Are Returns to Mothers' Human Capital Realized in the Next Generation? The Impact of Mother's Schooling and Long-Run Nutritional Status on Child Human Capital in Guatemala

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21 September 2006

*The authors are listed alphabetically. This study was supported by NIH/Fogarty grant TW-05598 on "Early Nutrition, Human Capital and Economic Productivity," NSF/Economics grants SES 0136616 and SES 0211404 on "Collaborative Research: Nutritional Investments in Children, Adult Human Capital and Adult Productivities," NIH grant HD046125 on "Education and Health over the Life Course in Guatemala," R01 HD045627-01 grant on "Resource Flows Among Three Generations in Guatemala" NIH/NIA grant P30 AG12836 to PARC at the University of Pennsylvania, the Boettner Center for Pensions and Retirement Security at the University of Pennsylvania, and the Mellon Foundation grant to the Population Studies Center of the University of Pennsylvania. The authors thank their colleagues on the larger project of which this study is a part and Meng Wang for excellent research assistance in the preparation of the data for this paper. The corresponding author is Agnes R. Quisumbing A.Quisumbing@cgiar.org..

Abstract:

There are many estimates in the literature of significant cross-sectional positive associations between maternal human capital – usually represented by schooling attainment and, in some cases, by height (as an indicator of long-run nutritional status) – and child human capital. But almost all of this literature ignores possible estimation biases due to maternal human capital being behaviorally determined in the presence of intergenerationally-correlated endowments. A small number of recent studies, primarily on developed economies, report that treating maternal schooling attainment as behaviorally determined affects substantially (generally reducing) the estimated causal impact of maternal schooling on child outcomes. No previous studies consider what happens to estimates of the impact of maternal long-run nutritional status if this "biological" component of maternal human capital is treated as behaviorally determined. The contribution of this paper is to explore, for the first time, how estimates of the impact of both maternal schooling attainment and maternal long-run nutritional status on child human capital are affected if both of these components of maternal human capital are treated as behaviorially determined, using an unusually rich longitudinal data set collected over 35 years in Guatemala. The estimates are provocative. They suggest that the standard procedures in which maternal human capital is treated as exogenous may yield misleading coefficient estimates for the impacts of maternal human capital on child human capital in the Guatemalan context. For maternal schooling, our results suggest that the OLS estimates may understate slightly the impact on grades of schooling relative to the age-cohort mean, but may overstate substantially the magnitude and the significance of the effect on being ever-schooled. For maternal height, the estimates suggest that, for all but one of the child outcomes considered, the OLS estimates understate, in some cases substantially, the causal impact of maternal long-run nutritional status on both anthropometric and schooling outcomes in children. The estimates imply, thus, not only that in a number of cases are the standard estimates likely to be misleading due to endogeneity of maternal human capital, but they are likely to understate the importance of long-run maternal nutritional status relative to maternal schooling attainment in determining child human capital.

1. Introduction

Investments in women's human capital often are justified because of presumed large positive causal effects on the next generation. Several influential papers and books argue that the effect of maternal human capital on child health and education is large and causal (e.g., Summers 1992, 1994; Stern 2001; World Bank 2001). Returns to investments in women through childbearing and childrearing could be realized through a number of pathways. First, better maternal nutritional status prior to and during pregnancy through "biological" pathways may lead to better nutrition *in utero* and higher birth weights of her children, which result in healthier offspring over their life cycles. Second, more maternal human capital may be associated with shifts from child quantity to child quality, in part because more maternal human capital raises the opportunity costs of women's time for investing in child quantity versus child quality. Third, more maternal schooling may make women more aware of, and more likely to engage in, behaviors that result in better-educated and healthier children. These behaviors could include those related to the nutrition and care of children, such as breastfeeding and proper diet, as well as behaviors that enhance the intellectual development and school performance of their children. Though all three of these pathways, improved maternal human capital is thought by many to result in improved health and schooling outcomes of children that in turn result in higher lifetime incomes of the next generation.

Many studies, both for developing and developed countries, present positive associations between maternal human capital and child human capital that are consistent with the importance of these various pathways. However, within a dynamic life-cycle framework, schooling and adult nutritional status reflect behavioural choices that depend on observed and unobserved individual and family background and other characteristics. Some individuals and some families may have unobserved characteristics, such as ability and motivation for schooling that are rewarded in labor markets, or better health-seeking behaviours and greater food availability, that lead to greater investments in schooling and better nutritional status. If the estimation methods used do not control for the behavioural determinants of schooling and adult nutritional status, the estimated associations of maternal schooling and nutritional status with outcomes in the child generation may be largely results of the impact of such unobserved factors on all of these behavioural outcomes.

A small subset of articles, primarily recent studies for developing countries, have investigated what happens to estimates of the impact of maternal human capital – in particular, maternal schooling attainment – on child outcomes if there is control for the behavioural determination of maternal human capital within a life-cycle framework with unobservables such as innate ability and health. For example, Behrman and Rosenzweig (2002, 2005) report that the positive association between mothers' and child schooling in cross-sectional estimates evaporates or possibly becomes negative once there is control for innate ability and marriage market matching using special data on identical twin mothers in the United States, perhaps because increasing the schooling of women in this context induces a shift from time spent on child care to time spent in the labor market.[1] Likewise Plug (2004) uses data on adoptees in the United States to investigate the intergenerational schooling relation and finds that the positive association between maternal schooling and their children's schooling in cross-sectional estimates disappears if maternal schooling is treated as endogenous using adoption as instrument. Similarly, Black, Devereux and Salvanes (2005) use phased-in changes in compulsory schooling in Norway as an instrument for endogenizing parental schooling; despite strong cross-sectional associations between parental and child schooling, they find little evidence of causal effects of parental schooling, though the impact of mothers' schooling on sons' schooling remains significant. We are aware of only two articles for developing countries that investigate what happens to the estimated impact of mothers' schooling on child schooling or health when mothers' health is treated as endogenous. These are two studies from about two decades ago that use data on adult sisters in Nicaragua to control for the common genetic environment and parental family environment shared by siblings (Behrman and Wolfe 1987a, b). Both of these studies find that most of the association between maternal education and child human capital in the form of health and schooling in Nicaragua reflects family background and genetic endowments, not causal effects of maternal schooling per se.

We are not aware of studies that investigate what happens if maternal long-run nutritional status is treated as endogenous in relations that determine child human capital in a parallel fashion. However, we are aware of one study in progress that finds that when long-run nutritional status is treated as endogenously determined for data from Guatemala, what appears to be a strong positive role of long-run nutritional status in OLS wage production function estimates evaporates (Behrman *et al.* 2005). This result suggests the possibility that the treatment of maternal long-run nutritional status as endogenous in intergenerational relations for child human capital may also change the estimated effect substantially.

Thus, there are large literatures for developing and developed economies that indicate strong positive cross-sectional associations between maternal human capital – both schooling attainment and long-run nutritional status – and child human capital. But the vast majority of these studies do not control for the endogenous determination of maternal human capital with persistent intergenerationally-correlated unobservables such as genetic endowments and home environments. A few studies for developed countries and a smaller number for developing countries find that the estimated impact of maternal schooling on child human capital lessens or disappears when special data are used to control for the endogeneity of maternal schooling. No such studies of which we are aware address the impact of maternal nutritional status on children's human capital.

In this study, we investigate the impact of maternal schooling attainment and long-run nutritional status at the life-cycle stage in which initial parenting decisions on a number

[1] Research in sociology (e.g. Bianchi 2000, 2005) suggests that "what gives first" when educated women enter the labor market is domestic labor, time with spouse, and leisure time spent alone.

of child human capital outcomes tend to be made in the society under consideration. We use unusually rich data collected over 35 years in a particular developing country context, Guatemala. Specifically, we examine the impact of years of maternal schooling and long-run nutritional status on children's anthropometry at birth (birth weight, birth length), nutritional status at 36 months (z-scores for length-for-age, weight-for-age, and weight-for- length), and schooling (deviation of grades completed from the age-cohort mean, whether the child ever attended school). We advance beyond almost all of the previous literature because we treat maternal schooling as behaviourally determined and advance beyond all of the previous literature because we also treat long-run nutritional status as behaviourally determined.

We organize this study by first presenting a conceptual framework (Section 2), then presenting the data (Section 3), then presenting our alternative estimates for each of a number of outcomes (Section 4), and finally concluding (Section 5).

2. Conceptual Framework

Our conceptual framework considers the life cycle to have a series of stages. One of those stages is adolescence-young adulthood, during which time (for the society under consideration) most individuals initiate first unions, parenting, and child rearing. Women have a vector of human capital stocks (K) that includes schooling attainment and long-run nutritional status and that determines the results of their union formation in terms of spousal characteristics and child's human capital. Let Y be a vector of child human capital outcomes – e.g health and schooling of children. The basic interest of this study is to estimate how Y depends on maternal schooling and long-run nutritional status (K), measured at the ages at which first parenting decisions are made in the society under study. We posit that there is a linear approximation to what determines Y given maternal human capital stocks (K) as well as predetermined observed individual characteristics (I) of the child such as multiple birth and gender and unobserved inherent endowments (E_0), such as innate ability and health that are correlated across generations and a vector of stochastic disturbance terms, one each for each different outcome (V):

$$(1)\ Y = a_0 + a_1 K + a_2 I + a_3 E_0 + V,$$

where the a_i are matrices of coefficients to be estimated. Note that this relation pertains to the gross effects of maternal human capital on child human capital, part of which work through mate selection in addition to direct effects conditional on mate characteristics.

The questions that are posed in the introduction pertain to obtaining good (consistent) estimates of the coefficients of maternal schooling and long-run nutrition in relation (1), which are the two components of K, for each of the components in Y. But estimation of relation (1) is a challenge because parenting behaviors that result, say in better nutritional status or schooling outcomes of children all reflect previous behavioral choices so OLS or similar estimates of relation (1) are likely be inconsistent due to endogeneity of maternal human capital, particularly if there are intergenerationally-corrrelated endowments, such as genes.

To deal with these possible estimation problems, we first assume that individuals and their parental families make investments in prior life-cycle stages that determine the components of K. These investments are made within a dynamic, reduced-form demand context, given initial conditions (including parental family background F_0, initial community prices and policies C_0, genetic and other endowments E_0, and individual characteristics I_0 such as age) and changes that occurred over time such as those in markets, policies and other conditions ΔC (conditional on the individual's birth year and subsequent age):

(2) $K = K(F_0, C_0, E_0, I_0, \Delta C, W)$, where Δ refers to observed changes or shocks from the initial conditions to critical ages for the determination of K, and W refers to unobserved idiosyncratic influences.

This expression encapsulates the results of many decisions that parents, and then increasingly the children themselves, make over the adolescent/young adult's life cycle, given initial conditions and time-varying factors outside of the control of the family. The elements in relation (2) are in general vectors of opportunities and constraints to which the family responds. One example is the genetic endowments (E_0), a vector that includes innate "ability" endowments related to learning and "physical" endowments related to physical growth.[2]

Consideration of relations (1) and (2) illustrates the two important aspects of the problems of estimates in the literature of relation (1) that are mentioned in the introduction. First, mother's human capital (schooling and long-run nutritional status) are determined in part by genetic and other endowments (E_0) that also are posited to have direct effects on the outcomes of interest in relation (1) directly or because there are intergenerational correlations between maternal and child endowments. This means that to obtain consistent estimates of the impacts of maternal schooling and long-run nutritional status on the child outcomes of interest, some combination of data and estimation method must be used to avoid the bias that otherwise would result from correlations between schooling, long-run nutritional status, and the expanded compound error term in relation (1) that includes unobserved genetic and other endowments (E_0) in addition to the idiosyncratic error (V). This problem can be addressed in principle by direct control for genetic and other endowments (E_0) or by instrumental variable (IV) estimates in which the components of maternal human capital (K) in relation (1) are replaced by their predicted values from relation (2), which are not correlated with the unobserved E_0 and therefore not correlated with the compound disturbance in relation (1). In the present (and almost all) data sets, only the second of these is an option, which we

[2] These various endowments may be significantly correlated but they need not be positively correlated. A recent study for the United States, for example, finds that the endowments related to schooling and earnings on one hand and those related to physical health on the other, are negatively correlated (Behrman and Rosenzweig 2004). If that were the case in this study, the failure to control for genetic endowments could result, for example, in an overestimate of the effect of maternal schooling on child schooling, but an underestimate of the effect of maternal nutritional status on child schooling.

will explore.[3] Second, the components of K are determined in part by the same initial conditions (F_0, C_0, E_0, I_0) and some common observed community changes ΔC and unobserved influences W. This means that maternal schooling and long-run nutritional status are likely to be correlated, with the result that estimates that do not control for both of these components of K—as in most of the literature—are likely to be subject to omitted variable bias because of the failure to control for the other components of K.

3. Data

The data demands are considerable for estimating the relations posited in Section 2. We utilize an unusually rich longitudinal data set collected over a 35-year period with parenting histories, child health and schooling outcomes, alternative measures of own human capital, family background, and shocks from an experimental nutrition intervention as well as market and policy changes. We first provide a brief general description of the data[4] and then focus on the particular variables that we use in the analysis.

Section 3.1 General Description of the Data

In the early and mid-1960s, protein deficiency was seen as the single most important nutritional problem facing the poor in developing countries, and there was considerable interest in the possibility that this deficiency affected children's ability to learn. The Institute of Nutrition for Central America and Panama (INCAP), based in Guatemala, became the locus of a series of preliminary studies on this subject in the latter half of the 1960s (see Habicht and Martorell 1992, Martorell, Habicht and Rivera 1995 and, especially, Read and Habicht 1992). These preliminary studies informed the development of a larger scale supplementation trial that began in 1969.

The data used in this study are from that larger supplementation trial, collected for individuals who were 0-7 years old during 1969-77 in four villages in Eastern Guatemala. Three villages—San Juan, Conacaste, and Santo Domingo—are located in mountainous areas with shallow soils, while the agricultural potential of Espíritu Santo, located in a river valley, is somewhat higher. All four villages are located relatively close to the Atlantic Highway, connecting Guatemala City to Guatemala's Caribbean coast—from 36km to 102km from Guatemala City. From January 1969 to February 1977 INCAP implemented a nutritional supplementation trial in these four villages, together with data collection on child growth and development.[5] The data collection focused on all village children aged seven years or less and all pregnant and lactating women. Cohorts of

[3] Within-sibling estimates could be used to control for the average family genetic endowments as in Behrman and Wolfe (1987a,b), but previous studies suggest that the individual-specific deviations from those family averages have important impact on human capital investments (Behrman, Rosenzweig and Taubman 1994, 1996).

[4] For more extensive discussion, see Grajeda *et al.* (2005), Maluccio *et al.* (2005), Maluccio, Murphy and Yount (2005), Martorell *et al.* (2005), Quisumbing *et al.* (2005), Ramakrishna *et al.* (2005) and Stein *et al.* (2005).

[5] The intervention began in the larger villages, Santo Domingo and Conacaste, in February 1969, and in the smaller villages, Espíritu Santo and San Juan, in May 1969.

newborns were included until September 1977. Data collection for individual children ceased when they reached seven years of age. The birth years of children included in the 1969-77 longitudinal data collection thus range from 1962 to 1977, so when the intervention ended, their ages ranged from 0 to 15 years.[6] Therefore, the length and timing of exposure to the nutritional interventions (described below) for particular children depended on their respective birth dates. For example, only children born after mid-1968 and before October 1974 were exposed to the nutritional intervention for all of the time they were from six to 36 months of age, which often is posited to be a critical time period for child growth in the nutrition literature (see Maluccio *et al.* 2006, Martorell, Habicht and Rivera 1995 and Martorell *et al.* 2005 and the references therein)

The principal hypothesis underlying the intervention was that improved pre-school nutrition accelerates physical growth and mental development. To test this hypothesis, 300 villages were screened to identify those of appropriate size, compactness (so as to facilitate access to feeding stations, health centers and psychological testing sites, see below), ethnicity, diet, educational levels, demographic characteristics, nutritional status and degree of physical isolation. From this screening, village pairs similar in these characteristics were determined: Conacaste and Santo Domingo (relatively populous villages) and San Juan and Espíritu Santo (relatively less populous villages).

Two villages, Conacaste and San Juan, were randomly assigned to receive a high protein-energy drink, *Atole* as a dietary supplement. *Atole* contained Incaparina (a vegetable protein mixture developed by INCAP), dry skim milk, and sugar and had 163 kcal and 11.5 g of protein per 180 ml cup. This design reflected the prevailing view of the 1960's that protein was the critically limiting nutrient in most developing countries. *Atole*, the Guatemalan name for hot maize gruel, was pale gray-green and slightly gritty, but with a sweet taste.

In designing the data collection, there was considerable concern that the social stimulation associated with attending feeding centers—such as the observation of children's nutritional status, the monitoring of their intakes of *Atole* and so on—also might affect child nutritional outcomes, thus confounding efforts to understand the impact of the supplement. To address this concern, in the remaining villages of Santo Domingo and Espíritu Santo, an alternative drink, *Fresco*, was provided. *Fresco* was a cool, clear-colored, fruit-flavored drink. It contained no protein and only sufficient sugar and flavoring agents for palatability. It contained fewer calories per cup (59 kcal/180 ml) than *Atole.* Several micronutrients were added to the *Atole* and *Fresco* in amounts that achieved equal concentrations per unit volume. This addition was made to sharpen the contrast between the drinks to protein; the energy content differed, of course, but this was not recognized to be of central importance at the time.

The nutritional supplements (i.e., *Atole* or *Fresco*) were distributed in supplementation centers and were available daily, on a voluntary basis, to all members of the community

[6] Though the number of villages is small, the number of village-birth-year cohorts is 64, which provides a reasonable number of clusters for estimation of standard errors that account for clustering. See Maluccio *et al.* (2006) and Behrman *et al.* (2006).

during times that were convenient to mothers and children but that did not interfere with usual meal times. For this study, with the differential "intent to treat" exposure to these nutritional supplements as first-stage instruments to estimate relation (2), a critical question is to what extent the intervention design resulted in differences in access to calories, proteins and other nutrients. In addressing this question, we can exploit the intensive nature of the survey and observational work associated with the intervention. Averaging over all children in the *Atole* villages (i.e., both those that consumed any supplement and those who never consumed any), children 0-12 months consumed approximately 40-60 kcal per day, children 12-24 months consumed 60-100 kcal daily and children 24-36 months consumed 100-120 kcal per day as supplement (Schroeder, Kaplowitz and Martorell, 1992, Figure 4). Children in the *Fresco* villages, in contrast, consumed virtually no *Fresco* between the ages of 0-24 months (averaging at most 20kcal per day) with this figure rising to approximately 30 kcal daily by age 36 months (Schroeder, Kaplowitz and Martorell, 1992, Figure 4). A program of primary medical care was provided free of charge throughout the period of data collection. Periodic preventive health services, such as immunization and deworming campaigns, were conducted in all villages. Micronutrient intakes from the supplements were also larger for *Atole* than *Fresco* villages; also, the *Atole* contributed significant amounts of high-quality protein, while the *Fresco* contributed none. This population has been studied intensively in the years since the original data collection, with particular emphasis on the impact of the nutritional intervention (Martorell *et al.* 2005 gives references to many of these studies).

Multidisciplinary research teams conducted several follow-up studies on participants in the 1969-77 study as well as on their children. The first conducted in 1987-88 targeted the same individuals born between 1962 and 1977 who had been participants in the INCAP longitudinal study and were 11 to 26 years of age in 1988. It was feasible to include those who remained in the original study villages and only those migrants who moved to Guatemala City and to the provincial capital of the study area. This study focused on the impact of nutritional improvements in the critical period of gestation and the first three years of subsequent human capital formation as measured by body size, working capacity, maturation, intellectual functioning, school achievement, as well as family formation and occupation (Martorell *et al.*, 1995). Of the 2392 individuals 0-15 years old in the original 1969-77 data collection, 224 had died by 1987. Of the 2168 surviving subjects, 1574 participated in the 1987-88 study, a coverage rate of approximately 73%. When disaggregated by gender, the coverage was slightly higher for women (~76%) than for men (69%) (Rivera *et al.*, 1993).

Between 1991 and 1996, investigators conducted a surveillance of births (offspring of original subjects) in the four study villages (outmigrants were not studied). In 1996 the study was expanded to include a surveillance of pregnancies and to carry out a longitudinal study of these offspring. Between 1996-1999 in what is called the Generational Effects study, all recognized pregnancies in the four villages were identified and followed through intensive surveillance, and information was collected on the pregnancies and the children born during that time period. Children (including those born prior to the launch of the Generational Effects study) were followed to age 3 years or study closeout, whichever came first. Data included birth weight and length, weight and length at periodic intervals, morbidity and health-seeking behavior, breastfeeding, consumption of complementary foods, mother's functional competence and intellectual functioning, and mother-child interaction (Ramakrishnan *et al.*, 1999). Between 1991 and 1999, 698 children of 392 women who were original study participants were measured at birth. From 1996-1999, 573 children of 364 original participant mothers were routinely followed for anthropometry measurements through age 3 years.

Finally, a multidisciplinary team of investigators, including the authors of this paper, undertook follow-up data collection in 2002-4 on all participants in the 1969-77 data collection. In 2002-4, sample members ranged from 25 to 42 years of age. Figure 1 shows what happened to the 1162 women 0-15 years old in the original 1969-77 sample by the time of our 2002-4 data collection: 919 (79%) were alive and known to be living in Guatemala (10% had died, 6% had migrated abroad, 5% were not traceable). Of these 919, 521 lived in the original villages, 95 lived in nearby villages, 222 lived in or near to Guatemala City, and 81 lived elsewhere in Guatemala. For the 919 traceable sample members living in Guatemala, 649 (71%) finished the complete battery of applicable interviews and measurements and 818 (89%) completed at least one interview during the 2002-4 data collection (Grajeda *et al.* 2005).

During each major study, a census of the four villages was conducted: in 1967, 1975, 1987, 1996, and 2002. This census information contains valuable information on the family background of the original subjects from the 1969-1977 study, including their parents' age, schooling attainment, and asset-holdings. The census in more recent years is an additional source of information about their children, including attendance and completed grades of schooling. Using the 1996 and 2002 censuses, we are able to identify 1318 children over age 7 whose 558 mothers were participants in the original study.

From this point forward, we refer to the original study subjects, who were age 0-7 in 1969-1977, as our middle generation, or G2. We refer to their parents as the first generation, or G1, and to their children as the third generation, or G3. We draw upon information on all three generations in the analyses. While this greatly enriches our analyses, it also increases the chance of attrition. Of the 919 potential female subjects in 2002-4, 679 (74%) have information on schooling and fertility from the 2002-4 study and have had at least one live birth. However, only 517 (56%) additionally have late adolescent height to represent long-run nutritional status, calculated from information collected during either the 1988 or 2002-4 studies, depending on age. These requirements for G2 inclusion into the analyses, coupled with those for G3 inclusion reduce our sample sizes available for analysis. For example, G2 mothers in our analyses on G3 anthropometry at birth had to have completed the fertility and schooling questionnaires during the 2002-4 study, have height information from either the 2002-4 or 1987-8 studies, have had at least one live birth that must have taken place between 1991-1999 while the G2 mother was living in one of the four study villages. These various requirements reduce the analysis sample to 532 G3 children of 298 G2 mothers (32% of the 919 potential female subjects in 2002-4 and 58% of the 517 women who had data on their schooling, height and fertility, and at least one live birth). Likewise, our analyses of the determinants of G3 nutritional status at 36-months include 277 mothers and 432 of their children, and our analyses of G3 schooling include 384 mothers and 916 of their children. The high rates of attrition are troubling because they reduce sample size; but more troubling is the potential that this attrition is non-random, especially considering that subjects who do appear in our analyses are a select group of G2 mothers who were generally non-migrants: present in the four communities during the intervention and in the 1990's, and who were accessible (though not necessarily in the communities) from 2002-4. We will consider possible attrition bias in the next revision of this paper, using methods parallel to those in Fitzgerald, Gottschak and Moffitt (1998). We note that such explorations in other studies on these data (e.g., Behrman *et al*. 2006, Maluccio *et al.* 2006) do not find large impacts of attrition on the estimated coefficients. A number of these G2 individuals, finally, are siblings or half-siblings so we control for mother cluster effects in the estimation of the standard errors that are reported in Tables 2-4 below.

[Figure 1 about here]

Section 3.2 Central Variables for the Analysis

Tables 1a and 1b present means and standard deviations (SD) for the outcome variables, explanatory variables, and instruments, respectively. .

[Tables 1a & b about here]

Dependent Variables: G3 Children Outcomes (Y):

(1) Birth Anthropometry: birth length (cm) and weight (kg) collected during 1991-1999 in four villages. The mean birth weight is above the standard cutoff for low birth weight of 2.5 kg, but there is a fair amount of variance, and 13% of the births were below this low birth weight cutoff.

(2) Nutritional status at 36-months: represented by length for age, weight for age, and weight for length z-scores from the 1996-1999 study. Not all G3 children were measured at exactly 36-months. Dummy variables for age at measurement were regressed upon all z-scores in order to generate correction coefficients. These corrections were then made to the z-scores closest to when the child was 36 months. These scores suggest, not surprisingly, a population that is relatively malnourished in comparison with the reference population, particularly with regard to stunting (LAZ), which generally is considered to be an indicator of the long-run impact of early childhood nutrition on subsequent development. Indeed, 43% of the children have LAZ values below -2.0, which is the standard cutoff used in the literature for severe stunting.

(3) Schooling of children by 2002-4: Ever having attended school, and the difference in grades of schooling completed from the age-cohort mean for all children over age 7, taken from the 1996 and 2002-4 census. The difference

from mean measure of schooling is positive when a child has more schooling than the cohort mean. A tenth of the G3 children aged 7-23 who we were able to link with their G2 mother via the community censuses had not entered school at the time of the survey.

Right-side Endogenous Variables:

G2 Capital Stocks At or Prior to First Parenting (K):

1) Maternal Schooling: Completed formal schooling attainment, as measured in 2002-4. Since in our population most individuals complete their schooling during adolescence, we use the 2002-4 adult measure as our best approximation of schooling before first parenting. Of all 2002-4 study participants, two-thirds completed their schooling by age 13 and over four-fifths by age 15. Additionally, only 4% initiated their first union (which for most signals impending first parenthood) prior to completing schooling.

2) Maternal Long-Run Nutritional Status: Height is widely used to represent long-run nutritional status (e.g., see Behrman *et al.* 2005 and the references therein). We use maternal height(cm) at age 18, by which age most females have attained their adult height. We use a combination of the 1988 and 2002-4 data to construct this variable, taking the 1988 measure for those older than 18 in 1988, and the 2002-4 measures for those aged 11-18 in 1988.

Right-side G3 Individual Characteristics (I):

1) Gender (male=1)

2) Multiple Birth

Initial Conditions (F_0, C_0, E_0, I_0) for IV Estimates:

G2 parental characteristics and family background (F_0): We include parental characteristics including the G1 mother's and father's schooling attainments, and a constructed socioeconomic status score that is the first principal component of both the assets owned and the housing characteristics of the G1 household in 1975 (Maluccio, Murphy and Yount 2005). We also include a dummy variable for whether the G2 suffered the death of either G1 parent before age 15.

Community characteristics during G2's childhood (C_0): Communities differ significantly in their learning environments, in part because of different experiences of prior generations regarding schooling and occupational structure (Bergeron 1992; Maluccio, *et al*. 2005). We control for the permanent, or fixed, dimensions of these differences by including village fixed effects in the first stage regressions.

Genetic endowments (E_0): We do not have direct observations on genetic endowments beyond the sex of individual, and in these analyses we use only G2 mothers.

Observed individual characteristics (I_0): These include age at the time of the 2002-4 interview and whether the individual was a twin, which may have longer-run implications associated with the generally lower birth weight of twins (e.g., Behrman and Rosenzweig 2004).

Observed shocks and events (ΔC):

Natural, market or policy events (ΔC): We construct variables at the community level that relate as closely as possible to the timing of key decisions in the mothers' human capital development. For example, using information reported in earlier work about infrastructure, markets and services in the villages (Pivaral 1972; Bergeron 1992), complemented with a retrospective study in 2002 (Estudio 1360, 2002), we construct variables such as student-teacher ratios (a proxy for school quality) when the mothers most likely started their schooling, age 7, the availability of a permanent primary school structure as well as work in local markets when the mothers most likely were making the decision to continue schooling or to join the work force, age 15. The variable reflecting work availability in local markets is equal to one if a "boom" was occurring in any local market: *yuquilla* production in San Juan, vegetable cooperatives in Conacaste, or intensive hiring of community members at a cement factory near to Conacaste and Santo Domingo. Thus, while reflecting community level characteristics, these variables vary by single-year age cohorts within each village, as well as across villages. Since this measure more closely relates the availability and longevity of schools and markets to the period in an woman's life when critical decisions (e.g., attending school, working in the labor market) were being made, it is an improvement over the more typical approach of including indicators about such factors in a given year for a population with different ages at that point.

Experimental nutritional shocks (ΔC): The set of observed nutritional shocks that we consider relate to the nutritional interventions underlying the original study (see Section 3.1). We construct two measures of exposure to the interventions based entirely on the birth year of the mother, the dates of operation of the interventions, and where the mother lived as a child. The first is a dummy variable control for cohort effects and the second is the exposure to the *Atole* treatment for that cohort. For each mother, we determine whether she was exposed to *either* intervention for the entire period from birth to 36 months of age. The *Atole* intervention indicator is then calculated by multiplying the cohort measure by a dummy variable indicator of whether or not the mother as a child lived in one of the two *Atole* villages. We include these two types of measures separately.

4. Results

Tables 2, 3, and 4 present both OLS and IV estimates of the impact of mother's schooling and height on a number of next-generation outcomes: anthropometry at birth, z-scores at

36 months, and schooling. Appendix 1 presents first-stage results for endogenous right-side variables, mother's schooling and height, in selected G3 outcome regressions, weight at birth, length-for-age z-score at 36 months, and years of schooling, defined as the deviation from the age-cohort mean. Instruments that are excluded from the second stage include dummy variables for exposure to the intervention during critical ages when the mother was an infant, village dummies, birth year, the student-teacher ratio when the mother was age 7, whether a permanent school was in the community when the mother was age 15, an age when she would have been contemplating schooling-work-marriage decisions, and the availability of local markets when the mother was age 15. Controls for family background include her mother's schooling, her father's schooling, her natal household's wealth index in 1975, and dummy variables indicating whether each of the above is missing, whether any parent of the G2 mother died before she reached 15, and whether the mother was a twin.

In all of the IV regressions, the F-test of the instruments excluded from the second stage indicate that the instrument set is jointly significant at less than the 0.000 level in predicting the endogenous right-side regressor. The Craig-Donald F-test for weak instruments exceeds the critical value of 4.45 (for two endogenous regressors and 17 excluded instruments), which implies a bias relative to OLS of less than 0.30 (Stock and Yogo 2002) for the estimates in Tables 2 and 4, though the apparent bias is somewhat greater in the estimates in Table 3. The p values for the Hansen J statistic for overidentification do not reject the null hypothesis that the instruments are independent of the second-stage disturbance term at the usual 0.05 significance level with the exception of the last estimate in Table 4 for ever-schooled (and, at the 0.10 significance level, for the first estimate in Table 3 for LAZ). Thus these diagnostics suggest that our IV estimates, which we prefer to OLS on a priori grounds because they attempt to deal with the behavioral determination of maternal human capital, generally are fairly satisfactory. Though we ideally would like estimates that performed better on the weak instrument test, nevertheless, if, despite a 0.30 bias relative to IV they suggest different impacts of maternal human capital on child human capital than do the standard OLS estimates, this suggests some reason for being concerned about standard estimates and how well they represent the true causal impact of maternal human capital on child human capital.

Comparisons of OLS and IV coefficient estimates for maternal schooling and height are of central interest to this paper because they reveal to what extent treating maternal human capital as endogenous affects the coefficients of the impacts of maternal human capital on child human capital. In the birth weight equation (Table 2), the coefficient estimate for mother's schooling increases (in absolute value) from -0.007 to -0.027 and is significant only at the 0.10 level in the IV estimates. The coefficient estimate for mother's height increases from 0.021 to 0.034 when schooling and height are endogenized, and is significant at the standard 0.05 level for both the OLS and IV estimates. In the birth length equation (Table 2), however, the coefficient estimate for mother's height drops from 0.105 to 0.078 when both maternal human capital measures are treated as endogenous and is only significant at the 0.10 level in the IV estimates. The coefficient estimate for mother's schooling in the birth length equation is not significant in either the OLS or IV regressions.

Table 3 presents OLS and IV estimates of length-for-age (LAZ), weight-for-age (WAZ) and weight-for-length (WLZ) z-scores at 36 months. Mother's schooling is not significant in any of the three outcomes, regardless of estimation procedure. Mother's height, however, exerts a positive and significant impact on all three outcomes (though only at the 0.10 level for WLZ). The coefficient estimates for mother's height for all three of these child anthropometric outcomes *increase* in magnitude if maternal human capital is treated as endogenous. The coefficient in the LAZ regression increases from 0.074 to 0.121; in the WAZ regression from 0.057 to 0.123, and in the WLZ regression, from 0.008 to 0.043. These increases suggest that failure to account for the endogeneity of mother's human capital stocks may underestimate the returns to mother's long-run nutritional status, at least in terms of child anthropometric outcomes.

Finally, we examine the impact of mother's human capital on two schooling outcomes in Table 4. Note that because the G3s under consideration can be as young as 7 years old (and range from 7 to 25 years of age), many children will not have completed school at the time of our data collection and completed grades of schooling is censored. To deal with the problem of age censoring, we examine the deviation of each child's completed grades of schooling from the age cohort mean, and whether the child ever attended school. As expected from the earlier literature, in the OLS estimates mother's schooling attainment has a positive and significant impact on whether the child ever attended school and his or her schooling relative to the cohort mean. Interestingly, in the OLS estimates for completed grades of schooling as a deviation from the age cohort mean, mother's height at age 18 also has a significantly positive coefficient estimate. The coefficient estimate for mother's schooling in the grades of schooling regression increases modestly from 0.12 to 0.16 when we take both schooling and height as endogenously determined. The coefficient for height in the same regression increases four-fold, from 0.04 to 0.16 and the coefficient estimates for height in the ever-schooled regression increases seven-fold from 0.002 to 0.014, when both maternal schooling and height are endogenized. However, the coefficient estimate for mother's schooling *decreases* substantially from 0.009 to 0.001 and become insignificant in the ever-schooled regression, when mother's human capital stocks are endogenized. .

5. Conclusions

Our estimates in this paper are preliminary but still provocative. They suggest that the standard procedures in which maternal human capital is treated as exogenous may yield misleading coefficient estimates for the impacts of maternal human capital on child human capital in the Guatemalan context, as is reported in the small subset of studies that have considered this question before at least with regard to the causal impacts of maternal schooling in the United States, Norwegian, and Nicaraguan contexts. For maternal schooling, the comparison between our OLS and IV estimates under the assumption that the IV estimates are "improved" estimates suggests that the OLS estimates may understate slightly the impact on grades of schooling relative to the age-cohort mean, but overstate substantially the magnitude and the significance of the effect on ever-schooled. For maternal height, a similar comparison suggests that for all but one of the child outcomes considered (the exception is birth length) the OLS estimates understate, in some cases substantially, the causal impact of maternal long-run nutritional status on both child anthropometric and schooling outcomes. The previous literature to our knowledge does not consider the possibility that maternal height should be treated as endogenous in estimates of such intergenerational human capital relations. Our estimates imply, thus, not only that in a number of cases are the standard estimates likely to be misleading regarding the magnitude and significance of components of maternal human capital due to endogeneity of maternal human capital, but they are likely to understate the importance of long-run maternal nutritional status relative to maternal schooling attainment in determining child human capital. Apparently "biological" human capital, which is thought to work substantially through the development of cognitive potential early in the children's life cycle (e.g., Engle *et al.* 2006) is substantially more important relative to "intellectual" human capital for the determination of child human capital than would be perceived in the standard estimates that dominate in the previous literature.

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Figure 1

Sample sizes for residents and migrants – Women only

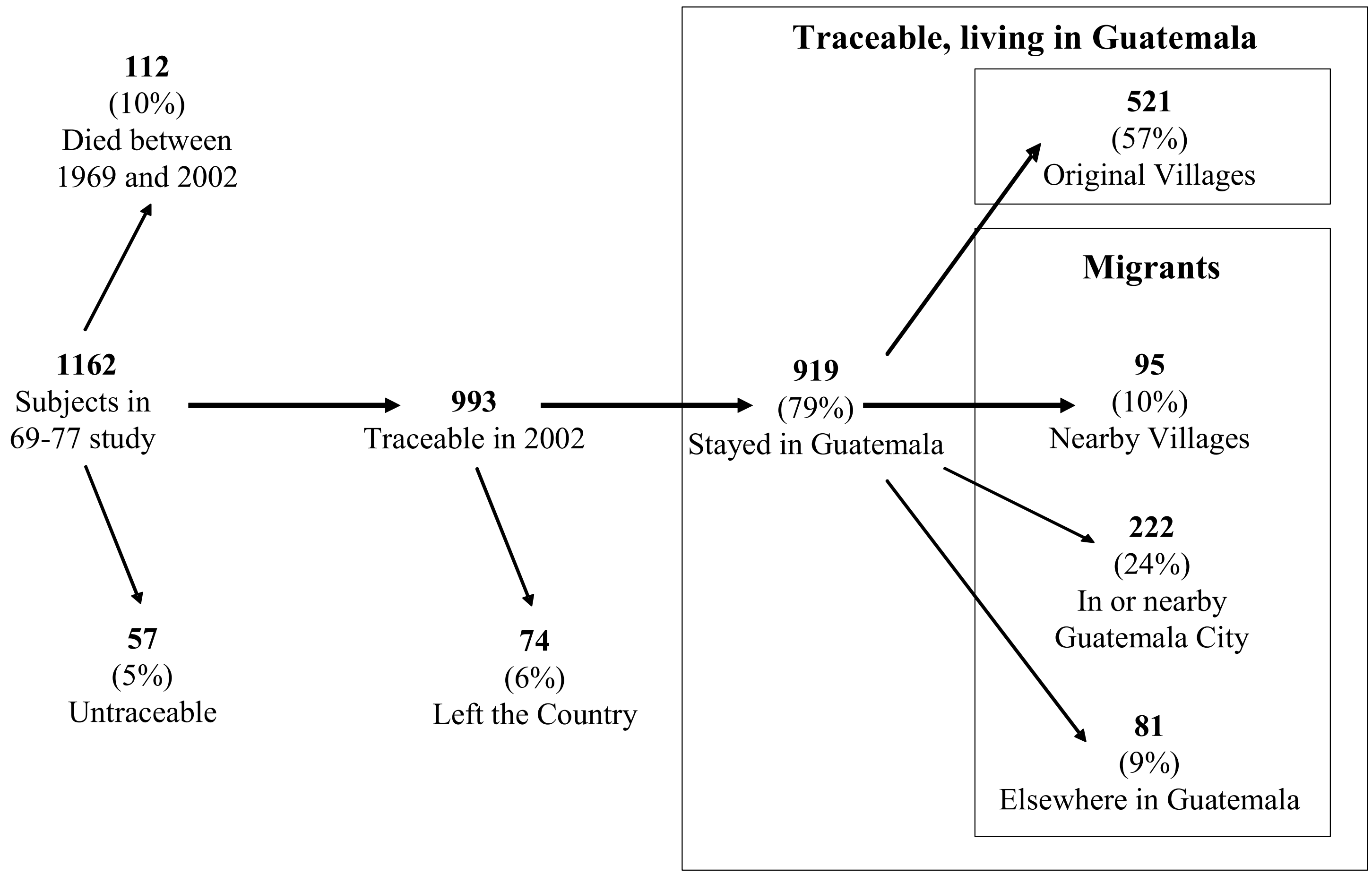

Table 1a. Summary of G3 Outcomes

	Mean	SD	n
Anthropometry at Birth			
Birth weight (kg)	2.98	0.45	532
Birth length (cm)	48.23	2.14	515
36-month Z-scores			
LAZ	-1.78	1.03	432
WAZ	-1.25	1.09	432
WLZ	-0.25	0.96	432
Schooling			
Years schooling[1]	-0.12	2.58	916
Ever schooled	0.90	0.30	916

1 Difference in years schooling from age-cohort mean (positive if grades schooled > cohort mean)

Table 1b. Summary of explanatory variables and instruments, by G3 outcome

	Anthropometry at Birth *(n=532, clusters=298)*		36-month Z-scores *(n=432, clusters=277)*		Schooling *(n=916, clusters=384)*	
	Mean	SD	Mean	SD	Mean	SD
Explanatory variables						
G2 Mother's Grades Formal Schooling	3.45	2.72	3.56	2.82	3.39	2.74
G2 Mother's Height(cm) at age 18	149.88	5.32	149.94	5.25	149.96	5.43
G3 Gender (1=male)	0.50	0.50	0.52	0.50	0.51	0.50
G3 Multiple Birth	0.02	0.12	0.01	0.11	0.01	0.10
Instruments: G2 Mother's Community Characteristics and Shocks						
Lived in communities at 0-36 months of age	0.39	0.49	0.40	0.49	0.28	0.45
Exposed to atole at 0-36 months of age	0.22	0.41	0.22	0.41	0.17	0.38
Born in San Juan	0.16	0.36	0.18	0.38	0.23	0.42
Born in Conacaste	0.35	0.48	0.31	0.46	0.33	0.47
Born in Espiritu Santo	0.20	0.40	0.24	0.43	0.17	0.38
Student-teacher ratio in community when G2 age 7	40.83	10.72	40.65	10.90	42.98	11.19
Permanent school in community when G2 age 15	0.93	0.25	0.95	0.22	0.81	0.40
Local markets available when G2 age 15	0.70	0.46	0.65	0.48	0.69	0.46
Instruments: G2 Mother's Family & Individual Characteristics						
G1 Mother's schooling	1.07	1.47	1.08	1.48	1.10	1.51
G1 Father's schooling	1.48	1.93	1.52	1.97	1.38	1.82
Household wealth index in 1975	-2.90	0.88	-2.92	0.86	-2.86	0.91
Missing G1 mother's schooling	0.01	0.09	0.01	0.10	0.01	0.07
Missing G1 father's schooling	0.03	0.17	0.03	0.18	0.05	0.21
Missing household wealth index in 1975	0.07	0.25	0.07	0.26	0.09	0.28
Death of G1 mother or father b/f G2 reaches age 15	0.09	0.29	0.08	0.28	0.07	0.26
Birth year	1969.66	4.22	1970.25	4.18	1967.38	3.85
G2 is a twin	0.01	0.11	0.02	0.15	0.02	0.12

Table 2. Determinants of G3 Anthropometry at Birth (*n=532, clusters=298*)

	Birth Weight (kg)			Birth Length (cm)[1]		
	Coeff	t/z		Coeff	t/z	
OLS						
G2 Mother's Grades Formal Schooling	-0.007	-0.85		-0.041	-0.98	
G2 Mother's Height(cm) at age 18	0.021	4.01	***	0.105	4.35	***
G3 Gender (1=male)	0.066	1.75	*	0.613	3.49	***
G3 Multiple Birth	-0.697	-5.09	***	-3.293	-3.48	***
F-test	14.97			14.86		
p-value	0.000			0.000		
IV: G2 Schooling & Height Endogenized						
G2 Mother's Grades Formal Schooling	-0.027	-1.79	*	-0.058	-0.9	
G2 Mother's Height(cm) at age 18	0.034	3.12	***	0.078	1.77	*
G3 Gender (1=male)	0.075	2.04	**	0.668	4.12	***
G3 Multiple Birth	-0.801	-6.49	***	-4.227	-5.96	***
F-test	13.37			16.1		
p-value	0.000			0.000		
Weak ID: Craig Donald F-test	5.22			5.23		
Overid: Hanson J statistic p-value	0.485			0.428		

1 sample size for birth length slightly different: n=515, clusters=292

*** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 3. Determinants of 36 month Z-scores (*n=432, clusters=277*)

	LAZ			WAZ			WLZ		
	Coeff	t/z		Coeff	t/z		Coeff	t/z	
OLS									
G2 Mother's Grades Formal Schooling	0.017	0.9		0.014	0.59		0.002	0.1	
G2 Mother's Height(cm) at age 18	0.074	7.47	***	0.057	5.17	***	0.008	0.77	
G3 Gender (1=male)	-0.103	-1.15		-0.004	-0.04		-0.007	-0.08	
G3 Multiple Birth	-0.597	-2.1	**	-0.028	-0.04		0.445	0.57	
F-test	15.83			7.74			0.27		
p-value	0.000			0.000			0.894		
IV: G2 Schooling & Height Endogenized									
G2 Mother's Grades Formal Schooling	-0.010	-0.3		0.023	0.6		0.039	1.17	
G2 Mother's Height(cm) at age 18	0.121	4.83	***	0.123	4.28	***	0.043	1.77	*
G3 Gender (1=male)	-0.072	-0.76		0.053	0.51		-0.014	-0.15	
G3 Multiple Birth	-0.673	-1.9	*	-0.105	-0.24		0.207	0.33	
F-test	6.76			5.81			2.21		
p-value	0.000			0.000			0.068		
Weak ID: Craig Donald F-test	3.88			3.88			3.88		
Overid: Hanson J statistic p-value	0.062			0.194			0.219		

*** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 4. Determinants of G3 Schooling (*n=916, clusters=384*)

	Years Schooling[1]			Ever schooled		
	Coeff	t/z		Coeff	t/z	
OLS						
G2 Mother's Grades Formal Schooling	0.124	3.68	***	0.009	2.26	**
G2 Mother's Height(cm) at age 18	0.040	2.07	**	0.002	1.03	
G3 Gender (1=male)	0.358	2.08	**	-0.008	-0.39	
G3 Multiple Birth	-1.779	-2.05	**	-0.016	-0.16	
F-test	7.07			2.42		
p-value	0.000			0.048		
IV: G2 Schooling & Height Endogenized						
G2 Mother's Grades Formal Schooling	0.161	3.17	***	0.001	0.09	
G2 Mother's Height(cm) at age 18	0.161	2.84	***	0.014	2.22	**
G3 Gender (1=male)	0.430	2.4	**	-0.013	-0.72	
G3 Multiple Birth	-2.088	-2.27	**	-0.067	-0.83	
F-test	10.41			1.83		
p-value	0.000			0.122		
Weak ID: Craig Donald F-test	5.22			5.22		
Overid: Hanson J statistic p-value	0.544			0.033		

1 difference in years schooling from age-cohort mean (positive if grades schooled > cohort mean)

*** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Appendix Table 1: First stage results for select G3 outcomes

	Weight at Birth (*n=532*)				36-month LAZ (*n=432*)				Years Schooling[1] (*n=916*)			
	Schooling		Height		Schooling		Height		Schooling		Height	
	Coeff	t	Coeff	t	Coeff	t	Coeff	t	Coeff	t	Coeff	t
Included instruments:												
G3 Gender (1=male)	0.144	0.88	0.056	0.13	0.558	2.44 **	0.028	0.06	-0.121	-0.72	-0.294	-0.84
G3 Multiple Birth	-0.219	-0.35	2.649	1.17	-0.946	-2.04 **	1.952	0.60	1.233	1.54	4.110	2.06 **
Excluded instruments:												
Lived in communities at 0-36 months of age	-0.635	-1.41	-1.354	-1.37	-0.386	-0.79	-1.033	-1.08	-0.395	-0.83	-0.560	-0.54
Exposed to atole at 0-36 months of age	0.637	1.00	0.726	0.52	0.666	0.95	0.826	0.58	0.554	0.85	-0.324	-0.24
Born in San Juan	-0.069	-0.10	1.452	0.93	-0.123	-0.17	0.734	0.42	0.013	0.02	1.976	1.55
Born in Conacaste	-0.754	-1.64	2.354	2.20 **	-0.182	-0.34	2.008	1.93 *	-0.657	-1.54	2.780	2.76 ***
Born in Espiritu Santo	-0.490	-0.62	0.767	0.43	-0.177	-0.21	0.424	0.23	-0.553	-0.81	-0.375	-0.27
Student-teacher ratio in community when G2 age 7	-0.027	-1.29	-0.001	-0.03	-0.041	-1.80 *	0.006	0.17	-0.028	-1.73 *	-0.007	-0.21
Permanent school in community when G2 age 15	1.326	2.04 **	-0.395	-0.22	0.980	1.39	-1.110	-0.53	1.197	2.14 **	0.845	0.73
Local markets available when G2 age 15	-0.824	-1.47	0.948	0.70	-1.024	-1.59	0.450	0.31	-0.793	-1.97 **	-0.468	-0.48
G1 Mother's schooling	0.378	3.51 ***	0.789	3.44 ***	0.376	3.45 ***	0.785	3.47 ***	0.265	2.79 ***	0.513	2.34 **
G1 Father's schooling	0.165	1.81 *	-0.071	-0.47	0.229	2.25 **	-0.133	-0.89	0.186	2.44 **	-0.070	-0.41
Household wealth index in 1975	0.905	4.08 ***	0.662	1.76 *	0.983	4.27 ***	0.648	1.65	0.718	3.83 ***	0.864	2.24 **
Missing G1 mother's schooling	1.326	2.24 **	-1.254	-0.59	1.011	1.68 *	-1.279	-0.78	3.272	2.78 ***	3.255	2.79 ***
Missing G1 father's schooling	-1.552	-3.32 ***	1.376	0.81	-1.323	-2.69 ***	1.852	1.36	-1.070	-1.76 *	-1.570	-1.10
Missing household wealth index in 1975	0.637	1.02	-1.257	-1.19	0.775	1.29	-0.492	-0.54	1.769	3.35 ***	0.233	0.21
Death of G1 mother or father b/f G2 reaches age 15	-1.086	-2.33 **	-1.502	-1.13	-1.022	-2.17 **	-1.961	-1.63	-1.591	-3.48 ***	-1.577	-1.21
Birth year	-0.012	-0.28	0.185	2.22 **	-0.036	-0.74	0.205	2.30 **	-0.020	-0.49	-0.001	-0.01
G2 is a twin	1.562	4.11 ***	-1.343	-0.77	1.103	1.82 *	-1.726	-1.26	0.937	1.04	-1.732	-1.79 *
Tests of excluded instruments:	stat	p	stat	p	stat	p	stat	p	stat	p	stat	p
F-test of excluded instruments	12.78	0.000	3.51	0.000	7.84	0.000	3.49	0.000	9.98	0.000	3.24	0.000
Partial R2 of excluded instruments	0.303		0.151		0.270		0.146		0.270		0.098	

1 difference in years schooling from age-cohort mean

*** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.