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Schooling, Violent Conflict and Gender in Burundi[1]

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Abstract: We investigate the effect of exposure to violent conflict on human capital accumulation in Burundi. We combine a nationwide household survey with secondary sources on the location and timing of the conflict. Only 20% of the birth cohorts studied (1971-1986) completed primary education. Depending on specification we find that the probability to complete primary schooling for a boy exposed to violent conflict declined by 6 to 16 percentage points compared to a non-exposed boy. We also find that exposure to violent conflict reduces the gender-gap in schooling, but only for girls from non-poor households. Forced displacement is found to be one of the channels through which the impact of conflict on schooling is felt. Our results are robust to various specifications and estimation methods.

Key words: *schooling, violent conflict, gender, Africa, Burundi*

JEL codes: *012, I21, J16*

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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). Economists have studied 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 long-term economic consequences are particularly much debated in the literature. Davis and Weinstein (2002) for Japan, Brakman, Garretsen, and Schramm (2004) for Germany, Miguel and Roland (2006) for Vietnam, and Bellows and Miguel (2006) for Sierra Leone find rapid economic recovery after 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.

The relatively fast recovery of economic growth and other macro-level indicators does not tell us much about the distribution of long-term consequences at the micro-level. This paper considers the consequences of civil war for human capital accumulation at the individual level. Gender differences are a critical source of heterogeneity in this respect but the direction of the gender effect is an empirical question. When, for example, the conflicting parties engage child soldiers, it is likely that boys are more affected than girls. Existing gender inequalities may be exacerbated during violent conflict, but they may also be attenuated. When a country needs the brains and work of young women to work in the military industry during a dispute with a neighbor, for example, the labor market position of women may benefit from that conflict. There is no universal rule to predict what the gender specific impact will be. 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.

This paper focuses on the effect of civil war on schooling in Burundi. 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 men and women, 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 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 as well as the history of migration 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.

We find that the completion of primary schooling in Burundi is affected by the massacres and the subsequent civil war. For every year that a school-aged boy was exposed to conflict in his province of residence, the probability to complete primary schooling decreases by 3 percentage points per year of exposure. Boys from poor as well as non-poor households loose out from war. Girls suffer a general schooling disadvantage in Burundi but we find that violent conflict reduces the gender gap, although this only holds for girls from non-poor households. We show that forced displacement is one of the channels through which violent conflict affects schooling.

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 mechanisms 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 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, Marxist or Cultural Revolution type 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 on violent conflict 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 male Rwandan children in non-poor households incur 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.

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 León (2011) for Peru; Angrist and Kugler (2008) and Rodriguez and Sanchez (2009) for Colombia; Chamarbagwala and Morán (2009) for Guatemala, de Walque (2006) for Cambodia.

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 (Justino, 2011). 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. Their 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.

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) and Shemyakina (2006) 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) 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. We add that not only the young generation is prevented from acquiring human capital, educated members of older cohorts may be disproportionably 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 socio-economic 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). 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).

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. In 1972, a Hutu insurgency started in southwestern Burundi resulting in considerable loss of life among the Tutsi residents. The subsequent Tutsi army repression eliminated all educated Hutu (Lemarchand 1994).

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. The democratic transition did not last very long. 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 rural provinces, Hutu peasants committed large-scale massacres of Tutsi and Hutu supporting Uprona. Chrétien (1997) writes that districts in certain provinces were "almost completely 'cleansed' of all Tutsi elements." The Tutsi army retaliated against Hutu, continuing what would become the most severe civil war in Burundi's history, both in terms of human lives and socioeconomic destruction. (Ndikumana 2000).

3.1 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. As the exact timing and location of the massacres and the civil war plays an important role for our identification strategy (see section 4), 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
- 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 lost their father in 1993. Table 2 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. 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 spatial spread was influenced by geography and natural endowments: (i) the proximity of the Democratic Republic of Congo's North Kivu region where the rebels had a base, explaining the early onset of war in the provinces of Cibitoke, Bubanza, and Bujumbura Rural; (ii) the presence of the Kibira forest in the north, which also served as a rebel base, explaining the spread of war to Kayanza and Ngozi provinces and (iii) the

Tanganyika Lake which allowed the use of boats to bring the war to the southern province of Makamba.

3.2 Civilian Impacts of the Conflict

Between 1994 and 2001, an estimated 200,000 people lost their lives, a majority of them civilians (UNFPA 2002). To understand the micro-level impact of the war, we focus on displacement, looting of household assets, and the theft and burning of crops.[4]

First, in its 2002 Demographic and Health Survey, the United Nations Population Fund (UNFPA) finds that over 50 percent of the rural population had been displaced from their homes at least once between 1993 and 2000 due to violence. 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. 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 (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.

When conflict ended in a given province, displaced households 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

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

foreign NGO workers and journalists who left Bujumbura to visit war regions. 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 capital stock. 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).

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 who had less income to pay for other expenditures, including purchasing food crops, school fees or health care.

And *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. Gender roles became more entrenched as boys and men were drafted by the army are recruited by rebel movements.

3.3 Education and Conflict in Burundi

Access to education has been a long-standing source of inequality, tension and conflict. In the cohorts under study only 20% completed primary schooling. Education is directly related to jobs in the public sector for which degree holders have the monopoly. The education system together with jobs in the administration was dominated by Tutsi from the southern region of Bururi. Nkurunziza and Ngaruko (2002) write that in 1972 almost all educated Hutu 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 (Ministère de l'Education, 1999). The conflict-induced trend reversal can be observed from Figure 1 which we computed with the UNFPA 2002 data: the birth cohorts who could finish primary schooling before the start of the conflict show an upward trend in primary school completion, from a rate of less than 0.20 to almost 0.30. The cohorts born between 1975 and 1980 show the highest primary school completion rates in the history of Burundi (up to 2005). 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 officially attend primary school from age 7 to 12 when they finish 6^{th} grade (UNESCO 2011). Some children may start schooling later and complete primary schooling at later age.

Figure 2 gives the key variables of our empirical approach (see below): girls fare worse than boys, children from poor households fare worse compared to children from non-poor households and exposure to conflict negatively affects completion rates. Poverty at the household level is defined by livestock ownership before the start of the massacres and the civil war. This variable is the only pre-conflict wealth indicator available in the UNFPA survey (see below) and was registered through a recall question. Livestock ownership is one of the most important manifestations of wealth in rural Burundi.

The interaction of gender, poverty and exposure in Figure 2 offers surprising insights: the completion rate for girls from non-poor households exposed to conflict is almost the same as for boys exposed to conflict, while it is widely different for non-exposed boys and girls from non-poor households. Moreover, we do not find this gender-gap reducing effect of conflict on schooling for children in poor households.

In an extensive review of the damage done to the education sector during the conflict in Burundi, Obura (2008, p.94-96 and p.99) 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 from 0.80 to 0.83. Obura also remarks that a church-led education

initiative, called Yaga Mukama, which 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 Identification Strategy

In 2002, UNFPA collected demographic and health data (EDS) on almost 7,000 households. Descriptive data are presented in tables 1 and 2. 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.

4.1 Conflict variables

We construct four conflict exposure variables: the first two are general indicators of exposure to violent conflict and the last two are variables that represent potential impact channels of conflict on education. We determined the number of years that a child was exposed to violent conflict during its primary school ages. This variable is based on the combination of year of birth and province of residence at the onset of conflict. We cannot exclude that children moved to a more peaceful province after the onset of war in their province of residence. In that case we would overestimate the duration of exposure, yielding a conservative estimate of the effect of conflict on schooling. However, UNFPA (2012, p.141) writes that most conflict-induced migration happened locally, within the same province. In addition, we also test for individual exposure channels such as the frequency of forced displacement and the time spent in a displacement camp which is not measured at the level of the province of residence.

Many children born in the 1981-1986 period have experienced at least one year of conflict during their primary school career. The oldest ones, born in 1981, were about to graduate from primary school when the conflict started. Depending on the province of residence, younger children experienced none, some or a lot of violent conflict during their school ages. Not all provinces were affected at the same time. In principle, the

maximum duration of exposure to conflict during primary school age is six years. In practice, we do not find children exposed for more than four years in our sample.

In order to determine which provinces were affected by the massacres (in 1993-1994) and the civil war (1995-1998), we use two sources.[5] For the massacres we rely on Bundervoet (2009) who computed the percentage of people 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 2) we distinguish between eight provinces with a death rate higher than the median death rate (7%) and eight provinces with a lower death rate. The eight provinces with a lower than median death rate are defined as non-affected provinces, the eight other ones are defined as affected provinces. For exposure to the civil war, we rely on Chrétien and Mukuri (2000) who describe the spread of the violence over space and time. A child residing in a province engulfed by civil war during its primary school age is defined as exposed to civil war. We combine the exposure to massacres and to the subsequent civil war in one's province of residence during school age into one exposure variable. This variable gives us the number of years during the child's primary school years that the child was exposed to violence.

The two other exposure-to-conflict variables are constructed directly from the UNFPA survey to index channels of influence of conflict on education. One is the number of times the child had to move residence forcibly during the massacres and the civil war. The other one is the number of years the child spent in a displacement camp, again during primary school-age.

4.2 Identification Strategy

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 province of residence during their school-aged years with children of the same age residing in provinces that were not much affected, as well as with children who were old

[5] While the civil war lasted longer (till 2005), we only consider the relevant part of the war for our identification strategy, to wit the potential exposure of children in primary school age from the birth cohorts under consideration (1971-1986)

enough to finish their schooling before the conflict started in both affected and not affected provinces. Building on Figure 2 and previous tabulations, our baseline specification is a linear probability model of the form:

$$Schooling_{ijt} = \alpha_j + \delta_t + \beta_1(Exposure_{jt}) + \gamma Z_i + \varepsilon_{ijt} \quad (1)$$

where *Schooling* is our binary education variable for having completed primary school or not, measured for a child *i* residing in province *j* and born at time *t*. With α_j the province fixed effects, δ_t the birth cohort fixed effects and ε_{ijt} a random error term. The latter term has an individual and a household level component. We calculate the $Exposur_{jt}$ variable first as a binary measure to indicate a child residing in a province *j* that experienced violent conflict at the time when birth cohort *t* was at primary school age. 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. The provincial fixed effects control for any unobserved effect that does not change over time. In order to capture potential trends at the province level we estimate a specification with province level time trends. In section 6 we will also test whether or not our variable of interest is picking up pre-war province level trends correlated with the duration of conflict.

In some specifications we control for 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. *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. We do not include the Z variables in all specifications as by 1993 the older cohorts had already left their parental household and thus these variables cannot impact their school completion.

We cluster our standard errors at the province level to control for intra-province correlations (Bertrand et al, 2004). Clustering should occur at the province level since our shock is coded at this level. We face a problem of low number of clusters (Cameron et al,

2008) leading to larger standard errors and coefficients that are imprecisely estimated. An argument can be made that our shock is actually coded at the province-birth year level. The latter increases the number of clusters to 256 resulting in precise estimates. Results of this alternative are discussed in section 6 and presented in the Appendix, Table A1.

As stated in section 3.1 the spatial onset and subsequent spread of the war was determined by the proximity of a province to the border with DR Congo, the Kibira forest and/or the Tanganyika Lake. These factors are exogenous to the level of education of other household characteristics. Voors et al (2012), do not find evidence of the endogeneity of education (and other household characteristics) and exposure to violence at the household and village level. While they cannot exclude occurrences of targeted violence, they write that "the probability of incorrectly maintaining the null of non-targeted violence is acceptably small" (p.950).

Furthermore, as the impact of conflict may differ according to the age at which the impact is felt we will also account for age-specific onset of conflict in a separate set of regressions. In this case, the coefficient of the variable of interest indicates the effect of the onset of conflict in the province of residence at a given age on the probability to complete primary schooling.

As we are also interested in a potential gendered effect of the impact of the civil war on human capital accumulation, we estimate the following specification

$$Schooling_{ijt} = \alpha_j + \delta_t + \beta_1(Exposure_{jt}) + \beta_2 S_i + \beta_3(S_i * Exposure_{jt}) + \gamma Z_i + \varepsilon_{ijt} \quad (2)$$

whereby S_i is the sex of the child (S_i=1 for girls) and the other variables are as in specification (1). In this specification β_1 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.

The above specifications do not specify the mechanism through which the impact of the conflict is channeled, it only provides a generic 'exposure to civil war variable', either in binary form, in number of years of exposure or in age-specific onset. In order to

go deeper into particular channels we develop other specifications where we use alternative measures of conflict indicating a specific mechanism.

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

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.

5. Findings

In table 3 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. The regressions in columns 1-6 are linear probability models (LPM). In the first column we control for a linear time trend and province fixed effects and find that the coefficient of our variable of interest (exposure the violent conflict) is -0.16 which means that the probability to complete primary schooling is 16 percentage points lower for children exposed to violence. Girls have a lower probability to complete primary schooling but there is a small, positive coefficient for the female*conflict exposure interaction variable. Pre-conflict wealth, measured as livestock holdings increases the probability to complete primary school. This basic result does not change when, in the next few columns we introduce year of birth cohort fixed effects, province specific time trends and household random effects. The coefficient of exposure to violent conflict however becomes smaller and reduces to -0.06 in column 4. Columns 5 and 6 repeat the regression of column 4 by wealth groups. It shows that the exposure to violent conflict affects the probability to complete primary schooling for boys from poor as well as non-poor households in a negative way. While the magnitude of the effect is larger for boys from non-poor households, a test for the equality of both coefficients shows that they should be regarded as equal (Chi Squared (df=2) = 3.32, p=0.19).

The linear interaction of the female and female*conflict dummy in poor households yields a coefficient of -0.06 (= -0.07+0.01), statistically significant at the 1% level, meaning that the gender-gap in schooling has diminished only a little bit. For girls

from non-poor households the effect is larger: the linear combination of the female and the female*exposure variables yields a coefficient of -0.03 (= -0.13+0.10), statistically insignificant at the usual thresholds. This means that the gender-gap in schooling between girls and boys from non-poor households who are exposed to violent conflict is not statistically significant. The Chi Squared test for the equality of the coefficients of interest for girls from poor and non-poor households confirms the above finding ((df=2) 8.64**, p=0.013), meaning that both coefficients are not equal. This result corroborates the intuition behind Figure 2 where boys and girls from non-poor households exposed to conflict have virtually similar completion rates. In sum: in non-poor households, school completion rates for boys go down to the level of girls, so that the gender gap almost disappears. In contrast: in poor households, where school completion rates are lower anyway, girls are just as much affected as boys, so that the gender gap persists.

When we perform a logistic year-of-birth and province fixed effects regression with and without household random effects in columns 7 and 8 we find a coefficient of interest of respectively -0.60 and -0.45. This means that the odds to finish primary schooling for boys decrease substantially because of conflict exposure. As in the previous specifications, conflict exposure diminishes the gender-gap in the completion of primary schooling.

Moving on to *years of exposure* as our variable of interest in table 4, we find that the magnitude of the coefficients is about half compared to column 4 in the binary case. Every additional year of exposure to violent conflict reduces the probability to complete primary schooling by 3 percentage points (column 1), and by 5 percentage points for boys from non-poor households (column 3). As in table 3, exposure to conflict diminishes the gender-gap in primary school completion. Exposure to conflict seems to have no negative effect on school completion for girls of non-poor households.

As the exposure to shocks may have a different impact according to the age at which the child was exposed, we regress our outcome variable on a series of age-specific shocks. In table 5, we determined for each child the age at which it experienced the onset of violent conflict in its province of residence. And we interact these dummy variables with the female variable. We find that the first four years (ages 7 to 10) are crucial, in declining order of magnitude, rather than the last two (ages 11 and 12). For boys of poor

households however, only the onset of conflict at age 7 seems to matter in a statistically significant sense. The probability of the complete primary schooling for boys from non-poor households decreases by 28, 25, 19 and 13 percentage points respectively for the onset of conflict at ages 7, 8, 9 and 10. Table 5 makes clear that a cohort of children in Burundi was particularly affected by exposure to violent conflict when the conflict started in the province of residence right at the moment that the cohort should have started their primary school career. A child who had yet to start school or had only completed one or two years of schooling at the start of the conflict may have been compelled to give up school all together. The gender-gap in primary school completion however is reduced when conflict starts at age 7 or 8 in the province of residence of the child. Children confronted with conflict for the first time at ages 11 or 12 are probably more likely to make-up for potential lost months or years of schooling because there parents may believe that the degree is within reach. The crux for Burundi seems to be that the above reasoning applies for boys as well as girls, but exposure at young ages hurts the schooling of boys more than of girls, in particular for children from non-poor households.

In Table 6 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 table 3, with similar magnitudes for the coefficients in corresponding specifications. We also find that the education of the head as well as a female head positively affects the probability to complete primary schooling, in particular for children in poor households. 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.

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 7 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 proofs 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 effect of length of 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 probability to complete primary schooling declines by 3 percentage points for every additional year of exposure and with 2 percentage points for every instance of forced displacement. The magnitude of these effects is somewhat larger for children of non-poor households and the effects are not statistically significant for children from poor households.

In columns 7 and 8 we perform robustness check in which we have left out the 1978-1982 and 1971-1974 birth cohorts respectively from the analysis. For the 1978-1982 cohorts the argument can be made that we are not sure whether or not these birth cohorts are affected by the violence. Some pupils may still be in primary school when they are 13 to 16 years old, in which case these older birth cohorts would also be affected by the massacres and the civil war towards the end of their primary school career and 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 7 of table 7 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, birth cohort fixed effects as well as household random effects is very similar to the one obtained earlier. For the 1971-1974 cohorts, the argument can be made

that these cohorts are rather old which may lead to a bias in the estimation of the time trend if the slope changes a lot over time. However, leaving these cohorts out of the estimation leads to similar results as before.

6. Issues of concern to the identification strategy: poverty, pre-war trend, low number of clusters, selective survival and selective migration

A *first* issue of concern for our identification strategy is that we may measure the effect of something else than exposure to violent conflict. If, for example, massacres were more intense or the civil war lasted longer in poor provinces compared to non-poor provinces, then we may be measuring the effect of poverty in stead of exposure to violent conflict. 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, we analyzed data on the death rate in 1993 and the duration of the civil war in poor and non-poor provinces. Poverty is measured as the percentage of the population under the poverty line in 1990 (prior to the start of the massacres and the civil war). A province is defined as poor if the percentage of the population under the poverty line is higher than 36.2%, the poverty headcount in Burundi in 1990. The difference in the 1993 death rate between poor and non-poor provinces is -5.3 percentage points but it is not statistically significant at the usual thresholds. Similarly, the difference in the duration of the civil war between poor and non-poor provinces is 6 months, but it is not statistically significant. In addition, Bundervoet et al (2009) find very few correlations between the timing of conflict onset (no, early or late) at the province level, the length of exposure to conflict at the individual level on the one hand and a range of household characteristics on the other hand. We conclude from this that there seems to be no selection into violence of provinces or individuals based on pre-war characteristics. It seems therefore unlikely that our exposure variable is picking up a wealth or other effect.

Secondly, while we include province fixed effects in our specifications to control for time-invariant province characteristics, there could still be a problem of endogeneity with time-varying province characteristics. In order to test for that we analyse the potential correlation between a pre-war province-level trend in primary school

completion rates and the duration of conflict in that province. To that effect we compute a pre-war trend, defined as the difference between the average school completion rate of the three oldest (1971-1973) and the three youngest (1978-1980) pre-war cohorts, and regress this trend on the duration of violent conflict in the province. We do not find any statistically significant effect in various specifications using the usual thresholds. Results are in the Appendix, Table A2. We remark that the specification in Table 3, column 2 also includes a province-specific time-trend.

Thirdly, standard errors for our variable of interest are typically ½ to ¼ of the magnitude of the coefficient of interest, except in the specification for the poor households where the standard errors are as large as the coefficients of interest. Our coefficients are imprecisely estimated because of the low number of clusters. As a robustness check we cluster standard errors at the province-year of birth level (as outlined in section 4.2). This increases our number of clusters to 256. Results are presented in the Appendix, Table A1. Coefficients are the same, standard errors are very small and statistical significance has increased. These coefficients are thus very precisely estimated. And the results are very similar as in the case of province level clustering of standard errors, giving confidence to our previous results. However, we present these results in the appendix rather than in the main text because the intra-province year of birth clusters may be correlated with one another.

A *fourth* 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 (Bundervoet 2009; Voors et al 2012). 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 who already completed their primary education. Education in times of conflict in Burundi has proven to be a liability. This would mean that our non-affected cohort (1971-1980, in our approach not affected during their primary school career) 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 the 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 then be an underestimate of the true effect. We investigate this claim together with the next issue.

Fifth, 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/refugees. (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.

Persons 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 at the time of 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. We compared the profiles of households with and without at least one child killed in the 1993-2002 period (available in the Appendix, Table A3.). We do this for the loss of boys as well as girls. And we do 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 loose 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 *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.

These 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 deceased fathers. A large part of the latter (and thus of Bundervoet's assertion) are born before 1971, 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 where born in the 1971-1986 period, and given that most of the above findings 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.

We draw the same conclusion for the case of the migrants/refugees. Since we do not have data on the people who did not return to Burundi at the time of the survey we try to obtain a profile *by proxy*. The closest we can get to the long term refugees not registered in the 2002 survey is to consider the profile of those refugees who were abroad for many years and then returned to Burundi. From the figures (Table A3.3), and comparing with the stayers, these long-term refugees were slightly older, had somewhat fewer female and more educated heads of households. Had they returned, it would have increased the percentage that completed primary schooling *in the non-affected cohort* (born 1971-1980). In that case the estimates we find for the cohorts affected by the violence can be considered an underestimate of the true effect.

7. Conclusion

There is no universal theory that allows us to predict the direction of the gender effect of violent conflict on schooling. In time of peace, 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. But is there an additional

gender effect on schooling as a result of violent conflict? We find that the schooling of boys is negatively affected by the conflict. For girls, we find that exposure to violent conflict reduces the gender-gap in schooling, but only for girls from non-poor households. This is confirmed across specifications. This finding 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. Exposure to violent conflict did not affect the gender-gap in schooling between boys and girls from poor households.

Our findings, the losses in terms of schooling as well as the narrowing of the gender-gap, do not necessarily apply to other settings. Justino (2011) observes that the micro-level effects depend on the type of conflict and the socio-economic profile of the victims. The magnitude of the observed effect in Burundi, a decline in the probability to complete primary schooling of 3 percentage points per year of exposure cannot be compared straightforwardly with findings in papers using other dependent or independent variables. In her overview, Justino (2011) mentions a range of 0.4 to 1.2 years of education lost because of violent conflict. The magnitude of the effect in Shemyakina (2006), between 4 and 7 percentage points lower probability to complete the mandatory nine years in Tajikistan, is somewhat lower than our range of estimates of 6 to 16 for the exposed versus the non-exposed.

Policymakers should consider that conflict shocks may have different distributional consequences than the well-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 necessarily 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 in general and boys from non-poor households in particular – are severely affected by violent conflict. At the same time, girls from non-poor households were able to close the gender-gap in schooling during the conflict.

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TABLES

Table 1: Individual and Household Characteristics, by Exposure to Violent Conflict, N=5856

Name of the variable	Values	Not exposed to violent conflict (n=3586) (+)	Exposed to violent conflict (n=2266) (+)	t-test on the means (2)-(1)
		(1)	(2)	(3)
At the individual level				
Age	16-31	25.1 [0.06] [3.79]	17.8 [0.03] [1.51]	-7.3***
Sex (% female)	0-1	60.6 [0.81] [0.48]	56.5 [1.04] [0.49]	-4.1***
Completed primary education	0-1	19.8 [0.66] [0.40]	16.1 [0.77] [0.38]	-3.7***
N. of years exposed to violent conflict	0-4	0 [0.00] [0.00]	2.28 [0.02] [0.94]	2.28***
Number of times moved residence	0-4	0.087 [0.01] [0.39]	1.00 [0.02] [1.03]	0.91***
Years spent in a displacement camp	0-8	0.015 [0.01] [0.18]	0.89 [0.03] [1.66]	0.87***
At the household level				
Livestock_1993	0-20	1.45 [0.55] [4.76]	2.02 [0.84] [5.76]	0.57***
One or both parents died in 1993	0-1	5.76 [0.38] [0.23]	10.37 [0.63] [0.30]	4.61***
Head educated	0-1	35.56 [1.0] [0.50]	45.19 [0.8] [0.48]	9.63***
Sex of the head	0-1	22.00 [0.6] [0.41]	38.90 [1.02] [0.49]	16.90***
Age of the head	20-87	37.20 [0.23] [13.93]	45.36 [0.28] [13.41]	8.15***

Sources: UNFPA Enquête Démographique et de Santé (2002) and 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. (+) proportions in case of binary variables, averages in case of continuous variables. Standard errors are stacked upon standard deviations, both in brackets.
*** significant at 1%, ** at 5%, * 10%.

Table 2: Primary education completed, by Province of residence and Exposure to Violent Conflict

Province of residence in 1993	Death rate 1993	Poverty Head Count in 1990	Timing of the civil war 1995-1998	Primary Education (% completed)		t-test on the means (6)-(5)
				Not exposed to violent conflict	Exposed to violent conflict	
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bubanza	4.2	22.4	1995-1998	15.90 [3.9]	2.4 [2.4]	-13.47**
Bujumbura Rurale	5.4	25.7	1995-1998	26.20 [2.7]	28.64 [3.3]	2.43
Bururi	3.8	37.7	1995/1996	25.04 [1.7]	18.97 [2.4]	-6.06**
Cankuzo	2.5	25.1	not affected	16.36 [2.0]	-	
Cibitoke	4.9	19.6	1995-1998	8.60 [2.3]	6.94 [3.0]	-1.66
Gitega	21.9	35.2	1996/1997	32.81 [3.4]	28.50 [2.4]	-4.03
Karuzi	26.7	66.8	1995/1996	23.20 [3.8]	9.60 [2.6]	-13.60***
Kayanza	35.4	44.9	1995/1996	27.01 [3.0]	20.70 [2.4]	-6.30*
Mwaro	12.8	24.0	1996/1997	20.85 [3.2]	10.81 [2.6]	-10.04***
Makamba	1.7	39.7	1996-1998	9.70 [1.1]	8.38 [1.5]	-1.32
Kirundo	12.1	34.0	1996/1997	22.00 [3.4]	16.23 [3.0]	-5.76*
Muyinga	16.0	27.8	1995/1996	21.17 [3.5]	11.29 [2.4]	-9.86***
Muramvya	7.8	24.0	1996/1997	39.43 [5.8]	25.97 [2.4]	-13.46**
Ngozi	25.7	42.5	1995/1996	16.81 [2.5]	9.44 [1.9]	-7.37***
Rutana	5.3	58.0	not affected	9.9 [2.8]	-	
Ruyigi	6.7	41.0	1995/1996	19.05 [3.8]	25.00 [8.3]	5.95
Rural Burundi	7% median	36.2	2.28 years average	19.79 [0.7]	15.98 [0.8]	-3.81***

Sources : (2) % of survey respondents whose father was killed in the 1993 massacres (Bundervoet, 2009): (3) Poverty Head Count (Republic of Burundi and World Bank (1995); (4) 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; (5) 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; (6) 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. *** significant at 1%, ** at 5%, * 10%. Standard errors between brackets.

Table 3: Linear Probability and Logistic regressions of Schooling, Conflict, Gender, and Household Wealth, with *binary conflict exposure variable*

Dependent variable: Child completed 6 years of primary schooling	(1) All	(2) All	(3) All	(4) All	(5) poor only	(6) non-poor only	(7) All	(8) All
	LPM	LPM	LMP	LPM	LPM	LPM	Logit	Logit
Violent Conflict Shock	-0.16***	-0.17***	-0.09**	-0.06*	-0.03	-0.13**	-0.60**	-0.45*
	[0.04]	[0.04]	[0.04]	[0.03]	[0.03]	[0.06]	[0.27]	[0.23]
Child is Female	-0.08***	-0.07***	-0.08***	-0.09**	-0.07**	-0.13***	-0.56***	-0.80***
	[0.02]	[0.02]	[0.02]	[0.03]	[0.04]	[0.04]	[0.14]	[0.11]
Violent Conflict * Female	0.04	0.04	0.04	0.03	0.01	0.10	0.25	0.28
	[0.03]	[0.03]	[0.03]	[0.04]	[0.04]	[0.06]	[0.28]	[0.19]
Age (in years)	-0.009***							
	[0.003]							
Livestock_1993	0.008***	0.007***	0.008***	0.01***	0.02*	0.007***	0.05***	0.08***
	[0.003]	[0.002]	[0.002]	[0.002]	[0.01]	[0.002]	[0.02]	[0.01]
Intercept	0.40***	0.28***	0.06*	0.17***	0.06	0.33***	-2.02**	-3.23***
	[0.08]	[0.06]	[0.03]	[0.03]	[0.04]	[0.47]	[0.33]	[0.47]
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Province Specific time trend	No	Yes	No	No	No	No	No	No
Household Random Effects	No	No	No	Yes	Yes	Yes	No	Yes
Sample Weights	Yes	Yes	Yes	No	No	No	Yes	No
Sample Size	5706	5706	5706	5706	3398	1708	5706	5706

*** significant at 1%, ** at 5%, * 10%. Standard errors between brackets. All regressions are clustered at the province level, except (8).

Table 4: Linear Probability and Logistic regressions of Schooling, Conflict, Gender, and Household Wealth, with *continuous conflict exposure variable*

Dependent variable:	(1)	(2)	(3)	(4)
Child completed 6 years of primary schooling	All	Poor only	Non-poor only	All
	LPM	LPM	LPM	Logit
Years of violent conflict exposure	-0.03**	-0.02	-0.05**	-0.30***
	[0.01]	[0.14]	[0.02]	[0.10]
Child is Female	-0.09***	-0.08**	-0.13***	-0.85***
	[0.03]	[0.03]	[0.03]	[0.11]
Violent Conflict * Female	0.02*	0.014	0.05**	0.20**
	[0.01]	[0.01]	[0.02]	[0.08]
Livestock_1993	0.01***	0.02*	0.007***	0.08***
	[0.003]	[0.013]	[0.002]	[0.014]
Intercept	0.07*	0.06	0.13*	-3.05**
	[0.04]	[0.04]	[0.07]	[0.47]
Province Fixed Effects	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes
Household Random Effects	Yes	Yes	Yes	Yes
Sample weights	No	No	No	No
Sample Size	5706	3998	1708	5706

*** significant at 1%, ** at 5%, * 10%. Standard errors between brackets.
All regression are clustered at the province level, except (4)

Table 5: Linear Probability and Logistic regressions of Schooling, Conflict, Gender, and Household Wealth, with *age-specific conflict onset*

Dependent variable:	(1)	(2)	(3)	(4)
Child completed 6 years Of primary schooling	All	Poor only	Non-poor only	All
	LPM	LPM	LPM	Logit
Age at onset of conflict				
Seven	-0.18***	-0.17***	-0.28**	-1.75***
	[0.07]	[0.06]	[0.13]	[0.58]
Eight	-0.11**	-0.05	-0.25***	-0.81
	[0.06]	[0.05]	[0.09]	[0.59]
Nine	-0.10**	-0.06	-0.19**	-0.87**
	[0.04]	[0.04]	[0.10]	[0.39]
Ten	-0.07*	-0.06	-0.13*	-0.53
	[0.04]	[0.04]	[0.08]	[0.40]
Eleven	-0.01	-0.01	-0.05	-0.10
	[0.04]	[0.04]	[0.06]	[0.31]
Twelve	-0.01	-0.04	-0.10	-0.01
	[0.04]	[0.05]	[0.07]	[0.34]
Child is Female	-0.08***	-0.07**	-0.13***	-0.83***
	[0.03]	[0.03]	[0.05]	[0.12]
Female*Age at onset of conflict				
Seven	0.11*	0.08*	0.18*	1.22**
	[0.06]	[0.05]	[0.11]	[0.51]
Eight	0.11**	0.05	0.23***	1.04*
	[0.05]	[0.06]	[0.07]	[0.54]
Nine	0.05	0.05	0.08	0.50
	[0.05]	[0.04]	[0.10]	[0.34]
Ten	0.05	0.02	0.15*	0.48
	[0.5]	[0.05]	[0.08]	[0.39]
Eleven	-0.02	-0.02	-0.01	-0.19
	[0.04]	[0.05]	[0.05]	[0.33]
Twelve	-0.01	-0.09*	0.14	-0.13
	[0.05]	[0.05]	[0.11]	[0.39]
Livestock_1993	0.01***	0.02*	0.007***	0.08***
	[0.002]	[0.013]	[0.002]	[0.014]
Intercept	0.18***	0.05	0.34***	-2.82***
	[0.03]	[0.04]	[0.08]	[0.54]
Province Fixed Effects	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes
Household Random Effects	Yes	Yes	Yes	Yes
Sample weights	No	No	No	No
Sample Size	5706	3998	1708	5706

*** significant at 1%, ** at 5%, * 10%. Standard errors between brackets.
All regression are clustered at the province level, except (4)

Table 6: Linear Probability and Logistic regressions of Schooling, Conflict, Gender, and Household Wealth, *only sons and daughters living at home, with household level control variables*

Dependent variable:	(1)	(2)	(3)	(4)
Child completed 6 years Of primary schooling	All	Poor only	Non-poor only	All
	LPM	LPM	LPM	Logit
Violent Conflict Shock (binary)	-0.10**	-0.08	-0.11**	-0.62**
	[0.46]	[0.07]	[0.05]	[0.30]
Child is Female	-0.07**	-0.04	-0.11**	-0.57***
	[0.04]	[0.05]	[0.05]	[0.27]
Violent Conflict Shock * Female	0.03	-0.008	0.07	0.15
	[0.04]	[0.05]	[0.06]	[0.27]
One or both parents died in 1993	0.03	-0.001	0.04	0.27
	[0.02]	[0.04]	[0.04]	[0.32]
Livestock_1993	0.009***	0.03**	0.07***	0.07***
	[0.003]	[0.01]	[0.016]	[0.02]
Household Head Is Educated	0.04***	0.05**	0.008***	0.44***
	[0.02]	[0.02]	[0.003]	[0.17]
Household Head is Female	0.04***	0.06***	0.03	0.40**
	[0.02]	[0.02]	[0.03]	[0.17]
Household Head's Age	0.003	0.001	0.003	0.005
	[0.001]	[0.001]	[0.002]	[0.009]
Intercept	0.15	-	-	-3.80***
	[0.13]			[0.86]
Province Fixed Effects	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes
Household Random Effects	Yes	Yes	Yes	Yes
Sample weigths	No	No	No	No
Sample Size	2639	1541	1098	2639

*** significant at 1%, ** at 5%, * 10%. Standard errors between brackets.
All regressions are clustered at the province level, except (4).

Table 7: Linear Probability and Logistic regressions of Schooling, Conflict and Gender and Household Wealth with *alternative measures of conflict exposure (robustness)*

Dependent variable: Child completed 6 years of primary schooling	(1) Moved residence	(2) Displaced in camp	(3) Three measures	(4) Poor only	(5) Non-poor only	(6) Three measures	(7) 1978-82 cohorts excluded	(8) 1971-74 cohorts excluded
	LPM	LPM	LMP	LPM	LPM	Logit	LPM	LPM
Years of conflict exposure			-0.03*	-0.02	-0.04*	-0.29***	-0.03**	-0.03*
			[0.01]	[0.02]	[0.02]	[0.10]	[0.01]	[0.01]
Times moved residence	-0.03***		-0.02*	-0.01	-0.03**	-0.20*		
	[0.01]		[0.01]	[0.14]	[0.01]	[0.11]		
Years in displacement camp		-0.006	0.005	0.02	-0.007	0.06		
		[0.01]	[0.01]	[0.01]	[0.01]	[0.08]		
Child is Female	-0.08***	-0.07***	-0.09***	-0.07**	-0.13***	-0.83***	-0.11***	-0.07***
	[0.02]	[0.02]	[0.03]	[0.03]	[0.04]	[0.11]	[0.04]	[0.03]
Violent Conflict * Female			0.02	0.02	0.03*	0.21**	0.03*	0.01
			[0.02]	[0.02]	[0.02]	[0.08]	[0.02]	[0.01]
Moved residence* Female	0.02**		-0.00	-0.01	0.01	-0.09		
	[0.01]		[0.01]	[0.14]	[0.02]	[0.16]		
Years in camp * Female		0.01	0.00	-0.01	0.01	0.04		
		[0.01]	[0.01]	[0.02]	[0.02]	[0.10]		
Livestock_1993	0.01***	0.01***	0.01***	0.02*	0.007***	0.08***	0.01***	0.01***
	[0.002]	[0.002]	[0.003]	[0.01]	[0.002]	[0.01]	[0.002]	[0.003]
Intercept	0.17***	0.16***	0.08**	0.06	0.33***	-2.92***	0.09***	0.20***
	[0.03]	[0.03]	[0.04]	[0.04]	[0.07]	[0.47]	[0.03]	[0.02]
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Random Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Weights	No	No	No	No	No	No	No	No
Sample Size	5706	5706	5706	3998	1708	5706	3981	4550

*** significant at 1%, ** at 5%, * 10%. Standard errors between brackets. All regressions are clustered at the province level, except (6)

Figure 1: Primary school completion by birth cohort

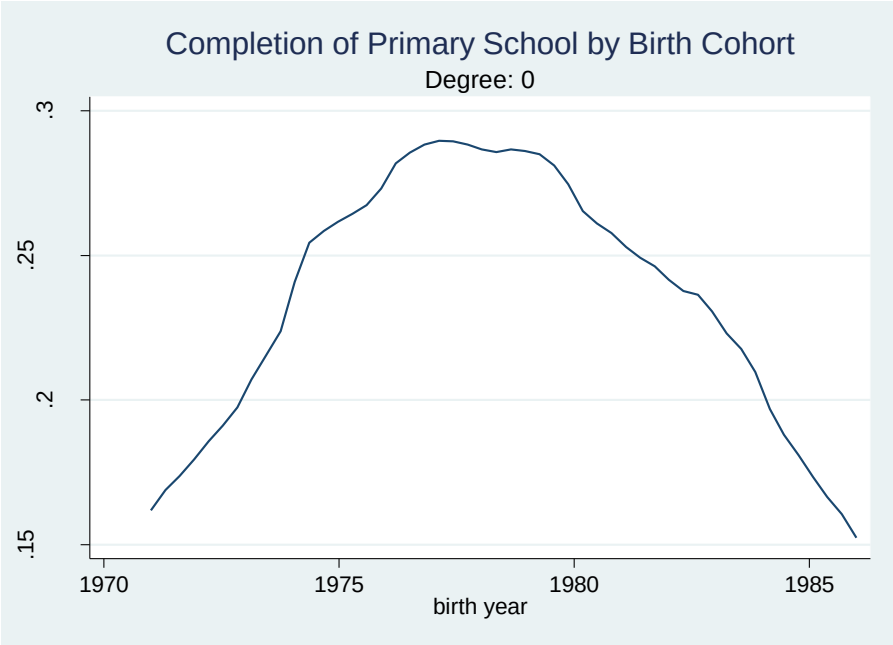
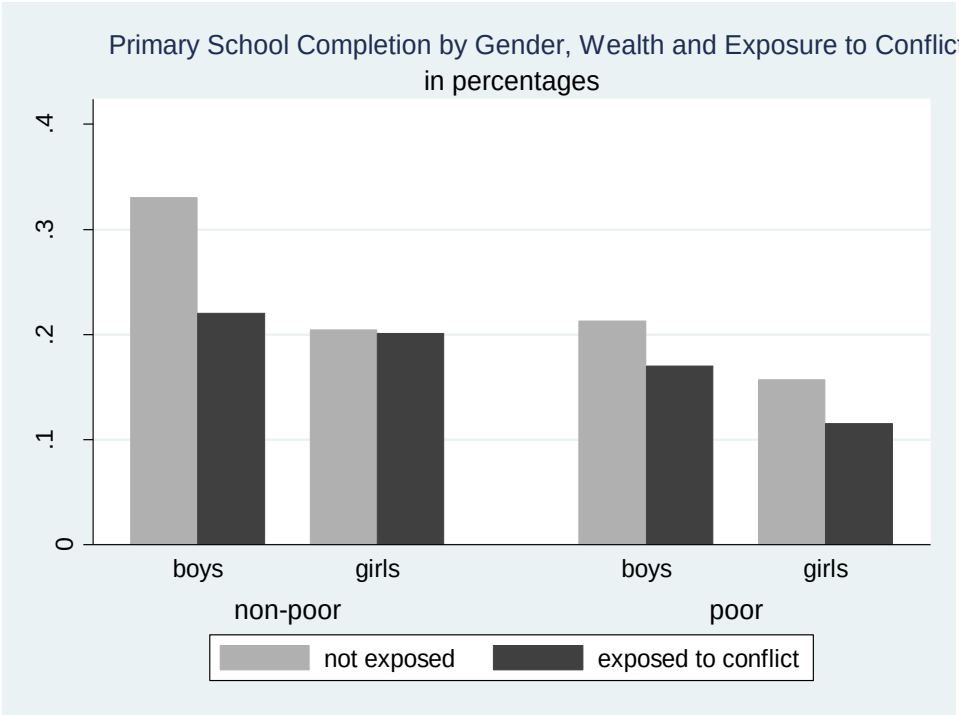


Figure 2: Primary School Completion by Gender, Wealth and Exposure to Conflict



APPENDIX

TABLE A1: Clustering of standard errors at the province-year level

Dependent variable: Child completed 6 years of primary schooling	Binary Shock variable	Continuous Shock variable	Continuous Shock variable
	(1)	(2)	(3)
	LPM	LPM	LPM
Violent Conflict Shock	-0.09***	-0.05***	-0.05**
	[0.03]	[0.01]	[0.02]
Child is Female	-0.08***	-0.08***	-0.08***
	[0.02]	[0.02]	[0.02]
Violent Conflict * Female	0.04	0.02**	0.02*
	[0.03]	[0.01]	[0.01]
Livestock_1993	0.008***	0.008***	0.008***
	[0.002]	[0.003]	[0.003]
Intercept	0.06*	0.06*	0.06*
	[0.04]	[0.04]	[0.04]
Province Fixed Effects	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes
Household Random Effects	No	No	No
Sample Weights	Yes	Yes	Yes
Sample Size	5706	5706	5706

*** significant at 1%, ** at 5%, * 10%. Standard errors between brackets.
Standards errors in regressions (1) and (2) are clustered at the province-year level, in (3) at the province level.

TABLE A2: Pre-war common trend in primary school completion rates

	Pre-war trend in primary school completion	Pre-war trend in primary school completion
	OLS	OLS
Completion rate at baseline	0.03 [0.40]	-0.03 [0.41]
Duration of violent conflict	0.003 [0.02]	0.05 [0.07]
Duration squared		-0.01 [0.01]
Constant	0.09 [0.01]	0.06 [0.09]
N	16	16

*** significant at 1%, ** at 5%, * 10%. Standard errors between brackets. Trend is defined as the difference in the average primary school completion rate between the oldest three pre-war cohorts (1971-1973) and the youngest three (1978-1980).

TABLE A3: potential selection problems

Table A3.1: Sons and daughters born in 1971-1986 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)	(2)	(3)	(4)	(5)	(6)
	boys	girls	boys	girls	boys	girls
Head of household completed primary education	0.34	0.34	0.29	0.20	-0.05	-0.14**
mother completed primary education	0.07	0.07	0.05	0	-0.02	-0.07**
Same-sex siblings completed primary 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 A3.2: Widowed persons born in 1971-1986 who lost their husband/wife 1993-2002

	Households without death 1993-2002		Households with at least one death 1993-2002		Difference (3)-(1) and (4)-(2)	
	(1)	(2)	(3)	(4)	(5)	(6)
	husband alive	wife alive	husband died	wife died	husbands	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 A3.3: Migration abroad after 1993 and return before 2002; 1971-1986 birth cohorts

	(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*
Sexe	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	6725	1169	173	