



Schooling, Violent Conflict and Gender in Burundi

Philip Verwimp and Jan Van Bavel

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Abstract

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1. Introduction

During the past 30 years, civil conflict affected almost three-fourths of all countries in sub-Saharan Africa (Gleditsch et al. 2002). Academic economist and other social scientists have begun focusing on this topic, trying to understand the causes of war and its role in reducing growth and development (Collier and Hoeffler 1998; Miguel, Satyanath, and Sergenti 2004; Guidolin and La Ferrara 2007). The nature and magnitude of a conflict's long-term economic consequences are debated in the literature. Davis and Weinstein (2002) for Japan; Brakman, Garretsen, and Schramm (2004) for Germany; Miguel and Roland (2006) for Vietnam; Bellows and Miguel (2006) for Sierra Leone find rapid economic recovery after civil war. Convergence towards the country's long-term growth path is reached relatively fast, often within 15 years, as a neo-classical growth model would predict. Nevertheless, this macro-economic, aggregate picture does not seem to square with the immediate negative economic impacts through the destruction of productive capacity and the disruption of normal activity.

The relatively fast rate of convergence to the longer-term growth path of the economy does not tell us who is responsible for the recovery. Imagine the fate of Guido, a civil engineer and his younger niece Nathalie. If war breaks out when Guido is in his thirties, he will not be able to use his skills as a civilian engineer for peaceful purposes during the war. But, under the assumption of his survival, he will be able to put them again into productive use again after the war, when he is in his forties. His generation can thus shoulder the economic recovery. The same cannot be said of Nathalie who was in her teens during the war and lost out of her secondary and potentially university education. She will have accumulated less human capital than she otherwise would have and her generation may not be able to shoulder the recovery the country needs, at least not in the immediate aftermath of conflict.¹ The point is that we have to do with a cohort effect that has to work all its way through the demographic pyramid. The longer-term impact, in particular in terms of foregone schooling, may only be felt many years after the end of the recovery, beyond a period of 10 to 20 years that one would normally

¹ During his frequent visits to Central Africa the first author has noticed how hard generations of young adults try to make up for their lost education in the post-conflict period, witnessed by the organization of evening programs and the blossoming of private universities.

consider as a period of economic recovery. By the same token, as Nathalie was not able to finish her education she may have decided to marry earlier than she would have in the absence of war. This could for example be for reasons of personal safety, income security or death of a parent. Earlier marriage could in turn increase the number of children that Nathalie will have.

The heterogeneity of the potential impact of conflict on birth cohorts also counts for gender. There is no universal rule to predict what that impact will be. Existing gender inequalities may be exacerbated during violent conflict, but they may also be attenuated. The direction of the effect is an empirical question. It may be that they are exacerbated in one domain, e.g. sexual violence, but at the same time the conflict may offer new opportunities, e.g. in paid labor or business. The direction of the effects as well as their magnitude will differ from country to country and context to context, depending on pre-existing gender inequalities, the type of conflict, the duration of the conflict and the institutional particularities of the war-affected country.

When, for example, the conflicting parties engage child soldiers, it is likely that boys are more affected than girls. Similarly, when a country needs the brains and work effort of young women such as Nathalie to work in the military industry during a long standing militarized dispute with a neighbor, the labor market position of women stands to benefit from that conflict. In contrast, when a liberal society with relatively good access for girls to education is overrun by a violent anti-liberal rebellion, women are often the first victims as they may not anymore be allowed to go to school.

We focus in this paper on the effect of civil war on schooling. We want to know the direction and magnitude of such effect in terms of foregone schooling for both boys and girls. If schooling is negatively affected, then this may in turn affect subsequent choices and opportunities for both man and women in Burundi, including access to paid labor, age at marriage, number of children, socio-economic characteristics of the spouse, and so on. The level of schooling attained as a child and young adult is a fundamental driver of welfare throughout one's entire life.

We find that the completion of primary schooling in Burundi is heavily effected by the massacres (1993-1994) and the civil war (1995-2005), for boys as well as for girls. For every year that a school-aged boy was exposed to conflict in his province of

residence, the odds to complete primary schooling decreases by around 50 to 60% depending on specification. For girls the odds decrease by around 20 to 30% per year of exposure during school-age. The results for boys are robust for all specifications, including province fixed effect and household random effects regression models. Results for girls are robust in most specifications.

In this paper we also investigate several channels through which violent conflict affects schooling, to wit exposure to violence in one's community as well as forced migration. Displaced children may be deprived of education but also of the support provided by educational structures in unstable or violent settings. The availability and quality of education in refugee or IDP camps may vary a lot, from well-organised to absent or very disorganised. Accessing schools outside the camps may not be an option due to issues of safety.

We work with the *Enquête Démographique et de Santé* (EDS) collected by UNFPA in 2002. This survey has very detailed information on each member of the interviewed households, including all births and deaths, schooling and wealth and well as the entire migration history during the civil war. We combine these surveys with event data on the location and timing of the conflict. The empirical identification strategy exploits variation in the onset and duration of conflict across Burundi's provinces and the related variation determining which cohorts of children were exposed to the massacres and the civil war during the children's school aged years.

The paper is structured as follows. After a review of the literature on conflict, gender, schooling and displacement we discuss the state of the economy in Burundi as well as the history of the country's civil war. We then present the data set that we use and proceed with the estimation strategy for schooling. Results are presented in section 5 followed by a robustness analysis and conclusion.

2. Review of the Literature on schooling, gender and conflict

While there is a body of research analyzing how households cope with economic or agricultural shocks such as crop failures, famines or droughts (Fafchamps, Udry and Czukas, 1998; Dercon, 2004), there is not much work on the micro-economic consequences of political shocks, be them violent or non-violent. While few households

have formal insurance against economic shocks, many have a set of informal insurance mechanisms that they can use, like self-insurance (portfolio spread, income diversification, temporary migration), village level solidarity mechanisms or even outside insurance against weather calamities (Dercon, 2004). Such insurance mechanism appear not to be available for political risks. Or, at least, the scholarly community has largely failed to study potential coping mechanism for political shocks. One of the findings of the coping literature in development economics is that non-poor households are better able to cope with negative economic shocks compared to poor households. Using assets, savings or their social capital, they succeed better in cushioning the negative impact of weather, disease, or price shocks. The scarce but nascent literature on political shocks suggests that this poor versus non-poor divide in terms of coping is non-existent or in any case much smaller than in the case of economic shocks. In the event of anti-urban/anti-intellectual conflicts, the non-poor educated part of the population may even be hit harder than the poor uneducated part, thereby having completely different effects on their welfare in comparison to economic or agricultural shocks.

Shemyakina (2006) finds from her empirical work in Tajikistan, that girls suffer the greater loss in education compared to boys and she attributes this to concerns over safety and low returns to girls' education. In contrast, Akresh and de Walque (2008) find that, in Rwanda, it is amongst the male children in non-poor households that violent shocks have the strongest effect. Evans and Miguel (2004) find that young children in rural Kenya are more likely to drop out of school after the parent's death and that effect is particularly strong for children who lost their mothers. While Kenya was not the scene of violent conflict during the observed period, the finding is relevant because violent conflict produces many orphans, which may have a similar effect on their schooling.

Combining a household panel with detailed data on allied bombings of German cities during WW II, Akbulut-Yuksel (2009) finds significant, long-lasting detrimental effects of bombing on human capital and labour market outcomes of individuals who were at school-age during WWII. These individuals had 0.4 fewer years of schooling on average in adulthood in comparison to those not affected by the bombings. Affected children experienced on average a reduction of 6 percent in labour market earnings in relation to those not affected. Merrouche (2006) arrives at similar results for Cambodia.

She finds that land mine contamination has caused significant educational losses. A conservative estimate at the mean level of landmine exposure suggests a loss of about 0.4 years of education. This again represents an educational setback of 11% given a sample average number of years of education of about 4.5 years in 1997.

Alderman, Hoddinott and Kinsey (2006) find that Zimbabwean children affected by the civil war in the 1970s completed less grades of schooling and/or started school later than those not affected by the shocks. Similar results are found by Angrist and Kugler (2008) and Rodriguez and Sanchez (2009) for Colombia; Chamarbagwala and Morán (2009) for Guatemala, de Walque (2006) for Cambodia.

Next to this loss of educational attainment in general, Justino (2011) observes a second pattern in the results of emerging studies, to wit that secondary schooling seems to be disproportionately affected. Stewart et al. (2001) find that primary school enrolments decreased in only three out of eighteen countries in their sample of countries affected by civil wars. Swee (2009) provides evidence on the effects of the civil war in Bosnia (1992-1995). He finds that individuals in cohorts affected by the civil war are less likely to complete secondary schooling if they resided in municipalities which experienced higher levels of war intensity. He finds no noticeable effects on primary schooling.

Reasons of why education during the war may be affected negatively include school closure, migration and displacement, quality and availability of school facilities and shocks to income and security. Chamarbagwala and Morán (2008) find that individuals who were at school age in areas more affected by the war (1979-1984) in Guatemala completed fewer years of schooling, and that this effect was stronger for girls. The authors find a significant positive correlation between conflict intensity and education at low levels of schooling. Girls exposed to the 1979-1984 war during their school-age years completed 0.44 years of school (or 12%) less than girls living in departments not affected by the fighting. Older female cohorts exposed to the war completed 0.64 years (17%) less schooling than those not affected by warfare. The effect for males is smaller. Female education continued to lag behind male education throughout the country, but especially so in the areas of high war intensity between 1979 and 1984, almost two decades after the worst conflict outbreak. The study suggests that loss of property and massive displacement led households to reallocate limited resources

towards providing young boys and, to a lesser extent, young girls, with at least some primary education. While both boys and girls received less secondary education as a result of the civil war, the effects were more pronounced for girls. Girls in higher grades seem to be the main victims. Similarly to Akresh and de Walque (2008), Chamarbagwala and Morán find that a lower probability of progressing from one grade to another rather than not attaining any education appears to drive the results.

Tembon and Fort (2008) analyse the importance of girl education for economic growth. Justino (2011) observes that children needed to replace labour may be removed from school, which may in turn deplete the household of their stock of human capital for future generations. Akresh and de Walque (2009), Merrouche (2006), Shemyakina (2006) and Swee (2009) point to this mechanism as an explanation for the reduction in educational attainment and enrolment observed in contexts of civil war. In a recent paper, Rodriguez and Sanchez (2009) test directly the effect of war on child labour and find that violent attacks in Colombian municipalities by armed groups have increased significantly the probability of school drop-out, and have increased the inclusion of children in the labour market. They show that increased mortality risks, negative economic shocks and reduction in school quality due to violence are the main channels through which armed conflict reduces human capital investments at the household level and increases child labour. We add that not only may the young generation be prevented from acquiring human capital, educated members of older cohorts may be disproportionately killed, thereby depriving the country from its human capital stock.

3. Conflict, the economy and education in Burundi

The 1990s were a particularly violent decade in Central Africa's history. Burundi and Rwanda experienced several episodes of mass murder and genocide, and the regional civil war in the Democratic Republic of Congo created an enormous loss of life and socioeconomic destruction. Most of the recent work on Burundi focuses on the causes of the latest episode of civil conflict (Nkurunziza and Ngaruko 2000), the progression of the crisis (Chrétien and Mukuri 2000), the year-by-year political dimensions of the conflict (Reyntjens and Vandeginste 1997; Reyntjens 1998), or the possible solutions to it (Ndikumana 2000).

Between 1990 and 2002, per capita income in Burundi fell from \$210 to \$110 leaving Burundi as the world's poorest country. The proportion of people living below the nationally defined poverty line increased during this period from 35 to 68 percent, and the conflict led to double digit inflation rates, which peaked at over 30 percent in 1997 (all figures from IMF 2007).

3.1 Short Political History of the Conflict²

Civil conflict in Burundi began in 1965, three years after independence from the Belgian colonial administration, when a group of Hutu officers unsuccessfully tried to seize power and overthrow the monarchy. This failed coup led to a purge of Hutu from the army and government and marked the beginning of political exclusion of the Hutu majority by the Tutsi minority. Power became the sole monopoly of the Tutsi, who effectively seized power in 1966 and proclaimed the First Republic, headed by Captain Michel Micombero. During Micombero's regime, power was increasingly concentrated in the hands of the Tutsi Hima clan, a small ethno-regional group from the southern province of Bururi, that the French historian Chrétien (1997) calls the Bururi mafia.

In 1972, a Hutu insurgency started in southwestern Burundi resulting in considerable loss of life among the Tutsi residents. The subsequent Tutsi army repression was dramatic. From May to August 1972, all educated Hutus and members of the Hutu elite were systematically eliminated or fled into exile (Lemarchand 1994). This massacre of educated Hutus reduced their status to an oppressed underclass and reduced future Hutu opposition for over a generation.

The next major confrontation was in 1988, when a Hutu insurgency began in the north. As in 1972, army repression was swift and took a heavy toll on local Hutus. However, unlike 1972, the international community condemned the massacres and pressured the Buyoya regime to liberalize its political system. In June 1993, this led to the first free and fair elections in post-independence Burundi. Unfortunately, the democratic transition did not last. In October 1993, Melchior Ndadaye, the first democratically elected president and a Hutu, was assassinated by Tutsi army elements in a failed coup attempt, marking the start of another civil war. As the news spread to the

² This section relies on Bundervoet, Verwimp, Akresh (2009).

rural provinces, Hutu peasants committed large-scale massacres of Tutsis and Uprona Hutus. Chrétien (1997) describes the massacres saying districts in certain provinces were “almost completely ‘cleansed’ of all Tutsi elements.” Within days, approximately 100,000 Burundians lost their lives in what was later acknowledged as genocide (United Nations 1996). The Tutsi army retaliated against the Hutus, continuing what would become the most severe civil war in Burundi’s history, both in terms of human lives and socioeconomic destruction. Unlike prior wars that began with a localized Hutu insurgency followed by severe but random Tutsi army reprisals, this crisis was a more traditional war, with two opposing armed and organized factions and an impact on almost the entire country (Ndikumana 2000).

3.2 Spatial and Temporal Intensity of the Conflict

In this paper we use the term violent conflict to describe the massacres that occurred in the 1993-1994 period as well as the subsequent civil war in the 1995-2005 period. As the exact timing and location of the massacres and the civil war plays an important role for our identification strategy (see below), we describe the evolution of the massacres and the civil war through time and space as follows:

- 1993 and 1994: massacres in many parts of the country but with different intensities
- End of 1994 to July 1996: Spread of civil war throughout the country (see Figure 1)
- July 1996 to August 2000: Return of Major Buyoya to power after a bloodless coup. Lower civil war intensity in most provinces and signing of the Arusha Peace and Reconciliation Agreement in 2000.

The massacres were particularly intense in central and northern Burundi. Bundervoet (2009) estimates that in half of the provinces more than 7% of individuals interviewed in the UNFPA survey (see below) lost their father in 1993. Table 5 gives the data per province and sketches the evolution of the civil war based on Chrétien and Mukuri (2000). Fighting began in October 1994 in the northwestern provinces of Cibitoke, Bubanza, Bujumbura Rural and Ngozi. By early 1995, violence spread to the bordering Kayanza province, and by April 1995, massacres of civilians and confrontations between

army and rebel forces happened in Karuzi, Bururi, Ruyigi and Muyinga. By late 1995, fighting took place in the central provinces of Gitega and Muramvya and the northern province of Kirundo. Figure 1 depicts the situation at the end of 1995. By then, conflict had spread to almost all of the provinces of Burundi, with the exception of Cankuzo (in the east of the country) and Rutana and Makamba (in the south of the country). In July 1996, former president Buyoya seized power again in a bloodless coup d'état backed by the army. During late 1996 and early 1997, armed conflict continued in Kayanza, Muramvya, Kirundo and Gitega. Meanwhile in April 1997, the Arusha Peace talks between the principal conflict parties began. As of late 1997, insecurity increased again in Cibitoke, Bubanza and Bujumbura Rural, provinces which remained unsafe until 1999.

The various conflict accounts provide no definitive explanation for why the massacres and the civil war affected some provinces earlier than others. However, the conflict's spread was clearly influenced by the rebel base locations in the Democratic Republic of Congo's North Kivu region next to the borders of Cibitoke, Bubanza, and Bujumbura Rural, which explains why these provinces were first to experience war. The presence of the Kibira forest bordering these provinces could explain the subsequent spread of war to Kayanza and Ngozi, since rebels passed undetected through the forest. From these initial conflict provinces, the war spread to the rest of the country.

3.3. Civilian Impacts of the Conflict

According to Human Rights Watch (1998), the civil war in Burundi “has above all been a war against civilians.” They were widely used as proxy targets, with both sides targeting civilians deemed supportive of the other group. Between 1994 and 2001, an estimated 200,000 people lost their lives in the war, a majority of them civilians (UNFPA 2002). To understand how the war affected child schooling, we focus on displacement, looting of household assets, and the theft and burning of crops.³

First, the demographic household survey conducted by the United Nations Population Fund (UNFPA), which we use in this paper, found over 50 percent of the rural Burundi population had been displaced from their homes at least once between 1993 and

³ For an analysis of the health consequences of the civil war in Burundi, we refer to Akresh et al (2009), Health and Civil War in Rural Burundi, *Journal of Human Resources*, 44, 2, p.536-563.

2000 due to violence (UNFPA 2002). The average displacement duration for the entire sample was just over one year, meaning three agricultural seasons were missed as households could not cultivate or harvest their fields while displaced (UNFPA 2002). Displacement also meant individuals were more likely to contract water and vector-borne diseases while hiding in the forest. As people could not carry significant amounts of food when fleeing their village, adequate nutrition was a problem. Finally, displacement also implied a lack of access to markets, health clinics or schools as roads were unsafe or these structures had been damaged. Later in the war, civilians were forced into local resettlement camps by the government and camp conditions were poor, being overcrowded and with a lack of food supplies, clean water, or waste disposal (HRW 2000). The displacement's impact on aggregate production from 1993 to 1998 showed production declines in cereals of 15 percent, roots and tubers 11 percent, and fruits and vegetables 14 percent, with particularly dramatic declines in 1994 and 1995 (FAO 1997). Later on in the paper we will test the impact of displacement on schooling as a potential channel by which exposure to violent conflict can affect child schooling.

We add that when conflict ended in a given province, households who were displaced could and did return to their homes and fields. However, humanitarian interventions by either the government or non-governmental organizations (NGO) after the fighting ended were practically nonexistent, due to the continued insecurity on all roads linking the capital, Bujumbura, to the countryside. By early 1995, rebels groups had begun to target and kill foreign NGO workers and journalists who left Bujumbura to visit war regions. Moreover, international development assistance dropped sharply during the crisis, from almost \$320 million before 1993 to below \$100 million in 1999 (IMF 2007).

Second, besides the displacement and killing of civilians, both rebel and government forces engaged in the looting of civilian property, in particular livestock, causing an unprecedented drop in household welfare levels. Aggregate national figures show the number of tropical livestock units in the country declined by 23 percent from 1990 to 1998, a decline that is predominantly due to theft and pillaging (FAO 1997). At the household level, the results of the UNFPA survey show that the average number of tropical livestock units per household fell from 2.37 before the crisis to 0.42 in 2001

(UNFPA 2002). In the regression analysis below the size of livestock before the start of the conflict will be used as an indicator of pre-conflict wealth. Luckily, these retrospective questions were asked in the 2002 survey. Given its importance in rural Burundi we are confident that people are able to remember how many cows and other animals they possessed at the start of the conflict.

Third, Human Rights Watch reports (1998) document the theft and burning of household crops. Crops were stolen from the field or granaries and coffee trees were particularly targeted for burning. As coffee is the government's main source of tax revenue, rebels frequently burnt coffee plantations to reduce government revenue, although we cannot quantify the extent of this. Coffee is also an important source of income for small farmers, so by losing their crop, farmers had less income to pay for other expenditures, including purchasing food crops, school fees or health care. The UNFPA survey however has no data on theft or burning of crops.

Fourth, the conflict in Burundi is notorious for its adverse impacts on women and girls. Rape was widespread and there have been many instances of brutality, even against children. Note, on the other hand, that Burundi also had a female prime minister during the conflict and other women have been gaining prominence in Burundese society.

3.4 Education and Conflict in Burundi

Access to education, in particular secondary and university education has been a long-standing source of inequality, tension and conflict. This is because it is directly related to jobs in the public sector who demand a formal degree. The education system together with jobs in the administration was dominated by Tutsi from the southern region of Burundi. Nkurunziza and Ngaruko (2002) write that in 1972 almost all educated Hutus were killed by the Tutsi army. Education was clearly a liability then.

In a new report on education and violent conflict UNESCO (2011, p.51) calculates that the onset of conflict in Burundi marked an abrupt change in enrolment. The decade before the conflict (1981-1991) oversaw an expansion of enrolment for each new cohort, male as well as female. The gross enrolment rate increased from 33.2 to 70 in that decade (Ministry of Education, 1999). The conflict-induced trend reversal can be observed from Figure 2 which we computed with the UNFPA data: the older birth

cohorts (who could finish primary schooling before the start of the conflict) show an upward trend in primary school completion. The cohorts born between 1975 and 1980 show the highest primary school completion rates in the history of Burundi (up to 2010). This was due to the expansion of primary education which doubled the enrolment rate in five years (1982-1987) (UNESCO 1999; UNICEF, 2008). Progress stops for the birth cohorts born at the end of the seventies and is reversed from the 1980 birth cohorts onwards, just when the first birth cohorts are confronted with the start of the violence. Children in Burundi attend primary school from age 7 to 12 when they finish 6th grade (UNESCO Institute for Statistics, January 2008). Some children may start schooling later and complete primary schooling at later age. Figures 3 and 4 demonstrate the decrease in primary school completion per number of years of exposure to conflict for boys and girls.

In an extensive review of the damage done to the education sector during the conflict in Burundi, Obura (2008) observes that schools were destroyed or looted and teachers and children killed or displaced. Importantly, while the gross enrolment rate decreased, the Gender Parity Index did not decline during the conflict and even improved slightly (see tables 1 and 2). Obura also remarks that a church-led education initiative, called Yaga Mukama, that existed before the war and provided two days of primary school level education per week to the rural poor, became very popular during the war and even acted as a sort of substitute for formal education in affected areas.

4.Data and Estimation Strategy

4.1 Conflict variables

In 2002, UNFPA collected demographic and health data (EDS) on almost 7,000 households. Descriptive data are presented in tables 3 and 4. Since many Burundese lived in camps for internally displaced persons, a particular feature of this survey is that it is stratified over urban, rural and camp locations. The focus of the survey was on household composition, schooling and health with a lot of attention for the potential impact of the conflict through displacement. In order to determine who was affected by the massacres and the civil war at school-age, we use two sources. For the massacres we rely on Bundervoet (2009) who computed the percentage of people in the 2002 UNFPA survey (EDS) whose father was killed. He applied the method proposed by Gakidou and King

(2006) to correct for selection bias resulting from the absence from the survey of households where everyone was killed. Using that estimate (reproduced in Table 5) we have eight provinces with a death rate higher than the median death rate (7%) and eight provinces with a lower death rate. We define the high mortality provinces as the area (heavily) affected by the massacres and the low mortality area as the non-affected area.

For exposure to the civil war, we rely on Chrétien and Mukuri (2000) referred to in section (3.2) who describe the spread of the violence over space and time. This allows us to construct an exposure variable on birth cohort – province level basis. We also use the UNFPA data to construct alternative conflict variables at the individual level (see robustness analysis), to wit the number of times the person was forcibly displaced and the duration of stay in a displacement camp. In summary, we have three conflict exposure variables:

- (i) the length of exposure to violent conflict. This is the number of years the child was exposed to massacres and to the civil war measured at the birth cohort – province level. For example: a child born in 1985 and residing in the province of Karuzi was affected 4 years during its school-age years, being the sum of 2 years (1993 and 1994) because of the location of massacres in this province and 2 years (1995 and 1996) because of the spread of civil war into Karuzi in these years.
- (ii) The number of times the child had to move residence forcibly during the massacres and the civil war;
- (iii) The number of years the child spent in a displacement camp, again at primary school-age. We believe that this is the more relevant indicator compared to residence in a displacement camp at the time of the survey.

Many children born in the 1981-1986 period have experienced at least one year of conflict during their primary school career. The oldest, born in 1981 was about to graduate from primary school when the conflict started. Not all of these children are affected however. Some provinces were not affected by the civil war (Cankuzo and Rutana) and we defined half of the provinces with low mortality resulting from the 1993 massacres as being not affected by those massacres. Children born before 1981 had the chance to finish their primary schooling before the start of the conflict and we define

these older cohorts as being not affected. Since we do not know the exact year or the exact age at which a child finished her primary schooling, we cannot lengthen the exposure time beyond age 12: as exposure is measured on a year of birth – province level basis we would wrongly consider a child affected at age 13 in a given province when in effect she may have completed her primary schooling the year before. So the maximum duration of exposure is six years. Using the method just described we do not find children exposed for more than four years in our sample.

4.2 .Empirical Specifications

Our basic approach is a difference-in-differences strategy. We use the spatial and temporal variation of violent conflict in Burundi to infer the effect of exposure on child schooling. We compare children who were exposed to several years of conflict in their area of residence during their school-aged years with children of the same age residing in areas who were not much affected, as well as with children who were old enough to finish their schooling before the conflict started in both heavily affected and not much affected areas.

Building on Figure 2 and previous tabulations, we first estimate the following baseline province and birth cohort fixed effects regressions:

$$Schooling_{ijt} = \alpha_j + \delta_t + \beta_1 (Conflict\ Province_t * Exposure_j) + \varepsilon_{ijt} \quad (1)$$

where *Schooling* is our education variable, which in our case is a binary variable for having completed primary school or not, measured for a child *i* residing in area *j* and born at time *t*. With α_j the area fixed effects, δ_t the birth cohort fixed effects and ε_{ijt} is a random error term. We calculate the *Conflict Province_t * Exposure_j* variable first as a binary measure to indicate a child residing in a province that experienced violent conflict and second, as a continuous measure to indicate the duration (in years) of exposure for a child residing in an affected province. In the latter case, β_1 , the coefficient of interest, measures the impact on schooling of an additional year of exposure to violent conflict. Including all provinces allows us to use variation in onset as well as the duration of conflict across provinces to identify the war's causal impact on children's schooling.

4.3 The Gender differential

$$Schooling_{ijt} = \alpha_j + (\delta_t * S_i) + \beta_1(Conflict\ Province_t * Exposure_j) + \beta_2 S_i + \beta_3(S_i * Conflict\ Province_t * Exposure_j) + \varepsilon_{ijt} \quad (2)$$

Whereby S_i is the sex of the child and the other variables are as in specification (1). In this specification β_1 variable gives the effect of violent conflict on schooling for boys. The interaction effect between gender and conflict tells us whether or not there is an additional effect for girls and the linear combination of β_2 and β_3 gives the total effect of conflict on schooling for girls. One of the specifications in the empirical part will control for gender specific time trends.

4.4 Expansion of the baseline – Channels of Impact

Apart from the conflict measure used in (1) and (2), being the binary conflict exposure variable and the number of years of exposure to conflict in the area of residence during primary school age, we employ other variables to give us more insight in the potential channel by which violent conflict impacts child schooling. We therefore develop other specifications where we use alternative measures of conflict indicating a specific mechanism.

$$Schooling_{ijt} = \alpha_j + \delta_t + \beta_1(Channel * Exposure_j) + \gamma Z_i + \varepsilon_{ijt} \quad (3)$$

The channels are the time spent in a displacement camp during school age and the number of times the child moved residence during school age. In the above specification we have introduced characteristics that are specific to the household in which the child lives. Importantly and to avoid endogeneity, these household level characteristics are measured in 1993, i.e. *before* the start of the conflict. Where Z is a vector of child specific characteristics such as the age, sex, level of education of the head of the household and the wealth of the household.

5. Issues of concern to the identification strategy: poverty, selective survival and selective migration

A first issue of concern for our estimation strategy is that we may measure the effect of something else than exposure to civil war. If, for example, the war lasted longer in poor provinces or if it did not affect relatively well-off provinces, than we may be measuring the effect of poverty in stead of exposure to civil war. While we do control for wealth (under the form of livestock) in our regression analysis, this variable is measured at the household level. Since our exposure variable is measured at the province level, we have to make sure we are not picking up another effect. To that effect, in table A1 in the appendix, we present data on the death rate in 1993 and the duration of the civil war in poor and non-poor provinces. Poverty is measured prior to the start of the massacres and the war. There is no statistically significant difference in the death rate or the duration of the civil war in poor versus non-poor provinces. While more people were killed in poor provinces, the war actually lasted longer in the non-poor provinces. We conclude from this that there seems not to be selection into violence of the poorer provinces. It seems therefore unlikely that our exposure variable is picking up a wealth effect.

A second issue is potential bias caused by selective survival. As the survey by definition only has data on children who survived the violence up to the time of the survey, we need to account for potential survivor bias in the sample. More in particular, when death during the conflict was not a random event, we may over or underestimate the effects of the conflict on schooling, depending on the direction of the bias. The debate on the selectivity of violence in Burundi is ongoing. Recent empirical work (Bundervoet, 2009) on the one hand shows that parents of educated children were more likely to be killed in the 1993 massacre. On the other hand Voors et al (2010) show that the profile of migrants leaving conflict-affected areas is not much different from non-migrants, thereby reducing the potential bias based on survey data collected in those villages after the civil war.

The findings in Bundervoet (2009) mean that the effect of violent conflict on schooling was not limited to children who are at school-age during the conflict, but also affected those children who already completed their primary education. Education in times of conflict in Burundi has proven to be a liability. Following Bundervoet (2009)

this would mean that our non-affected cohort suffers survival bias in which the most educated cohort members are killed in 1993. This would also mean that on average this cohort was more educated than we infer from the survivors in the 2002 survey. Assuming there is no such survival bias for the affected cohort (as is likely because in 1993 they were too young to be targeted) the negative effect that we find for the affected cohort would be an underestimate of the true effect. We investigate the claim below.

Next to selective killing we may also face a problem of selective migration. If migrants have another profile than stayers, then we may over- or underestimate the impact of violent conflict on the stayers. We thus need to address two potential threats: (i) people killed in the 1993 massacres and the subsequent civil war may have had a different profile than survivors; (ii) people who have migrated since 1993 may have a different profile than those who did not migrate. The latter issue can be divided in three categories of migrants. (ii.a) Those who were internally displaced, (ii.b) those who were refugees and who returned to Burundi before 2002 and (ii.c) those who went abroad but did not return before 2002.

People in categories (ii.a) and (ii.b) are included in the sample. As mentioned above, one of the strengths of the survey design in 2002 was that it also surveyed people living in displacement camps. In this way the survey design captures the internally displaced and there is no danger for not being selected in the survey. People who fled abroad but returned before 2002 are also captured, because they are part of the target population at the time of the design of the survey. This means that only groups (i) and (ii.c) represent a potential selection problem.

The 2002 survey allows us to investigate the profile of people who were killed as well as that of migrants. Each surveyed household was asked to report on the death of a father, mother, husband, spouse or child and also report the cause of death (the latter not for husband or wives however). Each household was also asked to report the migration history of each member since the start of the conflict.

Table A2 in Appendix compares the profiles of households with and without at least one child that was killed in the 1993-2002 period. We do this for the loss of boys as well as girls. Table A3 does the same with widowed persons. We find that parents who lost at least one daughter in the violence were less educated compared to parents who did

not lose a daughter. Inferring from this that the killed daughters are more educated than the survived ones is premature given that the siblings (above age 15) of the deceased girls have a higher probability to complete primary education. The finding remains inconclusive for two other reasons: the low number of girls killed and the fact that we only dispose of the education data for siblings who still live in the parental home at the time of the survey. We do not find significant differences between the profiles of parents and siblings with and without at least one son killed. As to the death of husbands/spouses, we find a difference in the pre-war level of livestock ownership. Households where the husband died in the 1993-2002 period had significantly more livestock than households where the husband was alive at the time of the survey. Our findings are not at odds with those of Bundervoet (2009). *First*, he also finds a higher level of pre-war livestock among households with members killed and *second* his finding was based on the observation that fathers who were killed had more educated children, while we are interested in the level of education of the deceased children, not of the fathers. A large part of the latter (and thus of Bundervoet's assertion) are born before 1970, a cohort that is not relevant for this paper.

Given that we only computed the profiles of parents, siblings or husband/wives of people who were born between 1970 and 1987, and given that most of the findings in Tables A2 and A3 are not very conclusive or point in one clear direction, we conclude that selection bias caused by non-random killings is unlikely to bias our estimates in one or the other direction.

The same conclusion can be drawn from Table A4 for the case of the migrants. Since we do not have data on migrants who did not return to Burundi at the time of the survey we try to obtain a profile *by proxy*. The closest we can come to these long term refugees not registered in the 2002 survey is by taking the profile of those refugees who were abroad for several years and then returned to Burundi. As one can see from Table A4, compared to the stayers, these longer term refugees were slightly older, a little less female and had more educated heads of households. To the extent that the migrants who did not return have a comparable level of education as their heads of households (which is not necessarily the case, see Table A4) and given their age (23 on average), they would have increased the percentage that completed primary schooling in the non-affected

cohort (born between 1970 and 1981). This means that the estimates we find for the cohorts affected by the violence are an underestimate of the true effect.

6. Results

In table 6 we use the binary shock exposure variable that takes the value of 1 for children exposed to violent conflict in their province of residence during their school age years and 0 for non-exposed individuals. This regression in the first column controls for linear age effects and province fixed effects. We find that the coefficient of our variable of interest (exposure the violent conflict) is -0.89 which, in a logistic regression framework corresponds to an odds ratio of 0.40. This means that the odds to complete primary schooling are 60% lower for children exposed to violence. The second column of Table 6 adds the gender variable and interacts it with exposure to violent conflict. We find that the coefficient for exposure to violence decreases to -1.07 which corresponds to an odds ratio of 0.34. This means that the odds for boys who are exposed to violent conflict are 66% lower compared to non-exposed boys. The pure gender effect is negative: the coefficient of the gender variable is -0.52 which corresponds to an odds ratio of 0.59, meaning that the odds of girls to complete primary schooling are 41% lower than for boys. The coefficient of the interaction variable between gender and exposure to violent conflict is statistically insignificant, which means that there is no additional effect on primary school completion for girls exposed to the conflict. In order to calculate the decrease in the odds to complete primary schooling for girls who were exposed to violent conflict we take the linear combination of the estimates for exposure to violence and the gender*exposure interaction variable, yielding a coefficient of 0.82 [0.19] which is statistically significant at the 1% level. This corresponds to an odds ratio of 0.44 which means that the odds of girls exposed to conflict to complete primary schooling are 56% lower compared to girls not exposed to conflict.

Results remain the same in column three where livestock ownership before the start of the conflict is introduced as an additional regressor. In column four we consider that observations made for members of the same household or not independent from one another. We use household random effects to test for differences within and between

households. Girls are now worse off, but this is a pure gender effect, not an effect of exposure to conflict.

Using population weights and clustering at the province level in the regression to obtain robust standard errors does not change the results: the level of statistical significance of the estimators remains at 1%. We do not apply the latter two in the remaining regressions because STATA does not accept population weights in random effects regressions neither can we combine household random effects with clustering. For reasons of parsimony of presentation we do not present clustered regressions in Table 6.

In the first two columns of Table 7 we perform the analysis separately for poor and non-poor individuals. Boys and girls from poor as well as non-poor households suffer from conflict with the coefficient for poor girls (linear combination of exposure and gender*exposure) -0.89^{***} the same as for poor boys. This corresponds to an odds ratio of 0.40 meaning that the odds for girls and boys from poor households to complete primary schooling is 60% lower compared to non-exposed children from poor households. For children from non-poor households the gender difference is more pronounced. The coefficient for boys is -1.3 corresponding to an odds ratio of 0.27 meaning a 73% decrease in the odds to complete primary schooling. For girls, the gender-exposure interaction is positive and statistically significant, meaning that they do better than the boys. The effect of the conflict on the odds of girls from non-poor households can then be computed using a linear combination of the exposure and the interaction variable which yields an odds ratio of 0.56 and which means that the odds of the girls from non-poor households exposed to violent conflict to complete primary schooling are 44% lower compared to non-exposed girls from non-poor households. This means that between the four groups, the schooling of boys from non-poor households suffers the most from conflict exposure and the schooling of girls from non-poor households the least. The pure gender effect remains negative for poor as well as non-poor girls.

Moving to the years of exposure as our variable of interest in columns 3 and 4, the magnitude of the coefficients is about half compared to the binary case and the levels of statistical significance remain the same, for boys as well as for girls. Of course, the magnitude now has to be interpreted in a continuous way, not a binary way, meaning that the odds to complete primary schooling decline with 56% for every additional year of

exposure (column 3) and with 24% when we control for birth cohort fixed effects and gender specific time trend as well as household random effects (column 4). The estimates remain statistically significant at the 5% level for boys, but the linear combination of the exposure and the exposure*gender variable (a decline in the odds to complete primary schooling by 11%) is not any longer significant at conventional levels. The pure gender effect however is negative, its magnitude large and significant at the 1% level. This means that, after controlling for gender specific time trends as well household random effects (admittedly a severe test), only the schooling of boys seems to suffer from exposure to violent conflict. Dividing the sample in poor and non-poor households yields the result that only the schooling of boys from non-poor households is negatively affected by exposure to conflict at conventional levels of statistical significance (results not shown).

In Table 8 we limit the sample to sons and daughters living with their parents at the time of the survey. This allows us to control for additional household characteristics dating from just before the conflict. We control for the education, the gender and the age of the head of the household. In addition we test for the loss of at least one parent in 1993. Results for our variable of interest confirm the results of tables 4 and 5, with similar magnitudes depending on specification. We also find that the education of the head as well as a female head positively affects the probability to complete primary schooling. The death of one or both parents has no statistically significant effect. This may have to do with the policy to abolish school fees for orphans of the 1993 massacres. When we distinguish between poor and non-poor we find again that daughters from non-poor households seem to suffer the least and boys from non-poor households the most. Thus, while there is no difference between the magnitude of the effect of exposure for sons and daughters in poor households, it is twice as large for sons in non-poor households compared to daughters.

Exposure to violent conflict remains somewhat a broad term, defined at the province-birth cohort level. From such definition we cannot derive the exact channel by which the education of children at school age is affected during conflict. Possible channels are the destruction of school buildings or insecurity that makes parents keep children at home. One possible channel that affected almost one out of three households

in Burundi during the war was forced displacement. Our data allow us to test this channel in two ways. The survey has registered the number of times that each household member had to move residence because of the fighting and also the length of stay in a displacement camp. It seems plausible that both would have a negative effect on the probability of a child at school age to complete its primary schooling. Columns 1-3 in table 9 test these two channels. We find that it is the frequency of forced displacement that matters, not the length of stay in a displacement camp. Being uprooted from one's village because of ongoing or imminent violence proves to be disruptive for one's school career to the extent that it decreases the probability to complete primary schooling, in particular when it happens several times. The length of one's stay in a displacement camp is not statistically significant, which may have to do with the supply of schooling in such camps. When we test the effect of the three channels of violence (exposure to battles, forced displacement and duration of stay), it are the first two that exercise a negative and statistically significant effect on the completion of primary schooling. The odds to complete primary schooling decline by 16% for every additional year of exposure and with 21% for every instance of forced displacement.

Finally in column 4 of table 9 we perform of robustness check in which we have left out the 1978-1982 birth cohorts from the analysis. The argument can be made that we are not sure whether or not these birth cohorts are affected by the violence. One could for example argue that some pupils are still in primary school when they are 13, 14 or 15 years old, in which case the older birth cohorts would also be affected by the massacres and the civil war towards the end of their primary school career. If that would be the case then they would not constitute an adequate control group. Valente (2011) in her paper on the schooling consequences of the conflict in Nepal makes a similar argument to drop a few birth cohorts from her analysis. In column 4 of table 9 we thus infer the effects of violent conflict on affected cohorts where we are certain that the control group never experienced violence during their school careers and the treated group does. The result, with province fixed effects, gender specific time trends as well as household random effects is very similar to the one obtained in column 4 of table 7 (-0.23 versus -0.27).

7. Conclusion

Large scale massacres and civil war have a detrimental effect on the schooling of survivors. This means that, in addition the obvious negative effects of violent conflict such as the destruction of lives and infrastructure comes the long term negative effect under the form of diminished human capital. This moves a country into a lower development trajectory for the longer run.

There is no universal theory that allows us to predict the direction of the gender effect of violent conflict on schooling. In peace-time girls in Burundi are less likely to complete primary schooling compared to boys. This negative gender effect, irrespective of violent conflict, is a robust finding in all our specifications. The question whether or not there is an additional gender effect on schooling as a result of violent conflict depends on the specification. In most specifications we find that the schooling of boys as well as girls is negatively affected by the conflict. This means that there is no additional gender effect of the violence. The magnitude of the effect however is larger for boys. This is confirmed in a specification which includes gender and birth cohort fixed effects as well as gender specific time trends. Controlling for all these effects makes the gender*violence interaction term as well as the linear combination of the exposure to violence and the gender*violence interaction term statistically insignificant, meaning that we only observe an effect on schooling for boys. Thus is in accordance with the observations in Obura (2008) where she presents declining gross enrolment rates during the civil war, but a stable and even slightly increasing Gender Parity Index.

When the sample is split between poor and non-poor households, the results reported above become even more pronounced. Boys from poor as well as non-poor households suffer, but the magnitude of the loss is larger for boys from non-poor households. In summary, it seems that in rural Burundi, the schooling of boys from non-poor households seems to be affected most, followed by boys and girls from poor households (with an effect of about the same magnitude) and lastly, girls from non-poor households.

Policymakers should consider that conflict shocks have different distributional consequences than the better known economic or climatic shocks. Where price fluctuations or rain level variability is known to affect the poorest part of the population

much more than the non-poor part, this is not the case in the event of shocks of a political nature such as massacres or civil war. This paper demonstrates that groups considered to be the least vulnerable in the development economics literature – boys from non-poor households – are most affected by violent conflict.

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TABLES

Table 1: Education indicators and violent conflict

Indicator	1993	1996	Destruction/dysfunction	Re-gained pre-war level
Pupils	651086	426535	35% decline	1999
Gross enrolment rate	67%	42%	37% decline	2002
Net enrolment rate	50%	28%	44% decline	2003
Teachers	10165	8700	14% decline	1997
Schools	985	703	29% decline	1999
classrooms	9211	6548	29% not operational	1996

Source: Obura (2008) p.94-96

Table 2: Evolution of the Gross enrolment rate and the Gender Parity Index

year	GER	GPI
1981-1982	34	0.79
1984-1985	52	0.72
1989-1990	71	0.80
1992-1993	70	0.80
1995-1996	42	0.83
1996-1997	43	0.83
1997-1998	52	0.79
1998-1999	62	0.80
1999-2000	65	0.80
2000-2001	69	-
2001-2002	73	0.75
2002-2003	77	0.83

Source: Obura (2008) p.99

Table 3: Household, Individual and Conflict variables in the EDS data (UNFPA, 2002)

Name of the variable	Label	values
<i>Individual</i>		
Age	Age in years	16-31
Sex	Gender (Female=1)	0-1
Primary schooling	Completed 6 th grade (Yes=1)	0-1
<i>Household</i>		
Livestock in 1993	Pre-war level of livestock owned, expressed in Tropical Livestock units (tlu-	0-20
Education of head	Has the head of the household attended school? (Yes=1)	0-1
Age of the head	Age of the head of the household	20-87
Sex	Gender of the head of the household	0-1
Orphan	Lost at least one parent in 1993	0-1
<i>Conflict</i>		
Conflict Exposure	Number of years a school-aged child is exposed to conflict in its province of residence	0-4
Camp duration	Number of years school-aged child lived in a displacement camp	0-4
Forced displacement	Number of times a school-aged child had to move residence since 1993	0-8

Table 4: Individual and Household Characteristics, by Exposure to Violent Conflict, N=5856

Name of the variable	Not exposed to violent conflict (n=3586)	Exposed to violent conflict (n=2266)	Difference (2)-(1)
	(1)	(2)	(3)
<i>At the individual level</i>			
Age	25.1 [0.06]	17.8 [0.03]	-7.3***
Sex (% female)	60.6 [0.81]	56.5 [1.04]	-4.1***
Completed primary education	19.8 [0.66]	16.1 [0.77]	-3.7***
Number of years exposed to violent conflict	0 [0.00]	2.28 [0.02]	2.28***
Number of times moved residence	0.087 [0.01]	1.00 [0.02]	0.91***
Years spent in a displacement camp	0.015 [0.01]	0.89 [0.03]	0.87***
<i>At the household level</i>			
Livestock_1993	1.45 [0.55]	2.02 [0.84]	0.57***
One or both parents died in 1993	5.76 [0.38]	10.37 [0.63]	4.61***
Head educated (%)	35.56 [1.0]	45.19 [0.8]	9.63***
Sex of the head (% female)	22.00 [0.6]	38.90 [1.02]	16.90***
Age of the head	37.20 [0.23]	45.36 [0.28]	8.15***

Table 5: Primary education completed, by Province of residence and Exposure to Violent Conflict

Province of residence in 1993	Death rate 1993	Timing of the civil war 1995-1998	Primary Education (% completed)		Difference (5)-(4)
			Not exposed to violent conflict	exposed to violent conflict	
(1)	(2)	(3)	(4)	(5)	(6)
Bubanza	4.2	1995-1998	15.90 [3.9]	2.4 [2.4]	-13.47**
Bujumbura Rurale	5.4	1995-1998	26.20 [2.7]	28.64 [3.3]	2.43
Bururi	3.8	1995/1996	25.04 [1.7]	18.97 [2.4]	-6.06**
Cankuzo	2.5	not affected	16.36 [2.0]	-	
Cibitoke	4.9	1995-1998	8.60 [2.3]	6.94 [3.0]	-1.66
Gitega	21.9	1996/1997	32.81 [3.4]	28.50 [2.4]	-4.03
Karuzi	26.7	1995/1996	23.20 [3.8]	9.60 [2.6]	-13.60***
Kayanza	35.4	1995/1996	27.01 [3.0]	20.70 [2.4]	-6.30*
Mwaro	12.8	1996/1997	20.85 [3.2]	10.81 [2.6]	-10.04***
Makamba	1.7	1996-1998	9.70 [1.1]	8.38 [1.5]	-1.32
Kirundo	12.1	1996/1997	22.00 [3.4]	16.23 [3.0]	-5.76*
Muyinga	16.0	1995/1996	21.17 [3.5]	11.29 [2.4]	-9.86***
Muramvya	7.8	1996/1997	39.43 [5.8]	25.97 [2.4]	-13.46**
Ngozi	25.7	1995/1996	16.81 [2.5]	9.44 [1.9]	-7.37***
Rutana	5.3	not affected	9.9 [2.8]	-	
Ruyigi	6.7	1995/1996	19.05 [3.8]	25.00 [8.3]	5.95
Rural Burundi	7% (median)	2.28 years (average)	19.79 [0.7]	15.98 [0.8]	-3.81***

*** significant at 1%, ** at 5%, * 10%

Sources : (2) % of survey respondents whose father was killed in the 1993 massacres (Bundervoet, 2009); (3) spread of the civil war over time and space following Chrétien and Mukuri (2000), United Nations (1996) and Bundervoet et al (2009). We only take the 'relevant' duration into account, this is the period that school age children from birth cohorts 1981 to 1986 could have been exposed to the violence; (4) are birth cohorts not exposed to violent conflict (neither the massacres nor the civil war when they were between 7 and 12 years of age); (5) birth cohorts exposed to violent conflict (either the massacres or the civil war or both) when they were between 7 and 12 years of age.

Table 6: Baseline specification:
Logistic regression of Schooling on Gender and Conflict Exposure

Dependent variable:					
Child completed 6 years of primary schooling		All	All	All	All
		(1)	(2)	(3)	(4)
Violent Conflict Shock (binary)		-0.89*** [0.17]	-1.07*** [0.20]	-1.08** [0.20]	-1.02** [0.18]
Child is Female			-0.52*** [0.12]	-0.53*** [0.12]	-0.72*** [0.11]
Violent Conflict * Female			0.25 [0.11]	0.25 [0.20]	0.27 [0.18]
Age (in years)		-0.06* [0.016]	-0.07* [0.016]	-0.06 [0.016]	-0.03 [0.020]
Livestock_1993				0.05** [0.02]	0.08*** [0.013]
Intercept		-0.25*** [0.49]	0.09 [0.50]	-0.02 [0.50]	-1.29** [0.53]
Household Control variables (apart from livestock_1993)	No	No	No	No	No
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	No	No	No	No	No
Year of Birth*Female Fixed Effects	No	No	No	No	No
Household Random Effects	No	No	No	No	Yes
Sample Size		5852	5854	5706	5706
Wald Chi square test statistics		241.34***	255.28***	261.22***	233.17***

*** significant at 1%, ** at 5%, * 10%

Table 7: Fixed Effects and Random Effects estimation of Schooling, Gender, Poverty and Years of Conflict Exposure

Dependent variable: Child completed 6 years Of primary schooling	Poor only (1)	Non-poor only (2)	Years exposed (3)	Years Exposed (4)
Violent Conflict Shock (binary)	-0.90*** [0.23]	-1.3** [0.31]		
Years of violent conflict exposure (continuous)			-0.57*** [0.079]	-0.27** [0.11]
Child is Female	-0.65*** [0.13]	-0.94*** [0.24]	-0.76*** [0.10]	-1.1*** [0.42]
Violent Conflict * Female	0.01 [0.23]	0.72** [0.30]	0.19** [0.08]	0.15 [0.123]
Age (in years)	-0.03 [0.02]	-0.03 [0.03]	-0.05*** [0.015]	
Livestock_1993	0.17 [0.13]	0.05*** [0.15]	0.07*** [0.013]	0.08*** [0.013]
Intercept	-1.48** [0.64]	-1.05 [0.99]	-0.95* [0.52]	-2.7*** [0.54]
Household Control variables (apart from livestock_1993)	No	No	No	No
Province Fixed Effects	Yes	Yes	Yes	Yes
Year of Birth FE	No	No	No	Yes
Year of Birth*Female Fixed Effects	No	No	No	Yes
Household Random Effects	Yes	Yes	Yes	Yes
Sample Size	3998	1708	5706	5706
Chi square test statistics	161.55***	71.49***	246.36***	305.48***

*** significant at 1%, ** at 5%, * 10%

Table 8: Fixed Effects and Random Effects estimation of Schooling, Gender, Poverty and Conflict Exposure, *only sons and daughters living at home*

Dependent variable: Child completed 6 years of primary schooling	All (1)	All (2)	Poor only (3)	Non-poor only (4)
Years of violent conflict exposure	-0.45*** [0.11]	-0.45*** [0.11]	-0.36** [0.15]	-0.58*** [0.17]
Child is Female	-0.65** [0.18]	-0.65** [0.19]	-0.49** [0.25]	-0.82*** [0.27]
Violent Conflict * Female	0.15 [0.10]	0.15 [0.10]	-0.04 [0.14]	0.28* [0.16]
Age (in years)	0.06** [0.03]	0.06** [0.03]	0.006* [0.04]	0.05 [0.04]
One or both parents died in 1993		0.23 [0.31]	0.13 [0.40]	0.17 [0.48]
Livestock_1993	0.07*** [0.016]	0.07*** [0.016]		
Household Head Is Educated	0.44*** [0.16]	0.43*** [0.16]	0.42* [0.22]	0.40* [0.24]
Household Head is Female	0.43*** [0.16]	0.40** [0.17]	0.60*** [0.21]	0.27 [0.26]
Household Head's Age	0.004 [0.009]	0.004 [0.009]	0.001 [0.01]	0.008 [0.01]
Intercept	-3.82*** [0.94]	-3.83*** [0.95]	-3.55*** [1.18]	-4.27** [1.68]
Province Fixed Effects	Yes	Yes	Yes	Yes
Year of Birth FE	No	No	No	No
Year of Birth*Female FE	No	No	No	No
Household Random Effects	Yes	Yes	Yes	Yes
Sample Size	2639	2639	1541	1098
Chi square test statistics	145.77***	146.04***	95.99***	62.90***

*** significant at 1%, ** at 5%, * 10%

Table 9: Fixed Effects and Random Effects estimation of Schooling, Gender and Conflict Exposure, *alternative conflict shock measures*

Dependent variable: Child completed 6 years of primary schooling	alternative conflict shock measures			
	All (1)	All (2)	All (3)	1978-82 cohort excluded (4)
Years of violent conflict exposure			-0.17** [0.09]	-0.23** [0.13]
Number of times moved residence	-0.19* [0.10]		-0.24*** [0.08]	
Number of years spent in a displacement camp		0.01 [0.07]	0.08 [0.05]	
Child is Female	-1.13*** [0.43]	-2.37*** [0.76]	-2.37*** [0.76]	-2.24*** [0.77]
Violent Conflict *Female				0.18 [0.16]
Times Moved *Female	-0.046 [0.15]			
Years in Camp *Female		0.02 [0.10]		
Livestock_1993	0.08*** [0.013]	0.08*** [0.013]	0.08*** [0.013]	0.08*** [0.017]
Intercept	-1.53*** [0.49]	-1.53*** [0.49]	-2.68*** [0.53]	-3.4*** [0.66]
Province Fixed Effects	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes
Year of Birth* Gender FE	Yes	Yes	No	Yes
Household Random Effects	Yes	Yes	Yes	Yes
Sample Size	5706	5706	5706	3981
Chi square test statistics	306.09***	303.26***	308.04***	213.05***

*** significant at 1%, ** at 5%, * 10%

Figure 1: spread of the civil war over space

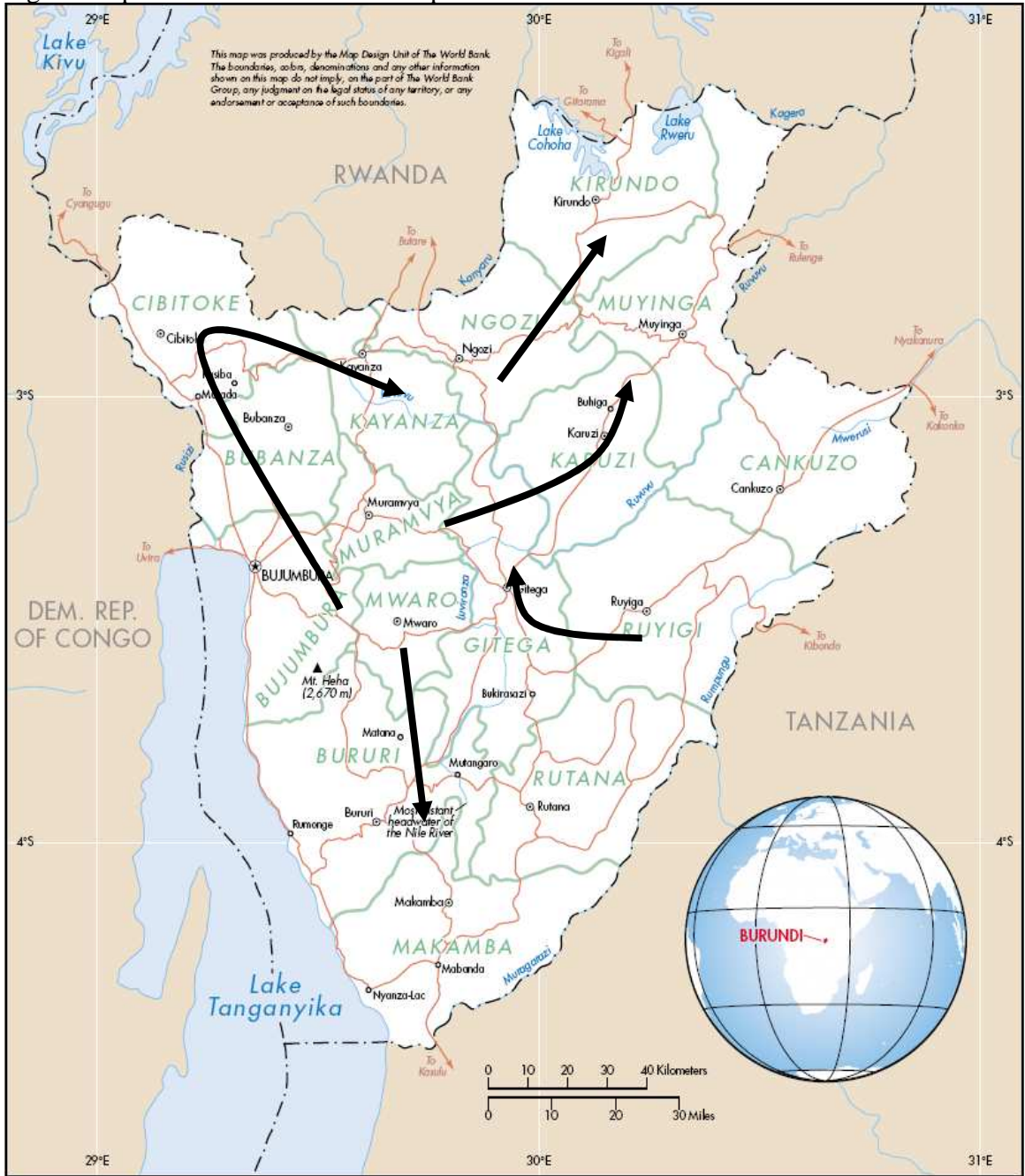
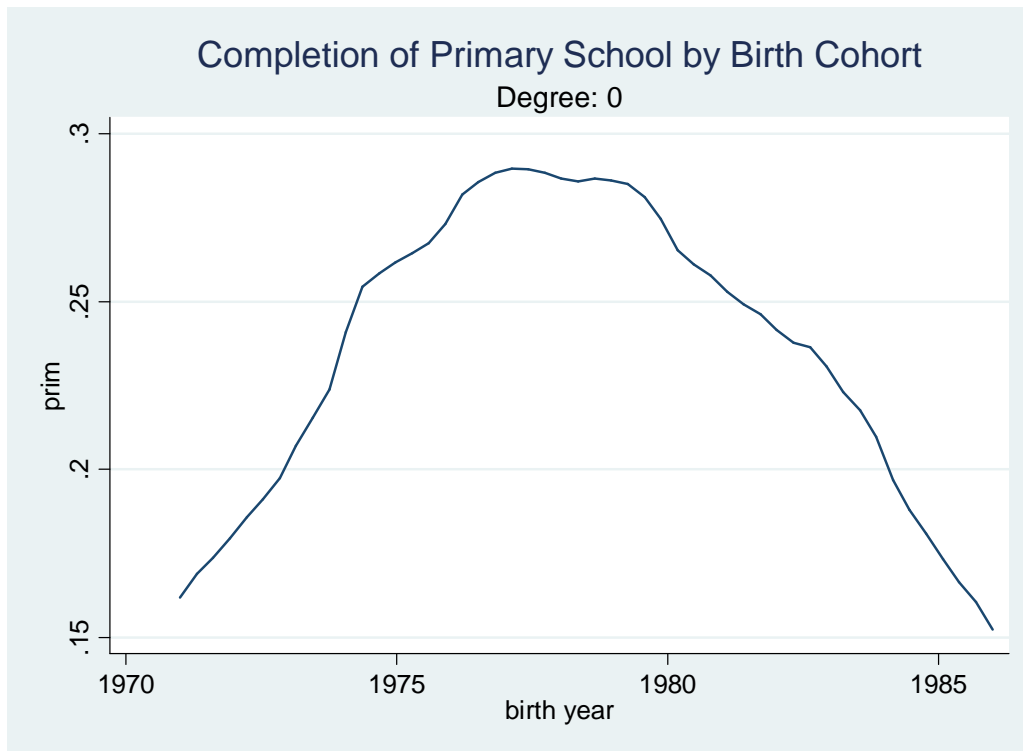
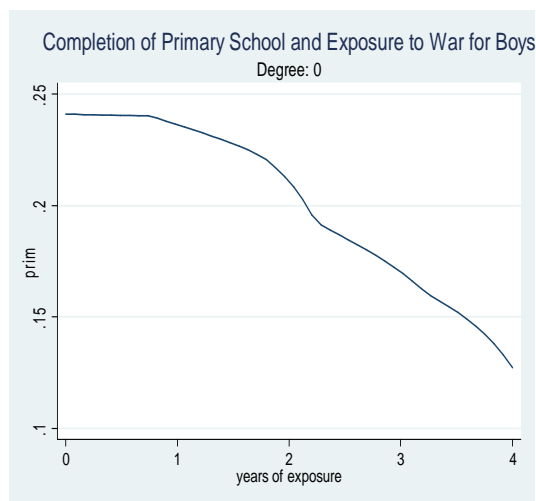


Figure 2: Primary school completion by birth cohort

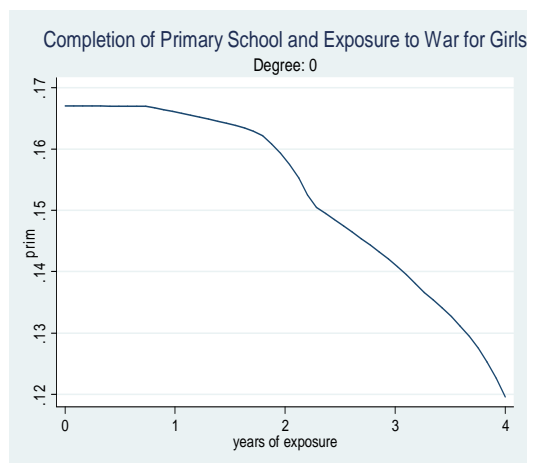


Figures 3 and 4: Primary school completion and years of exposure to conflict

For men



For women



Appendix: Tables for potential selection problems

Table A1 Poverty and Conflict in poor and non-poor provinces

	Poor provinces	Non-poor provinces	Difference (means test)
Death rate 1993	15.0	9.7	-5.3
Duration of civil war (in years)	1.85	2.44	0.59

*** significant at 1%, ** at 5%, * 10%

Notes: a province is poor when its headcount poverty ratio is larger than the average in rural Burundi. Data are from the 1986-1990 expenditures survey (ECB).

The death rate is the % of persons in the 2002 UNFPA survey who lost their father in 1993 (based on Bundervoet, 2009) and the duration of the civil war is based on Chrétien and Mukuri (2000), United Nations (1996) and Bundervoet et al (2009).

Table A2: Sons and daughters born in 1970-1987 who died violently in 1993-2002

	Households with no violent death 1993-2002		Households with at least one violent death 1993-2002		Difference (3)-(1) and (4)-(2)	
	(1) boys	(2) girls	(3) boys	(4) girls	(5) boys	(6) girls
Head of household completed prim education	0.34	0.34	0.29	0.20	-0.05	-0.14**
mother completed prim education	0.07	0.07	0.05	0	-0.02	-0.07**
Same-sex siblings completed prim education	0.23	0.17	0.21	0.25	-0.02	+0.08
Livestock 1993	2.05	2.0	2.12	2.1	0.07	0.1
N	1278	1420	55	35		

Table A3: Widowed persons born in 1970-1987 who lost their husband/wife 1993-2002

	Households with no death 1993-2002		Households with at least one death 1993-2002		Difference (3)-(1) and (4)-(2)	
	(1) husband alive	(2) wife alive	(3) husband died	(4) wife died	(5) husbands	(6) wives
Wife completed prim education	0.16	0.19	0.18	0.26	0.02	0.07
Livestock 1993	0.96	0.99	1.73	0.59	0.77***	-0.40
N	1547	1544	85	19		

Note: Correlation coefficient between level of education of both partners in a married couple is 0.48***

Table A4: 1970-1987 cohorts who migrated abroad after 1993 and returned before 2002

	(1)	(2)	(3)	(4)
	Never moved	Moved abroad and returned before 2002	Moved abroad for at least 4 years and returned before 2002	Difference (3)-(1)
Age	22.35	22.57	22.85	0.50*
% female	0.59	0.53	0.52	-0.07**
Livestock_93	1.72	1.79	1.62	-0.10
Head of household educated (sons and daughters only)	0.37	0.46	0.58	0.20***
N	4514	1169	173	