War and the Destruction of Human Capital

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ABSTRACT

Most studies on the human capital costs of war tend to focus on children of school going age at the time of conflict. This paper estimates the effect of war on the destruction of stock of human capital by estimating "missing educated" cohorts. Results show that highly educated individuals are "missing" at a rate that is at least 24.1% higher than the less educated. Moreover, Rwanda's average years of schooling is lower by 0.645 years. When comparisons with Uganda are made, these estimates remain robust. Interestingly, when the cross-sectional 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, Education, Human capital, 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[1] 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 and time series methodology (second differences) on three census data sets at the sub-national as well as national level. This involved digitizing novel historical data from the first Rwandan census carried out in 1978.

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 Duflo (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

[1]For a review of select studies, please refer to Table S.

these studies.

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 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.[2] 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

[2]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

targeted at the more educated Tutsis as well as moderate Hutus, it is not clear how high the returns from 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 and second differences methodology on three census data sets at the 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.

In this paper, we introduce differences-in-differences and second differences methodologies, 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 of cohorts in the post-genocide 2002 Rwandan census against their size in the pre-genocide 1991 and 1978 censuses. Two types of analysis are performed- aggregate and subnational. For aggregate analysis, second differences are estimated using the 1978 and 1991 censuses as controls (differences between 2002 and 1991 are compared to differences between 1991 and 1978). In addition, a differences-in-differences method is carried whereby Rwanda between 1991 and 2002 is compared to Uganda (a neighboring country which despite experiencing civil war, like Rwanda, did not experience genocide in the same period.) For subnational analysis, within Rwanda variation in intensity of the genocide is utilized to carry out differences-in-differences involving high versus low conflict areas between 1991 and 2002, as well as a triple differences approach which controls for pre-genocide trends in high versus low conflict areas using the 1978 census.

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 (13) years in 1991 (1978) respectively. 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 for the analysis utilizing 1991 and 2002 censuses.[3] 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.

The genocide reduced the stock of human capital in Rwanda severely. The aggregate analysis conducting second differences over time show that highly educated individuals (i.e., those with primary education or more) are missing at a rate that is 24.1% higher than the less educated. Moreover, Rwanda's average years of schooling is lower by 0.645 years. When comparisons of Rwanda with Uganda are performed between 1991 and 2002 in the context of a differences-in-differences method, one gets even higher estimates, giving one confidence in robustness of the second differences estimates. 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 result holds if one uses data from the 1991 and 2002 censuses with data on spatial intensity of genocide, and if one controls for trends between 1991 and 2002, by using 1978 and 1991 data as additional controls (triple differences). Our results support the hypothesis that 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 7). 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

[3]The 1978 census we have access to does not help us determine the citizenship status

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 Intègrale 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 who 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. 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. In addition, relevant data from the 1978 census was digitized from the Demography Library and Penn Libraries (University of Pennsylvania).[4] [5]

3.1 Missing Rwandans

Figure 3 shows raw data for log population before (1991 and 1978) and after the genocide (2002) in Rwanda. There are many more young than old, as one would expect in a developing country 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

[4]We would like to thank Nykia M. Perez Kibler from the Penn Demography library for her help in getting access to the 1978 census data

[5]There were a few limitations of the data available to us. The data tables form 1978 were in 5 year intervals in contrast to dat from 1991 and 2002 from, IPUMS which allows one to explore individual birth years. Second, we could not disentangle the native from the immigrant population.

2002 population is strictly less than 1991 and the 1978 population. For all age cohorts. Furthermore, the differences between 2002 and 1991 is greater than that between 1991 and 1978. Figure 4 shows second differences- log differences between 2002 and 1991 versus 1991 and 1978 across birth year cohorts more clearly. There are fewer people during the period corresponding to the genocide across all birth cohorts.

To get an alternate perspective of how big the changes in the Rwandan population are during the 1991 and 2002 period, in Figure 4 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 baseline sample to those 13 or older so as to limit our sample to those old enough to have completed primary schooling.

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. For analysis using 1991 and 2002 census alone, 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 competed their primary schooling. This makes it less likely that someone identified as low educated in 1991 (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. Similarly, for analysis with 1978 data, we limit our analyses to those born before 1965 (13 or more years old in 1978) but after 1929. Years of schooling is censored at 12, as the people with more than 12 years of schooling in 1978 were more likely to be outliers.

A cohort for Rwanda is defined by birth year, gender, 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 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, per province, during genocide when killings occurred. Measure A is a continuos variable. However, we define a discrete version of A for 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 (7) 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

Table 1 shows summary statistics in two panels. Panel A is for within Rwanda analysis using 1978, 1991 and 2002 census, whereas panel B is for Rwanda versus Uganda comparison using 1991 and 2002 data. The analysis in Panel A has 140 cohorts compared to 1,010 in Panel B because it follows those born between 1930 and 1964, in intervals of 5 years, where as in Panel B we are able to follow a larger cohort of those born between 1930-1978 and that too in yearly intervals.The 1978 census data we have access to does not have birth data in yearly intervals. To explore how much of a concern this could be, we re-estimated some of our analysis involving 1991 and 2002 census data with 5 year birth intervals instead of yearly

intervals. The results are robust.

Panel A shows that there are fewer people in Rwanda over time, although the number of educated people went up between 1978 and 1991, they decreased between 1991 and 2002, showing reversal of trends. The ratio of educated to less educated also went up over time, however, the rate at which it went up fell between 1991 and 2002 compared to the trend between 1991 and 1978. Years of education also mirrors the same trend in that instead of rising between 1991 and 2002, as it did between 1978 and 1991, it actually falls. Summary statistics for Measures A-D are similar across the censuses (and are the same for Panel B) as these measures are time invariant measures of the genocide (please see data appendix for details on what these measures represent).

Panel B compares Rwanda with Uganda between 1991 and 2002.[6] 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. 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. In contrast to Rwanda, where in 1991 for every 100 individuals who did not complete primary schooling there were 41 who did, in Uganda the corresponding number was 79. 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 36. 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 years of schooling.

[6]In Panel A, data from 1991 and 2002 were adjusted by a factor of 10, because the 1978 census is the actual population data but the 1991 and 2002 data are 10% samples of the population. However, such an adjustment was not undertaken in Panel B as there was no need to harmonize the data with the 1978 census

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 happened 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 α_i and α_j are fixed effects at the cohort and space-level, respectively. Thus, β_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 α_j and that there are not any time-varying unobserved characteristics which could potentially bias estimates of β_1, the key parameter of interest. Note also the impact evaluated here by β_1 is comparing areas with high against low intensity of conflict. In most cases the levels of violence in the low-intensity areas may not be zero, as in the case of Rwanda, thus the effect measured by β_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 leads 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 β_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

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

Equation 2 incorporates two new sets of parameters- the survey-year fixed effect and γ_{ijt}, the parameter that 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 β_2 is the parameter of interest. Unlike β_1, β_2 might exhibit less bias because the effect is capturing differences with respect to 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

$$y_{ic} = \beta_3 G_{ic} + \alpha_i + \alpha_c + \gamma_{ic} X_{ic} + \epsilon_{ic} \tag{3}$$

In equation 3 we substituted the index j for c that indexes the countries. Equation 3 has an effect that accounts for nation-wide effects. Whether equation 3 adequately captures 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. Consider now the case where there are two surveys per country

$$y_{ict} = \beta_4 G_{ict} + \alpha_i + \alpha_c + \alpha_t + \gamma_{ict} X_{ict} + \epsilon_{ict} \tag{4}$$

In this case the effect can account for country and time unobserved factors. Finally, if one had two rounds of pre-conflict data, one can control for not only pre-conflict levels but also trends. The data from the 1978 Rwandan census will allow us to carry out such an analysis. A key advantage of this paper is our ability to explore aggregate and subnational variation using not only pre-conflict levels as controls, but also controlling for pre-conflict

trends in Rwanda, in addition to controlling for trends from a neighboring country (Uganda). Our data allows us to compare the sensitivity of the estimates depending on the assumption about the aggregate nature of the genocide.

5 Results

5.1 Aggregate Estimates

Table 2 shows results for three outcomes- number of educated people, ratio of educated to less educated individuals, and years of schooling. For our analysis, each observation is a cohort defined by birth year, province/district of birth, gender and census year, and is weighted by the cohort size. Our analysis controls for gender and year of birth fixed effects. There are two types of analysis- aggregate and subnational. Within aggregate analysis, we perform two types of analysis. Differences-in-differences, whereby Rwanda is compared to Uganda between 1991 and 2002. 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. The analysis reveals that there were 19.8% fewer educated people in Rwanda compared to Uganda in 2002 versus 1991. However, it may have been the case that the genocide did not discriminate between people by their education level so that less educated people also died at similar levels. To explore excess number of missing educated, we explore ratio of educated to less educated as well as years of schooling as outcomes. The analysis reveals that when cohort size of educated to less educated ones are compared between Rwanda and Uganda over time, there are 44.8% fewer educated people. A similar picture emerges when one estimates effects on years of schooling. After controlling for temporal and country differences, 0.83 years of schooling is lost in Rwanda compared to Uganda between 2002 and 1991. This estimate is significant for at least two reasons. First, the magnitude is large- more than three fourth of an year of schooling is lost. Second, because these estimates are more than double the

estimates for the effect of conflict on investment in child schooling by Akresh et. al. (2008). This suggests that destruction of human capital was large and significant.

However, it is not clear if Uganda would be a good counterfactual for Rwanda between 1991 and 2002. To address this concern, we utilize the 1978 census data from Rwanda so that Rwanda is compared to itself over time. Second differences are estimated i.e. the excess differences in missing cohorts between 1991 and 2002 is compared to those in 1978 and 1991, allowing us to control for pre-conflict trends. The analysis reveals that there were 48.3% fewer educated people in Rwanda between 1991 and 2002 compared to 1991 and 1978. However, when these trends are compared to those for less educated cohorts, one finds that are 24.1% fewer educated individuals in Rwanda over time. It is important to note that the second differences involves a different cohort (those born between 1964 and 1930) instead of the analysis involving Uganda (where we have cohorts between 1930 and 1978). Despite such differences it is remarkable that both exercises which utilize trends in Uganda and Rwanda itself (pre-genocide), reveal a similar story- that genocide lead to disproportionally more educated people missing than less educated ones. This is further confirmed when one explores effects on years of schooling- 0.645 years of schooling are lost in Rwanda between 2002 and 1991 compared to its own past (1991 and 1978).

5.2 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. To control for time-invariant province level unobservables which may be confounding the estimates from equation 1, we take advantage of our 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. Instead we are able to control for not only levels but also trends pre-genocide, because of two census before the genocide. When we estimate the equivalent of equation 2 for Rwanda, we find that there although there are fewer educated people in

high versus low conflict areas within Rwanda between 2002 and 1991, there are also similar estimates for less educated people, so that the ratio of educated to less educated is not significantly different from zero. A similar story emerges for years of schooling estimates. There is no evidence of any significant effects of the genocide intensity within Rwanda on years of schooling.

However, since we have two rounds of pre conflict data, one can carry out triple differences between high and low conflict areas in Rwanda in 2002 versus 1991 and compare them to trends between 1978 and 1991. The triple differences reveals a broadly similar story- there is little evidence of any significant within Rwanda variation in missing educated people. This subnational estimates are intriguing, 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.

5.3 Alternate Measures of Genocide Intensity Within Rwanda

Table 3 explores the robustness of subnational estimates in Table 2 with the alternate measures of genocide intensity. Measure B is a dummy for the four provinces with most number of reported killings, Measure C measures number of mass graves and memorial sites per province, whereas MeasureD is an intent-to-treat measure of proportions of Tutsis per province in 1991 (the genocide is known to have targeted Tutsis). Like Measure A, Measures B-D all show that there are fewer educated people in high versus low conflict areas between 2002 and 1991. However, there is no signifiant difference between size of educated and less educated cohorts, and neither is there any significant effect on years of schooling. These result give us confidence that estimates from Table 2 are robust, and are unlikely to be due to measurement error per se.

5.4 Heterogeneity by Age

Figure 6 shows regression results for log cohort size with triple interaction of higher vs low education cohorts in Rwanda between 2002 versus 1991, and birth year cohorts between 1930 and 1978. The results show that there are fewer educated people for most cohorts, but with particularly large effects for younger adults. As Figure 1 documents, war related casualties are often found to be the highest for those between 15-44, so that our findings of stronger effects for younger adults is consistent with war related casualties driving our missing educated estimates at the aggregate level. However, consistent with our earlier findings about the genocide being aggregate in nature, Figure 7 shows that none of these findings hold when within Rwanda variation in conflict is explored so that there is no evidence that younger educated cohorts are more likely too be missing than older one in high versus less conflict areas between 1991 and 2002.

6 Discussion and Conclusion

Since the 1960s, one out of every 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. Neighboring Uganda is used to test for robustness of aggregate effects in Rwanda. In addition, novel data from the first Rwandan census (1978) helps to control for pre-genocide trends.

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 Intègrale 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.[7]

Many studies choose to only carry out subnational or aggregate analysis. Table S reviews the current literature and finds that most studies are interested in subnational analysis, with few studies carrying out cross-national analysis. We do both. And find that the two present a very different picture. When subnational comparisons are performed, we find no statistically significant differences among high versus 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, differences-in-differences over time in Rwanda versus Uganda finds 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. When average years of schooling is used as a measure, we get a very similar story. The differences-in-differences results show that the average years of schooling reduced by 0.829 years in Rwanda compared to Uganda over time,

[7]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.

with results from second differences suggesting an estimate of 0.645 years lost in schooling. 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. Estimates which explore heterogeneity by age suggests that younger cohorts are disproportionally more likely to be missing, supporting the hypothesis that war related casualties are driving the missing educated estimates at the aggregate level.

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 0.645 years of schooling, which is much larger than that which Akresh et al. (2008) find in terms of effects on children's schooling (their effects size was around 0.4 years of lost 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 lead 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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7 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. 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.

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.

Table 1: Summary Statistics

VARIABLES	Rwanda			Uganda	
	1978	1991	2002	1991	2002
Panel A					
Cohort Size	14196	12745	8487		
	(8403)	(7455)	(5557)		
Number of High Educated	1996	2149	1382		
	(2556)	(2569)	(1577)		
Ratio HighEd/LowEd	0.131	0.165	0.171		
	(0.121)	(0.147)	(0.141)		
Years of Education	1.617	1.918	1.855		
	(0.957)	(1.02)	(1.01)		
Measure A	15.4	15.4	15.4		
	(9.023)	(9.023)	(9.023)		
Measure B	0.3	0.3	0.3		
	(0.460)	(0.460)	(0.460)		
Measure C	10.8	10.8	10.8		
	(6.866)	(6.866)	(6.866)		
Measure D	9.1	9.1	9.1		
	(5.551)	(5.551)	(5.551)		
Cohorts	140	140	140		
Panel B					
Cohort Size		3530	2298	2736	1882
		(2397)	(1605)	(2690)	(1732)
Number of High Educated		1070	628	1010	813
		(1175)	(694)	(1203)	(988)
Ratio HighEd/LowEd		0.409	0.363	0.785	.9266
		(0.400)	(0.351)	(1.115)	(1.093)
Years of Education		2.967	2.711	3.841	4.211
		(1.513)	(1.336)	(1.894)	(2.065)
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). Each cell shows mean with standard deviations in parenthesis. Cohort sizes/number educated are rounded to the nearest integer. In Panel A, cohorts are limited to those born between 1930 and 1964 and 5 year birth intervals are used, instead of birth years, to harmonise the data across the three waves for Rwanda. In Panel B, sample includes only natives who are born between 1930 and 1978. In Panel A, years of schooling are censored at 12, and to harmonise the province code for all years, Kigali Nigali and Kigali City were treated as one entity for all three censuses. In addition in Panels A and B, Umutara and Byumba are treated as one.

Table S: Summary of Recent Empirical Studies on Costs of Conflict on Human Capital Outcomes

Study	**Countries**	**Methods**	**Outcomes**	**Spatial/Cohort Variation**	**Time Variation**	**Adolescents/Adults**
Subnational Data						
Shemyakina (2006)	Tajikistan	Diff-n-diff	School enrollment	Yes	No	No
Akresh et al. (2012)	Eritrea	Diff-n-diff, FE	Height for Age	Yes	No	No
Miguel and Roland (2011)	Vietnam	IV	Literacy level, poverty	Yes	No	No
Akresh and De Walque (2008)	Rwanda	Diff-n-diff	Education attainment	Yes	Yes	No
Alderman et al. (2006)	Zimbabwe	IV, FE	Height for Age, Schooling	Yes	Yes	No
Leon (2012)	Peru	Diff-n-diff	Education attainment	Yes	No	Yes
Blattman and Annan (2007)	Uganda	Coerced abduction	Schooling, wages	Yes	No	Yes
Verwimp and Van Bavel (2014)	Burundi	Diff-n-diff	Primary school completion	Yes	Yes	No
Justino et al. (2014)	Timor Leste	Diff-n-diff	Primary school attendance, completion	Yes	Yes	No
Aggregate Data						
Montalvo and Reynal-Querol (2007)	Cross Country	Panel	Malaria	Yes	Yes	Yes
Aguero and Deolalikar (2012)	Rwanda, Zimbabwe	Diff-n-diff	Height	Yes	Yes	Yes

Table 2: Genocide Linked to Missing Educated at Aggregate but not Subnational Level

VARIABLES	(1) Highed	(2) Highed Ratio	(3) Years of Schooling
Aggregate Estimates			
Difference-in-Differnece	-0.198***	-0.448***	-0.829***
	(0.038)	(0.042)	(0.063)
	8,250	8,250	8,250
Second Difference	-0.483***	-0.241**	-0.645***
	(0.088)	(0.095)	(0.079)
	420	420	420
Subnational Estimates			
Difference-in-Differnece	-0.009***	-0.002	-0.001
	(0.002)	(0.002)	(0.003)
	2,020	2,020	2,020
Triple Difference	0.002	0.003	-0.004
	(0.013)	(0.012)	(0.009)
	420	420	420

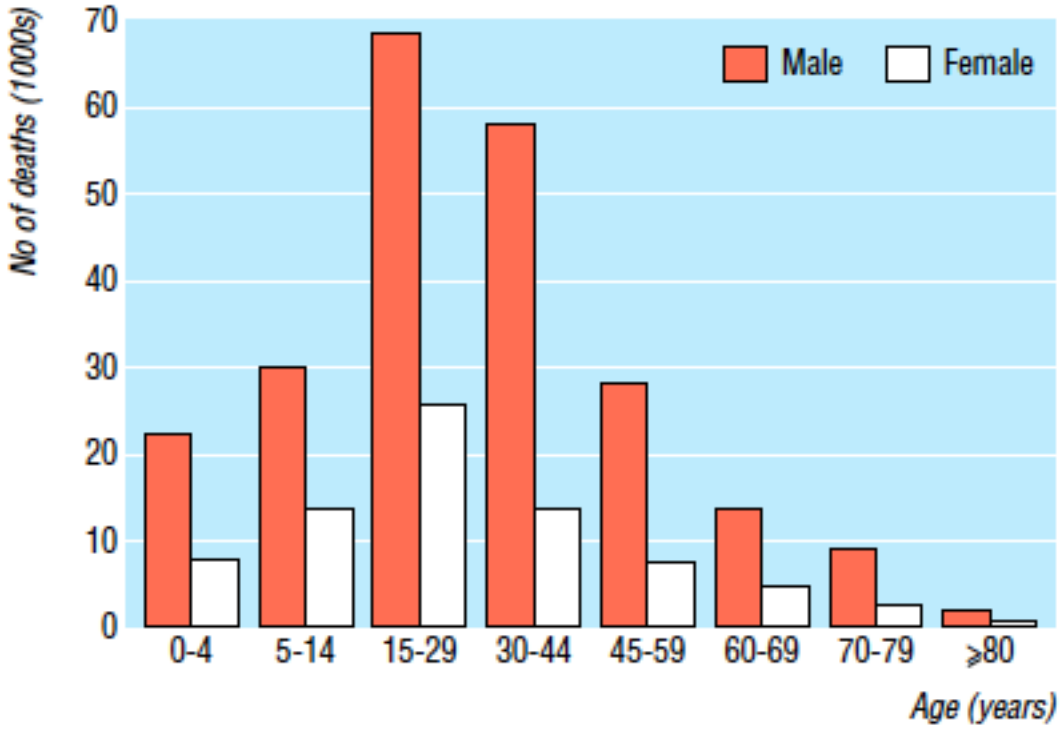
Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Dependent variables: (1) log cohort educated size (completed primary school), (2) the log ratio of high to low educated cohorts, (3) the average years of school within the cohort. Difference-in-difference exploits time variation between 1991 and 2002 and spatial variation- Measure A for subnational analysis and Rwanda vs Uganda for cross-national analysis. Measure A represents number of days, per province, that the genocidal killings occurred. Sample for difference-in-difference includes only native borns between 1930 and 1978 (observed in 1991 and 2002), whereas for the tipple/second difference, sample is limited to the 1930-1964 cohort (observed in 1978, 1991 and 2002). For the analysis with years of schooling, individuals with undefined years of schooling are not included and years of schooling are censored at 12 years for all samples. 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 the analysis using the 1978 census we use 5 year birth intervals instead of birth year. For Rwanda, we treat Byumba and Umutara provinces as one since between the three censuses the Rwanda government merged these two provinces. In addition, for the analysis with the 1978 census, Kigali City and Kigali Nigali are treated as one entity to make all the three waves comparable. Ugandan observations are from districts included in both 1991 and 2002 censuses. All regressions include controls for gender and year of birth fixed effects, province(district) of birth fixed effects and interaction between province and year of birth fixed effects, where applicable.
Source: IPUMS International

Table 3: Subnational Effects of Genocide Are Robust to Alternate Measures

VARIABLES	(1) Highed	(2) Highed Ratio	(3) Years of Schooling
Measure B			
Difference-in-Differnece	-0.195***	-0.071	-0.084
	(0.048)	(0.048)	(0.062)
	2,020	2,020	2,020
Triple Difference	-0.234	-0.088	-0.050
	(0.267)	(0.221)	(0.217)
	420	420	420
Measure C			
Difference-in-Differnece	-0.014***	-0.003	-0.001
	(0.003)	(0.003)	(0.004)
	2,020	2,020	2,020
Triple Difference	0.009	0.010	-0.000
	(0.015)	(0.013)	(0.011)
	420	420	420
Measure D			
Difference-in-Differnece	-0.011***	0.000	0.003
	(0.004)	(0.004)	(0.005)
	2,020	2,020	2,020
Triple Difference	0.007	0.010	-0.004
	(0.017)	(0.016)	(0.014)
	420	420	420

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 for difference-in-difference includes only those native Rwandans born between 1930 and 1978, where as for triple differnece, sample is limited to the 1930-1964 cohort. 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 three censuses the Rwanda government merged these two provinces. In addition, for the analysis with 1978 census, Kigali City and Kigali Nigali are treated as one entity to make all the three waves comparable. 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 starting 1978

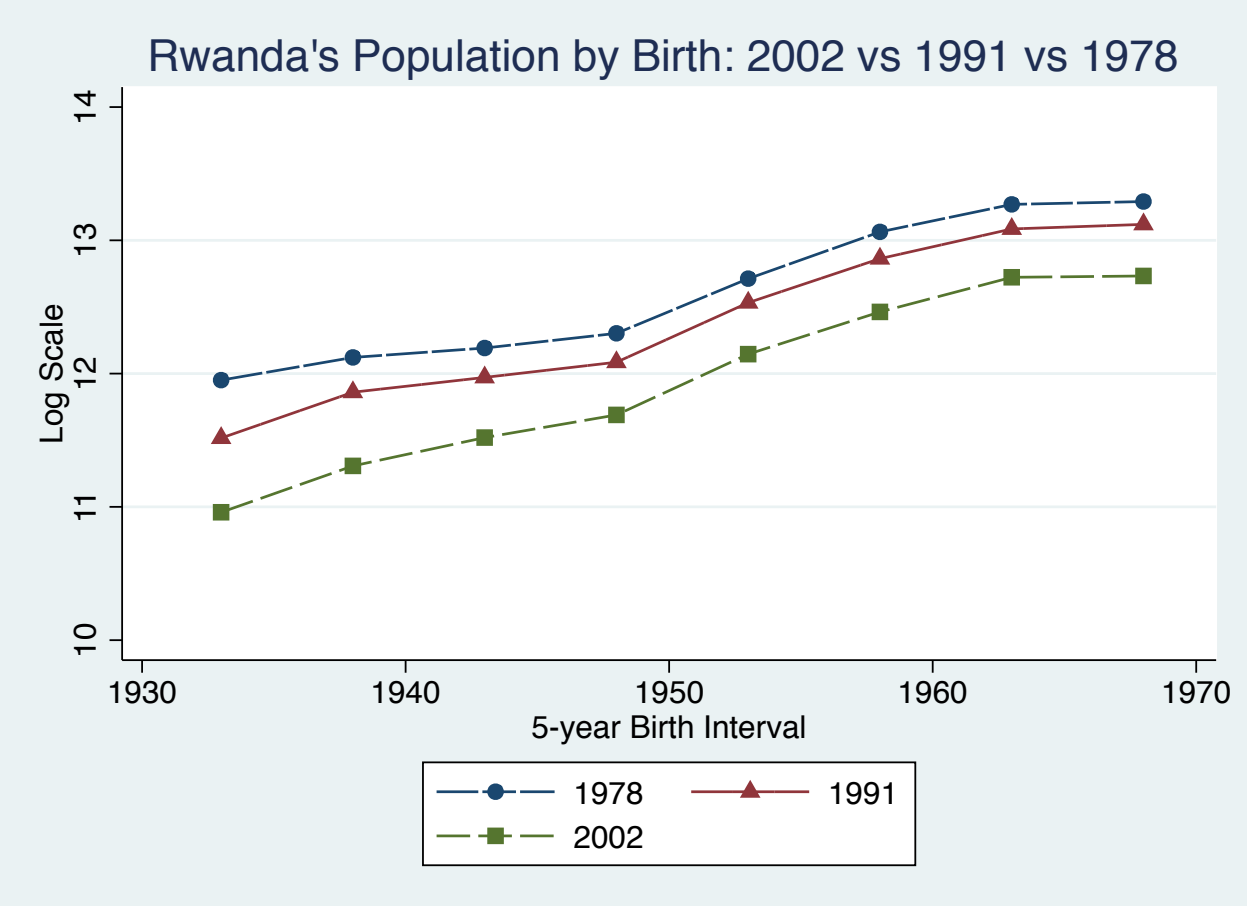


Figure 4: Second difference by cohort in Rwanda

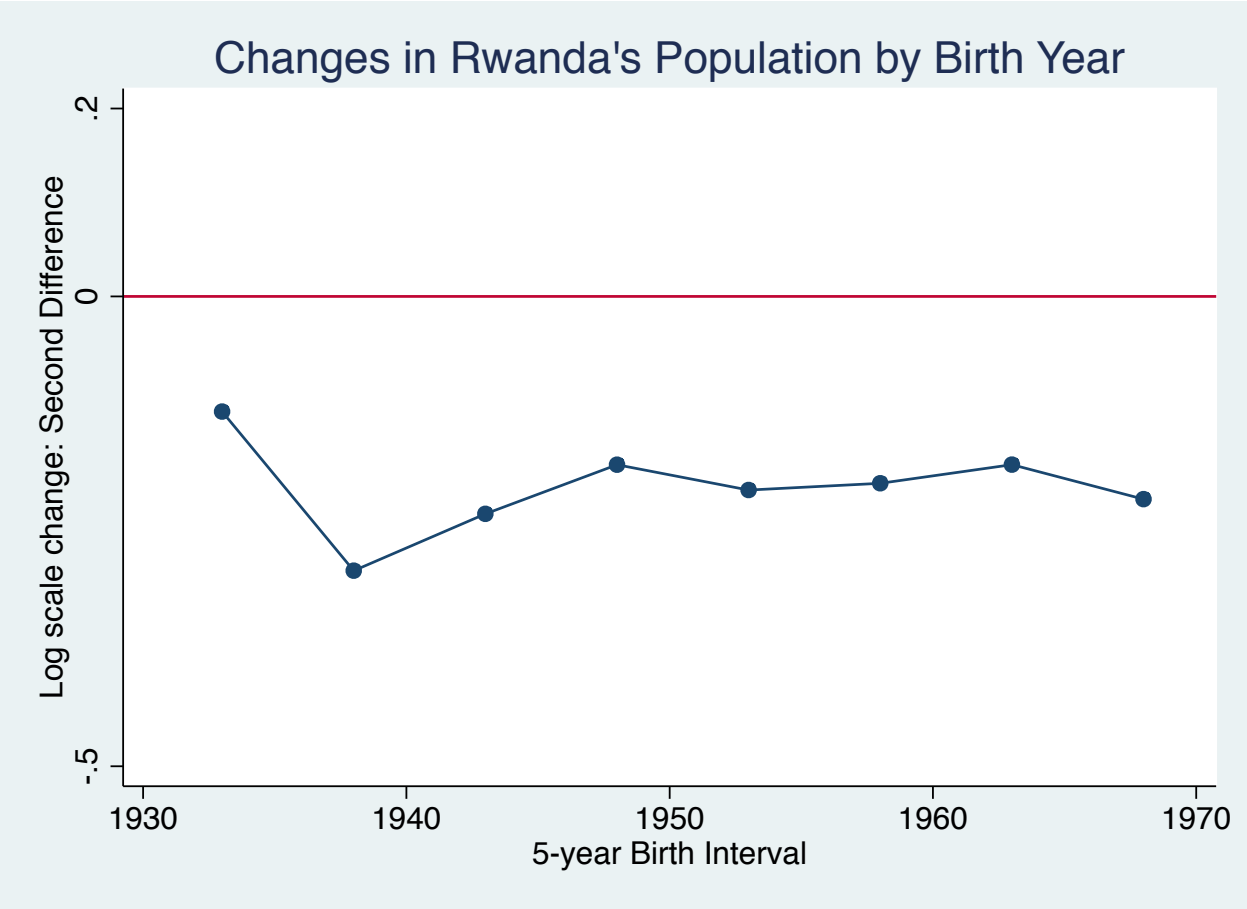


Figure 5: Mortality by Year of Birth Interval: Before and After Genocide in Rwanda vs Uganda

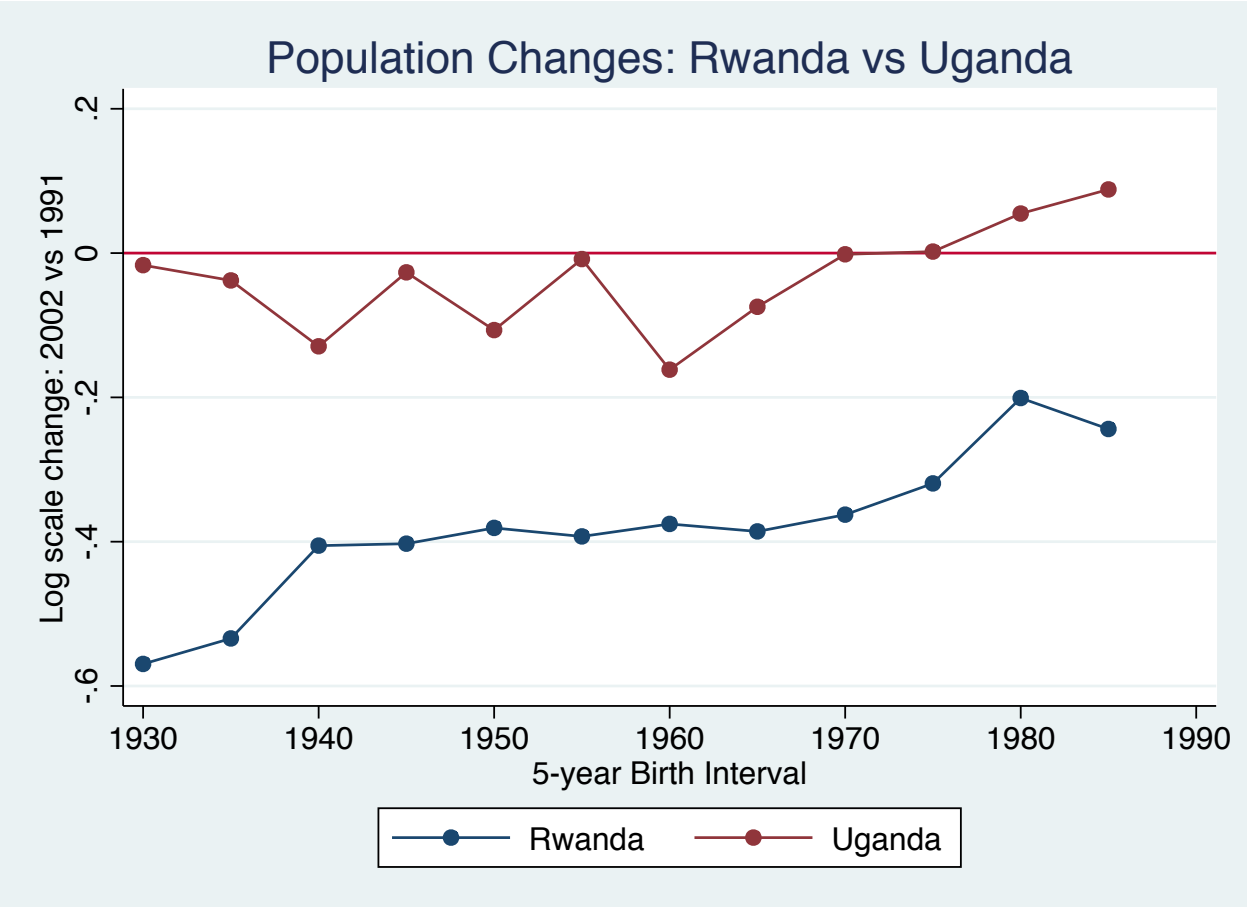


Figure 6: Triple Difference-in-Difference: Time Variation

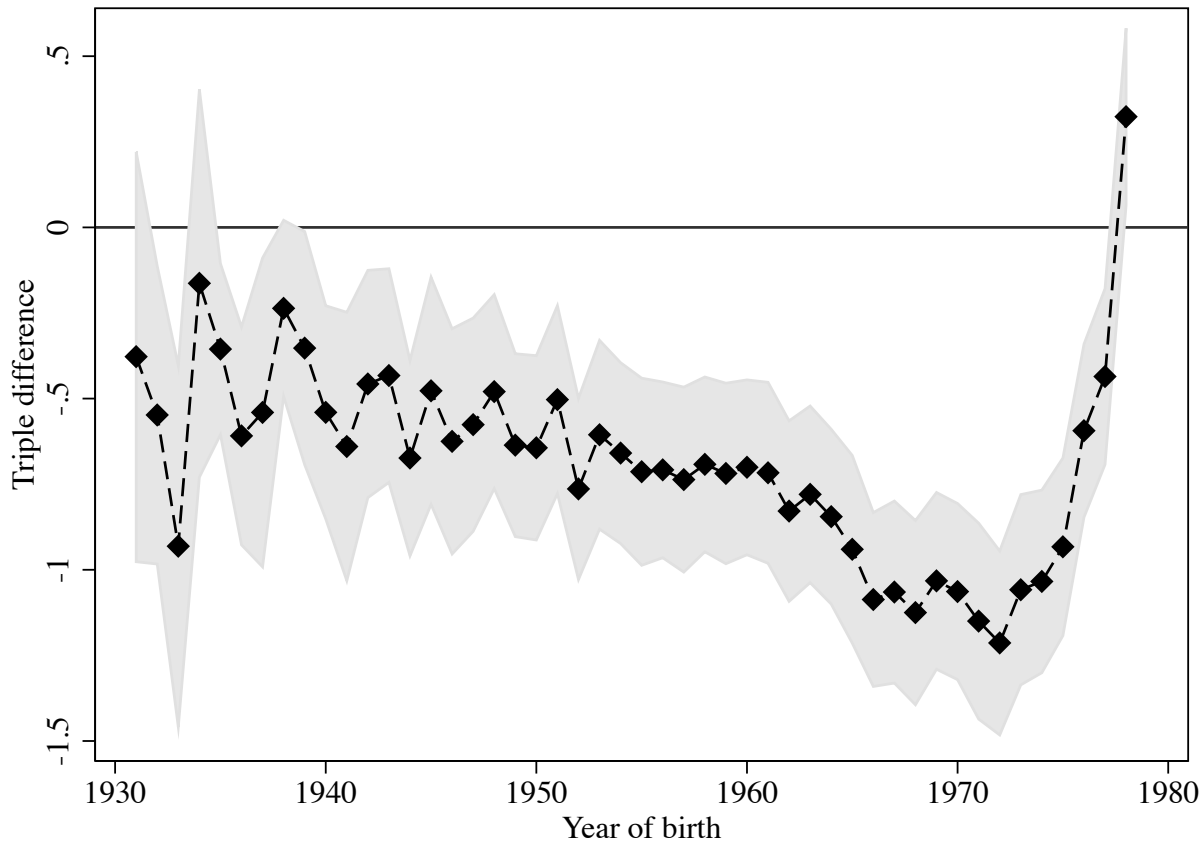


Figure 7: Quadruple Difference-in-Difference:Time and Spatial Variation