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

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*Abstract: Previous studies report that adult height has significant associations with wages even controlling for schooling. 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 body 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. Traditionally, the emphasis in examining such returns has been placed on intellectual human capital, individuals' knowledge and cognitive skills that contribute to their economic productivity. In much of this literature, intellectual human capital is represented by completed grades of schooling.[1] A second strand in the literature, particularly for developing economies, has focused on the effects of physical or health human capital, individuals' capacity to undertake manual labor. Often height is used to represent health human capital, based on evidence that taller individuals are usually stronger 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 for social skills attained in adolescence, cognitive ability, social class, or general healthiness.[2]

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

<u>Our first contribution</u> is to include direct proximate measures of both adult intellectual human capital (adult cognitive skills) and adult health human capital (fat-free body mass). Much of the extant literature uses measures such as schooling attainment[3] and height that are largely determined in childhood and adolescence and therefore are likely to be biased as measures of adult human capital for the following reasons.

First, these indicators do not fully capture even the human capital investments in childhood and adolescence. For intellectual human capital production in childhood and adolescence, for example, time in school is only one input. In addition family characteristics, child innate abilities and school qualities are likely to be important. While these other inputs are likely to be correlated with

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

[2] Persico, Postlewaite and Silverman (2004) argue that "It's all in teen height." After controlling for height in adolescence, they find no return to adult height for income. They posit that individuals who are taller during adolescence are more likely to participate in a range of clubs and social activities and that the skills acquired through these activities are rewarded in labor markets. These mechanisms appear not to play much of a role in developing countries. Case and Paxson (2008) argue that the height premium is due to the positive association between height and cognitive ability, with the latter being rewarded in labor markets. Steckel (2009) notes that an association between height and wages can exist if there is discrimination against short individuals; this might reflect the perceived associations between height and social class or work capacity, for example. Lundborg, Nystedt and Rooth (2009) cite bio-medical studies linking height to physical strength which is in turn linked to lower risk of premature death. They posit that height could be used by employers as a signal of an individuals' healthiness.

[3] Some studies in the economics literature incorporate adult cognitive skills to represent adult intellectual human capital, so this critique does not apply to the subset of these studies. 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), Case and Paxson (2008), as well as others summarized in Hanushek and Wößmann (2008). These studies do not, however, also include adult health human capital.

schooling attainment,[4] the correlations are not likely to be perfect so schooling attainment generally does not represent fully childhood and adolescent human capital.

Second, there are likely to be investments in both intellectual and health human capital after childhood and adolescence. Within dynamic models of human capital accumulation, of course, in part these investments are conditional on (and therefore correlated with) previous adolescent human capital stocks. The higher are these correlations, the better indicators of adolescent human capital stocks are likely to represent variations in adult human capital stocks.[5] But these correlations are unlikely to be perfect. Other factors including shocks experienced in adulthood, and not only adolescent human capital stocks, affect adult human capital stocks. Learning occurs through work and other experiences affected by shocks in adulthood beyond those perfectly correlated with adolescent schooling. Similarly, nutritional investments[6] and health shocks that are not correlated with height may occur after late adolescence.

Our second contribution is to treat both intellectual and health human capital as behaviorally determined and measured with error in the estimation of wage equations. There are hundreds of 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 micro studies that treat adult cognitive skills as endogenous and measured with error.[7] For health human capital, short-run nutritional status (such as the body mass index) generally is treated as endogenous, but longer-run nutritional status as typically represented by height usually is treated as predetermined.[8] Therefore, as Strauss and Thomas (2008) note, interpretation of coefficients on human capital in wage equations is "plagued by potential bias due to unobserved heterogeneity" (p. 3382) correlated with adult stocks of intellectual and health capital.[9]

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 longitudinal information over long periods of the life cycle reaching back to the details of childhood. Therefore, estimates may confound the effects of long-run persistent unobservables, such as individual and other endowments, with those of observed

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

[5] But, again, 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 include in part the impacts of the subsequent correlated investments.

[6] Examples of studies that treat as endogenous short-run adult nutrition 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 nutritional inputs reflect, inter alia, current prices and resources and not just adult height.

[7] Among the studies cited in footnote 3, only Alderman *et al.* (1996) and Behrman, Ross and Sabot (2008) treat adult cognitive skills as endogenous.

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

[9] A selected list of studies that find that such factors are important includes 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).

schooling, height or other representations of intellectual or health human capital, all further contaminated by measurement error. 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 measurement error, and therefore obtain 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 capital in labor markets may be considerable as occupations vary substantially from, for example, government salaried work to own-account agriculture. Section 2 outlines our conceptual framework, a production function for adult wages dependent on endogenous adult intellectual and health human capital based on investments over the life cycle. Section 3 describes the data we use that have been collected over 35 years and 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 rather than just childhood and adolescent determined schooling and height. 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 taken into account. By contrast, when adult health human capital is treated endogenously, it has no effect on wages. 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, 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 (childhood and adolescence) and t = 2 for the second period (adulthood). 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 ($A_2$), individual endowments ($\mathbf{E_I}$, including genetic endowments, one of whose elements is sex), 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 body 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 ($A_2$), individual endowments ($\mathbf{E_I}$), village-of-origin endowments ($\mathbf{E_V}$) and stochastic disturbance terms ($U_{2I}$, $U_{2B}$) 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 both forms of second-period human capital. In addition first-period human capital measures, however, there are other choice variables ($\mathbf{X_1, X_2}$)—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, in addition to directly affecting second-period wage rates in relation (1). Individual endowments in addition to sex include, for example, factors such as innate abilities and innate healthiness that are not likely to be observed in most data sets. 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 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[10] 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).[11] For the first-period investments, if there is risk neutrality 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 interpreting previous estimates, as well as for our estimation strategy.

(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 that also affect directly these human capital stocks as in relations (2) and (3), and (6) and (7). 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), but much less so with regard to estimates of the impact of measures of longer-run health capital on wages (Schultz 1997).

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[10] 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 traits), a possibility we consider below in our empirical work.

[11] These reduced-form disturbance terms incorporate the disturbances for the production of both forms of human capital as well as other disturbances that the household experiences.

(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[12] instruments for the included intellectual human capital, the estimates of the impact of this variable are likely to be biased since the omitted health human capital may be correlated with instrumented intellectual capital because they both depend on the same set of variables (relations 6 and 7).

(3) In most of the limited literature that does include both intellectual and health human capital variables, as well as in most of the literature that includes only intellectual human capital, the empirical representations of these variables 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 these variables if in fact they 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 they are likely to be correlated with $I_1$ and $H_1$, are excluded from the specification. If $I_1$ and $H_1$ are highly correlated with the omitted $\mathbf{X_1}$ and $\mathbf{X_2}$ variables, moreover, 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 $H_1$ proxy well for the omitted variables.

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

(5) 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, there is the possibility of attenuation bias.

(6) The framework points to potential instruments that can be used in IV estimates of relation (1) to control for endogeneity of human capital and random measurement errors in measure human capital, namely the right-side variables in the reduced-form demand relations (6) and (7) that do not appear 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 the next section).

## 3. Data

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[12] "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 with the correct specification that includes both forms of human capital.

The data demands are considerable for estimating the adult wage production function posited in Section 2, in which the relevant human capital stocks are treated as endogenous, reflecting decisions back to infancy. We use an unusually 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 at most 7 years old) and policy and market changes over time.

### 3.1 The Institute of Nutrition of Central America and Panama (INCAP) Longitudinal Data[13]

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 (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 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 dietary 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.

In 2002–04, a team of investigators, including the authors of this paper, undertook a follow-up survey targeted towards all participants in the 1969–77 survey, called the Human Capital Study (HCS).[14] At that time, sample members ranged from 25 to 42 years of age. Of the 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, 1113 lived in the original villages, 155 lived in nearby villages, 419 lived in or near Guatemala City, and 168 lived elsewhere in Guatemala (Grajeda *et al.* 2005).

In the main analyses, we focus on the 962 respondents (54% male) interviewed in the 2002–04 HCS for whom wage, hours, income and both of the proximate adult human capital measures (adult reading comprehension scores to represent adult cognitive skills and adult fat-free body mass to represent adult work capacity) are available.[15] They comprise 52% of the 1855 individuals who were known to be alive and living in Guatemala at the time of HCS, 44% of those known to be alive

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[13] See Martorell *et al.* (2005) for a more extensive discussion.

[14] 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.* (2008, 2009), 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 after HCS on a subset of the population carried out in 2007–08 (Melgar *et al.* 2008), which we use in Section 4.3.3.

[15] 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 both reading comprehension scores and fat-free body mass. Finally, we exclude 31 men and 19 women who reported an extreme number of hours worked (i.e. more than 12 hours per day for all 365 days of the year) because of concern about these implausible values (though this exclusion does not affect our conclusions – see note 34).

and 40% of the original sample of 2392.[16] 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 both currently and in the previous calendar year. Topics covered included: a) wage labor activities (type of work; hours, days and months worked; wages and fringe benefits received; and a description of the employers); b) agricultural activities (amount of land cultivated; crops grown; production levels and values; use of inputs; hours, days and months worked); and c) non-agricultural own-business activities (type of activities; value of goods or services provided; capital stock held; 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 and respondents could report working in more than one domain, and often did. Virtually all men (98%) and most women (69%) were engaged in some sort of 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 9 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.

[TABLE 1 – ABOUT HERE]

Within wage labor, individuals worked in a wide range of occupations, categorized into the following groups: casual agricultural workers; casual non-agricultural workers (for example, individuals working on road construction); domestic workers (for example, maids, gardeners or guards); unskilled workers in the formal sector; semi-skilled or skilled workers in the formal sector; and white-collar workers (including individuals holding clerical, administrative, technical or professional positions) in the formal sector. In own account agricultural activities, the dominant crops grown were maize and beans, staples of the Guatemalan diet. Much smaller percentages 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 the dominant income source for most. Finally, the most common forms of non-agricultural own businesses activities were services (for example, 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.

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[16] Most measures of attrition in the literature (e.g., Alderman *et al.* 2001, Fitzgerald, Gottschalk and Moffitt 1998a,b) refer to households or individuals who were past infancy and early childhood when the sample was initiated, so they do not include the effects of infant and early childhood mortality that account for over a quarter of the attrition in the data used for this study.

## 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. To construct these data, we draw on the questions on income-generating activities in the previous year (Hoddinott, Behrman and Martorell 2005). 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 about gross earnings as well as additional payments and deductions such as bonuses, transport and food, for the time unit (hourly, daily, weekly and so on) most relevant to their job. In the case of own-agriculture activities, information on the value of crops produced was collected (including those consumed by the household). The cost of land rentals was deducted and, for each person, a return calculated based on the number of hours that individual worked.[17] 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, a return was calculated based on the number of hours that individual worked.

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 greater wages than women in own-account activities.[18] 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 agriculture while hours worked in own-business activities were broadly equal between men and women. Corresponding to this, men’s annual earnings are more 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 contribute 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.

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)*

Within the framework presented in Section 2, our preferred representation of adult intellectual human capital ($I_2$) is adult reading comprehension cognitive skills.[19] For comparisons with other

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[17] A potential problem with this approach is that we are unable to subtract out the cost of purchased inputs, though their use is relatively uncommon. Of those individuals who report operating their own farm, 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. We partially explore these concerns in Section 4.3.4.

[18] We use “significant” to mean at the 5% level unless otherwise noted.

[19] As an alternative, we also have examined instead the Raven’s Progressive Matrices test, 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 not surprising 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, as coefficient estimates were fairly imprecise.

estimates from the literature, however, we also consider schooling attainment, which we interpret as a first-period investment ($I_1$) in childhood and adolescence that affects second-period (adult) intellectual 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 passed a pre-literacy screen.[20] 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 36.2 (38.2 for men and 33.9 for women). In all the regression analyses reported below, RCS are expressed as z-scores standardized to have mean 0 and SD 1 within the sample.

Schooling attainment was measured as the highest grade completed. Over 95% of respondents started school, with no difference between males and females. Conditional on starting 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 relatively uncommon for this population. The mean is 4.7 grades (5.1 for men and 4.3 for women).[21] The correspondence between the completed grades measure reported in HCS and an earlier measure taken during a related study completed in 1996 on the vast majority 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 is similar to previous studies in the literature suggest that the noise-to-signal ratio for self-reported schooling attainment is about 0.1 (Ashenfelter and Krueger 1994; Behrman, Rosenzweig and Taubman 1994).

[TABLE 2 – ABOUT HERE]

### *3.3.3 Health human capital (H)*

For adult health human capital ($H_2$), our preferred measure is fat-free body mass.[22] For comparisons with previous literature, however, we also use an indicator of early-life nutritional investments ($H_1$), adult height.[23]

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[20] Subjects who reported completing six or more grades of schooling were assumed to be literate for the pre-screening. 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 a five-word sentence aloud.

[21] Additional details on highest grade attained and reading comprehension skills are found in Stein *et al.* (2005) and Maluccio *et al.* (2009).

[22] Another measure of physical work capacity is maximum oxygen consumption (VO2max). It measures the capacity of individuals to deliver oxygen to muscle while doing physical work. Fat-free body mass is a good proxy for VO2max because muscle mass is a key factor determining differences among individuals in the demand for oxygen. In preliminary estimates, we used predicted VO2max as our measure of health human capital. It exhibited the same pattern

Fat-free body mass was estimated using predictive equations derived from studies of body density and anthropometry in a similar population; the equations used weight, height and waist circumference in women and weight and waist circumference in men (Ramírez-Zea *et al.* 2005). Fat-free body mass is a good measure of the health capital stock that affects work productivity and wage rates because it reflects muscle mass to a considerable extent, and thus the capacity to carry out work (McArdle, Katch and Katch 1991). Average estimated fat-free body mass is 45.5 kg (51.3 for men and 38.7 for women). Because this measure is based on predictive equations, and because it can vary more over time, fat-free body mass is likely to be measured with error.

Adult height was measured to the nearest 0.1 cm, and the mean is 157.1 cm (162.8 for men and 150.5 for women).[24] 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. (e.g., Martorell 1997, 1999).

In all the regression analyses reported below, we use the logarithms of fat-free body mass 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 and/or gender-related labor market discrimination. We also include age (at the time of the interview, $A_2$) to reflect the impact of intellectual and/or physical maturity and/or experience. The average age is 32.4 years with little difference between men and women. We further include village-of-origin fixed effects, as noted in Section 2, to control for fixed village endowments ($\mathbf{E_V}$).

### *3.3.5 Instruments excluded from the second stage*

The set of excluded instruments is motivated by relations (4)–(7) and includes variables reflecting various prices and policies (in particular $\mathbf{P_1}$, but possibly $\mathbf{P_2}$) and components of individual, family

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of results as those reported below, but because the Kleibergen-Paap statistics were poor, we do not report it here. We also used as an alternative isometric handgrip strength. Handgrip strength correlates with total strength of 22 other muscles of the body (deVries 1980, p. 401) The test was performed using a Lafayette dynamometer (Model 78010, Lafayette Instrument Co., Lafayette, IN). All subjects were asked to exert a maximal and quick handgrip. Two trials were allowed alternatively with each hand, with at least 30 seconds between trials. The maximal values of the dominant and non-dominant hands, expressed in Newtons (the force required to cause a mass of one kilogram to accelerate at a rate of one meter per second squared), were recorded. Weight-adjusted grip strength was calculated by dividing the sum of values for each hand by body weight (Montoye and Lamphier 1977). We use the dominant hand strength in our alternative estimates and the patterns are similar to those obtained using fat-free body mass (but with less precision) which is unsurprising since the two are correlated at 0.79 in the raw data.

[23] We also considered analyses using BMI, as done in several previous papers (Strauss and Thomas 1997; Thomas and Strauss 1998; Schultz 2002, 2003). As in the comparison with height, fat-free mass, which contains current weight and height as components, is a preferable measure for work capacity. When we do use BMI, including various cut-offs to capture non-linearities, results are similar to those reported in the paper.

[24] Both of these measures of physical human capital, and the procedures for taking them, are discussed in more detail in Ramirez-Zea *et al.* (2005). For example, height was measured with respondents barefoot, standing with their backs to a stadiometer, and the average of two measurements was taken, unless the difference between the first two measurements was greater than 1.0 cm, in which case a third measurement was taken and the two closest measurements were used.

and village endowments (**$E_I$, $E_F$, $E_V$**), conditional in some cases on when the individual was born ($A_0$). Many of these types of instruments have been suggested and used to endogenize height and/or shorter-term health human capital measures such as body mass index (BMI) in other contexts as well (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, it is likely that there may have been village-level changes over the years, possibly occurring at critical ages for particular individuals that should also 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 their 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 a community-randomized food supplement. 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 were not. We construct two measures, based on the date of birth of each individual and the dates and type of operation of the nutritional interventions underlying the original study (Section 3.1). The first is a control for cohort effects and the second is the potential exposure to the *Atole* supplement, the nutritious supplement. For each individual, we calculate whether s/he was exposed to the 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.

Thus, while reflecting community level characteristics, these price and policy shock 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 critical decisions (e.g., attending 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 shocks due to the earthquake may habe been particularly deleterious for infants and very young children.

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). We do observe individuals' sex, of course, but that 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 and/or gender-related labor market discrimination. One individual endowment variable on which we do have information, however, is whether an individual was a twin. For the population being studied, being a twin was basically a stochastic event as fertilization treatments that lead to multiple births in developed countries were not practiced in these villages in the 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 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.[25] 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 articulated in Section 2, our preferred specification includes indicators of adult intellectual human capital (adult reading comprehension scores, RCS) and adult health human capital (adult fat-free body mass), 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 of our preferred direct, more-proximate measures of adult intellectual and health human capital. In addition to controlling directly 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.[26,27] 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 instrumental variable 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 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. They also include an indicator of whether the child was a twin. Lastly, they

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

[26] 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. We note, however, that the standard errors are likely to be too small if there is substantial serial correlation across birth year cohorts, since none of the methods we employ completely address this problem.

[27] 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 on adult labor markets and human resources, 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, treated as exogenous.

include parental characteristics such as parental schooling and mother's height.[28] 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 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 utilize strongly predict the human capital measures and we fail to reject overidentification tests.

We present the first-stage estimates in Table 3 for each human capital measure that we consider; they have a number of significant coefficient estimates consistent with our hypotheses about how the excluded instruments influence human capital. 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, but 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=0.07). 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.[29] That early-childhood nutritional supplementation is most related to one of the intellectual human capital variables (RCS), 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 fat-free body mass relationships. Both mothers' and fathers' schooling attainment positively and significantly affect the intellectual human capital measures, but not the health capital measures. Mothers' height, on the other hand, is highly significant and positively affects 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 61–70% for health human capital.

[TABLE 3 – ABOUT HERE]

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 14 or higher (Bound, Jaeger and Baker 1995). In addition, as shown in Table 4 below, the Kleibergen-Paap statistics for weak instruments appear satisfactory,[30] and the Hansen J tests indicate that the first-stage instruments are not correlated with the second-stage disturbance terms.[31]

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[28] 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).

[29] 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).

[30] Using the critical values presented by Stock and Yogo (2005), with a Kleibergen-Paap test statistic (Kleibergen and Paap 2006; Kleibergen 2007) of 6.2 (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

### Section 4.2 Wage rate production function estimates

In Table 4, we present the logarithmic wage rate estimates using four specifications: 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 more proximate adult intellectual and health human capital measures, adult reading comprehension skills and adult fat-free body mass.

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 direct adult measures (third column) are used. If these estimates represent the causal impact of human capital, then they indicate that for the context studied, there are large and significant positive returns to both intellectual and health human capital. The estimated standardized impacts (the impact of a one standard deviation change in the independent variable) on the ln wage rate are 0.29 for schooling attainment, 0.23 for adult RCS, 0.11 for height, and 0.19 for adult fat-free body mass. These associational results suggest the possibility that “brains” has a larger effect than “brawn”, though both are significant. They also suggest that fat-free body mass has a larger association with wage rates than height, which may be because it is a more proximate measure of adult work capacity as we argued above. Finally, they indicate that attained schooling is more predictive of ln wages than is RCS, though as noted above, this may be because schooling is in part a proxy for other inputs into intellectual human capital and in part because (random) measurement errors are 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 excluded human capital measure. Though we find little change in the coefficient estimates on the intellectual human capital measures if they alone are included, the coefficient estimates on the health human capital measures increase by 100% for ln height and 50% for ln fat-free body mass if the intellectual human capital variables are not included (estimates not presented). Thus if the true production function has both intellectual and health human capital, including only health human capital would result in substantial overestimates of the wage rate impact of health human capital.

The OLS estimates treat both intellectual and health human capital as pre-determined. If measures of human capital respond in part to unobserved endowments, and such endowments also have a direct impact on wages as posited in relations (1) and (4)–(7), such estimates are biased. Moreover, they also are biased if measures of human capital are co-determined with wages and employment, due to possible feedback effects of adult short-run nutrition on productivity (Strauss, 1986) or of work experience on cognitive skills (Behrman *et al.* 2008).

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results. To the extent that our estimates are biased, however, conditional on the validity of the excluded instruments they are biased toward the OLS estimate, suggesting that the results we report are, if anything conservative and *understate* the differences between OLS and GMM. 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 GMM approach, and all physical human capital measures remain insignificant.

[31] 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, which is likely to be exogenous, we interpret this as strong evidence of the validity of all the instruments.

For these reasons we prefer to treat the human capital variables as endogenous, predicting them with the instruments in Table 3, and estimating via two-step (instrumental variable, IV) generalized methods of moments (GMM) (Hayashi 2000; Baum, Schaffer and Stillman 2007). The principal difference from the OLS estimates 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. By contrast, both of the intellectual human capital measures remain positive and significant, with substantially larger but less precisely estimated effects (on the order of 25% larger for completed schooling and 75% larger for RCS).

For the health human capital measures, it is possible that indicators such as adult height and fat-free body mass appear significant in OLS estimates because the effect of early childhood nutrition on wage rates operates through intellectual human capital or some of the other mechanisms discussed earlier rather than directly through health per se. Once there is control for the endogeneity of both types of human capital, there is no evidence of a direct link between measures of health human capital and wage rates.[32]

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 random measurement errors with such errors being relatively larger for RCS than for schooling attainment, which is consistent with the evidence we have on the correlation of repeat measurements for these items. But the increases in the estimated coefficients are unlikely to be due to measurement error alone, since they 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 negative “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 of whether 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 11.8%, similar to the central tendency in the estimates reported in Psacharopoulos and Patrinos (2004), which implies a standardized impact of about 0.41. The estimated standardized impact for RCS is virtually identical. That these standardized impacts are the same in contrast to the OLS estimates where they were quite different is consistent with a larger noise-to-signal ratio for RCS than for schooling attainment. Despite the arguments presented in Section 2 for favoring the use of the more proximate adult human capital measure, the estimates using RCS in this case do not yield substantively different findings and, if anything, are less consistent with the variance in ln wage rates than the estimates using schooling attainment. Schooling attainment, as also noted in Section 2, 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, its coefficient estimate does not reflect alone the impact of increasing schooling attainment. Unfortunately we do not have data on what would seem to be many of the important inputs in relation (2) that would permit the direct estimation of this production function as would be clarifying regarding to what extent schooling is a proxy for such inputs. Finally, we note that the

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[32] Although not directly comparable, these results contrast with some of the findings of Schultz (2002, 2003) in which he finds for some countries that the return to height increases after instrumentation of height, as well as other measures of human capital including BMI.

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 adult health human capital does not have a significant contemporaneous impact on adult wage rates.[33]

[TABLE 4 – ABOUT HERE]

### 4.3 Robustness considerations

Despite our improving upon much of the previous literature by including measures of both adult proximate intellectual and health human capital, treated as endogenous, of course to do so still requires some assumptions. In this subsection, we relax some of these in an effort to explore the sensitivity of our principal finding that adult intellectual human capital, rather than adult health human capital, is the key determinant of hourly wage rates for our sample from Guatemala.[34]

#### *4.3.1 Gender Differences*

Adult males earn 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 was substantial overlap in occupational patterns across gender (Section 3.2), there were 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 capital for men. Finally, women had lower labor market participation rates. Such differential selection processes also may bias the findings.

We directly explore how robust the results in Table 4 are to gender differences by estimating the models for men and women separately. Replication of the results shown in Table 4 for men and women separately yield the same substantive findings as when we estimate jointly, though results for the first- and second-stage diagnostic tests for the instrument sets are somewhat less strong, likely due in part to smaller sample sizes (Appendix Table 1). After endogenizing them, intellectual human capital measures are statistically significant while health human capital measures are not, for both men and women. Moreover, estimated returns to intellectual human capital are similar.

#### *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 is substantial. The estimates presented above are based on selected samples of 962 individuals (Section 3.2), 40% of the original 2392 sample members or 52% 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, per se.[35]

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[33] 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 by 43% of the sample mean (Hoddinott *et al.* 2008).

[34] In addition to the robustness considerations in this section, we note that including those individuals reporting more than 12 hours a day for 365 days (see footnote 15) does not change substantively the reported results.

[35] 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

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 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 an instrumental variables strategy not emphasized above is that it also serves 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 unlikely to be a first-order concern for men given the 98% participation rate). Since our instrument set is also plausibly unrelated to labor market participation except through its influence on human capital, 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 fieldwork, locating sample members was typically facilitated by having access to other family members from whom the field team could gather information. Therefore, we also include a number of variables that capture this feature of the success of data collection: 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 our preferred estimates 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

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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 special methodology to do so beyond the control for selection described in this section.

contexts with high attrition (Fitzgerald, Moffitt and Gottschalk 1998a, Alderman *et al.* 2001, Behrman, Parker and Todd 2008), including other analyses with these data (Behrman *et al.* 2008, 2009, Hoddinott *et al.* 2008, 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.

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" or IGT (Melgar *et al.* 2008). IGT administered an identical economic activity module to a subsample of the same individuals examined above, as well as 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, who had at least one living parent. These interviews provided all the necessary information to calculate the dependant variable examined above, hourly wage rates, about 3–4 years later.[36] 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, such as fat-free body mass. Results for instrumental variables GMM estimations using the more than 500 individuals re-interviewed in 2007–08 are strikingly similar both in the general pattern and the specific magnitudes of the estimated effects as those presented in Table 4 (Appendix Table 3).

The second alternative sample we consider 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 for a selected older sample who were living in the original villages in the 1970s. 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 a more limited set of instruments with which to endogenize these human capital measures. Given these caveats, 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).

Lastly, in results not shown we examined the sample of those working in occupations on might traditionally think of as requiring more "brawn", such as casual laborers and agricultural workers. Even for this selected subsample, intellectual human capital, but not health, remains statistically significant in the wage relation.

### *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 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

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[36] Apart from verification of completed grades of schooling, however, measures of intellectual and physical human capital were not updated in this 2007–08 study.

health human capital. The principal findings that health human capital does not increase wages suggest this potential bias is not a major concern. Nevertheless, we consider two alternative calculations of hourly wage rates.

In the first, we address the possibility that agricultural and non-agricultural income are likely to be overstated without a careful solicitation of the costs for all inputs, and 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 different substantively from those shown in Table 4 (Appendix Table 5).

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 this job. The second alternative income measure we use calculates average hourly wage rates using these reported replacement wages. 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—adult intellectual, but not health, human capital determines hourly wage rates. Magnitudes of the effects of intellectual human capital are similar, but in general about 10% smaller when these replacement wage rates are used (Appendix Table 5).

## 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 next 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 5 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 ¼ of an SD) leads to an approximate 11% increase in annual hours worked, and a one-quarter standard deviation 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 standard deviation increase in adult RCS again has a roughly similar effect. On the other hand, 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 5, where, for completeness, we present the results for the logarithm of total annual income.

[TABLE 5 – ABOUT HERE]

## 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 for an interpretation that brawn is rewarded in the labor market for at least four reasons. 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 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 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 body mass both have significantly positive associations with wage rates, annual hours worked, and annual income in OLS estimates. These disappear, however, when the health human capital measures are treated as endogenously determined. Indicators of intellectual human capital also have significant positive associations with the wage outcomes we examine. In contrast to the results for health human capital, however, the magnitudes of their impact increases when these are treated as endogenous, and considerably so for reading comprehension skills. These results are robust to a number of different specification changes, including controlling for attrition. 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.

Do these results imply that investments such as those intended to improve early childhood nutrition —believed to be a major determinant of adult height—do not yield private returns in the labor market? The answer to this question is negative. Elsewhere, we demonstrate that early childhood nutrition is causally linked to adult intellectual human capital in adulthood in the form of 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.

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**Figure 1**


Log hourly wage by sex
Density
0
.1
.2
.3
.4
.5
-2
0
2
4
6
linchr_totg
Women
Men

**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* |
| Sample size labor force participation | [655] | [769] |
| *Hourly wages, annual hours and income (Quetzales, Q7.6= US$1.00)* | | |
| Hourly wage from wage labor | 11.9 | 8.4* |
| | (10.7) | (7.1) |
| | [411] | [217] |
| Hourly wage for own-account agriculture | 7.9 | 11.9 |
| | (12.9) | (26.6) |
| | [207] | [134] |
| Hourly wage for own-account non-agricultural | 17.4 | 15.2 |
| | (30.4) | (56.5) |
| | [142] | [208] |
| Hourly wage (all individuals) | 11.9 | 9.5* |
| | (12.2) | (14.3) |
| | [515] | [447] |
| Hours worked in wage employment | 2119 | 1592* |
| | (963) | (1081) |
| Hours worked on own farm | 725 | 193* |
| | (666) | (292) |
| Hours worked on own business | 1323 | 1216 |
| | (1045) | (1199) |
| Total annual hours (all individuals) | 2351 | 1398* |
| | (902) | (1198) |
| Wage employment income | 23744 | 12383* |
| | (20987) | (13314) |
| Own agricultural income | 5604 | 1044* |
| | (13667) | (1821) |
| Own non-agricultural income | 17397 | 9396* |
| | (25571) | (14923) |
| Total annual labor income (all individuals) | 26459 | 10862* |
| | (23391) | (14658) |

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

**Table 2: Key variables – means and standard deviations (N=962)**

| | **Mean** |
|---|---|
| **Dependent variables** | |
| Hourly wage rate (Quetzales, Q7.6= US$1.00) | 10.7<br>(13.3) |
| Total annual hours | 1908<br>(1152) |
| Total annual income | 19212<br>(21282) |
| **Human capital measures** | |
| ($I_1$) Completed grades of schooling | 4.7<br>(3.5) |
| ($I_2$) Adult Reading-Comprehension Cognitive Skills (RCS) | 36.2<br>(22.8) |
| ($H_1$) Adult height (cm) | 157.1<br>(8.5) |
| ($H_2$) Adult fat-free body mass (kg) | 45.5<br>(7.7) |
| **Individual characteristics** | |
| Male | 0.54 |
| Age in years at interview in 2002–04 | 32.5<br>(4.2) |
| **Village effects** | |
| San Juan | 0.23 |
| Espíritu Santo | 0.21 |
| Santo Domingo | 0.25 |
| Conacaste (reference) | 0.31 |
| **Excluded Instruments** | |
| *Prices and policies* ($P_1$, $P_2$) | |
| Student-teacher ratio at student age 7 years | 40.1<br>(9.1) |
| Ministry of health post in village at age 2 years | 0.13 |
| Born in the two years prior to the 1976 earthquake | 0.12 |
| 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<br>(1.7) |
| Mothers’ height in cm | 148.8<br>(4.5) |
| Mothers’ height missing dummy | 0.21 |
| Fathers’ schooling attainment in grades | 1.70<br>(2.1) |

Notes: Standard deviations in parentheses.

**Table 3: First-Stage predictions of human capital measures (N=962)**

| | ($I_1$) Completed grades of schooling | ($I_2$) Adult reading comprehension skills (RCS Z score) | ($H_1$) Ln adult height | ($H_2$) Ln adult fat-free body mass |
|---|---|---|---|---|
| **Second stage controls** | | | | |
| Male ($E_1$) | 0.798 | 0.169 | 0.078 | 0.278 |
| | (0.226)** | (0.069)* | (0.002)** | (0.006)** |
| Age ($A_2$) | -0.041 | -0.012 | -0.000 | 0.000 |
| | (0.025) | (0.008) | (0.000) | (0.001) |
| San Juan ($E_V$) | 1.335 | 0.378 | -0.011 | -0.036 |
| | (0.331)** | (0.097)** | (0.004)** | (0.012)** |
| Espiritu Santo ($E_V$) | 2.142 | 0.325 | -0.002 | -0.006 |
| | (0.313)** | (0.101)** | (0.003) | (0.010) |
| Santo Domingo ($E_V$) | 0.748 | 0.166 | -0.004 | -0.023 |
| | (0.333)* | (0.103) | (0.003) | (0.009)* |
| **Excluded instruments** | | | | |
| *Prices and policies ($P_1$, $P_2$)* | | | | |
| Student-teacher ratio at age 7 | -0.033 | -0.012 | -0.000 | 0.000 |
| | (0.012)* | (0.004)* | (0.000) | (0.000) |
| Ministry of health post in village at age 2 | -0.192 | -0.065 | 0.012 | 0.042 |
| | (0.392) | (0.110) | (0.005)* | (0.014)** |
| Born in 2 years prior to 1976 earthquake | -0.488 | 0.001 | -0.005 | -0.015 |
| | (0.357) | (0.079) | (0.003) | (0.010) |
| Exposure to either intervention 0-36 months | 0.042 | -0.013 | -0.001 | 0.001 |
| | (0.338) | (0.076) | (0.002) | (0.007) |
| Exposure to atole intervention 0-36 months | 0.294 | 0.209* | -0.001 | -0.013 |
| | (0.389) | (0.106) | (0.004) | (0.011) |
| *Individual endowments ($E_I$)* | | | | |
| Twins indicator | -0.955 | -0.139 | -0.035 | -0.080 |
| | (0.782) | (0.376) | (0.007)** | (0.024)** |
| *Family endowments ($E_F$)* | | | | |
| Mother's completed grades | 0.437 | 0.120 | 0.001 | 0.001 |
| | (0.073)** | (0.018)** | (0.001) | (0.002) |
| Ln mother's height | 13.942 | 3.790 | 0.512 | 0.896 |
| | (3.465)** | (1.276)** | (0.040)** | (0.099)** |
| Mother's height missing | 0.027 | 0.115 | 0.006 | 0.010 |
| | (0.215) | (0.054)* | (0.003) | (0.008) |
| Father's completed grades | 0.340 | 0.075 | -0.000 | -0.002 |
| | (0.058)** | (0.015)** | (0.000) | (0.002) |
| Constant | -64.911 | -18.725 | 2.459 | -0.840 |
| | (17.551)** | (6.450)** | (0.204)** | (0.501) |
| $R^2$ | 0.22 | 0.15 | 0.61 | 0.70 |
| F-statistic: excluded instruments | 18.9 | 14.2 | 24.5 | 16.9 |
| 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%.

## Table 4: Ln hourly wage rate production functions (N=962)

| | OLS | IV | OLS | IV |
|---|---|---|---|---|
| ($I_1$) Completed grades of schooling | 0.084 | 0.114 | | |
| | (0.008)** | (0.024)** | | |
| ($I_2$) Adult RCS Z score | | | 0.228 | 0.404 |
| | | | (0.027)** | (0.083)** |
| ($H_1$) Ln adult height | 2.117 | -1.765 | | |
| | (0.792)** | (1.997) | | |
| ($H_2$) Ln adult fat-free body mass | | | 1.079 | -0.553 |
| | | | (0.294)** | (0.962) |
| Male | 0.207 | 0.484 | 0.102 | 0.524 |
| | (0.078)* | (0.152)** | (0.095) | (0.268) |
| Age | 0.014 | 0.016 | 0.012 | 0.018 |
| | (0.007)* | (0.005)** | (0.007) | (0.006)** |
| $R^2$/Centered $R^2$ | 0.19 | 0.16 | 0.16 | 0.11 |
| Kleibergen-Paap test | | 11.71 | | 8.34 |
| Hansen J test | | 8.42 | | 7.96 |
| p-value | | [0.39] | | [0.44] |

Notes: Instrumental variables GMM estimates using instruments and second stage controls indicated in Table 3. 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%.

**Table 5: Ln annual hours and total income production functions (N=962)**

| | Ln Annual Hours | | | | Ln Total Income | | | |
|---|---|---|---|---|---|---|---|---|
| | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| ($I_1$) Completed grades of schooling | 0.023 | 0.105 | | | 0.107 | 0.206 | | |
| | (0.009)* | (0.027)** | | | (0.013)** | (0.037)** | | |
| ($I_2$) RCS z score | | | 0.085 | 0.367 | | | 0.313 | 0.772 |
| | | | (0.034)* | (0.092)** | | | (0.046)** | (0.132)** |
| ($H_1$) Ln adult height | 1.601 | -3.306 | | | 3.718 | -3.169 | | |
| | (0.899) | (2.078) | | | (0.969)** | (2.486) | | |
| ($H_2$) Ln fat-free mass | | | 0.402 | -1.173 | | | 1.482 | -1.417 |
| | | | (0.367) | (0.961) | | | (0.462)** | (1.206) |
| Male | 0.931 | 1.234 | 0.949 | 1.350 | 1.139 | 1.590 | 1.051 | 1.802 |
| | (0.081)** | (0.165)** | (0.111)** | (0.272)** | (0.114)** | (0.214)** | (0.155)** | (0.353)** |
| Age | 0.007 | 0.014 | 0.007 | 0.018 | 0.021 | 0.023 | 0.020 | 0.028 |
| | (0.007) | (0.007) | (0.007) | (0.008)* | (0.011) | (0.010)* | (0.011) | (0.011)** |
| $R^2$/Centered $R^2$ | 0.22 | 0.16 | 0.22 | 0.16 | 0.31 | 0.25 | 0.29 | 0.20 |
| Kleibergen-Paap test | | 11.7 | | 8.3 | | 11.7 | | 8.3 |
| Hansen J test | | 4.9 | | 4.9 | | 6.3 | | 5.4 |
| p-value | | [0.77] | | [0.77] | | [0.62] | | [0.71] |

Notes: OLS and instrumental variables GMM estimates using instruments and controls indicated in Table 4. 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 1: Ln hourly wage rate production functions, by sex

| | Male (N=515) | | | | Female (N=447) | | | |
|---|---|---|---|---|---|---|---|---|
| | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| ($I_1$) Completed grades of schooling | 0.092 | 0.117 | | | 0.073 | 0.098 | | |
| | (0.010)** | (0.023)** | | | (0.015)** | (0.026)** | | |
| ($I_2$) RCS z score | | | 0.246 | 0.436 | | | 0.205 | 0.462 |
| | | | (0.035)** | (0.087)** | | | (0.048)** | (0.116)** |
| ($H_1$) Ln adult height | 1.991 | -2.256 | | | 2.289 | -0.205 | | |
| | (0.961)* | (1.858) | | | (1.195) | (2.684) | | |
| ($H_2$) Ln fat-free mass | | | 1.675 | -0.806 | | | 0.333 | -0.428 |
| | | | (0.403)** | (0.975) | | | (0.404) | (1.272) |
| Age | 0.010 | 0.007 | 0.005 | 0.010 | 0.018 | 0.018 | 0.020 | 0.028 |
| | (0.008) | (0.006) | (0.008) | (0.007) | (0.010) | (0.009)* | (0.010)* | (0.009)** |
| Kleibergen-Paap test | | 6.9 | | 4.3 | | 4.3 | | 8.3 |
| Hansen J test | | 12.1 | | 10.9 | | 4.8 | | 5.4 |
| p-value | | [0.15] | | [0.22] | | [0.68] | | [0.78] |

Notes: OLS and instrumental variables GMM estimates using instruments and controls indicated in Table 4. 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=962)**

| | OLS | IV | OLS | IV |
|---|---|---|---|---|
| ($I_1$) Completed grades of schooling | 0.080 | 0.120 | | |
| | (0.009)** | (0.024)** | | |
| ($I_2$) Adult RCS Z score | | | 0.214 | 0.413 |
| | | | (0.030)** | (0.084)** |
| ($H_1$) Ln adult height | 2.206 | -2.288 | | |
| | (0.830)** | (2.024) | | |
| ($H_2$) Ln adult fat-free body mass | | | 1.130 | -0.971 |
| | | | (0.277)** | (1.025) |
| Male | 0.212 | 0.542 | 0.104 | 0.664 |
| | (0.078)** | (0.150)** | (0.090) | (0.280)* |
| Age | 0.013 | 0.016 | 0.011 | 0.018 |
| | (0.007) | (0.005)** | (0.007) | (0.006)** |
| Kleibergen-Paap test | | 10.75 | | 8.28 |
| Hansen J test | | 8.33 | | 9.99 |
| p-value | | [0.40] | | [0.27] |

Notes: Instrumental variables GMM estimates using instruments and second stage controls indicated in Table 4 and weighted 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 production functions: Select sample of HCS respondents re-interviewed in 2007-8 as part of IGT study (N=534)**

| | OLS | IV | OLS | IV |
|---|---|---|---|---|
| ($I_1$) Completed grades of schooling | 0.088 | 0.097 | | |
| | (0.010)** | (0.028)** | | |
| ($I_2$) Adult RCS Z score | | | 0.247 | 0.388 |
| | | | (0.037)** | (0.101)** |
| ($H_1$) Ln adult height | 2.610 | 1.305 | | |
| | (0.938)** | (2.192) | | |
| ($H_2$) Ln adult fat-free body mass | | | 1.317 | 1.562 |
| | | | (0.393)** | (0.996) |
| Male | 0.179 | 0.263 | 0.054 | -0.028 |
| | (0.105) | (0.160) | (0.129) | (0.276) |
| Age | 0.013 | 0.019 | 0.014 | 0.023 |
| | (0.008) | (0.006)** | (0.007) | (0.006)** |
| Kleibergen-Paap test | | 7.8 | | 6.1 |
| Hansen J test | | 13.8 | | 12.8 |
| p-value | | [0.09] | | [0.12] |

Notes: Instrumental variables GMM estimates using instruments and second stage controls indicated in Table 3. 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

| | **Men and Women** | | **Women only** | | **Men only** | |
|---|---|---|---|---|---|---|
| | OLS | GMM | OLS | GMM | OLS | GMM |
| ($I_1$) Completed grades of schooling | 0.117 | 0.427 | 0.132 | 0.296 | 0.114 | 0.350 |
| | (0.027)** | (0.204)* | (0.050)** | (0.659) | (0.032)** | (0.151)* |
| ($H_1$) Ln adult height | 1.626 | 7.956 | -0.484 | 4.092 | 3.575 | 5.873 |
| | (1.558) | (16.190) | (2.747) | (44.980) | (1.777)* | (12.092) |
| Male | 0.003 | -0.718 | | | | |
| | (0.168) | (1.193) | | | | |
| Age | -0.035 | -0.015 | -0.028 | -0.023 | -0.035 | -0.022 |
| | (0.008)** | (0.015) | (0.012)* | (0.053) | (0.008)** | (0.013) |
| N | 367 | 367 | 167 | 167 | 200 | 200 |
| $R^2$ | 0.12 | | 0.07 | | 0.19 | |
| Kleibergen-Paap test | | 1.1 | | 0.2 | | 1.4 |
| Hansen J test | | 1.8 | | 1.3 | | 1.7 |
| p-value | | [0.61] | | [0.73] | | [0.63] |

Notes: 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 production functions: Alternative wage measures (N=962)

| | Adjustments to agricultural and non-agricultural income | | | | Replacement agricultural and non-agricultural wages | | | |
|---|---|---|---|---|---|---|---|---|
| | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| ($I_1$) Completed grades of schooling | 0.070 | 0.098 | | | 0.087 | 0.127 | | |
| | (0.008)** | (0.019)** | | | (0.009)** | (0.023)** | | |
| ($I_2$) RCS z score | | | 0.188 | 0.341 | | | 0.237 | 0.446 |
| | | | (0.025)** | (0.067)** | | | (0.029)** | (0.084)** |
| ($H_1$) Ln height | 1.100 | -2.451 | | | 2.111 | -2.336 | | |
| | (0.571) | (1.506) | | | (0.790)** | (2.062) | | |
| ($H_2$) Ln fat free mass | | | 0.987 | -1.147 | | | 1.058 | -0.933 |
| | | | (0.204)** | (0.813) | | | (0.296)** | (1.004) |
| Male | 0.297 | 0.558 | 0.133 | 0.695 | 0.286 | 0.606 | 0.188 | 0.710 |
| | (0.060)** | (0.121)** | (0.067) | (0.230)** | (0.079)** | (0.157)** | (0.095) | (0.279)* |
| Age | 0.014 | 0.017 | 0.013 | 0.020 | 0.011 | 0.014 | 0.009 | 0.016 |
| | (0.006)* | (0.006)** | (0.005)* | (0.006)** | (0.007) | (0.006)* | (0.007) | (0.006)* |
| Kleibergen-Paap test | | 12.4 | | 8.6 | | 11.8 | | 8.3 |
| Hansen J test | | 5.8 | | 6.4 | | 7.8 | | 7.3 |
| p-value | | [0.67] | | [0.61] | | [0.46] | | [0.50] |

Notes: Instrumental variables GMM estimates using instruments and controls shown in Table 4. 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%.