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What Determines Adult Cognitive Skills? Influences of Pre-Schooling, Schooling, and Post-Schooling Experiences in Guatemala

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Keywords
Cognitive skills, Guatemala, Nutrition, Schooling

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What Determines Adult Cognitive Skills?
Influences of Pre-Schooling, Schooling, and Post-Schooling Experiences in Guatemala*

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Journal of Economic Literature Classification Codes: I.2 Education, O.1 Economic Development

Section 1. Introduction

Increasing the stock of human capital is central to expanding individual options and to economic development. One dimension of human capital, cognitive skills—the capacity to assess and solve problems, or in T.W. Schultz's (1975) memorable phrase, "the ability to deal with disequilibria"—is widely believed to affect productivity in many activities, including work, raising children, and improving one's own and others' health and nutrition. Substantial empirical literature, mostly based on associations between schooling attainment and the outcomes of adult activities, is interpreted in support of this proposition.[1]

Consequently, it is critical to understand the processes by which dimensions of adult human capital, such as cognitive skills, are determined. This may seem to be well-trodden territory, as there are hundreds of studies of the determinants of schooling in Latin America and elsewhere.[2] But schooling attainment is not a direct measure of adult cognitive skills. As such, it is only one of the potential inputs, although likely an important one, into the production of those skills. Moreover, in developing countries, schooling typically is limited to particular periods of individuals' lives, childhood and adolescence. Other experiences, both before and after schooling years, also may affect cognitive skills. For example, a substantial literature emphasizes the importance of nutrition from conception onward for neural and cognitive development.[3] There is also a substantial literature that emphasizes the importance of post-schooling experiences, particularly in the labor market, in determining adult cognitive skills or in determining productivity and wages, which usually are interpreted as reflecting such skills.[4] If pre-schooling or post-schooling experiences have significant impacts on the cognitive skills of adults *and* are correlated with schooling—as is likely if there is correlation among human capital investments across life-cycle stages—analysis that uses schooling attainment alone to represent adult cognitive skills are likely to misrepresent the production process, including the role of schooling itself.

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[1] For example, there are hundreds of empirical studies that are interpreted as showing the effect of cognitive and other skills obtained through education on wages or incomes and the vast majority of them use schooling attainment to represent these skills (Psacharopoulos and Patrinos 2004). There is a smaller number of studies that use direct measures of adult cognitive skills (including Boissiere et al. 1985; Murnane et al. 1995; Alderman et al. 1996; and Glewwe 1996). The many empirical studies of the effects of cognitive and other skills on outcomes such as health, nutrition, and fertility nearly all use schooling attainment to represent these skills (Strauss and Thomas 1998).

[2] See references in the surveys by Strauss and Thomas (1995) and Behrman (2009).

[3] See, for example Glewwe and Jacoby (1995), Engle et al. (2007), Heckman (2007), Grantham-McGregor et al. (2007), and Victora et al. (2007).

[4] There is considerable emphasis on post-schooling learning both "on-the-job" and through formal training programs. Standard earnings functions, whether motivated by a human capital investment models (e.g., Mincer 1974) or as hedonic price indices (e.g., Rosen 1974), generally include some measure of post-schooling work experience.

We examine the importance of pre-schooling, schooling, and post-schooling experiences in the production of two representations of cognitive skills of adults: reading-comprehension and nonverbal cognitive skills.[5] We do this using data from Guatemala, a low-income context in which it is widely expected that greater cognitive skills will lead to increases in individuals' options and welfare, and subsequently advance economic development. Specifically, we investigate the following questions related to the determination of adult cognitive skills:

- How important is schooling attainment?
- How important are pre-schooling and post-schooling experiences?
- Does controlling for the possibility that schooling, as well as pre-schooling and post-schooling, experiences are behavioral choices change their apparent importance?

We find that (1) schooling has a significant and substantial effect on adult reading comprehension, although not on adult nonverbal cognitive skills; (2) both pre-schooling and post-schooling experiences—represented respectively by the nutritional status of the individual as a pre-schooler and years of skilled post-school work experience—have substantial and significant effects on both of the adult cognitive skill measures; and (3) failure to control for the behavioral determinants of these experiences leads to a substantial overestimate of the importance of schooling and a substantial underestimate of the importance of pre- and post-schooling experiences.

We utilize rich data from Guatemala that have been collected over 35 years (1969–2004) with sample members 25–42 years of age during the 2002-4 survey round and with substantial prospective and recall information on their development through the pre-schooling, schooling, and post-schooling years, as well as on plausibly exogenous factors that conditioned these experiences. We investigate the effect of experiences during these three life-cycle stages on adult cognitive skills, incorporating the possibility that the experiences reflect behavioral choices in the presence of unobserved factors such as genetic endowments that might affect adult cognitive skills directly, as well as indirectly via their effects on the different life-cycle stage experiences.

Section 2. Conceptual Framework

[5] We are unaware of studies that estimate adult cognitive skills production functions, though there is work in which what might be called cognitive skills production functions for students are estimated as functions of inputs such as teacher training, teacher/student ratios, availability of books and other attributes of schools (see Hanushek 1996, as well as more recent work such as Todd and Wolpin 2003, 2007 and Cunha and Heckman 2008, 2010). These studies do not consider prime-age adults, the endogeneity of inputs, or the impacts of pre-school nor post-school experiences.

We estimate production functions for two types of adult cognitive skills, reading-comprehension skills (RCS) and nonverbal skills (NVS). We posit that each of these skill types is produced by: a vector of genetic and other observed and unobserved endowments related to learning capacities and motivations ($\mathbf{E_0}$); previous experiences (E_i, i=1, 2, 3 for the three life-cycle stages defined below); and a stochastic term (U) reflecting all other idiosyncratic, and assumed exogenous, learning experiences. Because of the nearly exclusive emphasis in the literature on schooling, its plausibly central role in forming cognitive skills, and the structure of the data, we organize life-cycle experiences into the following three life-cycle stages:

Stage 1: pre-schooling (from conception through about age six years old),

Stage 2: schooling (from age seven through about age 15 years old),[6]

Stage 3: post-schooling (from about age 15 years old to age at the time of survey, i.e., 25–42 years old).

The production functions for the two types of cognitive skills (K) are[7]

(1r) $K_{3r} = K_{3r}^{\mathrm{p}}$ ($\mathbf{E_1, E_2, E_3, E_{0r}}$, U_{3r}) and

(1n) $K_{3n} = K_{3n}^{\mathrm{p}}$ ($\mathbf{E_1, E_2, E_3, E_{0n}}$, U_{3n}).

The first subscript refers to the life-cycle stage, the second subscript refers to reading-comprehension cognitive skills (r) or nonverbal cognitive skills (n), and the superscript p indicates that the relation is a production function. All variables except the left-side dependent variables and the disturbance terms are potentially vectors (indicated in boldface), although in the presentation and subsequent empirical analysis, we treat $\mathbf{E_1}$, $\mathbf{E_2}$, and $\mathbf{E_3}$ as scalars E_1, E_2, and E_3.

The questions posed in the introduction pertain primarily to the first derivatives (i.e., K^{p}_{E1}, K^{p}_{E2}, K^{p}_{E3}) of the general adult cognitive skills production function. If K_{E1} is significantly positive and E_1 and E_2 are positively correlated, for example, a specification that excludes E_1 is likely to overestimate the effect of school-years experience (E_2). Estimation of relation (1) is challenging, however, because the experiences for the three life-cycle stages on the right side all reflect previous behavioral choices.

To motivate our modeling of these life-cycle stage experiences and to elucidate the possible influences of the endowments on estimates that do not control for them, we describe a stylized

[6] The legal starting age for schooling in Guatemala is seven years of age and 82% of the respondents in the sample completed schooling by age 15 years.

[7] An alternative production function specification is a value-added form in which the change in cognitive skills across stages (periods) is posited to depend on the experiences at the end of the previous stage (Todd and Wolpin 2003, 2007). For example, the change in cognitive skills during the school years is posited to depend on the intervening schooling. We do not have sufficient data to explore this specification.

model in which the "dynasty" (first the parents, then the children themselves as they age into youth and adulthood) make decisions as if they were maximizing a welfare function (W) for each individual in adulthood that depends, *inter alia*, on the adult cognitive skills of that individual:

(2) $W = W(K_{3r}, K_{3n}, \ldots U_{3W})$.

For instance, W might represent consumption that is financed by resources generated by labor earnings that depend in part on cognitive skills. Welfare is maximized subject to the constraints at each life-cycle stage related to relevant current and expected production functions, family resources allocated to this individual, community characteristics (including community services and markets that affect household decisions), and stochastic factors.

Life-Cycle Stage 1 (pre-schooling ages): The parents allocate resources to obtain the optimal E_1 given a production function and the current community-determined options (e.g., availability of nutritional and health programs), expected future community-determined options (e.g., schooling options in the second life-cycle stage and labor market options in the third), the expected relation between E_1 and W, and child-specific endowments. The E_1 production function is

(3) $E_1 = E_1^{p} (\mathbf{N_1}, \mathbf{C_{1p}}, \mathbf{E_{0p}}, U_{1E})$,

where $\mathbf{N_1}$ is a vector of family-determined inputs into the production of E_1 (e.g., family-provided nutrients), $\mathbf{C_{1p}}$ is a vector of community inputs into the production of E_1 (e.g., community-provided nutrients, community-provided pre-school programs), $\mathbf{E_{0p}}$ is the vector of child endowments that directly enter into the production of E_1 (e.g., innate robustness), and U_{1E} is a stochastic disturbance term that directly affects the production of E_1 (e.g., fluctuations in the infectious disease environment). Parents choose the inputs $\mathbf{N_1}$ and therefore E_1 to maximize the expected welfare W, given: a vector of parental family resources such as parental schooling and assets ($\mathbf{F_1}$); all relevant community characteristics for this life-cycle stage $\mathbf{C_1}$ (which includes the community characteristics that directly affect the production of E_1 through $\mathbf{C_{1p}}$, but also other community characteristics that may affect the household through other channels); all of the child endowments $\mathbf{E_0}$ (which includes $\mathbf{E_{0p}}$, but also $\mathbf{E_{0r}}$ and $\mathbf{E_{0n}}$ that affect the decision to invest in E_1 because the impact of E_1 on W in general may depend on these other endowments); all the stochastic terms that affect outcomes in the first life-cycle stage of the child U_1 (which includes U_{1E} but also other stochastic factors that affect the family during the first life-cycle stage for this child, for example, stochastic factors affecting the health of other siblings may influence the inputs devoted to this child); and lastly the vectors of expected values of these variables in the next two life-cycle stages ($\mathbf{F_{12}^{e}}$, $\mathbf{F_{13}^{e}}$, $\mathbf{C_{12}^{e}}$, $\mathbf{C_{13}^{e}}$, U_{12}^{e}, U_{13}^{e}, where the superscript *e* indicates that the variable is an expected value, the first subscript refers to the life-cycle stage *at* which the expectations are held, and the second subscript refers to the stage *for* which the expectations are held) because the optimal decision for investing in E_1 to maximize W depends in part on expectations of these variables over the next two life-cycle stages. The resulting relation is

(4) $E_1 = E_1^d\,(\mathbf{F_1}, \mathbf{C_1}, \mathbf{E_0}, U_1, \mathbf{F_{12}^e}, \mathbf{F_{13}^e}, \mathbf{C_{12}^e}, \mathbf{C_{13}^e}, U_{12}^e, U_{13}^e)$,

where the superscript d indicates that it is a reduced-form demand relation.

Life-Cycle Stage 2 (schooling ages): The dynasty (initially the parents but increasingly the child) decides on the schooling attainment E_2 of the child/youth conditional on (a) the outcome of Stage 1, E_1, (b) life-cycle Stage 2 family, community, and stochastic factors, and (c) the expected values of those factors for life-cycle Stage 3. Using (4), this yields the reduced-form demand relation for E_2:

(5) $E_2 = E_2^d\,(\mathbf{F_1}, \mathbf{C_1}, \mathbf{E_0}, \mathbf{F_{12}^e}, \mathbf{F_{13}^e}, \mathbf{C_{12}^e}, \mathbf{C_{13}^e}, \mathbf{F_2}, \mathbf{C_2}, \mathbf{F_{23}^e}, \mathbf{C_{23}^e}, U_1, U_2, U_{12}^e, U_{13}^e, U_{23}^e)$.

If the outcome of Stage 1, E_1, encompasses all the impacts of first life-cycle stage exogenous variables,[8] then the conditional (on E_1) demand function for E_2 is

(6) $E_2 = E_2^c\,(E_1, \mathbf{F_2}, \mathbf{C_2}, \mathbf{F_{23}^e}, \mathbf{C_{23}^e}, U_2, U_{23}^e)$,

where the superscript c refers to a conditional reduced-form demand relation. This relation allows for the possibility, for example, that the investment in schooling is dependent on pre-school nutritional status.

Life-Cycle Stage 3 (post-schooling ages): The dynasty (primarily the post-school youth/young adult, but perhaps with some input from parents) decides on the individual's post-schooling experience E_3 conditional on (a) the outcome of Stage 1 E_1, (b) the outcome of Stage 2 E_2 and (c) life-cycle Stage 3 family, community, and stochastic factors, yielding the reduced-form demand relation for E_3:

(7) $E_3 = E_3^d\,(\mathbf{F_1}, \mathbf{C_1}, \mathbf{E_0}, \mathbf{F_{12}^e}, \mathbf{F_{13}^e}, \mathbf{C_{12}^e}, \mathbf{C_{13}^e}, \mathbf{F_2}, \mathbf{C_2}, \mathbf{F_{23}^e}, \mathbf{C_{23}^e}, \mathbf{F_3}, \mathbf{C_3}, U_1, U_2, U_3, U_{12}^e, U_{13}^e, U_{23}^e)$.

The conditional demand function for E_3 under the assumption that the outcome of Stage 2, E_2, encompasses all the impacts of second life-cycle stage predetermined variables (including E_1) is

(8) $E_3 = E_3^d\,(\mathbf{E_2}, \mathbf{F_3}, \mathbf{C_3}, U_3)$.

This relation allows for the possibility, for example, that post-schooling experiences depend, *inter alia*, on schooling (and, through schooling, on pre-school nutritional status).

Some implications of the model for estimation of adult cognitive skills production functions are the following:

[8] That the pre-school experience encompasses all the effects of pre-school exogenous variables is a strong assumption that we relax in the empirical specifications. We make it here and in relation (8) to highlight the possible dependence of one life-cycle experience on the previous life-cycle experience(s).

(1) *Omitted variable biases from omitted life-cycle experiences*: If the true relations ($1r$) and ($1n$) include all three life-cycle experiences but estimated relations exclude one or more of those life-cycle experiences, important omitted variable biases are likely. The endowments and the actual or expected values of the family, community, and stochastic factors for the three life-cycle stage experiences are all on the right side of each of the reduced-form demand relations for the three life-cycle stage experiences (relations 4, 5, and 7), and therefore the three life-cycle experiences are likely correlated. The same conclusion can be drawn from the conditional demand functions (relations 6 and 8) in which the school-age experience (E_2) depends explicitly on the pre-school age experience (E_1) and the post-school-age experience (E_3) depends explicitly on the school-age experience (E_2) and indirectly, through the school-age experience, on the pre-school age experience (E_1). That the three life-cycle stage experiences are likely to be correlated is also intuitive. A priori, a child with better parental family background or who lives in a community with better health and educational services or employment opportunities, is likely not only to have more schooling but also better pre- and post-schooling experiences.

(2) *Endogeneity biases*: Even if all three life stages are included, estimates of relations ($1r$) and ($1n$) that do not control for their behavioral determinants are also likely to be biased. Moreover, the signs of such biases are ambiguous. For instance, the "ability bias" on which the returns-to-schooling literature often focuses is consistent with positive correlation between E_2 (schooling) and the innate ability component of $\mathbf{E_0}$, suggesting a likely upward bias on schooling in ordinary least squares (OLS) estimates of relations ($1r$) and ($1n$). However, if the summary measure of pre-schooling experience is some variable such as child nutritional status (see Section 3 below), and if ability and physical endowments that influence nutritional status are negatively correlated as suggested by Behrman and Rosenzweig (2002), coefficient estimates for stunting of relations ($1r$) and ($1n$) also may be negatively biased at the same time that coefficient estimates for schooling are positively biased.[9]

(3) *Potential instruments to identify all three life-cycle experiences needed*: The reduced-form demand relations for the three life-cycle stage experiences in relations (4), (5), and (7) suggest potential instruments to be used to identify the different life-cycle experiences in the adult cognitive achievement production functions in relations ($1r$) and ($1n$). Although some instruments may seem

[9] If the correctly-specified regression model is $K_{3r} = \mathbf{E}\boldsymbol{\beta}_1 + \mathbf{E_{0r}}\boldsymbol{\beta}_2 + U_{3r}$, where $\mathbf{E}$ is a vector with the three life-cycle experiences and $\mathbf{E_{0r}}$ is a vector with unobserved endowments, then the standard omitted variable result is that $E[\mathbf{b}_1] = \boldsymbol{\beta}_1 + \mathbf{P_{12}}\boldsymbol{\beta}_2$ where $\mathbf{P_{12}}$ is the variance-covariance matrix between $\mathbf{E}$ and $\mathbf{E_{0r}}$. With certain structures of $\mathbf{P_{12}}$, this can lead to the first component of $\boldsymbol{\beta}_1$ (for E_1) being negatively biased and the second component of $\boldsymbol{\beta}_1$ (for E_2) being positively biased in OLS estimates, even if all components of $\boldsymbol{\beta}_2$ are positive.

likely to have first-order effects on particular life-cycle experiences (e.g., pre-school nutrition programs on E_1, school characteristics on E_2, labor market characteristics on E_3), the three reduced-form demand relations indicate that the same endowments and the actual or expected values of the family, community, and stochastic factors can all influence each of the three life-cycle stages. Therefore, there is a potential set of instruments that identifies the set of life-cycle experiences. This also means that it would not be a test of the plausibility of the instruments to examine if subsequent life-cycle stage family- or community-level variables are significant in previous life-cycle stage outcome equations (e.g., if schooling characteristics or post-schooling labor market characteristics significantly determine pre-schooling experience E_1) because the (expected) values of those variables should be included.

(4) *Instruments must be uncorrelated with the disturbance term (including unobserved endowments) to be valid*: The reduced-form demand relations indicate the potential set of instruments, but not all of the right-side variables in them are likely to be valid instruments in the sense of being uncorrelated with the disturbance terms in relations (1*r*) and (1*n*). For example, previous studies make clear that there can be intergenerational correlations of endowments through genetics and probably other means (Behrman and Rosenzweig 2002, 2005; Stein et al. 2003). To avoid this possible correlation, we do not include variables such as parental schooling and wealth as instruments in our primary models.

Section 3. Data

Data demands are considerable for estimating the adult cognitive skills production functions posited in Section 2. We use rich data longitudinal data from Guatemala collected over a 35-year period with measures of adult cognitive skills, socioeconomic, and anthropometric measures at different points in the life cycle, shocks from an experimental intervention and a major earthquake, and other market and policy changes.

Section 3.1 The INCAP Experimental Nutritional Intervention

In 1969 the Institute of Nutrition of Central America and Panama (INCAP), based in Guatemala, began a nutritional supplementation trial (Martorell et al. 1995). After screening approximately 300 villages, two sets of village pairs (one pair of “small” villages with about 500 residents each and the other “large” villages with about 900 residents each) in eastern Guatemala were selected purposively for the trial. Two of the villages, one from within each pair matched on population size were assigned randomly to receive as a dietary supplement a high protein-energy drink, *atole*. The other two villages received an alternative no-protein, low-energy supplement, *fresco*.

Collection of the data we use in this paper began with the supplementation trial in these four villages. From February 1969 to February 1977, INCAP implemented the nutritional supplementation program, together with data collection on child growth and development. The survey associated with the intervention focused on all children in the four villages aged under seven years of age with children added to the study when they were born. Data collection for individual children ceased when they reached seven years of age or when the study ended, whichever came first. The children included in the 1969–77 longitudinal survey were thus born between 1962 and 1977. Therefore the length and timing of exposure to the intervention for specific children depended on their individual birth dates. For example, only children born after early-1969 and before February 1974 were exposed to the intervention for all of the time from birth to 36 months of age, posited in the nutritional literature (Section 3.2) to be a critical time period for child growth. *Atole* and *fresco* were distributed in each village in centrally-located feeding centers and were available daily, on a voluntary basis, to all members of the community, regardless of their age or participation in the survey components of the research, during times that were convenient to mothers and children, but that did not interfere with usual meal times.

Because we use children's differential exposure to the availability of the nutritional supplements as instruments to estimate first-stage relations (4), (5), and (7) and that are then excluded from relation (1), it is important to establish that the two interventions resulted in differential consumption of calories, protein, and other nutrients. Approximately 70% of children between the ages of 0–36 months consumed at least some *atole*, with no difference between boys and girls. Similar overall participation rates were observed in *fresco* villages. Averaging over all children in the *atole* villages (regardless of their levels of voluntary participation), children 6–12 months consumed approximately 70 kcal of *atole* supplement per day; children 12–24 months, 90 kcal; and children 24–36 months, 120 kcal. Children in the *fresco* villages, however, consumed only 20 kcal of *fresco* supplement per day between the ages of 6–24 months, rising to approximately 30 kcal by the age of 36 months (Schroeder et al. 1992; Islam and Hoddinott 2009).

In 2002–4, a team of investigators, including most of the authors of this paper, undertook a follow-up survey targeted toward all participants in the 1969–77 survey.[10] At that time, sample members ranged from 25 to 42 years of age. Of the 2,392 individuals 0–15 years old in the original

[10] This population has been studied in the intervening years since the original survey, with particular emphasis on the impact of the nutritional intervention. Martorell et al. (2005) provides references to many of these studies.

1969–77 sample by the time of the 2002–4 HCS: 1,855 (78%) were alive and known to be living in Guatemala (11% had died—the majority due to infectious diseases in early childhood, 7% had migrated abroad, and 4% were not traceable). Of these 1,855, 60% lived in the original villages, 8% lived in nearby villages, 23% lived in or near to Guatemala City, and 9% lived elsewhere in Guatemala. For the 1,855 traceable sample members living in Guatemala, 1,571 (85%) completed at least one interview during the 2002–4 survey (Grajeda et al. 2005). This study includes the 1,448 respondents (54% female) interviewed in 2002–4 for whom the two measures of adult cognitive skills we use in the main analyses (Section 3.2) are available. They comprise 78% of the 1,855 individuals who were alive and known to be living in Guatemala and 61% of the original sample. Measured from 1977 to 2002, the latter figure indicates an annual attrition rate of approximately 2%, low when compared to shorter-term longitudinal surveys in developing countries (Alderman et al. 2001) or to long-term longitudinal surveys in developed countries (Fitzgerald et al. 1998a).[11] Nevertheless, nearly 40% represents substantial attrition, and therefore we assess potential attrition biases in several ways (Section 4.2.4).

Section 3.2 Central Variables for the Analysis

Table 1 presents summary statistics for the 1,448 individuals used in the main analyses.

Dependent variables: Adult cognitive skills (K):

1) Adult Reading-Comprehension Cognitive Skills (RCS): The vocabulary (Level 3, approximately fourth-grade equivalent) and reading-comprehension (Level 2, approximately 3rd grade equivalent) modules of the Inter-American Reading and Comprehension Tests (Manuel 1967) were administered to the 1,197 individuals (83% of the sample of 1,448) who passed a pre-literacy screen.[12] The vocabulary portion had 45 questions and reading comprehension had 40 questions, yielding a maximum possible score of 85 points. The distribution of test scores (for those who took the test) appears to be symmetric and approximately normal (although it fails to pass standard normality tests). The 17% of the sample who did not pass the pre-literacy screen are assigned a value of zero for the reading-comprehension tests. Including those we score at zero, the mean score

[11] Most measures of attrition refer to households or individuals who were past infancy and early childhood when the sample was taken, so they do not include the effects of infant and early childhood mortality that account for over a quarter of the attrition in the data used for this study.

[12] Subjects who reported completing six or more grades of schooling were assumed to be literate. Respondents who reported having completed fewer than three grades of schooling, and those who reported three to five grades of schooling but could not read correctly the headline of a local newspaper article, were given a pre-literacy test that began with reading letters aloud. They were considered literate if they passed the test with fewer than five errors out of 35 questions, the most difficult of which was reading aloud a five-word sentence.

is 36.1 with a standard deviation (SD) of 22.3, indicating substantial sample variation (Table 1). Males (mean 37.9, SD 22.7) average significantly higher scores than do females (mean 34.4, SD 21.8). These tests have demonstrated adequate test-retest reliability (correlation coefficients of 0.87 and 0.85 for vocabulary and reading) in previous studies in this population when the subjects were adolescents and young adults (Pollitt et al. 1993). Because of the nature of the distribution of the test scores, in the empirical analysis, Section 4.2.3 explores the robustness of our results to changes in the specification of these test scores, including incorporation of the pre-literacy test score.

2) Adult Nonverbal Cognitive Skills (NVS): All respondents were administered Raven's Progressive Matrices (Raven 1984), a widely-used nonverbal measure of interpretative cognitive skills that consists of a set of shapes and patterns, with the respondent asked to supply the "missing piece." NVS thus measures different aspects of cognition than the reading comprehension skills discussed above, although they are significantly correlated (0.57) and possibly include some common components of cognitive skills. We used the first three of the five scales (A, B, and C, with 12 questions each, for a maximum possible score of 36) because pilot data suggested and subsequent survey data confirmed that few respondents were able to progress beyond the third scale. As with the test of reading and vocabulary, there is considerable sample variance; the distribution of these test scores also appears to be symmetric and approximately normal (although, again, not passing standard normality tests), with a mean of 17.7 points (SD 6.1). Males (mean 19.4, SD 6.5) average significantly higher scores than do females (mean 16.3, SD 5.4). The Raven's test also has exhibited adequate test-retest reliability (correlation of 0.87) in this population in the past Pollit et al. (1993). We use the Raven's test scores as the dependent variable for NVS.

Life-cycle stage experiences (E_1, E_2, E_3): We assume (1) is linear and include one commonly used indicator each for the first two life-cycle stages, under the assumption that each is a sufficient statistic for that experience.[13] For the third life-cycle stage, we consider multiple indicators because it is less obvious *a priori* which representation of post-schooling experience is most relevant.

[13] If we were to approximate the function in relation (1) with one indicator each for the three life-cycle stages in a second-order Taylor series expansion to allow diminishing marginal returns and interactions, we would need to estimate 9 parameters on endogenous variables. This more flexible specification exceeds the limits of what we are able to estimate with any precision. When we add squared and interaction terms of the three life-cycle experience measures to our preferred specification, none of the individual coefficients of these terms is statistically significant and joint tests fail to reject the significance of them as a group, so it is not possible for us to assess empirically possible interactions of the life-cycle stages through such an approach.

Pre-schooling experience: There is substantial emphasis in the literature on the importance of nutrition—reflecting nutrient intakes and exposure to infections—as measured by height in early childhood development. We use pre-schooling child height-for-age Z-scores (HAZ),[14] a widely-accepted indicator of childhood long-run nutritional status, to calculate our indicator for pre-schooling experience (Appendix A provides details of the construction of this variable.)

Schooling experience: We use completed schooling attainment (highest grade completed) as our indicator of learning experiences during this life-cycle stage. As it is the standard indicator used in the literature, this facilitates comparisons with most previous studies. The mean is 4.7 grades (SD 3.5). Males (mean 5.2, SD 3.6) average significantly more grades of completed schooling than do females (mean 4.3, SD 3.3). The distribution has a mode at six grades (29% of the individuals), completion of primary school. There are also secondary modes at zero grades (14%) and three grades (11%). The highest grade completed of school is significantly correlated with the indicators for the pre-schooling experience (correlation of 0.10) and post-schooling experiences described below (skilled work tenure, 0.13; whether living in Guatemala City, 0.28), suggesting that if relation (1) is the true relation but only schooling is included in the estimation, the coefficient on schooling is likely to be biased.

Post-schooling experience: Based on *a priori* considerations about which post-schooling experiences are likely to be important for cognitive skills, we consider two measures for this third life-cycle stage.

- *Tenure in skilled occupations*: We use the duration in years of continuous work experience prior to the 2002–4 survey in skilled occupations[15] as our first indicator of post-schooling experience that may contribute to adult cognitive skills. The survey does not permit the calculation of total experience in all jobs since individuals left school, but rather tenure in all jobs held in 1998 and in 2002–4.[16] Such experience is likely to strengthen cognitive skills via (1) learning-by-doing through problem solving; (2) furthering skills learned in school by

[14] Z-scores are the standard deviations from the medians as given in WHO (2006).

[15] We define skilled jobs to include white collar and administrative jobs, those with specialized skills (e.g., carpenters and mechanics), social service occupations (e.g., teachers, nurses) and own farm/own enterprise work that yields income in the top quintile for such activities in 2002–4, under the assumption that such relatively large-scale enterprises required greater managerial and other skills to operate. We have explored various definitions for the skills measure and our results are not changed importantly. For example, results are similar if we 1) treat as skilled labor only those with skilled wage employment; 2) use (1) along with a redefinition of skilled labor for agricultural work (based on planting a cash crop) and own business (based on the value of assets in the business); or 3) use the skilled years measure described in the text, but truncate it to 10 years to avoid the influence of outliers.

[16] While we are unable to test whether total experience has a greater effect than recent experience, if there is depreciation of unused knowledge recent experience is likely to be more important than earlier experience.

using them in real world applications; and (3) exposing individuals to a wider environment, through interactions with coworkers and customers. The mean number of years of tenure in a skilled job is only 2.8, but with a lot of variation (SD 4.8). Two-thirds of the individuals were not in skilled occupations in 1998 or 2002–4 and therefore have reported tenure of zero years. Among the 530 sample members who had at least some tenure in a skilled occupation, the mean is 7.8 years, with a SD of 5.0. There are substantial and significant differences for tenure in skilled occupations by gender, with 58% of men having such experience (mean 8.1 years, SD 5.0), but only 18% of women (mean 6.9 years, SD 5.0).

- *Migration to Guatemala City:* Living in Guatemala City, rather than in one of the original sample villages or elsewhere in Guatemala, might alter experiences through work, but also via shopping, entertainment, and in other ways that might affect adult cognitive skills. Seventeen percent of the sample lived in Guatemala City at the time of the 2002–4 survey, 18% of the females and 15% of the males. The mean of the estimated number of years that these individuals had been away from their origin village is 3.9 years (SD 6.6), with no significant difference by gender, even though males have been in the city for, on average, one additional year (i.e., on average, women had spent an additional year in their original village).

The two indicators of post-schooling experiences are positively correlated, but the correlation is relatively small (0.04), underscoring the possibility that they may represent different dimensions of post-schooling experiences.

Initial conditions ($\mathbf{E_0}$,$\mathbf{F_0}$,):

Genetic and other endowments and other individual characteristics ($\mathbf{E_0}$): We do not have direct observations on genetic endowments beyond the gender of the individual, which we control for in our first-stage estimates. Other individual characteristics that we observe include age at the time of the 2002–4 interview (which we include in both the first and second stages) and whether the individual was a twin, which may have longer-run implications through the human capital investments in the three life-cycle experiences associated with the generally lower birth weight of twins (Behrman and Rosenzweig 2004). If the instruments that are only in the first stage are uncorrelated with individual-level endowments, however, failure to include direct measures of the endowments will not lead to biased estimation of relation (1), since the predicted life-cycle stage experience measures will be orthogonal to endowments in the error term.

Wealth and parental schooling attainment in childhood ($\mathbf{F_0}$): In some specifications, although not our preferred ones, we also consider the role of parental characteristics in the production of cognitive skills; these parental characteristics include mother's and father's schooling attainment and a household wealth index.[17]

<u>Observed events and shocks (C)</u>:

<u>Experimental nutritional shocks</u>: One observed shock is the nutritional intervention underlying the original study. The *atole* intervention has been shown to have improved child growth (Martorell et al. 1995). We construct two intent-to-treat measures, based on the village and date of birth of each individual and the dates of operation of the interventions. For each individual, we calculate whether s/he was exposed to either intervention for the entire period from 0–36 months of age. A potential exposure to the *atole* intervention (relative to *fresco*) is then calculated by multiplying this cohort measure by a dummy indicator of whether or not the child lived in one of the two *atole* villages.

<u>Natural, market, or policy events</u>: Guatemala suffered a major earthquake in 1976 and we incorporate a dummy variable for whether an individual was born in the two years prior to it because the economic shock due to the earthquake may have been particularly deleterious for infants and very young children. We also include community-level variables that relate as closely as possible to the timing of key educational- and labor-market-related decisions in each individual's development. Using information reported in earlier work about infrastructure, markets, and services in the villages (Bergeron 1992), complemented with a retrospective study in 2002, we construct variables including (1) the student-teacher ratio and (2) an indicator of whether a lower secondary school (grades 7–9) was available, both measured in the village in which the individual lived when the individual was seven years old. To capture changes in local market conditions, we construct a variable indicating the logarithm of the (national) salary in manufacturing when the individual was 18 years old and likely to be in their early working years. Thus, while reflecting community-level characteristics (except for the manufacturing salaries), these variables vary by single-year age cohorts within each village, as well as across villages. This is preferable to the more common approach of using indicators about such factors at a given time for a population with different ages at that point in time, since these indicators more closely relate to periods in individuals' lives when critical decisions (e.g., starting school) were made. We also include an additional individual-specific shock—whether the individuals' father or mother had died (prematurely and possibly

[17] For the small percentages of individuals without parental schooling or the wealth index, we replace the missing item response with sample medians. The wealth index is constructed using data on a set of household durables as well as housing characteristics observed in the early 1970s. Using principal components, these assets and characteristics were combined into an index that we call a "wealth" index (Maluccio et al. 2005).

accidentally) by the time they were 18, which relates to a critical juncture for entering the labor force.[18]

4. Results

In Section 4.1, we summarize our main estimates, using indicators of experiences during each of the three life-cycle stages, and endogenizing using a two-step IV feasible generalized methods of moments (GMM) (Baum, Schaffer and Stillman 2007). In Section 4.2, we consider how robust our estimates are to gender differences, variations in the instrumental variables used, alternative representations of RCS and NVS, and controls for attrition. We allow for clustering at the birth year-village cohort level in the calculation of the standard errors to control for potential serial correlation among children born in the same villages in the same year; this yields 64 clusters.[19]

4.1 Basic Specification of the Adult Cognitive Skills Production Function

Table 2 presents four sets of estimates related to the linear version of the adult cognitive skills production function in relation (1) for reading-comprehension cognitive skills (RCS, the Inter-American Reading-Comprehension Tests) in the first panel of the table and for nonverbal skills (NVS, Raven's Progessive Matrices) in the second panel. Both are represented as Z-scores (i.e., standardized in terms of their sample means and SDs). Three of the sets of estimates pertain to the chronological nature of the three life-cycle experiences discussed in Section 2: Only pre-school experience (HAZ at age six) included (Set 1A); only pre-school experience (HAZ at age six) and school-age experience (schooling attainment) included (Set 2); and all three life-cycle experiences included—HAZ at age six, schooling attainment, and skilled job tenure (Set 3). Presenting these alternative specifications shows how the coefficient estimates of the earlier life experiences change as later life experiences are incorporated. The fourth set of estimates includes only schooling attainment (Set 1B) to permit comparisons with previous literature that does not include pre- or post-schooling experiences. We present both OLS and IV GMM estimates (Baum et al. 2007;

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[18] Some of these shocks relate to the individuals' parental families, particularly whether individuals' parents were alive when individuals were at age 18. However, in contrast to when we include other parental characteristics (Section 4.3.2), inclusion of the parental death shocks in the instrument set does not result in a rejection of the Hansen J overidentification test.

[19] Wooldridge (2003) suggests that standard corrections for clustering are valid only when the number of groups or clusters is large. In light of this, following Bertrand, Duflo, and Mullainathan (2004), we also estimated the models using block bootstrapped standard errors, using the same 64 clusters and resampling 1,000 times. Standard errors calculated from this approach were typically slightly larger than those reported in the paper, but did not change any of the statistical significance reported in our preferred specification (the final column of Table 3), except that HAZ is no longer significant in the RCS relationship.

Hayashi 2000), using all the instruments listed in Table 1 except for the household wealth index and parental schooling attainment measures ($\mathbf{F_0}$).

Our identification strategy has three components. First, using the framework developed in Section 2, we select plausibly exogenous characteristics from the individuals' backgrounds and communities to predict the three E_i life-cycle stage outcomes. These include community-level market and policy shock variables derived from the detailed community histories and the intent-to-treat exposure variables derived from the original nutritional supplementation intervention, as well as an indicator for having been born just prior to the 1976 earthquake. Second, we include *all three* of these life stages, which means that the possibility that the instruments have direct effects that do not operate through the included measured experiences in the second stage of the model is mitigated substantially. For example, in our main specifications in Section 4.1, for the presence of a lower secondary school at age seven years to be an invalid instrument would mean that it had a direct effect above and beyond its (linear) effect operating through each of the three E_i, pre-schooling nutrition (through expectations), schooling attainment, *and* post-schooling skilled work tenure. Third, we carry out a range of diagnostic tests to assess the strength and validity of the instruments, as well as consider alternative instrument sets and estimation procedures.

The instruments we utilize have reasonable power and we fail to reject the overidentification tests. At the bottom of each of the top two panels are the second-stage diagnostics for the IV estimates: the Hansen J (HJ) statistics for overidentification and the Hausman test comparing OLS and IV estimates (Hayashi 2000; StataCorp 2011). In the bottom panel, we present the first-stage diagnostics assessing the strength of the instruments, including F-statistics on the excluded instruments (Bound et al. 1995) and the Kleibergen-Paap (KP) statistics for weak instruments for each model. Coefficient estimates and test-statistics significant at the 5% significance level or lower are shown in bold.

<u>First-Stage Estimates</u>: The first-stage estimates (Table 2) have a number of significant coefficient estimates consistent with our hypotheses about how the instruments affect each of the life-cycle stages. Being a twin and/or being born within the two years prior to the 1976 earthquake reduce the HAZ at age 6, whereas exposure to the *atole* intervention in the first three years of life (relative to *fresco*) increases it. Being male, an individual who lived in a village with a lower secondary school when she or he started primary school or being exposed to higher salaries in manufacturing when the individual was 18 years old all lead to higher schooling attainment. A notable period of rising wages occurred in the late 1970s with reconstruction after the 1976

earthquake and in the late 1980s and early 1990s, associated with a building boom in Guatemala City and higher completed schooling was one of the criteria used in hiring in manufacturing and other industries. For example, completed third grade was a requirement for even the lowest level jobs available in a local cement factory that opened in the 1980s. The loss of a parent before age 18 years and higher student-teacher ratios, on the other hand, are associated with lower schooling attainment. Males have significantly more tenure in skilled jobs. Exposure to *atole* when 0–36 months, lower secondary school available at age six, and higher manufacturing wages at age 18 are linked to lower tenure in skilled jobs, consistent with individuals being more likely to go into low-skill manufacturing. The probability of migration to Guatemala City increases at a diminishing rate with age, and increases with lower secondary school being available at age six years and with higher manufacturing wages at age 18 years.

IV Diagnostics: For the full specifications with all three life-cycle experiences (Set 3), the first- and second-stage diagnostics are satisfactory for both RCS and NVS (Table 3). The F tests on excluded instruments range from 9.6 to 26.3, the KP statistics for weak instruments are satisfactory[20] and the HJ tests indicate that the first-stage instruments are not correlated with the second-stage disturbance term.[21] Moreover, the Hausman tests indicate that the IV estimates differ significantly from the OLS estimates. By contrast, in four of the six specifications in which one or two of the life-cycle experiences are excluded from our preferred specification of relation (1), the HJ tests indicate that there is a specification problem, consistent with concerns that the excluded instruments are correlated with the omitted life-cycle experience.

Pre-School Experience (E_1)—HAZ at Age Six: The OLS associations between HAZ at age six years and both cognitive skills measures, conditional on a quadratic in age, are positive,

[20] Using the critical values presented by Stock and Yogo (2007, Table 5.1), with a Kleibergen-Paap (KP) test statistic (Kleibergen and Paap 2006) of 6.3 (or higher), we reject at a 5% significance level the hypothesis that the instruments are weak, where weak in this case means having bias in the IV results that is larger than 20% of the bias in the OLS results. To the extent that our estimates are biased, however, conditional on the validity of the excluded instruments, they are biased toward the OLS estimate, suggesting that the results we report are conservative and *understate* the differences between OLS and IV. Evidence supporting this includes the finding that when we instead estimate using limited information maximum likelihood (LIML), as suggested by Stock and Yogo (2007). The results of that estimation (not shown) confirm the possible direction of bias.

[21] The Hansen J (HJ) statistic for overidentification does not reject the null hypothesis that the overidentifying restrictions are valid (i.e., that the model is well-specified and the instruments do not belong in the second-stage equation) at usual significance levels. Failure to reject the null hypothesis for the Hansen test is evidence that if any one of the instruments is valid, so are the others. Since the instrument set includes the randomly allocated exposure to the intervention and the earthquake indicator, both of which are likely to be valid, we interpret this as strong evidence of the validity of all the instruments. Further supporting this is the finding that in models in which we include additional parental characteristics (Section 4.2.2), the overidentification test fails, indicating that the HJ test has sufficient power to reject with these data.

significant, and fairly substantial, 0.20 SD for RCS and 0.19 SD for NVS (Table 3, first column in top two panels). If schooling attainment and skill job tenure are included and both of them and HAZ at age six years are treated as behaviorally-determined (our preferred estimates in the final column), the estimated effect of HAZ at age six years is basically the same, though more imprecisely estimated for RCS but much larger for NVS (0.41 SD). Therefore, our preferred estimates indicate a substantial gain in terms of both RCS and NVS 20+ years later for higher HAZ at age six years, controlling for both school-age and post-school experiences and for the endogenous determination of all three of these life-stage experiences. The alternative specifications for NVS indicate that the coefficient estimate for HAZ at age six years and for skilled job tenure increase markedly when moving from OLS to IV estimates in the full specifications in the final two columns. This pattern is consistent with a negative correlation between the endowment components positively related to skill development and schooling and the endowment components positively related to biological development (as suggested in Behrman and Rosenzweig 2004), and skilled job tenure. Absent control for the endogenous determination of the life-cycle experiences, the effects of HAZ at age six years and of skilled job tenure on adult NVS cognitive skills are substantially underestimated.[22]

School-Age Experience (E_2)—Schooling Attainment: If only schooling attainment is included using OLS, the estimated associations indicate increases of 0.22 SD of RCS and 0.15 SD of NVS for each additional completed grade of school. These are fairly substantial effects, implying, for example, increases of 0.75 SD of RCS and of 0.50 SD of NVS for a one SD increase in schooling attainment. For RCS, however, this estimated effect is halved if IV estimates are used for schooling attainment, whether or not the other life-cycle experiences are included. Our preferred estimate for the schooling attainment coefficient from the IV estimates with all three life-cycle experiences included, is 0.09 SD in RCS for every grade of schooling, or 0.34 SD in RCS for a one SD increase in schooling attainment. This still is an important effect, but about half of what is estimated in the OLS estimates likely due to "ability bias" in the form of not controlling for unobserved endowments that may include genetic ability endowments but also other unobserved aspects of family background (Behrman and Rosenzweig 1999). For NVS, the combination of including post-school experience and treating all three life-cycle experiences as behaviorally-determined in the IV estimates results in a decline in the estimated schooling attainment coefficient to 0.04 SD for every additional grade of schooling, and it is no longer statistically significant. These

[22] Increases in the estimated effect of HAZ at age six and skilled-job tenure after we instrument likely reflects some random measurement error bias for these variables, although the approximate quadrupling or more of the coefficient estimates is not likely to be due solely to measurement error.

last results are consistent with the claim that the Raven's tests are not affected by schooling and instead may reflect attributes that affect schooling (Schweizer et al. 2007).

Post-School-Age Experience (E_3)—Tenure in Skilled Job: For RCS we find a small but significant impact of skilled-job tenure of 0.03 SD for every year of such tenure. For NVS, we find a significant impact in the IV estimates of skilled job tenure that implies an increase of 0.14 SD for every additional year of skilled-job tenure. This is a fairly substantial effect, implying an increase of 0.67 SD in NVS for a one SD increase in skilled-job tenure.[23] In Section 3.2, we described the possibility that migration to Guatemala City, the capital, also might provide experiences that could improve cognitive skills. We examined this possibility by including an indicator for migration to the capital, in addition to the other three experience measures, to the specifications shown in the final column of Table 3, treating it as endogenous. While the F statistic for the excluded instruments on the migration indicator was 7.8, the KP statistic (3.4) suggests that the instruments as a set are weak when we attempt to endogenize these four experience measures. We fail to reject the HJ test at 5%. For both RCS and NVS, the results for the other life-cycle stages are virtually unchanged and the coefficient on migration is insignificant (results not shown).

4.2 Robustness Considerations

4.2.1 Gender Differences

Adult men score significantly higher on the tests of adult cognitive skills than do adult women: 3.5 points for RCS and 3.1 for NVS. This raises the question to what extent the RCS and NVS differences are due to cognitive skills production function differences between men and women or to differences in their life-cycle experiences.

To explore how robust the results in Table 3 are to gender differences, we considered a fully interacted model, adding interactions of a dichotomous variable for male with each of the variables and a male dummy variable to our basic specifications (results not shown). Because we now endogenize three additional variables in each regression n, the first-stage results are substantially weaker. Despite this concern, we test the set of interacted terms and find that they are jointly insignificant in each of the estimated production functions. These estimates, therefore, do not

[23] As for pre-school experience, the increase in this coefficient estimate with instrumenting may, in part, be due to random measurement error, but such measurement error is not likely to account for all of this increase.

provide direct evidence that there are important gender differences in the adult skills production functions.[24]

An alternative approach is to allow the production function to shift based on gender, as we do with age. Specifically, we include a male dummy variable in the second-stage estimated relation (Table 4).[25] When we do so, the first-stage diagnostics indicate the instruments are weak, with a KP statistic of 1.7, although the second-stage diagnostics are reasonable. As described above, skilled experience is predominantly a male phenomenon. Even though manufacturing salaries are significant, the F statistic on the excluded instruments (which no longer include the male dummy) in the first-stage skill tenure equation is now only 3.6. The second-stage estimates on the impact of HAZ at age six on NVS and of grade attainment on RCS are similar to those obtained when gender was included as part of the set of excluded first-stage instruments. By contrast, HAZ at age six years in the relation for RCS (p=0.15) and skilled job tenure in both relations are no longer significant. Although insignificant in RCS, the male dummy variable is positive and significant in NVS (with a coefficient of 0.66), with a corresponding decrease in the coefficient on skilled tenure, which becomes negative and insignificant. In the case of NVS, then, it is unclear whether the better specification is one which excludes gender in the second stage or includes it. In either case, however, the substantive findings regarding the positive effect of HAZ at age six years and the insignificance of schooling attainment on NVS remain the same.

4.2.2 Alternative instrumental variable sets

Household wealth and parental schooling: Table 5 (Set 1) presents estimates parallel to those in Table 3, but including the household wealth index and parental schooling attainment in the instrument set. Patterns of significance for the three life-cycle stages are different for both RCS and NVS, although a number of the estimated coefficients on the endogenous variables fall between the OLS and GMM estimated coefficients reported in the final two columns of Table 3. However, for the outcomes considered in this paper, the HJ statistic soundly rejects the null hypothesis that the overidentifying restrictions are valid. For this reason, we excluded household wealth and parental schooling from the first-stage instruments in our preferred estimates in Table 3.

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[24] We also considered estimating males and females separately, but results are much less precise, likely due in part to halving the sample size for the regressions.

[25] For these estimates, we augment the instrumental variable set with two additional variables—an interaction between male and each of the exposure to the nutritional intervention variables, since there is evidence that the intervention affected males and females differently (Maluccio et al. 2009).

Birth-year village dummies: All community-level derived instrumental variables are simply functions of when and where an individual was born -- for example, the presence of a lower secondary school when an individual was seven years old. An alternative approach to identifying the second-stage regression is to use all such birth-year village-level shocks, in other words birth-year village dummy variables. These capture all fixed differences in resources or opportunities for the birth-year village cohorts. The maintained assumption is that such factors affect adult cognitive skills only through the three life-cycle stages we include in the model (controlling for age). When we estimate the relations using the set of 64 dummies (as well as the individual-level male, exposure to the intervention and twins dummies), we find roughly similar magnitudes for all of the life-cycle experiences on NVS and there is some evidence that completed grades is significant in the relation. In contrast to our preferred results in Table 3, the results for RCS, however, no longer indicate significance of HAZ or skilled experience. In general, the instruments are weak, with F statistics below 5 and a KP of 1.9, and the HJ test is rejected at the 7% significance level. In contrast to the claim that the HJ test is a weak test (because it is almost never rejected), these instrument sets demonstrate that there is sufficient power in these data for the HJ test to be rejected.

4.2.3 Alternative Representations of RCS and NVS

Due to the literacy screen, RCS has a mass point at zero. In Table 6, we present three alternative IV estimates (using the same instrument set as our preferred estimates in Table 3) that address this concentration of low scores on the reading and comprehension test used for RCS: (1) instrumental variables Tobit estimates using RCS that account for the lower bound and mass point at zero; (2) GMM estimates using RCS + pre-literacy; and (3) GMM estimates using RCS-quartile.[26]

Although NVS does not have a similar mass point at zero, we explored whether our findings in Table 3 are robust to variants in how we treat the dependent variable by examining estimates for NVS-quartile. For RCS, the significance of HAZ appears not to be robust to these alternative specifications, but the findings regarding schooling attainment and skilled job tenure hold, although the latter are now only significant at the 10% level in two of the three specifications. For NVS, the quartile results are similar to our main findings, although HAZ scores are now significant only at

[26] For RCS + pre-literacy score, we sum the raw score from the pre-literacy screen and the reading and comprehension tests under the assumption that those who were exempt from the pre-literacy test would have earned a perfect score had they taken it. Since those who failed the pre-literacy test have scores in the 0–35 range, this leads to variation in scores among those to whom we assigned zero for the RCS alone. For the RCS-quartile, each individual's reading and comprehension test score is recorded in the quartile of the distribution in which it falls. All those who failed the pre-literacy test (17%) are in the first (lowest) quartile.

the 10% level. These alternative representations for RCS and NVS do not alter substantially our qualitative findings or change our interpretations in Section 4.1.

4.2.4 Attrition

Despite the considerable effort and success in tracing and reinterviewing participants from the original sample, attrition is substantial. Moreover, overall attrition in the sample is associated with a number of initial conditions with effects differing by the reason for attrition (Grajeda et al. 2005). What is of ultimate concern in this analysis, however, is not the level of attrition, but whether the attrition invalidates the inferences we make using these data. For example, does excluding migrants who were not located and who may have different characteristics lead to systematic bias of the estimates presented here?

To explore these concerns, we implement the correction procedure for selective attrition on observed characteristics outlined in Fitzgerald et al. (1998a, 1998b). We first estimate an attrition probit conditioning on all the right-side variables (including instruments) considered in the main models, as well as an additional set of (endogenous) variables potentially associated with attrition, for all original sample members (N=2,392). The latter variables include ones that reflect family structure in previous years because these are likely to be associated with migration status: whether individuals lived with both their parents in 1975 and in 1987. During the fieldwork, locating sample members was typically facilitated by having access to other family members from whom the field team could gather information. Therefore, we also include variables that capture this feature of the success of data collection: whether the parents were alive in 2002, whether they lived in the original village, whether a sibling of the sample member had been interviewed in the 2002–4 follow-up survey, and the number of siblings in the sample in each family. While we do not formally have adjustments to correct for selection on unobservable characteristics, by including a number of endogenous observables indicated above, which are likely to be correlated with unobservables, we expect that we are reducing the scope for attrition bias due to components of unobservables that are not correlated with the included observed variables, as well.

Nearly all of the factors described above are significant and highly associated with attrition, above and beyond the conditioning variables already included in the models (Appendix Table B.1). Following Fitzgerald et al. (1998b), we construct weights that give greater weight to observations in the sample reinterviewed in 2002–4 that had lower predicted probabilities of having been reinterviewed. Table 7 shows that application of these weights affects only slightly the results and that the central patterns of the coefficient estimates remain similar to those in our main estimates in the final column of Table 3, with the exception that the previously marginally HAZ scores are now insignificant (p=0.16) in the RCS relation. We interpret these findings to mean that, as found in other contexts with high attrition our results do not appear to be driven by attrition biases (Alderman et al. 2001; Fitzgerald et al. 1998b).

Section 5. Conclusions

Most empirical investigations of the effects of cognitive skills assume that they are produced by schooling, and that schooling is exogenous. We argue that such approaches are likely to lead to incorrect inferences not only about the effect of schooling, but also about the potential importance of pre-schooling and post-schooling experiences on adults cognitive skills. To explore this, we draw on a rich longitudinal data set to estimate production functions for adult reading-comprehension cognitive skills and adult nonverbal cognitive skills as dependent on behaviorally-determined pre-schooling, schooling, and post-schooling experiences. We present a basic specification as well as a range of tests of how robust the basic specification is. While some of the robustness tests suggest qualifications regarding particular coefficient estimates, they all support the following results:

(1) Schooling attainment has a significant and substantial effect on adult reading-comprehension cognitive skills *but not* on adult nonverbal cognitive skills.

(2) Pre-schooling and post-schooling experiences have substantial positive significant effects on adult cognitive skills.[27] Pre-schooling experiences that increase HAZ at age six years substantially and significantly increase adult reading-comprehension and nonverbal cognitive skills, even after controlling for schooling attainment and post-school skilled job tenure. The effect of HAZ on RCS, however, is less robust to changes in the methodology that we examine. Post-schooling tenure in skilled jobs also has a significant positive effect on adult reading-comprehension and nonverbal cognitive skills, although the latter estimate is somewhat tenuous because it depends on how we treat gender. Age also has significant positive effect with diminishing returns on adult reading-comprehension cognitive skills until about age 30, probably because of learning with experience in early adulthood.

(3) Estimates that do not account for the endogenous determination of these three life-cycle experiences are misleading. Estimates of the effect of schooling on adult cognitive skills that do not

[27] Stein et al. (2008) present a related finding that the nutritional intervention had significant impacts on adult cognitive skills even with controls for schooling attainment (although they do not attempt to control for the determinants of schooling attainment). Maluccio et al. (2009) also present a related finding that reduced-form estimates of adult cognitive skills indicate significant and substantial effects of the nutritional intervention.

account for schooling being behaviorally-determined are biased upward substantially for adult reading-comprehension cognitive skills and make the impact on adult nonverbal cognitive skills appear highly positively significant rather than insignificant. Treating pre- and post-schooling experiences as statistically predetermined substantially underestimates their impacts. This contrasts with the upward bias for schooling, suggesting that the underlying physical and job-related components of genetic endowments are negatively correlated with those for cognitive skills.

While these results are of considerable interest in their own right, they also speak to four broader literatures.

First, in both developed and developing countries, there is growing interest in investing in disadvantaged children at an early stage in life. Drawing on a wide body of evidence from economics, psychology, and neuroscience, for example, Heckman (2006) argues that returns to such investments are much higher than those made later in life. However, the empirical base for these arguments is not as deep as would be desirable. For example, there are few studies that follow disadvantaged individuals over long periods of time. Our study adds to this literature by demonstrating that having relatively good nutritional status as pre-schoolers results in greater cognitive skills decades later as adults.

Second, a growing body of evidence suggests that, across a wide range of countries, scores on certain measures of cognitive ability[28]—including the Raven's Progressive Matrices used in our study—are increasing over time.[29] This phenomenon is referred to as the Flynn effect (Flynn 1987). Dickens and Flynn (2001) posit several pathways by which changes in environmental or behavioral factors, rather than in genetic factors, could cause scores on cognitive ability tests to increase over time. One of these is improved childhood nutrition. A second is increased cognitive complexity in the workplace. Our results for the impact of early childhood nutrition and for years of experience in skilled jobs—both treated as endogenous—provide direct evidence of the importance of these factors in shaping dimensions of cognitive skills that are consistent with the environmental and behavioral factors hypothesized to underlie the Flynn effect.

[28] "Ability" is often used in the literature to refer to innate characteristics, perhaps genetically determined. "Skills," in contrast, tends to be used to refer to capabilities that have been affected by various experiences, such as education. The Raven's scores that we use in this paper have been interpreted primarily as if they represent innate abilities, so they have been referred to as measures of "ability." But the interpretation that they are innate is contested and, indeed, we explore their possible endogeneity above. We refer to them as nonverbal skills to reflect their possible endogeneity as opposed to they measuring innate abilities.

[29] For example, Dutch scores on Raven's Progressive Matrices increased by 1.3 standard deviations between 1952 and 1982 (Flynn 1987).

Third, a relatively small amount of literature attempts to use what are interpreted to be direct measures of innate ability to examine whether human capital is associated with greater productivity as opposed to being merely a signaling device (Boissiere et al. 1985; Alderman et al. 1996). Implicit in such approaches is the assumption that causality runs from cognitive abilities to productivities. But if more productive, higher remunerated work is more complex, and if undertaking complex work improves cognitive skills, causality (also) runs the other way. We find evidence of this latter relationship. Therefore, our findings imply that studies that regress productivity on contemporaneous measures of cognitive abilities are flawed if they fail to take the endogeneity of cognitive skills into account.

Fourth, there is considerable debate over the impact of human capital on income levels and growth. In this literature, aggregate country-level estimates are presented in which human capital is typically represented by converting schooling enrollment rates into estimates of the stock of schooling (Nehru et al. 1995) or by school attainment for those older than 25 (Barro and Lee 1993). In doing so, these approaches assume that individuals do not accumulate additional human capital after completing schooling or after a certain age, nor does their human capital depreciate. Our results are at odds with this common assumption—we find that adult cognitive skills increase with experience in higher-skilled occupations (treated endogenously) as well as modestly with age for our sample of 25–42 year olds. At the cross-country level, this implies that widely-used representations of knowledge are flawed—they likely overstate human capital in slow-growing or traditional/subsistence economies and understate it in faster growing, modernizing economies. These biases are likely to result in underestimates of the importance of human capital in economic growth processes.

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Table 1. Summary statistics: Means and standard deviations (N = 1,448)

	All	Women	Men
K: 1. Reading-Comprehension Skill (RCS)	36.05	34.41	37.95*
	(22.3)	(21.9)	(22.7)
RCS + pre-literacy scores (RCS + prelit)	66.93	64.74	69.45*
	(29.7)	(29.9)	(29.2)
2. Nonverbal Skill (NVS)	17.70	16.26	19.36*
	(6.1)	(5.4)	(6.5)
$\mathbf{E_1}$: HAZ at age six (1268 non-missing cases)	-2.22	-2.20	-2.30
	(0.96)	(0.95)	(0.97)
$\mathbf{E_2}$: Schooling attainment (grades)	4.69	4.29	5.15*
	(3.5)	(3.3)	(3.6)
$\mathbf{E_3}$: Skilled job tenure (years)	2.85	1.27	4.67*
	(4.8)	(3.4)	(5.5)
Living in Guatemala City in 2002–4 dummy	0.17	0.18	0.15
$\mathbf{I_0}$: Age at interview in 2002–4	32.33	32.42	32.23
	(4.2)	(4.3)	(4.2)
Age at interview in 2002–4 squared	1,063.2	1,069.5	1,056.0
	(277.8)	(281.6)	(273.4)
Instruments			
$\mathbf{E_0}$: Genetic endowment			
Male	0.46	-	-
Twin	0.02	0.02	0.01
$\mathbf{F_0}$: Household and parental characteristics			
Household wealth index (1,330 non-missing cases)	-3.10	-3.10	-3.09
	(0.92)	(0.91)	(0.93)
Mother's schooling attainment (1,427 non-missing cases)	1.36	1.25	1.48*
	(1.7)	(1.6)	(1.8)
Father's schooling attainment (1,344 non-missing cases)	1.71	1.69	1.77
	(2.1)	(2.0)	(2.3)
C: Natural, market or policy shocks			
Mother or father had died by child age 18 dummy	0.08	0.08	0.09
Student-teacher ratio at age seven	39.93	40.26	39.55
	(9.0)	(9.4)	(8.5)
Lower secondary school available at age seven	0.21	0.21	0.21
Born in the two years prior to 1976 earthquake	0.14	0.15	0.13
Ln salary in manufacturing industry at age 18	1.60	1.58	1.61
	(0.5)	(0.5)	(0.5)
Intent to treat nutritional intervention dummies			
Exposure 00–36 months	0.39	0.37	0.42
Exposure 00–36 months × *atole*	0.21	0.21	0.22
Number of observations	1,448	775	673

Notes: Standard deviations are shown in parentheses. * indicates statistical difference between women and men based on a two-sided t-test with unequal variances, at a 5% significance level or lower.

Table 2. First-stage estimates of life-cycle stages (without parental education or wealth)

	E1	**E2**	**E3a**	**E3b**
	HAZ at age 6	**Grades of schooling**	**Skilled job tenure (years)**	**Migrant to Guatemala City**
I_0: Individual characteristics				
Age at interview in 2002–4	-0.123	1.284	0.203	**0.478**
	(0.168)	(0.835)	(0.676)	(0.092)
Age at interview in 2002–4 squared	0.002	-0.015	-0.007	**-0.005**
	(0.002)	(0.011)	(0.009)	(0.001)
E_0: Genetic endowment				
Male	-0.094	**0.802**	**3.419**	**-0.040**
	(0.048)	(0.183)	(0.250)	(0.018)
Twins	**-0.901**	-0.311	-0.645	-0.041
	(0.133)	(0.716)	(0.666)	(0.087)
ΔC: Natural, market or policy shocks				
Mother or Father had died by age 18	0.045	**-0.941**	-0.400	-0.020
	(0.084)	(0.263)	(0.532)	(0.034)
Student-teacher ratio at age six	0.003	**-0.034**	-0.007	-0.001
	(0.002)	(0.014)	(0.013)	(0.002)
Lower secondary school available at age six	-0.024	**1.527**	**-0.599**	**0.102**
	(0.053)	(0.251)	(0.290)	(0.027)
Born in the two years prior to 1976 earthquake	**-0.235**	-0.403	-0.251	0.012
	(0.086)	(0.418)	(0.279)	(0.042)
Ln salary in manufacturing industry at age 18	0.633	**3.132**	**-3.503**	**1.174**
	(0.332)	(1.442)	(1.276)	(0.176)
ΔC: Intent to treat nutritional intervention				
00–36 months	**-0.278**	0.232	0.053	0.060
	(0.068)	(0.391)	(0.387)	(0.045)
00–36 months × *Atole*	**0.279**	-0.348	**-0.767**	0.017
	(0.074)	(0.308)	(0.368)	(0.040)
Constant	-1.785	-24.698	8.694	**-11.742**
	(3.718)	(17.535)	(14.370)	(2.000)
F Statistic on instruments to be excluded in 2nd stage	**9.6**	**15.4**	**26.3**	**7.8**
F Statistic overall regression	**16.2**	**17.6**	**24.8**	**7.2**

Notes: OLS regressions. Standard errors calculated allowing for clustering at the birth-year-village level (64 clusters) shown in parentheses below coefficient estimates, which are in bold if significant at 5% or lower.

Table 3. Estimated impacts of pre-schooling, schooling, and post-schooling experiences on cognitive skills (N = 1,448)

		Set 1A		Set 1B		Set 2		Set 3	
Life stage	**Representation**	**OLS**	**IV**	**OLS**	**IV**	**OLS**	**IV**	**OLS**	**IV**
	RCS Z-scores								
E1	HAZ score at age six	**0.202**	-0.096			**0.070**	0.105	**0.070**	**0.195+**
		(0.029)	(0.156)			(0.021)	(0.111)	(0.021)	(0.124)
E2	Schooling attainment			**0.221**	**0.101**	**0.218**	**0.105**	**0.218**	**0.087**
				(0.006)	(0.018)	(0.006)	(0.018)	(0.006)	(0.020)
E3a	Skilled job tenure							-0.001	**0.029**
								(0.004)	(0.014)
E3b	Age at interview in 2002–4	**0.224**	0.036	0.076	0.097	0.111	**0.174**	0.112	**0.212**
		(0.094)	(0.126)	(0.075)	(0.054)	(0.074)	(0.094)	(0.074)	(0.102)
	Age at interview squared × 10	**-0.038**	-0.011	-0.012	**-0.018**	-0.018	**-0.029**	-0.018	**-0.035**
		(0.014)	(0.019)	(0.012)	(0.009)	(0.011)	(0.014)	(0.011)	(0.015)
	Constant	-2.767	-0.218	-2.164	**-1.709**	**-2.585**	**-2.800**	**-2.595**	**-3.153**
		(1.502)	(1.814)	(1.201)	(0.836)	(1.17)	(1.355)	(1.182)	(1.470)
	F Statistic	**26.1**	**17.4**	**472.2**	**32.3**	**398.1**	**23.9**	**332.5**	**18.6**
	Hansen J-test [p-value]	**17.3**	[0.03]	11.6	[0.17]	11.7	[0.11]	9.3	[0.16]
	Hausman test [p-value]	**3.7**	[0.29]	**47.1**	[<0.01]	**47.4**	[<0.01]	**54.0**	[<0.01]
	NVS Z-scores								
E1	HAZ score at age six	**0.190**	0.163			**0.104**	0.015	**0.101**	**0.414**
		(0.034)	(0.141)			(0.034)	(0.120)	(0.034)	(0.149)
E2	Schooling attainment			**0.145**	**0.125**	**0.141**	**0.125**	**0.137**	0.044
				(0.007)	(0.025)	(0.073)	(0.026)	(0.007)	(0.037)
E3a	Skilled job tenure							**0.018**	**0.140**
								(0.006)	(0.019)
E3b	Age at interview in 2002–4	**0.247**	0.056	0.121	0.122	0.173	0.137	0.146	**0.094**
		(0.090)	(0.109)	(0.102)	(0.085)	(0.098)	(0.091)	(0.097)	(0.105)
	Age at interview squared × 10	**-0.042**	-0.014	-0.021	-0.021	-0.029	-0.024	-0.025	-0.020
		(0.013)	(0.016)	(0.015)	(0.013)	(0.015)	(0.014)	(0.015)	(0.015)
	Constant	**-3.092**	-0.665	**-2.348**	-2.260	**-2.974**	-2.491	**-2.529**	-0.563
		(1.466)	(1.663)	(1.674)	(1.419)	(1.602)	(1.453)	(1.594)	(1.652)
	F Statistic	**24.6**	**18.6**	**166.1**	**23.1**	**118.8**	**17.2**	**96.5**	**29.3**
	Hansen J-test [p-value]	**40.7**	[<0.01]	**36.7**	[<0.01]	**36.6**	[<0.01]	10.1	[0.12]
	Hausman test [p-value]	7.3	[0.06]	0.7	[0.87]	0.5	[0.97]	**54.5**	[<0.01]
	First stage diagnostics								
	Kleibergen-Paap test		**8.8**		**13.0**		**6.3**		**6.3**
	F-stat on excluded instr.								
	HAZ at age six	**9.6**							
	Schooling attainment	**15.4**							
	Skilled job tenure	**26.3**							

Notes: Instrumental variables (GMM) estimation using all instruments identified in Table 1 except wealth and parental schooling. Standard errors are calculated allowing for clustering at the birth-year-village cohort level (64 clusters) shown in parentheses below coefficient estimates, which are in bold if significant at 5% or lower; p-values are in brackets. + indicates significance at p=0.10.

Table 4. Estimated impacts of pre-schooling, schooling, and post-schooling experiences on adult cognitive skills allowing for an intercept difference between males and females

Panel A		**RCS**	**NVS**
Life-cycle stage	**Representation**	**IV**	**IV**
E1	HAZ score at age six	0.189	**0.307**
		(0.133)	(0.142)
E2	Schooling attainment	**0.087**	0.009
		(0.022)	(0.031)
E3a	Skilled job tenure	0.025	-0.041
		(0.050)	(0.064)
E3b	Age at interview in 2002-4	**0.213**	**0.322**
		(0.104)	(0.119)
	Age squaredX10	-0.035	**-0.052**
		(0.015)	(0.017)
	Male	0.012	**0.660**
		(0.176)	(0.231)
	Constant	**-3.184**	**-4.600**
		(1.523)	(1.947)
	F Statistic	**12.5**	**21.3**

Panel B	**First-stage diagnostics**		**Second-stage diagnostics**		
	Endogenous variable	**F stat on excluded instruments**		**RCS**	**NVS**
E1	HAZ score at age six	**9.9**	Hansen J test	9.2	6.5
E2	Schooling attainment	**13.9**	p	[0.10]	[0.26]
E3a	Skilled job tenure	**3.6**	Hausman test	**21.4**	**22.9**
			p	[<0.01]	[<0.01]
	Kleibergen-Paap test	**1.7**			

Notes: Instrumental variables (GMM) estimation using all instruments identified in Table 1 *except* wealth and parental schooling but also including a interactions between male and the intent-to-treat nutrition variables. Standard errors calculated allowing for clustering at the birth-year-village cohort level (64 clusters) shown in parentheses below coefficient estimates which are in bold if significant at 5% or lower; p-values in brackets.

Table 5. Estimated impacts of pre-schooling, schooling, and post-schooling experiences on adult cognitive skills with alternative first-stage instrument sets

		Set 1		Set 2	
Panel A		**RCS**	**NVS**	**RCS**	**NVS**
Life cycle stage	**Representation**	**IV**	**IV**	**IV**	**IV**
E1	HAZ score at age six	**0.285**	0.064	0.094	**0.267**
		(0.090)	(0.113)	(0.077)	(0.101)
E2	Schooling attainment	**0.192**	**0.102**	**0.160**	**0.058**
		(0.012)	(0.017)	(0.015)	(0.022)
E3a	Skilled job tenure	0.007	**0.113**	0.009	**0.113**
		(0.010)	(0.016)	(0.009)	(0.013)
E3b	Age at interview in 2002-4	**0.212**	0.008	0.117	0.110
		(0.075)	(0.009)	(0.068)	(0.096)
	Age squaredX10	**-0.033**	-0.007	-0.020	-0.022
		(0.012)	(0.013)	(0.001)	(0.014)
	Constant	**-3.631**	-0.219	**-2.218**	-1.185
		(1.087)	(1.429)	(1.022)	(1.476)
	F Statistic	**108.4**	**49.6**	**41.3**	**29.1**

Panel B	**Set 1: First-stage diagnostics**		**Set 1: Second-stage diagnostics**		
	Endogenous variable	**F stat on excluded instruments**		**RCS**	**NVS**
E1	HAZ score at age six	**6.8**	Hansen J test	**31.9**	**28.3**
E2	Schooling attainment	**35.3**	p	[<0.01]	[<0.01]
E3a	Skilled job tenure	**18.8**	Hausman test	**9.5**	**50.0**
			p	[0.09]	[<0.01]
	Kleibergen-Paap test	**6.8**			

Panel C	**Set 2: First-stage diagnostics**		**Set 2: Second-stage diagnostics**		
	Endogenous variable	**F stat on excluded instruments**		**RCS**	**NVS**
E1	HAZ score at age six	**2.7**	Hansen J test	78.7	81.8
E2	Schooling attainment	**3.0**	p	[0.07]	[0.07]
E3a	Skilled job tenure	**4.7**	Hausman test	-	**94.4**
			p		[<0.01]
	Kleibergen-Paap test	**1.9**			

Notes: Set 1: Instrumental variables (GMM) estimation using all instruments identified in Table 2 (as well as dummy variable indicators for missing parental education or wealth). Set 2: IV-GMM estimation using birth-year-village dummy variables, as well as male, exposure from 0-36 months and its interaction with *atole*, and the twins dummy variable as instruments. Standard errors calculated allowing for clustering at the birth-year-villagecohort level for Set 1 and at the mother level (592 mother clusters) for Set 2 presented in parentheses below coefficient estimates which are in bold if significant at 5% or lower; p-values in brackets.

Table 6. Estimated impacts of pre-schooling, schooling, and post-schooling experiences on adult cognitive skills: Alternative variable specifications (N=1,448)

Panel A		**RCS**			**NVS**
Life-cycle stage	**Representation**	**Tobit**	**RCS+ prelit**	**Quartile**	**Quartile**
E1	HAZ score at age six	0.098	0.820	**0.317**	0.349+
		(0.160)	(3.854)	(0.140)	(0.203)
E2	Schooling attainment	**0.098**	**2.702**	**0.086**	0.010
		(0.030)	(0.568)	(0.026)	(0.045)
E3a	Skilled job tenure	0.026+	0.698+	**0.036**	**0.176**
		(0.015)	(0.392)	(0.014)	(0.023)
E3b	Age at interview in 2000–4	0.142	3.603	**0.229**	-0.022
		(0.115)	(3.301)	(0.111)	(0.132)
	Age squared	-0.025	-0.656	**-0.037**	-0.004
		(0.017)	(0.494)	(0.016)	(0.019)
	Constant	-	7.136	-0.727	3.862
		-	(47.15)	(1.616)	(2.073)
	Chi^2 or F Statistic	**61.7**	**19.5**	**14.0**	**33.8**
Panel B	**Second-stage diagnostics**				
	Hansen J test	-	7.7	8.5	11.1
	Chi^2 p-value		[0.26]	[0.20]	[0.08]
	Hausman test	**20.7**	**41.7**	**41.6**	**47.7**
	Chi^2 p-value	[<0.01]	[<0.01]	[<0.01]	[<0.01]

Notes: Instrumental variables estimation using all instruments identified in Table 1 *except* wealth and parental schooling. Columns not indicated as tobit or probit (where we report derivatives for continuous variables evaluated at the mean (*dP/dx*)) are IV-GMM. Standard errors calculated allowing for clustering at the birth-year-village cohort level (64 clusters) presented in parentheses below coefficient estimates which are in bold if significant at 5% or lower; p-values in brackets.

Table 7. Estimated impacts of pre-schooling, schooling, and post-schooling experiences on adult cognitive skills: Weighting for attrition

Panel A		**RCS**	**NVS**
Life cycle stage	**Representation**	**IV**	**IV**
E1	HAZ score at age six	0.184	**0.383**
		(0.130)	(0.168)
E2	Schooling attainment	**0.084**	0.025
		(0.022)	(0.041)
E3a	Skilled job tenure	**0.037**	**0.153**
		(0.015)	(0.021)
E3b	Age at interview in 2002–4	0.159	0.026
		(0.104)	(0.113)
	Age squared	-0.027+	-0.010
		(0.015)	(0.017)
	Constant	-2.352	0.582
		(1.510)	(1.783)
	F statistic	**15.9**	**27.2**

Panel B	**1st stage diagnostics**		**2nd stage diagnostics**		
	Endogenous variable	**F stat on excluded instruments**		**RCS**	**NVS**
E1	HAZ score at age six	**10.4**	Hansen J test	9.4	9.9
E2	Schooling attainment	**12.9**	p-value	[0.15]	[0.13]
E3a	Skilled job tenure	**24.6**			
	Kleibergen-Paap test	**6.2**			

Notes: Instrumental variables (GMM) estimation, using all instruments identified in Table 1 *except* wealth and parental schooling, and weighted as described in Section 4.5. Standard errors calculated allowing for clustering at the birth-year-village level shown in parentheses below coefficient estimates, which are in bold if significant at 5% or lower; p-values in brackets.

Appendix A. Data Appendix for Pre-School HAZ Score Indicator

The data include from one to 15 measurements of height-for-age Z-scores on each of 1,954 children from the original sample between 1969 and 1977 (World Health Organization 2006). Not all of these individuals, however, were measured at the same ages or at any particular given age. For example, the greatest number of individuals was measured at nine months (8.5 m–9.5 m)—951 children or 49% of those ever measured as infants and children. Because of the tendency for similar age patterns in the Z-scores for a poorly nourished population such as this one (Martorell 1997), we use this information to obtain an estimate of the height-for-age Z-score for individuals in the sample at a common pre-schooling age. For children in this sample, in the early months of life there is a tendency for a sharp drop in Z-scores for height-for-age that then levels off and reaches a minimum at about 30 months of age, after which it increases slightly and approaches an asymptote just below -2.0 (the common cutoff for stunting) throughout the remainder of the pre-schooling period. Based on our objective of summarizing the entire pre-schooling experience, we use the height-for-age Z-score at age 72 months (6 years) as our indicator of pre-schooling experience, which is both close to the age of starting school and an age where Z-scores are relatively stable.[30] Since this measure is not available for the entire sample, when it is missing we estimate it using measurements of the Z-score for the child at ages other than 72 months. We first estimate the Z-score-age relation with dummy variables for age categories[31] other than 72 months, controlling for child-level fixed effects and then use the estimates of the age category dummy variables to adjust the nearest observed measurement of each child (for whom we do not have an observation at age 72 months) by the average difference between the measurement at the observed age and at 72 months.[32]

Even though the data permit the estimation of height-for-age Z-scores at age 72 months for 1,954 individuals in the sample as compared with 1,448 for whom we have both adult cognitive

[30] In many studies there is particular focus on the nutritional status at 36 months as being critical, particularly for linear growth (e.g., Maluccio et al. (2009) and the references therein). We note that the correlation between the measured height-for-age Z-score at 36 months and our indicator of height-for-age Z-score at 72 months is 0.97, so the use of 36 rather than 72 months does not change our basic results. We prefer to use the indicator at 72 months rather than at 36 months because we want to represent the entire pre-schooling period.

[31] The age categories are those used in the 1969–77 survey, with finer divisions for earlier ages to capture the more rapid growth during those ages: 15 days; and 3, 6, 9, 12, 15, 18, 21, 24, 30, 36, 42, 48, 54, 60, 72, and 84 months (with a small range around each targeted age). We also explored using single month intervals and obtained similar results; we prefer the age-category estimates because they smooth the estimates over months for which there are fewer observations.

[32] The resulting estimates for the height-for-age Z-scores at age 72 months are based on actual observations for 41% of the cases and age categories for 48 months and above (and therefore on an individual child curve parallel to the asymptote described in the text) for 68% of the remaining cases. The estimates for the other 32% of the imputed cases are based on the younger age categories, with the 28.5–31.5 month interval accounting for 5% of the total, and all other categories less than 5%.

skills test scores, for 180 individuals (12%) for whom we have the test scores we do not have information with which to estimate the height-for-age Z-score at age 72 months. For the 1,268 individuals for whom we have an actual or predicted height-for-age Z-score at age 72 months, the mean value is -2.24 (median -2.22), almost at the cutoff for the definition of stunting, with a SD of 0.96. The means do not differ significantly for males versus females. To retain the 180 observations on individuals without pre-schooling height-for-age when estimating the impact of the experiences during the schooling and post-schooling periods, we replace the missing height-for-age Z-score with the sample median of -2.22.

Appendix B

Appendix Table B.1. Attrition probits to construct weights used in Appendix Table B.6, correcting for attrition bias (N=2,392)

	Model 1	**Model 2**
Covariates	(1) if in sample	(1) if in sample
Male	**-0.109**	**-0.121**
	(0.019)	(0.021)
Age at interview in 2002–4	**-1.780**	**-1.690**
	(0.168)	(0.162)
Age at interview in 2002–4 squared	**0.020**	**0.019**
	(0.002)	(0.002)
Twins	**-0.187**	**-0.235**
	(0.081)	(0.082)
Student-teacher ratio at age seven	0.002	0.002
	(0.003)	(0.002)
Lower secondary school available at age seven	0.044	0.047
	(0.045)	(0.045)
Born in the two years prior to 1976 earthquake	0.097	0.043
	(0.061)	(0.062)
Mother or father had died by age 18	0.091	**0.163**
	(0.046)	(0.042)
Ln Salary in manufacturing at age 18	**-4.031**	**-3.817**
	(0.263)	(0.264)
Exposure to intervention 0–36 months	0.039	0.027
	(0.069)	(0.071)
Exposure to intervention 0–36 months × *atole*	0.023	-0.013
	(0.061)	(0.058)
Child lived with both mother and father in 1975	-	**0.077**
		(0.038)
Child lived with both mother and father in 1987	-	**0.058**
		(0.027)
Mother alive in 2002	-	**0.254**
		(0.042)
Father alive in 2002	-	**0.120**
		(0.031)
Mother living in original village in 2002	-	**0.094**
		(0.037)
Father living in original village in 2002	-	0.046
		(0.031)
Number of siblings in survey	-	-0.002
		(0.002)
Whether any sibling reinterviewed in 2002–4	-	**0.269**
		(0.043)
Chi^2 statistic on variables in model 2 only	-	**312.6**
		[<0.01]
Model Chi^2 statistic	**328.9**	**618.5**
	[< 0.01]	[< 0.01]
Pseudo-R^2	0.22	0.30

Notes: Sample consists of all 2,392 individuals who were exposed to the INCAP supplementation intervention between 1969 and 1977. Standard errors are calculated allowing for clustering at the birth-year-village level shown in parentheses below coefficient estimates, which are in bold if significant at 5% or lower; p-values in brackets (StataCorp 2011). Derivatives evaluated at the mean (*dP/dx*) presented.