

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

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Abstract: A number of studies report that, controlling for schooling, adult height has significant associations with wages. But schooling and height are imperfect measures of adult cognitive skills (“brains”) and strength (“brawn”); further they are not exogenous. Analysis of rich longitudinal data over 35 years in rural Guatemala finds that proximate wage determinants—adult reading comprehension skills and fat-free body mass—have significantly positive associations with wages, but that only brains, and not brawn, is significant when human capital is treated as endogenous. Even in a poor developing economy in which physical attributes plausibly have 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 to economists are the potential 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 human capital, individuals' capacity to undertake manual labor. Typically, height is used to represent physical human capital, based on evidence that taller individuals are usually stronger and that strength is rewarded in labor markets (Strauss and Thomas 2008). 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 have focused on possible dimensions of human capital for which attained height might be a proxy measure.²

In this paper, we assess the returns to both adult intellectual and physical 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.

Our first important contribution is to include direct proximate measures of adult intellectual human capital (adult cognitive skills) and adult physical human capital (fat-free body mass). Much of the extant literature uses measures such as schooling attainment and height that are likely to be biased systematically for at least one of the following reasons.

Because these measures largely capture only human capital investments in childhood and adolescence, they are likely to be biased measures of adult human capital if, as seems plausible within dynamic models of human capital investments, subsequent investments respond to these earlier decisions. For example, often schooling is used to represent adult intellectual human capital,³ but post-schooling learning due, for instance, to whether one is engaged in a skilled occupation or not, is likely to be determined sequentially by schooling. Therefore in wage function estimates the coefficient of schooling is likely to represent the effect of schooling *and* any correlated post-schooling investments.⁴ For adult physical human capital, often height is used as an indicator.

¹ Completed grades of schooling are often referred to as years of schooling, though completed grades and years of schooling 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. By contrast, 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.

³ A small number of studies in the economics literature incorporate adult cognitive skills to represent adult intellectual human capital, so this critique does not apply to 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).

⁴ For example, with the same sample as that used in this paper, Behrman et al. (2008) show that tenure in skilled occupations acquired after school completion has significant impact on adult cognitive skills and that the impact of schooling on these skills is overestimated when post-schooling experiences are ignored.

Height, however, is determined largely by genetics, early life nutrition and adolescent nutrition, so it also represents in substantial part investments in childhood and adolescence. There is considerable evidence that endogenous adult short-term nutrition in poor societies affects adult productivity and some evidence that nutrition within poor households is allocated towards those who are better endowed and taller.⁵ Therefore, height may be proxying in part for allocations during adulthood that affect strength and productivity, and not just the longer-run nutritional effects on which most interpretations focus.

There is a further possible systematic bias regarding using schooling attainment to represent cognitive skills even at the time of completion of schooling. Schooling attainment is but one of multiple inputs, albeit possibly an important one, into the production of adult human capital. If there are multiple inputs and any of those others than schooling attainment are correlated with schooling attainment and not included in the estimation, this results in omitted variable bias in the estimated schooling attainment coefficient. This bias may be positive or negative, depending on the correlation of the excluded inputs with schooling attainment and the nature of the production technology for the adult human capital stock. Behrman and Birdsall (1983) note that within standard models of schooling investment, increased public school quality induces greater schooling attainment, so the failure to include school quality results in a substantial overestimate of the impact of school “quantity” and a mischaracterization of the relative gains from increasing different inputs into adult intellectual human capital in their estimates for Brazil. Hanushek and Wößmann (2008) also suggest that focus on the quantity of intellectual human capital—as typically represented by grades of schooling attainment—can be misleading because of unobserved quality of schooling. Suppose that the true production function for wages should be specified in terms of proximate human capital determinants of productivity such as cognitive skills and physical strength, rather than a specification with completed grades of schooling and adult height. If cognitive skills are produced by childhood nutrition, time in school, school quality and post-school work experience,⁶ and if adult height is produced at least in part by childhood nutrition, then completed grades of schooling are an imperfect indicator of adult cognitive skills (being only one input into the production of such skills) and height is in part a proxy measure for early childhood nutrition that may improve learning and cognitive skills, rather than only an indicator of adult strength.

Our second important contribution is to treat both intellectual and physical 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 physical 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),⁷ but very few micro studies that treat adult cognitive skills as endogenous and measured with error.⁷ For physical 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 almost

⁵ Examples of studies that report that endogenously determined short-run nutrition improves productivity include Strauss (1986), Behrman and Deolalikar (1989), Deolalikar (1988), Behrman, Foster and Rosenzweig (1997), Foster and Rosenzweig (1994), Haddad and Bouis (1991) and Sahn and Alderman (1988). Pitt, Rosenzweig and Hassan (1990) provide evidence that intrahousehold allocations of nutrients favor those better endowed in terms of productivity.

⁶ We have investigated the determinants of adult cognitive skills for the sample used in this study and find that pre- and post-schooling experiences, not just schooling, are important (Behrman *et al.* 2008).

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.

always 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 physical 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 schooling, height or other representations of intellectual or physical 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 thus are able to obtain consistent estimates of the causal effect of human capital on wages.

We investigate the impact of adult intellectual and physical human capital on wages earned in Guatemala, a poor developing country in which the rewards to physical human capital in labor markets potentially may be considerable (e.g., Immink and Viteri 1981). Section 2 outlines our conceptual framework, a production function for adult wages dependent on endogenous adult intellectual and physical human capital. 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 inputs related to 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, endogenously treated adult physical human capital has no effect on wages. In Section 5, we examine the effects of intellectual and physical human capital on hours worked, finding again that intellectual, but not physical human capital, is what matters, increasing hours worked. In Section 6, we conclude.

2. Conceptual Framework

Consider a simple two-period model with two forms of human capital: intellectual (I_t) and physical or biological (B_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 physical (B_2) human capital in that period, age (A_2), individual

⁸ Exceptions include Alderman *et al.* (1996) 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).

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}):

$$(1) Y_2 = Y^P(I_2, B_2, A_2, \mathbf{E}_I, \mathbf{E}_V, U_{2Y})$$

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 physical human capital (B_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, B_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}):

$$(2) I_2 = I^P(I_1, B_1, \mathbf{X}_1, \mathbf{X}_2, A_2, \mathbf{E}_I, \mathbf{E}_V, U_{2I}) \text{ and}$$

$$(3) B_2 = B^P(I_1, B_1, \mathbf{X}_1, \mathbf{X}_2, A_2, \mathbf{E}_I, \mathbf{E}_V, U_{2B}).$$

In these second-period production functions, first-period human capital measures, such as attained schooling (I_1) and early-life nutrition (B_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 ($\mathbf{X}_1, \mathbf{X}_2$)—such as parental stimulation and time studying in the first period, and nutrition and work experience in the second period—that also likely affect second-period human capital. Moreover, both individual endowments (\mathbf{E}_I) and village-of-origin endowments (\mathbf{E}_V) may enter into the production of second-period human capital, 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 ($\mathbf{P}_1, \mathbf{P}_2$); individual, family¹⁰ 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_{ij} , where $t = 1, 2$ for the two periods and $j = I, B$ for the two forms of human capital).¹¹ For the first-period investments, if there is risk neutrality the expected, rather than actual, values of the second-period variables (indicated by superscript “ e ”) are relevant:

$$(4) I_1 = I_1^d(\mathbf{P}_1, \mathbf{P}_2^e, \mathbf{E}_I, \mathbf{E}_F, \mathbf{E}_V, A_0, W_{1I}, W_{2I}^e),$$

$$(5) B_1 = B_1^d(\mathbf{P}_1, \mathbf{P}_2^e, \mathbf{E}_I, \mathbf{E}_F, \mathbf{E}_V, A_0, W_{1B}, W_{2B}^e),$$

$$(6) I_2 = I_2^d(\mathbf{P}_1, \mathbf{P}_2, \mathbf{E}_I, \mathbf{E}_F, \mathbf{E}_V, A_0, W_{1I}, W_{2I}), \text{ and}$$

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 in the 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.

$$(7) B_2 = B_2^d(\mathbf{P}_1, \mathbf{P}_2, \mathbf{E}_I, \mathbf{E}_F, \mathbf{E}_V, A_0, W_{1B}, W_{2B}).$$

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 physical human capital (B_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 B_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 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 has been largely ignored with regard to estimates of the impact of measures of longer-run physical capital on wages.
- (2) If relation (1) with both intellectual and physical human capital is the true relation but only intellectual human capital is included, even if there are valid¹² instruments for the included intellectual human capital, the estimates of the impact of this variable are likely to be biased since the omitted physical 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 small literature that does include both intellectual and physical 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 B_1 (i.e., schooling attainment and height, an indicator for early-life nutrition) than to I_2 and B_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 B_1 , are excluded from the specification. If I_1 and B_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 B_2 and relation (1) with I_1 and B_1 on the basis of consistency with the variance in the dependent variable Y_2 because I_1 and B_1 proxy well for the omitted variables.
- (4) There may be important cross-effects of prices and policies ($\mathbf{P}_1, \mathbf{P}_2$). Changes, for example, in early life nutritional policies may affect the second-period intellectual human capital stock and changes in school characteristics may affect second-period physical 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 physical 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

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.

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

The data demands are considerable for estimating the adult wage production function posited in Section 2, when the relevant human capital stocks are treated as endogenous. We utilize 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 physical human capital as well as of schooling attainment and height, and shocks from an experimental nutritional intervention and policy and market changes over time.

3.1 The Institute of Nutrition of Central America and Panama (INCAP) Longitudinal Data¹³

Beginning in 1969, 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 had been 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, referred to as 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 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 to Guatemala City, and 168 lived elsewhere in Guatemala (Grajeda *et al.* 2005).

¹³ For a more extensive discussion, see Martorell *et al.* (2005).

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), 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 2006–07 (Melgar *et al.* 2008), which we use in Section 4.3.3.

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 strength) were available.¹⁵ They comprise 52% of the 1855 individuals who were known to be alive and living in Guatemala at the time of the HCS, 44% of those known to be alive 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 activities¹⁷

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).

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

¹⁵ Beginning with 1424 respondents who completed the income modules, we exclude from the analyses those who were not participating in the labor market 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 the concern about these implausible values.

¹⁶ Most measures in the literature of attrition 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.

¹⁷ Hoddinott, Behrman and Martorell (2005) provide more detail on the material in this section.

beans. For example, the typical casual agricultural laborer working full time earned approximately 660 Quetzales (US\$ 87) *per month*, an amount equivalent to the median value of 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, farming was not an economically important activity for the majority of the sample. Finally, the most popular 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 physical human capital in the labor market for this population.

3.3 Central Variables for the Analysis

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

Descriptive statistics on hourly wage rates, annual hours worked and total labor income also are given in Table 1 for the estimating sample. To construct these data, we draw on the questions on income-generating activities in the previous year.¹⁸ 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.¹⁹ 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 higher hourly wages from wage labor and (insignificantly) higher wages from non-agricultural activities, whereas women had slightly higher hourly wages from agricultural activities (statistically significant at the 11% level). Combining hourly wages from all three activities (i.e., the weighted average wage), men earned 30% more on average than women, and were substantially less likely to be in the lowest hourly wage occupations (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. Consequently, men earned more than women. 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 agriculture was relatively unimportant 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 labor income, conditional on working in at least one of the activities (shown for men and women combined in Table 2).

¹⁸ A detailed description of the collection of these data and additional descriptive statistics are found in Hoddinott, Behrman and Martorell (2005).

¹⁹ 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.

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. For comparisons with other 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 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% of the sample, 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 Physical human capital (B)

For adult physical human capital (B_2), our preferred measure is fat-free body mass. For comparisons with previous literature, however, we also use an indicator of early-life nutritional investments (B_1), adult height.

²⁰ Subjects who reported completing six or more grades of schooling were assumed to be literate. Respondents who reported having completed fewer than three grades of schooling, and those who reported three to five grades of schooling but could not read correctly the headline of a local newspaper article, were given a pre-literacy test that began with reading letters aloud. They were considered literate if they passed the test with fewer than five errors out of 35 questions, the most difficult of which was reading 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).

Fat-free body mass was estimated using predictive equations derived from a similar population using weight, height and waist circumference in women and weight and waist circumference in men (Ramírez-Zea *et al.* 2005). Fat-free body mass might be a good measure of the physical capital stock that affects work productivity and wage rates because xxx 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).²² There is a substantial literature documenting that, in contexts such as those we are studying, adult height is determined substantially by early-life nutrition but also by nutrition in late childhood and early adolescence (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 labor income) with respect to physical 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 ($\mathbf{P}_1, \mathbf{P}_2$) and components of individual, family and village endowments ($\mathbf{E}_I, \mathbf{E}_F, \mathbf{E}_V$), conditional in some cases on when the individual was born (A_0).

Price and policy ($\mathbf{P}_1, \mathbf{P}_2$) 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 an indicator of one such change, student-teacher ratios at the primary school level when the individual was seven years old. A second policy change of this type was the availability of the experimental nutritional supplements. 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 the supplements 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* treatment. For each individual, we calculate whether s/he was

²² Both of these measures of physical human capital, and the procedures for taking them, are discussed in more detail in Ramírez-Zea *et al.* (2005). 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.

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.

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 an improvement over 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.

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 (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 low birth weight that has negative impacts on early life cognitive and physical development, which in turn possibly limit later intellectual and physical development (e.g., Behrman and Rosenzweig 2004).

Family endowments (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 physical human capital characteristics, specifically mothers' and fathers' schooling attainment and mothers' height.²³ Since such measures might be correlated with unobserved individual endowments that are in the disturbance term of the second-stage relation, however, we empirically assess whether they satisfy standard overidentification tests.

4. The effects of adult intellectual and physical human capital on wage rates

We estimate the wage rate production function with the spotlight on the roles of *adult* intellectual and physical 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 physical 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 physical 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 at the same time; this yields 64 clusters.^{24,25} Before presenting estimates of the wage rate production

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

²⁴ Angrist and Lavy (2002) and Wooldridge (2003) suggest that standard corrections for clustering are valid only when the number of groups or clusters is large. In light of this, following Bertrand, Duflo and Mullainathan (2004), we also estimated the models using block bootstrapped standard errors, using the same 64 clusters and resampling 1000 times.

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 nutritional supplementation intervention and the community histories. They also include parental characteristics such as parental schooling and mother's height.²⁶ Second, we include *both* intellectual and physical 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 physical 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 have reasonable power and we fail to reject overidentification tests.

The first-stage estimates, shown in Table 3 for each human capital measure that we consider, 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. Exposure to the *Atole* intervention in the first three years of life (relative to *Fresco*) is positively associated with three of the four outcomes, and statistically significant ($p=0.064$) in the reading comprehension skills relationship.²⁷ That early-childhood nutritional supplementation is most related to one of the intellectual human capital variables (RCS), rather than to the physical human capital variables suggests the possibility that there may be what in Section 2 we referred to as a "cross effect" of the "physical" investment of early life nutrition on wages through intellectual human capital as represented by RCS, regardless of whether there is a direct

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. On the other hand, the fact that we find similar results across different outcome measures (hourly wage rates and hours) raises the confidence that the results are not simply driven by sampling variation.

²⁵ As discussed in Section 2, for both the village fixed effects and the clustering for the standard errors, we focus on the four villages of origin because, within the relatively segmented markets of developing countries when the sample members were children, general cultural attitudes towards human capital investments and work and the opportunity set of economic alternatives in the village and through migratory linkages to other labor markets probably had strong village components. By the time of the 2002–04 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.

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

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

“own effect” through physical human capital. Being a twin negatively affects most 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 physical 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 all of the human capital measures, 15–22% for intellectual human capital and 61–69% for physical 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 17 or higher (Bound, Jaeger and Baker 1995). In addition, as shown in Table 4 below, the Kleibergen-Paap statistics for weak instruments appear satisfactory,²⁸ and the Hansen J tests indicate that the first-stage instruments are not correlated with the second-stage disturbance terms.²⁹

Section 4.2 Wage rate production function estimates

Table 4 presents ln wage rate estimates for four specifications: both OLS and IV estimates using the most common intellectual and physical human capital measures in the literature, schooling and adult height, and both OLS and IV estimates using our preferred more proximate adult intellectual and physical 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 physical human capital, whether the more common indicators (column 1) or our preferred direct adult measures (column 3) 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 physical human capital. The estimated standardized impacts (i.e., the impact of a one standard deviation change) 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 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

²⁸ 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 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.

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

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 physical 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 physical human capital, including only physical human capital would result in substantial overestimates of the wage rate impact of physical human capital.

The OLS estimates treat both intellectual and physical 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, as in Strauss (1986), or of work experience on cognitive skills, as in Behrman *et al.* (2008).

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 physical human capital, with either 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 estimated effects (on the order of 25% larger for completed schooling and 75% for RCS).

For the physical 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 rather than directly through physical human capital per se. Once there is control for the endogeneity of human capital, there is no evidence of a direct link between measures of physical human capital and wage rates.

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 certain 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.

In any case, under the standard interpretation in the literature, the rate of return to schooling is estimated to be 11.8%, which implies a standardized impact (again, the impact on ln wage of a one standard deviation change) of about 0.41. The estimated standardized impact for RCS is very similar, 0.43. That these standardized impacts are basically the same in contrast to the OLS estimates where they were quite different is consistent with the 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 have

different implications 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 which, if possible, would be clarifying regarding to what extent schooling is proxying for such inputs. Finally, we note that the first-stage estimates in Table 3 indicate significant effects ($p=0.064$) 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 has an impact through adult intellectual human capital even though adult physical human capital does not have a significant impact on the adult wage rate.³⁰

[TABLE 4 – ABOUT HERE]

4.3 Robustness considerations

Despite going beyond and, we argue, improving upon much of the literature by including measures of both adult proximate intellectual and physical human capital, treated as endogenous, doing so still requires several important 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 physical human capital, is the key determinant of hourly wage rates in Guatemala.³¹

4.3.1 Gender Differences

Adult males earn substantially higher wage rates than females (Table 1), as well as differ along all 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 human capital of different types. Moreover, while there was substantial overlap in occupational patterns across gender (Section 3.2), there were some important differences, for example lower labor market participation rates for women. Such differential selection processes may be biasing our results.

We directly explore how robust the results in Table 4 are to gender differences by estimating the instrumental variables models for men and women separately, as well as consideration of models permitting gender interactions. Replicating 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 human capital for all specifications for each gender, intellectual human capital measures are statistically significant while physical human capital measures are not. Moreover, estimated returns to intellectual human capital in no case are statistically different between men and women (based on estimates, not presented, including interactions of the human capital measures with a male dummy variable).

³⁰ This result suggests that this cross effect is underlying the reduced-form estimates using these data that exposure to *atole* (relative to *fresco*) during the entire first three years of life increased male (but not female) hourly wage rates by 43% of the sample mean (Hoddinott *et al.* 2008).

³¹ 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 14) does not change substantively the reported results.

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 at least 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, but were not in the labor market; therefore they are missing due to labor market selection rather than attrition, *per se*.³²

Grajeda *et al.* (2005) show that the overall attrition in the sample is associated with a number of 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 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 plausibly unrelated to labor market participation except through its influence on human capital, the potential correlation between the human capital measures and the resulting 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 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 explicitly have adjustments to correct for selection on unobservable characteristics, by including a number of endogenous observables indicated above, which are likely

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

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 physical human capital remain insignificant (Appendix Table 2). We interpret these findings to mean that, as found in other contexts with high attrition (Fitzgerald, Moffitt and Gottschalk 1998a, Alderman *et al.* 2001a, Behrman, Parker and Todd 2008), including other analyses with these data (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 is a part.

In 2006–07, 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 utilized 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 provide all the necessary information to calculate the dependant variable examined above, hourly wage rates, between 3–4 years later.³³ 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 break further 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 2005–06 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 physical 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 physical and intellectual human capital and hourly wages measured in 2005–06, when the parents were between 50 and 80 years of age, mimic the patterns described throughout, particularly for men (Appendix Table 4).

4.3.4 *Alternative income measures*

³³ Apart from verification of completed grades of schooling, however, measures of intellectual and physical human capital were not updated in this 2006–07 study.

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 physical 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 physical human capital is more important for such activities, then, we might expect upward bias on the estimated effects of physical human capital. The principal findings that physical human capital does not increase wages, however, 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 hourly wage rates accordingly. Consistent with the possibility of upward bias on physical 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 physical 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 experience to do this job with the same level of responsibility. 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 physical, 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 physical human capital on annual hours worked

Having established that brains, but not brawn, are the key wage rate determinants, we next explore whether and how these different forms of human capital affect annual hours worked.

Table 5 presents estimates for the logarithm of annual hours worked. Without accounting for the endogeneity of the human capital measures, the OLS estimates show that both intellectual and physical human capital have positive associations with total hours worked and that these are significant for both intellectual human capital measures and for one physical human capital measure, height (at the 10% significance level). 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 leads to an approximate 11% increase in annual hours worked, and 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. On the other hand, the estimated IV coefficients on each of the physical 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.

6. Conclusions

In both developing and developed countries, evidence exists that physical human capital (brawn) as represented by adult height has significant associations with labor income, even after controlling for intellectual human capital (brains) as usually represented by completed grades of schooling or less commonly, by adult cognitive skills. But this literature is problematic 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 adolescents, but in general might combine with other inputs such as nutrition and school quality 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 physical attributes such as strength, but also through intellectual human capital.

We explore these issues using rich longitudinal data collected over 35 years in rural Guatemala, a market environment with substantial unskilled manual labor for which physical 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 and annual hours worked in OLS estimates. However, these disappear when they are treated as endogenously determined. Indicators of intellectual human capital also have significant positive associations with wage rates and hours worked. In contrast to the results for physical 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 and hours worked 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 probably 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 physical capital but rather through improving adult intellectual human capital.

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Figure 1

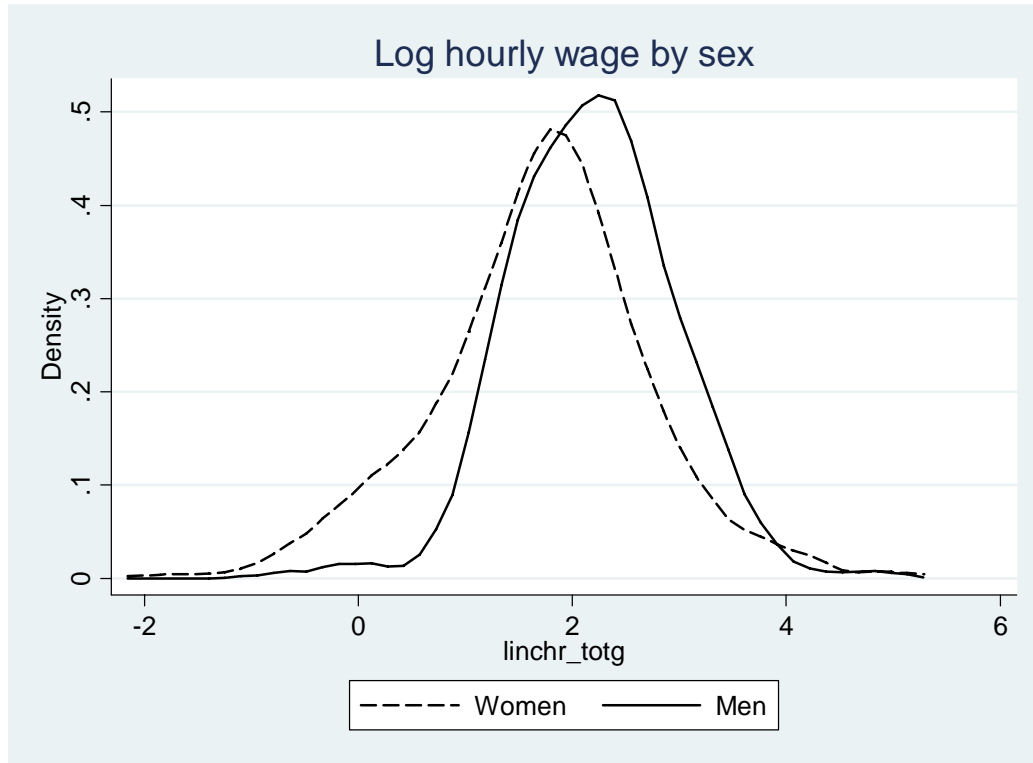


Table 1: Income generating activities by sex

	Men	Women
<i>Labor force participation (%)</i>		
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]
<i>Hourly wages, annual hours and income (Quetzales, Q7.6= US\$1.00)</i>		
	11.9	8.4*
Hourly wage from wage labor	(10.7)	(7.1)
	[411]	[217]
	7.9	11.9
Hourly wage for own-account agriculture	(12.9)	(26.6)
	[207]	[134]
	17.4	15.2
Hourly wage for own-account non-agricultural	(30.4)	(56.5)
	[142]	[208]
	11.9	9.5*
Hourly wage (all individuals)	(12.2)	(14.3)
	[515]	[447]
	2119	1592*
Hours worked in wage employment	(963)	(1081)
	725	193*
Hours worked on own farm	(666)	(292)
	1323	1216
Hours worked on own business	(1045)	(1199)
	2351	1398*
Total annual hours (all individuals)	(902)	(1198)
	23744	12383*
Wage employment income	(20987)	(13314)
	5604	1044*
Own agricultural income	(13667)	(1821)
	17397	9396*
Own non-agricultural income	(25571)	(14923)
	26459	10862*
Total annual labor income (all individuals)	(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 (7.6 Quetzales = US\$1.00)	10.7 (13.3)
Total annual hours	1908 (1152)
Total annual income	19212 (21282)
Human capital measures	
(I ₁) Completed grades of schooling	4.7 (3.5)
(I ₂) Adult Reading-Comprehension Cognitive Skills (RCS)	36.2 (22.8)
(B ₁) Adult height (cm)	157.1 (8.5)
(B ₂) Adult fat-free body mass (kg)	45.5 (7.7)
Individual characteristics	
Male	0.54
Age in years at interview in 2002–04	32.5 (4.2)
Village effects	
San Juan	0.23
Espíritu Santo	0.21
Santo Domingo	0.25
Conacaste (reference)	0.31
Excluded Instruments	
<i>Prices and policies (P₁, P₂)</i>	
Student-teacher ratio at student age 7 years	40.1 (9.1)
Intent to treat nutritional intervention	
0–36 months	0.40
0–36 months * <i>Atole</i>	0.22
<i>Individual endowments (E_I)</i>	
Twin	0.01
<i>Family endowments (E_F)</i>	
Mothers' schooling attainment in grades	1.41 (1.7)
Mothers' height in cm	148.8 (4.5)
Mothers' height missing dummy	0.21
Fathers' schooling attainment in grades	1.70 (2.1)

Notes: Standard deviations in parentheses.

Table 3: First-stage estimates of human capital measures (N=962)

	(I ₁) Completed grades of schooling	(I ₂) Adult Reading- Comprehension Cognitive Skills (RCS Z score)	(B ₁) Ln adult height	(B ₂) Ln adult fat-free body mass
Second-stage controls				
Male (E _I)	0.809 (0.227)**	0.168 (0.069)*	0.078 (0.002)**	0.279 (0.006)**
Age (A ₂)	-0.017 (0.025)	-0.011 (0.007)	-0.000 (0.000)	0.000 (0.001)
San Juan (E _V)	1.225 (0.249)**	0.340 (0.074)**	-0.004 (0.003)	-0.011 (0.010)
Espiritu Santo (E _V)	2.138 (0.321)**	0.321 (0.101)**	-0.001 (0.003)	-0.003 (0.010)
Santo Domingo (E _V)	0.716 (0.352)*	0.162 (0.103)	-0.004 (0.003)	-0.021 (0.010)*
Excluded instruments				
<i>Prices and policies (P₁, P₂)</i>				
Student-teacher ratio at age 7	-0.032 (0.012)*	-0.012 (0.004)*	-0.000 (0.000)	0.000 (0.000)
Exposure to either intervention 0-36 months	0.162 (0.321)	-0.010 (0.072)	-0.000 (0.003)	0.002 (0.006)
Exposure to atole intervention 0-36 months	0.266 (0.400)	0.196 (0.104)+	0.002 (0.004)	-0.005 (0.011)
<i>Individual endowments (E_I)</i>				
Twins	-0.984 (0.793)	-0.145 (0.376)	-0.034 (0.007)**	-0.076 (0.024)**
<i>Family endowments (E_F)</i>				
Mother's completed grades	0.436 (0.073)**	0.119 (0.018)**	0.001 (0.001)	0.001 (0.002)
Ln mother's height	13.903 (3.472)**	3.792 (1.275)**	0.511 (0.041)**	0.893 (0.099)**
Mother's height missing	0.045 (0.214)	0.118 (0.053)*	0.005 (0.003)	0.008 (0.008)
Father's completed grades	0.341 (0.059)**	0.075 (0.015)**	-0.000 (0.000)	-0.002 (0.002)
Constant	-65.601 (17.438)**	-18.774 (6.429)**	2.462 (0.205)**	-0.825 (0.498)
R ²	0.22	0.15	0.61	0.69
F-statistic: excluded instruments	22.8	17.1	32.6	20.9
p-value	[<0.01]	[<0.01]	[<0.01]	[<0.01]

Notes: Ordinary least squares estimates using all non-missing observations for each specification. Standard errors calculated allowing for clustering at the birth- year and village-of-origin cohort level (64 clusters) shown in parentheses. * indicates significance at 5%, ** at 1%, and + at 6.4%.

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

	OLS	IV	OLS	IV
(I ₁) Completed grades of schooling	0.084 (0.008)**	0.118 (0.024)**		
(I ₂) Adult RCS Z scores			0.228 (0.027)**	0.432 (0.088)**
(B ₁) ln adult height	2.117 (0.792)**	-2.333 (2.078)		
(B ₂) ln adult fat-free body mass			1.079 (0.294)**	-1.255 (1.079)
Male	0.207 (0.078)*	0.526 (0.157)**	0.102 (0.095)	0.715 (0.298)*
Age	0.014 (0.007)*	0.019 (0.006)**	0.012 (0.007)	0.022 (0.006)**
R ² /Centered R ²	0.19	0.15	0.16	0.07
Kleibergen-Paap test		14.12		8.83
Hansen J test		2.58		1.95
p-value		[0.86]		[0.92]

Notes: Instrumental variables GMM estimates using instruments and controls indicated in Table 3. Standard errors calculated allowing for clustering at the birth-year and village-of-origin cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

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Table 5: Ln annual hours production functions

	OLS	IV	OLS	IV
(I ₁) Completed grades of schooling	0.023 (0.009)*	0.111 (0.030)**		
(I ₂) Adult RCS Z scores			0.085 (0.034)*	0.402 (0.102)**
(B ₁) ln adult height	1.675 (0.894)	-3.228 (2.369)		
(B ₂) ln adult fat free body mass			0.402 (0.367)	-1.714 (1.212)
Male	0.917 (0.083)**	1.222 (0.182)**	0.949 (0.111)**	1.494 (0.333)**
Age	0.008 (0.007)	0.016 (0.007)*	0.007 (0.007)	0.019 (0.008)*
Observations	979	979	962	962
R ² /Centered R ²	0.22	0.15	0.22	0.14
Kleibergen-Paap test		14.36		9.16
Hansen J test		3.35		4.23
p-value		[0.76]		[0.65]

Notes: Ordinary least squares estimates (OLS) and instrumental variables GMM estimates using instruments and controls indicated in Table 3. Standard errors calculated allowing for clustering at the birth-year and village-of-origin cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

Appendix Table 1: Ln hourly wage production functions, by sex (IV-GMM)

	Male		Female	
(I ₁) Completed grades of schooling	0.114 (0.024)**		0.107 (0.028)**	
(I ₂) Adult RCS Z scores		0.444 (0.090)**		0.508 (0.131)**
(B ₁) ln adult height	-1.440 (1.939)		-1.712 (3.103)	
(B ₂) ln adult fat-free body mass		-0.776 (1.029)		-1.838 (1.927)
Age	0.011 (0.007)	0.014 (0.008)	0.021 (0.009)*	0.033 (0.010)**
Observations	515	515	447	447
Kleibergen-Paap test	8.12	4.87	11.05	4.56
Hansen J test	8.43	9.14	3.75	2.11
p-value	[0.21]	[0.17]	[0.71]	[0.91]

Notes: Instrumental variables GMM estimates using instruments and controls indicated in Table 3. Standard errors calculated allowing for clustering at the birth-year and village-of-origin 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)

(I ₁) Completed grades of schooling	0.124 (0.025)**	
(I ₂) Adult RCS Z scores		0.431 (0.089)**
(B ₁) ln adult height	-3.371 (2.102)	
(B ₂) ln adult fat-free body mass		-1.884 (1.123)
Male	0.605 (0.154)**	0.886 (0.303)**
Age	0.018 (0.006)**	0.020 (0.006)**
Kleibergen-Paap test	13.04	8.92
Hansen J test	3.34	2.38
p-value	[0.77]	[0.88]

Notes: Weighted instrumental variables GMM estimates using instruments and controls shown in Table 3. Standard errors calculated allowing for clustering at the birth-year and village-of-origin cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

Appendix Table 3: Ln hourly wage rate production functions: Select sample of HCS respondents re-interviewed in 2006 as part of IGT study

(I ₁) Completed grades of schooling	0.110 (0.027)**	
(I ₂) Adult RCS Z scores		0.436 (0.093)**
(B ₁) ln adult height	-3.010 (2.356)	
(B ₂) ln adult fat-free body mass		-1.332 (1.155)
Male	0.650 (0.182)**	0.817 (0.330)*
Age	0.033 (0.014)*	0.036 (0.016)*
Observations	541	534
Kleibergen-Paap test	9.06	6.13
Hansen J test	4.27	1.79
p-value	[0.64]	[0.94]

Notes: Instrumental variables GMM estimates using instruments and controls shown in Table 3. Standard errors calculated allowing for clustering at the birth-year and village-of-origin cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.

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

	Men and Women		Women only		Men only	
	OLS	IV	OLS	IV	OLS	IV
(I ₁) Completed grades of schooling	0.117 (0.027)**	0.368 (0.200)	0.132 (0.050)**	0.296 (0.659)	0.114 (0.032)**	0.318 (0.148)*
(B ₁) ln adult height	1.626 (1.558)	10.840 (17.343)	-0.484 (2.747)	4.092 (44.980)	3.575 (1.777)*	6.083 (12.529)
Age	-0.035 (0.008)**	-0.015 (0.016)	-0.028 (0.012)*	-0.023 (0.053)	-0.035 (0.008)**	-0.023 (0.013)
Male	0.003 (0.168)					
Observations	367	367	167	167	200	200
R ²	0.12		0.07		0.19	
Kleibergen-Paap test		0.97		0.21		1.27
Hansen J test		1.75		1.28		1.86
p-value		[0.63]		[0.73]		[0.61]

Notes: Controls not shown include a constant and village-of-origin fixed effects. 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	
(I ₁) Completed grades of schooling	0.129 (0.024)**		0.104 (0.020)**	
(I ₂) Adult RCS Z scores		0.474 (0.089)**		0.375 (0.071)**
(B ₁) ln adult height	-2.789 (2.151)		-2.783 (1.571)	
(B ₂) ln adult fat-free body mass		-1.555 (1.114)		-1.554 (0.866)
Male	0.636 (0.162)**	0.873 (0.306)**	0.570 (0.125)**	0.795 (0.243)**
Age	0.016 (0.006)**	0.020 (0.007)**	0.016 (0.006)**	0.020 (0.006)**
Kleibergen-Paap test	14.17	8.91	14.88	9.10
Hansen J test	2.42	1.87	3.76	3.23
p-value	[0.88]	[0.93]	[0.71]	[0.78]

Notes: Instrumental variables GMM estimates using instruments and controls shown in Table 3. Standard errors calculated allowing for clustering at the birth-year and village-of-origin cohort level (64 clusters) shown in parentheses. * indicates significance at 5% and ** at 1%.