Armed Conflict and Schooling: Evidence from the 1994 Rwandan Genocide [*]

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Abstract

To examine the impact of Rwanda's 1994 genocide on children's schooling, the authors combine two cross-sectional household surveys collected before and after the genocide. The identification strategy uses pre-war data to control for an age group's baseline schooling and exploits variation across provinces in the intensity of killings and which children's cohorts were school-aged when exposed to the war. The findings show a strong negative impact of the genocide on schooling, with exposed children completing one-half year less education representing an 18.3 percent decline. The effect is robust to including control variables, alternative sources for genocide intensity, and an instrumental variables strategy.

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1. Introduction

Civil conflict has affected almost three-fourths of all countries in Sub-Saharan Africa, and it is critical but difficult to measure the medium to long-term consequences of wars (Gleditsch et al, 2002). Pioneering research by Collier (1999) examines the impacts of armed conflict from a macroeconomic angle and there is a growing literature in which researchers debate the magnitude of the long-term negative consequences of wars and armed conflicts. Davis and Weinstein (2002) find that recovery from World War II Allied bombings on Japanese cities was extremely strong. Brakman, Garrtesen and Schramm (2004) arrive at similar conclusions, showing that Allied bombings on West German cities had a significant but only temporary impact. Miguel and Roland (2006) present similar evidence from the American bombing in Vietnam, finding no impact of heavier bombing on poverty rates, consumption levels, literacy, infrastructure, or population density. Chen, Loayza and Reynal-Querol (2008), analyze 41 countries involved in civil wars between 1960 and 2003 and find that once the war ends and lasting peace begins, recovery in economic performance, health, education, and political development is significant. Despite the deaths and destruction caused by civil wars, there is limited research that examines the microeconomic impacts of war for non-combatants, although this is slowly changing as data from war regions becomes available (De Walque, 2006; Shemyakina, 2006; Bellows and Miguel, 2009; Akresh, Verwimp, Bundervoet, 2011).[1]

The 1994 Rwandan genocide is one of the most violent episodes in recent history, killing at least 800,000 people (10 percent of the country's population), in approximately one hundred days (Des Forges, 1999). The rate of killing during the genocide makes it "the most efficient mass killing since the atomic bombings of Hiroshima and Nagasaki" (Gourevitch, 1998).

[1] Research focusing on soldiers finds large negative impacts on earnings for these individuals, and soldiers exposed to more violence face a harder time reintegrating into civilian society (Angrist, 1990; Imbens and van der Klaauw, 1995; Humphreys and Weinstein, 2007; Blattman and Annan, forthcoming).

However, the war was short and the country was taken over by a relatively well-organized regime after the end of the hostilities. Armed conflicts often have immediate negative economic impacts and Rwanda is no exception. Rwandan per capita GDP plummeted almost 50 percent and consumer prices increased 64 percent during the genocide but by 1996 had returned to near pre-war levels (all figures from IMF, 1998). Coffee exports, the predominant export crop in Rwanda, declined 54 percent in 1994 but returned to pre-war levels in 1995. Given the rapid return to pre-war economic levels, the long-run impacts might not be severe.

In this paper, we examine the impact of the Rwandan genocide on children's human capital investment, focusing on primary level schooling as 93 percent of the population under age 35 has not completed primary school. We examine if and how shocks, such as the genocide, affect children's enrollment and the probability of completing a particular grade for those exposed children. We combine two nationally representative cross-sectional household surveys, one collected in 2000 (6 years after the genocide ended) and one collected in 1992 (2 years prior to the genocide). This is unique as few studies of conflicts have data bracketing a conflict event. The empirical identification strategy uses the pre-war data to control for baseline schooling levels for a given age group and exploits variation in which birth cohorts of children were still in school when they were exposed to the war. We find that school-age children exposed to the genocide experience a drop in educational achievement of almost one-half year of completed schooling and are 15 percentage points less likely to complete third or fourth grade, and the effect is robust to including a variety of control variables, alternative sources for measuring conflict intensity, and an instrumental variables strategy. Based on the existing literature, it is likely that the negative schooling impact of the genocide will have a long-run welfare impact through an adverse effect on future adult wages and productivity.

This paper is structured as follows. Section 2 describes the data and the empirical setting where the Rwandan genocide occurred. Section 3 explains the empirical identification strategy. Section 4 presents the main results as well as robustness tests. Section 5 concludes.

2. Data and Empirical Setting

2.1 Political History and Impacts of the Rwandan Genocide

Rwanda is a small, landlocked country in Central Africa with a current population of nine million and a high population density (337 people per square km). Since independence in 1962, political turmoil over the sharing of power and access to opportunities resulted in explosions of ethnic violence which have marked much of the recent history of the country.[2] A civil conflict pitting the Hutu-led government against the Rwanda Patriotic Front (RPF), a Tutsi-led rebel movement, culminated in genocide lasting from April to June 1994. Approximately 800,000 people (mainly Tutsis and moderate Hutus) were massacred by the army and the extremist Interahamwe militia. The RPF overthrew the regime in June 1994. More than two million refugees fled to Tanzania and the Democratic Republic of Congo. On July 19, 1994, a national unity government was established and a fragile process of recovery began to take hold. The country continued to experience large movements of people. Following unrest in the eastern region of the Democratic Republic of Congo, more than 600,000 refugees returned to Rwanda in November 1996. This massive wave of repatriation was followed at the end of December 1996 by the return of another 500,000 from Tanzania (World Bank, 2003).[3]

[2] The first massacre of the minority Tutsis by the majority Hutus occurred in 1959, sending hundreds of thousands of Tutsis into exile in neighboring countries, and since then, Rwanda has experienced several episodes of ethnic fighting.

[3] Note that this population displacement does not invalidate our identification strategy, which uses province level pre-war data to control for baseline schooling levels for a given age group (see Section 3 for details), since most displaced individuals returned to their same birth province. A 2001 Rwandan nationally representative survey, Enquête Intégrale sur les Conditions de Vie des Ménages, contains information about the province of birth as well as the current province of residence and shows that approximately 88.5 percent of individuals currently live in the same province in which they were born.

To understand the Rwandan genocide's impact on children's schooling, we discuss the mechanisms by which the conflict could have affected education. The genocide was extremely violent and disrupted the school year throughout the entire country. Schools were closed, school buildings and supplies destroyed, teachers killed, students and teachers alike became refugees abroad, families lost their savings and became poor, and many students became orphans losing one or both parents to the genocide. However, the genocide lasted a short time, concentrated on 100 days during the spring of 1994, and at the micro-level households could have rebounded from this massive shock as is evidenced in the macroeconomic indicators discussed previously.

2.2 Data Overview

Our analysis, focusing exclusively on primary schooling as few Rwandans complete more than six years of school, attempts to measure the medium-term effects of the genocide on educational achievements several years after the conflict ended. We use the 1992 and 2000 Demographic and Health Surveys (DHS) for Rwanda to measure educational attainment and grade completion rates.[4] Table 1 reports the overall educational attainment for all individuals aged 6 to 35, and then for males and females separately, in 1992 and in 2000. For both genders and for all individuals overall, on average schooling outcomes improved between 1992 and 2000. The fraction of people with no education decreased from 30.0 to 23.8 percent, and the proportion with some primary education increased from 63.99 to 69.16 percent. There was also a small increase in secondary schooling from 6.01 to 7.04 percent. Similar results are seen for both men and women, although the improvements seem to be slightly larger for women. These results could be interpreted as providing evidence that there are not medium-term negative educational

[4] Rwanda's education system has a 6-3-3-4 structure: six years of primary schooling, three years of lower secondary schooling, three years of upper secondary schooling, and four years of higher education (World Bank, 2003).

impacts due to the conflict in 1994. Already in 2000, several years after the conflict, overall education rates seem to have improved, both in primary and secondary schooling.

However, analyzing which children were exposed to the conflict provides a different interpretation. In Figure 2, using the 1992 and 2000 DHS data restricted to individuals age 6 to 55, we estimate separate kernel-weighted local polynomial regressions of years of schooling against age using an Epanechnikov kernel. Depending on the survey, the horizontal axis records an individual's age in either 1992 (for individuals from the 1992 data) or in 2000 (for individuals from the 2000 data). Figure 2, by detailing schooling outcomes age by age, gives a different picture than the summary statistics discussed in Table 1. For individuals from ages 24 to 55, years of schooling is higher for those people interviewed in 2000 compared to similarly aged people interviewed in the 1992 survey. This observed increase in educational achievement from 1992 to 2000 for people in the 24 to 55 year old age range likely reflects the general tendency in developing countries for schooling outcomes to improve with each new birth cohort.

To highlight the differences between the age and years of schooling relationship in Rwanda and other neighboring countries and to confirm that educational achievement generally increases with more recent birth cohorts, in Figures 3a, 3b, and 3c, we present similar nonparametric regressions for Uganda, Tanzania, and Kenya. For these countries, we use similar Demographic and Health survey data, although the years of data are slightly different than for Rwanda.[5] In all three neighboring countries, there is a general improvement in the number of years of completed schooling for the more recent birth cohorts. This is the same pattern that is observed in Rwanda for those individuals over age 24. Individuals aged 24 to 55 in 2000 were

[5] For Uganda, DHS data is from 1995 and 2000; for Tanzania, DHS data is from 1996 and 1999; for Kenya, DHS data is from 1993 and 1998.

older than 18 at the time of the genocide in 1994, and for the majority of them, they had already completed their schooling by that time.

However, this result for older individuals contrasts with that seen for people age 6 to 24. Figure 2 shows that the number of years of education achieved is consistently higher for these younger individuals when measured in 1992, which contradicts the general tendency for education outcomes to increase over time in developing countries. Individuals aged 6 to 24 in 2000 were aged 0 to 18 in 1994 at the time of the genocide. Therefore, most of them were of school-going age in 1994 and their educational attainment is likely to have been negatively impacted by the consequences of the genocide.

Figures 4a and 4b further explore the relationship between schooling outcomes and exposure to the genocide by displaying separately, for each grade 1 to 6 in primary school, the proportion of individuals, for each age that completed that grade. For each of the six grades, the proportion completing that grade is higher in 1992 for the young ages, but at older ages, the completion rate is higher in 2000. This is consistent with the overall picture presented in Figure 2. Note that, as one progresses through the graphs, the difference between the 1992 and 2000 measure at early ages widens and that the crossing point between the 1992 and the 2000 curves moves to the right toward an older age. For first grade, the difference between the two curves is small and the crossing point is at age 12. Being a young child at the time of the genocide had a small, but negligible impact on these children's likelihood of completing first grade. However, by fourth, fifth and sixth grades, the difference between the two curves for the younger children is more substantial and the crossing point moves towards an older age. This widening gap between the two curves for younger children suggests that the schooling deficit of the cohorts who were of schooling age after 1994 is not so much due to a lack of access to education, since

the differences for the first grade are minimal, but rather to difficulties to progress through the grades or continue attending school.

3. Empirical Identification Strategy

The identification strategy can be further illustrated by using a two-by-two difference-in-differences table, which follows a similar approach to the previously discussed Figure 2. Panel A of Table 2 shows the average years of schooling for younger (age 6 to 15) and older (age 16 to 35) cohorts of children measured in the 2000 and 1992 datasets.[6] If we compare the older cohort of individuals in 2000 with the older cohort of individuals in 1992, we observe a small but insignificant increase of 0.123 years of schooling, which is consistent with an increasing secular trend in education. This finding contrasts with the younger cohort of children who showed a significant decline of 0.432 years of schooling from 1992 to 2000. As the young cohort measured in 2000 was exposed to the genocide and still likely to be in school, the conflict might have negatively impacted the amount of schooling they have so far achieved. Calculating the difference-in-differences estimator indicates a decline of a little more than half a year of education (-0.555) for the young cohort who was exposed to the impacts of the genocide, and the result is significant at the 1 percent level. Given the low level of education generally achieved in Rwanda, the magnitude of the effect is sizeable, and relative to the average educational attainment it represents a decline of 18.3 percent. The difference-in-differences results can be interpreted as the impact of the genocide on children's schooling under the assumption that, without the conflict, the younger cohort of children in 2000 would have experienced the same change in schooling outcomes relative to the older cohort as the children in 1992.

[6] Results in this table and the subsequent regressions are consistent if we use alternative cutoff ages for the young and old cohorts, defining the young cohort as aged 6 to 12, 6 to 15, 6 to 18, 6 to 21, and 6 to 24. We also estimate regressions defining the age cutoff for the old cohort as ages 16 to 25, 16 to 30, 16 to 35, 16 to 40, 16 to 45, 16 to 50, and 16 to 55 and results are similar.

From the results in Panel A, we cannot tell if the younger cohort will complete less education or if that education is only being delayed. Panels B to G in Table 2 begin to explore this issue using the same difference-in-differences strategy as in Panel A, but examining the proportion of individuals that complete grades 1 to 6. Most of the grades show a positive and significant increase for the older cohort from 1992 to 2000 in the proportion of individuals completing that given grade, but a negative and significant decrease for the younger cohort in the proportion of individuals completing that given grade. For each of the six grades, the difference-in-differences estimate is negative and statistically significant at the 1 percent level, indicating that the young cohort in 2000 is less likely to have completed a given grade. The magnitude of the negative impact increases from grade 1 to grade 4 (for which the estimate is -0.157) and then decreases for grades 5 and 6. This suggests that the largest impact of the genocide on schooling achievements seems to have occurred for children in grades 3 and 4.

The empirical identification strategy, illustrated by these cross-tabulations and in Figure 2, relies on the comparison between the schooling achievement of similarly aged individuals in 1992 (prior to the genocide) and 2000 (after the genocide). We examine whether school-aged children in 2000 who had been exposed to the consequences of the 1994 genocide have lower educational attainment than similar school-aged children in 1992, relative to the older cohorts of individuals who had completed their schooling. The change in schooling attainment for the older cohort of children acts as a control group for what the change in the younger cohort's education rates should have looked like in the absence of the conflict. Building on these cross-tabulations and figures, we estimate a baseline regression in which the key variable of interest is the interaction of a dummy variable for being in the young cohort and a dummy variable for being in the 2000 DHS data, as follows:

(1) $Schooling_{ijt} = \beta\ (Young\ Cohort_{ijt} * DHS\ 2000_{ijt}) + DHS\ 2000_{ijt} + \gamma_j + \delta_t + \varepsilon_{ijt}$

where $Schooling_{ijt}$ is the educational attainment for individual i born in province j in year t, δ_t are birth cohort fixed effects, γ_j are province fixed effects, $DHS\ 2000_{ijt}$ is an indicator for being in the 2000 DHS data, and ε_{ijt} is a random idiosyncratic error term.[7]

Despite the two datasets bracketing the time period of the genocide (one dataset two years prior and one dataset six year post), there could have been other changes or events occurring during this time interval that might be correlated with changes in schooling, and we would incorrectly be attributing the observed decline in educational attainment to the genocide. To address this issue, we present two alternative empirical estimation strategies to argue that the decline in schooling is directly caused by the conflict. First, we develop three distinct measures of province-level war intensity for each of the Rwandan provinces, and we compare schooling outcomes in provinces that experienced more destructive fighting with outcomes in provinces that experienced lower war intensity. We rely on the fact that the genocide's intensity varied across provinces in Rwanda, with some areas experiencing more intense fighting and significantly more killings.

We interact a given war intensity measure with the difference-in-differences variable ($Young\ Cohort_{ijt} * DHS\ 2000_{ijt}$) to estimate a difference-in-difference-in-differences regression. Identification comes from comparing the schooling achievement in high and low intensity war-impacted provinces for similarly aged individuals in 1992 (prior to the genocide) and 2000 (after the genocide). The change in schooling attainment for the older cohort of children in the high war intensity provinces relative to the lower war intensity provinces acts as a control group for what the change in the younger cohort's education rates should have looked like in the absence

[7] Correlation among the error terms of children living in the same local environment and experiencing similar education shocks might bias the OLS standard errors downward, so in all regressions we cluster the standard errors by enumeration area, which corresponds to local clusters of villages (Moulton 1986).

of the conflict. By taking advantage of geographical variation in the intensity of the genocide as well as having information from before and after the conflict, it makes our identification strategy significantly more robust than if we only relied on one source of variation.

As a final estimation strategy, we recognize that the intensity of the genocide might be endogenous and could be correlated to levels of educational achievement of the population, so we estimate a series of specifications in which we instrument for the genocide intensity measures using the distance to the Ugandan border as an instrument. Provinces close to the Ugandan border were reached faster by the RPF who stopped the killings perpetrated by the Interhamwe militia, and therefore these provinces are more likely to be low war intensity provinces. In using this instrumental variables strategy, we are assuming that the distance to the Ugandan border has no direct impact on children's educational achievement, except through the genocide.

4. Empirical Results

4.1 Baseline Difference in Differences Estimation

Table 3 presents the baseline regression results for the difference-in-differences estimation strategy using years of education as the dependent variable and including fixed effects for province and children's age.[8] All specifications also control for child gender, poverty status, an indicator for the dataset (2000 DHS data), age of the household head, the highest level of education among all adults in the household, the number of children under five in the household, and the rural/urban status of the household.[9] The difference-in-differences estimate is the coefficient on the interaction between the young cohort (age 6 to 15) and the variable indicating

[8] Since 1994 in Rwanda, it is forbidden in household surveys to ask questions about ethnicity and therefore the 2000 data do not contain this information so it is not possible to control for this variable in the regressions. However, the 1992 data contain this variable, so in robustness checks, we estimate the difference-in-differences regression separately for provinces with low and high percentages of Tutsi in 1992 and results are not significantly different.

[9] The poverty status measure is an indicator variable for whether the household has more assets than the population mean. Assets used to calculate this wealth measure include piped running water, refrigerator, radio, finished floor, bicycle, motorcycle, and car.

the DHS 2000 survey. Results suggest a negative impact of the genocide of -0.421 years of education, similar to the corresponding estimate in Table 2, Panel A that was obtained without any controls, and the coefficient is statistically significant at the 1 percent level.[10] Within a given province and for a given aged child, being in the young cohort of children in the 2000 data and exposed to the genocide during typical schooling ages implies significantly lower educational attainment for these children.[11]

The regressions in columns 2 and 3 explore the heterogeneity of this conflict impact. In column 2, we interact the difference-in-differences variable with a female indicator. Results suggest the negative impact of the genocide is stronger for males. A potential reason might be that female schooling outcomes were lower initially, and so they had less to lose after the genocide. In column 3, we estimate a regression including an interaction between the difference-in-differences variable and the indicator for being non-poor. Somewhat surprisingly, it is among the children in non-poor households in which the negative shock is strongest. Young children in these non-poor households experience a decline of 1.223 years of schooling. It appears the genocide's impact on schooling outcomes might have worked by leveling-off educational achievements to a low level for everyone, irrespective of gender or wealth.

Tables 4 to 6 follow the same logic as the Table 3 specifications, but for the dependent variables we use an indicator of whether the person completed grades 1 to 6, respectively.[12] The objective of these tables is to determine at which grade level the genocide's negative impact is strongest. For all three tables, the household level controls and province and children's age fixed

[10] Results are also robust when household level controls are not included in the regressions.
[11] The identification strategy is valid as long as changes over time in schooling attainment would be similar across provinces in the absence of the genocide. To address any potential differential time trends across provinces, we re-estimate the regressions in Table 3 and include a province specific age trend in the regressions and results are consistent and still statistically significant.
[12] We estimate the regressions with a linear probability model, although results are consistent using a logit model.

effects are the same as in Table 3. Table 4 presents the baseline regressions as in column 1 of Table 3. Across the 6 grades, females have a lower probability of completing a given grade (not significant for grades 3 and 4) and individuals from non-poor households have a higher probability to complete the grade. The indicator for the 2000 DHS is always positive and significant at the 1 percent level (except for grade 6) showing that for the older cohorts, educational achievement increases over time. However, the difference-in-difference variable, an interaction of the 2000 DHS indicator with the young cohort aged 6 to 15, always carries a negative and significant coefficient. The magnitude of these coefficients is similar to the difference-in-difference tabulations in Table 2, Panels B to G, obtained without controls, and they peak for children in grades 3 and 4. The coefficient of -0.145 in the regression examining fourth grade completion suggests a decline of almost 40 percent compared to the baseline completion rate of 38.4 percent.

In Table 5, using grades 1 to 6 completion proportions as the dependent variable, we estimate a similar regression to that presented in Table 3 column 2, interacting a female indicator with the difference-in-differences variable. The coefficient on the interaction term is positive and statistically significant at the 1 percent level in each regression, suggesting that the negative impact of the genocide on grade completion is relatively larger for boys, consistent with results in column 2 of Table 3. Table 6 replicates the specification in column 3 of Table 3 using the proportion completing grades 1 to 6 as the dependent variables. As in Tables 4 and 5, the results for each grade are similar to the results with years of education as the dependent variable. The interaction between the difference-in-differences variable and the non-poor indicator is consistently negative and statistically significant suggesting that it is among the non-poor that the negative impact of the genocide on educational outcomes is the strongest. The genocide's largest

impact is on the proportion completing sixth grade and shows that young children in non-poor households who are exposed to the conflict experience a drop of 16.9 percentage points.

These results reinforce the hypothesis that the impact of the genocide could be characterized as a negative shock bringing everybody to a low level of schooling outcomes, and therefore affecting disproportionately boys and non-poor children, who previously had enjoyed an advantage in terms of education. Another important feature of Tables 4 to 6 is that they suggest that the negative shock to educational outcomes was more due to difficulties for children to progress through the grades than to obstacles to initiate their schooling. Indeed, the magnitude of negative coefficients is larger for grades 2 to 4 than for first grade, and this is both in absolute terms and as a percentage of the baseline completion rates. However, with the available data, it is not possible to determine definitively if this impact of genocide on grade progression is due to children repeating grades or dropping out of school after the first few grades.

To understand what is the direct mechanism driving this decline in educational achievement following the genocide, we examine the role of orphan status and its ability to explain the results. Since approximately 10 percent of the Rwandan population died in 1994 and those deaths were concentrated among adults (de Walque and Verwimp, 2007), it is natural to think that orphanhood might be one of the main driving forces. In the 2000 DHS, 22.3 percent of the children aged 0 to 14 had lost at least one parent (ORC Macro, 2001). Figure 5 explores this hypothesis by displaying the years of education for orphans and non-orphans in 1992 and 2000 for children age 6 to 14 inclusive.[13] The figure illustrates that years of schooling is consistently higher in 1992 than in 2000, both for orphans and non-orphans. Strikingly, orphans in 1992 are better off, in terms of educational achievement, than both orphans in 2000 and non-orphans in

[13] The Demographic and Health Survey only reports orphan status for children under age 15, which limits the children for which we can explore this hypothesis.

2000. Given the results in Figure 5, it is unlikely that the decline in educational attainment for the young cohort exposed to the genocide is mainly due to orphanhood.

4.2 Robustness checks: Triple Difference and Instrumental Variables

As discussed in Section 3, the analysis so far is a comparison within a given province of younger children who were of school age compared to older children (who had finished school) and then a further comparison of a cross section of these children before and after the 1994 genocide. Given the genocide was such a large magnitude shock to Rwandan society, it is logical to think that most of the differences between 1992 and 2000 are due to this event. However, there could have been other changes occurring during this time interval and these changes could be correlated with changes in schooling and we might be incorrectly attributing the observed decline in schooling achievements to the impact of the genocide. To examine this possibility, in Table 7, we estimate a series of triple differenced regressions using three distinct measures of war intensity to argue that the decline in schooling is directly caused by the genocide. If we are able to show that the negative impact on schooling outcomes is larger in provinces that suffered the most from the genocide, then concluding that those negative impacts were due to the 1994 genocide rather than to other events that took place between 1992 and 2000 is more plausible.

In constructing the three distinct war intensity measures, we exploit the variation in the intensity of the genocide across provinces in Rwanda. The first measure, Measure A, is defined as the proportion of days during the genocide during which killings occurred in a given province and is based on the GenoDynamics database collected by Davenport and Stam (2007). The authors of the database attempted to collect all available information from local human rights organizations, Rwandan government ministries, and international organizations on the timing and geographic extent of all killings that took place during the one hundred days of genocide.

The second measure, Measure B, is an indicator variable for the three provinces (Butare, Rural Kigali, and Kibongo) that had the most number of genocide victims based on the GenoDynmaics database (Davenport and Stam, 2007). Finally, the third measure, Measure C, is defined as the number of mass graves sites and memorials in a particular province, as recorded on an administrative map of Rwanda that was edited as part of the Rwandan Genocide Project at Yale University (Rwandan Genocide Project, 2007). Using both sources of data (discoveries of mass grave sites and memorials as well as information gathering tabulated in the GenoDynamics database about the genocide victims) increases the reliability of these measures and strengthens the robustness of our analysis.

Table 7 presents results of the triple differenced regressions in which the key variable of interest is the interaction of the difference-in-difference variable used in the previous tables with one of the war intensity measures. The specifications also include the interaction between the war intensity measure and the young cohort indicator, the interaction of the war intensity measure and the 2000 DHS data indicator, and the interaction of the young cohort indicator and the 2000 DHS data indicator. All regressions also include province and child age fixed effects as well as child gender, poverty status, an indicator for the dataset, and the same household control variables used in the previous tables. In column 1 of Table 7, the dependent variable is the years of education achieved by the individual. For all three war intensity measures, the coefficient on the triple difference variable is negative and significant at the 10 percent level. Increasing by one standard deviation the proportion of days during which killings occurred (mean proportion is 12.4 percent, standard deviation is 6.2) implies a decline of 0.15 years of schooling based on the coefficient in column 1 using Measure A. Using Measure B and moving a child to one of the three provinces that experience the most genocide killings is correlated with a drop in schooling

for the exposed children of 0.33 years of school. Finally, using Measure C, an increase of one standard deviation in the number of mass grave sites and memorials in a province (mean number is 9.6, standard deviation is 6.2) implies a drop of 0.14 years of schooling. The results indicate that the negative impact on educational outcomes was stronger in the provinces where the intensity of the genocide was higher, suggesting that the substantial negative shock to schooling levels observed between 1992 and 2000 could be directly due to the 1994 genocide.

In Table 7 columns 2 to 7, we provide further detail by examining the relationship between war intensity and the probability to complete grades 1 to 6. Each cell in the table represents a separate regression, with the grade listed at the top of the column and the coefficient for the triple difference variable reported in the table. Every regression includes the control variables previously discussed. The coefficient on the triple difference variable is always negative but not always statistically significant. Note that in the regressions for fourth grade for all three war intensity measures, the triple difference variable is always significant and often it is the largest point estimate across the six grades, results which are consistent with the previous results in Tables 2 and Tables 4 to 6 showing the largest negative schooling impact occurring in Grades 3 and 4.

As a second robustness check, we recognize that the war intensity measures might be endogenous and could be correlated with observable or unobservable factors that are related to educational outcomes, so we use the distance from the capital of each province to the Ugandan border as an instrument for these measures. Since the Rwandan Patriotic Front, the “rebel” forces that eventually defeated the government army and their allied Interhamwe militias who perpetrated the genocide, was initially based in Uganda and first controlled the provinces close to the Ugandan border, those provinces were better protected and experienced lower genocide

intensity. Conversely, those provinces further away from Uganda took longer for the RPF forces to reach and subsequently more genocide killings occurred and the war intensity measures are higher for those regions. In using the distance to the Ugandan border as an instrument, we are assuming that the distance measure has no impact, except through the genocide, on children's educational attainments.

Table 8 Panel A contains the strong first stage results of the instrumental variables strategy and indicates that provinces further away from the Ugandan border were more likely to be high war intensity regions. In Panel B, we present the main results of the robustness check, and as in Table 7, each cell in this table represents a separate triple difference regression but the endogenous war intensity measures are instrumented for. As in previous tables, each regression includes province and child age fixed effects as well as household controls. The instrumental variables results strongly indicate that the genocide had a negative impact on exposed children's schooling outcomes. The magnitudes of the coefficients for the instrumented years of schooling regressions are each negative and significant at the 10 percent level, which is consistent with the Table 7 results. Increasing the war intensity measure by one standard deviation leads to a decline of 0.29 or 0.22 years of school using war intensity Measure A or C (columns 1 or 3), respectively and moving a child to one of the three most war impacted provinces lowers schooling by 0.84 years. Examining completion rates for grades 1 to 6 shows that exposed children are worse off, as the coefficients are always negative and most coefficients are significant at the 5 or 10 percent level. The only exception is for the first grade completion rate that shows no statistically significant impact of the genocide. As seen in previous tables and in these instrumented regressions, the largest negative impact is for child completion rates in grade 4. Overall, the

instrumented regressions provide a further robustness check reinforcing the conclusion that the genocide had a negative impact on exposed children's educational attainment.

5. Conclusions

In this paper, we investigate the impact of the 1994 genocide in Rwanda on the schooling outcomes of children. We compare the changes in educational achievement for younger cohorts of school-aged children in one nationally representative cross-sectional survey conducted two years prior to the genocide (in 1992) with the same aged children in a second cross-sectional survey conducted six year after the genocide (in 2000), and we contrast this with the changes in schooling rates for older cohorts of children in both surveys. The empirical identification strategy uses the pre-war data to control for baseline schooling levels for a given age group and exploits variation in which cohorts of children were still in school when they were exposed to the war. The empirical analysis is unique as most studies do not have data from before a given conflict. We find that young children exposed to the genocide experience a decline in schooling attainment of close to one-half year (an 18.3 percent drop relative to average educational achievement) and are 15 percentage points less likely to complete fourth grade (a 37.8 percent drop relative to baseline completion rates). To confirm that this negative impact can be attributed to the genocide and not to other events occurring between 1992 and 2000, we estimate a triple differenced regression that exploits the variation across provinces in the intensity of the killing and we show that the negative impact on schooling is stronger in provinces where the genocide intensity was higher. Finally, as an additional robustness check, recognizing that our three distinct war intensity measures could be endogenous, we estimate an instrumental variables regression using the distance to the Ugandan border as our instrument and results are consistent. This robust finding of a negative impact of the Rwandan genocide on the schooling attainment of

exposed children will likely have long-term negative welfare consequences by reducing future adult wages and productivity.

Understanding the specific mechanisms by which the genocide impacted children's schooling is critical for developing adequate policy responses to protect children from the negative conflict effects. While our data do not allow definitive conclusions, the results do offer some indications as to the likely mechanism. First, one possible direct mechanism is that the genocide disproportionately killed educated individuals. De Walque and Verwimp (2007) show that more educated individuals were also more likely to die in 1994. However, they report that this mortality difference by educational status was only significantly different for individuals over age 25, while for children aged 6 to 15 there was no difference in mortality rates. Older individuals with additional education were more likely to have been killed in the genocide, so the observed difference in schooling between old and young cohorts is less than it would have been without selective mortality. Given we find a significant negative impact of the genocide on school-aged children exposed to the conflict, our results likely underestimate the true negative impact in an environment of selective mortality for the older cohort, and it is unlikely that the killing of educated individuals is the mechanism explaining our results.

A second possible mechanism is orphanhood. The large proportion of orphans in post-1994 Rwanda suggests that the absence of one or both parents could be the principal driving factor. However, as discussed previously, Figure 5 does not confirm this intuition. While it is true that orphans do slightly worse than non-orphans in terms of schooling outcomes in 2000, it is striking that non-orphans in 2000 are doing worse than orphans in 1992.

A third possible mechanism is that households were made poorer due to the conflict (loss of crops and assets and disruption of business activities) and given the more difficult economic

circumstances might further discriminate against the schooling of girls. However, our results indicate that the negative impact on education was actually larger for boys and for children from non-poor families, suggesting that the genocide had a leveling-off effect that brought boys and the non-poor to the same lower level of schooling as girls and poor children.

Finally, a fourth possible mechanism is the destruction of schools or lack of teachers due to the genocide and the subsequent impact on children's schooling. However, we do not find evidence that infrastructure problems are driving the decline in educational attainment as we find almost no impact for grade 1 completion rates and the largest impact is not seen until grades 3 and 4. This seems to indicate that the most likely mechanism linking the genocide to reduced educational attainment is through grade progression, as opposed to not entering the school system, and this is consistent with large repetition rates (World Bank, 2003). Following the end of this brutal period in Rwandan history, aggregate measures of the economy as well as overall children's schooling rates have rebounded, although there will still be the generation of children exposed to the conflict that experiences adverse effects long after the fighting ends.

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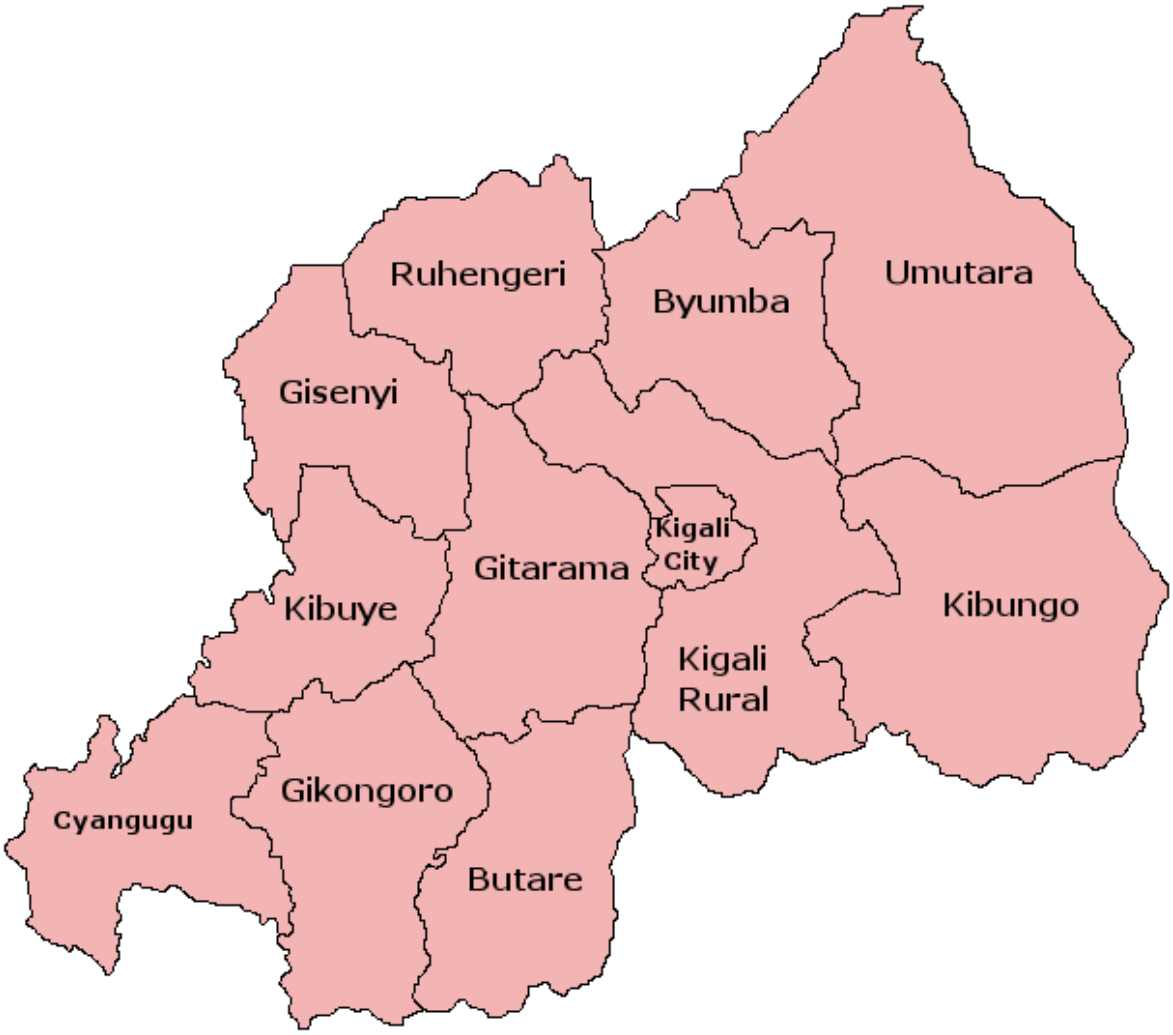
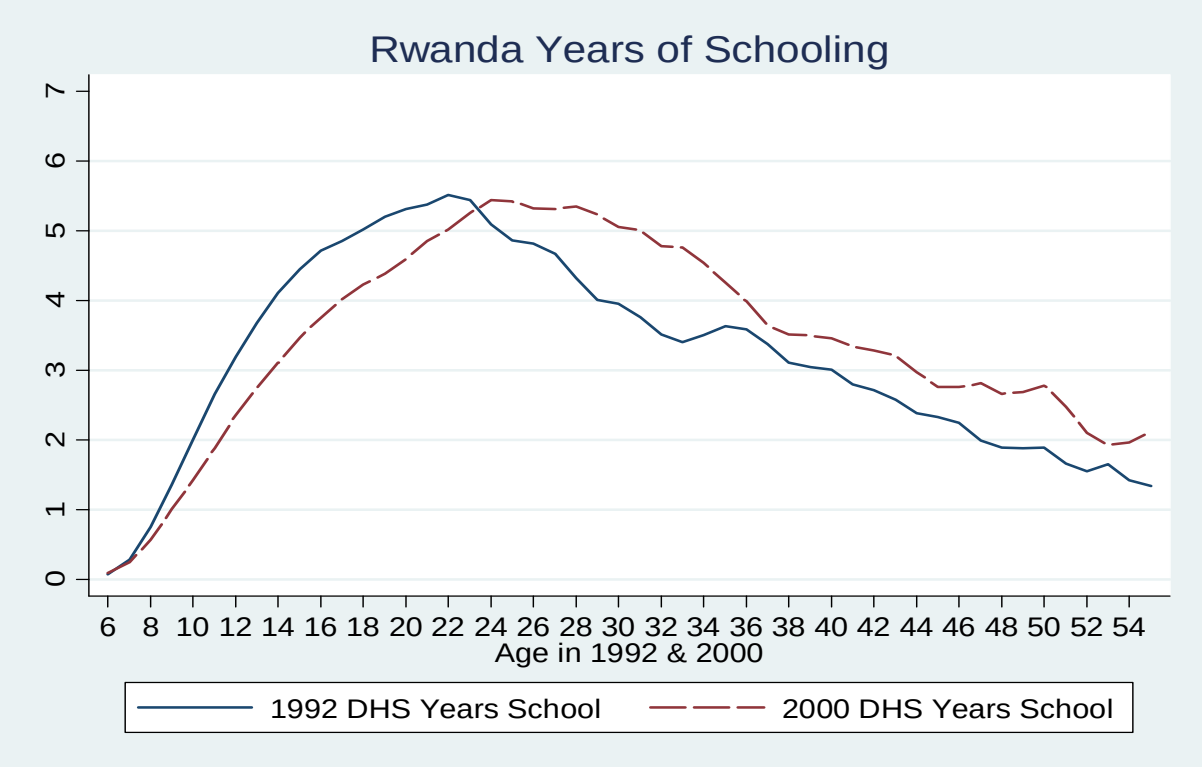
Figure 1: Map of Rwandan Provinces in 2000



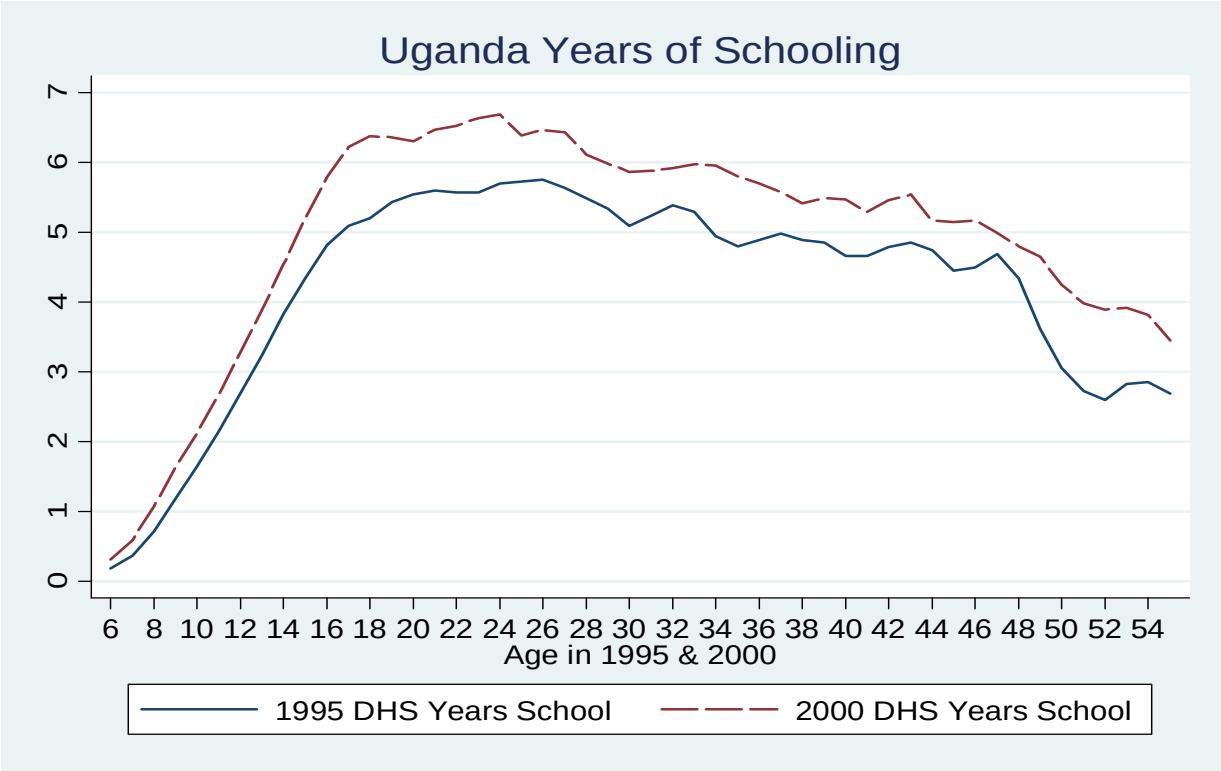
Map source: Made by Actnx in Wikimedia Commons.

Figure 2: Rwanda Total Years of Schooling, By Age



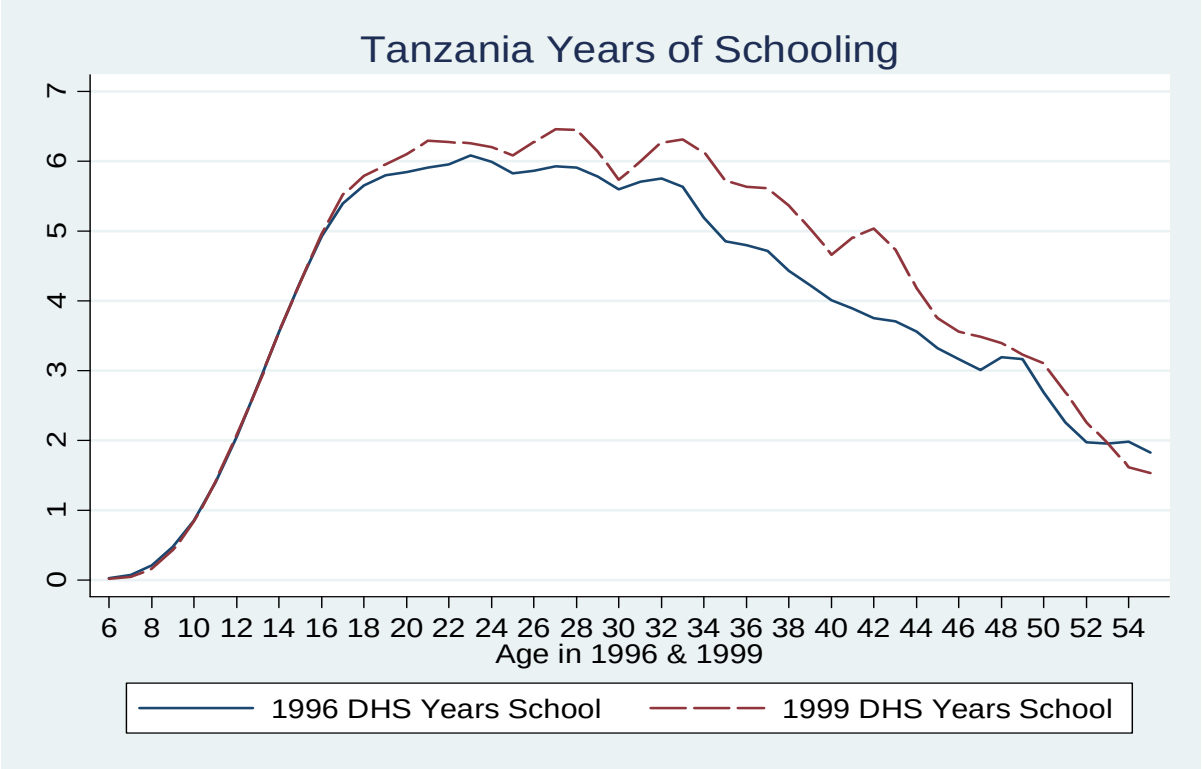
Notes: Figure 2 presents a kernel-weighted local polynomial regression (using Epanechnikov kernel) of years of schooling on children's age. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys (DHS).

Figure 3a: Uganda Total Years of Schooling, By Age



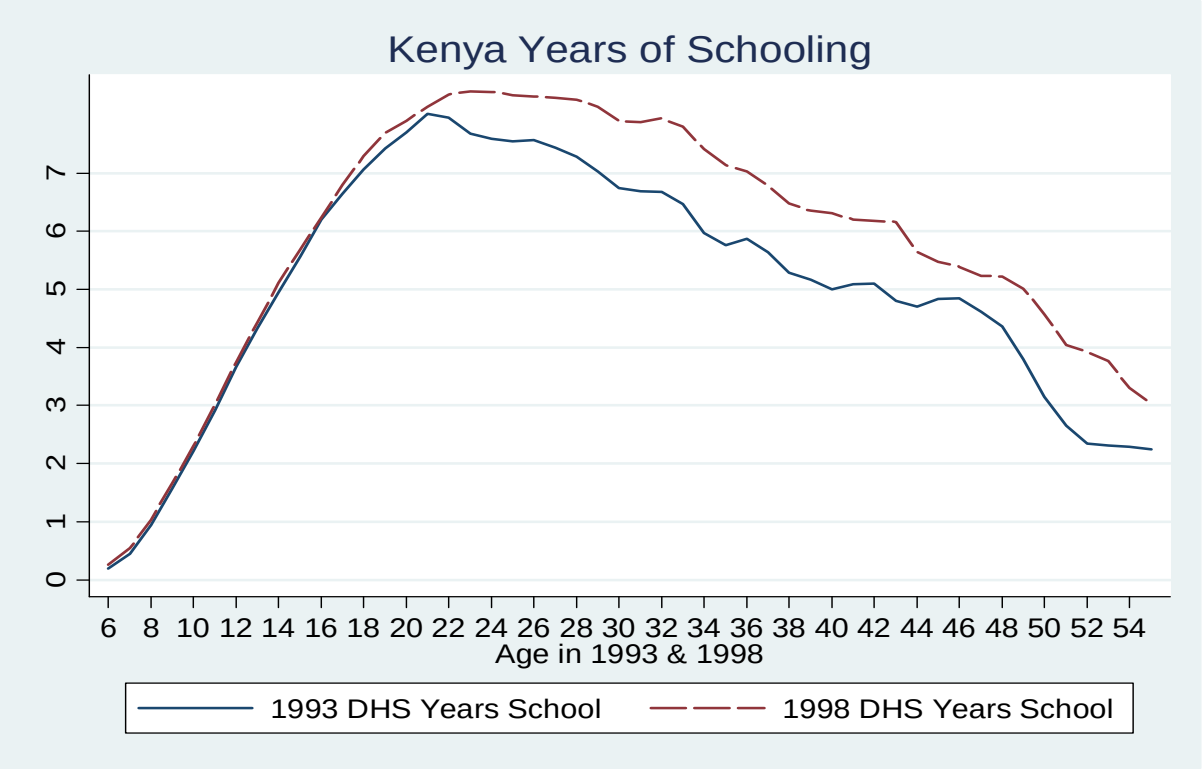
Notes: Figure 3a presents kernel-weighted local polynomial regressions (using Epanechnikov kernel) of years of schooling on children's age. Data source: 1995 and 2000 Uganda Demographic and Health Surveys.

Figure 3b: Tanzania Total Years of Schooling, By Age



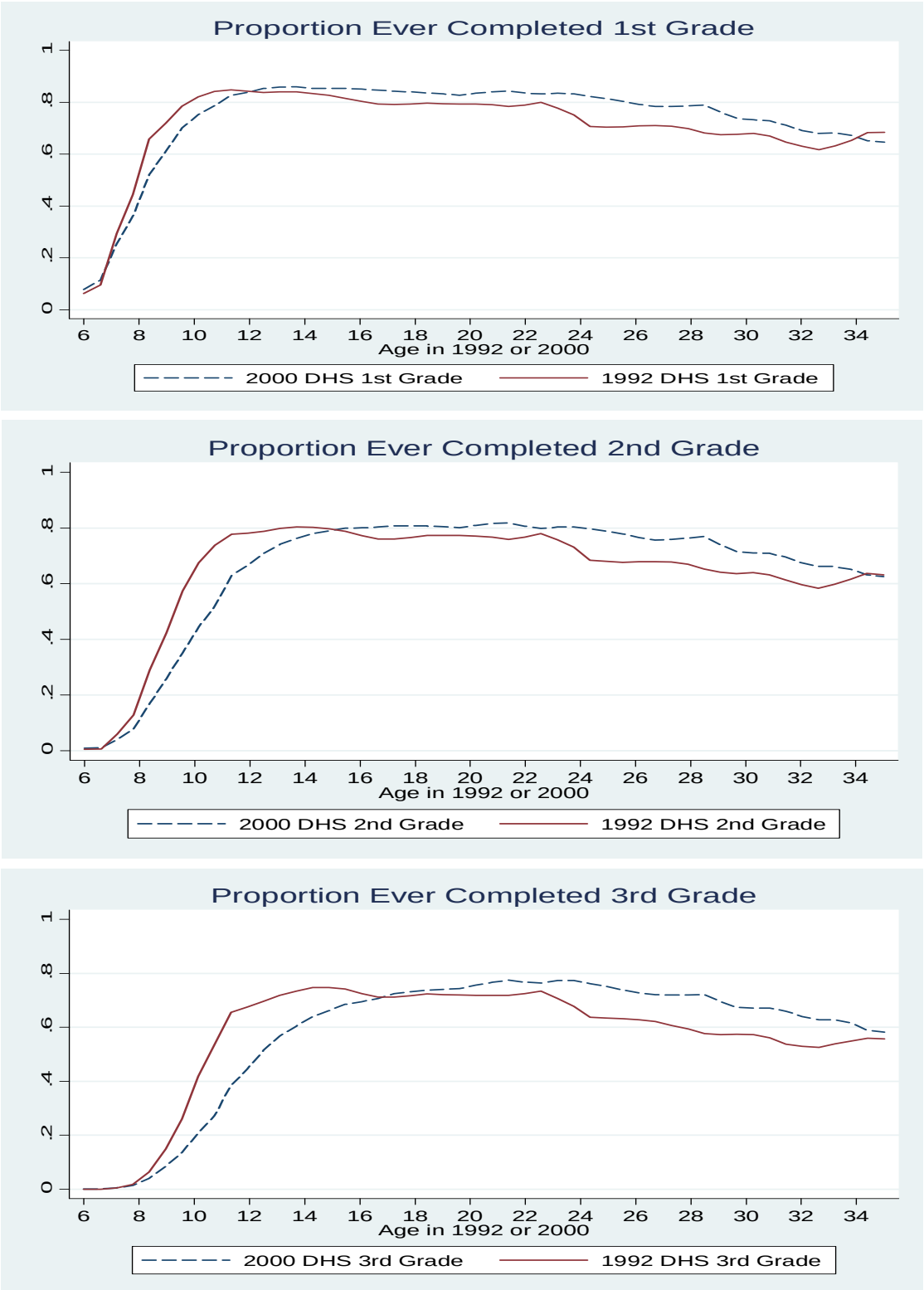
Notes: Figure 3b presents kernel-weighted local polynomial regressions (using Epanechnikov kernel) of years of schooling on children's age. Data source: 1996 and 1999 Tanzania Demographic and Health Surveys.

Figure 3c: Kenya Total Years of Schooling, By Age



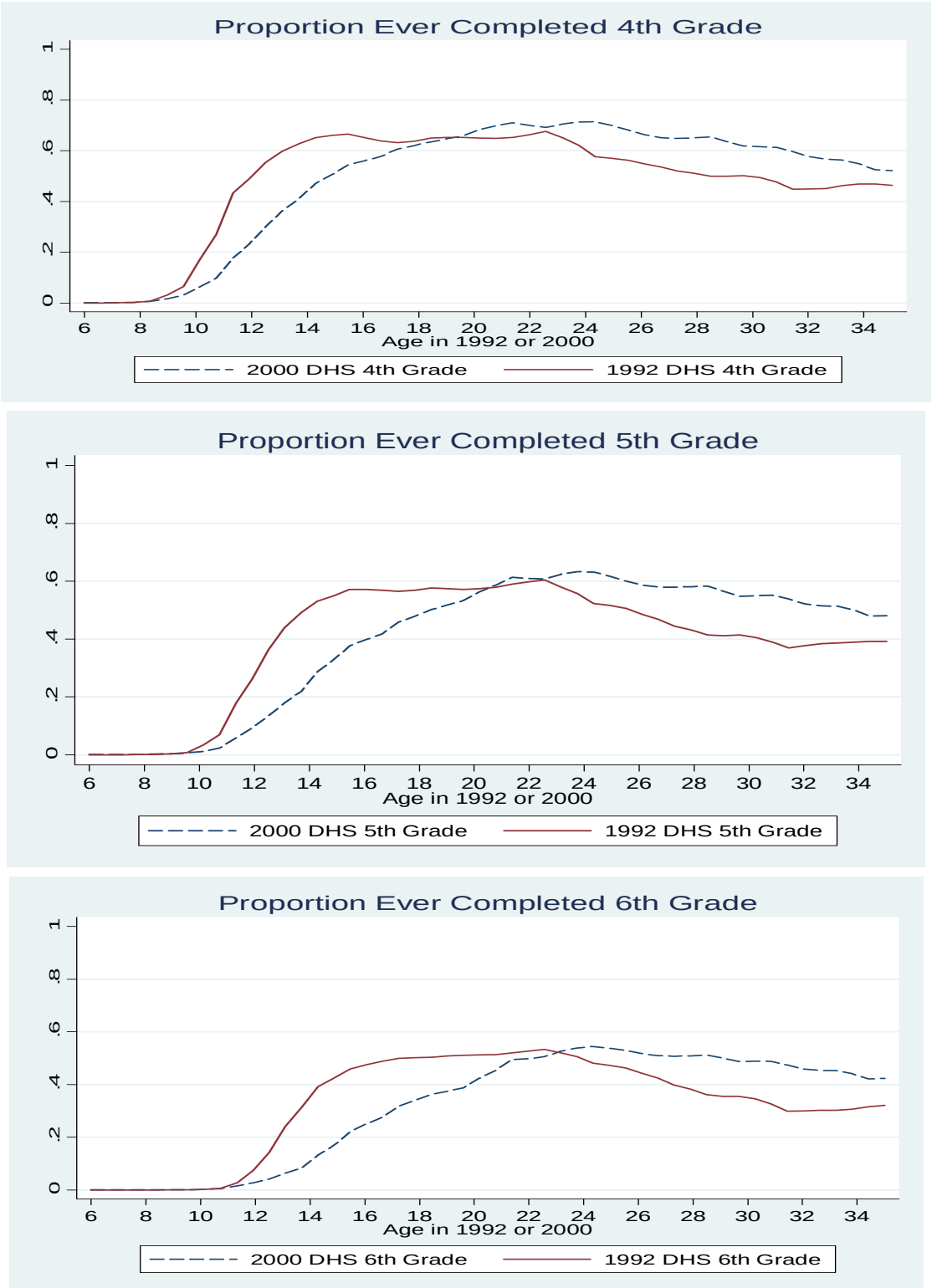
Notes: Figure 3c presents kernel-weighted local polynomial regressions (using Epanechnikov kernel) of years of schooling on children's age. Data source: 1993 and 1998 Kenya Demographic and Health Surveys.

Figure 4a: Rwanda Proportion Ever Completing Grades 1 to 3, By Age



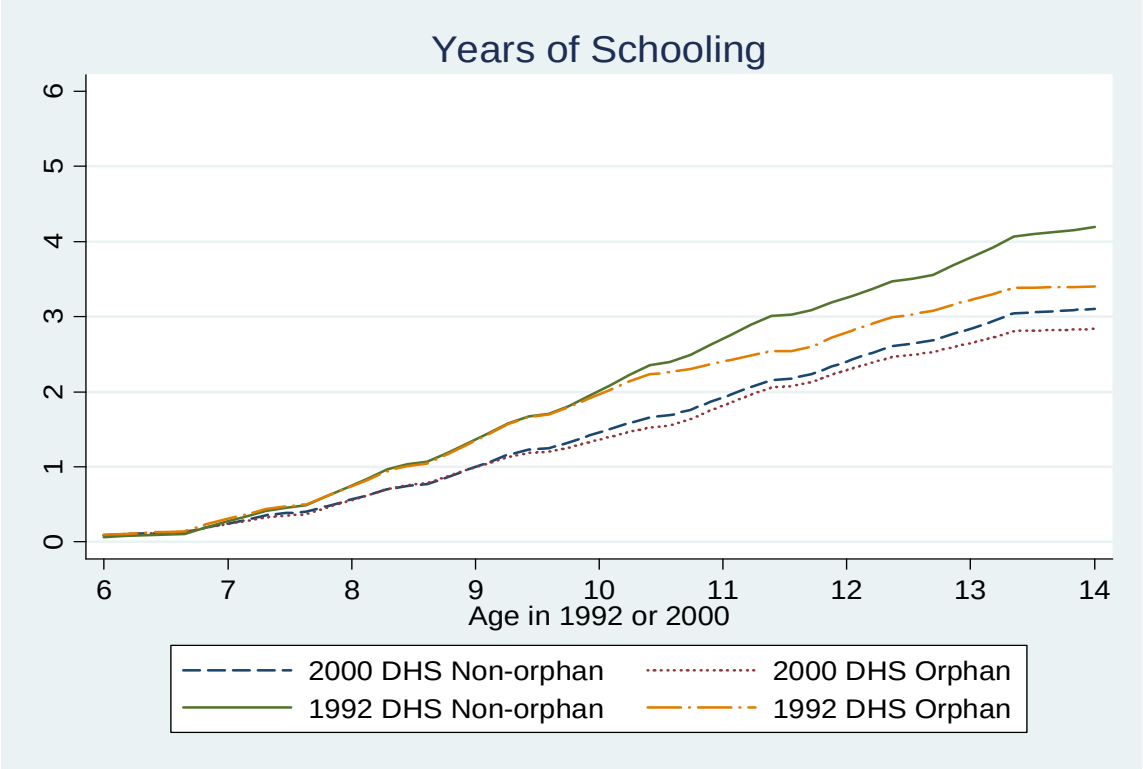
Notes: Figure 4a presents a kernel-weighted local polynomial regression (using Epanechnikov kernel) of proportion ever completed a given grade (1st grade, 2nd grade, or 3rd grade) on children's age. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Figure 4b: Rwanda Proportion Ever Completing Grades 4 to 6, By Age



Notes: Figure 4b presents a kernel-weighted local polynomial regression (using Epanechnikov kernel) of proportion ever completed a given grade (4^{th} grade, 5^{th} grade, or 6^{th} grade) on children's age. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Figure 5: Years of Schooling, By Age and Orphan Status



Notes: Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Table 1: Educational Attainment Before and After the Genocide

	All Individuals		Males Only		Females Only	
	DHS 2000	DHS 1992	DHS 2000	DHS 1992	DHS 2000	DHS 1992
Educational Attainment:						
No Schooling	23.80	30.00	23.19	27.75	24.34	32.11
Some Primary Schooling	69.16	63.99	69.27	65.47	69.06	62.60
Some Secondary Schooling	7.04	6.01	7.54	6.78	6.60	5.29
Observations	27114	18528	12769	9027	14345	9501

Notes: Summary statistics are restricted to individuals aged 6 to 35. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Table 2: Difference-in-Differences Comparing Children's Schooling Before and After the Genocide for Young and Old Cohorts

Panel A: Years of Schooling	DHS 2000	DHS 1992	Difference
Old Cohort	4.391***	4.268***	0.123
	[0.091]	[0.103]	[0.140]
Young Cohort	1.587***	2.019***	-0.432***
	[0.029]	[0.041]	[0.052]
Difference	-2.804***	-2.249***	-0.555***
	[0.078]	[0.085]	[0.116]

Panel B: Completed 1st Grade	DHS 2000	DHS 1992	Difference
Old Cohort	0.779***	0.713***	0.066***
	[0.007]	[0.011]	[0.013]
Young Cohort	0.615***	0.619***	-0.004
	[0.007]	[0.009]	[0.012]
Difference	-0.164***	-0.094***	-0.070***
	[0.006]	[0.009]	[0.011]

Panel C: Completed 2nd Grade	DHS 2000	DHS 1992	Difference
Old Cohort	0.749***	0.681***	0.068***
	[0.008]	[0.011]	[0.014]
Young Cohort	0.424***	0.482***	-0.058***
	[0.007]	[0.008]	[0.011]
Difference	-0.325***	-0.199***	-0.126***
	[0.007]	[0.009]	[0.011]

Panel D: Completed 3rd Grade	DHS 2000	DHS 1992	Difference
Old Cohort	0.690***	0.621***	0.069***
	[0.009]	[0.011]	[0.014]
Young Cohort	0.275***	0.360***	-0.085***
	[0.007]	[0.008]	[0.011]
Difference	-0.415***	-0.261***	-0.154***
	[0.007]	[0.010]	[0.012]

Panel E: Completed 4th Grade	DHS 2000	DHS 1992	Difference
Old Cohort	0.606***	0.544***	0.062***
	[0.010]	[0.011]	[0.015]
Young Cohort	0.158***	0.253***	-0.095***
	[0.005]	[0.007]	[0.009]
Difference	-0.448***	-0.291***	-0.157***
	[0.008]	[0.010]	[0.013]

Panel F: Completed 5th Grade	DHS 2000	DHS 1992	Difference
Old Cohort	0.500***	0.470***	0.030*
	[0.011]	[0.011]	[0.016]
Young Cohort	0.077***	0.159***	-0.082***
	[0.004]	[0.005]	[0.007]
Difference	-0.423***	-0.311***	-0.112***
	[0.009]	[0.010]	[0.014]

Panel G: Completed 6th Grade	DHS 2000	DHS 1992	Difference
Old Cohort	0.396***	0.402***	-0.006
	[0.011]	[0.011]	[0.016]
Young Cohort	0.028***	0.089***	-0.061***
	[0.002]	[0.004]	[0.005]
Difference	-0.368***	-0.313***	-0.055***
	[0.010]	[0.010]	[0.015]

Notes: Robust standard errors in brackets, clustered at the enumeration level. * significant at 10%, ** significant at 5%, *** significant at 1%. Cross tabulations are restricted to individuals aged 6 to 35. For every panel, each interior cell measures for that particular sub-group (old or young cohort, in 2000 or 1992) the average years of schooling (Panel A) or the proportion completing a given grade (Panels B-G). Young cohort is defined as individuals aged 6 to 15, while the old cohort is individuals aged 16 to 35. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Table 3: Province-Age Cohort Fixed Effects Regressions Measuring the Difference in Children's Years of Schooling Before and After the Genocide

Dependent Variable: Years of Schooling	(1)	(2)	(3)
Young Cohort * DHS 2000	-0.421***	-0.535***	0.160*
	[0.097]	[0.101]	[0.083]
Female * (Young Cohort * DHS 2000)		0.219***	
		[0.044]	
Non-poor * (Young Cohort * DHS 2000)			-1.223***
			[0.064]
Female	-0.113***	-0.179***	-0.103***
	[0.026]	[0.034]	[0.026]
Non-poor	0.165***	0.163***	0.536***
	[0.024]	[0.024]	[0.038]
DHS 2000	0.232***	0.235***	0.203***
	[0.064]	[0.064]	[0.059]
Household Level Controls			
Age of Household Head	0.005***	0.005***	0.005***
	[0.001]	[0.001]	[0.001]
Highest Education Level—Any HH Member	0.446***	0.446***	0.444***
	[0.006]	[0.006]	[0.006]
Number of Children Under 5	-0.113***	-0.110***	-0.098***
	[0.013]	[0.013]	[0.013]
Rural	-0.270***	-0.272***	-0.173*
	[0.103]	[0.103]	[0.097]
Child Age Fixed Effects?	Yes	Yes	Yes
Province Fixed Effects?	Yes	Yes	Yes
Observations	45642	45642	45642

Notes: Robust standard errors in brackets, clustered at the enumeration level. * significant at 10%, ** significant at 5%, *** significant at 1%. The difference-in-differences variable is the interaction of a dummy variable for being in the young cohort aged 6 to 15 and a dummy variable for being in the 2000 DHS. Regressions are limited to individuals aged 6 to 35. Non-poor is a dummy variable indicating the individual has more assets than the population mean. Assets used to calculate this wealth measure include piped running water, refrigerator, radio, finished floor, bicycle, motorcycle, and car. After 1992, the Rwandan government combined Byumba and Umutara provinces, so for consistency we treat them as one province across the two datasets. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Table 4: Province-Age Cohort Fixed Effects Regressions Measuring the Difference in Children's Grade Completion Before and After the Genocide, Baseline Regressions

Dependent Variable: Completion of:	1st Grade (1)	2nd Grade (2)	3rd Grade (3)	4th Grade (4)	5th Grade (5)	6th Grade (6)
Young Cohort * DHS 2000	-0.064***	-0.126***	-0.149***	-0.145***	-0.103***	-0.041***
	[0.009]	[0.009]	[0.009]	[0.010]	[0.011]	[0.012]
Female	-0.015***	-0.009**	-0.005	-0.007	-0.008**	-0.013***
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Non-poor	0.027***	0.029***	0.032***	0.025***	0.022***	0.017***
	[0.005]	[0.005]	[0.005]	[0.005]	[0.004]	[0.004]
DHS 2000	0.069***	0.071***	0.074***	0.070***	0.047***	0.013
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.009]
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Household Level Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45642	45642	45642	45642	45642	45642

Notes: Robust standard errors in brackets, clustered at the enumeration level. * significant at 10%, ** significant at 5%, *** significant at 1%. The difference-in-differences variable is the interaction of a dummy variable for being in the young cohort aged 6 to 15 and a dummy variable for being in the 2000 DHS. The linear regressions are restricted to individuals aged 6 to 35. Non-poor is a dummy variable indicating the individual has more assets than the population mean. Assets used to calculate this wealth measure include piped running water, refrigerator, radio, finished floor, bicycle, motorcycle, and car. Household level controls include age of household head, highest education level of any household member, number of children in the household under age 5, and a dummy indicating if the household lives in a rural area. After 1992, the Rwandan government combined Byumba and Umutara provinces, so for consistency we treat them as one province across the two datasets. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Table 5: Province-Age Cohort Fixed Effects Regressions Measuring the Difference in Children's Grade Completion Before and After the Genocide, With Gender Interactions

Dependent Variable: Completion of:	1st Grade (1)	2nd Grade (2)	3rd Grade (3)	4th Grade (4)	5th Grade (5)	6th Grade (6)
Young Cohort * DHS 2000	-0.080***	-0.147***	-0.170***	-0.159***	-0.114***	-0.050***
	[0.010]	[0.010]	[0.011]	[0.011]	[0.011]	[0.013]
Female * (Young Cohort * DHS 2000)	0.032***	0.040***	0.040***	0.026***	0.019***	0.016***
	[0.009]	[0.008]	[0.008]	[0.008]	[0.007]	[0.006]
Female	-0.025***	-0.021***	-0.017***	-0.015***	-0.014**	-0.018***
	[0.005]	[0.005]	[0.005]	[0.006]	[0.005]	[0.005]
Non-poor	0.027***	0.029***	0.032***	0.025***	0.022***	0.017***
	[0.005]	[0.005]	[0.005]	[0.005]	[0.004]	[0.004]
DHS 2000	0.069***	0.072***	0.075***	0.071***	0.047***	0.013
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.009]
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Household Level Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45642	45642	45642	45642	45642	45642

Notes: Robust standard errors in brackets, clustered at the enumeration level. * significant at 10%, ** significant at 5%, *** significant at 1%. The difference-in-differences variable is the interaction of a dummy variable for being in the young cohort aged 6 to 15 and a dummy variable for being in the 2000 DHS. The linear regressions are restricted to individuals aged 6 to 35. Non-poor is a dummy variable indicating the individual has more assets than the population mean. Assets used to calculate this wealth measure include piped running water, refrigerator, radio, finished floor, bicycle, motorcycle, and car. Household level controls include age of household head, highest education level of any household member, number of children in the household under age 5, and a dummy indicating if the household lives in a rural area. After 1992, the Rwandan government combined Byumba and Umutara provinces, so for consistency we treat them as one province across the two datasets. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Table 6: Province-Age Cohort Fixed Effects Regressions Measuring the Difference in Children's Grade Completion Before and After the Genocide, With Wealth Interactions

Dependent Variable: Completion of:	1st Grade (1)	2nd Grade (2)	3rd Grade (3)	4th Grade (4)	5th Grade (5)	6th Grade (6)
Young Cohort * DHS 2000	-0.049***	-0.112***	-0.124***	-0.102***	-0.038***	0.039***
	[0.010]	[0.010]	[0.010]	[0.010]	[0.010]	[0.010]
Non-poor * (Young Cohort * DHS 2000)	-0.032***	-0.029***	-0.052***	-0.091***	-0.139***	-0.169***
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]
Female	-0.015***	-0.009**	-0.005	-0.006	-0.007*	-0.012***
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Non-poor	0.036***	0.038***	0.048***	0.053***	0.064***	0.068***
	[0.005]	[0.005]	[0.006]	[0.006]	[0.006]	[0.006]
DHS 2000	0.068***	0.070***	0.073***	0.068***	0.044***	0.009
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Household Level Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45642	45642	45642	45642	45642	45642

Notes: Robust standard errors in brackets, clustered at the enumeration level. * significant at 10%, ** significant at 5%, *** significant at 1%. The difference-in-differences variable is the interaction of a dummy variable for being in the young cohort aged 6 to 15 and a dummy variable for being in the 2000 DHS. The linear regressions are restricted to individuals aged 6 to 35. Non-poor is a dummy variable indicating the individual has more assets than the population mean. Assets used to calculate this wealth measure include piped running water, refrigerator, radio, finished floor, bicycle, motorcycle, and car. Household level controls include age of household head, highest education level of any household member, number of children in the household under age 5, and a dummy indicating if the household lives in a rural area. After 1992, the Rwandan government combined Byumba and Umutara provinces, so for consistency we treat them as one province across the two datasets. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Table 7: Province-Age Cohort Fixed Effects Regressions Measuring the Difference in Children's Schooling Before and After the Genocide, With War Intensity Interactions

Dependent Variable: Years of Schooling (Column 1) Completion of Grade (Columns 2-7)	Years of Schooling	1st Grade	2nd Grade	3rd Grade	4th Grade	5th Grade	6th Grade
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High War Intensity Measure A * Young Cohort * DHS 2000	-0.0242*	-0.0005	-0.0010	-0.0013	-0.0028*	-0.0026	-0.0035*
	[0.0143]	[0.0014]	[0.0014]	[0.0015]	[0.0015]	[0.0016]	[0.0018]
High War Intensity Measure B * Young Cohort * DHS 2000	-0.3285*	-0.0107	-0.0316	-0.0368*	-0.0419**	-0.0277	-0.0232
	[0.1956]	[0.0196]	[0.0199]	[0.0207]	[0.0202]	[0.0223]	[0.0248]
High War Intensity Measure C * Young Cohort * DHS 2000	-0.0229*	-0.0011	-0.0026*	-0.0018	-0.0026*	-0.0021	-0.0021
	[0.0133]	[0.0015]	[0.0014]	[0.0015]	[0.0014]	[0.0016]	[0.0017]
Observations	45642	45642	45642	45642	45642	45642	45642

Notes: Robust standard errors in brackets, clustered at the enumeration level. * significant at 10%, ** significant at 5%, *** significant at 1%. Each cell represents a separate regression where the dependent variable is as indicated at the top of the column and the independent variables include the following variables (in addition to the difference-in-difference-in-differences coefficient presented): young cohort * DHS 2000, high war intensity *young cohort, high war intensity * DHS 2000, DHS 2000, female, non-poor, child age fixed effects, province fixed effects, and household level controls as in previous tables. The difference-in-difference-in-differences variable is the interaction of a variable measuring war intensity, a dummy variable for being in the young cohort aged 6 to 15, and a dummy variable for being in the 2000 DHS. High war intensity measure A is defined as the proportion of days during the genocide during which killings occurred. High war intensity measure B is a dummy variable indicating the 3 provinces that had the most number of genocide victims. High war intensity measure C is defined as the number of mass graves sites and memorials per province. Regressions are restricted to individuals aged 6 to 35. Non-poor is a dummy variable indicating the individual has more assets than the population mean. Assets used to calculate this wealth measure include piped running water, refrigerator, radio, finished floor, bicycle, motorcycle, and car. Household level controls include age of household head, highest education level of any household member, number of children in the household under age 5, and a dummy indicating if the household lives in a rural area. After 1992, the Rwandan government combined Byumba and Umutara provinces, so for consistency we treat them as one province across the two datasets. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.

Table 8: Instrumental Variables Regressions Measuring the Difference in Children's Schooling Before and After the Genocide, Instrumenting for War Intensity Interactions

	War Intensity Measure A (1)	War Intensity Measure B (2)	War Intensity Measure C (3)
Panel A: First Stage Regressions			
Dependent Variable:			
High War Intensity A * Young Cohort * DHS 2000	0.090***		
	[0.001]		
High War Intensity B * Young Cohort * DHS 2000		0.0043***	
		[0.0001]	
High War Intensity C * Young Cohort * DHS 2000			0.113***
			[0.001]
Panel B: Instrumental Variables Regressions			
Dependent Variable:			
Years of Schooling	-0.047*	-0.840*	-0.036*
	[0.025]	[0.466]	[0.019]
Completion of 1st Grade	-0.004	-0.061	-0.003
	[0.003]	[0.051]	[0.002]
Completion of 2nd Grade	-0.005**	-0.096**	-0.004**
	[0.002]	[0.047]	[0.002]
Completion of 3rd Grade	-0.005*	-0.087*	-0.004*
	[0.003]	[0.053]	[0.002]
Completion of 4th Grade	-0.006**	-0.111**	-0.005**
	[0.003]	[0.052]	[0.002]
Completion of 5th Grade	-0.006**	-0.112**	-0.005**
	[0.003]	[0.055]	[0.002]
Completion of 6th Grade	-0.006*	-0.112*	-0.005*
	[0.003]	[0.061]	[0.003]
Observations in Each Regression	45642	45642	45642

Notes: Robust standard errors in brackets, clustered at the enumeration level. * significant at 10%, ** significant at 5%, *** significant at 1%. Each cell represents a separate regression where the dependent variable is as indicated and the independent variables include the following variables (in addition to the difference-in-difference-in-differences coefficient presented): young cohort * DHS 2000, high war intensity *young cohort, high war intensity * DHS 2000, DHS 2000, female, non-poor, child age fixed effects, province fixed effects, and household level controls as in previous tables. The potentially endogenous war intensity variables are instrumented for using the distance from the province capital to the Ugandan border and results indicate provinces further from the Ugandan border were more likely to be high war intensity regions. The difference-in-difference-in-differences variable is the interaction of the variable measuring war intensity, a dummy variable for the young cohort aged 6 to 15, and a dummy variable for being in the 2000 DHS. High war intensity measure A is defined as the proportion of days during the genocide during which killings occurred. High war intensity measure B is a dummy variable indicating the 3 provinces that had the most number of genocide victims. High war intensity measure C is defined as the number of mass graves sites and memorials per province. Regressions are restricted to individuals aged 6 to 35. Other variables are as defined in Table 3. After 1992, the Rwandan government combined Byumba and Umutara provinces, so for consistency we treat them as one province across the two datasets. Data source: 1992 and 2000 Rwanda Demographic and Health Surveys.