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EARLY CHILDHOOD NUTRITION AND ACADEMIC ACHIEVEMENT: A LONGITUDINAL ANALYSIS

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ABSTRACT

Early childhood nutrition is thought to be an important input into subsequent academic achievement. This paper investigates the nutrition-learning nexus using a unique longitudinal data set, which follows a large sample of Philippine children from birth until the end of their primary education. We find that malnourished children perform more poorly in school, even after correcting for the effects of unobserved heterogeneity both across and within households. 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 appears to stem from greater learning productivity per year of schooling rather than from greater learning effort in the form of homework time, school attendance, and so forth. Despite these findings, our analysis suggests that the relationship between nutrition and learning is not likely to be of overriding importance either for nutrition policy or in accounting for economic growth.

CONTENTS

TABLES

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1. INTRODUCTION

Economists have long had a keen interest in the relationship between income and nutrition. One strand of research focuses on how income influences food consumption and, consequently, nutritional status (Behrman and Deolalikar 1987; Bouis and Haddad 1992; Subramanian and Deaton 1996). A second strand of research reverses the direction of causation, examining how nutritional status affects income via labor productivity (Leibenstein 1957; Stiglitz 1976; Gersovitz 1983; Strauss 1986). More broadly, this literature asks whether inadequate nutrition has constrained economic growth, historically in developed countries (Fogel 1994) and currently in developing countries (Dasgupta 1993).

In this paper, we consider a different route through which nutritional status affects income. Rather than investigate whether malnutrition among *adults* diminishes *physical effort*, we consider whether malnutrition among *young children* impedes their acquisition of *academic skills*. The possibility of a strong connection between nutrition and learning is of growing importance. As a result of technological progress, labor productivity more than ever depends on academic skills (see Bartel and Lichtenberg 1987). Recent empirical findings confirm a positive relationship between wages and academic achievement, as measured by test scores, in both developed (Murnane, Willett, and Levy 1995; Neal and Johnson 1996) and developing (Boissiere, Knight and Sabot 1985;

Glewwe 1996) countries. Based on these findings, if malnutrition does indeed hamper school performance, then economic growth and improved nutrition are mutually reinforcing, supporting Fogel's general thesis.

In recent years, policymakers 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 U.S. 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 success (see Brown and Pollitt 1996). Their view is that, in addition to having direct health benefits, early childhood nutrition programs 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.[1] But because of the ethical and practical issues raised by this “ideal” experiment, it has yet to be carried out and perhaps never will be.[2]

We estimate the impact of nutrition on learning using nonexperimental data collected in Cebu, Philippines, over a period of twelve years. A large sample of children were surveyed 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.

Though we will argue shortly that no nonexperimental study can hope to replicate the ideal experiment described above, we believe that our analysis makes considerable progress in sorting out the casual relationship between nutrition and learning. In particular, we find that better early childhood nutrition raises academic achievement. Our

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[1] 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.

[2] The INCAP study in Guatemala probably comes closest to such an experiment, at least in terms of length of follow-up. Pollitt et al. (1993) find significant effects of early childhood supplementary feeding on various measures of cognitive skills in adolescence. The experiment has two drawbacks, however: (1) it compares only two distinct nutritional interventions, each in only two villages, without a pure control group; and (2) the nutritional supplements were provided in a central feeding station, so that take-up was *voluntary*. Thus, within each of the four villages, participation in the treatment group was not random. For a general critique of the ability of experiments to produce clear-cut results, see Heckman and Smith (1995).

analysis also illuminates the pathways through which nutritional status 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 nutritional status influences learning effort in the form of school attendance, homework, and so forth.

Before turning to these results in Section 4, we lay out a framework for estimation in Section 2 and describe our data in Section 3. We discuss the economic significance of our findings in Section 4 and conclude in Section 5.

2. EMPIRICAL STRATEGY

AN ACHIEVEMENT PRODUCTION FUNCTION

Our main interest lies in 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. This production function answers the ceteris paribus question of how early childhood nutrition influences subsequent academic achievement. It is distinct from the "reduced form" relationship between achievement and nutrition, which does not control for other academic inputs, in that it is far more generalizable to other environments; in particular, the reduced form effect of nutrition on achievement may vary across environments because parents may differ in how they adjust other academic inputs in response to malnutrition.

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.[3]

To highlight our informational assumptions, we divide a child's life into three periods. Period 0 consists of (approximately) the first two years of life, which is thought to be the most crucial stage of postnatal development (see Cravioto and Arrieta 1986; Dobbing 1984; Levitsky and Stropp 1995). Period 1 is from two 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

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

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 history up until the end of that time period, and Z_{2k} is a vector of other inputs that influence academic performance.[4]

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[3] 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.

[4] 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 equation (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 equation (1) and therefore harder to detect.

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 η that is common among siblings i and j, and a component ϵ_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 ϵ_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 η.

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, η and all the ϵ_k's that have been "realized" up to that point; this leads to simultaneity bias in equation (1). On the other hand, the ϵ_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 $E[H_{0k}\epsilon_k] = 0$. This informational assumption is consistent with the conclusions of the psychology literature on child intelligence.[5]

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[5] 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)." 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 make nutritional allocations accordingly, prior to age two.

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

$$\Delta A_{2k} = \alpha_H \Delta H_{1k} + \alpha_Z' \Delta Z_{2k} + \Delta\epsilon_k \,, \tag{2}$$

where $\Delta A_{\theta k} = A_{\theta i} - A_{\theta j}$, 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, $E[\Delta H_{1k} \Delta\epsilon_k] \neq 0$; 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 two years of life, as an instrument for ΔH_{1k}.[6] Under our informational assumptions, this instrument is uncorrelated with both ϵ_i and ϵ_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

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[6] 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 U.S. study of preschool 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.

subsequent nutrition is less important or, in the extreme case, unimportant, then equation (2) could lead to an underestimate of the true impact of nutrition on achievement. In the extreme case, we can view H_{1k} as a noisy indicator of early childhood nutrition, and this "measurement error" in the nutrition indicator would lead to attenuation bias in equation (2). Of course, the best way of determining the true impact of period 0 nutrition would be to replace ΔH_{1k} by ΔH_{0k} in equation (2), but, unfortunately, in our data set, anthropometric measurements in early childhood (prior to age two) are available only for the older sibling. Nevertheless, our instrumental variables strategy corrects for the measurement error bias, provided that child physical *growth* after age two is uncorrelated with H_{0i}.[7]

A criticism of our basic identifying assumption, $E[H_{0i}\Delta\epsilon_k] = 0$, is that H_{0i} and ϵ_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 stature and innate 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 $E[H_{0i}\Delta\epsilon_k] \neq 0$, 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 (1) of sufficient magnitude and

[7] Such would not be the case if there were significant catch-up growth after age two, the possibility of which is still an unresolved issue in the nutrition literature (Martorell, Khan, and Schroeder 1994).

persistence to affect a child's stature (2) sufficiently variable across households, and (3) sufficiently transitory *not* to affect the sibling's stature would be nothing short of miraculous.[8,9] So, there does not seem to be any hope of replicating the ideal experiment laid out in the introduction with a natural experiment.[10] 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 ϵ_i, leading to an overestimate of α_H. Thus, in the presence of important physiological shocks, our estimate of α_H may be viewed as an upper bound on the impact of nutrition on achievement.

DELAYED ENROLLMENT AND SELECTION BIAS

An important feature of our data is delayed primary school enrollment; many children enter school after the minimum age at which they are allowed to enroll. This phenomenon is not unique to the Philippines, but has been noted in other low income

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[8] 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.

[9] The condition that the shock *differentially* affect the siblings' stature would not be necessary if equation (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.

[10] 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.

countries (see Glewwe and Jacoby 1995). The implications of delayed enrollment for the estimation of achievement production functions are twofold. 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 appear as one element of the vector Z_{2k} in equation (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.

To deal with this selection bias, we must first specify the equation for delayed enrollment for the younger sibling j.[11] Let D_j denote the number of months after the minimum age of school enrollment before child j actually enrolls. Ignoring factors other than nutritional status for the moment, a linearized dynamic decision rule for D_j can be written as

$$D_j = \beta_{Ho} H_{1j} + \beta_{Hc} H_{1i} + \beta_{\epsilon o} \epsilon_j + \beta_{\epsilon c} \epsilon_i + \beta_\eta \eta \,. \qquad \textbf{(3)}$$

[11] In our sample of sibling pairs, it is only the younger sibling who could conceivably not be in school because of delayed enrollment.

The coefficient subscripts *o* and *c* indicate own and sibling cross-effects of early childhood nutrition and the learning endowment on child *j*'s delay decision. Now let T_j be child *j*'s current age, measured as the number of months since reaching the minimum age of enrollment. The selection bias correction is derived as follows:

$$\begin{aligned} E[\Delta A_{2k}|\text{child } j \text{ enrolled}] &= \alpha_H \Delta H_{1k} + \alpha_Z' \Delta Z_{2k} + E[\Delta\epsilon_k | D_j < T_j] \\ &= \alpha_H \Delta H_{1k} + \alpha_Z' \Delta Z_{2k} + E[\Delta\epsilon_k | \beta_{\epsilon o}\epsilon_j + \beta_{\epsilon c}\epsilon_i + \beta_\eta \eta < T_j - \beta_{Ho} H_{1j} - \beta_{Hc} H_{1i}] \, . \\ &= \alpha_H \Delta H_{1k} + \alpha_Z' \Delta Z_{2k} + E[\Delta\epsilon_k \,|\, \nu_j < T_j - \tilde{\beta}_{Ho} H_{1j}| - \tilde{\beta}_{Hc} H_{1i}] \end{aligned} \qquad \textbf{(4)}$$

The last line of equation (4) requires explanation. Since H_{1j} and H_{1i} are correlated with ϵ_j, ϵ_i and η, single equation methods will not produce consistent estimates of the parameters of the selection rule. But here we are interested only in correcting for selection bias, not in recovering the structural parameters of the selection rule. Following Chamberlain (1980), we therefore assume that $E[\beta_{\epsilon o}\epsilon_j + \beta_{\epsilon c}\epsilon_i + \beta_\eta \eta | T_j, H_{\Theta j}, H_{\Theta i}]$ is a linear function of the conditioning variables, and use this assumption to decompose the terms involving ϵ_j, ϵ_i and η, in the second line of equation (4). The error term ν_j is still correlated with $\Delta\epsilon_k$, but not (by construction) with the regressors in the selection rule. Given a joint distribution for $\Delta\epsilon_k$ and ν_j, we can replace the conditional expectation in the last line of equation (4) by its estimate (e.g., an inverse Mills' ratio), with the age of the younger sibling *j* (T_j) serving to identify the selection bias.

3. DATA AND SAMPLE

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.[12] 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 3,289 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 child, were gathered every two months for the first two years of the child's life, along with household and community-level information. Follow-up surveys were conducted in 1991-92, when these "index" children (to distinguish them from their younger siblings) were about eight years old, and in 1994-95, when they were about 11 years old. Both follow-up surveys collected anthropometric data, and in our empirical analysis, we use the height-for-age Z-score taken closest to the time of the school enrollment decision. For index children, this is the 1991-92 Z-score and for younger siblings, the 1994-95 Z-score.

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[12] 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).

The 1994-95 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 1994-95, his or her younger sibling. Information includes the year the child first enrolled in school, grade repetition by grade, 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 2,192 index children still in the sample in 1994-95,[13] 1,239 have a younger sibling of school age; that is, who are at least six and a half years old by June 1994, the start of the 1994-95 school year. Of these sibling pairs, 1,149 of the younger siblings were enrolled in school by June 1994 and the remaining 90 had not yet enrolled. After dropping observations with missing data, we are left with a sample of 1,016 enrolled sibling pairs for the production function estimation.

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[13] 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 permanent migration out of the Metro Cebu area and death. About 72 percent of the original children were resurveyed in the 1991-92 follow-up, and the sample declined only slightly for the 1994-95 follow-up. See Appendix Table 6 for details.

DESCRIPTION OF KEY VARIABLES

Table 1 describes schooling outcomes for our sample in 1994–95. Virtually all index children had enrolled in primary school by age 11 (the nine who did not were dropped from the sample). Younger siblings born in 1984–1986 had enrollment rates upward of 95 percent, while only about three-quarters of those born in 1987 had enrolled by the 1994–95 school year.

Table 1 School enrollment, repetition, and grades completed

	Index Children		Younger Siblings			
Year born	**1983**	**1984**	**1984**	**1985**	**1986**	**1987**
Number of children	917	322	98	481	443	217
Percent ever enrolled in first grade	100	100	95.9	97.1	95.3	76.5
Delayed enrollment:						
Percent ever delayed	21.2	11.2	19.1	21.6	18.5	0
Number of years delayed (mean for those who delayed)	1.3	1.3	1.3	1.3	1.2	--
Grade repetition:						
Percent ever repeat any grade	29.3	18.9	23.5	15.2	8.4	2.3
Percent 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
Percent dropped out	5.3	4.4	2.0	4.4	2.9	4.6

Notes: The number of children and enrollment figures are based on a sample of 1,239 sibling pairs for which the index child could be matched to a younger sibling of school age. They exclude 9 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 1994-95 school year.

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 seven and a half years of age or older when he enters first grade. Table 1 shows that about 19 percent 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 also indicates that grade repetition is pervasive in Cebu, with 27 percent 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 percent 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 U.S. population.

4. EMPIRICAL RESULTS

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,[14] 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. 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

[14] 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.

Table 2 Achievement production function

			Sibling differences		
	Mean (Standard Deviation)	**GLS[a]**	**OLS**	**2SLS[b]**	**2SLS[b]**
Height-for-age Z-score	-1.98 (0.95)	1.336* (0.457)	0.310 (0.664)	8.891* (2.826)	4.979* (2.156)
Child is female	0.48 (0.50)	5 .284* (0.748)	6.050* (0.941)	3.735* (1.210)	4.831* (1.109)
Age enrolled (months)	85.45 (6.62)	0.204* (0.064)	0.345* (0.102)	0.555 (0.345)	0.634* (0.322)
Months in 1st to 2nd grade (non-repeated)	18.55 (3.52)	0.269* (0.120)	0.167 (0.149)	1.053* (0.454)	1.374* (0.526)
Months in 3rd to 6th grade	14.68 (12.54)	1.133* (0.038)	1.033* (0.088)	1.105* (0.256)	1.202* (0.237)
Months repeated in 1st and 2nd grade	2.01 (5.01)	-0.302* (0.080)	-0.211 (0.110)	0.102 (0.593)	-0.342 (0.544)
Months not in school	6.56 (4.83)	0.219* (0.097)	0.326* (0.135)	-0.699 (1.005)	-0.272 (0.950)
Mother's years of schooling	6.89 (3.26)	1.575* (0.167)	--	--	--
Mills' ratio	--	--	--	--	-24.724* (8.39)
Overidentification test: $\chi^2_{(10)}$ p-value	--	--	--	0.724	0.392

Notes: Standard errors in parentheses (asterisks denote statistical significance at the 0.05 level). All regressions include a constant and are based on a sample of 1016 sibling pairs. Mean achievement test score is 25.5 (25.5).

[a] Household random effects specification also includes a full set of school dummy variables.

[b] All variables endogenous except sex (and Mills' ratio). 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.

specifications (equation [2]) because almost all sibling pairs attend the same primary school.[15]

To assess the endogeneity and selection issues, we examine a series of estimators of the achievement production function. Column two reports a household random effects (GLS) specification of equation (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, equation (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

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[15] 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).

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 two years of life, specifically the Z-score at birth, at one year, and at two years as instruments.[16] To deal with the other endogenous variables, additional instruments include month of birth dummies for both siblings, the sibling age difference, and interactions of this age difference and the sex difference with the barangay level average grade repetition rate.[17] 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 cutoff (6.5 years) are "forced" to delay enrollment when

[16] 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.

[17] We assume that the birth-spacing of the siblings is uncorrelated with differences in their learning endowment, 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 birth weight 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.

they otherwise would choose not to do so. The full set of instrumental variables easily passes an overidentification test in Table 2.

Column four of Table 2 reports our first set of two-stage least squares (2SLS) estimates of the production function. The coefficient on height-for-age is many times larger than the OLS estimate, and the difference between the two is significant (p-value = 0.002). What is behind this large bias in the OLS estimate? Recall that 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. How large would such measurement error have to be to account for the entire OLS-2SLS discrepancy in the height-for-age coefficient? Assuming white noise measurement error, a rough calculation (that ignores other covariates) indicates that 52 percent of the total variance in height-for-age Z-scores would have to be noise.[18] Remarkably, a regression (for the index 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. Note also that the other strange findings in column three are reversed after correction for endogeneity, though the estimates do become quite imprecise.

[18] 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 ρ is the sibling correlation in height-for-age Z-scores, which equals 0.50 in our sample.

Lastly, we correct for selection bias due to delayed school enrollment of the younger siblings.[19] Column five includes the Mills' ratio, under the assumption of jointly normal errors.[20] Not only is the Mills' ratio significant at the five percent level, but the height-for-age coefficient estimate falls to almost half of its magnitude in column three, though it remains significant. Evidently, selection into the sample depends in part upon nutritional status. Selection bias appears to be important despite the fact that fewer than a hundred sibling pairs are dropped from a sample of over 1,200. Note also that the age of enrollment attracts a positive and statistically significant coefficient in column five. Thus, conditional on nutritional status, children do appear to benefit by delaying their entry into school.

Summing up then, our preferred estimate in column five implies that a one standard deviation increase in height raises the achievement test score by 5.0 (2.2) points, which is one-fifth 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 four extra months in school. But there may also be indirect effects of nutrition on achievement, which we turn to next.

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[19] 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.

[20] Probits used to obtain the Mills' ratio are available upon request. Month and year of birth dummies and height-for-age of the younger siblings are all highly significant determinants of nonenrollment. Standard errors in Table 2 and below are not adjusted parametrically, but we instead report Huber/White standard errors as a nonparametric approximation to the appropriate adjustment.

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 using the same econometric procedure as in column five of Table 2. Before getting to the results, there are two caveats to mention in interpreting these regressions. First, if the decision rule for delayed school enrollment (and grade repetition) takes the form of equation (3), and this equation is differenced across siblings, then the coefficient on ΔH_{1k}, β_{Ho} - β_{Hc}, reflects both the own and sibling cross effects of nutritional status; the two effects are not separately identified. In the calculations below, we assume that the cross effects are negligible, which is probably a reasonable assumption given the nature of these inputs. Second, we assume that the child who delays enrollment by one year (or repeats a grade) will ultimately complete one less grade; 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 the child who delays enrollment or repeats makes up the lost year by leaving school when he is one year older, then our estimate of the indirect effect of nutrition on ultimate achievement will be overstated (though, in this case, there will be a another cost, namely the earnings forgone by postponing entry into the labor force by a year).

With these caveats in mind, turn to the 2SLS estimates in Table 3. The age of enrollment regression indicates that a one standard deviation increase in height leads parents to enroll their child in school nearly two months earlier (Glewwe and Jacoby [1995] find a three-month enrollment age response in Ghana).[21] This effect is statistically significant, yet 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 a year; this finding supports the instrumental variables strategy used in Table 2.

For the first grade repetition regression, we use a slightly smaller sample of sibling pairs, those in which the younger sibling is old enough to have had the opportunity to complete first grade. We consider only repetition of first grade 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 is not a significant loss of information. The estimates are based on a linear probability model, since no discrete choice estimator can simultaneously handle selection bias, 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, though the effect falls barely short of significance at the five percent level. However, conditional on nutritional status, children who delay their enrollment into primary school are significantly less likely to

[21] 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, Hall, and Griliches 1984) for the number of years enrollment was delayed (without controlling for selection bias or endogeneity, which cannot be done with this estimator), we obtain a height-for-age effect quite similar to the corresponding OLS (sibling differences) estimator.

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 10 percent.

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. Since the coefficients on the latter two variables are far from significant in the achievement production function, we can ignore these effects. Hence, a one standard deviation increase in height translates into an improvement of 2.2 (1.4) points on the achievement test through the indirect effect, compared to the direct effect of 5 points.[22] The total effect (7.2 points) of a one standard deviation increase in height is equivalent to six months of school attendance.

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 do find some for the time in school variables

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[22] The standard error (in parentheses) is calculated by the delta method and ignores any covariance between the estimated parameters from the different regressions.

Table 3 Nutritional status, age of enrollment, and grade repetition: Sibling differences-2SLS estimates

	Age enrolled	Grade repetition
Height-for-age[a]	-1.786*	-0.117
	(0.893)	(0.061)
Child is female	-0.599	-0.027
	(0.384)	(0.025)
Age enrolled (months)[b]	--	-0.0122*
		(0.0053)
Mills' ratio	-1.809*	-0.005
	(0.637)	(0.028)
Month of birth effects (p-value)	0.000	--
Overidentification tests (p-value)[c]	0.807	0.874
Number of sibling pairs	1016	891

Notes: Standard errors in parentheses (asterisks denote statistical significance at the 0.05 level). All regressions include a constant.

[a] Endogenous variable. Excluded instruments: height-for-age of the older sibling at 0, 12 and 24 months.

[b] Endogenous variable. Excluded instruments: differenced month of birth dummy variables.

[c] Test has two degrees of freedom in column one and 12 degrees of freedom in column 2.

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 misspecification, we test whether a set of observed but omitted academic inputs respond to nutritional status. If these inputs are not influenced by nutritional status, 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 nutritional status, then the sign of the correlation will at least tell us something about the plausible direction of bias in the parameter of interest. Table 4 reports five input regressions. In all cases, we regress sibling differences in the input on sibling differences in height-for-age, age, and sex, using the same econometric procedure as in column five of Table 2.[23] The input regressions are jointly estimated 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.

Before reporting the result of this test, we discuss the input regression results individually. Reading frequency is a categorical variable. About 17 (25) percent of the index children (younger siblings) are read to regularly (weekly or more) by someone in the household, 23 (30) percent 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 five percent 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

[23] The input equations can be thought of as dynamic decision rules, analogous to equation (3) for delayed enrollment. Once again, since the own effect of nutrition is not separately identified, the sign of the coefficient cannot, strictly speaking, be used to infer anything about compensatory versus reinforcing behavior unless the cross sibling effects are assumed to be zero.

Table 4 Nutritional status and academic inputs: Sibling differences-3SLS estimates

	Reading frequency	Hours of homework	Help with homework	Percent days absent	Years of preschool
Height-for-age[a]	-0.169*	0.043	0.008	0.0027	0.111
	(0.071)	(0.052)	(0.046)	(0.0056)	(0.060)
Child is female	-0.102*	0.088*	-0.091*	-0.0138*	0.040
	(0.035)	(0.026)	(0.022)	(0.0028)	(0.029)
Age child tested (years)	-0.233*	0.009	-0.034	0.0003	-0.078*
	(0.041)	(0.030)	(0.026)	(0.0032)	(0.034)
Mills' ratio	0.343	-0.130	-0.199	-0.011	-0.023
	(0.211)	(0.154)	(0.135)	(0.017)	(0.178)

Notes: Standard errors in parentheses (asterisks denote statistical significance at the 0.05 level). All equations include a constant term and are estimated on a sample of 974 sibling pairs. Joint test of height-for-age coefficient in all equations: $\chi^2_{(5)} = 10.4$ (p-value = 0.065).

[a] Endogenous variable. Excluded instruments: height-for-age of the older sibling at birth, 12 months, and 24 months.

influence whether the child receives assistance with homework from a parent, sibling, or other household member (column three); 68 percent of the index children and 83 percent 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 974 sibling pairs). Our data indicate that only about 3.5 percent of days are missed, and the estimates in column four show that absenteeism is unrelated to nutritional status. Lastly, consider preschool/kindergarten enrollment. About 40 percent of index children and 45 percent of their younger siblings attended a preschool and/or a kindergarten for at least a year, and usually for no more than two years. The final column in Table 4 shows no significant relationship between nutritional status and preschool enrollment.

Taking all five inputs together, we fail to reject the joint hypothesis that the coefficients on height-for-age are zero at the five percent level, though we do reject at the ten percent level (see notes to Table 4). Based on this finding, it appears doubtful that omitted inputs, at least the ones we have data 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. 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 spillover effect should seriously enter into a cost-benefit analysis of a nutrition intervention, as some nutritionists have argued. By the same token, though our results imply that economic growth and improved nutrition are mutually reinforcing, they may not reinforce each other much. In this section, we use our production function estimates to gauge the impact on achievement of alternative policy and economic growth scenarios. Although we take into account both the direct and indirect effects (through time in school), as discussed in the previous section, it should be noted that only the former effect, being a structural parameter, is generalizable across different environments. Table 5 summarizes the results of this analysis.

Consider first a policy of subsidizing the price of corn, a staple in Cebu. Blau, Guilkey, and Popkin (1996), using the original 12 rounds of the CLHNS survey, find a statistically significant negative impact of higher corn prices on child height in the first two years of life. Based on their estimates, a 50 percent subsidy on the price of corn to families with infants would increase child height at age two by less than a quarter centimeter.[24] Assuming no change in growth trajectory after age two, this subsidy would improve test scores by the equivalent of just half a month of extra school. Such a

Table 5 Policy and economic growth scenarios

Scenario	Increase in height-for-age Z-score	Increase in achievement (months in school equivalents)		
50 percent corn price subsidy	0.08	0.33	0.15	0.48
12 month intensive nutrition supplementation program	0.30	1.25	0.55	1.80
20 years of economic growth at 2.3 percent per annum	0.35	1.46	0.64	2.10
20 years of economic growth at 8 percent per annum	2.34	9.75	4.29	14.0

Notes: Direct effect is calculated using height-for-age coefficient estimate in column five of Table 2. Indirect effect calculation is described in Section 4 of the text.

[24] Blau, Guilkey, and Popkin report first-differenced, lagged IV, estimates of a reduced form for child height over the 12 rounds of the bimonthly survey. The average effect per round of a one percent increase in the price of corn is to reduce height by .0023 centimeters. To calculate the overall effect of the subsidy on height at 24 months, we assume that reductions in the corn price have no effect in the first four months of life when the child is not consuming solid foods. We then use the estimates of the age-dependent impact of lagged height on current height to cumulate the impact of the price subsidy.

negligible impact on achievement hardly provides additional justification for a subsidy policy beyond those usually advanced. Intensive nutritional supplementation would seem to be a more sensible route toward meaningful achievement gains, so it is to such a policy that we turn next.

Though the track record of nutritional supplementation programs in developing countries has been mixed (Beaton and Ghassemi 1982), one of the more optimistic assessments comes from a randomized trial in Jamaica. Malnourished infants averaging 18 months old who were given large milk-based supplements for a period of 12 months showed a statistically significant improvement in height-for-age, averaging 0.3 of a standard deviation, compared to an unsupplemented control group (Walker et al. 1991).[25] If such a nutrition program had an equivalent impact on height in Cebu, and, once again, there was no change in growth trajectory after the program ended, then supplemented children would improve their test scores by the equivalent of less than two months in school. This small improvement would seem to belie the notion that intensive nutritional supplementation can have large education spillovers, let alone that without such intervention malnourished children would be "intellectually crippled" (Brown and Pollitt 1996). On the other hand, perhaps nontrivial achievement gains can be achieved by continuous and intensive supplementation over many years, but the net benefits of such a

[25] The supplement provided "two-thirds of the energy requirement and all of the protein requirement" (Walker et al. 1991). To get an idea of the cost of such a program, note that the supplements were delivered weekly to each home during the year of the intervention.

policy would have to be weighed against those of more direct approaches to enhancing learning such as improving the quality of schools.[26]

Finally, consider the impact of economic growth on achievement. From a cross-country regression of average height of adolescents on per capita income and other variables, Steckel (1995) estimates an income elasticity of height of around 3 percent. We can use this estimate in an admittedly crude calculation, which says that if per capita income in the Philippines continues growing at its 1990–95 annual rate of 2.3 percent over the next 20 years, then average height would increase by about a third of a standard deviation. As a consequence, achievement would improve by just over two months worth of schooling. If, on the other hand, we take the highly optimistic view that future Philippine economic growth will emulate that of its southeast Asian neighbors, Indonesia and Thailand, at around 8 percent per capita, then average height will approach (actually exceed) U.S. levels in 20 years. As a result, we estimate that students would make the equivalent of a grade and a half of academic progress, which is surely an upper bound on the effect of economic growth over one generation on achievement (via nutrition). Only in the very long run, say over the 200 years of history considered by Fogel (1994), do our estimates allow nutrition to powerfully influence academic achievement and thus for economic growth and improved nutrition to substantially reinforce each other.

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[26] Supplementation of specific micronutrients (e.g., iron) has also been advocated as a relatively inexpensive way of boosting academic achievement (see World Bank 1996), but our estimates cannot be used to assess such interventions directly.

To summarize, although the impact of nutrition on achievement is positive and statistically significant, our investigation suggests that it is small from a practical standpoint. This lack of economic significance is all the more telling if one views our estimate of α_H as an upper bound on the true structural impact (due to the presence of important physiological shocks). One qualification 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 a country like the U.S., where the incidence of stunting is low even among the poor, the scope for achievement gains through a nutrition-learning nexus is likely to be trivial.[27]

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 up until the end of their primary 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

[27] 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.

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 an extremely noisy indicator of early childhood nutritional status. Thus, the availability of anthropometric data in the first two years of life combined with data on siblings proves to be extremely valuable. Third, selection bias due to delayed enrollment, which has up to now been ignored in the education production function literature, turns out to be quite important. Fourth, there does not seem to be a strong connection between child 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, our analysis suggests that the relationship between nutrition and learning, though significant statistically, is not likely to be of overriding importance either for nutrition policy or in accounting for economic growth.

APPENDIX

Table 6 Sample attrition and selection

Live Births in 33 Sample *Barangays* of Metro Cebu	3,289	
Of which: Twin Births	27	(0.8 percent)
Refusals	97	(2.9 percent)
Missed by Survey (discovered later)	58	(1.8 percent)
Birth Interview Too Late	22	(0.7 percent)
Live Births in Metro Cebu with Birth Interview	3,085	
Of which: Migrated Out of Metro Cebu by Age 2	318	(10.3 percent)
Child Died by Age 2	156	(5.1 percent)
Refusal (at later date)	50	(1.6 percent)
Still in Sample When Child is 2 Years Old	2,561	
Of which: Migrated Out of Metro Cebu by Age 8	155	(6.1 percent)
Could Not find Child at Age 8	137	(5.3 percent)
Child Died by Age 8	38	(1.5 percent)
Still in Sample When Child is 8 Years Old	2,231	
Of which: Migrated Out/Could Not Find[a]	31	(1.4 percent)
Child Died	8	(0.4 percent)
Still in Sample When Child is 11 Years Old	2,192	
Of which: Never Enrolled in School	9	(0.4 percent)
Not Tested (e.g. refusal)	13	(0.6 percent)
Does Not Have Younger Sibling of School Age	931	(42.4 percent)
Sample with Younger Children of School Age	1,239	
Of which: Younger Sibling Not in School	90	(7.3 percent)
Sample of Sibling Pairs with Both Siblings in School	1,149	
Of which: Missing 1991 Height Data for Older Sibling	13	(1.1 percent)
Missing 1983–86 Height Data for Older Sibling	92	(8.0 percent)
Missing Height Data for Younger Sibling	23	(2.0 percent)
Missing Test Score Variables	5	(0.4 percent)
Observations with Complete Data	1,016	(88.4 percent)

[a] This figure of 31 children lost between 8 years and 11 years is a net figure. In fact, 77 children interviewed in 1991–92 could not be located in 1994–95. However 46 children in the 1983–86 sample who were not found in 1991–92 were found in 1994–95.

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