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War, Health, and Educational Attainment: A Panel of Children during Burundi's Civil War

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Abstract: This article examines the impact of war-induced ill early childhood health on educational attainment in early adolescence. Using data on a small panel of children we find that children who were malnourished at baseline had on average attained fewer grades than children of the same year of birth cohort who were healthier at baseline. The effect is particularly salient for the older children who were most exposed to violence in their early childhood years. We find that the worse educational status of malnourished children is due to both an enrolment effect and a poor school performance effect.

Keywords: childhood, health, education, nutrition, Burundi

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1. Introduction

In recent years, aid organisations and policymakers have become increasingly concerned with the issue of ill health in early childhood. This concern is driven by the finding that ill health and malnutrition in early childhood can have lifelong consequences. Ill health –and especially, malnutrition- in early childhood has been shown to be causally related to delayed school enrolment, fewer grades attained, and a lower probability of ever enrolling in school (Glewwe, Jacoby, and King, 2001; Alderman et al, 2001; Alderman, Hoddinott, and Kinsey, 2006; Alderman and Hoogenveen, 2009). In the economic realm, ill health in early childhood or even in utero reduces future labor supply and socioeconomic status (Almond, 2006; Meng and Qian, 2006).

If malnutrition in early infancy has lifelong consequences, then temporary shocks in early childhood which cause children to be malnourished can have permanent adverse effects. An increasing body of literature examines the impact of early childhood shocks on adolescent and adult outcomes. Findings are worrisome: Exposure to shocks in early childhood is associated with lower educational attainment, lower socioeconomic status, reduced labor supply and worse health in adulthood (Meng and Qian, 2006; Maccini and Yang, 2009; Banerjee et al, 2007). Even exposure to shocks while in utero is causally related to worse outcomes later in life (Almond, 2006).

One particularly violent type of adverse shock many children in the 1990s were exposed to is civil war. The incidence of civil wars peaked in the 1990s at 52 active armed internal conflicts, especially in Sub-Saharan Africa and Asia (Gleditsch et. al, 2002; UCDP, 2008). If early childhood exposure to violence adversely impacts childhood health, then the temporary consequences of exposure to violence might become permanent. This micro-level mechanism may partly explain the finding from cross-country studies that civil wars not only hurt immediate economic performance but also lower economic growth in the longer run (Kang and Meernik (2005) for instance find that nations that have experienced long and bloody civil wars can be expected to have double-digits negative growth in the first six post-conflict years). The rationale is that the shock's immediate and temporary effect on child health translates into lower educational attainment and productivity in adulthood, which in turns negatively affects economic growth.

In this paper we pull together an early childhood shock (exposure to violence in Burundi's civil war), its immediate impact on the health of under-fives, and the longer-run consequences for school enrolment and attainment. We exploit panel data from two household surveys in rural Burundi (1998/99 and 2007) coupled with event data on the spatial and temporal intensity of the war between 1994 and 1998. The link between the early childhood shock (exposure to violence between 1994 and 1998) and the nutritional status of under-fives in Burundi has already been established by Bundervoet, Verwimp and Akresh (2010). Using the 1998/99 household survey coupled with the event data on the evolution of the war, they found that each additional month of exposure to the war decreased height-for-age of under-fives by *0.047* standard deviations and that the average exposed child had a height-for-age that is one standard deviation lower than that of a non-exposed child. The question is now whether this health effect of the war has had consequences for schooling later in life. This is the main objective of this paper.

The paper proceeds as follows: The next section sketches the context of the Burundian civil war. Section 3 introduces the data that will be used throughout the paper. The identification strategy and the econometric specifications are discussed in Section 4, while Section 5 presents the main results. The final section concludes.

2. Setting and Data

1. The Conflict in Burundi

The latest episode of conflict in Burundi started on October 21 1993, when elements of the Tutsi army staged a coup to dispose of the first ever democratically elected president, a Hutu civilian, elected only a few months before. Although the coup was unsuccessful, the president and his closest aides were killed. As the news reached the hills, peasant-supporters of the president and his party, FRODEBU, committed large-scale massacres of Tutsis and "UPRONA-Hutus"[2]. Chrétien (1997) describes the massacres saying districts in certain provinces were "almost completely 'cleansed' of all Tutsi elements." In an operation to restore order, the army moved in and killed scores of Hutus. Overall, approximately 100,000 people were killed in the week or so following the assassination of the president (UNFPA, 2002). The massacres were labeled genocide by the United Nations (UN, 1996).

Burundi had known several intense massacres before, but social uprisings had always been silenced through brutal army repression. However, in the wake of the 1993 events, as many as seven Hutu rebel groups emerged, formed or supported by prominent Frodebu politicians. This marked the beginning of one of the most brutal civil wars in recent history, which claimed the lives of at least 300,000 people (Chretien, UNFPA, UVIN). The war continued unabated until 2003, when the main rebel group CNDD-FDD signed a peace agreement and descended victoriously on the capital city of Bujumbura[3]. Since one rebel group stayed outside the peace talks, pockets of insecurity and violence persisted, especially in the provinces of Bujumbura

[2] FRODEBU: Democratic Front of Burundi. Uprona-Hutus are Hutus loyal to the former Tutsi state party (Uprona) and are therefore seen as traitors to the Hutu cause.
[3] CNDD-FDD: National Council for the Defense of Democracy-Forces for the Defense of Democracy.

Rural, Bubanza, and Cibitoke. The last rebel group was transformed in a political party in 2009, finally ending a 16-year civil war.

The socioeconomic consequences of the war were disastrous: Per capita GDP fell by more than half from USD 187 in 1992 to USD 91 in 2004, leaving Burundi as the world's poorest country. Rural poverty headcount doubled from 36% in 1992 to 70.5% in 2004 (IMF, 2007). Life expectancy decreased with over five years during the war, dropping to 46.1 years in 2004. Child vaccination coverage fell from 80% before the war to 47% in 1998-1999 but –fortunately– picked up again in recent years. Parallel with this, infant mortality increased during the war but fell back to its pre-war level at the end of the war. The number of cases of malaria per 100,000 inhabitants showed a three-fold increase between 1992 and 2001, mainly as a result of steadily deteriorating sanitary conditions and widespread displacement (IMF, 2007). Agricultural production per capita decreased by 30% between 1992 and 2004. As a result, the prevalence of undernourishment increased from 47% in 1992 to 67% in 2003 (FAO, 2008).

2. The Data

The Burundi Priority Survey, organized by the Burundi Institute of Statistics and Economic Studies in cooperation with the World Bank, was designed to be nationally representative and took place between October 1998 and March 1999. The survey's goal was to evaluate the country's socioeconomic situation following five years of civil war (Republic of Burundi 1998). Overall, the survey collected data on 6,668 households, 3908 of whom lived in rural areas. The 3,908 rural households were spread out over 391 hills (the primary sampling units). 1,064 of the 3,908 households were randomly selected for an anthropometrics module that measured and weighted all children between 6 and 60 months of age[4]. Overall, the 1,064 selected households included 1,442 children between 6 and 60 months. Data on 246 children could not be used due to missing height data or measurement errors. In sum, the 1998/99 Priority Survey provided complete anthropometric data on 1,196 children between 6 and 60 months of age.

The 1998/99 Burundi Priority Survey was planned as a cross-section without intent to collect longitudinal data. Despite this, the records were carefully preserved in the archives of national statistics institute, making the recontacting of original respondents possible. In 2007, a team of researchers organized a follow-up to the 1998/99 Priority Survey. Due to budget limitations, it was impossible to try to track and re-survey all 3908 rural households in the 391 survey. Therefore, we decided to revisit 100 of the 391 baseline sites with the idea to track and re-survey 1000 original (1998/99) rural households[5]. To choose which hills to revisit, we listed, per stratum, all hills surveyed in 1998/99 and then picked each fourth hill until we selected 100 hills. The enumerators were instructed to track and resurvey, within each hill, the 10 original households. Overall, we managed to track and resurvey 874 of the 1000 selected households. Given the relatively long time-span between the two surveys (nine years) and the high incidence of displacement during Burundi's war (a UNFPA survey estimated that 50% of all Burundians had been displaced at least once during the 1994-2002 time period –see UNFPA (2002)), this can be considered a success.

There are several potential selection effects that are of concern to us. Table 1 shows that of the 1000 households selected in the 2007 sample, only 251 were included in the anthropometrics module during the 1998/99 survey (and have valid baseline data on height-for-

[4] Bundervoet, Verwimp, and Akresh (2010) examine in detail the potential selection bias arising from selecting only 1,064 households for the anthropometrics module. They find that included and excluded households and children look similar on observable dimensions.
[5] In the 1998/99 survey, 10 households were chosen randomly in each selected survey site. The survey site was the hill (*sous colline*), the smallest administrative unit.

age z-scores of the children in the household). Those households consisted of 293 children between 6 and 59 months of age in 1998/99. However, 14 households could not be traced during the 2007 survey, which means we lose information on the 19 children under five living in those households. The 237 households we managed to track and reinterview included 274 children between 6 and 59 months during the 1998/99 survey. Of these 274 children, 25 had died since the 1998/99 survey. Another six children did not live with their original household anymore and could not be tracked. Overall, we managed to track and re-interview 243 of the 293 children selected in the 2007 sample[6].

The 50 missing children potentially pose a selection bias problem. In Appendix A we use two alternative approaches to evaluate this problem. The first approach is to compare the 50 missing children with the 243 included children along as many observable dimensions as possible. Results for this comparison are presented in Appendix Table 1, which shows the mean differences in covariates for the included and excluded children. The children appear to come from fairly similar types of households, with the percentage of households headed by a woman and the proportion of adults in the household who are literate not showing significant differences. Children who were not re-interviewed in 2007 lived in households headed by slightly older persons than children who were re-interviewed (difference statistically significant at 10%). Excluded children lived in households where the household head was more likely to be educated (to have completed primary school), though the difference misses statistical significance at conventional levels. Livestock holdings of the households of the excluded children are lower than those of the included children, though the difference is not statistically significant[7]. The 50 children who were not re-interviewed in 2007 were on average a little

[6] This small sample size can be considered the main drawback in the analysis.

[7] Verwimp and Bundervoet (2009) examine the differences between the 1000 households selected in the 2007 sample and the 874 households who could actually be tracked and resurveyed. They find –in line with the findings

younger than the interviewed children and were somewhat less likely to be male, though the differences are small and statistically insignificant. Excluded children had a lower height-for-age z-score in 1998/99 than the included children, significant at the 10% level. This difference is driven by the 25 children who died between the survey rounds, whose average height-for-age z-score was -*2.97* in 1998/99 (and which probably had something to do with their premature death).

To examine whether the differences noted in Table A1 are likely to produce selection effects, we modify the method proposed by Fitzgerald, Gottschalk, and Moffit (1998) to analyze attrition in a panel data setting. In Appendix Table 2, we present results of a logit regression where the dependent variable is the probability of being included in the 2007 sample. For the 243 children included in both the 1998/99 and 2007 sample, the dependent variable is coded one. The 50 children only included in the 1998/99 sample are coded zero. The results in Table A1 show that two baseline variables are statistically significantly associated with attrition: Children who lived in households with an older head in 1998/99 were less likely to be included in the 2007 sample, and children with better height-for-age z-scores in 1998/99 were more likely to be re-interviewed in 2007. The other variables do not appear to have significantly influenced attrition (although the education of the household head approaches statistical significance at the 10%-level).

Overall, it seems unlikely that selection effects will considerably bias the results of the analysis. The finding that the children who died between the survey rounds were in poor nutritional status at baseline (low height-for-age z-score) can even be considered reassuring for the identification strategy (see next Section): As premature death can be considered the ultimate lack of human capital accumulation, any effect we would find of early childhood nutritional

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presented here- that households with an older head and fewer livestock at baseline were less likely to be resurveyed in 2007.

status on subsequent human capital accumulation is likely to be a lower bound of the true effect. Nevertheless, in the empirical section we will estimate Heckman's (1978) sample selection model to test the robustness of the results to attrition.

3. The Empirical Strategy

The empirical strategy we use resembles the "standard" strategy used in similar research. In its most simple form, the econometric specification would be:

$$G_{i,t+1} = \beta_0 sex_i + \beta_1 age_i + \beta_2 haz_{i,t} + \gamma Z_{i,t} + \varepsilon_i \quad (1)$$

Where $G_{i,t+1}$is the grade attained by child i in time period *t+1* (the 2007 survey), $haz_{i,t}$ the height-for-age z-score of child i in time-period t (the 1998/99 survey) and $Z_{i,t}$ a vector of baseline characteristics of the household and community of the child. ε_i represents unobserved heterogeneity in educational attainment.

Equation (1) is likely to produce a spurious relation between early childhood nutrition and future educational attainment. Since height-for-age is a measure of long-run nutritional intake, older children in poor countries have worse z-scores than younger children (they have had more time to accumulate a poverty-related deficit –see Duflo (2005) and Martorell and Habicht (1986)). Since older children are also more likely to be in higher grades (just because they are older), equation (1) would produce a negative estimate of β_2, suggesting that better nutrition in early childhood leads to worse performance in school. Table 2 shows that this is indeed the case in our sample. In the 1998/99 survey, the average z-score of young children (defined as younger than 24 months of age during the baseline survey) was -1.79, compared to -2.46 for older children (between 24 and 59 months at baseline). The median level of education as measured by the 2007 survey amounted to the second grade of primary school for young children and the third grade for older children. To avoid drawing wrong conclusions, we need to add the interaction between age and first-period health to equation 1:

$$G_{i,t+1} = \beta_0 sex_i + \beta_1 age_i + \beta_2 haz_{i,t} + \beta_3 (age_i * haz_{i,t}) + \gamma Z_{i,t} + \varepsilon_i \quad (2)$$

This article is fundamentally interested in the estimation of β_3: The effect of early childhood nutrition on educational attainment of children of the same age. The hypothesis is that β_3 is positive and statistically distinguishable from zero: For children of a given age, better nutrition in early childhood leads to a higher number of grades attained and vice versa.

There are a number of threats to the identification strategy. The first is related to the role of poverty as a potential driver of both health and education. If children in poor households were less likely to go to school (or more likely to drop out or repeat grades) and also less likely to have been well-nourished in early childhood, then any relation between early childhood nutrition and subsequent educational attainment might just be due to poverty. Table 3 shows baseline height-for-age z-scores of young and older children in poor and non-poor households. In both age categories, children in poor households did not have worse nutritional status in 1998/99 than children in non-poor households[8]. This suggests that poverty is unlikely to drive any association we would find between early childhood health and educational attainment. In the main analysis, we will add a baseline poverty dummy to specification (2).

At the level of the province, there are likely to be factors, both observed and unobserved, that simultaneously influence child health and educational attainment. A prominent example of this would be the infrastructural endowment of the province: If health and education infrastructure are concentrated in particular provinces, we would find spurious effects between health and education. To control for this, we add a full range of provincial dummy variables.

[8] The distinction between poor and non-poor households is based on a 1998 poverty line estimated by Bundervoet (2006).

As argued by Glewwe and Jacoby (1995) and Alderman and Hoogenveen (2009), investment in child health and schooling probably stem from similar unobserved household preferences, creating endogeneity bias of childhood nutritional status. If households who feed their children better are also more likely to send and keep them in school, we will overestimate the effect of nutrition on attainment. To control for this, we will instrument baseline nutritional status (during the 1998/99 survey) by the degree of exposure to the civil war during the period preceding the baseline survey. Bundervoet, Verwimp and Akresh (2010) studied the impact of pre-baseline exposure to war on nutritional status at baseline (the 1998/99 survey), and found that each additional month of war exposure reduced baseline height-for-age of children under five by 0.047 standard deviations. We will adopt their measure of war exposure to estimate nutritional status in 1998/99 in the first stage regression.

A final complication is related to the censored nature of the outcome variable: highest grade attained so far. The highest grade attained in 2007 does not reflect the highest grade ever attained for the children who were still in school in 2007. They will most likely go on to accumulate more grades. The 243 children with complete data in the 2007 sample are between 8 and 14-years-old. 206 of them (84.8%) were still in school at the time of the survey; eight children (3.3%) had been in school but already dropped out; the remaining 29 children (11.9%) had never been enrolled in school. The first group of children had not yet completed their education in 2007, and hence their highest grade attained is right censored. The eight children who already dropped out will most likely never return to school; their highest grade is uncensored. The children who had never been enrolled in school in 2007 likely consist of both censored and uncensored observations. 23 of those children are between eight and 12-years-old, and children in this age-interval still enrol in primary school in Burundi. Six children are 13 or older. They are less likely to ever enrol in school. In sum, this means that 94.2% of observations are censored (children still in school or expected to enrol in school) and 5.8% uncensored. Given the small proportion of uncensored observations, results of ordered and censored ordered probit will most likely be similar. In the main analysis, we will only present ordered probit estimations.

4. Descriptive Statistics

Our analysis focuses on the impact of nutrition in early childhood, measured by height-for-age z-score in the 1998/99 survey, on educational attainment conditional on age in 2007. Figure 1 plots the relationship between age in years in 2007 and educational attainment in 2007. The dashed line presents the relationship for the children who were stunted at baseline (height for age z-score smaller than -*2*), while the full line plots the association for the non-stunted children. For children of nine years-old, the highest grade attained is higher for non-stunted than for stunted children. The stunted children seem to catch up between nine and 12 years of age. At age 12 stunted and non-stunted children have on average the same schooling attainment. The big divergence occurs for children older than 12 years: While the average number of grades attained for non-stunted children between 12 and 14 years of age amounts to roughly three, it drops spectacularly for the stunted children in the same age-range: The average attainment of 13-year-olds who were stunted at baseline amounts to two grades, while for stunted 14-year-olds it amounts to zero grades.

The patterns in Figure 1 warrant some preliminary findings. First, any difference in educational attainment between stunted and non-stunted children is unlikely to be the result of stunted children delaying their school enrolment. Because stunted children are small for their age, they often start school at a later age. If the difference between stunted and non-stunted children would be the date of enrolment, we would expect to see a constant difference in highest grade attained between stunted and non-stunted children of the same age. Figure 1 is not compatible with this: highest grade attained is similar for stunted and non-stunted children up until the age of 12, after which the gap increases suddenly. This leaves two possible explanations: First, it is possible that the older children who were stunted in baseline never entered school. Since older children are more likely to be stunted (and more likely to suffer from severe stunting) than younger children, these children may have been considered too weak, too small, or otherwise unfit to go to school. Second, it is possible that the older stunted children – given the impact of early childhood malnutrition on cognitive development- repeated grades more often than the non-stunted children, or dropped out of school earlier.

Tables 4 and 5 support, albeit weakly, both hypotheses: Table 4 shows that children who were stunted at baseline were somewhat less likely to enrol in school between the survey rounds than non-stunted children (enrolment rate of 0.87 for stunted vs. 0.91 for non-stunted children). Given that the stunted children were on average older (11 years in 2007) than the non-stunted children (10.8 years in 2007), we would expect to see the opposite pattern if childhood health and subsequent school enrolment were statistically independent.

Table 5 shows educational attainment (in 2007) by age for stunted and non-stunted children who entered school. Conditional on age, the highest grade achieved in 2007 was lower for stunted than for non-stunted children (for children of nine, 10 or 13 years of age) or the same for both categories (children of 11 and 12 years of age). Since Table 5 exclude the children who never enrolled in school, these differences cannot be due to an enrolment effect of childhood stunting. In sum, children who were stunted in 1998/99 were somewhat less likely to ever enrol in school and somewhat more likely to repeat grades and/or drop out conditional on ever being enrolled in school.

5. Empirical Results

Table 6 presents results of ordered probit estimation of equation (2). Child age in 2007 is entered as a year-of-age dummy variable ranging from nine to 14 (the youngest children –eight years- are the reference). The years-of-age dummy variables are interacted with baseline height-for-age z-scores to identify the conditional effect of childhood nutrition on subsequent school attainment. We also add a dummy variable indicating whether the child was born after August, since those children usually have lower educational attainment than children of the same birthyear born earlier in the year (a child turning six on August 31 can start school the next day, while a child turning six in early September has to wait for the next schoolyear to enrol). Provincial dummy variables are included to control for province fixed effects.

In the basic specification of column (1), we find a negative effect of baseline height-for-age z-score on highest grade attained in 2007: the better childhood health, the lower subsequent school attainment. As explained in Section 3, this counter-intuitive result can be explained by the fact that in poor countries older children have worse z-scores than younger children. Since older children are also usually enrolled in higher grades, we find a spurious negative effect between baseline health and subsequent schooling attainment. The coefficients of interest in the regression are the parameters of the interaction terms between the years-of-age dummies and baseline health. We find positive and statistically significant coefficients on all interaction terms: Conditional on age, better health in early childhood (as measured by the child's z-score in the baseline survey) led to a higher number of grades attained in 2007. The magnitude of the effect increases with age: The impact of childhood health on schooling is higher for older than for younger children. Older children had had more time to accumulate health-induced schooling deficits. The coefficients of all the year-of-age dummies (not reported in Table) are positive and statistically significant: relative to the youngest children (eight years), older children had on average higher schooling attainments in 2007.

In column (2), we add baseline household characteristics to the basic specification. We find that education of the household head (in most cases the child's father) positively influences schooling attainment of children living in the household. Children in households who were poor at baseline (based on an absolute poverty line estimated by Bundervoet (2006)) had on average lower educational attainment in 2007, though the effect is far from statistically significant. The age and sex of the household head do not affect child schooling attainment either (coefficients are small and statistically indiscernible from zero). Adding household characteristics does not change the results on the key variables: Conditional on age, children who were stunted in 1998/99 had attained fewer grades in 2007 compared to non-stunted children. The effect of childhood health on schooling is more than twice as high for the oldest children than for the youngest children in the sample.

The third column of Table 6 presents results corrected for attrition bias using Heckman's sample selection model. In Section 2, we showed that children living in households with an older household head in 1998/99 and children with worse height-for-age z-score at baseline were less likely to be reinterviewed in 2007. To perform the sample selection model, we estimate in a first step the probability of a child being included in both the 1998/99 and 2007 survey based on baseline variables influencing this probability (baseline height-for-age z-score, age and education of the household head, and province of residence dummy variables). Based on the estimated probabilities obtained from this model, we calculate the inverse mill's ratio and add this as an additional control variable in a second stage ordered probit (the same specification as in column (2) of Table 6). Results of this second stage regression are presented in column (3). We find that controlling for potential selective attrition does not change the results. Differences in coefficients between columns (2) and (3) in Table 6 are small and nothing happens to the levels of statistical significance. Attrition bias is not likely to be a cause for concern in the analysis.

The results presented so far support the hypothesis of an adverse effect off ill health in early childhood on schooling attainment eight to nine years later in life: Conditional on age, children who had worse height-for-age z-scores in 1998/99 had fewer grades completed in 2007. What is however the magnitude of this effect? Table 7 presents simulations based on the results of regression (2) in Table 6. The simulations compare for each year-of-age cohort the estimated probability of having attained a given grade for a child with a baseline height-for-age z-score of -*2.22* (the sample average representing a moderately malnourished child) and a child with a baseline z-score of zero (a child in line with growth standards of well-nourished children). Other control variables are held at their sample mean. For the youngest children in the sample (nine and 10 years of age) better baseline health does not seem to spur educational attainment: The probability of a child of nine or 10 years completing the third grade of primary school is actually somewhat higher if the child was malnourished at baseline. For children of 11 years of age and older however malnutrition in early childhood seems to have been detrimental for subsequent schooling success (the more so the older the child). The probability of an 11-year-old child who was malnourished at baseline to complete the 4th grade of primary school amounts to 0.43, only 3 percentage points lower than the probability for a healthy child (0.46). For 12-year-old children, the difference in the probabilities of completing the 4th grade already rises to six percentage points. The key divergence however emerges at ages 13 and 14: While a 13-year-old child who was healthy at baseline has an estimated probability of 0.58 to complete the 4th grade, a malnourished 13-year-old only has a 0.33 chance. A 14-year-old child has an overall small probability to make it past 5th grade, but this probability is still a lot higher if the child was healthy at baseline (probability of 0.24) than if the child was malnourished (probability of 0.01).

Tables 6 and 7 show that the association between ill childhood health and educational outcomes later in life is mainly driven by the older children in the sample. Many of the older children suffered from severe malnutrition at baseline and performed poorly in school afterwards (if they were ever enrolled in school to begin with). Although we control for the effects of the province of residence and the effects of age, there might still be something specific to these older children that makes that they were both more likely to be malnourished as child and less likely to perform well in school (maybe something happened in their province at a certain time which makes that they were more likely to be malnourished and un(der)schooled). To address this concern, we add a province-specific time trend to specification (2). This time trend (the interaction between the province of residence and the age of the child) effectively controls for events that happened in a specific province during a specific year and which might have affected both nutritional status and educational outcomes. Adding a province-specific time trend (column (4) of Table 6) does not change the results on our key variables: Conditional on age, better health in early childhood is associated with a higher number of grades attained in early adolescence.

As already argued in Section 3, factors that influence childhood malnutrition may also influence education, resulting in the endogeneity of childhood nutritional status (Glewwe and Jacoby, 1995; Behrman, 1996; Glewwe, Jacoby, and King, 2001; Alderman, Hoddinott, and Kinsey, 2006; Alderman and Hoogenveen, 2008). To alleviate this potential endogeneity bias, we will instrument childhood nutritional status in the 1998/99 survey by the child's exposure to the war between 1994 and 1998[9]. The first stage regression includes a binary variable indicating whether the child was at all exposed to the war, a continuous variable indicating the length of exposure, and a series of individual – and household-level control variables. In the second stage, educational attainment in 2007 is regressed on the estimated height-for-age z-scores obtained from the first stage regression. Table 8 shows the results of the

[9] In a related paper using only the 1998/99 data, Bundervoet, Verwimp and Akresh (2009) study the impact of war exposure on nutritional status, and find that the number of months of war exposure between 1994 and 1998 negatively affects height-for-age z-score in the 1998/99 survey: each additional month of war exposure lowers height-for-age by 0.047 standard deviations.

second stage regression. While the magnitude of the coefficients is higher in the instrumented regression, the qualitative interpretation does not change: Conditional on age, children who were better nourished at baseline had on average attained more grades in 2007. The impact is however only statistically significant for ages 13 and 14 (the older children), compared to ages 10, 11, 12, 13 and 14 for the OLS estimates of Table 6. As such, the results of the 2SLS mirror the pattern of Figure 1.

The previous results show that the negative education effect of early childhood malnutrition is mainly due to the poor school performance of the older children. Those children had on average worse height-for-age z-scores at baseline and were most exposed to the war. If those children never entered school to begin with, due to their ill health or war-related factors (and hence have a schooling attainment of zero), the effect we find of early childhood malnutrition on education could be entirely driven by an enrolment effect. To explore this possibility, the first column of Table 9 estimates specification (2) for the subsample of children who ever entered school (the children who never went to school are excluded from the analysis). If the results we find are solely driven by enrolment, we would expect to find small and insignificant coefficients on the interaction terms (conditional on age, there would not be an effect of baseline z-scores).

Overall 214 children in our sample have ever enrolled in school. Although the magnitude of coefficients differs in the subsample of children who ever went to school (the magnitude of estimated coefficients is generally higher -see Table 9), the qualitative interpretation of the regression results is similar as for the full sample: For children (ever) enrolled in school, educational attainment for children who were malnourished at baseline is on average lower than that of children who were well-fed at baseline. This means that the effect of childhood nutrition on educational attainment does not run exclusively through enrolment but also through the worse school performance of malnourished children[10]. Remember that possible delayed school enrolment of malnourished children cannot account for the results we find, as the patterns in Figure 1 are inconsistent with delayed school enrolment (see Section 4).

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[10] Results of a logistic regression estimating school enrolment on baseline age, height-for-age z-score and their interaction (and a series of individual – and household level control variables) show that conditional on age better nourished children were more likely to ever enroll in school, the effect being statistically significant at *p.=0.066* (results not presented in this paper but available upon request).

6. Conclusions

This paper pulls together an early childhood shock (exposure to violence during Burundi's civil war), its immediate impact on early childhood health and the impact of childhood health on educational attainment in early adolescence. We find that children who were in a poor nutritional state in 1998/99 –proxied by low height-for-age z-scores- had on average accumulated fewer grades in 2007 compared to better nourished children of the same year-of-birth cohort. The impact is highest for older children, who were most exposed to the war in early childhood. In tentatively exploring the mechanisms, we find that the worse school performance of undernourished children is due to both an enrolment effect and a grade-repeating or early drop-out effect: Conditional on age, children who were malnourished at baseline were less likely to ever enrol in school and those who did enrol in school accumulated less grades than same-age children who were well-nourished at baseline.

In combination with the finding of a causal impact between war exposure and early childhood health in Burundi (see Bundervoet, Verwimp and Akresh (2009)), the findings of this paper document a micro-level pathway between war and poor longer-run economic performance: If exposure to war in early childhood leads to ill health, ill health in early childhood leads to poor school performance, then -insofar as low educational attainment is correlated with worse outcomes later in life- exposure to war in early childhood will have lifelong adverse economic consequences. This mechanism may in part account for the macro-level finding that wars have negative consequences for economic performance long after the war ends.

The empirical findings presented here corroborate the findings of a recent and rapidly growing body of literature on the long-run impacts of ill health in early childhood. By now, the long-run adverse impact of chronic malnutrition before the age of five is well-established. Despite this, chronic malnutrition (low height-for-age) is still largely neglected by the humanitarian and aid communities. Emergency levels for child malnutrition are traditionally based on the prevalence of acute malnutrition (low weight-for-age), and the humanitarian apparatus typically comes into action when the prevalence of acute malnutrition exceeds a specific threshold. This however neglects the fact that levels of acute malnutrition in a region can be very low while chronic malnutrition affects half or more of the region's children (as is the case in many countries in Africa). Given that chronic malnutrition is both a public health (chronic malnutrition negatively affects cognitive development and increases premature mortality) and an economic disaster (since it leads to lower education and diminished productivity and lower wages in adulthood), investments in better nutrition in early childhood could potentially have large positive effects on a country's or a region's future economic performance. Although the funding required to make dramatic reductions in chronic malnutrition possible is likely to be substantial given the sheer scale of the problem, pay-offs could potentially be large too.

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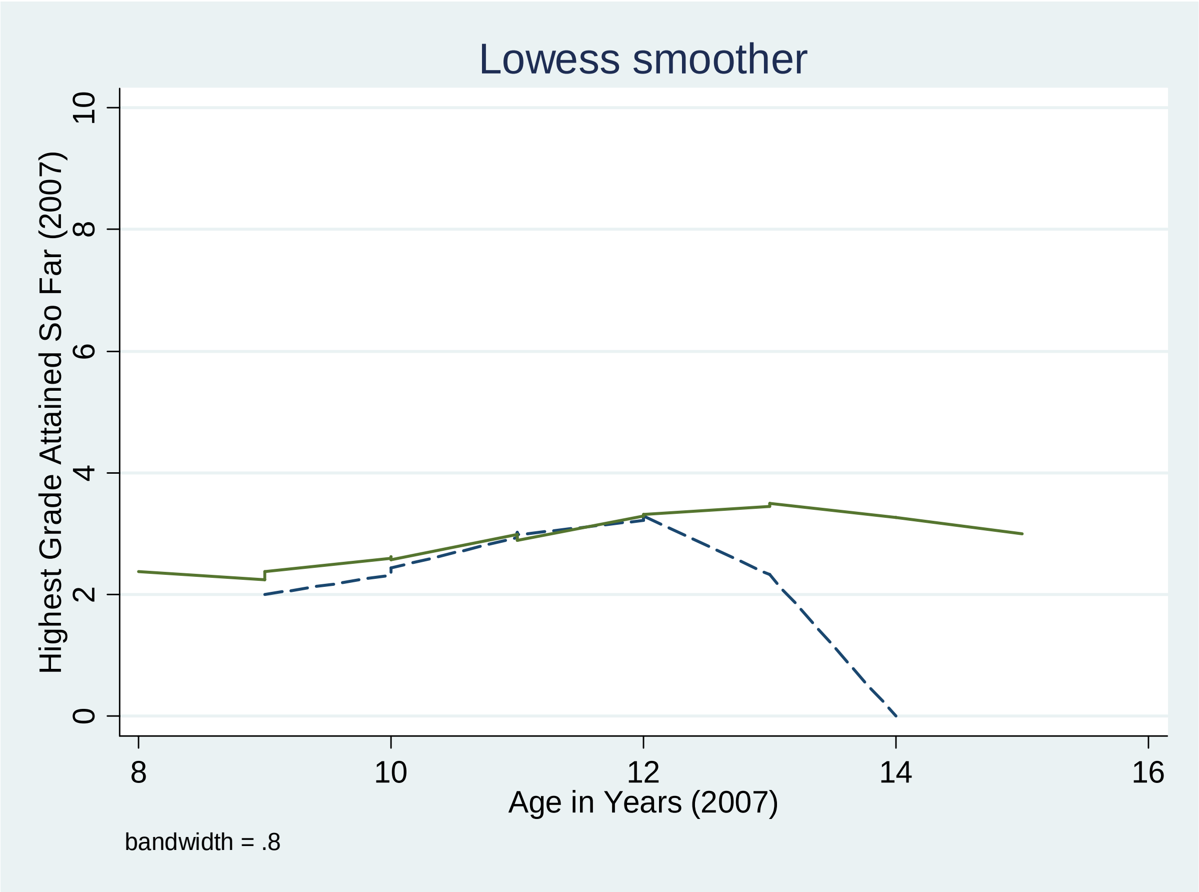
Figure 1: The Association between Age and Highest Grade Attained (So Far), Stunted vs. Non-Stunted Children



<u>Notes</u>: Kernel-weighted local polynomial regression (using Epanechnikov kernel) of highest grade attained on age in years. Full line represents non-stunted children, dashed line represents stunted children. Child is stunted if his/her baseline height-for-age z-score is below -2. Data Source: Republic of Burundi and World Bank (1998) and ISTEEBU (2007).

Table 1: Number of Household/Children Selected for Anthropometrics Module in 1998/99 that were Re-Interviewed in 2007

	1998/99 Survey	Selected in 2007 Sample	Tracked and Interviewed in 2007 Survey
Number of Households Selected for Anthropometrics Module in 1998/99	1064	251	237
Number of Children (6-59 Months of age with complete anthropometric information 1998/99 survey)	1196	293	243

Notes: During the baseline survey, 1,064 households were randomly selected for the anthropometrics module (1,196 children). 251 of those households were reinterviewed in 2007 (293 children at baseline). 243 of the 293 children were still present/alive in 2007.

Table 2: Height-for-Age Z-Score (1998/99) and Subsequent Educational Attainment (2007) for Young and Older Children

	Young Children (< 24 Months in the 1998/99 survey)	Older Children (Between 24 and 59 months in the 1998/99 Survey)
Height-for-Age Z-Score (1998/99 Survey)	-1.79	-2.46
Educational Attainment (2007 Survey)	2nd Grade	3rd Grade
N	87	156

Notes: Educational attainment is measured on an ordinal scale (highest grade completed so far). Figures in the Table correspond to median level of education. Data Source: Republic of Burundi and World Bank (1998) and ISTEEBU (2007).

Table 3: Baseline Height-for-Age z-Scores for Young and Older Children in Poor and Non-Poor Households

	Poor Households (1998/99)	Non-Poor Households (1998/99)	Mean Difference
Young Children	-1.83	-1.71	-0.13
	[0.20]	[0.22]	[0.33]
Older Children	-2.41	-2.57	0.15
	[0.13]	[0.20]	[0.24]
N	169	74	243

Notes: Young children are defined as being younger than 24 months in the 1998/99 survey. Older children were at least 24 months-old in the 1998/99 survey. The distinction between poor and non-poor households is based on the poverty line estimated by Bundervoet (2006). Data Source: Republic of Burundi and World Bank, 1998.

Table 4: Childhood Stunting and Subsequent School Enrolment

	Stunted at Baseline	Not Stunted at Baseline	Mean Difference
% Ever Enrolled in School	0.87	0.91	-0.04
	[0.028]	[0.029]	[0.042]
Age in Years in 2007	11	10.8	0.2
	[0.106]	[0.161]	[0.185]
N	143	96	243

Notes: A child is stunted at baseline if his/her height-for-age z-score was below -2 in the 1998/99 survey.
Data Source: Republic of Burundi and World Bank (1998) and ISTEEBU (2007).

Table 5: Childhood Stunting and Subsequent Schooling Attainment

	Stunted at Baseline	Not Stunted at Baseline
Nine Years of Age	2nd Grade	3rd Grade
10 Years of Age	2nd Grade	3rd Grade
11 Years of Age	3rd Grade	3rd Grade
12 Years of Age	4th Grade	4th Grade
13 Years of Age	3rd Grade	4th Grade
N	125	90

Notes: A child is stunted at baseline if his/her height-for-age z-score was below -2 in the 1998/99 survey. Only the 215 children who ever enrolled in school are included in the Table.

Table 6 : Baseline Height-for-Age Z-Score and Educational Attainment, 1998/99-2007

Dependent Variable: Highest Grade Attained in 2007	(1)	(2)	(3)	(4)
Height-for-Age Z-Score	-0.626***	-0.599**	-0.462	-0.625*
	[0.211]	[0.297]	[0.294]	[0.335]
Age 9 * Height-for-Age Z-Score	0.554*	0.532	0.511	0.324
	[0.308]	[0.384]	[0.403]	[0.434]
Age 10 * Height-for-Age Z-Score	0.591**	0.560*	0.547*	0.575*
	[0.232]	[0.317]	[0.326]	[0.350]
Age 11 * Height-for-Age Z-Score	0.613**	0.627*	0.608*	0.658*
	[0.258]	[0.334]	[0.357]	[0.345]
Age 12 * Height-for-Age Z-Score	0.664***	0.671**	0.659**	0.728**
	[0.230]	[0.295]	[0.306]	[0.324]
Age 13 * Height-for-Age Z-Score	0.896***	0.882**	0.864**	0.993***
	[0.316]	[0.379]	[0.395]	[0.387]
Age 14 * Height-for-Age Z-Score	1.299***	1.310***	1.302***	1.794***
	[0.294]	[0.370]	[0.374]	[0.388]
Female Child	-0.083	-0.082	-0.081	-0.108
	[0.156]	[0.165]	[0.164]	[0.176]
Age Head of Household		-0.011	-0.005	-0.011
		[0.012]	[0.026]	[0.012]
(Age Head of Household)2		0	0	0
		[0.000]	[0.000]	[0.000]
Head of Household Educated		0.310*	0.310*	0.344*
		[0.171]	[0.172]	[0.182]
Female Head of Household		-0.036	-0.036	-0.072
		[0.244]	[0.243]	[0.250]
Household Poor at Baseline		-0.147	-0.146	-0.171
		[0.202]	[0.203]	[0.222]
Year of Age Dummies	Yes	Yes	Yes	Yes
Born After August Dummy	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes
Province-Specific Time Trend	No	No	No	Yes
Pseudo R Squared	0.09	0.1	0.1	0.12
N	243	243	293	243

Notes: Column (3) shows the results of estimating Heckman's sample selection model to correct for attrition bias. All standard errors are robust and clustered at the level of the village. Household head is educated is s/he completed at least primary school. Data source: Republic of Burundi and

World Bank (1998) and ISTEEBU (2007). ***: Statistically significant at the 1%-level; **: Statistically significant at the 5%-level; *: Statistically significant at the 10% level.

Table 7: Estimated Impact of Baseline Health on Educational Attainment

Probability of Having Achieved:	Baseline Height-for-Age z-Score of -2.22	Baseline Height-for-Age z-Score of 0
3rd Grade at 9 Years	0.66	0.6
3rd Grade at 10 Years	0.71	0.68
4th Grade at 11 Years	0.43	0.46
4th Grade at 12 Years	0.56	0.62
4th Grade at 13 Years	0.33	0.58
5th Grade at 14 Years	0.01	0.24

Notes: The probabilities in the Table result from simulations based on the estimates of regression (2) in Table 6.

Table 8: Second Stage Regression of School Attainment in 2007 on Estimated Height-for-Age Z-Scores in 1998/99

Dependent Variable: Highest Grade Attained in 2007	(5)
Estimated Height-for-Age Z-Score	-1.622*
	[0.861]
Age 9 * Est. Height-for-Age Z-Score	1.267
	[0.920]
Age 10 * Est. Height-for-Age Z-Score	0.796
	[0.752]
Age 11 * Est. Height-for-Age Z-Score	0.918
	[0.693]
Age 12 *Est. Height-for-Age Z-Score	0.929
	[0.674]
Age 13 * Est. Height-for-Age Z-Score	2.251***
	[0.812]
Age 14 * Est. Height-for-Age Z-Score	3.554***
	[1.361]
Female Child	-0.312
	[0.237]
Age Head of Household	-0.009
	[0.012]
(Age Head of Household)2	0
	[0.000]
Head of Household Educated	0.309*
	[0.161]
Female Head of Household	-0.093
	[0.268]
Household Poor at Baseline	-0.073
	[0.194]
Year of Age Dummies	Yes
Born After August Dummy	Yes
Province Fixed Effects	Yes
Province-Specific Time Trend	No
Pseudo R Squared	0.105
N	243

Notes: Height-for-age z-Scores are estimated in a first-stage regression following the specification of Bundervoet, Verwimp and Akresh (2009). All standard errors are robust and clustered at the level of the village. Household head is educated is s/he completed at least primary school. Data source: Republic of Burundi and World Bank (1998) and ISTEEBU (2007). ***: Statistically significant at the 1%-level; **: Statistically significant at the 5%-level; *: Statistically significant at the 10% level.

Table 9: Height-for-Age Z-Score and Educational Attainment in Sample of School-enrolled children

Dependent Variable: Highest Grade Attained in 2007	(6)
Height-for-Age Z-Score	-1.051***
	[0.385]
Age 9 * Height-for-Age Z-Score	1.543**
	[0.650]
Age 10 * Height-for-Age Z-Score	1.350***
	[0.436]
Age 11 * Height-for-Age Z-Score	1.838***
	[0.581]
Age 12 * Height-for-Age Z-Score	2.150***
	[0.472]
Age 13 * Height-for-Age Z-Score	1.619*
	[0.883]
Age 14 * Height-for-Age Z-Score	-0.043
	[0.354]
Female Child	-0.036
	[0.147]
Age Head of Household	0.014
	[0.008]
(Age Head of Household)2	0
	0
Head of Household Educated	0.151
	0.199
Female Head of Household	-0.029
	[0.286]
Household Poor at Baseline	-0.239
	[0.202]
Year of Age Dummies	Yes
Born After August Dummy	Yes
Province Fixed Effects	Yes
Pseudo R Squared	0.13
N	214

Notes: Regression on subsample of children who ever enrolled in school. All standard errors are robust and clustered at the level of the village. Household head is educated is s/he completed at least primary school. Data source: Republic of Burundi and World Bank (1998) and ISTEEBU (2007). ***:

Statistically significant at the 1%-level; **: Statistically significant at the 5%-level; *: Statistically significant at the 10% level.

Table A1: Exploring Potential Selection Bias

	Included in 2007 Survey (N=243)	Not Included in 2007 Survey (N=50)	Mean Difference
Male Child (%)	0.47	0.44	0.03
	[0.032]	[0.071]	[0.078]
Child's Age (Months)	31.9	30.4	1.5
	[0.96]	[1.91]	[2.29]
Height-for-Age Z-Score	-2.22	-2.6	0.37*
	[0.091]	[0.209]	[0.223]
Female-Headed Household (%)	0.15	0.16	-0.01
	[0.023]	[0.052]	[0.055]
Age Head of Household	37.1	39.9	-2.8*
	[0.65]	[1.5]	[1.6]
Household Head Educated (%)	0.27	0.38	-0.11
	[0.028]	[0.069]	[0.07]
Proportion of Literate Adults	0.25	0.23	0.02
	[0.013]	[0.031]	[0.032]
Household Size	6	5.9	0.1
	[0.14]	[0.357]	[0.349]
Value of Livestock (BIF)	60292	43440	16852
	[7762]	[14243]	[18296]

Notes: Household Head is "Educated" is s/he completed primary school. Last column shows t-test for differences in means between children included in the 2007 sample and children who were not recontacted. Data source: Republic of Burundi and World Bank (1998) and ISTEEBU (2007). ***: Statistically significant at the 1%-level; **: Statistically significant at the 5%-level; *: Statistically significant at the 10% level.

Table A2: Logit Regression Estimating Probability to Be Included in the 2007 Sample, Using Fitzgerald-Gottschalk-Moffit Regression Method

Dependent Variable: Probability of Being Included in the 2007 Survey	(1)
Male Child	0.323
	[0.331]
Child's Age (Months)	0.012
	[0.011]
Height-for-Age Z-Score	0.211*
	[0.119]
Female-Headed Household	0.101
	[0.464]
Age Head of Household	-0.035**
	[0.016]
Household Head Educated	-1.919
	[1.172]
Proportion of Literate Adults	1.029
	[0.913]
Household Size	0.106
	[0.085]
Value of Livestock (100,000 BIF)	0.117
	[0.171]
Pseudo R Squared	0.06
N	293

Notes: Dependent variable is coded one for the 243 children included in the 2007 sample and zero for the 50 children not included. Robust standard errors clustered at the hill. *: Statistically significant at the 10% level; **: Statistically significant at the 5% level; ***: Statistically significant at the 1% level. Data source: World Bank and ISTEEBU (1998) and ISTEEBU (2007).