based violence (against rebels) 1990-1993, 1995-2000 (count) is a count variable measuring the number of violent events resulting in more than twenty five casualties that are classified as state based violence in the UCDP dataset occurring between January 1990 and December 1993, and between January 1995 and December 2000. One-sided violence (against civilians) 1990-1993, 1995-2000 (count) is a count variable measuring the number of violent events resulting in more than twenty five casualties that are classified as one-sided violence in the UCDP dataset occurring between January 1990 and December 1993, and between January 1995 and December 2000. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. Distance to Nyanza is the distance (in kilometers) between the centroid of a precolonial district and Nyanza. Fraction of population Tutsi 1991 is the total number of Tutsi divided by total population in the 1991 census. Travel distance is defined as the distance (in kilometers) between the centroid of a precolonial district and an endpoint along the Rwandan road network in 1988. Population 1991 is total population in the 1991 census. All regressions include fixed effects at the modern province level. In panel I, parentheses report bootstrapped standard errors. In panel II, parentheses report clustered standard errors at the level of the precolonial district. \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.



**Table 7: ESTIMATES OF HETEROGENEOUS TREATMENT EFFECTS OF STATE PRESENCE ON VIOLENCE AND MOBILIZATION**

<i>Dependent variable:</i>	Number of violent events 1990-1993, 1995-2000 (count)		Fraction of Hutu population prosecuted for murder			
	Below median radio ownership 1991	Above median radio ownership 1991	Below median radio ownership 1991	Above median radio ownership 1991	Opposition mayor	Government mayor
	(1)	(2)	(3)	(4)	(5)	(6)
<b>State Presence (Years)</b>	-0.0262 (0.0466)	-0.158*** (0.0498)	0.137 (0.106)	0.309*** (0.0500)	0.160 (0.137)	0.234** (0.0985)
Travel distance to Kigali along 1988 road	Y	Y	Y	Y	Y	Y
Distance to country border	Y	Y	Y	Y	Y	Y
Chow test of coefficient equality (p-value)	0.0275	0.0275	0.0832	0.0832	0.636	0.636
Number of clusters	50	50	50	50	50	50
Observations	769	784	709	740	221	1146
$R^2$	0.016	0.027	0.239	0.196	0.029	0.176

Notes: All regressions are estimated using OLS. The unit of observation is an administrative sector in 1994. All point estimates are standardized. Number of violent events 1990-1993, 1995-2000 (count) is a count variable measuring the number of violent events resulting in more than twenty five casualties that are classified as either state based violence or one-sided violence in the UCDP dataset occurring between January 1990 and December 1993, and between January 1995 and December 2000. Fraction of Hutu population prosecuted for murder is the number of individuals prosecuted for murder in the Gacaca courts divided by total Hutu population in the 1991 census. Travel distance is defined as the distance (in kilometers) between the centroid of a precolonial district and an endpoint along the Rwandan road network in 1988. All regressions include fixed effects at the modern province level. Parentheses report clustered standard errors at the level of the precolonial district. \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.

Table 8: THE EFFECT OF STATE PRESENCE ON RULE FOLLOWING

<i>Dependent variable:</i>	Compliance with tax demand (% of base)			
	(1)	(2)	(3)	(4)
State Presence (100 years) - Place of residence	8.916**	8.663**	8.687**	4.586
<i>Permutation test p-value</i>	0.040	0.041	0.041	0.368
State Presence (100 years) - Place of birth				6.862*
<i>Permutation test p-value</i>				0.0896
Demographic controls	N	Y	Y	Y
Income	N	N	Y	N
Education	N	N	Y	N
Distance to boundary * State Presence	Y	Y	Y	Y
Control group mean	68	68	68	68
Clusters	21	21	21	21
Observations	415	413	413	413
$R^2$	0.010	0.027	0.027	0.031

Notes: All regressions are estimated using OLS. The unit of observation is an individual respondent. Compliance with tax demand (% of base) is the tax paid by an individual in the random taxation game, divided by the theoretical tax base, 1500 Rwandan Francs. State Presence (100 years) is an indicator equal to one if a respondent lived to the East of the Akanyaru river in October 2014. Demographic controls include age, age squared and a dummy for gender. Income is equivalent daily income in Rwandan francs. Education is education in years. P-values of the permutation test give the fraction of t-statistics greater than the t-statistic of the estimated effect in the reported regression across 5000 permutations of the state presence dummy. \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.

Table 9: OBERVATIONAL EVIDENCE ON RULE FOLLOWING

TABLE 9A: ATTITUDES

<i>Dependent variable:</i>	Is it justifiable to:			
	Avoid a fare on public transport	Steal property	Cheat on taxes	For someone to accept a bribe
	(1)	(2)	(3)	(4)
<b>State Presence (Years)</b>	-0.0547** (0.0272)	-0.0534* (0.0277)	-0.0601** (0.0282)	-0.0481* (0.0269)
Permutation test p-value	0.045	0.052	0.033	0.080
Clusters	22	22	22	22
Observations	2593	2589	2593	2582
$R^2$	0.017	0.058	0.011	0.059

Notes: All regressions are estimated using OLS. The unit of observation is the individual. All point estimates are standardized. Dependent variables are several questions from the World Values survey waves of 2007 and 2012 that ask whether. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. All regressions include demographic controls and survey year fixed effects. Demographic controls include age, age squared and a gender dummy. Survey year fixed effects are indicator variables for whether an individual respondent was interviewed as part of the 2007 or the 2012 World Values survey wave. P-values of the permutation test give the fraction of t-statistics greater than the t-statistic of the estimated effect in the reported regression across 5000 permutations of the state presence dummy. Parentheses report heteroskedasticity robust standard errors. \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.

TABLE 9B: OBEDIENCE AND PROTEST AGAINST GOVERNMENT

<i>Dependent variable:</i>	Obedience to rulers important for democracy	Participation in community meetings	Government only speaking at meetings	Respondent speaks up in community meeting
	(1)	(2)	(3)	(4)
<b>State Presence (Years)</b>	0.103*** (0.0398)	0.0187 (0.0350)	0.0794** (0.0387)	-0.0540** (0.0202)
Permutation test p-value	0.010			
Clusters	22	50	50	50
Observations	1203	4550	3328	3638
$R^2$	0.015	0.020	0.013	0.033

Notes: All regressions are estimated using OLS. The unit of observation is the individual. All point estimates are standardized. Obedience to rules important for democracy is a question asking about how central to democracy obedience to rulers is. Participation in community meetings is an indicator variable equal to one if respondent indicated participation in community meetings. Government only speaking is an indicator equal to one if the government is the only party speaking at these meetings. Respondent speaks up in community meetings is an indicator variable equal to one if respondent indicated speaking up in these community meetings. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. All regressions include demographic controls. Demographic controls include age, age squared and a gender dummy. P-values of the permutation test give the fraction of t-statistics greater than the t-statistic of the estimated effect in the reported regression across 5000 permutations of the state presence dummy. Parentheses report heteroskedasticity robust standard errors in column (1) and clustered standard errors at the level of the precolonial district in columns (2)-(4). \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.

TABLE 9C: VIOLENCE AGAINST WOMEN

<i>Dependent variable:</i>	Justifiable to beat wife	Verbal violence	Physical violence	Sexual violence
	(1)	(2)	(3)	(4)
<b>State Presence (Years)</b>	-0.0472 (0.0349)	-0.0664* (0.0333)	-0.0652** (0.0318)	-0.0669** (0.0271)
Permutation test p-value	0.062			
Clusters	22	50	50	50
Observations	1280	1859	1858	1859
$R^2$	0.16	0.009	0.010	0.006

Notes: All regressions are estimated using OLS. The unit of observation is the individual. All point estimates are standardized. Justifiable to beat wife is question that asks how justifiable it is to beat your wife. Verbal violence is an indicator equal to one if a respondent experienced verbal abuse from his/her partner. Physical violence is a variable ranging between zero and two which is the sum of two indicators, one equal to one if a respondent experienced pushing, shaking, throwing of objects, slapping, punching, hitting with objects, arm twisting, hair pulling and one equal to one if a respondent experienced kicking, dragging, strangling, burning or threatening with a weapon from her partner. Sexual violence is an indicator equal to one if a respondent experienced forced sex or sexual acts from her partner. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. All regressions include demographic controls. Demographic controls include age, age squared and a gender dummy. P-values of the permutation test give the fraction of t-statistics greater than the t-statistic of the estimated effect using state presence across 5000 permutations of state presence. Parentheses report heteroskedasticity robust standard errors in column (1) and clustered standard errors at the level of the precolonial district in columns (2)-(4) \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.

Table 10: ESTIMATES OF THE EFFECT OF STATE PRESENCE ON LOCAL GOVERNMENT

<i>Dependent variable:</i>	Primary schools 1980	Hospital beds 1980	Post offices 1980	Markets 1980	Government revenue (Francs per capita) 1980	Government expenditure (Francs per capita) 1980
	(1)	(2)	(3)	(4)	(5)	(6)
<b>State Presence (Years)</b>	0.00124 (0.0200)	-0.173 (0.162)	-0.00231 (0.00282)	0.00219 (0.00527)	0.101 (5.815)	2.720 (5.880)
Travel distance to Nyanza along 1988 road	Y	Y	Y	Y	Y	Y
Travel distance to Kigali along 1988 road	Y	Y	Y	Y	Y	Y
Distance to country border	Y	Y	Y	Y	Y	Y
Mean of the dependent variable	11.3	21.2	0.45	1.33	4014	3923
Number of clusters	50	50	50	50	50	50
Observations	1553	1553	1553	1553	1297	1468
$R^2$	0.187	0.053	0.036	0.076	0.270	0.229

Notes: All regressions in Panel I are estimated using two stage least squares using distance to Nyanza as the instrument. The unit of observation is an administrative sector in 1994. Primary schools 1980 is the number of primary schools in 1980. Hospital beds 1980 is the number of hospital beds available in local hospitals in 1980. Post offices is the number of post offices in 1980. Markets is the number of government-sanctioned local markets in 1980. Government revenues (Francs per capita) 1980 is total government revenue from taxes divided by 1978 population in 1980 Rwandan Francs. Government expenditure (Francs per capita) 1980 is total government expenditure from taxes divided by 1978 population in 1980 Rwandan Francs. State presence (years) is the number of years a sector was under centralized rule before colonization in 1897. Travel distance is defined as the distance (in kilometers) between the centroid of a precolonial district and an endpoint along the Rwandan road network in 1988. All regressions include fixed effects at the modern province level. Parentheses report clustered standard errors at the level of the precolonial district. \* indicates significance at the 10 percent level, \*\* at the 5 percent level, \*\*\* at the 1 percent level.