Brains versus Brawn: Labor Market Returns to Intellectual and Physical Health Human Capital in a Developing Country*

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Abstract: Previous studies report that schooling and adult height have significant associations with wages. But schooling and height are imperfect measures of adult cognitive skills ("brains") and strength ("brawn"); further they are not exogenous. Analysis of rich Guatemalan longitudinal data over 35 years finds that proximate determinants—adult reading comprehension skills and fat-free mass—have significantly positive associations with wages, but only brains, and not brawn, is significant when both human capital measures are treated as endogenous. Even in a poor developing economy in which strength plausibly has rewards, labor market returns are increased by brains, not brawn.

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1. Introduction

Investment in various forms of human capital is believed to yield substantial benefits. Of particular interest in the economics literature are returns in the labor market. When examining such returns, the emphasis traditionally has been placed on intellectual human capital, individuals' knowledge and cognitive skills that plausibly contribute to their economic productivity. In most of this literature, intellectual human capital is represented by completed grades of schooling. A second strand in the literature, particularly for developing economies, has focused on physical or health human capital, reflecting individuals' capacity to undertake manual labor. Often height is used to represent health human capital, based on evidence that taller individuals are stronger on average and that strength is rewarded in labor markets (Immink and Viteri 1981; Strauss and Thomas 1996; Thomas and Strauss 1997; Schultz 2002, 2003). Height also is associated with increased earnings in developed economies (Persico, Postlewaite and Silverman 2004; Case and Paxson 2008). In the latter context, however, it is implausible that in most jobs—professional basketball apart—greater height directly leads to greater productivity, so interpretation of observed correlations has focused on possible dimensions of human capital for which attained height might be a proxy measure, such as social skills attained in adolescence (Persico, Postlewaite and Silverman 2004), cognitive ability (Case and Paxson 2008), social class (Steckel 2009) or general healthiness (Lundborg, Nystedt and Rooth 2009).

In this paper, we assess the returns to both adult intellectual and health human capital in labor markets in Guatemala, a poor developing country. Is it brains or brawn that matter? We make two contributions to the literature.

<u>Our first contribution</u> is to include proximate measures of both adult intellectual human capital (adult cognitive skills) and adult health human capital (fat-free mass). Much of the previous literature uses measures such as schooling attainment[1] and height that are largely determined in childhood and adolescence and therefore are problematic as measures of adult human capital for at least two reasons.

First, such indicators fail to fully capture human capital investments in childhood and adolescence. For example, time in school is but one of the inputs for intellectual human capital production in childhood and adolescence. Others that are likely to be important include child innate abilities, family characteristics and school characteristics. While these other inputs are almost certainly correlated with schooling attainment,[2] such correlations are unlikely to be perfect so that schooling attainment would not, in general, fully represent childhood and adolescent human capital.

Second, in general there are investments in both intellectual and health human capital after childhood and adolescence. Within a dynamic model of human capital accumulation, such investments are conditional on (and therefore correlated with) previous adolescent human capital stocks. The stronger these correlations are the more likely the indicators of adolescent human

[1] This critique does not apply to the subset of studies that incorporate adult cognitive skills to represent adult intellectual human capital. Examples include Boissiere, Knight and Sabot (1985), Murnane, Willet and Levy (1995), Alderman *et al.* (1996), Glewwe (1996), Glewwe *et al.* (1996), Behrman, Ross and Sabot (2008) and Case and Paxson (2008), as well as others summarized in Hanushek and Wößmann (2008). Most of these studies do not, however, include adult health human capital measures.

[2] Because of such correlations, schooling attainment is also in part a proxy measure for ability, family background and school quality so that the estimated coefficient of school attainment in a wage regression is likely to be a biased measure of the effect of increasing schooling attainment on wages, as is discussed in extensive literatures on ability biases and school quality.

capital stocks will capture variations in adult human capital.[3] But these correlations also are unlikely to be perfect. Other factors such as shocks experienced in adulthood, and not only adolescent human capital stocks, affect adult human capital. Learning occurs through work and other experiences affected by labor market and health shocks in adulthood beyond those perfectly correlated with adolescent schooling. Nutrition[4] shocks that are not correlated with height may occur after late adolescence.

Our second contribution is to treat both intellectual and health adult human capital as behaviorally determined and measured with error when estimating the wage equation. There are many studies documenting associations between intellectual human capital and labor market outcomes. A somewhat smaller literature presents associations between health human capital and labor market outcomes. Most of these studies treat these forms of human capital as statistically predetermined and without measurement error. For intellectual human capital, there is a growing literature that treats completed schooling as endogenous and measured with error (Card 1999; Duflo 2001), but very few studies that treat adult cognitive skills as endogenous and measured with error.[5] For health human capital, measures of short-run nutritional status (such as the body mass index or BMI) generally are treated as endogenous, but longer-run nutritional status as typically represented by adult height usually is treated as predetermined.[6] Therefore, as Strauss and Thomas (2008, p. 3382) note, interpretation of coefficients on human capital in wage equations is "plagued by potential bias due to unobserved heterogeneity" correlated with adult stocks of intellectual and health human capital.[7]

One reason for the failure to endogenize these measures is that the data used in most studies do not easily allow analysts to treat adult forms of human capital as reflecting earlier choices, in part because they do not include historical or longitudinal information reaching back to the details of early childhood. Estimates that do not endogenize human capital may confound the effects of long-run persistent unobservables, such as individual and other endowments, with those of observed schooling, height or other representations of intellectual or health human capital. We use data collected over nearly the entire life span of the individuals in the sample, including their exposure to a randomized nutrition supplementation intervention during infancy and early childhood, to control for possible correlations between human capital and unobservables (and to correct for random measurement error), thereby obtaining consistent estimates of the causal effect of human capital on wages.

We investigate the impact of adult intellectual and health human capital on wages earned in Guatemala, a poor developing country in which the rewards to both intellectual and health human

[3] Of course, this means that estimated coefficients of adolescent human capital stocks are biased estimates of the direct impact of the adolescent human capital stocks themselves because they may reflect in part the impacts of subsequent, correlated investments.

[4] Examples of studies that treat as endogenous short-run adult nutritional status and find it improves adult productivity include Strauss (1986), Behrman and Deolalikar (1989), Deolalikar (1988), Behrman, Foster and Rosenzweig (1997), Foster and Rosenzweig (1994), Haddad and Bouis (1991), Pitt, Rosenzweig and Hassan (1990) and Sahn and Alderman (1988). These short-run measures reflect, inter alia, current prices and resources and not just adult height.

[5] Among the studies cited in footnote 2, only Alderman *et al.* (1996) and Behrman, Ross and Sabot (2008) treat adult cognitive skills as endogenous.

[6] Exceptions include Alderman *et al.* (1996), Schultz (2002, 2003) and Alderman, Hoddinott and Kinsey (2006).

[7] Some of the many studies that find that unobserved heterogeneity is important include Rosenzweig and Schultz (1985, 1987), Pitt, Rosenzweig and Hassan (1990), Behrman, Rosenzweig and Taubman (1994, 1996), Rosenzweig and Wolpin (1995) and Behrman and Rosenzweig (1999, 2002, 2004, 2005).

capital in labor markets may be considerable as occupations vary substantially, from casual day laborers, to own-account agricultural workers, to white-collar professionals. Section 2 outlines our conceptual framework, a production function for adult wages dependent on adult intellectual and health human capital that are the result of investments over the individual's life cycle. Section 3 describes the data we use that make possible our estimation of both the usual specification of wage production functions and ones in which wages depend on endogenously treated human capital with indicators of proximate adult human capital stocks of skills and strength.

Section 4 presents our main estimates for wages. We begin with the usual specification in the literature, treating completed grades of schooling and adult height as predetermined and measured without error. These yield results similar to previous studies with apparent effects of both completed grades of schooling and adult height on wages. We then consider alternative adult human capital indicators that are more proximately related to productivity, and treat them as determined by previous choices. We find that adult intellectual human capital has significant effects on adult wages and that these effects are underestimated considerably when endogeneity and measurement error are not controlled. By contrast, when adult health human capital is treated endogenously, it does not significantly affect wages in the full sample. In Section 5, we examine the effects of intellectual and health human capital on hours worked, finding again that intellectual, but not health human capital, is what matters for increasing hours worked and, consequently, total annual income. In Section 6, we conclude.

2. Conceptual Framework

Consider a simple two-period model with two forms of human capital: intellectual (I_t) and health (H_t), where t = 1 for the first period (defined as childhood and adolescence) and t = 2 for the second period (adulthood). Parents in their children's early life, and then increasingly the children themselves as they age, select investments in a fashion consistent with maximizing their expected welfare. In the second period, the production function for wage rates Y_2 depends on both intellectual (I_2) and health (H_2) human capital in that period, age (A_2), a vector of individual endowments ($\mathbf{E_I}$, including genetic endowments), village-of-origin endowments ($\mathbf{E_V}$) and a stochastic disturbance term for wages in that period (U_{2Y}):

(1) $Y_2 = Y^p(I_2, H_2, A_2, \mathbf{E_I}, \mathbf{E_V}, U_{2Y})$

where the superscript p indicates that relation (1) is a production function. Intellectual human capital in the second period (I_2) might be represented, for example, by adult cognitive skills and health human capital (H_2) by adult fat-free mass. Both of these forms of second-period human capital are determined by production functions in which inputs include first-period human capital investments (I_1, H_1), other endogenous production function inputs in the first and second periods ($\mathbf{X_1}$, $\mathbf{X_2}$), age (A_2), fixed individual endowments ($\mathbf{E_I}$), village-of-origin endowments ($\mathbf{E_V}$) and stochastic disturbance terms (U_{2I}, U_{2H}) that represent shocks that are orthogonal to first-period human capital investment inputs and outcomes:

(2) $I_2 = I^p(I_1, H_1, \mathbf{X_1}, \mathbf{X_2}, A_2, \mathbf{E_I}, \mathbf{E_V}, U_{2I})$ and

(3) $H_2 = H^p(I_1, H_1, \mathbf{X_1}, \mathbf{X_2}, A_2, \mathbf{E_I}, \mathbf{E_V}, U_{2H})$.

In these second-period production functions, first-period human capital measures, such as attained schooling (I_1) and early-life nutrition (H_1), are endogenous variables that potentially affect <u>both</u> forms of second-period human capital. In addition to first-period human capital measures, however, there are other choice variables ($\mathbf{X_1}$, $\mathbf{X_2}$)—such as parental stimulation and time studying in the first period, and nutrition and work experience in the second period—that also likely affect second-period human capital. Moreover, both individual endowments ($\mathbf{E_I}$) and village-of-origin endowments ($\mathbf{E_V}$) may enter into the production of second-period human capital, as well as directly affect second-period wage rates in relation (1). Individual endowments (other than sex) include, for example, factors such as innate abilities and health frailty that are not likely to be observed. Village-of-origin endowments include general cultural attitudes towards human capital investments and work, role models and the opportunity set of economic alternatives in the village in local labor markets and through, for example, migratory linkages to other labor markets.

In addition to these second-period production functions, there are reduced-form dynamic demand relations (indicated by a superscript d) for each form of human capital in each period that depend on various prices and policies ($\mathbf{P_1, P_2}$); individual, family[8] and village-of-origin endowments ($\mathbf{E_I}$, $\mathbf{E_F}$, $\mathbf{E_V}$); when the individual was born (A_0); and stochastic disturbance terms for both periods (W_{tj}, where t = 1, 2 for the two periods and j = I, H for the two forms of human capital).[9] For the first-period investments, the expected (indicated by superscript "e"), rather than actual, values of the second-period variables are relevant:

(4) $I_1 = I_1^d(\mathbf{P_1}, \mathbf{P_2^e}, \mathbf{E_I}, \mathbf{E_F}, \mathbf{E_V}, A_0, W_{1I}, W_{2I}^e)$,

(5) $H_1 = H_1^d(\mathbf{P_1}, \mathbf{P_2^e}, \mathbf{E_I}, \mathbf{E_F}, \mathbf{E_V}, A_0, W_{1H}, W_{2H}^e)$,

(6) $I_2 = I_2^d(\mathbf{P_1}, \mathbf{P_2}, \mathbf{E_I}, \mathbf{E_F}, \mathbf{E_V}, A_0, W_{1I}, W_{2I})$, and

(7) $H_2 = H_2^d(\mathbf{P_1}, \mathbf{P_2}, \mathbf{E_I}, \mathbf{E_F}, \mathbf{E_V}, A_0, W_{1H}, W_{2H})$.

Despite its simplicity, this two-period model with two forms of human capital has a number of important implications for our estimation strategy, as well as for interpreting previous estimates.

(1) Treating second-period intellectual human capital (I_2) and health human capital (H_2) as predetermined in estimating relation (1) is likely to result in biased estimates of their respective impacts on wages because both I_2 and H_2 are likely to be correlated with unobserved individual endowments ($\mathbf{E_I}$ and $\mathbf{E_V}$ as suggested in relations (2) and (3), and (6) and (7)) that also affect directly wages. This concern has been emphasized and explored considerably in the literature with regard to obtaining estimates of the impact of intellectual human capital as represented by schooling on wages (Card, 1999), though less so with regard to estimates of the impact of measures of longer-run health capital on wages (Schultz 1997).

(2) If relation (1), with both intellectual and health human capital, is the true relation but only intellectual human capital is included, even if there are valid[10] instruments for it, the estimates of the impact of intellectual human capital are likely to be biased. This is because omitted health human capital may be correlated with instrumented intellectual capital, as they both depend on the same set of variables (relations 6 and 7).

(3) In most of the literature that includes both intellectual and health human capital, as well as in most of the literature that includes only intellectual human capital, their empirical

[8] Family background and resources are likely to be important because of imperfect capital markets and limited capacity for self-financing human capital investments in poor societies. It is possible, however, that such characteristics might be correlated with unobserved individual endowments (for example, via the intergenerational transmission of genetic traits), a possibility we consider in our empirical work below.

[9] These reduced-form error terms incorporate the disturbances for the production of both forms of human capital as well as other disturbances that the individual and the household experience.

[10] "Valid" in the sense that they predict well intellectual human capital and physical human capital and are not correlated with the second-stage disturbance term in the true specification that includes both forms of human capital.

representations are more akin to I_1 and H_1 (i.e., schooling attainment and height) than to I_2 and H_2. This also is likely to lead to biased estimates of the impacts of I_2 and H_2 if in fact I_1 and H_1 are inputs into the production of the second-stage human capital variables as in relations (2) and (3) and there are other choice variables in relations (2) and (3), such as time spent studying, nutrition or other health-related activities. This can be seen by substituting relations (2) and (3) into relation (1) and noting that in the usual specification $\mathbf{X_1}$ and $\mathbf{X_2}$, which are determined by reduced-form relations parallel to (4)–(7) so that they are likely to be correlated with I_1 and H_1, are excluded from the specification. Moreover, if I_1 and H_1 are highly correlated with the omitted $\mathbf{X_1}$ and $\mathbf{X_2}$ variables, it may not be possible to distinguish between relation (1) with I_2 and H_2 and relation (1) with I_1 and H_1 on the basis of consistency with the variance in the dependent variable Y_2 because I_1 and H_1 are good proxies for the omitted variables even though their coefficient estimates do not represent their causal effects.

(4) There may be important cross effects of prices and policies ($\mathbf{P_1}$, $\mathbf{P_2}$). For example, changes in nutritional policies concerning young children may affect second-period intellectual human capital (I_2) and changes in school characteristics may affect second-period health human capital (H_2), as is reflected in relations (6) and (7). Therefore, it would be incorrect to infer that early life nutrition, for example, has important effects on wages only if adult health human capital is significant in relation (1) or conversely that early schooling is important only if intellectual human capital has significant effects in relation (1). The impacts of early-life nutrition and early schooling depend on the estimated effects in relations (6) and (7), as well as on those in relation (1).

(5) The framework points to potential instruments that can be used in instrumental variable (IV) estimates of relation (1) to control for the endogeneity of human capital, namely the right-side variables in the reduced-form demand relations (6) and (7) that are not directly reflected in relation (1). Moreover, if there are critical life-cycle stages, such as windows of opportunity for early life nutrition when children are less than three years of age or for school entry when children are around seven years of age, these instruments may include the prices and policies at these critical ages (i.e., the impact of $\mathbf{P_1}$ and $\mathbf{P_2}$ may depend on A_0, as detailed further in section 3.3.5).

(6) The framework in this section is presented ignoring the possibility of measurement error in the relevant right-side variables. If there is random measurement error for the indicators of human capital stock of interest, even in the absence of endogeneity bias of the sorts outlined above, measurement error bias towards zero is likely. An IV approach, while motivated by endogeneity concerns, also corrects for such bias.

3. Data

The data demands are considerable for estimating the wage production function posited in Section 2, in which the relevant adult human capital stocks are treated as endogenous, reflecting decisions back to infancy. We use a rich longitudinal data set from Guatemala collected over a 35-year period with adult wage, hours and income information from all economic activities, measures of adult intellectual and health human capital as well as of schooling attainment and height, and shocks from an experimental nutritional intervention (when the individuals were children) and an earthquake, and policy and market changes over time.

3.1 The Institute of Nutrition of Central America and Panama (INCAP) Longitudinal Data

Beginning in 1969, the Institute of Nutrition of Central America and Panama (INCAP) began collecting longitudinal data on children 0–7 years old and their families and communities as part of a nutritional supplementation trial (for details, see Habicht and Martorell 1992; Read and Habicht 1992; Martorell, Habicht and Rivera 1995). After screening approximately 300 villages, two sets of village pairs (one pair of "small" villages with about 500 residents each and the other "large" villages with about 900 residents each) in eastern Guatemala were selected purposively for the trial. Two of the villages, one from within each pair matched on population size, were randomly assigned to receive a high protein-energy supplement drink, *atole*. The other two received an alternative no-protein, low-energy supplement, *fresco*. INCAP implemented the nutritional supplementation program and collected data on children under seven years of age until September 1977; children included in the 1969–77 longitudinal survey were born between 1962 and 1977. [11]

In 2002–04, a team of investigators, including the authors of this paper, undertook a follow-up survey targeting all participants in the 1969–77 survey, called the Human Capital Study (HCS). At that time, sample members ranged from 25 to 42 years of age. Of 2392 individuals in the original 1969–77 sample by the time of the 2002–04 HCS: 1855 (78%) were determined to be 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, 60% lived in the original villages, 8% lived in nearby villages, 23% lived in or near Guatemala City, and 9% lived elsewhere in Guatemala (Grajeda *et al.* 2005).

In the main analyses, we focus on the 1012 respondents (54% male) interviewed in the 2002–04 HCS for whom wage, hours, income and all relevant human capital measures are available.[12] They comprise 55% of the 1855 individuals who were known to be alive and living in Guatemala at the time of HCS, 46% of those known to be alive and 42% of the original sample of 2392. We assess potential biases due to attrition and labor market selectivity in Section 4.3.2.

3.2 Labor Market Activities

During HCS individuals were interviewed in 2003–04 about all of their income-generating activities at present and in the previous calendar year. Topics covered included: a) wage labor activities for every wage job (type of occupation; wages and fringe benefits; description of the employer; and hours, days and months worked); b) all agricultural activities (amount of land cultivated; crops grown; production levels and values; use of inputs; and hours, days and months worked); and c) non-agricultural own-business activities (type of activities; value of goods or services provided; capital stock employed; and hours, days and months worked) (Hoddinott, Behrman and Martorell 2005).

Table 1 presents descriptive statistics on labor force participation disaggregated by sex. The survey instrument was designed to capture engagement in the three broad sets of activities described above

[11] This population has been studied extensively since the original survey, with particular emphasis on the impact of the nutritional intervention. Martorell *et al.* (2005) gives references to many of these studies; more recent examples include Behrman *et al.* (2009, 2010), Hoddinott *et al.* (2008) and Maluccio *et al.* (2009). For part of the period covered by these surveys (particularly the 1980s and early 1990s), much of western and northern Guatemala was embroiled in civil war, though these survey villages were not directly affected. There was also a round of data collection on a subset of the population carried out in 2007–08 (Melgar *et al.* 2008), which we use in Section 4.3.3.

[12] Beginning with 1424 respondents who completed the income-generating activity modules, we exclude from the analyses those who were not participating in the labor market in any of the three categories examined (wage labor, own-account agriculture, and own-account non-agricultural business) since an hourly wage rate could not be calculated (12 men and 238 women). We next exclude 97 men and 65 women who did not have valid measurements for the four key human capital measures described in Section 3.3.

(wage, agriculture, non-agriculture) and allowed for multiple jobs or activities within each category as well as work in more than one category, both of which were common. Four highly experienced and specially trained interviewers carried out these economic interviews.[13] Virtually all men (98%) and most women (69%) were engaged in some sort of own-account income-generating activity. In the year prior to the interview, 79% of men were working for wages (with more than half of these in unskilled occupations), 42% in own-account agriculture and 28% in own-account non-agricultural business. A third of women were working for wages (with the majority in unskilled occupations) and a third in own-account non-agricultural business, and 20% were in own-account agriculture. 78% of men and 45% of women reported working nine or more months in the year in one of these activities while a small proportion (11% of men and 2% of women) worked that much in two or more of these activities.

Within wage labor, individuals worked in a wide range of occupations, categorized into the following groups: casual agricultural workers (approximately 15%); casual non-agricultural workers (for example, individuals working on road construction, 20%); domestic workers (for example, maids, gardeners or security guards, 10%); unskilled workers in the formal sector (20%); semi-skilled or skilled workers in the formal sector (20%); and white-collar workers in the formal sector (including individuals holding clerical, administrative, technical or professional positions, 15%). In own-account agricultural activities, the dominant crops grown were maize and beans, staples of the Guatemalan diet. Much smaller percentages (< 10%) of farmers grew any of the four next most-frequently-grown cash crops: squash, cucumbers, lemons or tomatoes. The apparent limited engagement in agricultural production is corroborated by other information collected in the survey. Plot sizes were small, as were the gross values of harvested maize and beans. For example, the typical casual agricultural laborer working full time earned approximately 660 Quetzales (US$ 87) per month, which when annualized was an amount equivalent to the median value of annual maize harvests and 70% of the median value of bean harvests. While income from own-farm operations was large for a small number of individuals, own-account agriculture was not a major source of income for most. Finally, the most common forms of non-agricultural own business activities were services (for example, bricklayers, tailors, cobblers, barbers or mechanics) and trading (for example, selling clothes, food and merchandise). Other less prevalent types included manufacturing and food processing. The mix of both skilled and unskilled occupations suggests potentially important roles for both intellectual and health human capital in the labor market for this population.

3.3 Central Variables for the Analysis

3.3.1 Dependent variables – hourly wage rates, annual hours worked and annual labor income

Descriptive statistics on hourly wage rates, annual hours worked and total labor income also are given in Table 1 for the sample used in our estimates. These data are constructed from questions on all income-generating activities in the calendar year previous to the interview. For each activity, individuals were asked the number of months in which they worked and how many days per month and hours per day they typically worked. These data are used to generate annual hours worked. In the case of wage labor, individuals were asked to report gross earnings, earnings nets of taxes and social security deductions as well as additional payments such as bonuses, transport and food, for the time unit (hourly, daily, weekly and so on) most relevant to their job, for each job they had held. In the case of own-agriculture activities, information on the value of crops produced was collected (including crops consumed by the household). The cost of land rentals was deducted and, for each

[13] The four interviewers received extensive office and field training. Periodic efforts were made to ensure that the implementation of questions was standardized across interviewers. Further, senior field staff carried out spot checks to ensure high quality data were collected.

person, an average labor return calculated based on the number of hours that individual worked.[14] In the case of own-business activities, information on net profits as well as the value of own consumption was collected for all own-business activities and, for each person, an average labor return was calculated based on the number of hours that individual worked.

Table 1: Income generating activities by sex

	Men	Women
Labor force participation (%)		
Any labor force participation	98	69*
Worked for wages	79	33*
Worked in own-account agriculture	42	20*
Worked in own-account non-agriculture business	28	33*
Worked 9+ months for wages	60	22*
Worked 9+ months in agriculture	7	1*
Worked 9+ months in non-agriculture	20	26*
Worked 9+ months in one sector	78	45*
Worked 9+ months by combining work in more than one sector	11	2*
	[655]	[769]
Hourly wages, annual hours and income (Quetzales, Q7.6= US$1.00)		
Hourly wage from wage labor	11.6 (10.5) [439]	8.2* (7.0) [224]
Hourly wage for own-account agriculture	8.0 (12.8) [222]	11.8 (26.4) [137]
Hourly wage for own-account non-agricultural	16.4 (29.1) [158]	14.8 (54.7) [223]
Hourly wage (all individuals)	11.6 (12.0) [546]	9.4* (14.3) [466]
Hours worked in wage employment	2225 (1107)	1649* (1151)
Hours worked on own farm	724 (675)	198* (294)
Hours worked on own business	1506 (1252)	1529 (1688)
Total annual hours (all individuals)	2523 (1144)	1584* (1513)
Wage employment income	24411	12792*

[14] A potential problem with this approach is that we are unable to subtract the cost of purchased inputs, though their use is relatively uncommon. Of those individuals who report work in own-account agriculture, less than 30% report purchasing seeds or hiring labor, though about 70% report buying fertilizer. Another potential problem is that we implicitly assume equal productivity across workers. The available data do not permit estimating production functions with different types of workers or some other strategy to allow for differential productivity by worker type. We partially explore these concerns in Section 4.3.4, where we use a self-reported "replacement" wage rather than an imputed wage.

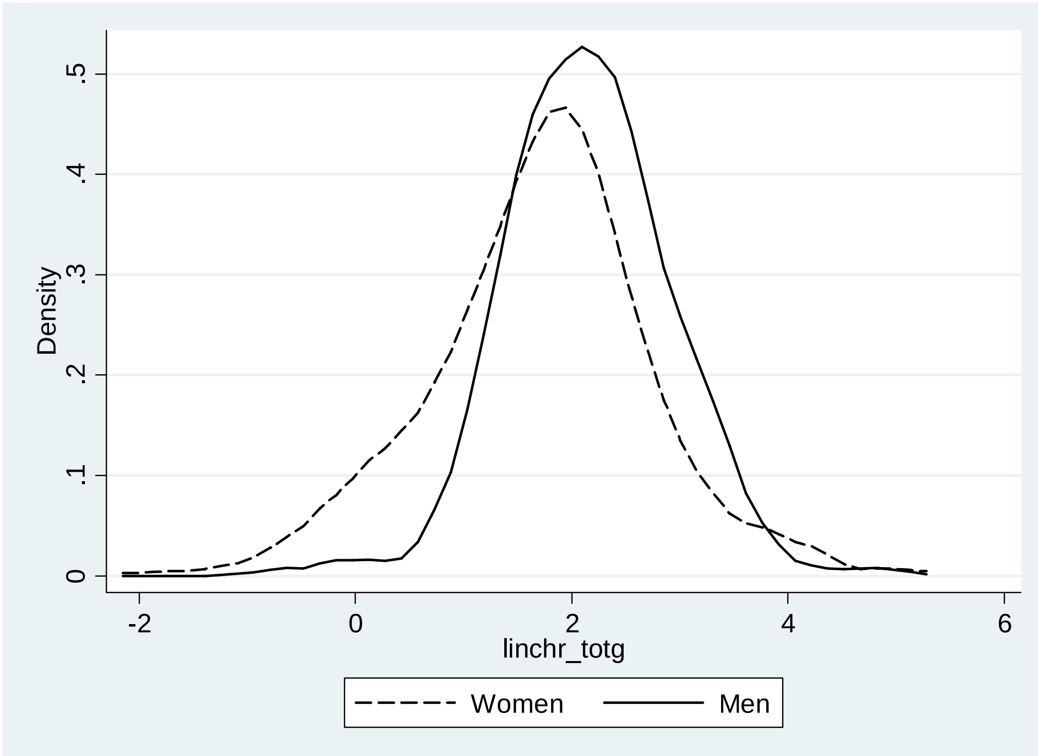
	(20575)	(13214)
Own agricultural income	5501	1061*
	(13250)	(1814)
Own non-agricultural income	17416	12774
	(24833)	(33313)
Total annual labor income (all individuals)	26904	12573*
	(23364)	(25311)

Notes: Means for wage labor, agricultural and non-agricultural activities limited to the sample of individuals who report working in (each of) those sectors. Standard deviations shown in parentheses and sample sizes in square brackets. * indicates men versus women statistically different at a 5% level or lower.

The first three rows in the second panel of Table 1 report the average hourly wage rates for each activity type, conditional on working in the particular activity. Men had significantly higher hourly wages from wage market labor than women, but not significantly higher returns to own-account activities.[15] Examining weighted average hourly wages from all three activities, men earned 30% more on average than women, and were substantially less likely to be earning the lowest hourly wages (Figure 1). Conditional on participating in the activity, men worked considerably longer hours in wage employment and in agriculture while hours worked in own-business activities were approximately the same for men and women. Correspondingly, men's annual earnings were higher than women's. Income from wage employment represents approximately 70% of all labor income earned by men; for women, income from wage employment and own business activities both comprise just under 40% each of total labor income. Income from own agriculture was less on average than wage employment or own non-agricultural income for both sexes.

[15] We use "significant" to mean at the 5% level unless otherwise noted. Patterns are the same if instead we compare logarithmic wages to reduce the influence of more extreme values.

Figure 1: Ln hourly wage rate



In the analyses, we model as the dependent variable the (logarithms of) average hourly wage rates for all activities, total annual hours worked, and total annual income, conditional on working in at least one of the activities (shown for men and women combined in Table 2).

3.3.2 Intellectual human capital (I)

Our preferred representation of adult intellectual human capital (I_2) within the framework presented in Section 2 is adult reading comprehension cognitive skills.[16] For comparisons with other estimates from the literature, however, we also use schooling attainment, which we interpret as a first-period investment (I_1) in childhood and adolescence that affects second-period (adult) intellectual and possibly health human capital.

Reading comprehension skills (RCS) were measured using the vocabulary (Level 3, approximately 4th grade equivalent) and reading-comprehension (Level 2, approximately 3rd grade equivalent) modules of the Inter-American Series Tests (Manuel 1967), administered to all individuals who

[16] As an alternative, we also examined the Raven's Progressive Matrices test as a measure of intellectual human capital, an assessment of nonverbal cognitive ability (Raven *et al.* 1984) given to the same adults. Raven's tests are considered to be a measure of eductive ability—"the ability to make sense and meaning out of complex or confusing data; the ability to perceive new patterns and relationships, and to forge (largely nonverbal) constructs which make it easy to handle complexity" (Harcourt Assessment 2008). The results are very similar if the Raven's tests are used instead of the reading comprehension tests, which is unsurprising because the two tests are highly correlated (0.57 in the raw data and 0.76 after being predicted). It proved infeasible to include and endogenize both at the same time. We prefer RCS because these skills are likely to be involved for a large proportion of the occupations in the sample and because these skills, but not Raven's test scores, are affected significantly by the extent of schooling attainment in the first period (Behrman *et al.* 2010).

passed a literacy screen.[17] Both tests were timed with 10 minutes allowed per test. The same tests (but Level 2 for vocabulary) were implemented in 1988–89 in an earlier survey on the same population, and demonstrated adequate test-retest reliability with correlation coefficients above 0.85 (Pollitt *et al.* 1993). The vocabulary portion has 45 questions and reading comprehension 40 questions, yielding a maximum possible score of 85 points. The distribution of test scores (for those who took the test) appears to be symmetric and approximately normal (though it fails to pass standard normality tests). The 19% of the sample who did not pass the pre-literacy screen were assigned a value of zero. Including those we score at zero, the mean score is 36.4 (38.6 for men and 33.9 for women). In all the regression analyses reported below, RCS are expressed as sample-specific Z scores standardized to have mean 0 and standard deviation (SD) 1.

<u>Schooling attainment</u> was measured as the highest grade completed.[18] Over 95% of respondents started school, with no difference between males and females. Conditional on starting school, almost half stopped before completing primary school, approximately 30% stopped after completing the full six grades of primary education, less than 20% continued on to secondary school and even fewer, less than 3%, continued beyond secondary school. Apart from formal schooling, it was also possible to complete primary or secondary school grades via informal schooling, in particular adult literacy programs. Our measure of grades completed includes both types of schooling, though informal schooling was rare. The mean is 4.8 grades (5.2 for men and 4.3 for women) (Stein *et al*. 2005). The correspondence between the completed grades measure reported in HCS and an earlier measure taken during a related study completed in 1996 on more than 90% of the same individuals is very high, with a correlation of 0.94 and only 8% of the observations differing by more than one grade of completed schooling. This accuracy is similar to previous studies in the literature (Ashenfelter and Krueger 1994; Behrman, Rosenzweig and Taubman 1994) and suggests that the noise-to-signal ratio for self-reported schooling attainment is about 0.1.

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

[18] Completed grades of schooling are often referred to as years of schooling, though the two are not identical when there is grade repetition or grade skipping, as is common in many developing countries. For Guatemala primary school grade repetition rates of 13 to 16% for 1970–2005 are reported in the UNESCO Global Education Digest (http://www.uis.unesco.org/ev_en.php?ID=7436_201&ID2=DO_TOPIC, accessed 20-2-09).

Table 2: Key variables – means and standard deviations (N=1012)

	Mean
Dependent variables	
Hourly wage rate (Quetzales, Q7.6= US$1.00)	10.6 (13.1)
Total annual hours	2090 (1406)
Total annual income	20305 (25298)
Human capital measures	
(I_1) Completed grades of schooling	4.8 (3.5)
(I_2) Adult Reading-Comprehension Cognitive Skills (RCS)	36.4 (22.8)
(H_1) Adult height (cm)	157.2 (8.5)
(H_2) Adult fat-free mass (FFM) (kg)	45.6 (7.7)
Individual characteristics	
Male	0.54
Age in years at interview in 2002–04	32.5 (4.2)
Village effects	
San Juan	0.23
Espíritu Santo	0.21
Santo Domingo	0.25
Conacaste (reference)	0.31
Excluded Instruments	
*Prices and policies (**P_1**, **P_2**)*	
Student-teacher ratio at student age 7 years	40.1 (9.1)
Ministry of health post in village at age 2 years	0.13
Born in the two years prior to the 1976 earthquake	0.13
Intent to treat nutritional intervention	
0–36 months	0.40
0–36 months * *atole*	0.22
*Individual endowments (**E_I**)*	
Twin	0.01
*Family endowments (**E_F**)*	
Mothers' schooling attainment in grades	1.41 (1.7)
Mothers' height in cm	148.8 (4.5)
Mothers' height missing dummy	0.21
Fathers' schooling attainment in grades	1.71 (2.1)

Notes: Standard deviations in parentheses.

3.3.3 Health human capital (H)

Our preferred representation for adult health human capital (H_2) is fat-free mass. Fat-free mass is a good measure of the reflecting components of health human capital stock that affect work productivity and therefore wage rates because it reflects muscle and skeletal mass and thus the capacity to carry out work (McArdle, Katch and Katch 1991).[19] For comparisons with previous

[19] Another measure of physical work capacity we considered was predicted maximum oxygen consumption (VO2max). It measures the capacity of individuals to deliver oxygen to muscle while doing physical work. FFM is a good proxy for VO2max because muscle mass is a key factor determining differences among individuals in the demand for oxygen. In separate estimates, we used predicted VO2max as our measure of adult health human capital. It exhibited the same pattern of results as those we report below, though the instruments were weak so we do not report it here. We also examined as an alternative measured isometric handgrip strength which correlates highly with total strength of 22 other muscles of the body (de Vries 1980). Using a Lafayette dynamometer, subjects were asked to exert a maximal and quick

literature, however, we also use adult height, an indicator of early-life and adolescent investments in health human capital (H_1).[20]

Adult fat-free mass (FFM) was estimated using predictive equations derived from studies of body density and anthropometry in a similar Guatemalan population; the equations used weight, height and waist circumference in women and weight and waist circumference in men.[21,22] Average estimated FFM is 45.6 kg (51.4 for men and 38.8 for women). Because this measure is based on predictive equations, despite the care taken in obtaining all its component anthropometric measurements, fat-free mass is still likely to be measured with error.

Adult height was measured to the nearest 0.1 cm, The mean is 157.2 cm (162.8 for men and 150.6 for women). Adult height reflects growth during the entire growth period, from fetal life to the end of adolescence growth. There is a substantial literature documenting that, in contexts of poverty and malnutrition such as those we are studying, growth failure is marked only in early-life. Beyond about the second year of life, growth velocities may in fact be, on average, similar in poor countries as found in developed countries until puberty, which may be delayed but otherwise normal. This maturational delay may allow for some catch up in height (Martorell 1997, 1999).

In all the regression analyses reported below, we use the logarithms of FFM and height. Therefore the estimated coefficients can be interpreted as conditional elasticities of wage rates (or hours worked or annual income) with respect to health human capital.

3.3.4 Additional second-stage controls

We include an indicator variable for male (54%) that may reflect one aspect of individual endowments ($\mathbf{E_I}$), gender-related expectations about labor market attachment or gender-related labor market discrimination. We include age (at the time of the interview, A_2) to reflect the impact of intellectual and physical maturity and experience. The average age is 32.5 years with little difference between men and women. We also include village-of-origin fixed effects to control for fixed village endowments ($\mathbf{E_V}$). This ensures that identification is not based merely on across village differences.

3.3.5 Instruments excluded from the second stage

The set of excluded instruments is motivated by relations (4)–(7) and includes various prices and policies (in particular $\mathbf{P_1}$, but possibly $\mathbf{P_2}$) and components of individual, family and village endowments not included above ($\mathbf{E_I}$, $\mathbf{E_F}$, $\mathbf{E_V}$), in some cases linked directly to when the individual was born (A_0). Several of these instruments have been suggested and used to endogenize height, as

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handgrip twice (alternatively with each hand, with at least 30 seconds between trials) and maximal values of the dominant and non-dominant hands were recorded. Weight-adjusted grip strength was calculated by dividing the sum of values for each hand by body weight (Montoye and Lamphier 1977). When we use the dominant weight-adjusted hand strength in alternative estimates, we obtain results similar to those obtained using FFM (but with less precision) which is unsurprising since the two are correlated at 0.79 in the raw data

[20] We prefer FFM to another measure of health human capital, BMI, because it is a better measure of work capacity. BMI is sensitive to variations in fat mass, which is increasing alarmingly in adults from most developing countries. When we use instead BMI, as has been used in several previous papers (Strauss and Thomas 1997; Thomas and Strauss 1998; Schultz 2002, 2003), results are similar.

[21] For women the estimating equation is FFM = 19.420 + (weight × 0.385) – (height × 0.215) + (abdominal circumference × 0.265). For men, FFM = – 48.472 – (weight × 0.257) + (abdominal circumference × 0.989) (Ramírez-Zea *et al.* 2006).

[22] Both of these measures of physical human capital, and the procedures for taking them, are discussed in more detail in Ramírez-Zea *et al.* (2005). They were carried out under the supervision of a physician.

well as shorter-term health human capital measures such as body mass index (BMI), in other contexts (Strauss and Thomas 1997; Schultz 2002, 2003).

Price and policy (**P_1, P_2**) shocks: Because the second-stage estimates control for the village of origin, the average prices and policies experienced by individuals in a particular origin village that can be treated as fixed over time are controlled for. However, there have been a number of village-level developments over the years, occurring at critical ages for particular individuals that also may have affected human capital development (and should therefore enter into the first-stage relations). Using information reported in earlier work about services in the villages (Pivaral 1972; Bergeron 1992), complemented with a retrospective study in 2002 (Estudio 1360 2002), we constructed indicators of such changes, including whether there was a Ministry of Health post operating in the village when the individual was two years old and the student-teacher ratios at the primary school level when the individual was seven years old. A second policy change of this type was the availability of the community-randomized nutritional supplement (Section 3.1). The nutritional and biomedical literatures emphasize the first 36 months of life as being a critical period (e.g., Martorell *et al.* 1995; Grantham-McGregor *et al.* 2007; Victora *et al.* 2008) for physical development. Some sample members in a given village were exposed to either of two supplements, one more nutritious than the other, for the entire 0–36 month window, while others from the same village who were born at different times were not. We construct two indicators, based on the date of birth of each individual and the dates of operation and type of nutritional intervention for which the individual was eligible. The first is a control for cohort effects and the second is the potential exposure to the high protein-energy supplement drink, *atole*. For each individual, we calculate whether as a child he or she was exposed to either program entirely from 0–36 months of age. The *atole* intervention exposure measure is then calculated by multiplying the cohort measure by an indicator of whether or not the child lived in one of the two *atole* villages. We include these two measures separately for each individual, which is equivalent to a difference-in-difference approach in estimation of the first-stage outcomes. These have been shown to have influenced adult outcomes for reading comprehension for both men and women and highest grade completed for women (Maluccio *et al*. 2009), and plausibly also influence the adult health human capital measures we model.

Thus, while reflecting community level characteristics, these variables vary by single-year age cohorts within each village, as well as across villages. This is preferable to the more typical approach of including indicators about such factors at a given time for a population with different ages at that point in time since these indicators more closely relate to periods in individuals' lives when there were critical vulnerabilities (e.g., nutritional insults during the first 36 months of life) or critical decisions (e.g., starting school) were made. The final indicator we incorporate is a dummy variable for whether an individual was born in the two years prior to the major 1976 earthquake in Guatemala because the economic shock due to the earthquake may have been particularly deleterious for infants and very young children. All of these variables are assumed exogenous to individual decisions.

Individual endowments (**E_I**): Most of the potential components of individual endowments, such as innate abilities and innate health, are unobserved in our and most socioeconomic data sets, though they are a central concern for unobserved variable bias in estimates of relations such as (1). While we observe individuals' sex, this is unlikely to help with identification because sex is likely to enter into the second-stage relation because of wage rate impacts of gender differences in endowments, gender-related expectations about labor market attachment or gender-related labor market discrimination. One individual endowment on which we do have information, however, is whether an individual was a twin. For the population being studied, being a twin can be regarded as a stochastic event as fertilization treatments that lead to multiple births in developed countries were certainly not practiced in these villages in the 1960s and 1970s. Being a twin tends to result in lower birth weight with negative consequences for early life cognitive and physical development, which then in turn possibly limit later intellectual and physical development (e.g., Behrman and Rosenzweig 2004).

Family endowments ($\mathbf{E_F}$): Family resources also are likely to be important determinants of investments in children because of imperfect capital markets and limited capacities for self-financing human capital investment in poor societies. We include parental intellectual and health human capital characteristics, specifically mothers' and fathers' schooling attainment and mothers' height.[23] Since such measures might be correlated with unobserved individual endowments that are in the disturbance term of the second-stage relation, however, we empirically assess whether they satisfy standard overidentification tests.

4. The Effects of Adult Intellectual and Health Human Capital on Wage Rates

We estimate the wage rate production function with the spotlight on the roles of *adult* intellectual and health human capital. For the reasons given in Section 2, our preferred specification includes indicators of adult intellectual human capital (adult reading comprehension scores, RCS) and adult health human capital (fat-free mass, FFM) with both types of adult human capital treated as endogenous. For comparison with the previous literature, we also present estimates in which both forms of human capital are assumed to be predetermined statistically and in which indicators of investments in childhood and adolescence, schooling attainment and height, are used instead. In addition to controlling for village-of-origin fixed effects in our estimates of relation (1), we allow for clustering at the birth year-village cohort level in the calculation of the standard errors, to control for potential serial correlation among children born in the same villages in the same year; this yields 64 clusters.[24,25] Before presenting estimates of the wage rate production function in Section 4.2, we discuss our identification strategy, first-stage estimates and diagnostics for the IV estimates.

4.1 Identification Strategy, First-Stage Estimates and Diagnostics

Our identification strategy has three components. First, using the framework developed in Section 2, we select plausibly exogenous characteristics from the individuals' backgrounds and

[23] We also include a dummy variable for the 20% of mothers' heights that are missing and imputed with the mean.

[24] Angrist and Lavy (2002) and Wooldridge (2003) suggest that standard corrections for clustering are valid only when the number of groups or clusters is large. In light of this, following Bertrand, Duflo and Mullainathan (2004), we also estimated the models using block bootstrapped standard errors, using the same 64 clusters and resampling 1000 times. Standard errors calculated from this approach were typically slightly larger than those reported in the paper (for example, in the models presented in Table 4, standard errors on intellectual human capital measures were 10–15% larger), but did not change any of the statistical significance reported in the tables.

[25] As discussed in Section 2, for both the village fixed effects and the clustering for the standard errors, we focus on the four villages of origin because, within the relatively segmented markets of developing countries when the sample members were children, general cultural attitudes towards human capital investments and work and the opportunity set of economic alternatives in the village and through migratory linkages to other labor markets probably had strong village components. By the time of the 2002–04 HCS survey, however, respondents had moved to over 100 communities and neighborhoods in nearly 20 municipalities throughout Guatemala. So our controls for village fixed effects and clustering are for villages of origin, not for adult labor market localities. Results are unchanged, however, if we instead condition on current location (represented using indicator variables for three of the four villages, other areas within the municipality of *El Progreso*, other municipalities of Guatemala, and Guatemala city). Given the data available to us, however, we are unable to endogenize current residence in addition to measures of human capital in the same specification.

communities to predict their adult human capital. These include time-varying community-level variables derived from the original randomized nutritional supplementation intervention and the community histories, including the 1976 earthquake. They also include an indicator of whether the child was a twin. Lastly, they include parental schooling and mother's height.[26] Second, we include *both* intellectual and health human capital in the second-stage estimates of adult wage rates. This substantially mitigates the possibility that the instruments we select have direct effects on adult wages beyond the controls in the second stage, in contrast to a framework in which only intellectual human capital is included in the second stage. For example, for the student-teacher ratio in the village at age seven to be an invalid instrument, it would mean that it had a direct effect (or was correlated with some other factor that had a direct effect) that was outside of its potential effects on both intellectual and health human capital. Third, we carry out a range of diagnostic tests to assess the strength and validity of the instruments. On the whole, we find that that the instruments we use strongly predict the human capital measures and we fail to reject overidentification tests, which plausibly have sufficient power to reject the null because the instrument set includes the randomly allocated exposure to the *atole* intervention during the first three years of life, which is likely to be exogenous.

The first-stage estimates for each human capital measure that we consider have a number of significant estimated coefficients, consistent with our hypotheses about how the excluded instruments influence the different forms of human capital (Table 3). The student-teacher ratio in the village of origin when the individual was seven years old is negatively and significantly associated with the intellectual human capital measures. The presence of a Ministry of Health post in the village when the individual was two years old is positively and significantly associated with the health human capital measures. Having been born in the two years prior to the 1976 earthquake is negatively associated with health human capital (and marginally statistically significant for adult height with p=0.057). Exposure to the *atole* intervention in the first three years of life (relative to *fresco*) is positively associated with the intellectual human capital outcomes, and statistically significant in the reading comprehension skills relationship.[27] That early-childhood nutritional supplementation is most related to one of the intellectual human capital variables (RCS) in these combined male and female regressions, rather than to the health human capital variables suggests the possibility that there may be what in Section 2 we referred to as a "cross effect" of the "health" investment of early life nutrition on wages through adult intellectual human capital, regardless of whether there is a direct "own effect" through health human capital. Being a twin negatively affects all of the outcomes, and is statistically significant in the adult height and FFM relationships. Both mothers' and fathers' schooling attainment positively and significantly associated with the intellectual human capital measures, but not the health capital measures. The logarithm of mothers' height, on the other hand, is highly significant and positively associated with all four measures of human capital. Finally, the set of covariates as a whole explain substantial portions of the variation in each of the human capital measures, 15–22% for intellectual human capital and 61–69% for health human capital. That the first-stage explanatory power is particularly high for the health human capital measures lessens concern that our subsequent findings of insignificant effects for health human capital are due to weak predictions of the endogenous variable.

[26] Father's height is not consistently available in the data but when used (replacing missing observations on father's height with the sample mean) the results are very similar (results not shown).

[27] This is consistent with the reduced-form estimates of a significant impact of receiving *atole* rather than *fresco* of about a quarter of a standard deviation of the RCS score for both men and women in the same sample (Maluccio *et al.* 2009).

Table 3: First-stage predictions of human capital measures (N=1012)

	(I_1) Completed grades of schooling	(I_2) Adult reading comprehension skills (RCS Z score)	(H_1) Ln adult height	(H_2) Ln adult fat-free mass (FFM)
Second-stage controls				
Male (E_1)	0.772	0.174	0.077	0.277
	(0.212)**	(0.067)*	(0.002)**	(0.006)**
Age (A_2)	-0.043	-0.013	-0.000	0.000
	(0.025)	(0.008)	(0.000)	(0.001)
San Juan (E_V)	1.259	0.366	-0.012	-0.035
	(0.342)**	(0.112)**	(0.004)**	(0.012)**
Espiritu Santo (E_V)	2.123	0.296	-0.003	-0.008
	(0.308)**	(0.099)**	(0.003)	(0.010)
Santo Domingo (E_V)	0.749	0.165	-0.005	-0.023
	(0.327)*	(0.101)	(0.003)	(0.009)*
Excluded instruments				
Prices and policies (P_1, P_2)				
Student-teacher ratio at age 7	-0.028	-0.011	-0.000	0.000
	(0.012)*	(0.004)*	(0.000)	(0.000)
Ministry of health post in village at age 2	-0.109	-0.067	0.013	0.040
	(0.399)	(0.122)	(0.005)**	(0.014)**
Born in 2 years prior to 1976 earthquake	-0.530	-0.023	-0.005	-0.016
	(0.346)	(0.081)	(0.003)	(0.010)
Exposure to either intervention 0-36 months	0.013	-0.031	-0.001	0.002
	(0.302)	(0.074)	(0.002)	(0.007)
Exposure to atole intervention 0-36 months	0.361	0.216	-0.000	-0.014
	(0.363)	(0.106)*	(0.004)	(0.012)
Individual endowments (E_I)				
Twin	-1.135	-0.038	-0.033	-0.069
	(0.688)	(0.353)	(0.007)**	(0.024)**
Family endowments (E_F)				
Mother's completed grades	0.453	0.123	0.001	0.001
	(0.072)**	(0.017)**	(0.001)	(0.002)
Ln mother's height	13.131	3.429	0.498	0.886
	(3.423)**	(1.226)**	(0.037)**	(0.094)**
Mother's height missing	-0.061	0.094	0.006	0.009
	(0.219)	(0.054)	(0.003)	(0.008)
Father's completed grades	0.332	0.075	0.000	-0.001
	(0.059)**	(0.014)**	(0.000)	(0.001)
Constant	-60.918	-16.915	2.532	-0.778
	(17.318)**	(6.196)**	(0.189)**	(0.476)
R^2	0.22	0.15	0.61	0.69
F-statistic: excluded instruments	19.1	15.1	24.9	16.4
p-value	[<0.01]	[<0.01]	[<0.01]	[<0.01]

Notes: Ordinary least squares estimates. Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

In addition to these plausible findings regarding the excluded instruments, both the first- and second-stage diagnostics also are generally good. The F tests on excluded instruments are 15 or higher (Bound, Jaeger and Baker 1995). In addition, as shown in Table 4 below, the Kleibergen-Paap statistics for weak instruments appear satisfactory,[28] and the Hansen J tests indicate that the first-stage instruments are not correlated with the second-stage disturbance terms.[29]

Section 4.2 Wage Rate Production Function Estimates

In Table 4, we present the logarithmic wage rate estimates using four specifications: Ordinary least squares (OLS) and IV estimates with the most common intellectual and health human capital measures in the literature, schooling and adult height, and OLS and IV estimates with our preferred measures, adult reading comprehension skills (RCS) and adult fat-free mass (FFM).

OLS yields significantly positive coefficient estimates for both intellectual human capital and health human capital, whether the more common indicators (first column) or our preferred measures (third column) are used. If these estimates represented the causal impact of human capital, they would indicate that for the context studied, there are large and significant positive returns to both types of human capital. The estimated standardized associations (due to a one SD change in the independent variable) on the ln wage rate are 0.28 for schooling attainment, 0.22 for adult RCS, 0.10 for height, and 0.18 for adult FFM. These associational results suggest the possibility that "brains" has a larger effect than "brawn", though both are significant. They also suggest that FFM has a larger association with wage rates than height, which may reflect that it is a better measure of adult work capacity. Finally, they indicate that attained schooling is slightly more predictive of the dependent variable, ln wages, than is RCS, though as noted in Section 2, this may be because schooling is in part a proxy measure for other inputs into intellectual human capital and in part because (random) measurement errors are relatively larger for RCS than for schooling.

Alternative OLS estimates suggest that if only one of the two human capital measures is included, the estimated coefficient on it may suffer from omitted variable bias stemming from the (other) excluded human capital measure. Though we find little change in the coefficient estimates on the intellectual human capital measures if they alone are included, coefficient estimates on the health human capital measures increase by 100% for ln height and 50% for ln FFM if the intellectual human capital variables are not included (estimates not shown). If the true production function included both intellectual and health human capital and the OLS estimates were valid, including

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[28] Using the critical values presented by Stock and Yogo (2005, Table 5.1), with a Kleibergen-Paap test statistic (Kleibergen and Paap 2006; Kleibergen 2007) of 6.23 (or higher) we reject at a 5% significance level the hypothesis that the instruments are weak, where weak in this case means having bias in the IV results that is larger than 20% of the bias in the OLS results. With a test statistic of 10.58 (or higher), we reject at 5% that the bias in the IV results is larger than 10%. To the extent that our estimates are biased, however, conditional on the validity of the excluded instruments they are biased toward the OLS estimate, suggesting that the results we report are conservative and *understate* the differences between OLS and IV. Evidence supporting this is includes the findings when we instead estimate using limited information maximum likelihood (LIML), as suggested by Stock and Yogo (2005). The results of that estimation (not shown) confirm the possible direction of bias—for all coefficients on the intellectual human capital measures we estimate very similar or larger coefficients than in the IV approach, and for all health human capital measures more negative coefficients that remain insignificant.

[29] The Hansen J statistic for overidentification fails to reject the null hypothesis that the overidentifying restrictions are valid (i.e., that the model is well specified and the instruments do not belong in the second-stage equation) at any conventional significance levels. Failure to reject the null hypothesis is evidence that that if any one of the instruments is valid, so are the others. Since the instrument set includes the randomly allocated exposure to the *atole* intervention during the first three years of life and the earthquake indicator, which are both likely to be exogenous, we interpret this as strong evidence of the validity of all the instruments.

only health human capital would result in substantial overestimates of the wage rate impact of health human capital, a finding similar to patterns described in Case and Paxson (2008).

In Table 4, we present the wage production function estimated via via a two-step instrumental variable feasible generalized methods of moments (GMM) (Hayashi 2000; Baum, Schaffer and Stillman 2007). The principal difference from the OLS results is that health human capital, regardless of the indicator and despite its being very well predicted in the first stage (R^2>0.60), no longer has a significant, or even positive, effect on wage rates. While standard errors on the estimated coefficients on height and FFM do increase substantially (and more so for FFM, possibly because it is a predicted measure to begin with and may have greater measurement error), coefficient estimates do not increase as often observed in IV estimates, but rather change signs. Moreover, Hausman tests comparing the estimated coefficients on the human capital variables indicate that three out of four of them are statistically different, at the 10% level. By contrast, both of the intellectual human capital measures remain positive and significant, with substantially larger but less precisely estimated effects (approximately 25% larger for completed schooling and 75% larger for RCS).

For the intellectual human capital measures, the increased coefficient sizes in the IV as compared with the OLS estimates are consistent with the possibility of attenuation bias due to random measurement errors with such errors being relatively larger for RCS than for schooling attainment. This pattern is consistent with the evidence we have on the correlation of repeat measurements for these two variables. But the increases in the estimated coefficients are unlikely to be due to measurement error alone, since that would imply very small reliability ratios (the ratio of the variance of the true measure to the variance of the true measure plus the variance of the measurement error), e.g., on the order of magnitude of 0.7 for completed schooling and 0.5 for RCS. Regardless, what is evident is that factors including the control for random measurement errors that might lead to a higher IV than OLS estimate outweigh any potential positive “ability” or other unobserved endowment bias that might lead to a lower IV than OLS estimate for the intellectual human capital measures. That raises the possibility that there are unobserved factors in the disturbance term of relation (1) that are negatively correlated with the intellectual human capital measures but positively related to wages, or vice versa.

Under the standard interpretation in the literature, the rate of return to schooling is estimated to be 9.8%, similar to the central tendency in the estimates reported in Psacharopoulos and Patrinos (2004), which implies a standardized impact of approximately 0.34, virtually identical to the estimated standardized impact for RCS of 0.35. That these standardized impacts are the same for the two measures of intellectual human capital after IV, in contrast to their having been somewhat different using OLS, is also consistent with a larger noise-to-signal ratio for RCS than for schooling attainment. Despite the theoretical arguments we presented in favor of using the more proximate adult human capital measure (Section 2.1), the estimates using RCS do not yield substantively different findings and, if anything, are less consistent with the variance in the dependent variable than the estimates using schooling attainment. Schooling attainment is a proxy measure for any other correlated inputs into the production of adult cognitive skills in relation (2). If it is sufficiently highly correlated with other inputs, it may be a good indicator for the overall investments in intellectual human capital. However, to the extent that it is a proxy for other investments, even after instrumentation its coefficient estimate may not reflect alone the impact of increasing schooling attainment. Unfortunately we do not have data on what would seem to be many of the other important inputs in relation (2) that would permit the direct estimation of the second-period intellectual capital production function and that could help clarify to what extent schooling is a proxy for such other inputs. Finally, the first-stage estimates in Table 3 indicate significant effects of exposure to the better nutritional supplement when 0–36 months of age on adult RCS. Therefore, our a priori preferred estimates of the wage rate production function in column (4) of Table 4 suggest that early life nutrition does have an impact through adult intellectual human capital, even though we do not find that adult health human capital has a significant contemporaneous impact on adult wage rates.[30]

Table 4: Ln hourly wage rate production functions (N=1012)

	OLS	IV	OLS	IV
Completed grades of schooling (I_1)	0.080	0.098		
	(0.008)**	(0.023)**		
Adult RCS Z score (I_2)			0.215	0.352
			(0.026)**	(0.078)**
Ln adult height (H_1)	1.862	-1.009		
	(0.757)*	(1.909)		
Ln adult fat-free mass (FFM, H_2)			1.054	-0.213
			(0.278)**	(0.864)
Male	0.214	0.414	0.093	0.410
	(0.075)**	(0.145)**	(0.090)	(0.240)
Age	0.013	0.016	0.011	0.017
	(0.006)*	(0.005)**	(0.006)	(0.005)**
R^2/Centered R^2	0.19	0.15	0.16	0.07
Kleibergen-Paap test		14.25		9.01
Hansen J test		2.85		1.95
p-value		[0.83]		[0.92]
Hausman test on I p-value		[0.36]		[0.05]
Hausman test on H p-value		[0.10]		[0.11]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates using instruments and controls indicated in Table 3 (the controls include the village-of-origin dichotomous variables, the coefficient estimates of which are not presented in this table). Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

4.3 Robustness Considerations

Our improving upon much of the previous literature by including measures of both adult proximate intellectual and health human capital, treated as endogenous, requires a number of assumptions. In this subsection, we relax some of these in an effort to explore the sensitivity of our principal finding

[30] This result suggests that this cross effect is underlying the reduced-form estimates using these data that exposure to *atole* (relative to *fresco*) during the entire first three years of life increased male (but not female) hourly wage rates (Hoddinott *et al.* 2008).

that adult intellectual human capital, rather than adult health physical human capital, is the key determinant of hourly wage rates for our sample from Guatemala.[31]

4.3.1 Gender Differences

Adult males have substantially higher wage rates than females (Table 1), and also differ along all of the dimensions of human capital we measure. This raises the possibility that our findings reflect differences between the returns to human capital for men and women, rather than just differences in the returns to different types of human capital. Moreover, while there is substantial overlap in occupational patterns across gender (Section 3.2), there are some important differences. If the occupations in which women more commonly participate are less physically demanding, for example, then the jointly estimated results may mask returns to health human capital for men. Finally, women had lower labor market participation rates. Such differential selection processes also may bias the findings.

We explore how robust the results in Table 4 are to gender differences by estimating the wage relations for men and women separately. This exploration yields the same substantive findings as when we estimate jointly, see Appendix Table 1. After endogenizing them, intellectual human capital measures are statistically significant while physical health human capital measures are not, for both men and women. Estimated returns to intellectual human capital are slightly higher for men, and the point estimates of health human capital for women, in contrast to most other results reported in this paper, are positive, though insignificant. Thus, the inclusion of women does not seem to be masking positive returns to health human capital for men, or vice versa.

4.3.2 Labor market selection and attrition

Despite the considerable effort and success in tracing and re-interviewing participants from the original sample, attrition in our 35-year follow-up is substantial. The estimates presented above are based on selected samples of 1012 individuals (Section 3.2), 42% of the original 2392 sample members or 54% of the 1855 sample members known to be living in Guatemala at the time of the survey. Approximately a fifth of those not in the sample, however, are women who were interviewed in HCS, but were not in the labor market; therefore they are missing due to labor market selection rather than attrition from the survey.[32]

Grajeda *et al.* (2005) demonstrate that the overall attrition in the sample is associated with several initial conditions and background characteristics. What is of ultimate concern in this analysis, however, is not the level of attrition and labor market selection, but whether these phenomena

[31] In addition to the robustness considerations reported in this section, excluding 50 individuals reporting unusually large numbers of hours of work (more than 12 hours a day for 365 days) strengthens further the main findings, in particular stronger rejection (at the 5% level) of the Hausman tests of significant differences between OLS and IV estimates. Such reports tend to be for those with small stores selling basic goods out of their home that are essentially open all the time.

[32] Another possible problem related to attrition in the sample is that of mortality selection (Pitt and Rosenzweig 1989; Ahn and Shariff 1995; Pitt 1997). Indirect evidence that mortality selection exists in the sample is that risk of death is associated with younger ages in the complete sample of 2392. These represent the survivors of their respective birth cohorts, and hence they experienced a lower mortality rate (most of which is driven by infant mortality) compared with the later birth cohorts in the study 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 villages born between 1962 and 1969 who died before the start of the survey. Moreover, the intervention has been shown to have decreased mortality rates among the younger sample members (Rose, Martorell and Rivera 1992). To the extent the variables included in our models are associated with this form of selection, our estimates partly control for mortality selection, though we do not implement any additional methodology to do so beyond the controls for selection described in this section.

invalidate the inferences we make using the resulting selected sample. For example, does excluding individuals who died in infancy and early childhood, national and international migrants, or women not in the labor force, all of whom may have different characteristics, lead to systematic bias of the estimates presented here?

One feature of our IV strategy not emphasized above is that it also may serve in part to identify selection into the labor market. For example, if more educated women also select into the labor market, we would have a standard selection problem if estimating only on the working population (this is not a first-order concern for men given their 98% participation rate). Since our instrument set is also plausibly unrelated to labor market participation except through its influence on human capital, in particular indicators of the randomized nutritional intervention, the potential correlation between the human capital measures and the labor force participation selection term may be broken.

To explore the sensitivity of our results to attrition, we implement the correction procedure for selective attrition on observed characteristics outlined in Fitzgerald, Gottschalk and Moffitt (1998a, 1998b). We first estimate an attrition probit for all original sample members (N=2392) conditioning on all the exogenous right-side variables (including instruments but not including the human capital measures that are only observed for those re-interviewed) considered in the main models, as well as an additional set of variables potentially associated with attrition. The latter variables include ones that reflect family structure in previous years because these are likely to be associated with migration status: whether individuals lived with both their parents in 1975 and, separately, in 1987. During the field work, locating sample members was 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: 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 HCS survey, and the logarithm of the number of siblings in the sample in each family. While we do not have explicit adjustments to correct for selection on unobservable characteristics, by including a number of endogenous observables indicated above, which are likely to be correlated with unobservables, we are reducing the scope for attrition bias due to components of unobservables that are correlated with the observed variables, as well.

The factors described above are significant and highly associated with attrition, above and beyond the conditioning variables already included in the models. Following Fitzgerald, Gottschalk and Moffitt (1998a), we construct weights that give greater weight to observations in the sample re-interviewed in 2002–04 that had lower predicted probabilities of having been re-interviewed. The application of these weights affects only slightly the results and the central patterns of the coefficient estimates on intellectual human capital measures are very similar to those in Table 4, and the coefficient estimates on health human capital remain insignificant (Appendix Table 2). We interpret these findings to mean that, as found in other contexts with high attrition (Fitzgerald, Moffitt and Gottschalk 1998a, Alderman *et al.* 2001, Behrman, Parker and Todd 2009), including other analyses with these data (Hoddinott *et al.* 2008, Behrman *et al.* 2009, Maluccio *et al.* 2009), our results do not appear to be driven by attrition biases.

4.3.3 Alternative selected samples

To assess further how sensitive the results reported above are to sample selection, we consider two alternative samples based on the larger study of which HCS was a part. A finding of similar patterns for differentially selective samples of the same individuals and, separately, their parents, provides additional evidence that the findings are not due primarily to selection.

In 2007–08, as part of a study focused on intergenerational relationships among individuals interviewed in HCS, their parents, and their children, we fielded a further follow-up survey, referred to as the "Intergenerational Transfers Study" (IGT) (Melgar *et al.* 2008). IGT administered an identical economic activity module to a subsample of the same individuals examined above, as well as to their parents. Over one-half of the individuals interviewed in HCS were re-interviewed in IGT, which targeted those living in *El Progreso*, the municipality where the original study villages are located or in Guatemala City and who had at least one living parent. The selection process for this subsample, therefore, is different from that of the main sample used in this paper. These interviews provided all the necessary information to calculate the dependent variable examined above, hourly wage rates, approximately 3–4 years later. Apart from verification of completed grades of schooling, however, measures of intellectual and health human capital were not updated. As a result, while these data do not enable panel data estimation they do permit a re-estimation of the basic specifications above on a further selected subset of the sample, using the same set of right-side controls but outcome variables measured 3–4 years later. An obvious advantage to using these data is that they further break the possible simultaneous relationship between measures of human capital that may vary over the short term, including both RCS and FFM. Results using the nearly 600 individuals re-interviewed in 2007–08 (a 59% subsample of the main sample we use) are very similar, both in the general pattern and the specific magnitudes of the estimated effects, to those presented in Table 4 (Appendix Table 3).

The second alternative sample we consider also is based on the 2007–08 IGT and comprises the parents of those individuals in the sample used for the rest of this study. The parental sample allows us to explore whether similar patterns exist presently for a selected older sample who had been living in the original four villages in the 1970s and now live in the same municipality or in Guatemala City. For the parents, we have only the more typical measures of intellectual and health human capital, school grades completed and height. We also only have available a more limited set of instruments with which to endogenize these human capital measures (in particular because the nutritional intervention did not affect them as children). Given these caveats, and with weaker first-stage results, we find that OLS and IV estimates of the relationship between health and intellectual human capital and hourly wages measured in 2007–08, when the parents were between 50 and 80 years of age, mimic the patterns described throughout, particularly for men (Appendix Table 4).

4.3.4 Alternative income measures

The calculation of income from own-account agricultural and non-agricultural activities described in Section 3.3.1 is potentially subject to systematic measurement error. For example, because we do not have details on the cost of inputs such as fertilizer and pesticides used for crop production, hourly wage rates from agriculture are likely to be slightly overestimated. To the extent that health human capital is more important for such activities, then, we might expect upward bias on the estimated effects of health human capital. The principal findings that health human capital does not increase wages suggest such potential upward bias is not a major concern. Nevertheless, we consider two alternative calculations of hourly wage rates.

For both own-account agricultural and non-agricultural activities, individuals also were asked for an estimated replacement wage, that is, how much it would cost to hire someone else "with the same level of responsibility and experience to do the same job." Using these reported replacement wages, we recalculate average hourly wage rates for all individuals. This measure addresses the implicit assumption of equal productivity for all the unpaid workers in an activity, for example in farm production, when we calculate returns to agricultural (and, correspondingly, non-agricultural) own-account activities. Results using this alternative measure of wages confirm those in Table 4—only adult intellectual, and not health, human capital are positively and significantly related to hourly wage rates. Magnitudes of the effects of intellectual human capital are similar to those shown in Table 4, but in general about 10% smaller when these replacement wage rates are used (Appendix Table 5).

The second alternative calculation of hourly wage rates addresses the possibility that agricultural and non-agricultural income are likely to be overstated without a careful solicitation of the costs for all inputs. For this calculation we uniformly reduce income from those sources by 25%, recalculating total labor income and consequently hourly wage rates accordingly. Consistent with the possibility of upward bias on health human capital measures posited above, results using this measure yield slightly larger returns to intellectual human capital and slightly smaller (i.e., more negative, though still statistically insignificant) returns to health human capital. In general, however, the results are not substantively different from those shown in Table 4 (Appendix Table 5).

4.4 Why are There no Apparent Returns to Brawn (or Adult Health Human Capital)?

At first glance, our findings appear to contradict earlier analyses, in both developing and developed economies, that find positive significant returns to health human capital. There are several possible explanations for the differences, however. First, identification strategies differ across these studies and may underlie in part the contrasting findings. Much of the earlier work relies on differences in prices to endogenize the health human capital or intake variables. Moreover, not all of that work finds positive effects of height after it is endogenized (e.g., Schultz 2003). Unfortunately it is not possible with available data to directly test whether differences in identification strategies underlie differences in findings. Another and possibly more important difference between our studies and earlier work in developing countries is that we examine the Guatemalan labor market in the early 2000s. Though still with substantial poverty, Guatemala has seen significant economic growth and stability, particularly since the signing of the Peace Accords in 1996, and particularly in areas close to Guatemala City where the vast majority of our sample members reside. Much of this growth has been away from agriculture, as reflected in our survey in which respondents rely only in small part on agriculture. This is also seen upon examination of an inventory of the types of jobs reported by respondents. Is it just the case that so few of the jobs carried out in our sample are physically demanding?

We explore this issue by re-estimating our analyses on select subsamples for which we expect health human capital on the job to be more or less important. It would seem likely that if there is a return to health human capital in the labor market, we would be more likely to observe it in those sorts of occupations one might traditionally think of as requiring more “brawn.” As part of the physical fitness module administered in HCS, we asked respondents the following question for their first two principal occupations: “In your work, are there physical exertions or activities that make your heart beat more rapidly or for which you have to breathe more heavily?” 20% reported that this was the case. In Appendix Table A6, we present results for subsamples based on these two subsamples who self-reported working in more or less physically demanding jobs. Mindful that these estimates are based on selected samples, and that we have fewer than 200 individuals who self-report physically demanding work, several interesting patterns emerge. In OLS estimates, there are positive correlations between wages and both intellectual and health human capital no matter the job type. In the IV estimates, point estimates of the return to intellectual human capital decrease and returns to health human capital increase for the subset of more physically demanding jobs.[33] Also for more physically demanding jobs there is a negative relationship with age. By contrast, for less physically demanding jobs we find results similar to those found for the overall sample. We draw three conclusions from this analysis. First, it is possible that for a small subset of the sample there are positive returns to health human capital. Second, our identification strategy appears capable of uncovering such returns. And last, the principal finding of insignificant returns to health human capital for the overall sample may be partly due to the occupational composition in the sample, where only a small portion of the jobs appear to be physically demanding.

4.5 What Explains the OLS Patterns of "Returns" to Health Human Capital?

In the introduction we outlined two arguments made in the literature as to why wages and height are highly correlated in developed economies. Case and Paxson (2008) present evidence that a substantial portion of the association is due to childhood cognitive ability. A priori, our findings are consistent with those of Case and Paxson (2008) because we include adult measures of cognitive skills and our IV approach uses instruments that are likely to capture childhood development and intergenerationally transmitted abilities.

While our framework differs in that in contrast to their work, which includes a rich set of parental background controls, we condition on adult intellectual human capital in the form of attained schooling or RCS (and when we do so OLS "returns" to height decrease substantially as described in Section 4.2), for a selected subsample we can examine whether adult health human capital largely reflects childhood cognition by directly including controls for two early childhood measures of development: height-for-age Z scores at age 6 and age-specific standardized cognitive skills tests (Pollitt *et al*. 1991). Inclusion of these measures, as well as various combinations and interactions among them, however, has little influence on the point estimates for height or fat-free mass, decreasing them by at most 10% (results not shown).

We also can examine partially the hypothesis stemming from Persico *et al*. (2005), recognizing that the context for social interactions in 1980s and early 1990s Guatemala is quite different from the U.S., in part because of limited secondary school attendance. As part of the on-going longitudinal study, respondents were measured in a survey in 1988. For the subsample of those between 15–18 in 1988 for whom we have measured heights, we include this early measure of height in the OLS model (in addition to adult height) finding that, if anything, it is negative and insignificant and its inclusion increases the association between wages and achieved adult height.

Lastly, we considered two other measures in 2002 to explore whether adult health human capital may be a proxy for them. First, we examined an indicator of group memberships (as well as the number of groups) as a proxy measure for social capital. Separately, we examined a measure of self-esteem, derived from a set of questions on how individuals deal with difficult situations, whether they are generally optimistic, and so on. While both of these measures had strong and significant positive associations with wages, neither of them substantially reduced the association between adult health human capital in the OLS wage production function, with only minor (<10%) reductions in the estimated coefficients on height and fat-free mass.

[33] For this sub-sample, these results are consistent with those of Schultz (2002, 2003) in which he finds for some countries that the return to height increases after instrumentation of height.

Having failed in the above explorations to find a “smoking gun”—some factor for which height is plausibly standing in for in OLS estimations of the wage relation—we turn to an alternative explanation, motivated by field observations that in Guatemala and elsewhere, taller youths often begin work earlier. HCS included a retrospective module on an individual’s first paid job after the age of 15. The vast majority of such jobs were casual laborers, and most of the sample reported having had a job for pay by age of 20. While unlike the wage measures used in other parts of this paper this does not include imputed wages for unpaid labor in own-agriculture and business, these data provide the opportunity to examine the role of height at a point in the past, i.e., at first entry into the labor market. For this analysis we use only the (adult) height measures. Although we do not observe height at the time of labor market entry, since adolescent and adult height are highly correlated adult height serves as a good proxy variable.

Table 5: Age and ln wage at first job

	Age at first job		Ln wage from wage labor only in 2002		Ln wage from first paid job	
	OLS	IV	OLS	IV	OLS	IV
Completed grades at age 13 (I_1)	0.131	0.507			0.047	0.183
	(0.051)*	(0.196)**			(0.015)**	(0.046)**
Completed grades of schooling (I_1)			0.082	0.103		
			(0.008)**	(0.016)**		
Ln adult height (H_1)	-1.870	-15.520	1.677	-2.122	-0.624	-4.562
	(3.120)	(9.594)	(0.607)**	(1.695)	(0.778)	(1.713)**
Male	-1.036	-0.218	0.189	0.481	0.372	0.585
	(0.330)**	(0.705)	(0.067)**	(0.150)**	(0.087)**	(0.142)**
Age	0.083	0.098	0.021	0.017	0.057	0.067
	(0.023)**	(0.022)**	(0.007)**	(0.006)**	(0.009)**	(0.009)**
R^2/Centered R^2	0.06	-	0.27	0.23	0.09	0.01
Kleibergen-Paap test		10.34		7.90		10.30
Hansen J test		7.54		11.90		8.19
[p-value]		[0.48]		[0.16]		[0.41]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates using instruments and controls indicated in Table 3 (the controls include the village-of-origin dichotomous variables, the coefficient estimates of which are not presented in this table). Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown

We first examine age at first paid wage job using the same wage production function framework (but using grades completed by age 13) (Table 5, columns 1 and 2). In the IV results, both attained schooling at age 13 and adult height have significant (the latter marginally so at 10%) associations with age at first paid job. Individuals who are taller as adults started working for pay earlier. What effect, then, does this have on their first wage? To examine this, we first verified that the same patterns for intellectual and health human capital reported throughout the paper hold when we limit measured wages to those from wage labor only, using the 2002 HCS data (Table 5, columns 3 and 4). Then, using annual CPI information for Guatemala, we inflated all wages for first job (based on the year of entry into the labor market) to 2002 values to estimate the wage production function for first paid wage. In the OLS estimation, based on 933 (of the 1012) observations with complete information, adult height is negatively related to first wage. When we instrument using the same strategy as in Table 4, this negative relationship is strengthened and shows a highly significant negative effect of height on first paid wage. The evidence suggests, therefore, that taller Guatemalans start working for pay at a younger age and, in doing so, earn lower first wages. In starting earlier they have greater experience and thus apparently higher returns in the cross section are associated with their height. For the IV results to make sense, however, it must be that height is negatively associated with wage growth over time, and this is what we find in an OLS regression of average wage growth from first wage to present (combined) wage (results not shown).

5. The Effect of Intellectual and Health Human Capital on Annual Hours Worked and Total Annual Income

Having established that brains, but not brawn, is the key wage rate determinant, we last explore whether and how these two different forms of human capital affect annual hours worked and, the combination of wage rates and hours, total annual income.

The left-hand panel of Table 6 presents the estimates for the logarithm of total annual hours. Without accounting for the endogeneity of the human capital measures, the OLS estimates show that both intellectual and health human capital have positive associations with total hours worked and that these are significant for both intellectual human capital measures and marginally significant for one health human capital measure, height (p=0.08). As with the hourly wage production function, the first- and second-stage diagnostics for the excluded instruments are generally good. The second-stage results largely mimic those for wage rates. In the IV estimates both of the measures of intellectual human capital have positive, substantial, and statistically significant effects on hours worked. For example, an increase by one grade of completed schooling (a little more than one fourth of a SD) leads to an approximate 11% increase in annual hours worked, and a one-quarter SD increase in adult RCS has a roughly similar effect. The IV estimates are substantially larger than the OLS counterparts, in part likely due to attenuation bias from random measurement error as in the wage rate estimates in Table 4. Taken together with the estimates in Table 4, these estimates imply that an increase by one grade of completed schooling leads to an approximate 20% increase in annual income and a one-quarter SD increase in adult RCS again has a roughly similar effect. By contrast, the estimated IV coefficients on each of the health human capital measures is negative and insignificant, indicating that similar to wages it is not random measurement error alone that underlies the differences in estimated coefficients. These findings are confirmed in the right hand panel of Table 6, where, for completeness, we present the results for the logarithm of total annual income.

Table 6: Ln annual hours and total income production functions (N=1012)

	Ln Annual Hours				Ln Total Annual Income			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Completed grades of schooling (I_1)	0.025	0.128			0.105	0.214		
	(0.009)**	(0.030)**			(0.012)**	(0.040)**		
Adult RCS Z score (I_2)			0.079	0.423			0.294	0.766
			(0.035)*	(0.103)**			(0.045)**	(0.143)**
Ln adult height (H_1)	1.879	-4.078			3.741	-3.737		
	(0.820)*	(2.195)			(0.926)**	(2.414)		
Ln adult fat-free mass (FFM, H_2)			0.721	-1.174			1.775	-1.279
			(0.374)	(1.015)			(0.454)**	(1.179)
Male	0.881	1.262	0.832	1.331	1.095	1.599	0.925	1.718
	(0.080)**	(0.172)**	(0.119)**	(0.285)**	(0.112)**	(0.212)**	(0.156)**	(0.346)**
Age	0.009	0.018	0.009	0.022	0.022	0.027	0.020	0.030
	(0.007)	(0.007)**	(0.007)	(0.007)**	(0.010)*	(0.009)**	(0.010)	(0.010)**
R^2/Centered R^2	0.21	0.12	0.21	0.12	0.29	0.23	0.28	0.18
Kleibergen-Paap test		12.90		8.58		12.90		8.58
Hansen J test		5.38		7.42		4.20		5.82
[p-value]		[0.61]		[0.39]		[0.76]		[0.56]
Hausman test on I [p-value]		[<0.01]		[<0.01]		[<0.01]		[<0.01]
Hausman test on H [p-value]		[<0.01]		[0.04]		[<0.01]		[<0.01]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates using instruments and controls indicated in Table 3 (the controls include the village-of-origin dichotomous variables, the coefficient estimates of which are not presented in this table). Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

6. Conclusions

In both developing and developed countries, evidence exists that health human capital (brawn) as represented by adult height has significant associations with wage rates, even after controlling for intellectual human capital (brains) as usually represented by completed grades of schooling or less commonly, by adult cognitive skills. But much of this literature is problematic. First, brains and brawn are usually treated as statistically predetermined rather than outcomes determined by dynamic investments made in the presence of persistent endowments and simultaneous feedbacks. Second, indicators of both brawn and brain may have considerable random or other measurement error. Third, height and completed grades of schooling relate to investments in human capital in childhood and adolescence, but in general might combine with other inputs such as adult nutrition and work experience to produce adult human capital. Fourth, early life events such as nutritional status as a pre-schooler may have impacts on adult labor income not just through adult health attributes such as strength, but also through intellectual human capital.

We explore these issues using rich longitudinal data collected over 35 years in Guatemala, a market environment with a substantial proportion of skilled labor but also with substantial unskilled manual labor for which health attributes would seem likely to have productivity and labor-market rewards. We find that adult height and adult fat-free mass both have significantly positive associations with wage rates, annual hours worked and annual income in OLS estimates, but that the returns to these characteristics disappear when health human capital is treated as endogenously determined. Indicators of intellectual human capital also have significant positive associations with the wage outcomes we examine. The magnitudes of their impact increase when these are treated as endogenous, considerably so for reading comprehension skills. These results are robust to a number of different specification changes, including controlling for attrition and sample selectivity. There are significantly positive and substantial proportional effects on wage rates, hours worked and annual income for males but, beyond this, no evidence of significant differences by gender. We present evidence suggesting that the positive association between height and wages among adults stems from the pattern that taller individuals entered the labor market at younger ages. We note that in our sample, a small number of individuals' report that they work in physically demanding occupations. For these individuals, returns to health human capital persist even when health human capital is treated as endogenously determined. While our results are necessarily context specific, as the distribution of income generating activity in developing countries shifts towards those requiring intellectual human capital, they may well be more widely generalizable.

Do these results imply that investments intended to improve early childhood nutrition—believed to be a major determinant of adult height—do not yield private returns in the labor market? No. Elsewhere, we demonstrate that early childhood nutrition is causally linked to adult cognitive skills (Maluccio *et al.* 2009). Given the high returns to these cognitive skills in the labor market, these results *strengthen* the economic argument in favor of investments in early childhood nutrition. The critical pathway, however, does not appear to be through improving adult health capital but rather through improving adult intellectual human capital.

Appendix Table 1: Ln hourly wage rate production functions, by sex

	Male (N=546)					Female (N=466)			
	OLS	IV	OLS	IV		OLS	IV	OLS	IV
Completed grades of schooling (I_1)	0.089	0.110				0.068	0.079		
	(0.010)**	(0.025)**				(0.015)**	(0.026)**		
Adult RCS Z score (I_2)			0.236	0.379				0.185	0.374
			(0.033)**	(0.087)**				(0.049)**	(0.111)**
Ln adult height (H_1)	1.809	-1.594				1.975	1.461		
	(0.892)*	(2.067)				(1.189)	(2.914)		
Ln adult fat-free mass (FFM, H_2)			1.514	-0.908				0.430	0.407
			(0.371)**	(1.127)				(0.415)	(1.633)
Age	0.012	0.011	0.008	0.011	0.013	0.013	0.015	0.020	0.030
	(0.008)	(0.006)	(0.008)	(0.006)	(0.010)	(0.009)	(0.010)	(0.009)*	(0.010)**
Kleibergen-Paap test		6.86		4.88			10.10		4.33
Hansen J test		10.89		10.70			6.31		5.16
[p-value]		[0.14]		[0.15]			[0.50]		[0.64]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates using instruments and controls indicated in Table 3 (the controls include the village-of-origin dichotomous variables, the coefficient estimates of which are not presented in this table). Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

Appendix Table 2: Attrition weighted ln hourly wage rate production functions (N=1012)

	OLS	IV	OLS	IV
Completed grades of schooling (I_1)	0.077	0.103		
	(0.008)**	(0.024)**		
Adult RCS z score (I_2)			0.199	0.355
			(0.028)**	(0.078)**
Ln adult height (H_1)	1.937	-1.595		
	(0.808)*	(1.984)		
Ln adult fat-free mass (H_2)			1.082	-0.702
			(0.271)**	(0.954)
Male	0.217	0.475	0.097	0.569
	(0.075)**	(0.145)**	(0.086)	(0.258)*
Age	0.013	0.015	0.011	0.016
	(0.007)	(0.005)**	(0.007)	(0.006)**
Kleibergen-Paap test		10.95		7.91
Hansen J test		9.14		10.11
[p-value]		[0.33]		[0.26]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates using instruments and controls indicated in Table 3 (the controls include the village-of-origin dichotomous variables, the coefficient estimates of which are not presented in this table) and weights as described in text. Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

Appendix Table 3: Ln hourly wage rate production functions for select sample of HCS respondents re-interviewed in 2007-8 as part of IGT study (N=599)

	OLS	IV	OLS	IV
Completed grades of schooling (I_1)	0.070	0.105		
	(0.009)**	(0.027)**		
Adult RCS Z score (I_2)			0.197	0.427
			(0.033)**	(0.094)**
Ln adult height (H_1)	1.414	-1.762		
	(0.674)*	(1.973)		
Ln adult fat-free mass (FFM, H_2)			0.513	-0.610
			(0.266)	(0.935)
Male	0.365	0.591	0.360	0.656
	(0.067)**	(0.146)**	(0.090)**	(0.266)*
Age	0.008	0.017	0.008	0.020
	(0.007)	(0.006)**	(0.007)	(0.006)**
Kleibergen-Paap test		7.39		6.12
Hansen J test		6.17		3.69
[p-value]		[0.63]		[0.88]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates using instruments and controls indicated in Table 3 (the controls include the village-of-origin dichotomous variables, the coefficient estimates of which are not presented in this table). Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

Appendix Table 4: Ln hourly wage production functions
Parents of respondents, interviewed in IGT

	Combined Male and Female (N=367)		**Males (N=200)**		**Females (N=167)**	
	OLS	IV	OLS	IV	OLS	IV
(I_1) Completed grades of schooling	0.117	0.427	0.114	0.350	0.132	0.296
	(0.027)**	(0.204)*	(0.032)**	(0.151)*	(0.050)**	(0.659)
(H_1) Ln adult height	1.626	7.956	3.575	5.873	-0.484	4.092
	(1.558)	(16.190)	(1.777)*	(12.09)	(2.747)	(44.98)
Male	0.003	-0.718				
	(0.168)	(1.193)				
Age	-0.035	-0.015	-0.035	-0.022	-0.028	-0.023
	(0.008)**	(0.015)	(0.008)*	(0.013)	(0.012)*	(0.053)
Kleibergen-Paap test		1.1		1.4		0.2
Hansen J test		1.8		1.7		1.3
p-value		[0.61]		[0.63]		[0.73]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates. Controls not shown include a constant and village fixed effects. Excluded instruments predicting completed schooling and ln height include mother and father's education, indicators of whether mother and father were alive and an indicator of whether there was a toilet in the house, all three at the time the individual was 12 years of age. Heteroskedasticity-robust standard errors shown in parentheses. * indicates significance at 5% and ** at 1%.

Appendix Table 5: Ln hourly wage rate production functions, alternative wage measures (N=1012)

	Replacement agricultural and non-agricultural wages				Adjustments to agricultural and non-agricultural income			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Completed grades of schooling (I_1)	0.067	0.085			0.083	0.107		
	(0.008)**	(0.020)**			(0.009)**	(0.023)**		
Adult RCS Z score (I_2)			0.182	0.307			0.224	0.390
			(0.024)**	(0.069)**			(0.028)**	(0.080)**
Ln adult height (H_1)	0.863	-1.780			1.804	-1.301		
	(0.553)	(1.549)			(0.751)*	(2.043)		
Ln adult fat-free mass (FFM, H_2)			0.849	-0.876			1.021	-0.598
			(0.207)**	(0.826)			(0.280)**	(0.995)
Male	0.323	0.511	0.175	0.624	0.299	0.521	0.183	0.597
	(0.058)**	(0.120)**	(0.068)*	(0.228)**	(0.075)**	(0.153)**	(0.089)*	(0.273)*
Age	0.013	0.015	0.012	0.017	0.010	0.013	0.008	0.014
	(0.006)*	(0.006)**	(0.005)*	(0.006)**	(0.006)	(0.005)*	(0.007)	(0.006)*
Kleibergen-Paap test		13.49		8.74		12.99		8.57
Hansen J test		6.66		6.30		7.64		8.21
[p-value]		[0.47]		[0.51]		[0.37]		[0.31]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates using instruments and controls indicated in Table 3 (the controls include the village-of-origin dichotomous variables, the coefficient estimates of which are not presented in this table). Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

Appendix Table 6: Ln hourly wage rate production functions, more or less physically demanding jobs

	More physically demanding jobs from self-report (N=196)				Less physically demanding jobs from self-report (N=808)			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Completed grades of schooling (I_1)	0.041	-0.052			0.087	0.095		
	(0.025)	(0.065)			(0.009)**	(0.020)**		
Adult RCS Z score (I_2)			0.117	-0.321			0.229	0.399
			(0.082)	(0.227)			(0.030)**	(0.083)**
Ln adult height (H_1)	2.211	3.079			1.678	-0.958		
	(1.748)	(4.132)			(0.825)*	(1.907)		
Ln adult fat-free mass (FFM, H_2)			1.102	3.345			1.051	-0.195
			(0.828)	(2.289)			(0.334)**	(0.995)
Male	0.306	0.349	0.199	-0.183	0.214	0.399	0.082	0.398
	(0.153)	(0.311)	(0.238)	(0.544)	(0.084)*	(0.149)**	(0.113)	(0.286)
Age	-0.004	-0.016	-0.005	-0.022	0.017	0.019	0.015	0.021
	(0.014)	(0.013)	(0.013)	(0.013)	(0.007)*	(0.006)**	(0.007)	(0.007)**
Kleibergen-Paap test		1.58		1.50		11.83		7.10
Hansen J test		10.13		8.31		5.97		4.35
[p-value]		[0.18]		[0.31]		[0.65]		[0.82]

Notes: Ordinary least squares and instrumental variables two-step feasible IV-GMM estimates using instruments and controls indicated in Table 3 (the controls include the village-of-origin dichotomous variables, the coefficient estimates of which are not presented in this table). Standard errors calculated allowing for clustering at the birth year-village cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

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