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LONG-TERM CONSEQUENCES OF EARLY CHILDHOOD MALNUTRITION

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Abstract

This paper examines the impact of preschool malnutrition on subsequent human capital formation in rural Zimbabwe using a maternal fixed effects-instrumental variables (MFE-IV) estimator with a long-term panel data set. Representations of civil war and drought "shocks" are used to identify differences in preschool nutritional status across siblings. Improvements in height-for-age in preschoolers are associated with increased height as a young adult and number of grades of schooling completed. Had the median preschool child in this sample had the stature of a median child in a developed country, by adolescence, she would be 4.6 centimeters taller and would have completed an additional 0.7 grades of schooling.

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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. 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 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 the health status of preschool children. We extend their analyses by linking transitory shocks experienced prior to age 3 to preschool 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. Maternal fixed effects-instrumental-variables (MFE-IV) estimates show that improvements in height-for-age in children under age 5 are associated with increased height as a young adult and the number of grades of schooling completed. The magnitudes of these statistically significant effects are functionally significant as well. Had the median preschool child in this sample had the stature of a median child in a developed country, by adolescence, she would be 4.6 centimeters taller and would have completed an additional 0.7 grades of schooling. 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 the *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 on the individual rather than the 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 preschool nutritional status and subsequent human capital attainments (see Pollitt 1990, Leslie and Jamison 1990, Behrman 1996, and Grantham-McGregor et al. 1999 [especially pages 65-70]). However, as Behrman (1996) notes, many of these studies document *associations* between preschool 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 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 (UN ACC/SCN 2000).[2] Because improving preschooler health and nutrition are seen to be important development objectives in their own right, many international organizations, such as the World Bank, are prioritizing improvements in child health and nutrition (see World Bank 2002). These organizations also emphasize increasing schooling attainments and are committed to the Millennium Development Goal of Universal Primary Education by 2015. An implication of our results is that improvements in preschool health status and primary education are not competing objectives; rather, improved preschool nutrition will facilitate meeting the education objectives. Further, if improving preschool nutritional status enhances the acquisition of knowledge at school, and leads to higher attained heights as adults, these

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

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

improvements have added value where there exist positive associations between schooling and productivity, and height and productivity.[3]

The paper begins by outlining a simple model and the econometric problems associated with its estimation. After describing the data available to us, we consider attrition bias in our sample and the endogeneity of preschooler health. Having satisfactorily addressed both concerns, we present the paper's core empirical findings. The final section concludes.

2. The Econometric Problem

The estimates presented below can be interpreted as the determinants of a vector of outcomes that is consistent with investments in human capital as part of a dynamic programming problem solved by the family of a child, subject to the constraints imposed by parental family resources and options in the community available to the individual as s/he ages (Behrman et al. 2003). Given the specific focus of this paper, we illustrate this approach 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 that can be denoted as a function of a vector of observable prices, individual and

[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 lifetime earnings. Evidence on returns to schooling and experience in the labor market for the manufacturing sector in Zimbabwe is found in Bigsten et al. (2000).

[4] This discussion draws heavily on work by Glewwe, Jacoby, and King (2001) and Alderman et al. (2001a).

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

$$H_{1k} = \alpha_{Z1} Z_{1k} + \nu_{1k}\ , \tag{1}$$

where

$$\nu_{1k} = \varepsilon_H + \varepsilon_k + \varepsilon_1$$

is a disturbance term with three components: ε_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); ε_k, which captures time-invariant, child-specific effects such as genetic potential; and ε_1, a white noise disturbance term. This particular specification is stated in terms of a reduced form rather than as a production function, though the key features regarding intertemporal correlations of errors holds in either approach.

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

$$A_{2k} = \alpha_H H_{1k} + \alpha_{Z2} Z_{2k} + \nu_{2k}\ , \tag{2}$$

where

$$\nu_{2k} = \eta_H + \eta_k + \eta_2\ ,$$

and A_{2k} is, say, the educational attainment of child k (realized in Period 2), Z_{2k} is a vector of other prices, individual and household characteristics that influence academic performance—possibly, but not necessarily, with elements common to Z_{1k}. Like ν_{1k}, ν_{2k} is a disturbance term with three components: η_H, representing aspects of the home environment that influence schooling and that are common to all children in the household (this would capture, for example, parents' attitudes toward schooling); η_k, which captures child-specific effects such as innate ability and motivation that are not controlled by parents; and η_2, a white noise disturbance term. The basic difficulty with a least-squares regression of equation (2), as noted by Behrman (1996), is the likelihood that $E(H_{1k}\nu_{2k}) \neq 0$, because of possible correlation between H_{1k} and η_H or between H_{1k} and η_k mediated through either the correlation of household effects or individual effects, or both. That is, either $E(\varepsilon_H\eta_H) \neq 0$ or $E(\varepsilon_k\eta_k) \neq 0$.

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 labor as an adult. In any of these cases, estimates of α_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 equation (2), while purging the correlation between H_{1k} and η_H, would leave unresolved the correlation between H_{1k} and η_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 preschool child health. Similarly, they do not have instruments that unambiguously identify factors that might have affected preschool 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 after age 2 is not correlated with height up to the age of 2 and that pre- or postnatal health shocks do not affect both the physical or mental development of a child.

Alderman et al. (2001a) 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 preschool 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 preschool nutritional status affects ultimate schooling and health attainments nor do they use household fixed effects.

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

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 preschoolers.[6] Second, we need data on the nutritional status of their siblings at a comparable age. 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.

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 agroclimatic 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 reinterviewed in the first quarter of 1987 and annually, during January to April, from 1992 to 2001. In the 1983/84 and 1987 rounds, valid measurements[7] on heights and

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[6] Preschool data are needed because children are at most risk of malnutrition in the early years of life, particularly ages 1 to 3. 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 3 onward 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 occurs years, if not decades, before investments on human capital are completed.

[7] 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.

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 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 80 percent 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 landholdings 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 percent of the original sample), leaving 665 "traceable" children living in 330 households. 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. This is indicated by the Z-score, 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.

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 four 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.[8] 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 preschoolers and those who were not. Children who were stunted as preschoolers 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 preschool nutritional status and subsequent human capital formation, for reasons already described, they cannot be regarded as definitive.

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

	Mean	Standard deviation
Height-for-age Z-score	–1.25	1.46
Percent children stunted	27.8%	0.45
Age (months)	39.9	21.7
Percent children male	49.2%	0.50

[8] The very few (eight individuals, or 1 percent of the sample) who had continued in school beyond the fourth form were coded as having completed 12 grades of school.

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

	Full sample	Resident children	Nonresident children
Number of children in this category	665	359	306
Mean height (in centimeters)	161.1	161.4	158.3
	(389)	(359)	(30)
Standard deviation, height	9.2	9.0	11.8
Mean completed grades	8.6	8.4	8.8
	(661)	(359)	(302)
Standard deviation, completed grades	1.9	1.9	2.0
Mean age start school (years)	7.2	7.2	7.4
	(656)	(359)	(297)
Standard deviation, age start school	1.4	1.2	1.5

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

Table 1c: Mean attainments of stunted and nonstunted children

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

Potential Selectivity Biases Caused by Attrition

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. Our sense is that these biases 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 ongoing 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. While the average height, given age, was slightly *higher* for those who died compared to the other children in the sample, a t test does not reject the null hypothesis that initial height-for-age Z-scores are equal for decreased and currently living children.

For these reasons, biases resulting from selective mortality 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 height could not be obtained for some children. This differs from the information on attainments—completed grades and age starting school. These data were available for 98.6 percent 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 outmigration—collectively accounting for about 93 percent of all cases—were marriage, looking for work, attending school, and moving "to live with other relatives."

Will this attrition bias our results? Note that the main 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 subsamples 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.

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

	Height measured	Height not measured	T statistic on difference in means
Preschool height-for-age Z-score	–1.35	-1.11	2.11**
Age (months) at first interview	37.5	43.4	3.48**
Percent children, male	57%	38%	5.08**

Note: ** significant at the 5 percent level.

Following the methods set out in Fitzgerald, Gottschalk, and Moffitt (1998) and Alderman et al. (2001b), 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.[9] 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 percent, respectively. Mutanda has the worst agricultural potential of the three areas. Further, among nonresident children originally from Mutanda, just over 27 percent had outmigrated because they had married, as opposed to 14 and 5 percent in Mupfurudzi and Sengezi, respectively. This suggests that the poorer agroclimatic 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.

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

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	–0.051
	(1.91)*	(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)**
Sample size	665	612

Notes: Sample is preschooler 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. Models in Table 2b were estimated as probits, with standard errors robust to clustered sample design. Numbers in parentheses are absolute values of z statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

Lastly, 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 coefficients of regressions explaining this initial nutritional status differ across these two subsamples. As Becketti et al. (1988) show, such results provide further support for our claim that attrition bias does 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 have information for virtually the entire sample of grade completion and age at which schooling commenced. Moreover, 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

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 η_H. Instrumental variables address the potential correlation between H_{1k} and η_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.[10] Although all household members face the shock at the same calendar year, the variables differ for the purpose of model identification, since we specify the identifying variables in terms of a shock at a given age for the individual. Note that we do not argue that either of these two shocks only affected nutrition—clearly war, even war in distant parts of the country, affects the economy as a whole

Since the MFE-IV approach looks at the *differential* impact of shocks on siblings, it makes it unnecessary to consider how long it took the local economy to recover. However, for the record, a reconstruction program to fix schools damaged during the war was completed prior to 1986, as was the implementation of a government decision to hire additional teachers so as to reduce class size in primary schools. School fees for primary school were abolished in September 1980 and the expansion of the availability of secondary schools was largely complete by 1986 (Zimbabwe 1998). We contend,

[10] Note that it is possible that given these shocks, parents might subsequently engage in compensatory actions. Our first-stage estimates are the effects of these shocks *net* of such parental actions.

therefore (and provide supportive evidence below), that 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.[11] 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 postindependence 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 children's height-for-age. 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.

Instrument Validity

Table 3 presents two sets of estimates for initial height-for-age Z-score 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,

[11] Also, see Jensen (2000).

Jaeger, and Baker (1995) show that the bias of our IV results will be less than 2 percent of simple OLS estimates.

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 (log of number of days child was living prior to 18 August 1980)	–0.048
	(3.36)**
1982–84 Drought shock (child was exposed to the 1982–84 drought when aged between 12–36 months)	–0.631
	(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: Dependent variable is child height-for-age Z-score. Sample is preschooler 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. Numbers in parentheses are absolute values of t statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

Before continuing, however, it is important to return to 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 preschool nutritional status. But the ending of the war involved school/road reconstruction as well as safe travel, so thus improving the accessibility of schools for the younger sibling relative to the older sibling. Arguably, wars leave psychological scars, induce geographical dislocations, and it might be hard to believe that such effects only work through initial heights. For these reasons, we construct an overidentification test as outlined in Wooldridge (2001, 123). We estimate our MFE-IV 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 overidentification 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 percent 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 (1) of sufficient magnitude and persistence to affect a child's stature, H_{1k}; (2) sufficiently variable across children; (3) sufficiently transitory *not* to affect the sibling's stature; and (4) sufficiently transitory not to affect subsequent attainments, A_{2k}.

The Impact of Preschooler 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 preschooler height-for-age on adolescent height. Four estimates are reported: a "naïve" least-squares estimate, with controls for child (age and sex) and maternal characteristics (age, education, and height); instrumental variables with fixed effects, a maternal fixed-effects estimate which, as noted above, eliminates correlation between H_{1k} and ε_H;[12] and a maternal fixed-effects-instrumental variables estimate, using the "shock" variables described above as instruments, to eliminate both the correlation between H_{1k} and ε_H and between H_{1k} and ε_k.

The first row of Table 4 indicates that better preschool nutritional status is associated with greater attained height in adolescence. The "naive" least squares results and the maternal fixed effects estimates are roughly comparable in magnitude and statistically significant. Similarly, the two models using fixed effects are comparable; controlling for the endogeneity of initial height-for-age, the impact of preschool nutritional status increases markedly. The equality of parameters between the MFE and preferred MFE-IV results can be rejected at the 10 percent level.

[12] 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 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 MFE-IV estimators is that the impact of such characteristics is differenced out.

Table 4: Determinants of adolescent height

	(1) "Naïve" village fixed effects	(2) Instrumental variables, village fixed effects	(3) "Naïve" maternal fixed effects	(4) Instrumental variables, maternal fixed effects
Child height-for-age Z-score	1.628	3.116	1.153	3.649
	(7.61)**	(2.69)**	(2.52)**	(2.34)**
Boy	3.374	3.756	3.909	4.199
	(5.09)**	(6.36)**	(4.13)**	(3.99)**
Current age (years)	2.405	2.417	2.555	2.536
	(12.77)**	(10.94)**	(12.03)**	(10.87)**
R^2	0.580	0.528	0.449	0.421

Notes: Sample is preschooler 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. For naïve village fixed effects and instrumental variables, village fixed effects estimates, maternal age, education, and height are included as additional controls. Numbers in parentheses are absolute values of t statistics (columns 1-3) and z statistics (column 4). Standard errors for the naïve village fixed effects and instrumental variables, village fixed effects estimates are robust to clustered (village) sample design. * Significant at the 10 percent level; ** significant at the 5 percent level. Sample size is 340.

As indicated in Table 5, increased height-for-age is associated with a greater number of grades attained in the naïve models as well as with the MFE-IV estimator. The magnitude of this impact is affected by whether we control for correlation between H_{1k} and ε_k. The MFE-IV estimates provide a larger estimate of impact, the difference between this parameter estimate and the MFE results being significant at the 13 percent confidence level.[13]

Table 6 looks at the relationship between preschool heights and the age at which children begin school. While the naïve models imply that taller children start school slightly younger, this is not observed using the MFE-IV estimates. Note that as a specification check, we included child's month of birth—a plausible determinant of the

[13] It is worth considering as to why the use of instrumental variables increases the parameter estimates for both stature and schooling. In addition to reducing attenuation bias from measurement error in the regressors, our estimation strategy is analogous to that described by Imbens and Angrist (1994), Card (2001), and Giles, Park, and Zhang (2003); we use cohort specific shocks as instruments and assume that these are independent of individual characteristics. One interpretation, again analogous to these three papers, is that there may be heterogeneity in returns to preschool nutrition. In keeping with Card (2001, pp. 1142-1143), if individuals with relatively high marginal returns to preschool nutrition face relatively higher costs of improving their preschool nutritional status, then our IV estimates identify individuals with higher returns to preschool nutrition than average.

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.

Table 5: Determinants of number of grades attained

	(1) "Naïve" village fixed effects	(2) Instrumental variables, village fixed effects	(3) "Naïve" maternal fixed effects	(4) Instrumental variables, maternal fixed effects
Child height-for-age Z-score	0.204	-0.053	0.145	0.566
	(3.29)**	(0.25)	(2.61)**	(1.99)**
Boy	-0.051	-0.120	0.153	0.201
	(0.41)	(0.99)	(1.20)	(1.41)
Current age (years)	0.452	0.457	0.442	0.420
	(9.74)**	(9.47)**	(15.32)**	(12.12)**
R^2	0.442	0.406	0.253	0.226

Notes: Sample is preschooler 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. For naïve village fixed effects and instrumental variables, village fixed effects estimates, maternal age, education, and height are included as additional controls. Numbers in parentheses are absolute values of t statistics (columns 1-3) and z statistics (column 4). Standard errors for the naïve village fixed effects and instrumental variables, village fixed effects estimates are robust to clustered (village) sample design. * Significant at the 10 percent level; ** significant at the 5 percent level. Sample size is 569.

Table 6: Determinants of age starting school

	(1) "Naïve" village fixed effects	(2) Instrumental variables, village fixed effects	(3) "Naïve" maternal fixed effects	(4) Instrumental variables, maternal fixed effects
Child height-for-age Z-score	-0.256	0.088	-0.140	-0.055
	(5.84)**	(0.65)	(3.00)**	(0.25)
Boy	0.346	0.417	0.270	0.279
	(4.59)**	(4.27)**	(2.52)**	(2.54)**
Current age (years)	0.156	0.151	0.177	0.172
	(4.92)**	(3.76)**	(7.32)**	(6.26)**
R^2	0.302	0.193	0.134	0.113

Notes: Sample is preschooler 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. For naïve village fixed effects and instrumental variables, village fixed effects estimates, maternal age, education, and height are included as additional controls. Numbers in parentheses are absolute values of t statistics (columns 1-3) and z statistics (column 4). Standard errors for the naïve village fixed effects and instrumental variables, village fixed effects estimates are robust to clustered (village) sample design. * Significant at the 10 percent level; ** significant at the 5 percent level. Sample size is 555.

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 MFE-IV parameter estimates reported in Tables 4 and 5, this would result in an additional 4.6 centimeters of height in adolescence and an additional 0.7 grades of schooling. In terms of exposure to the 1982-84 drought, recall that this shock reduced height-for-age Z-scores by 0.63. Using the MFE-IV estimates, this implies that this transitory shock resulted in a loss of stature of 2.3 centimeters and 0.4 grades of schooling.

These magnitudes can also be expressed 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 translates into a 12 percent reduction in lifetime earnings. The impact of the shocks, as described above, translates into a 7 percent loss in lifetime earnings. Such estimates are likely to be lower bounds. Fogel (1994) presents evidence that links short stature among males to the early onset of chronic diseases and to premature mortality. Although comparable evidence 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 childbirth, typically have children with higher birth weights, and experience lower risks of child and maternal mortality (World Bank 1993).

More on Robustness

We have presented MFE-IV estimates that show a causal link between preschool anthropometric status and subsequent health and schooling attainments. We have argued that our approach is robust to concerns regarding potential attrition bias and instrument validity. In this subsection, we consider a number of potential objections to these results that might call their robustness into question.

One concern might be that these estimates do not control for birth order. However, Horton's (1988) detailed exploration of the impact of birth order on nutritional status shows 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.

A second concern is that the sample includes young adolescents who are continuing to grow physically and who may continue to attend school beyond the date of observation. Note, however, that by construction, there is no child in the sample younger than 13½ years. Past this age, at least in well-nourished populations, there is little growth in a girl of median stature and about 10 centimeters of growth in a boy of median stature (see Hamill et al. 1979). Consequently, one way of addressing this concern is to reestimate the results presented in Tables 4, 5, and 6, but replace current age with current age less 14 and age less 14 interacted with being a boy. When we do so, the impact of preschool nutrition on stature, schooling, and age starting school remain unchanged.

A further concern is that our specification does not take into account nonlinearities. For example, children's growth velocity slows with age, yet only a linear specification of age is included in Tables 4, 5, and 6. A simple way of addressing this concern is to replace current age with log current age. When we do so, the results we obtain are virtually indistinguishable from those already reported.[14]

As a further check, we reestimate our three outcomes using a slightly different specification. In addition to our measure of preschool nutritional status, we include child sex, age at first measurement (in months), duration of time that passed between first measurement and reinterview in 2000, and the interaction between age and duration. Age and duration of observation are expected to be associated with increased attainments, while the interaction term captures potential nonlinearities. We also include a dummy variable that equals one for individuals who turn seven before January 1986. Suppose that, despite the results of our specification tests reported above, one still did not believe

[14] In preliminary work, we included a quadratic for age, but this produces nonsensical nonlinearities. The estimated parameters imply that children shrink past age 16 and that number of grades attained is maximized around age 8.

that we had valid instruments; rather, our instruments were merely picking up a cohort effect associated with the war, the drought, and access to schooling. If this is the case, the combination of controls for nonlinearities and the inclusion of a cohort effect should destroy the results obtained above. These results are reported in Table 7.

Table 7: Instrumental variables, maternal fixed effects estimates using alternative specification

	Dependent variable		
	Height as adolescent	**Grades attained**	**Age started school**
Child height-for-age Z-score	2.981	0.743	-0.456
	(2.20)**	(2.07)**	(1.61)
Boy	4.002	0.190	0.192
	(4.19)**	(1.26)	(1.63)
Age at first measurement (months)	1.012	0.098	0.011
	(3.93)**	(3.06)**	(0.44)
Duration of observation	4.517	0.673	0.002
	(5.57)**	(5.84)**	(0.02)
Age × duration of observation	-0.697	-0.057	0.009
	(3.04)**	(2.02)**	(0.39)
Turned seven before January 1986	-2.118	0.139	-0.260
	(0.43)	(0.29)	(0.69)
R^2	0.466	0.212	0.150

Notes: Sample is preschooler 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. Numbers in parentheses are absolute values of z statistics.* Significant at the 10 percent level; ** significant at the 5 percent level.

We begin by noting that the F statistic on the "relevance" of instruments falls to 5.36 with this re-specification. *However*, the estimates continue to pass the uncorrelatedness tests. Further, the MFE-IV results are *larger* in magnitude for two outcomes, grades attained and age starting school—the latter now significant at the 11 percent confidence level—and slightly smaller (but not significantly different) in the case of stature.

Lastly, another way of exploring the validity of our results is to determine whether there was any attenuation or reduction in the impact of preschool nutritional status on stature 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. 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) was independent of initial height (H_0). That is, since

$$[H_1 - H_0] = \alpha + \beta H_0 + \gamma X \text{ when } \beta = -1,$$

we are left with

$$H_1 = \alpha + \gamma X.$$

This is sometimes described as "complete catch-up"; see Hoddinott and Kinsey (2001) for a further discussion.

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

$$\underset{}{[H_1 - H_0]} = \underset{(2.67)}{51.67} + \underset{(2.39)}{(-0.513)}\,(H_0) + \underset{(4.21)}{(3.680)}\,(\text{boy}) + \underset{(4.06)}{(10.590)}(\text{initial age}) + \underset{(6.87)}{(4.854)}(\text{duration of observation})$$

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

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.[15] Note that 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. Moreover, we considered the possibility that if malnourished preschoolers 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.

5. Conclusions

Using longitudinal data from rural Zimbabwe, we have shown that improved preschooler 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 MFE-IV 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 MFE-IV 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 preschooler nutritional status enhances the acquisition of schooling, and leads to higher attained heights as adults (and that lost growth velocity as a

[15] 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.

preschooler 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 preschool 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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