

# Schooling, Violent Conflict, and Gender in Burundi

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## Abstract

This paper investigates the effect of exposure to violent conflict on human capital accumulation in Burundi. It combines a nationwide household survey with secondary sources on the location and timing of the conflict. Only 20 percent of the birth cohorts studied (1971–1986) completed primary education. Depending on the specification, the probability of completing primary schooling for a boy exposed to violent conflict declines by

7 to 17 percentage points compared to a nonexposed boy, with a decline of 11 percentage points in the preferred specification. In addition, exposure to violent conflict reduces the gender gap in schooling, but only for girls from nonpoor households. Forced displacement is one of the channels through which conflict affects schooling. The results are robust to various specifications and estimation methods.

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# **Schooling, Violent Conflict, and Gender in Burundi**

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During the past 30 years, civil conflict has affected almost three-fourths of all countries in sub-Saharan Africa (Gleditsch et al. 2002). Economists have studied the causes of war and their 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 debated in the literature. Authors who find rapid economic recovery after war include Davis and Weinstein (2002) for Japan, Brakman, Garretsen, and Schramm (2004) for Germany, Miguel and Roland (2006) for Vietnam, and Bellows and Miguel (2009) for Sierra Leone. Convergence toward the country's long-term growth path is reached relatively rapidly, often within 15 years, as predicted by a neo-classical growth model.

The relatively rapid recovery of economic growth and other macro-level indicators do 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; however, the direction of the gender effect is an empirical question. If, for example, conflicting parties engage child soldiers, it is likely that boys will be more affected than girls. Existing gender inequalities may be exacerbated during violent conflict; however, these inequalities may also be attenuated. For example, if a country needs the brains and labor of young women to work in the military industry during a dispute with a neighbor, the labor market position of women may benefit from the conflict. There is no universal rule to predict what the gender-specific impact will be. The gender-specific impact may be exacerbated in one domain (e.g., sexual violence), or the conflict may offer new opportunities (e.g., in paid labor or business). The direction of

the effects as well as their magnitude differ from country to country and context to context depending on preexisting 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 attempt to determine the direction and magnitude of this effect in terms of schooling for both boys and girls. If schooling is negatively affected, 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, and the socioeconomic characteristics of spouses. The level of schooling attained as a child and young adult is a fundamental driver of welfare throughout one's life.

We work with the *Enquête Démographique et de Santé* collected by UNFPA in 2002. This survey contains detailed information on each member of the interviewed households, including all births and deaths, schooling, wealth, and 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 the conflict across Burundi's provinces and related variation to determine which cohorts of children were exposed to the massacres and the civil war during their 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, his probability of completing primary schooling decreased by 6 percentage points in our preferred specification. Boys from both poor and nonpoor households suffer from war. Girls suffer a general schooling

disadvantage in Burundi; however, we find that violent conflict reduces the gender gap, although only for girls from nonpoor households. We show that forced displacement is one of the channels through which violent conflict affects schooling.

### **I. Review of the literature on schooling, gender, and conflict**

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), but there is little work on the microeconomic consequences of violent or nonviolent political shocks. Although few households have formal insurance against economic shocks, many have informal insurance mechanisms that they can use, such as 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; at least, the scholarly community has largely failed to study potential coping mechanisms for political shocks. One of the findings of the coping literature in development economics is that nonpoor households are better able to cope with negative economic shocks compared to poor households. Using assets, savings, or their social capital, nonpoor households are more successful at cushioning the negative impact of weather, disease, or price shocks. The nascent literature on political shocks suggests that this poor versus nonpoor divide in terms of coping is nonexistent or much smaller than in the case of economic shocks. In the event of antiurban, Marxist, or cultural revolution-type conflicts, the nonpoor, educated part of the population may be hit harder than the poor

uneducated part, with completely different effects on their welfare in comparison to economic or agricultural shocks.

Shemyakina's (2006) empirical work on violent conflict in Tajikistan finds that girls suffer a greater loss in education compared to boys. She attributes this finding to concerns over safety and low returns on girls' education. In contrast, Akresh and de Walque (2008) find that male Rwandan children in nonpoor 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 their parents' death and that this effect is particularly strong for children who lose their mothers. Although 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 children's schooling.

Combining a household panel with detailed data on allied bombings of German cities during WWII, Akbulut-Yuksel (2009) finds significant, long-lasting, detrimental effects of bombing on the human capital and labor market outcomes of individuals who were school aged during WWII. These individuals had 0.4 fewer years of schooling, on average, in adulthood in comparison to individuals not affected by the bombings. On average, affected children experienced a reduction of 6 percent in labor market earnings in relation to unaffected children.

Alderman, Hoddinott, and Kinsey (2006) find that Zimbabwean children affected by the civil war in the 1970s completed fewer grades of schooling or started school later than those not affected by the shocks. Similar results are found by León (2011) for Peru, by Angrist and Kugler (2008) and Rodriguez and Sanchez (2009) for Colombia, by

Chamarbagwala and Morán (2010) for Guatemala, and by de Walque (2006) for Cambodia.

The reasons that education during the war may be affected negatively include school closure, migration and displacement, the quality and availability of school facilities, and shocks to income and security (Justino 2011). Chamarbagwala and Morán (2010) find that individuals who were school aged in areas that were 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 to providing young boys and, to a lesser extent, young girls with at least some primary education. Although 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 who are needed to replace labor may be removed from school, which may deplete households 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 enrollment 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 significantly increased the probability of school dropouts and the inclusion of children in the labor market. We add that not only is the young generation is prevented from acquiring human capital, but educated members of older cohorts may be disproportionately killed, thereby depriving the country of its human capital stock.

## **II. 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), and possible conflict solutions (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 attempted 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 the 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 began 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 in 1972, the international community condemned the massacres and pressured the Buyoya regime to liberalize its political system. In June 1993, this situation led to the first free and fair elections in postindependence Burundi. The democratic transition did not last 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 Union for National Progress (*Union pour le Progrès national*). Chrétien (1997) writes that districts in certain provinces were “almost completely ‘cleansed’ of all Tutsi elements.” The Tutsi army retaliated against the Hutu, continuing what would become the most severe civil war in Burundi’s history in terms of both human lives and socioeconomic destruction (Ndikumana 2000).

#### <<B>>*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. Because the exact timing and location of the massacres and the civil war plays an important role in our identification strategy (see section 4), we describe the evolution of the massacres and the civil war through time and space as follows:

- In 1993 and 1994, massacres occurred in many parts of the country with different intensities.
- From the end of 1994 to July 1996, civil war spread throughout the country.

- From July 1996 to August 2000, Major Buyoya returned to power after a bloodless coup. The civil war intensity was lower in most provinces, and the Arusha Peace and Reconciliation Agreement was signed in 2000.

The massacres were particularly intense in central and northern Burundi. Bundervoet (2009) estimates that in half of the provinces, more than 7 percent of all individuals lost their fathers in 1993. Table 1 provides 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, the violence spread to the bordering Kayanza province, and by April 1995, massacres of civilians and confrontations between army and rebel forces occurred in Karuzi, Bururi, Ruyigi, and Muyinga. By late 1995, fighting occurred in the central provinces of Gitega and Muramvya and the northern province of Kirundo. By then, the 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 again seized power in a bloodless *coup d'état* backed by the army. During late 1996 and early 1997, the armed conflict continued in Kayanza, Muramvya, Kirundo, and Gitega. In April 1997, the Arusha peace talks between the principal conflict parties began. In late 1997, insecurity increased again in Cibitoke, Bubanza, and Bujumbura Rural, provinces that 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

TABLE 1. 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		<i>t</i> 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***

Source: Bundervoet (2009), Republic of Burundi and World Bank (1995), Chrétien and Mukuri (2000), United Nations (1996), Bundervoet et al. (2009), UNFPA Enquête Démographique et de Santé (2002).

Note: Column (2) shows the percentage of survey respondents whose fathers were killed in the 1993 massacres; column (3) shows the Poverty Head Count; and column (4) shows the spread of the civil war over time and space. We only consider the “relevant” duration, which is the period during which school-aged children from the 1981–1986 birth cohorts could have been exposed to violence. Column (5) shows the birth cohorts not exposed to violent conflict (neither massacres nor the civil war) when they were between 7 and 12 years of age; column (6) shows the birth cohorts exposed to violent conflict (massacres, civil war, or both) when they were between 7 and 12 years of age. Standard errors are presented between brackets. \*\*\* significant at 1 percent, \*\* at 5 percent, and \* at 10 percent.

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.

### *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.<sup>1</sup>

First, in its 2002 Demographic and Health Survey, UNFPA found that over 50 percent of the rural population had been displaced from their homes at least once between 1993 and 2000 as a result of violence. The average displacement duration for the entire sample was just over one year, meaning three agricultural seasons were missed because households could not cultivate or harvest their fields while they were displaced (UNFPA 2004). Displacement also meant that individuals were more likely to contract water- and vector-borne diseases while hiding in the forest. Because people could not carry significant amounts of food when fleeing their villages, adequate nutrition was a problem. Displacement also implied a lack of access to markets, health clinics, and schools because roads were unsafe or these structures had been damaged. Later in the war, civilians were forced into local resettlement camps by the government, and the camp conditions were poor (Human Rights Watch 2000). The displacement's impact on aggregate production from 1993 to 1998 showed production declines of 15 percent in cereals, 11 percent in roots and tubers, and 14 percent in fruits and vegetables, with particularly dramatic declines in 1994 and 1995. Later in this paper, we will test the

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impact of displacement on schooling as a potential channel by which exposure to violent conflict can affect child schooling.

When the conflict ended in a given province, displaced households returned to their homes and fields. However, humanitarian interventions by either the government or nongovernmental organizations after the fighting ended were practically nonexistent because of the continued insecurity on all roads linking the capital, Bujumbura, to the countryside. By early 1995, rebel groups had begun to target and kill foreign nongovernmental organization 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, in addition to the displacement and killing of civilians, both rebel and government forces engaged in the looting of civilian property, particularly livestock, causing an unprecedented drop in household capital stock. Aggregate national figures show that the number of tropical livestock units in the country declined by 23 percent from 1990 to 1998, a decline that was predominantly due to theft and pillaging.

Third, Human Rights Watch reports (1998) document the theft and burning of household crops. Crops were stolen from fields and granaries, and coffee trees were particularly targeted for burning. Because coffee is the government's main source of tax revenue, rebels frequently burned coffee plantations to reduce government revenue, although we cannot quantify the magnitude of this destruction. 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.

Fourth, the conflict in Burundi is notorious for its adverse impact on women and girls. Rape was widespread, and there were many instances of brutality, even against children. Gender roles became more entrenched as boys and men were drafted by the army or recruited by rebel movements.

### *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 percent completed primary schooling. Education is directly related to jobs in the public sector, for which degree holders have the monopoly. The education system and jobs in the administration were 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 at that time.

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 school enrollment. The decade before the conflict (1981–1991) saw an expansion of enrollment for each new cohort, male and female. The gross enrollment rate increased from 33.2 to 70 percent in that decade (Ministère de l'Éducation 1999). The conflict-induced trend reversal can be observed in figure 1, which we computed with UNFPA 2002 data. The birth cohorts that could finish primary schooling before the beginning of the conflict show an upward trend in primary school completion, from a rate of less than 20 to almost 30 percent. The cohorts born between 1975 and 1980 show the highest primary school completion rates in the history of Burundi (up to 2005). This high completion rate was

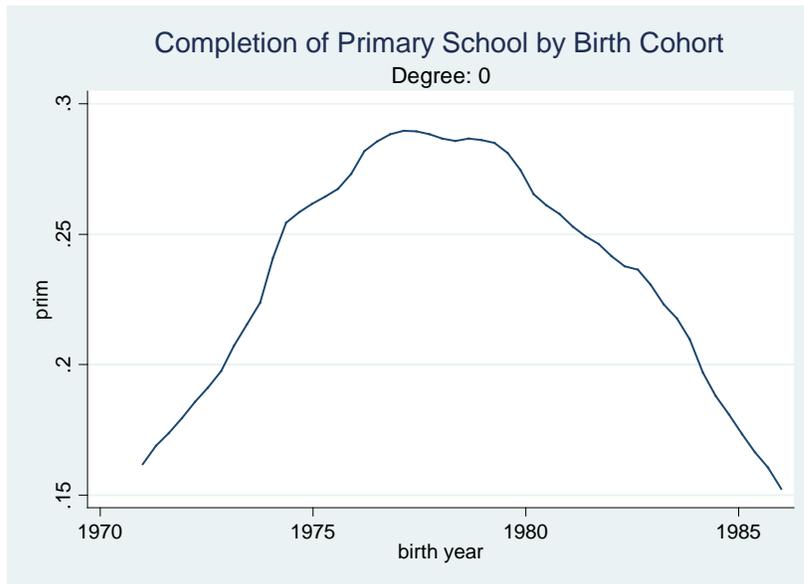
due to the expansion of primary education, which doubled the enrollment rate in five years (1982–1987) (UNESCO 1999; UNICEF 2008). Progress stops for the birth cohorts born at the end of the 1970s and is reversed from the 1980 birth cohorts onward, just when the first birth cohorts are confronted with the start of the violence. Children in Burundi officially attend primary school from age 7 to age 12, when they finish sixth grade (UNESCO 2011). Some children may start schooling later and complete primary schooling at a later age.

Figure 2 provides the key variables of our empirical approach (see below). Girls fare worse than boys, children from poor households fare worse compared to children from nonpoor 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 preconflict 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 nonpoor households exposed to conflict is almost the same as for boys exposed to conflict, but it is significantly different for nonexposed boys and girls from nonpoor households. Moreover, we do not find this gender-gap-reducing effect of conflict on schooling for children in poor households.

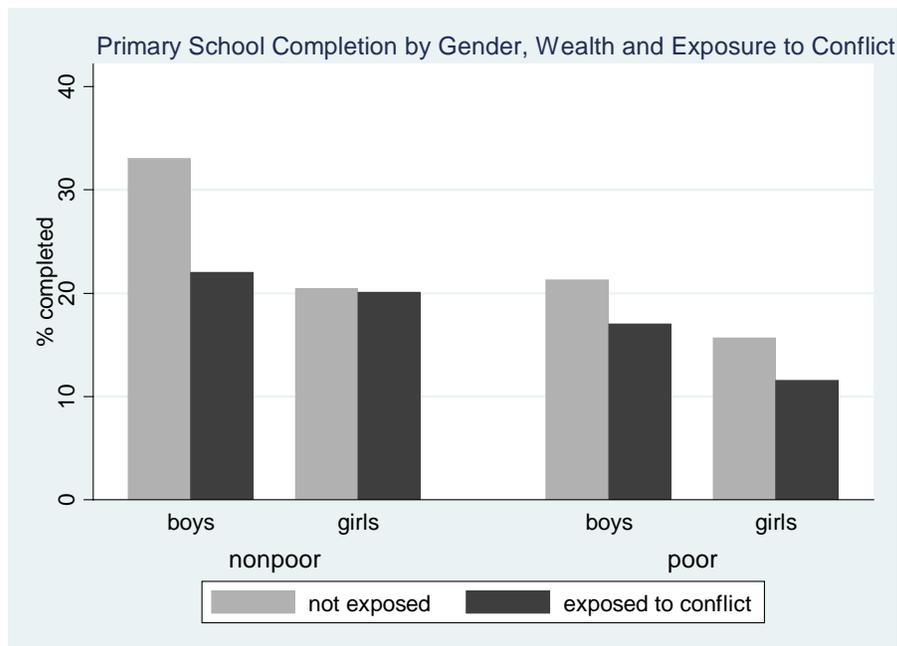
In an extensive review of the damage to the education sector during the conflict in Burundi, Obura (2008, 94-96 and 99) observes that schools were destroyed or looted and teachers and children killed or displaced. Importantly, although the gross enrollment rate

Figure 1. Primary School Completion by Birth Cohort



Source: UNFPA Enquête Démographique et de Santé (2002).

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



Source: UNFPA Enquête Démographique et de Santé (2002).

decreased, the Gender Parity Index did not decline during the conflict and even improved slightly, from 0.80 to 0.83. Obura also notes 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 substitute for formal education in affected areas.

### **III. Data and Identification Strategy**

In 2002, UNFPA collected demographic and health data (through the *Enquête Démographique et de Santé*) on almost 7,000 households. At the time of the survey, many Burundesi lived in camps for internally displaced persons. A particular feature of this survey is that it is stratified over urban, rural, and camp locations in each province of the country. Weights are assigned to each observation in the survey representing the inverse of the probability of that observation being drawn in each sampled location. The sampling is based on a total population count by the National Institute of Statistics (*L'Institut de Statistique et d'Etudes Economiques du Burundi*) the year before the survey.<sup>2</sup> The focus of the survey is on household composition, schooling, and health, with significant attention to the potential impact of the conflict through displacement. Descriptive data are presented in tables 1 and 2. In our analysis, we dropped the observations from the capital of Bujumbura because it is nearly impossible to determine exactly when the population of Bujumbura was affected by the conflict. Arguments can be made for a very short as well as a very long time.

TABLE 2. Individual and Household Characteristics, by Exposure to Violent Conflict (N = 5,856)

Name of the variable	Values	Not exposed to violent conflict (n = 3,586) (†)	Exposed to violent conflict (n = 2,266) (†)	t test on the means (2) – (1)
		(1)	(2)	(3)
<i>At the individual level</i>				
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***
No. 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***
<i>At the household level</i>				
Prewar wealth (Livestock in 1993)	0–20	1.45 [0.55] (4.76)	2.02 [0.84] (5.76)	0.57***

Source: UNFPA Enquête Démographique et de Santé (2002), Chrétien and Mukuri (2000), United Nations (1996), and Bundervoet et al. (2009).

Note: We only consider the “relevant” duration, which is the period that school-aged children from the 1981–1986 birth cohorts could have been exposed to the violence. (†) represents proportions in the case of binary variables and averages in the case of continuous variables. Standard errors are presented in brackets; standard deviations are presented in parentheses. \*\*\* significant at 1 percent, \*\* at 5 percent, and \* at 10 percent.

### *Conflict Variables*

We construct four conflict exposure variables. The first two variables are general indicators of exposure to violent conflict, and the last two variables represent potential impact channels of conflict on education. First, we construct a binary variable to indicate whether a child resided in a province characterized by violent conflict. To determine which provinces were affected by the massacres (in 1993–1994) and the civil war (1995–

1998), we use two sources.<sup>3</sup> For the massacres, we rely on Bundervoet (2009), who computed the percentage of people whose fathers were 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 percent) and eight provinces with a lower death rate. The eight provinces with a lower than median death rate are defined as nonaffected provinces; the eight other provinces 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 the child's 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.

Second, we determine the number of years that a child was exposed to violent conflict during the child's 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 the possibility that children moved to a more peaceful province after the onset of war in their province of residence. In that case, we overestimate the duration of exposure, yielding a conservative estimate of the effect of conflict on schooling. However, UNFPA (2012, 141) notes that most conflict-induced migration occurred locally, within the same province. In addition, we test for individual exposure channels, such as the frequency of

forced displacement and the time spent in a displacement camp, which are not measured at the level of the province of residence.

Many children born in the 1981–1986 period 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 began. Depending on the province of residence, younger children experienced no, some, or many 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.

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

### *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 significantly affected and with children who were old enough to complete their schooling before the conflict started in both affected and

nonaffected provinces. Building on figure 2 and previous tabulations, our baseline specification is a linear probability model of the following 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*. We denote with  $\alpha_j$  the province fixed effects, with  $\delta_t$  the birth cohort fixed effects, and with  $\varepsilon_{ijt}$  a random error term. The last term has an individual- and a household-level component. First, we calculate the *Exposure<sub>jt</sub>* variable 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 of primary school age. Second, we use our estimate of the duration in number of years of exposure for a child residing in an affected province. In the latter case,  $\beta_1$ , 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 in 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. To capture potential trends at the province level, we estimate a specification with province-level time trends. In section 6, we will test whether our variable of interest captures prewar 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, *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 because by 1993, the older cohorts had already left their parental households; thus, these variables cannot affect their school completion.

We cluster our standard errors at the province level to control for intraprovince correlations (Bertrand et al. 2004). Clustering should occur at the province level because our shock is coded at this level. We face a problem of low numbers of clusters, leading to larger standard errors and coefficients that are imprecisely estimated. We therefore use the bootstrapping method proposed by Cameron et al. (2008) to improve the estimation.

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 the Democratic Republic of the Congo, the Kibira forest, or the Tanganyika Lake. These factors are exogenous to the level of education or 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. Although the authors cannot exclude occurrences of targeted violence, they note that “the probability of incorrectly maintaining the null of non-targeted violence is acceptably small” (950).

Furthermore, because the impact of conflict may differ according to the age at which the impact is felt, we also account for the 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 of completing primary schooling.

Because 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:

$$Schooling_{ijt} = \alpha_j + \delta_t + \beta_1(Exposure_{jt}) + \beta_2S_i + \beta_3(S_i * Exposure_{jt}) + \gamma Z_i + \varepsilon_{ijt}, \quad (2)$$

where  $S_i$  is the sex of the child ( $S_i = 1$  for girls), and the other variables are as in specification (1). In this specification,  $\beta_1$  gives the effect of violent conflict on schooling for boys. The interaction effect between gender and conflict tells us whether there is an additional effect for girls, and the sum of  $\beta_1$  and  $\beta_3$  gives the total effect of conflict on schooling for girls. In addition, the sum of  $\beta_2$  and  $\beta_3$  gives the total effect of gender on schooling.

The above specifications do not specify the mechanism through which the impact of the conflict is channeled; the specification only provides a generic “exposure to civil war variable” in binary form, in number of years of exposure, or in age-specific onset. To analyze particular channels in more detail, we develop other specifications that use alternative measures of conflict to indicate 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.

#### IV. Findings

In table 3, we use the binary shock exposure variable that takes the value of one for children exposed to violent conflict in their province of residence during their school age years and zero for nonexposed individuals. The regressions in columns 1–3 are linear probability models. In the first column, we control for province and year of birth fixed effects and find that the coefficient of our variable of interest (exposure to violent conflict) is  $-0.09$ , which means that the probability of completing primary schooling is 9 percentage points lower for children exposed to violence. Girls have a lower probability of completing primary schooling; however, there is a small, positive coefficient for the *female\*conflict* exposure interaction variable. Given the rather large standard error, we are not able to interpret the point estimate of this interaction effect in column 1, an effect that is also not statistically significantly different from zero. Preconflict wealth, measured as livestock holdings, increases the probability of completing primary school. Columns 2 and 3 repeat the analysis controlling for linear age effects and for province-specific time trends. This control increases the magnitude of the coefficient of the variable of interest but affects neither the gender nor the interaction effect.

Driven by our exposure variable, which is measured at the province level, we need to cluster at that level (Bertrand et al. 2004). However, we have a small number of clusters. Standard errors for our variable of interest in the linear probability specifications are typically one-half to one-quarter of the magnitude of the coefficient of interest. These coefficients are thus imprecisely estimated and do not allow an interpretation of the point estimates.<sup>4</sup>

We use a well-developed method to address a small number of clusters, the CGM bootstrapping method, named after Cameron, Gelbach, and Miller (2008). The key element of the CGM method is that it resamples entire clusters and provides asymptotic refinement, which leads to improved inference. A drawback of this method is that the accompanying variance-covariance matrix cannot be calculated in the presence of fixed effects at the level of the clusters.<sup>5</sup> Therefore, we run the CGM regression without fixed effects. We keep in mind (referring to the difference between columns 2 and 1) that this may overestimate the effect of exposure to conflict. The magnitude of the resulting coefficient (0.11), however, remains on the order of the previous estimates.

Given the small standard errors in the CGM procedure and the statistical significance of the gender\*conflict dummy interaction, we are able to interpret the point estimates resulting from the regression in column 4. The female\*conflict interaction yields a coefficient of +0.03 with a standard error of 0.01 and is statistically significantly different from zero at the 1 percent level, meaning that the conflict in Burundi diminished the gender-gap in schooling somewhat. However, the gap still exists ( $-0.06 + 0.03 = -0.03$ ). This result in the case of the binary exposure variable is only obtained if we weigh the regression with the sample weights provided in the survey, as column 5 shows. Therefore, caution is required in the interpretation of the interaction effect.

Repeating the analysis for poor and nonpoor households separately, we find in columns 6 and 7 that violent conflict diminishes the female disadvantage in schooling only for nonpoor households. The linear combination of the female and the female\*exposure variables yields a coefficient of  $-0.02 (= -0.15 + 0.13)$  with a standard

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error of 0.02 and is statistically not significantly different from zero at the usual thresholds. Thus, there is no longer a gender gap in schooling between girls and boys from nonpoor households who are exposed to violent conflict. This result remains the same when we remove the sample weights: a coefficient of  $-0.02$  ( $= -0.127 + 0.106$ ), a standard error of 0.03, and not statistically significantly different from zero at the usual thresholds (result not shown, available upon request). For girls from poor households, the linear combination is  $-0.03$  ( $= -0.033 + 0.003$ ) with a standard error of 0.01 and remains statistically significantly different from zero at the 1 percent level.

The F-test for the equality of the coefficients of interest for girls from poor and nonpoor households (i.e., the interaction of exposure and gender) in both equations confirms the above finding:  $26.15^{***}$ , with  $p < 0.001$ , meaning that the coefficients are not equal. This result corroborates the intuition behind figure 2 in which boys and girls from nonpoor households exposed to conflict have similar completion rates. In sum, in nonpoor households, school completion rates for boys decrease to the level of girls such that the gender gap almost disappears. In contrast, in poor households where school completion rates are lower, girls are as affected as boys such that the gender gap persists.

TABLE 3. Linear Probability and CGM Regressions of Schooling, Conflict, Gender, and Household Wealth, with Binary Conflict Exposure Variable

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Child completed six years of primary schooling	All	All	All	All	All, no weights	Poor only	Nonpoor only
	LPM	LPM	LPM	CGM	CGM	CGM	CGM
Violent conflict shock	-0.09** [0.04]	-0.16*** [0.04]	-0.17*** [0.04]	-0.11*** [0.01]	-0.07* [0.04]	-0.11*** [0.03]	-0.14*** [0.04]
Child is female	-0.08*** [0.02]	-0.08*** [0.02]	-0.07*** [0.02]	-0.06*** [0.01]	-0.07** [0.03]	-0.03*** [0.04]	-0.15*** [0.02]
Violent conflict*female	0.04 [0.03]	0.04 [0.03]	0.04 [0.03]	0.03** [0.01]	0.03 [0.03]	0.003 [0.01]	0.13*** [0.04]
Age (in years)		-0.009*** [0.003]		-0.006*** [0.000]	-0.002 [0.000]	-0.006*** [0.001]	-0.006*** [0.000]
Prewar wealth	0.008*** [0.002]	0.008*** [0.003]	0.007*** [0.002]	0.008*** [0.003]	0.009*** [0.003]	0.04*** [0.01]	0.005*** [0.002]
Intercept	0.06* [0.03]	0.40*** [0.08]	0.28*** [0.06]	0.39*** [0.03]	0.27*** [0.03]	0.34*** [0.04]	0.46*** [0.47]
Province fixed effects	Yes	Yes	Yes	No	No	Yes	Yes
Birth cohort fixed effects	Yes	No	No	No	No	No	No
Province-specific time trend	No	No	Yes	No	No	No	No
Sample Size	5,706	5,706	5,706	5,706	5,706	3,398	1,708

Source: UNFPA Enquête Démographique et de Santé (2002).

Note: LPM represents linear probability model. Standard errors are presented between brackets. All regressions, except those in column (5), are weighted with the sample weights provided in the data set, and all regressions are clustered at the province level. \*\*\* significant at 1 percent, \*\* at 5 percent, and \* at 10 percent.

TABLE 4. Linear Probability and CGM Regressions of Schooling, Conflict, Gender, and Household Wealth, with Number of Years of Conflict Exposure Variable

Dependent variable:	(1)	(2)	(3)	(4)	(5)
Child completed six years of primary schooling	All	All	All, no weights	Poor only	Nonpoor only
	LPM	CGM	CGM	CGM	CGM
Years of violent conflict exposure	-0.05** [0.02]	-0.06*** [0.01]	-0.04*** [0.01]	-0.05*** [0.01]	-0.08*** [0.01]
Child is female	-0.08*** [0.02]	-0.06*** [0.01]	-0.08*** [0.03]	-0.04*** [0.01]	-0.14*** [0.02]
Violent conflict*female	0.02* [0.01]	0.02*** [0.00]	0.02 [0.01]	0.01*** [0.00]	0.06*** [0.01]
Age in years		-0.01*** [0.00]	-0.01 [0.00]	-0.01*** [0.00]	-0.01*** [0.00]
Prewar wealth	0.01*** [0.008]	0.01*** [0.002]	0.01*** [0.003]	0.04*** [0.007]	0.01*** [0.001]
Intercept	0.06 [0.04]	0.45 [0.03]	0.33 [0.08]	0.38 [0.04]	0.55 [0.04]
Province fixed effects	Yes	No	No	No	No
Year of birth fixed effects	Yes	No	No	No	No
Sample size	5,706	5,706	5,706	3,998	1,708

Source: UNFPA Enquête Démographique et de Santé (2002).

Note: LPM represents linear probability model. Standard errors are presented between brackets. All regressions, except those in column (3), are weighted and clustered at the province level. \*\*\* significant at 1 percent, \*\* at 5 percent, and \* at 10 percent.

Proceeding to *years of exposure* as our variable of interest in table 4, we find that the magnitude of the coefficients is approximately half of the binary case. Every additional year of exposure to violent conflict reduces the probability of completing primary schooling by 5 to 6 percentage points overall (columns 1 and 2) and by 8 percentage points for boys from nonpoor households (column 5). As shown in table 3, exposure to conflict diminishes the gender gap in primary school completion rates, particularly for girls from nonpoor households. The longer boys and girls are exposed to violent conflict, the stronger the reduction of the gender gap in school completion rates is. Again, referring to column 3, we arrive at this particular result only when using the sample weights provided in the survey.

Because 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 determine for each child the age at which the child experienced the onset of violent conflict in the province of residence. Furthermore, we interact these dummy variables with the female variable. Examining column 2, where the use of the CGM model yields small standard errors, we find that the first four years (ages 7 to 10) are crucial, in declining order of magnitude, rather than the last two years (ages 11 and 12). For boys of poor households, the magnitude of the coefficients is smaller compared to boys from nonpoor households at all ages. The probability of completion of primary schooling for boys from nonpoor households decreases by 28, 21, 14, and 9 percentage points for the onset of conflict at ages 7, 8, 9, and 10 years, respectively. Table 5 clearly indicates that a cohort of children in Burundi was particularly affected by exposure to violent conflict if the conflict started in the province of residence at the moment that the

cohort should have started primary school. A child who had yet to start school or who had only completed one or two years of schooling at the start of the conflict may have been compelled to give up school. The gender gap in primary school completion, however, is reduced if the conflict starts at a young age in the province of residence of the child. Onset at an early age (up to age nine) reduces the gender-gap for girls from poor households. Onset at ages 10 to 12 aggravates the school completion chances of girls from poor households, in contrast to girls from nonpoor households. Onset at the ages of 11 or 12 for boys from nonpoor households does not affect their completion chances. Most likely, these boys are able to make up for potential lost months or years of schooling because their parents believe that a degree is within reach. We also tested a linear interaction between age and gender to account for possible trends in female enrollment over time, separate from the effect of conflict. The results (not shown) indicate that the effect of onset of conflict for girls at a younger age is slightly smaller; the effect is larger for girls who are slightly older (11 and 12 years old).

Exposure to violent conflict remains a broad term that is defined at the province-birth cohort level. Based on this definition, we cannot derive the 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 registered the number of times that each household member had to move residence because of the fighting and the length of stay in a displacement camp. It appears plausible that these two events would have a negative effect on the probability of

a school-aged child completing primary schooling. Columns 1–3 in table 6 test these channels.

We find that the frequency of forced displacement and the length of stay in a displacement camp matter for school completion. Being uprooted from one's village because of ongoing or imminent violence proves to be disruptive to a child's school career to the extent that it decreases the probability of completing primary schooling, particularly if it occurs several times. The magnitude of the length of stay in a displacement camp is smaller but remains statistically significantly different from zero. As in previous regressions, the coefficient of the interaction between gender and the two alternative conflict measures is positive and statistically significant, meaning that displacement reduces the gender gap in school completion. When we test the effect of the three channels of violence (exposure to battles, forced displacement, and duration of stay in a camp, in column 3), all three channels exercise a negative and statistically significant effect on the completion of primary schooling. The probability of completing primary schooling declines by 8 percentage points as a result of exposure to conflict in the province of residence, by 6 percentage points for every instance of forced displacement, and by 2 percentage points for every year spent in a displacement camp. Importantly, the interaction effects of the alternative conflict measures with gender aggravate the completion chances for girls from poor households. This result in column 5 is worse than our baseline result, where the gender gap for girls from poor households was neither reduced nor aggravated as a result of conflict exposure. These alternative measures thus show that the channel through which the conflict operates affects girls from poor and nonpoor households (column 6) differently. Displacement reduces the gender gap for the

latter but widens it for the former.<sup>6</sup> We call for caution in the interpretation of the result because we cannot exclude the possibility of a third, unobserved intervening variable (or set of variables) that correlates both with the value of the channel variable and with the education variable.

In columns 6 and 7, we perform robustness checks in which we omit the 1978–1982 and 1971–1974 birth cohorts from the analysis. For the 1978–1982 cohorts, the argument can be made that we are not sure whether 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 be affected by the massacres and the civil war toward the end of their primary school career and would not constitute an adequate control group. Valente’s (2011) paper on the schooling consequences of the conflict in Nepal makes a similar argument for dropping a few birth cohorts from the analysis. In column 6 of table 6, we thus infer the effects of violent conflict on affected cohorts when we are certain that the control group never experienced violence during their school

TABLE 5. Linear Probability and CGM Regressions of Schooling, Conflict, Gender, and Household Wealth, with Age-Specific Conflict Onset

Dependent variable:	(1)		(2)	(3)
Child completed six years of primary schooling	All	All	Poor only	Nonpoor only
	LPM	CGM	CGM	CGM
<b>Age at onset of conflict</b>				
Seven	-0.19 [0.11]	-0.15*** [0.01]	-0.13*** [0.01]	-0.28*** [0.03]
Eight	-0.13 [0.09]	-0.12*** [0.01]	-0.09*** [0.01]	-0.21*** [0.02]
Nine	-0.13* [0.07]	-0.06*** [0.01]	-0.05*** [0.01]	-0.14*** [0.04]
Ten	-0.08 [0.07]	-0.08*** [0.01]	-0.09*** [0.01]	-0.09** [0.04]
Eleven	-0.04 [0.04]	0.01 [0.02]	0.01 [0.02]	-0.03 [0.02]
Twelve	-0.02 [0.06]	-0.02 [0.02]	-0.07*** [0.01]	0.07 [0.07]
Child is female	-0.08*** [0.02]	-0.06*** [0.01]	-0.03*** [0.01]	-0.16*** [0.02]
<b>Female*age at onset of conflict</b>				
Seven	0.09* [0.05]	0.09*** [0.01]	0.05*** [0.01]	0.24*** [0.04]
Eight	0.10** [0.03]	0.07*** [0.01]	0.04** [0.01]	0.17*** [0.04]
Nine	0.07 [0.06]	0.05** [0.02]	0.03 [0.02]	0.16*** [0.05]
Ten	0.05 [0.03]	0.04*** [0.01]	-0.01** [0.01]	0.18*** [0.06]
Eleven	-0.02 [0.04]	-0.03** [0.01]	-0.07*** [0.02]	0.12*** [0.03]
Twelve	-0.04 [0.07]	-0.03 [0.02]	-0.02* [0.01]	-0.01 [0.01]
Prewar wealth	0.01*** [0.003]	0.01** [0.003]	0.04*** [0.01]	0.005*** [0.001]
Intercept	0.06 [0.03]	0.23*** [0.01]	0.02*** [0.01]	0.32*** [0.03]
Province fixed effects	Yes	No	No	No
Year of birth fixed effects	Yes	No	No	No
Sample Size	5,706	5,706	3,998	1,708

Source: UNFPA Enquête Démographique et de Santé (2002).

TABLE 6. CGM Regressions of Schooling, Conflict, Gender, and Household Wealth  
with Alternative Measures of Conflict Exposure (Robustness)

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Child completed six years of primary schooling	Moved residence	Displaced in camp	Three measures	Poor only	Nonpoor only	1978–82 cohorts excluded	1971–74 cohorts excluded
	CGM	CGM	CGM	CGM	CGM	CGM	CGM
Violent conflict shock			–0.08*** [0.02]	–0.08*** [0.01]	–0.10** [0.04]	–0.09*** [0.02]	–0.06*** [0.01]
Times moved residence	–0.08*** [0.01]		–0.06*** [0.01]	–0.06*** [0.01]	–0.07*** [0.01]		
Years in displacement camp				0.01	–0.04***		
		–0.06*** [0.01]	–0.02*** [0.00]	[0.00]	[0.00]		
Child is female	–0.05*** [0.01]	–0.05*** [0.01]	–0.06*** [0.01]	–0.04*** [0.01]	–0.15*** [0.03]	–0.08*** [0.01]	–0.03*** [0.01]
Violent conflict*female			0.01 [0.02]	0.01 [0.01]	0.10*** [0.03]	0.06* [0.01]	–0.01 [0.01]
Moved residence*female	0.02*** [0.00]		0.02** [0.01]	–0.02*** [0.01]	0.03** [0.00]		
Years in camp*female		0.02*** [0.00]	0.00 [0.00]	–0.01*** [0.00]	0.02*** [0.00]		
Age in years		0.001	–0.01***	–0.01***	–0.01***	–0.01***	0.01
	–0.006*** [0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Prewar wealth	0.01*** [0.003]	0.01*** [0.003]	0.01*** [0.003]	0.04*** [0.01]	0.01*** [0.001]	0.01*** [0.001]	0.01** [0.003]
Intercept	0.31*** [0.02]	0.20*** [0.02]	0.46*** [0.03]	0.41*** [0.03]	0.52*** [0.05]	0.27*** [0.03]	0.18*** [0.04]
Province fixed effects	No	No	No	No	No	No	No
Birth cohort fixed effects	No	No	No	No	No	No	No
Sample size	5,706	5,706	5,706	3,998	1,708	3,981	4,550

Source: UNFPA Enquête Démographique et de Santé (2002).

Note: Standard errors are presented between brackets. All regressions are clustered at the province level and are weighted with the sample weights provided in the data set. \*\*\* significant at 1 percent, \*\* at 5 percent, and \* at 10 percent.

careers and the treated group did. The result, again computed with the CGM method, is very similar to the one obtained in our baseline regression. 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 significantly over time. However, omitting these cohorts from the estimation (in column 7) leads to similar results.

## **V. Issues of concern for the identification strategy:**

### **Poverty, prewar trends, selective survival, and selective migration**

A first issue of concern for our identification strategy is that we may measure the effect of something other than exposure to violent conflict. If, for example, massacres were more intense or the civil war lasted longer in poor provinces compared to nonpoor provinces, then we may be measuring the effect of poverty instead of exposure to violent conflict. Although we control for wealth (in the form of livestock) in our regression analysis, this variable is measured at the household level. Because our exposure variable is measured at the province level, we must ensure that we are not capturing another effect. To that end, we analyzed data on the death rate in 1993 and the duration of the civil war in poor and nonpoor 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 percent, the poverty headcount in Burundi in 1990. The difference in the 1993 death rate between poor and nonpoor provinces was  $-5.3$  percentage points; however, this difference is not statistically significant at the usual thresholds. Similarly,

the difference in the duration of the civil war between poor and nonpoor provinces is six months, but it is not statistically significant. Bundervoet et al. (2009) find very few correlations between the timing of the conflict onset (no, early, or late) at the province level, the length of exposure to conflict at the individual level, and a range of household characteristics. We conclude from this finding that there appears to be no selection into violence of provinces or individuals based on prewar characteristics. Therefore, it appears unlikely that our exposure variable is capturing a wealth or other effect.

Second, although we include province fixed effects in our specifications to control for time-invariant province characteristics, there may be a problem of endogeneity with time-varying province characteristics. To test for that endogeneity, we analyze the potential correlation between a prewar province-level trend in primary school completion rates and the duration of conflict in that province. To this end, we compute a prewar trend, defined as the difference between the average school completion rate of the three oldest (1971–1973) and the three youngest (1978–1980) prewar cohorts, and we regress this trend on the duration of violent conflict in the province. We perform this test at the province and the individual level. The results are presented in the online appendix, tables A1 and A2. We do not find any statistically significant effects in various specifications using the usual thresholds. We note that the specification in table 3, column 2 also includes a province-specific time trend.

A third issue is potential bias caused by selective survival. Because the survey, by definition, only contains data on children who survived violence until the time of the survey, we must account for potential survivor bias in the sample. Particularly, if 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) suggest that the violent conflict affected the schooling of not only children who were school aged during the conflict but also those who had completed their primary education. Education in times of conflict in Burundi has proved to be a liability. Thus, our nonaffected cohort (1971–1980, in our approach not affected during their primary school career) suffers survival bias; the most educated cohort members were killed in 1993. Thus, on average, this cohort was more educated than we infer from the survivors in the 2002 survey. Assuming that there is no such survival bias for the affected cohort (which 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.

Fourth, in addition to selective killing, we may face a problem of selective migration. If migrants have a profile that differs from stayers, then we may over- or underestimate the impact of violent conflict on 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 into 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 surveyed people

living in displacement camps at the time of the survey. People who fled abroad but returned before 2002 are included because they were part of the target population at the time of the design of the survey. Thus, only groups (i) and (ii.c) represent a potential selection problem.

The 2002 survey allows us to investigate the profiles of people who were killed as well as migrants. We compare the profiles of households with and without at least one child killed in the 1993–2002 period (available in the online appendix, table A3). We conduct this comparison for the loss of boys as well as girls. Furthermore, we investigate the profiles of 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 finding that the killed daughters were more educated than the surviving daughters is premature given that the siblings (above age 15) of the deceased girls have a higher probability of completing primary education. This 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. Regarding the death of spouses, we find a difference in the prewar level of livestock ownership. Households in which the husband died in the 1993–2002 period had significantly more livestock than households in which the husband was alive at the time of the survey.

These findings do not conflict with those of Bundervoet (2009). First, he also finds a higher level of prewar livestock among households with members killed, and second, his finding was based on the observation that fathers who were killed had more

educated children. However, we are interested in the level of education of the deceased children, not 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 were born in the 1971–1986 period and given that most of the above findings are not very conclusive or do not point in one clear direction, we conclude that selection bias caused by nonrandom killings is unlikely to bias our estimates in a particular direction.

We draw the same conclusion for the case of the migrants/refugees. Because we do not have data on the people who did not return to Burundi at the time of the survey, we attempt 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 in comparison with the stayers, these long-term refugees were slightly older, had a lower share of women, and had more educated heads of households. Had they returned, it would have increased the percentage that completed primary schooling in the nonaffected cohort (born 1971–1980). In that case, the estimates we find for the cohorts affected by violence can be considered an underestimate of the true effect.

## **VI. Conclusion**

There is no universal theory that allows us to predict the direction of the gender effect of violent conflict on schooling. In times 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. However, 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 conflict. For girls, we find that exposure to violent conflict reduces the gender gap in schooling, but only for girls from nonpoor households. This finding is confirmed across specifications and is consistent with the observations in Obura (2008), which present declining gross enrollment 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.

The losses that we find 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 socioeconomic profile of the victims. The magnitude of the observed effect in Burundi, a decline in the probability of completing primary schooling by 4 to 6 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 of completing the mandatory nine years in Tajikistan, is somewhat lower than our range of estimates of 7 to 17 percentage points for the exposed versus the nonexposed cohorts.

Policymakers should consider that conflict shocks may have different distributional consequences than the well-known economic or climatic shocks. Whereas price fluctuations or rain-level variability are known to affect the poorest part of the

population much more than the nonpoor 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 least vulnerable in the development economics literature—boys, in general, and boys from nonpoor households, in particular—are most severely affected by violent conflict in terms of educational attainment. Primary school completion rates for boys from nonpoor households decrease to the level of girls from nonpoor households in case of exposure to violent conflict. As a result, the gender gap in schooling narrowed considerably during the conflict in Burundi among nonpoor households.

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## Notes

<sup>1</sup> 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.

<sup>2</sup> For details on the sampling method, we refer to annex B of the survey report “Situation démographique et sociale du Burundi: Résultats de l’enquête socio-démographique et de santé de la reproduction ESDR Burundi 2002,” published by UNFPA Burundi.

<sup>3</sup> Although the civil war lasted longer (until 2005), we only consider the relevant part of the war for our identification strategy: the potential exposure of children of primary school age from the birth cohorts under consideration (1971–1986)

<sup>4</sup> We also clustered at the province-year of birth level, resulting in 256 clusters, which reduced the standard errors while yielding the same coefficients. However, we do not prefer this method because the intraprovince year of birth clusters may be correlated with each other. Cameron, Gelbach, and Miller (2008, 414) argue against clustering at the region-year level.

<sup>5</sup> This drawback was confirmed by Judson Caskey in an email to us. We used the `cgmreg` do-file on professor Caskey’s website to install the command.

<sup>6</sup> The figures in table 6, as in all tables, are rounded off at two decimals for aesthetic reasons. The coefficient and the standard error of the `gender*years in the camp` variable in column 4 are, for example,  $-0.0125$  [0.0032].

APPENDIX to “Schooling, Violent Conflict and Gender in Burundi”  
published in WBER, by Philip Verwimp and Jan Van Bavel

TABLE A1: Test for pre-war common trend in primary  
school completion rates at the province level

	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

Source: UNFPA Enquête Démographique et de Santé (2002).

\*\*\* 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 A2: Test for pre-war trend in school completion rates and duration of conflict, pre-war (1971-1980) cohorts only

Dependent variable: Child completed 6 years of primary schooling	(1)	(2)
	LPM	CGM
Duration of Conflict	-0.015	-0.010
	[0.07]	[0.09]
Age		-0.01*
		[0.02]
Violent Conflict * Age	0.0005	-0.0009
	[0.002]	[0.002]
Intercept	0.08	0.62*
	[0.05]	[0.21]
Province Fixed Effects	Yes	No
Birth Cohort Fixed Effects	Yes	No
Sample Size	3183	3183

Source: UNFPA Enquête Démographique et de Santé (2002)

\*\*\* significant at 1%, \*\* at 5%, \* 10%. Standard errors between brackets.

Standards errors are clustered at the province-year level and regressions are weighted with the sample weights provided in the survey.

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
Pre-war wealth	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
Pre-war wealth	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**
Pre-war wealth	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	

Source: UNFPA Enquête Démographique et de Santé (2002)