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The Impact of an Experimental Nutritional Intervention in Childhood on Education among Guatemalan Adults

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Abstract

Early childhood nutrition is thought to have important effects on education, broadly defined to include various forms of learning. We advance beyond previous literature on early childhood nutrition on education in developing countries by (1) using unique longitudinal data from a nutritional experiment with lifetime educational measures; (2) avoiding confounding the estimates by excluding potentially endogenous right-side variables; and (3) using estimators that allow for nonnormal distributions. Our results indicate significantly positive, and fairly substantial, effects of the randomized intervention a quarter century after it ended: increased grade attainment by women, via increased likelihood of entering and completing primary school and some secondary school; speedier grade progression by women; higher scores on cognitive tests for both men and women; and higher scores on educational achievement tests for both men and women. To account for possible biases in the calculation of standard errors and to control for sample attrition, alternative estimations were run and found to be robust.

Key words: early childhood nutrition, education, nutritional intervention, Guatemala

Contents

Tables

Appendix Table

Figures

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1. Introduction

Throughout the world, governments provide resources to improve the well-being of preschool children. Head Start is probably the best-known program in the United States, but hundreds of other programs exist both in the United States and elsewhere. In developing countries, for example, programs designed to improve preschool nutrition are common. In addition to their immediate effects, including improved survival and better growth and development, investments in such programs are often justified on the grounds that they provide longer-term benefits, such as improved health and nutritional status, school readiness, and educational attainment, as well as improved outcomes in adulthood, including employment and health.

Only limited evidence exists to support these claims. In the United States, a small number of experimental evaluations of interventions focusing on preschool children (but based on relatively small samples)—such as the Perry Preschool Experiment, the Home Intervention Program for Preschool Youngsters, and the Milwaukee and Abecedarin projects—find that such programs generate higher grades of schooling attained, test scores, and incomes, and lower welfare participation rates, out-of-wedlock births, and crime rates (Baker, Piotrkowski, and Brooks-Gunn 1998; Schweinhart and Weikart 1998; Ramey, Campbell, and Blair 1998). A well-known, nonexperimental evaluation (based on a much larger sample) is for the Head Start program. Estimates that control for mother- and child-specific unobservable characteristics indicate that the program had positive effects on test scores, immunization rates, and earnings in young adulthood, and lowered grade repetition, primarily among whites and Hispanics (Currie and Thomas 1995; Garces, Thomas, and Currie 2002).

In developing-country contexts, a body of literature, some of it outside economics, has explored the relationship between preschool nutritional status and the education of school-age children and adolescents. Malnourished children score lower on tests of cognitive functioning, have poorer psychomotor development and fine motor skills, have lower activity levels, interact with others less frequently, fail to acquire skills

at normal rates, have lower enrollment rates, and complete fewer grades of schooling (Alderman et al. 2001b; Alderman, Hoddinott, and Kinsey 2006; Behrman 1996; Behrman, Cheng, and Todd 2004; Glewwe, Jacoby, and King 2000; Glewwe and King 2001; Grantham-McGregor et al. 1997; Grantham-McGregor, Fernald, and Sethuraman 1999a, 1999b; Johnston et al. 1987; Lasky et al. 1981).

It is believed that these reflect, in part, biological pathways by which malnutrition affects neurological development. Controlled experiments with animals suggest that malnutrition results in irreversible damage to brain development such as that associated with the insulation of neural fibers (Yaqub 2002). The adverse effect of malnutrition on fine motor control suggests that physical tasks associated with attending school, such as learning to hold a pencil, are more difficult for those who have suffered from malnutrition.

A number of considerations, however, make many of the studies linking preschool nutritional status and education, particularly the nonexperimental ones, less compelling. First is the standard set of concerns applicable to all program evaluations, including the comparability of treatment and control groups, the need for adequate sample sizes, and the importance of accounting for selective attrition. Second is the need to use a sufficiently long time horizon to determine whether benefits persist or whether there is "fade out" over time (Garces, Thomas, and Currie 2002). Many of the studies that examine the impact of malnutrition on school-age populations, for example, can explore only partially the longer-term consequences. Third, it is important to control for factors that may influence these outcomes subsequent to the childhood intervention. While studies based on schooling attainments of individuals observed in adulthood may be better placed to address the extent of fade out, they often have only limited ability to account for other relevant factors that may have affected individuals after the intervention, with possible consequences for the precision of their estimates. Fourth, only limited information is typically available on educational outcomes. For example, while years or completed grades of schooling are often used as indicators of educational attainment, they are only crude indicators of what individuals have learned in school,

particularly in contexts where the quality of schooling varies. Fifth, there is an important set of estimation issues that must be confronted, even with randomized designs. For example, most schooling attainment distributions are lumpy; there are mass points in the distribution of outcomes around certain grades, such as completion of primary school, and in these cases the common use of ordinary least squares (OLS) estimators may be inappropriate. Studies using cross-sectional, cluster-based samples must trade off using controls for location-specific fixed unobservable characteristics (locality fixed effects) against including locality-specific characteristics. Cross-sectional, cluster-based samples also raise concerns about the within-cluster correlation of disturbance terms and their consequent implications for the estimation of standard errors.

In this paper, we provide new evidence of the effects of early childhood nutritional interventions on adult outcomes, using longitudinal data and methods well suited to address the concerns outlined above. Specifically, we investigate the long-term impact of a randomized, community-level health and nutritional intervention in rural Guatemala, fielded from 1969–77. We link information collected in the 1970s on individuals (and their families) exposed to the intervention when they were 0–15 years of age, with new data on these same individuals collected in 2002–04. We explore the effect of the intervention on several different education-related measures over the life cycle: grades of schooling attained; the rate at which individuals progressed through school; a test of cognitive ability; and an educational achievement test of literacy, vocabulary, and comprehension.

For each measure, we estimate the effect of exposure to the randomized health and nutritional intervention during the period from birth to 36 months of age. Exploiting detailed historical and socio-anthropological studies undertaken in the survey areas, the estimates control for relevant observed time-varying community influences that might otherwise adversely affect their precision. They also control for family background characteristics—parental education, parental age, and a wealth index measured at the time of the intervention. Recognizing that the different education-related outcome

measures have different distributions, we use a variety of estimators including OLS, probits, and ordered probits.

Our results indicate significantly positive, and fairly substantial, effects of the nutritional intervention. These include increased grade attainment by women, via increased likelihood of entry and completion of primary school and some secondary school; speedier grade progression by women; higher scores on cognitive tests for both men and women; and higher scores on educational achievement tests for both men and women.[1]

Thus we provide solid evidence that at least one type of preschool intervention—improved nutrition—conveys long-term benefits that do not fade out over time. The results suggest that antipoverty interventions that include improving the nutrition of preschool children may have more substantial and persistent effects than are commonly recognized. For example, the well-known *Programa Nacional de Educacion, Salud y Alimentacion* (PROGRESA) (now called *Oportunidades*) conditional cash transfer program in Mexico includes nutritional supplements to preschool children. This component of the intervention may have longer-term impacts over and above their direct, short-term effects on nutrition (Behrman and Hoddinott 2005). Finally, the evidence that early nutrition has an effect on subsequent educational attainments underscores the value of a life-cycle approach to schooling that includes the preschool period (Cunha et al. 2005).

The remainder of the paper is organized as follows. Section 2 describes the intervention and the areas in Guatemala where it occurred. Section 3 provides a

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[1]There have been some studies using earlier rounds of data collected on this population, but these also suffer from methodological limitations. Pollitt et al. (1993) find positive effects of the intervention on tests of general knowledge, numeracy, reading, and vocabulary, but no impacts on schooling attainment. They also find that effects of the nutrient supplement varied with socioeconomic status (SES) and schooling: effects were higher for children of low SES and among those children with more years of schooling. Li et al. (2003) and Li et al. (2004) assess the associations of the supplement, schooling, and physical growth, with a wide array of measures of educational attainment among a selected population of women in the study villages who bore a child between 1996 and 1999. They find that exposure to the intervention resulted in higher performance on a scale that combined literacy, numeracy, and cultural competency, conditional on completion of sixth grade. Since these earlier results are conditional on schooling, without controls for its behavioral determinants, causal interpretation is problematic.

conceptual framework for modeling the effect of nutrition on educational attainment. Section 4 describes in more detail the data that we use in this study. The effects of the early childhood health and nutritional intervention on educational attainment a quarter century later are presented in Section 5. Section 6 presents the conclusions.

2. The 1969-77 Health and Nutritional Intervention

In the mid-1960s, protein deficiency was seen as the most important nutritional problem facing the poor in the developing world, and there was considerable speculation that such deficiencies affected children's ability to learn. The Institute of Nutrition of Central America and Panama (INCAP), based in Guatemala, became the locus of a series of preliminary studies on this subject (Read and Habicht 1992). These informed the development of the larger-scale supplementation trial examined in this paper.

The principal hypothesis underlying the intervention was that improved preschool nutrition would accelerate mental development. An examination of the effects on physical growth was included to verify that the nutrition intervention had biological potency, which was demonstrated. To test the principal hypothesis, 300 communities were screened in an initial study to identify villages of appropriate size, compactness (so as to facilitate access to feeding centers), ethnicity and language, diet, access to health care facilities, demographic characteristics, nutritional status, and degree of physical isolation. From this group, two sets of village pairs were selected (one pair with about 500 residents each and the other relatively more populous, with about 900 residents each) that were similar in all these characteristics, though not necessarily in others. All four villages chosen are located relatively close to the Atlantic Highway, connecting Guatemala City to Guatemala's Caribbean coast. Santo Domingo is closest to Guatemala City, only 36 kilometers away; Espíritu Santo is furthest away, at 102 kilometers. Three villages—San Juan, Conacaste, and Santo Domingo—are located in mountainous areas with shallow soils, while Espíritu Santo is located in a river valley, with somewhat higher agricultural potential (Habicht and Martorell 1992; Martorell, Habicht, and Rivera 1995).

Two villages, Conacaste and San Juan, were randomly assigned to receive a high protein-energy drink, *atole*, as a dietary supplement.[2] *Atole* contained Incaparina (a vegetable protein mixture developed by INCAP and widely accepted in Guatemala as a food for young children), dry skim milk, and sugar, and had 163 kilocalories (kcal) and 11.5 grams of protein per 180 milliliter (ml) cup; this design reflected the prevailing view that protein was the limiting nutrient. The *atole* also contained iron, thiamin, riboflavin, niacin, ascorbic acid, and vitamin A. The *atole*, the Guatemalan name for maize porridge, was served hot; it was pale gray-green and slightly gritty, but sweet. In designing the study, there was considerable concern that the social stimulation for children attending feeding centers and included in the surveys—such as their social interactions, the observation of children's nutritional status, and the monitoring of their intakes of *atole*—might also affect child nutritional and cognitive outcomes, thus confounding efforts to understand the effects of the supplement. To address this concern, in the remaining villages, Santo Domingo and Espíritu Santo, an alternative drink, *fresco*, was provided. *Fresco* was a colored, fruit-flavored drink, served cool, a much appreciated refreshment in these areas. 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* but, from October 1971 onward, contained similar concentrations of micronutrients per unit of volume (Habicht and Martorell 1992; Martorell, Habicht, Rivera 1995; Read and Habicht 1992).[3,4]

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[2]Randomization was done within each pair of villages matched on population size. 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. The health and nutritional components of the intervention ended in all four villages in February 1977, six months prior to the end of data collection (Martorell, Habicht, and Rivera 1995).

[3]"It was originally intended that the *fresco* would be devoid of nutritional value, in effect to be a placebo as a control for the social stimulus and other factors associated with supplementation. The use of cyclamates for sweetening was considered, but concern about carcinogenicity led to sugar being used instead, which of course, introduced energy. Finally, other nutrients were introduced . . . in an attempt to narrow the contrast between the *atole* and *fresco* groups to differences in energy and, above all, in protein. Consequently, the *fresco* should not be viewed as a placebo control to the *atole*, because it contained some energy and important concentrations of micronutrients. Instead, both drinks are referred to as supplements . . ." (Habicht and Martorell 1992, p. 177).

The study subjects comprised all children aged seven years or younger and all pregnant and breastfeeding women residing in the villages. Newborns were included for study until September 1977. The analysis in this paper examines children only, though it incorporates information from mothers as well. Data collection for individual children ceased when the child reached seven years of age. The birth years of the children included in the 1969–77 longitudinal study thus range from 1962 to 1977. When funding for the intervention was terminated unexpectedly in 1977, the ages of the subjects ranged from 0–15 years. The length and timing of exposure to the nutritional interventions thus depended on village and birth year.

All residents of all villages received curative medical care free of charge throughout the duration of the intervention. Preventative health services, such as immunization and deworming campaigns, were conducted simultaneously in all villages. To ensure that the results were not influenced systematically by the characteristics of the research and survey teams, staff working on the intervention were rotated through the four study villages. In all villages, drinks were distributed in food supplementation centers and were available daily, on a voluntary basis, to all members of the village for 2–3 hours in the mid-morning and 2–3 hours in the mid-afternoon, times that were convenient to mothers and children but that did not interfere with usual meal times.

In interpreting the results, a critical question that arises is to what extent the intervention design resulted in differences in access to calories, protein, and nutrients. In addressing this question, we can draw on the intensive nature of the survey and observational work associated with the intervention. Assessments of diet in the home (exclusive of the supplements) indicate that energy intakes were largely similar among

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[4]Though children under four months of age in *atole* villages did not consume a great deal of the supplement, those who did were given a modified, age-appropriate mixture. Children identified at the clinic or at home as showing any signs of marasmus or kwashiorkor in the *fresco* villages were given the same skimmed milk and sugar beverage offered to those under four months of age in the *atole* villages and identified with a code so that they could be excluded from the analysis (Habicht and Martorell 1992). These codes, however, are no longer available. Though this apparently was a small group, including them in the analysis presumably biases downward the estimated impact of the *atole* intervention relative to the *fresco* intervention.

children under 36 months in *atole* and *fresco* villages. Averaging over all children in the *atole* villages (both those who consumed some 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. Children in the *fresco* villages consumed only small amounts of *fresco* between the ages of 0–24 months (averaging at most 20 kcal per day) with this figure rising to approximately 30 kcal daily by age 36 months (Schroeder, Kaplowitz, and Martorell 1992, Figure 4).

Finally, for the interpretation and consideration of the external validity of our findings, it is also important to underscore the nature of the intervention, which involved intensive contact between researchers and villagers and high-quality medical care. If these aspects of the intervention equally affect the impact of the two supplements, then the contrasts we explore below are externally valid to situations without the survey and medical care components of the study. If not, the observed effects may have been diminished or potentiated by these other aspects of the intervention (Habicht and Martorell 1992).

3. Conceptual Framework

Our conceptual framework treats investments in nutrition and education as part of a dynamic programming problem solved by the family of the individual, subject to the constraints imposed by parental family resources and options available in the community to the individual as he or she ages (see Cunha et al. 2005 for a formal statement). This programming problem can be solved to obtain a relation that we interpret as a reduced-form relation for the determinants of a vector of an individual's education-related outcomes. Each education-related indicator for individual i, at age a (E_{ia}), is posited to depend on a cohort control (N_i) indicating whether the individual was 0–36 months during the intervention period—regardless of the type of exposure received, whether the individual was exposed to the *atole* nutritional intervention from birth to 36 months of

age ($N^A{}_i$), fixed community characteristics ($C^f{}_i$), a vector of varying community characteristics related to schooling availability and quality at different ages of the *i*th individual ($C^v{}_{ia}$), a vector of individual characteristics such as sex and birth year (I_i), a vector of fixed family background characteristics for the *i*th individual (F_i), and a disturbance term that affects the educational outcome of interest (ε_{ia}):

$$E_{ia} = f(N_i, N^A{}_i, C^f{}_i, C^v{}_{ia}, I_i, F_i, \varepsilon_{ia}). \tag{1}$$

This specification has a number of important features. First, the cohort control (N_i) captures the effects common to both the *atole* and *fresco* interventions (for example, improved health-care services and increased social stimulation due to the intervention) as well as any secular effects or aggregate shocks specific to this cohort of Guatemalan children. The *atole* exposure term ($N^A{}_i$) measures the differential effect of exposure to *atole* relative to exposure to *fresco*, for this cohort. Second, the fixed community characteristics ($C^f{}_i$) control for factors such as the initial sizes of the villages and persistent cultural differences or differences in economic alternatives that might result in different educational investments across villages, even in the absence of the interventions. It is crucial to include these because of the small number of villages in the experiment.[5] Third, the varying community characteristics related to schooling are linked to the ages of the *i*th individual ($C^v{}_{ia}$), such as the student-teacher ratio in the local schools when the individual was seven years old. We include the same set of characteristics for all of the education-related dependent variables that we consider. Fourth, often in studies in this literature, linear approximations to such a functional form are imposed a priori. Our examination of the distributions of the dependent variables in Section 4 leads us generally to question the assumptions necessary for linear relations,

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[5]This analytical model controlling for community fixed effects is identical to one that mimics more closely the design of the original experiment. As explained in the text, two matched-pair villages were first selected and then each pair was randomly assigned to receive *fresco* or *atole*. Given this, an alternative approach would be to include a dummy control for village size, one for the randomized treatment of *atole*, and an interaction of the two. This leads to three dummy variables spanning the same space as any three of the village dummy variables. This equivalence underscores that the dummy variables for either of these alternative specifications capture village fixed-effect components.

and so we also consider the robustness of our results to nonlinear specifications. Fifth, the disturbance term, ε_{ia}, includes all other unobserved or unmeasured variables. These may include some fixed individual and family variables that affect the education-related indicators of interest directly and that are correlated with the included observed family background variables. For example, if innate ability affects education and is correlated across generations through genetic inheritances or household environments, such unobserved heterogeneity may bias the coefficient estimates on family background variables.[6] As a result, we do not emphasize these family background variables in the discussion but rather regard them as controls for variation unrelated to the intervention that enable more precise estimation. Crucially, while such unobserved fixed individual or family heterogeneities might bias estimates of the effect of the family background variables, they do not bias the variable of central interest—exposure to the *atole* nutritional intervention early in life—because of the experimental design and the inclusion of community fixed effects.

4. Data

The 2002-04 Follow-Up Survey

Subsets of the original sample of 2,393 children collected during 1969–77 have been resurveyed periodically in the years since the original data collection ended, most importantly in 1988–89 (Martorell, Habicht, and Rivera 1995), 1991–96 (Hruschka et al. 2003), and 1997–98 (Torun et al. 2002). Most of these surveys (and related studies) were aimed at measuring the effect of the original intervention on nutritional and health

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[6]For example, Behrman and Rosenzweig (2002, 2005) find that the cross-sectional significant positive association between mothers' and children's schooling that is reported in many studies not only becomes smaller but becomes negative. Using a sample of adult twins that permits control for unobserved, intergenerationally correlated endowments and the effects of assortative mating, they suggest that this occurs because more-schooled women, holding ability and motivation constant, spend more time in the labor force and less time raising their children. Behrman et al. (2005) provide evidence for the sample that we use in this study that the coefficient estimates of paternal schooling have biased estimates of the impact of parental schooling on child education if there are no controls for genetic endowments.

outcomes in early childhood, adolescence, and early adulthood (see Martorell et al. 2005 for references). Between 2002 and 2004, a multidisciplinary team of investigators, a subset of which are the authors of this paper, resurveyed individuals surveyed as children in the 1969–77 data collection, referred to as the Human Capital Study (Grajeda et al. 2005; Martorell et al. 2005).

To locate and interview these individuals, the data collection team began in the four original villages. A census of all households in the villages was done between January and April 2002. Sociodemographic information was collected for entire village populations, and records regarding the mortality and migratory status of the 2,393 original sample members were updated. A list of "missing" sample members was created, and then reviewed and corrected with the assistance of sample members' relatives, peers, (former) neighbors, and community leaders in each of the original villages. Original sample members who were alive but had moved away from their natal villages were classified as "migrants." While updating the lists, the data collection team also sought information on the migrants' current addresses and phone numbers, work addresses, or general whereabouts. Flyers soliciting this information and letters inviting migrants to participate in the survey were left with the relatives of migrants (Grajeda et al. 2005).

During the first year of fieldwork, data collection focused on residents of the original villages. While collecting these data, the data collection team also attempted to contact and interview migrants who visited their natal villages, for example, during village feast days or other holidays. Between January 2003 and April 2004, a two-person team comprising a man and a woman traveled throughout Guatemala locating migrants. Migrants to nearby villages, Guatemala City, and other cities or towns in Guatemala were visited wherever they lived and invited to participate in the survey. This team used a snowball approach in which the list of still-missing sample members was reviewed with each new migrant located. From April 2003 to April 2004, data collection focused on sample members residing elsewhere in nearby villages, in Guatemala City, and other towns and cities in Guatemala. The survey team traveled throughout Guatemala to

interview those living outside the communities, except for interviews of those living in Guatemala City, which were conducted at INCAP headquarters (Grajeda et al. 2005).

Of the 2,393 persons in the 1969–77 sample, 1,856 (77 percent) were determined to be alive and living in Guatemala (11 percent had died—the majority due to infectious diseases in early childhood, 8 percent had migrated abroad, and nothing could be learned about the remaining 4 percent). Of the 1,856 individuals eligible for reinterview in 2002–04, 1,113 lived in their original villages, 154 lived in nearby villages, 419 lived in or near Guatemala City, and 170 lived elsewhere in Guatemala. Of the 1,856, 1,570 (85 percent) completed at least one survey instrument during the 2002–04 data collection (when they were 25–42 years of age). Although they were known to be living in Guatemala, location information was insufficient to make contact with two-thirds of the 15 percent who did not complete a single instrument. The refusal rate for any participation among those who were contacted, however, was low—5 percent.

This analysis uses data from 1,469 individuals who completed the questionnaire module pertaining to schooling, a little over half of whom (54 percent) were women. This represents 79 percent of the 1,856 individuals who were known to be alive and living in Guatemala, but only 61 percent of the original 2,393 subjects. Measured from 1977 to 2002, the latter figure indicates an annual attrition rate of 2 percent, low when compared to shorter-term studies in developing countries (Alderman et al. 2001a) or to longer-term longitudinal studies in the United States (Fitzgerald, Gottschalk, and Moffitt 1998b).[7] Nevertheless, almost 40 percent is indeed substantial attrition; therefore we assess potential attrition bias in Section 5.

Efforts to ensure high-quality data collection were extensive. Manuals were prepared for each questionnaire or test. Each team of interviewers was trained and standardized in one or two study domains in the two weeks before the corresponding module was implemented. Interviewers performed a supervised reading of the manuals

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[7]Most measures of attrition refer to households or individuals who were past infancy and early childhood when the sample was taken, so they are not affected by the high infant and early childhood mortality rates that account for over a quarter of the attrition in the data used for this study.

and cross interviews with residents from nearby similar villages, after which interviewers' field experiences were shared with the whole fieldwork team and lessons learned were summarized and incorporated. At least two restandardization exercises were done when the module was implemented. The interviewers reviewed all forms after the completion of the interview. Fieldworkers received direct supervision, and the field supervisor observed 10 percent of the interviews in each study domain. Repeated measurements or cross interviews were done by the supervisor or other interviewers in at least 5 percent of the interviews. Results of observation and repeated measurements were used to field-train interviewers and improve ongoing data collection. The field supervisor reviewed 40 percent of the forms looking for nonpermissible data, missing information, or inconsistencies between questions. Forms with inconsistencies or missing data were returned to the field to be corrected. After review, the forms were delivered to the data center established at the fieldwork headquarters. Blinded double data entry was performed usually within several days of data collection (but occasionally up to four weeks afterward); values suspected to be incorrect were sent to the field for review, with the supervisor authorized to correct coding errors (Grajeda et al. 2005).

In addition to the individual-level data that were collected, a socioanthropological study was undertaken (Estudio 1360 2002). This consisted of focus group and key informant interviews as well as archival work that generated information on current and past education and health facilities, physical infrastructure, public services, and programs that operated in the study area, as well as important events that might have affected human capital formation.

General Data

The individuals reinterviewed in 2002–04, were relatively well off compared with other Guatemalans. On average, they had an expenditure-based poverty rate of 35 percent and an extreme poverty rate of only 3 percent, against national averages of 56 percent and 15 percent, respectively (Maluccio, Martorell, and Ramírez 2005). The vast

majority of men were engaged in some income-generating activity, with 80 percent working in wage labor for at least part of the year. Over two-thirds of women also participated in income-generating activities, though only a third in wage labor (Hoddinott, Behrman, and Martorell 2005).

Table 1 provides basic descriptive statistics. In general, the outcome variables are taken from the 2002–04 follow-up, while the right-side variables use individual- and household-level data collected from 1969 to 1977, supplemented by community information collected both during earlier studies and retrospectively in 2002. This approach has an advantage in that it does not include as explanatory variables factors simultaneously reported on or determined with the outcome measures. An identical set of right-side covariates is used in all regression results that we report.

Dependent Variables: Educational Outcomes

We examine four types of dependent variables (E_{ia} in equation 1) that capture educational outcomes across the life cycle: (1) measures of attained schooling, (2) measures of schooling progression rates, (3) a measure of cognitive ability, and (4) a measure of reading comprehension skills.

Schooling is measured as the number of grades completed. The formal educational system in Guatemala is divided into primary and secondary education. Primary school comprises grades one to six, and children are expected to enroll in the year in which they turn seven years old. Secondary school consists of five to seven grades, divided into two parts. The first three years of lower secondary school are the basic grades, and instruction is expected to provide academic and technical skills necessary to join the labor force. The fourth through seventh years of upper secondary school are the diversified grades and students can choose from among four specialized and career-oriented tracks such as the general (academic) high school education, known as *bachillerato*; primary school teaching; commercial education, such as an accounting

Table 1—Summary statistics, by sex

Variable	Full sample	Women	Men
Number of observations	1,469	786	683
(1) if Santo Domingo (large *fresco*)	0.259	0.253	0.265
(1) if San Juan (small *fresco*)	0.228	0.237	0.218
(1) if Conacaste (large *atole*)	0.303	0.298	0.309
(1) if Espíritu Santo (small *fresco*)	0.210	0.212	0.208
Birth year	1,970.1	1,969.9	1,970.2
	(4.21)	(4.30)	(4.12)
(1) if male	0.465		
(1) cohort control, 0–36 months of age	0.421	0.400	0.447
(1) if exposed to *atole* at 0–36 months	0.225	0.219	0.233
Log age of mother when child age 7	3.518	3.521	3.513
	(0.20)	(0.21)	(0.20)
Log age of father when child age 7	3.66	3.661	3.659
	(0.20)	(0.20)	(0.20)
Mother's years of schooling	1.353	1.246	1.476
	(1.67)	(1.60)	(1.75)
Father's years of schooling	1.671	1.649	1.697
	(2.09)	(1.99)	(1.75)
Household wealth index score in 1975	-0.176	-0.201	-0.147
	(1.64)	(1.65)	(1.63)
(1) if permanent primary school at age 7	0.477	0.464	0.492
(1) if primary school destroyed at age 7	0.899	0.885	0.915
Student–teacher ratio at age 7	39.933	40.277	39.536
	(8.97)	(9.35)	(8.487)
Student–teacher ratio at age 13	36.302	36.271	36.337
	(5.26)	(5.34)	(5.17)
(1) if ever went to school	0.954	0.950	0.959
(1) if completed first grade	0.854	0.854	0.870
(1) if completed sixth grade	0.461	0.385	0.545
Number of grades passed / number of years in school[a]	0.845	0.844	0.846
	(0.28)	(0.28)	(0.27)
Number of formal grades completed by age 13	3.577	3.393	3.788
	(2.18)	(2.14)	(2.20)
Number of completed grades	4.703	4.303	5.163
	(3.45)	(3.31)	(3.55)
Raven's Progressive Matrix score	17.704	16.266	19.374
	(6.14)	(5.40)	(6.51)
Inter-American Reading Test score	36.031	34.396	37.915
	(22.33)	(21.87)	(22.71)

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04.

[a] This variable is based on 1,337 observations (715 women and 622 men).

degree; and technical education, such as a secretarial degree. Typically, students who

plan to continue to university finish their general academic preparation in two years at the

secondary diversified level. More specialized (vocational) diversified degrees, such as accounting, can take up to four years (World Bank 2003).

Most respondents started school. Once having started school, however, approximately 8 percent dropped out at the end of grades 1 through 5 and nearly 30 percent stopped attending after completing the full six grades of primary education (Figure 1). Fewer than 20 percent continued on to secondary school. Only a small number of individuals in this cohort (less than 3 percent of the sample) continued beyond secondary school to attend university or take advanced studies in technical fields. Apart from formal schooling, it is also possible to complete grades via informal schooling, typically adult literacy programs. Our overall measure of grades completed includes both types of schooling, although informal schooling is relatively uncommon for this population, with only 15 percent of the respondents reported having ever participated in it. Other salient characteristics of schooling attainment in the sample are that (1) men

Figure 1—Distribution of highest grade attained
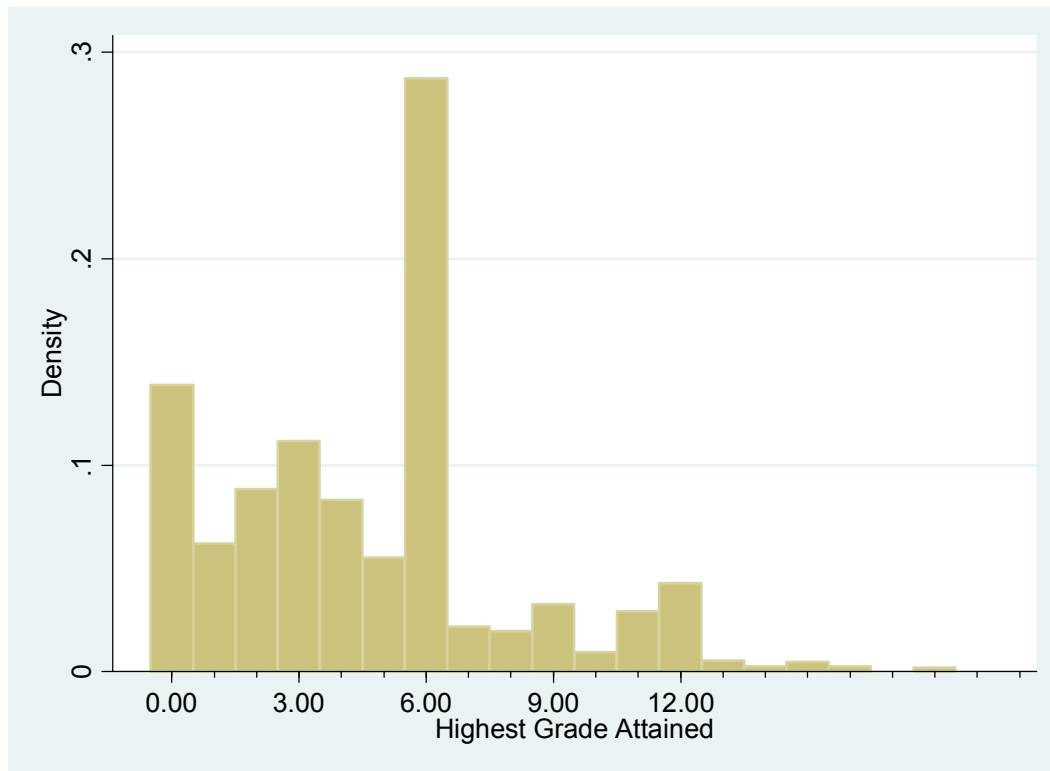
attain an average of 0.9 more grades than women; (2) schooling attainment has increased over time for both men and women; and (3) comparisons of unconditional means always show that grade attainments are higher in Espíritu Santo, one of the *fresco* villages, than in the other study villages, particularly for men (Stein et al. 2005). This is because Espíritu Santo has long benefited from its proximity to the municipal capital town of El Jícaro, where primary and secondary schools were present even before 1962, the birth year of the oldest individuals in the sample.

We consider two measures that capture how rapidly individuals progressed through schooling. The first is the number of grades attained in formal schooling (excluding informal adult education) by age 13. This measure combines age at school entry (over which there is little variation) and grade progression rates and is an integer from 0 through 8. A child who starts school at age 7 and successfully passes one grade each year should complete primary school (sixth grade) by age 13. The second measure of progression is the total number of grades passed divided by the number of years between when the respondent entered and terminated school, up to and including twelfth grade. This is a measure of grade repetitions and skips, dropouts and re-entries. An individual who does not repeat or skip a grade, or drop out and re-enter, will have a grade progression rate of 1.00, regardless of the grade at which he or she leaves school. Grade progression rates cannot be calculated for those who never attended formal schooling, and these individuals are excluded from the analysis of this measure. For the approximately 40 individuals in the sample who attended university, only the first year of university (corresponding to the twelfth grade) is included in the measure of grade progression rates. Figure 2 shows that approximately half of those individuals who attended formal school attained one grade per year attended; very few advanced faster than this and the remainder repeated (or dropped out of) at least one grade at least once.

All individuals were administered Raven's Progressive Matrices, a nonverbal assessment of cognitive ability (Raven, Court, and Raven 1984). Raven's Progressive Matrices are considered to be a measure of eductive ability: "the ability to make sense

Figure 2—Distribution of ratio of grades attained to years attended
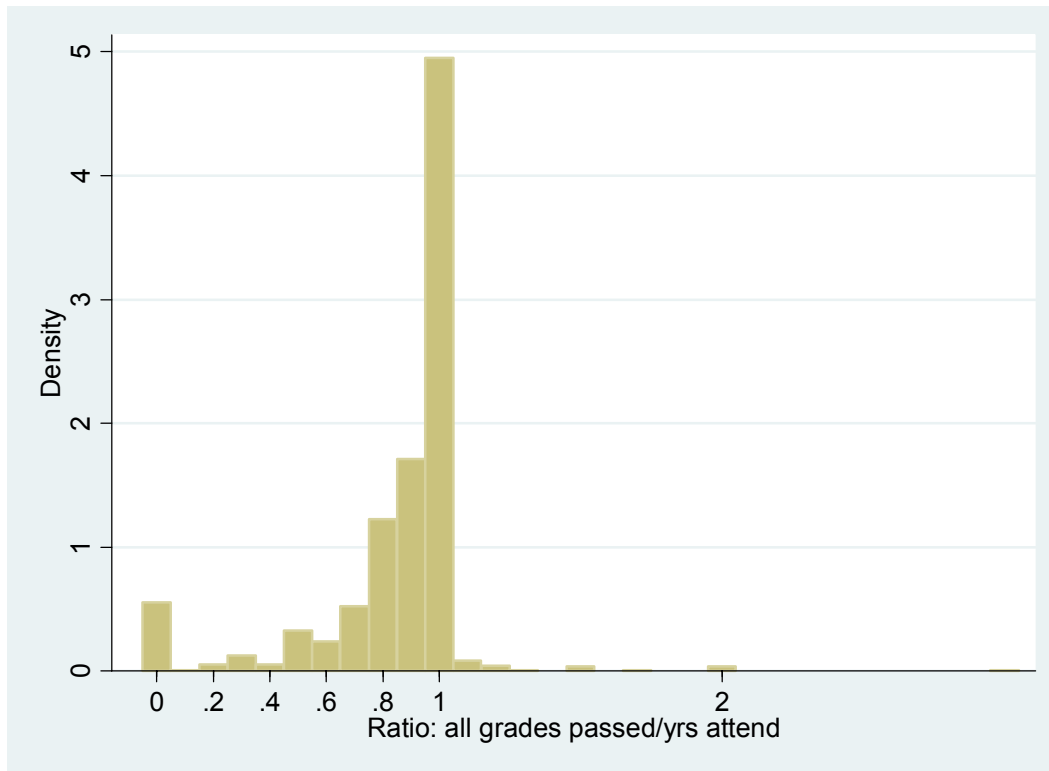
and meaning out of complex or confusing data; the ability to perceive new patterns and relationships, and to forge (largely nonverbal) constructs which make it easy to handle complexity" (Harcourt Assessment 2005). The test consists of a series of pattern-matching exercises with the respondent asked to supply a "missing piece" and with the patterns getting progressively more complex and hence harder to match correctly. We administered three of the five scales (A, B, and C with 12 questions each), since pilot data suggested and subsequent survey data confirmed that respondents were not able to progress beyond the third scale. These tests have demonstrated adequate test-retest reliability (correlation coefficient of 0.87), internal consistency, and validity in previous studies in this population (Pollitt et al. 1993). Respondents could take as long as they needed, with a typical test completed in 40 minutes. Results are presented as the number of correct answers summed across the three scales, with higher numbers denoting better

performance. The mean score on Raven's test was 17.7 with a standard deviation (SD) of 6.1 (Table 1). Figure 3 shows that these scores are approximately normally distributed, though they fail to pass standard normality tests.

Figure 3—Distribution of scores, Ravens Progressive Matrices
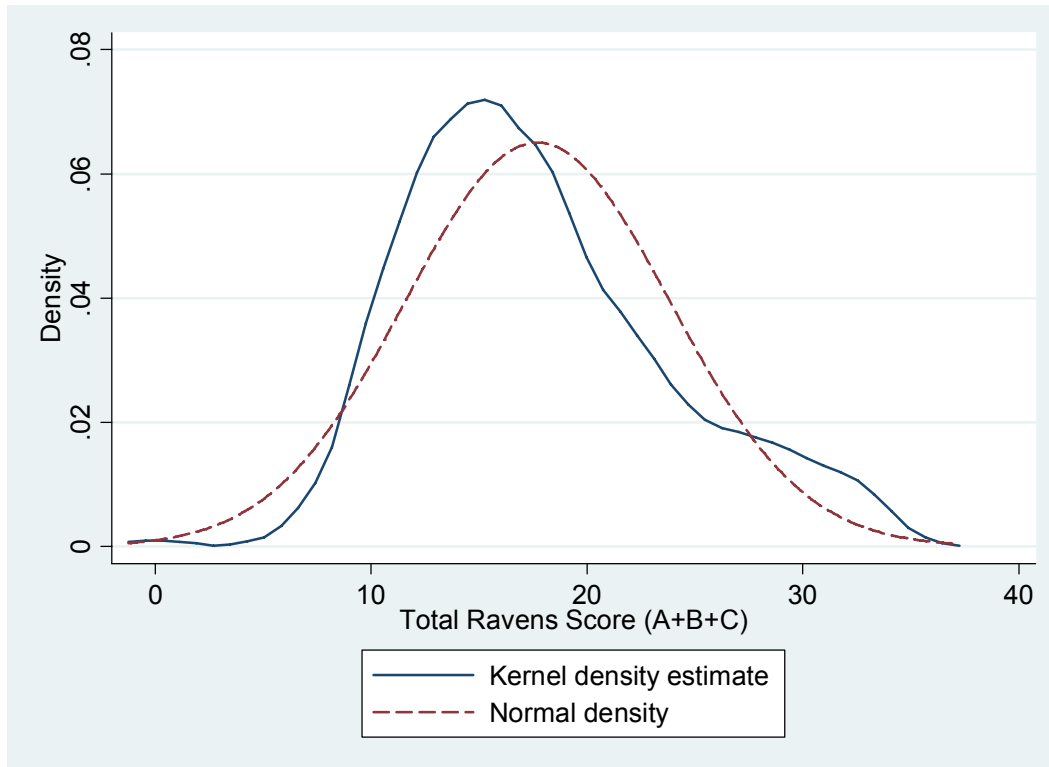
The test of reading comprehension had two components. Respondents who reported having passed fewer than four years of schooling, or those who reported four to six years of schooling but could not read the headline of a local newspaper article correctly, first were given a preliteracy test. Individuals who passed the literacy screen, or who reported more than six years of schooling (and thus were presumed to be literate) then took the Inter-American Reading Series vocabulary and comprehension modules (*Serie InterAmericana* [SIA]), which include several "levels" of difficulty. Based on pilot testing, we used level 3 for vocabulary and level 2 for comprehension. The SIA was designed to assess reading abilities of Spanish-speaking children in Texas (Manuel 1967).

These tests also have demonstrated adequate test-retest reliability (correlation coefficients of 0.87 and 0.85 for vocabulary and reading, respectively), internal consistency, and validity in this population in the past (Pollitt et al. 1993). Higher scores denote better performance and, notably, the test becomes progressively harder. Figure 4 shows the distribution of these test scores in the sample. Estimates of the determinants of scores on the SIA need to allow both for censoring at 0 (applicable to about 18 percent of the sample) and for the fact that higher scores become more and more difficult to attain.

Figure 4—Distribution of scores, Inter-American Reading Series
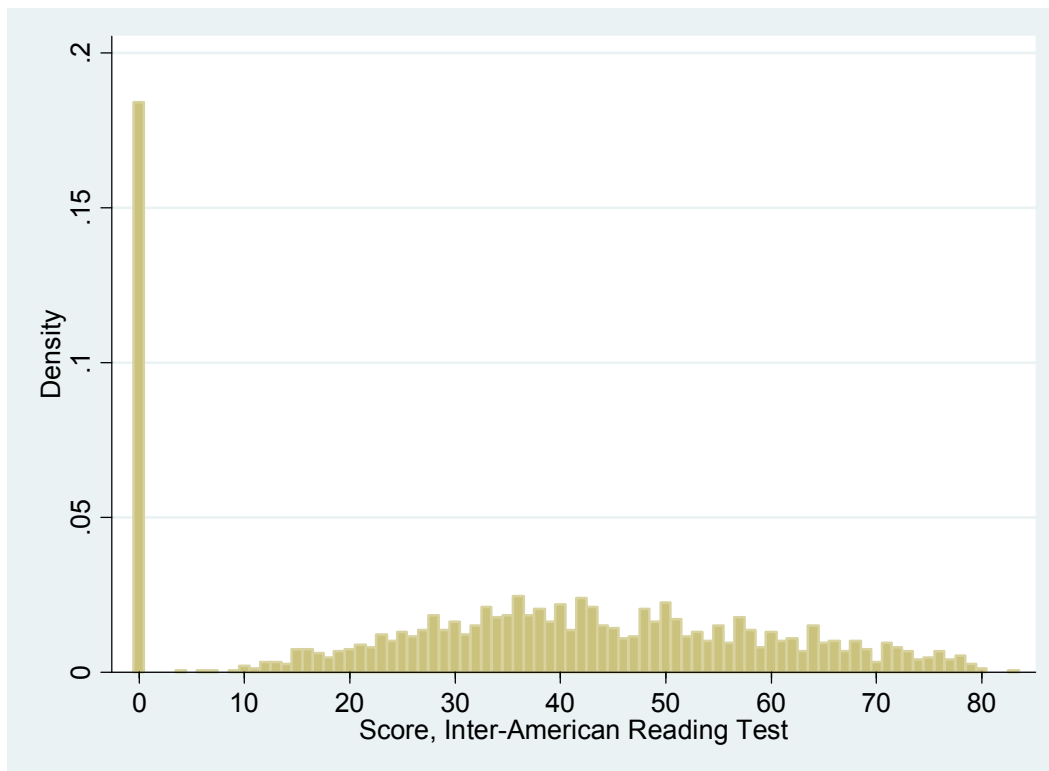
Right-Side Variables

Our right-side variables include a cohort control indicating whether the individual was at a given age during the intervention period regardless of the type of exposure received (N_i), measures of exposure to the *atole* nutritional intervention at a given age

(N^A_i), individual characteristics (I_i), a vector of fixed family background characteristics for the ith individual (F_i), as well as fixed and varying community characteristics, (C^f_i) and (C^v_{ia}), respectively.

We construct two measures of exposure to the interventions based entirely on the age of the child and the dates of operation of the intervention. The first is a control for cohort effects and the second is the potential exposure to the *atole* treatment.[8] For each child, we calculate the number of days when he or she was between birth and 36 months of age and the program was under way. In practice, this variable turns out to have mass points at 0 and the full 36 months, with relatively few observations between those two end points. Therefore, we use a dummy variable that takes a value of 1 when the respondent was exposed to intervention for at least half the period (> 91 days) between 0 and 6 months, and when the respondent was exposed to intervention entirely between 6 and 30 months, and when the respondent was exposed to intervention for at least half the period between 30 and 36 months. This measure is calculated for all individuals and represents a cohort effect that captures any general changes that affected all children in the villages in this age range, including the social stimulation and medical care due to either the *atole* or the *fresco* intervention that was available in the community in which they lived.[9] The *atole* intervention 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. Thus this latter measure, exposed to *atole* at 0–36 months, represents the differential effect in the two *atole* villages in comparison with children in the same cohort in the other two villages who were exposed to *fresco*. Because it is potential exposure and is not conditional on actual participation or intakes, our estimates represent the intent-to-

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[8]Although INCAP collected extensive information on participation in the program during the 1969–77 period, including precise measures of intake by each child under seven so that estimates of the effect of the "treatment on the treated" might be possible, we do not use this more detailed information here to avoid introducing into the specification right-side, choice-based factors that may be correlated with other factors that we do not observe.

[9]There is not sufficient information to identify the effect of the social stimulation or medical care separately from all other cohort effects.

treat effect of exposure to the high-protein, high-energy supplement, *atole*, versus the no-protein, low-energy supplement, *fresco*.

We choose the exposure period of 0–36 months based on earlier work with these data. The original study was designed to assess the effects of improved nutrition on child growth and development and included a food supplementation experiment. A key finding of that work is that growth failure occurred primarily in utero and in the first three years of life and was the cause of the short stature of adults. Differences between the Guatemalan sample and the international reference population increased until about three years of age and remained fairly constant thereafter (Martorell et al. 1995). Supplementation with *atole*, in comparison to *fresco*, increased the heights of three-year-old children by about 2.5 centimeters and reduced the prevalence of severe stunting by half. Supplementation produced its biggest effects by two years and, after three years of age, did not influence child growth rates (Schroeder et al. 1995).[10] The period from birth to 36 months is viewed by many as a window of opportunity for stimulating positive cognitive development through a variety of child development interventions, particularly in settings where ill health and malnutrition are common (Sternberg and Grigorenko 1997).

Individual characteristics include sex and birth year. The latter is important as it captures secular trends common to all individuals that might affect schooling outcomes, beyond the cohort control. We have explored the robustness of the results by including birth-year dummy variables rather than a continuous representation of birth year. Doing so has little effect on the parameters we estimate for exposure to *atole*, but test statistics

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[10]The nutritional literature emphasizes that undernutrition is most common and severe during periods of greatest vulnerability (Martorell 1997; UNICEF 1998). One such period is the first two-to-three years of life. Young children have high nutritional requirements, in part because they are growing so fast. The diets commonly offered to young children in developing countries to complement breast milk are of low quality (they are monotonous and have low energy and nutrient density), and as a result, multiple nutrient deficiencies are common. Young children are also very susceptible to infections because their immune systems (which are both developmentally immature and compromised by poor nutrition) fail to protect them adequately. Foods and liquids are often contaminated with feces and are thus key sources of frequent infections. Infections both reduce appetites and increase metabolic demands. Furthermore, in many societies, suboptimal traditional remedies for childhood infections, including withholding of foods and breast milk, are common. Thus infection and malnutrition reinforce each other.

marginally decrease, probably because we use 15 rather than one degree of freedom to represent the time trend.

Our vector of household characteristics include mother's and father's education, the logarithm of mother's and father's age when the individual was seven years old, dummy variables denoting the distance from the household to the feeding center where the supplements were distributed, and an index of household wealth. As part of the survey work accompanying the study in 1969–77, all households in these villages— including those with children participating in the supplementation trial—participated in a census in 1975 that ascertained ownership of a set of household durables as well as housing characteristics. Using principal components, these assets and characteristics were combined into an index we interpret as a "wealth" index. Though it may miss some dimensions of wealth, such as financial resources, at the time in the 1970s in these villages, those were not likely to have been very important and were likely to be correlated with the assets that were measured (Maluccio, Murphy, and Yount 2005). Altering the representations of parental schooling (for example, changing these to dummy variables equaling one if the father [mother] has any schooling) or changing the representations of parental age (to levels, for example, or including a quadratic) does not result in any meaningful change in our estimates of the impact of exposure to *atole*. We also explored whether other parental characteristics, such as religious affiliation and whether they were deceased while the child was attending school, had any effects, but these covariates were not statistically significant and so are not included in the results reported below.

We include dummy variables for three of the four study villages, capturing fixed characteristics of these localities that might affect education-related outcomes. One of these, for example, is the close link between Espíritu Santo and the town of El Jícaro, which facilitated more schooling in Espíritu Santo than in the other three villages. In addition, we exploit detailed qualitative and archival data found in Pivaral (1972) and Bergeron (1992), as well as a specially commissioned retrospective study undertaken in 2002 (Estudio 1360 2002; Maluccio et al. 2005), to control for community characteristics

that have changed over time. Specifically, we construct community-level covariates that relate as closely as possible to the timing of key education-related decisions in a child's development. In the estimates reported below, we include four such controls: the availability of a permanent structure for the primary school when the respondent was 7 and 13 years old, and primary school student-teacher ratios when the respondent was 7 and 13 years old. We chose age 7, since that is when most children are starting primary school, and age 13, since that is the point where children would begin secondary school if they entered at age 7 and progressed one grade each year through primary school. While these variables reflect community characteristics, they vary by individual (or, to be precise, by single-year age cohorts within each village). 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 it more closely relates the availability of the school to the period in the child's life when critical decisions about schooling were being made. Finally, we include indicators of proximity to the feeding center in each village, in an effort to account for differences in accessibility, which have been shown to affect intakes (Schroeder, Kaplowitz, and Martorell 1992), although, by design, few villagers lived more than a kilometer from the centrally located supplementation center. We also considered a broader set of such characteristics, including exogenous income shocks to these communities (booms and busts in the prices of key agricultural commodities; the availability of new sources of wage employment), changes in infrastructure (roads, water, and electricity), and other dimensions of school quality (for example, damage suffered by schools as a result of the 1976 earthquake), but their inclusion does not significantly alter the results reported below.

5. The Effect of Nutrition on Adult Educational Outcomes

Grades Completed

Table 2 presents the base specification we use to explore the effect of the early childhood experimental nutritional intervention on highest grade achieved (school

Table 2—Ordinary least squares estimates of determinants of highest grade attained, by sex

Covariates	Full sample	Women	Men
Exposure to intervention			
From birth to 36 months	0.125	-0.096	0.369
	(0.51)	(0.27)	(1.10)
From birth to 36 months × *atole*	0.335	1.004	-0.360
	(0.92)	(2.02)**	(0.66)
Controls			
Lived in San Juan	0.085	-0.326	0.440
	(0.22)	(0.67)	(0.72)
Lived in Conacaste	-0.618	-0.817	-0.419
	(1.94)	(2.05)**	(0.82)
Lived in Espírito Santo	1.575	1.263	1.883
	(5.60)**	(3.32)**	(4.51)**
Birth year	0.080	0.068	0.107
	(2.37)**	(1.55)	(2.05)**
Male	0.697		
	(4.35)**		
Log age, mother	-0.708	-0.802	-0.760
	(1.37)	(1.20)	(0.95)
Log age, father	0.622	0.391	1.165
	(1.24)	(0.60)	(1.51)
Grades schooling, mother	0.342	0.375	0.339
	(6.38)**	(4.89)**	(4.65)**
Grades schooling, father	0.275	0.198	0.338
	(6.03)**	(3.05)**	(5.64)**
Household wealth index	0.561	0.485	0.636
	(9.33)**	(6.00)**	(7.82)**
Village school had permanent structure when child was 7	-0.041	-0.128	0.063
	(0.14)	(0.33)	(0.15)
Village school had permanent structure when child was 13	-0.171	-0.123	-0.367
	(0.55)	(0.33)	(0.70)
Village student-teacher ratio when child was 7	-0.025	-0.032	-0.017
	(2.12)**	(2.10)**	(0.90)
Village student-teacher ratio when child was 13	-0.007	-0.012	0.003
	(0.33)	(0.44)	(0.10)
Model F statistic	21.21**	9.83**	13.03**
R square	0.27	0.24	0.30
Sample size	1,469	786	683

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04. Additional covariates included but not reported are dummy variables for distance to feeding centers and for observations with missing data on parental ages, parental education, household wealth index, and distance to feeding center. Standard errors are corrected for heteroscedasticity using Huber-White correction. Numbers in parentheses are absolute values of *t*-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

attainment) for adults interviewed in 2002–04, when they were between the ages of 25 and 42. We use an OLS estimator with the standard errors corrected for

heteroscedasticity using the method outlined by Huber (1967) and White (1980). Estimates for women and men combined are shown in the first column, and for women and men separately in the second and third columns, respectively. Reflecting the discussion above, conditional on the other variables in the specification, individuals born in Espíritu Santo have significantly higher education. Reflecting the trends and patterns in Guatemalan society as a whole, younger people have completed more schooling than older, and men have completed more schooling than women. While parental age (when the child was 7) is not associated with grades completed, completed grades of the mother and father, as well as the 1975 wealth index, are. Higher parental education and higher household wealth during childhood are associated with more grades completed. Formal primary education has been available in the villages since the early 1960s, but the quality of that schooling has improved over time at different rates across villages. As already noted, we capture some of the more important of these differences using indicators of school quality measured when children were at two critical ages in schooling progression: 7 and 13. The presence of a permanent (cement block) schooling structure does not seem to have had an effect on grades completed in the sample. The student-to-teacher ratio, however, does. This effect is dominant at younger ages and appears to be more important for women than for men.

For the full sample, we find no significant intent-to-treat effect of the *atole* intervention (relative to the *fresco* intervention) for children exposed from 0–36 months of age. A Chow test weakly indicates that the models are best estimated together by sex ($p = 0.13$), however, and one of the three individually significantly different coefficients is the *atole* exposure variable. When split by sex, there is a large and significant effect of *atole* for women—one full grade, which is nearly a third of a standard deviation in the sample. This is the first evidence to date that the nutritional intervention had an effect on schooling. Pollitt et al. (1993) found no effect on grades completed for this population when they are between the ages of 11 and 25. It would appear that the earlier conclusion, based on a younger population with censored educational outcomes, was premature. It is

also possible that the earlier conclusions were the result of not separating the sample by sex (though sex was controlled for with a single additive dummy variable).

To better understand what underlies these findings, we turn to an examination of where in the schooling cycle the intervention had an effect. In Figure 1, we saw that the distribution of schooling outcomes has several mass points, most notably at grades 0, 3, and 6. We therefore estimate an ordered probit model where we consider six outcomes: never attended school, completed 1–3 grades of primary school, completed 4–5 grades of primary school, completed the sixth grade of primary school, attained lower secondary school (grades 7–9), and attained upper secondary school or higher (10 or above). In this specification, a Chow test clearly indicates that the male and female models should be estimated separately ($p = 0.06$). In Table 3, we report the marginal effect of exposure to the *atole* supplement and its corresponding z-statistic for each category. These represent the discrete change in the proportion of individuals that complete the grades included in each category, when we change the dummy variable from 0 to 1.

Table 3—Ordered probit estimates of impact of exposure to *atole* from birth to 36 months on grades attained, by selected grades and sex

Outcome	Marginal effects of *atole* exposure (relative to *fresco*) from birth to 36 months		
	Full sample	Women	Men
Never attended school	-0.013	-0.061	0.034
	(0.59)	(2.02)**	(0.97)
Completed grades 1, 2 or 3, primary	-0.015	-0.075	0.038
	(0.57)	(1.89)*	(1.06)
Completed grades 4 or 5, primary	-0.002	-0.001	0.009
	(0.49)	(0.05)	(1.23)
Completed grade 6, primary	0.013	0.063	-0.032
	(0.59)	(2.18)**	(0.95)
Completed grades 7, 8, 9, lower secondary	0.007	0.028	-0.027
	(0.57)	(1.85)*	(1.41)
Completed grades 10 or above	0.010	0.045	-0.022
	(0.56)	(1.67)*	(1.07)

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04. Additional covariates included but not reported are location (San Juan, Conacaste, Espíritu Santo), birth year, sex, maternal and paternal ages and education levels, household wealth, distance from feeding centers, school structure at age 7 and at age 13, student teacher ratios at age 7 and at age 13, and dummy variables for observations with missing data on parental ages, education, household wealth, and distance to feeding center. Standard errors are corrected for heteroscedasticity using Huber-White correction. Numbers in parentheses are absolute values of t-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

For the full sample, again there is no evidence that *atole* affected the number of grades attained. However, women exposed to the *atole* supplement are more likely to have attended school. There is evidence that they are more likely to have completed primary school, entered secondary school, and completed both lower and upper secondary school and, consequently, commensurately less likely to have attained only the first few grades of primary school. For men, however, no significant pattern exists.

Progression through Schooling

With the finding that the intervention had an effect on grades attained by women, we next explore the mechanisms by which this effect occurred. There was little variation in age at starting school in the sample and there are no effects of the intervention on that indicator for either women or men (not shown). Nor do we find effects on the number of grades completed by age 13, indicating the effects do not seem to have occurred in primary school (Table 4). This is not altogether surprising since the gains in female education appear to be concentrated in the postprimary-school phase of schooling. This

Table 4—OLS estimates of impact of exposure to *atole* (relative to *fresco*) from birth to 36 months on schooling progression: Grades completed by age 13 and grades passed per year of school attended, by sex

Outcome	Full sample	Women	Men
Grades completed by age 13	0.064	0.411	-0.287
	(0.28)	(1.30)	(0.84)
	[N = 1,469]	[N = 786]	[N = 683]
Grades passed / years attended school	0.046	0.105	-0.025
	(1.31)	(2.14)**	(0.53)
	[N = 1,337]	[N = 715]	[N = 622]

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04. Additional covariates included but not reported are location (San Juan, Conacaste, Espíritu Santo), birth year, sex, maternal and paternal ages and education levels, household wealth, distance from feeding centers, school structure at age 7 and at age 13, student teacher ratios at age 7 and at age 13, and dummy variables for observations with missing data on parental ages, education, household wealth, and distance to feeding center. Standard errors are corrected for heteroscedasticity using Huber-White correction. Numbers in parentheses are absolute values of t-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

pattern is also consistent with our claim that earlier research on this population as adolescents and young adults did not find effects on education because of the censored nature of the measures of grades attained (Pollitt et al. 1993). When we consider the number of grades passed divided by the number of years attended school, however, we see a significant effect for women of 0.11 grades per year. Women who were exposed to *atole* during the first three years of life negotiated the educational system more effectively, passing grades more quickly than their counterparts who were exposed to *fresco* during the same period. This higher grade progression rate appears to be concentrated in the secondary school years and resulted from a combination of being less likely to repeat a grade or to drop out and re-enter, though it is not possible for us to distinguish between the two.

Cognition

Table 5 presents results of the intervention on the Raven's Progressive Matrices scores. These estimates are particularly important because they are the first to our knowledge to investigate the impact of a nutritional intervention on adult intellectual functioning after a quarter of a century or more. We find that for the full sample, exposing preschoolers to the *atole* supplement had a significant effect on cognition into adulthood. This finding holds when we consider different formulations of the dependent variable that take into account that the test gets progressively more difficult (for example, ordered probits and use of quadratic, cubic, or quartic transformations of the dependent variable). The effects for women and men, considered separately, while significant only for women, are quite similar, and tests suggest that in this case pooling the sample is preferred (the *p*-value on the Chow test is 0.61). It would seem that the insignificant separate results for men derive largely from the relatively small sample size.

Not only are the effects on the pooled sample significant, but they are also substantial. Average Raven's scores in the sample are 17.7 (6.1). The intent-to-treat effect of the *atole* intervention, 1.6 points, represents a 9 percent improvement over the

average score, one-quarter of a standard deviation. These results raise questions about the often-made interpretation of Raven's scores as reflecting innate abilities that are not altered by household and community allocations of resources (Alderman et al. 1996; Boissiere, Knight, and Sabot 1985). Parental education and resources as measured by the 1975 wealth index also have positive significant associations with the outcome (not shown), though these may be biased upward if the Raven's test score partly reflects intergenerationally correlated "ability" endowments.

Table 5—OLS estimates of impact of exposure to *atole* (relative to *fresco*) from birth to 36 months on Raven's Progressive Matrices, by sex

	Full sample	Women	Men
Score on Raven's Progressive Matrices	1.647	1.609	1.728
	(2.33)**	(1.79)*	(1.53)

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04. Additional covariates included but not reported are location (San Juan, Conacaste, Espíritu Santo), birth year, sex, maternal and paternal ages and education levels, household wealth, distance from feeding centers, school structure at age 7 and at age 13, student teacher ratios at age 7 and at age 13, and dummy variables for observations with missing data on parental ages, education, household wealth, and distance to feeding center. Standard errors are corrected for heteroscedasticity using Huber-White correction. Numbers in parentheses are absolute values of t-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

Reading Comprehension

As with the Raven's test scores, we find for the full sample that exposure to the *atole* intervention increases reading comprehension scores a quarter century later. If we consider the point estimate on the linear pooled specification as an indicator of the magnitude of the effect, we see that the intervention led to an improvement of approximately 14 percent in average scores (36.0 with a standard deviation of 22.3), nearly one-quarter of a standard deviation (Table 6). These effects are slightly larger and more precisely estimated for women, when we estimate separately for women and men, as is appropriate, given the Chow test ($p < 0.01$). However, these OLS estimates neglect both the censoring of the reading scores at zero as well as the fact that questions on these

tests become increasingly difficult. In light of this, we provide the results of two additional estimators. Table 7 shows the results of dividing the sample into two groups: those who scored above the median and those who scored below. Table 8 shows the results of dividing the sample into quartiles.

Table 6—OLS estimates of impact of exposure to *atole* (relative to *fresco*) from birth to 36 months on the Inter-American Reading test, by sex

	Full sample	Women	Men
Score on the Inter-American Reading test	5.075	6.318	4.233
	(2.02)**	(1.82)*	(1.16)

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04. Additional covariates included but not reported are location (San Juan, Conacaste, Espíritu Santo), birth year, sex, maternal and paternal ages and education levels, household wealth, distance from feeding centers, school structure at age 7 and at age 13, student teacher ratios at age 7 and at age 13, and dummy variables for observations with missing data on parental ages, education, household wealth, and distance to feeding center. Standard errors are corrected for heteroscedasticity using Huber-White correction. Numbers in parentheses are absolute values of t-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

The probit results reported in Table 7 indicate that exposure to the supplement increased men's reading scores but not those of women, and the effect is large in magnitude: men who were exposed to *atole* between the ages of 0–36 months increased the likelihood of being in the top half of the distribution by 22 percentage points. The marginal effects of the ordered probits reported in Table 8 show that moving these men out of the bottom two quartiles and into the top two quartiles generated this increase. Even though the intervention appears not to have increased the number of grades completed for men, there is evidence that it increased their reading ability. This may be the result of more learning during the same number of grades, or of postschooling experiences, such as in the labor market (Behrman et al. 2005). It may also be related to the improved cognition demonstrated earlier in this section.

Table 7—Probit estimates of impact of exposure to *atole* (relative to *fresco*) from birth to 36 months on the likelihood of being in the top half of scores on the Inter-American Reading test, by sex

Outcome	Marginal effects of *atole* exposure (relative to *fresco*) from birth to 36 months		
	Full sample	Women	Men
Individual is in top half of reading scores	0.147	0.105	0.220
	(2.28)**	(1.17)	(2.39)**

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04. Additional covariates included but not reported are location (San Juan, Conacaste, Espíritu Santo), birth year, sex, maternal and paternal ages and education levels, household wealth, distance from feeding centers, school structure at age 7 and at age 13, student teacher ratios at age 7 and at age 13, and dummy variables for observations with missing data on parental ages, education, household wealth, and distance to feeding center. Standard errors are corrected for heteroscedasticity using Huber-White correction. Numbers in parentheses are absolute values of t-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

Table 8—Ordered probit estimates of impact of exposure to *atole* (relative to *fresco*) from birth to 36 months on quartiles of scores on the Inter-American Reading test, by sex

Outcome	Marginal effects of *atole* exposure (relative to *fresco*) from birth to 36 months		
	Full sample	Women	Men
Bottom quartile	-0.093	-0.110	-0.083
	(2.63)**	(2.14)**	(1.82)*
Second quartile	-0.038	-0.039	-0.046
	(2.11)**	(1.62)	(1.51)
Third quartile	0.025	0.036	0.018
	(3.22)**	(2.55)**	(2.42)**
Top quartile	0.106	0.113	0.112
	(2.31)**	(1.83)*	(1.60)

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04. Additional covariates included but not reported are location (San Juan, Conacaste, Espíritu Santo), birth year, sex, maternal and paternal ages and education levels, household wealth, distance from feeding centers, school structure at age 7 and at age 13, student teacher ratios at age 7 and at age 13, and dummy variables for observations with missing data on parental ages, education, household wealth, and distance to feeding center. Standard errors are corrected for heteroscedasticity using Huber-White correction. Numbers in parentheses are absolute values of t-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

Robustness to the Calculation of Standard Errors

The original intervention occurred in only four villages. Duration of exposure to the intervention was dependent on date of birth. As explained above, failing to account for these features would make our estimates of the impact of exposure to the *atole* supplement vulnerable to omitted variable bias. For this reason, all regression results have included both village dummies and a covariate capturing cohort effects. As noted above, standard errors are calculated using the methods proposed by Huber (1967) and White (1980) that correct for heteroscedasticity of unknown form. However, Moulton (1990, p. 334) has noted, "It is reasonable to expect that units sharing an observable characteristic, such as industry or location, also share unobservable characteristics that would lead the regression disturbances to be correlated." These correlations, if positive, may cause the estimated standard errors to be biased downward. In the statistics literature, this issue is referred to as the design effect (see Kish 1965 and Deaton 1997).

In Table 9, we consider the implications of these possible correlations on the estimation of the standard errors for a selection of the key results described above: grade attainment by female respondents (highest grade attained and grades passed per year attended), Raven's and SIA test scores for women and men combined, and whether men and women combined were more likely to score in the top half of the distribution of SIA scores. We use this subset, given that we found positive and seemingly statistically significant impacts of exposure to the *atole* supplement between 0–36 months for all these outcomes. Table 9 reports the associated *t*-statistics for the exposure to *atole* based on the Huber-White correction for heteroscedasticity as well as the popular solution to this problem, namely the incorporation of the design effect into the construction of the regression standard errors (see Deaton 1997). Based on the design of the intervention, we have 64 clusters (4 villages x 16 different birth years).

Table 9—Select results with alternative calculations of standard errors

Outcome	T statistic for coefficient on exposure: Birth to 36 months × *atole* based on standard errors calculated		
	Using Huber-White method	Accounting for clustering (village × birth year)	Block bootstrapped (1,200 repetitions) drawn from clusters (village × birth year)
Highest grade attained, women	2.02**	2.05**	1.77*
Grades passed / years attended school, women	2.14**	2.24**	1.93*
Raven's Progressive Matrices	2.33**	2.45**	2.18**
Inter-American Reading Series	2.02**	1.79*	1.59
Inter-American Reading Series, likelihood of being in top half of distribution	2.28**	2.21**	1.90*

Notes: See Table 3 for a description of the sample and other covariates that are included but not reported. Standard errors are calculated in three different ways: using the Huber-White correction; accounting for clustering (village × birth year), using the cluster option in STATA; and by block bootstrapping. Numbers in parentheses are absolute values of t-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

Taking into account this clustering does not, in general, reduce the magnitudes of the *t*-statistics as one would expect if indeed there was substantial positive correlation of the error terms within clusters.[11] Indeed, the results of Table 9 indicate that this standard correction for clustering leads to smaller standard errors and larger *t*-statistics for most outcomes. This also holds true for a number of covariates for which one might expect the clustering problem to be most severe, such as the dummy variables controlling for village of birth and the characteristics of the local primary schools (not shown). Work by Angrist and Lavy (2002) and Wooldridge (2003), however, suggests that this arises because these standard corrections for clustering are valid only when the number of units or groups or clusters of observations is large. In light of this, we calculate two alternative standard error estimators that allow for clustering. The first, following Bertrand, Duflo, and Mullainathan (2004), is to block bootstrap the *t*-statistics. We construct a bootstrap sample by drawing, with replacement, 64 matrices consisting of outcomes and their regressors. We run the regressions on this sample, obtain the *t*-statistic, and then replicate this exercise 1,200 times. Table 9 also reports the results of these bootstrapped

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[11]As a further check, we estimated these models allowing for clustering among siblings. This makes very little difference to the estimated standard errors. To save space, we do not report these here.

t-statistics. They are all smaller than both the Huber-White *t*-statistics in the first column and those accounting for clustering reported in the second column. We continue to find a statistically significant impact of exposure to the supplement on all of these outcomes, except for the OLS specification of SIA, where the *p*-value falls from 0.09 (under Huber-White) to 0.12 (with block bootstrapped standard errors).

An alternative approach is to aggregate all covariates up to their group means and carry out estimation on the average data (Hoddinott 1996; Wooldridge 2003). The cost of this approach is a considerable loss in degrees of freedom as the sample size drops from more than 1,450 to 64 observations. Mindful of this, Table 10 reports the results of estimating the determinants of the selection of key outcomes that were also reported in Table 9. The parameter estimates are similar to those derived from individual-level data. Not surprisingly, given the large number of regressors and the much reduced sample size, two of the five coefficients in the first column are no longer statistically significant,

Table 10—Select group-level (village × birth year) results

Outcome	Coefficient on exposure: Birth to 36 months *x atole*	Coefficient on exposure: Birth to 36 months *x atole* Restricted specification
Highest grade attained, women	1.117 (1.79)*	1.113 (1.97)*
Grades passed / years attended school, women	0.104 (1.66)*	0.139 (2.48)**
Raven's Progressive Matrices	1.488 (1.44)	1.511 (1.81)*
Inter-American Reading Series	6.632 (1.69)*	6.586 (2.00)**
Proportion in top half of distribution of scores on Inter-American Reading Series	0.148 (1.49)	0.168 (2.28)**

Notes: Data used to estimate these results are group (village × birth year) means of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2003-04. Models are estimated as probits; results above are reported in terms of marginal effects. Additional group mean covariates included but not reported are location (San Juan, Conacaste, Espíritu Santo), birth year, sex, maternal and paternal ages and education levels, household wealth, distance from feeding centers, school structure at age 7 and at age 13 student teacher ratios at age 7 and at age 13 and dummy variables for observations with missing data on parental ages, education, household wealth, and distance to feeding center. The restricted specification drops parental age and education. Standard errors are calculated using the Huber-White correction. Numbers in parentheses are absolute values of *t*-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

though they generally have *p*-values on the order of 0.15. Several of the control variables, however, are no longer significant. If we exclude average maternal and paternal age and schooling (which are neither significant individually or jointly in these regressions)—the restricted regressions reported in Table 10—we get similar parameter estimates but increased *t*-statistics. This is consistent with a view that the apparent loss of statistical significance is driven largely by the loss of degrees of freedom associated with moving to group level means. We conclude that the results presented in the previous section are robust to alternative methods of calculating standard errors.

Robustness to Attrition

The estimates presented in this paper are based on a sample of 1,469 individuals, 61 percent of the original 2,393 subjects.[12] Despite the considerable effort and success in tracing and reinterviewing participants from the original sample, an attrition of 39 percent is substantial and raises concern about the validity of the estimates reported above. Moreover, as shown in Grajeda et al. (2005), the overall attrition in the sample is associated with a number of initial conditions with effects differing by the reason for attrition. However, what is of ultimate concern is not the level of attrition but whether, and to what extent, the attrition invalidates the inferences we make using these data. For example, does excluding sample members who died in infancy or early childhood or international migrants, who may have different characteristics, lead to systematic bias of the estimates presented here?

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[12]Another related problem is that of mortality selection (Pitt and Rosenzweig 1989; Pitt 1997). Indirect evidence that mortality selection exists in the sample is that higher risk of death is associated with younger ages in the complete sample of 2,393. 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 naturally excluded all children from the villages born between 1962 and 1969 who died before the start of the survey. Last, the intervention has been shown to decrease mortality rates among the younger cohort (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. Our identification strategy using a cohort control guards against the first type of selection and, in our judgment, leads to downward-biased estimates as a result of the second form.

We control for sample attrition in two ways. First, in the specifications already shown, we include a large number of covariates, many of which, in addition to playing a role in educational outcomes, are themselves associated with attrition (Grajeda et al. 2005). Conditional on the maintained assumptions about the functional form, attrition selection on right-side variables does not lead to attrition bias (Fitzgerald, Gottschalk, and Moffitt 1998b).

Second, we implement the correction procedure for attrition outlined in Fitzgerald, Gottschalk, and Moffitt (1998a; 1998b). We first estimate an attrition probit conditioning on all the exogenous variables considered in the main models, as well as an additional set of endogenous variables associated with attrition. We include a number of variables that reflect family structure in previous years, since these are likely to be associated with migration status. They include whether the parents were alive when each sample member was seven years old and whether the sample members lived with both their parents in 1975 and in 1987. During the fieldwork, locating sample members was typically facilitated by having access to other family members from whom the field team could gather information. Therefore, we also include a number of variables that capture this feature of the success of data collection. They include whether the parents were alive in 2002, whether they lived in the original village, whether a sibling of the sample member had been interviewed in the 2002–04 follow-up study, and the logarithm of the number of siblings in the sample in each family. We emphasize that this is *not* a selection correction approach in which we must justify that these factors can be excluded from the main equations, but rather we purposively exclude them from those regressions, since our purpose is to explore the determinants of educational outcomes outlined in equation 1 and not whether educational outcomes are associated with the family structure and interview-related factors included in the "first-stage" attrition regression (Fitzgerald, Gottschalk, and Moffitt 1998a).

Finally, while we do not formally have adjustments to correct for selection on unobservable characteristics, by including the large number of endogenous observables

indicated above, which are likely to be correlated with unobservables, we expect that we are reducing the scope for attrition on unobservables bias as well.

The factors described above are highly significant in predicting attrition, above and beyond the conditioning variables already included in the models (Appendix Table 12). They lead to weights between 0.33 and 2.24 for those individuals in the 2002–04 sample. Table 11 shows that they attenuate only slightly the results that do not correct for attrition, and with the exception of the OLS estimates of the effect of SIA, the central results all remain significant. We interpret these findings to mean that, as found in other contexts with high attrition (Fitzgerald, Gottschalk, and Moffitt 1998b; Alderman et al. 2001a), our results do not appear to be driven by attrition biases.

Table 11—Select results accounting for attrition bias

Outcome	Coefficient on exposure: Birth to 36 months x *atole*	Coefficient on exposure: Birth to 36 months x *atole* accounting for attrition
Highest grade attained, women	1.004 (2.02)**	0.837 (1.71)*
Grades passed / years attended school, women	0.105 (2.14)**	0.096 (1.86)*
Raven's Progressive Matrices	1.647 (2.33)**	1.379 (2.00)**
Inter-American Reading Series	5.075 (2.02)**	3.400 (1.36)
Inter-American Reading Series, likelihood of being in top half of distribution	0.147 (2.28)**	0.199 (2.17)**

Notes: See Table 3 for description of sample and other covariates that are individual but not reported. Standard errors are calculated using the Huber-White correction. Results account that account for attrition are based on applying sampling weights on the characteristics of individuals who remained in the sample and those who were attritional (see Section 5.6 and Appendix Table 1). Numbers in parentheses are absolute values of *t*-statistics. * Significant at the 10 percent level; ** significant at the 5 percent level.

6. Conclusions

This paper considers the impact of community-level randomized nutritional interventions in rural Guatemala on several different measures related to education over the life cycle. For each measure we estimate the effects of exposure to the intervention

between 0 to 36 months of age, exploiting the randomized nature of the intervention for identification of the program effects. We advance beyond the previous literature by using unique longitudinal data from a nutritional experiment carried out in rural Guatemala in the 1970s that enables us to consider educational measures not only for school-age years but up through prime adult years. Moreover, we avoid confounding the estimates by not including right-side variables that probably are endogenous and by using estimators that allow for nonnormal distributions.

The results indicate significantly positive, and fairly substantial, effects of the nutritional intervention a quarter century after it ended. These include increased grade attainment by women, via increased likelihood of entry and increased likelihood of completion of primary school and some secondary school; speedier grade progression by women; higher scores on cognitive tests for both men and women; and higher scores on educational achievement tests for both men and women. The findings are robust to the manner in which we calculate the standard errors as well as to sample attrition.

The results provide strong evidence of the role played by preschool nutrition in subsequent educational attainments and thus underscore the value of a life-cycle approach to education that includes the preschool period. They suggest that programs that include nutritional supplements to very young children, or in other ways improve their nutritional intakes, may have substantial, long-term educational consequences. But they also raise additional questions. What are the physical, social, and economic pathways by which these effects come about? What are their consequences for health, earnings, and other dimensions of well-being? These are the topics of ongoing research using these data from the Human Capital Study.

Appendix Table

Table 12—Attrition probits to construct weights used in Table 11

Covariates	Model 1	Model 2
	(1) if in sample	(1) if in sample
Exposure to intervention		
From birth to 36 months	0.0126	0.0187
	(0.38)	(0.52)
From birth to 36 months × *atole*	-0.0157	-0.0277
	(0.32)	(0.54)
Lived in San Juan	0.0373	0.0154
	(0.71)	(0.28)
Lived in Conacaste	-0.0405	-0.0430
	(0.95)	(0.98)
Lived in Espíritu Santo	0.0788**	0.0570
	(2.00)	(1.40)
Birth year	-0.0066	-0.0101*
	(1.44)	(1.91)
Male	-0.1206**	-0.1307**
	(5.69)	(5.99)
Log age, mother	-0.0274	-0.0193
	(0.38)	(0.25)
Log age, father	-0.0568	0.0181
	(0.77)	(0.24)
Grades of schooling, mother	0.0060	0.0074
	(0.86)	(1.00)
Grades of schooling, father	-0.0040	-0.0069
	(0.72)	(1.20)
Household wealth	-0.0034	-0.0038
	(0.48)	(0.52)
Village school had permanent structure when child was 7	-0.0760*	-0.0888**
	(1.93)	(2.20)
Village school had permanent structure when child was 13	-0.0052	0.0023
	(0.12)	(0.05)
Village student-teacher ratio when child was 7	-0.0033*	-0.0031*
	(1.85)	(1.74)
Village student-teacher ratio when child was 13	0.0015	0.0019
	(0.53)	(0.66)
Mother alive when child age 7	-	-0.1317
		(0.96)
Father alive when child age 7	-	0.0155
		(0.13)
Child lived with both mother and father in 1975	-	-0.0143
		(0.42)
Child lived with both mother and father in 1987	-	0.0922**
		(3.08)
Mother alive in 2002	-	0.1349**
		(3.83)
Father alive in 2002	-	0.1120**
		(3.72)
Mother living in original village in 2002	-	0.0421
		(1.16)
Father living in original village in 2002	-	0.0140
		(0.42)
Logarithm of number of siblings in survey	-	-0.2044**
		(6.57)
Whether any sibling re-interviewed in 2002-04	-	0.4127**
		(9.59)
Chi-square statistic on variables in model 2 only		159.9**
		[<0.01]
Model Chi-square statistic	311.6**	447.9**
	[< 0.01]	[< 0.01]
Pseudo-R^2	0.12	0.17
Sample size	2,393	2,393

Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977. Additional covariates included but not reported are dummy variables for distance to feeding centers, and observations with missing data on parental ages, education, household wealth, and distance to feeding center. Standard errors are corrected for heteroscedasticity using Huber-White correction. Derivatives evaluated at the mean (*dP/dx*) presented with the absolute value of corresponding z-statistics in parentheses. * Significant at the 10% level; ** significant at the 5% level. *P*-values are in brackets.

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