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Early Childhood Nutrition, Schooling, and Sibling Inequality in a Dynamic Context: Evidence from South Africa

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Abstract

This paper examines the effects of early childhood nutrition on schooling inputs and outcomes to assess the dynamic nature of human capital production, using panel data from South Africa. Height-for-age Z-score is used as a measure of health and nutritional status in early childhood. Based on a comparison of siblings, this analysis concludes that improving children's health significantly lowers the age when they start school, increases grade attainment, and decreases grade repetition in the early stage of schooling. However, this positive effect diminishes at later stages. The results also show that households allocate more of their resources (such as school fee expenditure) to healthy children at the early stage, although wealthier households may invest more in less well endowed children in an attempt to reduce sibling inequality. However, fewer resources are allocated to healthy children at later stages. By the time of transition from primary to secondary school, the healthy child can increase household income by seeking employment in the labor market. In other words, while health capital augments the efficiency of investment in schooling at the early stage, it may increase opportunity costs at the later stage, which may deter investment in schooling.

Key words: early childhood nutrition, health capital, height-for-age, schooling investments and outcomes, South Africa

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1. Motivations

Human capital takes a long time to accumulate, going through multiple phases from early childhood through higher education. While nutritional intake in early childhood forms the basis of a child's health capital, which provides a foundation for subsequent child development,[1] investments in schooling augment the child's knowledge capital, which is directly rewarded in advanced labor markets and technologies. In this paper, we examine the effects of nutritional status and health capital in early childhood on schooling investments and outcomes, using recently available panel data from South Africa.

The dynamic process of human capital development creates the possibility that investments in early childhood will influence the optimal amount and effectiveness of investments at later stages (Cunha et al. 2004). Some recent studies attempt to identify the causality from early-childhood nutritional status to schooling outcomes (see, for example, Alderman, Hoddinott, and Kinsey 2002; Glewwe, Jacoby, and King 2001). In general, the way that early-stage human capital investments determine the subsequent path of human capital accumulation and future income depends on (1) whether investments in different stages are mutual complements or substitutes, and (2) the extent to which early investments and outcomes alter the environment, information, and preferences of children and parents that affect investment decisions at later stages.

To answer the question of whether early investments in children affect future outcomes requires a good understanding of rather complex interactions of market forces and dynamic household behavior. First, health capital, as well as schooling investments, generates positive economic returns, especially in the developing-country context (Strauss 1986; Haddad and Bouis 1991; Thomas and Strauss 1997). Therefore, health

[1] For comprehensive discussion of the problem of child malnutrition in economic development, see Behrman, Alderman, and Hoddinott (2004).

capital may increase opportunity costs for schooling investments,[2] and it may affect intertemporal decisionmaking, creating nonlinearity in the effect of health capital on schooling investments.

Second, there is a technical relationship between health and schooling in the dynamic human capital production function. For example, if health capital[3] is an input in the schooling production function (for example, enabling children to attend classes every day), whether health capital augment the productivity of schooling investments or it substitutes for schooling inputs, it affects the optimal level of schooling investments. In the former case, we predict a cumulative process of widening inequality among siblings, given differences in nutritional status and health capital in early childhood, since more knowledge capital is invested in healthy children. If parents are averse to sibling inequality in the future earnings, they will make more schooling investment in unhealthy children.

Third, parents also learn about potential returns to schooling investments from outcomes of early-stage investments (in nutritional status and health in our context) and make decisions regarding optimal investments at later stages. In these decisions, parents' preferences concerning sibling inequality in human capital and future income matter. If parents are averse to the inequality among their children, they may increase investments in schooling of their less well-endowed children to equalize future incomes among their children (Quisumbing, Estudillo, and Otsuka 2003). In the context of dynamic human capital production, since outcomes of early-childhood investments signal the expected outcomes of investment at a later stage, parents can react to those signals by changing late-stage investments to maximize their objective.

[2] For example, with a relatively small sample of 367 adult workers (age above 20) in the KwaZulu-Natal Income Dynamics Study 1998, returns to height (measured in centimeters) are estimated as 0.0093 (t-value: 2.04) in the log-wage equation. Returns to schooling (in years) are 0.1055 (t-value: 8.26). The specification includes age, male dummy, and community fixed effects. Robust standard errors are used with community-level clusters.

[3] Health capital is part of human capital, measuring physical development and conditions in children (height, weight, and if the child is sick or well), but not including endowment such as in-born differences in intelligence.

To empirically assess this issue, we encounter a few challenging problems even with longitudinal data of children. The first problem arises from potential endogeneity of nutritional status, such as fixed household-specific unobserved factors that affect both child health capital and schooling decisions, creating a positive correlation between them. To eliminate this problem, our approach requires household fixed effects, which base inferences on (often small) sibling variations.[4] [5]

The next section describes the model. Human capital accumulation is modeled as a sequential process in which health is formed at an early stage and schooling investment is undertaken given the health outcomes. Both health and knowledge (education) capital determine earnings in the labor market. Section 3 discusses econometric issues, focusing on specification and identification strategy.

Data and variables are described in Section 4. To measure schooling outcomes, we use the 2004 KwaZulu-Natal Income Dynamics Study (Round 3), which collected individual-level information such as enrollment, age schooling started, grade completed, grades repeated, and expenditures from children aged 7 to 20. To supplement the main analysis, the survey also used the results of simple mathematics tests given to children

[4] The literature offers a few qualified empirical studies, which solve the above problems. Using longitudinal data for children from Zimbabwe, Alderman, Hoddinott, and Kinsey (2002) use civil war and drought periods that affected growth in children below three years to identify the effect of early childhood malnutrition on schooling in a maternal fixed effect model. This identification strategy is based on findings that income shocks, such as drought and flood, in credit constrained circumstances, change consumption, which affects child growth (Hoddinott and Kinsey 2001; Foster 1995). Glewwe, Jacoby, and King (2001) take a similar approach to sibling estimation, using longitudinal data of Filipino children, but their identification strategy uses a rather strong assumption on height changes among older and younger siblings. Alderman et al. (2000) use price data, interacted with parents' education and child gender, as instruments for child height growth in Pakistan.

[5] An earlier version uses the unique period in South Africa from the end of apartheid to the beginning of a democratic regime in the province of KwaZulu Natal. The country introduced the first democratic election in 1994, but the implementation of the local election was delayed one year in KwaZulu-Natal due to violent political conflicts and social turmoil. This period is also the start of widening economic and political opportunities for African populations, who were neglected during apartheid. These transitions seem to have exogenously affected the welfare of sample households, though the magnitude and direction of the changes depend on the community in which they resided as well as their initial income level. The sample of children in the longitudinal data from the province of KwaZulu Natal include children below the age of three in 1994 and 1995, whose physical growth is regarded as sensitive to nutrition intakes. However, it is shown that the instruments constructed are not statistically effective enough to correct for individual bias.

age 7 to 9 to measure their learning performance. Therefore, combined with the information on nutrition and health outcomes for children ages 1 to 5 available in the 1998 survey, we can investigate the effect of early childhood nutrition on schooling investments and outcomes of children 6 years and above. One advantage of focusing on early stages of schooling is the high enrollment rate at the primary school level, which minimizes a selectivity problem arising from child time allocation decisions at later stages of schooling.

Section 5 summarizes empirical results. First, data for siblings showing the effects of nutrition (as indicated by height) on the age schooling started and the grade completed show that for the majority, children of normal height (as measured by height-for-age Z-score) start school earlier, complete more grades, and repeat fewer grades. The analysis also identifies some outlying observations among taller children (less than 5 percent of the sample) that show a negative effect of the height Z-score on schooling outcomes. In the analysis with longer panel data (11 years), we find that, although better nutrition and health status in early childhood improves primary school outcomes, this positive effect diminishes as the child moves from primary to secondary school.

Second, the analysis on mathematics test results, using the sample of children age 7 to 9, shows that health capital, measured by height at early childhood, has a significantly positive effect among children age 7, implying that early-childhood nutrition affects learning performance at the very early stage of transition to schooling. Comparison of naïve—ordinary least squares (OLS)—and sibling estimates also demonstrates that some (unobserved) household factors improve both child health and learning performance. Similarly, our results suggest that household-specific unobservables are positively correlated with schooling investments and early childhood health capital.

Third, the information on individual-level school fee expenditure enables us to investigate intrahousehold resource allocation among siblings. We find that more resources are allocated to shorter (less healthy) children in the early stage of primary school (thus reducing inequality), but this increases inequality at later stages.

2. A Simple Model

This section introduces a simple model in which parents decide on how much to invest in child health and schooling, resulting in returns to labor. For simplicity, we treat the age distribution of children as exogenous and assume that children enter the labor market in the final stage. Health is formed in the first stage,[6] while schooling investment is undertaken in the next stage.

In the pre-primary stage, per capita consumption determines health capital *hj* for child *j*,

$$h_j = f(c_1, z) + \varepsilon_{j1},$$

where c_1 is per capita consumption in the household, z is predetermined household characteristics such as parents' schooling, and ε_{j1} is an idiosyncratic health shock. For simplicity, health capital accumulates only until age a^*, when the child enters the schooling stage. Investment component $f(c_1, z)$ is characterized by the properties:

$$\frac{\partial f}{\partial c_1} > 0 \text{ and } \frac{\partial^2 f}{\partial c \partial z} \leq 0 \text{ or } \geq 0 .$$

For simplicity, we assume that $c_1 = y$. Given that h is child height, c is specifically intended to capture nutritional intake.

At the second stage, knowledge capital, k_j, accumulates with schooling investments s_{jt}. The law of motion is given as

$$k_j = g(s_j, h_j, z) + \varepsilon_{j2},$$

where investment $g(s_{j2}, h_j, z)$ has health capital as its argument. Complementarity between schooling and health investments is captured by

[6] Nutrition intakes until the age of 3 are regarded as very important in forming child health capital, measured by height-for-age Z-score. Although weight-for-age Z-score fluctuates over time (age) due to changes in nutrition intakes (that is, consumption), height-for-age Z-score is less likely to change after the age of 3. In the context of dynamic human capital production, therefore, child health is measured by the height-for-age Z-score.

$$\frac{\partial^2 g}{\partial s \partial h} > 0 \text{ .}$$ [7]

Household budget constraint is

$$c_2 = y + \sum_i w(h_i)[T - s_i] + b \text{ ,}$$

and $c_1 = y$ where $w(h_i)$ is child wage, T is time endowment for the child, b is saving and loan, and y is exogenous household income. It is assumed that child wage increases with health capital, that is, $w'(h_i) \geq 0$. It is assumed that the child cannot work at the pre-primary stage and can work in the labor market only when he or she enters school.

Several reservations follow. First, it is assumed that income from siblings, parents, and credit are pooled in the household budget and therefore are perfectly substitutable. Second, to describe the income process, the model does not assume a production function where adult and child members supply labor inputs that are not perfectly substitutable. This framework is suitable in our empirical setting of South Africa, where wage employment (including formal and informal jobs) is a major source of income. Third, the model does not have leisure in the utility function, which is imperfectly substitutable between household members (Pitt and Rosenzweig 1990).

It is also important to note that the income opportunity in the child wage $w(h)$ is not necessarily related to labor markets. It may also capture activities such as childcare and self-employment in a family business.

Parents maximize the objective function,

$$\max_{s_i, b} E_1 \left[\sum_{t=1}^{2} \beta^{t-1} u(c_t) + \beta^2 \left\{ \sum_i W(k_i, h_i) - (1+r)b \right\} | \, z, y, \varepsilon_1 \right],$$

[7] Cunha et al. (2004) summarize some key concepts in the sequential development of child human capital. They focus on cognitive and noncognitive development. It seems that their analysis does not directly include health and nutritional status as part of human capital in child development. The exclusion of health capital from the analysis results in a framework in which they can focus on human capital production function and complementarity and substitutability of different inputs (early childhood and schooling stage). In this paper, children also work in the labor market where health capital has economic returns.

which captures the discounted sum of expected utilities from consumption over time and the final-period returns from children. Assume that $W(k_i, h_i)$ is strictly concave in both k_i and h_i. The concavity of the wage function implies that parents have incentives to equalize human capital among their children.[8]

The first order conditions at the second stage are

$$\lambda_2 = u'(c_2^*) = \beta(1+r)$$
$$w(h_j)\lambda_2 = \beta \frac{\partial g}{\partial s_j}(s_j, h_j, z) E_2 \frac{\partial W}{\partial k_j}(k_j, h_j),$$

where λ_2 denotes the Lagrange multiplier associated with the stage-2 budget constraint. These conditions provide the schooling function $k^*(y, h_j, z)$. At the first stage, the problem is trivial, since exogenous income and shocks determine investment in health capital. From these conditions, we define

$$F(s_j, h_j) = E_2\left[\frac{\partial g}{\partial s_j}(s_j, h_j, z)\frac{\partial W}{\partial k_j}(k_j, h_j)\right] - w(h_j)(1+r) = 0 .$$

The first term captures the expected marginal return from schooling investment, while the second term is the opportunity cost for time spent in school. Therefore, with a perfect loan market, the effect of health on schooling depends on

$$\frac{\partial s_j^*}{\partial h_j} \geq \text{or} \leq 0 \Leftrightarrow \beta E_2\left[\frac{\partial g_s(s_j, h_j) W_k(k_j, h_j)}{\partial h_j}\right] \geq \text{or} \leq w'(h_j)u'(c_2^*) ,$$

where c_2^* is such that $u'(c_2^*) = \beta(1+r)$. Therefore, preference does not actually enter the condition. In this case, parents compare returns and opportunity costs for schooling, and child health capital can change both. Household income does not enter the condition, so income level does not affect the optimal schooling and child health does not affect the schooling decision.

[8] Since household members have no preference for leisure, there is no income effect on labor supply.

If child wage does not increase with health capital (that is, $w'(h) = 0$), an increase in health capital will raise the optimal level of schooling if health and knowledge capital are complementary.

Consider the case that $b = 0$, where credit opportunity is closed. Given the second-order condition, the effect of h_j on s_j depends on

$$\frac{\partial s_j^*}{\partial h_j} \geq \text{or} \leq 0 \Leftrightarrow \beta E_2 \left[\frac{\partial g_s(s_j, h_j) W_k(k_j, h_j)}{\partial h_j} \right] \geq \text{or} \leq w'(h_j) u'(c_2(y)) + w(h_j) \frac{\partial \lambda_2(y)}{\partial h_j},$$

where

$$\frac{\partial \lambda_2(y)}{\partial h_j} = u''(c_2(y)) \frac{\partial c_2(y)}{\partial h} < 0 .$$

The right-hand side captures changes in the opportunity cost and the current liquidity. First, an increase in health capital raises the child wage, which discourages schooling investment. Second, an increase in health capital relaxes the current budget constraint, given that the child works to make a contribution to the household income. The concavity of the utility function guarantees that the second term is negative, which increases schooling investments.[9]

An increase in the income level decreases the marginal utility, so $\lambda_2(y)$. In the first term, poverty therefore magnifies the negative effect of health capital on schooling investment. If the absolute risk aversion measure is constant, it is easy to show that an increase in income weakens the negative effect of health capital on child schooling.

[9] An increase in child health capital improves the welfare:

$$\lambda_2 w'(h_j)\left[T - s_j\right] + \beta E_2 \frac{\partial W}{\partial h_j} + \sum_i \left[\beta E_2 \frac{\partial W}{\partial k_i} \frac{\partial g}{\partial s_i} - w(h_i)\lambda_2 \right] \frac{\partial s_i}{\partial h_j} = \lambda_2 w'(h_j)\left[T - s_j\right] + \beta E_2 \frac{\partial W}{\partial h_j} > 0 ,$$

where the Envelope theorem was applied to the third term. Even though we cannot predict the direction of the marginal impact of changes in health capital on schooling investments, it is shown that the direct effect of an increase in health capital is welfare augmenting.

Interestingly, the effect of other siblings' health capital on schooling investment only exists when the financial market is imperfect. An improvement in other siblings' health relaxes the budget constraint, which reduces the marginal utility $\lambda_2(y)$. This does not happen when the loan market is perfect.

We have two conjectures. First, in the case of the convex return function where the second order conditions do not hold, we expect some corner solutions, concentrating all schooling investments in some children, leaving no investment to others. Parents can maximize welfare by investing in some children, while ignoring the others. When health capital is complementary to schooling investment, parents concentrate schooling investments in well-endowed children, if the labor market wage is constant. However, greater health capital also raises wages (thus, opportunity costs), which decreases schooling investments. If the latter effect is sufficiently small, we expect that the inequality in schooling between siblings will diverge.[10]

Second, suppose that children are born in different time periods. When the timing (and the number) of children is exogenous, we have a substitution effect between investment in schooling of elder siblings and investment in health of younger siblings, if the discount factor is sufficiently large. In the case of a small discount factor, parents may want to invest more in schooling of elder siblings, sacrificing the human capital of younger siblings, in order to gain returns to human capital as early as possible. If elder siblings' time input is important (included) in child health capital production, elder siblings can work at home to take care of younger siblings. In this case, there is a trade-off between schooling investments in elder siblings and time input in health capital formation of younger siblings, which again depends on the discount factor.[11]

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[10] Quisumbing, Estudillo, and Otsuka (2003) presents evidence from the rural Philippines, where boys are likely to inherit land, showing that parents invest more in schooling of daughters than sons to equalize lifetime earnings between them. In the Philippines, Schady (2003) and Yamauchi (2005) both show that the schooling return function is convex, whereas the latter contrasts this finding to the case of Thailand where schooling returns are concave.

[11] Nonneutrality of birth order and sibling's sex composition in child human capital investment is pointed out and analyzed in, for example, Rosenzweig and Wolpin (2000), Rosenzweig (1986), and Butcher and Case (1994).

3. Empirical Framework

To assess the effects of children's health in their early years on schooling decisions and outcomes at a subsequent stage, we use the following framework. The schooling equation is

$$q_{ijt} = \alpha + \beta_1 h_{ijt-1} + \sum_a \beta_{2a} I(a_{ijt} = a) + z_{ijt}\gamma + \mu_i + \phi_j + \varepsilon_{ijt} , \tag{1}$$

where i, j, and t denote household, child, and time, respectively, and q_{ijt} is schooling inputs or outcomes, h_{ijt-1} is health capital, which is measured by the height-for-age Z-score (formed at $t - 1$), a_{ijt} is the age of the child, z_{ijt} is a set of control variables, μ_i is household-specific fixed unobservables, ϕ_j is child-specific fixed unobservables, and ε_{ijt} is an error term.

First, it is important to control the heterogeneity that arises from the current ages. For example, cumulative years of grades repeated increases (but weakly) as children spend more time in school, that is, as their age increases. The score of the mathematical tests also changes by age (and grade completed). In the analysis below, we assume that age structure in the sample of children is exogenous; it is uncorrelated with shocks in schooling decisions and outcomes, which justifies the inclusion of age fixed effects.

Second, since it is highly likely that household-specific unobservables μ_i are correlated with h_{ijt-1}, ordinary least square (OLS) estimates of β_1 are biased. This makes it necessary to eliminate this component from the errors. For this purpose, we include household fixed effects to control μ_i. Therefore, estimation is based on variations across siblings in the household.

In the context of panel analysis, the inclusion of household fixed effects has another advantage regarding the attrition bias, which may arise from endogenous migration and mortality during the sample period. Since we only look at variations within households, given household observations in the two rounds, we do not have to control for household-level attrition problems. Individual-level attritions are investigated in Section 4.

Third, even with household fixed effects, we still encounter a potential problem of bias that may arise from a correlation between ϕ_j and h_{ijt-1}. To wipe out this correlation, it is necessary to use a set of instruments that explains the variations in h_{ijt-1} but is uncorrelated with either ϕ_j or shocks in schooling investments and outcomes ε_{ijt}. However, the necessity depends on the magnitude of covariations in differences among siblings in Z-score and schooling endowments.[12]

Finally, we can examine how parents differentiate among their children in the quality of their investments in school fee payments. It is thought that options depend on income level. Low-income households, most likely to be credit constrained, cannot afford to change expenditures for different siblings. Children from those households are likely to attend inexpensive public schools in the neighborhood. An increase in income enables parents to differentiate between their children in school spending. In this regard, it is interesting to see whether parents compensate for less endowed children (those with

[12] The instruments used in the analysis below are influenced by a historical event specific to KwaZulu-Natal, from which our data come. Prior to 1994, under apartheid, South Africa prohibited freedom in various dimensions of social life for African and nonwhite populations. At the end of apartheid in 1994, after violent political struggles, the country held its first democratic national election, except in KwaZulu-Natal, where the African National Congress and a new political party could not agree to the election. For this reason, KwaZulu Natal had its election one year later in 1995. That year was marked by turbulence and violence in the province.

An instrument is constructed to have the value of one if children were less than 3 years old (inclusive) between the beginning of 1994 and the end of 1995. Before the age of three is regarded as the period when the child's growth is most sensitive to nutritional intake, which reflects economic conditions. This indicator is interacted with cluster fixed effects to capture possible heterogeneity in the impacts of the 1994–95 disturbances on child growth, $I(\text{Age} \leq 3 \text{ in Year} = 1994 \text{ or } 95) \times$ cluster indicators. This period also corresponds to the abolishment of apartheid, so new economic opportunities became open to the African population. Thus there may have been positive impacts as well as negative ones. In addition, to capture the heterogeneity in the impacts related to the initial income level, the indicator is also interacted with total monthly household income in 1993.

Although an F-test supports the joint significance of these instruments in explaining variations among siblings in height-for-age Z-scores, a Hausman-Wu test rejects the relevance of these instruments. We observed some differences between sibling OLS and sibling IV estimates, but the magnitude did not change the qualitative nature of our results. Therefore, we do not show sibling IV estimates here and focus on sibling OLS estimates in the following discussion.

lower health capital) by spending more on them, or whether they will or concentrate more resources on better-endowed children.

4. Data

The analysis requires information from different points in time for the same individuals. In this paper, we use data from the KwaZulu-Natal Income Dynamics Study of 1993, 1998, and 2004. Population was self-weighted designed in the first round in 1993, based on the 1991 population census, and enumeration-based weights were introduced in 1998). The 1993 and 1998 surveys provide information on anthropometric measures and health outcomes of children, enabling us to construct age-standardized Z-scores for height. The 2004 survey provides some detailed information on schooling decisions and outcomes. Our analysis combines the nutritional status of pre-primary aged children in 1998 and 1993 and their schooling inputs and outcomes until 2004.[13]

Figures 1a and 1b depict the distribution of height-for-age Z-scores by age in 1998 and 1993, respectively. Observations with inconsistent ages between 1993 or 1998 and 2004 were screened out from the sample. The sample includes those who were enrolled in school. These figures show symmetric distributions of height Z-scores by age group, though small samples seem to affect the smoothness of the empirical distributions.

In the main analysis, we use as schooling variables: (1) age started school, (2) grade completed (conditional on current age), (3) the number of grades repeated, (4) school fee expenditure, and (5) mathematics test results. For age started school, the 2004 survey asks for the calendar year in which the child started primary school. That year, compared with the current age in 2004, tells us the age at which the child started attending primary school.[14]

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[13] Constructing individual-level panel data from the 1998 and 2004 surveys, we have screened out observations recorded in multiple households (multiple memberships). The details of this procedure are available from the author.

[14] In the main analysis, we only use observations with estimated age started school greater or equal to age 4, restricting the sample to children who were aged less than 3.

Figure 1a—Distributions of height-for-age Z-score in 1998

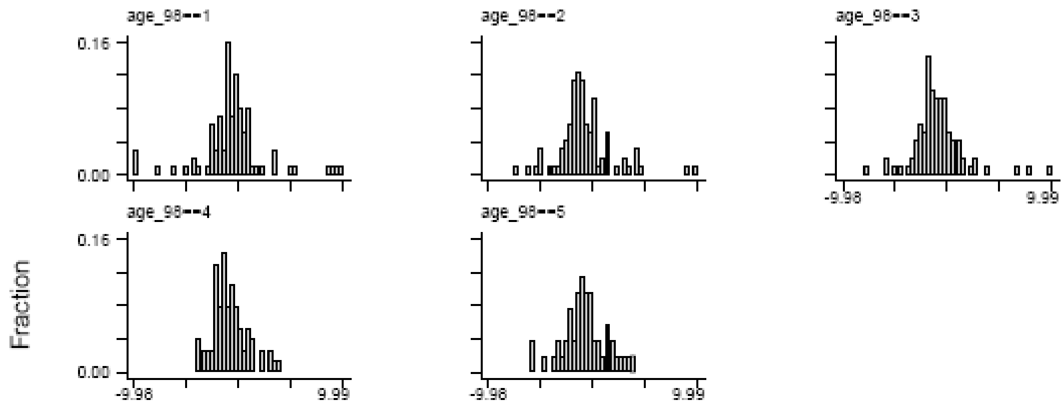


Figure 1b—Distributions of height-for-age Z-score in 1993

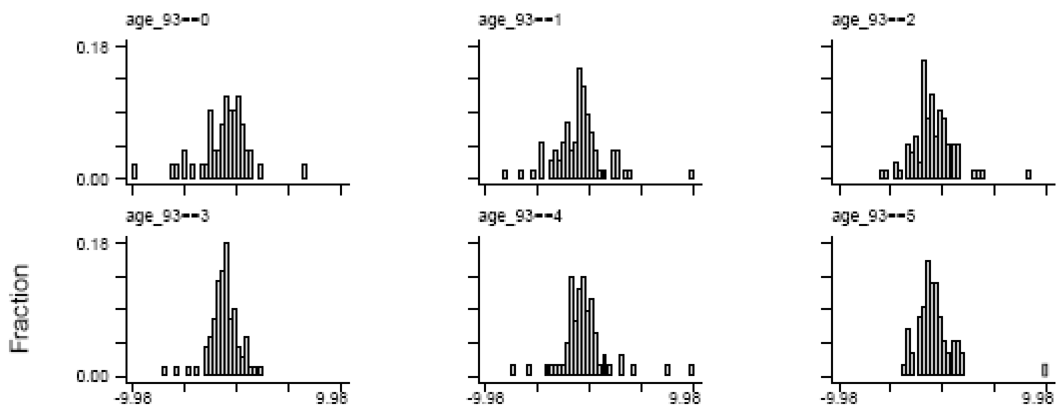


Table 1 reports the descriptive statistics of schooling outcome variables: age started school, the highest grade completed, and the cumulative number of grades repeated. First, the age started school increases as the current age increases, which suggests that younger cohorts enter school at an earlier age. Second, the highest grade completed and the cumulative number of grades repeated also increase with the current age. Figure 2 also depicts the distribution of individual-level school fee payment. It is highly skewed, with a concentration of low school fee expenditures.

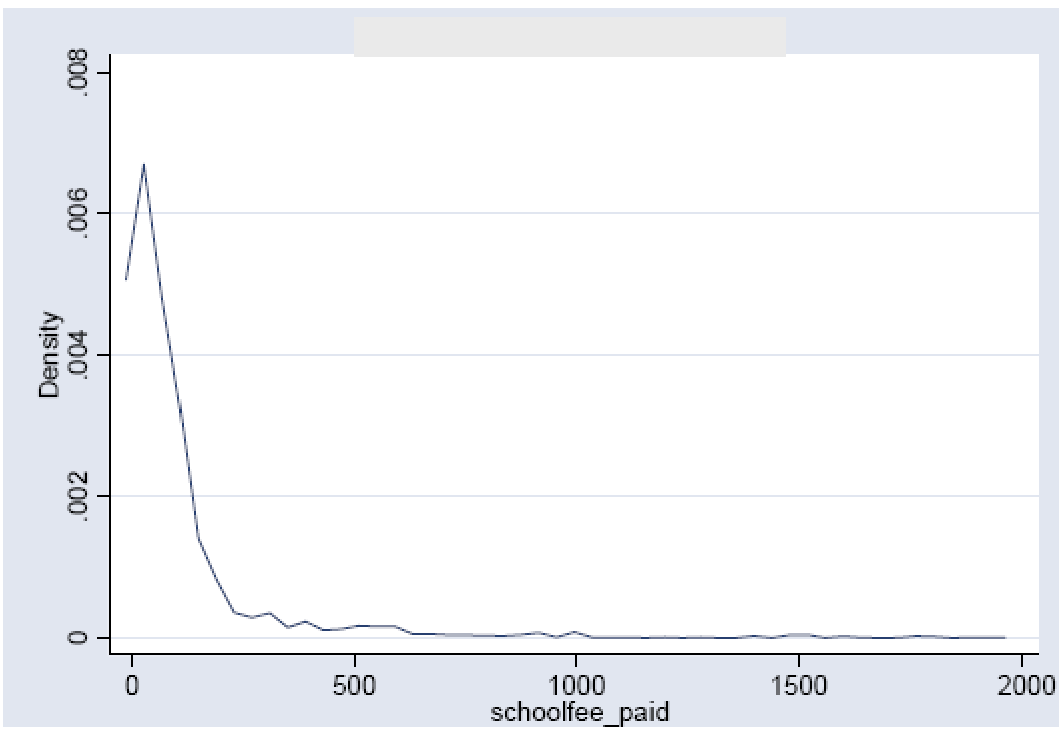
In the mathematical tests, we implemented four types of numerical tests for children age 7 to 9—summation, subtraction, multiplication, and division. The four

Table 1—Descriptive statistics: Schooling

Age	Age started		Grades completed		Grades repeated	
6	5.288	(0.756)	0.714	(0.756)	0.143	(0.378)
7	5.693	(0.922)	0.894	(0.689)	0.256	(0.597)
8	5.921	(0.829)	1.597	(0.764)	0.366	(0.540)
9	6.150	(0.989)	2.443	(1.049)	0.343	(0.563)
10	6.171	(1.064)	3.216	(1.168)	0.552	(0.777)
11	6.048	(1.434)	4.123	(1.398)	0.628	(0.876)
12	6.216	(1.470)	5.121	(1.469)	0.609	(0.897)
13	6.220	(1.281)	6.038	(1.480)	0.616	(0.946)
14	6.431	(1.322)	6.869	(1.468)	0.711	(0.968)
15	6.485	(1.282)	7.559	(1.445)	0.925	(1.047)
16	6.406	(1.399)	8.534	(1.855)	0.776	(1.016)
17	6.516	(1.511)	9.036	(1.790)	1.195	(1.241)
18	6.665	(1.951)	9.604	(2.052)	1.227	(1.223)
19	7.189	(1.772)	9.875	(1.815)	1.263	(1.186)
20	7.390	(2.140)	9.889	(2.539)	1.188	(1.106)

Notes: Means are shown with standard deviations in parentheses. There are seven observations in the age 6 group, although KwaZulu-Natal Income Dynamics Study Wave 3, Section 12.2 targets children aged 7 to 20.

Figure 2—School fee distribution



questions are 3 + 5 (summation), 7 − 3 (subtraction), 2 × 6 (product), and 12 ÷ 4 (division). Table 2 reports observations with correct and incorrect answers. Note that the sample size for each age group is nearly the same. First, the likelihood of giving a correct answer increases as age increases for all four questions. Second, the difficulty increases as we move from summation to division.

Table 2—Descriptive statistics: Math results

	Age group		
	7 year old	8 year old	9 year old
Number of observations	218	213	205
Summation			
Correct	157	186	189
Incorrect	61	27	16
Subtraction			
Correct	117	157	166
Incorrect	101	56	39
Product			
Correct	60	98	131
Incorrect	158	115	74
Division			
Correct	15	44	77
Incorrect	203	169	128

Table 3 reports the determinants of attritions from the 1998 to the 2004 rounds. Since our main analysis focuses on variations among siblings, controlling for household fixed effects, attritions at the individual level are of interest. Given observations in the 1998 round, our concern here is to determine whether the probability of being observed in the 2004 round depends on explanatory variables used in the schooling investment and outcome equations. The sample is restricted to children from households found in 2004 who were between the ages of 1 and 5 in 1998 with values of height-for-age Z-scores between –6 and 6,.,.[15]

Specifications include household fixed effects with initial age and gender indicators. Columns 1 and 2 use household fixed effects based on the 1998 and 2004

[15] In the analysis of nutrition-height effects on schooling, we screen out observations of children that show inconsistent ages in the two rounds (for example, age 6 in 2004 corresponds to age 0 or 1). In the analysis of age started school, we exclude those who were already enrolled in 1998. Mostly for the first reason, sample size differs between the above attrition analysis and schooling analysis. Screening observations of inconsistent ages between 1998 and 2004 based on the 2004 survey excludes large height-for-age Z-scores due to understated age in 1998.

Table 3—Attritions

Dependent = 1 if observed in 2004, and 0 otherwise
Estimation: linear probability model

	1998 to 2004				1993 to 2004	
	(1)	(2)	(3)	(4)	(5)	(6)
Height for age Z-score (Haz)	0.0081	0.0045	0.0321	0.0454	0.0126	-0.0220
	(0.62)	(0.31)	(1.25)	(1.57)	(0.83)	(0.58)
Age =1 * Haz						0.0372
						(0.80)
Age =2 * Haz			-0.0487	-0.0886		0.0201
			(1.34)	(2.04)		(0.42)
Age =3 * Haz			-0.0263	-0.0261		0.0747
			(0.70)	(0.63)		(1.54)
Age =4 * Haz			-0.0093	-0.0207		0.0562
			(0.23)	(0.47)		(1.04)
Age =5 * Haz			-0.0298	-0.0616		0.0226
			(0.83)	(1.55)		(0.37)
Age =1					-0.0256	0.0028
					(0.34)	(0.03)
Age =2	0.0750	0.1585	0.0466	0.1007	0.0145	0.0221
	(1.27)	(2.35)	(0.74)	(1.36)	(0.20)	(0.26)
Age =3	0.0632	0.1125	0.0565	0.1212	-0.0425	0.0265
	(1.10)	(1.76)	(0.89)	(1.68)	(0.60)	(0.32)
Age = 4	0.0636	0.0940	0.0730	0.1039	-0.0840	-0.0312
	(1.08)	(1.40)	(1.11)	(1.39)	(1.12)	(0.33)
Age =5	0.1176	0.1588	0.1071	0.1236	0.0082	0.0179
	(1.88)	(2.29)	(1.61)	(1.69)	(0.11)	(0.19)
Female	-0.0503	-0.1069	-0.0529	-0.1126	0.0329	0.0353
	(1.24)	(2.29)	(1.29)	(2.38)	(0.76)	(0.80)
Household fixed effects	1998	2004	1998	2004	1998	1998
Number of observations	516	516	516	516	689	689
Adjusted R-squared	0.1132	0.1121	0.1036	0.1188	0.2266	0.2218

Notes: Numbers in parentheses are absolute *t*-values. Sample consists of children with height for age Z-score between -6 and 6 in households observed in 2004. There were 45 attritions out of 516 children in the period 1998–2004 and 145 attritions out of 689 children in the period 1993 to 2004.

households, respectively. During the six-year interval between the two rounds, households were split and young people formed new households. The analysis in the next section uses the 2004 household definition, since decisions regarding child schooling are supposed to be made in current household units. In both cases, age in 1998 affects the likelihood of being observed in 2004. This suggests that child mortality and mobility depends on the age of the child. Columns 3 and 4 include the interactions of Z-scores and age in 1998, which again shows that even in each age group, height does not affect the attrition probability. Since age is controlled as a fixed effect in schooling equations, estimation of equation (1) is robust to attrition bias. Similarly, Columns 5 and 6 examine attritions from 1993 to 2004. Results confirm that attritions were not associated with height-for-age Z-score in 1993 (and interactions with the 1993 age), so we do not have to control attrition probability in the following analyses.

5. Outcomes at the Early Stage of Schooling

Age Started School

Table 4 shows the effect of the height-for-age Z-score in 1998 on the age started school. The sample consists of children aged 1 to 3 in 1998.[16] Column 1 controls only cluster-level fixed effects, while column 2 reports estimates for siblings. The specifications include current age indicators to control cohort effects. In column 1, greater child height is found to significantly lower the age when the child started school, though this estimate is likely to be biased due to a correlation between household-level factors and child height.

Table 4—Age started school

Dependent: Age started school

	(1)	(2)
Height-for-age Z-score 1998	-0.1036	-0.3568
	(2.99)	(2.75)
Female	-0.0722	-0.0377
	(0.57)	(0.11)
Current age fixed effects	Yes	Yes
Cluster fixed effects	Yes	
Household fixed effects		Yes
R-squared	0.3994	0.9829
Number of observations	215	215

Notes: Numbers in parentheses are absolute t-values. Robust standard errors are used in Column 1. Sample consists of children aged 1 to 3 in 1998, with consistent ages in 1998 and 2004, height-for-age Z-score in the range of –6 to 6 in 1998, and age started school equal to or above 4.

[16] Since older children are more likely to be already in school than younger children, this selection of the sample affects estimated height effects on age started school. If the height effect is positive, those who are relatively well endowed are likely to be dropped from the sample, especially among older children (ages 4 and 5). As a result, the selection biases the estimate upward (smaller in absolute value).

Column 2 confirms the above finding, showing an even greater effect of height on age starting school. The upward bias suggests that household-specific unobservables endowment (which increases the child's age to start school) is positively correlated with the height-for-age Z-score. These results imply that early childhood malnutrition delays the age when the child starts school.[17] Current age does not matter in age started school (not shown in the table), which suggests that the decision to start primary schooling did not change between 1998 and 2004.

Grade Completed

Table 5 reports the effects of the height-for-age Z-score on years of schooling completed. Columns 1 and 2 compare estimates with cluster and household fixed effects. Though a positive significant effect is found with cluster fixed effects, the effect is negative and insignificant with household fixed effects. In both cases, girls are more likely to advance grades than boys.

To check possible nonmonotonicity, columns 3 and 4 include step functions of height-for-age Z-score in different ranges. Interestingly, an improvement in the height Z-score significantly increases years of schooling completed, but the effect turns out to be negative at large values of the Z-score.

The negative effect of height on schooling is consistent with increased opportunity cost of schooling investment. However, we also have to take into account the possibility that those observations with large Z-scores come from an understatement of ages. As discussed, the number of observations in this range of Z-scores is extremely small.

[17] A preliminary analysis showed some potential nonmonotonicity in the effect of the height Z-score on age started school. First, estimates of height effects on age started school are significantly negative among relatively short children. Marginal gain in earlier age to start school from increasing height is greater among less endowed children. Second, the numbers of observations in these estimations suggest that the effect is negative and significant in more than 95 percent of the sample. Some outlying observations from exceptionally tall children (conditional on age) change the estimates. This also holds in the analysis of grade completion and repetitions.

Table 5—Grade completed

Dependent: Grade completed

	(1)	(2)	(3)	(4)
Height for age Z-score 1998 (Haz)	0.0619	-0.0278	0.1090	0.2873
	(2.11)	(0.43)	(2.49)	(3.30)
Haz 1 to 2			0.1476	-0.5789
			(0.61)	(1.35)
Haz 2 to 3			-0.5716	-1.6777
			(1.70)	(3.33)
Haz 3 to 4			-0.3658	-2.9630
			(0.89)	(4.83)
Haz 4 to 5			-0.5666	-2.2446
			(1.45)	(2.79)
Haz 5 to 6			-1.7554	n.a.
			(1.90)	
Female	0.2396	0.5316	0.2353	0.5560
	(2.39)	(2.63)	(2.39)	(3.26)
Current age fixed effects	yes	yes	yes	yes
Cluster fixed effects	yes		yes	
Household fixed effects		yes		yes
R squared	0.6778	0.9396	0.6864	0.9607
Number of observations	348	348	348	348

Notes: Numbers in parentheses are absolute *t* values. Robust standard errors are used in columns 1 and 3. Sample consists of children aged 1 to 5 in 1998, consistent with 2004 ages, and height-for-age Z-score in the range of –6 to 6 in 1998.

The results also show that household endowment is negatively correlated with height if the Z-score is smaller than one, but it is positively correlated with height if the score is larger. If the negative effect of height on schooling attainment comes from the incentive to work outside school, unobserved household-specific factors (such as parents' knowledge) tend to prevent children from supplying their labor.

Grade Repetition

Table 6 summarizes the results on grade repetitions. The dependent variable is the cumulative number of grades repeated. Columns 1 and 2 show estimates with cluster and household fixed effects respectively. The effect of the height Z-score is insignificant in both cases. Consistent with the previous finding on grades completed, girls experience a smaller number of grade repetitions than boys.

Columns 3 and 4 include step functions of the height Z-score to capture possible nonlinearity. Though nonmonotonicity is not detected statistically in the case of cluster fixed effects, sibling estimation demonstrates convexity. For those who are relatively stunted, an improvement in height Z-score reduces grade repetitions, but this effect becomes positive among relatively tall children. Again, as in the discussion of grades completed, this result can be consistent with both increased opportunity costs of schooling investment and possibly understated ages among them.

Table 6—Grade repetition

	Cumulative number of grades repeated				Repetition rate
Dependent	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Height for age Z-score 1998 (Haz)	0.0144	0.0463	-0.0222	-0.3018	-0.0604
	(0.82)	(1.06)	(0.52)	(2.44)	(1.05)
Haz -1 to 0			0.0432	0.6668	0.1585
			(0.40)	(2.16)	(1.07)
Haz 0 to 1			0.1464	0.7940	0.1597
			(0.98)	(1.87)	(0.81)
Haz 1 to 2			0.0514	1.4229	0.5291
			(0.25)	(2.40)	(1.90)
Haz 2 to 3			0.2859	1.6810	0.4670
			(1.11)	(2.43)	(1.43)
Haz 3 to 4			0.3679	2.8266	0.8290
			(1.16)	(3.20)	(2.01)
Haz 4 to 5			0.2406	2.5727	0.8517
			(0.73)	(2.69)	(1.91)
Haz 5 to 6			0.1018	n.a.	n.a.
			(0.23)		
Female	-0.2858	-0.2491	-0.2904	-0.2263	-0.0871
	(4.27)	(1.81)	(4.25)	(1.68)	(1.34)
Current age fixed effects	yes	yes	yes	yes	yes
Cluster fixed effects	yes		yes		
Household fixed effects		yes		yes	yes
R squared	0.3422	0.8715	0.3483	0.8961	0.8838
Number of observations	347	347	347	347	343

Notes: Numbers in parentheses are absolute *t* values. Robust standard errors are used in columns 1 and 3. Sample consists of children aged 1 to 5, consistent with 2004 ages, and height-for-age Z-score in the range of –6 to 6 in 1998.

Column 5 uses the rate of grade repetition as the dependent variable for checking whether the above finding results from an understatement of age. The rate of repetition is constructed as the cumulative number of repeated grades divided by the sum of grades repeated and completed. Those who show height-for-age Z-scores above the value of one are found to experience greater repetition rates, which supports the possibility that opportunity costs can be high among them.

School Fee Expenditure

Table 7 shows the results on school fee expenditures. In addition to the height-for-age Z-score, we include its interaction with total monthly household income in 1998.[18] In column 1, which controls for cluster fixed effects, the 1998 household total monthly income has a positive and significant effect on school fee payment. The effects of the Z-score and its interaction with the initial household income are both insignificant. Relatively wealthy households are able to pay more for school fees within clusters.

Column 2 shows the sibling estimates. The specification includes the interaction between the height Z-score and household income in 1998. Since the estimation is based on household variations of the height Z-score, the 1998 household income is exogenous.[19]

While the height Z-score has a positive (marginally significant) effect on school fee expenditure, higher household income is found to reduce this effect so that parents spend more for relatively short children only if they can afford it. The former effect (height) increases income inequality among siblings (given positive returns to height in the labor market), while the latter (higher income) reduces inequality among them.

To detect possible nonmonotonicity, column 3 includes step functions of the height-for-age Z-score. Except for some significant effects from scores of between 3 and 4, the finding remains robust. Note that less than 5 percent of the full sample had a Z-score greater than 3.

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[18] In preliminary analyses, height-for-age Z-score in sibling estimation was insignificant without its interaction with the 1998 total household income, no matter what range of Z-score was used. Therefore, it is important to control heterogeneity of the initial income level.

[19] In a preliminary analysis, the 2004 household monthly income was also interacted with the height Z-score in 1998, but it was shown to be insignificant.

Table 7—School fee expenditure 1998

Dependent: School fee paid

	(1)	(2)	(3)
Height-for-age Z-score 1998 (Haz)	-15.221	23.147	66.305
	(1.27)	(1.55)	(3.06)
Monthly household income 1998	0.0272		
	(1.84)		
Income * Haz	0.0021	-0.0110	-0.0383
	(0.43)	(2.59)	(5.70)
Haz 1 to 2			-157.950
			(1.34)
Haz 2 to 3			-201.140
			(0.93)
Haz 3 to 4			-542.351
			(2.88)
Haz 4 to 5			-233.005
			(0.96)
Haz 5 to 6			n.a.
Income * Haz 1 to 2			0.0471
			(0.96)
Income * Haz 2 to 3			0.0773
			(0.71)
Income * Haz 3 to 4			0.2814
			(4.62)
Income * Haz 4 to 5			0.1129
			(0.66)
Income * Haz 5 to 6			n.a.
Age fixed effects	yes	yes	yes
Cluster fixed effects	yes		
Household fixed effects		yes	yes
R squared	0.6531	0.9853	0.9907
Number of observations	341	341	341

Notes: Numbers in parentheses are absolute *t*- values. Robust standard errors are used in column 1. Sample consists of children aged 1 to 5 in 1998, consistent with 2004 ages, height-for-age Z-score in the range of –6 to 6 in 1998, and school fee paid is less than or equal to school fee charged.

Learning Outcomes

Tables 8a and 8b report the effects of height for age on mathematics test scores. In the 2004 survey, we implemented four types of basic mathematics tests for children aged 7 to 9. There are four questions, one question for each operation: 3 + 5 (summation), 7 – 3 (subtraction), 2 × 6 (product), and 12 ÷ 4 (division). For each question, an indicator is constructed to take the value of one if the answer is correct and zero otherwise.[20]

Table 8a—Mathematics test results: Probit with cluster fixed effects

Dependent = 1 if correct answer, and 0 otherwise
Estimation: Probit

Subject	Summation	Subtract	Product	Division
Height-for-age Z-score in 1998	0.1833	0.1538	0.0722	0.0518
	(2.91)	(2.73)	(1.35)	(0.85)
Age 8 in 2004	0.8973	0.6822	1.0605	0.7011
	(3.18)	(2.87)	(4.16)	(2.20)
Age 9 in 2004	1.2776	1.4943	1.8044	1.4711
	(3.64)	(5.22)	(6.41)	(4.79)
Female	0.1683	-0.2899	-0.1835	-0.2673
	(0.74)	(1.37)	(0.85)	(1.13)
Cluster fixed effects	yes	yes	yes	yes
Pseudo R squared	0.2501	0.2094	0.2826	0.2105
Number of observations	173	211	210	184

Notes: Sample consists of children age 7 to 9 in 2004 and with consistent ages in 1998 and 2004. Numbers in parentheses are absolute *t*- values, using robust standard errors.

Table 8b—Mathematics test results: Linear probability with household fixed effects

Dependent = 1 if correct answer, and 0 otherwise
Estimation: Linear probability

Subject	Summation	Subtract	Product	Division
Height-for-age Z-score in 1998 (Haz 98)	0.0837	0.1404	0.0715	-0.0169
	(2.06)	(2.09)	(1.11)	(0.33)
Haz 98 * age 8 in 2004	-0.2067	-0.2229	-0.2681	0.0374
	(1.58)	(1.03)	(1.29)	(0.23)
Haz 98 * age 9 in 2004	-0.1818	-0.3046	-0.1393	-0.0452
	(2.10)	(2.13)	(1.01)	(0.42)
Female	0.0622	0.0564	0.3833	0.0886
	(0.67)	(0.36)	(2.58)	(0.75)
Age 2004 fixed effects	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes
Adjusted R squared	0.6121	0.3172	0.4354	0.4361
Number of observations	260	260	260	260

Notes: Sample consists of children aged 7 to 9 in 2004 and with consistent ages in 1998 and 2004. Numbers in parentheses are absolute *t*- values, using robust standard errors.

[20] Table 1 summarizes descriptive statistics.

Table 8a shows Probit results with cluster fixed effects. There are two observations. First, significantly positive effects are found in summation and subtraction (relatively easy computations). The point estimate decreases as the difficulty of calculation advances. Second, age has a significantly positive effect on the probability of answering correctly. These estimates are again likely to be biased due to omitted household factors.

Table 8b reports sibling estimates, where the effects of the height Z-scores are differentiated by age in 2004. In preliminary analyses, without these age-height interactions, we have not found any significant effects of child height.[21] First, it is found that in summation and subtraction, height Z-score significantly increases the probability of making correct answers among children age 7, but not in the other age groups. Interestingly, the effects will decrease as children age. The lagged nutrition effect is significant in the transitional phase from pre-primary to primary school stages.

6. Outcomes at the Later Stage of Schooling

This section summarizes our findings on the effects of the children's height Z-score in 1993 on schooling outcomes in 2004. Before discussing the results, we also must note that South African education was in its historical transition from apartheid to democracy during the period 1993–96, and the South African School Act and Norms and Standards were introduced in 1996, making it compulsory for the school system to be nonsegregated. Due to these social changes, it is also expected that the estimation results are subject to imprecision.

It is also important to note that by 2004, this group of children was also in transition from primary to secondary stages of education. This may create heterogeneity by age in the effects of the height Z-score on schooling outcomes. Therefore, it is equally important to examine possible variations in the height effect by age as well as the

[21] The finding implies that household factors are positively correlated with both child height and learning outcomes (both conditional on age).

nonmonotonicity. The role of positive returns to health is greater among elder children than younger.

In the analysis of age started school results in Table 9, we expect more measurement errors than is the case in the 1998 sample due to recall problems. Columns 1 and 2 show sibling estimates (household fixed effect). The height Z-score has no significant effect on age started school in this group. Column 3, which uses cluster fixed effects instead of household fixed effects, shows that a greater height Z-score decreases the age they started school for those in the age-14 group (compared with the age-11 group), who were already age 3 in 1993 (before the political transition). All these results show that girls are likely to start school earlier than boys.

Table 9—Age started school—1993 sample

Dependent variable: Age started school

	(1)	(2)	(3)
Height-for-age Z-score 1993	0.0467	0.0654	0.0733
	(0.46)	(0.36)	(1.00)
* Age 12		0.0904	-0.1193
		(0.36)	(1.16)
* Age 13		-0.0696	-0.1529
		(0.34)	(1.42)
* Age 14		-0.2339	-0.4033
		(0.67)	(2.67)
Female	-0.4273	-0.4949	-0.2604
	(1.46)	(1.60)	(1.77)
Age fixed effects	yes	yes	yes
Cluster fixed effects			yes
Household fixed effects	yes	yes	
R squared	0.9338	0.9370	0.4567
Number of observations	230	230	230

Notes: Numbers in parentheses are absolute *t*- values. Robust standard errors are given in column 3. Sample consists of children aged 0 to 5, consistent with 2004 ages, and height-for-age Z-score in the range of –6 to 6 in 1993.

Table 10 shows the effects of the height-for-age Z-score on grades completed and repeated. All the specifications control household fixed effects. Column 1 has only the height Z-score, which shows its insignificance. Age heterogeneity is controlled in column 2. It is interesting to know that an improvement in height Z-score marginally increases years of schooling completed, but it is likely to decrease grades completed as they age (conditional on age). It is possible that greater health capital may discourage further schooling from primary to secondary stages, given the positive returns to health in the labor market. To confirm the robustness of this finding, column 3 includes step functions of the height Z-score. Contrary to the previous findings for younger children (and cohorts), nonlinearity is not found in the highest grade completed.

Table 10—Grade completed and repeated—1993 sample

	Grade completed			Grades repeated		
Dependent	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Height-for-age Z-score 1993 (Haz)	-0.0149	0.2829	0.2964	0.0227	-0.2904	-0.2887
	(0.22)	(1.77)	(1.78)	(0.39)	(2.17)	(2.11)
* Age 12		-0.4861	-0.4933		0.3043	0.3135
		(2.48)	(2.36)		(1.87)	(1.83)
* Age 13		-0.3052	-0.2816		0.3469	0.3478
		(1.71)	(1.46)		(2.33)	(2.16)
* Age 14		-0.0185	-0.0163		0.4438	0.4495
		(0.07)	(0.06)		(2.09)	(2.11)
* Age 15		-0.4692	-0.4743		0.6239	0.6447
		(1.61)	(1.60)		(2.58)	(2.64)
* Age 16		-0.2362	-0.1917		0.2526	0.1665
		(0.74)	(0.59)		(0.91)	(0.58)
Haz 1 to 2			-0.0653			-0.2382
			(0.13)			(0.54)
Haz 2 to 3			0.4461			-0.6461
			(0.55)			(0.96)
Haz 3 to 4			-0.6579			0.5029
			(0.68)			(0.63)
Haz 4 to 5			n.a.			n.a.
Haz 5 to 6			n.a.			n.a.
Female	0.7201	0.7202	0.7383	-0.4398	-0.4161	-0.4280
	(3.63)	(3.64)	(3.68)	(2.69)	(2.54)	(2.59)
Age fixed effects	yes	yes	yes	Yes	yes	yes
Household fixed effects	yes	yes	yes	Yes	yes	yes
R squared	0.9486	0.9539	0.9545	0.8583	0.8719	0.8755
Number of observations	379	379	379	377	377	377

Notes: Numbers in parentheses are absolute *t*- values. Sample consists of children aged 0 to 5, consistent with 2004 ages, and height-for-age Z-score in the range of –6 to 6 in 1993.

Columns 4 to 6 report on grade repetition. In Column 5, similar to the results on grade completion, the height Z-score has a negative effect on the age 11 group, but the effect is positive among older groups. This finding suggests that (conditional on age) greater health capital may discourage further investments in schooling at the transition stage from primary to secondary schools. Column 6 investigates potential nonlinearity by introducing step functions, which again show insignificance of nonlinearity in grade repetition.

In both grades completed and repeated, girls perform better than boys. However, preliminary analysis shows that gender does not matter in the effect of the height Z-score on these schooling outcomes.

Finally, Table 11 reports the effects of the height Z-score on school fee payment. Interestingly, the results show opposite signs in the key parameter estimates. Columns 1 and 2 use monthly household income in 1993 and 2004, respectively. In both cases, the height Z-score decreases the school fee paid, while an increase in income augments school-fee expenditure to healthy children. The difference between Tables 11 and 7 may result from (1) possible differences in the education system that these cohorts experienced during this transitional stage of South African education, (2) differences in parents' attitude to school fee expenditure and school choice behavior between early primary school and primary-to-secondary school stages. However, the results show a

Table 11—School fee expenditure—1993 sample

Dependent variable: School fee paid

	(1)	**(2)**
Height-for-age Z-score 1993 (Haz)	-245.68	-361.79
	(1.92)	(3.47)
Household monthly income 1993 * Haz	0.2153	
	(2.57)	
Household monthly income 2004 * Haz		0.1283
		(5.16)
Age fixed effects	yes	yes
Household fixed effects	yes	yes
R squared	0.7090	0.7686
Number of observations	366	329

Notes: Numbers in parentheses are absolute *t*- values. Sample consists of children aged 0 to 5, consistent with 2004 ages, height-for-age Z-score in the range of –6 to 6 in 1993, and school fee paid less than or equal to school fee charged.

process of increasing inequality among children, given positive returns to health capital and the positive association between school fee expenditure and school quality.

7. Conclusions

This paper examines the effect of early childhood health capital on schooling investments and outcomes, using panel data from South Africa. Good nutrition and health in early childhood are thought to be a precondition for child development and school learning at subsequent stages. However, in an environment where children may contribute to household income, as in developing countries where health capital has positive economic returns in the labor market, child health augments not only the efficiency of human capital production at the schooling stage but also the labor-market wage. Therefore the opportunity costs of going to school are higher.

Nutrition intake and health capital in early childhood, measured by the height-for-age Z-score of pre-primary-school-aged children, enhance schooling investments and improve the outcomes. That is, children who are well nourished and in good health start school at an earlier age, progress further, and repeat fewer grades. We also found that some taller children (Z-score above two) perform worse than shorter children, but since this segment of observations is very small in our sample, it is difficult to generalize this nonmonotonicity. Instead, the analysis with a longer panel data of 11 years suggests that good health may discourage further investments in schooling at the stage of transition from primary to secondary school, when a better health status may reinforce incentives to go to work. In the early stage of schooling, parents attempt to narrow the gaps in total human capital and future incomes among siblings by increasing expenditures on education for children with smaller health capital as household income increases. However, unequal allocation of resources among siblings is found to increase inequality at later stages. This difference may reflect some changes in the education system in South Africa during this period, or changes in parents' behavior that may happen when the child moves from primary to secondary school.

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