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Armed conflict and schooling in Rwanda: Digging deeper

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Abstract: We study how armed conflicts affected educational outcomes in Rwanda during the nineties, relying on two waves of population census data and on a difference-in-differences identification strategy. Our results indicate that the conflicts caused on average a 22% drop in schooling attainments, corresponding to about one year less of education, and that the drop was relatively larger for girls. Primary and secondary schooling attainments were both affected, although through different channels. While increased drop-outs and school delays explain the drop in primary schooling, secondary schooling was mainly affected by a drop in enrollments. Finally, in a within-country analysis, we find no robust link between subnational variations in the drop in schooling and the intensity of any specific form of violence, despite the refined geographical measures at our disposal and a large set of checks. We present possible explanations for the observed patterns and provide related policy implications.

Keywords: armed conflict, education, Rwanda, genocide.

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1. Introduction

More than 240 armed conflict in 153 different locations took place since the end of World War II (Themnér and Wallensteen, 2012). These conflicts typically reduced human capital in the affected countries, both because of the loss of human lives and because of their impact on factors determining the demand and supply for education - such as infrastructure and teacher availability, household resources and labor market conditions. Better understanding these effects is fundamental from a macro-perspective, in light of human capital's role as a key determinant of economic growth (see for instance Lucas, 1988), as well as from a micro-perspective, given that negative shocks to human capital accumulation can have long-lasting individual welfare effects (see for instance Mincer, 1974; and Akbulut-Yuksel, 2014). This paper adds to the increasing number of studies that, by exploiting micro-level data, have advanced our understanding of the way armed conflict affects schooling (see Justino (2011) for a comprehensive review). More specifically, in this paper we analyze the impact of armed conflicts on educational outcomes *and* investigate the underlying channels by relying on a pre- and post-conflict wave of population census data, which are representative for small administrative units, as well as for subsamples of different income strata, gender and age groups. We focus on the case of Rwanda, a country that experienced a dramatic conflict cycle, which included civil war, genocide, (counter-)insurgency and a massive refugee crisis, and which claimed an estimated 1,000,000 lives.

Our baseline identification strategy relies on a difference-in-differences (DiD) approach and compares schooling outcomes between the pre- and post-war census round across a young and an older age cohort. The young cohort in the post-war round includes the *treated* individuals, i.e. those that were at schooling age when (or immediately after) the violence exploded. In order to identify what their schooling outcomes would have been if the violence had not taken place, we combine the information on schooling outcomes of the young cohort in the pre-conflicts round with information on the evolution in schooling outcomes over the conflict period for the old cohort, which includes individuals that completed their education before the conflicts started. The identifying assumption is that the evolution in the outcomes for the young cohort would have been the same as the evolution for the old cohort, had the conflicts not taken place. We present a number of tests for the reliability of this parallel paths assumption. In particular, we show that it is valid for Uganda – a neighbor country that did not experience large-scale conflict during the nineties – and that it also holds when using comparable data for two older age cohorts

in Rwanda, neither of which were affected by conflicts during schooling age. Besides this baseline DiD approach, we also check our results with an alternative DiD, in which we completely discard the old age cohorts, and instead construct our counterfactual based on the evolution of young age cohorts in Uganda, across the same time period. In this case, the identifying assumption is that the evolution in the outcomes for the young Rwandan cohort would have been the same as the evolution for the young Ugandan cohort, had the Rwandan conflicts not taken place.

We first use this empirical framework to estimate the overall, nationwide impact of the conflicts on schooling in Rwanda. We then adapt it to study the heterogeneity of the impact across gender, and to investigate the underlying channels. In studying the channels, we first analyze how changes in school enrollment, dropouts, and school delays contributed to the change in educational attainments. Then we verify whether violence had localized effects, by studying the link between within-country variations in the change in schooling attainments and in the local intensity of the different forms of violence.

Our estimates suggest that conflicts in Rwanda caused a dramatic drop in schooling attainments of about 22% over the nineties, which corresponds to about one year less of education for individuals in the young cohort. This drop in schooling affected individuals that were at primary schooling age at the time of the violence, as well as older cohorts that were at secondary schooling age. Looking at heterogeneity across gender, we find that the drop in schooling was significantly larger for girls than for boys. Depending on the definition of the counterfactual, our estimates indicate an additional impact on girls equal to 37% to 88% of the (proportional) drop in schooling for boys. Turning to the channels, our results indicate that the drop in primary schooling attainments is driven by an increase in dropouts and significant delays in schooling, while enrollment remained virtually unaffected. When looking at secondary schooling the situation is reversed, with a large drop in enrollment and relatively smaller increases in dropouts and delays. The large drop in secondary schooling enrollment is however entirely driven by the drop in primary schooling attainments: once we restrict our focus to students who completed primary schooling, secondary school enrollment actually increases. Finally, we do not find evidence in support for localized effects of the violence on schooling. Using a large battery of specifications and various conflict-intensity proxies defined at low administrative levels, we do not find any robust link between any conflict intensity measure and

the drop in schooling within Rwanda. In other words, even areas where conflicts were much less intense experienced similar drops in schooling as the most affected areas. This result suggests that the impact of conflicts on schooling attainments was nationwide, an explanation that is supported by a number of additional checks, which indicate that school damages and teacher availability were unrelated to the local intensity of the violence.

This paper provides two main contributions to the existing literature. First, the very fine-grained data used for the analysis and a revised methodological approach allow us to update the existing evidence on the impact of violence in the context of Rwanda. More specifically, our results indicate that the drop in schooling attainments is larger than previously thought, is larger for girls than for boys and has affected in a similar fashion every corner of the country – not just those areas where the 1994 genocide was more intense. Second, the richness of our data allows us to study for the first time in a comprehensive way the impact of the violence on primary *and* secondary schooling and the relative importance of the underlying channels, i.e. enrollment, delays and drop-outs. With these contributions, this paper adds to the conflict literature (see Blattman and Miguel (2010) for a comprehensive review) and, more specifically, to the growing body of micro-empirical studies that investigate the consequences of the violence on education (see Justino (2011) and Buvinić *et al* (2014) for a review). Finally, this paper adds to the rich literature on the Rwandan conflicts (e.g. Agüero and Majid, 2014; de Walque and Verwimp, 2010; Guariso and Verpoorten, 2014; Lopez and Wodon, 2005; Serneels and Verpoorten, 2013; Rogall and Yanagizawa-Drott, 2013; Straus, 2004; Verpoorten, 2012a-c; Verwimp, 2005; Yanagizawa-Drott, 2014). Within this literature, a working paper by Akresh and de Walque (2011) also investigates the impact of conflicts on education in Rwanda relying on a DiD strategy. Despite some overlaps, there are important differences in the data and methodology used in the analysis, which lead to different results. More specifically, while Akresh and de Walque use DHS data, focus on primary schooling and only rely on Rwanda for their identification, we use the much richer census data, consider both primary and secondary education, and rely on both Rwandan and Ugandan data. Because of these differences, our analysis leads to significantly different answers to the overlapping research questions. In addition to this, the richer dataset at our disposal allows us to dig deeper into the channels behind the drop in schooling and to better explore its links with specific forms of violence.

The remainder of the paper is organized as follows. Section 2 gives background information on Rwanda. Section 3, 4 and 5 present our identification strategy, the data used, and our results, respectively. Section 6 provides a discussion of our findings and highlights the main policy implications.

2. Background

The history of Rwanda is marked by continuous tensions between the two major ethnic groups, the Hutu and the Tutsi.[1] Under the Belgian colonization (from World War I until 1962), the Tutsi minority represented the political and economic elite of the country and enjoyed a number of privileges. When the country gained its independence in 1962 the Hutu took over power and established a one-party state. Violence against Tutsi had already started in 1959, and in the decades following independence periods of relative peace alternated with episodes of violence, with the latter fueling the ranks of Tutsi living in exile. In late 1990, the Rwanda Patriotic Front (RPF), an army founded by Tutsi refugees, invaded Rwanda from the North. This led to a period of intermittent hostilities and negotiations with the Rwandan government until a peace agreement was reached in 1993.

But, in April 1994, the plane carrying the Hutu President Habyarimana was shot down and genocide broke out. Within a few days from the shooting down of the plane, extremist Hutu militia groups such as *Interhamwe* and *Impuzamugambi*, the Rwandan Armed Forces (FAR) and Rwandan police forces mobilized the civilian Hutu population to massacre the Tutsi minority and, to a lesser degree, Hutu who opposed the extremists. The civil war between the RPF and the Rwandan government, which had been halted the year before, restarted and intensified. By the end of June 1994, the RPF had taken control of the country and had put an end to the ethnic cleansing of Tutsi. Relative peace was established, although the RPF allegedly engaged in reprisal killings, and militias from the old regime (FAR and *Interahamwe*) who had fled across the border to DR Congo engaged in insurgencies in the North-West till the late nineties (African Rights, 1998).

During the nineties Rwanda thus experienced distinct forms of violence, which were concentrated in different geographical areas. While the civil war took place in most of the

[1] According to historians, 'Tutsi' and 'Hutu' share in fact a common ancestry and, rather than different people, designated different social classes of the same people (Newbury and Newbury, 1999). For a detailed description of the recent history of Rwanda see Prunier (1998), Mamdani (2001), and Des Forges (1999).

northern and eastern provinces, as well as in the center; the genocide was most intense in the South; and the (counter-)insurgencies were concentrated in the northwestern provinces. No corner of the small country was completely spared from the violence and all aspects of life were unavoidably affected: human resources were diminished, infrastructures were destroyed, and socio-economic activities were paralyzed (MINEPRISEC/MINESUPRES, 1994). It is estimated that the total death toll of all conflict events in the nineties amounts to more than a million (Verpoorten, 2005, 2012c), with the highest direct death toll caused by the 1994 genocide. In addition, the violence triggered the displacement of approximately 2 million people, and led to the imprisonment of more than 100,000 civilians for crimes related to the genocide.

3. Identification strategy

Ideally, in order to estimate the impact of armed conflicts on schooling attainments we should compare the actual attainments of individuals at schooling age during (or immediately after) the conflicts with the attainments they would have had in the absence of violence. Clearly, the problem is that the latter is not observable. We address this problem by reverting to a difference-in-differences (DiD) estimation. Relying on data collected (just) before (1991) and (just) after the main conflict period (2002), we identify a group of individuals whose schooling attainments were likely not affected by the violence, and then consider the evolution in attainments for this control group as a proxy for what the evolution in attainments would have been in the absence of the violence.

In our setting, we can identify two control groups. First, we can resort to an "internal" control, represented by an older cohort of Rwandan individuals that even in the post-conflicts data collection round is likely to have completed school before the violence started. The identifying assumption in this case is that the evolution in schooling attainments for the young cohort over the nineties would have been the same as the evolution in schooling attainments for the old cohort, had the conflicts not taken place. Although we will show that this assumption withstands two falsification tests, we also rely on a second option, in which we consider an "external" control group, represented by individuals at schooling age in Uganda, a neighbor country with comparable census data rounds. In this case the identifying assumption is that the evolution in schooling attainments for the young cohort in Rwanda over the nineties would have been the same as the evolution in schooling attainments for the young cohort in Uganda, had the

violence not taken place. In what follows, we will mostly rely on the "internal" control group, but we will use the "external" control to test the reliability of our approach and the robustness of our results.

The empirical model for our baseline DiD specification can be written as follows:

$$y_{i,a,c,t} = \alpha_0 + \alpha_1 (postconflicts_t \cdot young_a) + \alpha_2 postconflicts_t + \alpha_3 young_a + X_i\Delta + \eta_a + \pi_c + \varepsilon_{i,a,c,t} \qquad (1)$$

where $y_{i,a,c,t}$ indicates the natural logarithm[2] of the years of schooling of individual i of age a living in commune c, at time t; *postconflicts* is an indicator variable for being in the post-conflicts period; *young* is an indicator variable for being in the young age cohort; X is a vector of individual- and household-characteristics that are likely to influence the level of education of individual i and that will be better detailed below; η and π represent the age and commune fixed effects, respectively; and ε is the error term. A negative (positive) estimate of the DiD coefficient α_1 indicates that the young cohort in the post-conflicts era completed less (more) years of schooling, compared to the young cohort in the pre-conflicts era and relative to the difference in schooling between the old cohorts over the same period.

In our alternative DiD specification that relies on the Ugandan control, we only consider the young age cohort of individuals at schooling age and adopt the following empirical model:

$$y_{i,a,c,t} = \beta_0 + \beta_1 (postconflicts_t \cdot rwanda_c) + \beta_2 postconflicts_t + \beta_3 rwanda_c + X_i\Gamma + \nu_a + \lambda_c + \mu_{i,a,c,t} \qquad (2)$$

where the variable *rwanda* is an indicator variable for living in Rwanda and the fixed effects λ now comprise both Rwandan communes and Ugandan districts (both still indicated with subscript c). In this case a negative (positive) estimate of the DiD coefficient β_1 indicates that the young cohort in Rwanda in the post-conflicts era completed less (more) years of schooling, compared to the young cohort in Rwanda in the pre-conflicts era and relative to the difference in schooling in the young cohorts in Uganda over the same period.

To study the heterogeneity across gender and the underlying channels, we slightly modify equations 1 and 2. To study whether the impact was larger (smaller) for girls compared to boys, we estimate both DiD models adding a three-way interaction term (and all its components), by which the treated group is interacted with the individual-level indicator variable for being a

[2] Taking the logarithm of the dependent variable allows us to interpret the results in terms of approximate proportional changes, which will also simplify the comparison between the results on Rwanda and Uganda.

female: $postconflicts_t \cdot young_a \cdot female_i$ in Eq.1 and $postconflicts_t \cdot rwanda_c \cdot female_i$ in Eq.2. To estimate the relative contributions of changes in enrollment and dropouts to the drop in schooling attainments we maintain the previous set-up, but consider different outcome variables. Instead of looking at years of schooling, we construct variables for enrollment and drop-out on the basis of the past and present schooling status of individuals, i.e. whether they have ever attended school and whether they are still studying. We then run Eq.1 and Eq.2 separately for primary and secondary schooling. Finally, to study the contribution of delays in education we abandon the DiD approach and we estimate the following simple equation, again separately for primary and secondary schooling:

$$A_{i,g,c,t} = \delta_0 + \delta_1 postconflicts_t + X_i\Psi + \Gamma_g + \pi'_c + \mu_{i,g,c,t} \quad (3)$$

where the new dependent variable $A_{i,g,c,t}$ indicates the age of child i attending grade g in commune c, at time t, while Γ indicates grade fixed effects. The coefficient of interest is δ_1 and indicates whether, for a given grade, students are on average older in the post-conflicts population than in the pre-conflicts population. In this case the identifying assumption is that, in the absence of the conflict, the average age by grade would have been similar across both census rounds, which appears plausible given the absence of school reforms over the considered period.

Finally, to verify whether the impact of armed conflicts on schooling had localized effects (e.g. because of local infrastructure destruction, local labor market conditions, or war-induced household-level poverty), we study the subnational relationship between the drop in schooling and the intensity of specific forms of violence. Also in this case we rely on a DiDiD strategy, adding a three-way interaction term (as well as all its components) to Eq.1, in which the treated group (young age cohort in the post-conflicts period) is interacted with a measure of conflict intensity. In order to differentiate across different forms of violence, we rely on a number of different proxies for violence intensity. The identifying assumption behind this approach is that, in the absence of localized effects of violence, the changes in educational attainments would be similar across high- and low-violence intensity communes. As this assumption may be unwarranted, we will run a large set of robustness checks controlling for commune-specific time trends; adjusting for post-genocide migration, for selective killings during the genocide, and for scholarship programs in the post-genocide period; using Uganda as

a control group instead of the old cohort; and switching to an IV strategy that exploits exogenous variation in the transportation costs for the army and militia during the days of the genocide.

Before we can estimate the above empirical models, we need to define thresholds for the different age cohorts. Choosing such thresholds is not straightforward. In Rwanda - as in many developing countries – school classes are characterized by large variations in the age of the students and it is difficult to clearly define at which age students are attending and completing primary and secondary schooling. For our baseline estimation we consider ages 6 to 25 as the young cohort. In 2002, this cohort includes individuals that were 17 or younger at the time the violence reached its peak, during the 1994 genocide. According to the 1991 census, the group aged 17 or younger embraced more than 90% of the student population at that time. Hence, we are rather confident that our 2002 young cohort includes most of the individuals that were attending school at the time the armed violence erupted (or in the following years). We define the old cohort to include individuals aged 26 to 45, to bracket the same number of years as the young cohort. This means that the old cohort includes individuals that were 18 to 37 years old at the time of the genocide. We carefully check the robustness of our results to different age thresholds. Furthermore, to investigate the role played by school enrollments, drop-outs and delays, we specify different sets of age thresholds to study these channels separately for primary and secondary education (cf. below).

4. Data

4.1. Rwandan Population Census Data

The main source of data for our empirical analysis consists of two population census rounds collected in Rwanda in 1991 and 2002, thus bracketing the main conflict events. For the analysis we consider a 10% random draw from each census rounds. The 1991 10% sample is made available by IPUMS international, while for 2002 it has been drawn from the full 2002 census.[3] Restricting the analysis to individuals aged 6 to 45, leaves us with 468,475 observations from the 1991 census round and 499,203 observations from the 2002 round. Figure 1 displays our main

[3] Rwanda has undergone a number of administrative reforms in the past few decades. The 1991census indicates the Commune of residence of each individual. The administrative reform that took place in 2001 changed the 154 Communes in 106 Districts. The 2002 census indicates both District and Sector of residence of each individual, with the Sector being the lower administrative unit (there were 1536 Sectors in Rwanda by 2002). By merging two administrative maps of Rwanda, we have matched each of the 2002 Sectors with the corresponding 1991 Commune.

dependent variable - the number of (completed) years of schooling[4] – for the pre- and post- war round, across age. The figure is quite revealing. The old age cohorts display systematically higher average years of schooling in the post-conflict round compared to the pre-conflict round, suggesting that before the violence there was a positive trend in schooling in Rwanda. In contrast, educational attainments of the cohorts below 22 years is lower in the post-conflict round, compared to the pre-conflict round, suggesting a negative effect of the armed violence that took place during the nineties. From the census, we also construct indicator variables for school enrollment and dropouts, and a set of control variables that are likely to determine part of the variation in educational attainments across individuals (included in the vector X in the specifications presented above). These control variables are: an indicator for being female; age and education of the household head; number of children under five living in the household; an indicator for residing in a rural area; and an indicator for being asset-rich.[5] Panel A of Table 1 reports the summary statistics for the Rwandan census variables, both for the whole sample and disaggregated by census round.

---- Figure 1 about here ----

---- Table 1 about here ----

How reliable are the Rwandan census data? In general, population census data, especially from developing countries, do not enjoy a good reputation, because the collection methods and practices vary from country to country and because certain variables - such as citizenship and ethnicity - are often highly politicized. In the case of Rwanda, ethnicity is indeed politicized,

[4] The primary schooling cycle in Rwanda typically lasts for 6 years, although during the eighties a temporary reform (later abolished) increased it to 8 years (Obura, 2003). The complete secondary schooling cycle in Rwanda lasts also for 6 years, with the possibility of obtaining a certificate after 3 years. In order to keep the comparison across different age groups straightforward, we consider anybody completing primary school as having complete 6 years. We also cap the variable to a maximum of 16 years of schooling, assigning this value to anybody who completed at least four years of university (i.e. 6 years of primary schooling + 6 years of secondary schooling + 4 years of tertiary education). The number of individuals affected by our coding choices is quite small. Overall less than 4% of the individuals reports completing 7 or 8 years of primary school, while less than 0.1% reports completing more than 16 years of schooling. As a robustness check, we also tried alternative definitions of the variables (such as not capping the years of primary schooling to 6 years) and all results presented in the paper remain qualitatively unaffected (results not reported, but available from the authors).

[5] The indicator variable takes the value 1 when the households' asset index exceeds the population mean of the index. The asset index is constructed by giving equal weight to all four wealth indicator variables available in the different census rounds: whether the dwelling is owned by the household; whether there is (any type) of toilet; whether there is a finished floor; and whether there is tiles or cement roof.

which is reflected in the omission of ethnic identity in the 2002 census round, in line with the public rhetoric of national unity after the 1994 genocide. However, leaving ethnicity aside, the Rwandan census data has been shown to be very reliable. Verpoorten (2005) directly compared the 1991 census with the 1990 population data from the local administration, finding an almost exact match of the total number of women and men, which is indicative for the quality of both sources, as they were collected independently of each other. Moreover, the information contained in the two census rounds is perfectly in line with the information contained in two rounds of DHS data collected around the same period, in 1992 and 2000 (see Appendix A for more details about the comparison). Differently from the census, the DHS data are firmly established in empirical research as a reliable source of information, mainly because of their standard and transparent approaches to data collection, cleaning and coding. However, while DHS data are representative at the province level (there are 12 provinces in Rwanda), census data are representative for smaller administrative units, for different income strata, gender and age groups. Because this richness allows for a much more refined analysis, we rely on the census data for our analysis.

4.2. Ugandan population census data

To run one of our falsification tests and to estimate Eq.2, we rely on population census data from Uganda, Rwanda's northern neighbor. Besides sharing a border, these countries have in common the census years 1991 and 2002, and IPUMS International provides a 10% random draw from both Ugandan census rounds as well. Furthermore, the 1991 census rounds reveal that at the beginning of the nineties the two countries were similar in terms of school enrollment and dropout rates in both primary and secondary school (see Table 1). We use the Ugandan census to generate the same variables described above for Rwanda.[6] The summary statistics for the Uganda census variables, both aggregated and divided by census round are reported in Panel B of table 1. Figure 2 below replicates Figure 1, giving the average completed years of schooling by age, across the two Ugandan census rounds. It can be seen that average schooling attainments improved for every single age between 1991 and 2002, not just for the older cohorts as in the Rwandan case.

[6] The only difference relates to the fact that primary schooling in Uganda lasts for 7 years, rather than 6 (hence, for instance, individuals completing the full cycle of secondary school will have completed overall 13 years of schooling).

---- Figure 2 about here ----

4.3. Conflict intensity proxies

To test for the existence of localized effects of violence, we rely on different conflict intensity proxies. Among the different conflict events described in Section 2, the genocide stood out with an estimated death toll of 600,000 to 800,000 people in barely 100 days, and is the best documented form of violence. There are 5 different genocide intensity proxies that we can rely on: the share of Tutsi living in a Commune in 1991, the location of mass graves, the genocide-related death toll, the share of perpetrators of genocide-related crimes, and the Genocide Excess Mortality Index (GEMI). All these 5 variables are positively correlated among each other (see Table C3, in Appendix C), with the pairwise correlation being on average around 0.5 and varying from 0.1 (between the number of mass graves in a commune and the genocide death rate) to 0.8 (between the share of perpetrators and the GEMI). Rather than making the arbitrary choice of selecting one specific proxy, we repeat the analysis for each one of them separately as well as for a combination of them obtained through Principal Component Analysis (PCA). Proxies are much scarcer for the other forms of violence. We will consider what, to the best of our knowledge, are the only available measures: the Civil War Excess Mortality Index (CEMI) to proxy for the intensity of the civil war, the share of extrajudicial killings to proxy the intensity of (counter-) insurgency operations in the North-West, and the share of externally displaced households to proxy for the intensity of the refugee crisis. The correlation between these variables and the genocide measures is mostly negative and low (see again Table C3), confirming that violence did not cluster in space over the nineties, but that different communes of Rwanda were hit by different forms of violence. We have included detailed description, sources and summary statistics for all these variables in Appendix C. We consider all measures at the level of the 145 Rwandan communes, so to match them with the census-information on the commune of residence of each individual.

5. Results

5.1. The overall drop in schooling attainments

Table 2 reports the results of estimating Eq.1. Column (1) reports the estimates of the DiD regression without any control. In column (2) age and commune fixed effects are added, while column (3) reports the results of our preferred model that includes the full set of controls. The estimate of the DiD coefficient α_1 in the third column indicates that the young cohort in the post-conflict era completed on average 22% less years of schooling (equal to 1.03 year less education), compared to the young cohort in the pre-conflict era and relative to the difference in schooling between the old cohorts over the same period. The coefficients on the other variables are of the expected sign, indicating that being male, living in a wealthier household, having an older and more educated household head, having a lower number of siblings and living in urban areas are positively associated with schooling attainment.

---- Table 2 about here ----

To scrutinize our baseline result, we perform three different checks. First, we run two falsification tests to verify our identifying assumption; second, we re-define the control group taking Ugandan cohorts as a control; and finally we explore the sensitivity of the results to changes in the threshold between young and old age cohort.

In our baseline approach, identification rests on the assumption that the old age cohort is an appropriate control group, i.e. that it allows us to proxy the counterfactual "what would have been the evolution in education for the young cohort had the conflicts not taken place". We examine the reliability of this assumption by performing two falsification tests. First, we re-run Eq.1 for Uganda. If in the absence of violence the evolution in schooling in the old cohort is a good proxy for the evolution in the young cohort, we should find the DiD coefficient to be close to zero in the Ugandan case. Results are reported in Table 3, where column (1) reports the previous result for Rwanda, to make comparison easier. Column (2) shows that the DiD estimate for Uganda is very close to zero and statistically insignificant, as expected. The second falsification test examines the identifying assumption by considering two older age cohorts in Rwanda, whose schooling attainments should not have been affected by the conflicts in the nineties. More specifically, we re-estimate Eq.1 redefining the young cohort as including individuals aged 26 to 45 (i.e. our previous old cohort) and the old cohort as including

individuals aged 46 to 65.[7] The DiD coefficient, reported in Column 3, is again very close to zero.

---- Table 3 about here ----

Despite the results of our falsification tests, one might still be worried that our main DiD estimates are picking up a peculiarity in the evolution of the Rwandan old age cohort (i.e. our control group) rather than (or in addition to) the evolution in the young cohort. To address this concern we run Eq.2, which compares the evolution in the young cohort between Rwanda and Uganda. In this case we are interested in the coefficient β_1 between the post-conflicts indicator and an indicator variable for being in the Rwandan census rounds. Results reported in column (4) of Table 3 indicate that in Rwanda individuals aged 6 to 25 completed 21.5% less years of schooling in the post-conflicts era compared to the same cohort in the pre-conflicts era and relative to the difference in schooling in Uganda over the same period and for the same age group. The striking similarity between the estimates of β_1 and α_1, provides strong support to our empirical strategy.

To check the robustness of our results to different age thresholds, we rerun Eq.1 multiple times, each time changing the threshold by one year, covering the interval between 16 and 30 years. Figure 3a summarizes the results of this sensitivity check. It shows that the estimated drop in educational attainments is quite stable, moving between 16.4% and 24.3%. An alternative way to look at the importance of the specific threshold choice is to estimate the drop in schooling for each age separately (which is possible given the large number of observations at our disposal). To do so, we rerun our model replacing the interaction between the post-conflict and the young-cohort indicators with the full set of interactions between the post-conflict indicator and a separate indicator for each age included in the young cohort – i.e. from 6 to 25. Figure 3b reports the estimate of each coefficient together with the relative 95% confidence interval. The figure shows that for each age between 6 and 23 there is a significant drop in schooling attainments, oscillating between 5.1% and 34.2%. Taken together, figures 3a and 3b show that the large and significant drop in schooling observed above is not driven by a few specific groups and is robust to movements in the exact definition of the young age cohort. In addition, while acknowledging

[7] The result is robust to changes in the age cohorts chosen for the test (results not reported, but available on request).

that it is impossible to define the 'right' threshold, these figures do lend support to our choice of including ages up to 25 in the young cohort, as this allows including in the treatment group a large share of individuals that were likely to be affected by the violence at schooling age, and because estimates are stable for changes in the neighborhood of that threshold. [8]

---- Figure 3 about here ----

5.2. Boys versus Girls

Column (1) of Table 4 reports the results of estimating Eq.1, augmented with the three-way interaction term $postconflicts_t \cdot young_a \cdot female_i$ (and all its components). The estimated coefficient of the triple interaction term indicates that the drop in schooling was 13.8 percentage points larger for girls: 29.4% compared to 15.6%. This translates in an average drop of 1.2 and 0.9 years of education for girls and boys respectively, as boys started from a higher level of schooling.

These estimations rely on the underlying assumption that, had the conflicts not taken place, the difference in the 1991-2002 evolution in schooling attainments between girls and boys in the young cohort would have been the same as the difference in the evolution for girls and boys in the old cohort over the same period. This assumption may be questionable. In the old cohort, especially girls made significant progress in schooling attainments. In particular, between 1991 and 2002 the average years of schooling for the cohort of females aged 26-45 increased by 43%, moving from 2.3 years to 3.3 years. This compares to a 20% increase for males, for which average schooling attainments increased from 3.5 to 4.2 years. One may be concerned that this kind of catch-up by girls was exceptional, and would not have been repeated in the young cohort. The opposite argument could also be made: instead of slowing down, the catch-up for girls may have accelerated over time, in the absence of violence. Because of this concern, we put the parallel paths assumption to a falsification test using data from Uganda. Similarly to Rwanda, schooling attainments for girls were improving faster than for boys in Uganda between 1991 and 2002: 39% versus 11% for the cohort 26-45. But, differently from Rwanda, the young cohort in

[8] Figure B2 in Appendix shows the same analysis for Uganda. Figure B1a replicates Figure 3a and shows that, using the Ugandan data, none of the possible thresholds provides a DiD coefficient that is significantly different from zero. Figure B2b replicates instead Figure 3b and shows that the majority of individual-age coefficients is not statistically significant at conventional levels, and that in those few cases in which they are, the magnitude of the coefficients is very small.

Uganda was not exposed to the violence during the nineties. In support of our identifying assumption, column (2) of Table 4 shows that the DiDiD (as well as the DiD) coefficient is very close to zero and not statistically significant, whenever Ugandan data are used.

As a further check we also run Eq.2, comparing the evolution in schooling attainments in the young cohort in Rwanda to the evolution of the same cohort in Uganda. In this way, we get rid of possible issues related to the comparison with the evolution in the old cohorts and we simply consider the young cohorts in Uganda as the control group. The results reported in column (3) confirm that the average drop in schooling for girls is significantly larger than for boys, although the coefficient decreases by about a half of its previous size.

---- Table 4 about here ----

Overall, results in Table 4 indicate that girls disproportionately suffered the consequences of the conflicts in Rwanda, in terms of schooling attainments. The actual magnitude of the additional drop in schooling suffered by girls depends however on the definition of the control group and on the underlying assumption one is willing to make. If we believe that the improvements in education that girls experienced in the decades preceding the violence would have continued at a similar pace had the violence not broken out, then the proportional drop in girls' attainments caused by the violence is almost twice as large as the drop experienced by boys. If instead we take Uganda as comparison and we assume the evolution in schooling attainments for boys and girls in the young cohort would have been similar across the two countries, then we observe a smaller, but still significant additional drop for girls, equal to about an extra 37% compared to the drop suffered by boys.

5.3. Lower enrollment, dropouts, or delays?

The large drop in schooling attainments that we estimated in the previous sections can be due to children that do not enroll in school (HP1), students that drop out from school (HP2), or students that delay their education (HP3)[9].

A set of simple figures can shed light on the importance of these different channels for primary schooling. Figure 4a gives enrollment rates by age, proxied by the share of individuals

[9] Delays can be caused by two different factors: students repeating the same class more than once, or students postponing education. The dataset at our disposal does not allow us to disentangle these two factors.

that either are currently studying or completed at least one year of school. The figure starts at age 10, because the census does not give student status for younger ages. The figure shows that the enrollment rate has been constantly increasing in Rwanda over time. In particular, compared to 1991, individuals of all ages in 2002 were more likely to have ever attended school. Figure 4b provides information on primary schooling dropouts. Considering only individuals that ever enrolled in school, it shows for each age above 13 (i.e. the age at which students are expected to have completed primary school) the share of those that dropped out (i.e. are not students any more) before completing the primary schooling cycle (i.e. 6 years)[10]. The 1991 curve indicates that dropouts were declining over time. However, for the 2002 curve there is a clear peak between ages 15 and 24, an interval that brackets individuals that were likely to be enrolled in school at the peak of violence (7 to 16 years old in 1994). Adopting the simplifying approximation that each student started primary school at age 6 and completed one grade per year, this pattern implies that dropouts were concentrated around the years 1992-1998 (see Figure B3, in Appendix). Finally, Figure 4c reports the average age of students enrolled in primary schooling for grade 2 to 6.[11] The average age of students appears significantly higher in 2002 than in 1991 for each one of the primary grades. For instance, students who attend the final grade of primary school (i.e. who already completed 5 years of schooling) are on average 14.7 years old in 2002 compared to 12.5 in 1991.

---- Figure 4 about here ----

Overall, these figures suggest that increased drop-outs (H2) as well as delays (H3) are likely to be responsible for the large drop in schooling attainments observed above, while changes in primary school initiation (H1) do not seem to have played an important role. We now study these effects more formally.

To estimate the impact on school initiation (H1) we rerun Eq.1, replacing our dependent variable with *everbeentoschool,* an indicator variable for being currently a student or having

[10] Given our coding of the years of schooling variable explained in footnote 4, this also takes into account those students who attended the 8 years cycle of primary schooling in the eighties.

[11] We exclude grade 1 because, as mentioned above, we only have information on the student status for individuals aged 10 or above and in 1991 there are just 189 students enrolled in the first class of primary school aged 10 or above.

completed at least one year of school in the past.[12] We restrict the young-cohort (i.e. the affected group) to individuals aged 10 to 14, as they were likely to enroll during or after the peak of the violence (they were aged 2 to 6 at the time of the 1994 genocide). For consistency with our previous analysis, we maintain the standard definition of old cohort, hence dropping all individuals aged 15 to 25.[13] In line with what emerged from Figure 3, the first column of Table 5 reports a small and statistically insignificant DiD coefficient, indicating that the young cohort in 2002 was not less likely to enroll in primary school compared to the young cohort in 1991 and relative to the difference in enrollments between the old cohorts. Second, we look at the evolution of primary school dropouts (H2). In order to do so, we consider as dependent variable an indicator for dropping out from primary school and we redefine the young cohort to consider individuals aged 13 to 21, i.e. individuals that are supposed to have completed primary school between 1994 and 2002.[14] Also in this case we keep the standard old age cohort 26-45, thus dropping age 22 to 25 from the analysis.[15] The estimate reported in column (2) of Table 5 is large and significant: conditional on starting primary school, individuals in the 2002 young age cohort were 22.9 percentage points more likely to drop out before completing the 6-year cycle, compared to individuals of the same cohort in 1991 and relative to the difference in dropouts between the old cohorts (to facilitate interpretation, the bottom panel of the table reports the mean of the outcome variable for each one of the four different combinations of age group and census round). Finally, to study the case of delays in education (H3) we estimate Eq.3 that has the age of the student as dependent variable. As in Figure 4c, we restrict our analysis to individuals enrolled in grades 2 to 6 at the time of the census and consider the age group 10-16.[16] Our estimated coefficient reported in Column (3) of Table 5 indicates that on average students in

[12] Although this as well as (some of) the following dependent variables are binary, we always keep using the linear estimation model because it is more traceable. Results are in any case qualitatively similar when using a Probit model (not reported).

[13] Given that enrolment is sometimes delayed, dropping these ages also allows us to exclude individuals for which it is unclear whether the enrolment decision was affected by the violence or not. Table B1, in Appendix, shows that results reported in this section keep holding whenever the excluded groups are included in the old cohort.

[14] According to the 1991 census, the majority of the students in Rwanda switched from primary to secondary school between 13 and 14 years (Figure B1, in Appendix). Results are in any case robust to small changes in the definition of the cohorts (results not reported, but available on request from the authors).

[15] Once again, Table B1 in Appendix shows that the results remain qualitatively similar when the dropped ages are included in the old cohort, although in this case the coefficient becomes smaller. This is due to the fact that a significant share of the students in the excluded cohort were still in primary school at the time the violence exploded (Figure B1 shows that more than 20% of the students aged 14 were still attending primary school in 1991), and including them in the control group leads therefore to an underestimation of the effect on dropouts.

[16] Once again, results are robust to changes in the age thresholds (results available on request from the authors).

2002 were more than one and a half years older than students in the same grade in 1991. Overall, Panel A of Table 5 confirms what emerged from the figures: students do not enroll less in primary school as a consequence of the conflict, and the large reduction in schooling attainments is caused by both an increase in dropouts and a slower grade progression.[17]

---- Table 5 about here ----

We now replicate this analysis for secondary schooling.[18] We start again by considering school enrollment and we consider as dependent variable an indicator for ever attending secondary schooling (approximated by an indicator for either being currently enrolled in or completing at least one year of secondary school). Relying on pre-conflict data to make informed guesses about appropriate age thresholds, we consider the young cohort 14 to 22 to identify individuals that were likely to begin secondary schooling between the 1994 genocide and the 2002 census round, and we compare it with our standard old cohort.[19] Results are reported in Panel B of Table 5. Column (4) indicates a large and significant drop in secondary school enrollment of 11 percentage points. However, this result is driven by the significant increase in primary dropout rates that we have observed above. In Column (5) we repeat the analysis restricting the focus on students that actually completed primary school. The coefficient turns now positive, indicating that, if anything, individuals among the 2002 young cohort that completed primary school were more likely to enroll in secondary school (by 5.3 percentage points), compared to the same cohort in 1991 and relative to the difference in enrollment between the old cohorts. We next look at secondary schooling dropout rates. In Rwanda the full secondary schooling cycle lasts for 6 years, but there is the possibility of obtaining a certificate after 3 years (post-primary cycle). In column (6) we therefore look at actual dropouts within the first three years of secondary schooling, in order to avoid simply picking up changes in

[17] We have repeated the same analysis for Uganda, adapting the thresholds to take into account that primary schooling in Uganda lasts 7 years. Overall, the DiD findings for Uganda indicate very small changes - and mostly of the opposite sign - in enrolment, delays and dropout in primary school for the young cohort between 1991 and 2002 (results not reported, but available on request from the authors).

[18] Because of the large variation in the age at which primary schooling is completed, it is very difficult to precisely determine at which age students enroll and complete secondary schooling. Our results on secondary schooling should therefore be considered with more caution than those for primary schooling.

[19] Table B1 in Appendix shows that results are similar when age category 23 to 25 is included in the old cohort.

preferences for the type of secondary education cycle.[20] We include in the young cohort individuals aged 17 to 25 (i.e. likely to have completed the 3 years of secondary school between 1994 and 2002). Results indicate a significant increase of 4.1 percentage points in dropouts within the first three years of secondary school. Once again, if we look at the specific age cohorts in the 2002 sample for which dropouts increased, we see a peak for ages 22 to 27 (see Figure B4b, in Appendix), an interval that brackets individuals that were indeed likely to be attending secondary school at the time the violence took place. Finally, we look at grade progression in secondary schooling. Similarly to what we did for primary schooling, we run Eq.3 restricting the analysis to students enrolled in grades 7 to 13 and of ages 13 to 25. Results in Column (7) indicate that students enrolled in secondary schooling are on average about six months older in the post-conflict era compared to the pre-conflict era. Despite being still significant, this age difference is less than a third than the one we estimated for primary schooling, suggesting relatively faster grade progression for students going to secondary school.

Overall, these results indicate some heterogeneity in the impact of armed conflict on schooling across primary and secondary education. While primary schooling experienced stable enrollment rates, but large increases in dropouts and delays in education, the reverse is true for secondary schooling, where enrollment rates plummeted, while dropouts and delays played a smaller (albeit still significant) role. The large drop in secondary enrollment is however a direct consequence of the overall drop in primary schooling attainments. When we restrict the focus to students who finished primary schooling, dropouts and delays are again the two main direct channels through which conflicts affect secondary schooling attainments.[21]

5.4. Localized effects of violence?

As described in Section 2, the Rwandan conflict cycle included civil war, genocide, (counter-) insurgency operations and refugee crisis. All these events took place in the nineties, but differed in terms of geographic spread, intensity, duration, and identities of victims and perpetrators. As a first step to study whether any of these different events had localized effects on schooling, we run Eq.1 separately for each one of the 145 Communes of Rwanda. Figure 5 shows the spatial distribution of the estimated drop in schooling attainments, with darker shading indicating a

[20] While 4.5% of the overall sample completed 3 years of secondary schooling (3.7% in 1991 and 5.3% in 2002), only 2.3% completed more than that (1.4% in 1991 and 3.2% in 2002).
[21] The similarities are also visually confirmed by a comparison between Figure 4 and Figure B4 in Appendix.

larger drop. Eyeballing the figure, we note that every single commune suffered a drop in schooling attainments, with the communes located in the center-north of the country experiencing relatively larger ones.

---- Figure 5 about here ----

To test whether the spatial variation in the drop in schooling can be linked to specific forms of violence, we turn to the DiDiD strategy. We start by interacting the treated group in Eq.1 with the intensity of the genocide, by considering the 5 different proxies mentioned in Section 4.3. Table 6 reports the estimates: irrespectively from the specific measure, none of the DiDiD coefficients (in the third row) is significantly different from zero and of the expected negative sign, thus indicating that individuals in the young cohorts in areas more severely affected by the genocide have experienced on average similar drops in schooling attainments as individuals in areas that were relatively less affected by the genocide. The mismatch between the drop in schooling and genocide intensity is also clear when we look at Figure 6, which shows the 1991 commune-level share of Tutsi, and we compare it with Figure 5. For instance, since the share of Tutsi in the northern provinces was as low as 1.5% (compared to over 20% in some of the southern provinces) it is unlikely that the large estimated drop in schooling in the North is driven by localized effects of genocide.[22]

---- Table 6 about here ----

---- Figure 6 about here ----

Our (lack of) result is confirmed in a full set of robustness checks in which we relax the identifying assumption that, in the absence of localized effects of violence, the changes in educational attainments would be similar across high- and low-violence intensity communes. More specifically, our results are robust to controlling for commune-specific time trends; adjusting for post-genocide migration; adjusting for selective killings during the genocide; adjusting for scholarship programs in the post-genocide period; using Uganda as a control group

[22] Similar visual mismatches emerge with other genocide intensity proxies, whose geographical distributions are reported in Appendix C.

instead of the old cohort; and switching to an IV strategy that exploits exogenous variation in the transportation costs for the army and militia during the days of the genocide.[23] The full details of these checks are reported in Appendix C. Although these checks cannot be fully exhaustive, as we are constrained by data quality and availability, the fact that across all these tests the DiD coefficient remains very stable and the DiDiD estimates mostly small and insignificant indicates that areas less affected by the genocidal violence suffered a drop in schooling attainments that is comparable to the one experienced by more severely affected areas.

We now check whether the within-country variation in the drop in schooling can be explained by the other conflict cycle events, which were less intense than the genocide, but longer-lasting. To do so, we rely on the CEMI index, the number of extrajudicial killings of civilians in the north-west and the share of externally displaced individuals to proxy for the intensity of civil war, (counter-)insurgency operations and refugee crisis, respectively (see Section 4.3 and Appendix C for details). Table 7 shows the results for the DiDiD model using these three alternative conflict measures. Only for the CEMI index the DiDiD coefficient turns weakly significant (at 10%), indicating that areas experiencing higher civil war intensity suffered a larger proportional drop in schooling attainments. However, the coefficient is relatively small - indicating that moving from no-violence to maximum violence brings an additional 8% drop in schooling - and the DiD coefficient remains large and significant (18.9%), indicating that even areas unaffected by the civil war experienced large drops.

Combined, these results indicate that we cannot robustly link the large drop in schooling to any specific form of violence. Also, when we take into account the different forms of violence at the same time, either by combining them in a single index or by including them in the same regression, our (non-)results are confirmed (results not reported, but available from the authors).

Why do we fail to detect evidence for localized effects of conflict on schooling? One possible explanation is that the supply of education, determined by factors such as school buildings and teachers, was affected nationwide. We find some evidence in support of this hypothesis. A 1994 report from the Ministry of Education (MINEPRISEC/MINESUPRES, 1994) indicates that as many as 65% of the 1836 schools were damaged, needing urgent repair. To test whether damages to school buildings were correlated with genocide intensity, we turn to a

[23] For the IV we rely on sector-level intensity measures. The analysis has however to be restricted to a cross section, using only the 2002 data. See the Appendix C for more details.

nationally representative community survey collected in 1999/2000 (EICV1). We find that communities in more affected areas were equally likely to be in the proximity of an operating primary school, and not more likely to have been the location of reconstruction or renovation works of a school building since 1994.[24] Besides school infrastructure, teachers had become a very scarce resource all over the country: many were killed, because they were Tutsi or part of the moderate Hutu elite; several others were imprisoned (having participated in the killings); and still others had moved abroad or to urban centers (Obura, 2003). The result was "*the total erosion of faith in the education system*", with less than half of qualified teachers remaining in the primary system after the conclusion of the conflict (ibidem, p.48). When we look at information on occupation in the 2002 census we find no relationship between the share of teachers and genocide intensity in a commune.[25] Finally, the conflicts of the nineties heavily impacted the government budget, drying up resources for education. While the post-conflict era witnessed an increase in resources dedicated to the reconstruction of schools and infrastructures, by 2001 current spending to support the day-to-day running of the schooling system was still as low as 3% of the GDP, the same level that the country had in the eighties (World Bank, 2004).

6. Discussion

Our first empirical result indicated that armed conflicts caused on average a 22% drop in schooling attainments - corresponding to roughly one year less of education - for individuals aged 6 to 25, between 1991 and 2002. This baseline estimate remained almost identical when considering an alternative counterfactual, based on the evolution in schooling in Uganda over the same period. While the estimated negative impact of conflicts on schooling confirms the general finding in the literature, our figure is much larger than what was previously estimated in the context of Rwanda (Akresh and de Walque, 2011), thus highlighting the dramatic costs in terms of human capital caused by the violence. The larger magnitude compared to previous estimates

[24] The EICV1 surveyed about 6.500 households and records whether there is any operating primary school within the community of the respondent and whether any renovation or reconstruction works took place at the school since 1994. Overall the survey covers 439 sectors, located in 139 communes. Works were recorded in 16% of these sectors.

[25] According to the census, in 2002 there were about 37.000 teachers in Rwanda: 29.901 were employed in primary education, 5.608 in secondary education and in 1.521 tertiary education. We regressed the share of teachers in a commune over each one of the different conflict intensity proxies, including province fixed effects. All coefficients were very close to zero and none of them turned out significant. Considering the variable aggregated at the sector, rather than at the commune level or weighting the number of teachers by the number of individuals at schooling age did not change the results (results not reported, but available from the authors).

can be explained by the definition of the affected population. As shown in our results, the drop in schooling affected not only the youngest cohort of individuals that were at primary schooling age at the time of the violence, but also older cohorts that were at secondary schooling age. Including these relatively older cohorts in the control group, leads to underestimating the overall impact of the conflicts.[26]

Our second result revealed that the conflicts disproportionately affected girls. In this case, however, the exact magnitude of the effect depends on the definition of the counterfactual and thus on assumptions about what the trends in education for girls would have been in the absence of the violence. Our estimates indicated an additional impact on girls that is between 37% and 88% of the proportional drop in schooling observed for boys. The fact that girls are more heavily penalized by the violence suggests that in the aftermath of the conflicts limited household resources were reallocated towards boys. This is not surprising in the context of a traditionally patriarchal society like Rwanda, and is in line with what studies have found in Tajikistan, India and Guatemala (Shemyakina, 2011; Singh and Shemyakina, 2013; Chamarbagwala and Morán, 2011), although other studies have shown this result to be highly context-dependent (see Buvinić *et al*, 2014). Importantly, this result corrects previous findings on the gender gap in the drop in schooling in Rwanda (Akresh and de Walque, 2011; UNESCO, 2010).[27]

Our third set of results relates to what we believe is the first comprehensive micro-empirical analysis of the consequences of armed conflicts on school enrollment, dropouts and school delays, disaggregated by primary and secondary schooling.[28] Concerning primary

[26] In their working paper Akresh and de Walque (2011) perform on a DiD analysis setting the threshold between young and old cohort at age 16 and report a drop in schooling of 0.56 years. Similar figures are reported in a review piece published by UNESCO (2010).

[27] Akresh and de Walque (2011) report a significantly larger drop in schooling for boys than for girls and describe it as a "levelling-off effect" of the conflicts. This finding, which has been cited numerous times in the literature (e.g. Justino, 2011; Buvinić *et al*, 2014) and has then been replicated by UNESCO (2010), is based on the coefficient of the triple interaction term in a regression similar to the one we run in column (1) of table 6, in which however the individual components of the triple interaction are not controlled for. We believe this to be the main reason behind the discrepancy in the results, as whenever we omit the double interaction terms from the regression, we also obtain a positive coefficient on the triple interaction. It should also be pointed out that the authors focus on absolute drops in schooling, while our analysis looks at relative changes. Given that boys start from higher levels of education, the difference in absolute terms is smaller, although we still find it to be significantly larger for girls.

[28] Most micro-level studies make the distinction between the impact on primary versus secondary schooling attainments (Shemyakina, 2011; Rodriguez and Sanchez, 2012; Swee, 2009; Chamarbagwala and Morán, 2011; Oyelere and Wharton, 2013), without however being able to comprehensively study the different channels. There are also a number of cross-country studies that rely instead on aggregated data to look at the impact of conflicts on school enrolment and/or dropouts across primary and secondary schooling separately (Chen et al, 2008; Poirier, 2012; Stewart et al., 2001).

schooling, our results indicated that students were not less likely to enroll as a consequence of the conflicts. The large reduction in schooling attainments was instead driven by both an increase in dropouts and slower grade progression. The latter result puts the dramatic drop in education attainments in a somewhat different perspective, as it indicates that part of the drop is due to students simply delaying their education. Although more research is needed to identify the most effective policy, these findings suggest that to lessen the human capital shock in a post-conflicts environment, policies aimed at retaining primary school students (such as conditional cash transfer programs) and incentivizing their re-enrollment (such as the creation of specific programs or classes for older students) are likely to be more effective than those aimed at lowering barriers to enrollment (for instance providing school uniforms).

For secondary schooling we instead estimated a large drop in enrollment, which however can be fully attributed to the lower share of students completing primary school. Among those that actually completed the primary cycle, students were in fact more likely to enroll in secondary schooling in the aftermath of the conflict, suggesting that conflicts reinforced a selection effect, by which only the most able and motivated students tend to complete primary school and continue to secondary studies.

Finally, we failed to provide evidence for the importance of localized channels in explaining the large drop in educational attainments, as our within-country analysis could not detect a robust link between the intensity of the different forms of violence and the drop in schooling. This (lack of) result stands in contrast with findings of previous, less fine-grained, analyses on the impact of the 1994 genocide on schooling in Rwanda (Akresh and de Walque, 2011; UNESCO, 2010). It also contrasts with a number of empirical studies on Rwanda that find the intensity of the violence (and of the 1994 genocide in particular) to be associated with worse local-level outcomes, such as income (Serneels and Verpoorten, 2013), coffee production (Guariso and Verpoorten, 2013), and domestic violence (La Mattina, 2014). One possible explanation is that, compared to the other dimensions studied in the literature, education relies more heavily on factors such as infrastructures and teachers, which are more likely to have been heavily affected also by the other forms of violence that hit the country in the nineties, and by a general drying up of the national education budget. This means that the large drop in schooling that we estimated is due to nationwide events and/or national channels, rather than to the localized impact that certain forms of violence had. This would explain why schooling

attainments dropped sharply even areas where conflict intensity was relatively low. [29] This finding calls for caution in the design of post-conflict policies. After the end of the genocide there has been a proliferation of scholarship programs targeting especially regions where the genocide was more intense.[30] In light of our findings, there is a high risk of missing out on students that would be equally in need for support, which may lead to the creation of inequalities and grievances.

Although the fine-grained census data allowed us to dig deeper into the investigation of the link between conflicts and schooling, our study has some caveats and limitations. First, our estimations based on the difference-in-differences approach crucially depend on the definition of the control group used for the construction of the counterfactual. We presented a large number of checks for the validity of our empirical strategy, but we have also shown, when analyzing the heterogeneous effects across gender, that the exact definition of the counterfactual can affect the magnitude of the estimate (although the overall conclusion remains untouched). Second, our findings may not be easily generalized to other conflicts, e.g. of longer duration and in larger countries. As such, longer conflicts are more likely to affect enrollment, and conflicts in larger countries may have 'localized' effects.[31] Finally, our research still leaves some questions unanswered, notably regarding the exact mechanisms underlying the general channels. Future work based on micro-level data should for instance investigate which individual- or household-level characteristics determine the decision to drop out or delay schooling, so to guide the design of more targeted policies. Moreover, our focus was on the quantity of education, i.e. the number of years of schooling, but we could not say anything about the quality of education.[32] Further research is needed to better investigate the link between the quantity and the quality of education

[29] A recent working paper by Rogall and Yanagizawa-Drott (2013) finds that households located in areas where the genocide was more intense actually displayed better living standards (i.e. more assets, higher consumption and higher agricultural output per capita) six years after the end of the genocide. A possibly complementary explanation for our results is therefore that initial worsening of the schooling performances in more severely affected areas have been partly counteracted in the following years by improvements in living standards (which the authors attribute to a "Malthusian effect").

[30] Information contained in a nationally representative survey collected in 1999/2000 (EICV1), indicates that less than 5% of the students enjoyed a scholarship in the northern provinces, compared to more than 12% in some of the central and southern provinces.

[31] In the case of larger countries, it is also more likely that some parts of the country remained unaffected by violence, opening the way for another DiD strategy, in which the evolution in the unaffected areas is taken as a control group (ideally instrumenting the within-country variation in violence).

[32] In their paper on the consequences of the genocide on economic performances, among other things, Rogall and Yanagizawa-Drott (2013) find some evidence of a worsening in the cognitive skills of children in more genocide-affected areas.

in the aftermath of violence. As this research has shown, the availability of fine-grained data is crucial for addressing these increasingly fine-grained research questions. Constant improvements in the collection of timely and high-quality micro-level data will hopefully make it possible to provide additional answers.

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Figures

Figure 1. Average completed years of schooling by age in Rwanda

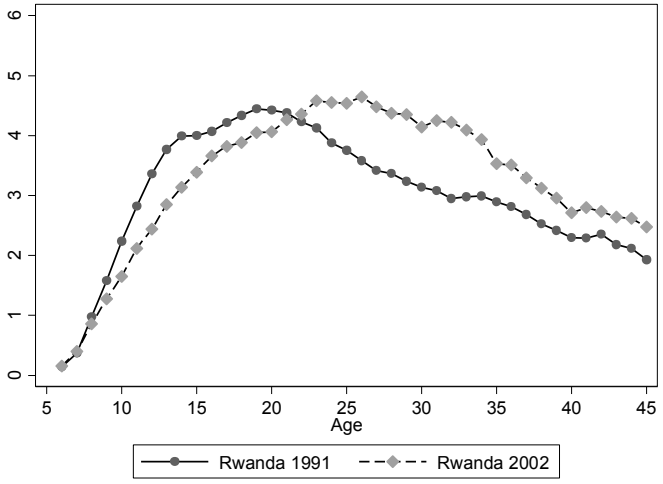


Figure 2. Average completed years of schooling by age in Uganda

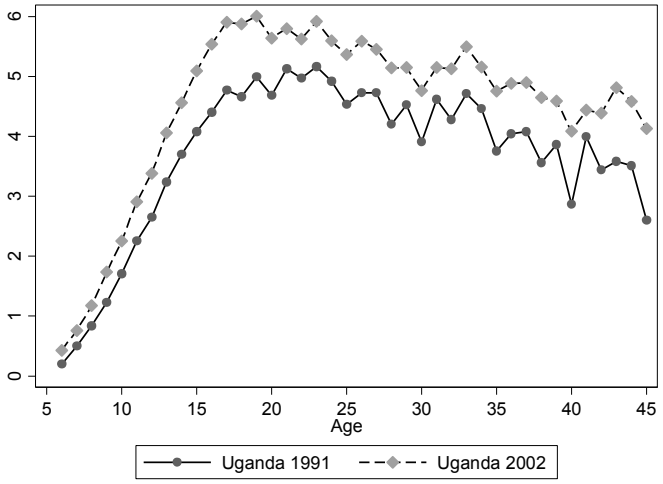
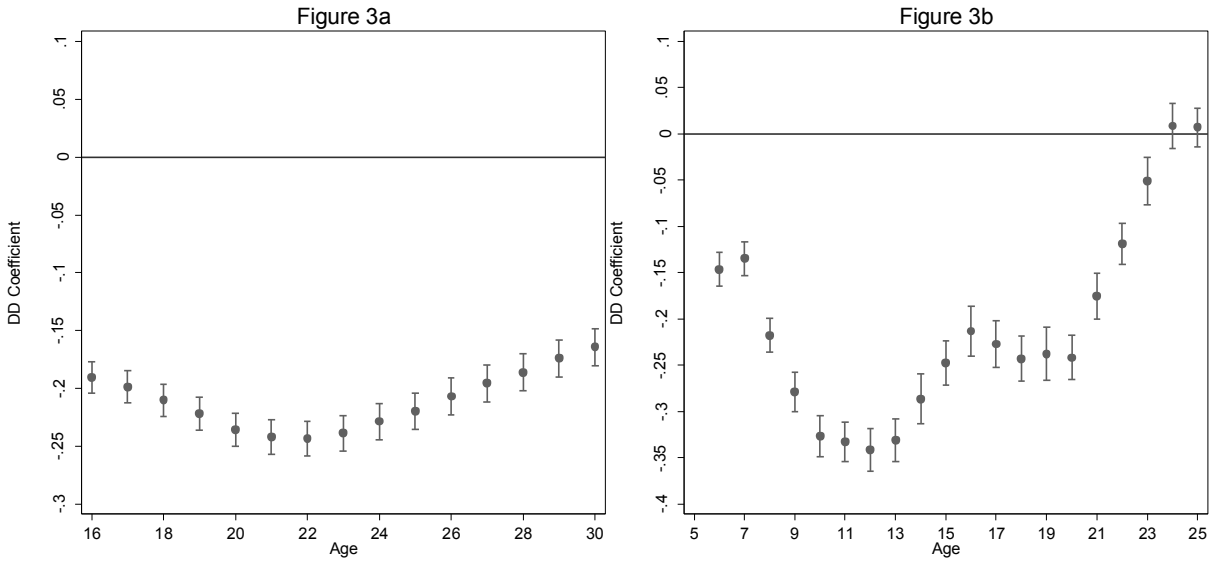
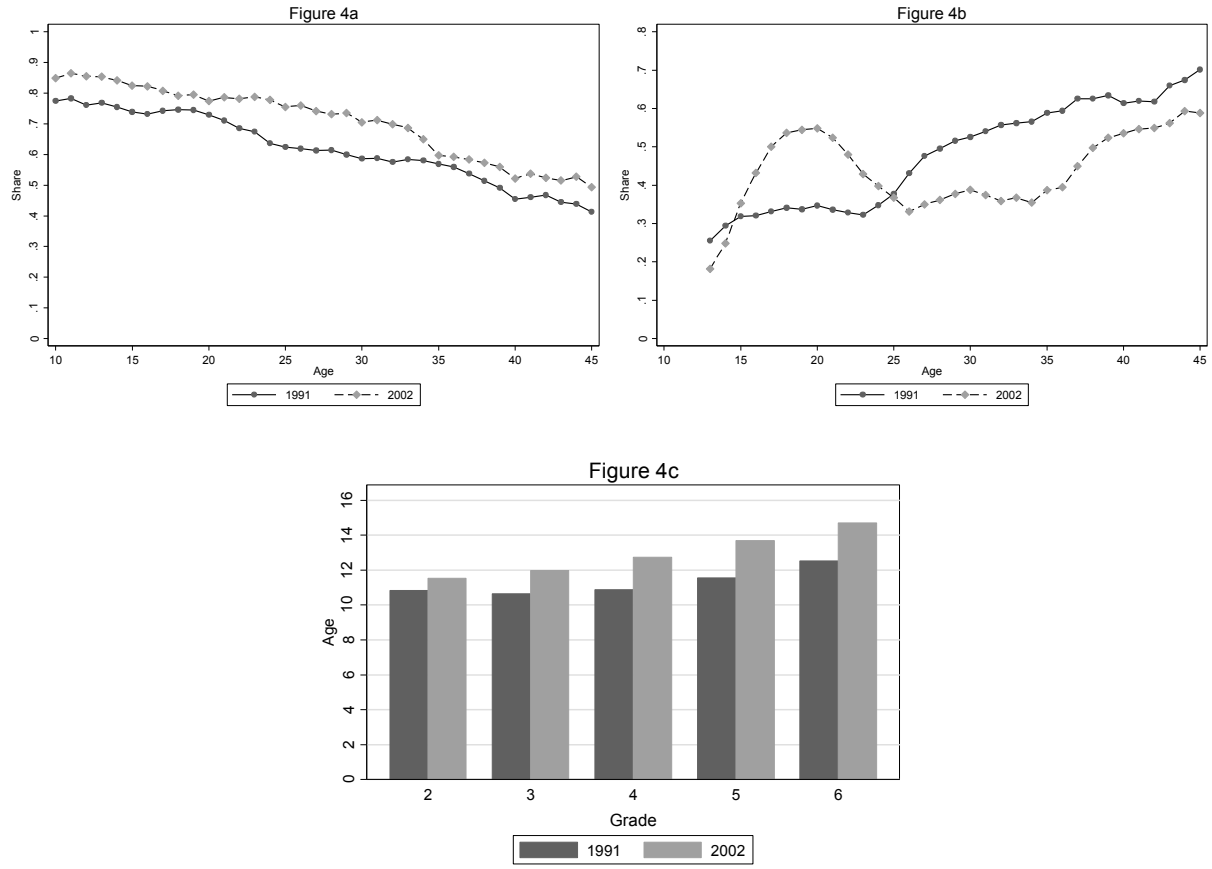


Figure 3. Estimation of the drop in schooling attainments by age



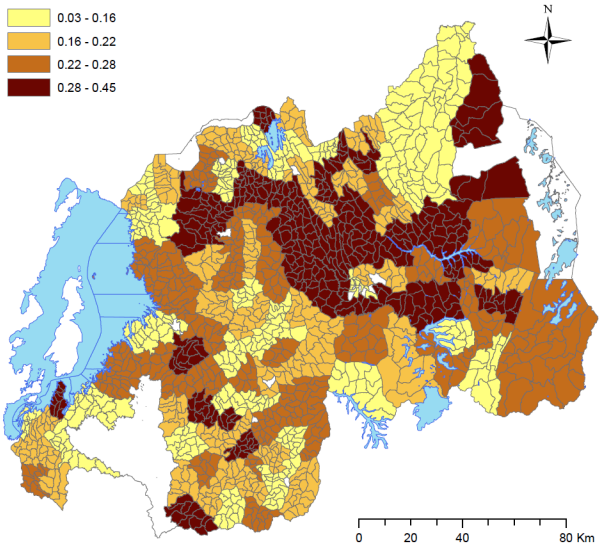
Notes: Figure 3a shows the point estimates and the 95% CI for the DiD coefficients of the interaction term between *Young Cohort* and *Post-Conflict Round* obtained in different regressions in which the threshold dividing young and old age cohort moved from 16 years to 30 years of age. Figure 3b shows instead the point estimates and the 95% CI for the different DiD coefficients obtained from a regression similar to the one reported in column (3) of Table 2, in which the interaction term between *Young Cohort* and *Post-Conflict Round* has been replaced by a set of interaction terms between each individual year included in the young cohort - i.e. from *age 6* to *age 25* - and *Post-Conflict Round*.

Figure 4. Channels - Primary Schooling



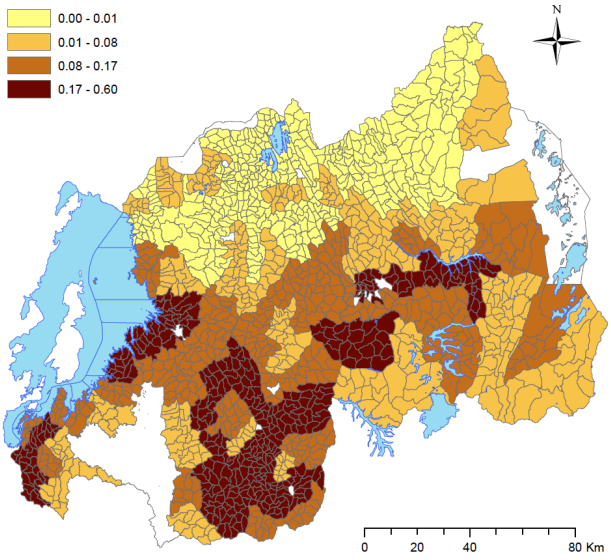
Note: Figure 4a shows enrollment rate by age, where the enrollment rate is proxied by the share of individuals that either are currently enrolled in the first year of school or completed at least one year. Figure 4b only considers individuals that ever enrolled in school and shows the share of those that dropped out (i.e. are not students any more) before completing the primary schooling cycle (i.e. 6 years). Figure 4c shows reports the average age of students enrolled in primary schooling for grade 2 to 6.

Figure 5. Spatial distribution of the drop in schooling



Note: The figure shows the spatial distribution of the drop in schooling attainments across Rwanda. The drop has been estimated calculating the DiD model separately for each commune. Different colors identifies different quartiles, with a darker shade indicating a larger drop. The administrative map also report the sector borders (i.e. one administrative level below the commune). No data are available for areas in white (they mostly include national parks and forests).

Figure 6. Spatial distribution of the share of Tutsi in 1991



Note: The figure shows the spatial distribution of the share of Tutsi living in each commune by 1991. The information is taken from the 1991 census. Different colors identifies different quartiles, with a darker shade indicating a higher share. The administrative map also report the sector borders (i.e. one administrative level below the commune). No data are available for areas in white (they mostly include national parks and forests).

Tables

Table 1. Summary Statistics

	Full sample					1991	2002
Variable	**Observations**	**Mean**	**Std Dev**	**Min**	**Max**	**Mean**	**Mean**
Panel A: Rwanda Census							
Years of Schooling (YoS)	967678	3.06	2.89	0	16	2.83	3.19
Post-Conflicts Round	967678	0.52	0.50	0	1	0	1
Young Cohort *(6-25 years)*	967678	0.68	0.46	0	1	0.67	0.69
Female	967678	0.52	0.50	0	1	0.51	0.54
Non-poor	967678	0.62	0.49	0	1	0.41	0.81
Age of Household Head	967678	42.48	13.28	10	100	42.63	42.34
YoS Household Head	967678	2.96	3.22	0	16	2.72	3.19
Number of Children Under Five	967678	0.73	0.93	0	6	0.43	1.01
Rural	967678	0.86	0.33	0	1	0.87	0.85
Student	805742	0.23	0.42	0	1	0.19	0.26
Ever Been to Primary School	805742	0.73	0.44	0	1	0.70	0.77
Ever Been to Secondary School	805742	0.12	0.32	0	1	0.12	0.11
Dropped out from Primary	805742	0.27	0.45	0	1	0.26	0.29
Dropped out from Secondary	805742	0.01	0.11	0	1	0.01	0.02
Migrated in the previous 8 years	967678	0.16	0.37	0	1	0.12	0.21
Panel B: Uganda Census							
Years of Schooling (YoS)	2691853	3.82	3.49	0	16	3.37	4.08
Post-Conflicts Round	2691853	0.63	0.48	0	1	0	1
Young Cohort *(6-25 years)*	2691853	0.70	0.46	0	1	0.70	0.70
Female	2691853	0.51	0.50	0	1	0.52	0.51
Non-poor	2691853	0.75	0.43	0	1	0.69	0.79
Age of Household Head	2691853	41.09	14.18	10	95	41.13	41.07
YoS Household Head	2691853	4.90	4.02	0	16	4.26	5.28
Number of Children Under Five	2691853	0.40	0.77	0	9	0.40	0.41
Rural	2691853	0.87	0.34	0	1	0.87	0.87
Student	2194582	0.32	0.47	0	1	0.25	0.36
Ever Been to Primary School	2194582	0.79	0.41	0	1	0.72	0.83
Ever Been to Secondary School	2194582	0.16	0.37	0	1	0.13	0.19
Dropped out from Primary	2194582	0.26	0.44	0	1	0.29	0.25
Dropped out from Secondary	2194582	0.04	0.19	0	1	0.03	0.04

Notes: Student-related variables are only available for individuals aged 10 or above. *Student* captures whether an individual reports studying as main occupation. *Ever Been to Primary School* and *Ever Been to Secondary School* takes value one if the student has completed at least one year of Primary or Secondary school, respectively. *Dropped out from Primary* takes value one if student has completed at least the first year of primary schooling but did not complete the full cycle (equal to 6 years for Rwanda and 7 years for Uganda) and is not a student any more. *Dropped out from Secondary* takes value one if student has completed at least the first year of secondary schooling but did not complete the lower cycle (equal to 3 years for both Rwanda and Uganda) and is not a student any more.

Table 2. Difference-in-Difference estimation of the drop in schooling attainments

Dependent Variable: ln (Years of Schooling+1)			
	(1)	(2)	(3)
Young cohort:	6-25	6-25	6-25
Old cohort:	26-45	26-45	26-45
Young Cohort * Post-Conflicts Round	-0.121***	-0.252***	-0.220***
	(0.010)	(0.009)	(0.008)
Post-Conflicts Round	0.180***	0.205***	0.103***
	(0.011)	(0.008)	(0.006)
Young Cohort	0.080***		
	(0.018)		
Controls X			
Female			-0.097***
			(0.008)
Non-poor			0.141***
			(0.006)
Age of HH Head			0.007***
			(0.000)
Education HH Head			0.100***
			(0.002)
Number of Children Under 5			-0.026***
			(0.002)
Rural			-0.080***
			(0.016)
Constant	0.991***	0.569***	-0.382***
	(0.027)	(0.014)	(0.014)
Age FE	NO	YES	YES
Adminstrative unit FE	Commune	Commune	Commune
Observations	967,678	967,678	967,678
R-squared	0.005	0.213	0.355

Notes: *Non-poor* is an indicator variable for living in an household whose asset index is above the population mean. The indexis constructed giving equal weight to four different indicator variables that capture households characteristics: whether the dwelling is owned by the household; whether there is (any type) of toilet; whether there is a finished floor; and whether there is tiles or cement roof. Robust standard errors in parentheses, clustered at the commune level. There are 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Robustness checks

Dependent Variable: ln (Years of Schooling+1)				
	(1)	(2)	(3)	(4)
Country:	Rwanda	Uganda	Rwanda	Rw & Ug
Young cohort:	6-25	6-25	26-45	6-25
Old cohort:	26-45	26-45	46-65	
Young Cohort * Post-Conflicts Round	-0.220***	-0.012	0.031***	
	(0.008)	(0.015)	(0.007)	
Rwanda * Post-Conflicts Round				-0.215***
				(0.013)
Post-Conflicts Round	0.103***	0.129***	0.056***	0.143***
	(0.006)	(0.013)	(0.004)	(0.014)
Controls X				
Female	-0.097***	-0.235***	-0.277***	-0.081***
	(0.008)	(0.025)	(0.008)	(0.014)
Non-poor	0.141***	0.108***	0.073***	0.136***
	(0.006)	(0.006)	(0.004)	(0.006)
Age of HH Head	0.007***	0.005***	0.010***	0.004***
	(0.000)	(0.000)	(0.000)	(0.000)
Education HH Head	0.100***	0.080***	0.189***	0.051***
	(0.002)	(0.002)	(0.004)	(0.001)
Number of Children Under 5	-0.026***	-0.021***	-0.003	-0.073***
	(0.002)	(0.004)	(0.002)	(0.006)
Rural	-0.081***	-0.134***	-0.055***	-0.135***
	(0.016)	(0.010)	(0.017)	(0.011)
Constant	-0.382***	-0.055	0.282***	0.080***
	(0.014)	(0.048)	(0.034)	(0.028)
Age FE	YES	YES	YES	YES
Adminstrative unit FE	Commune	District	Commune	District & Commune
Observations	967,678	2,691,853	423,784	2,552,291
R-squared	0.355	0.465	0.558	0.455

Notes: Robust standard errors in parentheses, clustered at the district level for Uganda and at the commune level for Rwanda. There are respectively 38 Districts and 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

Table 4. The effects by gender

Dependent Variable: ln (Years of Schooling+1)			
	(1)	(2)	(3)
Country:	Rwanda	Uganda	Rw & Ug
Young cohort:	6-25	6-25	6-25
Old cohort:	26-45	26-45	
Young Cohort * Post-Conflicts Round	-0.156***	0.005	
	(0.008)	(0.013)	
Post-Conflicts Round	0.026***	0.063***	0.094***
	(0.007)	(0.012)	(0.014)
Female * (Young Cohort * Post-Conflicts Round)	-0.138***	-0.021	
	(0.007)	(0.014)	
Female * Young Cohort	0.318***	0.405***	
	(0.007)	(0.022)	
Female * Post-war round	0.163***	0.118***	0.095***
	(0.008)	(0.014)	(0.010)
Female * (Rwanda * Post-Conflicts Round)			-0.067***
			(0.010)
Rwanda * Post-Conflicts Round			-0.182***
			(0.014)
Female * Rwanda			0.135***
			(0.025)
Controls X	YES	YES	YES
Age FE	YES	YES	YES
Adminstrative unit FE	Commune	District	District & Commune
Observations	967,678	2,691,853	2,552,291
R-squared	0.361	0.476	0.457

Notes: The list of controls X includes all the variables reported in Table 2. Robust standard errors in parentheses, clustered at the district level for Uganda and at the commune level for Rwanda. There are respectively 38 Districts and 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Channels

	PANEL A: Primary school			PANEL B: Secondary school			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	Been to school	Dropout rate	Age Students	Been to school	Been to school	Dropout rate 3 years	Age Students
Young cohort:	10-14	13-21	10-16	14-22	14-22	17-25	14-25
Old cohort:	26-45	26-45		26-45	26-45	26-45	
Young Cohort * Post-Conflicts Round	-0.002	0.229***		-0.109***	0.057***	0.040***	
	(0.006)	(0.007)		(0.005)	(0.010)	(0.007)	
Post-Conflicts Round	0.031***	-0.091***	1.648***	0.021***	0.026***	0.018**	0.420***
	(0.004)	(0.005)	(0.023)	(0.002)	(0.006)	(0.007)	(0.037)
Controls X	YES	YES	YES	YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES	YES
Grade FE	NO	NO	YES	NO	NO	NO	YES
Adminstrative unit FE	Commune	Commune	Commune	Commune	Commune	Commune	Commune
Observations	487,465	427,100	123,433	590,659	198,681	65,391	41,415
R-squared	0.276	0.151	0.334	0.190	0.249	0.096	0.460
Mean value dependent variable for:							
Young Cohort 1991	82%	32%	11.4	21%	43%	12%	16.3
Young Cohort 2002	88%	43%	12.4	14%	53%	16%	17.8
Old Cohort 1991	58%	56%	-	7%	26%	19%	-
Old Cohort 2002	65%	43%	-	12%	32%	19%	-

Notes: In Column 1 the dependent variable is an indicator taking value one if the individual has is currently enrolled in the first year of school or completed at least one year. In column 2 the dependent variable is an indicator taking value one if the individual dropped out from primary school before completing the 6-years cycle; the sample is restricted to individuals that completed at least one year of primary school. In column 3 we run empirical model 3 described in the text, where the dependent variable is the age of the student; the sample is restricted to students aged 10 to 16 enrolled in grades 2 to 6. In Column 4 the dependent variable is an indicator taking value one if the individual is currently enrolled in the first year of secondary school or completed at least one year. In column 5 the dependent variable is the same as in column 4, but the sample is restricted to individuals that have completed the 6 years of primary school. In Column 6 the dependent variable is an indicator taking value one if the individual dropped out from secondary school before completing the 3-years (lower) cycle; the sample is restricted to individuals that completed at least one year of secondary school. In column 7 we run empirical model 3 described in the text, where the dependent variable is the age of the student; the sample is restricted to students aged 14 to 25 enrolled in any secondary school grade (grades 7 to 13). The list of controls X includes all the variables reported in Table 2. Robust standard errors in parentheses, clustered at the commune level. There are 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Genocide intensity and the drop in schooling

Dependent Variable: ln (Years of Schooling+1)						
	(1)	(2)	(3)	(4)	(5)	(6)
Young cohort:	6-25	6-25	6-25	6-25	6-25	6-25
Old cohort:	26-45	26-45	26-45	26-45	26-45	26-45
Conflict Intensity Measure:	Share Tutsi	Mass Graves	Death rate	Prosecution rate	GEMI	Combination (PCA)
Young Cohort * Post-war round	-0.230***	-0.218***	-0.217***	-0.201***	-0.216***	-0.227***
	(0.011)	(0.009)	(0.009)	(0.029)	(0.017)	(0.008)
Post-War Round	0.114***	0.105***	0.104***	0.108***	0.108***	0.107***
	(0.009)	(0.007)	(0.007)	(0.022)	(0.013)	(0.006)
Conflict Intensity * (Young * Post-war)	0.031	-0.008	-0.058	-0.006	-0.023	-0.003
	(0.048)	(0.008)	(0.041)	(0.006)	(0.036)	(0.004)
Conflict Intensity * Young Cohort	-0.445***	-0.044**	-0.087	-0.027***	-0.170***	-0.025***
	(0.119)	(0.018)	(0.103)	(0.010)	(0.063)	(0.005)
Conflict Intensity * Post-war round	-0.063	-0.004	0.002	-0.001	-0.004	-0.002
	(0.044)	(0.005)	(0.054)	(0.005)	(0.026)	(0.003)
Controls X	YES	YES	YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES
Adminstrative unit FE	Commune	Commune	Commune	Commune	Commune	Commune
Observations	967,678	967,678	967,678	967,678	967,678	927,535
R-squared	0.356	0.356	0.355	0.355	0.355	0.356

Notes: All conflict variables are defined at the commune level. *Share Tutsi* indicates the 1991 share of Tutsi living in the commune. *Mass Graves* counts the number of mass graves located within a commune. *Death rate* refrs to the deaths caused by the 1994 political violence and is obtained by combining information from a different number of sources using a latent variable model. *Prosecution rate* indicate the (logarithm of the) share of category 1 and category 2 suspects for committing crimes during the genocide, weighted by the share of Hutu living in the commune. *GEMI* indicates the Genocide Excess Mortality Index and is obtained as the weighted sum of the first differences of five excess mortality proxies taken from the census data, combined with six genocide-specific proxies related to the share of geocide suspects. *Combination (PCA)* is the first principal component obtained performing Principal Component Analysis on all the previous 5 measures. See Table C1 for more details and sources. The list of controls X includes all the variables reported in Table 2. Robust standard errors in parentheses, clustered at the commune level. There are 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Alternative forms of violence

Dependent Variable: ln (Years of Schooling+1)			
	(1)	(2)	(3)
Young cohort:	6-25	6-25	6-25
Old cohort:	26-45	26-45	26-45
Conflict Intensity Measure:	CEMI	Extrajudicial killings	Externally Displaced
Young Cohort * Post-war round	-0.189***	-0.219***	-0.220***
	(0.016)	(0.008)	(0.012)
Post-War Round	0.107***	0.103***	0.126***
	(0.013)	(0.007)	(0.009)
Conflict Intensity * (Young * Post-war)	-0.080*	-0.507	0.004
	(0.043)	(0.634)	(0.025)
Conflict Intensity * Young Cohort	0.027	3.116***	-0.087
	(0.082)	(1.157)	(0.053)
Conflict Intensity * Post-war round	-0.011	0.014	-0.050***
	(0.031)	(0.755)	(0.017)
Controls X	Yes	Yes	Yes
Age FE	Yes	Yes	Yes
Adminstrative unit FE	Commune	Commune	Commune
Observations	967,678	967,678	967,678
R-squared	0.355	0.355	0.355

Notes: All conflict variables are defined at the commune level. *CEMI* indicates the Civil war Excess Mortality Index and is obtained as the part that is not explained by the GEMI of the weighted sum of the first differences of five excess mortality proxies taken from the census data. *Extrajudicial killings* indicate the share of extrajudicial killings of civilians during the (counter)insurgency operations that took place till the late nineties in 29 communes in the Northwest of Rwanda. *Extrenally displaced* indicates the share of individuals living in a household in whih (at least) one child was born in DRC, Tanzania or Burundi between 1994 and 1998. See Table C1 for more details and sources. The list of controls X includes all the variables reported in Table 2. Robust standard errors in parentheses, clustered at the commune level. There are 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

APPENDICES

Appendix A - DHS

In this Appendix we provide more details concerning the comparison between the census and the DHS data. Both sources provide two different rounds of data bracketing the main conflict period in Rwanda, with just few years of difference: the census rounds were collected in 1991 and 2002; the DHS in 1992 and 2000. We use the DHS data to generate the same variables that we use in our main analysis in the text. The only exception concerns the asset index used to generate the non-poor indicator variable, for which we have to rely on somewhat different components. More specifically, in the paper we construct the asset index by giving equal weights to four different household-level indicator variables: whether there is (any type) of toilet; whether there is a finished floor; whether the dwelling is owned by the household; and whether there is tiles or cement roof. As we cannot exactly match these variables in the DHS data, we build a similar asset index that combines however only three variables: whether there is (any type) of toilet; whether there is a finished floor; and whether the household owns a radio. The summary statistics for the different variables are reported in Table A1, divided by source and data collection round.

In order to test how similar the two sources are, we run our main DiD regression (Eq.2) separately for each data source. Since the DHS data only provide information on the province of residence of the respondent (and not on the commune, as it is in the census data), we only include province fixed effects in the regressions (this explains the small difference between some of the results reported in this table and those in Table 2 in the paper) and we cluster standard errors at the enumeration level for the DHS and at the commune level for the census regressions. Table A2 shows that results obtained with DHS and census data are very similar. Despite the differences highlighted above, when all controls are included in the regression, both sources indicate a drop in schooling attainments slightly larger than 21% for the young cohort in the post-conflict period. Importantly, also the coefficients of all the different control variables are very similar. This check highlights the reliability of the Rwandan census data and, given its much higher resolution compared to the DHS, we therefore decide to focus on this source for our empirical analysis.

Table A1. Comparison between Census and DHS data – Summary Statistics

	Full sample					Pre-Conflict	Post-Conflict
Variable	**Observations**	**Mean**	**Std Dev**	**Min**	**Max**	**Mean**	**Mean**
Panel A: Census							
Years of Schooling (YoS)	967678	3.06	2.89	0	16	2.83	3.19
Post-Conflicts Round	967678	0.52	0.50	0	1	0	1
Young Cohort *(6-25 years)*	967678	0.68	0.46	0	1	0.67	0.69
Female	967678	0.52	0.50	0	1	0.51	0.54
Non-poor	967678	0.62	0.49	0	1	0.41	0.81
Age of Household Head	967678	42.48	13.28	10	100	42.63	42.34
YoS Household Head	967678	2.96	3.22	0	16	2.72	3.19
Number of Children Under Five	967678	0.73	0.93	0	6	0.43	1.01
Rural	967678	0.86	0.33	0	1	0.87	0.85
Panel B: DHS							
Years of Schooling (YoS)	52082	3.17	3.22	0	16	3.21	3.15
Post-Conflicts Round	52082	0.60	0.49	0	1	0	1
Young Cohort *(6-25 years)*	52082	0.70	0.46	0	1	0.67	0.71
Female	52082	0.52	0.50	0	1	0.51	0.53
Non-poor	52082	0.46	0.50	0	1	0.44	0.48
Age of Household Head	52082	43.09	13.68	7	98	44.30	42.29
YoS Household Head	52082	3.48	3.83	0	16	3.26	3.63
Number of Children Under Five	52082	0.83	0.83	0	5	0.87	0.81
Rural	52082	0.79	0.41	0	1	0.83	0.76

Notes: *Non-poor* an indicator for being above the population mean of an asset index, constructed by giving equal weights to different indicator variables that capture households characteristics. Due to data limitations the asset index is constructed differently in the census and DHS data. In the census data it includes four variables: whether there is (any type) of toilet; whether there is a finished floor; whether the dwelling is owned by the household; and whether there is tiles or cement roof. In the DHS data it considers instead three variables: whether there is (any type) of toilet; whether there is a finished floor; and whether the household owns a radio. For DHS data, the pre-conflict period refers to 1992 and the post-conflict period to 2000. For census data, pre-conflict refers to 1991 and post-conflict to 2002.

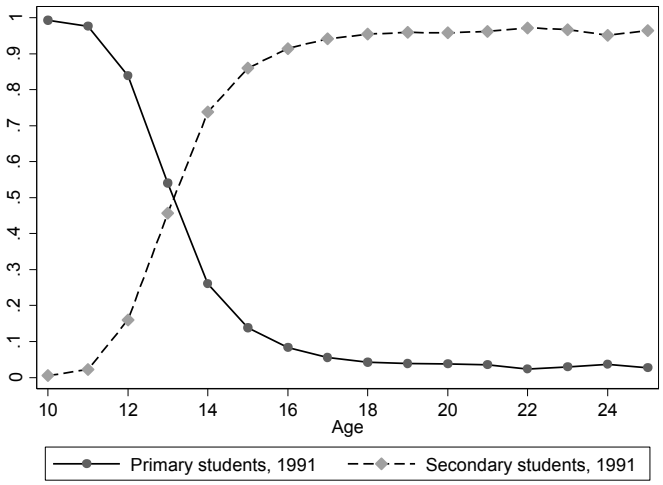
Table A2. Comparison between Census and DHS data – Regressions

Dependent Variable: ln (Years of Schooling+1)				
	(1)	(2)	(3)	(4)
Source:	Census	DHS	Census	DHS
Young cohort:	6-25	6-25	6-25	6-25
Old cohort:	26-45	26-45	26-45	26-45
Young Cohort * Post-Conflicts round	-0.253***	-0.226***	-0.211***	-0.213***
	(0.009)	(0.027)	(0.008)	(0.025)
Post-Conflicts Round	0.204***	0.157***	0.133***	0.119***
	(0.009)	(0.035)	(0.006)	(0.021)
Controls X				
Female			-0.093***	-0.080***
			(0.008)	(0.007)
Non-poor			0.157***	0.151***
			(0.004)	(0.008)
Age of HH Head			0.007***	0.006***
			(0.000)	(0.000)
Education HH Head			0.098***	0.074***
			(0.002)	(0.001)
Number of Children Under 5			-0.029***	-0.032***
			(0.002)	(0.004)
Rural			-0.028**	-0.090***
			(0.012)	(0.015)
Constant	0.102***	0.115***	-0.447***	-0.271***
	(0.029)	(0.034)	(0.019)	(0.030)
Age FE	YES	YES	YES	YES
Adminstrative unit FE	Province	Province	Province	Province
Observations	967,736	52,082	967,736	52,082
R-squared	0.198	0.282	0.352	0.416

Notes: Due to data limitations, *Non-poor* is constructed differently in the census and DHS regressions. In the census regressions the asset index that is used to define the non-poor variable is built using four variables: whether there is (any type) of toilet; whether there is a finished floor; whether the dwelling is owned by the household; and whether there is tiles or cement roof. In the DHS regressions the asset index is instead built using three variables: whether there is (any type) of toilet; whether there is a finished floor; and whether the household owns a radio. For DHS regressions the pre-conflict period refers to 1992 and the post-conflict period to 2000. For census regressions pre-conflict refers to 1991 and post-conflict to 2002. Standard errors are clustered at the enumeration level for the DHS regressions and at the commune level for the census regressions. There are 640 enumeration units and 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

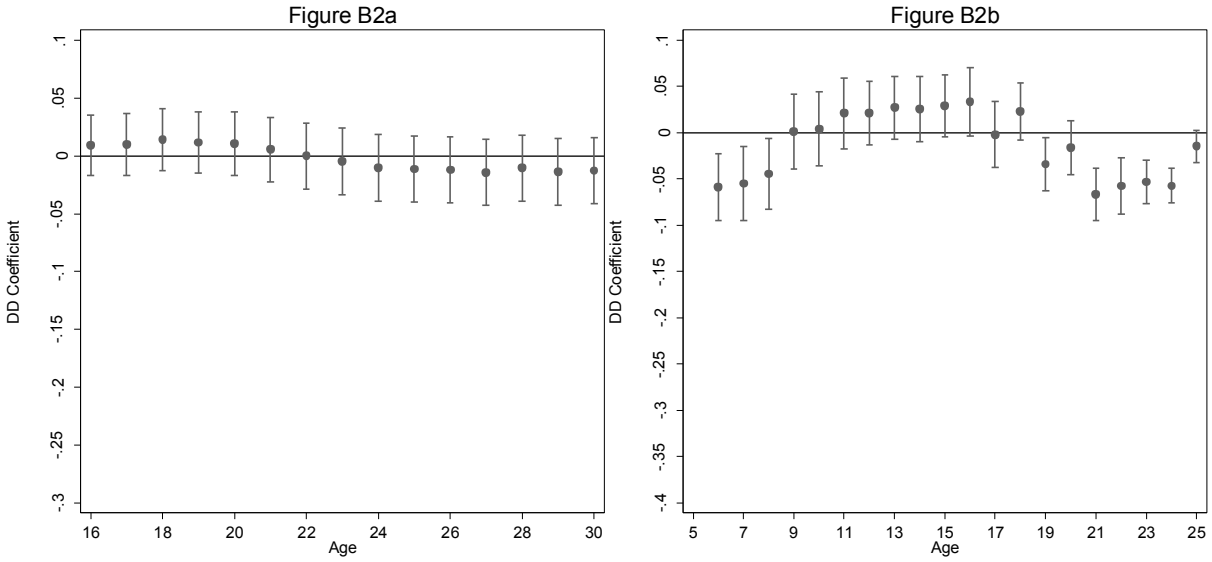
Appendix B – enrollment, drop-outs or delays?

Figure B1. Share of students enrolled in primary and secondary school in 1991 in Rwanda



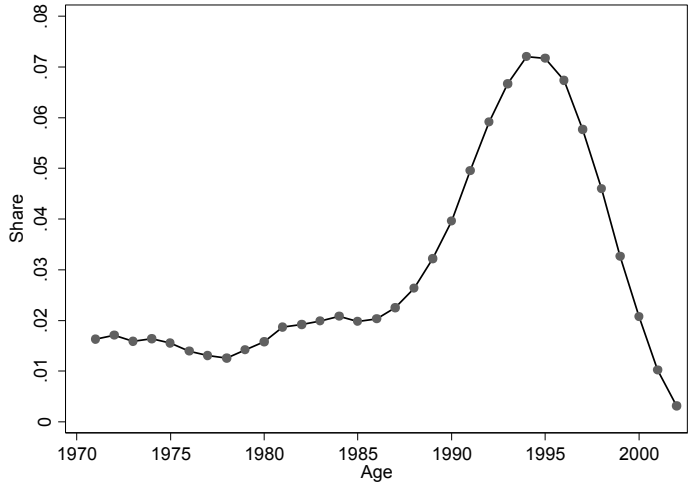
Note: the figure shows the share of students that are enrolled in primary and secondary school for each age, according to the Rwanda 1991 census.

Figure B2. Estimation of the drop in schooling attainments by age in Uganda



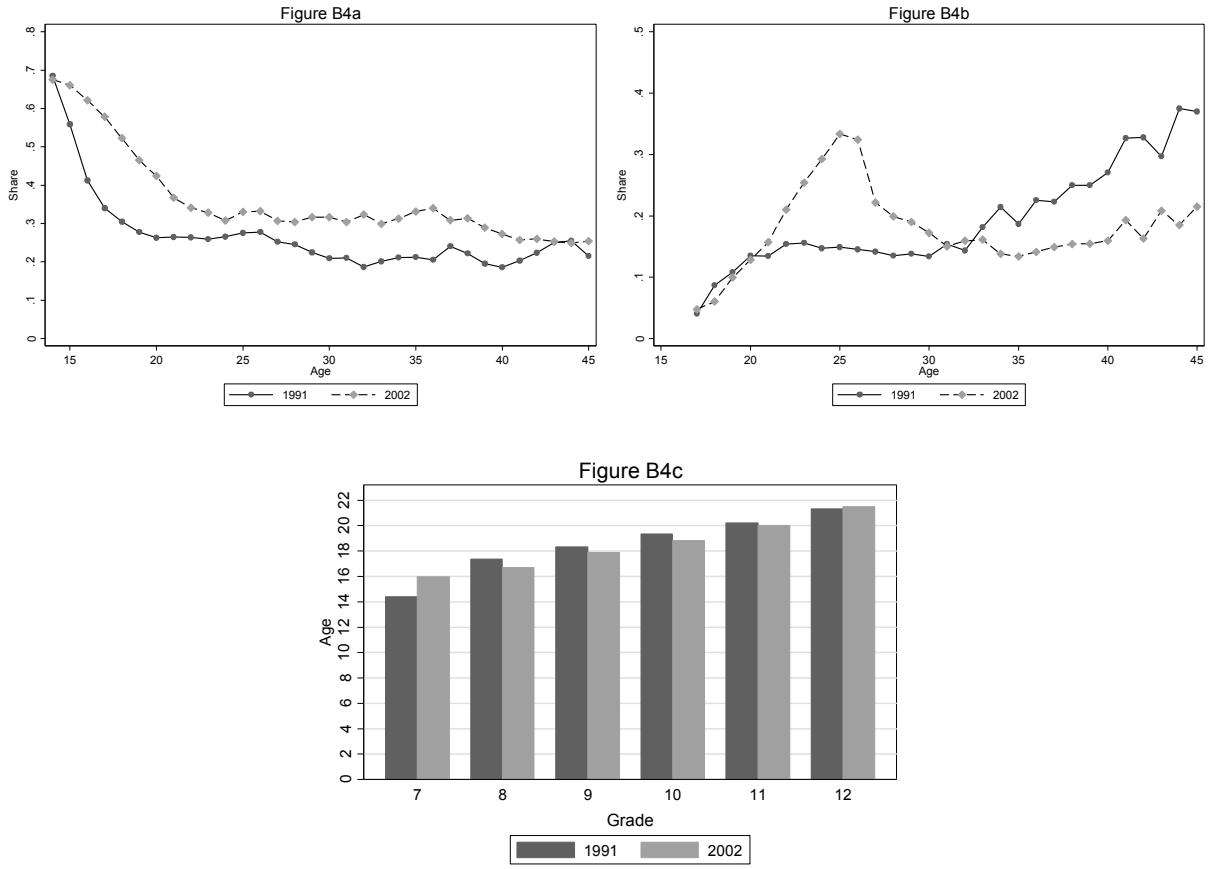
Notes: This Figure replicates Figure 3 in the text for Uganda. In particular, Figure B2a shows the point estimates and the 95% CI for the DiD coefficients of the interaction term between *Young Cohort* and *Post-Conflict Round* obtained in different regressions in which the threshold dividing young and old age cohort moved from 16 years to 30 years of age. Figure B2b shows instead the point estimates and the 95% CI for the different DiD coefficients obtained from a regression similar to the one reported in column (3) of Table 2, in which the interaction term between *Young Cohort* and *Post-Conflict Round* has been replaced by a set of interaction terms between each individual year included in the young cohort, i.e. from *age 6* to *age 25*, and *Post-Conflict Round*.

Figure B3. Estimated distribution of dropouts over time



Note: The figure illustrates how dropouts are distributed over time in Rwanda, based on the information contained in the 2002 census and under the assumption that each individual student started primary school at age 6 and completed one grade per year.

Figure B4. Channels - Secondary Schooling



Note: Figure B4a shows secondary schooling enrollment rate by age, conditional on completing the 6 years of primary school. The enrollment rate is proxied by the share of individuals that either are currently enrolled in the first year of secondary school or completed at least one year. Figure 4b also considers only individuals that ever enrolled in secondary school and shows the share of those that dropped out (i.e. are not students any more) before completing the 3rd years (i.e. lower-secondary). Figure 4c shows the average age of students enrolled in secondary school, for grade 7 to 12.

Table B1. Channels – different age cohort specifications

	Primary		Secondary	
	(1)	(2)	(3)	(4)
Dependent Variable:	Been to school	Dropout rate	Been to school	Been to school
Young cohort:	10-14	13-21	14-22	14-22
Old cohort:	15-45	22-45	23-45	23-45
Young Cohort * Post-Conflicts Round	0.007	0.170***	-0.102***	0.066***
	(0.005)	(0.006)	(0.005)	(0.009)
Post-Conflicts Round	0.025***	-0.034***	0.015***	0.019***
	(0.003)	(0.004)	(0.002)	(0.005)
Controls X	YES	YES	YES	YES
Age FE	YES	YES	YES	YES
Grade FE	NO	NO	NO	NO
Adminstrative unit FE	Commune	Commune	Commune	Commune
Observations	805,742	496,135	657,506	228,967
R-squared	0.216	0.141	0.190	0.244
Mean value dependent variable for:				
Young Cohort 1991	82%	32%	21%	43%
Young Cohort 2002	88%	43%	14%	53%
Old Cohort 1991	66%	50%	8%	27%
Old Cohort 2002	74%	43%	12%	32%

Notes: In Column 1 the dependent variable is an indicator taking value one if the individual is currently enrolled in the first year of school or completed at least one year. In column 2 the dependent variable is an indicator taking value one if the individual dropped out from primary school before completing the 6-years cycle; the sample is restricted to individuals that completed at least one year of primary school. In column 3 the dependent variable is an indicator taking value one if the individual is currently enrolled in the first year of secondary school or completed at least one year. In column 4 the dependent variable is the same as in column 3, but the sample is restricted to individuals that have completed the 6 years of primary school. The list of controls X includes all the variables reported in Table 2. Robust standard errors in parentheses, clustered at the commune level. There are 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

Appendix C - Subnational conflict-intensity and schooling

In this Appendix we provide more details concerning the analysis of the different forms of violence that took place in Rwanda during the 1991-2002 period. As explained in section 4.3 of the paper, there are overall eight different proxies for violence intensity. Five of these relate to genocide intensity, while the other three are meant to capture the intensity of civil war, (counter-)insurgency and the refugee crisis. The details and sources of each one these variables are reported in Table C1, while Table C2 and Table C3 report their summary statistics and pair-wise correlations, respectively. Finally, Figure C1 shows the geographical distribution of each variable, with darker-colored communes indicating higher violence intensity.

In the following two sections we provide more details concerning the robustness checks mentioned in Section "5.4. Localized effects of violence?". Section C.1 details the way we have addressed the five main potential source of bias that we believe are threatening our main DiDiD specification, while section C.2 describes the IV approach.

C.1.Confounding factors

Results reported in Table 6 clearly indicate the lack of any link between the within-country distribution of the drop in schooling and variation in the intensity of the genocide. Our results could however be confounded by a number of factors related to the distribution of genocide intensity across the country.

First, conflict intensity could be related to pre-war trends in schooling. This would be the case if for instance areas in which relatively more (or less) Tutsi were located were experiencing different paths in education attainments before the genocide. In order control for this, we exploit the large size of our dataset and add a complete set of commune time trends to the DiDiD regression model (i.e. for each one of the 145 communes c we create the indicator variable $Commune_c$ and we add to the regression the interaction $Commune_c$* *Post_Conflict*).

Second, armed conflicts are often associated with large migration flows, both during the conflict and in its aftermath. If especially highly educated adults moved out of the most affected communes, the gap between the young and old cohort remaining in those communes would be reduced, thus biasing our DiDiD estimate towards zero. The census data include information on place of birth, previous residence, current residence and time at current residence, allowing us to

trace an individual's migration history. To gauge whether migration is causing a bias we assign all individuals who moved between 1994 and 2002 - representing 21.7% of the 2002 sample - to their previous commune of residence and we re-estimate the DiDiD model.[33]

Third, de Walque and Verwimp (2010) demonstrate that the probability of being killed in the genocide was relatively higher for men, for the well-educated and for Tutsi, and was highest among the well-educated Tutsi male population. The targeted killing of Tutsi adults could thus bias downward our DiDiD estimates, since it would reduce the gap between the schooling of the young and old age cohorts in the post-war population in the provinces and communes with high genocide intensity. As it has been estimated that approximately 75% of Tutsi were killed during the genocide (Verpoorten, 2005), we gauge the magnitude of this bias by re-running our estimations after removing 75% of the most educated Tutsi from the 1991 population, i.e. after artificially introducing in the 1991 census a selection similar to the one caused by the genocide in 1994.

Fourth, in the aftermath of the conflict many assistance programs for genocide survivors were launched. One of the largest program is the FARG (Fonds d'Assistance aux Rescapés du Génocide), which by 1998 was awarding scholarships for secondary schooling to 24,000 students which is a sizeable share of genocide survivors at schooling age.[34] The support received by genocide survivors may partly confound the link between the intensity of the genocide and the drop in schooling, by lifting up educational attainments of the young cohort in genocide affected area (63% of the beneficiaries of the scholarship are reported to be paternal orphans). Unfortunately the census does not contain any information on scholarship. We can however rely on the information contained in a nationally representative survey collected in 1999/2000 (EICV1), to construct a measure for the share of students receiving the scholarship in each province. We can then get an idea of the potential bias these scholarships introduce in our estimation by removing from the young age cohort of each province a share of the best educated

[33] As explained in the main text, the 2001 administrative reform decreased the official number of Communes from 154 to 106, changing their name in Districts. While the 2002 census records the sector of current residence of each individual (which allows matching it to the pre-reform Commune), it only records the previous District of residence. In cases in which a District covers more than one pre-reform Commune we therefore assign the individual to the largest of those Communes. The estimation is however robust to randomly assigning to any of the corresponding communes as well as to completely dropping migrants from the analysis. Individuals previously living abroad are excluded from the sample.

[34] It has been estimated that in total 300,000 Tutsi survived (Prunier, 1998) – among which about 20% would have been at secondary schooling age in 1998. Information on the FARG can be found on their website www.farg.gov.rw.

individuals, equal to the province-level share of students that received a scholarship according to the EICV1 data.[35]

Fifth, given that most of the potential threats just outlined are induced by the comparison between young and old cohort in differently affected areas, we also compare the evolution in schooling attainments for the young cohort in Rwanda to the evolution in schooling attainments for the young cohort in Uganda (Eq.2), running two separate regressions with the Rwandan observations located in Communes that experienced genocide intensity above or below the median.

The results of these tests are reported in Table C4, where we consider the 1991 share of Tutsi as conflict proxy. Across all the different tests, the DiDiD coefficient remains statistically insignificant and the DiD coefficients remains very stable, thus confirming that areas more severely affected by the genocide did not experience a more (or less) severe drop in schooling attainments, compared to other areas.[36]

C.2. IV regression

One might however still be worried that our measures, defined at the commune level, are unfit to properly identify the localized impact of the genocide or that some additional confounding factors that we haven't been able to properly take into account are at the root of our (absence of) result. In this section we therefore abandon the DiD approach and switch to an instrumental variable (IV) approach to test for the specific impact of the genocide on schooling.

For our analysis we use the instrument proposed by Rogall (2014), consisting in a measure of the transportation costs that army and militia had to bare to reach the different sectors of Rwanda during the 100 days of the genocide. The instrument is constructed by interacting the distance between each sector and the nearest major road with the amount of rain that fell over that path during the genocide. The idea behind this instrument is that rainfall exogenously increased the difficulty of reaching some villages rather than others, as army and militia moved around the country using mostly motorized vehicles and the dirt roads became quickly muddy

[35] For instance, since 14% of students are reported to receive a scholarship in Gitarama province, compared to 5% in Gisenyi province, we drop from our sample 14% of the best educated children in the young cohort in Gitarama province and 5% in Gisenyi province.

[36] Considering any of the other four genocide proxies does not affect our conclusion (results not reported, but available from the authors). The DiDiD coefficients in Panel B (i.e. migration) and Panel D (i.e. scholarships) turn significant in a few cases, but the DiD coefficient remains always very large and significant.

and difficult to cross. We refer to the paper by Rogall for more details. Here we borrow from the paper the instrument as well as the full set of controls to ensure that the exclusion restriction is satisfied.[37] In our IV regression we instrument the share of Category 1 and Category 2 perpetrators combined.[38] In order to run the IV regressions consider the variable defined at the sector level (there are 1432 sectors in the 2002 census). Table C5 shows the result. Column 1 shows the results for a simple OLS regression using the new sample and control variables. It shows that educational attainments were if anything slightly higher in sectors in which there was a larger share of perpetrators. Column 2 reports the first stage of the IV strategy, showing that transportation costs are indeed significantly and negatively related to the share of perpetrators in a sector, with an F-statistic of 11.7. Finally, Column 3 reports the second stage, which confirms once again that genocide intensity is not significantly related to schooling attainments.

Additional References in the Appendix

Amnesty International. 1996-1998. *Amnesty international Reports*. Published by Amnesty International

Armstrong, D. 2014. Explanation of Rwanda Data Methodology. Mimeo.

Rogall, T. 2014. Mobilizing the Masses for Genocide. Mimeo

UNHCR. 2000. *The state of the world's refugees 2000: Fifty years of humanitarian action In The Rwandan genocide and its aftermath.* Oxford: Oxford University Press.

[37] The set of control variables includes what the author defines the "standard controls", "growing season controls" and "additional controls". *Standard controls* include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, long-term rainfall in the village during the 100 calendar days of the genocide period, rainfall along the way between village and main road during the 100 days of the genocide in 1994, long-term rainfall along the way between village and main road during the 100 calendar days of the genocide period and its interaction with distance to the main road. *Growing season controls* are rainfall during the growing season in 1994 in the village, long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road. *Additional controls* are distance to Kigali, main city, borders, Nyanza (old Tutsi Kingdom capital) as well as population density in 1991 and the number of days with RPF presence. All control variables, except "Number of Days with RPF presence", are in logs and al interactions are first logged and then interacted.

[38] Although the author uses the instrument to instrument just for "armed-group violence" – which is how he defines what the Gacaca courts listed as Category 1 perpetrators -, he shows in the paper that each additional army or militia man reaching a sector pushed more than 7 additional "civilian perpetrators" – i.e. what the Gacaca courts listed as Category 2 perpetrators - to join the violence.

Table C1. Conflict Intensity Variables – Sources and Description

Variable	Description	Source
Share of Tutsi 1991	Share of Tutsi living in the Commune in 1991. The variable is defined at the Commune level.	1991 Population Census
Mass Graves	Number of mass graves located within the boundary of an administrative sector. The variable is defined at the Sector level.	Genocide Studies Program at Yale University
Prosecution Rate	Share of people prosecuted for violent crimes committed during the Rwandan genocide. In 2001 Rwanda set up a court system that relied on the traditional judicial system to prosecute the hundreds of thousand people accused of committing crimes during the genocide. The suspected criminals were classified in three differnent categories. Here for the nominator we consider the sum of individuals included in the categories 1 and 2. Category 1 suspects have been accused of being planners or leaders during the genocide or of being murderers who distinguished themselves because of the zeal which characterized them in the killings, or of having committed rape and tortures. Category 2 suspects have been accused of being killers or of having committed act of physical violence during the genocide. The number of suspects is weighted by the estimated share of Hutu living in the Sector in 1991 (estimated from the Population Census). The natural logarithm is used in the analysis. The variable is defined at the Sector level.	National Service of Gacaca Jurisdiction
1994 Death Rate	Death rate caused by 1994 political violence, estimatd from information collected by the Ministry of Education in Rwanda, Ministry of Local Affairs in Rwanda, Ibuka (the Rwandan survivor organization), African Rights (the international human rights organization) and Human Rights Watch (the international human rights organization). The five data sources are combined and a latent variable model is used to estimate the number of casualties in the Rwandan genocide. See Armstrong (2014) for a detailed description of the methodology. The estimated number of deaths in a Commune is weighted by the population living in the Commune in 1991 (taken from the Population Census). Communes for which no deaths are reported are assigned value zero. The variable is defined at the Commune level.	Genodynamics Project
Excess Mortality Indexes (GEMI and CEMI)	Indexes capturing excess mortality between 1991 and 2002. An overall wartime excess mortality index is first generated as the weighted sum of the first differences of five excess mortality proxies, derived from the 1991 and 2002 population census rounds: the mortality of sons, the mortality of daughters, widowhood, orphanhood, and disability due to war or genocide. To estimate genocide excess mortality (GEMI), the set of excess mortality measures is augmented with six genocide-specific proxies: the three categories of genocide suspects and three categories of genocide survivors all taken proportional to the 1991 population. The civil war excess mortality index (CEMI) is then defined as the part of wartime excess mortality that cannot be explained by the GEMI. See Verpoorten (2012a) and Serneels and Verpoorten (2013) for further details.	From 1991 and 2002 Population Census Rounds (Verpoorten, 2012a; Serneels and Verpoorten, 2013)
Extrajudicial Killings	Share of extrajudicial killings of civilians during the (counter)insurgency operations that took place till the late nineties in 29 communes in the Northwest of Rwanda. Information is based on four Amnesty International Report. See Verpoorten (2012a) for more details. The estimated number of killings is weighted by the population living in the Commune in 1991 (taken from the Population Census). The variable is defined at the Commune level.	Amnesty International (1996, 1997a, 1997b, 1998)
Externally Displaced	Share of individuals living in a household in whih (at least) one child was born in DRC, Tanzania or Burundi between 1994 and 1998. The proxy is built using information contained in the 2002 census. It is estimated that almost 2 million people - about 25% of the Rwandan population – sought refuge in the camps set up across the border with neighboring countries, especially DRC, Tanzania and Burundi during the nineties. These externally displaced individuals only returned to Rwanda at a slow pace from 1996 onwards (UNHCR, 2000). Overall, 87% of the household in 2002 has at least one child born in that period and for 37% of these households the child was born in DRC, Tanzania or Burundi. The number of individuals is weighted by the population living in the Commune. The variable is defined at the Commune level.	2002 Population Census

Table C2. Conflict Intensity Variables – Summary Statistics

Variable	Observations	Mean	Std Dev	Min	Max
Share of Tutsi	145	0.13	0.13	0	0.60
Number of Mass Graves	145	0.48	0.80	0	4
Death rate	145	0.07	0.12	0	0.78
Prosecution rate	145	0.09	0.07	0.00	0.44
GEMI	145	0.39	0.20	0	1
CEMI	145	0.37	0.18	0	1
Extrajudicial Killings	145	0.00	0.01	0	0.05
Externally displaced	145	0.44	0.31	0.02	0.99

Notes: All variables defined at the Commune level. See Table C1 for details and sources.

Table C3. Conflict Intensity Variables – Correlations

	Share of Tutsi	Mass Graves	Death rate	Prosecution rate	GEMI	CEMI	Extrajudicial Killings	Externally Displaced
Share of Tutsi	1							
Mass Graves	0.34	1						
Death rate	0.56	0.10	1					
Prosecution rate	0.63	0.34	0.42	1				
GEMI	0.70	0.45	0.46	0.81	1			
CEMI	-0.15	-0.02	-0.04	-0.24	-0.02	1		
Extrajudicial Killings	-0.19	-0.10	-0.12	-0.19	-0.13	0.39	1	
Externally displaced	-0.04	0.18	-0.05	-0.07	-0.02	0.28	0.12	1

Note: All variables are defined at the Commune level (n=145). See Table C1 for details and sources.

Table C4. Robustness Checks

Dependent Variable: ln (Years of Schooling+1)

	Panel A Commune Trends		Panel B Migration		Panel C Selective Killings		Panel D Targeted Scholarships		Panel E Comparison with Uganda	
Model:	DD	DDD	DD	DDD	DD	DDD	DD	DDD	DD	DD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Young Cohort * Post-Conflicts Round	-0.221***	-0.230***	-0.217***	-0.215***	-0.231***	-0.236***	-0.277***	-0.276***		
	(0.008)	(0.011)	(0.008)	(0.011)	(0.008)	(0.011)	(0.008)	(0.011)		
Post-Conflicts Round	0.057***	0.004	0.097***	0.087***	0.119***	0.121***	0.104***	0.117***	0.146***	0.144***
	(0.007)	(0.009)	(0.006)	(0.009)	(0.006)	(0.009)	(0.006)	(0.008)	(0.015)	(0.015)
Conflict Intensity * (Young * Post-Conflicts)		0.032		-0.058		0.022		-0.076		
		(0.048)		(0.054)		(0.051)		(0.053)		
Conflict Intensity * Young Cohort		-0.446***		-0.290***		-0.436***		-0.441***		
		(0.118)		(0.080)		(0.122)		(0.117)		
Conflict Intensity * Post-Conflicts Round		0.299***		0.108**		-0.001		-0.066		
		(0.034)		(0.043)		(0.042)		(0.043)		
Rwanda * Post-Conflicts Round									-0.211***	-0.201***
									(0.014)	(0.015)
Controls X	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adminstrative unit FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune trends	Yes	Yes	No	No	No	No	No	No	No	No
Observations	967,678	967,678	921,899	921,899	937,520	937,520	938,458	938,458	2,193,585	2,248,257
R-squared	0.356	0.357	0.353	0.353	0.354	0.354	0.341	0.343	0.475	0.468

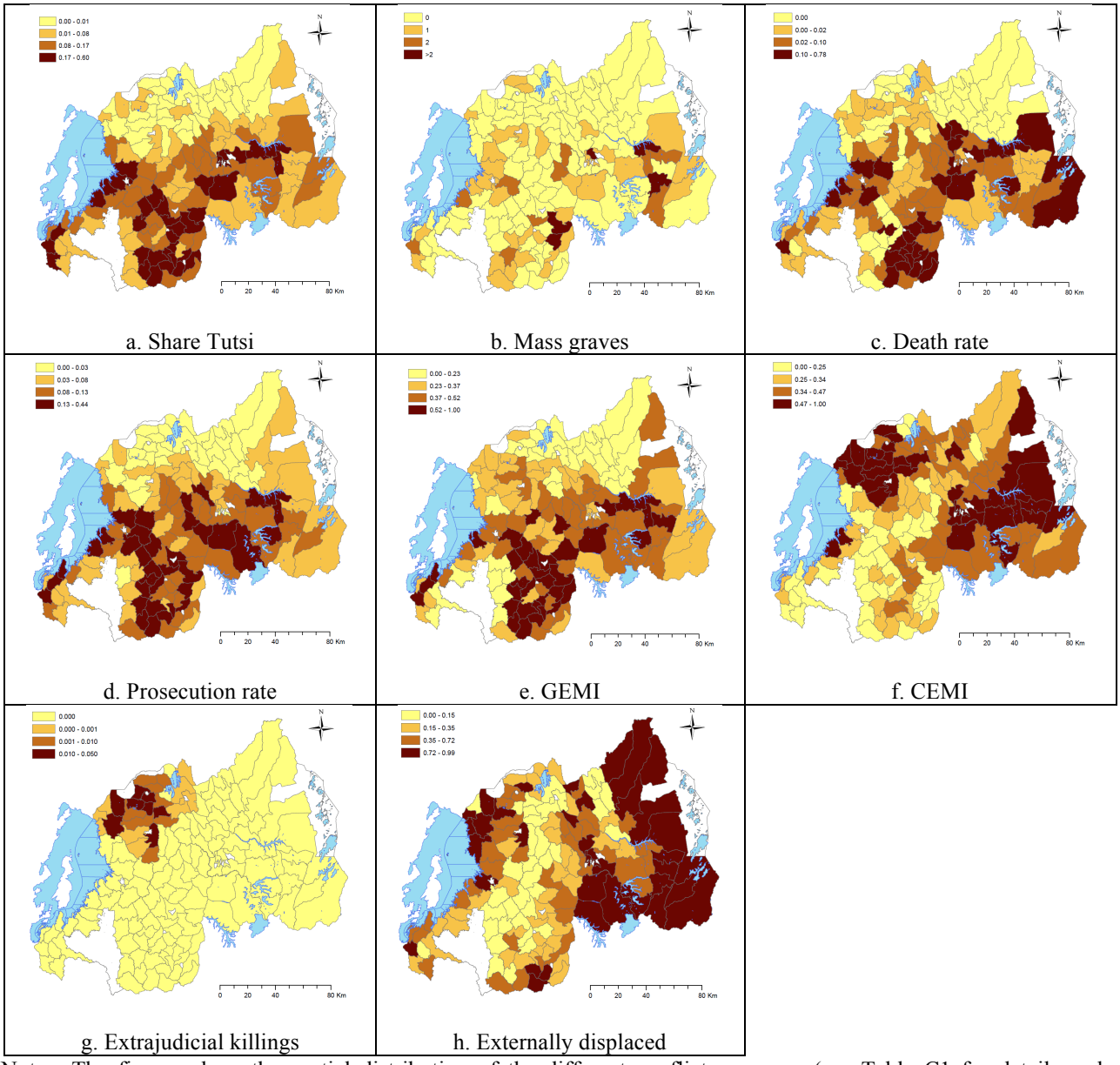
Notes: The *Conflict Intensity* variable considered in this table is the 1991 share of Tutsi living in the commune. In Panel A include a full list of commune time trends, meaning that for each one of the 145 communes in the sample we include the interaction *Commune*Post_Conflict*. In Panel B all individuals who moved between 1994 and 2002 - representing 21.7% of the 2002 sample - are assigned to their previous Commune of residence (as the 2002 census only records district of origin, for cases in which the 2002 desitrict covers two or more 1991 communes, we assign individuals to the largest of those communes); individuals previously living abroad are excluded from the sample. In Panel C we remove the 75% of Tutsi with the highest educational attainments from the 1991 sample. In Panel D we remove from the young age cohort of each province a share of the best educated individuals, equal to the province-level share of students that received a scholarship according to the EICV1 data. In Panel E we two separate regressions considering individuals located in communes that experienced genocide intensity below (column 9) or above (column 10) the median, and comparing them to Ugandan cohorts.The list of controls X includes all the variables reported in Table 2 in the main text. Robust standard errors in parentheses, clustered at the district level for Uganda and at the commune level for Rwanda. There are respectively 38 districts and 145 communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

Table C5. IV regression

Dependent variable:	(1) YoS OLS	(2) Prosecution rate OLS	(3) YoS IV
Prosecution rate	0.005*		0.040
	(0.003)		(0.030)
Transportation Costs		-0.531***	
		(0.155)	
Controls X	YES	YES	YES
Additional Controls	YES	YES	YES
Age FE	YES	YES	YES
Adminstrative unit FE	Prefecture	Prefecture	Prefecture
F test on instrument	-	11.7	-
Observations	327,939	327,939	327,939
R-squared	0.342	0.563	0.339

Notes: Sample restricted to the 2002 census and to the young cohort (age 6-25). *Prosecution rate* indicate the (logarithm of the) share of category 1 and category 2 suspects for committing crimes during the genocide, weighted by the share of Hutu living in the sector. Transportation Costs indicates the interaction between the distance from each sector to the nearest major road and the amount of rain that fell over that distance path during the 100 days of the 1994 genocide. Column 1 reports a simple OLS regression; the dependent variable is the natural logarythm of the years of schooling (plus 1). Column 2 reports the first stage of the IV regression; the dependent variable is the presecution rate. Column 3 reports the second stage of the IV regression; the dependent variable is the natural logarythm of the years of schooling (plus 1); in column 3 prosecution rate is instrumented using transportation costs. The list of controls X includes all the variables reported in Table 2 in the paper. Additional Controls include village population, distance to the main road, rainfall in the village during the 100 days of the genocide in 1994, long-term rainfall in the village during the 100 calender days of the genocide period, rainfall along the way between village and main road during the 100 days of the genocide in 1994, long-term rainfall along the way between village and main road during the 100 calender days of the genocide period and its interaction with distance to the main road; rainfall during the growing season in 1994 in the village, long-term average rainfall during the growing seasons in the village and both of these interacted with the difference between the maximum distance to the main road in the sample and the actual distance to the main road; distance to Kigali; distance to che closest main city; distance to the borders; distance to Nyanza (old Tutsi Kingdom capital); population density in 1991; the number of days with RPF presence. All control variables, except the number of days with RPF presence, are in logs and al interactions are first logged and then interacted. See Rogall (2015) for more details. Robust standard errors in parentheses, clustered at the commune level. There are 145 Communes in the sample. *** p<0.01, ** p<0.05, * p<0.1

Figure C1. Conflict Intensity Variables – Coverage and Geographic distribution



Notes: The figures show the spatial distribution of the different conflict measures (see Table C1 for details and sources). Different colors identifies different quartiles, with a darker shade indicating a higher conflict intensity. No data are available for areas in white (they mostly include national parks and forests).