Culture and Contracts: The Historical Legacy of Forced Labour

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January 28, 2021*

Can divide-and-rule colonial policy be responsible for contemporary ethnic tension? This paper empirically investigates the role of a divisive and extractive colonial policy on Hutu-Tutsi discord in Rwanda and Burundi. It shows that Hutu with a family history of subjugation to forced labour by Tutsi chiefs are less trusting of Tutsi today and less willing to partner with Tutsi for a cooperative task. This may have implications for agriculture insurance agreements since Hutu are more agrarian and Tutsi are more pastoral. Indeed, Hutu with a forced labour family history make fewer inter-household insurance agreements and are more likely to experience default.

JEL Codes: J15; L14; Q12; Z13
Keywords: Divide-and-rule; Inter-ethnic trust; Colonial institutions

1. Introduction

The colonial origins of economic underdevelopment are well documented (Acemoglu *et al.*, 2001, Lowes and Montero, 2020a,b) but the mechanisms underpinning this effect are less clear. In fact, some institutions that were clearly extractive seem to have had positive development impacts (Dell and Olken, 2020). This begs questions regarding how and why extractive institutions were so consequential. One nearly ubiquitous feature of extractive colonial rule that has been widely discussed as generating persistent underdevelopment is divide-and-rule, and its lasting influence on social cohesion.[1] For instance, Morrock (1973) notes that 'divide-and-rule [is] a policy that has played a crucial part in ensuring the stability -indeed, the viability- of nearly every major colonial system.' Yet, the ramifications have not been empirically investigated. Accordingly, this paper aims to measure ethnic attitudes in Rwanda and Burundi, to see if they are related to one particular divisive and extractive colonial policy: the subjugation of the Hutu to forced labour by the Tutsi.

In 1931 Belgian colonial authorities introduced a uniform village coffee quota as part of their rent-extraction efforts. This policy mandated the production of viable export crops, coffee in particular, to facilitate taxation by encouraging the use of money instead of barter. To satisfy the coffee quota, Tutsi chiefs were required to subject Hutu farmers to forced labour, being told 'you whip the Hutu or we whip you' (Watson, 1991). Whether the coffee quota was binding depends on land characteristics. In particular, pre-quota coffee production depends on the suitability of the land for both coffee and its alternatives, as well as the value of each crop. So, whether the quota was binding in a particular region may be viewed as plausibly exogenous variation in forced labour. The analysis in this paper therefore investigates whether ancestral land characteristics that hindered coffee production are associated with worse ethnic attitudes for the descendants of Hutu who would have been subjected to forced labour to meet the quota.

To implement this strategy, I use data from the trust game, family history, and economic agreements among a sample of Hutu and Tutsi farmers across Rwanda and Burundi. The trust game in this setting is played face-to-face between strangers, to observe the rule of thumb people use vis-à-vis trustworthiness

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[1]A few examples include the Hindu-Muslim conflict in India; the Kikuyu-Luo rivalry in Kenya. Both have been blamed on British colonial policy (Tharoor, 2017). The French in south Asia also engaged in divide-and-rule strategies (McCoy, 1971), as did America in the Philippines (Morrock, 1973) and the Dutch in Indonesia (Nawawi, 1971).

when interacting with someone of a different ethnicity.[2] I then link the trust and ethnicity data to respondents' family history.

The empirical strategy is to examine the relationship between the historical costliness of allocating land to coffee and contemporary ethnic attitudes. This works in part because the data strongly suggests that the costliness of allocating land to coffee influenced the implementation of forced labour in a non-linear way. Several different measures of forced labour intensity suggest a kink-like relationship with coffee costliness. The empirical strategy exploits this to examine whether inter-ethnic attitudes are related to the coffee costliness in the same non-linear way. I expect this to be true only for the descendants of the Hutu coffee farmers who would have been most influenced by forced labour (henceforth *forced labour Hutu*). This strategy sets up a number of natural falsification tests to help to identify forced labour as the causal mechanism.

Indeed, I find that forced labour Hutu made trust game offers to Tutsi that were about 20% lower than other Hutu from their district, and were also less willing to partner with Tutsi in the lab. Both effects are non-linear in land characteristics in the same way as forced labour. Meanwhile, Tutsi who would have been targeted had they been Hutu show similar levels of trust as their peers. Furthermore, forced labour Hutu only make lower trust game offers to Tutsi; co-ethnic trust appears unaffected.

To get a sense of whether these effects are economically meaningful, I collected data on inter-household agriculture insurance agreements. On average, we might expect Hutu and Tutsi to make good insurance partners because of high ethnic agricultural specialization (Destexhe, 1995).[3] In this context, partnering exclusively within-ethnicity would likely reduce insurance, given an increased likelihood that both partners simultaneously experience poor agricultural output. I find evidence consistent with partner-selection induced reductions in agricultural insurance.

These results make two contributions to the literature. The first is to show that divide-and-rule colonial policy contributed to Hutu-Tutsi enmity. Lowes and Montero (2020a) show rubber concessions in the Democratic Republic of Congo are associated with worse economic outcomes but more pro-sociality. There has been work showing that slave-trade intensity had long-lasting economic impacts (Nunn, 2008) as a result of lower generalized trust (Nunn and Wantchekon, 2012). Work more specific to ethnic attitudes has documented that people behave dif-

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[2]Group heuristics is one definition of culture (Boyd and Richardson, 2005, Nunn, 2012)).

[3]Tutsi are typically more pastoral; Hutu are more agrarian.

ferently towards those of another ethnicity (Knack and Keefer, 1997, Alesina and La Ferrara, 2002, Lowes *et al.*, 2015) however, empirical analysis of the variation in this effect has been limited. Notable contributions in this vein include Miguel (2004), who shows that Tanzanian policy was effective in bringing together ethnically diverse communities; Shayo and Zussman (2011) who show that terrorism influences out-group bias; and Voigtländer and Voth (2014) who show that infrastructure investment in Germany was positively associated with Naziism.[4] The second contribution is an analysis of how forced labour and inter-ethnic distrust relate to economic agreements. The literature on the economic importance of general trust is large,[5] however, microeconomic evidence of how inter-ethnic trust influences economic interactions is sparse. Notably Hjort (2014) shows that poor inter-ethnic co-operation in a Kenyan firm generated production inefficiencies.

2.   Historical Background

There is little evidence of Hutu-Tutsi conflict prior to the mid-19th century. They lived in segregated communities (Nyirubugara, 2013) that were economically and politically undeveloped. Communities relied on prominent local lineages for public goods, with non-monetary goods being voluntarily exchanged for protection and representation (Newbury, 1988). This traditional clientship transformed for the first time under king Rwabugiri (r. $1863 - 1895$) in a few ways.

Rwabugiri was Tutsi, and ushered in a wave of Tutsi chiefs, even in traditionally Hutu villages. Preferential treatment towards Tutsi citizens meant that Hutu took a subservient role in society for the first time (Nyirubugara, 2013). Second, Rwabugiri implemented mandatory taxation to replace the lineage system which, for the most part, was paid using cattle (*Umuheto* or *Ubuhake*).[6] Since Hutu did not traditionally keep cattle, mandatory payment with labour (*Ubureetwa*) was a substitute. 'Of the various services performed for chiefs, *Ubureetwa* 'was the most hated and humiliating.' It symbolized the servitude of the Hutu.' (Newbury, 1988).

When Belgium took control of the colony after World War I, their priority was was "modernization." Their immediate goals were twofold: to phase the economy into the monetary system and away from bartering; and to abolish what they

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[4]Eifert *et al.* (2010), Bazzi and Gudgeon (2021) study the intersection of ethnic conflict and politics.

[5]For a literature review see Alesina and LaFerrara (2005); for one on culture/institutions see Alesina and Giuliano (2015).

[6]In *Umuheto* a client would purchase 'protection' from the Chief in exchange for a cow. *Ubuhake* involved the loan of pasture land for protection (Newbury, 1988).

believed to be antiquated local institutions, like *Umuheto*, *Ubuhake* and *Ubureetwa*. Accordingly, they made several changes, two of which inadvertently impacted Hutu-Tutsi relations. First, they scaled back forced labour requirements under *Ubureetwa*, with the plan to phase out the practice completely. Concurrently, Belgium pursued an aggressive export strategy. One pillar of this strategy was the imposition of uniform agricultural requirements on each village, designed to boost exports (Bonaventure, 2010). Coffee was the export crop most suited to production in the region, and a regulation in 1931 made coffee a required crop (Page and Sonnenburg, 2003, page 664).

> Belgium also decided to initiate coffee production on Hutu land under the corvée system: compulsory labor demanded by a lord or king. Hutus were subjected to ten lashes a day to ensure a solid work ethic, in case these 'inferior' people strayed from their assigned duties. By the time of Rwandan independence in 1962, the Hutus were a subjugated population, manipulated by both their fellow Rwandans and colonial powers.

> (Mendis, 2014)

The coffee quotas had the largest impact on the regions that were least suitable for coffee. So why did Belgium believe that growing coffee in these regions was a good idea? While coffee was not heavily grown at the time, the fact was that almost all regions could (and arguably should) grow at least some coffee. In fact in Rwanda and Burundi very few villages are completely unsuitable for coffee according to data from Fischer *et al.* (2012) (figure A1).[7]

Coffee quotas influenced agriculture dramatically after 1931 (figure A2).[8] Coffee went from being one of twenty modestly produced crops to dominating the industry, mainly replacing subsistence crops like manioc and maize. The pressure from the chiefs to meet the quotas was especially burdensome for Hutu coffee farmers who were best positioned to scale production in regions where coffee was not already heavily produced.

> This was *ubureetwa*, one 'imposed specifically on Hutu' and left unreformed because officials argued that to do away with it would be to 'undermine the chiefs' authority over the population.' The chief who

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[7]Data are from FAO-GAEZ: description in section 3.C.

[8]This is based on data transcribed from Belgian colonial yearbooks. Description in Appendix B.5.

came out of the interwar period was expected to enforce and supervise obligatory cultivation of food exports...and even to become majority coffee producers by using corvée labour.

(Mamdani, 2014)

3. DATA

The analysis relies on survey, lab-in-the-field, and geographic information system (GIS) data. The outcomes and biographical controls come from a survey and a set of lab exercises implemented in Burundi and Rwanda in 2013. Data was collected from 869 farmers from 143 different villages (figure 1). We brought together 4-5 individuals from each of 4-5 different villages in any given data collection session, and conducted two such sessions per day, of about 20 people each. Villages were primarily in the south in Rwanda, in part to ensure that some Tutsi would attend,[9] while in Burundi most of the country was covered. Summary statistics for the data used in the analysis appears in table B1. Protocol details are in Appendix B.2.

Measurement is a challenge in this context for a variety of reasons, which I will address in turn.

3.A. Data Challenge 1: Measuring Outcomes

i) The trust game The trust game is a standard method to elicit ethnic attitudes (Fershtman and Gneezy, 2001). In our implementation, a randomly matched pair sat down with an enumerator to play face-to-face.[10] Pairs played only one round to remove strategic considerations. Because I only allowed pairings between people who were from different villages and for whom the field team confirmed had never met, respondents could only use decision heuristics to determine the trustworthiness of their partner.

The trust game worked as follows: one partner was randomly assigned to be 'the sender' and the other was assigned to be 'the receiver.' The sender was given 600RWF (approximately $1 USD), which they could share with their partner or keep to themselves.[11] They were given 6 $100 notes of Monopoly money, and asked to pass as much money as they wanted to their partner. Whatever they chose to share was matched by the enumerator and given to the receiver, who then decided

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[9]The approximate ethnic distribution is in figure B1

[10]Enumerator instructions appear in Appendix B.3

[11]Mean daily wage for the sample was about $1.05.

how to share that sum. The amount offered by the sender is used in the analysis as a measure of how much the sender trusts the receiver.

ii)   Partner Selections Respondents were told that they would be partnered with someone, and that their ability to cooperate with their partner could increase their payoff. They were given a chance to submit a list of five people that they would prefer to be partnered with for this exercise. Respondents looked around the room at all of the people in their session (having not yet interacted with anyone) and listed the ID-tag numbers of their choices. I am interested in the share of people the respondent selected that are *not* in their ethnic group.

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

The numerator is the number of people on the five person list that are not in the respondent's ethnic group. I divide this by the smaller of the number of out-group members available to be selected, and the total number of selections made.

iii)   Contracts The third outcome focuses on real world informal contracts. The idea was to examine a context where inter-group agreements may be beneficial, and to assess the outcomes in these agreements. To this end, I surveyed respondents about inter-household agricultural insurance agreements. We may expect agricultural shocks to be less correlated across ethnic groups than within them, since Tutsi are largely pastoral and Hutu largely agrarian. I focus primarily on the number of agreements, and whether they experienced default in these agreements.

3.B.   Data Challenge 2: Ethnicity

In Burundi, asking about ethnicity is permitted, and enumerators did so there. In Rwanda it is typically not permissible to ask individuals about ethnicity - either their own, or of people they interact with. So, I use the ethnicity proxy described in Blouin and Mukand (2019): the eligibility for a genocide survivor fund, which is available only to Tutsi in genocide regions. In combination with our sampling strategy, which was limited to regions where the fund operates, a positive response to the eligibility question signifies Tutsi status (see Appendix B.4 for more details).

   Also important is the identification of ethnicity by the respondents themselves. Respondents played in either a co-ethnic or inter-ethnic trust game, and could not be primed on which it is. So, the game is only a reliable measure of ethnic attitudes if respondents can infer ethnicity based on a brief interaction with their partner.

There are stereotypical physical differences between Hutu and Tutsi (Gourevitch, 1998), however misattribution almost certainly occurred. Measurement error is likely to bias results towards zero as it seems unlikely that attribution errors are correlated with family forced labour history.

3.C. Data Challenge 3: Variation in Forced Labour

To measure forced labour exposure I need the locations of the ancestors of respondents during the colonial era. Accordingly, the survey included a module on family migration history going back three generations. I use the location of the parents birth, which typically gives the location of the grandparents during the colonial era. Since there is some expectation that coffee farmers may have been disproportionately targeted for forced labour, I also asked about parent and grandparent crop production, and rely on respondent estimates of grandparent coffee cultivation intensity (share of land devoted to coffee).

Grandparent location was geocoded and matched to crop suitability data (figure 1). The land characteristic data comes from Fischer *et al.* (2012) (FAO GAEZ) who provide GIS data on the potential produceable tonnes per hectare for each crop across the globe.[12] This was matched to colonial price data for each crop, which was transcribed from Belgian colonial yearbooks (details in Appendix B.5).

Obtaining a measure of actual forced labour was also a challenge. It was a policy with a strong ethnicity and power element, and after consulting with local partners, it was determined that it would negatively prime ethnicity, so it was not mentioned in the survey. Instead, to get variation in actual forced labour, I scraped Google Books for any digitized colonial-era documentation in French that referenced both a particular district and some of the language typically used to discuss forced labour. The aim was to target the reports that were written by administrators in the region as closely as possible.

I investigate three terms that, after reading through reports by Belgian administrators (e.g. Rwa (Scan Date: October 17, 2012)), appeared to be frequently used in reference to forced labour. They are (1) *Ikiboko* which is a local word for whips with a hippo hide. This was the punishment for refusal to comply with labour requirements; (2) *corvée* which is french for forced labour; and (3) *Prestations Coutumières Dues* which is french for traditional labour requirements. For each term, I collected the share of documents about each grandparent-district that

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[12]Estimates are available for various input levels. To match historical conditions for Rwanda-Burundi, data chosen was for low-input and rain-fed conditions. The resolution is at the 5 arc-minute level.

mentioned the search term.

4. EMPIRICAL STRATEGY

I am interested in the average causal effect of forced labour on attitudes for those with a family history of forced labour. Consider the case of trust, and denote inter-ethnic trust as T and forced labour as FL. Two dimensions may have determined a respondent's forced labour family history.

The first, Φ_i, captures that some individuals (denoted i) had grandparents who were more likely to be selected into forced labour by the chief, *within each village*. For instance, Tutsi chiefs may have selected people disliked by the Tutsi community in a village. Φ_i is endogenous, and in any case is not directly observed because, as previously mentioned, this type of ethnically divisive question was problematic to collect in this context. I therefore need to proxy for Φ_i, and propose considering that coffee farmers were mostly selected to work to meet the quota. The variable C_i denotes whether the respondent's grandparents produced *any* coffee on their own land, and is used as a proxy for Φ_i.

One concern may be whether respondents reliably knew what their grandparents grew, and even if so, whether they are recollecting coffee production before or after forced labour. This is quite important, since we might expect Hutu coffee production under forced labour to be endogenously related to ethnic attitudes. However, it would be surprising if coffee production independent of forced labour was related to ethnic attitudes. A suggestive test is available. If respondents recollect whether their grandparents grew coffee *prior* to forced labour, we should expect that relative coffee suitability is positively correlated with coffee production. If on the other hand, they recall (the more endogenous) coffee production under forced labour, we would expect either no correlation, or possibly even the reverse. Table C1 and figure C1 reveal that observed Hutu coffee production was less common in forced labour regions, consistent with recollection of coffee production prior to the implementation of forced labour.

The second main factor determining forced labour, $\phi_{l^{gp}}$, captures that some grandparent locations (denoted l^{gp}) were exposed to forced labour and others were not. These may also have been selected based on pre-existing inter-ethnic trust. However, land characteristics may be related to FL through $\phi_{l^{gp}}$ if forced labour was used to meet the coffee quota, as suggested by the history literature. All villages have some level of unconstrained equilibrium coffee production, which may be above or below the quota. This production is a function of the region-

specific returns to coffee relative to all other crops. Using the FAO data on crop suitability, I observe produceable tons per hectare for each crop (denoted $q_{lgp,s}^{FAO}$ for each crop s), which I matched to colonial crop prices (denoted p_s).[13] This allows me to compute returns to producing each crop s as: $\pi_{lgp,s} = q_{lgp,s}^{FAO} p_s$.

Now, consider how crop returns ($\pi_{lgp,s}$) might have related to the regional distribution of forced labour (ϕ_{lgp}). Hypothetically, if the best crop in the region returned 500% of what coffee returned, then coffee would be unlikely to be produced naturally, and the quota would increase production dramatically, largely using forced labour. On the other hand, if the best coffee alternative returned only 20% of what coffee returned, the quota likely would not bind, so less forced labour would be required. Accordingly, consider:

$$\Pi_{lgp} = \frac{max\{\pi_{lgp,s}|s \neq c\}}{\pi_{lgp,c}}. \tag{2}$$

Where c denotes coffee and s can be any crop.[14] A histogram of this variable can be seen in figure C2. The figure suggests that the median value of Π_{lgp} is around 0.9, and of the 122 districts 62 are above $\Pi_{lgp} = 0.9$.

I hypothesize that $cov(\phi_{lgp}, \Pi_{lgp}) > 0$ because the higher the returns to the best non-coffee crop relative to coffee, the less likely coffee is to be grown prior to the quota, the more likely the quota was binding, and the more forced labour we might expect in that location. Table C3 examines the relationship between forced labour and crop returns. Indeed, there is a positive correlation between the costliness of allocating land to coffee and forced labour / mistreatment for each measure of forced labour.

The first principal component of the three measures is plotted in figure 2.[15] There is a vertical line in each graph at $\Pi_{lgp} = 0.975$, corresponding to where the slope turns positive for the mean of the three measures.[16] I interpret $\Pi_{lgp} = 0.975$ as an approximation of the typical point where the quota binds, and define $\tau_{lgp} = \mathbb{1}(\Pi_{lgp} > 0.975)$. The main specification used throughout the analysis can now be written as:

$$T_{i,lgp,lr} = \beta_0 + \beta_1 C_{i,lgp,lr} \cdot \tau_{lgp} + \beta_2 \tau_{lgp} + \beta_3 C_{i,lgp,lr} + \mathbf{\Lambda_{lr}} + \mathbf{X_{i,lgp}}.\mathbf{\Gamma} + \epsilon_{i,lgp,lr} \tag{3}$$

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[13]source: M. le Premier Ministre (1927(-1945))

[14]The other crops and their relative frequency appears in table C2.

[15]They are plotted separately alongside the average in figure C3, and all produce similar estimates. The analogous binscatter plots can be seen in figure C4.

[16]This is not driven by outliers. There are 51 districts out of a total of 122 with $\Pi_{lgp} > 0.975$, see figure C2.

Where $\mathbf{\Lambda_{lr}}$ represents respondent district fixed effects and $\mathbf{X_{i,lgp}}$ is a matrix of individual level covariates such as gender, age, risk aversion, raven score, and enumerator fixed-effects, all of which might influence observed attitudes.[17] It also captures characteristics of the grandparent location that might be correlated with Π_{lgp}, like suitability of each crop, a dummy for the best produceable crop and distance to the capital. The main identifying assumption required to interpret β_1 causally is that the only way $C_{i,lgp,lr} \cdot \tau_{lgp}$ matters for coffee farmers in non-coffee regions is through forced labour. I take two strategies to assess the validity of this assumption.

The first focuses on the non-linearity in the relationship between coffee returns and forced labour. If ethnic attitudes begin to decline just before $\Pi_{lgp} = 1$ for descendants of Hutu coffee farmers, and there is no difference to the left of this threshold, that lends credence to the idea that it had to be forced labour generating the difference. The logic is similar to a kink-design, although in this case I have neither the data nor the sharp boundary to estimate an actual kink.

The second strategy is to examine the Tutsi as a falsification group. I examine the attitudes of Tutsi with grandparents from a forced labour village, who would have likely been selected for forced labour given their agricultural activity, had they been Hutu. If there are strong selection effects into coffee production in a region where coffee is a secondary crop, then it may be reasonable to assume that these pressures exist similarly for both Hutu and Tutsi. Of course Tutsi and Hutu have different agricultural profiles, and may have faced differences in migration restrictions, so this strategy is limited to the extent that Hutu and Tutsi experienced different selection forces.

5. Main Results

5.A. Trust Game Results

Given that that mistreatment by the Tutsi towards the Hutu appears to have been most likely in regions where the coffee quota had the most bite, it seems reasonable to expect respondents with grandparents that grew coffee in these villages to exhibit the least inter-ethnic trust. This hypothesis is investigated in table 1 panel A, which presents evidence of differential trust game offers by the descendants of farmers believed most likely to have been exposed to forced

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[17]A figure showing the variation in each of gender, age risk and raven score appear in figure C5. There are differences, especially in gender and age near the $\Pi_{lgp} > 0.975$ but none are significant.

labour. Columns 1 and 2 of table 1 panel A suggest that forced labour Hutu make trust game offers that are about 20% lower than other Hutu who currently live in the same districts, but whose grandparents were likely not exposed to forced labour.[18] Columns 1 and 2 differ by the controls included. In column 2 we include income and education, but not in column 1 because education and income have been shown to be endogenous in other contexts.[19]

A more fundamental concern might regard the type of individual who grows coffee in regions that are less suitable for coffee in the first place, so I take a few approaches to deal with that.[20] First, consider the Tutsi who similarly grew some coffee in these same regions. Tutsi offers can be seen in panel B, columns 1 and 2. If anything, these Tutsi made *larger* offers than their non-coffee counterparts, though neither estimate is close to statistically ruling out zero.[21] The co-ethnic sample is an opportunity for a similar exercise. If we thought - independent of forced labour - that people who grew coffee in places where coffee was a secondary crop were more marginalized, then it might be natural to think of this as a general trust effect rather than as a specifically inter-ethnic trust effect. However, columns 3 and 4 of panels A and B show no evidence that co-ethnic trust has changed for either Hutu or Tutsi. However, the Tutsi-Tutsi estimate is a less conclusive null result, since the point estimate is large (although, positive) and the sample is small.

Another approach to investigating selection into treatment is to exploit the non-linearity between land characteristics and forced labour in figure 2. Similar to the Tutsi analysis, an investigation of the nature of the non-linear relationship between forced labour and trust suggests that the relationship is indeed causal (see appendix C.3 for details).

5.B. *Partner Preference Results*

One disadvantage of the trust game is I get an estimate only for the subset of respondents who played with a respondent from the other ethnic group. This

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[18] Appendix C.1 shows that trust game differences do not appear due to altruism or reciprocity. They also demonstrate that the results are not driven by differential trustworthiness.

[19] Bobonis and Morrow (2014), Lowes and Montero (2020a) show that forced labour causes lower education; Dell and Olken (2020), Lowes and Montero (2020a) show forced labour influences wealth. However, the balancing test in table C4 suggests that Hutu income and education are similar for those with and without an ancestral exposure to forced labour.

[20] The ethnicity proxy in Rwanda and the genocide in Rwanda may also be concerns. I examine results separately for Rwanda and Burundi (Appendix C.2), and show robustness of each result to the Burundi-only sample to rule-out Rwanda-specific concerns.

[21] Note that the inability to rule out zero could be due to power issues, as the Tutsi sample is very small. To check this I run a pooled-by-ethnicity version of the specification in table C5.

may not be a first order concern since partners were randomized, but since the partner preference measure is available for all Hutu, I can use that variable to both address the validity of the randomization, and show robustness to another measure of inter-ethnic attitudes. Panel A of table 2 shows forced labour Hutu are 13p.p. less likely to choose a Tutsi partner in the full Hutu sample (columns 1 and 2), and 18p.p. less likely in the inter-ethnic trust-game sample of individuals who were assigned to the inter-ethnic trust game instead of the co-ethnic trust game (columns 3 and 4).[22] The results highlight the robustness of the effect on attitudes to both different measures and samples.

6. Analysis of Related Hypotheses and Robustness

6.A. Insurance Agreements Results

Based solely on lab measures it is often difficult to assess magnitudes, because the context is artificial. To address this, I examine agricultural insurance agreements traditionally made between Hutu and Tutsi. I start by looking at the number of agreements made, to see if the effect on trust is large enough to have real world consequences. That said, it should be noted that there are a number of possible interpretations of the insurance results, especially given that these outcomes are not tied directly to insurance partners.

Table 3 panel A shows that forced labour Hutu make fewer agreements (column 1 and 3), but not Tutsi with grandparents who would have been exposed had they been Hutu (column 2 and 4). If we think of the inter-ethnic agreements as being more valuable, then the fact that forced labour Hutu make fewer agreements is consistent with their lower inter-ethnic trust. Insurance could be low either because individuals make fewer agreements, or because they are more likely to experience default. Accordingly I also look at default (panel B). Forced labour Hutu are more likely to experience default as well (columns 1 and 3), but observationally equivalent Tutsi are not (columns 2 and 4). Suggestive evidence implies that the increase in default is driven by a selection into less suitable Hutu-Hutu agreements rather than by Tutsi shirking on agreements with Hutu (Appendix C.7).

Again, this section is suggestive of real world implications in the sense that the results are consistent with possible within-ethnicity sorting. Without collecting

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[22]I also check for preference for other observable characteristics. However I find no evidence of differential preference on any other dimension (Appendix C.6). Other robustness results, like non-linearity (figure C6 panel b and c) and Tutsi table 2 panel B looks similar to the analogous estimates for the main trust results.

data on real world insurance partner ethnicity, it is perhaps not possible to pin this mechanism down entirely and rule out alternative explanations that may also be consistent.

6.B. Genocide

The genocide is clearly a related and very important issue, and there are a number of reasons why it has not featured more prominently in the analysis so far. Perhaps the most pressing is the way that the ethnicity proxy in Rwanda was collected. Because the ethnicity proxy in Rwanda measures whether individuals are 'genocide survivors', the sampling procedure in Rwanda targeted genocide regions to reduce the likelihood that any Tutsi would claim to be ineligible for the fund on the basis that their region did not experience genocide violence. The implication of this is that in the data I have only intensive margin variation in genocide, which could lead to imprecise, inconclusive estimates. Beyond that, it is not entirely clear that controlling for the genocide is a reasonable approach, given it is plausibly endogenous.

That said, I would be remiss to not discuss it. A more complete examination of the implication of the genocide appears in section C.4. To summarize those results, each of the main estimates are very similar in Rwanda-only and Burundi-only samples, suggesting that the genocide is not the main mechanism (table C8). Furthermore, controlling for the genocide does not meaningfully impact the results, and if anything the genocide is positively related to attitudes in Rwanda (tables C9 and C10).

Genocide prosecutions do, however, seem to have been more intense in forced labour regions by about 110 people per sector (p-value< 0.01). It seems reasonable to expect the genocide to potentially be more intense in regions with a history of ethnic exploitation, as authorities openly referenced historical Tutsi-led oppression to mobilize violence (Straus, 2013).

6.C. Migration

Migration could be another concern. If a substantial number of Hutu fled forced labour, then there may be concern that those who were unable to flee forced labour might be systematically different than the other (non-forced labour) Hutu in their regions. Colonial administrators were reportedly quite aware of the threat of flight, and responded with migration restrictions (352, 1925b,a) and punishments for fleeing (Newbury, 1978). However, both were inconsistently applied, and it is

very difficult to find clear and consistent formal rules or punishments regarding forced labour evasion (Butamire, 2012).

We can, however, check whether there was an exodus from forced labour regions in the data. In non-forced labour regions I expect individuals to migrate as usual, however likely not into forced labour regions. Migration restrictions for Hutu may have applied to (1) all Hutu; to (2) Hutu in forced labour regions; to (3) just forced labour Hutu; or (4) not enforced. The biggest concern would be differential migration by forced labour Hutu, which could result from either case (3) or (4). In either of those cases forced labour Hutu would plausibly be unobservably different from their comparison group: the other Hutu from those same places.

Table C6 examines whether coffee farmers whose families started in a forced labour region are more likely to now live elsewhere. We see in columns 1 and 3 that all Hutu from forced labour villages were much less likely to migrate, and there was no differential effect for forced labour Hutu. So, neither of the concerning cases seem to be true for Hutu. First, migration restrictions seem to have been enforced, essentially shutting down all Hutu migration out of forced labour villages. Second, the migration restrictions did not seem to have influenced forced labour Hutu differentially.

For Tutsi, the estimates warrant a bit more caution. Already, the Tutsi falsification exercise had been plagued by different selection pressures into coffee as a result of the differing agricultural profiles of Hutu and Tutsi. Additionally, table C6 columns 2 and 4 suggest that Tutsi may have faced less severe migration restrictions. As a result, Tutsi who would have been selected for forced labour had they been Hutu may have fled those regions differentially. This is consistent with Tutsi observing the forced labour imposed on the Hutu, being afraid that forced labour would expand to Tutsi (historically this had been discussed (Jefremovas, 2002)), and fleeing the worst areas while they still could. The results suggest that migration restrictions may have been more severe for Hutu, a conclusion that is not altogether surprising.

7. DISCUSSION

Divide-and-rule was a very prominent feature of colonialism (Morrock, 1973), and has been credited for countless civil strifes and development failures around the world. Notable cases include Hindu-Muslim tension in India, sectarian rivalry in Ireland, and Kikuyu-Luo conflict in Kenya. However, there has been little to no empirical evidence to document the long-run implications of this type of policy

on inter-group relations. Rwanda and Burundi have experienced both divisive colonial policies and extreme inter-group conflict. This paper has attempted to demonstrate that these may be causally related, and that even 50 years post-independence, the implications of divide-and-rule colonialism for inter-group relations continues to have consequences.

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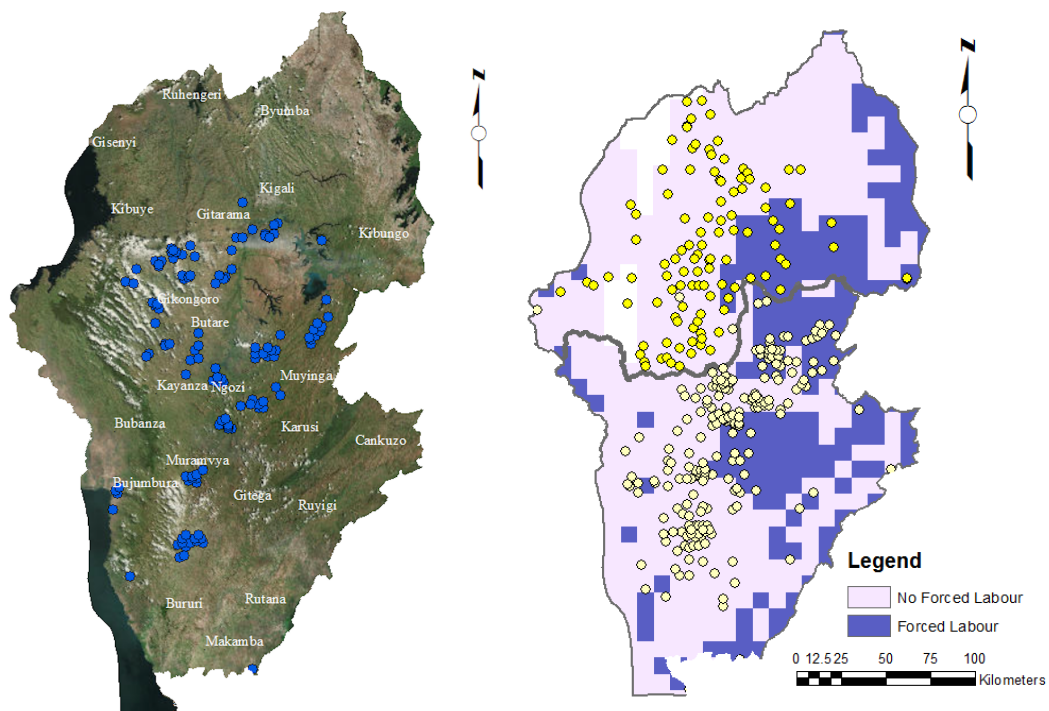
(a) Respondent Locations	(b) Ancestral Location Relative to Forced Labour

Figure 1: Maps of Relevant Respondent Locations

Notes: Panel (a): Dots represent the location where each respondent lives. Dots are clustered because individuals from 4-5 villages were brought to a district capital to be surveyed and play lab games in a data collection session. This was necessary to be able to ensure that respondents would be able to play against strangers in the one-shot trust game.

Panel (b): This map shows ancestral village and forced labour regions, defined as regions where coffee profitability is less than or equal to that of the most profitable non-coffee crop. It shows that respondents ancestors are scattered across both Rwanda and Burundi, and populate both forced labour regions (dark) and no forced labour regions (light).
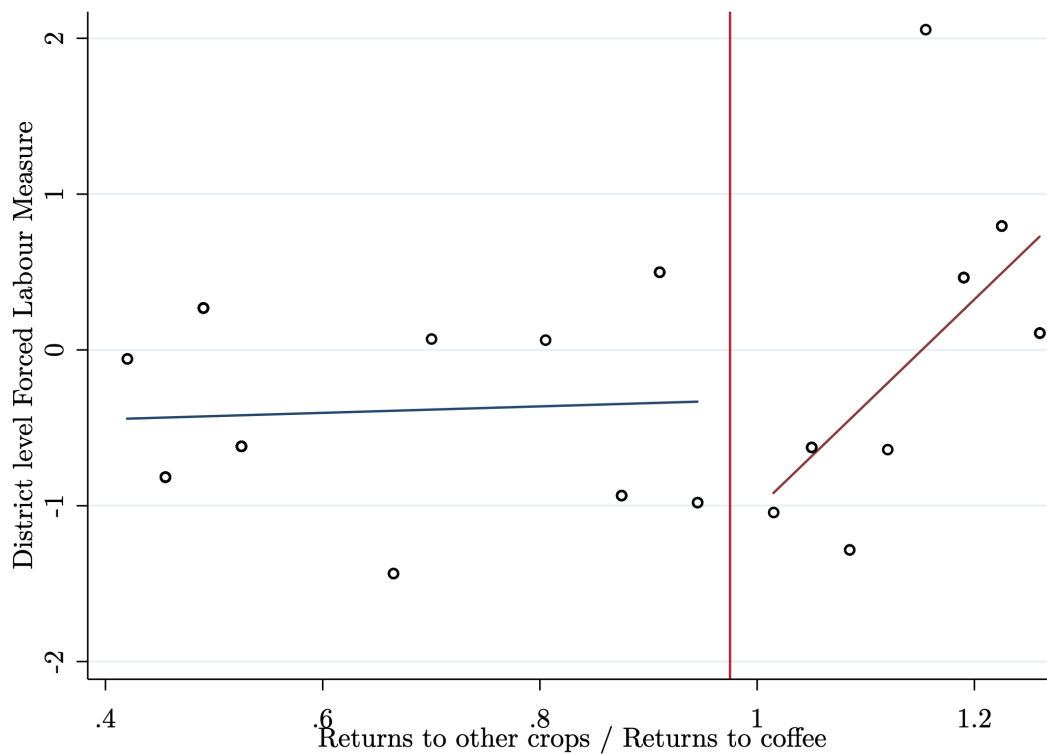
Figure 2: Kink in Forced Labour - Relative Coffee Returns Relationship

Notes: The figure plots the relationship between the returns to allocating land to coffee, which is defined as the quantity produceable times price of the best non-coffee crop divided by the same value for coffee. A value of 1.2 means that other crops return 120% the value of coffee, so coffee is not likely to be the most dominant crop in the region in the absence of quotas. A value of 0.6 means that the next best crop to coffee returns 60% of the value of coffee, so coffee is expected to be heavily produced in these regions. The y-axis is the first principal component of three measures capturing the share of colonial era french texts on Google Books about a district in Rwanda or Burundi that mention a particular phrase. In each each of the inputs to the principal component analysis investigated a different phrase: *Ikiboko*; *Prestations Coutumières Dues*; and *corvée*. The relationship is plotted using a linear best-fit line that is allowed to change on either side of a threshold of 0.975 on the x-axis. The figure plots 0.035 bins for all bins less than 1 as any bin reporting values of 1 contain one district without many books, and distorts the y-axis scale. As this is just to avoid a distortion of the axis, these observations remain in the broader analysis, and are not omitted elsewhere unless explicitly noted. To ensure outliers are not driving the effect, the data presented in the figure are winsorized at the 5% level.

Table 1: Effect of Forced Labour on Trust

	(1)	(2)	(3)	(4)
	Panel A: Hutu decisions			
Dependent Variable:	asinh(Trust Game Offer)			
Sample:	Hutu - Tutsi		Hutu - Hutu	
Grandparent farmed coffee where quotas are thought to be binding ($C_{i,l^{gp},lr} \cdot \tau_{l^{gp}}$)	-0.227	-0.228	-0.0486	-0.0376
	(0.106)	(0.108)	(0.0865)	(0.0799)
Grandparent from a village where quotas are thought to be binding ($\tau_{l^{gp}}$)	0.188	0.195	0.0791	0.0820
	(0.170)	(0.170)	(0.107)	(0.0948)
Grandparent farmed coffee ($C_{i,l^{gp},lr}$)	0.00461	0.0100	0.0587	0.0401
	(0.0554)	(0.0637)	(0.0307)	(0.0317)
Mean of dependent variable	6.21	6.21	6.35	6.35
Cluster 1: number of grandparent villages	64	64	64	64
Cluster 2: number of respondent villages	77	77	56	56
N	258	258	361	361
R^2	0.375	0.376	0.271	0.294
	Panel B: Tutsi decisions			
Dependent Variable:	asinh(Trust Game Offer)			
Sample:	Tutsi - Hutu		Tutsi - Tutsi	
Grandparent farmed coffee where quotas are thought to be binding ($C_{i,l^{gp},lr} \cdot \tau_{l^{gp}}$)	0.169	0.171	0.221	0.179
	(0.165)	(0.166)	(0.292)	(0.283)
Grandparent from a village where quotas are thought to be binding ($\tau_{l^{gp}}$)	-0.156	-0.153	-0.591	-0.569
	(0.159)	(0.152)	(0.264)	(0.258)
Grandparent farmed coffee ($C_{i,l^{gp},lr}$)	-0.0721	-0.0815	0.170	0.215
	(0.118)	(0.124)	(0.133)	(0.130)
Mean of dependent variable	6.32	6.32	6.18	6.18
Cluster 1: number of grandparent villages	42	42	41	41
Cluster 2: number of respondent villages	32	32	47	47
N	128	128	121	121
R^2	0.793	0.794	0.707	0.711
Respondent district FE	Yes	Yes	Yes	Yes
Grandparent village controls	Yes	Yes	Yes	Yes
Enumerator FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Education and Income	No	Yes	No	Yes

Notes: Standard errors are two-way clustered at the respondent sector and grandparent district levels. The dependent variable in all columns is the inverse hyperbolic sine transformation of the trust game offer. I denote $asinh(x)$ as inverse hyperbolic sine transformation of x throughout. Respondent district fixed effects are included in each regression. Grandparent village controls included are the suitability for each crop, and an indicator for the crop in the village with the highest return. Distance to the capital is also included in the 'grandparent village controls'. Enumerator fixed effects are included in each specification. Individual controls included throughout include gender, age, score on a raven IQ test, and the response to a survey question on risk preference (hypothetical, not incentivized). In columns 2 and 4 we also include years of education and self-reported income. Samples are split according to own ethnicity and partner ethnicity in the trust game. All ethnicity data is either self-reported (Burundi) or based on self-reported eligibility for a genocide survivors fund (Rwanda), as described in the text.

Table 2: Effect of Forced Labour on Partner Ethnicity Preference

	(1)	(2)	(3)	(4)
	Panel A: Hutu decisions			
Dependent Variable:	Preference for inter-ethnic partnership			
Sample:	Full Sample		Inter-ethnic trust-game sample	
Grandparent farmed coffee where quotas are thought to be binding ($C_{i,l^{gp},l^r} \cdot \tau_{l^{gp}}$)	-0.136 (0.0463)	-0.142 (0.0449)	-0.176 (0.0995)	-0.176 (0.0955)
Grandparent from a village where quotas are thought to be binding ($\tau_{l^{gp}}$)	-0.0416 (0.0393)	-0.0365 (0.0392)	0.0172 (0.0879)	0.0140 (0.0884)
Grandparent farmed coffee (C_{i,l^{gp},l^r})	0.0749 (0.0408)	0.0846 (0.0402)	0.0907 (0.0697)	0.118 (0.0678)
Mean of dependent variable	0.41	0.41	0.48	0.48
Cluster 1: number of grandparent villages	97	97	64	64
Cluster 2: number of respondent villages	89	89	77	77
N	619	619	258	258
R^2	0.336	0.339	0.333	0.338
	Panel B: Tutsi decisions			
Dependent Variable:	Preference for inter-ethnic partnership			
Sample:	Full Sample		Inter-ethnic trust-game sample	
Grandparent farmed coffee where quotas are thought to be binding ($C_{i,l^{gp},l^r} \cdot \tau_{l^{gp}}$)	-0.0161 (0.0460)	-0.00786 (0.0462)	0.0853 (0.0728)	0.0846 (0.0705)
Grandparent from a village where quotas are thought to be binding ($\tau_{l^{gp}}$)	-0.0473 (0.0630)	-0.0419 (0.0689)	0.0271 (0.115)	0.0356 (0.114)
Grandparent farmed coffee (C_{i,l^{gp},l^r})	0.00669 (0.0382)	0.00358 (0.0383)	-0.0822 (0.0738)	-0.0858 (0.0709)
Mean of dependent variable	0.62	0.62	0.57	0.57
Cluster 1: number of grandparent villages	61	61	42	42
Cluster 2: number of respondent villages	57	57	32	32
N	249	249	128	128
R^2	0.413	0.425	0.508	0.510
Respondent district FE	Yes	Yes	Yes	Yes
Grandparent village controls	Yes	Yes	Yes	Yes
Enumerator FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Education and Income	No	Yes	No	Yes

Notes: Standard errors are two-way clustered at the respondent sector and grandparent district levels. The dependent variable in all columns is the share of partner choices made that are not of the respondent's ethnicity. Respondent district fixed effects are included in each regression. Grandparent village controls included are the suitability for each crop, and an indicator for the crop in the village with the highest return. Distance to the capital is also included in columns 2 and 4. Enumerator fixed effects are included in each specification. Individual controls included throughout include gender, age, score on a raven IQ test, and the response to a survey question on risk preference (hypothetical, not incentivized). In columns 2 and 4 we also include years of education and self-reported income. Samples are split according to own ethnicity and partner ethnicity in the trust game. All ethnicity data is either self-reported (Burundi) or based on self-reported eligibility for a genocide survivors fund (Rwanda), as described in the text.

Table 3: Effect of Forced Labour on Insurance Agreements

	(1)	(2)	(3)	(4)
Sample:	Hutu	Tutsi	Hutu	Tutsi
	Panel A: Number of Agreements			
Grandparent farmed coffee where quotas are thought to be binding ($C_{i,l^{gp},l^r} \cdot \tau_{l^{gp}}$)	-3.884	1.856	-3.983	2.611
	(1.524)	(2.056)	(1.544)	(2.348)
Grandparent from a village where quotas are thought to be binding ($\tau_{l^{gp}}$)	-0.470	-2.312	-0.492	-2.470
	(0.671)	(1.849)	(0.666)	(1.843)
Grandparent farmed coffee (C_{i,l^{gp},l^r})	1.859	-1.930	2.022	-2.189
	(0.717)	(1.779)	(0.695)	(1.876)
Mean of dependent variable	2.92	3.85	2.92	3.85
Cluster 1: number of grandparent villages	97	61	97	61
Cluster 2: number of respondent villages	89	57	89	57
N	619	249	619	249
R^2	0.147	0.344	0.149	0.349
	Panel B: Experienced default			
	(among those making any agreements)			
Grandparent farmed coffee where quotas are thought to be binding ($C_{i,l^{gp},l^r} \cdot \tau_{l^{gp}}$)	0.209	-0.00835	0.222	-0.0110
	(0.0890)	(0.118)	(0.0912)	(0.125)
Grandparent from a village where quotas are thought to be binding ($\tau_{l^{gp}}$)	0.0666	0.445	0.0704	0.392
	(0.107)	(0.206)	(0.103)	(0.199)
Grandparent farmed coffee (C_{i,l^{gp},l^r})	-0.0376	-0.124	-0.0512	-0.0977
	(0.0686)	(0.0935)	(0.0747)	(0.0883)
Mean of dependent variable	0.61	0.52	0.61	0.21
Cluster 1: number of grandparent villages	75	48	75	48
Cluster 2: number of respondent villages	72	44	72	44
N	408	154	408	154
R^2	0.255	0.487	0.258	0.508
Respondent district FE	Yes	Yes	Yes	Yes
Grandparent village controls	Yes	Yes	Yes	Yes
Enumerator FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Education and Income	No	No	Yes	Yes

Notes: Standard errors are two-way clustered at the respondent sector and grandparent district levels. The dependent variable is the number of insurance agreements in panel A, and in panel B a binary variable for whether the respondent experienced default. Respondent district fixed effects are included in each regression. Grandparent village controls included are the suitability for each crop, and an indicator for the crop in the village with the highest return. Distance to the capital is also included in columns 2 and 4. Enumerator fixed effects are included in each specification. Individual controls included throughout include gender, age, score on a raven IQ test, and the response to a survey question on risk preference (hypothetical, not incentivized). In columns 2 and 4 we also include years of education and self-reported income. Samples are split according to own ethnicity. All ethnicity data is either self-reported (Burundi) or based on self-reported eligibility for a genocide survivors fund (Rwanda), as described in the text.