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

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Abstract
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Keywords
Adult cognitive skills, Behavioral determinants, Genetic endowments, Flynn effect, Guatemala

Disciplines
Demography, Population, and Ecology | Family, Life Course, and Society | Social and Behavioral Sciences | Sociology

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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) non-verbal 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 non-verbal 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 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 completed grades of schooling 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, 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.

Section 1. Introduction

It is widely believed that adult cognitive skills affect substantially productivity in many activities, ranging from economic work, to raising children, to improving one's own and others' health and nutrition. Vast empirical literatures from various disciplines, largely 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]

Schooling attainment, however, is not a direct measure of adult cognitive skills but a measure of grades completed. As such, it is only one input, though perhaps an important one, into the production of adult cognitive skills. Schooling typically is limited to particular periods of an individual's life, primarily childhood and adolescence. Other experiences, both before and after the school years, also may have important effects on adult cognitive skills. There is a substantial literature, for example, that emphasizes the importance of pre-schooling nutrition, from conception onward, for neural and cognitive development, which may affect adult cognitive skills through subsequent schooling as well as through other paths.[2] There also is a substantial literature that emphasizes the importance of post-schooling experiences, particularly in the labor market, in determining adult cognitive skills or what are interpreted to be the effects of such skills on productivity and wages.[3] If pre-schooling or post-schooling experiences have significant impact on adult cognitive skills and are correlated with schooling—as is likely because of correlations across life-cycle stages in human capital investments—standard approaches that represent adult cognitive skills using only

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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). There are only a few exceptions that instead use direct measures of adult cognitive skills (e.g., Alderman *et al.* 1986, Boissiere, Knight, Sabot 1985, Glewwe 1996, Glewwe *et al.* 1996, Murnane, Willet and Levy 1995). Likewise, the many empirical studies of the impacts of cognitive and other skills on outcomes such as health, nutrition and human fertility almost all use schooling attainment to represent these skills.

[2] See, for example, Alderman and Behrman (2005), Alderman, Hoddinott and Kinsey (2005), Alderman et *al.* (2001a), Behrman and Rosenzweig (2004), Bleichrodt and Born (1994), 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.* (2005a), Martorell (1997), Martorell *et al.* (1995, 1998), Pollitt (1990), Pollitt *et al.* (1993), Ramakrishnan et *al.* (1999), Richards *et al.* (2001) and Wachs (1995).

[3] 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 prices indices (e.g., Rosen 1974), for instance, generally include some measure of post-schooling work experience.

schooling attainment are likely to misrepresent even the impact of schooling itself.[4] Moreover, the experiences during the school years may interact with experiences of the pre-schooling and the post-schooling years in the determination of adult cognitive skills.

This paper investigates the importance of pre-schooling, schooling and post-schooling experiences in the determination of adult cognitive skills—both reading-comprehension and non-verbal 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:

1) How important is schooling attainment in determining adult cognitive skills?
2) How important are pre-schooling and post-schooling experiences?
3) Does the incorporation of pre-schooling and post-schooling experiences change the apparent importance of schooling?
4) Does controlling for the possibility that schooling, as well as pre- and post-schooling experiences are behavioral choices change the apparent importance of schooling?
5) Are there important differences between the determinants of adult reading-comprehension and those of non-verbal cognitive skills? Might schooling, for example, be more important for the former and post-schooling work experience more important for the latter?
6) Is there evidence that unobserved cognitive skills endowments (e.g., genetic abilities, infectious or nutritional factors in utero) affect significantly, and perhaps differentially, the pre-schooling, schooling and post-schooling experiences?
7) Are there significant gender differences in the determinants of adult cognitive skills?

While answers to these questions are of considerable interest in their own right, they also speak to three wider issues.

[4] The potential biases may be in either direction. The impact of schooling may be overestimated if, for example, families that invest more in pre-schooling human capital also invest more in schooling and both pre-schooling human capital and schooling have direct positive effects on adult cognitive skills. On the other hand, the impact may be under estimated if the durations of schooling and post-schooling learning experiences are negatively correlated because the longer is schooling the fewer are the post-schooling years for an adult of a given age (e.g., in the extreme case in which Mincerian (Mincer 1974) "potential work experience"—as defined by adult age minus age of starting school minus grades of school—is used to represent post-schooling experience as in many studies) but only schooling is included in the estimates.

First, there is considerable debate over the impact of human capital formation on income levels and growth. While studies such as Mankiw, Romer and Weil (1992) suggests an important role for schooling, a number of 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 school attainment for those older than 25 (Barro and Lee 1993). A critical feature of these approaches is the assumption that individuals neither accumulate further human capital after a certain age, nor does their knowledge depreciate. Our results are at variance with this common implicit assumption—we find that adult cognitive skills increase with (endogenous) experience in high skill occupations and depreciates with age. At the cross-country level, this implies that widely-used representations of knowledge are flawed - they overstate human capital in slow growing or traditional/subsistence economies and understate it in faster growing, modern economies.

Second, a growing body of evidence suggests that, across a wide range of countries, scores on certain measures of cognitive ability—including Raven's Progressive Matrices, a test we use 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. The Flynn effect poses a significant challenge to claims that intelligence 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, not genetic, factors could cause scores on cognitive ability tests to increase over time. One of these is improved nutrition in childhood. A second is increases in cognitive complexity in the workplace. Our results on the impact of early childhood nutrition and on years of experience in high skill jobs—both treated as endogenous—provide direct evidence on the importance of these factors in shaping dimensions of cognitive ability and, as such, provide direct evidence that is consistent with the posited environmental factors that underlie the Flynn effect.

Third, a relatively small literature attempts to use direct measures of ability (such as Ravens Progressive Matrices) to examine whether human capital is associated with greater productivity as opposed to merely being a signaling device. Bossiere, Knight and Sabot (1985) provide a good example of this approach. Implicit in these estimates 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 runs the other way. This is exactly what we find. It implies that studies that regress contemporaneous measures of cognitive ability on measures of productivity are flawed because they fail to take the endogeneity of cognitive skills into account.

The data demands for our investigation are considerable. We utilize an unusually rich data set: longitudinal data that we have 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, and on exogenous factors that conditioned their experiences during these three life-cycle stages. We thus are able to investigate the impact of experiences during these three life-cycle stages on adult cognitive skills, while incorporating the possibility that these experiences reflect behavioral choices over the life cycle in the presence of factors such as genetic endowments that might affect adult cognitive skills directly, as well as the experiences in different life-cycle stages that affect adult cognitive skills through which genetic endowments may have indirect effects.

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

Section 2. Conceptual Framework

We measure the cognitive skills of adults in 2002-4 when they are 25-42 years of age. We posit cognitive skills production functions that incorporate all previous experiences, as well as genetic (and other) unobserved endowments related to learning capacities and motivations. 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:

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

Stage 2: schooling (from age seven through about age 15)[5]

Stage 3: post-schooling (from about age 15 to time of survey)

[5] In the sample we use, 82% of the respondents have completed schooling by age 15.

Adult cognitive skills (K), therefore, depend on the experiences during these three stages (E_i where i = 1, 2, 3), genetic and other endowments (E_0) such as for abilities and motivation that may have direct effects on adult cognitive skills as well as indirect effects through the various experiences (see relation 2 below), and a stochastic term (U) to reflect all other idiosyncratic, and assumed exogenous, learning experiences: [6,7]

(1) $K = K(E_1, E_2, E_3, E_0, U)$.

The questions posed in the introduction pertain primarily to the first derivatives (i.e., K_{E1}, K_{E2}, K_{E3}) of the 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.

We assume that individuals and their parental families make investments in each of the three life-cycle stages within a dynamic, reduced-form demand context, given initial conditions, including parental socioeconomic status and schooling attainment (F_0), initial community prices and policies (C_0), genetic and other endowments (E_0), and individual characteristics (I_0) such as gender and year of birth, and changes over time in markets, policies and other conditions (ΔC), as well as unobserved idiosyncratic influences assumed to be exogenous (V):

(2) $E_i = E^i(F_0, C_0, E_0, I_0, \Delta C_i, V_i)$,

where i =1, 2, 3 for each of the life-cycle stages and Δ refers to observed changes or shocks from the initial conditions to the *i*th life-cycle stage.

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[6] Adult cognitive skills also may be related to earlier cognitive skills that may have affected each of the life-cycle stage experiences on the right side of the production function. For example, pre-schooling nutrition is likely to have affected cognitive skills at the time of entering school, which is likely to have affected schooling attainment, and so on through to occupational choice and adult cognitive skills. The general production function specified in relation (1) is consistent with such possibilities, even though it does not trace out explicitly all the links in this chain.

[7] 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 specification, 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). Moreover, it does not allow exploration of the interactive effects of the different stages.

This expression incorporates the results of many decisions that parents, and then increasingly the individuals themselves, make, given initial conditions and time-varying factors outside of the control of the family. The right side components in relation (2) can be viewed as vectors of opportunities and constraints to which the family responds. One example is the endowment vector (E_0), which might include innate "reading-comprehension ability" and "non-verbal ability" endowments related to learning, as well as "physical" endowments related to growth. These various endowments may be significantly correlated but they need not be positively correlated. A recent study for the United States, for example, finds that the endowments related to schooling and earnings on one hand and those related to physical health on the other, are negatively correlated (Behrman and Rosenzweig 2004). If that were the case in this study, the failure to control for genetic endowments could result in an overestimate of the effect of schooling on adult cognitive skills, but an underestimate of the effect of an indicator of physical growth (such as height-for-age) on those same skills (indeed, a pattern that we find in Section 4.2).

Relation (2) illustrates three important aspects of the inherent problems in estimating relation (1).

First, all three life-cycle experiences are determined in part by genetic and other endowments (E_0) that also may have direct effects on adult cognitive skills in relation (1). All three life-cycle experiences, but particularly schooling and post-schooling experiences, may be determined in part by earlier cognitive skills and, since cognitive skills are correlated over the life cycle, OLS estimates of relation (1) may be affected by reverse causality. These concerns mean that to obtain consistent estimates of the impacts of the life-cycle stage experiences on adult cognitive skills, some combination of data and estimation method must be used to avoid the bias that otherwise would result from correlations among the experiences in the three life-cycle stages and the expanded compound error term in relation (1) that includes unobserved genetic and other endowments (E_0) in addition to the idiosyncratic error (U). In principle, this problem can be addressed by random assignment of life-cycle experiences, by direct control for genetic and other endowments (E_0) or by instrumental variable (IV) estimates in which each E_i ($i = 1, 2, 3$) in relation (1) is replaced by its predicted value from relation (2), which are not correlated with the unobserved E_0 and therefore not correlated with the compound disturbance in relation (1). In the present (and almost all) data sets, only the third of these is an option.[8]

Second, relation (2) allows for the possibility that the start and end of schooling are not fixed, but depend on behavioral choices, which implies that the durations of all three stages reflect these choices. Treating the experiences for each life-cycle stage as behaviorally determined, as is discussed in the first point, deals with the fact that their durations reflect behavioral choices.

Third, all three of the life-stages experiences are determined in part by the same initial conditions (F_0, C_0, E_0, I_0) and some common observed community changes ΔC and unobserved influences V_i (common in the sense that the factors that affect the *i*th stage experience also may affect subsequent life-cycle stage experiences). This means that the experiences from the three life-cycle stages are likely to be correlated, with the result that estimates that do not control for all three of these life-cycle stages—as in most of the literature that only includes schooling or a smaller set of the literature that includes only schooling and post-school or schooling and pre-schooling experience—are likely to be subject to omitted variable bias because of the failure to control for the other life-cycle stage experiences. We investigate how important this problem is by comparing alternative specifications that have all three life-cycle stages with ones in which the pre- and post-schooling experiences are restricted *a priori* to have zero effects.

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 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 data and then focus in on the variables used in the analyses.

Section 3.1 General Description of the Data[9]

[8] Within-sibling estimates could be used to control for the average family genetic endowments, but previous studies suggest that the individual-specific deviations from those family averages have important impact on human capital investments (Behrman, Rosenzweig and Taubman 1994, 1996).

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

In the early and mid-1960s, protein deficiency was considered the single 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 Panama (INCAP), based in Guatemala, became the locus of a series of preliminary studies on this subject in the latter half of the 1960s, which 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).

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. 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 were selected (one pair with about 500 residents each and the other, about 900 residents each) that were similar in all these characteristics.

Two villages, from each pair (one large, Conacaste, and one small, San Juan), 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 critically 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—such as the observation of children's nutritional status, the monitoring of their intakes of *Atole* and so on—also might affect child cognitive and nutritional outcomes, thus confounding efforts to understand the impact of the supplement. To address this concern, in the two remaining villages, Santo Domingo and Espíritu Santo, 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*. In October 1971, several micronutrients were added to both the *Atole* and *Fresco*, in amounts that yielded equal concentrations per unit of volume.

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 (intent-to-treat) exposure to these nutritional supplements as first-stage instruments to estimate relation (2) 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 60-100 kcal daily and children 24-36 months consumed 100-120 kcal per day as supplement (Schroeder, Kaplowitz and Martorell, 1992, Figure 4). Children in the *Fresco* villages, however, consumed only 20kcal 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 program of primary medical care was provided free of charge throughout the period of data collection. Periodic preventive health services, such as immunization and antiparasite campaigns, were conducted in all villages.

The data used in this study begin with that supplementation trial in four villages in Eastern Guatemala. Three of the villages—San Juan, Conacaste and Santo Domingo—are located in mountainous areas with shallow soils, whereas the fourth, Espíritu Santo, 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 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).

A multidisciplinary team of investigators, including the authors of this paper, undertook follow-up data collection in 2002-4 on all participants in the 1969-77 data collection.[10] In 2002-4, sample members ranged from 25 to 42 years of age. Figure 1 shows what happened to the 2393 individuals 0-15 years old in the original 1969-77 sample by the time of our 2002-4 data collection: 1856 (77%) were alive and known to be living in Guatemala (11% had died, 8% had migrated abroad, 4% were not traceable). Of these 1856, 1113 lived in the original villages, 154 lived in nearby villages, 419 lived in or near to Guatemala City, and 170 lived elsewhere in Guatemala. For the 1856 traceable sample members living in Guatemala, 1054 (58%) finished the complete battery of applicable interviews and measurements and 1570 (85%) completed at least one interview during the 2002-4 data collection. For two-thirds of the 286 (15%) who completed no interviews, while we learned they were alive and living in Guatemala, we were unable to obtain current addresses and therefore could not make contact. The refusal rates for at least partial participation among those whom we were able to contact were low, around 5% (Grajeda *et al.* 2005). The present study includes the 1447 respondents (54% 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 1856 individuals who were alive and known to be living in Guatemala and 60% of the original sample (Figure 1); we test for possible attrition bias in Section 4.3.5.

[Figure 1 about here]

Section 3.2 Central Variables for the Analysis

[10] 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).

Table 1 presents summary statistics for the 1447 individuals for whom both tests of adult cognitive skills are available.

[Table 1 about here]

<u>Dependent variables: Adult cognitive skills (K)</u>:

<u>1) Adult Reading-Comprehension Cognitive Skills (RCS)</u>: In 2002-4, the vocabulary and reading comprehension modules of the Inter-American Reading and Comprehension Tests (IARC, see Manuel 1967) were administered to the 1196 individuals (83% of the sample of 1447) who passed a pre-literacy screen.[11] 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 38.0, SD 22.7) average significantly higher scores than do females (mean 34.4, SD 21.9) with $t = 3.1$ ($p < 0.01$). 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.* 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) <u>RCS + pre-literacy screen</u>: 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. Rather than a mass point at zero, those who failed the pre-literacy test have scores in the 0-35 range, with a secondary mode for the overall distribution in the 13-19 point range.

[11] Subjects who reported 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. 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 Non-verbal Cognitive Skills (NVS): In 2002-4, all respondents were given Raven's Progressive Matrices (Raven, Court and Raven 1984), a widely-used non-verbal measure of interpretative cognitive skills that consists of a set of shapes and patterns, with the respondent asked to supply a 'missing piece.' 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) with $t = 9.9$ ($p < 0.01$). 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.

Life-cycle stage experiences (E_1, E_2, E_3): Based on indicators used in the previous literature, we approximate (1) with a (linear) first-order Taylor series expansion and include one indicator each for the first two life-cycle stages under the assumption that each is a sufficient statistic for that experience.[12] For the third life-cycle stage, we explore the use of multiple indicators because *a priori* arguments and initial estimates suggest that more than one might be important for the representation of post-schooling experience.

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

[12] If we were to approximate the function in relation (1) with one indicator each for the three life-cycle stages in a second-order Taylor series expansion to allow diminishing marginal returns and interactions and include a constant, we would be estimating at least 11 parameters. While a flexible specification, that stretches the limits of what we are likely to be able to estimate with any precision, particularly in light of the correlations among many of the variables (see Section 4 below).

standard deviations from the median of widely-used WHO/NCHS/CDC standards), a widely accepted indicator of childhood nutrition, as our indicator for pre-schooling experience.

The data include from one to 15 measurements on each of 1954 children from the original sample for this anthropometric indicator 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 such a poorly nourished population (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 throughout the remainder of the pre-schooling period. Based on our objective of summarizing the entire pre-schooling experience, we employ the height-for-age z score at age 72 months (± six months) as our indicator of pre-schooling experience, which is both close to the age of starting school and an age where z scores are relatively stable.[13] 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[14] 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

[13] 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. 2005a 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 height-for-age z score at 72 months rather than at 36 months because we want to represent the whole pre-schooling period and because height-for-age z score at 72 months is more highly correlated with RCS and NVS than is height-for-age z score at 36 months.

[14] 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.

not have an observation at age 72 months) by the average difference between the measurement at the observed age and at 72 months.[15]

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 1447 for whom we have both adult cognitive skills test scores, for 180 individuals (12%) for whom we have the test scores we do not have information with which to estimate the height-for-age z score at age 72 months. For the 1267 individuals for whom we have an actual or predicted height-for-age z score at age 72 months, the mean value is -2.00 (median -1.94), the cutoff for the definition of moderate versus mild stunting, with a SD of 0.98. The population was, on average, stunted. The means do not differ significantly for males versus females ($t = 0.2$, $p = 0.87$). To retain the 180 observations on individuals without pre-schooling height-for-age when estimating the impact of the experiences during the schooling and post-schooling periods, we include in our estimates a dichotomous variable with a value of one if the estimated height-for-age z score at age 72 months is missing (and zero otherwise)[16] and replace the missing height-for-age z score with the sample mean of -2.0.

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 4.5 grades, with a SD of 3.3. Males (mean 5.0, SD 3.5) average significantly more grades of completed schooling than do females (mean 4.1, SD 3.2) with $t = 4.8$ ($p < 0.01$). The distribution has a mode at six years (26% of the individuals), completion of primary school. There also are secondary modes at zero grades (15%) and three grades (12%). The highest grade completed of school is significantly correlated ($p < 0.01$) with the indicators for the pre-school experience (correlation of 0.17) and post-schooling experiences described below (skilled work tenure 0.14, whether living in Guatemala City 0.27, age -0.14), suggesting that if relation (1) is the true relation but

[15] 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%.

[16] We treat this control dummy variable as endogenous in the 2SLS estimates, but do not report it in the tables. If we limit our basic estimates (Section 4.2) to the subsample for which we have height-for-age z scores at 72 months, they do not differ substantively from the estimates in Table 2.

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 of continuous work experience (tenure) prior to the 2002-4 survey in *skilled* occupations[17] as our first indictor of post-schooling experience that contributes to adult cognitive skills. The data do not permit the calculation of total experience in all jobs since individuals left school, but rather tenure all the jobs held in 1998 and in 2002-4.[18] 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 2.6, but with a SD of 4.6. 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 494 sample members who had at least some tenure in a skilled occupation, the mean is 7.5 years, with a SD of 5.1. There are substantial differences for tenure in skilled occupations by gender, with 356 out of 671 males (53%) having such experience (mean 7.8 years, SD = 5.0), but only 138 out of 776 females (18%) (mean 6.8 years, SD = 5.4). The difference in mean years across males and females is also marginally significant (t = 1.9, p=0.06).
- *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 measured 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

[17] We define skilled jobs to include white collar and administrative jobs and own farm/own enterprise work that yields income in the top quintile for such activities in 2002-4.

[18] 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, if there is depreciation of unused knowledge.

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 skills through more experience and the depreciation of skills if they are not used. The mean sample age is 32.4 years, with a SD of 4.3 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 0.08 and 0.11. Therefore, they may represent different dimensions of post-schooling experiences.

Initial conditions (F_0, C_0, E_0, I_0):

Parental SES and schooling attainment in childhood (F_0): We explore the role of parental characteristics in the production of cognitive skills, such as mother's and father's schooling attainment, and a constructed socioeconomic status (SES) score that is the first principal component of both the assets owned and the housing characteristics of the parental household in 1975 (Maluccio, Murphy and Yount 2005). Parental education is very low with both fathers and mothers averaging less than two completed grades.

Community characteristics in childhood (C_0): While the four communities were well matched in terms of their health and nutrition environments, at the outset they differed significantly in their learning environments, in part because of different schooling experiences of prior generations and also because of occupational structure (Bergeron 1992; Maluccio *et al*. 2005b). We control for the permanent, i.e., fixed, dimensions of these differences by including village fixed effects in the first stage regressions.

Genetic and other endowments (E_0): We do not have direct observations on genetic endowments beyond the gender of the individual, for which we control in our first-stage estimates. Even after including this measure, however, we are not able to identify confidently the extent to which this control reflects genetic differences or cultural and market-determined gender differences (see Section 4.3.1).

Observed individual characteristics (I_0): Apart from gender, these include age at the time of the 2002-4 interview and whether the individual was a twin (1.6% of the sample), which may have longer-run implications associated with the generally lower birth weight of twins (Behrman and Rosenzweig 2004). Of course, age is thus carrying double duty in our conceptual model; we return to this feature when interpreting the results.

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

<u>Natural, market or policy events (ΔC)</u>: We construct variables at the community level that relate as closely as possible to the timing of key learning- and labor market-related decisions in each individual's development. For example, using information reported in earlier work about infrastructure, markets and services in the villages (Pivaral 1972; Bergeron 1992), complemented with a retrospective study done in 2002 (Estudio 1360, 2002), we construct variables including: 1) an indicator of the availability of a pre-school program when the individual was two years old; and 2) and the student-teacher ratio when the individual was seven years old. To capture changes in local market conditions, we construct variables indicating: 1) whether the individual was 23 when the local tobacco industry, particularly in Espíritu Santo, was crippled as a result of Hurricane Mitch washing away much of the fertile topsoil; and 2) changes in the salary in manufacturing from age 18 to 23, when individuals were 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 more closely relate 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 (prematurely and possibly accidentally) by the time they were 18, which relates to a critical juncture for entering the labor force.[19]

<u>Experimental nutritional shocks (ΔC)</u>: 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 types of intent-to-

[19] 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 parental SES and schooling attainments. However, in contrast to the results for the inclusion of parental SES and schooling attainments in the first stage instrument set for NVS (Section 4.3.2), inclusion of these family-related shocks in the instrument set does not result in questionable results for the Hansen J overidentification test.

treat measures, based on the date of birth of each individual and the dates of operation of the intervention. For each individual, we calculate whether s/he was exposed to either intervention for the majority of days while 0-12 months, 12-24 months and 24-36 months of age. We interpret these as a set of cohort controls. The potential exposure to the *Atole* intervention is then calculated by multiplying each of these cohort measures 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, 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, plausibly exogenous characteristics from the individual's background and community to identify each of the E_i life-cycle stage outcomes. These include the community level variables derived from the detailed community histories, the market and policy shocks, and the intent-to-treat variables derived from the original nutritional supplementation intervention. Much of the related literature also uses parental characteristics; our prior is that these may be less valid instruments but we also include them in some specifications to assess their performance. 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 is substantially limited. It would require, for example, that the presence of a pre-school at age two would have to have a direct effect that was outside of its effect on pre-schooling nutrition, schooling attainment, *and* post-schooling skilled work tenure (as well as migration, in some specifications). Our view is that those pathways capture well the possible mechanisms by which the instrument could have an effect. The final component of our identification strategy is to trust, but verify. We carry out a range of diagnostic tests to assess the validity of the instruments. On the whole, we find that that they have reasonable power and, with the exception of those specifications that include parental characteristics (F_0), we fail to reject the null hypotheses for overidentification tests. Given the direction of the 2SLS results relative to OLS, where predictive power in the first stage estimates is not overwhelming we conclude that, if anything, our reported results are conservative.

4. Results

We first summarize in Section 4.1 estimates that show the association between completed grades of schooling 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 a few 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. Finally, in Section 4.3 we consider how robust our estimates are to gender differences, inclusion of parental SES and schooling attainment in the first-stage, nonlinearities in the experience measures, alternative representations of RCS and NVS, controls for clustering at the village-birth year level and controls for attrition. Given the nature of the sample, a number of these individuals are siblings or half-siblings so throughout we control for mother cluster effects in the estimation of the standard errors (with 591 clusters for the 1447 observations).

4.1 Basic Associations between Grades of Completed Schooling and Adult Cognitive Skills

Most previous literature treats completed grades of schooling as if it represents adult cognitive skills. Therefore, we first explore to what extent completed grades of schooling 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 completed grades of schooling. The associations are strong, though far from perfect—consistent with 59% of the variance in RCS and 25% of the variance in NVS. At the mean, each additional grade of completed schooling is associated with an additional 5.1 points (23% of a SD) on the RCS test and with an additional 0.9 points (15% of a SD) on the NVS test.

[Table 2 about here.]

These associations, however, do not represent the causal effect of completed grades of schooling on adult cognitive skills, as noted above, for two reasons: (1) If relation (1) is the true specification, then these relations are mis-specified because they exclude pre- and post-schooling experiences. As noted in Section 3.2 above, grades of completed schooling are significantly correlated with our indicators for pre- and post-schooling experiences, so precluding those experiences is likely to change the estimated coefficient of grades of schooling 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. (2) The OLS estimates treat schooling as if it is determined independently of the genetic endowments in relation (1). If, instead, schooling is instrumented to account for its being determined in part by past behaviors using the instruments in Table 1 (excluding parental SES and schooling attainment), the estimates change substantially (column 2 in Table 2). For RCS the estimated impact of grades of schooling drops by a third (to 3.4, 15% of a SD) with a much lower z value but one that still indicates significance (8.04). This suggests that the component of the vector E_0 in relation (1) that directly affects RCS is positively correlated with grades of completed schooling, as is generally hypothesized for innate learning ability (Behrman and Rosenzweig 1999). For NVS, the estimated impact of grades of schooling also drops, by 24% (to 0.70, 11% of a SD), again with a much lower z value than in the OLS estimates but one that still indicates significance (5.03). Hausman tests (not shown) indicate that for both RCS and NVS, the estimated coefficients on years of schooling are significantly different in the two stage least squares (2SLS) results. These results, however, are tentative because they are based on a potentially mis-specified model in which pre- and post-schooling experiences are constrained to have no effect. We explore whether these results persist under the full specification of relation (1) in Section 4.2.

4.2 Basic Specification of the Adult Cognitive Skills Production Function

Table 2 also presents two sets of estimates of a first-order Taylor series expansion of the adult cognitive skills production function in relation (1) for RCS and NVS: OLS estimates (column 3) and 2SLS estimates including all the instruments listed in Table 1 except for parental SES and schooling attainment (F_0) (column 4). Panel C gives first and second stage diagnostics for the 2SLS estimates presented in column 4: the Shea partial R^2 and F statistics for the first stage, Anderson IV relevance test, the Hansen J statistics for overidentification, the Cragg-Donald statistic for weak instruments and Hausman tests for the second stage.

The first-stage estimates (Appendix Table A1) have a number of plausible significant (at least at the 10 percent level) estimated coefficients consistent with our hypotheses about how they affect each of the life-cycle stages. Reporting being a twin predicts a lower height-for-age z score and the set of exposure variables and cohort controls are jointly significant in the that prediction, though it is difficult to tease apart their individual effects, likely due to high correlations among them (which range from 0.26 to 0.81). A pre-schooling program present at age 2 and lower student-teacher ratios at age 7 predict higher schooling attainment, as does the individual's age (consistent with secular increases in education in Guatemala over this period, see Maluccio *et al.* 2005b), whether the individual is a male, and a higher rate of change of salaries in manufacturing over the period when the individual was 18-23 years old.[20] While not strictly necessary, higher education may be one of the criteria used in hiring in manufacturing and other industries. Notable period of rising wages occurred in the late 1970s with reconstruction after the earthquake and in the late 1980s and early 1990s, associated with a building boom in Guatemala City. The loss of a parent before age 18, on the other hand, is associated with almost one year less of completed schooling. Tenure in a skilled job also is positively affected by age and by whether the individual is a male. Moreover, the market level indicators of the tobacco industry shock and manufacturing wage increases influence tenure, as well as migration to Guatemala City, in expected ways.

While the community effects are not particularly strongly associated with height-for-age z scores or skilled job tenure, they show strong patterns for schooling and migration. Each effect is relative to Espíritu Santo. This community is furthest from Guatemala City, thus making commuting rather costly whereas it has been possible in the other communities for a number of years. Espíritu Santo is also the community with the longest tradition of education, a pattern discerned in the initial surveys of the 1960s.

RCS (Inter-American Reading and Comprehension Tests): Our preferred estimates are the 2SLS in column 4 in which post-schooling experience is represented by skilled job tenure and age (which is not treated as endogenous). Below we describe the similar results obtained when we also include the indicator for migration to Guatemala City. For these estimates (Panel C), F tests indicate that the first-stage estimates have strong predictive power, 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 ($J = 17.9$, $p = 0.16$), and the Hausman tests indicate that the overall relation differs from the OLS estimates ($Chi^2 = 20.6$, $p < 0.01$).

As additional checks on the power of our instruments we report the Anderson IV relevance test and the Cragg-Donald (CD) statistic. The former is significant at $p < 0.01$, again suggesting that our instruments as a set, are strong predictors of the endogenous variables. To assess the importance of the latter statistic, we reference the literature on testing for weak instruments. Stock and Yogo (2004) offer guidance in the case of

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

multiple endogenous regressors that uses the CD statistic. In particular, they present tables which show 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 instruments 3) the desired significance level and 4) the extent of bias of 2SLS relative to OLS. While it is not possible to use their tables directly, since we have four endogenous variables, the CD statistic of 3.1 is below the critical values they indicate for three or fewer endogenous variables. Nevertheless, to the extent that indeed our 2SLS estimates are biased (perhaps even by 0.3), but are biased toward the OLS estimate, it suggests that the results we report below are, if anything conservative and *understate* the differences between OLS and 2SLS. Of greater concern, is whether the relative weakness of the instruments coexists with a weak correlation of the error terms between relations (1) and (2), but our relatively large F-statistics would seem to suggest that this would not be too severe of a problem (Bound, Jaeger, and Baker 1995; Staiger and Stock 1997).

The 2SLS estimates indicate a significant positive increase in test scores of about 2.9 points (or about 13% of a SD) for an *additional grade of schooling attainment*. This is an effect 42% smaller than that estimated by OLS with the same specification, suggesting substantial unobserved omitted ability (and other excluded endowments) bias. The failure to treat schooling as behaviorally determined thus apparently results in a considerable 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 17% if the pre- and post-schooling experiences are constrained to have no effects and schooling is treated as behaviorally determined, and by about 75% if also schooling is treated as exogenously determined (as in the OLS estimates in column 1).

The results do *not* indicate a significantly positive impact of the indicator of *pre-schooling experience*, height-for-age z score at 72 months on RCS. The point estimate actually suggests a decrease of about 1.9 points for every increase of 1.0 in the z score (which is about one SD in the sample), but it is imprecisely estimated with a z value of only 0.74. In contrast, the OLS estimate at 1.6 is significant at standard levels (t = 3.75). The 2SLS estimates raise doubts about the apparently significant association in the OLS estimates reflecting a causal effect.[21]

The 2SLS estimates indicate a significantly positive estimated impact of *post-schooling experiences*, suggesting that every additional year of *skilled work tenure* increases the RCS score by about 0.7 points (3% of a SD) with z = 1.96. This coefficient estimate is 15 times larger than the insignificant OLS estimate, suggesting a negative correlation between some component of E_0 that impacts positively on RCS and skilled work tenure so that OLS estimates result in substantial underestimates of the positive effect of skilled work tenure on RCS. In contrast, our preferred estimates indicate a significantly negative estimated impact of age on RCS, of about the same magnitude.

NVS (Raven's tests): Our preferred estimates again are the 2SLS estimates, but in this case with post-schooling experiences represented by skilled work tenure and age. As with RCS, the NVS results have satisfactory diagnostic statistics (Panel C). The first stage F-statistics and Anderson test are, of course, 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 (J= 15.4, p = 0.28), and the Hausman test indicates that the overall relation differs from the OLS estimates (Chi^2 = 51.8, p < 0.01).

The results (column 4, Panel B) indicate a positive increase in test scores of about 0.15 points (or about 2% of a SD) for an *additional grade of schooling completed*. This is less than 20% of the OLS estimate and much less precisely estimated and not significantly nonzero (z = 0.7). Comparison of our preferred 2SLS 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 five) of the direct impact of completed grades of schooling on NVS scores.[22]

[21] The lack of significance of pre-schooling experiences here is not necessary inconsistent with the significant impact of pre-schooling nutrition on adult cognitive skills reported in Maluccio et *al*. (2005a) because for the present estimates the pre-schooling nutrition may be working through schooling and post-schooling experiences in the chain described above in footnote 6.

[22] Though, of course, as discussed in note 6, schooling may be working through the post-schooling experiences—as is suggested by this same comparison.

In contrast, we find a significantly positive impact of the indicator of *pre-schooling experience*, height-for-age z score at 72 months, of about 2.8 points (nearly 50% of a SD) for every increase of one in the z score. Comparing this estimated effect with the OLS estimate suggests that the impact of pre-schooling experience is substantially underestimated (the OLS estimate is only one-quarter as large) if there is not adequate control 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 indicate a positive significant estimated impact of the type of *post-schooling experience* represented by skilled work tenure on NVS, indicating a 1.0 point increase (16% of a SD) for an additional year of skilled work tenure. This point estimate is nearly 10 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 negative significant effect of *age* on NVS—in this case a 0.18 point decrease (3% of a SD) for an additional year of age. This point estimate is two times larger than the one obtained with OLS.

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 relation (2) 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 IV 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 is 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.[23]

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

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

changed. The indicator for migration is well predicted in the first stage (with an F-test on the excluded instruments of 7.9, also see Table A1). The point estimates are, as expected, positive (6.0 in the RCS equation and 2.8 in NVS) but just insignificant at conventional levels ($p = 0.15$ for RCS; $p = 0.11$ for NVS). The other diagnostic statistics are similar to those reported in Table 2, except for the p-value for the Hansen J test that increasess for both outcomes.

Our conceptual framework 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 1, 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 both of the residual estimates of endowments are significant determinants of all three life-cycle indicators we use. 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 significant positive effects on all three 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 (height-for-age z score and skilled job tenure) and a significant positive one only on grades of formal 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 the impact of schooling tends to be overestimated if there is not control for endowments, but the impact of the pre- and post-schooling experiences, if anything, tend to be underestimated.

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: 3.6 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 2SLS 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, as indicated earlier, is a predominantly male phenomenon. The Shea partial R^2 declines 0.01 and the F-statistic on the excluded instruments, even though the market shock variables we include are significant, is 1.1. Second, although insignificant in NVS, the male dummy variable is negative and significant in RCS (-10.7), with a corresponding increase on the coefficient on skilled tenure, which increases over four-fold. Last, the coefficients on the other life-cycle stages remain essentially unchanged. In the case of RCS, 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. Adult males in the sample have on average significantly more completed grades of schooling (0.8, t = 4.81), years of tenure in skilled jobs (2.5, 2.14) and years of tenure in unskilled jobs (0.5, 2.61), though not height-for-age z scores at age 72 months (0.01, 0.16) or age (-0.29, 1.26). Given our preferred estimates of the cognitive skills production functions in Table 2, the gender differences in completed school grades 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 64% of the gap) but the gender gap in skilled job tenure only somewhat less important for RCS (47% of the gap) and somewhat more important for NVS (78% of the gap).

4.3.2 Including parental SES and schooling attainment among the first-stage instruments

On *a priori* grounds and on the bases of previous studies we question whether parental SES and schooling attainment can serve as legitimate instruments (Section 3.2). Table 3 presents estimates that parallel those in column 4 of Panels A and B of Table 2. Adding parental SES and schooling attainment to the instrument set yields estimated coefficients that fall between the OLS and 2SLS estimated coefficients reported in Table 2 (except for the coefficient on height-for-age z score which like the others moves toward the OLS value but is even larger than that reported in Table 2). The increased coefficient estimates on height-for-age z scores and schooling attainment are consistent with the possibility that parental SES and 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 parental SES and parental schooling attainment from our first-stage instruments in the estimates in Table 2.[24]

[Table 3 about here.]

4.3.3 Alternative Representations of RCS and NVS

RCS has a mass point at zero due to the literacy screen, as discussed in Section 3.2. Therefore we present in Table 4 four alternative IV estimates (all using the same instrument set as in our preferred estimates in Table 2) that deal differently with this concentration of low scores on the IARC test used for RCS (described in Section 3.2): (1) Tobit estimates using RCS that account for the lower bound and mass point at zero; (2) RCS + prelit; (3) RCS-quartile; and (4) RCS-median with probit estimates for being above the median. 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. In sum, these alternative representations for RCS and NVS do not alter our findings or change

[24] We also explored whether removal of the parental death instrument made any difference to our reported results in Table 2; it does not and, if anything, leads to slightly better first stage results in terms of CD (3.2) but slightly lower p-values for the Hansen J overidentification test.

our qualitative interpretations in Section 4.2, though there are some slight differences in the relative precision of some of the estimates (e.g., skilled tenure is significant only at the 0.09 level in the regression using RCS+prelit as the dependent variable, in column 2, and at the 0.11 level for RCS-median, in column 4).

[Table 4 about here.]

4.3.4 Calculation of Standard Errors

The original intervention occurred in only four villages and the duration of exposure to the intervention for critical child ages was dependent on date 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. 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 (see Deaton, 1997). Based on the design of the intervention, we construct 64 clusters (4 villages x 16 different birth years). While allowing for clustering at this level in the calculation of the standard errors (estimates not presented) tends to reduce the magnitudes of some of the t statistics as one would expect, the reductions are fairly small and do not change the findings presented in Section 4.2.[25]

4.3.5 Attrition

The estimates presented in this paper are based on a sample of 1447 individuals, 60% of the original 2393 subjects (see Figure 1).[26] Despite the considerable effort and success in tracing and re-interviewing

[25] The same basic finding of little impact on estimated precision is reported in Maluccio et *al.* (2005a), which uses these data set to evaluate the impact of the nutritional experiment on educational outcomes, including the adult cognitive scores.

[26] Another problem related to attrition for this sample is that of mortality selection (Pitt and Rosenzweig 1989; Pitt 1997). Indirect evidence that mortality selection exists in the sample is the *prima facie* counterintuitive observation that risk of death is positively associated with later year of birth in the complete sample of 2393. The older members of our sample represent the survivors of their respective birth cohorts through to inception of data collection, and since the risk of death is highest in early childhood these survivors experienced a lower mortality rate after inception of data collection (most of which was driven by infant mortality) compared with the later birth cohorts in the sample, who were followed from birth. Because the fieldwork began in 1969 and included all children under seven years of age, it excluded all children from the sample villages born between 1962 and 1969 who died before the start of the survey. Furthermore, the intervention itself decreased mortality rates among the younger sample members (Rose, Martorell and

participants from the original sample, attrition of 40% is substantial, and cause for possible concern about the validity of the estimates reported above. Moreover, the overall attrition in the sample is associated with a number of initial conditions, with effects differing by the reason for attrition (Grajeda et. *al* 2005). However, what is of ultimate concern 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 sample members who died in early childhood or international migrants, both of whom may have different characteristics, lead to systematic bias of the estimates presented here?

To explore these questions 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 exogenous variables considered in the main models, as well as an additional set of endogenous variables associated with attrition, for all original sample members (N=2393). 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 whether the parents were alive when each sample member was seven years old and whether the sample members 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 resurvey. 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 study and 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 cognitive skills in relation (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).

Finally, while we do not formally have adjustments to correct for selection on unobservable characteristics, by including a large number of endogenous observables indicated above, which are likely to

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Rivera 1992). To the extent the variables included in our models are associated with these two forms of selection, our estimates partly control for mortality selection, though we do not implement any special methodology to do so beyond the controls for overall attrition presented in this section.

be correlated with unobservable characteristics, we expect that we are reducing the scope for attrition on unobservable characteristics bias as well.

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. 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.* 2005a) 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 of and the 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 usual approaches may lead to misleading 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 the developing country of Guatemala to investigate production functions for adult reading-comprehension cognitive skills and adult non-verbal 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 has significant and substantial impact on adult reading-comprehension cognitive skills but not a significant impact on adult non-verbal 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 (by 74%) for adult reading-comprehension cognitive skills and make the

impact on adult non-verbal cognitive skills appear highly significant (with t = 16.15) rather than insignificant (z = 0.69). Also, if parental SES and 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 non-verbal cognitive skills appears significant (with a z = 3.26) rather than insignificant (t = 0.69). This phenomenon suggests that parental SES and 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) affect substantially and significantly adult non-verbal cognitive skills (though not adult reading-comprehension 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 impacts on adult reading-comprehension cognitive skills and non-verbal cognitive skills. Age, on the other hand, has significant negative impacts on adult reading-comprehension cognitive skills and on non-verbal cognitive skills, probably because of depreciation of these skills with age as formal schooling becomes more distant. 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 pre-schooling impact on adult reading-comprehension 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) Estimates that do not control for pre-schooling and post-schooling experiences related to learning overstate the impact of schooling on adult cognitive skills (by 18% for adult reading-comprehension cognitive skills and by a factor of about 4.5 for adult non-verbal cognitive skills).

(4) Gender differences in the coefficients of the three life-cycle stage experiences in the adult cognitive skills production functions are not significantly nonzero. Therefore most of the fairly substantial differences in adult cognitive skills favoring males on average originates from gender differences in completed grades of schooling favoring males for reading-comprehension cognitive skills and gender differences in experience in skilled jobs favoring males for both reading-comprehension cognitive skills and non-verbal 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 three wider literatures. *First*, the results suggest that the use of years of completed schooling in cross-country growth regressions is like 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. *Second*, the results provide support for hypotheses underlying the 'Flynn effect' that suggest that improved childhood nutrition and increased work complexity tend to enhance cognitive skills. *Third*, the results raise questions about studies that assume that predetermined cognitive skills enhance productivity rather than treating cognitive skills as endogenously determined and possibly interacting with productivity.

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