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

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Abstract

Most investigations of the importance of and the determinants of adult cognitive skills assume that (a) they are produced primarily by schooling and (b) schooling is statistically predetermined. But these assumptions may lead to misleading inferences about impacts of schooling and of pre-schooling and post-schooling experiences on adult cognitive skills. This study uses an unusually rich longitudinal data set collected over 35 years in Guatemala to investigate production functions for adult (i) reading-comprehension and (ii) nonverbal cognitive skills as dependent on behaviorally-determined pre-schooling, schooling and post-schooling experiences. Major results are: (1) Schooling has significant and substantial impact on adult reading comprehension (but not on adult nonverbal cognitive skills)—but estimates of this impact are biased upwards substantially if there are no controls for behavioral determinants of schooling in the presence of persistent unobserved factors such as genetic endowments and/or if family background factors that appear to be correlated with genetic endowments are included among the first-stage instruments. (2) Both pre-schooling and post-schooling experiences have substantial significant impacts on one or both of the adult cognitive skill measures that tend to be *under*estimated if these pre- and post-schooling experiences are treated as statistically predetermined—in contrast to the upward bias for schooling, which suggests that the underlying physical and job-related components of genetic endowments are negatively correlated with those for cognitive skills. (3) The failure in most studies to incorporate pre- and post-schooling experiences in the analysis of adult cognitive skills or outcomes affected by adult cognitive skills is likely to lead to misleading over-emphasis 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, suggesting 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 favoring males. These four sets of findings are of substantial interest in themselves. But they also have important implications for broader literatures, reinforcing the importance of early life investments in disadvantaged children in determining adult skills and options, pointing to limitations in the cross-country growth literature of using schooling of adults to represent human capital, supporting hypotheses about the importance of childhood nutrition and work complexity in explaining the "Flynn effect" of substantial increases in measured cognitive skills over time, and questioning the interpretation of studies that report productivity impacts of cognitive skills without controlling for the endogeneity of such skills.

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Section 1. Introduction

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

If the centrality of human capital for economic development is accepted, understanding the processes by which dimensions of human capital, such as cognitive skills, are determined represents a critically important research question. At first blush, however, it would seem that this is well-trodden territory. There are, for example, hundreds of studies of the determinants of schooling in developing countries.[2] Schooling attainment, however, is not a direct measure of cognitive skills but a measure of grades completed. As such, it is only one potential input, though perhaps an important one, into the production of cognitive skills. Schooling typically is limited to particular periods of individuals' life, primarily childhood and adolescence. Other experiences, both before and after the school years, also may have important effects on cognitive skills. There is a substantial literature, for example, that emphasizes the importance of nutrition from conception onward for neural and cognitive development, which may affect cognitive skills into adulthood through subsequent schooling as well as in other ways.[3] There is also a substantial literature that emphasizes the importance of post-schooling experiences (especially in the labor market) in determining adult cognitive skills or in determining productivity and wages, which are interpreted as reflecting such skills.[4] If pre-schooling or post-schooling experiences have significant effects on the cognitive skills of adults *and* are

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

[2] See, for example, references in the surveys in Behrman (2007), Schultz (1987, 1988) and Strauss and Thomas (1995).

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

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

correlated with schooling—as is likely because of correlations among human capital investments across life-cycle stages—standard approaches that represent adult cognitive skills using only schooling attainment are likely to misrepresent the production process, including the impact of schooling itself.

This paper examines the importance of pre-schooling, schooling and post-schooling experiences in the determination of two representations of cognitive skills in adults: reading-comprehension and nonverbal cognitive skills, in a low-income context in which it is widely expected that greater cognitive skills will lead to poverty alleviation and economic development. In more specific terms, we investigate the following questions:

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

Major results include: (1) Schooling has significant and substantial impact on adult reading comprehension (though not on adult nonverbal cognitive skills)—but estimates of this impact are biased upwards substantially if there is not control for behavioral determinants of schooling in the presence of persistent unobserved factors such as genetic endowments and/or if family background factors that appear to be correlated with genetic endowments are included among the first-stage instruments. (2) Both pre-schooling and post-schooling experiences – represented by the nutritional status of the individual as a pre-schooler and years of skilled work experience respectively - have substantial significant impacts on one or both of the adult cognitive skill measures that tend to be *under*estimated if these pre- and post-schooling experiences are treated as statistically predetermined—in contrast to the upward bias for schooling, which suggests that the underlying physical and job-related components of genetic endowments are negatively correlated with those for cognitive skills. (3) The failure in most studies to incorporate pre- and post-schooling experiences in the analysis of adult cognitive skills or outcomes affected by adult skills is likely to lead to misleading over-emphasis 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, suggesting 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 favoring males.

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

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

Second, 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). A common aspect of these studies is that human capital is 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). An implicit assumption of these approaches is that individuals do not accumulate additional human capital after completing schooling or after a certain age, nor does their human capital depreciate. Our results are at odds with this common assumption—we find that adult cognitive skills increase with experience in high skill occupations (treated endogenously) as well as modestly with age for our sample of 25–42 year olds. At the cross-country level, this implies that widely-used representations of knowledge are flawed—they likely overstate human capital in slow growing or traditional/subsistence economies and understate it in faster growing, modern economies.

Third, a growing body of evidence suggests that, across a wide range of countries, scores on certain measures of cognitive ability[5]—including the Raven's Progressive Matrices used in our study—have been 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 (1987, 1994) effect. 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 a large role for environmental factors. Dickens and Flynn (2001) posit several pathways by which changes in environmental rather than genetic factors could cause scores on cognitive ability tests to increase

[5] "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 to which reference is made in this and the next paragraph have been interpreted primarily as if they represent innate abilities, so they have been referred to as measures of "ability." But the interpretation that they are innate is not uncontested and indeed, we explore their possibility endogeneity below – where we refer to them as non-verbal skills to reflect their possible endogeneity (as opposed to their measuring innate abilities).

over time. One of these is improved childhood nutrition. A second is increased cognitive complexity in the workplace. Our results for the impact of early childhood nutrition and for years of experience in high skill jobs—both treated as endogenous—provide direct evidence of the importance of these factors in shaping dimensions of cognitive skills that are consistent with the environmental factors hypothesized to underlie the Flynn effect.

Fourth, 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.*, Bossiere, Knight and Sabot 1985, Alderman *et al.* 1996). Implicit in such approaches is the assumption that causality runs from cognitive ability to productivity. But if more productive, higher remunerated work is more complex, and if undertaking complex work improves cognitive skills, causality (also) runs the other way. This is exactly what we find. It implies that studies that regress productivity on contemporaneous measures of cognitive ability are flawed because they fail to take the endogeneity of cognitive skills into account.

The data demands for our investigation are considerable. We utilize unusually rich longitudinal data from Guatemala that we collected over 35 years (1969–2004) with sample members 25–42 years of age during the last survey round in 2002–4 and with substantial prospective and recall information on their development through the pre-schooling, schooling and post-schooling years, as well as on exogenous factors that conditioned their 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, as well as indirectly via their effects on the different life-cycle stage experiences that might in turn affect adult cognitive skills.

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

Section 2. Conceptual Framework

We estimate production functions for two types of adult cognitive skills (measured in 2002–4 when respondents are 25–42 years of age), adult reading-comprehension skills (RCS) and adult nonverbal skills (NVS). We posit that each of these skills is produced by all previous experiences (E_i, i=1, 2, 3 for the three life-cycle stages defined below); genetic (and other) observed and unobserved endowments related to learning capacities and motivations (E_0); and a stochastic term (U) to reflect all other idiosyncratic, and assumed exogenous, learning experiences. Because of the nearly exclusive emphasis in the literature on schooling, as well as its plausibly central role in forming cognitive skills, we organize experiences into the following three life-cycle stages:[6]

[6] For simplicity we do not consider explicitly a fourth life-cycle stage after the survey.

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

Stage 2: schooling (from age seven through about age 15)[7]

Stage 3: post-schooling (from about age 15 to time of survey, *i.e.*, ages 25–42)

The production functions are:[8]

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

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

where 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 refers to the function being a production function.

The questions posed in the introduction pertain primarily to the first derivatives (*i.e.*, K_{E1}, K_{E2}, K_{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. Estimation of relation (1), however, is challenging because the experiences for the three life-cycle stages on the right side all reflect previous behavioral choices—ordinary least squares (OLS) estimates of relation (1) are likely to be inconsistent due to endogeneity of the life-cycle stages.

To motivate the assumptions underlying our modeling of three life-cycle stage experiences and to elucidate the possible impact 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) make decisions as if they were maximizing a welfare (W) function for each individual in adulthood that depends, *inter alia*, on the vector of adult cognitive skills of that individual:

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

For instance, W might represent consumption that is financed by resources generated by labor earnings (wages) 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.

Life-Cycle Stage 1 (pre-schooling): The parents allocate resources to obtain the optimal E_1 for the child, given the child endowments, nutrients and other inputs into the E_1 production function that are allocated by the parents, the current community-determined options (*e.g.*, availability of nutrition programs), expected future community characteristics (*e.g.*, schooling options in the second life-cycle stage and labor

[7] We use this age range as suggestive of the age range for schooling. In the sample we use, the legal starting age for schooling is age seven and 82% of the respondents completed schooling by age 15.

[8] An alternative production function specification that has been 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 (*e.g.*, for each grade completed). We do not use this framework, however, in part because one must place strong functional form restrictions on the production technology in order to estimate it (Todd and Wolpin 2003, 2004).

market options in the third), the expected relation between E_1 and W, and child endowments. The E_1 production function is:

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

where N_1 is a vector of family-determined inputs into the production of E_1 (*e.g.*, family-provided nutrients), C_{1p} is vector of community inputs into the production of E_1 (*e.g.*, community-provided nutrients, community-provided pre-school 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.*, random fluctuations in the infectious disease environment). Parents choose the inputs N_1 into this production function and therefore E_1 in order to maximize the expected welfare W given a vector of parental family resources such as parental schooling and assets (F_1), all relevant community characteristics for this life-cycle stage C_1 (which includes the community characteristics that directly affect the production of E_1 through C_{1p}, but also other community characteristics that affect the household through other channels), all of the child endowments E_0 (which includes E_{0p}, but also E_{0r} and E_{0n} that also affect the decision to invest in E_1 because the impact of E_1 on W in general depends on these other endowments), all the stochastic terms that affect outcomes in the first life-cycle stage of the child U_1 (which includes U_{1E} but also other stochastic factors that affect the family during the first life-cycle stage for this child since, for example, 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 the first subscript refers to the life-cycle stage at which the expectations are held, the second subscript refers to the stage for which the expectations are held and the superscript e refers to expectations) 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:

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

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

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

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

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

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

Through this process the adult cognitive skills in relation (1) are determined as well.

<u>Some implications</u>:

(1) *Endogeneity biases in adult cognitive skills production function estimates:* Direct estimates of relations (1r) and (1n) without controlling for the behavioral determinants of the three life-cycle experiences are likely to be biased because (as indicated in the reduced-form demand relations 4, 5 and 6) each of the three life-cycle experiences depends on all the endowments. These biases could be in either direction. For instance, the "ability bias" on which the schooling literature has focused is consistent with E_2 (schooling) being correlated positively with both U_{3r} and U_{3n} with the result that the coefficient of schooling is likely to be upward-biased in OLS estimates of relations (1r) and (1n). On the other hand if the summary measure of pre-school experience is some variable such as child stunting (see Section 3 below) and if ability and physical endowments are negatively correlated as suggested by Behrman and Rosenzweig (2002, 2004), then OLS estimates of relations (1r) and/or (1n) may lead to biases towards zero in the coefficient estimate for this variable. The residuals in the production function estimates for relations (1r) and (1n) (and relation 3 if data were available to estimate it) are noisy measures of the respective endowments, so they can be used to obtain lower bounds on the correlations among the endowments or estimates of the impact of the endowments on the life-cycle experiences through estimating the reduced-form demand relations (4), (5) and (6) with the residuals from relations (1r) and (1n) included among the right-side variables.

(2) *Omitted variable biases in adult cognitive skills production function estimates:* Even if the life-cycle experiences are treated as behaviorally-determined, 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 (*e.g.*, only schooling is included), omitted variable bias is likely to result. This is the case because on the right side of each of the three reduced-form demand relations for the three life-cycle stage experiences (relations 4, 5 and 6) are the endowments and the actual or expected values of the family, community and stochastic factors for all three life-cycle stages, which means that the three life-cycle experiences may be fairly correlated. Of course this 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 job options is likely not only to have more schooling but also better pre- and post-schooling experiences.

(3) *Instruments with potential predictive power to identify the three life-cycle experiences in the estimation of the adult cognitive skills production functions:* The three reduced-form demand relations for the three life-cycle stage experiences in relations (4), (5) and (6) give the potential instruments to be used to identify the three life-cycle experiences in the adult cognitive achievement production functions in relations (1r) and (1n) (Section 3 discusses our empirical representation of these vectors).[9] Note that on the right-side of each of these three reduced-form demand relations are the same endowments and the actual or expected values of the family, community and stochastic factors for all three life-cycle stages. Though there may be instruments that seem *a priori* to have first-order effects on particular life-cycle experiences (*e.g.*, pre-school programs or nutrition on E_1, school characteristics on E_2, labor market characteristics on E_3), it would not be

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

correct to assert *a priori* that a particular instrument identifies a particular life-cycle experience. Instead, there is a potential set of instruments that identifies the set of life-cycle experiences. This also means that it would not be a test of the plausibility of the instruments to see if subsequent life-cycle stage family or community variables are significant (*e.g.*, if schooling characteristics or post-schooling labor market characteristics significantly determine pre-school experience E_1) because the expected values of those variables should be included. Instead it would be a test of whether expectations are rational in the sense that the expected values for subsequent stages are equal to the realized values.

(4) *Instruments that are not correlated with the disturbance term in the adult cognitive skills production functions:* Also it should be noted that the reduced-form demand relations indicate the potential set of instruments, but not all of the right-side variables in them are likely to be good instruments in the sense of being independent of the disturbance terms in relations (1r) and (1n). In particular, previous studies suggest that there are likely to be intergenerational correlations of endowments through genetics and perhaps other means (*e.g.*, Behrman and Rosenzweig 2002, 2005). If, say, (a) ability endowments affect adult cognitive skills as is posited in relations (1r) and (1n), (b) such endowments are correlated across generations and (c) such endowments for the parents affected their schooling, income-generation and asset accumulation, then such dimensions of parental family background are not likely to be good instruments. For this reason we do not include parental schooling and assets in our preferred set of estimates in Section 4.2, though we do report what happens to the overidentification test if they are added in alternative estimates in Section 4.3.

Section 3. Data

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

Section 3.1 The Experimental Nutritional Intervention[10]

In the early and mid-1960s, protein deficiency was considered 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 for Central America and Panamá (INCAP), based in Guatemala, became the locus of a series of preliminary studies on this subject that informed the development of an ambitious nutritional supplementation trial that began there in 1969 (Habicht and Martorell 1992, Martorell, Habicht and Rivera 1995, Read and Habicht 1992).

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

The principal hypothesis underlying the intervention was that improved pre-schooling nutrition would accelerate mental development. An examination of the effects on physical growth was included to verify that the nutritional intervention had biological potency, which was demonstrated (Martorell *et al.* 1995). To test the principal hypothesis, 300 communities were screened in an initial study to identify villages of appropriate size, compactness (so as to facilitate access to feeding centers, see below), ethnicity and language, diet, access to health care facilities, demographic characteristics, nutritional status and degree of physical isolation. From this group, two sets of village pairs (one pair with about 500 residents each and the other about 900 residents each) were selected that were similar in all these characteristics.

Two villages, one from each pair (one large and one small), were randomly assigned to receive a high protein-energy drink, *Atole* as a dietary supplement. *Atole* contained *Incaparina* (a vegetable protein mixture developed by INCAP), dry skim milk and sugar, and had 163 kcal and 11.5 g of protein per 180 ml cup. This composition reflected the prevailing view at the time that protein was the critical limiting nutrient in most developing countries. *Atole*, the Guatemalan name for hot maize gruel, was served hot; it was pale gray-green and slightly gritty, but with a sweet taste.

In designing the intervention, there was considerable concern that the social stimulation associated with attending feeding centers—due to social interactions, observations of children's nutritional status, monitoring of their intakes of *Atole* and so on—also might affect child cognitive and nutritional outcomes, thus confounding efforts to isolate the impact of the supplement. To address this concern, in the two remaining villages an alternative drink, *Fresco*, was provided. *Fresco* was a cool, clear-colored, fruit-flavored drink. It contained no protein and only sufficient sugar and flavoring agents for palatability. It contained fewer calories per cup (59 kcal/180 ml) than *Atole.*[11]

The data used in this study begin with that supplementation trial in four villages in Eastern Guatemala. Three of the villages are located in mountainous areas with shallow soils, whereas the fourth is located in a river valley, with somewhat higher agricultural potential. All four villages are located relatively close to the Atlantic Highway (connecting Guatemala City to Guatemala's Caribbean coast) and are from 36km to 102km to Guatemala City. From February 1969 to February 1977, INCAP implemented a nutritional supplementation program in these four villages, together with data collection on child growth and development. The observational data collection associated with the intervention focused on all village children aged seven years or less and all pregnant and lactating women. Cohorts of newborns were included from February 1969 until September 1977 (approximately six months after supplementation ceased). Data collection for individual children ceased when they reached seven years of age or when the study ended, whichever came first. The children included in the 1969–77 longitudinal data collection were thus born

[11] In October 1971, 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 also was 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.

between 1962 and 1977. Therefore the length and timing of exposure to the intervention (described below) for particular children depended on their individual birth dates. For example, only children born after mid-1968 and before February 1974 were exposed to the intervention for all of the time they were from six to 36 months of age, posited to be a critical time period for child growth in the nutrition literature (see Section 3.2).

The nutritional supplements (*i.e.*, *Atole* or *Fresco*) were distributed in supplementation centers and were available daily, on a voluntary basis, to all members of the community during times that were convenient to mothers and children, but that did not interfere with usual meal times. For this study, where we use children's differential exposure to the availability of these nutritional supplements as first-stage instruments to estimate relations (4), (5) and (6) that are then excluded from relation (1), a critical question is to what extent the experimental intervention resulted in differences in access to calories, proteins and other nutrients. To address this, we exploit the intensive nature of the original survey and observational work associated with the intervention, in which all supplement intake was measured for each participant. Averaging over all children in the *Atole* villages (regardless of their levels of voluntary participation), children 0–12 months consumed approximately 50 kcal per day, children 12–24 months consumed 80 kcal daily and children 24–36 months consumed 110 kcal per day as supplement (Schroeder, Kaplowitz and Martorell, 1992, Figure 4). Children in the *Fresco* villages, however, consumed only 20 kcal of *Fresco* per day between the ages of 0–24 months with this figure rising to approximately 30 kcal daily by age 36 months (Schroeder, Kaplowitz and Martorell, 1992, Figure 4).

A multidisciplinary team of investigators, including most of the authors of this paper, undertook follow-up data collection in 2002–4 targeted towards all participants in the 1969–77 data collection.[12] In 2002–4, sample members ranged from 25 to 42 years of age. Figure 1 shows what happened to the 2392[13] individuals 0–15 years old in the original 1969–77 sample by the time of our 2002–4 data collection: 1855 (78%) were alive and known to be living in Guatemala (11% had died—the majority due to infectious diseases in early childhood, 7% had migrated abroad and 4% were not traceable). Of these 1855, 1113 lived in the original villages, 155 lived in nearby villages, 419 lived in or near to Guatemala City, and 168 lived elsewhere in Guatemala. For the 1855 traceable sample members living in Guatemala, 1571 (85%) completed at least one interview during the 2002–4 data collection (Grajeda *et al.* 2005). The present study includes the 1448 respondents (54% of whom are female) interviewed in 2002–4 for whom the two measures of adult cognitive skills central to this analysis (see Section 3.2) are available. They comprise 78% of the 1855 individuals who were alive and known to be living in Guatemala and 61% of the original sample (Figure 1). Measured from 1977 to 2002, the latter figure indicates an annual attrition rate of 2%, low when compared to shorter-term longitudinal surveys in developing countries (Alderman *et al.* 2001a) or to longer-

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

[13] In previous work, *e.g.*, Grajeda *et al.* (2005), this figure has been cited as 2393. Fieldwork in 2006, however, identified an individual who had been counted twice in the original study.

term longitudinal surveys in developed countries (Fitzgerald, Gottschalk and Moffitt 1998b).[14] Nevertheless, almost 40% is substantial attrition, so we assess potential attrition bias in Section 4.3.5.

[Figure 1 about here]

Section 3.2 Central Variables for the Analysis

Table 1 presents summary statistics for the 1448 individuals for whom both tests of adult cognitive skills are available.

Dependent variables: Adult cognitive skills (K):

1) Adult Reading-Comprehension Cognitive Skills (RCS): In 2002–4, the vocabulary (Level III) and reading-comprehension (Level II) modules of the Inter-American Reading and Comprehension Tests (IARC, see Manuel 1967) were administered to the 1197 individuals (83% of the sample of 1448) who passed a pre-literacy screen.[15] The vocabulary portion has 45 questions and reading comprehension has 40 questions, yielding a maximum possible score of 85 points. The distribution of the IARC test scores (for those who took the test) appears to be symmetric and approximately normal (though it fails to pass standard normality tests). The 17% of the sample who did not pass the pre-literacy screen are assigned a value of zero for the reading-comprehension tests. Including those we score at zero, the mean score 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), internal consistency and validity in previous studies in this population when the subjects were adolescents and young adults (Pollitt *et al.* 1991, 1993). Because of the nature of the distribution of the test scores, in the empirical analysis below (Section 4.3.3) we (a) explore how our results change when we use tobit rather than linear estimators and (b) consider alternative indicators of adult reading-comprehension cognition, that are now described.

1A) RCS + pre-literacy score: For this indicator we sum the raw score from the pre-literacy screen and the IARC tests under the assumption that those who were exempt from the pre-literacy test (and therefore only took the IARC test) would have earned a perfect score had they taken it. Since those who failed the pre-literacy test have scores in the 0–35 range, this measure spreads out those to whom we assigned zero for the RCS alone.

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[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 over a quarter of the attrition in the data used for this study.

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

1B) RCS-quartile: For this indicator we summarize each individual's IARC test score by recording in which quartile of the distribution it falls. All those who failed the pre-literacy test (17%) are in the first (lowest) quartile.

1C) RCS-median: For this indicator we record whether the individual scored above the median on the IARC test (treating those who failed the pre-literacy test as having scored below the median).

2) Adult Nonverbal Cognitive Skills (NVS): In 2002–4, 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), since 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 (though, again, not passing standard normality tests), with a mean of 17.7 points and a SD of 6.1 points. Males (mean 19.4, SD 6.5) average significantly higher scores than do females (mean 16.3, SD 5.4). The Raven's test also has exhibited adequate test-retest reliability (correlation of 0.87) and internal consistency 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.

[Table 1 about here]

Life-cycle stage experiences (E_1, E_2, E_3): We linearize (1) 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 there is less consensus on which representation of post-schooling experience is most valid.

Pre-schooling experience: There is substantial emphasis in the literature on the importance of nutrition—reflecting nutrient intakes and exposure to infections—as measured by height in early childhood development. Therefore we use pre-schooling child height-for-age z scores (that give height in terms of standard deviations from the median of widely-used WHO/NCHS/CDC standards[17]), a widely accepted indicator of childhood nutrition, to calculate our indicator for pre-schooling experience. Because the nutrition literature places particularly emphasis on whether or not a child is stunted, *i.e.*, with height-for-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 (see below). Indeed, when we add squared and interaction terms to our preferred specification in Table 2, none of the individual coefficients of these terms appears significant and joint tests reject the significance of them as a group. This means it is not possible for us to assess empirically possible interactions of the life-cycle stages.

[17] The World Health Organization (WHO) recently has released new standards. We do not use them because they only cover children up to five years of age. For children under five years of age in our sample, the standards that we use and the new WHO standards yield height-for-age z scores that are highly correlated.

z score < -2.0, we focus on stunting in our analysis. In particular, we include a dummy variable for children who are *not* stunted, reflecting better nutrition.[18]

The data include from one to 15 measurements of height-for-age z scores on each of 1954 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 (8.5–9.5)—951 children or 49% 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 (based in part on earlier studies of this population, *e.g.*, Martorell 1997), we use the available information to obtain an estimate of the z score for height-for-age for individuals in the sample at a common pre-schooling age. As reported in earlier studies, for children in this sample, in the early months of life there is a tendency for a sharp drop in z scores for height-for-age that then levels off and reaches a minimum at about 30 months of age, after which it increases slightly and approaches an asymptote just below -2.0 throughout the remainder of the pre-schooling period. Based on our objective of summarizing the entire pre-schooling experience, we use the height-for-age z score at age 72 months (± six months) to calculate stunting at 72 months, our indicator of pre-schooling experience, which is both close to the age of starting school and an age where z scores are relatively stable.[19] Since this measure is not available for the entire sample, when it is missing we estimate it using measurements of the z score for the child at ages other than 72 months. We first estimate the z score-age relation with dummy variables for age categories[20] other than 72 months, controlling for child-level fixed effects and then use the estimates of the age category dummy variables to adjust the nearest observed measurement of each child (for whom we do not have an observation at age 72 months) by the average difference between the measurement at the observed age and at 72 months.[21] These estimates are carried out separately for children residing in *Atole* and *Fresco* villages.

[18] This is equivalent, of course, to including a dummy variable for being stunted (with the opposite sign). But we prefer this representation so that for this life-cycle experience, as for the others, a positive coefficient estimate implies that a better life-cycle experience implies greater adult cognitive skills.

[19] 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.* 2006 and the references therein). We note that the correlation between the measured height-for-age z score at 36 months and our indicator of height-for-age z score at 72 months is 0.97, so the use of 36 rather than 72 months would 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 whole pre-schooling period.

[20] The age categories are those used in the 1969–77 data collection, 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.

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

Even though the data permit the estimation of height-for-age z scores at age 72 months for 1954 individuals in the sample as compared with 1448 for whom we have both adult cognitive skills test scores, for 179 individuals (12%) for whom we have the test scores we do not have information with which to estimate the height-for-age z score at age 72 months. For the 1269 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), 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 pre-schooling height-for-age when estimating the impact of the experiences during the schooling and post-schooling periods, we replace the missing height-for-age z score with the sample median of -1.94. As a result, we classify all those with missing HAZ as not being stunted. The results reported below, in particular the magnitude and significance of the effect of not being stunted, are unchanged if instead we classify those with missing HAZ as being stunted.

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

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 (tenure) prior to the 2002–4 survey in skilled occupations[22] as our first indicator of post-schooling experience that contributes to adult cognitive skills. The data do not permit the calculation of total experience in

[22] 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 2002–4. We have explored various definitions for the skills measure and find the results are largely similar, though with some of the other measures there is difficulty with the diagnostic tests, in particular the overidentification test. 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).

all jobs since individuals left school, but rather tenure in all the jobs held in 1998 and in 2002–4.[23] Such experience is likely to develop cognitive skills via learning-by-doing through problem solving, furthering skills learned in school through using them in real world applications and exposure to a wider environment, through interactions with coworkers and customers. The mean number of years of tenure in a skilled job is only 2.8, but with a SD of 4.8. Two-thirds of the individuals for whom there are test scores (including the zero values for those who did not pass the literacy screen) are not in skilled occupations in 1998 or 2002–4 and therefore 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 a SD of 5.0. There are substantial differences for tenure in skilled occupations by gender, with 388 out of 673 males (58%) having such experience (mean 8.1 years, SD = 5.0), but only 142 out of 775 females (18%) (mean 6.9 years, SD = 5.0). The difference in mean years across males and females is also significant.

- *Migration to Guatemala City:* Living in Guatemala City, rather than in the original sample villages or elsewhere in Guatemala, might alter experiences through work, shopping, entertainment and in other ways that might affect adult cognitive skills. About 17% of the sample lived in Guatemala City at the time of the 2002–4 survey, 18% of the females and 15% of the males. The mean of the estimated number of years that these individuals have been away from their origin village is 3.9 years, with a SD of 6.6 and 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, with a SD of 4.2 and 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-school.

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

Initial conditions (F_0, E_0):

Wealth and parental schooling attainment in childhood (F_0): We explore the role of parental characteristics in the production of cognitive skills; these parental characteristics include mother's and father's schooling attainment and a constructed wealth index. As part of the survey work accompanying the intervention in 1969–77, all households in these villages—including those with children participating in the supplementation trial—participated in censuses in 1967–68 and 1975 that ascertained ownership of a set of household durables as well as housing characteristics. Using principal components, these assets and characteristics

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

were combined into an index we interpret as a "wealth" index. For those born before January 1st 1971 we use the 1967 index score and for those born after that date we use the 1975 index score.[24] Though this index surely misses some dimensions of wealth such as financial and productive assets, at the time in the late 1960s and early 1970s in these villages, such assets were 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 less than two completed grades.[25]

Genetic and other endowments (E_0): We do not have direct observations on genetic endowments beyond the gender of the individual, which we control for in our first-stage estimates. Even after including this measure, however, we are not able to identify confidently the extent to which gender reflects genetic differences or cultural and market-determined gender differences (see Section 4.3.1). Apart from gender, other individual characteristics that we observe include age at the time of the 2002–4 interview and whether the individual was a twin (1.0% of the sample),[26] 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.

Observed events and shocks (C):

Natural, market or policy events: We include community level variables that relate as closely as possible to the timing of key learning- 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 we had 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 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 salary in manufacturing when the individual was 18 years old and likely to be in their early working years. Thus, while reflecting community level characteristics, all of these variables vary by single-year age cohorts within each village, as well as across villages. Since these measures closely relate to the availability and longevity of schools and markets to the period in an individual's life when critical decisions (*e.g.*, starting school, entering the labor force) were 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 individuals' father or mother had died

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

[25] For the small percentages without parental schooling or the wealth index, we replace with sample medians.

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

(prematurely and possibly accidentally) by the time they were 18, which relates to a critical juncture for entering the labor force.[27]

Experimental nutritional shocks: Lastly, the set of observed shocks that we consider relate to the nutritional interventions underlying the original study (see Section 3.1). We construct two intent-to-treat measures, based on the village and date of birth of each individual and the dates of operation of the interventions. For each individual, we calculate whether s/he was exposed to either intervention for the entire period from 0–36 months of age. We interpret these as a set of cohort controls. The potential exposure to the *Atole* intervention is then calculated by multiplying this cohort measure by a dummy indicator of whether or not the child lived in one of the two *Atole* villages.

Identification strategy:

With the central variables defined, we now provide a more detailed discussion of our identification strategy, which is aimed at overcoming the various problems outlined in Section 2. There are three components to our strategy. The first is that we select, on a priori grounds related to the framework in Section 2, plausibly exogenous characteristics from individuals' background and community 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. Much of the related literature also uses parental characteristics; our prior hypothesis is that these may be less valid instruments but we also include them in some specifications and test that hypothesis. The second component of our identification strategy relates to the comprehensiveness of the life-cycle stage indicators. On conceptual grounds we cannot claim that the instruments we employ have absolutely no direct effects on the cognitive skills outcomes; we can, however, say that by incorporating all three of these life stages, as opposed to say only one of them, the possibility for direct effects above and beyond the factors in the model is substantially limited. It would require, for example, that the presence of a lower secondary school at age seven would have to have a direct effect that was outside of its effect on pre-schooling nutrition (through expectations), schooling attainment *and* post-schooling skilled work tenure (as well as migration, in some specifications). Our view is that the pathways for the three life-cycle experiences that we include capture well the possible mechanisms by which the instruments we employ could have effects. The final component of our identification strategy is to trust, but verify. We carry out a range of diagnostic tests to assess the strength and validity of the instruments, as well as consider alternative instrument sets and estimation procedures that are consistent in the presence of weak instruments, a potential concern. On the whole, we find that that the instruments we utilize have reasonable power and, with the exception of those

[27] Clearly some of these shocks also relate to the individuals' parental families, particularly whether individuals' parents were alive when individuals were at age 18, but also the 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 (Section 4.3.2), however, inclusion of the parental death shocks in the instrument set does not result in a rejection of the Hansen J overidentification test.

specifications that include parental characteristics (F_0), we fail to reject the null hypotheses for overidentification tests.

4. Results

We first summarize in Section 4.1 estimates that show the association between schooling attainment and our two measures of adult cognitive skills, RCS and NVS, to verify that these associations are strong, as has been usually assumed, and in some cases demonstrated, in the previous literature. We then turn to our main estimates in Section 4.2, using indicators of experiences during each of the three life-cycle stages, and endogenizing them in a generalized method-of-moments (GMM) framework, as well as considering other estimation approaches. Lastly, in Section 4.3, we consider how robust our estimates are to gender differences, variations in the instrumental variables used, alternative representations of RCS and NVS, alternative approaches to estimating the standard errors and controls for attrition. Given the nature of the sample, a number of these individuals are siblings or half-siblings. Throughout we control for mother cluster effects in the estimation of the standard errors (with 592 clusters for the 1448 observations), with the exception of Section 4.3.4 where we explore alternative approaches to calculating the standard errors.

4.1 Basic Associations between Schooling Attainment and Adult Cognitive Skills

Most previous literature treats schooling attainment as if it represents adult cognitive skills. Therefore, we first explore to what extent schooling attainment is associated with cognitive skills in this sample. The first column in Table 2 presents OLS estimates for RCS (Panel A) and NVS (Panel B) on schooling attainment. The associations are strong, though far from perfect—consistent with 59% of the variance in RCS and 26% of the variance in NVS. At the mean, each additional grade of schooling attainment is associated with an additional 5.0 points (22% of a SD) on the RCS test and 0.9 points (15% of a SD) on the NVS test.

These associations, however, do not represent the causal effect of schooling attainment on adult cognitive skills for at least two reasons. First, if relation (1) is the true specification, then these relations are mis-specified because they exclude pre- and post-schooling experiences. As described in Section 3.2 above, schooling attainment is significantly correlated with our indicators for pre- and post-schooling experiences, so ignoring those experiences is likely to change the estimated coefficient of schooling attainment due to omitted variable bias. We consider this possibility further when we turn to estimates of the full specification of relation (1) in Section 4.2. The second reason they do not capture the causal effect is that the OLS estimates treat schooling as if it is determined independently of the genetic and other endowments in relation (1). If instead, schooling is endogenized to account for its being determined in part by past behaviors using the instruments in Table 1 (excluding the household wealth index and parental and schooling attainment), the estimates change substantially (column 2 in Table 2). For RCS the estimated impact of schooling attainment drops by a third (to 3.2, 15% of a SD) and is much less precisely estimated, though still highly significant. This suggests that the component of the vector E_0 in relation (1) that directly affects RCS is positively correlated with schooling attainment, as is generally hypothesized for innate learning ability (Behrman and

Rosenzweig 1999). For NVS, however, the estimated impact of schooling attainment increases, by about 20% (to 1.1, 18% of a SD), though again estimated with much less precision than in the OLS. Hausman tests (not shown) indicate that for RCS the estimated coefficient on schooling attainment is significantly different across the specifications, but not for NVS. These results are tentative, however, because they are based on a potentially mis-specified model in which pre- and post-schooling experiences are constrained to have no effect.

4.2 Basic Specification of the Adult Cognitive Skills Production Function

Table 2 also presents two sets of estimates of the linearized version of the adult cognitive skills production function in relation (1) for RCS and NVS: OLS estimates (column 3) and two-step feasible (instrumental variable) GMM estimates (column 4) (Hayashi 2000, StataCorp 2005) including all the instruments listed in Table 1 except for the household wealth index and parental schooling attainment (F_0). Panel C gives first- and second-stage diagnostics for the estimates presented in column 4: the Shea partial R^2 (Shea 1997), F statistics on excluded instruments and the Cragg-Donald (CD) statistic for the first stage, the Hansen J (HJ) statistics for overidentification and the Hausman test comparing columns 3 and 4 (Hayashi 2000; StataCorp 2005).

[Table 2 about here]

The first-stage estimates (Appendix Table A1) are significant and plausible, and most estimated coefficients are consistent with our hypotheses about how the instruments affect each of the life-cycle stages, even though statistically this is not necessary. 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. Men, individuals who lived in a village with a lower secondary school when they were starting primary school and 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.[28] It is possible that more schooling is one of the criteria used in hiring in manufacturing and other industries. The loss of a parent before age 18 and higher student-teacher ratios, on the other hand, are associated with lower schooling attainment. As we saw in the raw means, males have significantly more tenure in skilled jobs, but women are slightly more likely to have migrated to Guatemala City (possibly reflecting marriage patterns or female specialization in domestic and *maquiladora* employment). Higher manufacturing wages at age 18 are linked to lower tenure in skilled jobs (as individuals were more likely to go into low-skill manufacturing), but also increases in migration to Guatemala City, where most manufacturing takes place.

RCS (Inter-American Reading and Comprehension Tests): Our preferred estimates are the GMM estimates (column 4) in which post-schooling experience is represented by skilled job tenure and a quadratic in age

[28] Because of the high correlations it is not possible to separate out the influence of these salaries at different ages.

(which is not treated as endogenous). Below we describe similar results obtained when we also include the indicator for migration to Guatemala City. Panel C presents the diagnostic tests for these estimates.

F tests indicate that the first-stage estimates have strong predictive power, with F statistics on the excluded instruments 9.6 or higher (Bound, Jaeger and Baker 1995, Staiger and Stock 1997). While these results are encouraging, they are less informative in the case of multiple endogenous variables, so as a further check on the power of our instruments we report the Cragg-Donald (CD) statistic. To assess the significance of this test statistic, we reference the literature on testing for weak instruments. Stock and Yogo (2004) offer guidance in the case of multiple endogenous regressors that uses the CD statistic. In particular, they calculate critical values for a test of weak instruments, using the CD statistic for different variations of 1) the number of endogenous regressors 2) the number of excluded instruments 3) the desired significance level and 4) the extent of bias relative to OLS. We have three endogenous variables and eight excluded instruments. By their Table 1 (Stock and Yoto 2004, page 39), then, with a test statistic of 5.1 we reject the hypothesis that the instruments are weak at a 5% significance level for a bias of between 0.20 and 0.30. To the extent that our estimates are biased (perhaps even by 0.3), however, they are biased toward the OLS estimate, suggesting that the results we report below are, if anything conservative and *understate* the differences between OLS and GMM. This is confirmed when we instead estimate using limited information maximum likelihood, as suggested by Stock and Yogo (2004). This approach is consistent in the presence of weak instruments. The results of that estimation (not shown) confirm the possible direction of bias and for the stunting and years of skilled labor we estimate slightly larger coefficients than in the GMM approach, though on a much smaller order of magnitude than 0.3.

The Hansen J (HJ) statistic for overidentification does not reject the null hypothesis that the instruments are independent of the second-stage disturbance term at usual significance levels, and the Hausman test indicates that the GMM 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 identified a set of valid instruments to do so.

The estimates indicate a significant positive increase in test scores of 2.5 points (11% of a SD) for an *additional grade of schooling attainment*. The failure to treat schooling as behaviorally determined results in a nearly 100% upwards ability (and other endowments) bias for the estimated impact of schooling on RCS. Comparison with the estimates in column 2 in Table 2 also suggests that the impact of schooling is overestimated by about 25% if the pre- and post-schooling experiences are constrained to have no effects and schooling is treated as behaviorally determined, and again by about 100% if schooling alone is included and treated as exogenously determined (as in the OLS estimates in column 1). It appears that in the simplest OLS specification, schooling is in considerable part proxying for ("signaling for") unobserved endowments and pre- and post-schooling experiences, on top of its own significant causal impact.

The results also indicate a significantly positive impact of *pre-schooling experience* as measured by not being stunted at 72 months, on RCS—better pre-schooling nutrition leads to higher RCS scores and the estimated effect is large, nearly 11 points on the test, a full 50% of a SD. (The 95% confidence interval, however, ranges from 0.8 to 20.8.) Consistent with the OLS results, however, the GMM estimates also do not indicate a significantly positive estimated impact of *post-schooling experiences* on RCS. Age, on the other hand, is positively associated with RCS over the range of ages in the sample, though at a slightly diminishing rate.

NVS (Raven's Progressive Matrices): Our preferred estimates are again the GMM estimates shown in Column 4 (of Panel B). As with RCS, the NVS results have satisfactory diagnostic statistics (Panel C). The first-stage diagnostics are the same as above. The Hansen J statistic for overidentification does not reject the null hypothesis that the instruments are independent of the second-stage disturbance term at usual significance levels and the Hausman test indicates that the overall relation differs from the OLS estimates.

The results (column 4, Panel B) indicate a positive increase in test scores of about 0.26 points (or about 4% of a SD) for an *additional grade of schooling attainment*. This is one-third the magnitude of the OLS estimate, much less precisely estimated and not significantly different from zero. The insignificance of schooling in the instrumental variables estimates is consistent with the effort to design these tests to be independent of formal education, but nevertheless they measure abilities that affect schooling so there is a strong association in the OLS estimates, possibly due to this omitted variable bias. Comparison of our preferred specification (column 4) with the parallel estimates without pre- and post-schooling experiences (column 2) suggests that excluding these other experiences results in a considerable overestimate (by a factor of over four) of the direct impact of schooling attainment on NVS scores. Thus in the simplest OLS specification for NVS (column 1), schooling is proxying for ("signaling for") both unobserved endowments and pre- and post-schooling experiences – and not representing any significant causal impact of schooling per se.

In contrast to no significant effect for schooling attainment, we find a significantly positive impact of the indicator of *pre-schooling experience*, not being stunted at 72 months, of 4.4 points (nearly 75% of a SD). (The 95% confidence interval ranges from 0.9 to 7.9.) Comparing this estimated effect with the OLS estimate suggests that the impact of pre-schooling experience is substantially underestimated (the OLS estimate is less than 20% as large) when there are not adequate controls for the behavioral determinants. This is consistent with the possibility of negatively correlated ability and anthropometric endowments, as found for the United States in Behrman and Rosenzweig (2004).

Our preferred estimates also indicate a positive significant estimated impact of *post-schooling experience* as represented by skilled work tenure on NVS, indicating a 0.8 point increase (13% of a SD) for an additional year of skilled work tenure. This point estimate is seven times larger than that obtained with OLS estimates—so the effect of controlling for endowments on the estimated impact of skilled work tenure on NVS is similar in direction, but even larger, than that for pre-schooling experience. As with RCS, we find a positive (though slightly diminishing) significant effect of *age* on NVS.

Though our estimates indicate that NVS is not affected by schooling, they do indicate that NVS is affected by both pre- and post-schooling experiences. Therefore studies that assume that NVS represents purely innate abilities that are unaffected by pre- and post-schooling experiences (*e.g.*, Alderman *et al.*1996, Boissiere, Knight and Sabot 1985) are likely to be mis-specified and difficult to interpret.

Response of life-cycle experiences to endowments (E_0): As noted in Section 2, there may be estimation biases if the life-cycle experiences in relations (4), (5) and (6) respond to endowments such as innate abilities and innate health, and those endowments also directly affect the adult cognitive skills in relation (1). The comparisons of our GMM and OLS estimates for the adult cognitive skills production functions in Panels A and B of Table 2 suggest that there are substantial biases in both directions in the OLS estimates that ignore the possible importance of such endowments. As also noted in Section 2, E_0 can be a vector with multiple components, and different components may be relatively more important for different outcomes. The residuals in the relations in Panels A and B of Table 2 can be interpreted as estimates (albeit noisy because they also include the term U) of the endowments relevant for RCS and NVS.[29]

Our conceptual framework in Section 2 posits that the life-cycle experiences (E_1, E_2, E_3) are in part determined by the initial endowments. Panel D in Table 2 explores this possibility and presents the estimated impacts of these residuals on the indicators of the three life-cycle experiences when they are included in the first-stage regressions for relation (1), in addition to the observed predetermined variables used as instruments for our preferred estimates (Tables A1). Thus each row in Panel D represents a separate regression for that life-cycle experience measure, where we report only the coefficients on the estimated endowments. The results show that with one exception both of the residual estimates of endowments are significant determinants of all three life-cycle indicators. This is consistent with the general finding of estimation biases when we use OLS and ignore the role of endowments in relation (1). These estimates also indicate different roles for the two residual estimates of endowments. The residual estimate of RCS endowments has a significant negative effect on our pre-schooling indicator but positive effects on the other life-cycle stage indicators controlling for the residual estimate of NVS endowments and for the other predetermined variables. The residual estimate of NVS endowments, in some contrast, has significant negative effects on both pre-schooling and post-schooling life-cycle experiences and an insignificant positive one only on years of schooling, again controlling for the residual estimate of RCS endowments and for the other predetermined variables. These patterns are consistent with the results discussed above in which, if there is no control for endowments, the impacts of pre-schooling experiences are underestimated, the impacts of schooling are overestimated, and the impact of post-schooling experience is understated for NVS.

When, in addition to years of skilled tenure we include an indicator for migration to Guatemala City (treated as endogenous) the results for the coefficients reported in Table 2 for both RCS and NVS are little

[29] A similar approach has been used in a number of previous studies (*e.g.*, Pitt, Rosenzweig and Hassan 1990, Rosenzweig and Schultz 1983, 1987).

changed. The indicator for migration is well predicted in the first stage (Table A1). The point estimates for migration to Guatemala City are both small and insignificant at conventional levels (p = 0.78 for the RCS equation, p = 0.91 for NVS). While it may be that this is too crude an indicator of the possible urban experiences that would influence cognition, we conclude that there does not seem to be much of evidence of such influence.

4.3 Some Robustness Explorations

4.3.1 Gender Differences

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

To explore how robust the results in Table 2 are to gender differences, we have added interactions of a dichotomous variable for male with each of the variables and with the constant (*i.e.*, the latter is the "level" effect or 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 that there are important gender differences in the adult skills production functions.

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 the following. First, the first stage results are similar for all the endogenous variables with the exception of skilled tenure, which is no longer significant in the NVS equation. As indicated earlier, skilled experience is predominantly a male phenomenon. The Shea partial R^2 declines to 0.01 and the F statistic on the excluded instruments in the skill tenure equation, even though the manufacturing salaries are significant, is 2.1. Second, although insignificant in RCS, the male dummy variable is positive and significant in NVS (with a coefficient of 3.2), with a corresponding decrease on the coefficient on skilled tenure, which becomes small an insignificant. Last, the coefficients on the other life-cycle stages remain essentially unchanged. In the case of NVS, then, it is unclear whether the better specification is one which excludes gender in the second stage or includes it. In either case, however, the substantive findings regarding the other life-cycle stage indicators and the positive influence of skilled tenure remain the same.

Returning then 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 due substantially to gender differences in the three life-cycle experience indicators. While the percentages of men versus women who were not stunted as children and the average age are virtually identical, adult males in the sample have on average significantly more schooling attainment (0.9 years) 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 in schooling attainment and post-schooling experiences account for almost all of the gender differences in adult cognitive skills test performance, with the gender gap favoring males in schooling important for the RCS (accounting for 66% of the gap) and the gender gap in skilled job tenure somewhat more important for NVS (87% of the gap).

4.3.2 Alternative instrumental variable sets

Household wealth and parental schooling: On *a priori* grounds and on the bases of previous studies we question whether household wealth and parental schooling attainment can serve as legitimate instruments (Section 3.2). Table 3 presents estimates parallel to those in Table 2, but adding the household wealth index and parental schooling attainment to the instrument set. With the exception of the coefficient on not stunted, estimated coefficients on the endogenous variables fall between the OLS and GMM estimated coefficients reported in Table 2. The increased coefficient estimates on not stunted and schooling attainment (relative to the GMM estimates in Table 2) are consistent with the possibility that the wealth index and parental schooling attainment represent in part unobserved (genetic) endowments, and thus are not satisfactory instruments. Moreover, the Hansen J statistic suggests clear rejection of the null hypothesis that the instruments are independent of the second-stage disturbance term for both models (with $p = 0.01$ in each case). We therefore exclude the wealth index and parental schooling attainment from the first-stage instruments in our preferred estimates (in Table 2).

[Table 3 about here]

Birth-year village dummies: All of the community level derived instrumental variables are simply functions of when and where an individual was born, for example the exposure to *Atole* intervention variables and the presence of a lower secondary school when you were seven years old. 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 (and the individual level male and twins dummies), we find similar results for all of the life-cycle experiences, except that there is 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. Given that, when we consider LIML estimation, which is consistent in the presence of weak instruments, the coefficient on schooling in the NVS relationship drops by 75%, casting doubt that it is in fact influential.

4.3.3 Alternative Representations of RCS and NVS

Due to the literacy screen, RCS has a mass point at zero. In Table 4, we present four alternative IV estimates (all using the same instrument set as in our preferred estimates in Table 2) that address this concentration of low scores on the IARC test used for RCS: (1) Tobit estimates using RCS that account for the lower bound and mass point at zero; (2) GMM estimates using RCS + preliteracy; (3) GMM estimates using RCS-quartile; and (4) RCS-median with probit estimates for being above the median (see Section 3.2). Though 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 4.2.

[Table 4 about here]

4.3.4 Calculation of Standard Errors

The original intervention occurred in four villages. Duration of exposure to the intervention was dependent on date and village of birth. Standard errors for the results presented above are calculated using the "robust" methods proposed by Huber (1967) and White (1980) that correct for heteroscedasticity of unknown form and allowing for within-family clustering for children with the same mother (StataCorp 2005). However, in case there is further clustering ("design effects") of the sort that Kish (1965), Moulton (1990), Deaton (1997) and others have emphasized, we have estimated our preferred relations in Table 2 with, in addition to the Huber-White correction for heteroscedasticity, the incorporation of the design effect into the construction of the regression standard errors (Deaton 1997). We now consider two alternative ways to calculate the standard errors.

Based on the design of the intervention we construct 64 clusters (4 villages × 16 different birth years). Allowing for clustering at this level in the calculation of the standard errors (estimates not presented) changes only slightly the magnitudes of the t statistics. It leaves the basic findings unchanged with one exception: skilled tenure appears to be significant for RCS. Wooldridge (2003), however, suggests that standard corrections for clustering are valid only when the number of groups or clusters is large.[30] In light of this, following Bertrand, Duflo and Mullainathan (2004), we also block bootstrap the standard errors. Specifically, we construct a bootstrap sample by drawing with replacement 64 matrices consisting of outcomes and their regressors, from each birth-year village cohort. We run the regressions on this sample, obtain the standard error, and then replicate this exercise 10,000 times. When we do this, we again obtain similar results to Section 4.2. Moreover, skilled tenure is no longer significant in RCS as with the clustering approach mentioned above (p=0.11).

4.3.5 Attrition

The estimates presented in this paper are based on a sample of 1448 individuals, 61% of the original 2392 subjects. Despite the considerable effort and success in tracing and re-interviewing participants from the original sample, an attrition of 39% is substantial and may raise concern about the validity of the estimates reported above. 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, and to what extent, 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?

[30] This is the reason we do not cluster at the village level with number of clusters equal to four.

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, since these are likely to be associated with migration status. They 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. They include whether the parents were alive in 2002, whether they lived in the original village, whether a sibling of the sample member had been interviewed in the 2002–4 follow-up survey and the 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, but rather we purposively exclude them from those regressions since our purpose is to explore the determinants of adult cognitive skills in equation (1) and not 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).

The factors described above are highly significant in predicting attrition, above and beyond the conditioning variables already included in the models (Appendix Table A2). Table 5 shows that they alter only slightly the results that do not correct for attrition, and the central patterns of the coefficient estimates remain similar to those in out preferred estimates in Table 2. The only difference is that there is some evidence now that skilled job tenure may have a significant effect on RCS, at least at the 10% level. We interpret these findings to mean that, as found in other contexts with high attrition (Fitzgerald, Gottschalk and Moffitt 1998b, Alderman *et al.* 2001a, Behrman, Parker and Todd 2005, Maluccio *et al.* 2006) our results do not appear to be driven by attrition biases.

[Table 5 about here]

Section 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. But these approaches may lead to incorrect inferences not only about the impact of schooling, but also about the importance of pre-schooling and post-schooling experiences on what adults know.

We use an unusually rich longitudinal data set that we have collected in Guatemala to investigate production functions for adult reading-comprehension cognitive skills and adult nonverbal cognitive skills as dependent on behaviorally-determined pre-schooling, schooling and post-schooling experiences. We present a basic specification as well as a range of tests of how robust is that specification. While some of the robustness tests suggest qualifications about particular coefficient estimates, all in all they support the following major results of our investigation:

(1) Schooling attainment has a significant and substantial impact on adult reading-comprehension cognitive skills but probably not a significant impact on adult nonverbal cognitive skills. However, estimates of the impact of schooling on adult cognitive skills that do not account for schooling being determined by previous behaviors are biased upwards substantially for adult reading-comprehension cognitive skills and make the impact on adult nonverbal cognitive skills appear highly significant rather than insignificant. Also, 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 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) Both pre-schooling and post-schooling experiences have substantial positive significant impacts on adult cognitive skills. Pre-schooling experiences (related to nutrition, health, exposure to infectious disease, water quality and paternal care and stimulation) that avoid stunting affect substantially and significantly adult reading-comprehension and non-verbal cognitive skills, even with control for schooling. Post-schooling tenure in skilled jobs (related to on-the-job learning through problem-solving, etc) has significant positive impact on adult nonverbal cognitive skills. Age also has significant positive impact with diminishing returns on adult reading-comprehension cognitive skills, probably because of learning with experience. The treatment of pre- and post-schooling experiences as statistically predetermined tends to lead to substantial underestimates of their impacts (with the exception of the age impact on adult non-verbal cognitive skills) in contrast to the upward bias for schooling, which suggests that the underlying physical and job-related components of genetic endowments are negatively correlated with those for cognitive skills. The failure in most studies to incorporate pre- and post-schooling experiences in the analysis of adult cognitive skills or outcomes affected by adult cognitive skills is likely to lead to misleading over-emphasis on the role of schooling relative to these pre- and post-schooling experiences in efforts to improve adult cognitive skills and outcomes affected by adult cognitive skills.

(3) Gender differences in the coefficients of the three life-cycle stage experiences in the adult cognitive skills production functions are not significantly different from zero. Therefore most of the fairly substantial differences in adult cognitive skills favoring males on average originates 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.

As discussed in the introduction, these results not only have important implications for understanding the micro implications of human capital investments for adult cognitive skills, but also have broader implications for four wider literatures. *First*, the results provide empirical support for the increased recent emphasis on the importance of investments in early childhood on individuals' adult skills and options. *Second*, the results suggest that the use of schooling attainment in cross-country growth regressions is likely to be a poor indicator of country-specific human capital that tends to overstate relatively human capital in slow growing and traditional societies relative to fast growing and modern societies, a pattern that is likely to result in downward biased estimates of the impact of human capital on growth. *Third*, the results provide support for hypotheses underlying the proposed 'Flynn effect' that argues that improved childhood nutrition and increased work complexity tend to enhance cognitive skills. *Fourth*, the results raise questions about studies that assume that cognitive skills ("abilities") are predetermined and enhance productivity rather than treating cognitive skills as endogenously determined and depending on various life-cycle experiences.

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Figure 1. Relation between original 1969–77 sample and 2002–4 sample (updated from Grajeda *et al.* 2005, Figure 1)

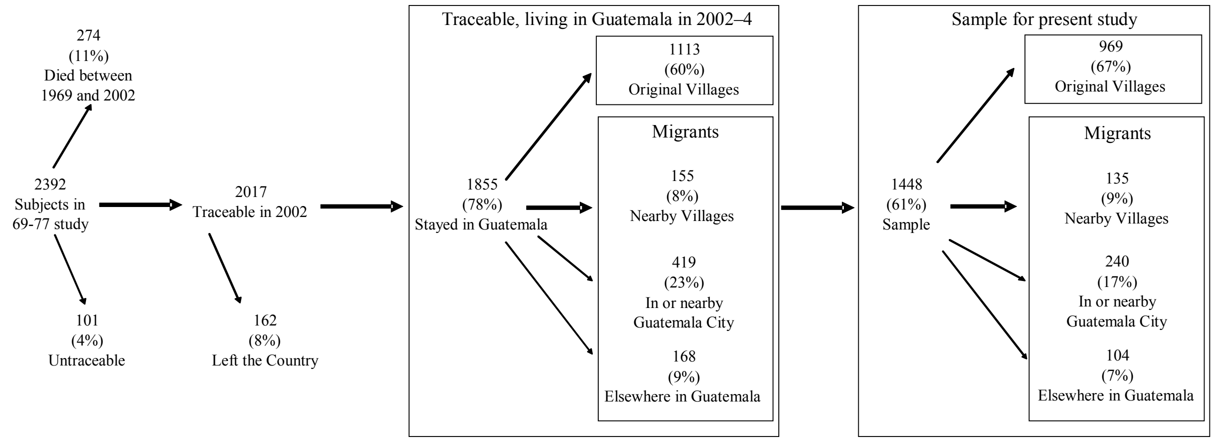

Table 1–Summary Statistics: Means and standard deviations (N = 1448)

	Mean	Standard Deviation
K: 1. Reading-Comprehension Score (RCS)	36.05	22.31
RCS + pre-literacy scores (RCS + prelit)	66.93	29.67
2. Nonverbal Scores (NVS)	17.70	6.14
E_1: Not Stunted at age 7: HAZ ≥ -2.00 (1269 non-missing cases)	0.53	0.50
HAZ (1269 non-missing cases)	-1.99	0.98
HAZ missing dummy (1 if missing)	0.12	0.33
E_2: Schooling attainment	4.69	3.45
E_3: a. Skilled job tenure (years)	2.85	4.83
b. Living in Guatemala City in 2002–4 dummy	0.17	0.37
C1. Age at interview in 2002–4	32.33	4.24
C2. Age at interview in 2002–4 squared	1063.2	277.8
Instruments		
F_0: Household and parental characteristics		
Household wealth index (1330 non-missing cases)	-3.10	0.92
Household wealth index missing dummy	0.08	0.27
Mother's schooling attainment (1427 non-missing cases)	1.36	1.69
Mother's schooling missing dummy	0.01	0.12
Father's schooling attainment (1344 non-missing cases)	1.71	2.13
Father's schooling missing dummy	0.07	0.26
E_0: Genetic endowment		
Male dummy	0.46	0.50
Individual characteristics		
Age at interview in 2002–4	32.33	4.24
Age at interview in 2002–4 squared	1063.2	277.8
Twins dummy (850 non-missing cases)	0.02	0.13
Twins information missing dummy	0.41	0.49
C: Natural, market or policy shocks		
Student-teacher ratio at age 7	39.93	9.00
Lower secondary school available at age 7	0.21	0.41
Mother or father had died by child age 18 dummy	0.08	0.28
Ln salary in manufacturing industry at age 18	1.60	0.51
Intent to treat nutritional intervention dummies		
00–36 months	0.39	0.49
00–36 months × *Atole*	0.21	0.41

Table 2–Estimated Impacts of Pre-Schooling, Schooling and Post-Schooling Experiences on Adult Cognitive Skills (N=1448)

Panel A		Reading-comprehension test score (RCS)				
Life stage	Representation	OLS	GMM	OLS	GMM	
E1	Not Stunted at age 7			**2.821**	**10.792**	
				3.27	*2.12*	
E2	Schooling attainment	**4.959**	**3.235**	**4.899**	**2.545**	
		35.0	*7.06*	*33.8*	*4.00*	
E3a	Skilled job tenure			-0.005	0.382	
				-0.06	*1.29*	
E3b	Age at interview in 2002–4			**2.350**	**4.463**	
				1.81	*2.27*	
	Age at interview squared			**-0.038**	**-0.074**	
				-1.89	*2.51*	
	Constant	**12.79**	**21.00**	-21.75	-38.01	
		16.04	*9.26*	*-1.04*	*-1.21*	
	F Statistic	**1224.7**	**49.72**	**256.2**	**14.67**	
Panel B		Nonverbal cognitive ability test score (NVS)				
Life stage	Representation	OLS	GMM	OLS	GMM	
E1	Not stunted at age 7			**0.771**	**4.419**	
				2.61	*2.47*	
E2	Schooling attainment	**0.910**	**1.099**	**0.858**	0.257	
		19.27	*7.27*	*17.63*	*0.96*	
E3a	Skilled job tenure			**0.114**	**0.803**	
				3.59	*6.96*	
E3b	Age at interview in 2002-4			**0.759**	0.613	
				1.60	*0.83*	
	Age squared			**-0.013**	-0.013	
				-1.86	*-1.19*	
	Constant	**13.43**	**12.35**	3.42	10.22	
		54.46	*17.40*	*0.45*	*0.86*	
	F Statistic	**372.3**	**52.7**	**89.7**	**18.9**	
Panel C	1st stage diagnostics			2nd stage diagnostics		
Life stage	Endogenous variable	Shea partial R^2	F stat on excluded instruments		RCS	NVS
E1	Not Stunted at age 7	0.038	**27.92**	Hansen J test	8.353	7.512
E2	Schooling attainment	0.050	**9.62**	*p-value*	[0.13]	[0.19]
E3a	Skilled job tenure	0.115	**24.16**	Hausman test	**25.25**	**45.19**
	Cragg-Donald weak instruments (F)		**5.1**	*p-value*	[<0.01]	[<0.01]
Panel D	Estimated impact of second-stage residuals on life-cycle jobs					
Life stage	Endogenous variable	RCS residual	NVS residual			
E1	Not stunted at age 7	**-0.003**	**-0.016**			
		-3.31	*-7.41*			
E2	Schooling attainment	**0.089**	0.016			
		17.57	*1.06*			
E3a	Skilled job tenure	**0.054**	**-0.398**			
		7.18	*-17.78*			

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); t or z statistics in italics below coefficient estimates which are in bold are significant at 10% or lower; p-values in brackets.

Table 3. Estimated Impacts of Pre-Schooling, Schooling and Post-Schooling Experiences on Adult Cognitive Skills with Wealth index and Parental Schooling Attainments among First-Stage Instruments

Panel A		RCS	NVS
Life cycle stage	Representation	GMM	GMM
E1	Not stunted at age 7	**17.916**	**4.835**
		4.38	*3.24*
E2	Schooling attainment	**4.275**	**0.630**
		12.78	*4.90*
E3a	Skilled job tenure	0.028	**0.732**
		0.10	*6.83*
E3b	Age at interview in 2002-4	**6.336**	0.660
		3.51	*0.95*
	Age squared	**-0.099**	-0.013
		-3.60	*-1.23*
	Constant	**-94.438**	2.291
		-3.09	*0.20*
	F Statistic	**56.4**	**31.3**

Panel B	1st stage diagnostics			2nd stage diagnostics		
	Endogenous variable	Shea partial R^2	F stat on excluded instruments		RCS	NVS
E1	Not stunted at age 7	0.063	**15.63**	Hansen J test	**24.57**	**23.91**
E2	Schooling attainment	0.204	**21.46**	p	[0.01]	[0.01]
E3a	Skilled job tenure	0.126	**15.52**	Hausman test	**18.46**	**43.26**
				p	[<0.01]	[<0.01]
	Cragg-Donald weak instruments (F)		**6.8**			

Instrumental variables (GMM) estimation using all instruments identified in Table 1. Standard errors calculated allowing for clustering at the mother level (592 mother clusters); z statistics in italics below coefficient estimates which are in bold are significant at 10% or lower; p-values in brackets.

Table 4–Estimated Impacts of Pre-Schooling, Schooling and Post-Schooling Experiences on Adult Cognitive Skills: Alternative Instrumental Variable Specifications (N=1448)

Panel A		RCS				NVS	
Life-cycle stage	Representation	Tobit	RCS+ prelit	Quartile	Median probit	Quartile	Median probit
E1	Not stunted at age 7	**9.872**	6.150	**0.708**	**0.990**	**0.975**	**0.857**
		1.79	*0.95*	*2.54*	*2.17*	*2.67*	*1.77*
E2	Schooling attainment	**2.614**	**3.207**	**0.125**	**0.163**	0.036	0.011
		3.80	*4.09*	*3.65*	*2.82*	*0.76*	*0.18*
E3a	Skilled job tenure	0.489	0.490	0.021	0.033	**0.162**	**0.218**
		1.50	*1.33*	*1.31*	*1.20*	*7.42*	*7.46*
E3b	Age at interview in 2000–4	**4.380**	4.109	**0.209**	0.292	0.091	0.063
		1.91	*1.58*	*2.07*	*1.50*	*0.64*	*0.31*
	Age squared	**-0.074**	**-0.071**	**-0.003**	-0.005	-0.002	-0.002
		-2.16	*-1.81*	*-2.30*	*-1.68*	*-0.96*	*-0.64*
	Constant	-47.695	-10.119	-1.645	-5.675	0.462	-1.288
		-1.18	*-0.22*	*-0.91*	*-1.66*	*0.18*	*-0.36*
	Chi^2 / F Statistic	**67.6**	**13.2**	**14.0**	**34.9**	**20.1**	**82.3**

Panel B	2nd 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]

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); z statistics in italics below coefficient estimates which are in bold are significant at 10% or lower; p-values in brackets.

Table 5: Estimated Impacts of Pre-Schooling, Schooling and Post-Schooling Experiences on Adult Cognitive Skills: Weighting for Attrition

Panel A		RCS	NVS
Life cycle stage	Representation	GMM	GMM
E1	Not stunted at age 7	**10.156**	**4.091**
		1.82	*2.17*
E2	Schooling attainment	**2.487**	0.273
		3.65	*1.01*
E3a	Skilled job tenure	**0.581**	**0.817**
		1.73	*6.32*
E3b	Age at interview in 2002–4	3.463	0.463
		1.54	0.58
	Age squared	**-0.058**	-0.011
		-1.72	*-0.91*
	Constant	-33.136	8.405
		-0.83	*0.59*
	F Statistic	**12.1**	**17.6**

Panel B	1st stage diagnostics			2nd stage diagnostics		
	Endogenous variable	Shea partial R^2	F stat on excluded instruments		RCS	NVS
E1	Not stunted at age 7	0.036	**33.4**	Hansen J test	6.8	8.9
E2	Schooling attainment	0.051	**10.6**	p-value	[0.24]	[0.11]
E3a	Skilled job tenure	0.101	**20.8**			
	Cragg-Donald weak instruments (F)		**5.1**			

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); z statistics in italics below coefficient estimates which are in bold are significant at 10% or lower; p-values in brackets.

Table A1: First Stage Estimates of Life-Cycle Stages

	E1 Not Stunted (HAZ ≥ -2.00)	E2 Grades of schooling	E3a Skilled job tenure (years)	E3b Migrant to Guatemala City
E_0: Genetic endowment				
Male dummy	-0.011	**0.811**	**3.425**	**-0.041**
	-0.43	*4.40*	*13.56*	*-2.16*
I_0: Individual characteristics				
Age at interview in 2002–4	-0.083	**1.122**	0.101	**0.483**
	-0.95	*1.80*	*0.13*	*6.07*
Age at interview in 2002–4 squared (× 100)	0.172	**-1.308**	-0.590	**-0.517**
	1.54	*-1.70*	*-0.56*	*-5.16*
Twins dummy	**-0.589**	-0.304	-0.640	-0.041
	-13.02	*-0.44*	*-0.93*	*-0.46*
ΔC: Natural, market or policy shocks				
Student-teacher ratio at age 7	0.001	**-0.034**	-0.006	-0.001
	0.10	*-3.27*	*-0.38*	*-0.54*
Lower secondary school available at age 7	-0.052	**1.527**	**-0.599**	**0.102**
	-1.18	*4.48*	*-2.00*	*2.97*
Mother or Father had died by age 18	0.000	**-0.940**	-0.399	-0.020
	0.00	*-2.86*	*-0.90*	*-0.48*
Ln salary in manufacturing industry at age 18	**0.319**	**2.878**	**-3.661**	**1.182**
	1.86	*2.25*	*-2.33*	*7.50*
ΔC: Intent to treat nutritional intervention				
00–36 months	**-0.135**	0.356	0.131	**0.056**
	-3.09	*1.27*	*0.34*	*1.71*
00–36 months × *Atole*	**0.235**	-0.344	**-0.765**	0.017
	4.77	*-0.96*	*-1.86*	*0.46*
Constant	0.941	-21.589	10.634	**-11.835**
	0.50	*-1.55*	*0.61*	*-6.77*
F Statistic on instruments to be excluded in 2nd stage	**27.92**	**9.62**	**24.16**	**9.82**
p-value of F test	[<0.01]	[<0.01]	[<0.01]	[<0.01]
F Statistic overall regression	**26.17**	**10.57**	**22.21**	**9.29**
p-value of F test	[<0.01]	[<0.01]	[<0.01]	[<0.01]

OLS regressions. Standard errors calculated allowing for clustering at the mother level (592 mother clusters); t-statistics in italics below coefficient estimates which are in bold are significant at 10% or lower; p-values in brackets.

Table A2–Attrition probits to construct weights used in Table 5 (N=2392)

	Model 1	**Model 2**
Covariates	(1) if in sample	(1) if in sample
Male	**-0.110**	**-0.121**
	-5.04	*-5.34*
Age at interview in 2002–4	**-1.730**	**-1.673**
	-15.97	*-14.65*
Age at interview in 2002–4 squared	**0.019**	**0.019**
	13.98	*12.91*
Twins	**-0.195**	**-0.237**
	-2.12	*-2.59*
Student-teacher ratio at age 7	0.002	0.002
	1.05	*1.01*
Lower secondary school available at age 7	0.043	0.047
	1.37	*1.53*
Mother or father had died by age 18	**0.092**	**0.163**
	2.06	*3.67*
Ln Salary in manufacturing at age 18	**-3.950**	**-3.782**
	-20.52	*-18.65*
Exposure to intervention		
From birth to 36 months	0.001	0.013
	0.28	*0.34*
From birth to 36 months × *Atole*	0.022	-0.013
	0.59	*-0.34*
Child lived with both mother and father in 1975	-	**0.086**
		2.72
Child lived with both mother and father in 1987	-	**0.055**
		1.80
Mother alive in 2002	-	**0.253**
		5.89
Father alive in 2002	-	**0.119**
		3.45
Mother living in original village in 2002	-	**0.095**
		2.34
Father living in original village in 2002	-	0.047
		1.27
Logarithm of number of siblings in survey	-	-0.002
		-1.11
Whether any sibling re-interviewed in 2002–4	-	**0.269**
		6.27
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 2392 individuals who were exposed to the INCAP supplementation intervention between 1969 and 1977. Standard errors are calculated allowing for clustering at the mother level (StataCorp 2005). Derivatives evaluated at the mean (*dP/dx*) presented with the corresponding z-statistics in parentheses and indicated in bold if significant at 10%. *P*-values are in brackets.