Long Term Consequences Of Early Childhood Malnutrition

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Abstract

This paper explores the long-term consequences of shocks on children's health and education using longitudinal data from rural Zimbabwe. We link exposure to the war preceding Zimbabwe's independence and the 1982-84 drought to the health status of the children in our sample (as measured by their height-for-age in 1983, 1984 and 1987) and to their health and educational attainments as adolescents measured in 2000. Instrumental variables- maternal fixed effects estimates show a statistically significant relationship between height-for-age in children under 5 and height attained as a young adult, the number of grades of schooling completed and the age at which the child starts school. Exposure to the 1982-84 drought resulted in a loss of stature of 2.3 centimeters, 0.4 grades of schooling, and a delay in starting school of 3.7 months. Had the median pre-school child in this sample had the stature of a median child in a developed country, by adolescence, she would be 4.6 centimeters taller, had completed an additional 0.7 grades of schooling and would have started school seven months earlier. We present calculations that suggest that this loss of stature, schooling and potential work experience results in a loss of lifetime earnings of 7 – 12 percent and that such estimates are likely to be *lower* bounds of the true losses.

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1. Introduction

Individuals in both developing and developed countries are subject to exogenous shocks. When such events generate variations in consumption – as in cases where households are unable to fully insure against such shocks - they lead to losses of utility. The significance of such losses, from a policy point of view, depends partly on whether such shocks induce path dependence. In the UK context, for example, Gregg (2001) argues that unemployment spells incurred by young men as a result of local labour market shocks raise susceptibility to future unemployment, a phenomenon described as scarring. Where temporary shocks have such long-lasting impacts, utility losses may be much higher. Assessing the impact of such shocks, however, is problematic for two reasons. First, unobservable characteristics correlated with the likelihood of exposure to such initial shocks could account for phenomenon such as scarring. Second, households or individuals might respond to such shocks in ways that mitigate or exacerbate their initial effects.

This paper explores the long-term consequences of shocks but in a different context. Following Foster (1995) and Hoddinott and Kinsey (2001), we link the literature on shocks to the literature on the determinants of pre-schooler health status. We extend their analyses by linking transitory shocks experienced prior to age three to pre-school nutritional status, as measured by height given age, to subsequent health and education attainments. Specifically, representations of civil war and drought "shocks" for a sample of children living in rural Zimbabwe are used to identify differences in preschool height-for-age across siblings. Instrumental variables- maternal fixed effects (IV-MFE) estimates show that improvements in height-for-age in children under 5 are associated with increased height as a young adult, a greater number of grades of schooling completed and an earlier age at which the child starts school. The magnitudes of these statistically significant effects are functionally significant as well. Had the median pre-school child in this sample had the stature of a median child in a developed country, by adolescence, she would be 4.6 centimeters taller, had completed an additional 0.7 grades of schooling and would have started school seven months earlier. We present calculations that suggest that this loss of stature, schooling and potential work experience results in a loss of lifetime earnings of 7 – 12 percent and that such estimates are likely to be *lower* bounds of the true losses.

As such, the paper speaks to several audiences. First, it contributes to the literature on shocks and consumption smoothing, but unlike much of the literature this paper looks at the impact at the individual rather than household level.[1] Second, it extends the literature on the determinants of human capital formation. There are numerous cross-sectional studies that document associations between pre-school nutritional status and subsequent human capital attainments, see Pollitt (1990), Leslie and Jamison (1990), Behrman (1996) and Grantham-McGregor et al., (1999; especially pp. 65-70). However, as Behrman (1996) notes, many of these studies document *associations* between pre-school malnutrition and subsequent attainments, not *causal* relationships. Preschooler health and subsequent educational attainments *both* reflect household decisions regarding investments in children's human capital. Having reviewed these studies, Behrman (1996, p. 24) writes, "Because associations in cross-sectional data

[1] Morduch (1995, 1999) and Townsend (1995) review the literature on shocks and consumption smoothing at the household level.

may substantially over- or understate true causal effects, however, much less is known about the subject than is presumed."

Third, the United Nations estimates that one out of every three preschoolers in developing countries - 180 million children under the age of 5 - exhibit at least one manifestation of malnutrition, stunting (United Nations ACC/SCN, 2000).[2] Because improving preschooler health and nutrition are seen to be important development objectives in their own right, many international organizations, including the Department for International Development (DfID) and the World Bank are prioritizing improvements in child health and nutrition, see DfID (2002) and World Bank (2002). These organizations also emphasize increasing schooling attainments, with the British government (along with others) committing itself to the International Development Targets of Universal Primary Education by 2015. An implication of our results is that improvements in pre-school health status and primary education are not competing objectives; rather improved pre-school nutrition will facilitate meeting the education objectives. Further, if improving pre-school nutritional status enhances the acquisition of knowledge at school, and leads to higher attained heights as adults, these improvements have added value where there exist positive associations between schooling and productivity, and height and productivity.[3]

[2] That is their heights given their ages are two standard deviations below international norms.

[3] Fogel (1994) documents that lower attained adult height is associated with increased risk of premature mortality. Behrman and Deolalikar (1989), Deolalikar (1988), Haddad and Bouis (1991), Strauss (1986) and Thomas and Strauss (1997) document positive associations between height and productivity. There are hundreds of studies on the impact of grades of schooling completed on wages, many of which are surveyed in Psacharopoulos (1994) and Rosenzweig (1995). If nutrition affects years of schooling there would be additional consequences of child malnutrition. In addition as reported by Glewwe and Jacoby (1995) – malnutrition may lead to delays in starting school resulting in a delay in entering the labor market (unless the child leaves school early). Such a delay will lead to a loss in life-time earnings. Recent evidence on returns to schooling and experience in the labor market for the manufacturing sector in Zimbabwe is found in Bigsten *et al.* (2000).

Lastly, while our focus lies in the realm of shocks and human capital attainments in developing countries, it is worth noting that our methods are applicable to a much wider class of problems. Specifically, we argue that studies that convincingly link initial states, whether they be health (as in this study) or other states including poverty or unemployment, to subsequent outcomes such as human capital formation (as in this study), or future poverty or future unemployment or earnings, must meet three criteria. First, they must control for unobservable heterogeneity. Second, such controls require the use of longitudinal data. But such data are not without their own dangers, most notably the potentially confounding effects of attrition bias. Third, they must take into account the fact that these initial states are also outcomes and, as such, are endogenous.

The paper begins by outlining why these criteria are important. After describing the data available to us, we consider attrition bias in our sample and the endogeneity of pre-schooler health. Having satisfactorily addressed both concerns, we present the paper's core empirical findings. The final section concludes.

2. The Econometric Problem

Given the specific focus of this paper, we illustrate the basic econometric problem as follows. We divide a child's life into two periods.[4] Period 1 is the period of investment in the child, say the preschool years while period 2 is the time of an individual's maturation. We presume that the measure of a child's nutritional status, height-for-age in period 1 (H_{1k}), reflects parental decisions on investing in his or her health can be denoted as a

[4] This discussion draws heavily on work by Glewwe, Jacoby and King (2001) and Alderman, Behrman, Lavy and Menon (2001).

function of a vector of observable prices and household characteristics (Z_{1k}) that determine the level and efficacy of investments in health.

$$H_{1k} = a'_Z Z_{1k} + ?_{1k} \quad (1)$$

where $?_{1k} = e_H + e_k + e_1$

is a disturbance term with three components: e_H , representing the time invariant home environment and is common to all children in the household (this would capture, for example, parents' tastes and discount rates as well as their ability); e_k , which captures time invariant child specific effects such as genetic potential; and e_1, a white noise disturbance term. The particular specification is stated in terms of a reduced form rather than as a production function, though the key features regarding inter-temporal correlations of errors holds in either approach.

A linear achievement function for attainment in the second period is given by:

$$A_{2k} = a_H H_{1k} + a'_Z Z_{2k} + ?_{2k} \quad (2)$$

where $?_{2k} = ?_H + ?_k + ?_2$

and A_{2k} is, say, the educational attainment of child k (realized in period 2), Z_{2k} is a vector of other prices and assets that influence academic performance – possibly, but not necessarily, with elements common to Z_{1k}. Like $?_{1k}$, $?_{2k}$ is a disturbance term with three components: $?_H$, representing aspects of the home environment which influence schooling and are common to all children in the household (this would capture, for example, parents' attitude towards schooling); $?_k$, which captures child specific effects such as innate ability and motivation that are not controlled by parents; and $?_2$, a white noise disturbance term. The basic difficulty with a least squares regression of (2), as noted by Behrman (1996) is the likelihood that $E(H_{1k}?_{2k})$? 0 because of possible correlation between H_{1k} and $?_H$ or between H_{1k} and $?_k$ mediated through either the correlation of household effects or individual effects or both. That is, either E($e_H?_H$)?0 or E($e_k?_k$) ? 0.

Such correlations could arise for several reasons. For example, a child with high genetic growth potential will be, relative to her peers, taller in both periods 1 and 2. Conversely, children with innately poor health may be more likely to die between periods 1 and 2, leaving a selected sample of individuals with, on average, better genetic growth potential. Parents observing outcomes in period 1 may respond in a variety of ways. For example, faced with a short child in period 1, parents might subsequently allocate more food and other health resources to that child, or perhaps encourage greater school effort on the presumption that the child is unlikely to be successful in the manual labour as an adult. In any of these cases, estimates of a_H using ordinary least squares will be biased. Further, as Glewwe, Jacoby and King (2001) note, household or maternal level fixed effects estimation (also described as a siblings difference model) of (2), while purging the correlation between H_{1k} and $?_H$, would leave unresolved the correlation between H_{1k} and $?_k$. They argue that an "ironclad" estimation strategy involves combining maternal fixed effects estimation with instrumental variables to sweep out this remaining correlation. This requires a longitudinal data set of siblings that also contains information on a shock (price or income) that was: "(i) of sufficient magnitude and persistence to affect a child's stature; (ii) sufficiently variable across households; and (iii) sufficiently transitory *not* to affect the sibling's stature", a condition they describe as "nothing short of miraculous" (Glewwe, Jacoby and King, 2001, p. 350).

Only a handful of studies control for the fact that both nutritional and educational attainment reflect the same household allocation decisions when examining the link between child health and school performance, though all have limitations when measured up to the stringent identification criteria listed by Glewwe, Jacoby and King, (2001).

Behrman and Lavy (1998) and Glewwe and Jacoby (1995) use cross-sectional data from the 1988-89 Ghanaian Living Standard Measurement Study to examine the relationship between *current* nutritional status and *current* cognitive achievement and the likelihood of delayed primary school enrollment respectively. Both find that the impact of child health on schooling is highly sensitive to the underlying behavioral assumptions and the nature of unobserved variables. Although both studies are carefully carried out, their reliance on a single cross-sectional survey is limiting. The authors cannot utilize direct measurement of pre-school child health. Similarly, they do not have instruments that unambiguously identify factors that might have affected pre-school outcomes yet do not determine schooling attainments themselves.

Glewwe, Jacoby and King (2001) use the Cebu Longitudinal Health and Nutrition Survey, finding that malnourished children enter school later and perform relatively poorly on tests of cognitive achievement.[5] They examine relations between *preschool* nutritional status and subsequent educational attainments using a siblings difference model to control for fixed locality, household, and maternal characteristics and use height-for-age of the older sibling to instrument for differences in siblings' nutritional status. However, data limitations force them to assume that growth in children's height

[5] A related study by Glewwe and King (2001) use these data to indicate the relation of nutrition to IQ.

after age two is not correlated with height up to the age of two and that pre- or postnatal health shocks do not affect both the physical or mental development of a child.

Alderman, Behrman, Lavy, and Menon (2001) use a data set that meets many of these requirements described above. They use information on current prices at the time of measurement as the instrument or “shock” variable for pre-school height-for-age. By interacting these with levels of parental education, they induce variability in these shocks at the household level. They find “fairly substantial effects of preschool nutrition on school enrollments” (p. 26). However, they cannot determine whether pre-school nutritional status affects ultimate schooling and health attainments nor do they use household fixed effects.

3. Data

Given our interest in estimating equation (2) in the specific context of an exploration of the long term consequences of early childhood malnutrition and the problems associated with such estimation, data requirements are high. First, we need data on children’s nutritional status as pre-schoolers.[6] Second, we need data on the nutritional status of their siblings as pre-schoolers. Third, we need to identify shocks that meet the criteria described above. Fourth, we need data on children and their siblings as young adults that are free of attrition bias.

[6] Pre-school data are needed because children are at most risk of malnutrition in the early years of life, particularly ages one to three. In this period, children are no longer exclusively breastfeed, they have high nutritional requirements because they are growing quickly, and they are susceptible to infection because their immature immune systems fail to protect them adequately (Martorell, 1997). From age three onwards into the school period, there is evidence that even children from very poor countries will grow as quickly as children in industrialized countries such as the United States or Britain, neither catching-up nor falling further behind (Martorell, 1995, 1999). Thus, the manifestation of nutritional shocks occur years, if not decades, before investments on human capital are completed.

a) The sample

Our data are drawn from longitudinal surveys of households and children residing in three resettlement areas of rural Zimbabwe. In 1982 one of the authors (Kinsey) constructed an initial sampling frame consisting of all resettlement schemes established in Zimbabwe's three agriculturally most important agro-climatic zones in the first two years of the program. One scheme was selected randomly from each zone (Mupfurudzi, Sengezi and Mutanda), random sampling was then used to select villages within schemes, and in each selected village, an attempt was made to cover all selected households. Approximately 400 households, located in 20 different villages, were subsequently interviewed over the period July 1983 to March 1984. They were re-interviewed in the first quarter of 1987 and annually, during January to April, from 1992 to 2001. At the household level, there is remarkably little sample attrition; approximately 90% of households interviewed in 1983/84 were still being re-interviewed in the late 1990s. In the 1983/84 and 1987 rounds, valid measurements[8] on heights and weights for 680 children, who were offspring of the household head and aged 6 months to 6 years were obtained.

The fact the initial surveys, 1983/84 and 1987, were spread out over time is advantageous as it leads to a wide range of birth dates, from September 1978 to September 1986. This was a tumultuous period in Zimbabwe's history. Children born in the late 1970s entered the world during a vicious civil war. Nearly half the sample was

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[7] There is no systematic pattern to the few households that drop out. Some were inadvertently dropped during the re-surveys. A small number were evicted by government officials responsible for overseeing these schemes, and a few disintegrated, such as those where all adults died.
[8] We exclude six children that had probable errors in either height or age data resulting in a height-for-age z score that was less than -6 or greater than 6.

born into families that, during this period, were housed in what were euphemistically described as "protected villages". In areas where conflict was most intense, residents were forced to abandon their homesteads and move to these hastily constructed villages with no amenities and restrictions on physical movement. More than 90 per cent of all households reported some adverse affect of the civil war. By mid-1980, with the transition to majority rule complete and starting in 1981, households in our sample began the process of resettlement with this process continuing intermittently until 1983. However, almost immediately after acquiring access to considerably larger land holdings than they had enjoyed in the pre-Independence period, they were affected by two back-to-back droughts, in 1982/83 and 1983/84. Circumstances began to improve substantially in the years that followed with better rainfall levels and improvements in service provision, such as credit, extension and health facilities. We argue below that these shocks – the war and the drought – are plausible instruments for initial nutritional status.

In February and March 2000, we implemented a survey designed to trace the children measured in 1983/84 and 1987. Of the original sample, 15 children died (2.2% of the original sample), leaving 665 "traceable" children. The survey protocol involved visiting the natal homes of the children measured in 1983/84 and 1987. We encountered no refusals to participate in the survey. There were a few cases where the child was not resident, but lived nearby and was traced to their current residence. In the remaining cases, the parent – often in consultation with other household members – was asked questions regarding the child's educational attainments.

Table 1 provides summary statistics. Table 1a shows that, on average, these children have poor height-for-age relative to a well-nourished reference population.

Roughly 1 in 4 children are stunted. Table 1b provides information on three attainments – current height, number of grades completed and the child's age when starting school. Zimbabwe's school system consists of seven grades of primary schooling, followed by 4 forms of lower secondary schooling. The vast majority of students attending secondary school do not continue after sitting examinations at the end of their Fourth form. The number of completed grades is the number of primary plus secondary grades completed as of February 2000.[9] Age started school is the difference between the date the child started school and his or her birth date. Table 1c contrasts the attainments of two groups of children, those who were stunted as pre-schoolers and those who were not. Children who were stunted as pre-schoolers were shorter, had completed fewer grades of schooling and had started school later. While these differences, which are statistically significant, are suggestive of associations between pre-school nutritional status and subsequent human capital formation, for reasons already described, they cannot be regarded as definitive.

b) Potential selectivity biases caused by attrition

As already noted, any study using longitudinal data needs to take seriously the possibility that estimates may be biased because of selective sample attrition. In these survey data, such biases emanate from two sources. One relates to the fact that 15 children measured in 1983/84 or 1987 died prior to 2000. If these children were particularly unhealthy, then our estimates based on surviving children will be biased.[10] Our sense is that these biases

[9] The very few (8 individuals, or 1% of the sample) who had continued in school beyond the fourth form were coded as having completed 12 grades of school.
[10] A t test does not reject the null hypothesis that initial height-for-age z scores are equal for deceased and currently living children.

may not be as severe as one might perceive and that, in practical terms, it is unlikely that much could be done about them. Drawing on the on-going household survey, we examined the causes of death of these children. Our impression is that a variety of causal factors are at work, including such unfortunate instances such as road accidents. There are, fortunately, too few deaths to determine whether there is a systematic pattern. Further, the very few studies that take this into account, such as Pitt and Rosenzweig (1989) find that even in much higher mortality populations (compared to this sample), the impact of mortality selection is minimal. For these reasons, although biases resulting from selective mortality cannot be definitively ruled out, they are not addressed further. The results can be interpreted as the impact of malnutrition on the education and attained heights of survivors. That is, the study looks as the *additional* costs of malnutrition over any contribution to mortality risk.

The second source of potential attrition bias may stem from the fact that it was not always possible to physically trace a child and, thus, information on heights could not be obtained for some children. This differs from the information on attainments - completed grades and age starting school. This data were available for 98.6 per cent of all children. Table 1b indicates that the availability of height data is largely conditioned by whether the child was currently resident in the household. The four most common reasons for out-migration - collectively accounting for about 93 per cent of all case - were marriage, looking for work, attending school and moving “to live with other relatives”.

Will this attrition bias our results? Note that the results reported here are based on maternal level fixed effects estimates. Any attrition resulting from maternal, household, or locality characteristics is thus swept out by differencing across siblings (Ziliak and

Kniesner, 1998). However, since attrition may affect sibling pairs we also look at indicators of selective sample loss. The findings presented in Table 2a compare the unconditional mean values of three child characteristics – initial height-for-age, age and sex – between sub-samples where height as an adult was collected and was not obtained. The comparison for attrition on height indicates that we were more likely to measure boys' heights, the heights of younger children and individuals with poorer initial height-for-age. All differences in means are statistically significant.

Following the methods set out in Fitzgerald, Gottschalk and Moffitt (1998) and Alderman et al. (2001), we estimated a probit to determine whether there was attrition based on observable variables. This is reported in Table 2b. The dependent variable equals 1 if the attainment (height or examination score) is observed in 2000, 0 otherwise. In specification (1), only initial height-for-age (that is, in the language of Fitzgerald et al, the lagged outcome variable) is included as a regressor. In specification (2), child characteristics (age and sex), maternal education and dummy variables denoting locality were also included. With the addition of these controls, however, there is no longer a statistically significant relationship between initial height-for-age and subsequent measurement of these attainments.[11] Especially notable is that, relative to the omitted resettlement scheme, Mutanda, a child was more likely to be measured if he or she originated from either Mupfurudzi or Sengezi, the marginal effects being 25 and 10 per cent respectively. Mutanda has the worst agricultural potential of these three areas. Further, amongst non-resident children originally from Mutanda, just over 27 per cent had out-migrated because they had married, as opposed to 14 and 5 per cent in Mupfurudzi and Sengezi respectively. This suggests that the poorer agro-climatic

[11] Adding additional maternal or paternal characteristics does not change this finding.

conditions in Mutanda reduces the likelihood of finding the child in the 2000 follow-up survey because female children are more likely to marry earlier and leave the parental household.

Following Becketti, Gould, Lillard and Welch (1988), we also estimated the determinants of initial height-for-age z score separately for children traced, and not traced, in the follow up survey. We do not reject a null hypothesis that these determinants differ across these two sub-samples, providing further support to the claim that attrition bias will not affect these results.

To conclude, we surmise that biases resulting from selective mortality are likely to be minimal, though we cannot completely rule these out. Conditional on this, we note that we information for virtually the entire sample of grade completion and age at which schooling commenced. The impact of attrition resulting from maternal, household, or locality characteristics will be swept out by differencing across siblings. Lastly, although an initial comparison of means suggests that there is some selection bias associated with obtaining data on attained heights, this disappears when we condition on a number of fixed characteristics, notably location.

4. Findings

a) Estimation strategy

We estimate equation (2) with three measures of attainments: height (measured in centimeters), number of grades attained, and child's age (in years) when she started school using a maternal fixed effects – "siblings difference"- instrumental variables (MFE-IV) estimator. Sibling differences sweeps out any correlation between H_{1k} and $?_H$.

Instrumental variables addresses the potential correlation between H_{1k} and ?$_k$. Addressing this requires that we find instruments that affect H_{1k}, vary across children within the same household, and are sufficiently transitory *not* to affect A_{2k}. That is, we will need some elements of Z_{1k} that are not contained in Z_{2k}.

We identify two shocks: the negative shock resulting from the war period; and the negative shock resulting from the 1982-84 drought. Both are plausibly linked to differences in siblings' height-for-age yet are unlikely to have persistent effects on outcomes observed subsequently. Note that we do not argue that either of these two shocks only affect nutrition – clearly war, even war in distant parts of the country, affects the economy as a whole. However, we argue (and provide some supportive evidence below) that the impact of these shocks affect the relative long term differences in siblings mainly through the impact on short term nutritional vulnerability. There is a large literature, surveyed in Hoddinott and Kinsey (2001), emphasizing that such shocks have their largest effects on children younger than 36 months.[12] Consequently, we construct two "child-specific" shock variables. The first is the log of number of days child was living prior to 18 August 1980. This captures all the "shocks" associated with the war and the immediate post-Independence period. The second is a "1982-84 drought shock" dummy variable. Recall that this drought was spread out over a two-year period (and that many of the households in the sample had only just been resettled prior to the shock) and that the first survey was spread out between July 1983 and March 1984. Hoddinott and Kinsey (2001) demonstrate that in rural Zimbabwe, the age range of 12-24 months is the one where drought shocks seem to have their greatest impact on kids' height-for-age.

[12] Also, see Jensen (2000).

Hence, this variable takes on a value of 1 if: the child was observed in 1983 and was between 12 and 24 months; or was observed in 1984 and was between 12 and 36 months; and equals 0 otherwise.

b) Instrument validity

Table 3 presents estimates for initial height-for-age z score[13] using maternal fixed effects. The two shock variables are correctly signed – greater exposure to civil war affects child height adversely as does drought – and are significant at standard levels. Bound, Jaeger and Baker (1995) make the important argument that using instruments with low correlation with the endogenous variable can result in two stage least squares parameter estimates with significant levels of bias relative to that obtained via simple ordinary least squares. For example, they show that with 10 instruments and an F statistic of 1, IV estimates would still have almost half the bias of an OLS estimate. The F statistic on these shock variables is 7.55; with this value, Bound, Jaeger and Baker (1995) show that the bias of our IV results will be less than 2% of simple OLS estimates.

Before continuing, however, it is important to consider a possible objection to the use of these drought and war shocks as instruments. Implicitly, we are assuming that these shocks only operate through their impact on pre-school nutritional status. But the ending of the war could have involved school/road reconstruction as well as safe travel so thus improving the accessibility of schools for the younger sib relative to the older sib. Arguably, wars leave psychological scars, induce geographical dislocations and it might hard to believe that such effects only work through initial heights.

[13] The z score is calculated by standardizing a child's height given age and sex against an international standard of well nourished children. A z score of -1 indicates that given age and sex, the child's height is one standard deviation below the median child in that age/sex group.

For these reasons, we construct an over-identification test as outlined in Wooldridge (2002, p. 123). We estimate our IV-MFE model for each attainment and extract the residuals. After estimating maternal fixed effects regressions where the dependent variables are these residuals and the regressors are all exogenous variables, we calculate the chi-squared over-identification test statistic as the sample size multiplied by the R^2 calculated for these regressions. We do not reject the null hypothesis that these instruments are uncorrelated with our three outcome variables at the 90 per cent confidence level implying that the identifying variables have no influence on the outcome variables except via the measure of nutritional status. As such our instruments meet the criteria described earlier. They are (i) of sufficient magnitude and persistence to affect a child's stature, H_{1k}; (ii) sufficiently variable across children; (iii) sufficiently transitory *not* to affect the sibling's stature; and (iv) sufficiently transitory not to affect subsequent attainments, A_{2k}.

c) The impact of pre-schooler height for age on adolescent attainments

Having satisfied ourselves that our instruments are valid, we turn to Table 4 which reports the results of estimating the impact of pre-schooler height for age on the number of grades attained, age starting school and adolescent height.[14] Three estimates are reported for each dependent variable: a "naïve" least squares estimate, with controls for child (age and sex) and maternal characteristics (age, education and height); a maternal fixed effects estimate which, as noted above, eliminates correlation between H_{1k} and e_H;[15]

[14] Full results are available on request.
[15] Rosenzweig and Wolpin (1988) argue that assessment of impact can be hampered by considerations of selective migration. There are strong *a priori* grounds for believing that this will not affect these results. Land access, not human capital considerations, was the driving force behind resettlement for these

and an instrumental variables – maternal fixed effects estimate, using the "shock" variables described above as instruments, to eliminate both the correlation between H_{1k} and e_H and between H_{1k} and e_k.

The first row of Table 4 indicates that increased height-for-age is associated with a greater number of grades attained, irrespective of the estimator used. The magnitude of this impact is affected by whether we control for correlation between H_{1k} and e_k. The IV-MFE estimates provide a larger estimate of impact, the difference between this parameter estimate and the MFE results being significant at the 13% confidence level.

Improvements in pre-school heights result in children starting school slightly younger, somewhere between two to seven months younger depending on the estimator. Again, the IV-MFE estimates produce the largest measure of impact, though in this case the parameter is only significant at the 10% level. Note that as a specification check, we included child's month of birth – a plausible determinant of the age of school initiation - as either an instrument for height-for-age or as an additional regressor but doing so did not alter this result.

Better pre-school nutritional status is associated with greater attained height in adolescence.[16] The "naïve" least squares results and the maternal fixed effects estimates

households. Even if one does not accept this view, then noting that concerns regarding selective migration embodies concerns regarding correlations between regressors and fixed, unobservable locality characteristics. An attraction of the MFE and IV-MFE estimators is that the impact of such characteristics is differenced out.

[16] A possible concern with these results is that the sample includes young adolescents who are continuing to grow. Note, however, that by construction, there is no child in the sample younger than 13 ½ years. Past this age, at least in well-nourished populations, there is little growth in a girl of median stature and about 10 cm of growth in a boy of median stature (see Hamill et al, 1979). We also estimated the model using number of years over 14 and number of years over 14 interacted with being a boy. This does not affect our results on the impact of height-for-age. We also attempted to estimate models using only older children. Doing so is somewhat problematic because as we eliminate younger individuals, the sample size available for maternal fixed effects estimates begins to shrink rapidly. Estimating a maternal fixed effects model, but restricting the sample to children aged over 15 or over 16 produces identical results. We do not have enough pairs of siblings to estimate the model with any further age restrictions.

are roughly comparable in magnitude and statistically significant. Controlling for the endogeneity of initial height-for-age, the impact of pre-school nutritional status increases markedly. The equality of parameters between the MFE and IV-MFE results can be rejected at the 10% level.[17]

The magnitudes of these impacts are meaningful. The mean initial height-for-age z score is –1.25. If this population had the nutritional status of well-nourished children, the median z score would be 0. Applying the IV-MFE parameter estimates reported in Table 4, this would result in an additional 4.6 centimeters of height in adolescence, an additional 0.7 grades of schooling and starting school seven months earlier. In terms of exposure to the 1982-84 drought, recall that this shock reduced height-for-age z scores by 0.63. Using the IV-MFE estimates, this implies that this transitory shock resulted in a loss of stature of 2.3 centimeters, 0.4 grades of schooling, and a delay in starting school of 3.7 months.

We can also explore the magnitudes of these impacts in terms of lost future earnings. Using the values for the returns to education and age/job experience in the Zimbabwean manufacturing sector provided by Bigsten *et al.* (2000, Table 5), the loss of 0.7 grades of schooling and the 7 month delay in starting school translates into a 12 per cent reduction in lifetime earnings. The impact of the shocks, as described above, translates into a 7 per cent loss in lifetime earnings. Such estimates are likely to be lower bounds. Fogel (1994) presents evidence that links short stature amongst males to the early onset of chronic diseases and to premature mortality. Although comparable evidence

[17] Note that these estimates do not control for birth order. Horton (1988) provides a detailed exploration of the impact of birth order on nutritional status. She finds that higher-order children are more likely to have poorer nutritional status because of competition with siblings for resources, maternal depletion and a greater likelihood of infection. For this reason, our results cannot be attributed to a birth order effect - by construction, children observed in 1983/84 are of *lower* birth order.

from developing countries does not yet exist, Fogel's evidence is consistent with a view that shorter adult stature reduces lifetime earnings either by reducing life expectancy – and thus the number of years that can be worked - or by reductions in physical productivity brought about by the early onset of chronic diseases. Further, these estimates neglect other long-term consequences of these shocks. For example, taller and better educated women experience fewer complications during child birth, typically have children with higher birthweights and experience lower risks of child and maternal mortality (World Bank, 1993).

In addition to determining the long-term consequences of these initial shocks, we also explored whether there was any attenuation or reduction of their impacts over time. Specifically, we regressed growth in stature (i.e. the change in height between 1983/84 or 1987 and 2000) against initial height – instrumented using the civil war and drought shock variables described above – as well as child sex, initial age (in years), duration of observation (in years) and the interaction of these latter two terms.[18] These control variables absorb the marginal impact of changes in height associated with age (there is less growth as children get older), sex (boys have higher growth potential than girls) and duration of observation (more growth is expected with a longer period of observation). A coefficient on initial height that is not significantly different from zero would indicate that growth subsequent to initially measured height was independent of that initial height. This would imply no reduction in the impact of the initial shocks over time. A coefficient on initial height not significantly different from negative one would indicate that these shocks have no long term effect in the sense that attained height as an adolescent (H_1)

[18] Altering this specification, for example by adding a quadratic term for duration of observation, or interacting duration of observation with child sex does not substantively alter the coefficient on initial height.

was independent of initial height (H_0). That is, since $[H_1-H_0] = ? + ?H_0 + ?X$, when ? = -1 we are left with $H_1 = ? + ?X$. This is sometimes described as "complete catch-up", see Hoddinott and Kinsey (2001) for a further discussion.

As before, we estimate this relationship using an instrumental variables – maternal fixed effects estimator, obtaining the following results (absolute values of t statistics in parentheses):

$$[H_1-H_0] = \underset{(2.67)}{51.67} + \underset{(2.39)}{(-0.513)} (H_0) + \underset{(4.21)}{(3.680)} (\text{boy}) + \underset{(4.06)}{(10.590)}(\text{initial age})$$

$$+ \underset{(6.87)}{(4.854)}(\text{duration of observation}) + \underset{(4.48)}{(-0.855)}(\text{age x duration of observation})$$

Our coefficient on initial height of –0.513 indicates that we can reject both the hypothesis that there are no permanent consequences from early malnutrition and also the hypothesis that there is no catch up growth at all after an early nutritional shock controlling for maternal, household and community fixed effects as well as the endogeneity of initial height.[19] Moreover, we considered the possibility that if malnourished pre-schoolers experienced a more prolonged pubertal growth spurt, as has been suggested in the nutrition literature (Martorell, Khan and Schroeder, 1994), this might be an underestimate of catch-up. However, restricting the sample to slightly older children – those aged 16 or older in 2000 – does not change the magnitude of the parameter on initial height.

[19] The interaction term implies that the older the child the less increment to growth from an additional year. The marginal effect on growth of an additional year – that is, an increase in duration – is 4.85 –0.885(initial age). This marginal growth is positive at 1.3 standard deviations above the mean of initial age for our sample.

5. Conclusions

Using longitudinal data from rural Zimbabwe, we have shown that improved pre-schooler nutritional status, as measured by height given age, is associated with increased height as a young adult, a greater number of grades of schooling completed, and an earlier age at which the child starts school. The use of IV-MFE estimates means that these results are robust to unobservable maternal fixed effects as well as correlations between initial nutritional status and child-specific unobservables. Further, we have demonstrated that the instruments used in the first stage regressions are valid. The use of the full sample with no correction for maternal fixed effects or child specific endogeneity would result in parameters that, although apparently statistically significant, are much smaller in absolute value than those in the preferred IV-MFE approach.

As noted in the introduction, it is widely recognized that improving preschooler health and nutrition are important development objectives in their own right. Having shown that improved pre-schooler nutritional status enhances the acquisition of schooling, and leads to higher attained heights as adults (and that lost growth velocity as a pre-schooler is only partially recovered subsequently), then these improvements also have instrumental value where there existed positive associations between schooling and productivity, and height and productivity. Lastly we note that the determinants of pre-school heights include 'shocks' such as war and drought and that these 'temporary' events have long lasting impacts. As such, our findings strengthen the value of "forward looking" interventions that mitigate the impacts of shocks (see Holzmann, 2001).

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Table 1a: Descriptive statistics on children surveyed in 1983/84 and 1987

	Mean	**Standard deviation**
Height-for-age z score	**-1.25**	**1.46**
% Children stunted	**27.8%**	**0.45**
Age (months)	**39.9**	**21.7**
% Children male	**49.2%**	**0.50**

Table 1b: Descriptive statistics on outcome measures of educational attainments and height on children surveyed in 2000, by residency

	Full sample	Resident children	Non-resident children
Number of children in this category	665	359	306
Height (in centimeters)	161.1	161.4	158.3
	(389)	*(359)*	*(30)*
Completed grades	8.6	8.4	8.8
	(661)	*(359)*	*(302)*
Age start school (years)	7.2	7.2	7.4
	(656)	*(359)*	*(297)*

Notes:
1. Italicized numbers in parentheses are sample sizes for each attainment.

Table 1c: Mean attainments of stunted and non-stunted children

	Malnourished children (stunted)	Non-malnourished (not stunted)
Height (in centimeters)	157.7	162.7
Completed grades	9.9	10.8
Age start school (years)	7.6	7.1

Table 2a: Testing for selective attrition, comparison of means

	Height measured	Height not measured	T statistic on difference in means
Pre-school height for age z score	-1.35	-1.11	2.11**
Age (months) at first interview	37.5	43.4	3.48**
Per cent children, male	57%	38%	5.08**

Table 2b: Testing for selective attrition using Fitzgerald, Gottschalk and Moffitt method

	Height measured in 2000	
	(1)	(2)
Initial height-for-age z score	-0.071 (1.91)*	-0.051 (1.06)
Age at first interview	-	-0.010 (2.77)**
Child is boy	-	0.614 (6.47)**
Maternal schooling, years	-	-0.025 (1.20)
Child from Mupfurudzi	-	0.787 (4.12)**
Child from Sengezi	-	0.303 (1.97)**

Notes:
1. Sample is pre-schooler children of the household head who, when measured in 1983/84 or 1987 had height-for-age z scores between –6 and +6 and were still alive as of February 2000.
2. Models in Table 2b were estimated as probits, with standard errors robust to clustered sample design.
3. Numbers in parentheses are absolute values of z statistics.
4. Sample sizes in Table 2b are: 665, column (1); 612, column (2).
5. * Significant at the 10% level; ** significant at the 5% level.

Table 3: Maternal fixed effects estimates of the impact of shocks on child height-for-age

Variable	Parameter estimate and t statistic
Exposure to Civil war	-0.048
(log of number of days child was living prior to 18 August 1980)	(3.36)**
1982-84 Drought shock	-0.631
(Child was exposed to the 1982-84 drought when aged between 12-36 months)	(3.16)**
Boy	-0.125
	(1.01)
Age	0.146
	(3.64)**
F statistic on fixed effects	2.33**
F statistic on significance of "shocks"	7.55**
Sample size	571

Notes:
1. Dependent variable is child height-for-age z score.
2. Sample is pre-schooler children of the household head who, when measured in 1983/84 or 1987 had height-for-age z scores between –6 and +6 and were still alive as of February 2000.
3. Numbers in parentheses are absolute values of t statistics.
4. * Significant at the 10% level; ** significant at the 5% level.

Table 4: The impact of pre-schooler height for age on adolescent height, grade attainment and age starting school

Dependent variable	"Naïve" least squares	Maternal fixed effects	Instrumental variables – maternal fixed effects
Completed grades	0.265	0.145	0.566
	(4.15)**	(2.61)**	(1.99)**
Age start school (years)	-0.274	-0.156	-0.489
	(6.73)**	(3.37)**	(1.66)*
Height (in centimeters)	1.679	1.153	3.649
	(8.60)**	(2.52)**	(2.34)**

Notes:
1. Sample is pre-schooler children of the household head who, when measured in 1983/84 or 1987 had height-for-age z scores between –6 and +6 and were still alive as of February 2000.
2. Controls included but not reported are child age and sex. For naïve least squares, maternal age, education and height are included as additional controls.
3. Numbers in parentheses are absolute values of z statistics (IV-MFE) and t statistics (naïve least squares).
4. Standard errors for the naïve least squares estimates are robust to clustered sample design.
5. * Significant at the 10% level; ** significant at the 5% level.
6. Sample sizes are 340 (height), 569 (grade attained), 555 (age start).
7. F statistics on MFE estimates are 1.40** (height), 3.02** (grade attained) and 2.17** (age start). F statistics on IV-MFE estimates are 1.15 (height), 2.68** (grade attained) and 1.86** (age start).