The Impact of Experimental Nutritional Interventions on Education into Adulthood in Rural Guatemala: Preliminary Longitudinal Analysis

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Abstract

Early childhood nutrition is thought to have important effects on education, broadly defined to include various forms of learning. A number of previous studies have contributed to the current conventional wisdom about the impact of nutrition on education in developing countries. Yet there are considerable limitations in what we know about the impact of nutrition on education in such contexts. In this paper we investigate the impact of community-level experimental nutritional interventions in rural Guatemala on a number of aspects of education, using the well-known INCAP longitudinal data set dating back to the initial intervention in 1969-77 (when the subjects were 0-15 years of age) with the most recent, and not previously utilized, information collected in 2002-3 (when the subjects were 25-40 years of age). We consider the impact of these interventions on several different measures related to education over the life cycle. For each of these measures we estimate the impacts of experimental nutritional interventions during the critical period when individuals were six months (roughly when complementary feeding becomes important, including the consumption of supplements) through 24 months of age. We advance beyond the previous literature through the combination of utilizing longitudinal data from a nutritional experiment, having longitudinal data on educational measures not only for school-age years but up through prime adult years, avoiding confounding the estimates by not including right-side variables that probably are endogenous and using estimators that allow for non-normal distributions (which make a considerable difference in the estimated magnitude and precision of the estimates). Our substantive results indicate significantly positive and fairly substantial effects of consumption of a nutritious supplement called Atole on all of the outcomes that we consider related to education: increasing the probability of attending school and of passing the first grade, increasing the grade attained by age 13 (through a combination of increasing the probability of ever enrolling, reducing the age of enrolling, increasing the grade completion rate per year in schooling, and reducing the dropout rate), increasing completed schooling attainment, increasing adult Raven's test scores and increasing adult cognitive achievement scores. Thus there are important education-related effects, some of which were not explored or, if explored, were not found in previous research (including that using data on the same nutritional interventions) and some of which persist well into adulthood. In the estimates in which we include interactions with family background characteristics, in a number of cases we find a mixture of patterns – in some cases there is the suggestion that at a gross level children coming from better family backgrounds benefited more from the nutrition interventions (e.g., when considering the role of socioeconomic status measured in 1975) and in other cases the reverse (e.g., when considering the role of mother's education).

1. Introduction

The determinants of education continue to fascinate scientists and social scientists in many fields. This reflects education's intrinsic and instrumental value. Most would argue that increasing levels of educational attainments are valuable in their own right. Further, education has considerable value in alleviating poverty and increasing productivity.

These dual values are reflected in the manner in which economists have typically approached the modeling of education outcomes. Education is an argument in individual or household welfare functions that are maximized subject to a set of constraints including time and income (and the factors that affect current and future incomes), as well as factors that directly affect the production of education such as parental and school characteristics. There are a number of features of the education literature, however, that remain unsatisfactory. Many studies are based solely on cross-sectional data, have limited representations of education, are based on school-age individuals so that any long-run consequences can only be simulated rather than observed, assume that external constraints on schooling are absent, or use estimation strategies that a) condition on behavioral right-side variables – such as grades of schooling – without controlling for the determinants of such behaviors, or b) (implicitly) assume unlikely stochastic distributions to justify using ordinary least square.

Analysts have placed increasing emphasis on what is sometimes described as the life cycle approach to education and human capital formation. At the core of this approach is the view that events and outcomes that affect children have effects that persist and thus affect subsequent outcomes as those children age. For example, factors that affect early childhood nutrition are thought to have important effects on subsequent learning and schooling performance. While a number of previous studies have contributed to the current conventional wisdom about the impact of nutrition on education in developing countries, data limitations and methodological weaknesses also limit the extent to which causal associations can be made confidently.

This paper contributes to knowledge regarding the impact of nutrition on education, using data and methods not vulnerable to the concerns delineated above. Specifically, we investigate the impact of a community-level experimental nutritional intervention in rural Guatemala, fielded between 1969 and 1977. We link information on individuals exposed to the intervention (collected during the intervention when the subjects were 0-15 years of age) with new data on these same individuals collected in 2002–3 (when the individuals were 25-40 years of age). We consider the impact of these interventions on several different education-related measures over the life cycle:

- Whether ever enrolled in formal schooling
- Whether ever passed the first grade of formal schooling
- What formal schooling level had been completed by age 13
- Highest completed grade of (formal and informal) schooling
- Educational achievement test performance in adulthood (literacy, vocabulary comprehension)

- Raven's test performance in adulthood.

For each measure, we estimate the impacts of a randomized nutritional intervention during the critical period when individuals were between six (roughly when supplementation to breastfeeding was initiated) and 24 months of age. These estimates control for time invariant community characteristics. Exploiting detailed historical and anthropological studies undertaken in the survey areas, they also control for relevant observed time-varying community shocks that might otherwise confound the impact of the nutritional intervention. We also present estimates in which we control for several family background characteristics – family socio-economic status (SES), parental education, parental age – and, in separate estimates, interactions between family SES and parental education with the nutritional interventions to explore whether the impacts of the nutritional interventions vary across family background. While better-off families may be better able to exploit these interventions, it is also possible that such interventions have diminishing marginal products as family background improves. We use estimators – probits or ordered probits – that allow for the fact that the distributions of all but one of the dependent variables (the exception being the Raven's scores) have mass points at zero (e.g., highest completed grade of schooling, what schooling level had been completed by age 13, cognitive test scores) or at other values (e.g. at six for completion of primary schooling for highest completed grade of schooling and what schooling level had been completed by age 13).

Our substantive results indicate significantly positive and fairly substantial effects of the nutritional intervention, called *Atole* (see Section 3 below for description of the intervention) on all of the outcomes that we consider related to education: increasing the probability of attending school and of passing the first grade, increasing the grade attained by age 13 (through a combination of increasing the probability of ever enrolling, reducing the age of enrolling, increasing the grade completion rate per year in schooling, and reducing the dropout rate), increasing completed schooling attainment, increasing adult achievement test scores, and increasing adult Raven's test scores. In the estimates in which we include interactions with family background characteristics, results are mixed– in some cases there is the suggestion that at a gross level children coming from better family backgrounds benefited more from the nutrition intervention (e.g., when considering the role of socioeconomic status measured in 1975) and in other cases the reverse (e.g., when considering the role of mother's education).

We emphasize, however, that while the framework and identification strategy we use are viable, these results are subject to an important caveat. Data collection is still ongoing for the 2002–3 study; in particular, the data we use do not include migrants (from the original study villages) who are currently being traced and re-surveyed. Since migration is almost certainly non-random, these results are genuinely preliminary. The results in this paper will be re-estimated to incorporate these individuals when data on them are available.

The paper is organized as follows. Section 2 briefly reviews related literature on the importance of nutrition in the first two years of life and on the impact of such nutrition on subsequent education. Section 3 provides a conceptual framework for modeling these relationships. Section 4 describes the areas in Guatemala where we are working and the data in general terms. Section 5 describes in more detail the data that we use in this study. The results of applying our conceptual framework to these data are found in section 6. Section 7 concludes.

2. Previous Related Literature

Two aspects of the previous literature are of particular relevance for this study. These include studies of the relative importance of nutrition in the initial years of life and studies that investigate the impact of improved nutrition early in the life cycle on subsequent education-related measures.[1]

2.1 Importance of Nutrition in First Two Years of Life

The nutritional literature emphasizes that undernutrition is most common and severe during periods of greatest vulnerability (Martorell 1997, UNICEF 1998). One such period is *in utero*. In developing countries with nationally-representative samples, up to a fifth or more of babies are born malnourished, as measured by a birth weight of 2.5 kg or less (UNICEF, 2001; Behrman and Rosenzweig, 2004). A second vulnerable period is the first two years or so of life. Young children have high nutritional requirements, in part because they are growing so fast. Unfortunately, the diet commonly offered to young children in developing countries to complement breast milk are of low quality (i.e., 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. In poor countries, foods and liquids are often contaminated with feces and are thus key sources of frequent infections. Infections both reduce appetite 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.

Severe protein-energy malnutrition presents itself as kwashiorkor and marasmus, syndromes that are characterized by clinical signs, marked metabolic disturbances, and high case fatality rates in the absence of high quality care (Waterlow 1992). Severe malnutrition is rare, however, even in very poor countries. Most malnutrition in children is best described as mild to moderate and is measured in terms of growth failure against a standard reference population. Weight and height are expressed as age-specific Z-scores[2] and the criterion of –2 Z for height and weight is commonly used to identify stunting and

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[1] More detailed reviews are found in Knowles and Behrman (2003a, 2003b), Hoddinott and Quisumbing (2003), Grantham-McGregor et al. (1999) and Martorell (1997).

[2] Z scores are used to normalize measured heights and weights against those found in well-nourished populations. They are age and sex specific; for example, a z score of height-for-age is defined as measured height minus median height of the reference population all divided by the standard deviation for that age/sex category.

underweight respectively. The percentage of stunted or underweight children is the standard way of describing the extent of child malnutrition in societies (ACC/SCN 2000).

The longitudinal data on which we build in this study has contributed much to understanding the nature of child growth failure in developing countries. This longitudinal study was designed to assess the effects of improved nutrition on child growth and development and included a food supplementation experiment. A key finding 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 till about three years of age and remained fairly constant thereafter (Martorell, et al. 1995). Supplementation with a nutritious beverage, in comparison to a low nutrient beverage, increased the heights of three year-old children by about 2.5 cm and reduced the prevalence of severe stunting by half. However, supplementation produced its biggest effects by two years and after three years of age did not influence child growth rates (Schroeder, et al. 1995).

The consequences of child malnutrition during the preschool period have been studied extensively. One consequence is increased mortality. It is estimated that about half of all deaths in developing countries in children less than five years of age are due to the interaction between malnutrition and common infections such as diarrheal diseases, respiratory infections and measles (Pelletier et al. 1993, 1995). These infections kill children easily only in the presence of malnutrition, which impairs immune function and lowers resistance to infections. Supplementation with foods or with specific nutrients improves survival. In the INCAP study population, infant mortality rates were reduced by 70 percent in the two villages receiving a nutritious supplement, compared to a reduction of 24 percent in villages receiving a less nutritious, control beverage (Rose, Martorell, and Rivera 1992). Meta-analyses of controlled trials suggest that supplementation with vitamin A reduces child mortality rates by 23 percent and that supplementation with zinc reduces the incidence of diarrheal diseases by 18 percent and of pneumonia by 40 percent (Beaton et al. 1993; Bhutta et al. 1999). Numerous studies have demonstrated that motor and mental developments are affected adversely by poor nutrition (measured as underweight or stunting) and that these relationships persist after controlling for socioeconomic characteristics (Wachs 1995). Food supplementation experiments, including the INCAP study, indicate that improved dietary intakes lead to modest improvements in cognition (Martorell 1997). Deficiencies of two micronutrients, iodine and iron, are important causes of poor cognitive development, particularly when they affect children under two years of age (Bleichrodt and Born 1994; Lozoff and Wachs in press). Other nutrients of potential importance to child development include zinc and long chain polyunsaturated fatty acids, especially docosahexaenoic acid (DHA).

2.2 Impact of Early Childhood Nutrition on Education-Related Indictors for Children and Youth

There is a large body of literature derived from cross-sectional studies that relates parental family background to child and youth outcomes, including education-related outcomes like those we consider in this paper. This literature suggests that parental decisions are important determinants of early childhood development and of human capital development for school-age children and adolescents. Some of these studies include representations of nutrition of the children among the determinants (see Behrman 1996 and Pollitt 1990 for surveys that include a number of such studies). For the most part, however, these cross-sectional estimates are difficult to interpret as reflecting causal effects because of the problems in controlling for the behavioral determinants of the right-side nutritional variables.[3]

In reviewing the existing literature, we focus on longitudinal studies, like our own, that link measures of early childhood nutrition to subsequent measures of education-related indicators for school-age children and adolescents.[4] There are few such studies, especially from developing countries; these studies indicate that nutrition in early childhood is a strong determinant of human capital in school children and adolescents.

Studies from Barbados and Jamaica, for example, find that severe malnutrition is associated with the subsequent behavioral and cognitive development of children (Galler, et al. 1983; Galler 1984; Richardson, Birch, and Ragbeer 1995). Children who were severely malnourished as young children were less well liked and were unhappier than classmate controls. Affected children also behaved immaturely more often and were clumsier. Children with a history of malnutrition were either more active or more lethargic than their classmates and were more often withdrawn, solitary, or unsociable. In both Barbados and Jamaica, previously severely malnourished children had poorer relationships with classmates and teachers, exhibited a greater degree of attention deficit, and received poorer grades in school. Anemia causes children to be lethargic; by diminishing their interaction with the environment it thus impairs cognitive functioning and learning (Lozoff and Wachs in press). Evidence from prospective studies from five

[3] A few of these studies explore these relationships using sibling data and are thus able to control for factors constant to families, including family average genetic endowments and other aspects of family and neighborhood background though not for unobserved time-varying factors (Behrman and Wolfe 1984, 1987a,b, 1989)

[4] Because of the focus in this paper on the effects of nutrition in the first two years of life, we do not review extensively the literature on the impact of pre-natal nutrition. Prenatal malnutrition in developing countries is predominantly manifested as intrauterine growth retardation (IUGR), i.e., low birth weight (<2.5 kg) at normal gestational age (Villar and Belizan 1982). Studies of the long-term effects of IUGR on mental performance and behavior provide varying and conflicting results for many reasons, including the heterogeneity of the populations studied, variations in the quality of neonatal care provided, and the fact that many postnatal conditions may confound outcomes. Hack's (1998) review of follow-up studies to school age and beyond, all from developed countries, reported overall normal intelligence with a trend to lower IQ scores among IUGR subjects. Stein et al. (1975) showed that acute undernutrition *in utero* was not associated with any measurable change in performance on Raven's progressive matrices among Dutch conscripts. Although the rates of major handicap in IUGR were low, there tended to be higher rates of minimal cerebral dysfunction, as evidenced by learning and subtle neurological and behavioral problems. The only report from a developing country is from the Guatemalan study - Pollitt, Gorman and Melallinos-Katasaras (1991) found that poor growth during infancy, rather than low birth weight, was negatively related to adolescent performance on cognitive and achievement tests.

countries indicates that having had anemia in early childhood has long-term adverse consequences for school performance and cognitive functioning.

Despite these findings, it is difficult to attribute unequivocally cognitive, behavioral, or physical effects to severe malnutrition. For example, wide-ranging disadvantages in the homes and families of malnourished children make it nearly impossible to match index and comparison children adequately (Grantham-McGregor 1995). A number of recent studies attempt to redress these limitations. Two use the Cebu, Philippines longitudinal sample of children born in 1983–4. The first gives within-sibling estimates for the impact of height Z-scores at initial school enrollment on cognitive achievement at age 11, age of initial school enrollment, grade repetition experience up to age 11, and various aspects of time use (e.g., hours of homework) (Glewwe, Jacoby and King 2000). This study found that better nourished children (i.e., less stunted) performed significantly better in school, partly because they entered school earlier but mostly because of greater learning productivity per year of schooling. Another finding was that controlling for family endowments and for behavioral choices by using instrumental variables affected substantially the estimated impact of early childhood nutrition. The second study with these data by Glewwe and King (2001) investigated whether malnutrition in the first six months had the greatest adverse effects on child cognitive development, as argued by some observers. The authors found instead that the second year of life was most critical once there was control for household choices affecting child nutritional status at the start of each period of interest, but that simpler estimates gave misleading results in this regard. A third study by Alderman et al. (2001) investigated the impact of pre-school child nutritional status on school enrollment in rural Pakistan. This study used price shocks when children were of preschool age to control for household choices that might have influenced child nutrition. It found that child nutrition was three times as important for enrollment than was suggested by "naive estimates" that assumed that child health was independent of household choices. Finally, Alderman, Hoddinott and Kinsey (2003) assess the impact of early childhood nutrition on grade attainment and age at which children started school. Using time varying shocks such as exposure to drought to control for household choices that might affect child nutrition, they – like Alderman et al. (2001) find that controlling for such choices markedly increases the impact of child nutrition on education-related outcomes.

We end this review by briefly summarizing some of the previous work undertaken on the individuals in the earlier rounds of the sample used in the present study. A partial follow-up to the 1969-77 intervention took place in 1988, when the subjects were 11 to 26 years of age. Supplementation improved performance on psychoeducational tests of knowledge, numeracy, reading, and vocabulary and also led to faster reaction times for information processing tasks, though no impact on schooling attainment was reported (Pollitt et al. 1993). Effects of the supplement varied with socioeconomic status (SES) and schooling: effects were higher for children of low SES (suggesting diminishing marginal returns to the improved nutrition) and among those children with more years of schooling. However the study does not control for the fact that grades of schooling are determined by household behaviors so it is difficult to know how to interpret the coefficient for the nutrition-schooling interaction.

Similar results were found in analyses that use height at three years as the measure of early childhood nutrition (Martorell et al. 1995). The cohort was stratified into three groups depending upon the degree of stunting at age three: severe, below –3 Z; moderate, between –3 and –2 Z; and mild, above –2 Z. In males, growth failure prior to three years of age was associated with late entry into school, fewer years of schooling, and with lower scores on tests of intelligence and poorer performance in tests of general knowledge, numeracy, reading and vocabulary. For females, significant relationships were observed only for age at school entry and reading ability. These analyses controlled for maternal schooling, family socioeconomic characteristics, and other variables. More recently, Li et al. (2003a, b) assessed the associations of the supplement, schooling, and growth in one of three periods (prenatal, 0-24 months, and post-24 months) with a wide array of measures of educational attainment among women in the study villages who bore a child between 1996 and 1999. They find that exposure to *Atole* resulted in higher performance on the tests of educational attainment (a scale that combined literacy, numeracy, and cultural competency) conditional on completion of sixth grade, and that growth in the first two years of life was associated with increased odds of performing in a higher quintile. Once again, however, the results in this study are conditional on a right-side variable that would seem endogenous (i.e., sixth grade completion) without controls for the behavioral determinants of that variable, making causal interpretation problematic.

3. Conceptual Framework

The estimates presented below can be interpreted as a simple reduced-form approach for the determinants of a vector of an individual's education-related outcomes that is consistent with investments in 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 in the community available to the individual as s/he ages. Each education-related indicator for individual i, at age a (E_{ia}), is posited to depend on whether the individual was exposed to the *Atole* nutritional intervention when s/he was six to 24 months of age ($N^A{}_i$), a cohort or control effect indicating whether the individual was six to 24 months during the intervention period regardless of the type of exposure received (N_i), fixed community characteristics ($C^f{}_i$), varying community characteristics related to schooling and labor markets at different ages of the ith individual ($C^v{}_{ia}$), whether the ith individual is a male (M_i), a vector of fixed family background characteristics for the ith individual (F_i) and a disturbance term that affects the education-related indicator of interest (u_{ia}):

(1) $E_{ia} = f(N^A{}_i, N_i, C^f{}_i, C^v{}_{ia}, M_i, F_i, u_{ia})$.

It is useful to make a few explicit comments about some aspects of this specification. First, the fixed community characteristics ($C^f{}_i$) control for factors such as persistent cultural differences or differences in economic alternatives that would result in different educational investments across communities in the absence of the nutritional interventions. Second, the varying community characteristics related to schooling and labor markets are linked to the ages of the ith individual ($C^v{}_{ia}$), such as the teacher/student ratio in the local schools when the individual was seven years old. We include the same set of such characteristics for all of the education-related dependent variables that we consider.[5] Third, 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 5 leads us generally to not make the assumptions necessary for such a linear relation, but to use nonlinear specifications. Fourth, the disturbance term includes all unobserved 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 cause biases in the estimated coefficient estimates of family background variables.[6] However, while such unobserved fixed individual or family heterogeneities might bias estimates of the impact of the family background variables, they do not cause biases in the community level variables – including importantly those related to the nutritional interventions of central interest – because the disturbance term is purged of all fixed community characteristics.

We present estimates of three variants of relation (1) for (most of) the education-related indicators that we consider. In the first variant we include only the community level variables (N_i, N^A_i, C^f_i, C^v_{ia},). In the second we add the family background variables (F_i,). In the third variant we further add explicit interactions between the nutrition intervention variables (N_i, N^A_i) and the parental education and SES components of the vector of family background variables (F_i,) to investigate whether there are lesser effects for better-off families due to diminishing marginal effects or greater effects because such families are better able to exploit the opportunities provided by the nutritional intervention. These interactions are also of interest because of emphasis on such possibilities in previous related analyses (Pollitt et al., 1993) and for better understanding the possible distributional consequences of the intervention. Furthermore, while unobserved individual and family heterogeneity may taint the estimated coefficients of the family background variables on their own, this is not a concern for the interactions because of the random nature of the intervention.

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[5] In a few cases this may raise the question of whether time-varying changes are anticipated. For example, schooling attainment as of age 13 could be affected by future employment opportunities at age 15 only to the extent that these are anticipated in advance.

[6] For example, Behrman and Rosenzweig (2002) 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 (they suggest because more-schooled women, holding ability and motivation constant, spend more time in the labor force and less time raising their children) in the sample of adult twins that they use that permits control for unobserved intergenerationally correlated endowments and the effects of assortative mating.

4. The Nutritional Intervention and Context[7]

The setting for the initial nutritional intervention is four villages in Eastern Guatemala: Santo Domingo, San Juan, Conacaste and Espiritu Santo. All four villages are located relatively close to the Atlantic Highway, connecting Guatemala City to Guatemala's Caribbean coast. Santo Domingo is closest to Guatemala City, only 36km away; Espiritu Santo is furthest away at 102km. Three villages – San Juan, Conacaste and Santo Domingo – are located in mountainous areas with shallow soils, while the agricultural potential of Espiritu Santo, located in a river valley, is somewhat higher. From January 1969 to September 1977 the *Instituto de Nutricion de Centro America y Panana* (INCAP) implemented a nutritional supplementation trial in these four villages together with a study of child growth and development. The subjects of the study were all village children aged seven years or less and all pregnant and lactating women. Cohorts of newborns were included for study until September 1977. Data collection for individual children ceased when they reached seven years of age. The birth years of the subjects included in the 1969-77 longitudinal study thus range from 1962 to 1977 and so when funding for the intervention was ended, the ages of the subjects ranged from 0 to 15 years. The length and timing of exposure to the nutritional interventions (described below), thus, depended on birth year. Only children born after mid- 1968 and before October 1975 were exposed to the nutrition intervention for all or most of the time they were from six to 24 months of age.

The principal hypothesis underlying the intervention was that improved pre-school nutrition would accelerate physical growth and mental development. To test this hypothesis, the initial study screened 300 villages to identify those of appropriate size, compactness (so as to facilitate access to feeding stations, see below), ethnicity, diet, education levels, demographic characteristics, nutritional status and degree of physical isolation. From this, it was possible to derive village pairs comparable in these characteristics: Conacaste and Santo Domingo; and San Juan and Espiritu Santo.

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

In designing the study, there was considerable concern that the social stimulation associated with attending feeding centers – such as the observation of children's nutritional status, the monitoring of their intakes of *Atole* and so on – might also affect child nutritional outcomes, thus confounding efforts to understand the impact of the supplement. To address this concern, in the remaining villages, Santo Domingo and

[7] Additional information on the design of the intervention is found in Martorell, Habicht and Rivera (1995) and

Espiritu Santo, an alternative drink, *Fresco*, was provided. *Fresco* was a cool, clear colored fruit flavored drink. It contained no protein and only sufficient sugar and flavoring agents for palatability. It contained fewer calories per cup (59 kcal/180 ml) than *Atole* but similar concentrations of micronutrients.

All residents of all villages received curative medical care free of charge throughout the duration of the study. Preventative health services, such as immunization and deworming campaigns, were conducted simultaneously in all villages. In all villages, drinks were distributed in food supplementation centers and were available daily, on a voluntary basis, to all members of the community during times that were convenient to mothers and children but did not interfere with usual meal times. For interpreting the results we present, a critical question that arises is to what extent the intervention design resulted in differences in access to calories and nutrients. In addressing this question, we can exploit the intensive nature of the survey and observational work associated with the intervention. Assessments of diet in the home indicate that energy intakes were virtually identical amongst children aged six to 24 months in *Atole* and *Fresco* villages. Children in the *Atole* villages consumed an additional 48 kcal (children six to nine months) to 104 kcal (children 21-24 months) per day as supplement. Children in the *Fresco* villages consumed virtually no *Fresco*, an additional 3 kcal (children six to nine months) to 15 kcal (children 21-24 months) per day as supplement.[8]

This cohort has been studied periodically in the years since the original study ended (some examples of these studies were summarized in section 2). A multidisciplinary team of investigators, a subset of which are the authors of this paper, are undertaking a follow-up study in which data again are being collected on all offspring participants of the 1969-77 study (i.e., subjects born between 1962 and 1977). The age of the subjects in 2002-3 ranged from 25 to 40 years of age. As of the time of the writing of this paper, data are available only from respondents in the original communities since data still are being collected from migrants to other areas in Guatemala. This should be borne in mind when considering these results.

5. Data

Though not without its potential weaknesses, such as attrition in the sample (particularly for this preliminary analysis), the unique strengths of this study is the long-term, longitudinal nature of the information available on subjects, starting at birth for half of them, combined with the randomized nutrition intervention. We use household and individual level data collected both in the 1969–77 study and in the 2002–3 follow-up, augmented by community level information collected during the earlier studies and retrospectively in 2002. In general, pre-2002 information is used for the independent variables while the outcomes we examine were measured in 2002–3. This approach has the advantage that it does not include any simultaneously (to the outcome measures) determined factors as explanatory variables. Above we described why we also think our

[8] By contrast, their mothers consumed considerable quantities.

approach generally avoids other endogeneity biases due to unobserved heterogeneity in the population, with the possible exception of the level variables for family background.

5.1 Right hand side variables

To make concrete our basic approach, consider the case of completed grades of education. We assess the role of individual, household, and community level factors, including the nutrition intervention, by examining how those factors conditioned decisions about schooling at critical points in an individual's life. A strategy we use throughout is to construct variables at the household and community level that relate as closely as possible to the timing of key education related decisions in a child's development. For example, using information reported in earlier work about schooling infrastructure and services in the villages (Pivaral, 1972; Bergeron, 1992), complemented with a retrospective study in 2002 (Estudio 1360, 2002), we construct variables such as an indicator of the availability of a permanent structure primary school when the child was seven years old. Thus, while reflecting community level characteristics, this variable varies 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 our measure more closely relates the availability of the school to the period in the child's life when the critical decisions about schooling were being made.

Other factors related to schooling opportunities that we construct in a similar fashion include an indicator of whether the primary school was severely damaged (and closed) during the 1976 earthquake (this occurred towards the end of the intervention period in the two fresco villages), the student-teacher ratio, and an indicator of whether there was electricity available in the community – all calculated when the individual was seven years old.

Decisions regarding schooling are not conditioned by school characteristics alone, however, but also depend crucially on the resources available to families and the opportunity costs of schooling. The next set of community level factors we include measure various aspects of these factors – whether a major local employer (*Cementos Progreso)* a cement factory that remains an important employer to this day was operating and available to members of the village when the child was seven, 10, and 15 years old. It was rare for individuals under 15 to work in the factory so our interpretation for this variable at ages younger than 15 is that it represents a potential resource for income generation for parents of subjects. At age 15, however, it more likely captures the opportunity cost of continued schooling. These distinctions are illustrative but should not be treated as definitive in "predicting" the signs of effects from these measures; all the measures pick up both types of influences to some extent. For example, if a father goes off to work in the cement factory this may also raise the opportunity cost of child schooling if there are agricultural activities in the household that the father can no longer attend to. A similar measure of labor market opportunities is an indicator of the crash in prices for yuquilla starting in 1984, a product grown extensively in San Juan but not in the other three villages.

The next set of community level factors we consider relate to the nutrition interventions underlying the study. We construct two measures, 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 (or intent to treat).[9] For each child, we calculate the number of days when s/he was between six and 24 months of age and the program was under way.[10] In practice, this variable turns out to have mass points at 0 and the full 18 months, with only minor variation between those two end points. Therefore we use a dummy variable that takes a value of 1 when the program was in operation for nearly the entire period (greater than or equal to 500 days) when the child was six to 24 months of age and 0 otherwise. This measure is calculated for all individuals and basically represents a cohort effect that captures any general changes that affected all children in the age range of particular focus, including the social simulation due to either the *Atole* or the *Fresco* that was available in the community in which they lived.[11] 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 six–24 months, represents the differential effect in the two *Atole* villages in comparison with children in the same cohort in the other two villages.

Turning to household level factors, we include an array of parental and household characteristics including mother and father's education, the logarithm of mother and father's age when the individual was seven years old, and a constructed socioeconomic status score based on information from 1975. This variable is an equally weighted combination of maternal education, paternal occupation, and an asset index (transformed into a mean 0 standard deviation 1 variable), and is described in detail in Pollitt et al. (1993). While inclusion of this variable somewhat complicates interpretation of the maternal education measures that we also directly include, it allows some comparison with previous work. Lastly, we include a partial set of interactions between the two exposure variables and each of what we believe to be the most important household level factors – mother's education, father's education, and the 1975 SES score.

5.2 Dependent variables

Of the 913 in our current sample, 93 percent had ever attended school. This overall average, however, masks some important differences across communities. For respondents from one village in particular, Conacaste, the percentage is much lower (86

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[9] INCAP collected extensive information on participation in the program during the 1969-77 period including exact measures of intake by each child under seven. We choose not to use this somewhat more detailed information, however, in order to avoid introducing choice based factors into the model that may be correlated with other factors that we do not include or observe and therefore cause estimation biases.
[10] We also explored the sensitivity of our estimates to different age ranges. For example, we extended the exposure period to 36 months (using a cut-off of 900 days in the during the six–36 month period), in which case similar patterns emerge to those reported below, although the effects of the intervention are not as precisely estimated
[11] We do not have sufficient information to permit us to identify the possible effect of the social stimulation separate from a more general cohort effects.

percent) whereas the others all indicate around 95 percent. The next education outcome we consider is an indicator of having passed (at least) the first grade – a full 85 percent of the sample has done so. Mimicking the previous pattern, the probability of having passed any grade is much lower for those in Conacaste (73 percent).

The next set of outcomes we consider are number of grades passed, first the number of formal grades by age 13 and then the number of all grades, including informal schooling, by the time of the survey in 2002. These are presented in Figures 1 and 2, respectively. The number of formal grades passed by age 13 is in part a measure of how efficiently or quickly an individual was progressing through the schooling system. Moving from Figure 1 to Figure 2 we see how many of those individuals who were in the in the third, fourth and fifth grades managed to continue on after age 13, achieving complete primary and beyond. Not surprisingly, the educational differences observed above across villages persist in these measures as well. Espiritu Santo has the highest average grades completed (5.4) compared with Conacaste with the lowest (3.4). In all villages and across the sample, boys have completed one grade more than girls, on average.

Turning to our measures of achievement and intelligence we first display the results from the Inter-American Reading and Comprehension Tests. The educational battery administered to this population in 1988 included tests of general knowledge related to daily life experiences, numeracy (numbers and simple calculations), a preliteracy test (letters, words, and phrases), a reading test of an article from a local newspaper, and two educational achievement tests (on comprehension and vocabulary), which were part of the Interamerican Series originally designed to assess reading abilities of Spanish-speaking children in Texas (Manuel 1967). In 2002-3, respondents who reported having achieved 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 were given a preliteracy test. Subjects who reported more than six years of schooling were presumed to be literate. The reading test and the two educational achievement tests on levels of comprehension and vocabulary were administered to individuals who were considered to be literate or who had passed the preliteracy test with fewer than five errors.

All interviewers were standardized in the testing procedures. The protocol for this test has a series of filters that lead to those who have only basic literacy or lower not taking the test. In accordance with standard practice we score those who did not undertake the test as 0 (leading to a sample of 895 observations, slightly smaller than 913 because of refusals). The implication of this for the distribution is shown in Figure 3 where we see a very large mass point of density at 0.

As described at the outset, for the previous three measures of educational outcomes – based on the empirical distributions just presented – it would be unwise to estimate OLS. Instead, we estimate ordered probit models to more flexibly handle these distributions. Another advantage of the ordered probit, particularly relevant when estimating grade completion, is that while there is an ordinal ranking to the grades there is no cardinal ranking as would be implicit with OLS. That is to say, the step between grades 0 and 1 is not necessarily the same "magnitude" of the step between grades five and six, for example. While having these advantages, our estimation strategy is not without some disadvantages. The principal one is that it is less straightforward to interpret the reported coefficients without further manipulation. In particular, when there is a positive, significant coefficient we can unequivocally say that an increase in that factor reduces the probability of the lowest category considered, for example no completed grades, and increases the probability of the highest category considered, third year of high school (because it shifts the entire distribution of outcomes to the right). It is not possible, however, to see directly from the coefficients (without further manipulation) whether increasing any factor increases or decreases the probability of being in the second grade, say, because it both shifts individuals into the second grade who otherwise would have been in the first grade and shifts individuals into the third grade who otherwise would have been in the second grade. Nevertheless it is clear in such a case that the whole distribution is shifted to the right.

The final measure we consider is the score on Ravens Progressive Matrices. This is a widely-used non-verbal measure of intelligence that consists of a set of shapes and patterns, with the respondent asked to supply a 'missing piece'. These were completed for 874 respondents and shown in Figure 4 with a normal curve of equal mean and variance to the empirical distribution superimposed on the figure. Consistent with common priors on the distribution of ability in the population, these test scores appear to be close to normally distributed in the population.[12]

Before turning to our analysis of the determinants of these educational outcomes and the role played by the nutrition intervention, we first step back and consider the select nature of our sample. Table 1 presents means of the conditioning variables we use in the analyses to follow, for those in our sample (913) and not in our sample (1480). It should be pointed out that this analysis combines refusals, migrants, and those who are no longer alive in the not-in-sample category. As such, it is simply our attempt to characterize how those observations we use in the estimations presented below differ from the original sample – as of September 2003, we expect that about half of the remaining 1480 will be re-interviewed in the 2002–3 follow-up. As shown by the stars after the second column, there are a number of significant differences between the groups, though in many cases the absolute magnitude of the difference is not very large. Women and (slightly) younger respondents are more likely to be in the sample, possibly due to the greater propensity of males and older individuals to migrate for work. There are also some differences in the ages of parents when the child was age seven – those not in the sample tended to have younger parents. Sample respondents appear to have fewer resources (0.13 standard deviations) as measured by the 1975 SES score but their fathers were better educated. Slightly fewer of those in the sample were exposed to *Atole* during the six – 24 month period.

If we use these factors to predict whether an observation is in the sample, the child age and gender factors, as well as the SES scores appear to significantly predict whether we

[12] Even though, as noted in section 6, they appear to be affected by variables included in relation (1), including the nutrition intervention and therefore do not seem to reflect only innate abilities.

observe an individual – though the cohort control and exposure variables do not. The employment opportunity factors related to the cement factory also "predict" to some extent whether an individual is observed in our sample, although we do not distinguish whether this effect differs for males or females. At this point, we make no attempt to interpret these results – recall we are working with a partial sample – but point out that indeed the sample we are working with appears to be slightly different than the overall sample at the outset of the study but not with respect to the exposure variables. To the extent that what we observe in these simple regressions is the entire difference between the groups we in part control for it by conditioning on those factors.

6. Estimates

Probability of having ever attended school: In Table 2, we begin to explore the relationship between formal schooling and household and community level factors, including the nutritional intervention, presenting probit estimations of the probability of having ever attended school (Column 1) and having passed (at least) the first grade (Column 2). The coefficients presented are the derivatives dP/dX evaluated at the sample mean and z-statistics are calculated for those derivatives using robust standard errors and allowing for clustering at the village level (StataCorp 2001). For dummy variables, dP/dX represents the discrete change in probability when we change the dummy variable from 0 to 1.

Despite the fact that the vast majority of the sample attended school (93 percent), we are still able to identify some factors associated with the school entry decision. Conditional on the other factors, children living in San Juan and Conacaste were less likely than their counterparts in Santo Domingo to have attended school; as emphasized earlier, those in Espiritu Santo perform relatively well on schooling outcomes and were more likely to have attended. Few of the community level school and labor market factors conditioned the decision to attend, though household level measures do. Individuals with younger mothers (at the time the individual was seven), more educated mothers, older fathers and from families were higher SES scores were more likely to have attended school. Larger age-gaps between parents (mothers are almost always younger) appear to raise the probability of having ever attended school, possibly because, for given mother's age, an older father is at a stage in his life cycle where he would have earned more income or accumulated more wealth.

Turning to the nutrition intervention variable, exposure to *Atole* increased the probability by a significant 5.6 percentage points greater than for the members of the same cohort without exposure to *Atole*. This is a substantial effect.

Probability of having passed at least first grade: Examining an indicator of whether an individual has (at least) passed the first grade (86 percent have) slightly different patterns emerge, particularly with respect to the community level schooling measures. As with ever attended, children living in San Juan and Conacaste were less likely to have passed first grade than their counterparts in Santo Domingo while those in Espiritu Santo were more likely to have passed. Unlike in the ever attended specification, community level factors do appear to influence this outcome. An indicator of whether the school was a permanent structure when the individual was seven years old is negatively related to passing.[13] In 1976, a devastating earthquake hit Guatemala, closing the schools in San Juan and Espiritu Santo– if this occurred when a child was seven s/he was nearly 7 percentage points less likely to have passed first grade. The other significant community level factors (presence of electricity, cement factor opportunity available at ages seven and 15) conform to our expectations about how they condition child schooling – cement factory availability at age seven predominantly reflects parental opportunities and has a positive effect where as at age 15 the effect is negative as it likely reflects more the opportunity costs of the child. Unlike in the ever attended specification, age of parents is no longer a significant predictor but parental education and 1975 SES scores all improve the probability of having passed the first grade.

With respect to the intervention, we find that the *Atole* effect is significant and substantially greater (by 7.2 percentage points) than found for the same cohort.

Formal school grades completed by age 13: Table 3 presents three specifications for this dependent variable – one with community level variables only, a second adding household level factors, and a third including interactions between *Atole* exposure and household level factors. In the second column (which includes community and household level factors) we see many of the patterns described above now more precisely estimated. Espiritu Santo continues to have a relative advantage in schooling outcomes. The labor market factors all work in the direction consistent with our dominant hypotheses. And finally, parental resources continue to play the role seen previously. Consistent with earlier analyses using the 1988 INCAP follow-up, *Atole* exposure does not appear to play a significant role (Pollit, et al. 1993).

In the third column, we include a set of interactions between our household measures of resources and *Atole* exposure to explore to what extent the effects of the intervention varied based on these pre-existing conditions.[14] When we do this, the estimated roles of the other conditioning variables change little and the role of the *Atole* exposure variable remains weak with the exception of their interaction with SES scores. Higher SES households appeared to benefit significantly and positively from exposure to *Atole*

[13] One possibility is the following. Children in Conacaste had the poorest access to a permanent structure for their primary school; it was only available for children born after 1973. However, in 1980 and 1981, a cooperative was set up to grow vegetables, particularly tomatoes, in Conacaste. In their anthropological overview of these study villages, Estudio 1360 (2002, p. 52) report that "During harvesting and planting times, there was a shortage of workers ... many women were hired, as well as children." Note that children born after 1973 would be of just the right age to help with these activities (7 and 8 onwards) and perhaps this caused them to spend more time working in agriculture and less time in school. So one possibility is that what this variable is really capturing is not so much an "improvement in school quality effect" but rather a shock that could plausibly adversely affect educational outcomes.

[14] Note that this contrasts with the inclusion of children's schooling or completion of grade six, variables that would seem to be determined endogenously with other aspects of child education, on the right side as in Pollitt et al. (1993) and Li et al. (2003a,b).

Total completed grades of school by 2002-3: In Table 4, we present the same set of specifications using total completed grades as the outcome measure (including formal and informal schooling). On the whole, the majority of the results are quite similar to those for grade progress by age 13, with the exception of the *Atole* exposure variables, which now play a stronger role. In the third column with the complete set of interactions, we see that exposure to *Atole* has positive and significant effects relative to the control. The increasing effects by SES score described above are also evident and, in addition, an opposite pattern emerges for mother's education.

Educational achievement test performance in adulthood (literacy, vocabulary comprehension): These estimates are important because they are the first to our knowledge to investigate the impact of an experimental nutritional intervention on adult intellectual functioning a quarter of a century or more later. The estimates in Table 5 suggest significantly greater test performance in Espiritu Santo than in the other three village (consistent with its higher schooling levels), with significantly less performance in Santo Domingo than elsewhere (though less than in Concaste only at the 10 percent level). Almost all of the non-nutrition community variables have significant coefficient estimates, suggesting persistent effects of these shocks decades later. The family background variables also all have significant level effects, again negative for the age of the mother when the child was seven and positive for the father's age, mother's and father's schooling and SES.

The central coefficient estimates of interest pertain to the impact of the *Atole* exposure and indicate significant and positive effects relative to the same cohort that was not exposed to *Atole*. The interaction terms indicate more positive effects as SES and father's schooling increase, but again more negative effects as mother's schooling increases. Perhaps the latter pattern reflects that more-schooled mothers spend more time in economic production and less time in child care (and thus were less likely to exploit the opportunities afforded by the *Atole* intervention) along the lines suggested by Behrman and Rosenzweig (2002).

Raven's test performance in adulthood: These estimates suggest less significant effects of the included variables than are found for the adult achievement test performance. However they still suggest some significant effects. Interestingly, despite the general result from the previous tables that community fixed effects for Espiritu Santo tend to be significantly more positive than for the other three communities, in this case the coefficient estimate for this community is not significantly different from that for Santo Domingo and significantly below those for San Juan and Concaste. Among the non-nutrition intervention shocks, there is a significantly negative long-run response to the student-teacher level at age seven, and a significantly positive effect of the cement factory at age ten (probably reflecting an income effect related to parents' employment). Most of the family background variables have positive significant effects (except for age of mother), though these may be biased upward if Raven's test score in part reflects intergenerationally-correlated "ability" endowments.

The *Atole* intervention has significantly positive effects relative to members of the same cohort who were not exposed to this intervention – though again with a negative coefficient estimate for the interaction with mother's schooling. Note that this result, as well as some of those in the previous paragraph, raise questions about the oft-given interpretation of Raven's scores as reflecting innate abilities that are not altered by household and community allocations of resources.

7. Conclusions

This paper considers the impact of community-level experimental nutritional intervention in rural Guatemala on several different measures related to education over the life cycle. For each of these measures we estimate the impacts of an experimental nutritional intervention during the critical period when individuals were six months (roughly when complementary feeding was introduced) through 24 months of age. We advance beyond the previous literature by using longitudinal data from a nutritional experiment, using longitudinal data on educational measures not only for school-age years but up through prime adult years, avoiding confounding the estimates by not including right-side variables that probably are endogenous, and using estimators that allow for non-normal distributions (which make a considerable difference in the estimated magnitude and precision of the estimates). Our substantive results indicate significantly positive and fairly substantial effects of the *Atole* nutritional intervention on all of the outcomes that we consider related to education: increasing the probability of attending school and of passing the first grade, increasing the grade attained by age 13 (through a combination of increasing the probability of ever enrolling, reducing the age of enrolling, increasing the grade completion rate per year in schooling, and reducing the dropout rate), increasing completed schooling attainment, increasing adult Raven's test scores and increasing adult cognitive achievement scores. Thus there are important education-related effects that appear to persist well into adulthood. In the estimates in which we include interactions with family background characteristics, we find a mixture of patterns – in some cases there is the suggestion that at a gross level children coming from better family backgrounds benefited more from the nutrition interventions (e.g., the interaction with socioeconomic status measured in 1975) and in other cases the reverse (e.g., the interaction with mother's education) – perhaps with the price of time effect outweighing the income effect in the latter case. We end, however, by once again cautioning that these results are genuinely preliminary and may change when we incorporate data on migrants that are currently being collected.

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Table 1 – Sample Attrition: Probit estimates for selection into sample, accompanied by means & sd

	in sample mean (sd)	not in sample mean (sd)	in sample probit
(1) if San Juan	0.17415	0.24392 ***	-.1938529 ***
	(0.379)	(0.430)	(4.4)
(1) if Conacaste	0.32421	0.30676	-.1589356 ***
	(0.468)	(0.461)	(2.9)
(1) if Espiritu Santo	0.22344	0.18041 **	-.1313765
	(0.417)	(0.385)	(1.6)
Age in years	31.36254	31.72230 **	-.0184261 ***
	(4.331)	(4.110)	(2.9)
(1) if male	0.47426	0.53919 ***	-.0575579 ***
	(0.500)	(0.499)	(4.4)
(1) cohort control 6–24 months of age	0.66922	0.67838	-.0171525
	(0.471)	(0.467)	(1.0)
(1) if exposed to atole at 6–24 months	0.33625	0.37297 **	-.0305459
	(0.473)	(0.484)	(0.7)
(1) if permanent primary school at age 7	0.64294	0.68378 **	-.0435427
	(0.479)	(0.465)	(0.9)
(1) if primary school destroyed at age 7	0.03724	0.03851	.0229146
	(0.189)	(0.192)	(0.8)
Student – teacher ratio at age 7	39.46988	40.17230	-.0007107
	(8.872)	(8.760)	(0.6)
(1) if electricity in community at age 7	0.34173	0.36622	-.1185042
	(0.475)	(0.482)	(1.2)
(1) if cement factory available at age 7	0.26397	0.28716	-.1424087 ***
	(0.441)	(0.453)	(3.5)
(1) if cement factory available at age 10	0.39102	0.39459	-.0755635
	(0.488)	(0.489)	(1.2)
(1) if cement factory available at age 15	0.51588	0.50878	-.074723 *
	(0.500)	(0.500)	(1.8)
(1) if yuquilla market crash at age 15	0.15882	0.22635 ***	-.0445047
	(0.366)	(0.419)	(0.6)
Ln age mother when child age 7	3.50971	3.36849 ***	.0873476
	(0.388)	(0.711)	(1.1)
Ln age father when child age 7	3.50767	3.17292 ***	-.0282405
	(0.797)	(1.259)	(0.7)
Mother's years of schooling	0.94414	0.88311	-.0116872
	(1.397)	(1.432)	(1.3)
Father's years of schooling	1.26725	1.05270 ***	-.0105606
	(1.839)	(1.724)	(1.2)
Household SES score in 1975	-0.10277	0.02800 ***	-.020055 *
	(0.889)	(0.829)	(1.8)
Dummy controls for missing parental and SES data			yes
Pseudo R^2			0.12
Number of observations	913	1480	2393

The column after the means and standard deviations of the not-in-sample group shows the results from a t-test of the equality of the means across the two groups, allowing for unequal variances (StataCorp 2001). Test-statistics based on standard errors robust standard errors calculated allowing for clustering at the village level (StataCorp 2001). * indicates significance at 10%, ** indicates significance at 5%, and *** indicates significance at 1%.

Table 2 — Probit estimation of (1) Ever attended school and (2) Passed first grade

	(1) Ever Attended	(2) Passed 1st Grade
(1) if San Juan	–0.08034 ***	–0.20126 ***
	(6.5)	(5.2)
(1) if Conacaste	–0.06438 ***	–0.16441 ***
	(10.6)	(4.0)
(1) if Espiritu Santo	0.02549 **	0.05897 *
	(2.2)	(1.9)
Age in years	0.00054	0.00209
	(0.4)	(0.6)
(1) if male	0.01973	0.02104
	(1.0)	(0.8)
(1) cohort control 6–24 months of age	–0.02679 **	–0.02439
	(2.1)	(0.7)
(1) if exposed to atole at 6–24 months	0.05614 ***	0.07197 **
	(2.6)	(2.1)
(1) if permanent primary school at age 7	–0.01288	–0.05652 ***
	(1.3)	(5.3)
(1) if primary school destroyed at age 7	0.02171	–0.06659 ***
	(0.8)	(9.5)
Student – teacher ratio at age 7	–0.00018	–0.00105
	(0.3)	(1.0)
(1) if electricity in community at age 7	0.03209	0.03461 *
	(1.2)	(1.7)
(1) if cement factory available at age 7	0.03470	0.07549 *
	(0.9)	(1.7)
(1) if cement factory available at age 10	0.00967	0.03404
	(0.5)	(1.5)
(1) if cement factory available at age 15	–0.03589 ***	–0.05554 **
	(3.0)	(2.8)
(1) if yuquilla market crash at age 15	0.00900	0.10351 ***
	(0.4)	(19.3)
Ln age mother when child age 7	–0.11845 **	–0.15565
	(2.1)	(1.5)
Ln age father when child age 7	0.10466 **	0.08857
	(2.5)	(1.2)
Mother's years of schooling	0.02167 ***	0.01938 ***
	(10.1)	(2.9)
Father's years of schooling	0.00277	0.01220 **
	(1.2)	(2.1)
Household SES score in 1975	0.02729 ***	0.04729 **
	(11.1)	(2.5)
Dummy controls for missing parental and SES data	no	no
Pseudo R^2	0.15	0.13
Number of observations	913	913

* indicates significance at 10%, ** indicates significance at 5%, and *** indicates significance at 1%. See notes to Table 1.

Table 3 — Ordered probit, dependent variable is formal grades of school attained by age 13

(1) if San Juan	-0.45835	***	-0.24639		-0.26238	*
	(3.9)		(1.5)		(1.6)	
(1) if Conacaste	-0.48252	***	-0.33839	*	-0.35565	**
	(3.7)		(1.8)		(2.1)	
(1) if Espiritu Santo	0.70640	***	1.0581	***	1.08329	***
	(3.5)		(3.1)		(3.6)	
Age in years	-0.00257		0.00110		0.00074	
	(0.3)		(0.1)		(0.0)	
(1) if male	0.24441	***	0.21870	**	0.20758	**
	(2.6)		(2.1)		(2.1)	
(1) cohort control 6–24 months of age	0.03520		0.05921		-0.02558	
	(0.3)		(0.4)		(0.2)	
(1) if exposed to atole at 6–24 months	-0.00179		0.01346		0.15651	
	(0.0)		(0.1)		(1.3)	
(1) if permanent primary school at age 7	-0.42487	***	-0.54546	***	-0.5608	***
	(6.9)		(4.3)		(4.4)	
(1) if primary school destroyed at age 7	-0.47670	***	-0.35627	***	-0.42311	***
	(8.3)		(3.6)		(3.5)	
Student – teacher ratio at age 7	-0.00388		-0.00353		-0.00450	*
	(1.2)		(0.9)		(1.3)	
(1) if electricity in community at age 7	0.25279	***	0.28802		0.20560	
	(2.7)		(1.6)		(1.0)	
(1) if cement factory available at age 7	0.41025	**	0.57300	***	0.63438	***
	(2.3)		(2.6)		(3.0)	
(1) if cement factory available at age 10	0.42407	***	0.42635	***	0.40927	***
	(8.8)		(5.1)		(5.4)	
(1) if cement factory available at age 15	0.02833		0.03800		0.02410	
	(0.5)		(0.5)		(0.4)	
(1) if yuquilla market crash at age 15	0.78098	***	0.71579	***	0.78735	***
	(5.5)		(2.9)		(3.6)	
Ln age mother when child age 7			-0.50676	**	-0.46330	**
			(2.8)		(2.5)	
Ln age father when child age 7			0.48009	**	0.43377	*
			(2.2)		(1.9)	
Mother's years of schooling			0.13013	***	0.14174	***
			(6.4)		(3.9)	
Father's years of schooling			0.08496	***	0.08304	**
			(4.3)		(2.5)	
Household SES score in 1975			0.29541	***	0.36387	***
			(5.8)		(9.5)	
Cohort control 6–24 months × Mother's schooling					0.01781	
					(0.3)	
Exposure to atole 6–24 months × Mother's schooling					-0.0555	
					(1.1)	
Cohort control 6–24 months × Father's schooling					0.01807	
					(0.5)	
Exposure to atole 6–24 months × Father's schooling					-0.01742	
					(0.7)	
Cohort control 6–24 months × 1975 SES					-0.28664	***
					(5.3)	
Exposure to atole 6–24 months × 1975 SES					0.24378	***
					(4.5)	
Dummy controls for missing parental and SES data	no		yes		yes	
Joint Chi-square test exposure 6 – 24 months atole interacted with parental education and SES					111.5	***
					[<0.01]	
Pseudo R^2	0.04		0.07		0.07	
Number of observations	913		913		913	

* indicates significance at 10%, ** indicates significance at 5%, and *** indicates significance at 1%. See notes to Table 1.

Table 4 – Ordered Probit, dependent variable is highest grade of schooling achieved

	(1)		(2)		(3)	
(1) if San Juan	-0.45513	***	-0.20004		-0.23434	*
	(4.3)		(1.3)		(1.9)	
(1) if Conacaste	-0.28839	***	-0.10035		-0.13024	
	(3.3)		(0.6)		(1.2)	
(1) if Espiritu Santo	0.88852	***	1.33377	***	1.36558	***
	(6.7)		(4.3)		(6.4)	
Age in years	-0.01357	**	-0.00949		-0.01022	
	(2.0)		(0.6)		(0.6)	
(1) if male	0.35303	***	0.33670	***	0.32362	***
	(6.7)		(6.3)		(7.4)	
(1) cohort control 6–24 months of age	-0.09725		-0.07818		-0.21033	**
	(0.5)		(0.4)		(2.0)	
(1) if exposed to atole at 6–24 months	0.12115		0.15648		0.29490	***
	(0.6)		(0.7)		(4.6)	
(1) if permanent primary school at age 7	-0.45788	***	-0.62026	***	-0.64379	***
	(10.9)		(5.4)		(5.5)	
(1) if primary school destroyed at age 7	-0.59613	***	-0.49880	***	-0.62240	***
	(12.3)		(5.0)		(7.0)	
Student – teacher ratio at age 7	-0.00486		-0.00426		-0.00582	
	(1.1)		(0.7)		(1.1)	
(1) if electricity in community at age 7	0.28566	***	0.35653	***	0.23066	
	(2.8)		(2.6)		(1.6)	
(1) if cement factory available at age 7	0.45546	**	0.66980	***	0.76118	***
	(2.3)		(3.0)		(4.5)	
(1) if cement factory available at age 10	0.46078	***	0.48045	***	0.44938	***
	(6.1)		(6.1)		(5.5)	
(1) if cement factory available at age 15	-0.06390		-0.05985		-0.05921	
	(1.3)		(1.0)		(1.0)	
(1) if yuquilla market crash at age 15	0.80485	***	0.72084	***	0.84019	***
	(22.0)		(4.1)		(6.9)	
Ln age mother when child age 7			-0.74387	***	-0.66239	***
			(5.6)		(4.8)	
Ln age father when child age 7			0.55390	**	0.46620	**
			(2.5)		(2.0)	
Mother's years of schooling			0.14732	***	0.14675	***
			(7.0)		(5.0)	
Father's years of schooling			0.09760	***	0.08974	***
			(4.6)		(2.7)	
Household SES score in 1975			0.34423	***	0.40973	***
			(5.1)		(9.9)	
Cohort control 6–24 months × Mother's schooling					0.05086	***
					(4.1)	
Exposure to atole 6–24 months × Mother's schooling					-0.06339	*
					(1.7)	
Cohort control 6–24 months × Father's schooling					0.01309	
					(0.3)	
Exposure to atole 6–24 months × Father's schooling					0.02830	
					(1.2)	
Cohort control 6–24 months × 1975 SES					-0.44033	***
					(6.8)	
Exposure to atole 6–24 months × 1975 SES					0.51318	***
					(4.9)	
Dummy controls for missing parental and SES data	no		yes		yes	
Joint Chi-square test exposure 6 – 24 months atole interacted with parental education and SES					265.0	***
					[<0.01]	
Pseudo R^2	0.03		0.07		0.08	
Number of observations	913		913		913	

* indicates significance at 10%, ** indicates significance at 5%, and *** indicates significance at 1%. See notes to Table 1.

Table 5 – Ordered Probit, dependent variable is Interamerican Series Score (adult achievement)

(1) if San Juan	0.11748 **	0.43857 ***	0.42838 ***
	(2.3)	(3.9)	(3.8)
(1) if Conacaste	0.05724 *	0.27802 ***	0.27327 ***
	(1.8)	(3.1)	(3.3)
(1) if Espiritu Santo	0.42781 ***	0.77169 ***	0.80908 ***
	(3.4)	(3.1)	(4.1)
Age in years	0.00061	0.00620	0.00565
	(0.2)	(0.7)	(0.5)
(1) if male	0.17648 *	0.14648	0.13378
	(1.9)	(1.4)	(1.3)
(1) cohort control 6–24 months of age	-0.12470 **	-0.11601 *	-0.16746 ***
	(2.1)	(1.8)	(3.3)
(1) if exposed to atole at 6–24 months	0.15950	0.20440 **	0.28022 ***
	(1.6)	(2.0)	(5.5)
(1) if permanent primary school at age 7	-0.27255 **	-0.37926 ***	-0.39504 ***
	(2.4)	(4.1)	(6.6)
(1) if primary school destroyed at age 7	-0.57452 ***	-0.46670 ***	-0.55618 ***
	(5.5)	(4.3)	(5.6)
Student – teacher ratio at age 7	-0.00888 **	-0.00849	-0.00962 ***
	(2.0)	(1.7)	(2.2)
(1) if electricity in community at age 7	0.44212 ***	0.52755 ***	0.43373 ***
	(7.5)	(5.1)	(3.3)
(1) if cement factory available at age 7	0.41534 **	0.57903 ***	0.65153 ***
	(2.2)	(3.5)	(4.3)
(1) if cement factory available at age 10	0.40491 ***	0.44229 ***	0.42143 ***
	(4.6)	(7.8)	(6.5)
(1) if cement factory available at age 15	0.08224 *	0.09759	0.07531
	(1.8)	(1.2)	(1.2)
(1) if yuquilla market crash at age 15	0.24571 ***	0.08974	0.16956
	(4.5)	(0.8)	(1.6)
Ln age mother when child age 7		-0.61355 ***	-0.53572 ***
		(3.3)	(2.6)
Ln age father when child age 7		0.50638 ***	0.43995 ***
		(3.7)	(2.9)
Mother's years of schooling		0.11662 ***	0.12250 ***
		(4.0)	(5.6)
Father's years of schooling		0.09202 ***	0.09520 ***
		(3.7)	(2.6)
Household SES score in 1975		0.29228 ***	0.40496 ***
		(4.3)	(5.7)
Cohort control 6–24 months × Mother's schooling			0.051548
			(1.0)
Exposure to atole 6–24 months × Mother's schooling			-0.10725 **
			(2.0)
Cohort control 6–24 months × Father's schooling			-0.02469
			(0.6)
Exposure to atole 6–24 months × Father's schooling			0.07869 ***
			(2.6)
Cohort control 6–24 months × 1975 SES			-0.37034 ***
			(5.5)
Exposure to atole 6–24 months × 1975 SES			0.22886 **
			(2.4)
Dummy controls for missing parental and SES data	no	yes	yes
Joint Chi-square test exposure 6 – 24 months atole interacted with parental education and SES			275.0 ***
			[<0.01]
Pseudo R^2	0.01	0.04	0.04
Number of observations	895	895	895

* indicates significance at 10%, ** indicates significance at 5%, and *** indicates significance at 1%. See notes to Table 1.

Table 6 – OLS, dependent variable is adult Raven's Score

(1) if San Juan	1.06400 **	2.22495 ***	2.18163 ***
	(3.3)	(15.6)	(15.7)
(1) if Conacaste	2.66451 ***	3.34824 ***	3.34858 ***
	(5.6)	(17.8)	(22.7)
(1) if Espiritu Santo	-0.02889	0.85769	0.89884
	(0.0)	(0.7)	(0.7)
Age in years	0.01566	0.04011	0.04268
	(0.2)	(0.7)	(0.7)
(1) if male	3.36328 ***	3.16511 ***	3.18887 ***
	(21.0)	(20.7)	(19.6)
(1) cohort control 6–24 months of age	-0.54048	-0.57175	-0.85120
	(1.1)	(1.2)	(1.9)
(1) if exposed to atole at 6–24 months	1.20415 *	1.33034 *	1.48870
	(2.5)	(2.9)	(1.8)
(1) if permanent primary school at age 7	1.41909	0.97297	1.00409
	(1.7)	(1.5)	(1.5)
(1) if primary school destroyed at age 7	-0.90372	-0.48949	-0.44309
	(0.7)	(0.4)	(0.3)
Student – teacher ratio at age 7	-0.04899 **	-0.03986 ***	-0.04170 ***
	(5.0)	(7.3)	(8.5)
(1) if electricity in community at age 7	1.24366	1.64283 **	1.55535
	(1.6)	(2.8)	(2.1)
(1) if cement factory available at age 7	-1.02271	-0.27547	-0.39407
	(0.8)	(0.3)	(0.4)
(1) if cement factory available at age 10	2.28924 **	2.33353 **	2.36459 **
	(4.0)	(3.7)	(4.0)
(1) if cement factory available at age 15	-0.90739	-0.87649	-0.86322
	(1.7)	(2.0)	(1.8)
(1) if yuquilla market crash at age 15	-0.11997	-0.86026	-0.70679
	(0.1)	(0.8)	(0.6)
Ln age mother when child age 7		-2.39167	-2.28974
		(1.5)	(1.4)
Ln age father when child age 7		1.40745 *	1.38722 *
		(2.7)	(3.1)
Mother's years of schooling		0.59062 *	0.33696
		(2.6)	(1.6)
Father's years of schooling		0.37956 *	0.45966 *
		(2.3)	(2.4)
Household SES score in 1975		0.61913 *	0.73799
		(2.5)	(1.6)
Cohort control 6–24 months × Mother's schooling			0.75307 *
			(2.3)
Exposure to atole 6–24 months × Mother's schooling			-0.65666 *
			(2.8)
Cohort control 6–24 months × Father's schooling			-0.26610
			(1.3)
Exposure to atole 6–24 months × Father's schooling			0.30570
			(0.8)
Cohort control 6–24 months × 1975 SES			-0.26016
			(0.3)
Exposure to atole 6–24 months × 1975 SES			-0.08473
			(0.1)
Dummy controls for missing parental and SES data	no	yes	yes
Constant	14.68863 **	15.71968 *	15.90979 *
	(4.4)	(3.00)	(2.89)
Joint F test exposure 6 – 24 months atole interacted with parental education and SES			24.6 ***
			[0.01]
R^2	0.13	0.18	0.19
Number of observations	874	874	874

* indicates significance at 10%, ** indicates significance at 5%, and *** indicates significance at 1%. See notes to Table 1.

Figure 1 – Formal grades completed by age 13 (913 observations)

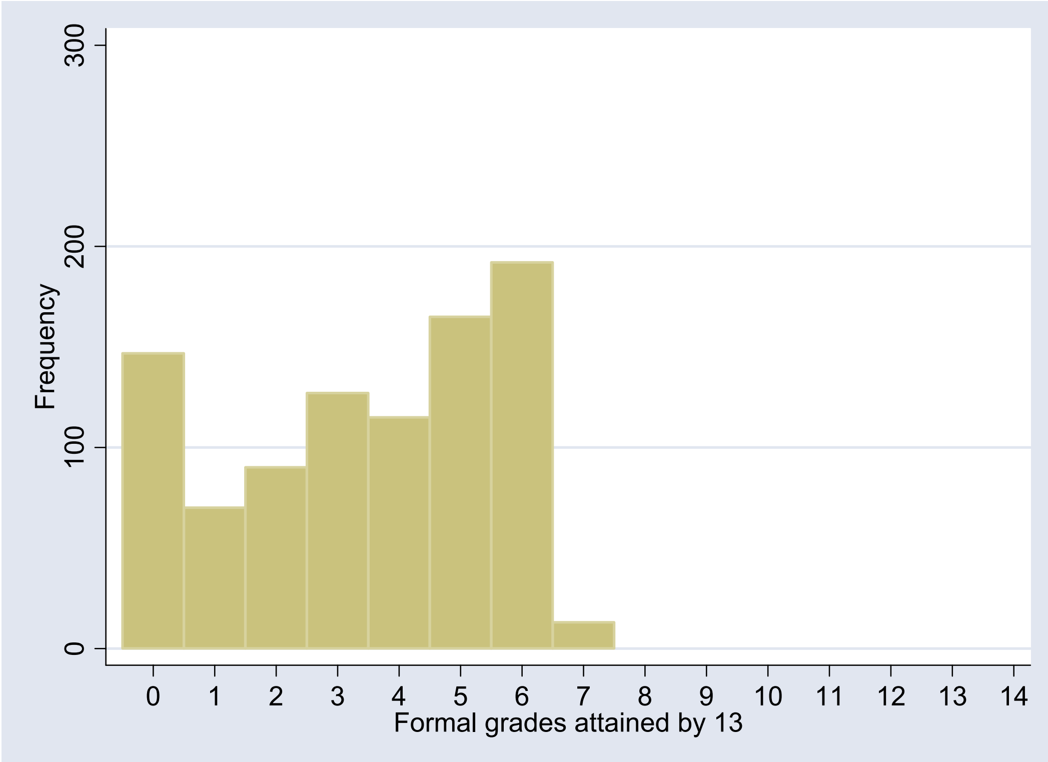

Figure 2 – All grades completed (913 observations)

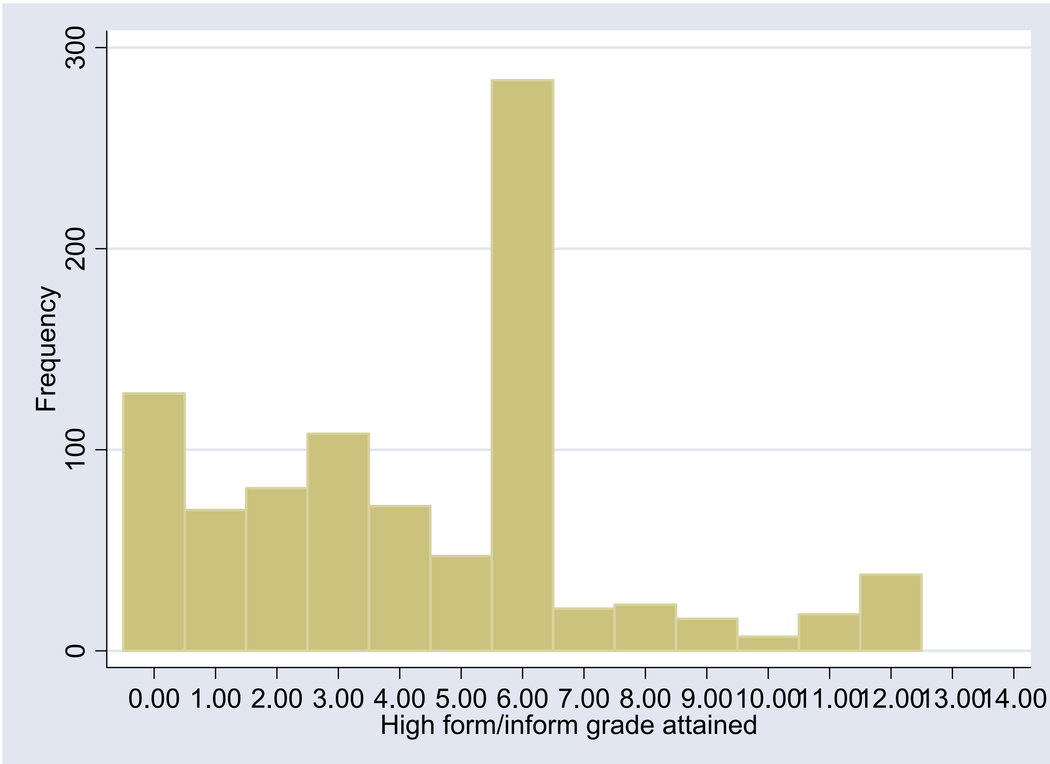

Figure 3 – SIA test score – sum of vocabulary and comprehension scores (895 observations)

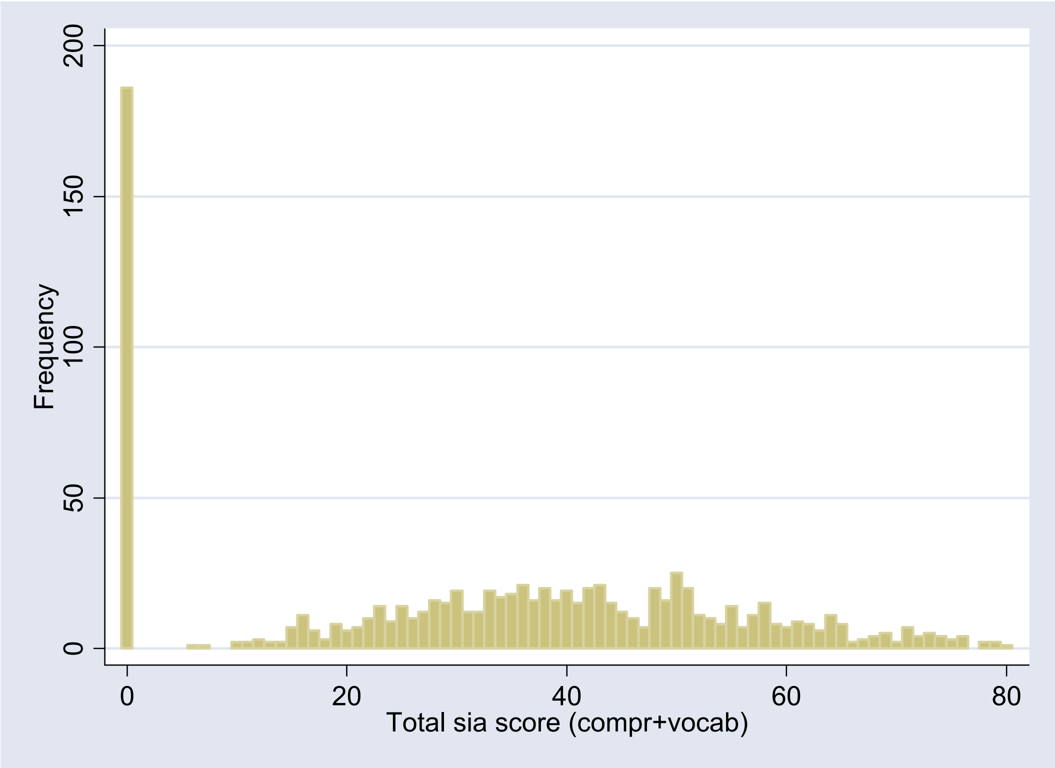

Figure 4 – Ravens test score (874 observations)