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What Determines Adult Cognitive Skills?

Impacts of Preschooling, Schooling, and Post-Schooling Experiences in Guatemala

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Contents

List of Tables

List of Figures

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ABSTRACT

Most investigations into the importance and determinants of adult cognitive skills assume that (1) they are produced primarily by schooling, and (2) schooling is statistically predetermined or exogenous. This study uses longitudinal data collected in Guatemala over 35 years to investigate production functions for adult cognitive skills—that is, reading-comprehension skills and nonverbal cognitive skills—as being dependent on behaviorally determined preschooling, schooling, and post-schooling experiences. We use an indicator of whether the child was stunted (child height-for-age Z-score < –2) as our representation of preschooling experiences, and we use tenure in skilled occupations as our representation of post-schooling experiences. The results indicate that assumptions (1) and (2) lead to a substantial overemphasis on schooling and an underemphasis on pre- and post-schooling experiences. The magnitudes of the effects of these pre- and post-schooling experiences are large. For example, the impact on reading-comprehension scores of not being stunted at age 6 is equivalent to the impact of four grades of schooling. These findings also have other important implications. For example, they (1) reinforce the importance of early life investments; (2) point to limitations in using adult schooling to represent human capital in the cross-country growth literature; (3) support the importance of childhood nutrition and work complexity in explaining the "Flynn effect," or the substantial increases in measured cognitive skills over time; and (4) lead to doubts about the interpretations of studies that report productivity impacts of cognitive skills without controlling for skill endogeneity.

Keywords: human capital, cognitive skills, nutrition, stunting, work experience, development, Guatemala

1. INTRODUCTION

Increasing the stock of human capital is seen by many to be central to increasing individual options and economic development. For example, cognitive skills, or the capacity to assess and solve problems—or in T. W. Schultz's (1975) memorable phrase, "the ability to deal with disequilibria"—are widely believed to be one dimension of human capital that affects productivity in many activities, including work, raising children, and improving one's own and others' health and nutrition. Substantial empirical literature from various disciplines, most based on significant associations between schooling attainment (i.e., highest completed school grade or level) and the outcomes of adult activities, has been interpreted to support this proposition.[1]

If the centrality of human capital for increasing individual options and economic development is accepted, then understanding the processes by which dimensions of human capital, such as cognitive skills, are determined represents a critically important research question. This may seem to be well-trodden territory—there are, for example, hundreds of studies on the determinants of schooling in developing countries.[2] Schooling attainment, however, is not a direct measure of cognitive skills; rather, it is a measure of grades completed. As such, it is only one potential input, although perhaps an important one, into the production of cognitive skills. In a developing country, schooling is typically limited to particular periods of an individual's life, primarily childhood and adolescence. Other experiences, both before and after the school years, may also have important effects on cognitive skills. For example, substantial literature emphasizes the importance of nutrition from conception onward for neural and cognitive development, which may affect cognitive skills into adulthood.[3] There is also 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 are usually interpreted as reflecting such skills.[4] If preschooling or post-schooling experiences have significant effects on the cognitive skills of adults *and* are correlated with schooling—as is likely, because of correlations among human capital investments across life-cycle stages—then standard approaches that represent adult cognitive skills using only schooling attainment are likely to misrepresent the production process, including the effects of schooling itself.

This paper examines the importance of preschooling, schooling, and post-schooling experiences in determining two representations of adult cognitive skills (reading-comprehension skills and nonverbal cognitive skills) in Guatemala. In this low-income context, it is widely expected that greater cognitive skills will lead to increases in an individual's options and welfare, reductions in poverty, and advancements in economic development. Specifically, we investigate the following questions related to determining adult cognitive skills:

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[1] For example, hundreds of empirical studies are interpreted as showing the impact of cognitive and other skills obtained through education on wages or incomes. The vast majority of these studies use schooling attainment to represent these skills (see Psacharopoulos and Patrinos 2004). A small number of studies instead use direct measures of adult cognitive skills (e.g., Boissiere, Knight, and Sabot 1985; Murnane, Willet, and Levy 1995; Alderman et al. 1996; Glewwe 1996a, 1996b). The many empirical studies of the effects of cognitive and other skills on outcomes such as health, nutrition, and fertility almost all use schooling attainment to represent these skills (see Strauss and Thomas 1998).

[2] See, for example, references in the surveys in Schultz (1987, 1988), Strauss and Thomas (1995), and Behrman (2009).

[3] See Galler et al. (1983); Galler (1984); Pollitt (1990); Engle et al. (1992, 2007); Pollitt et al. (1993); Bleichrodt and Born (1994); Glewwe and Jacoby (1995); Grantham-McGregor (1995); Martorell et al. (1995); Martorell et al. (1998); Wachs (1995); Grantham-McGregor et al. (1997); Martorell (1997); Hack (1998); Grantham-McGregor, Fernald, and Sethuraman (1999a, 1999b); Ramakrishnan et al. (1999); Glewwe, Jacoby, and King (2000); Alderman et al. (2001a); Glewwe and King (2001); Lozoff and Wachs (2001); Richards et al. (2001); Li et al. (2003, 2004); Behrman and Rosenzweig (2004); Alderman and Behrman (2006); Alderman, Hoddinott, and Kinsey (2006); Maluccio et al. (2008); and Victora et al. (2008).

[4] There is considerable emphasis in the economics literature, for example, on post-schooling learning both on-the-job and through formal training programs. For instance, standard earnings functions, whether motivated by 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.

- How important is schooling attainment?
- How important are preschooling and post-schooling experiences?
- Does the incorporation of preschooling and post-schooling experiences change the apparent importance of schooling?
- Does controlling for the possibility that schooling (as well as pre- and post-schooling experiences) is a behavioral choice change the apparent importance of schooling?
- Are there important differences between the determinants of adult reading-comprehension skills and nonverbal cognitive skills?
- Is there evidence that unobserved cognitive skills endowments (e.g., genetic abilities) affect significantly, and perhaps differentially, the preschooling, schooling, and post-schooling experiences?
- Are there significant gender differences?

Major results include the following: (1) schooling has a significant and substantial impact on adult reading comprehension (although not on adult nonverbal cognitive skills). However, estimates of this impact are substantially biased upward if there are no controls for schooling's behavioral determinants in the presence of persistent unobserved factors, such as genetic endowments, and if family background factors that appear to be correlated with genetic endowments are included among the first-stage instruments. (2) Both preschooling and post-schooling experiences—represented by the nutritional status of the individual as a preschooler and years of skilled work experience—have substantial and significant impacts on one or both of the adult cognitive skill measures. These measures tend to be underestimated if pre- and post-schooling experiences are treated as statistically predetermined. This finding is in contrast to 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 or that random measurement errors are large. (3) Failing to incorporate pre- and post-schooling experiences in the analysis of adult cognitive skills or the outcomes affected by such skills is likely to yield a misleading overemphasis on schooling relative to these pre- and post-schooling experiences. (4) Gender differences in the coefficients of the adult cognitive skills production functions are not significant. This suggests that most of the fairly substantial differences in adult cognitive skills favoring males, on average, originate from gender differences in schooling attainment and in experience in skilled jobs that favor males.

The data demands for our investigation are considerable. We utilize unusually rich longitudinal data from Guatemala collected over 35 years (1969–2004), with sample members 25–42 years of age during the last survey round in 2002–2004 and with substantial prospective and recall information on their development through the preschooling, schooling, and post-schooling years, as well as on exogenous factors that conditioned these experiences. We investigate the impact 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 and indirectly via their effects on the different life-cycle stage experiences, which might, in turn, affect adult cognitive skills.

Section 2 presents the conceptual framework that guides our analysis. Section 3 describes the data we use. Section 4 presents our results, and Section 5 concludes.

2. CONCEPTUAL FRAMEWORK

We estimate production functions for two types of adult cognitive skills (measured in 2002–2004, when respondents were 25–42 years of age): adult reading-comprehension skills (RCS) and adult nonverbal skills (NVS). We posit that each skill type is produced by genetic and other observed and unobserved endowments related to learning capacities and motivations (E0); to all previous experiences (Ei, with i = 1, 2, 3 for the three life-cycle stages defined below); and to a stochastic term (U), which reflects all other idiosyncratic, and assumed exogenous, learning experiences. Because of the nearly exclusive emphasis in the literature on schooling, as well as schooling's plausibly central role in forming cognitive skills, we organize experiences into the following three life-cycle stages:

Stage 1: preschooling (from conception through about age 6);
Stage 2: schooling (from age 7 through about age 15)[5];
Stage 3: post-schooling (from about age 15 to age at the time of survey—that is, 25–42).

The production functions are as follows:[6]

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

and

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

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 and the disturbance term, are potentially vectors.

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 impact of school-years experience (E_2). Estimation of relation (1), however, is challenging, because the experiences for the three life-cycle stages on the right all reflect previous behavioral choices.

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

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

For instance, *W* might represent consumption financed by resources generated by labor earnings that depend in part on cognitive skills. This welfare function 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.

[5] In Guatemala, the legal starting age for schooling is age 7 and 82 percent of the respondents in the sample we use completed schooling by age 15.

[6] An alternative production function specification used in some studies 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. For example, the change in cognitive skills during the school years is posited to depend on the intervening schooling. We do not use this framework, however, because its estimation requires strong functional form restrictions on the production technology (Todd and Wolpin 2003, 2007) and because the necessary data are not available.

Life-Cycle Stage 1 (Preschooling)

Parents allocate resources to obtain the optimal E1 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 E1 and W, and child-specific endowments. The E1 production function is

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

where N_1 is a vector of family-determined inputs into the production of E_1 (e.g., family-provided nutrients), C_{1p} is a vector of community inputs into the production of E_1 (e.g., community-provided nutrients, community-provided preschool programs), E_{0p} is the child endowment that directly enters 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 N_1 and, therefore, E_1 to maximize the expected welfare W. This welfare maximization is constrained by a vector of parental family resources F_1, such as parental schooling and assets; all relevant community characteristics for this life-cycle stage C_1, including community characteristics that directly affect the production of E_1 through C_{1p}, as well as other community characteristics that may affect the household through other channels; all child endowments E_0, including not only E_{0p} but also E_{0r} and E_{0n}, which also affect the decision to invest in E_1, because the impact of E_1 on W generally depends on these other endowments; and all stochastic terms that affect outcomes in the first life-cycle stage of the child U_1, including U_{1E}, as well as other stochastic factors that affect the family during the first life-cycle stage for this child (e.g., stochastic factors affecting the health of other siblings may affect the inputs devoted to this child)—plus the expected values of these variables in the next two life-cycle stages—F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, U_{12}^e, U_{13}^e, where 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 regarding these variables over the next two life-cycle stages:

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

where the superscript d refers to reduced-form demand relations.

Life-Cycle Stage 2 (Schooling)

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

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

If the outcome of Stage 1 (E_1) encompasses all the impacts of first life-cycle stage exogenous variables,[7] then the conditional (on E_1) demand function for E_2 is

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

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

[7] That the preschool experience encompasses all the effects of preschool exogenous variables is a strong assumption that we do not maintain for our estimates of the cognitive production functions below. 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).

Life-Cycle Stage 3 (Post-Schooling)

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

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

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

$$E_3 = E_3^d\,(E_2, F_3, C_3, U_3). \qquad (8)$$

This relation allows for the possibility, for example, that post-schooling experiences depend, inter alia, on schooling (and, through schooling, on preschool nutritional status). Through this process, the adult cognitive skills in relation (1) are determined as well.

Some Implications of the Model for Estimation

1. *Omitted variable biases in estimates of adult cognitive skills production function:* If the true specification in relations (1r) and (1n) includes all three life-cycle experiences but the estimated specification excludes one or more of the life-cycle experiences, then omitted variable biases are likely. This is because 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 the three reduced-form demand relations [relations (4), (5), and (7)], which means that 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 preschool–age experience (E_1) and the post-school–age experience (E_3) depends explicitly on the school-age experience and indirectly, through the school-age experience, on the preschool age experience. That the three life-cycle stage experiences are likely to be correlated is hardly surprising. A priori, a child with better parental family background or who lives in a better community in terms of health and educational services and employment opportunities is likely not only to have more schooling but also better pre- and post-schooling experiences.

2. *Endogeneity biases in estimates of adult cognitive skills production function:* Direct estimates of relations (1r) and (1n) that do not control for the behavioral determinants of the three life-cycle experiences are also likely to be biased. As is evident in the reduced-form demand relations (4), (5), and (7), each of the three life-cycle experiences depends on all the endowments. Moreover, these biases could be in either direction. For instance, the ability bias on which the returns-to-schooling literature has often focused is consistent with E_2 (schooling) and the innate ability component of E_0 being positively correlated to the result that the coefficient on schooling is likely to be upward biased in ordinary least squares (OLS) estimates of relations (1r) and (1n). However, if the summary measure of preschooling experience is some variable such as child stunting (see Section 3) and if ability and physical endowments that influence stunting are negatively correlated, as suggested by Behrman and Rosenzweig (2002, 2004), then coefficient estimates for stunting in OLS estimates of relations (1r) and (1n) may be negatively biased at the same time that coefficient estimates for schooling are positively biased.[8]

[8] If the correctly specified regression model is $K_{3r} = E\beta_1 + E_{0r}\beta_2 + U_{3r}$, where E is a vector with the three life-cycle experiences and E_{0r} is a vector with unobserved endowments, then the standard omitted variable result is that $E[b_1] = \beta_1 + P_{12}\beta_2$, where P_{12} is the variance-covariance matrix between E and E_{0r} (e.g., Greene 2000). With certain structures of $\mathbf{P_{12}}$, this can lead to the first component of β_1 (for E_1) being negatively biased and the second component of β_1 (for E_2) being positively biased in OLS estimates, even if all components of β_2 are positive.

3. *Need for instruments with potential predictive power to identify the three life-cycle experiences in estimating the adult cognitive skills production:* The three reduced-form demand relations [relations (4), (5), and (7)] give the potential instruments for identifying the three life-cycle experiences in the adult cognitive achievement production functions in relations (1r) and (1n).[9] Although some instruments may at first seem to have first-order effects on particular life-cycle experiences (e.g., preschool nutrition or programs on E_1, school characteristics on E_2, labor market characteristics on E_3), it would not be correct to assert a priori that a particular instrument identifies a particular life-cycle experience. This can be seen from the three reduced-form demand relations, in which the same endowments and the actual or expected values of the family, community, and stochastic factors are all on the right side for each life-cycle stage. 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 instruments' plausibility to examine whether subsequent life-cycle family- or community-level variables were significant in previous life-cycle outcome equations (e.g., if schooling characteristics or post-schooling labor market characteristics significantly determined preschooling experience E_1), because the (expected) values of those variables should be included.

4. *Need for instruments that are not correlated with the disturbance term (including unobserved endowments) in the adult cognitive skills production functions:* The reduced-form demand relations indicate the potential set of instruments. However, not all of the right-side variables in those relations are likely to be good instruments in the sense of being independent of the disturbance terms in relations (1r) and (1n). For example, previous studies suggested that there can be intergenerational correlations of endowments through genetics and perhaps other means (e.g., Behrman and Rosenzweig 2002, 2005; Behrman et al. 2008; Stein et al. 2003). To avoid this possible correlation, we do not include parental schooling and assets as instruments in our preferred set of estimates. This approach is confirmed empirically for our cognitive achievement production function estimates, because their inclusion leads to rejection of overidentification tests.

[9] The instruments should be directly indicated by the economic model, as here, although often that is not the case (e.g., Heckman 1997).

3. DATA

The data demands for estimating the adult cognitive skills production functions posited in Section 2 are considerable. We utilize an unusually rich longitudinal data set from Guatemala that has been collected over a 35-year period and that includes measures of adult cognitive skills, socioeconomic and anthropometric measures, shocks from an experimental intervention, and market and policy changes. We first provide a general description of the nutritional intervention around which the initial survey was organized. We then focus on the variables used in this analysis.

3.1. The Experimental Nutritional Intervention[10]

In the mid-1960s, protein deficiency was seen as the most important nutritional problem facing the poor in developing countries, and there was considerable concern that this deficiency affected children's ability to learn. The Institute of Nutrition of Central America and Panama (INCAP), based in Guatemala, was the locus of a series of studies on this subject, leading to a nutritional supplementation trial begun in 1969 (Habicht and Martorell 1992; Read and Habicht 1992; Martorell, Habicht, and Rivera 1995). The principal hypothesis underlying the trial was that improved preschool nutrition would accelerate mental development. To test this hypothesis, 300 rural communities, each with 500–1,000 inhabitants, in eastern Guatemala (in areas not directly affected by the civil war) were screened in an initial study to identify villages of appropriate compactness (to facilitate access to feeding centers; see below), ethnicity and language, diet, access to health-care facilities, demographic characteristics, child nutritional status, and degree of physical isolation.[11] From this group, two sets of village pairs (one pair of "small" villages with about 500 residents each and another pair of "large" villages with about 900 residents each) were selected based on their similarities in all these characteristics. Two villages, one from each pair, were randomly assigned to receive a dietary supplement of a high-protein energy drink, known as *atole*. *Atole*, the Guatemalan name for porridge, is made from Incaparina (a vegetable protein mixture developed by INCAP and widely accepted for young children in Guatemala), dry skim milk, and sugar. It was served hot and was slightly gritty, but with a sweet taste.

In designing the intervention, there was considerable concern that the social stimulation for children—resulting from their social interactions while attending feeding centers where the supplement was distributed, the observation and measurement of their nutritional status, and the monitoring of their intakes of *atole*—also might affect child cognitive and nutritional outcomes, thus confounding efforts to isolate the impact of the *atole* supplement. To address this concern, the two remaining villages were provided with an alternative supplement, known as *fresco*. *Fresco* was a cool, clear-colored, fruit-flavored drink that contained no protein, had only sufficient sugar and flavoring agents for palatability, and contained about one-third of the calories of *atole* per unit volume.[12]

Collection of data for this study 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 village children under seven years of age. Cohorts of newborns were included from February 1969 until September 1977, approximately six months after supplementation ceased. Data

[10] For a more extensive discussion, see Grajeda et al. (2005), Hoddinott, Behrman, and Martorell (2005), Martorell et al. (2005), and Stein et al. (2005).

[11] There is no particular reason to believe that the four villages ultimately selected were substantially atypical from the 300 potential candidates. For example, none of the four had had significant previous public health interventions (Habicht and Martorell 1992). Information collected during the screening process on the other villages, however, is no longer available to explore this further.

[12] Several micronutrients were added to both the *atole* and *fresco,* in amounts that yielded equal concentrations per unit of volume. A program of primary medical care was also provided free of charge throughout the period of data collection. Periodic preventive health services, such as immunization and antiparasite campaigns, were conducted simultaneously in all villages.

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 1969–1977 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 the entire time from birth to 36 months of age, posited to be a critical time period for child growth in the nutritional literature (Section 3.2). *Atole* and *fresco* were distributed in each village in centrally located feeding centers. They were available daily, on a voluntary basis, to all members of the community, regardless of age or participation in the research's survey components, and during times that were convenient to mothers and children but that did not interfere with usual mealtimes.

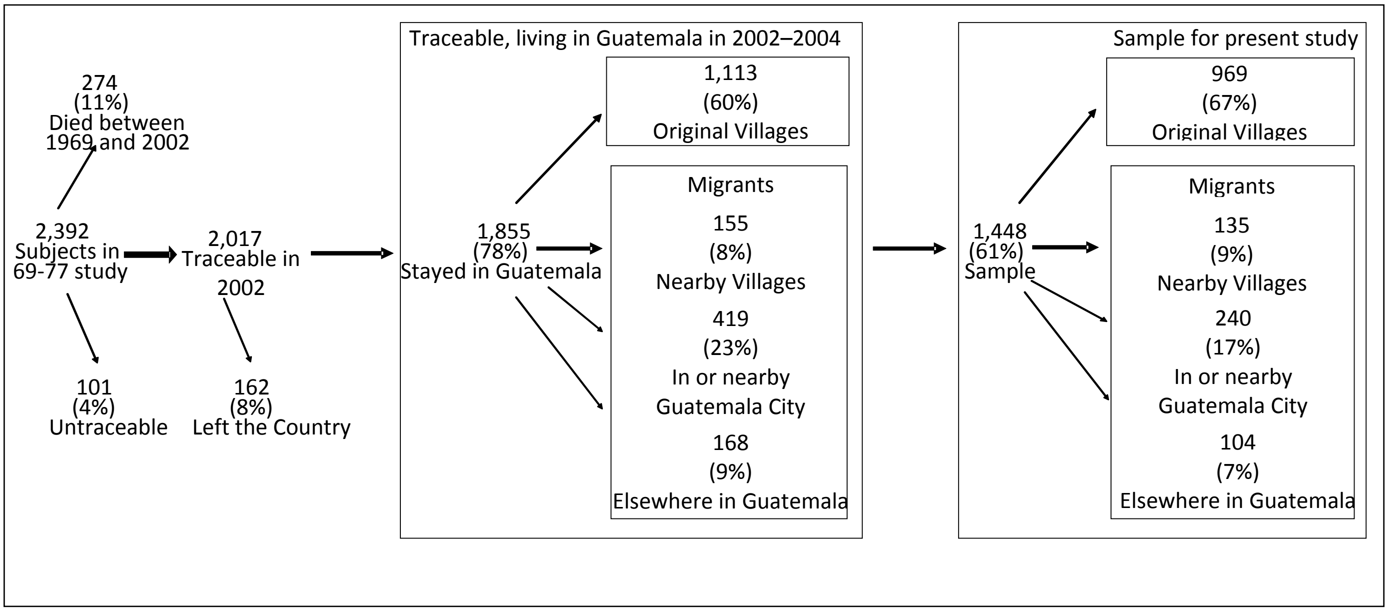
Because we use children's differential exposure to the availability of the nutritional supplements as first-stage instruments to estimate relations (4), (5), and (7), which are then excluded from relation (1), it is important to establish that the two interventions resulted in differential consumption of calories, protein, and other nutrients. Approximately 70 percent of children between the ages of 0 and 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 age 6–12 months consumed approximately 70 kilocalories (kcal) of *atole* supplement per day; children 12–24 months, 90 kcal; and children 24–36 months, 120 kcal. Children between the ages of 6 and 24 months in the *fresco* villages, however, consumed only 20 kcal of *fresco* supplement per day, rising to approximately 30 kcal by the age of 36 months (Schroeder, Kaplowitz, and Martorell 1992; Islam and Hoddinott, forthcoming).

In 2002–2004, a multidisciplinary team of investigators, including most of the authors of this paper, undertook a follow-up survey targeted toward all participants of the 1969–1977 survey.[13] In a more recent survey, sample members ranged from 25 to 42 years of age. Figure 1 shows what happened to the 2,392 individuals 0–15 years old from the original sample by the time of the 2002–2004 survey: 1,855 (78 percent) were alive and known to be living in Guatemala (11 percent had died, the majority due to infectious diseases in early childhood; 7 percent had migrated abroad; and 4 percent were not traceable). Of these 1,855, 1,113 lived in the original villages, 155 lived in nearby villages, 419 lived in or near Guatemala City, and 168 lived elsewhere in Guatemala. For the 1,855 traceable sample members living in Guatemala, 1,571 (85 percent) completed at least one interview during the 2002–2004 survey (Grajeda et al. 2005). This study includes the 1,448 respondents (54 percent of whom are female) interviewed in 2002–2004 for whom the two measures of adult cognitive skills central to this analysis (Section 3.2) are available. These respondents make up 78 percent of the 1,855 individuals who were alive and known to be living in Guatemala and 61 percent of the original sample. Measured from 1977 to 2002, the latter figure indicates an annual attrition rate of approximately 2 percent, low when compared with shorter-term longitudinal surveys in developing countries (Alderman et al. 2001b) or to long-term longitudinal surveys in developed countries (Fitzgerald, Gottschalk, and Moffitt 1998b).[14] Nevertheless, almost 40 percent represents substantial attrition, so we assess potential attrition bias in Section 4.2.4.

[13] 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 referenced many of these studies; more recent examples include Behrman et al. 2008, Hoddinott et al. 2008, and Maluccio et al. 2008.)

[14] Most measures of attrition refer to households or individuals who were past infancy and early childhood when the sample was taken, so they would not even include the effects of infant and early childhood mortality that account for more than one-quarter of the attrition in the data used for this study.

Figure 1. Relation between original 1969-1977 sample and 2002-2004 sample



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 Cognition Skills (RCS): The vocabulary (Level 3, approximately fourth-grade equivalent) and reading-comprehension (Level 2, approximately third-grade equivalent) modules of the Inter-American Reading and Comprehension Tests (IARC) (see Manuel 1967) were administered to the 1,197 individuals (83 percent of the sample of 1,448) who passed a preliteracy screen.[15] The vocabulary portion has 45 questions and reading comprehension has 40 questions, yielding a maximum possible score of 85 points. Distribution of the IARC 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 percent of the sample who did not pass the preliteracy screen were assigned a value of 0 for the reading-comprehension tests. Including those we score at 0, the mean score is 36.1, with a standard deviation (SD) of 22.3, indicating substantial sample variation. Males (mean 37.9, SD 22.7) average significantly higher scores than do females (mean 34.4, SD 21.8). The IARC tests have demonstrated adequate test-retest reliability (correlation coefficients of 0.87 and 0.85 for vocabulary and reading, respectively) in previous studies in this population when the subjects were adolescents and young adults (Pollitt, Gorman, and Metallinos-Katasaras 1991; Pollitt et al. 1993). Because of the nature of the test score distribution, in the empirical analysis below we (1) explore how our results change when we use Tobit rather than linear estimators and (2) consider three alternative indicators of adult reading-comprehension skills.

[15] 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 preliteracy test that began with reading out letters. They were considered literate if they passed the test with fewer than 5 errors out of 35 questions, the most difficult of which was reading a five-word sentence aloud.

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	*
	(22.31)	(21.85)	(22.71)	
RCS + preliteracy scores (RCS + prelit)	66.93	64.74	69.45	*
	(29.67)	(29.94)	(29.18)	
2. Nonverbal skill (NVS)	17.70	16.26	19.36	*
	(6.14)	(5.40)	(6.51)	
$\boldsymbol{E_1}$: Not stunted at age 6: HAZ ≥ –2.00 (1,269 non-missing cases)	0.53	0.53	0.53	
HAZ (1,269 non-missing cases)	–1.99	–2.00	–1.99	
	(0.98)	(0.98)	(0.98)	
$\boldsymbol{E_2}$: Schooling attainment (grades)	4.69	4.29	5.15	*
	(3.45)	(3.29)	(3.56)	
$\boldsymbol{E_3}$: Skilled job tenure (years)	2.85	1.27	4.67	*
	(4.83)	(3.42)	(5.53)	
Living in Guatemala City in 2002–2004 dummy	0.17	0.18	0.15	
Age at interview in 2002–2004	32.33	32.42	32.23	
	(4.24)	(4.30)	(4.17)	
Age at interview in 2002–2004 squared	1,063.2	1,069.5	1,056.0	
	(277.8)	(281.6)	(273.4)	
Instruments				
$\boldsymbol{E_0}$: Genetic endowment				
Male dummy	0.46	—	—	
Twins dummy (850 non-missing cases)	0.02	0.02	0.01	
$\boldsymbol{F_0}$: 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	*
	(1.69)	(1.61)	(1.77)	
Father's schooling attainment (1,344 non-missing cases)	1.71	1.69	1.77	
	(2.13)	(2.02)	(2.25)	
C: Natural, market or policy shocks				
Student-teacher ratio at age 7	39.93	40.26	39.55	
	(9.00)	(9.39)	(8.50)	
Lower secondary school available at age 7	0.21	0.21	0.21	
Mother or father had died by child age 18 dummy	0.08	0.08	0.09	
Ln salary in manufacturing industry at age 18	1.60	1.58	1.61	
	(0.51)	(0.51)	(0.50)	
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	
N	1,448	775	673	

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

(1A) RCS + preliteracy score: We sum the raw score from the preliteracy screen and the IARC tests under the assumption that those who were exempt from the preliteracy test (and therefore who only took the IARC test) would have earned a perfect score had they taken it. Because those who failed the preliteracy test had scores in the 0–35 range, this leads to variation in scores among those to whom we assigned 0 for the RCS alone.

(1B) RCS-quartile: Each individual's IARC test score is recorded in the quartile of the distribution in which it falls. All those who failed the preliteracy test (17 percent) are in the first (lowest) quartile.

(1C) RCS-median: We record whether the individual scored above the median on the IARC test, treating those who failed the preliteracy test as having scored below the median.

(2) Adult Nonverbal Cognitive Skills (NVS): All respondents were administered Raven's Progressive Matrices (Raven, Court, and 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." We administered 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 IARC, 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 exhibited adequate test-retest reliability (correlation of 0.87) in this population in the past (Pollitt et al. 1993). We use the Raven's test scores as the dependent variable for NVS, but also consider two alternatives paralleling (1B) and (1C) above: NVS-quartile and NVS-median.

Life-Cycle Stage Experiences (E1, E2, E3)

We assume that relation (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.[16] For the third life-cycle stage, we examine multiple indicators, because it is less obvious a priori which representation of post-schooling experience is most relevant.

Preschooling 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 preschooling child height-for-age Z-scores,[17] a widely accepted indicator of childhood nutrition, to calculate our indicator for preschooling experience. Because the nutrition literature places particular emphasis on whether a child is stunted (i.e., height-for-age Z-score < -2.0), we focus on stunting in our analysis. In particular, we include a dummy variable indicating whether the child was *not* stunted at age six and therefore had better nutrition. (Appendix A provides details on the data underlying this variable.)

Schooling experience: We use completed schooling attainment (highest grade completed) as our indicator of learning experiences during this life-cycle stage. As schooling experience 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 percent of the individuals), or completion of primary school. There are also secondary modes at zero grades (14 percent) and three grades (11 percent). The highest grade completed is significantly correlated with the indicators for the preschooling experience (correlation of 0.10) and the post-schooling experiences described below (skilled work tenure, correlation of 0.13; whether living in Guatemala City, correlation of 0.28; and age,

[16] If we 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 need to estimate at least 11 parameters. This more flexible specification exceeds the limits of what we are able to estimate with any precision, particularly in light of the correlations among many of the variables. Indeed, when we add squared and interaction terms of the three life-cycle experience measures to our preferred specification in Table 2, none of the individual coefficients of these terms is significant, and joint tests reject the significance of them as a group. Therefore, it is not possible for us to assess empirically possible interactions of the life-cycle stages through such an approach.

[17] Z-scores are the standard deviations from the median of the World Health Organization (WHO) / National Center for Health Statistics (NCHS) / Centers for Disease Control and Prevention (CDC) standards. WHO (2006) recently released new standards; the standards that we use and the new WHO standards are highly correlated for height-for-age.

correlation of –0.13). This suggests that if relation (1) is the true relation but only schooling is included in the estimation, then 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 multiple measures for this third life-cycle stage.

- *Tenure in skilled occupations:* We use the duration in years of continuous work experience in skilled occupations prior to the 2002–2004 survey[18] as our first indicator of post-schooling experience that may contribute to adult cognitive skills. The data do not permit the calculation of total experience in all jobs since individuals left school, but rather tenure in all the jobs held in both 1998 and in 2002–2004.[19] Such experience is likely to strengthen cognitive skills via (1) learning by doing through problem solving, (2) furthering skills learned in school by 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 an SD of 4.8. Two-thirds of the individuals for whom there are test scores were not in skilled occupations in 1998 or in 2002–2004; therefore, they report no tenure. Among the 530 sample members who had at least some tenure in a skilled occupation, the mean is 7.8 years, with an SD of 5.0. There are substantial and significant differences for tenure in skilled occupations by gender, with 388 out of 673 males (58 percent) having such experience (mean 8.1 years, SD 5.0), but only 142 out of 775 females (18 percent) having such experience (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, shopping, entertainment, and other ways that might affect adult cognitive skills. About 17 percent of the sample lived in Guatemala City at the time of the 2002–2004 survey—18 percent of the females and 15 percent of the males. The mean of the estimated number of years that these individuals had been away from their origin villages 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.

- *Age*: Age can affect both the accumulation of adult cognitive skills through more experience and the depreciation of skills if they are not used. The mean sample age is 32.3 years (SD 4.2), with no significant difference by gender. Given the limited schooling attainment of individuals in the sample, most of these years (on average, nearly two decades) are post-schooling.

The indicators of post-schooling experiences are positively correlated with one another, but all three pair-wise correlations are relatively small, lying between 0.04 and 0.12. This underscores the possibility that they may represent different dimensions of post-schooling experiences.

Initial Conditions (E_0, F_0)

Genetic and other endowments and other individual characteristics (E_0): We do not have direct observations on genetic endowments beyond the individual's gender, which we control for in our first-stage estimates. Other individual characteristics that we observe include age at the time of the 2002–2004

[18] 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., police), and own farm/own enterprise work that yields income in the top quintile for such activities in 2002–2004. We have explored various definitions for the skills measure, and our results are unchanged. For example, results are similar if we (1) use the skills measure described above but truncate it to 10 years to avoid outliers; (2) treat as skilled labor only those with skilled wage employment; and (3) use (2) 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).

[19] Although we are unable to test whether total experience has a greater effect than recent experience, there are a priori arguments for recent experience to be more important than earlier experience, particularly if there is depreciation of unused knowledge.

interview and whether the individual was a twin (1.0 percent of the sample),[20] which may have longer-run implications associated with the generally lower birth weight of twins (Behrman and Rosenzweig 2004). Because we include age directly in the second-stage estimates to reflect depreciation/learning effects with experience, controlling for secular birth cohort effects in the first stage does not provide an identifying instrument.

Wealth and parental schooling attainment in childhood (F_0): In some specifications, although not our preferred ones, we directly 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 wealth index constructed at the household level. As part of the survey work accompanying the 1969–1977 intervention, all households in these villages, including those with children participating in the supplementation trial, participated in censuses in 1967–1968 and 1975. These censuses ascertained ownership of a set of household durables as well as housing characteristics. Using principal components, these assets and characteristics were combined into an index that we interpret as a "wealth" index.[21] For those born before January 1, 1971, we use the 1967–1968 index score, while for those born after that date, we use the 1975 index score. Although this index surely misses some dimensions of wealth, such as financial and productive assets, in these villages in the late 1960s and early 1970s, such unmeasured assets were not only uncommon, but also likely to be highly correlated with the assets that were measured (Maluccio, Murphy, and Yount 2005). Parental schooling is very low, with both fathers and mothers averaging fewer than two completed grades.[22]

Observed Events and Shocks (C)

Experimental nutritional shocks: 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 he or she 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 the child lived in one of the two *atole* villages.

Natural, market, or policy events: We include community-level variables that relate as closely as possible to the timing of key learning-related and labor market–related decisions in each individual's development. Using information reported in earlier work about infrastructure, markets, and services in the villages (Pivaral 1972; Bergeron 1992), complemented with a retrospective study done in 2002 (Estudio 1360, 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 variables are measured in the village 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 (likely to be in their early working years). Although reflecting community-level characteristics, these variables vary by single-year age cohorts within each village, as well as across villages (except for the manufacturing salaries). Because these measures closely relate to the availability and longevity of schools and markets for the period in an individual's life when critical decisions (e.g., starting school) are being made, they are an improvement over the more typical approach of including indicators about such factors in a given year for a population with different ages at that point. We also include an additional individual specific shock—whether the individual's father or mother had died (prematurely and possibly

[20] For those individuals missing information on twins status, we assume they were not twins.

[21] This asset measure is not likely to have been affected by the intervention due to the small monetary value of the intervention to households (that moreover roughly equally affected households across the two intervention types).

[22] For the small percentages of individuals without parental schooling or the wealth index, we replace the missing item response with sample medians.

accidentally) by the time that individual was 18, which relates to a critical juncture for entering the labor force.[23]

Identification Strategy

There are three components to our identification strategy. First, we select, on a priori grounds related to the framework in Section 2, plausibly exogenous characteristics from individuals' backgrounds and communities to identify 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 variables derived from the original nutritional supplementation intervention. Second, we incorporate all three of these life stages. This substantially mitigates the possibility of the instruments having direct effects that do not operate through the measured experiences in the model's second stage. For example, in our main specifications in Section 4.1, for the presence of a lower secondary school at age 7 to be an invalid instrument, it would have to have a direct effect that was outside its effect on preschooling nutrition (through expectations), schooling attainment, *and* post-schooling skilled work tenure. Finally, we carry out a range of diagnostic tests to assess the instruments' strength and validity, as well as to consider alternative instrument sets and estimation procedures. On the whole, we find that the instruments we utilize have reasonable power and, with the exception of those specifications that include parental schooling and household wealth (F_0), we fail to reject the overidentification tests.

[23] Clearly some of these shocks also relate to individuals' parental families, particularly whether individuals' parents were alive when individuals were age 18, but also to market shocks. Because of the relation to parental families, these shocks might be related to endowments, as is suggested above, for household wealth and parental schooling attainments. In contrast to the results that include wealth and parental schooling attainments in the first-stage instrument set for NVS, however, inclusion of the parental death shocks in the instrument set does not result in a rejection of the Hansen J overidentification test.

4. RESULTS

Section 4.1 summarizes our main estimates, using indicators of experiences during each of the three life-cycle stages and endogenizing them in an instrumental variables framework. We also present estimates that show the association between schooling attainment alone and our two measures of adult cognitive skills—RCS and NVS—to verify that these associations are strong, as usually has been assumed in the previous literature. In Section 4.2, we consider the robustness of our estimates in terms of gender differences, variations in the instrumental variables used, alternative RCS and NVS representations, and controls for attrition. Given the nature of the survey, a number of individuals in the sample are siblings or half-siblings. Throughout, we control for mother cluster effects in estimating the standard errors (with 592 clusters for the 1,448 observations), with the exception of Section 4.2.5, where we explore alternative approaches to calculating the standard errors.

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 RCS (Inter-American Reading and Comprehension Tests) in the first panel of the table and for NVS (Raven's Progressive Matrices tests of nonverbal skills) in the second panel. Both are represented as Z-scores (i.e., they are standardized in terms of the standard deviations from their sample mean) in order to facilitate comparisons of the effects of each right-side variable on the two different measures of cognitive skills. Three of the sets of estimates pertain to the chronological nature of the three life-cycle experiences discussed in Section 2: only preschool experience (not stunted at age 6) included (Set 1A); only preschool experience (not stunted at age 6) and school-age experience (schooling attainment) included (Set 2); and all three life-cycle experiences (not stunted at age 6, schooling attainment, and skilled job tenure) included (Set 3). Presenting these alternative specifications shows how the coefficient estimates of the earlier life experiences change as later life experiences are added to the specification, as would be expected if the latter life experiences depended on the earlier ones or were correlated with them because of their dependence on the same right-side variables[24] in the underlying reduced form relations (4), (5), and (7). The fourth set of estimates (Set 1B) includes only schooling attainment, permitting comparisons with previous literature that does not include pre- or post-schooling experiences. We present both ordinary least squares (OLS) and two-step (instrumental variable, IV) generalized method of moments (GMM) estimates (Hayashi 2000; StataCorp 2007), using all the instruments listed in Table 1 except the household wealth index and parental schooling attainment measures (F_0). 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 2007). The bottom panel presents the first-stage diagnostics assessing the strength of the instruments, including F-statistics on the excluded instruments (Bound, Jaeger, and Baker 1995) and the Cragg-Donald (CD) statistic for each model.[25] Coefficient estimates are bold if significant at the 5 percent significance level or lower.

We begin by examining the first-stage estimates and the diagnostics for the IV estimates.

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[24] In the earlier life-cycle stages, as noted, expectations for later-stage right-side variables, rather than their realizations, are included.

[25] Because the first stages are the same regardless of whether we are estimating RCS or NVS, there is only one set of first-stage diagnostics for the two outcomes.

Table 2. Estimated impacts of preschooling, 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	Not stunted at age 6	**0.270**	0.153			**0.13**	**0.51**	**0.13**	**0.48**
		(0.060)	(0.284)			(0.04)	(0.22)	(0.04)	(0.23)
E2	Schooling attainment			**0.22**	**0.11**	**0.22**	**0.13**	**0.22**	**0.11**
				(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.03)
E3a	Skilled job tenure							–0.0002	0.02
								(0.004)	(0.01)
E3b	Age at interview in 2002–2004	**0.19**	0.20	0.08	0.11	0.11	**0.23**	0.11	**0.20**
		(0.09)	(0.11)	(0.06)	(0.07)	(0.06)	(0.08)	(0.06)	(0.09)
	Age at interview squared × 100	**–0.33**	**–0.35**	–0.12	–0.20	–0.17	**–0.37**	–0.17	**–0.33**
		(0.13)	(0.17)	(0.09)	(0.10)	(0.09)	(0.13)	(0.09)	(0.13)
	Constant	–2.70	–2.88	**–2.16**	–2.01	**–2.71**	**–4.38**	**–2.71**	**–3.80**
		(1.41)	(1.92)	(0.93)	(1.07)	(0.93)	(1.45)	(0.94)	(1.56)
	F-statistic	**13.2**	**8.4**	**413.4**	**20.1**	**319.6**	**19.0**	**256.2**	**14.7**
	Hansen J test [p-value]	**18.6**	[<0.01]	13.0	[0.07]	10.4	[0.11]	8.4	[0.14]
	Hausman test [p-value]	**—**	**—**	**22.1**	[<0.01]	**25.6**	[<0.01]	**24.4**	[<0.01]
	NVS Z-scores								
Life stage	**Representation**	**OLS**	**IV**	**OLS**	**IV**	**OLS**	**IV**	**OLS**	**IV**
E1	Not stunted at age 6	**0.22**	0.366			**0.13**	**0.90**	**0.13**	**0.72**
		(0.06)	(0.263)			(0.05)	(0.28)	(0.05)	(0.29)
E2	Schooling attainment			**0.15**	**0.13**	**0.14**	**0.17**	**0.14**	0.04
				(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.04)
E3a	Skilled job tenure							0.02	**0.13**
								(0.01)	(0.02)
E3b	Age at interview in 2002–2004	**0.20**	**0.288**	0.12	0.14	**0.15**	**0.31**	0.12	0.10
		(0.09)	(0.108)	(0.08)	(0.08)	(0.08)	(0.11)	(0.08)	(0.12)
	Age at interview squared × 100	**–0.36**	**–0.49**	–0.21	**–0.25**	**–0.26**	**–0.49**	–0.22	–0.21
		(0.14)	(0.16)	(0.12)	(0.12)	(0.12)	(0.16)	(0.12)	(0.18)
	Constant	**–2.90**	**–4.394**	**–2.35**	**–2.59**	**–2.90**	**–6.15**	**–2.45**	**–1.94**
		(1.41)	(1.856)	(1.23)	(1.23)	(1.26)	(1.89)	(1.26)	(2.104)
	F-statistic	**14.8**	**12.4**	**137.2**	**21.6**	**103.3**	**16.2**	**89.7**	**18.9**
	Hansen J test [p-value]	**77.3**	[<0.01]	**79.5**	[<0.01]	**61.1**	[<0.01]	7.5	[0.19]
	Hausman test [p-value]	**—**	**—**	0.	[0.96]	8.2	[0.09]	**44.8**	[<0.01]
	First-stage diagnostics								
	Cragg-Donald		**8.6**		**13.7**		**5.3**		**5.1**
	F-stat on excluded instr.								
	Not stunted at age 6	**27.9**							
	Schooling attainment	**9.6**							
	Skilled job tenure	**24.2**							

Notes: Instrumental variables (GMM) estimation using all instruments identified in Table 1 *except* wealth and parental schooling. Standard errors calculated allowing for clustering at the mother level (592 mother clusters) shown in parentheses below coefficient estimates, which are in bold if significant at 5 percent or lower; p-values in brackets.

First-Stage Estimates

The first-stage estimates (Appendix Table B.1) have a number of significant coefficient estimates consistent with our hypotheses about how the instruments affect each life-cycle stage. Being a twin reduces the probability of not being stunted, whereas exposure to the *atole* intervention in the first three years of life (relative to *fresco*) increases it. Being male, being 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. Higher completed schooling was one 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 and higher student-teacher ratios, however, are associated with lower schooling attainment. As we saw in the unconditional means, males have significantly more tenure in skilled jobs. Higher manufacturing wages at age 18 are linked to lower tenure in skilled jobs, which is consistent with individuals being more likely to go into low-skill manufacturing.

IV Diagnostics

The first- and second-stage diagnostics are generally good for the full specifications of all three life-cycle experiences (Set 3) for both RCS and NVS. The F tests on excluded instruments range from 9.6 to 27.9, the CD statistics for weak instruments are satisfactory,[26] and the HJ tests indicate that the first-stage instruments are not correlated with the second-stage disturbance term.[27] Moreover, the Hausman tests indicate that the IV estimates differ significantly from the OLS estimates. The framework in Section 2 indicates that it is necessary to endogenize the life-cycle experiences, and we have thus identified a set of valid instruments to do so. For four of the six specifications in which one or two of the life-cycle experiences are excluded from what we claim to be the preferred specification in relation (1), the HJ tests indicate that there is a specification problem, so that the first-stage instruments are correlated with the second-stage disturbance term; in the other two, the p-value for the test is 0.11 or lower. As a result, those results need to be interpreted with caution.

Preschool Experience (E_1), Not Stunted at Age 6

The OLS associations between not being stunted at age 6 and both cognitive skills measures, conditional on a quadratic in age, are positive, significant, and fairly substantial—0.27 SD for RCS and 0.22 SD for NVS (see Table 2, first column in top two panels). The estimates become fairly imprecise and insignificant, however, if they are treated as endogenous, with only this early life-cycle experience included in the specification (second column). If schooling attainment is also included and if both it and

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[26] Using the critical values presented by Stock and Yogo (2004, Table 1, page 39), with a test statistic of 5.1 for the model specified in Set 3, we are able to reject at a 5 percent 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 30 percent of the bias in the OLS results. To the extent that our estimates are biased, however conditional on their validity, they are biased toward the OLS estimate, suggesting that the results we report below, if anything, are conservative and *understate* the differences between OLS and GMM. This is confirmed when we instead estimate using limited information maximum likelihood (LIML), as suggested by Stock and Yogo (2004). The results of that estimation (not shown) confirm the possible direction of bias—for all coefficients on the life-stage experience variables, we estimate very similar or slightly larger coefficients than in the GMM approach, although observed increases are on a much smaller order of magnitude than 0.3.

[27] 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. Moreover, 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. Because the instrument set includes the randomly allocated exposure to the intervention, which is likely to be valid, we interpret this as strong evidence of the validity of all the instruments. Further supporting this position is the finding that in some models (Section 4.2.2), the overidentification test fails, indicating that the HJ test has sufficient power to reject with these data.

not being stunted at age 6 are treated as behaviorally determined, then the estimated impact of not being stunted at age 6 is much larger—0.51 SD for RCS and 0.90 SD for NVS (sixth column). If skilled job tenure is added, the coefficient estimate for not being stunted at age 6 remains about the same for RCS, at 0.48 SD, but declines somewhat to 0.72 SD for NVS. These are our preferred estimates because they include all three life-cycle stages and are estimated using an instrument set that passes the relevance and exogeneity tests. These estimates indicate a substantial gain in terms of both RCS and NVS 20 years or more later for not being stunted at age 6, controlling for both school and post-school experiences, and for the endogenous determination of all three of these experiences. For RCS, for example, the effect of not being stunted is equivalent to having four additional grades of schooling. The alternative specifications indicate that not being stunted at age 6 partly proxies for skilled job tenure if the latter is excluded from the IV specification for NVS—resulting, in this case, in an overestimate of the impact of not being stunted at age 6. The alternative specifications also indicate that the coefficient estimate for not being stunted at age 6 markedly increases when moving from OLS to IV estimates if schooling attainment is also included in the specification. This pattern is consistent with there being 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). Absent controls for schooling attainment and for the endogenous determination of the life-cycle experiences, the impact of not being stunted at age 6 on adult cognitive skills is substantially underestimated.

School-Age Experience (E_2)—Schooling Attainment

If only schooling attainment is included using OLS, the estimated associations are 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 0.76 SD of RCS and 0.50 SD of NVS for a 1 SD increase in schooling attainment. These results are consistent with the substantial association between schooling attainment and adult cognitive skills, or outcomes that are assumed to be affected by adult cognitive skills in much of the literature. For RCS, however, this estimated effect is cut in half 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 0.11 SD in RCS for every grade of schooling, or 0.39 SD in RCS for a 1 SD increase in schooling attainment. Still, this is an important effect, although half of what would be estimated in the OLS estimates likely due to "ability bias" in the form of not controlling for unobserved endowments, which may include genetic ability endowments as well as 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, which is no longer statistically significant. These last results are consistent with the claim of some literature that the Raven's tests are independent of schooling and may reflect attributes that affect schooling (Schweizer et al. 2007).

Post-Schooling Experience (E_3)—Tenure in Skilled Job and Age

For RCS, we find no evidence of a significant impact of skilled job tenure. There is a significant impact for RCS, however, of the quadratic in age, which implies an increase in RCS per year of about 0.05 SD at around age 25, with a peak at a little over 33 years of age. Apparently there are some gains from general maturity in the early part of the adult life, although losses dominate with further aging. For NVS, we find a significant impact in the IV estimates of skilled job tenure, which implies an increase of 0.13 SD for every additional year of tenure. This is a fairly substantial effect, implying an increase of 0.63 SD in NVS for a 1 SD increase in skilled job tenure. The alternative estimates suggest that if skilled job tenure is dropped from this specification, schooling attainment proxies for it, resulting in an overestimate of the impact of schooling attainment.

In Section 3.2, we described the possibility that migration to the capital, Guatemala City, might also 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 2, treating it as endogenous. While the F-statistic for the excluded instruments on the migration indicator was 10, the CD statistic (2.5) suggests that the instruments, as a set, are weak. The HJ test fails to reject at 5 percent. For both RCS and NVS, the results for the other life-cycle stages are nearly identical to those shown in column 6. Moreover, the coefficient on migration is small and insignificant.

4.2. Robustness Considerations

Despite going beyond and, we believe, improving upon much of the current literature in how we specify and estimate the cognitive production functions presented in Section 4.1, doing so still requires a number of important assumptions. In this subsection, we relax several of these in an effort to explore the sensitivity of our principal findings.

4.2.1. Gender Differences

Adult males perform significantly better on the adult cognitive skills tests (3.5 points for RCS and 3.1 for NVS) than do adult females. This raises the question: To what extent are the differences in RCS and NVS due to differences in cognitive skills production function between females and males or to differences in life-cycle experiences between females and males?

To explore the robustness of the results in Table 2 in relation to gender differences, we consider a fully interactive model, adding interactions of a dichotomous variable for male to each variable and a male dummy variable to our basic specifications (results not presented). Because we now endogenize three additional variables in each regression, the first-stage results are necessarily 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 do not provide evidence of important gender differences in the adult skills production functions.[28]

An alternative approach is to allow the production function to shift based on gender, as we do with age. When we do this, thus taking gender out of the set of excluded first-stage instruments, we find that although the second-stage diagnostics are acceptable, the first-stage diagnostics indicate that the instruments are weak, with a CD statistic of 1.2 (information presented in Appendix Table B.2). As indicated earlier, skilled experience is predominantly a male phenomenon. The F-statistic on the excluded instruments (which no longer include the male dummy) in the first-stage skill tenure equation, even though manufacturing salaries are significant, is now only 2.1. Nevertheless, the second-stage results are similar for all endogenous variables, with the notable exception of skilled tenure, which is no longer significant in the NVS equation. Although insignificant in RCS, the male dummy variable is positive and significant in NVS (with a coefficient of 0.52), with a corresponding decrease on the coefficient on skilled tenure, which becomes small and insignificant. In the case of NVS, then, it is unclear whether the better specification is one that excludes gender in the second stage or one that includes it. In either case, the substantive findings regarding the other life-cycle stage indicators and the positive influence of skilled tenure remain the same.

Returning to the gender gaps in RCS and NVS, our basic estimates in Table 2 suggest that the significant differences by gender in the test scores are substantially due to gender differences in the three life-cycle experience indicators. Although the percentages of men versus women who were not stunted at age six and the average age are virtually identical, adult males in the sample have, on average, significantly more schooling attainment (0.9 grades) and years of tenure in skilled jobs (3.3 years). Using the estimated coefficients from the cognitive skills production functions in Table 2, the gender differences

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

in schooling attainment and post-schooling experiences account for almost all the gender differences in adult cognitive skills test performance, with the gender gap favoring males in schooling being important for RCS (accounting for 66 percent of the gap) and the gender gap in skilled job tenure being somewhat more important for NVS (87 percent of the gap).

4.2.2. Alternative Instrumental Variable Sets

Household wealth and parental schooling: Appendix Table B.3 presents estimates parallel to those in Table 2, but including the household wealth index and parental schooling attainment in the instrument set. With the exception of the coefficient of not being stunted at age six, which is substantially larger, the estimated coefficients on the endogenous variables fall between the OLS and GMM estimated coefficients reported in Table 2. However, for the outcomes considered in this paper, the HJ statistic is soundly rejected. For this reason, we exclude these characteristics from the first-stage instruments in our preferred estimates in Table 2.

Birth-year village dummies: All of the community-level-derived instrumental variables (e.g., the presence of a lower secondary school when one was seven years old) are simply functions of when and where an individual was born. As such, 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. 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 similar results for all the life-cycle experiences. There is also some evidence that completed grades is significant in the NVS relation. In general, however, the instruments are weak, with F-statistics below 6 and a CD of 1.7.

4.2.3. Alternative Representations of RCS and NVS

Due to the preliteracy screen, RCS has a mass point at zero. In Appendix Table B.4, we present four alternative IV estimates (using the same instrument set as our preferred estimates in Table 2) that address this concentration of low scores on the IARC test used for RCS: (a) Tobit estimates using RCS that account for the lower bound and mass point at zero; (b) GMM estimates using RCS + preliteracy; (c) GMM estimates using RCS-quartile; and (d) RCS-median with probit estimates for being above the median (Section 3.2). Although NVS does not have a similar mass point at zero, to explore whether our inferences based on Table 2 are robust to variants in how we treat the dependent variable, we also include estimates for NVS-quartile and NVS-median. Except for the specification using the sum of RCS and the preliteracy scores (in column 2) where not being stunted is insignificant, these alternative representations for RCS and NVS do not alter our qualitative findings or change our interpretations in Section 4.1.

4.2.4 Attrition

The estimates presented in this paper are based on a sample of 1,448 individuals, or 61 percent of the original 2,392 subjects. Despite the considerable effort and success in tracing and re-interviewing participants from the original sample, attrition is substantial. Moreover, as shown in Grajeda et al. (2005), the overall attrition in the sample is associated with a number of initial conditions, with effects differing by the reason for attrition. 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 attrition outlined in Fitzgerald, Gottschalk and Moffitt (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 = 2392). We include a number of variables that reflect family structure in previous years, as these are likely to be associated with migration status. These variables include indicators of 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 a number of variables that capture this feature of the success of data collection, including 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 2002–2004 follow-up survey, and the logarithm of the number of siblings in the sample in each family. We emphasize that this is *not* a selection correction approach in which we must justify that these factors can be excluded from the main equations; rather, we purposively exclude them from those regressions, because our purpose is to explore the determinants of adult cognitive skills in relation (1) and not to explore whether cognitive skills are associated with the family structure and interview-related factors included in the "first-stage" attrition regression (Fitzgerald, Gottschalk, and Moffitt 1998a). Although we do not formally have adjustments to correct for selection on unobservable characteristics, by including the large number of endogenous observables indicated above, which are likely to be correlated with unobservables, we expect that we are also reducing the scope for attrition bias due to unobservables.

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.5). Following Fitzgerald, Gottschalk, and Moffitt (1998a), we construct weights that give greater weight to observations in the sample re-interviewed in 2002–2004 that had lower predicted probabilities of being re-interviewed. Appendix Table B.6 shows that application of these weights only slightly affects the results and that the central patterns of the coefficient estimates remain similar to those in our preferred estimates in Table 2. We interpret these findings to mean that, as found in other contexts with high attrition, including other analyses with these data (Fitzgerald, Gottschalk, and Moffitt 1998a; Alderman et al. 2001b; Behrman, Parker, and Todd 2008; Hoddinott et al. 2008; Maluccio et al. 2008), our results do not appear to be driven by attrition biases.

4.2.5. Calculation of Standard Errors

Throughout the paper, standard errors are calculated allowing for clustering at the mother level (StataCorp 2007). For the principal findings in the final column of Table 2, we considered alternative ways of calculating the standard errors to address the implications of possible serial correlation. First, we considered clustering at the village birth-year level (yielding 64 clusters = 4 villages × 16 different birth years). Work by Angrist and Lavy (2002) and Wooldridge (2003), however, suggests that standard corrections for clustering are valid only when the number of groups or clusters is large, and we cannot be sure that N = 64 is large enough. In light of this, following Bertrand, Duflo, and Mullainathan (2004), we block bootstrapped the standard errors, using the same 64 clusters and resampling 10,000 times. All the main findings remain statistically significant (at 5 percent) across the various approaches, with the exception of the coefficient on not stunted in the RCS equation, which was significant at 9 percent in the block bootstrapping results. Calculating standard errors based on clustering at the mother level turned out to be the most conservative approach. For this reason, we report those results in the paper.

5. CONCLUSIONS

Most previous empirical investigations of the importance and determinants of adult cognitive skills in the social sciences assume that cognitive skills are produced only, or primarily, by schooling and that schooling is predetermined in a statistical sense, rather than determined by past behaviors in the presence of persistent unobservable factors, such as genetic endowments. These approaches are likely to lead to incorrect inferences, not only about the impact of schooling but also about the importance of preschooling and post-schooling experiences on what adults know.

In this paper, we use an unusually rich longitudinal data set to estimate production functions for adult reading-comprehension cognitive skills and adult nonverbal cognitive skills as being dependent on behaviorally determined preschooling, schooling, and post-schooling experiences. We present a basic specification, as well as a range of tests, of the robustness of the basic specification. Although some of the robustness tests suggest qualifications about particular coefficient estimates, they all support the following results:

1. Schooling attainment has a significant and substantial impact on adult reading-comprehension cognitive skills but not on adult nonverbal cognitive skills. Estimates of the impact of schooling on adult cognitive skills that do not account for schooling being behaviorally determined are substantially biased upward for adult reading-comprehension cognitive skills, making the impact on adult nonverbal cognitive skills appear highly positively significant rather than insignificant. In addition, if household wealth and parental schooling attainment—factors that appear to be correlated with genetic endowments—are included among the instruments (despite the rejection of their inclusion by the Hansen J overidentification test), the estimated schooling impact on adult nonverbal cognitive skills appears significant rather than insignificant. This phenomenon suggests that household wealth and parental schooling attainment characteristics are correlated with components of unobserved endowments.

2. Preschooling and post-schooling experiences have substantial and significant positive impacts on adult cognitive skills.[29] Preschooling experiences that prevent stunting substantially and significantly affect adult reading-comprehension skills and nonverbal cognitive skills, even after control for schooling. Post-schooling tenure in skilled jobs (related to on-the-job learning through problem solving, etc.) has a significant positive impact on adult nonverbal cognitive skills. Age also has a significant positive impact, with diminishing returns, on adult reading-comprehension cognitive skills, probably because of learning with experience. 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. Failing to incorporate pre- and post-schooling experiences in the analysis of adult cognitive skills—or outcomes affected by adult cognitive skills—is likely to overemphasize the role of schooling relative to these pre- and post-schooling experiences.

3. The substantial differences in adult cognitive skills favoring males, on average, appear to originate from gender differences in schooling attainment, favoring males for reading-comprehension cognitive skills, and gender differences in experience in skilled jobs, favoring males for nonverbal cognitive skills.

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

1. In both developed and developing countries, there is growing interest in investing in disadvantaged children at an early stage of life. Drawing on a wide body of evidence from economics, psychology, and neuroscience, for example, Heckman (2006) argued that returns on such investments are much higher than on those made later in life. However, the empirical base for these arguments is not as

[29] 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).

deep as would be desirable. For example, there are few studies that follow disadvantaged individuals over long periods. Our study adds to this literature by demonstrating that having relatively good nutritional status as preschoolers results in greater cognitive skills decades later as adults.

2. There is considerable debate over the impact of human capital on income levels and growth. While studies such as Mankiw, Romer, and Weil (1992) suggest an important role for schooling, other researchers find limited or negative effects (Benhabib and Spiegel 1994; Pritchett 2001). In these studies, human capital is typically represented by converting schooling enrollment rates into estimates of the stock of schooling (Nehru, Swanson, and Dubey 1995) or by school attainment for those older than 25 (Barro and Lee 1993). These approaches assume that individuals do not accumulate additional human capital after completing schooling or after a certain age; they also assume that human capital does not depreciate. Our results are at odds with these common assumptions. We find that adult cognitive skills increase with experience in higher-skilled occupations (treated endogenously). We also find that for our sample of 25-to-42-year-olds, these skills modestly increase with age. At the cross-country level, this implies that widely used representations of knowledge are flawed, likely overstating human capital in slow-growing or traditional/subsistence economies and understating it in faster-growing, modern economies. These biases are likely to result in underestimates of the importance of human capital in economic growth processes.

3. A growing body of evidence suggests that across a wide range of countries, scores on certain measures of cognitive ability,[30] including the Raven's Progressive Matrices used in our study, are increasing over time. For example, Dutch scores on Raven's Progressive Matrices increased by 1.3 standard deviations between 1952 and 1982. This phenomenon is referred to as the Flynn effect (Flynn 1987, 1994). Assuming that the tests accurately reflect ability, the Flynn effect poses a significant challenge to claims that intelligence, as measured by such tests, is largely inherited; the existence of such large changes over time suggests roles for environmental factors and behavioral decisions. Dickens and Flynn (2001) posited 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 pathways 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. These results are consistent with the environmental and behavioral factors hypothesized to underlie the Flynn effect.

4. A relatively small 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 (e.g., Boissiere, Knight, and Sabot 1985; Alderman et al. 1996). Implicit in such approaches is the assumption that causality runs from cognitive abilities to productivities. However, if more productive, higher-remunerated work is more complex and if undertaking complex work improves cognitive skills, then causality (also) runs the other way. This is exactly what we find. 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.

[30] "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 in this paper have been interpreted primarily as if they represent innate abilities; therefore, they have been referred to as measures of "ability." However, the interpretation that they are innate is contested; indeed, we explore their possible endogeneity herein. We refer to these characteristics as nonverbal skills to reflect their possible endogeneity, as opposed to their measuring innate abilities.

APPENDIX A: DATA FOR PRESCHOOL STUNTING INDICATOR

The data include 1–15 measurements of height-for-age Z-scores on each of 1,954 children from the original sample between 1969 and 1977. 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 percent of those ever measured as infants and children. Because of well-known 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 preschooling age. For children in this sample, there is a tendency in the early months of life for a sharp drop in height-for-age Z-scores, which then levels off, reaching a minimum at about 30 months of age, after which it increases slightly and approaches an asymptote just below –2.0 throughout the remainder of the preschooling period. Based on our objective of summarizing the entire preschooling experience, we use the height-for-age Z-score at age 72 months (6 years) to calculate stunting at 72 months, our indicator of preschooling experience, which is both close to the age of starting school and an age where Z-scores are relatively stable.[31] Because 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[32] other than 72 months, controlling for child-level fixed effects; we 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.[33] These estimates are carried out separately for children residing in *atole* and *fresco* villages.

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 skills test scores—for 179 individuals (12 percent) 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 1,269 individuals for whom we have an actual or predicted height-for-age Z-score at age 72 months, the mean value is –1.99 (median –1.94), which is almost at the cutoff for the definition of stunting, with a SD of 0.98. The means do not differ significantly for males versus females. To retain the 179 observations on individuals without preschooling height-for-age Z-scores 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 -1.94. As a result, we classify all those with missing height-for-age Z-scores as not being stunted.

[31] 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. 2008 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; thus, 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 preschooling period.

[32] The age categories are those used in the 1969–1977 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.

[33] The resulting estimates for the height-for-age Z-scores at age 72 months are based on actual observations for 41 percent 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 percent of the cases. The estimates for the other 32 percent of the cases are based on the younger age categories, with the 28.5–31.5-month interval accounting for 5 percent of the total, and all other categories less than 5 percent.

APPENDIX B: SUPPLEMENATARY TABLES

Table B.1. First-stage estimates of life-cycle stages (without parental education or wealth)

	***E*1**	***E*2**	***E*3a**
	Not stunted (HAZ ≥ –2.00)	**Grades of schooling**	**Skilled job tenure (years)**
***E*₀: Genetic endowment**			
Male dummy	–0.011	**0.812**	**3.425**
	(0.025)	(0.185)	(0.253)
***I*₀: Individual characteristics**			
Age at interview in 2002–2004	–0.083	1.122	0.101
	(0.087)	(0.623)	(0.810)
Age at interview in 2002–2004 squared (× 100)	0.172	–1.308	–0.591
	(1.118)	(0.769)	(0.810)
Twins dummy	**–0.598**	–0.304	–0.640
	(0.048)	(0.700)	(0.685)
ΔC: Natural, market or policy shocks			
Student-teacher ratio at age 6	0.0002	**–0.034**	–0.006
	(0.002)	(0.010)	(0.017)
Lower secondary school available at age 6	–0.052	**1.527**	**–0.599**
	(0.044)	(0.341)	(0.300)
Mother or father had died by age 18	0.00005	**–0.940**	–0.399
	(0.054)	(0.329)	(0.445)
Ln salary in manufacturing industry at age 18	0.319	**2.878**	**-3.661**
	(0.171)	(1.280)	(1.572)
ΔC: Intent to treat nutritional intervention			
00–36 months	**–0.135**	0.356	0.131
	(0.044)	(0.281)	(0.387)
00–36 months × Atole	**0.235**	–0.344	–0.765
	(0.049)	(0.359)	(0.410)
Constant	0.941	–21.589	10.634
	(1.900)	(13.918)	(17.376)
F-statistic on instruments to be excluded in second stage	**27.9**	**9.6**	**24.2**
F-statistic overall regression	**26.2**	**10.6**	**22.2**

Notes: OLS regressions. Standard errors calculated allowing for clustering at the mother level (592 mother clusters) shown in parentheses below coefficient estimates, which are in bold if significant at 5 percent or lower.

Table B.2. Estimated impacts of preschooling, 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**			
*E*1	Not stunted at age 6	**0.583**	0.456			
		(0.258)	(0.306)			
*E*2	Schooling attainment	**0.128**	–0.008			
		(0.033)	(0.044)			
*E*3a	Skilled job tenure	0.058	–0.003			
		(0.057)	(0.068)			
*E*3b	Age at interview in 2002-2004	0.161	0.229			
		(0.104)	(0.131)			
	Age squared	–0.003	**–0.004**			
		(0.002)	(0.002)			
	Male	–0.152	**0.519**			
		(0.209)	(0.256)			
	Constant	**–3.286**	–3.674			
		(1.744)	(2.193)			
	F-statistic	**12.1**	**21.3**			
Panel B	**First-stage diagnostics**			**Second-stage diagnostics**		
	Endogenous variable	**F-stat on excluded instruments**			**RCS**	**NVS**
*E*1	Not stunted at age 6	**31.8**		Hansen J test	7.6	5.5
*E*2	Schooling attainment	**6.9**		p	[0.11]	[0.24]
*E*3a	Skilled job tenure	**2.1**		Hausman test	**23.7**	**23.9**
				p	[<0.01]	[<0.01]
	Cragg-Donald weak instruments (F)	**1.2**				

Notes: Instrumental variables estimation using all instruments identified in Table 1 *except* wealth and parental schooling. Standard errors calculated allowing for clustering at the mother level (592 mother clusters) presented in parentheses below coefficient estimates, which are in bold if significant at 5 percent or lower; p-values in brackets

Table B.3. Estimated impacts of preschooling, 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**
*E*1	Not stunted at age 6	**0.802**	**0.788**	**0.317**	**0.532**
		(0.183)	(0.243)	(0.119)	(0.158)
*E*2	Schooling attainment	**0.191**	**0.103**	**0.162**	**0.073**
		(0.015)	(0.021)	(0.015)	(0.022)
*E*3a	Skilled job tenure	0.001	**0.119**	0.008	**0.106**
		(0.012)	(0.017)	(0.009)	(0.013)
*E*3b	Age at interview in 2002–2004	**0.284**	0.108	**0.148**	0.083
		(0.081)	(0.114)	(0.062)	(0.089)
	Age squared	**–0.004**	–0.002	**–0.002**	–0.002
		(0.001)	(0.002)	(0.001)	(0.001)
	Constant	**–5.840**	–2.511	**–3.094**	**–1.696**
		(1.369)	(1.908)	(1.027)	(1.501)
	F-statistic	**56.4**	**31.3**	**42.1**	**30.1**

	Set 1: First-stage diagnostics		**Set 1: Second-stage diagnostics**		
Panel B	**Endogenous variable**	**F-stat on excluded instruments**		**RCS**	**NVS**
*E*1	Not stunted at age 6	**19.8**	Hansen J test	**24.6**	**23.9**
*E*2	Schooling attainment	**10.6**	p	[0.01]	[0.01]
*E*3a	Skilled job tenure	**23.5**	Hausman test	**18.5**	**43.3**
			p	[<0.01]	[<0.01]
	Cragg-Donald weak instruments (F)	**6.5**			
	Set 2: First-stage diagnostics		**Set 2: Second-stage diagnostics**		
Panel C					
*E*1	Not stunted at age 6	**5.5**	Hansen J test	73.1	76.3
*E*2	Schooling attainment	**3.0**	p	[0.20]	[0.14]
*E*3a	Skilled job tenure	**4.7**	Hausman test	0.1	**70.0**
			p	[<0.90]	[<0.01]
	Cragg-Donald weak instruments (F)	**1.7**			

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 mother level (592 mother clusters) presented in parentheses below coefficient estimates, which are in bold if significant at 5 percent or lower; p-values in brackets.

Table B.4. Estimated impacts of preschooling, 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 (OLS)**	**Median probit**	**Quartile (OLS)**	**Median probit**
*E*1	Not stunted at age 6	**0.442**	6.150	**0.708**	**0.990**	**0.975**	0.857
		(0.247)	(6.502)	(0.279)	(0.456)	(0.366)	(0.484)
*E*2	Schooling attainment	**0.117**	**3.207**	**0.125**	**0.163**	0.036	0.011
		(0.031)	(0.785)	(0.034)	(0.058)	(0.048)	(0.061)
*E*3a	Skilled job tenure	0.022	0.490	0.021	0.033	**0.162**	**0.218**
		(0.015)	(0.368)	(0.016)	(0.028)	(0.022)	(0.029)
*E*3b	Age at interview in 2000–2004	0.196	4.109	**0.209**	0.292	0.091	0.063
		(0.103)	(2.605)	(0.101)	(0.194)	(0.143)	(0.203)
	Age squared	**–0.003**	–0.071	**–0.003**	**–0.005**	–0.002	–0.002
		(0.002)	(0.039)	(0.002)	(0.003)	(0.002)	(0.003)
	Constant	—	–10.119	–1.645	—	0.462	—
		—	(45.45)	(1.811)	—	(2.534)	—
	Chi^2 / F-statistic	**67.6**	**13.2**	**14.0**	**34.9**	**20.1**	**82.3**
Panel B	**Second-stage diagnostics**						
	Hansen J test	—	7.6	6.3	—	**9.2**	—
	Chi^2 p-value		[0.18]	[0.28]		[0.09]	
	Hausman test	**22.5**	**26.6**	**21.4**	**21.4**	**55.4**	**50.4**
	Chi^2 p-value	[<0.01]	[<0.01]	[<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 evaluated at the mean (dP/dx)) are GMM. Standard errors calculated allowing for clustering at the mother level (592 mother clusters) presented in parentheses below coefficient estimates, which are in bold if significant at 5 percent or lower; p-values in brackets.

Table B.5. 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	**–0.110**	**–0.121**
	(0.022)	(0.023)
Age at interview in 2002–2004	**–1.730**	**–1.673**
	(0.106)	(0.110)
Age at interview in 2002–2004 squared	**0.019**	**0.019**
	(0.001)	(0.001)
Twins	**–0.195**	**–0.237**
	(0.094)	(0.091)
Student-teacher ratio at age 7	0.002	0.002
	(0.001)	(0.002)
Lower secondary school available at age 7	0.043	0.047
	(0.031)	(0.030)
Mother or father had died by age 18	**0.092**	**0.163**
	(0.042)	(0.039)
Ln salary in manufacturing at age 18	**–3.950**	**–3.782**
	(0.192)	(0.196)
Exposure to intervention 0–36 months	0.001	0.013
	(0.035)	(0.038)
Exposure to intervention 0–36 months × *atole*	0.022	–0.013
	(0.038)	(0.040)
Child lived with both mother and father in 1975	—	**0.086**
		(0.032)
Child lived with both mother and father in 1987	—	**0.055**
		(0.031)
Mother alive in 2002	—	**0.253**
		(0.043)
Father alive in 2002	—	**0.119**
		(0.035)
Mother living in original village in 2002	—	**0.095**
		(0.039)
Father living in original village in 2002	—	0.047
		(0.037)
Logarithm of number of siblings in survey	—	-0.002
		(0.002)
Whether any sibling re-interviewed in 2002–2004	—	**0.269**
		(0.043)
Chi^2 statistic on variables in model 2 only	—	250.8
		[<0.01]
Model Chi^2 statistic	**483.0**	**631.0**
	[< 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 mother level shown in parentheses below coefficient estimates, which are in bold if significant at 5 percent or lower; p-values in brackets (StataCorp 2007). Derivatives evaluated at the mean (*dP*/*dx*) presented.

Table B.6. Estimated impacts of preschooling, schooling, and post-schooling experiences on adult cognitive skills: Weighting for attrition

Panel A		**RCS**	**NVS**			
Life cycle stage	**Representation**	**IV**	**IV**			
*E*1	Not stunted at age 6	0.466	**0.640**			
		(0.245)	(0.302)			
*E*2	Schooling attainment	**0.112**	0.034			
		(0.030)	(0.043)			
*E*3a	Skilled job tenure	0.026	**0.136**			
		(0.015)	(0.021)			
*E*3b	Age at interview in 2002–2004	0.175	0.044			
		(0.100)	(0.130)			
	Age squared	–0.003	–0.001			
		(0.002)	(0.002)			
	Constant	–3.427	–0.956			
		(1.781)	(2.291)			
	F-statistic	**12.6**	**17.3**			
Panel B	**First-stage diagnostics**			**Second-stage diagnostics**		
	Endogenous variable	**F stat on excluded instruments**			**RCS**	**NVS**
*E*1	Not stunted at age 6	**33.4**		Hansen J test	6.8	8.9
*E*2	Schooling attainment	**10.6**		p-value	[0.24]	[0.11]
*E*3a	Skilled job tenure	**20.8**				
	Cragg-Donald weak instruments (F)	**5.1**				

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 mother level (592 mother clusters) presented in parentheses below coefficient estimates, which are in bold if significant at 5 percent or lower; p-values in brackets.

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