The Long-Term Effects of the Rwandan Genocide on Child Work

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Abstract

We estimate the long-term effect of the Rwandan genocide on the work of the children born after the genocide, using variation in genocide intensity across communes and child work incidence in the 2010 Demographic Health Survey. We instrument for the number of killings at the commune with its distance to the Ugandan border. We find evidence for a lingering effect of genocide. The genocide causes an increase in work and a decrease in school attendance of the children born after the genocide. Doubling the number of killings per capita increases the probability of a child working for someone who is not a household member by 3.35 percentage points, while decreasing the probability of a child attending school by 3.68 percentage points. Our empirical results suggest a long-term impact of the genocide on human capital development that is likely to contribute to Rwanda's underdevelopment in the future.

Keywords: Conflict, Genocide, Child Work, Schooling, Intergenerational Effect

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1. Introduction

The 1994 Rwandan genocide was a dark period in history. In just about three months, an estimated 800,000 people were systematically killed, about 20 percent of the population of Rwanda and 70 percent of the Tutsi population. HIV infection resulted from the pervasive rape and many households were left with single mothers. The larger civil conflict between the Hutu majority and the Tutsis resulted in over 2 million displaced refugees. More than two decades after this tragedy, the long-term effects on health, education are still felt in Rwanda.

In this study, we focus on one particular long-term consequence of the genocide: child labor. We estimate the effect of the Rwandan genocide on child work using the 2010 Rwanda Demographic Health Survey (RDHS). With geocodes from the RDHS, we are able to match clusters to pre-genocide communes which allows us to match estimates on the number of killings from a retrospective survey on genocide victims.[1] To address the potential endogeneity arising from other commune-level factors that could be associated with both the intensity of the genocide and the incidence of child work, we use distance from the commune to the Ugandan border as an instrument for genocide intensity. The motivation for this instrument comes from a historical detail of the genocide. The genocide was part of the bigger civil conflict between the Hutu majority government and the minority Tutsi-led Rwandan Patriotic Front (RPF). The RPF launched an offensive from its exile base to the north in Uganda in April of 1994, marching southward into Rwanda, finally toppling the Hutu government in July. The three-month span resulted in a pattern where the genocide lasted longer the further one got from the Ugandan border. Distance thus generated an exogenous variation in the number of killings which we can use to estimate the causal

[1] The report "The Counting of Genocide Victims" was developed by the Ministry of Local Administration and Department of Information and Social Affairs. The survey was conducted from 2000 to 2002. Details of the survey can be found at https://genodynamics.weebly.com/data-on-violence.html.

impact of killing intensity on child work. We also use household demographics and pre-genocide commune characteristics to control for other confounding factors.

We find that the genocide caused a positive and statistically significant impact on child work. Doubling the number of killings in a commune increases the probability of a child working outside the home fifteen years later by 3.35 percentage points, about a 40 percent increase. Consistent with this result, school attendance falls with the increase in genocide intensity. We find that doubling the number of killings per capita decreases children's school attendance by 3.68 percentage points, about a 5 percent reduction.

There are a number of possible mechanisms consistent with our results. First, the brutality and targeted nature of the genocide destroyed social institutions that might have led to subsequent state dysfunction, corruption and instability. This is likely to depress productive investment and economic exchange which leads to underperforming local economies. The resulting lower incomes may have forced families to send children into the workforce (see Basu and Van 1998 and Edmonds and Pavcnik 2005). We find evidence of lower levels of household wealth, urbanization, economic performance, and infrastructure in communes with a higher killings intensity fifteen years previous. Second, returns to education in such low-performing economies are likely to be low which reduce incentives for investments in human capital and thus increase incentives for children to work. Third, the genocide significantly reduced the ratio of men to women in Rwanda (see La Mattina 2012 and de Walque and Verwimp 2010). In particular, more educated men were disproportionately killed. This creates a lower return to education to both women and men in the marriage matching market and would contribute to less incentive for education.

Our paper contributes to the small but growing literature on the economic effects of the Rwandan genocide. The existing literature covers a variety of issues such as the determinants of

the intensity of the genocide (Yanagizawa-Drott 2014; Friedman 2010), the effects on household welfare (Verpoorten and Berlage 2007), child mortality (Ciani and Giannelli 2011), schooling of children, (Akresh and de Walque 2008) and domestic violence (La Mattina, 2012). None of the existing studies, however, address the effects of the genocide on child labor.

More importantly, our findings illuminate how persistent and far reaching an adverse effect of an armed conflict can be. Some of the existing microeconomic studies document a long-lasting harm of armed conflicts on the people who were exposed to them (Akbulut-Yuksel 2014; Kesternich, Siflinger, Smith and Winter 2014; Leon 2012). But our study goes one step further by identifying the effects on the children who were born after the outbreak of the conflict.

We also contribute to a large literature on child labor. The idea underlying our analysis that child labor can result from low incomes and low productivity is motivated by the theoretical model found by Basu and Van (1998). Empirical studies confirm that poverty and negative income shocks are important determinants of child labor (Amin, Quayes, and Rives 2004; Beegle, Dehejia, and Gatti 2006; Duryea et al. 2007; Edmonds and Pavcnik 2005; Guarcello, Mealli and Rosati 2010; Ray 2010; Skoufias and Parker 2006). Our results support the existence of a trade-off between child work and school attendance consistent with studies that find a negative impact of child labor on education attainment (Psacharopoulos 1997; Ray 2010). The resulting effects of child labor on human capital accumulation have been studied by others: in Baland and Robinson's (2000) model of child labor, in Emerson, Ponczek, and Souza's (2017) finding that child work negatively affects test scores and in Beegle, Dehejia, and Gatti (2009) where child labor increases the probability of adults doing wage work. We expand the existing literature by identifying armed conflict as a determinant of child labor. Furthermore, taken together with the studies that link child

labor, human capital and adult labor outcomes, our study suggests that the negative effects of the genocide are likely to persist for many generations.

The paper proceeds as follows. Section 2 provides a brief review of the literature on the economic impact of civil conflicts. Section 3 discusses the historical background of our study. Section 4 explains our empirical methodology and data. Section 5 presents our results. Section 6 discusses mechanisms of our results and provides robustness checks. And section 7 concludes.

2. Literature Review

During the last half of the twentieth century, the number of countries that experienced civil conflicts has steadily increased. In the mid-1990s, for example, one-third of the countries in sub-Saharan Africa had armed conflicts. Despite its prevalence, persistence and critical role in economic development, armed conflict had long been a peripheral issue, and only recently has it become a part of mainstream interests in development economics (Blattman and Miguel 2010). A growing interest in the consequences of wars and armed conflicts led to a multitude of research, but results are still inconclusive.

Studies based on macroeconomic perspectives generally find that the devastating effects of wars and armed conflicts are temporary and quickly disappear over time. Economic growth is resilient to a large-scale shock (Brakman, Garretsen and Schramm 2004; David and Weinstein 2002; Miguel and Roland 2011), and a country tends to recover quickly in the aftermath of wars as long as the end of the war intersects with the beginning of a lasting peace (Chen, Loayza and Reynal-Querol 2008).

Microeconomic studies, on the other hand, find a direct negative effect on health and education of the exposed in the short run. Most of these studies use a difference-in-differences

estimation method by combining geographic variations in timing and intensity of conflicts with differential exposure to conflicts by birth cohort. A study on the Rwandan genocide finds that children who were exposed to armed conflicts have lower height for age Z scores than non-exposed children (Akresh, Verwimp and Bundervoet 2007). The same finding was made in studies on the Eritrean-Ethiopian conflict (Akresh, Lucchetti and Thirumurthy 2012) and a civil war in Burundi (Akresh, Verwimp and Bundervoet 2009) as well as in Cote d'Ivoire (Minoiu and Shemyakina 2014). Using the same identification strategy, others find that exposure to armed conflicts leads to lower years of schooling in Rwanda (Akresh and de Walque 2008; Guariso and Verpoorten 2015) and Guatemala (Chamarbagwala and Morán 2011), or a lower likelihood of school completion in Tajikistan (Shemyakina 2011) and Burundi (Verwimp and Bavel 2014).

Unlike the macroeconomic studies, numerous microeconomic studies find that a loss in human capital at an early age is irreversible and its detrimental effect persists into adulthood. Leon (2012) finds that children who were exposed to political violence in Peru could not catch up with early loss in schooling and ended up having lower years of schooling when reaching adulthood. Kesternich, Siflinger, Smith and Winter (2014) find that World War II adversely affected late-life socioeconomic status and health outcomes of the elderly who had experienced the war when young. Similarly, Akbulut-Yuksel (2014) finds that the damaging effects of World War II on health, education, and labor market outcomes of those who had experienced the war persisted even 40 years after the war.

In most of these existing microeconomic studies, the focus lies on the effects on schooling and health, and child labor has largely been overlooked, although child labor can be directly affected by conflicts and is highly correlated with quantity and quality of human capital.[2]

[2] We find only two papers that consider the effects of armed conflicts on child labor. Rodriguez and Sanchez (2012) find that the armed conflicts in Columba make children drop out of school and enter the labor market at an early age.

Moreover, there are few studies on the intergenerational effect of armed conflicts on human capital outcomes of the next generation.[3] In this study, we attempt to fill the gap.

3. Background

The Rwandan genocide originated from deep-rooted antagonism between the majority Hutu and the minority Tutsi in Rwanda. Tutsis, while comprising only 17% of the population, had politically dominated the country before and during the colonial era. There is disagreement among historians whether their relationship prior to the colonial era was hostile or collaborative. But the consensus is that, a discriminatory two-tier system of the elite Tutsi and the second-class Hutu imposed by the Europeans during the colonial period generated or intensified hostility between the two (Kuperman 2001). Against the backdrop of Pan-Africanism and the decolonization movement in Africa in the mid-50s, the majority Hutu successfully mobilized a political movement claiming emancipation from Tutsi domination. After numerous outbreaks of large-scale violence between the two ethnic groups in the late-50s and the early-60s, political power was transferred to the majority Hutu through elections along with a referendum, ending Tutsi monarchy and establishing a Hutu-dominated republic in 1961. The country obtained independence from Belgium in 1962. The political regime change begot even more conflicts. Tutsi refugees in neighboring countries formed armed groups and launched a series of attacks to regain their political power, while the Hutu government oppressed and attacked the remaining Tutsis, causing more Tutsi refugees in

Di Maio and Nandi (2013) find that Israeli-Palestinian conflicts increase child labor and decrease school attendance in the West Bank.

[3] Eder (2014) finds that displaced parents during the Bosnian war invest less in the education of their children years after the war than non-displaced parents. While his study attempts to confirm an intergenerational effect, the outcome variable is essentially a change in parents' investment decisions, not a change in children's actual educational attainment. They acknowledge that a short time lapse between the end of the war and the data collection does not allow them to directly investigate children's educational outcomes. Caruso (2015) investigates the intergenerational effect of shocks on education and child labor but focuses on natural disasters rather than armed conflicts.

neighboring countries. By the end of the 1960s, Tutsis represented only 9% of the Rwandan population (Kuperman 2001).

In 1990, the Rwandan Patriotic Front (RPF), the armed Tutsi rebels, invaded northern Rwanda from Uganda and started the Rwandan civil war. After persistent offensives by the RPF and massacres of Tutsis by the Hutu, the RPF and the Hutu government entered negotiations and finally signed a peace agreement in 1993. However, after the assassination of the Hutu president Habyarimana in early April 1994, Hutu extremists effectively took over the government and commenced ethnic cleansing of Tutsis within hours. The RPF, which had been constrained to the small area of northern Rwanda according to the peace agreement, restarted attacks advancing through Rwanda. The RPF chose a circuitous route by moving first east then surrounding Kigali clockwise, and finally reaching the southwest (see Kuperman (2001), page 43). This battle plan was helpful for effectively neutralizing dispersed Rwandan army units in the east, while avoiding the route between the two strongholds of the Rwandan army in Kigali and Ruhengeri, which led to an eventual victory for the Tutsi rebels (Kuperman 2004). The RPF overturned the Hutu government in July 1994.

4. Empirical Method

4.1. Definition

Results of child labor studies crucially depend on the way a child is defined, the way work is defined, and the way survey questions are asked (Basu and Van 1998; Dayıoğlu 2012). Following numerous existing studies that define child labor based on the ILO's Convention No. 138, we define a child as a person between ages 5 and 14 (Basu 1999). Further, a working child is defined as a child who did any kind of work for someone who is not a household member in the last seven

days of the survey.[4] Later, we expand this definition of child work to include activities that are performed for the household but readily recognized as work. The modified definition comes from UNICEF: a child worker is any child aged between 5 and 11 who worked for someone who was not a member of the household in the past week, or did household chores for 28 or more hours, or engaged in any family business. For children aged between 12 and 14, a child worker is any child who worked for someone who was not a member of the household for more than 14 hours in the past week, or did household chores for 28 or more hours, or engaged in any other family work for more than 14 hours (NISR et al., 2012).

4.2. Data

Our main objective is to identify the effect of the Rwandan genocide on human capital accumulation of the next generation, measured by child work. Our data on child work come from the Rwanda Demographic and Health Survey (RDHS) 2010. There are five rounds of RDHS available (1992, 2000, 2005, 2010, and 2015) and RDHS 2010 is the only survey that deals with child work. Relevant questions in the survey include: whether a child worked for someone who is not a member of the household during the last seven days and the number of hours if so; whether a child did fetch water or collect firewood for household use and the number of hours if so; whether a child did other family work on the farm or in a business and the number of hours if so; whether a child did household chores and the number of hours if so. There are 12,540 households and each resides in one of 492 clusters. The survey provides geocodes (latitude and longitude) for each cluster which we use to match a cluster with a pre-genocide commune. Using a shape file of

[4] In our study, we mainly use the term 'child work' instead of 'child labor' to avoid a negative connotation associated with the term child labor. While we acknowledge that child work could potentially harm children's human capital accumulation (especially when it is coupled with lower school attendance), we do not argue that all the work done by children is detrimental or associated with abuse.

Rwandan communes prior to the genocide, we identified 142 communes. Three of these communes did not have data on the number of victims, and 2 communes did not contain any clusters, leaving 137 communes in the final analysis.

We use the number of deaths per capita during the genocide as our measure of the commune's intensity of genocide. Data on the number of deaths are obtained from a retrospective survey by the Ministry of Local Administration and Department of Information and Social Affairs in Rwanda. The objective of the survey was to identify the names and the number of victims of the genocide. Victims were people identified and declared dead by survivors who were interviewed over a period between July 2000 to November 2002.

We further use auxiliary data sets for additional variables and analyses. Pre-genocide socio-economic characteristics of communes come from the Rwanda 1991 census. Child work variables for a falsification analysis also comes from the 1991 census. Children's school attendance in 2005 is obtained from RDHS 2005. For a robustness check, we use data from Rwanda National Child Labor Survey (RNCL) 2008.

Table 1 shows summary statistics for variables used in our study. In our main sample of approximately 15,000 observations in RDHS 2010, about 8% of children aged 5-14 worked outside of their homes in the past week. When we use the modified definition of child work, about 6% of the children do so. The mean number of genocide killings per capita in commune is about 0.12, which is -2.14 in natural log.

4.3. Estimation Equation

The primary regression equation is defined by:

$$y_{ic} = \delta + \beta G_C + \alpha X_{ic} + \gamma \Pi_c + \varepsilon_{ic} \qquad (1)$$

where y_{ic} is a dummy that indicates whether a child i in commune c worked for someone who is not a household member in the past week, G_c is genocide intensity measured by log number of killings per capita in commune c during the genocide, X_{ic} is a vector of individual characteristics including age, dummy variables for gender, whether or not father alive, mother alive, urban residence, family size, number of children under five, education of household head in single years, and household wealth index.[5] Π_c is a vector of commune characteristics prior to the genocide, including percentage of Tutsi (the number of Tutsi/(the number of Tutsi + the number of Hutu)), percentage of individuals with radio at home, average grade completed of adults, percentage of adults employed, and percentage of Hutu being employer or employed. Standard errors are clustered at the commune level. Additional outcome variables include school attendance of children. The same equation and control variables are used for the additional analysis.

4.4. Identification Strategy

There are several challenges to estimating genocide's effect on child work. First, as explained in the previous section, our measure of genocide intensity comes from a retrospective survey. This measure of genocide has shortcomings the chief among which is the likely reluctance of those interviewed to give honest answers for fear of repercussions. This counting of victims occurred around the time of the post-genocide Gacaca tribunals that attempted to find justice for the victims and their families. Witnesses might not answer the interviews truthfully since they could perceive possibly affecting the Gacaca trials. It is unclear, however, whether the responses would bias the count high or low as a result of these concurrent trials. Interviewees could also fear retribution from genocide perpetrators or sympathizers. Those interviewed could also merely have forgotten

[5] This wealth index is computed using a principal component analysis with values following a standard normal distribution of mean zero and standard deviation one.

the details given the number of years that had passed since the genocide. These could be classical measurement errors which cause attenuation bias.

Second, there might be factors associated with a commune that make it more likely to experience more killings during the genocide, which, at the same time, cause child work to occur. Commune level education and employment, for example, are found to be associated with participation in violence during the genocide (Friedman 2010), and they are also determinants of labor market outcomes. To address this issue, we include in our regressions a number of pre-genocide commune level socio-economic aggregates defined above (Π_c) from the Rwanda 1991 census. However, there could still be commune-level factors unobserved in the data that are associated with both the intensity of the genocide and the incidence of child work.

We address measurement errors and endogeneity with an instrumental variable approach. Following Akresh and de Walque (2008), our instrumental variable is based on the details of the military response of the Tutsi rebels during the genocide explained in Section 3.[6] In a nutshell, given the initial location and the moving path of the rebels, communes close to the Ugandan border were better protected and experienced less killings, while communes farther away from the Rwandan border experienced a higher number of killings. We exploit this episode of history by using the commune's distance to the Uganda border as an instrument for the number of killings. This instrumental variable allows us to use only the variation in genocide intensity generated by physical distance in estimating the genocide's causal effect on child work.

The first-stage regression that reveals the relationship between the distance and the number of killings is defined by:

$$G_c = \sigma + \theta d_C + \rho X_i + \mu \Pi_c + \omega_c \qquad (2)$$

[6] While Akresh and de Walque (2008) use the distance from provinces to the Ugandan border, we use the distance from communes to the border, generating more variation.

where d_c is the distance between a commune and the Ugandan border. [7] In using the distance as an instrument, the identifying assumption is that the distance has no impact on child work of the next generation, other than through the genocide itself. We do falsification exercises to rule out the possibility that other factors could be correlated with this distance variable that could in turn be correlated with child work.

4.5. First-stage

In Table 2, we report the first stage regressions of genocide intensity as measured by log number of killings per capita on the distance to the Ugandan border (equation 2). Columns 1 through 3 report the estimated coefficient of the distance, progressively controlling additional variables . Estimates on distance are all positive and significant at the one percent level. The result is in line with the fact that the genocide was more intense the farther a commune was from the Ugandan border. The F-statistic exceeds 40 when distance is the only control (column 1) or individual demographic variables are additionally controlled (column 2). The F-statistic, however, becomes smaller and falls short of the rule of thumb threshold for a weak instrument (F>=10), once the pre-genocide commune variables are controlled (column 3). Therefore, although a just-identified two-stage least squares estimator is approximately unbiased (Angrist and Pischke 2009), we augment our result with a weak-instrument robust inference by Anderson-Rubin confidence interval in our second-stage main regressions.

4.6. Exclusion Restriction

[7] It was a shortest distance between a commune and the border computed using the nearest neighbor analysis plugin of QGIS.

To check the exclusion restriction of our identification strategy, we also regress a pre-genocide child work indicator against distance to the Ugandan border as a falsification exercise in Table 2. If distance is correlated with pre-genocide child work, this pre-existing correlation would be evidence of a violation of the exclusion restriction. Pre-genocide child work comes from the Rwanda 1991 census and is a dummy that indicates whether a child worked or looked for first job most of the time in the reference week.[8] The estimation results are reported in columns 4 through 6 in Table 2, progressively controlling additional variables. In all the specifications, estimates of the coefficient of the distance variable are virtually zero and statistically insignificant, suggesting that there was no pre-existing correlation between distance and child work.

5. Results

5.1. Main Results

Table 3 shows results from ordinary least squares and instrumental variable regressions of child work (equation 1) using the main sample. Columns 1 through 3 report OLS regression results, progressively controlling additional variables (genocide intensity, individual characteristics, pre-genocide commune characteristics). The estimates are statistically significant when only the number of killings or the number of killings and individual characteristics are controlled at the 10 percent and 5 percent level, respectively (columns 1 and 2). Once pre-genocide commune characteristics are controlled, the estimate becomes fairly small and statistically insignificant. Doubling the number of killings increases the probability of a child working for someone outside the home by 0.02 percentage point and the effect is insignificant (column 3).

[8] The census question covers only the children at least 10 years old, while our data cover children aged between 5 and 14. For comparison, therefore, we also estimate the effect of genocide on the likelihood of child work using a restricted sample of children aged 10 and 14 in our main regressions in Table 3.

Once instrumented, the effect of genocide becomes larger. Columns 4 through 6 report the instrumental variable estimates of the effect of genocide intensity on child work, progressively controlling additional variables. Estimates are positive and statistically significant in all three specifications. When genocide intensity is the only control, doubling the number of killings per capita increases the likelihood of child work by 1.89 percentage point (column 4). When all the additional variables are controlled, doubling the number of killings increases the likelihood of a child working for someone outside the home by 3.35 percentage point (column 6), which is about a 40 percent increase. Since the first-stage F-statistic becomes smaller than 10 in this specification, we report the Anderson-Rubin confidence interval, which is robust to a weak instrument. The lower limit is a 1.3 percentage point increase and the upper limit is 9.1 percentage point increase. The interval is wide but certainly suggests a positive effect.

Column 7 reports the instrumental variable estimate of the genocide effect when child work is redefined based on UNICEF definition of child worker. The effect becomes somewhat smaller but still positive and statistically significant at the 5 percent level. The reduction of the effect is reasonable given that the modified definition applies a stronger threshold for child worker especially for older children. Column 8 reports the result using a restricted sample of children aged between 10 and 14 only, to facilitate the comparison of our result with the falsification exercise. The effect of genocide becomes larger and is still statistically significant for this group of older children. On the other hand, when the falsification sample is used, the effect of genocide on the likelihood of pre-genocide child work is largely insignificant, as expected (column 9).

5.2. Effects on School Attendance

We report in Tables 4 the effect of genocide on school attendance of the children residing in a commune with high genocide intensity.[9] While child work is available only in RDHS 2010, data on school attendance is available in other survey rounds. We use RDHS 2005 in addition to RDHS 2010, as a robustness check. Columns 1 and 3 report results using RDHS 2005. While OLS results are small and largely insignificant, IV estimation provides a significant negative effect at the 10 percent level. Again, we provide a week instrument robust inference by the Anderson-Rubin confidence interval. Columns 2 and 4 report results using the main sample of RDHS 2010. Both OLS and IV results report significant negative effects at the 10 percent level, but the IV estimates are larger in magnitude. Doubling the number of killings per capita decreases children's school attendance by 3.68 percentage point, which is approximately a 5 percent reduction, about fifteen years after the genocide. Comparing the IV results using 2005 and 2010 surveys, we find that the effect in 2010 is smaller in absolute value, which potentially suggests that the genocide effect tapers over time.

5.3. Heterogeneous Effects

Table 5 reports heterogeneous effects of the genocide by age and gender. We run regressions using all groups, while letting separate coefficients estimated on the interaction between a group dummy and genocide intensity. This approach is the same as running the regressions separately for each group assuming that the coefficients on the other controls are the same (Oster 2012).[10] The estimation equation is defined by:

[9] We find that children in the southern region were less likely to be students before the genocide. We address this potential source of a violation of the exclusion restriction by controlling the percentage of students in pre-genocide communes in the first-stage regression.

[10] As is noted by Oster (2012), we do not need to include the main effect of genocide intensity in this set-up, because this is merely to allow the effect of genocide to vary across groups.

$$y_{igc} = \delta + \beta(I_{gc} \times G_C) + \alpha X_{igc} + \gamma \Pi_{gc} + \varepsilon_{igc} \quad (3)$$

where I_{gc} is an indicator for group g in commune c.

Overall, there is no heterogeneity in the effect of genocide on child work across different age groups (column 1). Likewise, we do not see a difference in the effect of genocide on child work by gender (column 2). While the negative effect is slightly bigger for boys, the difference is not statistically significant. On the other hand, the effect on school attendance varies across age groups. Interestingly, youngest and oldest children (five-year old and fourteen-year old) become more likely to attend school as the genocide intensity increases. For the other age groups, the effect is negative, and it becomes progressively larger in magnitude as they age, hitting the lowest probability of attendance among the eight-year old, then it bounces back and becomes smaller in magnitude, generating a U-shaped curve (column 3). But there is no gender difference in the effect of genocide on school attendance (column 4).

6. Channels and Robustness Check

6.1. Poverty

It is well established that poverty or low income is a major driving force of child labor (Amin, Quayes, and Rives 2004; Bass 2004; Basu and Van 1998; IPEC and NISR 2009). For impoverished households, childhood without work is a luxury good (Amin, Quayes, and Rives 2004; Basu and Van 1998). This link between poverty and child work is also confirmed in Rwanda. RNCL-2008 finds that the proportion of working children in poor households is higher than those in middle-income or rich households.

In Table 6, we estimate the effects of genocide on a household wealth index. If an adverse effect of genocide on economic capacity of the households persists, it could adversely affect work

and school attendance of the children in the next generation. In column 1, we find a significant negative effect (at the 10 % level) of genocide on household wealth about fifteen years after its outbreak. On the other hand, we find a virtually zero correlation between the distance from the border, our instrument, and wealth index before genocide using the 1991 census data.[11] The result suggests that the damage to household wealth caused by genocide and its adverse effect could persist far in the future.[12]

6.2. Family size and structure

Genocide could affect family size and structure, thereby, changing incentives of the households to make children work. Table 6 further reports the effects of genocide on household size and whether a household has an extended family. We find no evidence that genocide affected household size (column 3), while it increased formation of extended family (column 2). The results might be driven by an effort to mitigate a negative shock due to a loss of family members with unification of relatives. Also, it is possible that children in an extended family may not enjoy the same level of human capital investment as those in an immediate family, which could partially explain how genocide caused more child work and less school attendance.

6.3. Urbanization

The majority of working children are employed in agriculture. Approximately 80% of the working children in Rwanda are engaged in the agricultural sector, mostly on plantations (RNCL-2008).

[11] Results not reported for brevity, but available upon request.
[12] It has to be noted that our estimation results in Table 3 are not confounded with this wealth effect, because wealth index is one of the controls in equation 1. However, household wealth index is essentially a proxy measure of wealth, and may not completely capture its true value. We interpret that the result in column 1 of Table 6 may not be conclusive but still suggestive of wealth as a channel.

These findings are in line with some stylized facts about child labor in sub-Saharan Africa: child labor is highest in countries with low income, higher proportion of rural population, and higher proportion of national income from agricultural production (Bass 2004). Countries with higher rates of urbanization tend to show lower rates of child labor, which is explained by higher income and more educational opportunities in urban areas and lower demand for child labor in the non-agricultural sector (Bass 2004). Based on these stylized facts, we suggest urbanization as a potential channel through which genocide can affect child work and schooling.

In Table 7, we estimate the effects of genocide on some geospatial characteristics that capture the degree of urbanization and development of a cluster, using distance from the Ugandan border as an instrument.[13] Given the close link between child labor and rurality, it is possible that genocide influences child work by hindering urbanization and income growth of an area. Column 1 reports the effect of genocide on the average Global Human Footprint Index (GHFI) of an area.[14] The regression result indicates that the areas that are hard hit by genocide show lower levels of urbanization fifteen years after the genocide. Column 2 reports the effect of genocide on the average Gross Cell Product (GCP) of an area in year 2005. GCP is a similar concept as Gross Domestic Product but uses not only economic data but also geophysical data such as climate, terrain, and location indicators. The regression result indicates that the areas that experienced a high level of genocide show a lower GCP after more than a decade later of the incident. Further, columns 3 and 4 report the effect of genocide on the average minutes required to the nearest city of 50,000 or more people from an area in year 2000 and 2015 respectively. Travel times are an effective measure of the degree of urbanization of an area. We find a significant positive effect of

[13] Refer to Mayala et al. (2018) for the detailed description of the variables in Table 7.
[14] GHFI is a normalized index that measures human influence on ecosystem using data from 1995 to 2004. Given that urbanization is an important manifestation of human influence, urban areas have higher values of the index.

genocide on travel times, suggesting a lower level of urbanization in the areas with intense genocide. While columns 3 and 4 show a noticeable reduction in the effect over time, a significant positive effect still remains.

The results in Table 7, however, need to be interpreted with caution for the reason that we could not test whether the lower levels of urbanization and GCP in areas far from the Ugandan border were pre-existing conditions. Therefore, the result in Table 7 is suggestive, but may not be conclusive of the negative effect of genocide on urbanization and economic performance in the next generation.

6.4. Migration

Given the massive displacement of the people driven by the genocide, migration can be another potential channel through which genocide influences child work of the next generation. If, for example, highly educated and ambitious families had been more likely to leave the areas that were strongly hit by the genocide, jobs that could not be filled with existing adult labor could have caused an increase in demand for child work. Akresh and de Walque (2008), however, find that 88.5 percent of the Rwandan population lived in their birth place in a nationally representative survey in 2001, suggesting massive return migration. Likewise, Gourevitch (1998) reported that the majority of the genocide refugees had returned to their homes by December 1996. Further, we investigate the issue using the sample of households that had never changed their residence in 2008-RNCL, our auxiliary data for robustness checks. In Table 8, we find that the effects of genocide on child work and school attendance in this restricted sample show the same patterns as in the main sample (columns 5 and 6), suggesting that migration did not play a major role in linking genocide and child work.

6.5. Robustness check

The 2008-RNCL also provides information on child economic activities. The survey targets children aged 5-17 living in ordinary households and covers similar questions as in 2010 RDHS. We restrict our sample to children aged 5 -14. Table 8 reports a robustness check of our main analyses in Tables 3 and 4, using 2008-RNCL.[15] Overall, we find similar results as in our main analyses with somewhat less precision. Genocide intensity increases the likelihood of child work at the 10 percent significance level, while it decreases the likelihood of school attendance. Although the effect on school attendance is not statistically significant (p-value: 0.126), the magnitude of the effect is very similar to the effect in the main analyses. Also, the Anderson-Rubin confidence interval does not include zero, suggesting a negative effect. The results in Table 8, therefore, remarkably resemble our results in Tables 3 and 4, bolstering our main findings.

7. Concluding Remarks

In this study, we estimate the long-term causal effects of the Rwandan genocide on human capital of the next generation. We use data on child work in the 2010 RDHS matched with the commune level number of killings during the genocide in the Rwandan government survey. Our identification strategy exploits a historical detail of the genocide. Based on the battle plan of the Tutsi rebels, we instrument for the number of killings using distance from the commune to the Ugandan border. We find that the genocide caused a positive statistically significant impact on child work fifteen years after the genocide. Consistent with this result, school attendance falls with the increase in genocide intensity. This intergenerational effect shows how persistent and far-

[15] 2008-RNCL covers 238 out of 416 sectors. We match the available sectors with pre-genocide communes, again using the geocodes. The matched data cover 117 communes.

reaching a devastating effect of an armed conflict can be. Our empirical results suggests a long-term impact of the genocide on human capital development that is likely to contribute to Rwanda's underdevelopment in the future.

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Compliance with ethical standards

Conflict of interest The authors declare that they have no conflict of interest.

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Table 1. Summary Statistics

	Observation	Mean	S.D.
Outcomes			
Work [5-14] [2010 DHS]	14,991	0.08	0.28
Work [5-14] [2010 DHS, UNICEF]	14,991	0.06	0.24
Work [10-14] [1991 Census]	87,778	0.28	0.45
Work [10-14] [2010 DHS]	6,919	0.11	0.31
Work [5-14] [2008 RNCLS]	8,860	0.06	0.23
Attend School [2010 DHS]	15,052	0.79	0.41
Attend School [2005 DHS]	12,756	0.72	0.45
Attend School [2008 RNCLS]	8,860	0.83	0.38
Extended Family	15,059	0.29	0.45
Family Size	15,059	6.12	2.00
Controls (individual demographic characteristics)			
Age	15,059	9.19	2.85
Female	15,059	0.50	0.50
Mother Alive	15,020	0.95	0.21
Father Alive	14,901	0.88	0.32
Urban	15,059	0.12	0.33
Wealth	15,059	-0.07	0.87
Children under Five	15,059	1.08	0.91
Head Education	15,059	3.79	3.52
Controls (pre-genocide commune characteristics)			
Genocide	15,059	-2.14	1.39
Distance (km)	15,059	71.53	40.58
Ratio	15,059	0.08	0.08
Radio	15,059	0.35	0.11
Education	15,059	8.99	1.33
Adult Employment	15,059	0.89	0.06
Hutu Formal Employment	15,059	0.09	0.08

Means are weighted by probability weights provided in the household module. Work: a dummy that indicates whether a child (aged between 5 and 14) did any kind of work for someone who is not a member of this household during the past week. Work [UNICEF]: a dummy that indicates whether a child (aged between 5 and 11) worked for someone who was not a member of the household in the past week, or did household chores for 28 or more hours, or engaged in any family business, or whether a child (aged between 12 and 14) worked for someone who was not a member of the household for more than 14 hours in the past week, or did household chores for 28 or more hours, or engaged in any other family work for more than 14 hours. Work [1991 Census]: a dummy that indicates whether a child (aged between 10 and 14) worked or looked for work most of the time in the reference week. Attend School: a dummy that indicates whether a child attends school in the current year. Extended family: a dummy that indicates whether a child lives in an extended family. An extended family is defined by a family in

which the household head lives with his/her daughter/son-in-law, grandchild, parent, parent-in-law, siblings, relatives, niece or nephew by blood or marriage. Family size: total number of household members. Age: age of a child in years. Female: a dummy that indicates whether a child is a female. Mother alive: a dummy that indicates whether the mother of a child is alive. Father alive: a dummy that indicates whether the father of a child is alive. Urban: a dummy that indicates a child resides in an urban area. Wealth: household wealth index reported by 2010 RDHS based on households' ownership of durable goods and housing characteristics. It is computed using principal component analysis and the resulting scores have a normal distribution with mean 0 and standard deviation of 1. Children under five: the number of children under five in the household. Head Education: the years of schooling of the household head. Genocide: natural log of the number of homicide deaths per capita in a commune (ln (number of victims/population)). Distance (km): a linear distance between the centroid of a pre-genocide commune and the Ugandan border in km. It is computed using the nearest neighbor analysis plugin of QGIS. Ratio: percentage of Tutsi population in a commune (the number of Tutsi / (the number of Tutsi+the number of Hutu)) before genocide. Radio: percentage of individuals with radio at home in a commune before genocide (from 1991 census). Education: Average last grade completed of adults in a commune before genocide (from 1991 census). Adult Employment: percentage of adults employed in commune before genocide. Hutu formal employed: percentage of Hutu who is an employer or employed in a commune before genocide. This is the definition of formal employment by Friedman (2010).

Table 2. First-stage & Falsification

	First-stage			Falsification		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Genocide	Genocide	Genocide	Work	Work	Work
Specification	OLS	OLS	OLS	OLS	OLS	OLS
Distance (km)	0.0198**	0.0204**	0.0092**	-0.0000	-0.0000	0.0002
	(0.003)	(0.003)	(0.003)	(0.000)	(0.000)	(0.000)
Observations	15,059	14,869	14,869	87,778	83,308	83,308
Demographic variables	No	Yes	Yes	No	Yes	Yes
Commune variables	No	No	Yes	No	No	Yes
First-stage F	40.97	48.38	8.95	0.03	0.06	1.43
Dependent variable mean	-2.14	-2.15	-2.15	0.28	0.28	0.28

Table 2 reports first-stage regression results using RDHS 2010 (columns 1 through 3) and falsification test results using Rwanda Census 1991(columns 4 through 6). Other controls include: age in years, female dummy, dummy for mother being alive, dummy for father being alive, dummy for urban residence, household wealth index, family size, number of children under five, head education, percentage of Tutsi population in commune before genocide, percentage of people with radio at home in commune before genocide, average grade completed of adults in commune before genocide, percentage of adults employed in commune before genocide, and percentage of Hutu being employer or employed in commune before genocide. Definitions of other controls are in notes under Table 1. Standard errors, clustered at the commune level, are in parentheses. ** $p<0.01$, * $p<0.05$, + $p<0.1$

Table 3. Main Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable:	Work	Work	Work	Work	Work	Work	Work	Work	Work
Specification	OLS	OLS	OLS	IV	IV	IV	IV	IV	IV
Genocide	0.0076+	0.0099**	0.0003	0.0274**	0.0240**	0.0484*	0.0359*	0.0584*	0.0184
	(0.004)	(0.004)	(0.006)	(0.007)	(0.006)	(0.021)	(0.017)	(0.029)	(0.016)
Anderson-Rubin CI						[0.0191, 0.1311]	[0.0108, 0.1031]	[0.0184, 0.1716]	
Anderson-Rubin χ^2						12.23	7.55	9.45	
p-value						[0.0005]	[0.0060]	[0.0021]	
Observations	14,991	14,804	14,804	14,991	14,804	14,804	14,804	6,816	81,215
Demographic variables	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Commune variables	No	No	Yes	No	No	Yes	Yes	Yes	Yes
First-stage F				41	48.36	8.92	8.92	9.30	12.88
Dependent variable mean	0.08	0.08	0.08	0.08	0.08	0.08	0.06	0.11	0.28

Table 3 reports the effect of genocide on the likelihood of a child working for someone who was not a member of the household in the past week. Work in column 7 is based on UNICEF definition: a dummy that indicates whether a child (aged between 5 and 9) worked for someone who was not a member of the household in the past week, or did household chores for 28 or more hours, or engaged in any family business, or whether a child (aged between 10 and 14) worked for someone who was not a member of the household for more than 14 hours in the past week, or did household chores for 28 or more hours, or engaged in any other family work for more than 14 hours. Other controls include: age in years, female dummy, dummy for mother being alive, dummy for father being alive, dummy for urban residence, household wealth index, family size, number of children under five, head education, percentage of Tutsi population in commune before genocide, percentage of people with radio at home in commune before genocide, average grade completed of adults in commune before genocide, percentage of adults employed in commune before genocide, and percentage of Hutu being employer or employed in commune before genocide. Definitions of other controls are in notes under Table 1. P-values of Anderson-Rubin χ^2 statistics are in brackets under the statistics. Standard errors, clustered at the commune level, are in parentheses.

** p<0.01, * p<0.05, + p<0.1

Table 4. School Attendance

	(1)	(2)	(3)	(4)
Dependent Variable:	Attend School [2005]	Attend School [2010]	Attend School [2005]	Attend School [2010]
Specification	OLS	OLS	IV	IV
Genocide	0.0012	-0.0136+	-0.0731+	-0.0532+
	(0.007)	(0.008)	(0.044)	(0.029)
Anderson Rubin CI			[-0.2447, -0.0228]	[-0.1686, -0.0101]
Anderson-Rubin χ^2			11.33	5.80
p-value			[0.0008]	[0.0160]
Observations	12,416	14,862	12,416	14,862
Demographic variables	Yes	Yes	Yes	Yes
Commune variables	Yes	Yes	Yes	Yes
First-stage F			5.56	7.21
Dependent variable mean	0.72	0.79	0.72	0.79

Table 4 reports the effect of genocide on the likelihood of a child attending school using RDHS 2005 (columns 1 and 3) and RDHS 2010 (columns 2 and 4). Other controls include: age in years, female dummy, dummy for mother being alive, dummy for father being alive, dummy for urban residence, household wealth index, family size, number of children under five, head education, percentage of Tutsi population in commune before genocide, percentage of people with radio at home in commune before genocide, average grade completed of adults in commune before genocide, percentage of adults employed in commune before genocide, percentage of Hutu being employer or employed in commune before genocide, and percentage of children (aged between 10 and 14) who were attending school in commune before genocide. Definitions of other controls are in notes under Table 1. P-values of Anderson-Rubin χ^2 statistics are in brackets under the statistics. Standard errors, clustered at the commune level, are in parentheses. ** p<0.01, * p<0.05, + p<0.1

Table 5. Heterogeneous Effects

	(1)	(2)	(3)	(4)
Dependent Variable:	Work	Work	Attend School	Attend School
Specification	IV	IV	IV	IV
Age 5 X Genocide	0.0540**		0.0906*	
	(0.019)		(0.043)	
Age 6 X Genocide	0.0432*		-0.0255	
	(0.021)		(0.036)	
Age 7 X Genocide	0.0458*		-0.1343**	
	(0.022)		(0.032)	
Age 8 X Genocide	0.0440*		-0.1766**	
	(0.021)		(0.032)	
Age 9 X Genocide	0.0500*		-0.1542**	
	(0.021)		(0.030)	
Age 10 X Genocide	0.0471*		-0.1177**	
	(0.022)		(0.029)	
Age 11 X Genocide	0.0560*		-0.0674*	
	(0.024)		(0.028)	
Age 12 X Genocide	0.0523*		-0.0223	
	(0.023)		(0.028)	
Age 13 X Genocide	0.0444+		0.0303	
	(0.023)		(0.030)	
Age 14 X Genocide	0.0481*		0.0924**	
	(0.023)		(0.031)	
Female X Genocide		0.0455*		-0.0524+
		(0.021)		(0.030)
Male X Genocide		0.0516*		-0.0541+
		(0.022)		(0.030)
Observations	14,804	14,804	14,862	14,862
Demographic variables	Yes	Yes	Yes	Yes
Commune variables	Yes	Yes	Yes	Yes
Dependent variable mean	0.08	0.08	0.79	0.79

Table 5 reports the differential effects of genocide on the likelihood of a child working for someone who is not a member of the household in the past week and of a child attending school across age and gender groups. Other controls include: age in years, female dummy, dummy for mother being alive, dummy for father being alive, dummy for urban residence, household wealth index, family size, number of children under five, head education, percentage of Tutsi population in commune before genocide, percentage of people with radio at home in commune before genocide, average grade completed in commune before genocide, percentage of adults employed in commune before genocide, percentage of Hutu being employer or employed in commune before genocide, and percentage of children (aged between 10 and 14) who were attending school in commune before genocide (for columns 3 and 4 only). Definitions of other controls are in notes under Table 1. Standard errors, clustered at the commune level, are in parentheses. ** $p<0.01$, * $p<0.05$, + $p<0.1$

Table 6. Channels (Individual-level)

	(1)	(2)	(3)
Dependent Variable:	Wealth Index	Extended Family	Family Size
Specification:	IV	IV	IV
Genocide	-0.2320+	0.0584*	0.2938
	(0.137)	(0.030)	(0.237)
Anderson Rubin CI	[-0.7708, -0.0089]	[0 .0101, 0.1750]	[-0.0347, 1.2230]
Anderson-Rubin χ^2	3.79	5.10	2.65
p-value	[0.0514]	[0.0239]	[0.1036]
Observations	14,869	14,869	14,869
Demographic variables	Yes	Yes	Yes
Commune variables	Yes	Yes	Yes
First-stage F	8.88	8.81	8.81
Dependent variable mean	-0.07	0.29	6.14

Table 6 reports the effect of genocide on household wealth and family size and structure. Column 1 reports the effect on wealth index of a household. Column 2 reports the effect on the likelihood of a child living in an extended family (a family in which the household head lives with his/her daughter/son-in-law, grandchild, parent, parent-in-law, siblings, relatives, niece or nephew by blood or marriage.) Column 3 reports the effect on the family size. Other controls include: age in years, female dummy, dummy for mother being alive, dummy for father being alive, dummy for urban residence, household wealth index, family size, number of children under five, head education, percentage of Tutsi population in a commune before genocide, percentage of people with radio at home in commune before genocide, average grade completed in commune before genocide, percentage of adults employed in commune before genocide, percentage of Hutu being employer or employed in commune before genocide. Definitions of other controls are in notes under Table 1. P-values of Anderson-Rubin χ^2 statistics are in brackets under the statistics. Standard errors, clustered at the commune level, are in parentheses. ** p<0.01, * p<0.05, + p<0.1

Table 7. Channels (Cluster-level)

	(1)	(2)	(3)	(4)
Dependent Variable:	Global Human Footprint	Gross Cell Product	Travel Time 2000	Travel Time 2015
Specification:	IV	IV	IV	IV
Genocide	-5.7567**	-181.3589**	120.6137*	28.1060**
	(2.158)	(66.329)	(47.465)	(10.814)
Observations	468	468	468	468
Commune variables	Yes	Yes	Yes	Yes
First-stage F	45.73	45.73	45.73	45.73
Dependent variable mean	35.53	707.69	186.14	39.20

Table 7 reports the effect of genocide intensity on urbanization (measured by Global Human Footprint Index and travel times) and economic performance (measured by gross cell product) of a cluster. Column 1 reports the effect of genocide on the average Global Human Footprint Index in the area within 2 km (for urban) or 10 km (for rural) buffer around the centroid of a survey cluster. Global Human Footprint Index is Human Influence Index normalized by biome and realm. The index is given at spatial resolution of 1 x 1 km grid cells based on nine global data layers, including population density, built-up areas, nighttime lights, land use/cover, roads, railroads, and navigable rivers, using data from 1995 to 2004. Column 2 reports the effect of genocide on the average gross cell product in the area within 2km (for urban) or 10km (for rural) buffer around the centroid of a survey cluster. Gross cell product is measured at 1° longitude by 1° latitude at global scale and calculated by combining economic data with geophysical data such as climate, terrain, and location indicators of year 2005. Column 3 reports the effect of genocide on the average minutes required to the nearest city of 50,000 or more people from the area within 2km (for urban cluster) or 10km (rural cluster) buffer around the centroid of a survey cluster in 2000. Column 4 reports the effect of genocide on the average minutes required to the nearest city of 50,000 or more people from the area within 2km (for urban cluster) or 10km (rural cluster) buffer around the centroid of a survey cluster in 2015. Other controls include: age in years, female dummy, dummy for mother being alive, dummy for father being alive, dummy for urban residence, household wealth index, family size, number of children under five, and head education (all these individual level characteristics are collapsed at the cluster level), population size of a cluster in 2005, percentage of people with radio at home in commune before genocide, average grade completed in commune before genocide, percentage of adults employed in commune before genocide, and percentage of Hutu being employer or employed in commune before genocide. Definitions of other controls are in notes under Table 1. Standard errors, clustered at the commune level, are in parentheses. ** $p<0.01$, * $p<0.05$, + $p<0.1$

Table 8. Robustness Check

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Work	Attend School	Work	Attend School	Work	Attend School
Specification	OLS	OLS	IV	IV	IV	IV
Genocide	0.0058	-0.0155+	0.0873+	-0.0736	0.0828+	-0.0821+
	(0.008)	(0.009)	(0.051)	(0.048)	(0.045)	(0.048)
Anderson Rubin CI			[0.0252, 0.2855]	[-0.2622, -0.0145]	[0.0274, 0.2598]	[-0.2689, -0.0274]
Anderson-Rubin χ^2			8.23	5.75	7.98	10.20
			[0.0041]	[0.0165]	[0.0047]	[0.0014]
Observations	8,860	8,860	8,860	8,860	8,288	8,288
Demographic variables	Yes	Yes	Yes	Yes	Yes	Yes
Commune variables	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F			7.11	7.14	8.58	8.46
Dependent variable mean	0.06	0.83	0.06	0.83	0.06	0.82

Table 8 reports the effect of genocide on the likelihood of a child working for someone who is not a member of the household in the past week and of a child attending school using the 2008 Rwanda National Child Labor Survey (2008-RNCLS). Columns 5 and 6 report results using only the households that have never changed the place of residence. Other controls include: age in years, female dummy, household wealth index, family size, number of children under five, head education, percentage of Tutsi population in commune before genocide, percentage of people with radio at home in commune before genocide, average grade completed in commune before genocide, percentage of adults employed in commune before genocide, percentage of Hutu being employer or employed in commune before genocide, and percentage of children (aged between 10 and 14) who were attending school in commune before genocide (columns 2, 4, and 6 only). P-values of Anderson-Rubin χ^2 statistics are in brackets under the statistics. Definitions of other controls are in notes under Table 1. Standard errors clustered at the commune level are in parentheses. ** p<0.01, * p<0.05, + p<0.1