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Malnutrition, subsequent Risk of Mortality and Civil War in Burundi

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Abstract: The paper investigates the effect of child malnutrition on the risk of mortality in Burundi, a very poor country heavily affected by civil war. We use anthropometric data from a longitudinal survey (1998-2007). We find that undernourished children, as measured by the height-for-age z-scores (HAZ) in 1998 had a higher probability to die during subsequent years. In order to address the problem of omitted variables correlated with both nutritional status and the risk of mortality, we use the length of exposure to civil war prior to 1998 as a source of exogenous variation in a child's nutritional status. Children exposed to civil war in their area of residence have worse nutritional status. The paper finds that one year of exposure translates into a 0.15 decrease in the HAZ, resulting in a 10% increase in the probability to die for the whole sample as well as a 0.34 decrease in HAZ per year of exposure for boys only, resulting in 25% increase in the probability to die. We show the robustness of our results. Food and income transfer programs during civil war should be put in place to avoid the long-term effects of malnutrition.

Keywords: malnutrition, mortality, children, war, Africa, instrumental variables

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Introduction

Childhood malnutrition and civil war are prevalent in Africa. But how do they relate to each other? In two recent anthropometric studies on Burundi and Rwanda, Akresh, Verwimp and Bundervoet (2009, 2011) compare children exposed to civil war in their area of residence with non- exposed children and find that exposure reduces the height-for-age z-score of an child by 0.047 standard deviations for every additional month of exposure in Burundi and with one standard deviation for exposure measured as a binary shock in Rwanda. This means that the consequences of civil war will stay with these children there entire live, as they will attain a lower physical stature as adults. As investigated by Alderman, Hoddinott and Kinsey (2006) for Zimbabwe, this will mean that their cognitive skills will be less developed and that they will accumulate less human capital. On top, many medical studies have observed an increased risk of mortality among children with low nutritional status (Chen, Chowdury, Huffman, 1980; Yambi er al, 1991; Pelletier et al, 1994, Young and Jaspers 2009). Bad nutritional status is considered a predictor of increased mortality risk. However, as these studies do not deal with the problem of endogeneity and omitted variable bias, they cannot determine the *causality* of the effect. When nutritional status as well as the risk of mortality are determined by observed (but omitted) or unobserved characteristics such as genetic endowment or other innate child, mother or household characteristics, then the estimated effect of nutritional status on the risk of mortality is biased.[1] If studies on the anthropometry-mortality relationship are to be used to guide resource allocations for child survival between nutritional and health interventions, then it is important to know the extent to which malnutrition plays a causal role in child mortality.

One approach to solve this problem is to use an instrumental variable for child nutritional status. In this paper I apply such approach analysing nutritional status, subsequent risk of mortality and exposure to civil war in a sample of children living in rural Burundi, born between January 1994 and October 1998 and aged between 6 and 59 months at the time of the first round of data collection (1998). The paper also sheds light on a related debate in the literature on the potential selectivity bias in the measurement of nutritional status through child deaths. In a recent contribution, Alderman, Lokshin and Radyakin (2011) using data for India found that selective mortality had only a minor impact on the measurement of nutritional status.

[1] For a general treatment of endogeneity, we refer to Green, W., *Econometric Analysis*, fifth edition, 2003, Pearson Education, New York

The course of Burundi's Civil War

The latest episode of civil war in Burundi began in October 1993, when the first democratically elected president – and for the first time a Hutu president – was assassinated by paratroopers from the Tutsi-dominated army in a failed coup d'etat. This was followed by large-scale massacres in the countryside, with peasant-supporters of the president killing Tutsis and *UPRONA*-Hutus, and the army killing all Hutus in sight in an operation to 'restore order'[2]. In a matter of days, 100,000 people lost their lives in what the UN calls a genocide (UN, 1996). The massacres were followed by the spread of violence and warfare throughout the country, with several Hutu rebel factions opposing the regular government (Tutsi) army. This marked the beginning of one of the most brutal and bloody civil wars in recent history (Uvin, 1999)

In August 2000, some minor rebel groups signed the Arusha peace agreements with the still Tutsi dominated Burundian government. This had little effect on the security situation in the field since the two major rebel groups, CNDD-FDD and FNL, were not involved in the peace talks. In 2003, the new president (Hutu) announced a one-sided cease fire and allowed the largest rebel group CNDD-FDD to descend from the hills and march victoriously on Bujumbura. Rebel leader Nkurunziza was incorporated in the government and rebel combatants were integrated in army and police forces. The intensity of the civil war decreased dramatically and in 2005 Nkurunziza was elected as the new president. One rebel group (FNL) remained outside the peace process and continued murdering and pillaging, as a result of which pockets of insecurity still exists throughout the country.

Human Rights Watch (1998, 2003) describes the Burundian war as a war against civilians. Civilians were widely used as proxy targets, with both sides (rebel groups and the regular army) targeting civilians deemed supportive of the other group. Direct battles between the army and the rebel forces were relatively rare despite the duration of the war. Both sides of the conflict engaged in massive looting of civilian property and massive human rights violations. Civilians had to flee battle zones, lost wealth and livestock, and were put in camps in often deplorable conditions. Displaced individuals and families were prone to attacks, deprivation, bad sanitation and housing conditions and malnutrition. In their strategy to avoid open confrontation with the army, rebel groups were very mobile and obliged villagers to supply food and to carry food and weapons over hilly areas with them. They also requested

[2] UPRONA-Hutus are Hutus loyal to the Tutsi-dominated political party UPRONA, and are therefore seen as traitors to the Hutu cause.

contributions in cash. Upon return home displaced people would found their land occupied by neighbors or strangers.

The war had devastating effects on the economy. Income per capita halved from USD 162.7 in 1993 to USD 82.6 in 2003 (IMF, 2007, p.7). Rural poverty headcount increased from 39.6% in 1993 to just over 70% in 2003, making Burundi the world's poorest country. Social indicators also worsened: life expectancy fell from 51.1 in 1993 to 46.3 in 2003, and the prevalence of malnutrition increased with 20 percentage points (67% in 2003).

Data Collection

The data used in this paper consist of a nine-year panel with two data points, 1998 and 2007. In 1998, the World Bank and the Burundi Institute of Statistics and Economic Studies (BISES) conducted a nationally representative general-purpose household survey to analyse living standards. The 2007 Priority Survey was designed as a follow-up to the 1998 Priority Survey Due to budget limitations, it was impossible to track and re-survey all 3908 rural households (in 391 survey sites) included in the 1998 survey. It was therefore decided to randomly draw 100 of the 391 baseline sites with the purpose to track and re-survey all 1000 original (1998) rural households in these sites. 65 interviewers we trained during a one-week training period that was also helpful in improving the questionnaire. The questionnaire was pilot tested in an out of sample village and final corrections were made. 50 interviewers were selected in a competitive exam that included a case study on household composition, consumption and production as well as a range of questions on research ethics. Each team of 5 interviewers was supervised by a team leader. Two out of five team members were women. The interviewers were instructed to track and re-interview, within each hill, the 10 original households. Overall the 2007 survey managed to locate and re-interview 874 of the 1000 selected household (87.4%). This is a good result in a very poor country affected by civil war. The supervisor of each team of interviewers undertook a village level community survey in which (s)he asked questions on infrastructure, history, population, attacks and war-related violence.

The anthropometric data were registered in the 1998 survey and the mortality and conflict exposure data in the 2007 survey. One limitation of the 1998 survey is that children were only measured in half of the survey sites and thus in half of the households. Fortunately, the selection of sites where anthropometric measurement of children was taken in 1998, occurred randomly: All households residing on every second site of the 391 sites where the survey was implemented, were selected for anthropometric measurement of their children below age 5. The 1998 survey obtained valid anthropometric data on 1196 children in 204 of the 391 sites. The 2007 survey did not only collect information on the household members that were already present in the household in 1998 and alive in 2007, but also on newborn children and children who had died in the 1998-2007 period. We asked for sex, date of birth and – if applicable - date of death and cause of death for all children ever born to mothers in the 1998 survey.

Exposure to Civil War Variable and Anthropometric Indicators

In the analysis I exploit the spatial and temporal variation of the civil war in Burundi. During the period when the children in our sample were born (01/1994-09/1998), the civil war did not affect all villages (in Burundi called sous-collines, the lowest administrative unit) at the same time. At some point, it affected some sous-collines and at other points in time, it affected others. This kind of variation is very useful in our estimation strategy (see below) as it allows us to compare children exposed to the violence with non-exposed children of the same birth-cohort and children from different birth-cohorts in the same sous-collines. Our measure of exposure to civil war is the number of years the child lived in an area affected the by civil war. In the 2007 survey we asked in every sous-collines and for every year when the sous-colline was attacked (by one or more parties to the conflict).[3] These data were then merged with the birth date of the children from that sous-colline. The maximum years of exposure is 5 and the minimum is 0.

Relevant literature on nutritional status in childhood uses height conditional on age and gender (HAZ, stunting or chronic malnutrition) as a good indicator for long-term nutritional status. Weight-for-age (WAZ), which is more difficult to interpret as it is mix of a short-term and long-term indicator, will be used as a robustness check. These anthropometric indicators have been shown to predict mortality. In our study, I want to analyse the risk of mortality in the years following the 1998 survey. I do not investigate an acute health or food crisis, but more of a creeping situation. I am interested in duration. In our sample, only 3% of the children have a weight-for-height z-score below -2. In contrast, 66% of the children suffer from stunting (HAZ<-2) and 20% are underweight (WAZ<-2).[4] Next to malnutrition, other factors such as injuries or illness can also contribute to premature death. Also, malnutrition need not to be caused by war. In neighbouring Rwanda, for example, the percentages are 45% stunting, 4% wasting, 23% underweight (DHS, 2005).

[3] Recollection of war events may be subject to measurement error many years after the violence. In order to minimize this; our interviewers were instructed to cross-check with three older adults from the village.
[4] I use WHO reference values for the calculation of HAZ and WAZ

Description

Table 1 present the survival/death status of the 1998 children who were re-interviewed in 2007. The 1998 survey had obtained valid anthropometric measurements of 338 children in 47 of the 100 sites that were re-visited in 2007.[5] In 2007, we were able to find out the fate of 283 of these children (85%). 22 of these children had died in the period between the two survey rounds (1998-2007) .One child was killed (violent death) and the other 21 died of disease. Table 1 shows that, in 1998, these 21 deceased children were, on average, not younger or older than the children who were still alive. However, the deceased children had worse nutritional status in 1998. This can also be observed in Figures 1 and 2. At the descriptive level, this table and these figures suggest that the 1998 nutritional status can serve as a predictor for the subsequent risk of mortality of a child. A result that later will be qualified in a regression framework using an instrumental variables estimation model. Remark that the distribution of HAZ and WAZ for the diseased children intersects with those of the surviving children.

[Table 1 here]

[Figure 1 and 2 here]

In the next step, the exposure to civil war is introduced. We are particularly interested in the effect of civil war on the child's nutritional status. Table 2 presents a breakdown of the data according to the exposure to civil war, child characteristics and nutritional status. The table shows that exposed children are older and have worse nutritional status. As it is well-known that the nutritional status of young children worsens between the ages 0 and 3 and then levels-off, we have to be careful that the relationship between nutritional status and exposure to civil war, as observed in table 2, does not simply reflect the older age of the exposed children. In the subsequent regression analysis we will therefore control for potential age effects by including year of birth fixed effects. Figures 3 and 4 help us not be become confused by the age-effect. These figures depict the relationship between the length of exposure to violent conflict and both nutritional indicators. The longer the child is exposed the more it is stunted and wasted.

[5] Since we did not revisit all of the 204 sites where anthropometric measurements were taken in 1998, we face a potential selection problem. We treat this problem in the Appendix.

[Table 2 here]

[Figures 3 and 4 here]

Estimation Strategy

Before we discuss the models I refer to figures 5 and 6 for a visualisation of the length of exposure to violent conflict and mortality. Both figures show that the deceased children were exposed to war longer than the children who survived. It is my argument in this paper that malnutrition – not represented in Figures 5 and 6 - is *the causal mechanism* linking war exposure to mortality. Figures 3 and 4 already pointed in that direction.

Since the purpose of our approach is to find out whether or not the nutritional status of children measured in 1998 is able to predict the survival of these children, we need to exploit the longitudinal element in our dataset. We do this by using the following models. The first is a binary choice Probit model whereby the survival/death of a child in the 1998-2007 period is predicted by the child's nutritional status in 1998.

$$\Pr(y)_{07} = \alpha_0 + \beta_1 N_{98} + \sum \beta_n Z_n + \gamma + \lambda + \varepsilon \qquad (1)$$

where y is the survival status, a binary variable (0/1), of the child in 2007. N is the nutritional status of that same child in 1998, Z a vector of child, mother and household characteristics, γ are year-of-birth fixed effects (FE), λ are agro-ecological area FE and ε is a random error component. Fixed Effects (FE) control for time invariant unobserved heterogeneity.

In such models, the researcher has to deal with the potential "endogeneity" of nutritional status. Endogeneity may be caused by the failure to control for all relevant variables (Greene, 2003; Heckman, 2008). This is a serious concern in nutrition and mortality research since the researcher may fail to account for or does not have information on genetic endowment or other unobserved innate characteristics of the child, the mothrer or the household. When such omitted variables are correlated with both nutritional status and the risk of mortality then spurious correlations between these two variables may occur. In such cases, an IV model allows consistent estimation of the coefficients of interest.

In order to overcome the above type of bias we identify exposure to violent conflict as a source of exogenous variation in child nutritional status. I argue that the length of exposure is a good instrument for nutritional status and I use two types of arguments to make the case, a narrative and a statistical one.

(i) The narrative derives from the available evidence: (i.a) Bundervoet et all (2009), using a nationwide dataset for Burundi have demonstrated that exposure to civil war affects nutritional status, measured as HAZ. The channels or mechanisms through which the violence produced in civil war affects nutritional status are not so easy to disentangle. Bundervoet et al recognise this and, based on their empirical findings they suggest that the looting of household assets such as livestock is unlikely to be the main channel. Their results offer more support for two other channels, to wit violence-induced displacement and the theft and burning of crops. Both negatively affect nutrition. The longer a child is exposed to these type of events the more adverse the impact, which corresponds with my results.

(i.b) There is very few evidence in my data but also in the scholarly literature on the conflict in Burundi that young children are directly killed or murdered by the warring parties. Should this be the case then I would wrongly exclude the instrument from my second stage regression. In my admittedly small sample I have found only 1 child (1/21 being 5%) which was directly killed. This child is excluded from my analysis. Rather than directly killing children, warring parties chased people away from the villages, or households would flee the sites of battle, leaving behind their food and their farms. Typically, children affected by this type of event do not die immediately, but they are weakened and over the course of the years of exposure to violence they are more likely to die.

(ii) On the statistical front, following Nichols (2007, p.524) I am using the linear probability model to obtain test-statistics on the validity and relevance of the instrument and present the results alongside the IV probit results (Table 5). The Wald test for the IV probit is chi2(1) = 5.15 with Prob > chi2 = 0.023 which indicates that the null hypothesis that the error term in the structural equation is correlated with the error term in the reduced form equation should be rejected. From the test-statistics provided in the linear probability model I learn that the Kleibergen-Paap rk LM statistic for underidentification is 2.98 with a Chi-sq(1) p-value of 0.084. This means that the null-hypothesis of the irrelevance of the instrument is rejected and thus that the instrument is correlated with the endogenous regressor.

The Kleibergen-Paap rk Wald F statistic for weak identification is 4.6, which is low when compared with the critical values provided by Stock-Yogo (2005). Stock–Yogo weak ID test critical values provides one set for various percentages of “maximal IV relative bias” (largest bias relative to OLS) and one set for "maximal IV size" (the largest size of a nominal 5% test). The critical value obtained is 30% of the largest size.[6]

Given that I obtain a rather low value for the Wald F one may wonder if the IV should be preferred to the OLS estimate. The reply to this question is given by a formal test for endogeneity. The result of this test (Chi-squared distributed with 1 degree of freedom) is between 2.6 and 3.2 with p-values between 0.07 and 0.10 (depending on specification) meaning that the null-hypothesis that the endogenous regressor can actually be treated as exogenous, should be rejected.

Since we only have one instrument, the equation is exactly identified and there is no Hansen-J statistic for overidentification. However, when I add the squared value of the exposure variable as a second instrument then the p-value of the Hansen-J test for overidentification is 0.90, which is well above 0.10 meaning that the instruments are correctly excluded from the second stage regression. In the first stage of the IV Probit however, the squared exposure variable is statistically insignificant. I thus prefer to use one instrument since the relationship between exposure and stunting appears to be linear and not quadratic.

Formally, the two stage approach can be written as:

$$N_{98} = c_0 + \delta_1 V_{94-98} + \sum \beta_n Z_n + \gamma + \lambda + v \tag{2}$$

$$\Pr(y)_{07} = \alpha_0 + \beta_1 N^*_{98} + \sum \beta_n Z_n + \gamma + \lambda + \varepsilon \tag{3}$$

The (*) in the upper corner of N in equation (3) signals that the variable is instrumented for, whereby V in equation (2) is the exposure to violent conflict in the village of the child during the 1994-1998 period, measured in years of exposure. In a robustness analysis on the timing of exposure later on, only exposure in the first three years of the life of the child are taken into account. The IV should be interpreted as a particular Local Average Treatment Effect (LATE, Angrist and Imbens, 1994), which captures the effect on the outcome variable of a marginal increase in exposure to civil war of a sub-group of the exposed

[6] We shall see below that the test-statistic for weak identification improves substantially when the analysis is performed for boys only.

Analysis

I start the analysis showing that the number of years the child was exposed to civil war is a statistically significant predictor of the child's nutritional status. First, a full model is specified to determine malnutrition, with covariates for the characteristics of the child (sex, year of birth), characteristics for the mother (age, literacy, marital status), and characteristics of the household (size) as well as community variables (altitude and index of infrastructure). Next, exposure the civil war is added to the regression. I have implemented this procedure for both HAZ and WAZ butt the results for WAZ are reported as a robustness check below. Results are presented in table 3. An additional year of exposure to civil war lowers the height-for-age z-score with -0.15 for the whole sample and with -0.34 for the sub-sample of boys. To put the magnitude of this effect in perspective: a boy who is exposed to the violence during one year, will be 1/3 of a standard deviation (as measured by the z-score) shorter compared to a boy of the same age who is not exposed. The effect for girls presented in column 5 is -0.10 for an additional year of exposure but the effect is not statistically significant at the usual standards.

Regressions in columns 2-4 control for year of birth and area of residence (agro-ecological zone) fixed effects. This result is comparable to the results reported in Bundervoet et al (2009). Using exposure on a monthly basis these authors find a magnitude of -0.047 for every additional month of exposure in their preferred regression. When I perform their regression with my years of exposure variable, I obtain a coefficient of -0.36 per year, which, although it is larger than the result obtained for the sub sample in this paper, remains in the realm of effects reported in Bundervoet et al. Also, the result in Bundervoet et al applies to a sample of 1196 children whereas the result reported here only used a sub sample of 283 children. [7]

Table 4 presents the results of the Probit model and the second stage of the IV linear probability and IV Probit models. In the first column, three factors, measured in 1998, have a statistically significant effect on the probability of the child to die in the years after the 1998 survey: the child's nutritional status in; the age of the mother and the literacy of the mother. The higher these indicators, the lower the probability to die. All regressions are controlled for year of birth and area of residence fixed effects. Standard errors are clustered at the level of the survey site (villages or sous-collines in Burundi).

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[7] The exposure variable does not have a direct or independent effect on the risk of mortality. Once we control for nutritional status, the coefficient of the exposure variable is statistically insignificant (result not shown).

[Table 3 here]

[Table 4 here]

The coefficients of the Probit model in column 1 of table 4 are not straightforward to interpret. I use two approaches to make sense of the effect of HAZ on the probability to die. (a) in terms of standard deviations: an increase of one standard deviation in the height-for-age z-score decreases the underlying latent variable y* with 0.23 standard deviations[8]; and (b) evaluating the marginal effects at the means of all regressors: an increase in the height-for-age z-score with one unit decreases the probability to die by 1.6%. For boys only, in column 1 of table 5, this yields -0.28 standard deviations in the first approach and -1.9% for the second approach.

As described in the methodology section, we have run and presented the linear probability model to obtain test-statistics, which are above the usual thresholds for the entire sample (apart from the test for weak identification which is on the margin of acceptance) and which perform very well for the sub sample of boys. As for the coefficients, we are interested in the IV Probit model. The Hausman specification test for exogeneity (using a Wald Statistic) tells us to reject the null hypothesis of equality between the Probit and the IV Probit estimators and to prefer the IV estimate.

Column 3 of table 4 (entire sample) and column 3 of table 5 (boys only) present the results of the second stage of the IV Probit. The values of -0.70 and -0.76 for the coefficients can be interpreted as the effect of a unit change in the height-for-age z-score – resulting from exposure to civil war – on the probability to die. In order to obtain a unit change in the HAZ (for example from -1.5 to -2.5) one needs approximately 6 years of exposure (6*0.15) in case of the entire sample and 3 years of exposure for boys only (3*0.34). Since it is more straightforward to calculate the effect of one year of exposure, I find that one year of exposure translates into a 0.15 decrease in HAZ, resulting in a 10% increase in the probability to die (0.15*0.70) for the whole sample and a 0.34 decrease in HAZ per year of exposure for boys only, resulting in 25% increase in the probability to die (0.34*0.76). Since the IV Probit model for the girls sub sample does not converge, estimates can not be provided. The test-

[8] Using the listcoef command in STATA and taking the value in the bStdXY column.

statistics in the linear probability model for girls only, reported in column 5 of table 5, do not reach the usual thresholds.

[Table 5 here]

Robustness checks

Several factors may confound the above analysis. I distinguish four of them. (i) age-specific impacts of war exposure; (ii) the measurement of health status; (iii) socio-economic status of the household and (iv) morbidity. *First*, there is a medical literature saying that children are most vulnerable to health shocks when they are 0 to 3 years old. In stead of measuring exposure over a child's total life, I now limit the measurement of war exposure to this age interval. Column 1 of Table 6 presents the results, which have not changed. I do not find these results when I limit exposure to the years 4 and 5. This means that the results presented in the paper are driven by the exposure to violence when the children were very young. Also, results do not change when I use age in months as a control variable in stead of year of birth fixed effects.

[Table 6 here]

Second, it could be that there is measurement error in the health indicator, the height-for-age z-score. One could also argue that some deaths occurred in the period immediately after the survey (eg.1999 or 2000) meaning that HAZ – which is a long-term indicator - may not be the adequate indicator to use. In order to address these issues I execute a robustness test on the dependent variable using weight-for-age (WAZ) as an alternative indicator of nutritional status. WAZ is a mix of a short term and a long term indicator of nutritional status making it more difficult to interpret. When however, the deaths in the sample are spread over time (they occur right after the 1998 survey as well as just before the 2007 survey), then WAZ may serve well as a robustness check. Columns 2-6 in Table 6 present the results. In terms of the magnitude of the coefficients as well as in terms of the performance of the various test-statistics, WAZ performs very well and even better/stronger then HAZ, meaning lower p-values on the underidentification test and higher value (10.67) for the Wald F statistic for detecting weak identification. Following Stock-Yogo (2005) the critical value obtained was

15% of the largest IV size, which is reasonably good and even increases to 14.68 when we do the analysis for boys only

Third, the socio-economic status of the household. In all the regressions that I present in the paper, I have re-run the analysis with household level expenditures in 1998 as an additional control variable (results not presented but available from the author). This does not change the results. The effects of exposure to war and nutritional status on the risk of mortality remain unchanged and household level expenditures is statistically insignificant in tables 3, 4 and 5 and 6. This finding is consistent with Pelletier e.a. (1994, p1 and p.13) who write that 'As regards confounding, the results indicate that the anthropometry-mortality relationship is not due to confounding by socioeconomic factors when all grades of malnutrition are considered.' I remark that this robustness check is imperfect as the variable was registered during and not before the war.

And *fourth* is illness, injuries, handicaps and no access to treatment. Illnesses caused by war exposure to e.g. vector borne or respiratory diseases can affect mortality risk. This is also the case for serious injuries or handicaps. Omitting such variables may lead to an overestimation of the effect of malnutrition on mortality. In descriptive tables similar to tables 1 and 2 (not shown but available from the author) I demonstrate that exposed and non-exposed children on the one hand and survived and deceased children on the other hand did not differ in the prevalence of illness and the access to treatment at the time of the 1998 survey, neither did they suffer more from injuries or handicaps. Introducing the illness variable in the IV Probit regressions for HAZ and WAZ presented in tables 5 and 6 (results available from the author) does not change the sign nor the magnitude of the coefficients of interests. The coefficient of the illness variable is statistically insignificant.

Discussion

The magnitude of our coefficients of interest is relatively high. One reason for that is the length of our follow-up period, to wit 9 years (1998-2007). Other studies have a much shorter follow-up period, often only one or a few years. In that way children stand a better chance to survive the follow-up period. A second reason is that the results of studies not dealing with the endogeneity of nutritional status and mortality risk yield biased estimates.

The results speak to two issues in the literature. One is the gender difference and the second is the distribution of HAZ for survivors compared to non-survivors. As for the first, there is a recent literature on the gender difference as a result of nutrition, income or war shocks. There is more evidence of gender discrimination in times of peace and prosperity in

Asian compared to African households. In times of severe political or economic crisis however, this may change. Mu and Zhang (2011) observe higher male excess mortality in the Great Chinese Famine whereas Verwimp and Van Bavel (2005) find a larger increase in the mortality of girls during Rwanda's refugee crisis. Differences that I believe can be explained by the capacity and the desire parents may have to discriminate between sons and daughters. Their preferences to have at least one male offspring survive may be linked to the prevailing system of dowry as well as inheritance rights/practices. Their capacity to shield children or sons for that matter from negative shocks may be impaired by the magnitude and the speed of the shock. Akresh, Verwimp, Bundervoet (2011) argue that such discrimination is not possible (or at least much less effective) in times of civil war or political violence. These events cannot be much anticipated and because of its covariate nature is hard to insure against using informal mechanisms. Parents have much less time and capacity to discriminate and the negative shock of civil war affects boys as well as girls. In Verwimp and Van Bavel (2011), the schooling of boys in Burundi is affected worse compared to that of girls. Hence, the result in this paper, that only the HAZ of boys is affected by the civil war and that the exposure to war can be used as in instrument in the analysis of mortality/survival, should be situated in that nascent literature: exposure to civil war seems to explain the nutritional status and subsequent survival of boys much better than that of girls. I explain this by the above mentioned lack of possibilities to discriminate, but I concede that I am not able to pinpoint the exact mechanism.

As for the second issue, researchers remarked that when we are confronted with a population of living children (such as in a DHS), there is a selection bias as the worst nourished died previously. The higher the mortality, the greater the selection bias. This paper, esp. Figures 1 and 2, give a rather precise answer to this question: While poor nutritional status increases the mortality risk, it does not appear to be the case that there is a substantial mortality bias among the surviving children as the distribution of dead children is similar (but slightly shifted to the left) to the distribution of alive children (if there was a strong selectivity effect, one would expect the distribution of dead children to hardly intersect with the distribution of surviving children).[9]

Based on these results, a case can be made for a nutrition-based intervention in areas affected by civil war. Food transfer programs in war-affected areas should target young children in order to avoid stunting or underweight. Such intervention will reduce mortality in

[9] This pertinent observation was made by one of the anonymous reviewers.

the post-conflict years. This does not mean that nutrition is the only domain in which intervention is beneficial. Integrated aid or humanitarian packages offering access to health care, clean water and hygiene in addition to nutrition may reinforce the beneficial effect of each item in the package. The contribution of the paper is not to forget the food part when planning an intervention. Malnourished children are more vulnerable to illness whereby the latter maybe the direct cause of death and the former the indirect. Young girls are not the only ones needing food assistance in times of crises.

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Tables

Table 1: Survival/Death status of re-interviewed children, N=283

Average Weight for Age Z-score	(1) Child alive in 2007 N=262	(2) Child not alive in 2007 N=21[a]	(3) Mean Difference (1)-(2)
Child's Age (in months) at the time of the 1998 measurement	32.3 [0.90]	29.0 [3.25]	3.2 [3.37]
Average Height for age Z-score in 1998	-2.45 [0.09]	-3.26 [0.27]	0.81***[0.29]
Average Weight for age Z-score in 1998	-1.25 [0.05]	-1.62 [0.19]	0.36*[0.20]

[a]One child (excluded) died violently.

Table 2:Nutritional Status, Civil War Exposure, Age and Gender, N=283

Average Weight for Age Z-score	(1) Child not exposed to Civil War N=208	(2) Children exposed to Civil War n=75	(3) Mean Difference (1)-(2)
Child's Age (in months)	29.6 [1.02]	37.9 [1.53]	-8.24***[1.84]
Number of years exposed	0	1.76 [0.12]	
Average HAZ-score	-2.31 [0.10]	-3.04 [0.13]	0.72***[0.17]
Average WAZ-score	-1.18 [0.06]	-1.56 [0.08]	0.38***[0.10]
Child's Gender (% female)	51.4 [0.03]	54.6 [0.06]	-3.2 [0.07]

Notes: Robust standard errors in brackets. *** significant at 1%; ** significant at 5%; * significant at 10%. Data source: World Bank and Burundi Statistics Institute 1998

Table3: Determinants of Child Nutritional Status, N=283

Dependent Variable: Nutritional Status measured by Height-for-age z-score	OLS (1)	OLS (2)	OLS (first stage of column 2 and 3 in table 4) (3)	OLS (first stage of column 2 and 3 in table 5) *boys only* (4)	OLS (first stage of column 5 and 6 in table 5) *girls only* (5)
Years of Exposure			-0.15**[0.07]	-0.34***[0.12]	-0.10 [0.089]
Age of the child	-0.21*** [0.64]				
Child is Female	0.41**[0.16]	0.39**[0.17]	0.42**[0.17]		
Age of the mother		-0.011 [0.11]	-0.013 [0.01]	-0.017 [0.02]	-0.01 [0.015]
Mother is literate		0.05 [0.20]	-0.021 [0.19]	-0.079 [0.27]	-0.021 [0.29]
Marital status of mother		0.07 [0.25]	0.11 [0.24]	0.20 [0.54]	-0.043 [0.29]
Size of the household		0.03 [0.04]	0.03 [0.04]	0.009 [0.83]	0.03 [0.06]
Altitude of the village		-0.0002 [0.0004]	-0.0004 [0.0004]	0.0002 [0.0008]	-0.0007 [0.0008]
Village infrastructure		-0.003 [0.02]	-0.004 [0.02]	0.05 [0.03]	-0.04 [0.03]
Year of Birth FE	NO	YES	YES	YES	YES
Agro-Ecol.Zone FE	YES	YES	YES	YES	YES
Constant	-1.7***[0.19]	-2.13***[0.70]	-1.80**[0.72]	-2.13 **[1.01]	-0.99 [1.46]
N	283	283	283	135	148
R squared	0.12	0.12	0.13	0.14	0.18
F-Statistic	16.55***	8.15***	10.48***	3.61***	5.24***

Notes: Robust standard errors in brackets. *** significant at 1%; ** significant at 5%; * significant at 10%. Data source: World Bank and Burundi Statistics Institute 1998.

Table 4: Nutritional Status, Mortality Risk and Exposure to Civil War, Probit, Linear Probability and IV Probit models (second stage), N=283

Dependent Variable: 0/1 Alive/Death	Nutritional Status measured as Height-for-age (stunting), *boys and girls*		
	Probit	IV Linear Probability	IV Probit
	(1)	(2)	(3)
Nutritional Status	-0.19***	-0.20*	-0.70***
	[0.06]	[0.12]	[0.082]
Child is Female	-0.051	0.068*	0.21*
	[0.19]	[0.003]	[0.13]
Age of the Mother	-0.027*	-0.05	-0.022*
	[0.016]	[0.016]	[0.014]
Mother is Literate	-0.81***	-0.08*	-0.43
	[0.30]	[0.046]	[0.27]
Marital status of Mother	0.36	0.065	0.24
	[0.34]	[0.066]	[0.21]
Size of the household	-0.04	-0.003	-0.007
	[0.053]	[0.009]	[0.036]
Altitude of the village	0.001	0.00008	0.0006
	[0.001]	[0.0009]	[0.006]
Village Infrastructure	0.03	0.003	0.014
	[0.03]	[0.005]	[0.021]
Year of Birth FE	YES	YES	YES
Agro-Ecol. Zone FE	YES	YES	YES
Constant	-3.76**	-0.54*	-3.59***
	[1.92]	[0.29]	[1.13]
Test for endogeneity		2.6*	
K-P test for Underidentification		2.98*	
K-P test for weak Identification		4.6	
Hausman specification test for exogeneity of the instrument			5.15**

Notes: Robust standard errors in brackets. *** significant at 1%; ** significant at 5%; * significant at 10%.
Data source: World Bank and Burundi Statistics Institute 1998.

Table 5 Nutritional Status, Mortality Risk and Exposure to Civil War,
Probit, Linear Probability and IV Probit models (second stage), N=283

Dependent Variable: 0/1 Alive/Death	Nutritional Status measured as Height-for-age (stunting), *boys only, N=135*			Nutritional Status measured as Height-for-age (stunting), *girls only, N=148*		
	Probit	IV Linear Probability	IV Probit	Probit	IV Linear Probability	IV Probit
	(1)	(2)	(3)	(4)	(5)	(6)
Nutritional Status	-0.25**	-0.28*	-0.76***	-0.24	0.04	
	[0.10]	[0.14]	[0.074]	[0.18]	[0.17]	Model
Age of the Mother	-0.013	-0.006	-0.009	-0.04	-0.003	does not
	[0.027]	[0.004]	[0.018]	[0.03]	[0.003]	
Mother is Literate	-0.84	-0.09	-0.35	-0.83	-0.085*	converge
	[0.50]	[0.09]	[0.34]	[0.53]	[0.046]	
Marital status of Mother	0.47	0.11	0.21	0.089	0.015	
	[0.41]	[0.15]	[0.39]	[0.71]	[0.09]	
Size of the Household	-0.19	-0.010	-0.087	0.02	-0.005	
	[0.10]	[0.024]	[0.079]	[0.09]	[0.012]	
Altitude of the village	0.0018	0.0002	0.0009	0.001	0.0008	
	[0.001]	[0.002]	[0.0008]	[0.001]	[0.001]	
Village Infrastructure	-0.11	0.019	0.026	0.048	0.008	
	[0.06]	[0.014]	[0.033]	[0.051]	[0.01]	
Year of Birth FE	YES	YES	YES	YES	YES	
Agro-Ecol.Zone FE	YES	YES	YES	YES	YES	
Constant	-3.4	-0.77*	-3.20**	-12.82**	-0.57*	
	[2.5]	[0.47]	[1.34]	[2.42]	[0.33]	
Test for endogeneity		3.98**			0.19	
K-P test for underidentification		3.7*			1.2	
K-P test for weak identification		7.15			1.4	
Hausman specification test for exogeneity of the instrument			9.34***			

Notes: Robust standard errors in brackets. *** significant at 1%; ** significant at 5%; * significant at 10%. Data source: World Bank and Burundi Statistics Institute 1998.

Table 6: Nutritional Status, Mortality Risk and Exposure to Civil War, *Robustness* test using alternative exposure period and alternative indicator for Nutritional Status, N=283

Dependent Variable: 0/1 Alive/Death	Height-for-age, *crucial period*	Weight-for-age (Underweight), *boys and girls*			Weight-for-age (Underweight), *boys only*	
	IV Probit	Probit	IV Linear Probability	IV Probit	IV Linear Probability	IV Probit
	(1)	(2)	(3)	(4)	(5)	(6)
Nutritional Status	-0.74***	-0.24*	-0.28*	-1.05***	-0.35**	-1.3***
	[0.082]	[0.12]	[0.14]	[0.19]	[0.18]	[0.19]
Child is Female	0.25*	-0.067	0.025*	0.08		
	[0.13]	[0.19]	[0.03]	[0.15]		
Age of the Mother	-0.019*	-0.026	-0.006	-0.027*	-0.002	-0.01
	[0.012]	[0.016]	[0.004]	[0.014]	[0.004]	[0.02]
Mother is Literate	-0.28	-0.77***	-0.05	-0.38	-0.034	-0.31
	[0.24]	[0.29]	[0.043]	[0.28]	[0.078]	[0.46]
Marital status of Mother	0.18	0.32	0.02	0.10	0.137	0.45
	[0.17]	[0.36]	[0.07]	[0.27]	[0.11]	[0.40]
Size of the Household	-0.004	-0.04	-0.007	0.005	-0.003	-0.089
	[0.034]	[0.055]	[0.012]	[0.05]	[0.02]	[0.10]
Altitude of The village	0.0004	0.001	0.0001	0.001	0.0001	0.001
	[0.0005]	[0.001]	[0.001]	[0.007]	[0.0002]	[0.001]
Village Infrastructure	0.01	0.01	0.003	0.02	0.016	0.018
	[0.018]	[0.018]	[0.005]	[0.025]	[0.015]	[0.06]
Year of Birth FE	YES	YES	YES	YES	YES	YES
Agro-Ecol.Zone FE	YES	YES	YES	YES	YES	Yes
Constant	-3.12***	-3.74**	-0.50*	-3.89***	-0.55*	-3.9**
	[1.05]	[1.90]	[0.29]	[1.18]	[0.32]	[2.06]
Test for endogeneity			2.4+		3.7**	
K-P test for underidentification			5.0**		4.6**	
K-P test for weak identification			10.67		14.68	
Hausman specification test for exogeneity Of the instrument	6.10**			4.57**		5.57**

Notes: Robust standard errors in brackets. *** significant at 1%; ** significant at 5%; * significant at 10%; + significant at 12%.
Data source: World Bank and Burundi Statistics Institute 1998.

Figures

Figure 1

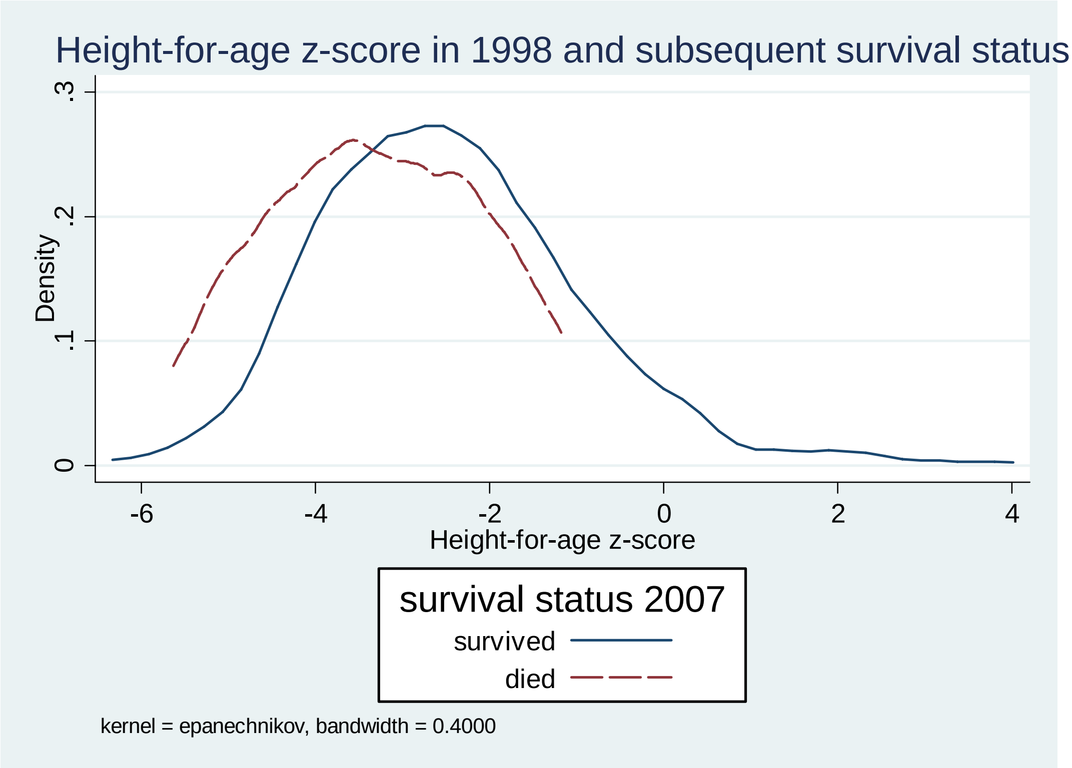


Figure 2

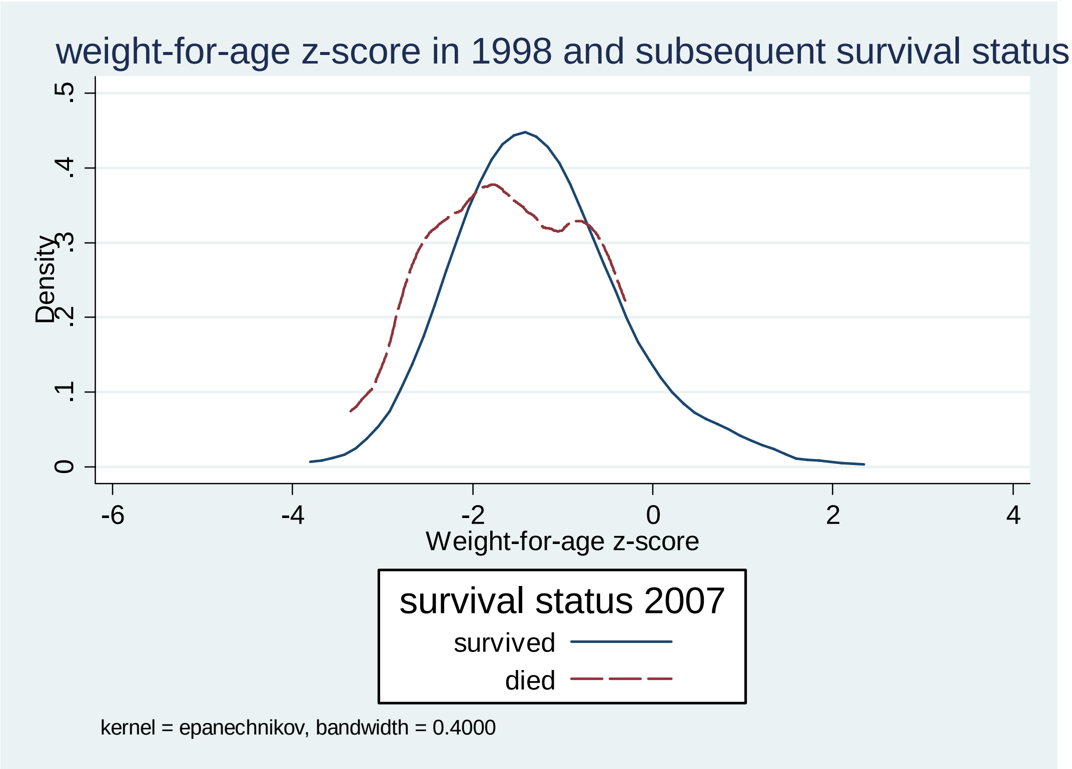


Figure 3

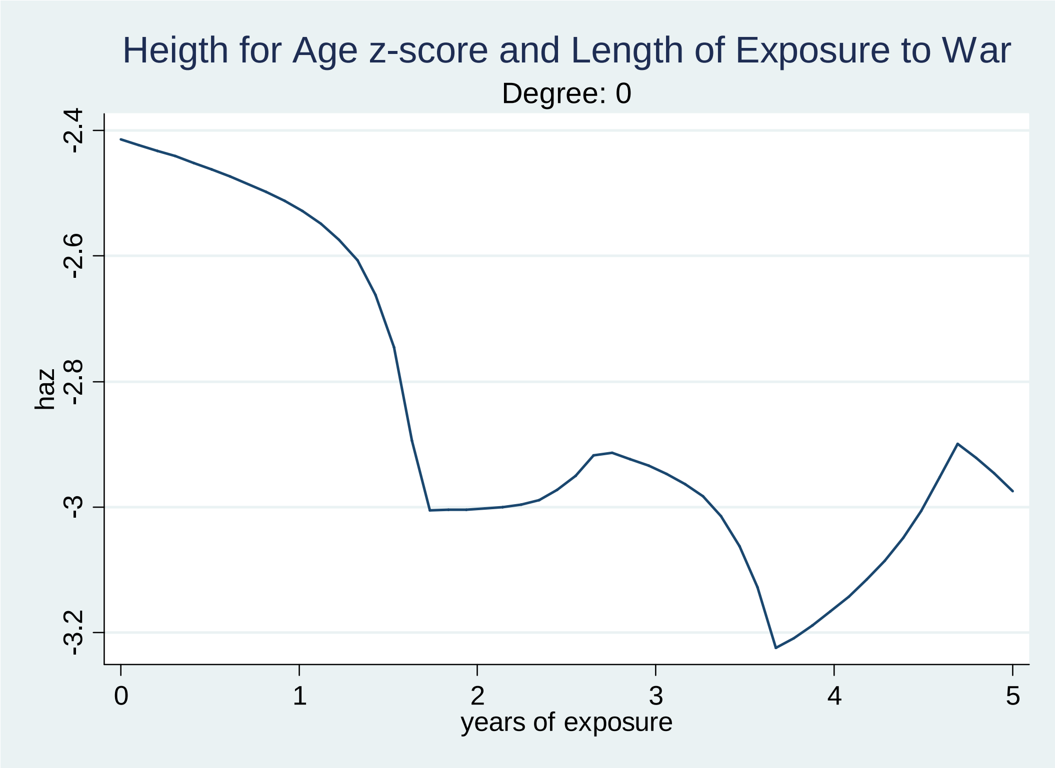


Figure 4

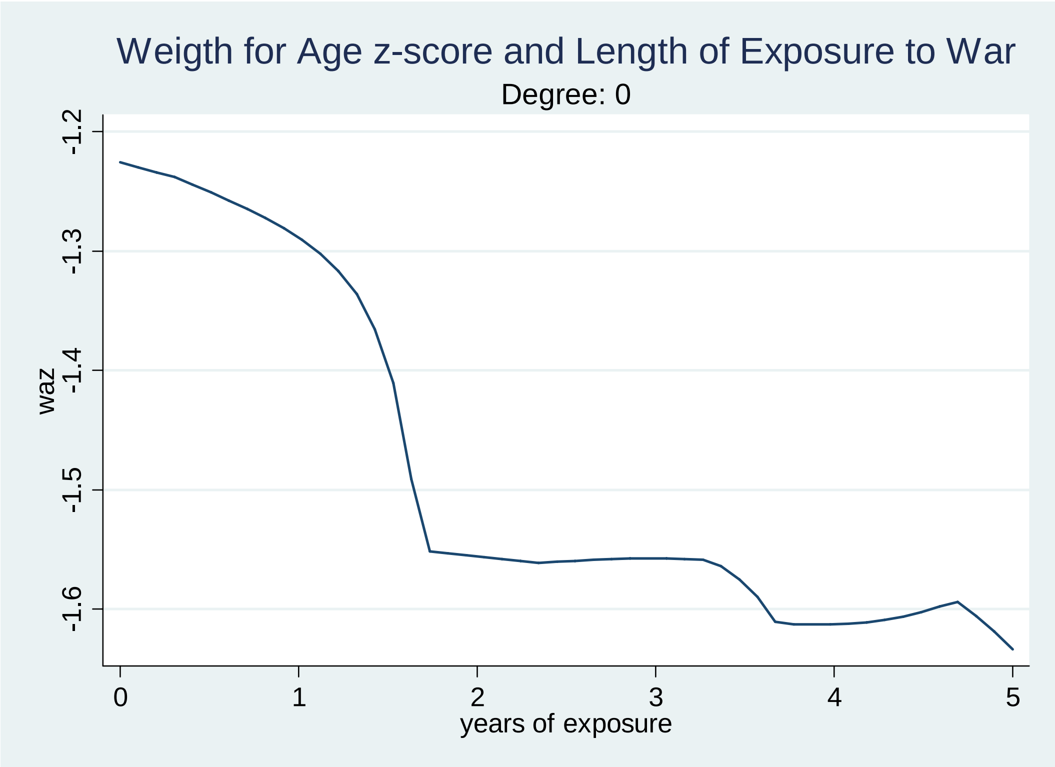


Figure 5

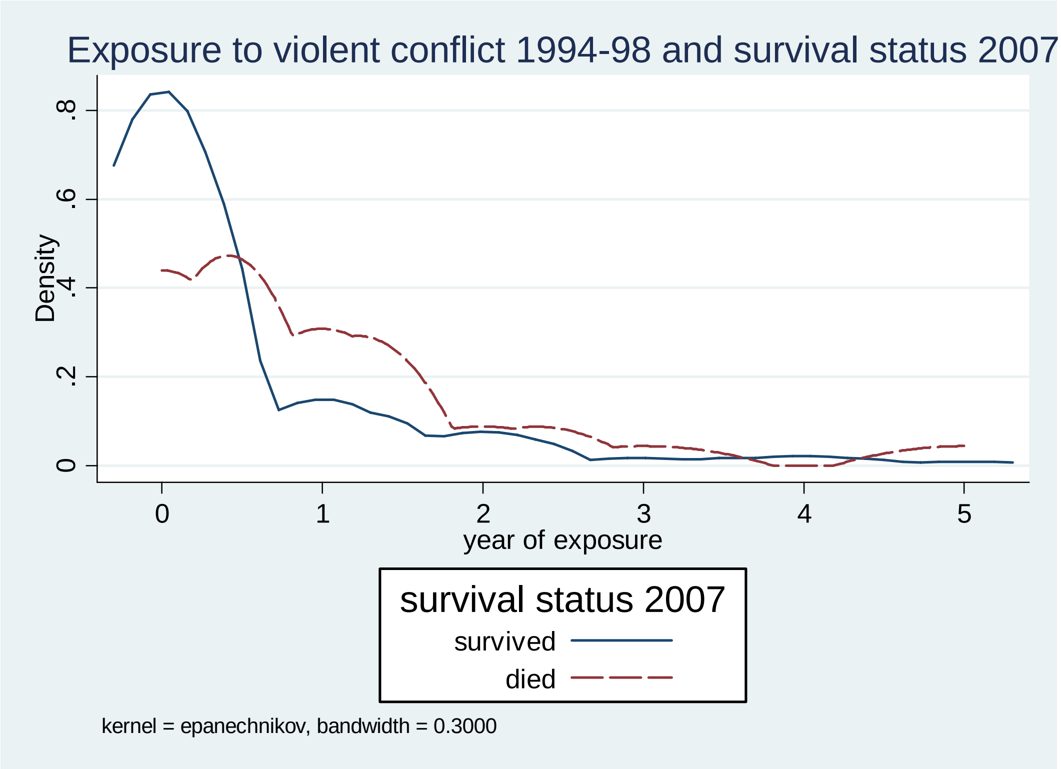
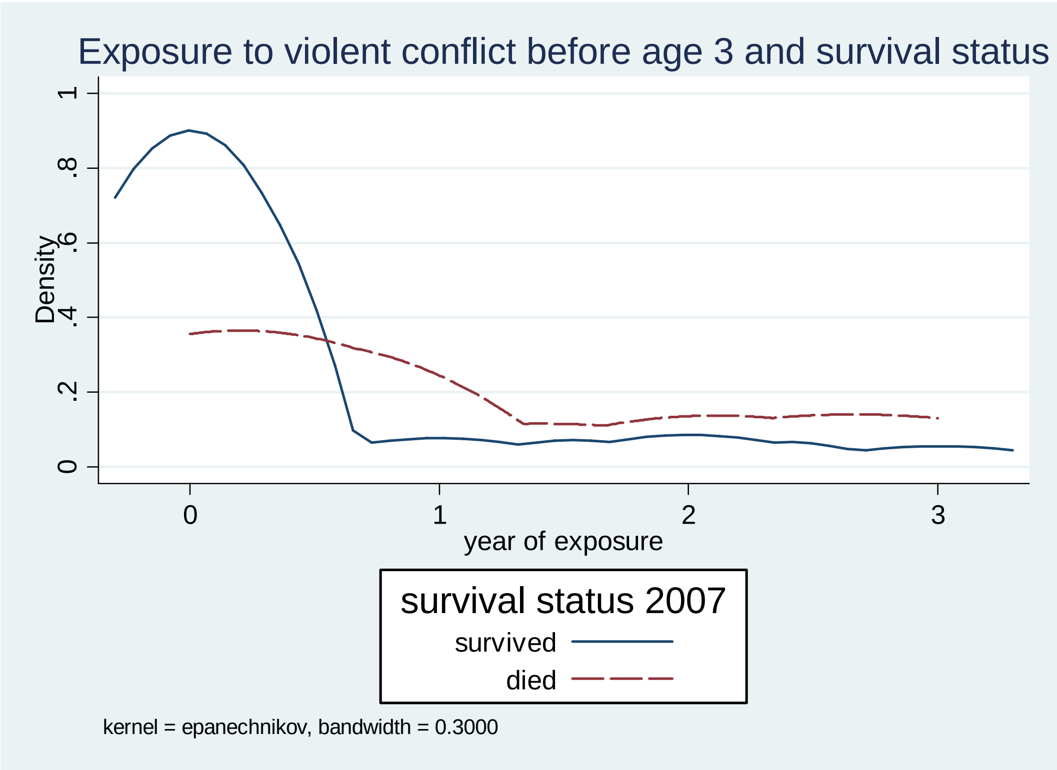


Figure 6



Appendix: Issues of attrition

Because not all children measured in the 1998 survey were re-interviewed in 2007, our analysis may face potential problems of selectivity bias. First, before we performed any statistical analysis, we were relatively confident that selectivity would not pose a problem because the selection of household where children were measured in 1998 as well as our selection of households to be traced and re-interviewed occurred randomly. The only source of selectivity bias can therefore come from households who we selected for tracing but who could not be traced or re-interviewed. Panel A in Table A presents means difference tests on the 1998 means for traced and non-traced children. Panel B shows Probit regressions estimating the probability that the child is included in the second round of the survey. None of the variables has a statistically significant difference on the means and none of the variables predicts inclusion into the sample in a probit regression. We can thus be relatively confident that potential selection is not biasing our analysis.

I have also performed an analysis of attrition in the 1993-1998 period using the 2002 UNFPA-EDS dataset that has the migration history of each individual starting in 1993. The results (not shown) do not raise concerns that violence-induced migration or drop out from the sample may be a cause of potential bias.

Table A: Exploring Potential Selection Bias, N=1170[a]

Child and Household characteristics in the 1998 survey	(1) Child not traced in 2007 N=887	(2) Child traced in 2007 n=283	(3) Mean Difference (1)-(2)
Panel A: Age Distribution			
Child's Age (in months)	31.35 [0.48]	31.85 [0.88]	-0.50 [1.00]
Number of year exposed	0.72 [0.30]	0.66 [0.51]	0.06[0.20]
Child is Female	52.64 [0.16]	52.29 [0.03]	0.34 [0.03]
Size of the household	6.05 [0.07]	5.99 [0.13]	0.06 [0.15]

Panel B : Probit Analysis of Potential Selection Problem	Dependent variable: 0/1 traced and included in 2007	
	(1)	(2)
Number of years exposed	-0.07 [0.07]	-0.17 [0.17]
Child Female	-0.009 [0.07]	-0.03 [0.077]
Size of the Household	0.001 [0.03]	-0.004 [0.03]
Mother characteristics	*Yes*	*Yes*
Year of Birth Fixed Effects	No	Yes
Agro-Ecological Zone FE	No	Yes
Constant	-0.50**[0.24]	-2.64[0.53]
Chi2 Statistic	3.58	7.19

(a) The 2007 survey only recorded village-level exposure to civil war in a subset of the 1998 villages. Consequently, exposure in Table A is measured at the province level (using secondary literature, as in Bundervoet, Verwimp, Akresh 2009) and in number of months exposed. Of the 1196 children with valid anthropometric data in 1998, 1170 had valid data on mother's characteristics.