

# War and the Destruction of Human Capital

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August 7, 2013

## ABSTRACT

The identification of the effect of wars on human capital tends to focus on the population of school age children at the time of the conflict. Our paper introduces a methodology to estimate the effect of war on the stock of human capital by examining the changes in the presence of educated people after the Rwanda genocide. We find that the genocide reduced the stock of human capital in Rwanda severely. The before-and-after results show that highly educated individuals (i.e., those with primary education or more) are “missing” at a rate that is 19.4% higher than the less educated. Moreover, Rwanda’s average years of schooling is lower by 0.37 years. When comparisons with Uganda are made, these estimates more than double suggesting that, if anything, the previous finding were biased downwards. Interestingly, when the cross-sectional variation within Rwanda variation in intensity of genocide is exploited there is no evidence of statistically significant differences. This suggests that the losses in the stock of human capital due to the Rwandan genocide were aggregate in nature.

KEYWORDS: Civil war, Mortality, Human capital investment, Education, Genocide, Africa

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# 1 Introduction

Civil conflict has potentially serious consequences for economic development. A growing literature documents the effects of conflict on education (Shemyakina (2006), Blattman and Annan (2007), Akresh and De Walque (2008)) and health (Alderman et al. (2006), Akresh et al. (2009), Agüero and Deolalikar (2012)). However, not much is understood about the quantitative importance of different channels through which conflict affects human capital accumulation (Blattman and Miguel, 2009). This paper studies the medium to long-term effects of the 1994 Rwanda genocide, which is known to be the deadliest civil war of the 1990's, killing up to approximately 1 million people in a span of about 100 days (Murray et al., 2002). Our paper is unique in two major respects. First, in contrast to most recent studies which focus on the effects of conflict on human capital investments in children of school going age (such as changes in child height and grade progression), we study the effects of the Rwandan genocide on the stock of human capital (education) as a potential source through which genocide may have long-term consequences on human capital accumulation and economic development in general. Second, we identify the effects of conflict by applying the differences-in-differences methodology on three census data sets at the sub-national as well as national level.

A key contribution of this paper is to focus on the stock of human capital (educated people) rather than educational attainment of people who were of school going age at the time of the genocide. This is important for at least four reasons. First, stock of human capital has been shown to be correlated with not only aggregate output across the globe and over time, but is also associated with social outcomes such as fertility and schooling of children (see for example Barro and Lee (1994, 2010), Breierova and Duffo (2004), Cutler et al., (2006), Lucas (1988) and Mankiw et al.(1992)). Second, the dropout age in low-income countries is low-around 11 in Rwanda-leaving a large proportion of the population out of these studies. This limitation is amplified when the war is relatively short as in the case of 100-day Rwanda genocide.

Third, in contrast to most of the current literature which has focused on children of school going age or younger, this paper focuses on older individuals who are at least as likely to be targeted during war than children. Figure 3.1 shows age and sex distribution of global war casualties in 2000. As is clear, although a large number of children died in war, the most deaths, for either males or females, are concentrated in those aged between 15-44. This suggests that older individuals are at least as potentially vulnerable, if not more, to war related mortality shocks than young children. Recently, Akresh et al. (2012) study the impact of the 1967-1970 Nigerian civil war on adult height. In contrast to earlier studies, the authors find that the children who were exposed to war during adolescence had the largest impact. Early childhood matters. But there exists certain types of shocks- such as wars- and certainly channels- violent deaths vs disease induced deaths- the most vulnerable population for which may not be children. Even if children were as likely to be killed from war than adolescents and adults, violent deaths may be far more concentrated among adolescents and adults rather than younger children.

Last, irrespective of the age group one studies, it is important to study effects of shocks- such as war- not only on investments but also destruction in the stock of human capital. If educated people are less likely to be killed or displaced in war, then it increases the returns to investing in education. Alternatively, if educated people are more likely to be killed or displaced then it raises questions about the value of education. Or at the very least, it is a call for providing complementary goods along side education which ensure that the investments made in schooling are not made redundant. <sup>1</sup> It may very well be the case that during critical periods in a country's history, such as the time just before a genocide or a revolution against educated elites, the returns to schooling may be negative for they may make one more likely to be attacked. Given that the Rwanda genocide is known to be targeted at the more educated Tutsis as well as moderate Hutus, it is not clear how high the returns from

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<sup>1</sup>For example, Barro and Lee (2010) find that returns to secondary schooling is higher than primary schooling, which even has negative returns for many countries. Does this imply one should not invest in primary schooling? To the contrary, it suggests that investing in just primary schooling is not enough and should be complemented by secondary schooling

schooling were at the time just before the genocide.

Other than asking an important yet ignored question, our identification strategy involves applying the differences-in-differences methodology on three census data sets at the sub-national as well as national level. The identification of the effects of conflict on economic development represents an empirical challenge for at least two reasons. First, it is difficult to isolate the effects associated to conflicts from other possible confounding factors including political and economic instability (Miguel and Blattman, 2009). Second, the use of household surveys relies on “survivors”. As the number of deaths increase during conflicts, it creates a change in the demographic composition of a country. Those who are interviewed after a conflict ends are unlikely to be a random sample of the population. If educated people are targeted during a genocide, then educated people may more likely to be missing. Estimating the effects of the conflict based on a sample of survivors could be biased if the probability of survival is correlated with the outcome of interest. If pre-conflict indicators such as education levels affect the probability of survival then since most development-related outcomes are likely to be correlated with these variables, the resulting estimates will be biased.

A possible solution is to obtain information about those who died from those still alive post-conflict. DeWalque (2010) and DeWalque (2005) use this method. The former uses the maternal mortality module of the Demographic and Health Survey of Rwanda in 2000 which asks reproductive-age women about the survival of her siblings as well as their gender and age. For those siblings who died, the DHS also collects information about the year of death. Socio-economic variables are inferred by assuming that siblings shared similar levels of education. The authors use this information to identify the demographic characteristics of the dead.

However, two sources of non-random selection bias emerge. First, deaths are registered based on surviving women. If complete families were killed (or displaced out of Rwanda) or if a survivor is a male these families are not going to be included in the DHS. Thus, the death records are representative of families where a women member of the family survived

and that is unlikely to be a random sample. Second, the DHS itself is not a random sample of all women. It is only for women of reproductive age. Those aged between 15 and 49 are subject to the maternal mortality module. Thus, the sample for the analysis is only for the subset of households where at least one survivor was a woman between the ages of 15 and 49 and this sample, almost certainly, is not representative of the population at large. For example, if there is an increase in the number of child headed households, it suggests that all adult members of the family were most likely killed. DHS maternal mortality schedule could not capture such cases.

An alternate method used by demographers is the reconstruction approach based on demographic accounting. Between any two dates, “excess mortality” can be inferred as a residual from changes in population unexplained by changes in number of births, deaths, immigrants and emigrants.<sup>2</sup> To get an estimate on the excess mortality, one needs to know how different the actual trend is from the potential trends in population which would have existed in the absence of the shock. The solution of the reconstruction approach is to exclusively rely on temporal variation to determine the excess mortality. Moreover, it uses data on mortality, fertility and migration flows during the time of the genocide. As Heuveline (1998a) states, such data is not only hard to get but is highly prone to measurement error. Furthermore, the reconstruction approach is not only sensitive to over /underestimation of population size at the start relative to the end of the period, but also to migration, the other cause of changes in cohort size.

In this paper, we introduce a differences-in-differences methodology, which will identify the demographic changes generated by the 1994 genocide in Rwanda that avoids the sample selection problems described above. We estimate missing persons by comparing the size

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<sup>2</sup>One of the most careful reconstruction exercise has been carried out, for Cambodia, by Heuveline (1998 (a), 1998 (b), 2001 (a), 2001 (b)). To compare demographic effects of civil war between 1970 and 1980, he projected data from the 1962 census to get an estimate of the 1970 population, which served as a baseline. An estimate of 1980 population is derived from by backward projection of 1993 electoral lists data. In addition, estimates of “natural” mortality and of “natural” migration are used to project backward the 1980 estimates to 1970 and project forward the 1970 estimates into 1980. Using the backward and forward projections, residuals from the actual vs. projected estimates are calculated. The number of “excess mortality”, is estimated from the average of the backward and forward projection residuals

of cohorts in the post-genocide 2002 Rwandan census against their size in the pre-genocide 1991 census. Temporal variation - comparing before vs. after the genocide- is combined with the spatial variation of the conflict to further identify the characteristics of those missing. Rwanda is compared with Uganda, a neighboring country which despite experiencing civil war, like Rwanda, did not experience genocide in the same period.

To avoid any contamination due to fertility changes generated by the genocide and to not confound our estimates from naturally high infant mortality rates, we focus on those who were older than 12 years in 1991. Similarly, to disentangle our results from the naturally high mortality of the older cohorts, we focus on those aged 60 or less. To address concerns regarding immigration, we limit our attention to native born only. As an overwhelming majority of native born were present in their place of birth at the time of the census was carried out, this gives us some confidence that migration may not be driving our results. Lastly, we explore spatial heterogeneity in intensity of conflict within Rwanda to explore if the genocide was more of an aggregate nature.

The genocide reduced the stock of human capital in Rwanda severely. The before-and-after results show that highly educated individuals (i.e., those with primary education or more) are missing at a rate that is 19.4% higher than the less educated. Moreover, Rwanda's average years of schooling is lower by 0.37 years. When comparisons with Uganda are made, these estimates more than double suggesting that, if anything, the previous finding were biased downwards. Interestingly, when the subnational variation within Rwanda variation in the intensity of conflict is exploited for the same measures, there is no evidence of statistically significant differences. This suggests that the losses in the stock of human capital due to the Rwandan genocide were aggregate in nature.

## 2 Background

Rwanda is a landlocked country in East Africa with a small but highly dense population. It neighbors Uganda, Tanzania, Burundi and Congo (see Figure 2). For about 100 days from April 6 1994, when the then President of Rwanda, Habyarimana's plane was shot down, more than 500,000 people of largely Tutsi ethnicity were massacred by the Hutus. Estimates of the death toll have ranged widely from 500,000 to about 1 million people (Prunier, 1995). This genocide did not happen in a vacuum but was the result of long standing rivalry between the majority Hutu peoples and a relatively more urbanized and educated Tutsi minority.

Although the two distinct ethnicities of Hutu and Tutsi did exist even before Rwanda was colonized, the Belgian colonizers sharpened the divide by discriminating against the Hutu majority, based on various factors including physical appearance. Things changed between 1950's to 1960's. The Hutu rallied for, and won, Rwandas independence, but not without a violent campaign against Tutsi's which led to numerous deaths and large-scale refugee movements into neighboring countries such as Uganda.

Rwanda continued to be ruled under different Hutu military dictatorships for the next three decades. During this period there was relatively less violence within Rwanda. And then came the year 1990. A rebel Tutsi group, The Rwandan Patriotic Front (RPF), invaded northern Rwanda from Uganda. The Rwandan civil war had begun as the Rwandan armed forces (Forces Armes Rwandaises, FAR) responded. More than three years of ethnic violence led to killings and emigration of numerous Hutus from northern Rwanda accompanied by similar but localized attacks against Tutsis in the south. Under international pressure, the Hutu-led government of President Habyarimana agreed to cease-fire in 1993 , sharing significant power with the Tutsi RPF group. This, however, proved to be unstable. In 1994 when Habyarimana's plane was shot down, the Hutu extremists unleashed what will be remembered as one of the most horrific genocides in human history. For in depth history of Rwanda, the reader may want to explore books written by G. Prunier (1995), C. Newbury (1988), D. De Lame (1996), F. Reyntjens (1994) and J.P. Chrtien (2000), among others.

As a response to the genocide, many fled to neighboring countries such as Congo and Burundi. Because of the reality of migration, we focus on missing people rather than deaths. But having said that, between 1994 and 2002, many of these migrants returned back because of conflict in those countries. In 1996 and 1997, for example, violence in Burundi, Tanzania and Congo forced numerous Rwandans to repatriate (World Bank, 2003). A 2001 Rwandan nationally representative survey, *Enquête Intégrale sur les Conditions de Vie des Ménages*, shows that approximately 88-89% percent of individuals currently live in the province in which they were born. Thus although not all those we find as missing were killed, the majority are most likely to have been killed. This conclusion is further strengthened from data on net migration collected from World Development Indicators which show that net migration was about as little as 15,327 in 1990, fell to about -1.5 million by 1995 but rebounded to a positive 1.8 million by 2000, suggesting that if anything between 1990 and 2000, there has been a net positive migration of about 300,000 people.

### 3 Data

Main data are 10 percent random samples of the 1991 and 2002 Rwanda and Uganda population censuses (obtained from IPUMS international: <https://international.ipums.org/>). The samples are restricted to native borns only. <sup>3</sup> <sup>4</sup>

#### 3.1 Missing Rwandans

Figure 3.2 shows raw data for log population before (1991) and after the genocide (2002) in Rwanda. There are many more young than old, as one would expect in a developing country

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<sup>3</sup>Comparisons with Uganda are made for two main reasons. First, its also an East African country so that trends which are common at the regional level for East Africa are not likely to explain the results. Second, Uganda is the only country African country in the IPUMS dataset for which census in available in 2002 and 1991, the period for which Rwandan data is available

<sup>4</sup>There is a census available for Rwanda pre-genocide from the 1970s. However, this data is not in IPUMS, and was rather hard to make use of. Future work may want to explore such data as additional source of variation for trends specific to Rwanda



like Rwanda. If the conflict did lead to missing people, one would be able to observe this by comparing the pre-genocide vs post-genocide population size series. The 2002 population is strictly less than 1991. For all age cohorts. Figure 3.3 shows the log differences between 2002 and 1991 across birth year cohorts more clearly. There are fewer people across all birth cohorts. It is interesting to note that, those 12 or younger in 1991, are at least as likely to be missing than the older individuals. This confirms the basic insight of Figure 3.1 that young adults are particularly vulnerable to mortality and displacement than young children. Hence, the need to study the adolescent and adult sample.

To get a better idea of how big the changes in Rwandan population are, we recompute Figure 2.3 in Figure 2.4 whereby Rwanda is compared to Uganda. Rwanda's series is particularly distinct. It has a much larger magnitude of missing than Uganda. In the main analyses of this paper, we will restrict the sample to those 13 or older so as to limit our sample to those old enough to have completed primary schooling. It is worth noting, that although Rwandans are missing at higher rates than Ugandans, the Rwandan series is very flat for the bulk of individuals aged between 13-45 in 1991. Since our sample is focused mainly on this sub-sample, it suggests that there may not be much heterogeneity by age in our adolescent-adult sample.<sup>5</sup>

### 3.2 Cohort Analysis and Education Variable

Each census contains region and year of birth of all individuals as well as gender and education level. Those defined as having no schooling or less than 6 years (primary) level of education are labelled as "low" educated. Those undocumented are assigned missing values. We limit our main sample to those born before 1979 so that we do not have natural infant mortality biasing our results and so that individuals are old enough to have completed their primary schooling. This makes it less likely that someone identified as low educated in 1991

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<sup>5</sup>For the rest of the paper age dimension is not exploited further. We did run regressions individuals heterogeneity by age and they confirmed the basic insight of these figures i.e. there is not much heterogeneity by age within old enough adolescents and adults

(13 or more years old at the time) will be still in the process of completing their primary schooling, and thus likely to become high educated by 2002. We also exclude those born before 1930 so that naturally high mortality rate for the old does not confound our estimates of excess mortality. All empty cohorts and cohorts with no observed high educated people were omitted along with corresponding cohort from opposite census year.

A cohort for Rwanda is defined by birth year, gender, whether completed primary schooling or not, province of birth and census year. For Uganda, instead of province of birth, we have district of birth. As a result, we exclude all districts for whom we did not have before and after data. Information about spatial intensity of genocide within Rwanda comes from [www.genodynamics.com](http://www.genodynamics.com) and has been used by other scholars like Akresh et al.(2008). Measure A is a measure of genocide intensity. It is proportion of days during genocide when killings occurred. Measure A is a continuous variable. However, we define a discrete version of A for some of our analysis which assumes value 1 when a given regression has higher than average exposure to days with attacks, and 0 otherwise. This measure has also used by Akresh et al. (2008). See data appendix ( 8) for details on other measures used.

By aggregating data at cohort level we address the problem of clustered standard errors. The assumption is that although randomization may not happen at individual level, the unit of randomization is at the cohort level. This is one of the solutions to the problem of clustering proposed by Bertrand et al. (2004). The cohort being defined by year of birth, region of birth, gender, education and census year. Sample weights are defined by inverse of cohort size and are used in all weighted regressions. All regressions use robust standard errors which accounts for heteroscedasticity in distribution of error terms.

### **3.3 Summary Statistics**

Row 1 of Table 3.1 shows cohort size in 1991 versus 2002 for Rwanda and Uganda. Both Rwanda and Uganda witness a decline in cohort size. Uganda has been going through a low-intensity civil war during the same period as well. Row 2 explores the cohort size

of educated individuals (those completed primary schooling). In 1991, although Rwanda and Uganda had a similar cohort size of educated people of around 1000, twice as many educated Rwandans are missing than educated Ugandans between 1991 and 2002. It may be the case that a similar trend exists for less educated individuals, so that the educated are not particularly more likely to be missing. Row 3 explores ratios of educated to less educated individuals. In contrast to Rwanda, where in 1991 for every 100 individuals who did not complete primary schooling there were 40 who did, in Uganda the corresponding number was 78. But by 2002, in contrast to Uganda where we see that the ratio of educated people has increased to almost parity, in Rwanda it falls even further to around 31. A clear divergence. A similar story emerges when average years of schooling is explored. In the 1991 sample, it was about 3 years for Rwanda versus 3.8 for Uganda. But by 2002, although Uganda witnesses an increase in years of schooling to 4.2, Rwanda witnesses a decline to 2.7.

## 4 Empirical strategy

Estimating the effect of a conflict is complicated, among other factors, by the identification of the control group. The main missing data problem comes from the inability to observe what would have happen to the treatment group—those exposed to the conflict—in the absence of the war. The literature has used several approaches based on different assumptions about the nature of the control group. Several papers including (Mansour and Rees (2012), Akresh, Lucchetti, Thirumurthy (2012), Akresh et al. (2011)) use a post-conflict cross-sectional survey and exploit (within-country) spatial variation in the intensity of the conflict (the intensity could include total absence of conflict). In this case the data consists of observations varying by cohort  $i$  and space  $j$ . The following equation represents this methodology

$$y_{ij} = \beta_1 G_{ij} + \alpha_i + \alpha_j + \epsilon_{ij} \tag{1}$$

where  $y_{ij}$  is the outcome of interest and  $\alpha_i$  and  $\alpha_j$  are fixed effects at the cohort and

space-level, respectively. Thus,  $\beta_1$  is the parameter of interest as it captures the difference in  $y_{ij}$  for the treatment group comparing its actual value against the predicted value based on the observed  $y_{ij}$  of the control group.

This model assumes that all differences in the treatment and control groups are captured by  $\alpha_j$  and that there are not time-varying unobserved characteristics. Note also the impact evaluated here by  $\beta_1$  is comparing areas with high against low intensity of conflict. In most cases (references needed) the levels of violence in the low-intensity areas is not zero, as in the case of Rwanda, thus the effect measured by  $\beta_1$  could be underestimating the true effect because the control group has been “contaminated.” This possible bias is amplified when the country-wide effect of the conflict lead to small variation across spaces or regions within the country. If the only data available is post-conflict cross-section then we cannot test for the possible bias in  $\beta_1$ .

Suppose that there is another cross-sectional survey that took place prior to the conflict period or when the war was just starting. The observations are now given by cohorts (i), space (j) but also time, denoted by (t). Thus, equation 1 can be rewritten as

$$y_{ijt} = \beta_2 G_{ijt} + \alpha_i + \alpha_j + \alpha_t + \gamma_{ijt} X_{ijt} + \epsilon_{ijt} \quad (2)$$

Equation 2 incorporates two new sets of parameters. First at is the survey-year fixed effect and second, the  $\gamma_{ijt}$  captures the effect of the cross-products (by pairs included in  $X_{ijt}$ ) of cohorts, space and time. In this equation  $G_{ijt}$  is now the triple interaction and  $\beta_2$  is the parameter of interest. Unlike  $\beta_1$ ,  $\beta_2$  might exhibit less bias because the effect is capturing differences with respect a period of “peace” or less conflict. This is the approach followed, for example, by Akresh et al. (2008). An alternative to model 2 is to observe not a pre-conflict dataset but to use a different country as an alternative control group. In this case the model is given by

$$y_{ijc} = \beta_3 G_{ijc} + \alpha_i + \alpha_j + \alpha_c + \gamma_{ijc} X_{ijc} + \epsilon_{ijc} \quad (3)$$

In equation 3 we substituted the index  $t$  (year of survey) for  $c$  that indexes the countries. Like 2, equation 3 has an effect that accounts for nation-wide effects. Whether equation 2 or 3 are better at capturing these nation-wide or aggregate effects depend on the validity of the external country as a comparison group and the presence of violence in the pre-conflict data. The choice between equation 2 and 3 depends on what datasets are available to the researchers. Consider now the case where there are two surveys per country

$$y_{ijct} = \beta_4 G_{ijct} + \alpha_i + \alpha_j + \alpha_c + \alpha_t + \gamma_{ijct} X_{ijct} + \epsilon_{ijct} \quad (4)$$

in this case the effect we can account for country and time unobserved factors. A key advantage of this paper is the access to all four data sources. This will allow us to compare the sensitivity of the estimates depending on the assumption about the aggregate nature of the genocide. In the next section we present the empirical framework for the case of Rwanda.

## 5 Results

### 5.0.1 Subnational Estimates

If we just had a post-genocide survey and only exploited spatial intensity of the genocide within Rwanda, we would be estimating the equivalent of equation 1 for Rwanda. For our analysis, each observation is a cohort defined by birth year, province of birth, gender and census year, and is weighted by the cohort size. Our analysis controls for gender and year of birth fixed effects. We treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. To explore the spatial intensity of genocide, we explore the number of days, per province, that the genocidal killings occurred in Rwanda (Measure A). Using Measure A, the effect of the genocide on average years of schooling is estimated in Column (1) of Table 3.2. The results show that the higher the genocide intensity, the higher the average years of schooling. At first this may sound surprising, as one does not expect genocide to have a positive effect on schooling. However,

once we realize that the genocide may be targeted in areas which had higher schooling levels (Tutsis), these results are suggestive of selection effects.

To control for time-invariant province level unobservables which may be confounding the estimates from Column (1), we take advantage of our unique pre-genocide census in 1991. Many conflict studies, do not have such data available, and are left with exploiting spatial and cohort variation only, instead of time variation. When we estimate the equivalent of equation 2 for Rwanda in Column (2) , we find that there is no evidence of any significant effects of the genocide intensity within Rwanda on years of schooling. This is an intriguing result, as previous research (for e.g. DeWalque 2010) suggests that genocide is correlated with excess mortality of educated people. It may, however, be the case that the Rwandan genocide is of an aggregate nature so that subnational comparisons are biased downwards and do not reveal the true effects on education.

### **5.0.2 Aggregate Estimates**

Column (3) shows estimates of the effect of the genocide at the national level rather than the sub-national level as was done in the previous section. Instead of exploiting variation within Rwanda, only variation over time is exploited to analyze the aggregate effect of the genocide. The estimates reveal that genocide lowers the stock of average years of schooling by as much as 0.37 years of schooling! This estimate is significant more for at least two reasons. First, the magnitude is large- more than one third of an year of schooling is lost. And second, because estimates of effect of conflict on investment in child schooling by Akresh et al. (2008) also show a remarkably similar estimate. This suggests that destruction of human capital is not only large, it is broadly comparable to changes in investment in child schooling.

An alternative to estimating equation 2 is to observe not a pre-conflict dataset as a control but to use a different country as an alternative control group. Uganda, which is Rwanda's neighbor and which did not suffer any genocide during the time period, is the only other African country we could identify which has census data for 1991 and 2002 as well as

relevant data on years of schooling. Column (5) shows estimates for equation 3 in which post-genocide comparisons between Rwanda and Uganda are carried out. Consistent with the summary statistics in Table 3.1, we find that there is 1.42 fewer years of schooling in Rwanda compared to Uganda. This is a massive effect. However, it may also be reflecting unobservable differences between Rwanda and Uganda which would have existed even in the absence of genocide.

To explore how much of this difference may be driven by time varying factors which would have changed in Rwanda even in the absence of genocide, I estimate the counterpart of the Column (3) for Uganda in (4). In stark contrast to Rwanda, Uganda witnessed a 0.49 increase in years of schooling during this time.

One may be skeptical about the magnitudes in cross-sectional comparisons in 2002 between Rwanda and Uganda, and about the time varying changes within Rwanda for they may be reflecting preexisting changes or because we do not understand the counterfactual trend in the absence of genocide very well. We now carry out a differences- in-differences method, as in equation 4, exploiting both temporal variation and across country variation to address such a concern. Column (6) shows that after controlling for temporal and country differences, 0.83 years of schooling is lost in Rwanda compared to Uganda between 2002 and 1991. This significant estimate is more than double than the loss of 0.37 years of schooling found by using a pre-genocide control within Rwanda, suggesting that if anything the massive effect within Rwanda may be biased downward.

## **5.1 Alternate Measure of Human Capital: Ratio of Highly Educated to Less Educated**

Table 3.3 carries out a similar exercise as in Table 3.2, but with log ratio of educated to less educated people. Column (1) shows that there are more highly educated people compared to less educated ones in areas which witnessed more days of conflict. At first this may sound puzzling, just like the estimate from Column (1) in Table 3.2. However, once one realizes

that the genocide is known to be targeted at Tutsis who were generally more educated than Hutus, it should not be surprising that despite the genocide, the regions where conflict was most intense still has a higher ratio of educated people. This suggests that just utilizing within country spatial variation in genocide is not enough, if one does not have a good control.

To control for time-invariant province level unobservables which may be confounding the estimates from Column (1), we take advantage of our unique pre-genocide census in 1991. When we estimate the within difference model for Rwanda in Column (2), we find that similar to the estimates for years of schooling, there is no evidence of any significant effects between cohort size of educated and less educated. This provides us further confidence that estimates for war's effects on stocks of human capital may be biased downwards if they only consider subnational comparisons. Nonetheless, it is important to verify if indeed aggregate effects are also found for the ratio of highly educated to less educated measure.

Column (3) shows estimates of the effect of the genocide at the national level rather than the sub-national level as was discussed above. Instead of exploiting variation within Rwanda, only variation over time is exploited to analyze the aggregate effect of the genocide. The estimates reveal that genocide lowers the stock of educated people versus less educated people by as much as 19.4% !

An alternative to estimating equation 2 is to observe not a pre-conflict dataset as a control but to use a different country as an alternative control group. Column (5) shows estimates for equation 3 in which post-genocide comparisons between Rwanda and Uganda are carried out. Consistent with the summary statistics in Table 3.1, we find that the ratio of educated to less educated is 79.1% lower in Rwanda compared to Uganda. This is a massive effect, consistent with the large Table 3.2 estimates. However, it may also be reflecting unobservable differences between Rwanda and Uganda which would have existed even in the absence of genocide.

To explore how much of this difference may be driven by time varying factors which



would have changed in Rwanda even in the absence of genocide, I estimate the counterpart of the Column (3) for Uganda in (4). In stark contrast to Rwanda, Uganda witnessed a 27.8% increase in size of educated to less educated people.

As mentioned earlier in discussion of Table 3.2 results, one may be skeptical about the magnitudes in cross-sectional comparisons in 2002 between Rwanda and Uganda, and about the time varying changes within Rwanda for they may be reflecting preexisting changes or because we do not understand the counterfactual trend in the absence of genocide very well. We now carry out a differences- in-differences method, as in equation 4, exploiting both temporal variation and across country variation to address such a concern. Column (6) shows that after controlling for temporal and country differences, there are 44.8% fewer educated people compared to less educated ones in Rwanda versus Uganda, before versus after the genocide. This significant estimate is more than double than the loss of 19.4% in stock of educated versus less educated people found by using a pre-genocide control within Rwanda, suggesting that if anything the massive effect within Rwanda may be biased downward.

The results from Table 3.3 are very encouraging as they corroborate the findings in Table 3.2. However, the fact that there is no subnational effects on years of schooling and ratio of educated cohorts raises some interesting questions: Was the genocide so homogenous that there is no effect on just the educated cohorts (without comparing with less educated cohort)? Or is that that there is subnational evidence as well, but for educated as well as uneducated so that the differential effect is zero? Similarly is the years of schooling effect (say based on temporal variation within Rwanda) driven by excess losses in educated cohorts compared to less educated cohorts at the aggregate level or is that the rate at which educated cohorts were increasing is less than that of less educated cohorts? To explore these questions, we now look at the pure effect at the subnational and aggregate level on size of educated cohorts.

## 5.2 Educated Cohort Size Variation in Rwanda

Table 3.4 carries out a similar exercise as in Tables 3.2 and 3.3, but with log ratio of educated to less educated people. In contrast to Tables 3.2 and 3.3, Column (1) shows that there are less highly educated people compared to less educated ones in areas which witnessed more days of conflict. This suggests that although there were more educated to less educated people in high vs less conflict intense regions of Rwanda (Tables 3.2 and 3.3), educated people are also missing but they are missing at a lower rate than less educated in these areas. The difference-in-difference model in Column (2) further supports this hypothesis, as even at the subnational level, there is evidence of fewer educated people. Results from Tables 3.2 and 3.3 showed that aggregate estimates are often very large. To verify if the large aggregate effect also holds of the effect on educated cohort size, temporal variation within Rwanda is explored in Column (3). The estimates reveal that the Rwandan genocide indeed lowered the stock of educated people as as much as a staggering 47.6%, in contrast to 19.4% (Table 3.3) effect when effects on less educated are taken into account.

An alternative to using a pre-conflict dataset as a control is to use a different country as an alternative control group. Column (5) shows estimates for equation 3 in which post-genocide comparisons between Rwanda and Uganda are carried out. We find that post-genocide, the size of educated cohort was 26.8% lower in Rwanda compared to Uganda. This is in contrast to the massive 79.1% effect found in Table 3.2. This suggests that the size of uneducated cohort was much higher in Rwanda versus Uganda post-genocide.

To explore how much of this difference may be driven by time varying factors which would have changed in Rwanda even in the absence of genocide, I estimate the counterpart of the Column (3) result for Uganda in (4). Between 2002 and 1991, the cohort size of educated fell by 24.1% even in Uganda. Given how big and negative this impact is, it highlights the general need to take into account time variation for placebos, to get more accurate estimates of the impact of conflict. Nonetheless, Column (6) which shows the estimates for the differences-in-differences model shows even that after controlling for temporal and country differences,

there are 19.8% fewer educated people compared in Rwanda versus Uganda, before versus after the genocide. In contrast to Tables 3.2 and 3.3, this significant estimate is less than half of the loss of 47.6% in stock of educated people found by using a pre-genocide control within Rwanda.

Overall, results from Table 3.3 tell us the following. First, there are both subnational and aggregate effects of the genocide on size of educated cohorts, which shows that there are indeed missing educated people. Second, there are missing educated people in Rwanda even postgenocide, which to a large extent is explained by the excess missing educated Rwandans versus Ugandans between 2002 and 1991. Third, there are interesting subnational versus aggregate difference in these effects. At the subnational level Tables 3.2, 3.3 and 3.4 together suggest that there were indeed fewer educated cohorts, but the rate of which they were missing is similar to less educated cohorts, leading to no effects for years of schooling and ratio of educated to less educated measures. However, since the aggregate effects for years of schooling and ratio of educated to less educated measures are massive, this shows that one needs to look at the aggregate level and not the subnational level to really see the stark differences in educated versus less educated cohorts.

## **6 Further Robustness Tests**

### **6.1 Cohort Size Variation Within Rwanda**

It may be the case the first set of results (Tables 3.2 and 3.3) we showed on subnational comparisons within Rwanda were showing inconsistent results because our measures are not really capturing genocide intensity well enough. After all, it's possible that the genocide was homogenous enough there is not much differential conflict intensity within Rwanda. However, this argument is not convincing, Akresh et al. (2008) for example exploit the same spatial intensity of the Rwandan genocide to identify the effects of the genocide on completion of schooling for children. Moreover, Table 3.3 clearly shows that there are fewer

educated people at the subnational level. We now verify that there are fewer people in general in more conflict hit areas in Table 3.5. Apart from Measure A, we also use three other genocide intensity measures. Measure B, a more sub-aggregated measure, is dummy for all those provinces which were most hit. Measure C uses satellite data to identify mass grave sites and memorials per province, and Measure D represents the proportion of Tutsis per province in 1991. All of our measures of genocide intensity are negative and statistically significant. With Measure B showing that those provinces with high intensity have as much as 15% fewer people compared to less intensity areas in 2002 vs 1991.

## **6.2 Alternate Measures of Genocide Intensity Within Rwanda**

Table 3.6 explores for the robustness of subnational estimates in Tables 3.2, 3.3 and 3.4 with the alternate measure of genocide intensity. Column (1) further verifies that there among the fewer people in conflict hit areas, there were educated people as well. But since both educated and less educated were targeted at the subnational level, consistent with Tables 3.2 and 3.3, there are no statistically significant differential effects for the educated compared to the less educated and for the average years of schooling (Columns (2) and (3)) when the difference-in-difference model is estimated.

## **7 Discussion and Conclusion**

Since the 1960s, one out of very three nations has been affected by a civil war, with as many a 20% of nations witnessing at least 10 years of civil war (Blattman and Miguel, 2010). The economic impact of war in general has not been understood well enough. Rodrik (1999) argues that conflict is the main factor in explaining lack of persistence in economic growth rates for many nations and in explaining why several countries have experienced a negative growth shock ever since the mid-1970s. Justino and Verwimp (2006) find that in Rwanda alone, 20% of the population slipped into poverty after the 1994 genocide. This paper has

explored the effects of the Rwandan genocide of 1994 on the stock of educated people in Rwanda, whereby approximately 1 million people are reported to have been killed in just 100 days and which is known to have been targeted at the more educated (Tutsis). We have carried out our analysis using novel census data which brackets the genocide in Rwanda, to explore not only the effects at the sub-national level but also at the aggregate level by exploiting various control groups. The stock of human capital is measured in three alternate ways: cohort size of educated individuals, cohort size of educated relative to the less educated individuals, and average years of schooling. Four different measures of genocide intensity within Rwanda have been used for robustness of sub-national estimates. And neighboring Uganda used to test for robustness of aggregate effects in Rwanda.

The neoclassical growth framework provides a useful starting point to think about channels. One of the ways through which war can effect the society is through human capital, an important factor of production. But the framework also tells us that the stock of human capital at any given point is determined not only by investments made in the last period, but by the depreciation in (destruction of) the stock of accumulated human capital. Most of the recent literature which has focused on the effects of war on human capital, has focused on war's effects on investment in children of schooling going age, analyzing children's health and schooling outcomes. But there is an alternate channel through which human capital is affected- the destruction of existing stock of human capital.

To measure the destruction of human capital, this paper adopts a rather unconventional approach in the economics of conflict literature. Since we cannot distinguish between those who were killed or those displaced, we estimate the rate at which individuals are missing. That said, we have argued that the majority of the missing are likely to have been killed. First, much of the displacement happens from one town/village to another within the same province. And not everyone is able to leave Rwanda. A 2001 Rwandan nationally representative survey, *Enquête Intégrale sur les Conditions de Vie des Ménages*, shows that approximately 88-89% percent of individuals currently live in the province in which they

were born. Second, existing evidence suggest that many of the migrants who fled Rwanda to neighboring countries returned back by 2002. In 1996 and 1997, for example, violence in Burundi , Tanzania and Congo forced numerous Rwandans to repatriate (World Bank, 2003). This conclusion is further strengthened from data on net migration collected from World Development Indicators which show that net migration was about as little as 15,327 in 1990, fell to about -1.5 million by 1995 but rebounded to a positive 1.8 million by 2000, suggesting that if anything between 1990 and 2000, there has been a net positive migration of about 300,000 people. From this paper's perceptive, both displacement and mortality present a negative shock to the stock of human capital which we put under the common umbrella term of missing.<sup>6</sup>

Many studies choose to only carry out subnational or aggregate analysis. We do both. And find that the two present a very different picture. When sub-national comparisons are done, although we do find that there are fewer educated people compared to less educated ones, we find no significant differences in high vs low conflict areas, in either years of schooling in areas or ratio of educated to less educated cohorts. We do however document that there are fewer educated people both at the subnational and aggregate level. Even although educated people are more likely to be missing in high conflict areas at the sub-national level, the rate at which they are missing is no different than the rate at which less educated are missing.

In contrast to sub-national analysis, aggregate analysis suggests a rather stark effect of the Rwandan genocide. In terms of the ratio of educated to less educated cohorts, the before-and-after results within Rwanda show that highly educated individuals are missing at a rate that is 19.4% more than less educated individuals. When a differences-in-differences

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<sup>6</sup>There may be different reasons to believe that migration will have different effects on accumulation of human capital than mortality. For one, the educated can return in a later period. Although, to the extent that they don't return in the period under consideration, it may still be treated as a loss. Even if the migrated do not return, the mental health effects on survivors who know that they have relatives and friends in other countries may be very different than those whose close ones died. Moreover, the migrated may send back remunerations to their close ones at home and that may serve as a buffer in mitigating some of the negative shocks on human capital accumulation. Mortality may have different effects on investments in the next generation's human capital as well. Although if entire families migrate or are killed, such remunerations may not be as relevant.

over time in Rwanda versus Uganda is carried out, we find that 44.8% more are missing in Rwanda than Uganda. This is driven by the fact that instead of having fewer educated people, Uganda had fewer less educated individuals by as much as 26.8%.

When average years of schooling is used as a measure, we get a very similar story. The before-and-after results within Rwanda show that the average years of schooling is 0.37 years less. An estimate strikingly similar to what Akresh et al. (2008) find in terms of investment effects on child schooling in Rwanda. But at the aggregate level when a difference-in-difference over time in Rwanda versus Uganda is carried out, we find that average years of schooling is less in Rwanda by as large as 0.83 years of schooling. This is driven by the fact that instead of having lesser years of schooling, Uganda had 0.49 more years of schooling over the same period. Rwanda not only had fewer educated people, the loss of the educated cohort was in stark contrast even compared to its own less educated citizens and to its neighbors.

Together these results reveal that there is much heterogeneity in the effects of the Rwandan genocide on destruction of human capital. The largest effects are found at the aggregate level, compared to the subnational level. In terms of average years of schooling lost, the smallest effect size is 0.37 years of schooling, remarkably similar to what Akresh et al. (2008) find in terms of effects on children's schooling.

The Rwandan genocide was destructive not only because it had large effects on schooling and health investment of the future generations (children). The genocide was not only destructive because of the more than 800,000 who have been known to be killed in a matter of just 100 days. It was destructive also because it led to strikingly large loss of educated cohorts over and above the rest of the population leading to substantial destruction in Rwanda's stock of human capital. Moreover, these effects are manifest at the aggregate level rather than at the subnational level, suggesting that the Rwandan genocide was not a subnational phenomena, but a nation wide catastrophe.

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## 8 Data Appendix

### GENOCIDE INTENSITY MEASURES

Measure A: Number of days, per province, that the genocidal killings occurred. The data is obtained from an online database : [www.genodynamics.com](http://www.genodynamics.com). 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.

Measure B: Dummy for the four provinces - Kigali Ngali , Butare, Kibuye and Kibungo - with most reported killings as reported in [www.genodynamics.com](http://www.genodynamics.com).

Measure C: Number of mass graves sites and memorials per province, with data taken from the Rwandan Genocide Project at Yale University (Rwandan Genocide Project, 2007).

Measure D: Proportion of Tutsis per province found in 1991 Census in Rwanda. This a a measure of potential exposure to genocide, since Tutsis are known to have been systematically targeted.

### DEMOGRAPHIC VARIABLES

Lowed : Those defined as having no schooling or less than 6 years (primary) level of education are labelled as “low” educated.

Highed : Those not missing and not “low” educated are labelled highed or “highly” educated. The undocumented are assigned missing values.

## 9 Tables

Table 1: Summary Statistics

VARIABLES	Rwanda		Uganda	
	1991	2002	1991	2002
Cohort Size	3530	2472	2738	1882
Number of High Educated	1070	630.7	1010	813.1
Ratio HighEd/LowEd	.4087	.3183	.7848	.9266
Years of Education	2.967	2.711	3.839	4.211
Cohorts	1,010	1,010	3114	3114

Cohorts are defined by year of birth, gender, and province of birth (Rwanda) or district of birth (Uganda). Sample includes only natives who are born between 1930 and 1978. All empty cohorts and cohorts with no observed high educated people were omitted along with corresponding cohort from opposite census year.

**Table 2: Effects of the Rwandan Genocide on Years of Schooling**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
<i>Subnational Estimates</i>						
MeasureA	0.0174*** (0.00163) 1,010					
C2002 X MeasureA		-0.001 (0.003) 2,020				
<i>Aggregate Estimates</i>						
C2002			-0.371*** (0.029) 2,020	0.490*** (0.040) 6,230		
Rwanda					-1.419*** (0.041) 4,124	
C2002 X Rwanda						-0.829*** (0.063) 8,250
<i>Type of variation</i>						
Within Rwanda Post-Genocide	YES	YES				
Within Rwanda Diff-n-diff		YES				
Within Rwanda Temporal		YES	YES			YES
Within Uganda Temporal				YES		YES
Across Country Post-Genocide					YES	YES
Across Country Diff-n-diff						YES

Note: Robust standard errors in parentheses; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Dependent variable is the average years of schooling within the cohort. Individuals with undefined years of education are not included and years of education are censored at 12 years. Measure A represents number of days, per province, that the genocidal killings occurred. Each observation is a cohort defined by birth year, province(district) of birth for Rwanda(Uganda), gender and census year, and is weighted by the cohort size. For Rwanda, we treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. Ugandan observations are from districts included in both 1991 and 2002 censuses. All regressions include controls for gender and year of birth fixed effects. In addition (3), (4) include controls for province(district) of birth fixed effects and interaction between province and year of birth fixed effects. The Rwandan (Ugandan) samples includes only those native Rwandans (Ugandans) born between 1930 and 1978.

Source: IPUMS International

<b>Table 3: Effects of the Rwandan Genocide on Ratio of Educated People</b>						
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
<i>Subnational Estimates</i>						
MeasureA	0.009*** (0.001) 1,010					
C2002 X MeasureA		-0.002 (0.002) 2,020				
<i>Aggregate Estimates</i>						
C2002			-0.194*** (0.023) 2,020	0.278*** (0.024) 6,230		
Rwanda					-0.791*** (0.025) 4,124	
C2002 X Rwanda						-0.448*** (0.042) 8,250
<i>Type of variation</i>						
Within Rwanda Post-Genocide	YES	YES				
Within Rwanda Diff-n-diff		YES				
Within Rwanda Temporal		YES	YES			YES
Within Uganda Temporal				YES		YES
Across Country Post-Genocide					YES	YES
Across Country Diff-n-diff						YES
Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1						
Dependent variable is log ratio of educated cohort size to less educated cohort size. Measure A represents number of days, per province, that the genocidal killings occurred. Each observation is a cohort defined by birth year, province(district) of birth for Rwanda(Uganda), gender and census year, and is weighted by the cohort size. For Rwanda, we treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. Ugandan observations are from districts included in both 1991 and 2002 censuses. All regressions include controls for gender and year of birth fixed effects. In addition (3), (4) include controls for province(district) of birth fixed effects and interaction between province and year of birth fixed effects. The Rwandan (Ugandan) samples includes only those native Rwandans (Ugandans) born between 1930 and 1978.						
<i>Source:</i> IPUMS International						

**Table 4: Effects of the Rwandan Genocide on Educated Cohort Size**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
<i>Subnational Estimates</i>						
MeasureA	-0.003** (0.002)					
	1,010					
C2002 X MeasureA		-0.009*** (0.002)				
		2,020				
<i>Aggregate Estimates</i>						
C2002			-0.476*** (0.022)	-0.241*** (0.023)		
			2,020	6,230		
Rwanda					-0.268*** (0.024)	
					4,124	
C2002 X Rwanda						-0.198*** (0.038)
						8,250
<i>Type of variation</i>						
Within Rwanda Post-Genocide	YES	YES				
Within Rwanda Diff-n-diff		YES				
Within Rwanda Temporal		YES	YES			YES
Within Uganda Temporal				YES		YES
Across Country Post-Genocide					YES	YES
Across Country Diff-n-diff						YES

Note: Robust standard errors in parentheses; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Dependent variable is log of educated cohort size. Measure A represents number of days, per province, that the genocidal killings occurred. Each observation is a cohort defined by birth year, province(district) of birth for Rwanda(Uganda), gender and census year, and is weighted by the cohort size. For Rwanda, we treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. Ugandan observations are from districts included in both 1991 and 2002 censuses. All regressions include controls for gender and year of birth fixed effects. In addition (3), (4) include controls for province(district) of birth fixed effects and interaction between province and year of birth fixed effects. The Rwandan (Ugandan) samples includes only those native Rwandans (Ugandans) born between 1930 and 1978.

Source: IPUMS International



<b>Table 5: More People Missing in High Conflict Areas</b>				
VARIABLES	(1)	(2)	(3)	(4)
C2002 X MeasureA	-0.008*** (0.001)			
C2002 X MeasureB		-0.147*** (0.031)		
C2002 X MeasureC			-0.012*** (0.002)	
C2002 X MeasureD				-0.011*** (0.003)
Observations	2,020	2,020	2,020	2,020

Note: Robust standard errors in parentheses; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1  
Dependent variable: Log cohort size. Sample includes only those native Rwandans born between 1930 and 1978. Each observation is a cohort defined by birth year, province of birth, gender and census year, and is weighted by the cohort size. Regressions include controls for gender and year of birth fixed effects. We treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. For (3) individuals with undefined years of education are not included and years of education are censored at 12 years.  
Source: IPUMS International

**Table 6: Subnational Effects of Genocide Are Robust to Alternate Measures**

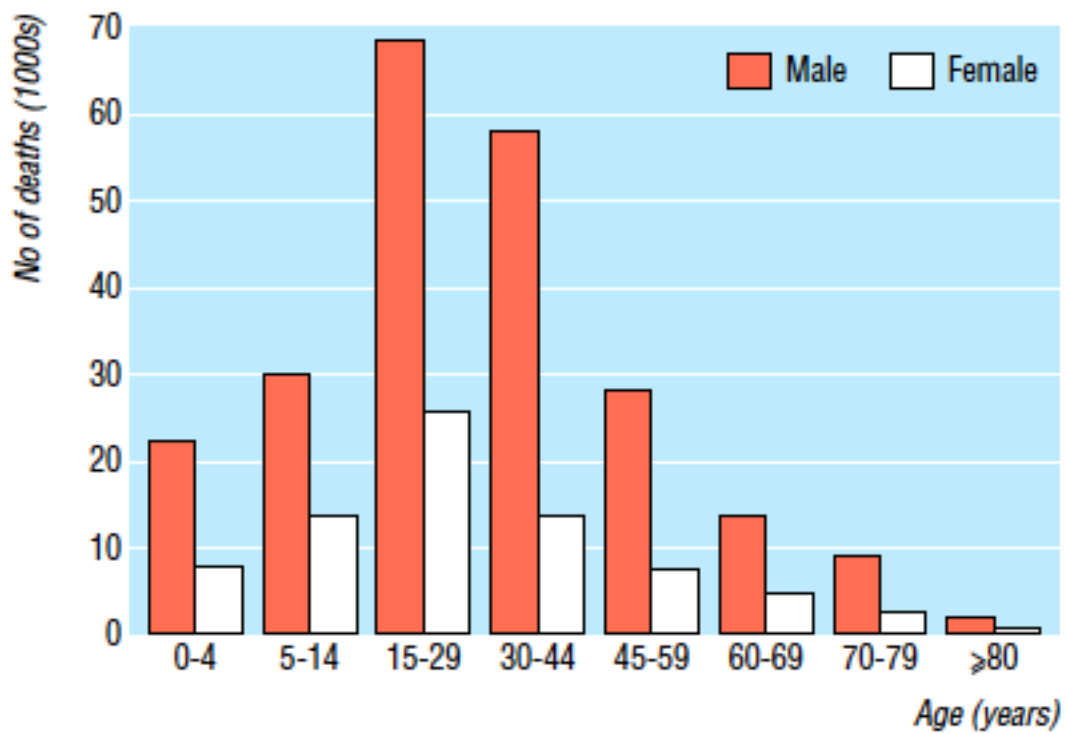
VARIABLES	(1) Highed	(2) Highed Ratio	(3) Years of Schooling
<i>Within Rwanda Post-Genocide Variation</i>			
MeasureB	-0.120*** (0.033)	0.053** (0.026)	0.0585 (0.0363)
MeasureC	-0.010*** (0.002)	0.014*** (0.002)	0.0241*** (0.00228)
MeasureD	-0.008*** (0.003)	0.019*** (0.002)	0.0324*** (0.00298)
Observations	1,010	1,010	1,010
<i>Within Rwanda Diff-n-diff</i>			
C2002 X MeasureB	-0.195*** (0.048)	-0.071 (0.048)	-0.084 (0.062)
C2002 X MeasureC	-0.014*** (0.003)	-0.003 (0.003)	-0.001 (0.004)
C2002 X MeasureD	-0.011*** (0.004)	0.000 (0.004)	0.003 (0.005)
Observations	2,020	2,020	2,020

Note: Robust standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Dependent variables: (1) log cohort educated size (completed primary school), (2) the log ratio of high to low educated cohorts, (3) the average years of school within the cohort. Sample includes only those native Rwandans born between 1930 and 1978. Each observation is a cohort defined by birth year, province of birth, gender and census year, and is weighted by the cohort size. Regressions include controls for gender and year of birth fixed effects. We treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. For (3) individuals with undefined years of education are not included and years of education are censored at 12 years. See Appendix for details of genocide measures.

Source: IPUMS International

Figure 1: Estimated global age and sex distribution of war casualties in year 2000.



Source: Murray et al. (2002)

Figure 2: Map of Rwanda



Figure 3: Population by cohort and census in Rwanda

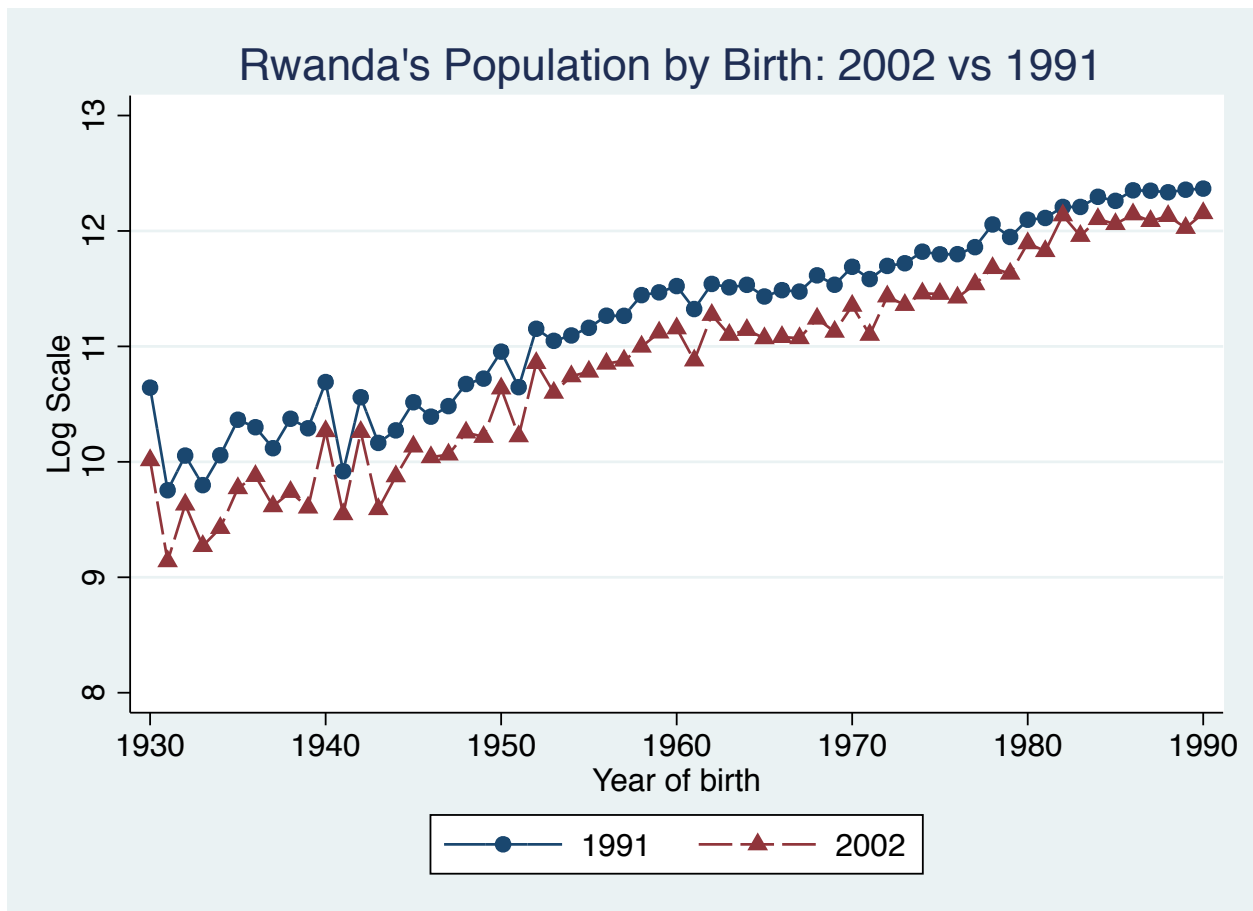


Figure 4: Differences in population by cohort in Rwanda

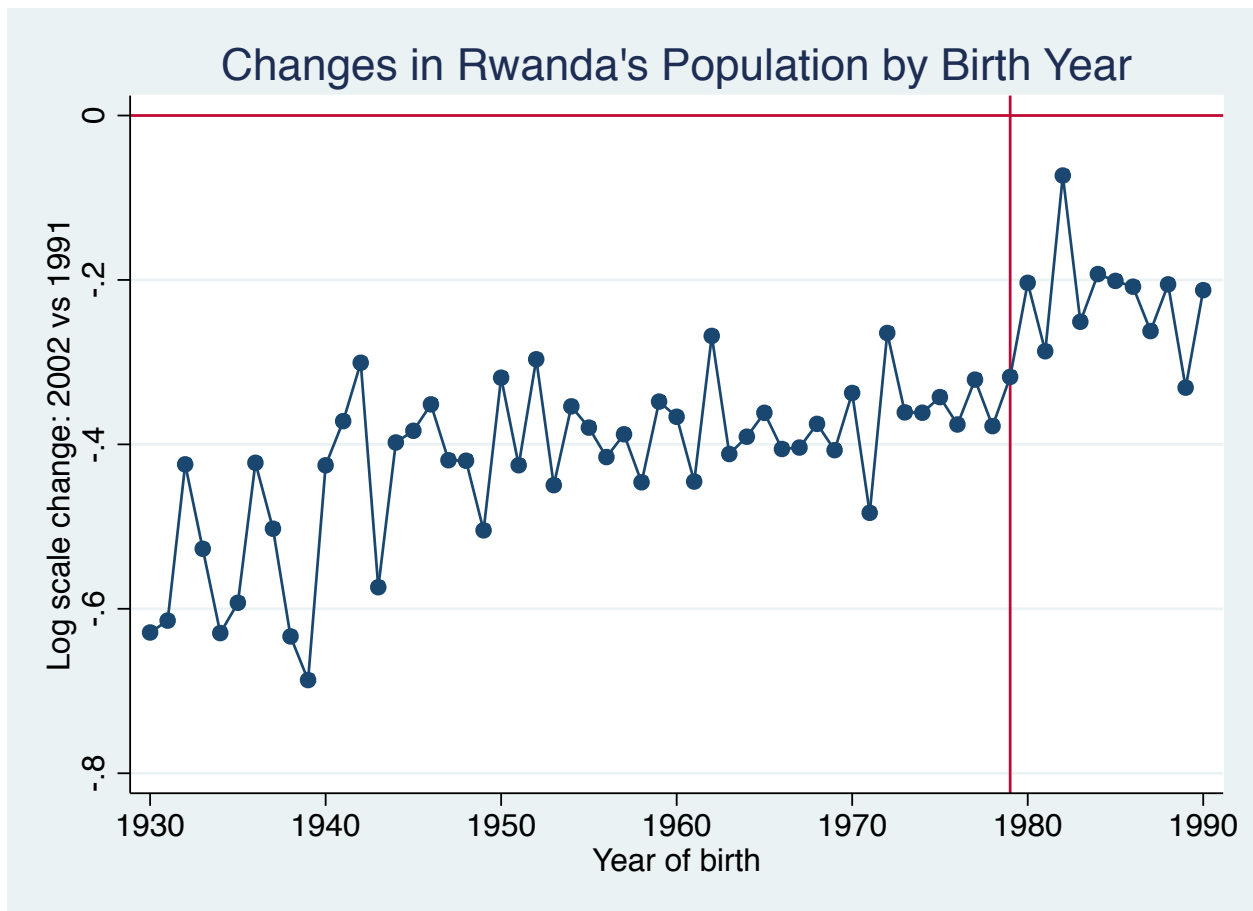


Figure 5: Mortality by Year of Birth Interval: Before and After Genocide in Rwanda vs Uganda

