

War and the Stock of Human Capital

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Abstract

Previous studies have shown that wars restrict the acquisition of human capital. We expand this literature by exploring how the *stock* of human capital is affected by wars. Applying a “missing people” approach to the 1994 Rwanda genocide we find that the size of the educated cohort --those with completed primary school or more-- shrunk by 39 percent relative to their less educated counterparts and compared to its size in 1991. This excess missing rate for the educated is not due to outmigration and contrasts with the patterns observed in other countries of the region and also with the pre-genocide trends in Rwanda. When the spatial variation in the intensity of the genocide is exploited there is no evidence of statistically significant differences, suggesting an aggregate effect from the genocide. We discuss how this reduction affects labor markets post-conflict and the returns to education.

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1. Introduction

Civil conflict and war are often seen as development in reverse (Collier, 2003). Indeed, a growing literature documents the adverse effects of conflict on the human capital accumulation of children exposed to these negative shocks. For instance, Blattman and Annan (2010), Shemyakina (2011), Akresh and De Walque (2011), León (2012) and Akbulut-Yuksel (2014), among others, show how the schooling attainment of children is restricted when exposed to wars. Similarly, Alderman et al. (2006), Akresh et al. (2011), Grimard and Laszlo (2014), Agüero and Deolalikar (2012) and Minoui and Shemyakina (2014), for example, find similar negative effects for health outcomes of young children. However, there are still important gaps in our knowledge regarding the ways in which wars harm the *stock* of human capital (Blattman and Miguel, 2010). This is especially important because the likelihood and the speed of the post-conflict economic recovery depend on the productivity of the available labor force. In particular, it is the productivity of prime-age adults that survive the conflict that matters the most for the immediate recovery (Blattman, 2012). Despite its importance, research on how the stock of human capital is affected by these wars has not received enough attention in the existing literature. The goal of our paper is to address this knowledge gap.

Specifically, our work focuses on the losses in the stock of human capital that resulted from the 1994 Rwandan genocide, a conflict that killed approximately ten percent of the population in a span of about 100 days (Murray et al., 2002). There are several reasons to focus on the stock of human capital in addition to the educational attainment of school-going aged children at the time of the genocide. First, the stock of human capital has been shown to be highly correlated with long-term aggregate output

and economic growth across countries¹, (e.g., Mankiw et al., 1992; Barro and Lee, 1994, 2013; and Sala-I-Martin et al., 2004).

Second, in low-income countries, children dropout from school at an early age. In Rwanda, according to the population census, the school attendance rate before the genocide peaks at eleven years of age. By the time the children were 15, only 25.5 percent of them were enrolled in school. Therefore, studies focusing on school-going age children (i.e., fifteen and younger) leave a large proportion of the population out of the analysis.

Third, in a conflict, the largest share of deaths is concentrated among those aged between 15 and 44, rather than younger children. Murray et al. (2002) surveyed all major global conflicts in the year 2000 and found that approximately more than half of all deaths correspond to people between the ages of 15 and 44. Therefore, to start closing the knowledge gap in the literature, our study focuses on the destruction of human capital directly affecting the labor supply post-conflict rather than on the constraints for human capital accumulation of the next generation as previously explored.

We inferred the loss in the stock of human capital due to the Rwanda genocide using a “missing people” approach (e.g., Sen, 1990; Jayachandran, 2009). In particular, we took advantage of the existence of micro-level census data before and after the 1994 genocide; the 1991 and 2002 population censuses. Using this data, we computed the

¹ Cross-country regressions linking schooling and economic output have received several criticisms (e.g., Bils and Klenow, 2000). For instance, Benhabib and Spiegel (1994) show that these results do not hold when using first differences. However, a recent paper by Cohen and Soto (2007) suggests that most of the early weakness can be attributed to lower quality of data. When data of higher quality are used, the effects hold even in first differences.

‘missing rate’ in the size of the educated cohort –those who completed primary school or more– relative to the size of their less educated counterparts. Our sample includes all Rwandan-born individuals aged between 13 and 61 in the 1991 population census (born between 1978 and 1930, respectively). The older-age limit allows us to disentangle our results from the naturally high mortality of the older cohorts. The younger-age bound implies that our focus is on those who were most likely out of school by 1994. This allows us not only to concentrate on the impact on the stock of human capital but also lets us avoid any contamination due to selection into being born during the conflict, which is a common problem in this literature (Blattman and Miguel, 2010).

We find an excess missing rate of the *educated* of 39 percent, relative to their size in 1991. This effect contrasts with the patterns observed in five other countries in the region. Using the same methodology and applying it to micro-level census data from Kenya, Senegal, Tanzania, Uganda and Zambia we find an excess missing rate for the *less educated*. The genocide-led excess mortality of the educated also differs from the pre-genocide trends in Rwanda. After digitizing the published historical records of the 1978 population census, we find an excess missing rate for the *less educated* between 1978 and 1991.

Further analysis shows that the effect is larger for the educated cohort born between 1978 and 1950. That is, there is an excess missing rate of the educated aged 16 to 44 at the time of the genocide. While men are missing at a higher rate, there are no gender differences among the educated. This implies that the post-conflict demographics in Rwanda are characterized by a labor force where the most educated prime-age adults are missing.

The use of the missing people approach with pre- and post-genocide census data allows us to address several key concerns in this literature. First, as described by Jayachandran (2009), by counting survivors we are able to estimate “deaths” or “missing rates” for the entire population rather than for just a sample. This is an advantage compared to de Walque (2005, 2010). For example, de Walque and Verwimp (2010) used the maternal mortality module of the Demographic and Health Survey of Rwanda in 2000 that asks reproductive-age women (15-49) about the survival of their siblings as well as their gender and age. Almost certainly, such samples are not representative of the population at large² because the inference is based on the subset of households where at least one survivor was a woman of reproductive age. Our missing people approach represents an important advantage as we can count all the missing between two censuses³.

Second, as argued by Blattman and Miguel (2010), the growing microeconomic literature on conflict, including the papers described earlier, uses a difference-in-difference approach comparing high conflict regions with low conflict areas within the country and rely mainly on post-conflict data. The lack of pre-conflict surveys makes it difficult to validate the assumption of parallel underlying trends in economic

² For example, if all adult members of the family were killed leading to an increase in the number of child headed households, the DHS maternal mortality module will be unable to capture such cases.

³ In general, those who are interviewed after a conflict ends are unlikely to be a random sample of the pre-conflict population. For example, in Peru, León (2012) and Castillo and Petrie (2007) show that the victims of the conflict were disproportionately farmers, belonged to indigenous groups, and had low levels of education. 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, such as education.

development among these regions. We avoid this problem by using two pre-genocide datasets as described above.

Third, the use of spatial variation in the intensity of conflict could lead to a downward bias in the estimates of the previous literature if the relatively peaceful regions are also negatively affected by the internal conflict (Blattman and Miguel, 2010). Our paper is, to the best of our knowledge, the first one to be able to test for such a bias. When using four measures of spatial variation in the intensity of the genocide, we find that the effects are small and statistically insignificant. While the aggregate effects document an excess mortality of the educated close to 39 percent, the localized effects are less than one percent.

Our paper is organized in five sections, including this introduction. Section two briefly describes the Rwanda genocide and shows that our findings are not driven by outmigration. While the genocide created a mass influx of refugees to neighboring countries, the vast majority came back by the end of 1996. By 2002-the year of the post-genocide census-there were less than 76,000 refugees outside Rwanda. The data used in this paper is described in section three followed by the methodology (section four). Section five presents our results. We discuss how our findings affect the labor market and specifically the returns to education in post-conflict Rwanda. Section six summarizes our findings and concludes.

2. Background on the Rwanda genocide

This section briefly describes the 1994 Rwanda genocide. A much larger discussion can be found in Prunier (1995), Newbury (1988), de Lame (1996), Reyntjens (1994) and

Chrétien (2000), among others. To provide some basic context, Rwanda is a landlocked country in East Africa with a small but dense population. It neighbors Uganda (North), Tanzania (East), Burundi (South) and the Democratic Republic of Congo (West).

The genocide is defined as the mass slaughter of Tutsis, but also moderate Hutus, that took place for about 100 days in 1994: From April 7, the day after the plane carrying the President of the Hutu-led government, Juvénal Habyarimana, was shot down, until mid-July when the Tutsi-led Rwanda Patriotic Front (RPF) took control of the capital, Kigali. Estimates of the death toll have ranged widely from 500,000 to about 1 million people, representing between five to ten percent of the total population at the time (Prunier, 1995).

The genocide did not happen in a vacuum but was the result of a long-standing rivalry between a Hutu majority 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 physical appearance and race (Mamdani, 2002). Things changed in the 1950's and 1960's; The Hutu rallied for, and won, Rwanda's independence, but not without a violent campaign against Tutsis. This 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 until 1990. That year, RPF, the rebel Tutsi group, invaded northern Rwanda from Uganda instigating a civil war against the Rwandan armed forces. More than three years of ethnic violence led to killings and the 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 a 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 one of the most horrific mass killings in human history.

As a response to the violence, many fled to neighboring countries during and right after the genocide. As documented by the United Nations High Commission of Refugees (UNHCR, 1997) this massive migration was a coordinated response facilitating Hutus to escape the advances of the Tutsi-led RPF (from Uganda). For example, between April 28th and 29th, over half a million Hutus fled to Tanzania and by May 1994 close to 200,000 fled south to Burundi. After the RPF took control of the country, many more Hutus fled to the Democratic Republic of the Congo. Table 1 documents the stock of Rwandan refugees around the world by year (column 1) as well as the flow of Rwandan refugees returning to Rwanda by year. As shown in Table 1, by the end of 1994 an estimated 2.26 million Rwandan refugees were estimated to be located in neighboring countries creating what is known as the Great Lakes Refugees Crisis that was described by U.S. President Bill Clinton as the “world’s worst humanitarian crisis in a generation.” This prompted a large reaction of the international community to solve this problem.

Between July 1994 and December 1996, most of these refugees returned to Rwanda due to conflict in the neighboring countries and pressure from the international community.⁴ This returned flow is shown in column (2) of Table 1. As of 2002, the end

⁴ Most of the refugees who had fled Rwanda were Hutus, and included the members of the ex-FAR/Interahamwe (considered perpetrators of the Rwandan genocide). In some cases, such as in the DRC

year of our analysis, only slightly more than 75,000 people remained as refugees outside Rwanda. As explained below, comparing cohort size between 1991 and 2002 will significantly reduce the likelihood that the estimated missing rates come from outmigration and rather suggest that we are indeed capturing true deaths⁵.

3. Data sources

The main sources of data for our empirical analysis are the 1991 and 2002 Rwandan population censuses obtained from Integrated Public Use Microdata Series (IPUMS)-International. IPUMS stores a 10% random draw from each census. Relevant to our study, each census contains region and year of birth of all individuals as well as gender and education levels. To avoid migration issues we focus on the sample born in Rwanda, regardless of their nationality⁶. We classified all individuals who have at least six years of education as “high” educated, which is, those with completed primary schooling or

(then Zaire), the ex-FAR forces began to mobilize militarily and started taking control of the refugee camps. In October 1996, the Rwandan army launched a counter offensive in Zaire and cleared the refugee camps, forcing refugees to return to Rwanda (Lischer, 2005; Salehyan, 2008). As Salehyan (2008) states “The Rwandan army began to fire upon the refugees in order to force people back into Rwanda, and worked to cut off humanitarian aid and disperse the ex-FAR militants. [...] With food aid cut off, the refugees had no choice but to return home. By mid-November, the largest camps had been cleared and over 700,000 refugees returned home” (p.19).

⁵ This conclusion is further strengthened from data on net migration collected from World Bank’s World Development Indicators, which show between 1990 and 2000, there has been a net positive migration of about 300,000 people.

⁶ This permits us to avoid including individuals of Rwandan nationality but born in exile (mainly in Uganda).

more.⁷ Considering that the average years of schooling was 1.8 and 2.8 years in 1990 and 2005, respectively (UNDP's Rwanda country profile⁸), our threshold seems appropriate.

We limit our main sample to those born before 1979 so that our data is not affected by birth rate patterns during the genocide or by the high prevalence of infant and child mortality. This restriction also implies that individuals are old enough to have completed their primary schooling; and given the high dropout rates, makes it less likely that someone identified as low educated in 1991 (13 years old or more at the time) will continue their education in between censuses. However, as discussed below, we consider alternative year of birth restrictions as robustness checks. We also exclude those born before 1930 so that the naturally higher mortality rate for the older individuals does not confound our estimates.

For this sample we construct cohorts by the year of birth, province of birth, sex and education group (low and high). Note that we cannot use ethnicity as a variable for the construction of the cohorts because the collection of such information was declared illegal in Rwanda after the genocide and before the 2002 census.

Table 2 shows the average cohort sizes in the 1991 and 2002 censuses by characteristics: education (primary education or more), age (younger than 44) and the intensity of violence (see Measure B below). In Panel A, we show that the overall (log) difference in the average cohort size between censuses is 32% smaller in 2002. This decline is observed across all cohorts. However, the educated cohort exhibits a more pronounced reduction (67 percent) compared to their less educated counterparts (30

⁷ We also explored an alternate measure of human capital- years of schooling as a robustness exercise. See Appendix B1 for details.

⁸ <http://hdr.undp.org/sites/default/files/Country-Profiles/RWA.pdf>, access on July 21st, 2015.

percent). Thus, a simple difference-in-difference calculation yields an estimated reduction in the stock of human capital of about 37 percent. This is the first key finding of our paper and it will be validated with the methodology described below.

In Table 2, Panel A, we show that there is a slightly higher missing rate for older individuals relative to those younger than 44. Also, as expected, the missing rate is much higher in the most violent provinces: 30.6 percent for the less violent compared to 51.9 percent for the more violent. However, when considering the missing rates for these variables comparing the educated versus the less-educated, we find interesting results. Panel B of Table 2 shows that among the less educated, there is an excess missing rate for the older cohorts. For the high educated we find the opposite. A simple triple-difference based on average cohorts sizes indicates an excess mortality among the young and educated (36.9 percent). Furthermore, in Panel C we find little evidence of an excess missing rate for the educated when comparing provinces with low and high levels of violence. This is our second main finding. As suggested by Blattman and Miguel (2010), estimates of the effect of violence on, for example, education outcomes that use within-country variations are likely to be biased downwards. This bias occurs when the severity of the violence in some areas creates negative spillovers for the relatively more peaceful regions of the country. In Table 2 we find that the nation-wide, or aggregate estimate, of the genocide on the stock of human capital is large (around 37 percent). Yet, the within-country variation in violence suggests a small excess missing rate of the educated: 3.2 percent. This finding is confirmed below with our econometric model.

Also, to conduct our falsification tests, we digitized the published books of the 1978 Rwandan census (see Appendix B2 for details on the 1978 census). As explained

below, we also use micro level census data from IPUMS for the following Sub-Saharan African countries and years: Kenya (1989 and 1999), Senegal (1988 and 2002), Tanzania (1988 and 2002), Uganda (1991 and 2002), and Zambia (1990 and 2000).

The methodology described in the next section allows us to compare the impact of the genocide not only as an aggregate shock, but also as a localized effect. For the latter, we use information capturing the spatial variation in the intensity of the genocide. We consider three measures as used by Akresh and de Walque (2011) and obtained from www.genodynamics.com (a website that contains a database that collects all available information from local human rights organizations), the Rwandan government ministries, and from international organizations on the timing and geographic extent of all killings that took place during the Rwandan genocide.

Measure A captures the proportion of days, by province, when killings occurred during the genocide. *Measure B* is a binary variable equal to one for the four provinces with the most reported killings (Kigali Ngali, Butare, Kibuye and Kibungo) and zero otherwise. *Measure C* uses the number of mass grave sites and memorials, per province, with data taken from the Rwanda Genocide Project at Yale University (Rwandan Genocide Project, 2007). In addition to the three measures used by Akresh and de Walque (2011), we added a fourth metric, *Measure D*: the proportion of Tutsis per province found in the 1991 Rwandan Census. This is a measure of potential exposure to genocide and can be thought of as an ‘intent to treatment’ (ITT) estimate, since Tutsis were systematically targeted during the genocide. The next section describes our methodology to estimate the impact of the genocide on the stock of human capital.

4. Methodology

The purpose of the paper is to estimate the effects of the 1994 genocide on the stock of human capital. Formally, this is done using the following regression:

$$\begin{aligned} \text{Ln}(\text{CohortSize}_{ipsjt}) = & \beta(\text{Educated}_j * \text{PostConflict}_t) + \gamma \text{Educated}_j + \theta \text{PostConflict}_t \\ & + \alpha_i + \alpha_p + \alpha_s + e_{ipsjt} \end{aligned} \quad (1)$$

The outcome measure, $\text{Ln}(\text{CohortSize}_{ipsjt})$ is the natural logarithm of the number of people born in year i , in province p , of sex s , that achieved level of education j as observed in census year t . The binary variable Educated_j takes the value of one for cells composed by individuals with completed primary school or more and PostConflict_t is equal to one for the post genocide census of 2002 (and zero for 1991). Thus, β is the parameter of interest as it approximates the proportional change in the size of the educated cohort before and after the genocide relative to the temporal changes in the size of the less educated cohort. To obtain parameters that represent the average effect for Rwanda, each observation is weighted by the cell's size (the number of people enumerated in each cell). Equation (1) also includes fixed-effects for year of birth, and province of birth and sex. The treatment is at the individual level (i.e., a person missing due to the genocide) but our estimation uses aggregated data as the unit of analysis. Following Bertrand et al. (2004), we use standard errors that are robust to any form of heteroscedasticity, but not clustered. In Appendix A, Table A1 shows that our conclusions are unaltered when we use cluster-robust standard errors.

Our methodology follows the approach used in the “missing people” literature (e.g., Sen, 1990; Jayachandran, 2009) and it has key advantages compared to other methods. For instance, by counting survivors we are able to estimate “deaths” or “missing rates” for the entire population rather than for just a sample. The latter occurs when using household surveys such as the Demographic and Health Survey (e.g., de Walque 2005, 2010). Also, given the low quality of the mortality data in Rwanda (Mahapatra, et al. 2007), especially during the genocide, population counts provide a much better measure of deaths that is free of recall errors. Indeed, previous work on Rwanda by de Walque and Verwimp (2010) uses recall data from the maternal mortality module of the Demographic and Health Survey of Rwanda in 2000 which asks reproductive-aged women about the survival of her siblings as well as their gender and age. Our approach represents an improvement on previous studies.

Nonetheless, there could be several concerns regarding the use of population counts to identify measures of mortality. First, individuals born at different times may have different survival rates. This is especially true for estimates coming from one cross-section. For example, Jayachandran (2009) uses data from the June 2000 census to study mortality induced by Indonesia’s wildfires. As explained by the author, for children born in December of 1997 (around the time of the fires) her paper measures survival for the first 2.5 years and for those born in December of 1996 the survival is for the first 3.5 years. These survival rates could be different even in the absence of the fires. Adding birth year—birth month fixed effects, Jayachandran is able to control for any average difference, because the estimates will reflect within-cohort differences. In our case, we include birth year fixed-effects to avoid an analogous problem but expand the strength of

our identification strategy by incorporating the size of the cohort *before* the genocide as an additional control. Thus, relying on data before *and* after the genocide eliminates the concern arising from differential survival rates (even in the absence of the treatment).

A second concern comes from the possibility that the genocide could affect the cohort size by altering the birth rate. By focusing on individuals who were at least thirteen years old in the 1991 census we avoid such worries. A third important concern is the possibility that the genocide affected migration patterns. This would imply that relying on place of residence could bias our results when comparing population counts by province. We are able to avoid this problem by using place of birth, rather than place of residence in each census.

Finally, it is difficult to differentiate between “missing rates” coming from true deaths as opposed to migration outside Rwanda. As documented above (section two), there was an outmigration flow during and after the genocide. However, the vast majority of the refugees returned to Rwanda by the end of 1996. Therefore using the 2002 census we reduced the likelihood that the missing rates come from outmigration and rather suggest that we are indeed capturing true deaths. However, we take a conservative approach and refer to our measures as capturing *missing rates* rather than deaths.

5. Results

The methodology described above assumes that the effect of the genocide on the stock of human capital operates as an aggregate shock. Below, we present the results under that assumption and then explore heterogeneous impacts (section 5.2). In section 5.3, however,

we consider the possibility that the genocide had within-country effects by exploiting regional variation in the intensity of the violence.

5.1 The genocide as an aggregate shock

Table 3 shows the estimates of our key parameter as well as the fixed effects for higher level of education (*Educated_j*) and the 2002 census (*PostConflict_t*). Column (1) shows three patterns; First, relative to 1991, the average cohort size is smaller in the post conflict census (30.7 percent decline). This is partly mechanical because countries tend to experience a reduction in cohort size between any two censuses⁹. Second, on average, cohorts composed of educated people –those with at least completed primary schooling – are smaller than their less educated counterparts. This reflects the low levels of human capital in a country such as Rwanda.

Third, and most importantly, column (1) shows that the genocide is associated with a decline in the survival rate of the educated cohort. We find that the educated cohort is 39 percent *smaller* post-genocide compared also to the less educated group. The effect size is statistically significant at the one percent level. To put this number in context, Jayachandran (2009) finds that a five-month prenatal exposure to the Indonesian fires led to a one percent reduction in cohort size of children. Below we show that our estimates are not affected when considering several robustness checks.

⁹ This is less likely to be the case if, for example, several people were born in Rwanda but left the country before 1991 and return before 2002. If this situation, our estimates are biased downwards. In fact, there is evidence that several Rwandans that lived in Tanzania even before the genocide and returned afterwards (Prunier, 1995). However, many were foreign born and they are not included in our analysis as we focus on those *born* in Rwanda.

As explained in the data section, we restrict our analysis to individuals born in Rwanda, and who by the 1991 census were at least thirteen years old (i.e., born in 1978 or earlier). We imposed this limit so that our sample would have been old enough to complete primary schooling or would have dropped out of school before the genocide. Indeed, Akresh and de Walque (2011) show that children exposed to the genocide had a much lower probability of staying in school. Nonetheless, if some children with incomplete primary schooling in 1991 survive the genocide and received additional schooling insofar as they finish primary school, they would be classified as “high” educated in 2002. This would inflate the size of the educate cohort in the post-conflict census suggesting a *downward* bias in our previous estimates.

To elaborate more on the potential downward bias of our estimates, we restrict the sample to those born in 1976 or before. Estimating equation (1), we should expect a larger estimate for β because older cohorts would have a higher dropout rate and are less likely to continue their education after 1991, even in the absence of the genocide. This is precisely what we observe in column (2) of Table 3: the relative size of educated cohorts is reduced by 51 percent (column 2) compared to 39 percent in the original sample (column 1). Restricting the sample furthermore to older cohorts continues to show an effect that is never below our main estimate with the full sample. Thus, if anything, our original estimate could possibly be a lower bound.

We now consider a second robustness check; In the absence of conflict the more educated cohorts may have lower mortality rates (Lleras-Muney, 2005; Cutler et al. 2006; Krueger et al., 2015). If this is the case, the missing rate of the educated cohort could be bigger than what our estimates suggest. We explore two possible ways to construct this

counterfactual; First, we digitize tables from the published books of the 1978 population census in Rwanda. This permits us to test whether between two pre-genocide years (1978 and 1991) there was also a decline in the relative size of the educated cohort or not. The results of this exercise are shown in Table 4. To facilitate the comparison, in column (1) we reproduce our original analysis (as in Equation 1) using the 1991 and 2002 census for the full sample. In column (2), we replicate the analysis but now focusing on the change between 1978 and 1991. For these pre-genocide years we find a *positive* effect: the educated cohort *grew* relative to the less educated by 29.6 percent (and statistically significant at 1 percent). Therefore, the decline in the relative size of the educated cohort post-genocide goes in the *opposite* direction compared to the Rwanda trend pre-genocide. If the 1978-1991 trend is used as a counterfactual for the post-genocide change, the effect of the genocide would be larger.

A second way to build a possible counterfactual is obtained by using contemporaneous data from other countries in the region. We estimate equation (1) as before, but using census data from IPUMS International for all the countries in Sub-Saharan Africa that satisfy two criteria; First, they needed to have two censuses conducted circa the dates of the Rwanda censuses: early 1990s and early 2000. This yielded six counties¹⁰. Second, they needed to have all the variables that would allow us to construct the same type of cells in both years: year of birth or age, within-country place of birth (e.g., province, district, etc.), education level, gender, and census year.

¹⁰ In September of 2015 samples from Ethiopia (census years: 1997 and 2007) and Mozambique (1994 and 2007) were added to IPUMS International. The latest round of these samples seems too distant from the 2002 Rwanda census so we have not added these countries in our analysis.

Only one country did not have all these variables and was not included in the analysis (South Africa did not have place of birth within the country recorded).

In columns (4) to (8) of Table 4 we show the results of estimating equation (1) for each of these countries: Kenya, Senegal, Tanzania, Uganda and Zambia. For all these five countries, the size of their educated cohorts *increases* in between censuses, relative to the size of their educated counterparts. Only in Senegal, the parameter is not statistically different from zero but the point estimate is still positive, however small. It is important to note that some of these countries did experience violence between their two censuses. For example, in Uganda, civil war under the lead of the Lords Resistance Army (LRA) gained momentum between 1994 and 2003 when Sudan began supplying them with weapons and territory. Abductions drastically increased with 60,000 to 80,000 people kidnapped by the LRA and were held for at least a day (Annan et al., 2011; Pham et al., 2007; Van Acker, 2004). Nonetheless, we still find an *increase* in the size of the educated cohort of about 20%. This increase in the relative size of the educated population is exactly the *opposite* of what we observe in Rwanda¹¹ post-genocide.

To make a better comparison between these findings and the original results from Rwanda we included column (3). Our main specification uses year of birth rather than

¹¹ A possible concern is that the increase in the size of the educated in those other African countries could be unrelated to a higher longevity of the educated. For example, if people in Kenya, leave school at a later age than in Rwanda, using the latter's age cut-off would artificially increase the size of the cohort in Kenya's second census as the less educated acquire more schooling in between censuses. However, we find the same results --an increase in the relative size of the educated cohort-- when we restrict the sample to older individuals who were more likely to have completed with their education in the first census (results not shown but available upon request).

age to construct our cells. However, the former is not available for the other five African countries. Thus, in column (3) we modify our Rwanda analysis and construct the cells using age rather than year of birth. Doing this does not change our main finding: the size of the educated cohorts becomes smaller after the genocide, relative to the less educated ones, with a reduction of 38 percent instead of the original 39 percent. However, the confidence intervals of these estimates overlap.

As discussed before, the higher survival rate of the educated observed in these five African countries --including Rwanda itself between 1978 and 1991-- is consistent with a literature showing that a possible additional gain from education is increased longevity. This implies that while the post-genocide missing rate of the educated is large--based on the demographic trends in the region, or in Rwanda itself before the genocide--it seems to suggest an even larger missing rate resulting from the conflict. In other words, if the patterns shown above are used as possible counterfactuals for what would have happened in Rwanda in the absence of the genocide, we would find a much larger effect because the “natural pattern” seems to be an excess missing rate for the *less* educated as opposed to their excess missing rate for *more* educated as found in Rwanda post-genocide.

The negative effect towards the educated cohorts is partially explained by the ethnic targeting of the genocide as described in section two. However, the targeting of the educated population also contrasts with the observed patterns even outside Africa. For example, Leon (2012) and Castillo and Petrie (2007) show that the victims of the conflict in Peru were disproportionately farmers, were part of indigenous groups, and had *lower* levels of education relative to the rest of the population.

5.2 Heterogeneous impacts

In this section we explore several heterogeneous patterns. For instance, we investigate whether the reduction in the stock of human capital, measured by the relative size of the educated cohort post-genocide, varies by year of birth (YOB_i). To estimate such effects we expand equation (1) to include the triple interaction $Educated_j * PostConflict_t * YOB_i$ plus the pair wise interaction among these three variables. Given that year of birth takes several values we present our findings as a graph rather than a table and focus on the triple interaction only (the estimates of all the other parameters followed the previous patterns and are available upon request).

Figure 1 shows that the relative decline in the educated cohort affected mainly those born after 1950. That is, the largest reduction in the stock of human capital occurred among those of prime age at the time of genocide: 16 to 44 year olds in 1994.

We further explore whether this heterogeneous impact varies by gender. For this we now have a quadruple interaction: $Educated_j * PostConflict_t * YOB_i * SEX_s$ and the corresponding smaller interactions among these four variables. Figure 2, shows that the effect does not vary by the gender of the younger cohort. As we will see in Table 6, there are fewer males post-conflict overall, relative to females, but there is no evidence of missing men by educational characteristics after the genocide.

5.3 The genocide: aggregate or local effect?

Our previous analysis has assumed that the genocide was an aggregate shock. In this section we relax that assumption and explore whether the decline in the stock of human

capital has a local component. We use this by exploring four variables about the intensity of the violence as described in section four. These variables allow us to test whether the excess missing rate of the educated post-genocide follows the spatial distribution of the genocide.

This is a widely used approach in the literature. For example, Akresh and de Walque (2011) include three of the four measures¹² we use and show larger negative impacts on schooling for those living in areas with the highest intensity of the conflict. However, all these previous studies do not account for the fact documented here: the educated are missing at higher rates and therefore are not included in the post-genocide surveys. Furthermore, as discussed by Blattman and Miguel (2010), such papers cannot validate the parallel trends assumption between (relatively) peaceful and violent zones in the underlying human development variables due to the limited time horizon of the available data for these countries.

We take advantage of the availability of a pre-genocide census to investigate possible localized effects. To do so we go back to our original specification, and ignoring other heterogeneous effects, we estimate Equation (1) adding the interaction with each the four measures of within-country violence (one at a time):

$Educated_j * PostConflict_t * MEASURE_p$. The results are shown in Table 5. In column (1) we reproduce our main result to ease the comparison. In columns (2)-(5) we add the triple interaction with each measure of violence, respectively. Note that our previous findings remain unchanged: the cohort size of the educated is smaller after the genocide, relative to the less educated group. While we originally found a reduction of 39 percent (column

¹² Akresh and de Walque (2011) do not use proportion of Tutsis in 1991 as a measure of genocide intensity.

1), the inclusion of the regional measures of violence does not alter the magnitude significantly and ranges from -36 percent (column 2) to -41.1 percent (column 5).

Consistent with the idea that the genocide targeted the Tutsis, who on average have higher levels of schooling, we observed that the educated cohorts tend to be larger in areas where the violence was more intense ($Educated_j * Measure_p$). Also, these measures are further validated as proxies for the intensity of the violence within the country as we find the cohorts are smaller in these areas post-genocide ($PostConflict_{jt} * Measure_p$).

However, we do not find any evidence that the destruction in the stock of human capital correlates with the within-country variations in violence. The parameter for the triple interaction between the measures of violence, the educated cohort, and the post-conflict dummy is very small in all estimates and lacks statistical significance. While the aggregate effect lies between -36 percent and -41 percent, the local effect is -5 percent at best (but it is not statistically different from zero).

We explore this local effect furthermore by interacting it with the sex of the cohort (Table 6) and the conclusion remains unaltered: there is an aggregate effect but we find no evidence of local effect and no effect by gender¹³. Further analysis combining the spatial intensity of the violence with the year of birth (Figure 3) or with all possible combinations (education, census year, year of birth and gender) as shown in Figure 4 indicates a clear absence of a localized effect. This should not be a surprise. As Blattman and Miguel (2010) argue, using spatial variations in war or conflict intensity assumes that

¹³ Although the education gradient is not significant for any of the measures, the estimate for gender gradient by education is significant at the 10 percent level only for Measure C.

relatively peaceful areas are not adversely affected by the violence occurring in neighboring, more-violent, zones. These authors suggest that the literature using this within country spatial variation (in a difference-in-difference framework) could underestimate these effects. Our approach allows us to test for such biases and our findings suggest that these biases could be large, as we find very small local effects at best, but large impacts at the national level.

This evidence suggests that the nature of the 1994 genocide in Rwanda created an aggregate shock that disproportionately affected the educated. This pattern is exactly the opposite of what was contemporaneously observed in other countries in the region as well as in the pre-conflict trend in Rwanda. Furthermore, we find no evidence that the effects were local; rather, the genocide operated analogously to a nationwide shock.

5.4 Discussion

The previous subsections document a large and statistically significant missing rate for the educated (those with completed primary schooling or above) after the genocide, relative to their less educated counterparts. What are the labor market consequences of this demographic change? In this section we provide a partial answer by estimating the returns to education on the likelihood of wage employment before and after the genocide.

We focus on wage employment, as opposed to income or earnings, due to data limitations.¹⁴ Nonetheless, the study of wage employment is relevant because when labor

¹⁴ An alternative data source would be to use household surveys. Unfortunately that option is not available to us. There are pre and post genocide waves of the Rwanda Demographic and Health Survey. However, they do not include earnings questions and the samples are restricted to women of reproductive age and

markets are not fully competitive, returns to education are not equalized across different income-generating activities. For example, as described by Vijverberg (1993), in the context of Sub-Saharan Africa, labor market imperfections could arise from entry barriers to wage employment, pervasive regulation of wages, colonial influences on wages of skilled nationals, and the presence of a large public sector where wage scales are not driven by productivity. Consequently, many papers have found that more schooling is associated with a higher probability of wage employment in countries such as Guinea (Glick and Sahn, 1997), Nigeria (Aminu, 2010) and Kenya (Wambugu, 2011). Van der Sluis et al (2005) conducted a meta analysis of over 80 studies in less developed countries (not limited to Sub-Saharan Africa) and found a systematic support for the this pattern: more educated workers are more likely to participate in wage employment.

We estimate the following equation using the *individual*-level data from the 1991 and 2002 censuses (instead of cohorts):

$$\Pr(\text{WageEmployment}_{it}) = \lambda \text{Educated}_{it} * \text{PostConflict}_t + \pi \text{Educated}_{it} + \varphi \text{PostConflict}_t + \delta X_{it} + u_{it} \quad (2)$$

Using a linear probability model, equation (2) tests whether the private returns to education are higher in the post-conflict period relative to the pre-conflict environment.

$\text{WageEmployment}_{it}$ takes the value of one if person i observed at time t works for a wage (or salary) and zero if she works as self-employed or as an unpaid worker. As before,

their partners. Surveys about living conditions such as the Enquête Intégrale sur les Conditions de Vie des Ménages, include detailed questions about labor supply but were created only after the genocide. One important caveat comes from the panel collected by Verwimp (2005) that finds 350 households from a survey originally conducted between 1989 and 1992 that were interviewed again in 2000. Alas, the small sample size prevents us from using it.

$PostConflict_t$ is a binary variable equal to one for the 2002 census and zero for observations in 1991. $Educated_{it}$ takes the value of one for individuals with completed primary education or more and zero otherwise. Thus, the parameter of interest is λ . If returns to education are higher after the genocide, we should expect $\lambda > 0$. Equation (2) includes controls for fixed effects for place of birth, year of birth, sex, religion, language spoken, and marital status (all included in vector X_{it}). As before, the sample is limited to those born between 1930 and 1978. Standard errors are heteroscedasticity- and cluster-robust using year of birth (49 clusters).

The results of estimating equation (2) are shown in Table 7 with and without the controls included in vector X_{it} , columns (1) and (2), respectively. In both cases we find an education premium with respect to the likelihood to be wage employed that is 1.3-1.4 percentage points *higher* after the conflict year, with better precision when adding controls. This implies that the post-genocide premium for wage employment is 40 percent larger ($=0.014/0.035$, column 2) than the overall education premium.

Following our findings of heterogeneous patterns by year of birth, we modify Equation (2) to include interactions with year of birth (at five-year intervals). This means adding the triple interaction $Educated_{it} * PostConflict_t * YOB5_i$ as well as the pairwise interaction among these three variables. In Figure 5 we show that the post-genocide premium is larger for the educated group that is missing at higher rates: those born in the late 1960s and early 1970s. While the education premium increase was around 1.4 percentage points across all ages (column 2, Table 7), the premium for those born in the first half of the 1970s increased by 15.0 percentage points.

The size of the increase in the education premium on wage employment is consistent with recent evidence showing large Mincerian returns to schooling in Rwanda. Montenegro and Patrinos (2014) construct comparable estimates using the same specification, estimation technique and similar data for 139 economies worldwide. They find that Sub-Saharan Africa is the region with the highest private returns per year of schooling (12.3 percent) and that Rwanda (in 2010) has the highest returns of all the worldwide estimates (22.4 percent).

Our finding of a higher education premium post-genocide regarding wage employment is consistent with the relative reduction in the size of the educated cohort post-genocide and echoes similar episodes when the supply of higher levels of schooling are constrained. For instance, Mwabu and Schultz (1996, 2000) and Schultz (1999) estimate Mincerian wage premiums by race in South Africa right before the end of Apartheid. During that regime, the government restricted access to the public education system for non-whites. Using a national representative household survey from 1993, the authors find that the private returns to education were higher for blacks compared to whites. Schultz (1999) argues that the higher returns for non-whites provide clear economic arguments for South Africa to offer greater educational opportunities to this group, which constitute nearly 90 percent of the population.

In the case of Rwanda, given the excessive missing rate of the educated population of prime age and the documented higher returns to education on the probability of wage employment, post-conflict investments in education should have a prediction similar to post-apartheid education policies in South Africa: there are efficiency and equity reasons to provide more educational opportunities.

As a final remark, as Akresh and de Walque (2011) have also noted, overall school enrollment rates (investments) have increased post-genocide in Rwanda. Given the increase in returns to schooling we documented, the increase in schooling enrolment rates should be no surprise.

6. Conclusions

In their review of the existing literature of the economics of civil war, Blattman and Miguel (2010) conclude that some of the leading pending issues in this field are the ways through which war affects human capital as well as by how much and for whom. Our paper answers these questions by showing that the Rwandan genocide had a negative impact on the stock of human capital. We identify a decline between 36-41 percent in the relative size of the educated cohort post-genocide. This was largely focused on prime age adults: those aged between 16 and 44 at the time of the genocide. While more men are missing, we find little evidence of gender bias in the missing rate of the educated. When compared to the trends in Rwanda before the genocide or in other Sub-Saharan African countries, the evidence suggests that our estimates could represent a lower bound. While after the genocide we find excess missing educated in Rwanda, for all other scenarios the excess missing are found among the less educated. The latter is consistent with a lower mortality rate among the highly educated.

We confirmed the previously suspected hypothesis that the spatial differences in the intensity of violence would produce downward biased estimates if the (relatively) more peaceful areas are affected by the violence in the more violent zones. Considering

spatial variation in the intensity of the genocide yields very small effects compared to the large impacts observed at the aggregate level.

Our paper explores the labor market consequences of the demographic change induced by the genocide. Consistent with a decline in the size of the educated cohort, we document an increase in the returns to schooling on the probability of wage employment after the genocide. This increase provides an efficiency and equity rationale for reconstruction policies that facilitate investments in human capital, such as the removal of school fees for primary school enrollment that took place in Rwanda in 2003 (Nkurunziza et al, 2012). It remains to be seen how such policies compare to alternative interventions in post-conflict areas such as the agricultural training and provision of capital inputs conducted in Liberia (Blattman and Annan, 2015) or the cash transfers program implemented in Uganda (Blattman, et al., 2014), in the context of a significant reduction in the size of a country's educated cohort.

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Table 1. End of year refugee population

Year	Refugees from Rwanda (1)	Returned to Rwanda (2)
1993	450,462	307
1994	2,257,573	1,208,005
1995	1,819,366	240,698
1996	469,136	1,410,782
1997	68,003	220,454
1998	77,743	10,939
1999	88,944	38,420
2000	119,056	26,262
2001	84,513	21,656
2002	75,216	38,643

Source: 2002 UNHCR Statistical Yearbook Country Data Sheet - Rwanda

Table 2. Average cohort size by census and characteristics

Cohort characteristic	Census		(Log) Difference	Tripple Difference
	1991 (1)	2002 (2)	(2)-(1) (3)	
Panel A. Full sample				
All	1163.4	793.3	-31.8	
By level of education				
Low-educated	2154.2	1600.8	-0.297	
High-educated	1106.5	566.4	-0.670	
(Log) Difference	-0.666	-1.039	-0.373	
By age group				
Young	1570.7	1088.2	-0.367	
Old	559.7	355.0	-0.455	
(Log) Difference	-1.032	-1.120	-0.088	
By level of violence				
Low violence	1129.7	832.1	-0.306	
High violence	1223.3	727.7	-0.519	
(Log) Difference	0.080	-0.134	-0.214	
Panel B. By age				
B.1. Sample: low-educated				
Young	2785.3	2187.5	-0.242	
Old	1312.9	818.6	-0.472	
(Log) Difference	-0.752	-0.983	-0.231	
B.2. Sample: high-educated				
Young	1768.4	886.6	-0.690	
Old	156.2	89.9	-0.553	
(Log) Difference	-2.427	-2.289	0.138	0.369
Panel C. By level of violence				
C.1. Sample: low-educated				
Low violence	2121.0	1676.1	-0.235	
High violence	2212.3	1469.1	-0.409	
(Log) Difference	0.042	-0.132	-0.174	

Continued on next page....

C.2. Sample: high-educated

Low violence	1065.0	587.6	-0.595	
High violence	1178.5	529.4	-0.800	
(Log) Difference	0.101	-0.104	-0.206	<i>-0.032</i>

Note: Sample limited to those born between 1930 and 1978. Authors' calculations based on the ten percent samples of the Rwanda population censuses of 1991 and 2002. High (low) levels of education correspond to those who have completed (not completed) primary education in 1991. Old are those born in 1950 or earlier (aged 44 or older in 1994).

Table 3. Missing people by level of education

Year of birth (upper bound):	Dependent variable: (Log) Cohort size					
	1978 (1)	1976 (2)	1974 (3)	1972 (4)	1970 (5)	1968 (6)
PostConflict	-0.307*** [0.012]	-0.289*** [0.012]	-0.298*** [0.011]	-0.317*** [0.011]	-0.338*** [0.011]	-0.356*** [0.011]
Educated	-0.524*** [0.019]	-0.512*** [0.020]	-0.577*** [0.021]	-0.665*** [0.022]	-0.764*** [0.022]	-0.877*** [0.023]
Educated*PostConflict	-0.391*** [0.027]	-0.514*** [0.026]	-0.540*** [0.026]	-0.503*** [0.027]	-0.443*** [0.029]	-0.385*** [0.030]
Observations	4231	4055	3879	3703	3527	3351
Adjusted R-squared	0.768	0.771	0.786	0.798	0.813	0.830

Note: Robust standard errors in brackets. All regressions include year and place of birth as well as sex fixed effects. Sample limited to those born after 1930 (inclusive). The upper bound for year of birth is shown in each column.* p<0.10, ** p<0.05, *** p<0.01.

Table 4. Missing people by education: Rwanda and other Sub-Sharan African countries

	Dependent variable: (Log) Cohort size							
	Rwanda			Kenya	Senegal	Tanzania	Uganda	Zambia
	Standard	Placebo	Adjusted					
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
End year	-0.307*** [0.012]	-0.135*** [0.018]	-0.300*** [0.011]	-0.308*** [0.031]	-0.112*** [0.024]	-0.394*** [0.021]	-0.441*** [0.014]	-0.352*** [0.019]
Educated	-0.524*** [0.019]	-1.502*** [0.073]	-0.564*** [0.018]	0.111*** [0.031]	-1.007*** [0.044]	-0.100*** [0.021]	-0.435*** [0.016]	0.033 [0.025]
Educated*End year	-0.391*** [0.027]	0.296*** [0.091]	-0.380*** [0.026]	0.305*** [0.041]	0.033 [0.065]	0.317*** [0.029]	0.204*** [0.021]	0.174*** [0.035]
First year	1991	1978	1991	1989	1988	1991	1988	1990
End year	2002	1991	2002	1999	2002	2002	2002	2000
Observations	4231	560	4239	3083	4198	9882	17217	3919
Adjusted R-squared	0.768	0.844	0.780	0.705	0.642	0.627	0.722	0.722

Note: Robust standard errors in brackets. All regressions include year of birth, place of birth and sex fixed effects. Sample limited to those born between 1930 and 1978. Adjusted Rwanda was created using age at census, instead of year of birth, to be comparable with all other countries. * p<0.10, ** p<0.05, *** p<0.01

Table 5. Missing people by intensity of violence

	Dependent variable: (Log) Cohort size				
	Base model (1)	Measure of violence intensity			
		A (2)	B (3)	C (4)	D (5)
PostConflict	-0.307*** [0.012]	-0.211*** [0.021]	-0.251*** [0.014]	-0.200*** [0.020]	-0.259*** [0.020]
Educated	-0.524*** [0.019]	-0.632*** [0.036]	-0.549*** [0.024]	-0.641*** [0.035]	-0.603*** [0.035]
Educated*PostConflict	-0.391*** [0.027]	-0.364*** [0.050]	-0.370*** [0.033]	-0.386*** [0.048]	-0.411*** [0.049]
Educated*Measure		0.007*** [0.002]	0.063* [0.037]	0.011*** [0.003]	0.009*** [0.003]
PostConflict*Measure		-0.007*** [0.001]	-0.162*** [0.024]	-0.011*** [0.002]	-0.006*** [0.002]
Educated*PostConflict*Measure		-0.001 [0.003]	-0.048 [0.056]	0.001 [0.004]	0.003 [0.005]
Observations	4231	4231	4231	4231	4231
Adjusted R-squared	0.768	0.772	0.772	0.773	0.770

Note: Robust standard errors in brackets. All regressions include year of birth, place of birth and sex fixed effects. Sample limited to those born between 1930 and 1978.

* p<0.10, ** p<0.05, *** p<0.01

Table 6. Missing people by sex and intensity of violence

	Dependent variable: (Log) Cohort Size					
	Measure of violence intensity					
	(1)	(2)	A (3)	B (4)	C (5)	D (6)
PostConflict	-0.307*** [0.012]	-0.235*** [0.016]	-0.174*** [0.029]	-0.197*** [0.020]	-0.168*** [0.027]	-0.217*** [0.028]
Educated	-0.524*** [0.019]	-0.648*** [0.031]	-0.881*** [0.055]	-0.728*** [0.039]	-0.912*** [0.052]	-0.838*** [0.051]
Male	-0.200*** [0.011]	-0.210*** [0.017]	-0.258*** [0.032]	-0.237*** [0.022]	-0.272*** [0.031]	-0.255*** [0.031]
Educated*PostConflict	-0.391*** [0.027]	-0.437*** [0.044]	-0.355*** [0.077]	-0.383*** [0.055]	-0.358*** [0.075]	-0.413*** [0.076]
Educated*Male		0.224*** [0.037]	0.439*** [0.068]	0.322*** [0.047]	0.478*** [0.066]	0.414*** [0.065]
PostConflict*Male		-0.184*** [0.022]	-0.096** [0.040]	-0.140*** [0.027]	-0.084** [0.038]	-0.110*** [0.039]
Educated*PostConflict* Male		0.139** [0.054]	0.026 [0.096]	0.073 [0.067]	-0.008 [0.093]	0.051 [0.096]
Educated*Measure			0.014*** [0.003]	0.193*** [0.059]	0.023*** [0.004]	0.021*** [0.004]
PostConflict*Measure			-0.004** [0.002]	-0.111*** [0.033]	-0.007*** [0.002]	-0.002 [0.003]
Educated*PostConflict* Measure			-0.004 [0.004]	-0.114 [0.089]	-0.006 [0.006]	-0.002 [0.007]
Male*Measure			0.003* [0.002]	0.070* [0.036]	0.006** [0.002]	0.005* [0.003]

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Educated*Male*Measure			-0.013*** [0.004]	-0.236*** [0.074]	-0.022*** [0.005]	-0.021*** [0.006]
PostConflict*Male* Measure			-0.006*** [0.002]	-0.123*** [0.045]	-0.010*** [0.003]	-0.009** [0.004]
Educated*PostConflict* Male*Measure			0.007 [0.005]	0.140 [0.112]	0.013* [0.007]	0.010 [0.009]
Observations	4231	4231	4231	4231	4231	4231
Adjusted R-squared	0.768	0.780	0.785	0.785	0.787	0.783

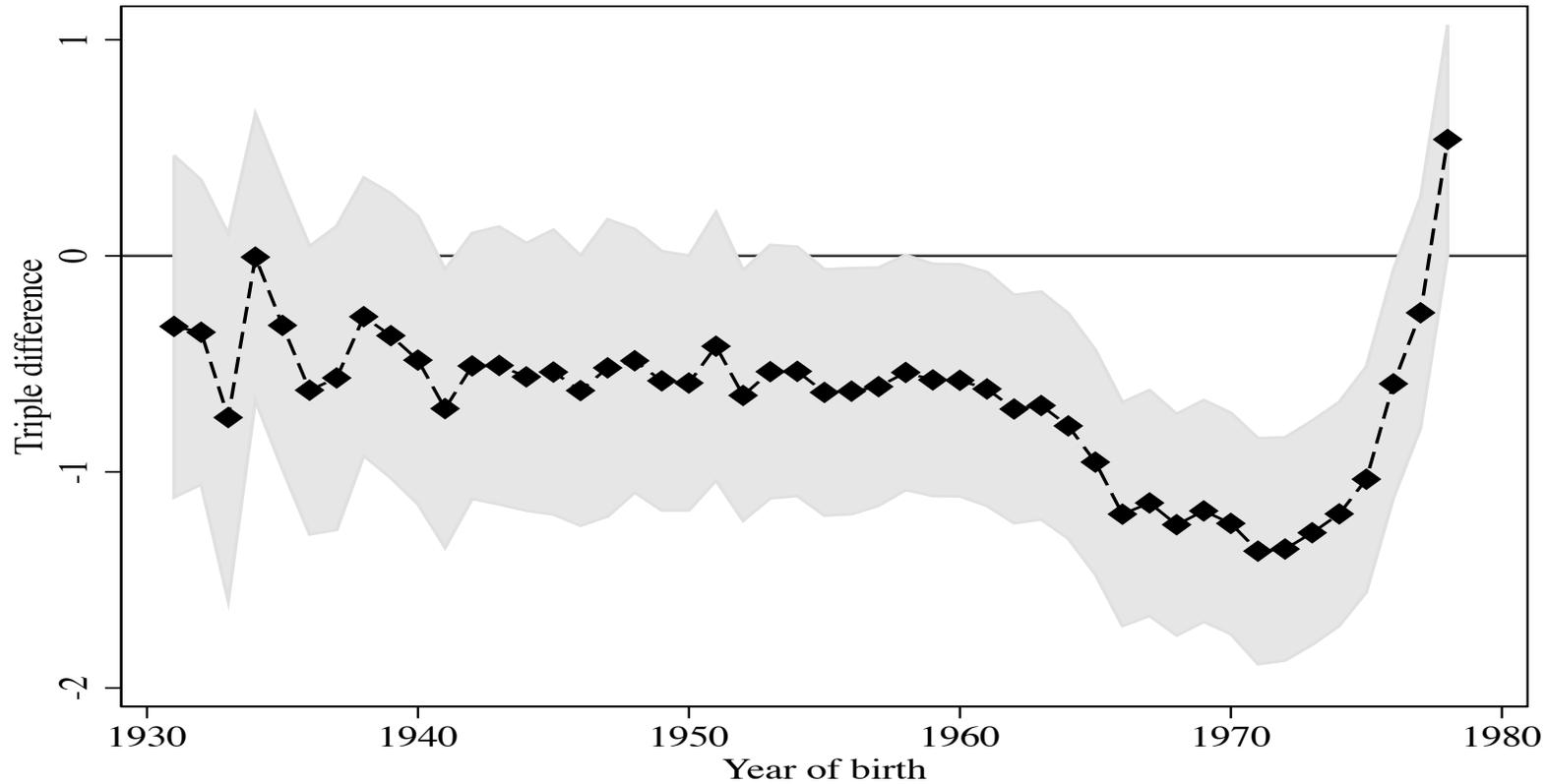
Note: Robust standard errors in brackets. All regressions include year of birth and place of birth fixed effects. Sample limited to those born between 1930 and 1978. * p<0.10, ** p<0.05, *** p<0.01.

Table 7. Changes in the education premium for wage employment

	Dependent variable: Works for a wage or salary (=1)	
	(1)	(2)
PostConflict Census	-0.019*** [0.002]	-0.004 [0.003]
Educated	0.117*** [0.014]	0.035*** [0.006]
Educated*PostConflict	0.013 [0.016]	0.014* [0.008]
Controls	No	Yes
Mean of dependent variable		0.069
Observations		499,315
Adjusted R-squared	0.047	0.199

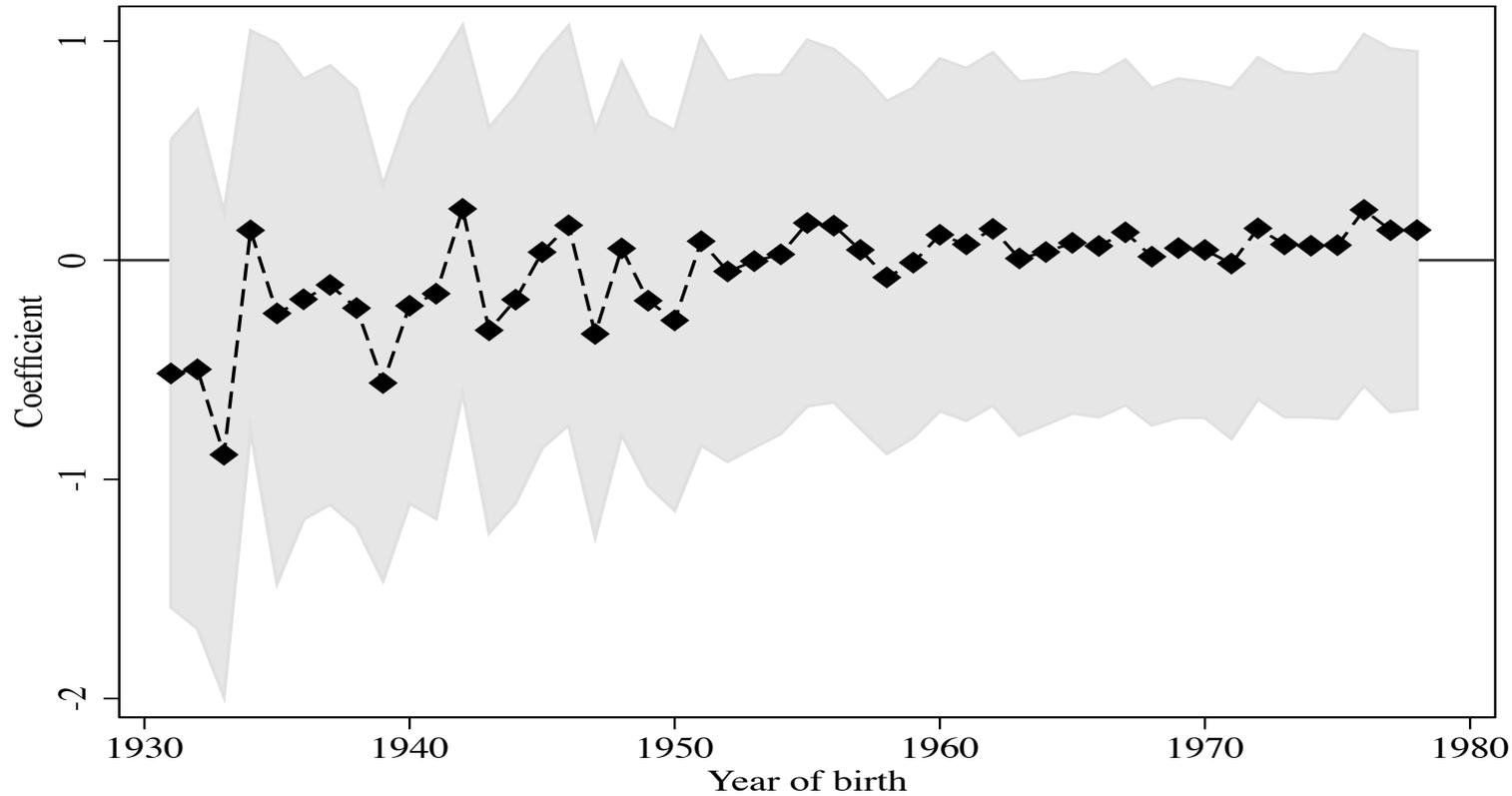
Note: Robust standard errors clustered at the year of birth in brackets. Controls fixed effects for place of birth, year of birth, sex, religion, language spoken and marital status. Sample limited to those born between 1930 and 1978. * p<0.10, ** p<0.05, *** p<0.01.

Figure 1. Missing people by year of birth



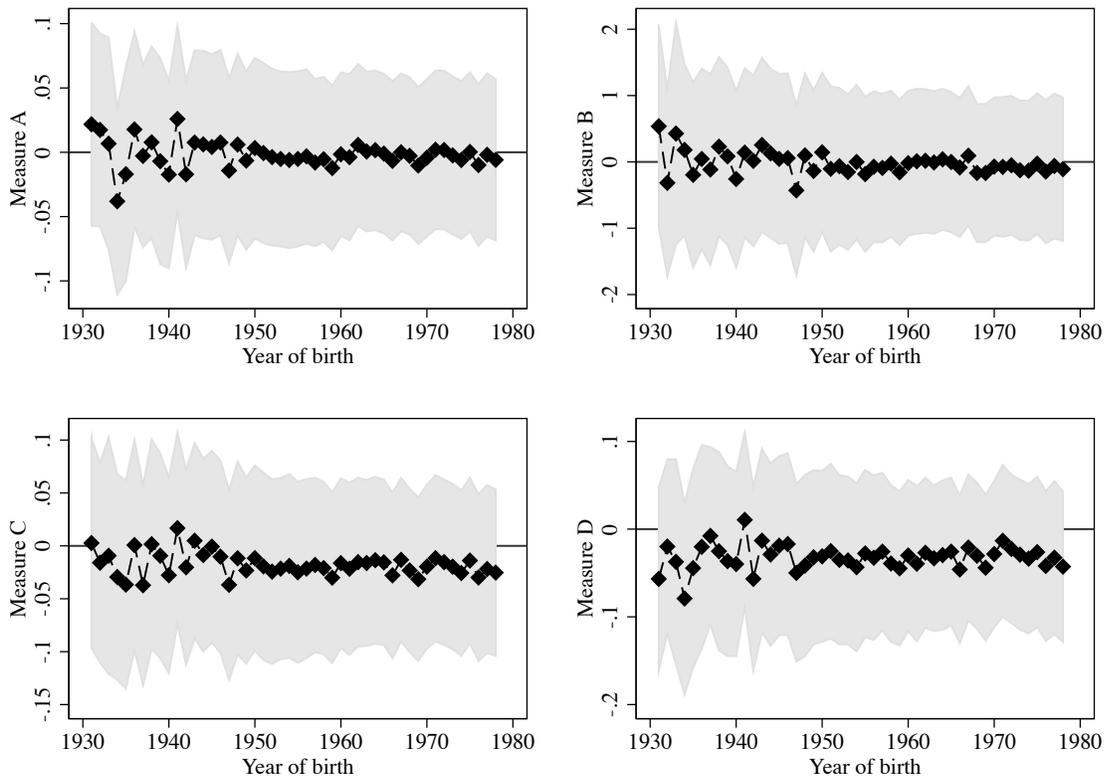
Note: Markers show the parameters for the triple interaction between census year (2002), high education and year of birth. Robust confidence intervals at the 95% are shown as shaded areas. The regression include year of birth and place of birth fixed effects, controls for education, census year and sex as well as the pairwise interactions between census year, high education and year of birth. Sample limited to those born between 1930 and 1978.

Figure 2. Missing people by year of birth, level of education and sex



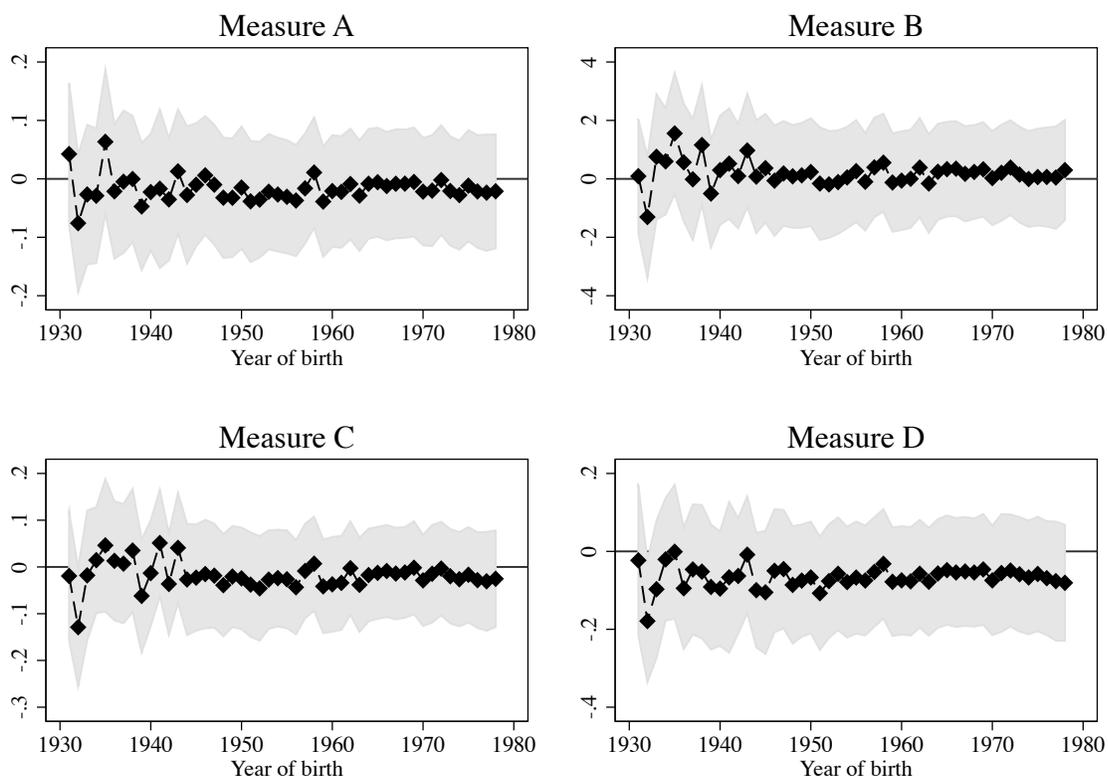
Note: Markers show the parameters for the quadruple interaction between census year (2002), high education, year of birth and sex (male). Robust confidence intervals at the 95% are shown as shaded areas. The regression include year of birth and place of birth fixed effects, controls for education, census year and sex as well as a the pairwise and triple interactions of census year, high education, year of birth and sex. Sample limited to those born between 1930 and 1978.

Figure 3. Missing people by year of birth, violence intensity and education level



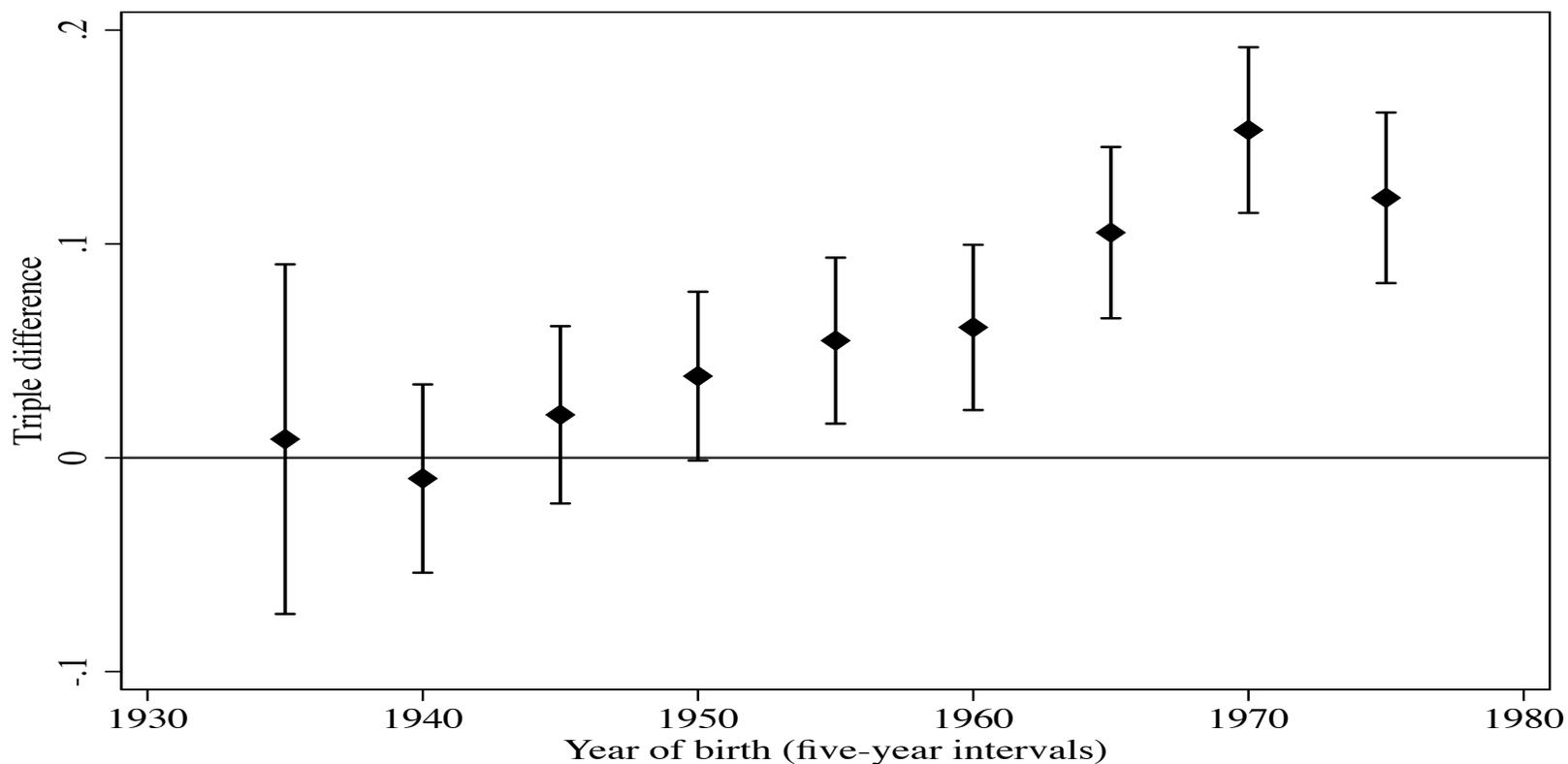
Note: Markers show the parameters for the quadruple interaction between census year (2002), high education, year of birth and a measure of violence intensity. See text for definitions. Robust confidence intervals at the 95% are shown as shaded areas. The regression include year of birth and place of birth fixed effects, controls for education, census year and sex as well as a the pairwise and triple interactions of census year, high education, year of birth and a measure of violence intensity. Sample limited to those born between 1930 and 1978.

Figure 4. Missing people by year of birth, intensity of violence, sex and education level



Note: Markers show the parameters for the quintuple interaction between census year (2002), high education, sex, year of birth and a measure of violence intensity. See text for definitions. Robust confidence intervals at the 95% are shown as shaded areas. The regression include year of birth and place of birth fixed effects, controls for education, census year and sex as well as a the pairwise and triple interactions of census year, high education, year of birth and a measure of violence intensity. Sample limited to those born between 1930 and 1978.

Figure 5. Change in the education premium for wage employment, by year of birth



Note: Markers show the parameters for the triple interaction between census year (2002), high education and year of birth (five-year interval). Robust confidence intervals at the 95%, clustered at year of birth, are shown as vertical ranges. The regression include year of birth and place of birth fixed effects, controls fixed effects for place of birth, year of birth, sex, religion, language spoken and marital status, as well as the pairwise interactions between census year, high education and year of birth (five-year interval). Sample limited to those born between 1930 and 1978.

Appendix A

Table A1. Alternative clustering of standard errors for Table 3

Year of birth (upper bound):	Dependent variable: (Log) Cohort size					
	1978 (1)	1976 (2)	1974 (3)	1972 (4)	1970 (5)	1968 (6)
High educated X 2002	-0.391	-0.514	-0.540	-0.503	-0.443	-0.385
Standard errors						
Heteroscedasticity robust	0.027	0.026	0.026	0.027	0.029	0.030
Cluster-robust on						
Year of birth	0.104	0.060	0.058	0.063	0.061	0.062
Province of birth	0.021	0.020	0.021	0.019	0.016	0.015
Combination	0.034	0.022	0.022	0.023	0.023	0.023
Observations	4231	4055	3879	3703	3527	3351
Adjusted R-squared	0.768	0.771	0.786	0.798	0.813	0.830

Note: All regressions include year and place of birth as well as sex fixed effects. Sample limited to those born after 1930 (inclusive). The upper bound for year of birth is shown in each column. Two-way clustering uses the methods developed in Cameron et al. (2011) and replaces all negative eigenvalues to positive to create a positive-semidefinite matrix.

Appendix B1: Years of Schooling

In this section we explore the relationship between the Rwanda genocide and an alternate measure of stock of human capital-years of schooling. Akresh and de Walque (2011) estimates that the Rwandan genocide lead to 0.37 years of lost investments in children of primary school going age. How does this compare to the effect of the genocide on those who were old enough to have completed primarily schooling? To measure the aggregate effects of the genocide, we can estimate temporal variation between 1991 and 2002 in Rwanda. Consider Equation A1:

$$\text{School}_{ajst} = \beta_1 C2002_t + \gamma_a + \gamma_j + \gamma_s + \varepsilon_{ajst} \quad (1a)$$

where School_{ajst} is years of schooling and γ_a , γ_j and γ_s are fixed effects for the job, province of birth and gender, respectively. Thus, β_1 is the parameter of interest as it captures change over time in School_{ajst} for Rwanda comparing its actual value against the predicted value based on pre-genocide levels.

Do these aggregate changes in years of schooling also reflect subnationally within Rwanda. To answer this question, we estimate the following equation:

$$\text{School}_{ajst} = \beta_1 G_j + \beta_2 C2002_t + \beta_3 G_j * C2002_t + \gamma_a + \gamma_s + \varepsilon_{ajst} \quad (1b)$$

where G_j is a province level measure of genocide intensity. β_3 is the parameter of interest as it captures change over time in School_{ajst} for high genocide areas comparing its actual value against the predicted value based on the change over time in School_{ajst} for low intensity of genocide provinces in Rwanda.

Table B1. Missing people by intensity of violence

	Dependent variable: Years of Schooling				
	Measure of violence intensity				
	(1)	A (2)	B (3)	C (4)	D (5)
2002	-0.378*** [0.026]				
2002 X Measure		-0.001 [0.003]	-0.084 [0.062]	-0.001 [0.004]	0.003 [0.005]
Observations	2020	2020	2020	2020	2020

Note: Robust standard errors in brackets. Difference-in-difference exploits time variation between 1991 and 2002 and spatial variation. (1) Compares Rwanda over time between 1991 and 2002, where as (2)-(5) exploits spatial variation within Rwanda over time. Sample for difference-in-difference includes only native borns between 1930 and 1978. 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 of birth for Rwanda, gender and census year and is weighted by the cohort size. We treat Byumba and Umutara provinces as one since between the two censuses the Rwanda government merged these two provinces. All regressions include controls for gender and year of birth fixed effects.

* p<0.10, ** p<0.05, *** p<0.01

Column (1) in table B1 estimates equation (1a). We find that 0.378 years of schooling is lost in Rwanda between 2002 and 1991. This estimate is significant for at least two reasons. First, the magnitude is large- more than a third of a year of schooling is lost. Second, because these estimates are remarkably comparable to the estimates for the effect of conflict on investment in child schooling by Akresh and de Walque (2011). This suggests that destruction of human capital was large and significant. Columns (2)- (5) in table B1 estimates equation (1b). Like our main estimates we use four different subnational measures of genocide intensity and find robust results: there is no evidence of

any significant effects of within Rwanda genocide intensity measures on years of schooling.

Appendix B2: Note on Pre-Conflict Placebo (1978 Census):

Although our main analysis uses data from 1991 and 2002 census, the first census in Rwanda was collected in 1978. An attractive feature of having two rounds of pre-genocide census is that this allows testing the assumption of parallel trends between more educated and less educated cohorts between 1991 and 2002, using data from Rwanda itself, instead of other countries.

This census is not readably available at the IPUMS, and to the best of our knowledge has not been used in the economics of conflict lecture. However, we were able to find relevant data for our placebo analysis from University of Pennsylvania's Demography Library and Penn Libraries¹. The data have a few limitations: a) our analysis suggests that the genocide had the strongest effects on the younger adults. However, many cohorts (those born between 1965 and 1978 in particular) for whom we find large effects in our main analysis were too young to have completed primary schooling in 1978. This suggests that estimates from the 1978 data may be biased downwards b) the data tables from 1978 were in 5 year intervals in contrast to data from 1991 and 2002, which restricts the number of cohorts we can analyze; c) we could not disentangle the native from the immigrant population from the data we have available to us.

¹ 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.