

## Early childhood nutrition and academic achievement: a longitudinal analysis

Paul Glewwe<sup>a,b,\*</sup>, Hanan G. Jacoby<sup>a</sup>, Elizabeth M. King<sup>a</sup>

<sup>a</sup>*Development Research Group, The World Bank, 1818 H Street N.W., Washington, DC 20433, USA*

<sup>b</sup>*University of Minnesota, Department of Applied Economics, St. Paul, Minnesota, MN 55108, USA*

Received 1 June 1999; received in revised form 1 June 2000; accepted 1 July 2000

---

### Abstract

Many policymakers propose early childhood nutrition programs as a way to increase students' academic achievement. This paper investigates the nutrition–learning nexus using a unique longitudinal data set that follows a large sample of Filipino children from birth until the end of their primary education. We find that better nourished children perform significantly better in school, partly because they enter school earlier and thus have more time to learn but mostly because of greater learning productivity per year of schooling. Our cost–benefit analysis suggests that a dollar invested in an early childhood nutrition program in a developing country could potentially return at least three dollars worth of gains in academic achievement, and perhaps much more. © 2001 Elsevier Science B.V. All rights reserved.

*Keywords:* Early childhood; Nutrition; Academic achievement

---

### 1. Introduction

In recent years, policy-makers have increasingly promoted early childhood nutrition programs as a way to raise living standards in developing countries (World Bank, 1993; Young, 1996), as well as among the US poor (GAO, 1992). Proponents of such programs argue that improved diet, particularly in the crucial first years of life, enhances intellectual development and, ultimately, academic

---

\*Corresponding author.

success (see Brown and Pollitt, 1996). Their view is that, in addition to having direct health benefits, early childhood nutrition programs and broader food security initiatives could also be an instrument of education policy. Yet, the evidence in support of this view is surprisingly sparse (Behrman, 1996).

Behind this lack of evidence is a paucity of good data, specifically data that allow one to address the problem of spurious correlation between nutritional status and academic achievement (*conditional* on other academic inputs). Such correlation could arise from parental behavior; for example, parental allocations of nutritional inputs may respond to unobserved variation in learning efficiency (e.g., child ability or motivation) both across and within households. In principle, the problem could be addressed using data generated from an experiment in which treatment and control groups of infants are randomly selected from a malnourished population. The treatment group is provided an improved diet during the first few years of life and a decade or so later both groups are given school achievement tests. One could then estimate the relationship between academic achievement and indicators of early childhood nutrition, such as height, using treatment status as an instrumental variable for the latter.<sup>1</sup> But because of the ethical and practical issues raised by this ideal experiment, it has yet to be carried out on a large scale and perhaps never will be.<sup>2</sup> Approaching this ideal experiment, at least in terms of length of follow-up, is the INCAP study in Guatemala (Pollitt et al., 1993), in which significantly positive effects of early childhood supplementary feeding on cognitive skills in adolescence are found. However, the experiment has two drawbacks: First, it compares just two distinct nutritional interventions, each in only two villages, without a pure control group. Second, the nutritional supplements were provided in a central feeding station, so that individual participation was *voluntary*, not randomized.

In this paper, we estimate the impact of nutrition on learning using nonexperimental data collected in Cebu, Philippines over a period of 12 years. A large sample of children was followed from shortly before their birth and up through primary school, providing information on early childhood nutrition and subsequent school performance, as measured by achievement tests. Achievement test scores and other information are also available for the younger siblings of the original children. Our study is thus the first to combine longitudinal information on children with data on their siblings to investigate the nutrition–learning nexus.

---

<sup>1</sup>Note that this procedure would only identify the reduced form effect of nutrition on achievement. To estimate the structural relationship (see below), one would still have to control for all other academic inputs, which might also have to be explicitly randomized across children.

<sup>2</sup>Recently, Grantham-McGregor et al. (1997) analyze data from a randomized early childhood nutrition and stimulation intervention in Jamaica with follow-up cognitive skills testing at ages 7–8 years. However, the sample size (122 children) is too small to obtain firm conclusions about the benefits of supplementation.

Though we will argue shortly that no nonexperimental study can hope to replicate the ideal experiment described above, we believe that our structural production function analysis makes considerable progress in sorting out the causal relationship between nutrition and learning. In particular, we find that better early childhood nutrition raises academic achievement. Our analysis also illuminates the pathways through which nutritional status in the first years of life affects learning in a developing country. Part of the advantage that well-nourished children enjoy arises from the fact that they enter school earlier and thus have more time to learn. The rest of their advantage stems from greater learning productivity per year of schooling. We find little evidence that early nutritional status influences learning effort in the form of school attendance, time spent on homework, and so forth.

The remainder of this paper is organized as follows. We lay out a framework for estimation in Section 2 and describe our data in Section 3. Section 4 presents the results of our estimation and in Section 5 we use these results in a policy evaluation. We summarize our findings in Section 6.

## 2. Empirical strategy

Our analysis focuses on the achievement production function, which relates early childhood nutrition and other academic inputs to a child's scholastic output as measured by a score on an achievement test. The academic input on which we focus is the nutritional history of the child in the early years of life. However, as a practical matter, measuring *cumulative* nutrition inputs is extremely difficult, and a simple alternative is to use the child's height-for-age as a summary statistic for the nutritional history up to that age.<sup>3</sup>

To highlight our informational assumptions, we divide a child's life into three periods. Period 0 consists of (approximately) the first 2 years of life, which is thought to be the most crucial stage of post-natal development (see Cravioto and Arrieta, 1986; Dobbing, 1984; and Levitsky and Stropp, 1995). Period 1 is from 2 years to the minimum age of primary school enrollment, and period 2 is the primary school period. A linear achievement production function is given by

$$A_{2k} = \alpha_H H_{1k} + \alpha_Z Z_{2k} + \eta + \varepsilon_k \quad (1)$$

where  $A_{2k}$  is the academic achievement of child  $k$  (realized in period 2),  $H_{1k}$  is height-for-age in period 1, which is taken to represent the child's nutritional

---

<sup>3</sup>A criticism of this approach, discussed further below, is that height-for-age reflects more than just a child's cumulative nutritional history, because it also captures the effects of illness and other environmental and genetic influences.

history up until the end of that time period,<sup>4</sup> and  $Z_{2k}$  is a vector of other inputs that influence academic performance.<sup>5</sup>

Achievement also depends on the child's learning efficiency or "endowment", which represents factors, such as ability and motivation, that are out of parents' control. We decompose the learning endowment into a component  $\eta$  that is common among siblings  $i$  and  $j$ , and a component  $\varepsilon_k$  that is child specific. The common component is assumed to be known to parents prior to the birth of any one of their children, but a given child's  $\varepsilon_k$  is not known until some time after his or her birth. Note also that any element of  $Z_{2k}$  that is common across siblings, such as those reflecting "home environment", will be impounded in  $\eta$ .

With this stochastic specification, we are ready to consider the problem of spurious correlation between nutritional status and achievement. The most general economic model of human capital investment (see, e.g., Rosenzweig and Wolpin, 1988) implies that  $H_{1k}$  and  $Z_{2k}$  are functions of, among other things,  $\eta$  and all the  $\varepsilon_k$ s that have been "realized" up to that point; this leads to simultaneity bias in Eq. (1). On the other hand, the  $\varepsilon_k$  of a child who has not yet been born is unknown and thus cannot influence parental behavior. We further argue that parents are unlikely to know child  $k$ 's learning endowment in period 0, when they make the nutritional investments for that child that are reflected in  $H_{0k}$ , so that  $E[H_{0k}\varepsilon_k] = 0$ . This informational assumption is consistent with the conclusions of the psychology literature on child intelligence.<sup>6</sup>

Suppose now that we have a sample of sibling pairs, with information on academic achievement, academic inputs, and height-for-age. By differencing Eq. (1) across siblings  $i$  (older) and  $j$  (younger) we can purge  $\eta$  to eliminate part of the correlation between the error term and the inputs. This procedure yields

<sup>4</sup>Here we ignore potential spillovers or cross-effects among siblings, whereby the height-for-age of older siblings enter child  $k$ 's production function. Such effects could arise if, for example, a malnourished child is unable to provide his or her younger sibling with as much help in their schoolwork as a well-nourished child. Since we have information on the nutritional status of older siblings for only some children in our sample, it is difficult to address the spillover issue with our data set. However, we doubt that any cross-sibling effects that do exist would be of the same order of magnitude as the effect of own nutrition on achievement, which is difficult enough to detect in a data set of typical size.

<sup>5</sup>In contrast to this cumulative achievement production function, Hanushek (1986) advocates a value-added specification in which lagged achievement is included on the right hand side of Eq. (1) and only incremental inputs are considered. The problem with such a specification in the present context is that the incremental effect of nutrition on the achievement gain between any one or two grades would be smaller than the cumulative effect considered in Eq. (1) and therefore harder to detect.

<sup>6</sup>McCall (1979) summarizes the view prior to 1980: "after nearly one-half century of data collection and analysis, the results unequivocally show that scores on instruments of infant mental performance during the first 18 months of life do not predict later IQ to any practical extent (p. 707)." More recently, psychologists have developed new tests of infant mental capacity that show at least a weak correlation with intelligence in later life (Siegel, 1989). Nonetheless, it would seem safe to assume that parents in developing countries cannot detect their children's mental acuity, and thus cannot make nutritional allocations accordingly, prior to age two.

$$\Delta A_{2k} = \alpha_H \Delta H_{1k} + \alpha'_Z \Delta Z_{2k} + \Delta \varepsilon_k \quad (2)$$

where  $\Delta A_{2k} = A_{2i} - A_{2j}$ , and so forth. Differencing also has the advantage of purging any unobserved inputs that are constant across siblings; for example, in our sample nearly all sibling pairs attend the same school, so that school quality is one such common input. Still, we are left with an endogeneity problem because, in general,  $E[\Delta H_{1k} \Delta \varepsilon_k] \neq 0$ ; once a child's ability or motivation for learning is realized by parents it may influence their allocations of nutritional inputs. A natural solution to this problem is to use  $H_{0i}$ , the nutritional status of the older child in his first 2 years of life, as an instrument for  $\Delta H_{1k}$ .<sup>7</sup> Under our informational assumptions, this instrument is uncorrelated with both  $\varepsilon_i$  and  $\varepsilon_j$ . As for the endogeneity of the other academic inputs,  $Z_{2k}$ , we return to this issue in Section 4.

There is another reason to adopt this instrumental variable strategy. If period 0 nutrition, as reflected by  $H_{0k}$ , is crucial for a child's cognitive development, but subsequent nutrition is less important or, in the extreme case, unimportant, then Eq. (2) could lead to an underestimate of the true impact of nutrition on achievement. In other words,  $H_{1k}$  is a noisy indicator of early childhood nutrition, and this "measurement error" in the nutrition indicator leads to attenuation bias in Eq. (2). Of course, the best way of determining the true impact of period 0 nutrition would be to replace  $\Delta H_{1k}$  by  $\Delta H_{0k}$  in Eq. (2). Unfortunately, in our data set anthropometric measurements during early childhood (prior to age two) are available only for the older sibling. Nevertheless, our instrumental variables strategy corrects for this measurement error bias, provided that child physical growth after age two is uncorrelated with  $H_{0i}$ .<sup>8</sup> On the other hand, there is another source of measurement error in our nutritional status indicator: height-for-age varies across siblings within the same household not just because of nutrition, but also for genetic reasons. Since  $H_{0i}$  is correlated with the genetic component of the sibling height-for-age difference, its use as an instrument will only reduce, not eliminate, the attenuation bias. Thus our instrumental variable approach will still tend to underestimate  $\alpha_H$ .

A final criticism of our basic identifying assumption,  $E[H_{0i} \Delta \varepsilon_k] = 0$ , is that  $H_{0i}$  and  $\varepsilon_i$  may be correlated for physiological reasons. For example, pre- or postnatal health shocks may influence both the physical and mental development of the child. It is also conceivable that there is a genetic correlation between physical

<sup>7</sup>Behrman and Lavy (1994) also use a sibling differences procedure to study the nutrition–learning nexus in Ghana, but they do not have longitudinal data to deal with the endogeneity problems that remain. In their US study of pre-school cognitive skills, Rosenzweig and Wolpin (1994) use data on siblings as well and also control for the correlation between child-specific unobservables and prenatal inputs, which are the focus of their analysis, but they find no evidence of bias from the latter source.

<sup>8</sup>Such a correlation would exist if there were significant catch-up growth after age two, the possibility of which is still an unresolved issue in the nutrition literature (Martorell et al., 1994).

stature and innate mental ability, though we know of no research that supports this. Assuming that all other relevant academic inputs are indeed controlled for, an iron-clad identification strategy when  $E[H_{0i} \Delta \varepsilon_k] \neq 0$  is to search for a “natural” experiment, such as an income or price *shock* (or a combination of shocks), that leads to differences between siblings in height-for-age. However, finding in any data set a shock that is: (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, would be nothing short of miraculous.<sup>9,10</sup> So, there does not seem to be any hope of replicating the ideal experiment laid out in the introduction with a natural experiment.<sup>11</sup> Despite our inability to correct for the potential bias due to physiological shocks, we can plausibly sign its direction. Since poor health is likely to impair both a child’s physical growth and mental development, it will induce a positive correlation between  $H_{0i}$  and  $\varepsilon_i$ , leading to an overestimate of  $\alpha_H$ .

The last issue concerning our empirical strategy is how to handle delayed primary school enrollment. In our data, many children enter school after the minimum age at which they are allowed to enroll. This phenomenon is not unique to the Philippines, and has been noted in other low income countries (see Glewwe and Jacoby, 1995). The implications of delayed enrollment for the estimation of achievement production functions are two-fold. First, delaying enrollment may improve learning productivity because, *ceteris paribus*, an older child is better prepared for school. Indeed, one of the reasons parents might delay enrolling their children is to make up for an initial lack of school readiness (Glewwe and Jacoby, 1995). Thus, the age of enrollment should be one element of the vector  $Z_{2k}$  in Eq. (1). Second, an inherent, yet little noticed, feature of scholastic achievement data is that only children who have already entered school (or, more generally, a particular grade) are tested. Yet, children who delay enrollment and thus are not tested may differ systematically from those who enroll on time. This implies that delayed enrollment can lead to selection bias in the achievement production function. As we show below, judicious choice of the sample should remove almost all potential for selection bias.

---

<sup>9</sup>The classic example of such a natural experiment is the stunting caused by famine during the Second World War in Holland (Stein et al., 1975), but to be useful in practice exposure to the famine must vary exogenously across the sample. An intensive feeding program (not necessarily randomly assigned) that happens to be phased in or out after the younger sibling is born would also suffice provided, again, that its coverage varied in the sample.

<sup>10</sup>The condition that the shock *differentially* affect the siblings’ stature would not be necessary if Eq. (1) is estimated directly, i.e. without differencing across siblings. However, the problem is then to find price or income variables that are correlated with child height, but do not reflect unobservable household or geographic characteristics that directly influence achievement, such as school quality.

<sup>11</sup>Yet another identification strategy would be to use household or mother characteristics (including prenatal inputs) realized prior to the birth of the older sibling as instruments. We investigated many such variables but found them to be very weakly correlated with sibling height differences in our sample.

### 3. Data and sample

#### 3.1. The Cebu longitudinal health and nutrition survey

Our data come from the Cebu Longitudinal Health and Nutrition Survey (CLHNS), which was carried out in the Metropolitan Cebu area on the island of Cebu, Philippines.<sup>12</sup> Metro Cebu includes Cebu City, the second largest city in the Philippines, and several surrounding urban and rural communities. The CLHNS tracks a sample of 3289 children born between May 1, 1983 and April 30, 1984 in 33 randomly selected barangays (districts). The interviews began before birth, when the mothers were seven months pregnant. Detailed health and nutrition data, including anthropometric measurements of both the mother and the “index child” (to distinguish them from their younger siblings, who were only interviewed in later rounds), were gathered every two months for the first 2 years of the child’s life, along with household and community-level information. Follow-up surveys were conducted in 1991–1992, when the index children were 8 years old, and in 1994–1995, when they were about 11 years old. Both follow-up surveys collected anthropometric data on the index children *and* their younger siblings. Our empirical analysis uses the height-for-age Z-score closest to the time of the school enrollment decision. For index children this is the 1991–1992 Z-score and for younger siblings it is the 1994–1995 Z-score.

The 1994–1995 follow-up survey gathered detailed information on schools, academic inputs, and student achievement. English reading comprehension and mathematics tests were developed for the survey based on the official primary school curriculum. We use the sum of the math and English scores in our analysis. This follow-up survey also provides a detailed schooling history of each index child and, if in school in 1994–1995, his or her younger sibling. Information includes the year the child first enrolled in school, grade repetition, and current grade (or last grade attended if no longer enrolled). Finally, exhaustive information was collected on current academic inputs for each child, including time allocation on school days.

Our analysis focuses on sibling pairs of school age. Of the 2192 index children still in the sample in 1994–1995,<sup>13</sup> 1239 have a younger sibling of school age (at least 6.5 years old by June, 1994, the start of the 1994–1995 school year). Of these sibling pairs, 1149 of the younger siblings were enrolled in school by June 1994 and the remaining 90 had not yet enrolled. After dropping observations with

---

<sup>12</sup>The survey was jointly conducted by the Office of Population Studies at the University of San Carlos, Philippines, the Nutrition Center of the Philippines, and the Carolina Population Center of the University of North Carolina (Chapel Hill).

<sup>13</sup>Eighty-three percent of the 3,085 children originally interviewed in 1983 were still in the sample by age two. Attrition was mainly due to death and permanent migration out of the Metro Cebu area. About 72% of the original children were resurveyed in the 1991–1992 follow-up, and the sample declined only slightly for the 1994–1995 follow-up. See Appendix Table A.1 for details.

missing data, we are left with a sample of 1016 enrolled sibling pairs for the production function estimation.

### 3.2. Description of key variables

Table 1 describes schooling outcomes for our sample in 1994–1995. Virtually all index children had enrolled in primary school by age 11 (the 9 who did not were dropped from the sample). Younger siblings born in 1984–1986 had enrollment rates upward of 95%, while only about three-quarters of those born in 1987 had enrolled by the 1994–1995 school year. This implies that selection bias due to delayed enrollment of the younger sibling is an issue only among pairs for which the younger sibling was born in 1987.

At the time the children in our sample began to enter school, six and a half was the minimum age of school enrollment in Cebu. A child can be said to have delayed enrollment if he is 7.5 years of age or older when he enters first grade. Table 1 shows that about 19% of index children enter school late (usually waiting until the next academic year to enroll), but delays are much more common among those born in 1983 than among those born in 1984. This greater tendency to delay is due to the fact that the 1983 births occurred later in the calendar year than the 1984 births and hence these children were relatively younger when they had their first opportunity to enroll (in the Philippines the school year begins in late June); presumably, their parents viewed them as less “ready” for school.

Table 1  
School enrollment, repetition, and grades completed<sup>a</sup>

	Index children		Younger siblings			
	1983	1984	1984	1985	1986	1987
Year born						
Number of children	917	322	98	481	443	217
Ever enrolled in first grade (%)	100	100	95.9	97.1	95.3	76.5
Delayed enrollment:						
Ever delayed (%)	21.2	11.2	19.1	21.6	18.5	0.0
Number of years delayed (mean for those who delayed)	1.3	1.3	1.3	1.3	1.2	–
Grade repetition:						
Ever repeat any grade (%)	29.3	18.9	23.5	15.2	8.4	2.3
Ever repeat first grade (%)	21.2	13.4	20.4	13.9	8.4	2.3
Number of grades repeated (mean for those who repeated)	1.3	1.2	1.2	1.1	1.0	1.0
Current grade in school (mean)	4.2	3.6	3.3	2.5	1.7	1.1
Dropped out (%)	5.3	4.4	2.0	4.4	2.9	4.6

<sup>a</sup> Notes: The number of children and enrollment figures are based on a sample of 1239 siblings pairs for which the index child could be matched to a younger sibling of school age. They exclude nine pairs for which the index child never enrolled in school. The remaining figures are conditional on school enrollment (90 younger siblings were not enrolled). The data refer to the 1994–1995 school year.

Table 1 also indicates that grade repetition is pervasive in Cebu, with 27% of the index children repeating at least one grade. Repetition is more common among children born in 1983 than in 1984, probably for the same reason that delays are more common; children born in 1983 were less ready for school and hence those who did not delay their enrollment were more likely to repeat. First grade is by far the most frequently repeated, and most children repeat only once. Although repetition appears to decline for later cohorts of younger siblings, this trend is spurious. Later cohorts are more likely to be in grade one for the first time, and thus have not had the opportunity to repeat. Table 1 also shows that dropping out of primary school is a rare phenomenon, with only about 4% of the sample having left school.

Finally, a word about the nutritional status of the sample, which is quite low despite the fact that Cebu is not desperately poor: Almost half of the children in the sample are currently stunted, i.e., their height-for-age is at least two standard deviations below the mean for a healthy US population.

## 4. Empirical results

### 4.1. Achievement production function

The central results of this paper are presented in Table 2. Besides the sex and height-for-age *Z*-score of the child at the time of school enrollment, the achievement production function includes the age of enrollment, time spent in school, and time not in school; these variables sum to give current age, which is *potential* time in school.<sup>14</sup> Time in school is distinguished by grade (first and second versus third through sixth) and by whether it is spent repeating (either first or second) grades, since the marginal impact of a repeated grade may be different than a nonrepeated grade. Time not in school is the sum of school break time and, for children no longer in school, time since the child dropped out. This variable is included to capture learning depreciation. School characteristics are assumed to affect achievement as well, but drop out of the sibling differences specifications [Eq. (2)] because almost all sibling pairs attend the same primary school.<sup>15</sup>

To assess the endogeneity and selection issues, we examine a series of

---

<sup>14</sup>In a preliminary analysis, we also included variables reflecting birth order (a dummy variable for whether the child was first born as well as the prior birth interval in months), but these never attained statistical significance and thus were subsequently dropped.

<sup>15</sup>All but 47 of the sibling pairs attend the same school. To check whether these cases would affect our results, we estimated a regression analogous to those in Table 4 below for school “quality” as measured by the school average of the sixth grade National Education Achievement Test. A test of whether school quality responds to child nutrition status has a *P*-value of 0.48. Hence, ignoring school quality differences among these 47 sibling pairs should not bias our estimate of the height-for-age coefficient in the production function (see the discussion of Table 4).

Table 2  
Achievement production function<sup>a</sup>

	Mean (Std. Dev.)	GLS <sup>b</sup>	Sibling differences		
			OLS	2SLS <sup>c</sup>	2SLS <sup>c</sup>
Height-for-age Z-score	−1.98 (0.95)	1.336* (0.457)	0.310 (0.664)	8.891* (2.826)	9.825* (3.042)
Child is female	0.48 (0.50)	5.284* (0.748)	6.050* (0.941)	3.735* (1.210)	4.229* (1.246)
Age enrolled (months)	85.45 (6.62)	0.204* (0.064)	0.345* (0.102)	0.555 (0.345)	0.604 (0.364)
Months in 1st to 2nd grade (non-repeated)	18.55 (3.52)	0.269* (0.120)	0.167 (0.149)	1.053* (0.454)	0.745 (0.743)
Months in 3rd to 6th grade	14.68 (12.54)	1.133* (0.038)	1.033* (0.088)	1.105* (0.256)	1.060* (0.258)
Months repeated in 1st and 2nd grade	2.01 (5.01)	−0.302* (0.080)	−0.211 (0.110)	0.102 (0.593)	−0.219 (0.795)
Months not in school	6.56 (4.83)	0.219* (0.097)	0.326* (0.135)	−0.699 (1.005)	−0.134 (0.992)
Mother's years of schooling	6.89 (3.26)	1.575* (0.167)	–	–	–
Overidentification test: $\chi^2_{(10)}$ P-value	–	–	–	0.724	0.587
Sample size (sibling pairs)	1016	1016	1016	1016	863

<sup>a</sup> Robust standard errors in parentheses (\* denotes statistical significance at the 0.05 level). All regressions include a constant. Mean achievement test score is 25.5, with a standard deviation of 25.5.

<sup>b</sup> Household random effects specification also includes a full set of school dummy variables.

<sup>c</sup> All variables endogenous except sex. Excluded instruments: height-for-age of older sibling at 0, 12 and 24 months, months of birth dummy variables for both siblings, sibling difference in age tested, interaction terms between sibling difference in age tested and the three height-for-age variables, and the proportion of children repeating grades in the barangay where the older child was born interacted with the sex and age difference variables. The sample in the last column drops sibling pairs for which the younger sibling was born in 1987.

estimators of the achievement production function. Column two reports a household random effects (GLS) specification of Eq. (1). In addition to the variables mentioned above, this specification includes a set of school dummy variables to fully capture variation in school characteristics, as well as mother's years of schooling, to soak up some of the heterogeneity in home environment. The coefficient on height-for-age is positive and significantly different from zero as well as from the less restrictive fixed effects estimate ( $P$ -value=0.033). Also, the random effects specification as a whole is strongly rejected in favor of fixed effects ( $P$ -value=0.000). Indeed, were it not for the inclusion of the school dummies and mother's schooling in column two, the height-for-age coefficient would be much larger (2.822 with a standard error of 0.464) and would differ very significantly from its fixed effects counterpart ( $P$ -value=0.000).

Next we examine the sibling difference specification, Eq. (2). Except for the

addition of a constant term, the ordinary least squares (OLS) estimates in column three are identical to household fixed effects, given two siblings per household in our sample. Notice that the height-for-age coefficient is small and insignificant, just as Behrman and Lavy (1994) find in Ghana using a similar specification. Other curious findings in column three are the insignificant impact of months in the first two grades, the almost significant but negative effect of months repeated in these grades, and the significantly positive effect of months out of school. These estimates could reflect endogeneity bias, which we now address using instrumental variables.

As discussed in Section 2, we use data on the older sibling's height-for-age in the first 2 years of life, specifically the Z-score at birth, 1 year, and 2 years, as instruments.<sup>16</sup> To deal with the other endogenous variables, additional instruments include month of birth dummies for both siblings, the sibling age difference, and the sibling age and sex differences interacted with the barangay level average grade repetition rate.<sup>17</sup> Using month dummies exploits the "natural experiment" created by the minimum primary school enrollment age. In particular, parents whose children are slightly younger than this age cut-off (6.5 years) are "forced" to delay enrollment when they otherwise would choose not to do so. The sibling age gap reflects differences in potential time in school, whereas only actual time in school enters the production function.<sup>18</sup> First-stage regressions for the key endogenous variables are reported in Appendix Table A.2.

Column four of Table 2 shows the two-stage least squares (2SLS) estimates of the production function based on the sibling differences specification. The coefficient on height-for-age is many times larger than the OLS estimate in column three, and the difference between the two is significant ( $P$ -value=0.002). Note

---

<sup>16</sup>Although parents conceivably could have known the older sibling's learning ability prior to his second year of life and made nutritional allocations accordingly, such behavior does not seem to be important for our estimates since we cannot reject the overidentifying restrictions as reported in Table 2. (Note that if parents did know the older sibling's learning ability prior to age two, our model would still be identified because height-for-age at birth surely could not reflect this knowledge). One important caveat, however, is that the statistical power of this test is unlikely to be high.

<sup>17</sup>The justification for these interactions as instruments is that barangays vary in their propensity to make children repeat. In barangays where repetition is common, the larger the age difference between the two siblings the greater the difference in "opportunities" to repeat, so this interaction term predicts differences in repetition across siblings. A similar argument applies for interacting sex differences and the barangay repetition rate, since girls are less likely to repeat than boys.

<sup>18</sup>We assume that birth-spacing is uncorrelated with sibling differences in learning endowments, which would be the case if the learning endowment of the older sibling is not observed until after the younger sibling is conceived. Note that in three studies of birthweight production functions (Rosenzweig, 1986; and Rosenzweig and Wolpin, 1988 and 1995), birth-spacing is taken to be endogenous. Parents are assumed to observe the *health* endowment of the older child immediately after birth, and thus prior to the couple's next conception. Nevertheless, our reading of the evidence from these studies is that the endogeneity bias in the production function is, in general, not statistically important.

also that the other strange findings in column three are reversed after correction for endogeneity, though these estimates become quite imprecise.

Could these very different results be explained away by the fact that the sibling age gap is excluded as an instrument? After all, birth-spacing could capture differences across siblings in unobserved academic inputs, such as parental attention. One indication that this exclusion restriction is valid, albeit not an especially powerful one, is that the full set of instrumental variables easily passes an overidentification test in Table 2. A more convincing check is to re-run the 2SLS regression by including the sibling age difference. However, losing it as an instrument requires imposing restrictions on certain coefficients to aid identification. In particular, the coefficient on time in grades 1 and 2 is constrained to equal that on time in grades 3–6, and months repeated and months not in school are dropped.

The results of this exercise are shown in Appendix Table A.3. In the first column, age enrolled is also dropped. Not only is the sibling age difference not significant, but the height-for-age coefficient is quite close to that reported in Table 2. The second column now includes (endogenous) age enrolled, though this may be asking too much of our instrumental variables. Age in months still does not achieve significance at the 5% level and the height-for-age coefficient is only marginally smaller. Finally, in the third column we drop the sibling age difference variable both as an instrument and as an explanatory variable, with similar results to those in Table 2. We conclude that using the sibling age difference variable as an instrument improves precision without leading to substantial bias. This result is not particularly surprising since most of the variation in the sibling age gap is already captured by the sibling difference in years in school.

What is behind this large bias in the OLS estimate of  $\alpha_H$ ? Recall the two potential problems of endogeneity of nutrition and measurement error in height-for-age. Bias due to endogeneity could go in either direction; if parents use nutrition inputs to reinforce differences in children's learning ability, the estimated impact of height-for-age is biased upward, but, if parents instead attempt to equalize learning outcomes, the bias is downward. Regarding measurement error, height-for-age at the time of school enrollment may be an error-ridden measure of early childhood nutritional status, which is what might really matter for academic performance. As a result,  $\alpha_H$  would be biased toward zero. How large would such measurement error have to be to account for the entire discrepancy between the OLS and 2SLS estimates of the height-for-age coefficient? A rough calculation (that ignores other covariates) indicates that 52% of the total variance in height-for-age Z-scores would have to be noise.<sup>19</sup> Remarkably, a regression (for the index

<sup>19</sup> Assuming that the 2SLS estimate  $\hat{\alpha}_{2SLS}$  is consistent, the asymptotic bias of the OLS sibling difference estimator in the presence of measurement error in height-for-age is  $-\hat{\alpha}_{2SLS}(1-r)/(1-\rho)$ , where  $r$  is the "reliability coefficient" (ratio of true variance to total variance) and  $\rho$  is the sibling correlation in height-for-age Z-scores, which equals 0.50 in our sample.

children) of height-for-age in 1991 on height-for-age at age two, age, and sex produces an  $R^2$  of only 0.49, which leaves just the right amount of noise to explain the discrepancy.

Next, we assess the robustness of the estimates to sample selection bias due to delayed school enrollment of the younger siblings.<sup>20</sup> Column five drops sibling pairs for whom the younger sibling was born in 1987, which are the pairs for whom selection bias may be a problem (see Table 1). The results are quite similar to those in column four, though non-repeated months in first and second grade is no longer statistically significant (observe, though, that much of the variation in this variable comes from the youngest of the younger siblings, who are now excluded from the sample). Overall, selection bias does not appear to be an overriding concern in our data.

Another specification issue we consider is the adequacy of our measure of nutritional status, height-for-age. If achievement is influenced by *current* as well as the early childhood nutritional inputs captured by height-for-age, then our estimates may be biased to the extent that current and past nutrition are correlated. Weight relative to height is an indicator of short-term malnutrition, so we add the sibling difference of body mass index (weight divided by height squared) in 1994 to the column four specification.<sup>21</sup> To do so, we also add the older sibling's *weight* at age 0, 1 and 2 years to the instrument set. Body mass index does not attain significance in the production function ( $t$ -value = 1.50), while the coefficient on height-for-age falls only slightly to 6.98 and remains significant ( $t$ -value = 2.68). In sum, though our power to distinguish the effects of current and past nutrition may be limited (the first-stage  $R^2$  is lower for body mass index than it is for height-for-age), the data appear to support a production function that conditions only on height-for-age. This finding is consistent with the notion that nutrition in the crucial early childhood years is more important for learning than current nutrition.

Lastly, it is worth exploring the extent to which the impact of nutrition on achievement is nonlinear, in contrast to the linear (in  $Z$ -score) specifications just reported. The substantive issue is whether improvements in early childhood nutrition would enhance the academic success of all children in the population or only of the most malnourished. To examine this question, we re-estimate the regression in column 4 of Table 2 by including a spline in height-for-age with a knot at a  $Z$ -score of  $-2.5$  (i.e., two and a half standard deviations below the mean of a healthy population); about a third of the sample is below this threshold. We

---

<sup>20</sup>A different source of selection bias arises from excluding those households in which the mother had only one child at the time of interview. This selectivity is not a problem for our sibling difference estimates if, as assumed above, birth-spacing is uncorrelated with sibling differences in learning endowments.

<sup>21</sup>We could not use the weight for height  $Z$ -score, which is considered preferable to body mass index for measuring young children, because it is not defined for girls older than 10 and for boys older than 11.

find a height-for-age coefficient of 19.6 (standard error=6.18) for those children with Z-scores below  $-2.5$ , but of only 1.38 (5.04) for children with Z-scores above  $-2.5$ . The difference between these coefficient estimates is in the expected direction (larger effects for the more malnourished) and is significant at the 6% level. We also tried setting the knot of the spline at Z-scores of  $-2.0$  and  $-3.0$ , but found no improvement in fit or in significance of the nonlinearity.

Returning to Table 2, our preferred estimate in column four implies that, for a typical child in Cebu, a one standard deviation increase in height raises the achievement test score by 8.9 (2.8) points, which is one-third of the 25.5 point standard deviation of the test score. This direct effect of nutrition on learning productivity per year of school is equivalent to spending about eight extra months in school. Using the nonlinear specification discussed in the previous paragraph shows a much stronger effect for a child with a Z-score below  $-2.5$ ; a one standard deviation increase in height would raise that child's test score by 19.6 (6.18) points, which is equivalent to an additional 18 months (1.8 academic years) of schooling. Yet both these estimates ignore any indirect effects of nutrition on achievement, the topic to which we turn next.

#### *4.2. Indirect impact of nutrition: age of enrollment and grade repetition*

The height-for-age coefficient estimate in Table 2 may not fully capture the impact of nutrition on school performance if nutritional status also influences time spent in school. To assess this indirect effect, we present age of enrollment and first grade repetition regressions in Table 3 based on a 2SLS procedure similar to that used for the production function. The results reported in the first column indicate that one standard deviation increase in height leads parents to enroll their child in school a little more than two months earlier (Glewwe and Jacoby, 1995, find a three month enrollment age response in Ghana).<sup>22</sup> This effect is statistically significant on the full sample, but attenuated on the smaller sample of sibling pairs not subject to selection bias (column two). In either case, the most important determinant of enrollment age, in terms of explained variance, is month of birth. Children born in the later months of a year are far more likely to have delayed enrollment by 1 year; this finding supports the instrumental variables strategy used in Table 2.

The first grade repetition regressions use a slightly smaller sample of sibling pairs, those for which the younger sibling is old enough to have had the opportunity to complete first grade. We limit attention to repetition of first grade

---

<sup>22</sup>Failure to account for the discrete nature of the age of enrollment – namely, that children can only enroll at 12 month intervals – does not appear to be a problem. When we estimate a fixed effects Poisson count model (Hausman et al., 1984) for the number of years enrollment was delayed (without controlling for endogeneity, which cannot be done with this estimator), we obtain a height-for-age effect quite similar to the corresponding OLS (sibling differences) estimator.

Table 3  
 Nutritional status, age of enrollment, and grade repetition: sibling differences-2SLS Estimates<sup>a</sup>

	Age enrolled		Grade repetition	
	Full sample	Reduced sample	Full sample	Reduced sample
Height-for-age <sup>b</sup>	–2.394*	–1.605	–0.123*	–0.113
	(1.051)	(1.157)	(0.059)	(0.062)
Child is female	–0.292	–0.539	–0.028	–0.027
	(0.414)	(0.438)	(0.025)	(0.026)
Age enrolled (months) <sup>c</sup>	–	–	–0.0147*	–0.0162*
			(0.0053)	(0.0058)
Month of birth effects ( <i>P</i> -value)	0.000	0.000	–	–
Overidentification tests ( <i>P</i> -value) <sup>d</sup>	0.702	0.446	0.886	0.966
Number of sibling pairs	1016	864	912	829

<sup>a</sup> Notes: Robust standard errors in parentheses (\* denotes statistical significance at the 0.05 level). All regressions include a constant. The reduced sample drops sibling pairs for which the younger sibling was born in 1987.

<sup>b</sup> Endogenous variable. Excluded instruments: height-for-age of the older sibling at 0, 12 and 24 months.

<sup>c</sup> Endogenous variable. Excluded instruments: differenced month of birth dummy variables.

<sup>d</sup> Test has two degrees of freedom in column one and 12 degrees of freedom in column 2.

because only about half of the younger siblings are old enough to have had the opportunity to repeat second grade. In any case, most repetition occurs at first grade, so this may not be a serious loss of information. The estimates are based on a linear probability model, since there is no straightforward estimator that can simultaneously handle a discrete dependent variable, fixed effects and endogenous covariates. We also control separately for age enrolled and nutritional status, using month of birth dummies as instruments for the former. The results show that malnourished children are more likely to repeat first grade, an effect that is significant at the 5% level. However, conditional on nutritional status, children who delay their enrollment into primary school are significantly less likely to repeat, which is consistent with the positive impact of age of enrollment in the achievement production function. The overall effect (direct effect plus the indirect effect through age of enrollment) of a one standard deviation increase in height is to reduce the probability of repeating first grade by around 9%.

We can combine the results in Tables 2 and 3 to calculate the indirect effect of an improvement in child nutritional status on achievement. Such an improvement will affect the age of enrollment, nonrepeating time spent in school, time spent repeating grades, and time not in school. But since the coefficients on the latter two variables are far from significant in the achievement production function, we ignore them. We also assume in calculating these indirect effects that the child who delays enrollment by 1 year (or repeats a grade) will ultimately complete one less grade; that is, we assume that age of leaving school is fixed. Because we do not

observe final grade attainment for the vast majority of our sample, we cannot tell if children who delay enrollment actually leave school later. If they do, then our estimate of the indirect effect of nutrition on ultimate achievement will be overstated (though, in this case, as will be discussed in Section 5, another cost of malnutrition must be taken into account, namely the earnings foregone by postponing entry into the labor force). With this caveat in mind, a one standard deviation increase in height translates into an increase of 3.0 (1.9) points on the achievement test through the indirect effect, compared to the direct effect of 8.9 points.<sup>23</sup> The total effect of 11.9 points is equivalent to eleven months (1.1 academic years) of school attendance. If, however, we focus on the most malnourished children in our sample and use the spline estimate discussed above, the total effect rises to 22.6 points (19.6+3.0), which is equivalent to 21 months (2.1 academic years) of school attendance.<sup>24</sup>

#### 4.3. Other academic inputs

The production function in Table 2 is quite parsimonious given the richness of our data on academic inputs. However, including more inputs into the production function is problematic because it is hard to find instrumental variables that are correlated with sibling differences in inputs (though we did find some for the time in school variables considered in Table 2). The question remains whether our estimate of the impact of nutrition on achievement is biased due to important omitted inputs. Such bias can go either way, depending, in part, on whether parents compensate or reinforce sibling ability differences in their allocation of academic inputs.

To assess the potential for mis-specification, we test whether a set of observed but omitted academic inputs respond to early nutritional status as measured by height-for-age. If these inputs are not influenced by height-for-age, then no bias in the parameter of interest, namely the impact of nutrition on achievement, will result from omitting them from the production function. If the inputs are influenced by height-for-age, then the sign of the correlation will at least tell us something about the plausible direction of bias in the parameter of interest.<sup>25</sup> Table 4 reports joint estimates of the five input regressions by three-stage least squares (3SLS) to allow a test of the cross-equation restriction that all the coefficients on height-for-age are zero.

---

<sup>23</sup>The standard error (in parentheses) is calculated by the delta method and ignores any covariance between the estimated parameters from the different regressions.

<sup>24</sup>We assume more malnourished children have the same indirect effects as their better nourished counterparts. Lack of precision in the delayed enrollment regression precluded an exploration of nonlinear indirect effects of malnutrition on age of enrollment.

<sup>25</sup>A referee suggested that current academic inputs may be more responsive to current malnutrition than to past malnutrition. While this may be true, our purpose here is only to assess whether past malnutrition is correlated with omitted academic inputs.

Table 4  
Nutritional status and academic inputs: sibling differences – 3SLS estimates<sup>a</sup>

	Reading frequency	Hours of homework	Help with homework	% Days absent	Years of pre-school
Height-for-age <sup>b</sup>	–0.198* (0.085)	0.071 (0.061)	0.012 (0.054)	0.0041 (0.0065)	0.109 (0.070)
Child is female	–0.084* (0.038)	0.075* (0.027)	–0.103* (0.024)	–0.0141* (0.0029)	0.030 (0.031)
Age child tested (years)	–0.193* (0.033)	–0.003 (0.024)	–0.055* (0.021)	–0.0001 (0.0025)	–0.079* (0.027)

<sup>a</sup> Notes: Robust standard errors in parentheses (\* denotes statistical significance at the 0.05 level). All equations include a constant term and are estimated on a sample of 967 sibling pairs. Joint test of height-for-age coefficient in all equations: ( $P$ -value=0.071).

<sup>b</sup> Endogenous variable. Excluded instruments: height-for-age of the older sibling at birth, 12 months, and 24 months.

Before reporting the result of this test, we discuss the input regression results individually. Reading frequency is a categorical variable. About 17 (25)% of the index children (younger siblings) are read to regularly (weekly or more often) by someone in the household, 23 (30)% are read to occasionally, and the rest are never read to. According to the estimates in the first column of Table 4, malnourished children do appear to be read to more frequently, with the effect significant at the 5% level. As for homework, an activity which occupies the average child in our sample for less than an hour each school day, we find no evidence that nutritional status affects the number of hours spent doing homework (column two). Nor does nutritional status significantly influence whether the child receives assistance with homework from a parent, sibling, or other household member (column three); 68% of the index children and 83% of their younger siblings do receive such help. Turn now to school absenteeism, which is very low in our sample. Rather than rely on self-reported school attendance, actual attendance records for at least one full semester were gathered at the schools for each child (missing values for school attendance lower our overall sample to 967 sibling pairs). Our data indicate that only about 3.5% of days are missed, which corresponds quite closely to the self-reported attendance information collected from mothers. In any case, the estimates in column four show that absenteeism is unrelated to nutritional status. Lastly, consider preschool/kindergarten enrollment. About 40% of index children and 45% of their younger siblings attended a preschool and/or a kindergarten for at least 1 year, and usually for no more than 2 years. The final column in Table 4 shows no significant relationship between nutritional status and preschool enrollment.<sup>26</sup>

<sup>26</sup> All the regressions shown in Table 4 were also re-estimated after dropping sibling pairs for which the younger sibling was born in 1987. The results, which are not reported here, are very similar to those shown in Table 4.

Taking all five inputs together, we fail to reject the joint hypothesis that the coefficients on height-for-age are zero at the 5% significance level, though we do reject it at the 10% level (see notes to Table 4). Based on this finding, it appears doubtful that omitted inputs, at least the ones we have information on, seriously bias our estimate of the height-for-age coefficient in the achievement production function. If anything, the result for reading frequency suggests that our estimate of this coefficient might be downward biased, since parents may be compensating their malnourished children by spending more time reading to them.

## **5. Policy implications of findings**

It remains for us to assess the economic significance of the nutrition–learning nexus. It is one thing to say that better nutrition significantly improves school performance, but quite another thing to say that this education effect should seriously enter into the cost–benefit calculus of nutrition interventions, let alone wholly justify such interventions as some nutritionists have argued. In this section, we use our production function results to estimate the achievement benefits of a supplementary child feeding program, both in terms of equivalent months of schooling and in dollars, taking into account both the direct and indirect effects (through time in school).

The track record of nutritional supplementation programs in developing countries has been mixed. However, a few large-scale feeding programs have been studied using longitudinal data with controls and show significant improvements in anthropometric outcomes (Beaton and Ghassemi, 1982). Here we focus on one successful experimental feeding program in India, the costs and growth benefits of which were both carefully monitored. The Narangwal project (Kielmann and Associates, 1983) provided nutritional supplements for children less than 3 years of age, along with intensive health surveillance and nutrition education, in several Punjabi villages from 1968 to 1973. Kielmann and Associates estimate that for children ages 2–3 average heights improved by about 2 cm in villages receiving the nutrition intervention relative to control villages, which translates into a Z-score improvement of 0.6 standard deviations. This estimated impact is statistically significant.

If such a nutrition program had an equivalent impact on height in Cebu<sup>27</sup> and there was no change in growth trajectory after the program ended, then supplements to the average child in Cebu would improve his or her test scores by the equivalent of six months in school, a quarter of which would be through the indirect effect. If supplements are given only to malnourished children (i.e., those

---

<sup>27</sup>A lower impact might be expected in Cebu because nutritional status of children in our sample is somewhat better than in the Narangwal population. At age two, boys in Cebu are about 2 cm taller and girls 4 cm taller on average than their Narangwal counterparts.

with a Z-score less than  $-2.5$ ), the impact would be larger, equivalent to about 12 months (1.2 academic years) of schooling. These improvements suggest that educational spillovers could very well, on their own, justify nutritional supplementation, but belie the more dramatic claims of some nutritionists that malnourished children would be “intellectually crippled” without such interventions (Brown and Pollitt, 1996). Still, these educational benefits need to be translated into a dollar value and compared to program costs.

The benefit calculation depends upon how improved nutrition affects school attainment. In one scenario, academic achievement is held constant after the nutrition program, but children can be withdrawn from school and put to work earlier than would be the case without the program. Based on the average hourly wage in our adult sample (16.5 Pesos per hour) and a 2000 hour work-year, an untargeted nutrition program would give the average child an extra half year of earnings, worth 16,500 Pesos (about \$650 in 1994 dollars). However, this benefit must be discounted because it occurs about 15 years after the costs of the nutrition program are incurred (assuming typical labor force entry at age 16). Using a 3% real interest rate yields a benefit of about \$415, while using a 5% real interest rate yields a benefit of about \$310. A nutrition program targeted toward malnourished children would double these benefit figures.

In a second scenario, the age of school leaving is unchanged by the program, but the average child leaves school with additional human capital equivalent to six months worth of schooling for an untargeted program and 12 months for a targeted program. A standard wage function estimated on a sample of 3000 wage earners from our CLHNS data (mainly the parents of the children in our production function analysis), yields a rate of return on an additional year of schooling of about 7%. Thus, a nutrition program directed to the average child would raise yearly earnings by 1440 pesos. Again assuming labor force entry at age 16, and a working life of 45 years, the present discounted value of this wage increase is about \$480 using a 5% interest rate, and \$920 using a 3% interest rate. The corresponding figures for a targeted nutrition program are \$960 and \$1840, respectively.

Based on these admittedly crude calculations, we find that an untargeted nutrition intervention that can achieve an average improvement in height-for-age of 0.6 of a standard deviation at a cost of about \$300 or less per child would be justified on the education benefits alone, while a targeted program costing less than \$600 per child would be justified. Kielmann and Associates put the total cost (including capital, labor, and transport) of feeding and monitoring a child in the Narangwal program for the two-year period necessary to realize the above height gain at about \$150 (in 1994 dollars). However, food costs are estimated to comprise about a third of this total. If the value of the food itself is ignored because it is only a redistributive transfer, the actual program cost per child would be around \$100. Therefore, for every dollar spent on a Narangwal-type feeding program in Cebu, we estimate a labor market return of at least \$3, and possible as

much as \$18, from just the gain in academic achievement. Of course, there are other potential benefits of nutrition supplementation besides educational ones (e.g., lower mortality), but these are much more difficult to put in dollar terms; indeed, our analysis suggests that, as far as making a policy decision is concerned, such calculations may not even be necessary.

A final point is that Cebu has an exceptionally high primary school enrollment rate. In countries where nonenrollment is pervasive, better nutrition might encourage entry into school and this extensive margin would have to be taken into account on the benefit side of the calculation. On the other hand, in developed countries, where the incidence of stunting is low even among the poor, the scope for improving achievement through nutrition interventions is likely to be much more limited.<sup>28</sup>

## **6. Conclusion**

In this paper, we have studied the relationship between early childhood nutrition and subsequent academic achievement using a unique longitudinal data set, one that follows a large sample of children in a low income country from birth to the end of their primary school education. Several important empirical findings emerge from this analysis. First, heterogeneity in learning endowments, home environment, or parental “tastes” for that matter, cannot fully explain why malnourished children perform relatively poorly in school. The positive relationship between nutrition and achievement persists even after controlling for these factors. Our results thus support a causal link between nutrition and academic success, though arguably a definitive answer to the causality question is only possible from an ideal (and consequently improbable) empirical experiment. Second, height-for-age measured at or beyond the time of school enrollment seems to be a noisy indicator of early childhood nutritional status. Thus, the availability of anthropometric data in the first 2 years of life proves to be extremely valuable. Third, there does not seem to be a strong connection between early childhood nutrition and learning effort, such as homework time and school attendance. However, we do find evidence that the primary school enrollment of malnourished children tends to be delayed, probably because they are deemed unready for school at the minimum age of enrollment. Lastly, a rather simple cost–benefit analysis suggests that, in a country like the Philippines, a dollar invested in an early childhood nutrition program could potentially return at least three dollars worth of gains in academic achievement, and possibly much more.

---

<sup>28</sup>Currie and Thomas (1995) find positive effects of participation in the Head Start program on the academic performance of white children, but not black children. However, since they find no significant effect of program participation on height-for-age, the improvements in academic performance are unlikely to be the result of the nutritional component of Head Start.

## Acknowledgements

Financial support for this project was provided by USAID, the Asian Development Bank, and the World Bank (RPO #679-57). We wish to thank the Office of Population Studies at the University of San Carlos in Cebu, Philippines and the Carolina Population Center at the University of North Carolina for their collaboration in the collection and analysis of the data. Azot Derecho and Nauman Ilias provided capable research assistance. We are also grateful to Harold Alderman, Andrew Foster, Michael Kremer and two anonymous referees for comments on previous drafts of this paper. The second author would like to thank the International Food Policy Research Institute for their hospitality during the writing of this paper. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank, its Executive Directors, or the countries they represent.

## Appendix A

Table A.1. Sample attrition and selection

Live births in 33 sample barangays of metro Cebu		3289	
Of which:	Twin births	27	(0.8%)
	Refusals	97	(2.9%)
	Missed by survey (discovered later)	58	(1.8%)
	Birth interview too late	22	(0.7%)
Live births in metro Cebu with birth interview		3085	
Of which:	Migrated out of metro Cebu by age of 2	318	(10.3%)
	Child died by age 2	156	(5.1%)
	Refusal (at later date)	50	(1.6%)
Still in sample when child is 2 years old		2561	
Of which:	Migrated out of metro Cebu by age 8	155	(6.1%)
	Could not find child at age 8	137	(5.3%)
	Child Died by Age 8	38	(1.5%)
Still in sample when child is 8 years old		2231	
Of which:	Migrated Out/Could Not Find <sup>a</sup>	31	(1.4%)
	Child died	8	(0.4%)
Still in sample when child is 11 years old		2192	
Of which:	Never enrolled in school	9	(0.4%)
	Not tested (e.g. refusal)	13	(0.6%)
	Does not have younger sibling of school age	931	(42.4%)
Sample with younger children of school age		1239	

Of which:	Younger sibling not in school	90	(7.3%)
Sample of sibling pairs with both siblings in school		1149	
Of which:	Missing 1991 height data for older sibling	13	(1.1%)
	Missing 1983–1986 height data for older sibling	92	(8.0%)
	Missing height data for younger sibling	23	(2.0%)
	Missing test score variables	5	(0.4%)
Observations with complete data		1016	(88.4%)

<sup>a</sup> This figure of 31 children lost between 8 years and 11 years is a net figure. In fact, 77 children interviewed in 1991–1992 could not be located in 1994–1995. However 46 children in the 1983–1986 sample who were not found in 1991–1992 were found in 1994–1995.

Table A.2. First-stage sibling differences regressions<sup>a</sup>

	Months in 1st–2nd grade	Months in 3rd–6th grade	Age enrolled	Height-for- age Z-score
Child is female	0.230 (0.359)	–0.861 (0.728)	–0.453 (0.671)	0.166* (0.081)
Age	16.112* (1.392)	–8.220* (3.443)	5.584 (3.063)	–0.683* (0.323)
Age <sup>2</sup>	–0.696* (0.071)	0.766* (0.177)	–0.281 (0.157)	0.028 (0.017)
Repetition rate × age	3.219 (2.963)	–34.015* (5.271)	7.664 (5.158)	–1.535 (0.520)
Repetition rate × female	5.594 (9.688)	51.669* (20.176)	–5.672 (18.133)	2.080 (1.855)
Index child Z-scores at:				
Birth	–0.389 (0.411)	–1.971 (1.065)	2.082* (0.896)	–0.098 (0.106)
12 months	0.321 (0.450)	0.836 (1.261)	–1.824 (1.195)	0.337* (0.138)
24 months	–0.663 (0.452)	1.697 (1.168)	0.905 (1.131)	0.059 (0.120)
Age × Z-scores at:				
Birth	0.129 (0.181)	0.715 (0.438)	–0.827* (0.344)	0.067 (0.042)
12 months	–0.083 (0.225)	–0.105 (0.533)	0.597 (0.502)	–0.114* (0.056)
24 months	0.238 (0.216)	–0.252 (0.516)	–0.533 (0.511)	0.056 (0.049)
<i>F</i> -test: index child month of birth dummy variables	1.04 [0.408]	3.47* [0.000]	5.05* [0.000]	0.95 [0.495]
<i>F</i> -test: younger sibling month of birth dummy variables	1.56 [0.105]	1.94* [0.032]	3.81* [0.000]	0.120 [0.280]

<sup>a</sup> Notes: Robust standard errors in parentheses and *P*-values of *F*-tests in brackets (asterisks denote statistical significance at 0.05 level). Sample size is 1016 pairs for all regressions.

Table A.3. Sensitivity analysis of 2SLS results<sup>a</sup>

Height-for-age Z-score	8.730*	6.897*	8.973*
	(2.869)	(3.060)	(2.954)
Child is female	3.550*	3.395*	3.768*
	(1.193)	(1.222)	(1.232)
Age Enrolled	–	1.236*	0.722
		(0.491)	(0.418)
Months in 1st–6th grade (non-repeated)	1.001*	2.015*	1.318*
	(0.316)	(0.528)	(0.377)
Age (months)	0.020	–0.814	–
	(0.254)	(0.429)	

<sup>a</sup> Notes: See notes to Table 2. All regressions use sibling differences.

## References

- Beaton, G.H., Ghassemi, H., 1982. Supplementary feeding programs for young children in developing countries. *American Journal of Clinical Nutrition* 35, 864–916.
- Behrman, J.R., 1996. The impact of health and nutrition on education. *World Bank Economic Review* 11 (1), 23–37.
- Behrman, J.R., Lavy, V., 1994. Child health and schooling achievement: Causality, association and household allocations. *Living Standards Measurement Study Working Paper No. 104*. The World Bank, Washington D.C.
- Brown, J.L. Pollitt, E., 1996. Malnutrition, poverty and intellectual development. *Scientific American*, February, 38–43.
- Cravioto, J., Arrieta, R., 1986. Nutrition, mental development and learning. In: Falkner, F., Tanner, J. (Eds.), *Human Growth: A Comprehensive Treatise*. Plenum Press, New York.
- Currie, J., Thomas, D., 1995. Does Head Start make a difference? *American Economic Review* 85, 341–364.
- Dobbing, J., 1984. Infant nutrition and later achievement. *Nutrition Reviews* 42 (1), 1–7.
- GAO (US General Accounting Office), 1992. Early intervention: Federal investments like WIC can produce savings. *GAO/HRD-92-18*, Washington, D.C.
- Glewwe, P., Jacoby, H., 1995. An economic analysis of delayed primary school enrollment in a low income country: The role of childhood nutrition. *Review of Economics and Statistics*, February 77 (1), 156–169.
- Grantham-McGregor, S.M., Walker, S.P., Chang, S.M., Powell, C.A., 1997. Effects of early childhood supplementation with and without stimulation on later development in stunted Jamaican children. *American Journal of Clinical Nutrition* 66, 247–253.
- Hanushek, E., 1986. The economics of schooling. *Journal of Economic Literature* 24, 1141–1177.
- Hausman, J., Hall, B.H., Griliches, Z., 1984. Econometric models for count data with an application to the patents-R&D relationship. *Econometrica* 52 (4), 909–938.
- Kielmann, A.A., & Associates, 1983. *Child and Maternal Health Services in Rural India: The Narangwal Experiment*. Johns Hopkins University Press, Baltimore MD.
- Levitsky, D., Stropp, B., 1995. Malnutrition and the brain: Changing concepts, changing concerns. *Journal of Nutrition* 125 (85), 2211S–2220S.
- McCall, R.B., 1979. The development of intellectual functioning and the prediction of later IQ. In: Osofsky, J.D. (Ed.), *Handbook of Infant Development*. Wiley, New York.
- Martorell, R., Khan, L.K., Schroeder, D., 1994. Reversibility of stunting: epidemiological findings in children from developing countries. *European Journal of Clinical Nutrition* 48 (1S), S45–S57.

- Pollitt, E., Gorman, K., Engle, P., Martorell, R., Rivera, J., 1993. Early Supplemental Feeding and Cognition. Monographs of the society for Research in Child Development. University of Chicago Press.
- Rosenzweig, M.R., 1986. Birth spacing and sibling inequality: Asymmetric information within the family. *International Economic Review* 27, 55–76.
- Rosenzweig, M.R., Wolpin, K.I., 1988. Heterogeneity, intrafamily distribution and child health. *Journal of Human Resources* 23, 437–461.
- Rosenzweig, M.R., Wolpin, K.I., 1994. Are there increasing returns to the intergenerational production of human capital? Maternal schooling and child intellectual achievement. *Journal of Human Resources* 29 (2), 670–693.
- Rosenzweig, M.R., Wolpin, K.I., 1995. Sisters, siblings, and mothers: The effect of teen-age childbearing on birth outcomes in a dynamic family context. *Econometrica* 63, 303–326.
- Siegel, L., 1989. A reconceptualization of prediction from infant test scores. In: Bornstein, M.H., Krasnegor, N.A. (Eds.), *Stability and Continuity in Mental Development: Behavioral and Biological Perspectives*. Lawrence Erlbaum Associates, Hillsdale, NJ.
- Stein, Z., Susser, M., Saenger, G., Marolla, F., 1975. *Famine and Human Development: The Dutch Hunger Winter of 1944–45*. Oxford University Press, New York.
- World Bank, 1993. *World Development Report 1993: Investing in Health*. Oxford University Press.
- Young, M.E., 1996. *Early Childhood Development: Investing in the Future*. World Bank, Washington, DC.