Education In Extreme Environments, Does Mother's Education Still Matter?

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Keywords:
child labor, market imperfections, wealth paradox, sub-Saharan Africa

JEL codes:
012, D13, J13, J82

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Keywords: child malnutrition, wasting, stunting, underweight, maternal education, mid-upper arm circumference, Somalia

JEL classification: I1, I2, I3

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Introduction

Malnutrition and child health in general have been the subject of numerous studies and it has generally been argued that mother's education, was an essential determinant of child health. The relevance of mother's education might however be questioned in environments where her ability to provide care is challenged. Extreme situations often involve addressing the most pressing issues and the provision of education is often left for later. However if educated mothers could buffer extreme events for their children, then providing education may still be deemed relevant as a factor of resilience. To investigate whether education could still matter, we examine Internally Displaced Person (IDP) Camps in Somalia after the famine was declared. The camps offer another perspective to understand how education influences offspring' health.

IDP camps are comparable to refugee camps except that IDP have not crossed any internationally recognized borders. Families coming into such camps usually had to flee their home because of conflict and/or famine and find themselves in a new environment with almost always no asset and dependent on what the camp may provide. Internally displaced children of conflict-affected regions have one of the highest rates of acute malnutrition in the world. However there is no or little research on malnutrition in these camps.

The present study focuses on Somalia which has one of the highest number of IDPs in the world. According to UNHCR, more than 1.3 million people are classified as IDPs in Somalia. More than 60% of them are in an acute food security crisis. Children in IDP camps face the highest food security crisis of the whole country (FSNAU *Gu* 2012 report). Studying malnutrition in Somali camps is therefore a key element to fight malnutrition overall.

Most research on Somalia focused on its lack of government, (Leeson 2007, Powell, Ford and Nowrasteh 2008). However the North of the country has had institutions working as a government for more than 20 years. It is even more important now as a new government is in place since August 2012 for the whole country, offering new hope to tackle the nationwide issues linked with IDPs. Some studies look at IDPs but focus on host communities (Alix-Garcia and Saah, 2010 for Sudan; and Alix-Garcia, Bartlett and Saah, 2012 for refugees in Tanzania). A study by Engel and Ibanez (2007) in Colombia investigated internal displacement by examining the household decision of leaving the home. Given the temporary nature of camps, it is quite understandable that no study focused on them. However, in Somalia, the proportion and duration of the situation should trigger more interest. As mother education is one of the strongest determinant of child health and because IDP camps provide different mechanisms for mothers, looking at how educated mothers tackle child malnutrition is crucial.

Caldwell (1979) emphasized the role of maternal education in child mortality in Nigeria. He advances several pathways to explain their relative advantage: they could disrupt with traditional belief typically involving a fatalistic acceptance about illness; they could interact better with medical staff and infrastructure or they could simply have more power inside the (extended) household and have a higher control over health-related decisions. Number of studies have since addressed mother education and its effect on offspring's survival and health and it is generally agreed that mother's schooling improves child health (using school supply increase, see Günes, 2015; Maiga, 2012 . For a review and a focus on sub-Saharan Africa, see Hobcraft, 1993). Boyle and alii (2006) even argue that education matters more than wealth.

In the context of IDP camps, some of the pathways might be shut down. First, the typical shift in power structures inside the household will likely be experienced by most women. When a household sets in an IDP camp, it is usually solely the wife and children who settle in while the father remains with the (potential) cattle. Furthermore, women may have ration cards giving them more economic independence and therefore more power for family decisions. As a result many families will experience changes in familial power structures regardless of the mother's education. Second, assistance being provided to all, the usual higher association of educated mothers with health infrastructure (due to a higher association to the "modern" world or higher self-confidence) is no longer limited to the educated mothers.

The aim of this study is two-fold. First, we want to see whether mother education influences child health in the camps and if so, whether it differs from what can be observed outside camps in Somalia. Second, we want to know how this effect varies with time spent in the camp. Though being displaced should be a temporary situation it can be seen in many parts of the world that this situation may last. If educated mothers simply learn faster, their relative advantage should not last. We find that the effect of maternal education slightly differs for short to middle term health measures. Educated women are even more effective at tackling malnutrition when they are in the camp than when they are outside. Relying on the pseudo-panel dimension of the data, we find that the relative advantage of educated mothers widens as time spent in the camp increases. The rest of the paper is structured as follows. The next section describes the data. The third section presents the empirical strategy. Section four shows the results. Section five discusses the mechanisms and limitations. The last section concludes.

Data description

Somalia has been under civil war since Siad Barre's regime collapse in 1991. The North with Puntland in the East and Somaliland in the West, rapidly separated from the South and gained security while South-Central Somalia is still unstable. In addition to the conflict, the famine hit the country in 2011 and left many pastoralists destitute which exogenously increased the number of IDPs in the whole country.

The Food Security and Nutrition Analysis Unit for Somalia (FSNAU) was created to provide data and analysis on food security, nutrition and livelihoods. It has been conducting surveys since 1994 and since 2011 it provides, coordinating with other UN agencies, representative surveys for the Northern and Central regions of Somalia twice a year.

The present study relies on two nutrition assessments called the (post) *Deyr* 2011 and the (post) *Gu* 2012[1]. As children represent the most sensitive segment of the population, their assessment is used as an indicator for

[1] *Gu* and *Deyr* are Somalia's rainy seasons.

the general situation in the nutrition surveys. IDPs are surveyed in camps around 8 cities: Berbera, Borao, Bossaso, Dhuusamareb, Galkayo, Garowe, Hargeysa and Qardho. The data are collected according to a two stage cluster sampling except for camps around Dhuusamareb and Qardho where only a rapid assessment was made. In the *Deyr* 2011 assessment, data were collected in November for IDP camps and December outside camps. In the *Gu* 2012 survey, data were collected in May for IDP camps and July for non camps. Different individuals were surveyed across the two waves so that the data are not panel data. For security reasons, the south of Somalia was surveyed using focus groups (not household surveys) and cannot be included in our analysis.

The post *Deyr* assessment took place after the 2011 famine (officially declared in July) where four million people, that is more than half of Somalia's population, faced an acute food security crisis. In the *Gu* 2012 survey, the figure dropped to 2.12 million people (28% of the population). Despite this improvement, the number of people in need remained among the highest in the world. 70% of people in food security crisis live in the South. 236,000 under-five-year-old children (16% of the child population) are acutely malnourished, 54,000 are severely malnourished, that is, in need of immediate intervention. Focusing on IDPs, 800,000 of the total 1.36 million in the country were in acute food security crisis at the time of the *Gu* 2012 assessment (figures are taken from the FSNAU Nutrition *Gu* 2012 Report).

The survey supplies information on age, gender, weight, height and mid-upper-arm circumference for children aged 6 to 59 months allowing for the computation of four anthropometric outcomes. Information on immunization, vitamin A supplementation, self-reported illnesses (for the caretaker) and child illnesses (reported by the caretaker) is also available. Information on the caretaker's age and education is collected, that is, information on maternal education is not actually given. However, in our context, clearly, the caregiver is the mother unless she is not present. As we select on 15-49 year old females reporting their mid-upper arm circumference and because the first reporting should be the mother's, the issue should be very limited. Furthermore, the effect of the person caring for the child also matters. Lindelow (2008) shows that not only maternal education matters but also education of other (non spouse) household members. Handa (1999)'s work on Jamaica suggests that household education, not necessarily maternal education, strongly correlates with child health. Chen and Li (2009) found on a survey of adoptees in China that the effect of maternal education on adoptees was similar to the effect on own birth children. In the end, we will interpret the effect of the caretaker's education as the mother's effect even though we cannot guarantee that it is so.

We now present the anthropometric measures. The MUAC or mid-upper-arm circumference, gives an indication for both fat reserve and muscle mass and is easy to collect. As argued by Alderman, it is a good estimate for child health (O'Donnell, 2008; Roy, 2011) and can be used to assess the local average health of children. Stunting, wasting and underweight are standard measures. A child is stunted if s/he has a low height-for-age (HAZ). It is a sign of sustained and accumulated episodes of undernutrition and/or repeated infections. Wasting, defined by a low weight-for-height (WHZ), is a sign of acute (moderate or severe) malnutrition and is a result of a weight loss associated with recent periods of starvation or disease. Wasting is by definition less sensitive to age measurement errors typically associated with age heaping in developing countries (as age is only needed at the stage of standardization, not before). Underweighted children are too light for their age (WAZ). It is an intermediate measure between stunting and wasting. Note that "too low" for anthropometric measures means more than two standard deviations away from the distribution of healthy children of same age and sex as given by WHO in 2006 (Espejo, 2007). The score computed from this comparison and measured in standard deviations is called a Z-score. The healthy population for standardization is international which is arguably unsatisfactory. Nonetheless, studies have shown that distributions from upper or middle-class families in developing countries do not differ from international references and heights or weights for refugee children from Asia in the US, converge rapidly to those of American born children (WHO, 1995; Habicht and others, 1974; Yip, Scanlon and Trowbridge, 1992). The distinction between moderate (below two standard deviations) and severe (below three) outcomes is not undertaken in the data description so that moderate outcomes comprise severe outcomes. For the regressions, we will specifically investigate the Z-score associated with each health outcomes and whether they fall below two standard deviations.

Table 2 presents descriptive statistics and reveals that for any indicator chosen, malnutrition is extremely alarming, especially in the camps. A striking 37% of surveyed children suffered from measles, diarrhea, pneumonia or fever in the last month (simply referred to as "sick" in the table). One in five child suffers from acute malnutrition, 21 % are stunted, 27% are suffering from underweight, 19% of children are wasted and 14 % have a low MUAC. Comparing households inside and outside camps, one finds that the caretaker and the children are both slightly younger inside camps though the difference is small. Outside camps, the typical household is slightly smaller with 5.3 members including two children. Households in IDP camps seem to be, in general, better taken care of: the source of drinking water seems safer, they are more likely to be vaccinated against measles or to have received the three injections against polio. Mothers are also receiving more injections against tetanus. Children outside camps are on average healthier. The difference for wasting is lower than the difference for stunting probably due to the fact that stunting reflects repeated shocks or chronic malnutrition when wasting only shows recent periods of nutritional shock. MUAC usually gives results similar to wasting which is roughly what is found here. Even if children outside camps are doing better than children living in camps, at least 10% are suffering from stunting, wasting, underweight or having a low MUAC. Note that children in camps are twice as likely to be registered in a feeding center (consistent with their twice as bad health outcomes).

Regarding the caretaker's education, table 2 shows very low levels of education inside and outside camps. Only one woman out of three completed primary school outside camps and 15% in camps. As assistance (when available) aims at being provided equally, it is possible that schooling no longer matters inside camps. To assess the share of variance accruing to differences between households in camps and household outside camps, a variance analysis is run and both health outcomes and education vary enough within group so that being in a camp does not explain it all. Cities around which the camps are gathered are far from one another and reflect different intensity at which households may be hit by undernutrition. Climate and in particular rainfall may differ and lead to differences in agricultural output that may translate into more stressed situations regarding food security. Camps further away from the south have on average better health outcomes. Another variance analysis is run and shows that most of the variation comes from within the camps (as opposed to across camps). All descriptions are based on the *Gu* 2012 assessment. Results are qualitatively similar for the previous assessment.

Empirical Strategy

Simple comparison strategy

To best investigate health, accurate variables are essential and anthropometric outcomes will be used. They are measured by trained enumerators and do not suffer from the reporting bias. The reporting bias is the inability of poorly "health aware" individuals to notice that their child is sick. Because "health aware" individuals might not be randomly distributed among educated and uneducated caregivers, the results might be biased. Using anthropometric measures avoid such bias.

To compare the effect of education inside and outside camps, we will simply pool in the same regression households in camps and outside camps and see whether the term *education*camp* is significant. With region fixed-effects, the empirical model assessing the health H_{ijl} of child i, from household j in community l is simply:

$$P(Y_{ijl}=1)=\beta_0+X_i\Gamma+\beta_1 edu_j+X_j\Delta+\alpha C_j\times edu_j+\delta C_j+\sum_{l=1}^{L}\alpha_l r_l+\epsilon_{ijl} \quad (1)$$

with $Y_{ijl}=\mathbb{1}_{(H_{ijl}<-2)}$ equals one for a lower than normal Z-score; zero otherwise. X_i corresponds to child sex and age, edu_j indicating whether caretaker j for child i has at least a primary school level, X_j includes caretaker's age and its square, C_j indicates if the household lives in a camp, $r_l=1$ if the area considered is l. Errors are assumed to follow a standard Gaussian distribution.

There is no control related to the father (in particular education) simply because information on the father is lacking. Aslam and Kingdon (2012) find that maternal education mattered more for actual health and paternal for vaccination. There is very often, if not always, a strong and positive correlation between spouses' education. To that extent by measuring the caretaker's education, we are capturing the global effect of education in the household.

Wealth is not reported whereas it is an important determinant of child health. It is not clear however that people in camps have any wealth and if so whether some households would actually be wealthier than others. In many countries in sub-Saharan Africa, wealth is often measured by the land quantity or owned cattle which is typically left behind. Wealth is sometimes measured by an asset index and asset possession used to be part of the questionnaires for IDPs. However less than 5% of the population hold each of the assets (and we expect a much lower percentage for the crisis period under study) and for this reason it is no longer collected and therefore building an asset index is not possible. In the end, the regression does not include controls for wealth.

It should be emphasized that it is not possible to capture the causal effect of education. This is not an issue in the sense that our interest lies in assessing the differential effect between the two groups. If educated mothers are also healthier then they are more likely to be educated and transmit good genes to their child. Innate ability might translate into higher schooling and better care of the children. Finally time preferences translate into more patient individuals being more likely to go to school and also take better care of the children (through more long term investment such as vaccination for example). To summarize, whether in the camps or outside the camps, the effect of education could be overestimated (the level of education is so low that, as argued later, the bias is likely small). Since all biases go in the same direction, the sign of the difference between the educated and uneducated mothers will be correctly captured. Coming back to the selection into camp bias, we expect this bias to be orthogonal to the bias existing between educated and uneducated women (especially since the unpredictability of the famine can be seen as an exogenous shock for entering camps).

As the conditions outside camps are not representative of what all Somalis experience, another way to look at the effect of the camp is to investigate whether time spent in the camp has any effect which is undertaken hereafter.

Double difference strategy

We want to see whether the relative advantage of educated mothers vanishes away with time spent in the camp. As the camp provides a homogeneous provision of care, the differences previously shown might simply translate the ability of educated mothers to take advantage of the newly available opportunities as they arrive. If the relative advantage of educated mothers widens then educated mothers might also be better at utilizing the infrastructure available. We are therefore interested in the interaction term between education and time spent in the camp, "*duration*".

However, *duration* is not readily available. Fortunately in *Deyr* 2011 and *Gu* 2012 assessments, a question regarding the residency status indicates whether households have spent more or less than six months in the camp (they could also report to be residents, i.e. households from the region who came into the camp). Based on this, a double difference strategy is used. The first difference lies in the comparison of households who spent less than six months in the camp, later referred as "new"; with households who spent more ("old"). Note that it will cancel out any year specific effect. The second difference compares regressions for *Deyr* 2011 and six months later (i.e. *Gu* 2012). The assessments not constituting panel data, the analysis therefore consists in exploiting the pseudo-panel dimension of the data and the following assumptions are made:

- Assumption A: The population of "old" and "new" people in Deyr 2011 are similar to the "old" people in Gu 2012, except for the fact that they have spent six more months in the camp.

- Assumption B: "New" individuals have spent an amount of time d (in months) in the camp. In other words, "new" people in Deyr 2011 had on average spent $d \in (0, 6)$ months in the camp at the time of the Deyr 2011 survey. People considered as "new" in Gu 2012 had also spent an average of d months in the camp at the time of the Gu 2012 survey.

The effect of education on health for the new people is captured by:

$$\beta_0 \ edu \ + \beta_1 \ edu \ \times \ duration \ = \beta_0 \ edu \ + \beta_1 \ edu \ \times \ d$$

The *Deyr* 2011 survey has two groups of people: the "new" and the "old". In the *Gu* 2012 survey, taking place six months after the *Deyr* 2011, we have three groups of people: the new in 2012, and, among the old in 2012, those who were old in 2011 and those who were new in 2011 (that is the new in 2011 become "old" in 2012). The double difference strategy would consist in comparing the four groups made up of: people new in 2011 (who spent d months in the camp), people new in 2012 (who also spent d months in the camp), old people in 2011 (who spent $D > d$ months in the camp), and finally old people in 2011 six months later (who spent $D + 6$ months in the camp). Again, as we do not have panel data, "new" people six months later should be read as a group representative of the people new in 2011, interviewed in *Gu* 2012 and "old" people six months later should be read as a group representative of the people old in 2011 interviewed in *Gu* 2012.

The effect of education on each group is best described by the table hereafter:

	Deyr 2011	Gu 2012
Old	β_0 *edu* $+ \beta_1$ *edu* $\times D$	$\boldsymbol{\beta_0}$ ***edu*** $\boldsymbol{+ \beta_1}$ ***edu*** $\boldsymbol{\times (D+6)}$
New	β_0 *edu* $+ \beta_1$ *edu* $\times d$	β_0 *edu* $+ \beta_1$ *edu* $\times d$
Difference (Old - New)	β_1 *edu* $\times (D - d)$	β_1 *edu* $\times (D + 6 - d)$

Differentiating out the effect in the *Gu* 2012 survey from the effect in the *Deyr* 2011 survey, one obtains the differential effect of mother education according to whether she has spent six more months in the camp:

$$[\beta_1 \ edu \ \times (D + 6 - d)] - [\beta_1 \ edu \ \times (D - d)] = 6\beta_1 \ edu$$

However this is not exactly what we have. New households in 2011 became old households in 2012. And old households in 2011 are still old households in 2012. To that extent, among the "old" households in 2012, it is not possible to distinguish who was also old in 2011 from who was new in 2011. In other words, the old group in 2012 (the group in bold in the previous table) is not solely composed of the old households from 2011. The groups at hand are the four groups illustrated in the following table:

	Deyr 2011		Gu 2012
Old	$\boxed{D}$	$\longrightarrow$	$\boxed{\alpha(D+6) + (1-\alpha)(d+6)}$
New	$\boxed{d}$	$\nearrow$	d

with α the proportion of "old" households in *Deyr* 2011 (and $(1 - \alpha)$ the proportion of "new" households in *Deyr* 2011). Table 1 shows the differentiation that is actually taking place when the double difference strategy is used.

Table 1: Arrivals in the camps, assessment period and effect of education

	Deyr 2011	Gu 2012
Old in 2011 and 2012	$\beta_0\ edu + \beta_1\ edu \times D$	$\beta_0\ edu + \beta_1\ edu\ \times (D+6)$
New in 2011, old in 2012	$\beta_0\ edu + \beta_1\ edu \times d$	$\beta_0\ edu + \beta_1\ edu\ \times (d+6)$
New in 2012		$\beta_0\ edu + \beta_1\ edu \times d$
Difference (Old - New)	$\beta_1\ edu \times (D-d)$	$\beta_1\ edu\ (\alpha(D+6) + (1-\alpha)(d+6) - d)$
		$= \beta_1\ edu \times (\alpha(D-d)+6)$

d stands for the duration of households who spent less than six months; $D > d$ stands for those who spent more than six months. α is the share of people "old" in *Deyr* 2011.

Finally differentiating out between the two dates, one obtains:

$$\beta_1\ edu\ \times ((1-\alpha)(d-D)+6) \tag{2}$$

Of course, assuming α close to one, leads to the previous case and it is as if we had the four previous groups. α can a priori be assessed from the proportion of newcomers in *Deyr* 2011 and to the extent that the population and proportion are roughly the same six months later the values of α, d and D can be recovered. Assuming that people who spent less than six months in the camp have spent on average 3 months and that people who spent more than six months spent on average two years (based on qualitative interviews with the enumerators) leads to $d = 3$ and $D = 24$. From the data, $(1-\alpha)$ could be assessed to equal 0.12. However the number of people who spent less than six months seems too small for differentiating out any effect in Berbera, Bossaso, Burao and Hargeysa. Withdrawing these camps leads to $(1-\alpha) = 0.18$. With the preceding values, one obtains:

$$\beta_1 \ edu \ ((1-\alpha)(d-D)+6) = \beta_1 \ edu \ (0.18(3-24)+6) = \beta_1 \ edu \ 2.22$$

So that the double difference strategy would capture the average additional effect of education when spending an extra 2.22 months in the camp. Our evaluation of α might be an upper bound (newcomers are more likely to leave and therefore their proportion six months later is supposed to be smaller) and the extra number of months is a priori between 2.22 and 6. In the rest of the paper this value is only referred to as "spending an extra semester" in the camp.

Table 1 simplified the notations by showing only the effect for education, yet the model is (removing individual and mother subscripts) :

$$\begin{aligned} P(Z<-2) =& \alpha_0 \ edu \ + \alpha_1 \ edu \times duration \ + \alpha_2 \ duration \\ & + \alpha_3 \times \mathbb{1}_{(t=2012)} + \alpha_4 \times \mathbb{1}_{(t=2012)} \times \ duration \\ & + \alpha_5 \ edu \times \mathbb{1}_{(t=2012)} + \alpha_6 \ edu \ \times \mathbb{1}_{(t=2012)} \times \ duration \\ & + \Gamma X_c + \sum_{l=1}^{L} \alpha_l r_l + \epsilon \end{aligned}$$

X_c encompasses child gender, age, its square, age of the caretaker and its square. $\mathbb{1}_{(t=2012)}$ is an indicator equal to one if the assessment corresponds to *Gu* 2012. *duration* is time spent in the camp. Note that the value of *duration* is not known (as d and D are not known and we do not impute the values we assessed to them into the regression). Instead, an indicator called "more6m" equal to one if households have been there for more than six months (zero otherwise) is introduced. The effect on health of spending an extra semester in the camp interacted with education will be captured by $\mathbb{1}_{(t=2012)} \times$ more6m $\times\, edu$, that is, coefficient α_6.

As a robustness check, the result section considers a similar model with the Z-score as the dependent variable and adds mother's health as a control.

Results

Among the city camp surveyed, three are in the North West where information outside the camp is lacking. A test of mean comparison between camps in the North West and the other camps showed no statistically significant difference for wasting and for education but did for the incidence of stunting and low mid-upper arm circumference. As the effect of education between camps in the North West and North East was found statistically different in the regressions (not shown) and no information outside camps in the North West is available, camps in that region are dropped. Camps next to Dhuusamareeb are removed since information on maternal education is missing for the Deyr 2011 assessment. For the Gu 2012 assessment, there were no significiant difference whether the camp was included in the sample or not.

Table 3 presents the coefficient for the probit model in equation 1[2]. Table

[2] The coefficients of the probit models are reported as opposed to the marginal effect as the interest lies in knowing whether there is a differential effect between the groups of educated and uneducated mothers (Puhani, 2012). Unsurprisingly the magnitude of the interaction term (with a computation that accounts for the fact that this magnitude is not reflected by the marginal effect of the interaction term as highlighted by Ai and Norton (2003)) leads to similar conclusions.

4 presents the estimates the underlying variables height for age, weight for height, weight for age and the mid-upper arm circumference (muac). The first column of table 3 in panel A shows the results with local fixed effects of the logit regression of the four health outcomes for people living outside camps. The second column for households living in the camps and the third column for both samples. Panel B shows the same regressions without regional fixed effects. To partially account for local characteristics, the specifications without fixed effects controls for the prevalence of malnutrition at the local level. Results are similar whether these controls are included or not. More generally the results are similar with or without fixed effects (which may be due to the large areas taken into account for the fixed effects) and we focus on panel A. The results of the third column panel A indicates that the effect of the caregiver's education is significantly different inside and outside camps for wasting. The total effect of education (by itself *and* through its interaction) is also significant and negative. None of the other outcomes show a statistical difference inside and outside camps. The total effect of education is also significant for underweight. In the end, table 3 shows that the effect of education varies for short-to-middle term health outcomes. Table 4 mirrors this result with the total effect of education being significant and positive for the four health outcomes. However, none of the health outcomes seem to be impacted differently according to whether the caregiver lives in the camp or not[3]. This indicates that the effect of education in the camp is

[3]For the sake of comparison and clarity, the results displayed restrict the population in camps to be the same as the population in camps used for the double difference strategy afterwards. The double difference strategy requires dropping the camps around Bossaso. If they were included, we would find similar results: the relationship with education is significantly different only for short to middle term health outcomes and appears only

particularly salient to combat malnutrition more than to simply improve the nutritional status which seems consistent with the opportunity that a camp may offer.

We now turn to the second estimation strategy. Table 5 displays the coefficients for the double difference regressions with region fixed effects. The camps next to Bossaso are dropped because the number of households present for less than six months is too small to capture any effect. As a result, only the camps next to Galkayo, Garowe and Qardho are used. The results show that the effect of maternal education on child health can differ for stunting and underweight. However, computing the total effect of education, it is only significant and negative for the underweight outcome. This indicates that as time spent in the camp increases, educated mothers interact more or better in their new environment. Table 6 indicates the same patterns for the Z-scores. The regression in column 3, with the weight for age Z-score (associated with underweight) indicates a differential impact for educated mothers who spent a semester more in the camp. The total effect of education is positive and significant at the 5% level, thereby confirming the results associated with underweight.

The previous estimations were based on having a cut-off value discriminate children in poor health from children in better health. Studies using Z-score are usually able to partial out the effect of parental health. For example, studies examining height-for-age (associated with stunting) usually control for the mother's height. In the dataset, there is no clear measure

when investigating whether the z-score falls below two standard deviations.

to account for the caregiver's health. A measure of self-reported health is available but such measures might be biased if some mothers fail to realize that they are sick. Another objective measure is available, the mid-upper arm circumference of the mother. There is no standardization available for this measure as the debate regarding the relevant cut-off point for adult MUAC has not been settled yet. If we nevertheless include a measure of the mid-upper arm circumference of the caretaker, we find similar results as shown in the appendix tables 8 and 7. Controls regarding pregnancy or lactating status (which may be of particular interest when using the caretaker's arm circumference as a control) of the caretaker do not change the results. These results are robust to the inclusion of camps in the North West[4]. To summarize the results, the comparison showed that educated mothers were more effective at tackling wasting inside the camps and that their relative advantage in tackling underweight increases with time spent in the camp.

Discussion

The previous section reveals that the situation inside and outside camps differs slightly for short-to-middle-term outcomes. The effect of mother's education was significantly different only for wasting. Examining the differential effect of education after spending an extra semester in the camp, we find that children of educated women are doing even better as measured by a higher weight for age (and a lower probability for underweight). As wasting is an indicator of short term health and underweight more of a short to mid-

[4]Namely camps close to Hargeysa, as the two other city camps had less than 5% of newcomers and were therefore excluded.

dle term outcome, it is possible that a certain amount of time in the camp is needed in order to see how the last episodes (which must have happened in the camp to see the actual effect of the camp) were handled by educated and uneducated caregivers. This would explain why the differential effect for underweight is only captured when specifically looking at the evolution of the effect of education in the camp and not when comparing with educated mothers outside the camps[5]. Indeed when we compare households inside and outside the camps, the households inside the camps might have been there for a short time, yet all households are pooled together regardless of the time spent in the camp.

As has been argued in the introduction, the typical power structure change inside the household is unlikely driving the effect of education. Also the ability to manipulate the modern world, as care is provided equally, is unlikely to occur. However if the effect of education is not strictly in the higher probability to interact with medical infrastructure but in their better utilization, then the effect of education will still occur and should be higher for short term outcomes inside camps than outside camps where medical care might be less available (medical aid provided in IDP camps usually aims at addressing immediate needs though vaccination, which is not only for short-term outcomes and is always provided when possible). This explanation seems to fit in with the results obtained. Indeed, the effect on underweight is stronger inside camps (actually there seems to be no effect of

[5]In the simple comparison, the total effect for underweight was significant, it was simply not significantly different from the effect that educated mothers had on their offspring outside camps.

education outside camps). The total effect of education for wasting was not significant but the interaction on its own was still suggesting a potentially different effect of education inside camps.

If the effect of maternal education transits through this better utilization of health infrastructure, then as long as aid and health care are provided, this effect should hold. Provided that it is only about utilizing health infrastructure better, then uneducated mothers should be able to learn and catch up their educated counterparts so that the relative difference of educated mothers should vanish away[6]. The results focusing on the comparison of educated and uneducated mothers in the camp according to time spent in the camp showed that on the contrary the relative advantage of mother education increased as time spent in the camp increased. A possible explanation is that educated mothers are more likely to notice when their child is sick. If it is so, then in addition to the care provided quasi uniformly in the camp, educated mothers might interact faster whenever their child needs assistance.

We come back to the potential non causal effect of education. As education is not instrumented, one could think that the caretaker's schooling encompasses different preferences for the present, better health or higher ability that would overestimate the effect of education. As argued, the previous strategy assessed the differential effect of education when in the camp

[6]Thomas et al. (1991) find on Brazilian data that almost all of the impact of mother's education could be explained by indicators of her access to information so that both the availability and procession of information are determinant in the impovement of a child's health.

so that the implications of not assessing the causal effect were very limited. We would like to add here that the endogeneity of education when schooling levels are low is likely small as well. Indeed taking the endogeneity linked with health, it can be argued that missing days of schooling or paying less attention to school because one is sick, might have stronger implications for higher level of education. The primary schools are relatively more widespread than other types of school and to that extent any distance that could discourage less healthy children to be sent to school is likely to be more important for higher level of schools. Time preferences are often linked to the relative cost of schooling compared with the future potential foregone revenues. Clearly the cost of schooling as a real cost (in terms of transportation for example) or as an opportunity cost (if the child could be working/helping) is higher as the level of education increases and therefore the time preferences are less obvious for such low level of schooling. Second, we are interested in the differential effect of being in a camp. The fact that preferences for the present could be different inside and outside camps is arguable. What is even more arguable, would be to believe that these preferences could induce a different effect of the education on the health outcomes measured between households inside the camps and households outside the camps, knowing that we are controlling for the effect of the camp. A priori, endogeneity issue should be removed by (at least) the double difference strategy.

The fact that time spent in the camp tend to increase the differences between educated and uneducated mothers might not necessarily be true for all amount of time spent in the camp. It is possible that for households who have already spent a year and a half in the camp, the effect is no longer true. Because time spent in the camp cannot be assessed and because only two assessments were available, assessing a non linear effect of time spent in the camp is difficult. Adding more waves would certainly not allow for a non linear assessment either as the assumption that households remain in the camp is difficult to defend for a longer period of time, in particular its exogeneity. As each person in the camp has been there for a different amount of time, it is at least possible to say that on average spending an extra semester gives a stronger advantage to educated mothers.

Conclusion

This paper has examined how maternal education affects her offspring's health in the context of internally displaced person camps. Comparing households inside camps with households outside camps, we find that the effect of mother's education differs for wasting. We also compare households inside camps depending on their length of stay and we find that education is still an important factor for tackling malnutrition. The effect of mother's education seems to be increasing with time spent in the camp for underweight and the associated weight for age so that the advantage of mothers with at least primary schooling does not vanish away. In a context of destitution, the results highlight the role played by maternal education on child health not only as a way to improve child health but also as a factor of resilience.

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Figure 1: Map of Somalia with the main cities, including the cities with the IDP camps in the survey.

Table 2: Descriptive Statistics for households living inside camps and outside camps, *Gu* 2012 assessment.

	Camp		Outside camp		
	N. Obs	Mean	N. Obs	Mean	p-value
child age, months	4386	29.78	1893	32.39	0.00
caretaker's age	2286	29.23	1063	29.75	0.05
education	2531	0.17	1170	0.29	0.00
primary education (or more)	2531	0.15	1170	0.27	0.00
nb. <5 y.o.	2537	1.84	1170	1.71	0.00
household size	2537	5.76	1170	5.26	0.00
nb. mosquitoe nets	2527	0.49	1170	0.71	0.00
child slept under net	4358	0.30	1893	0.36	0.00
feeding center	4368	0.11	1891	0.05	0.00
vitamin A	4372	0.80	1893	0.72	0.00
measles vaccination	4368	0.81	1893	0.70	0.00
polio, 3 inject.	4350	0.54	1888	0.50	0.00
diarrhea	4380	0.18	1893	0.09	0.00
pneumonia	4377	0.12	1893	0.08	0.00
fever	4378	0.24	1893	0.20	0.00
measles	4374	0.04	1890	0.03	0.07
sick	4386	0.37	1893	0.28	0.00
stunting	4386	0.21	1893	0.13	0.00
underweight	4283	0.27	1893	0.13	0.00
wasting	4386	0.19	1893	0.13	0.00
MUAC < 2 st.dev.	4386	0.14	1893	0.11	0.00
weight for age	4283	-1.44	1893	-0.94	0.00
height for age	4386	-1.09	1893	-0.75	0.00
weight for height	4386	-0.97	1893	-0.78	0.00
mid-upper-arm circumference	4386	-0.96	1893	-0.79	0.00
married, with spouse	2525	0.81	1170	0.75	0.00
married, no spouse	2525	0.08	1170	0.14	0.00
latrine, private	2528	0.42	1169	0.55	0.00
safe water	2485	0.91	1169	0.42	0.00

The p-value gives the probability that IDP and non IDP have the same mean. Variables in bold are *not* child specific (corresponding to caretaker or household). Education equals zero if the caretaker has no education, 1 for primary education, 2 for secondary education and 3 for tertiary education. Polio, 3 injections takes value one if the child had the three polio injections. Feeding center takes value one if the child is registered in one.

Table 3: Probit estimations for the binary health outcomes of children 7 - 59 months old

	Panel A: Fixed effects			Panel B: No fixed effects		
Stunting						
primary school (edu)	-0.121	-0.109	-0.124	-0.150	-0.107	-0.149
	(0.093)	(0.095)	(0.092)	(0.092)	(0.095)	(0.091)
Camp			0.335**			0.050
			(0.154)			(0.093)
Camp * edu			0.007			0.029
			(0.132)			(0.131)
Wasting						
primary school (edu)	0.058	-0.169*	0.058	0.059	-0.165*	0.061
	(0.088)	(0.100)	(0.088)	(0.088)	(0.100)	(0.088)
Camp			0.085			-0.015
			(0.371)			(0.182)
Camp * edu			-0.238*			-0.240*
			(0.133)			(0.133)
Underweight						
primary school (edu)	-0.202**	-0.179*	-0.201**	-0.217**	-0.180*	-0.214**
	(0.092)	(0.093)	(0.092)	(0.091)	(0.093)	(0.091)
Camp			0.686*			0.185
			(0.386)			(0.195)
Camp * edu			0.007			0.018
			(0.130)			(0.130)
mid-upper arm circumference < -2						
primary school (edu)	0.000	-0.099	0.002	0.004	-0.112	0.002
	(0.095)	(0.104)	(0.093)	(0.094)	(0.104)	(0.093)
Camp			0.021			0.072
			(0.098)			(0.074)
Camp * edu			-0.122			-0.125
			(0.139)			(0.139)
N	1602	1720	3322	1602	1720	3322
Population	Not Camp	Camp	all	Not Camp	Camp	all
FE	Yes	Yes	Yes	No	No	No

*p<.10, ** p<.05, *** p<.01; standard errors clustered at the household level in parentheses. Controls include child gender, gender interacted with age, an order three polynomial for the child's age and an order three polynomial for caretaker's age. Region fixed effects included when indicated.

Table 4: Linear regressions for the health outcomes of children 7 - 59 months old

	Panel A: Fixed effects			Panel B: No fixed effects		
height for age						
primary school (edu)	0.157**	0.114	0.156**	0.170***	0.161**	0.171***
	(0.061)	(0.073)	(0.062)	(0.061)	(0.074)	(0.061)
Camp			-0.431***			-0.451***
			(0.056)			(0.048)
Camp * edu			-0.013			0.016
			(0.095)			(0.096)
weight for height						
primary school (edu)	0.071	0.118	0.065	0.068	0.101	0.061
	(0.064)	(0.074)	(0.064)	(0.064)	(0.074)	(0.064)
Camp			-0.212***			-0.196***
			(0.055)			(0.046)
Camp * edu			0.070			0.062
			(0.097)			(0.097)
weight for age						
primary school (edu)	0.142***	0.154**	0.138***	0.147***	0.170***	0.144***
	(0.052)	(0.064)	(0.052)	(0.052)	(0.064)	(0.052)
Camp			-0.387***			-0.388***
			(0.048)			(0.040)
Camp * edu			0.044			0.055
			(0.082)			(0.082)
mid-upper arm circumference						
primary school (edu)	0.096*	0.069	0.066	0.077	0.122**	0.074
	(0.054)	(0.061)	(0.054)	(0.054)	(0.061)	(0.054)
Camp			-0.336***			-0.252***
			(0.046)			(0.038)
Camp * edu			0.054			0.068
			(0.081)			(0.081)
N	1602	1720	3322	1602	1720	3322
Population	Not Camp	Camp	all	Not Camp	Camp	all
FE	Yes	Yes	Yes	No	No	No

*p<.10, ** p<.05, *** p<.01; standard errors clustered at the household level in parentheses. Controls include child gender, gender interacted with age, an order three polynomial for the child's age and an order three polynomial for caretaker's age. Region fixed effects included when indicated.

Table 5: Double difference strategy

	Stunting	Wasting	Underweight	Muac<-2
more6m	-0.040	-0.137	-0.014	-0.193
	(0.128)	(0.137)	(0.126)	(0.136)
$\mathbb{1}_{(t=2012)}$	-0.038	-0.159	-0.033	-0.315
	(0.179)	(0.197)	(0.177)	(0.208)
more6m # t	-0.164	0.212	-0.112	0.200
	(0.196)	(0.214)	(0.193)	(0.225)
edu	-0.413	-0.291	-0.486	0.113
	(0.310)	(0.359)	(0.320)	(0.306)
more6m # edu	0.410	0.124	0.359	-0.311
	(0.338)	(0.388)	(0.346)	(0.344)
$\mathbb{1}_{(t=2012)}$ # edu	0.635	0.448	0.814*	0.023
	(0.452)	(0.515)	(0.462)	(0.487)
more6m # $\mathbb{1}_{(t=2012)}$ # edu	-0.863*	-0.483	-0.973*	0.085
	(0.501)	(0.559)	(0.506)	(0.540)
N	1720	1720	1720	1720
Total effect edu (p-value)	0.157	0.194	0.069	0.589
FE	Yes	Yes	Yes	Yes

*p<.10, ** p<.05, *** p<.01; standard errors clustered at the household level in parentheses. Controls include child gender, gender interacted with age, an order three polynomial for the child's age and an order three polynomial for caretaker's age. Region fixed effects included.

Table 6: Double difference strategy

	(1) height for age	(2) weight for height	(3) weight for age	(4) muac
more6m	0.048	0.073	0.084	0.038
	(0.122)	(0.110)	(0.096)	(0.092)
$\mathbb{1}_{(t=2012)}$	-0.144	0.127	0.017	0.099
	(0.165)	(0.169)	(0.137)	(0.131)
more6m # t	0.253	-0.194	-0.008	-0.030
	(0.178)	(0.182)	(0.148)	(0.140)
edu	0.239	0.113	0.230	0.309
	(0.282)	(0.265)	(0.239)	(0.273)
more6m # edu	-0.202	0.053	-0.092	-0.220
	(0.304)	(0.287)	(0.257)	(0.287)
$\mathbb{1}_{(t=2012)}$ # edu	-0.329	-0.604	-0.639*	-0.483
	(0.406)	(0.374)	(0.328)	(0.350)
more6m # $\mathbb{1}_{(t=2012)}$ # edu	0.492	0.598	0.741**	0.425
	(0.435)	(0.406)	(0.355)	(0.372)
FE	Yes	Yes	Yes	Yes
N	1720	1720	1720	1720
Total effect edu (p-value)	0.050	0.165	0.014	0.728
FE	Yes	Yes	Yes	Yes

*p<.10, ** p<.05, *** p<.01; standard errors clustered at the household level in parentheses. Controls include child gender, gender interacted with age, an order three polynomial for the child's age and an order three polynomial for caretaker's age. Region fixed effects included.

Appendix

Table 7: Double difference strategy controlling for the caregiver's health

	(1) Stunting	(2) Wasting	(3) Underweight	(4) Muac<-2
more6m	-0.046	-0.145	-0.026	-0.206
	(0.128)	(0.137)	(0.126)	(0.136)
$\mathbb{1}_{(t=2012)}$	-0.040	-0.144	-0.028	-0.297
	(0.179)	(0.197)	(0.177)	(0.208)
more6m# $\mathbb{1}_{(t=2012)}$	-0.160	0.202	-0.109	0.189
	(0.196)	(0.214)	(0.193)	(0.226)
primary school (edu)	-0.425	-0.317	-0.504	0.075
	(0.309)	(0.361)	(0.322)	(0.306)
more6m # edu	0.424	0.151	0.383	-0.275
	(0.338)	(0.389)	(0.348)	(0.345)
$\mathbb{1}_{(t=2012)}$ # edu	0.652	0.482	0.834*	0.071
	(0.452)	(0.517)	(0.463)	(0.489)
more6m# $\mathbb{1}_{(t=2012)}$ # edu	-0.880*	-0.511	-0.996**	0.047
	(0.501)	(0.560)	(0.507)	(0.542)
N	1720	1720	1720	1720
Total effect edu (p-value)	0.161	0.213	0.070	0.625
FE	Yes	Yes	Yes	Yes

*p<.10, ** p<.05, *** p<.01; standard errors clustered at the household level in parentheses. Controls include child gender, gender interacted with age, an order three polynomial for the child's age and an order three polynomial for caretaker's age. Region fixed effects included. A polynomial of order three is introduced to account for the caretaker's mid-upper arm circumference.

Table 8: Double difference strategy controlling for the caregiver's health

	(1) height for age	(2) weight for height	(3) weight for age	(4) muac
more6m	0.047	0.088	0.093	0.048
	(0.122)	(0.110)	(0.096)	(0.092)
$\mathbb{1}_{(t=2012)}$	-0.147	0.121	0.011	0.094
	(0.165)	(0.169)	(0.137)	(0.130)
more6m # $\mathbb{1}_{(t=2012)}$	0.255	-0.195	-0.007	-0.028
	(0.178)	(0.181)	(0.148)	(0.140)
edu	0.238	0.147	0.252	0.338
	(0.283)	(0.266)	(0.240)	(0.275)
more6m # edu	-0.201	0.017	-0.116	-0.247
	(0.305)	(0.287)	(0.258)	(0.289)
$\mathbb{1}_{(t=2012)}$ # edu	-0.326	-0.648*	-0.668**	-0.522
	(0.407)	(0.377)	(0.330)	(0.353)
more6m # $\mathbb{1}_{(t=2012)}$ # edu	0.490	0.637	0.766**	0.456
	(0.436)	(0.408)	(0.357)	(0.375)
N	1720	1720	1720	1720
Total effect edu (p-value)	0.049	0.185	0.016	0.788
FE	Yes	Yes	Yes	Yes

*p<.10, ** p<.05, *** p<.01; standard errors clustered at the household level in parentheses. Controls include child gender, gender interacted with age, an order three polynomial for the child's age and an order three polynomial for caretaker's age. Region fixed effects included. A polynomial of order three is introduced to account for the caretaker's mid-upper arm circumference.