FOOD CONSUMPTION AND NUTRITION DIVISION June 2006

FCND Discussion Paper 207

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

John A. Maluccio, John Hoddinott, Jere R. Behrman, Reynaldo Martorell, Agnes R. Quisumbing, and Aryeh D. Stein

2033 K Street, NW, Washington, DC 20006-1002 USA • Tel.: +1-202-862-5600 • Fax: +1-202-467-4439 • ifpri@cgiar.org
www.ifpri.org

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

Acknowledgments

This research was supported by National Institutes of Health grants TW-05598 on "Early Nutrition, Human Capital, and Economic Productivity" and HD-046125 on "Education and Health across the Life Course in Guatemala" and National Science Foundation/Economics grants SES 0136616 and SES 0211404 on "Collaborative Research: Nutritional Investments in Children, Adult Human Capital, and Adult Productivities."

We thank Alexis Murphy and Meng Wang for excellent research assistance in the preparation of the data for this paper. We also thank participants for their comments during presentations at the 2003 Northeast Universities Development Consortium Annual Meetings, the 2003 Latin American and Caribbean Economic Association Annual Meetings, the 2003 Inter-American Development Bank Social Policy Network Conference, the 2004 Population Association of America Annual Meetings, the 2005 Minnesota International Economic Development Conference, and meetings at the Chinese University of Hong Kong, Dartmouth University, the University of California-Riverside, the University College of London, and Williams College.

John A. Maluccio
Middlebury College

John Hoddinott and Agnes R. Quisumbing
International Food Policy Research Institute

Jere R. Behrman
University of Pennsylvania

Reynaldo Martorell and Aryeh D. Stein
Emory University

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

[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]

[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

---

[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,

---

[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

[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

[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)

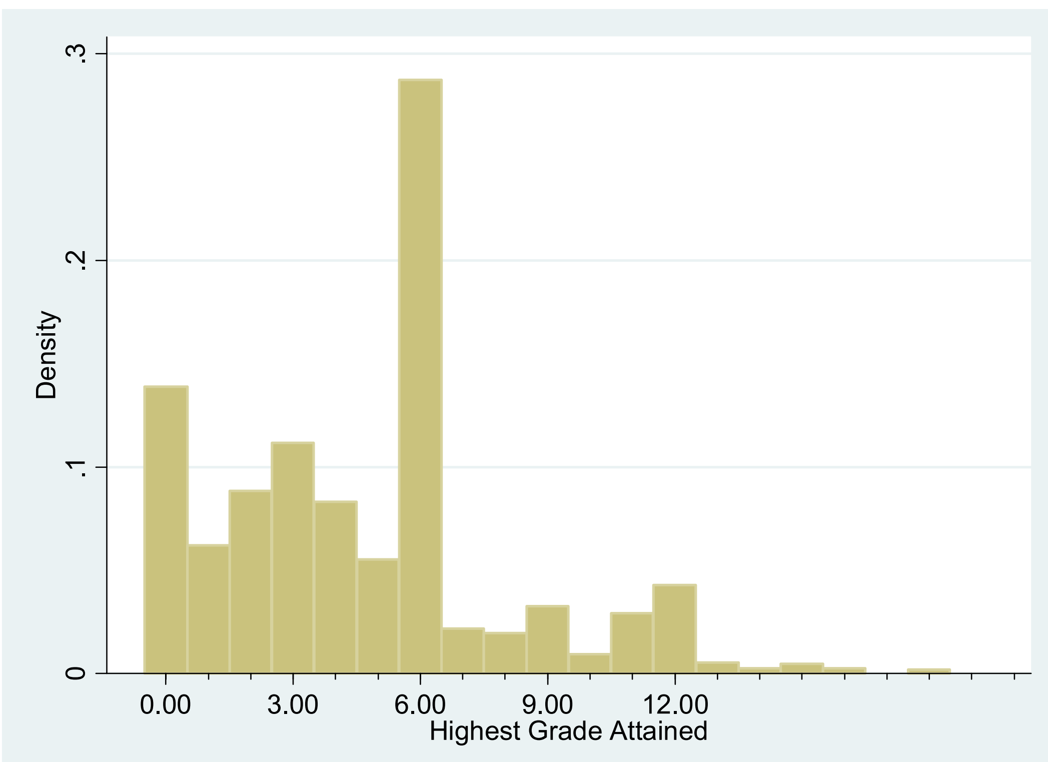
Notes: Sample consists of all individuals who were exposed to the INCAP supplementation project between 1969 and 1977 and who were subsequently reinterviewed in 2002–04.

[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

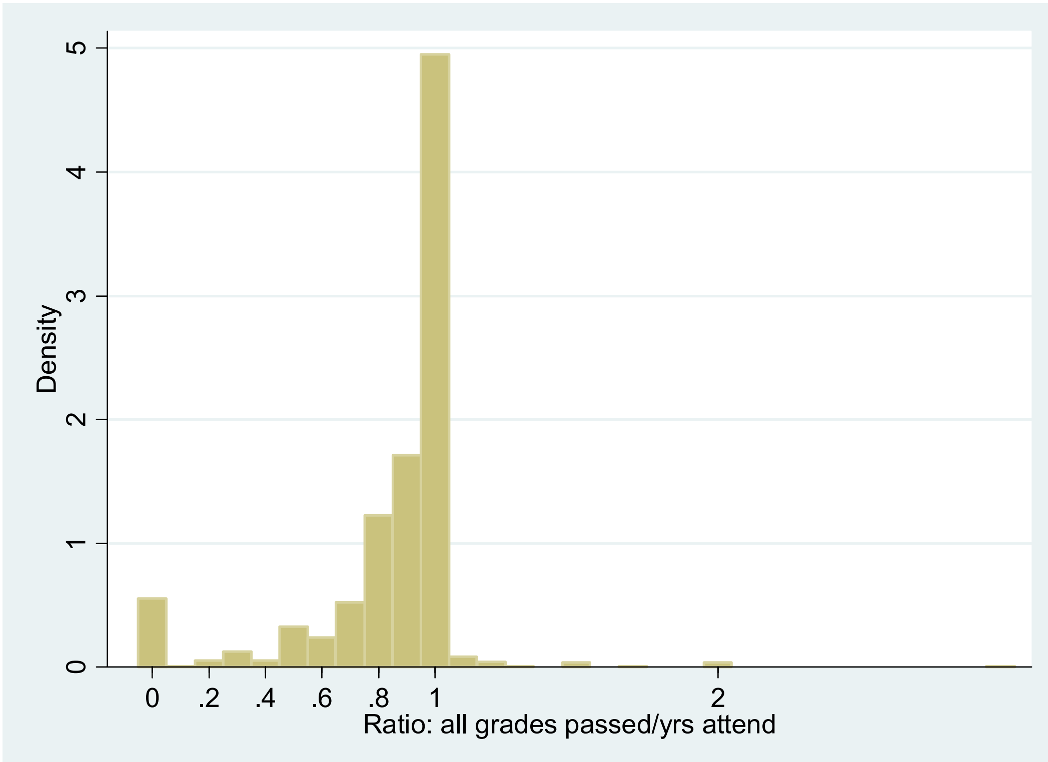


attain an average of 0.9 more grades than women; (2) schooling attainment has increased over time for both men and women; and (3) comparisons of unconditional means always show that grade attainments are higher in Espíritu Santo, one of the *fresco* villages, than in the other study villages, particularly 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.

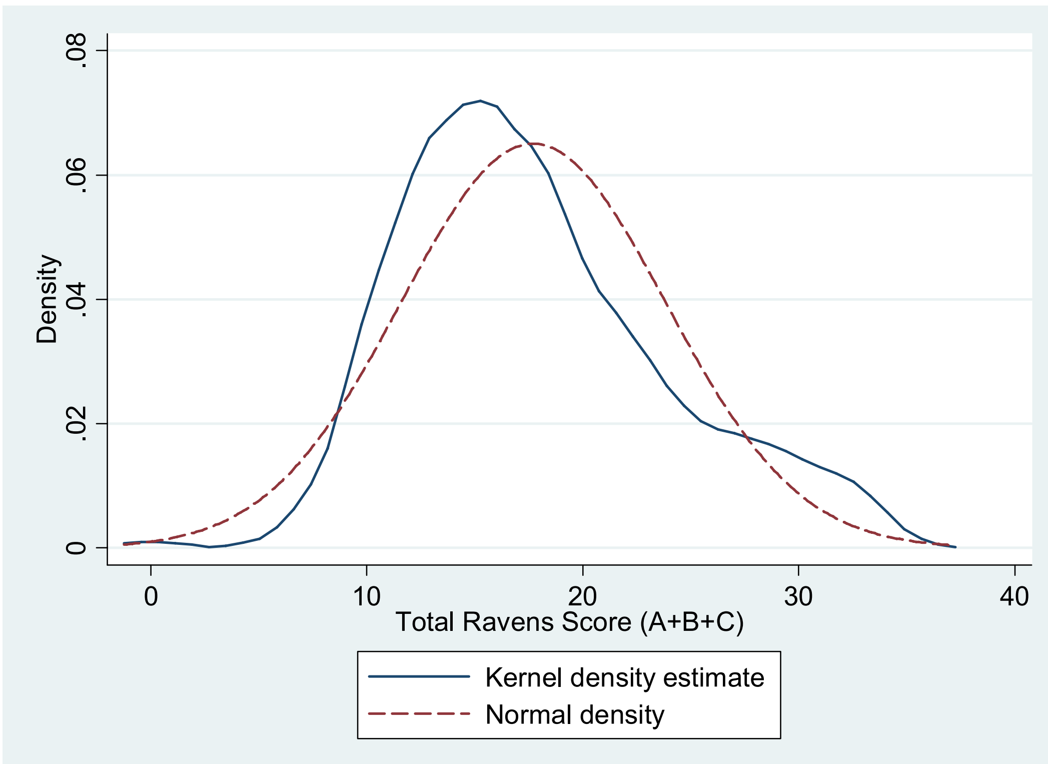
All individuals were administered Raven's Progressive Matrices, a nonverbal assessment of cognitive ability (Raven, Court, and Raven 1984). Raven's Progressive Matrices are considered to be a measure of eductive ability: "the ability to make sense

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.

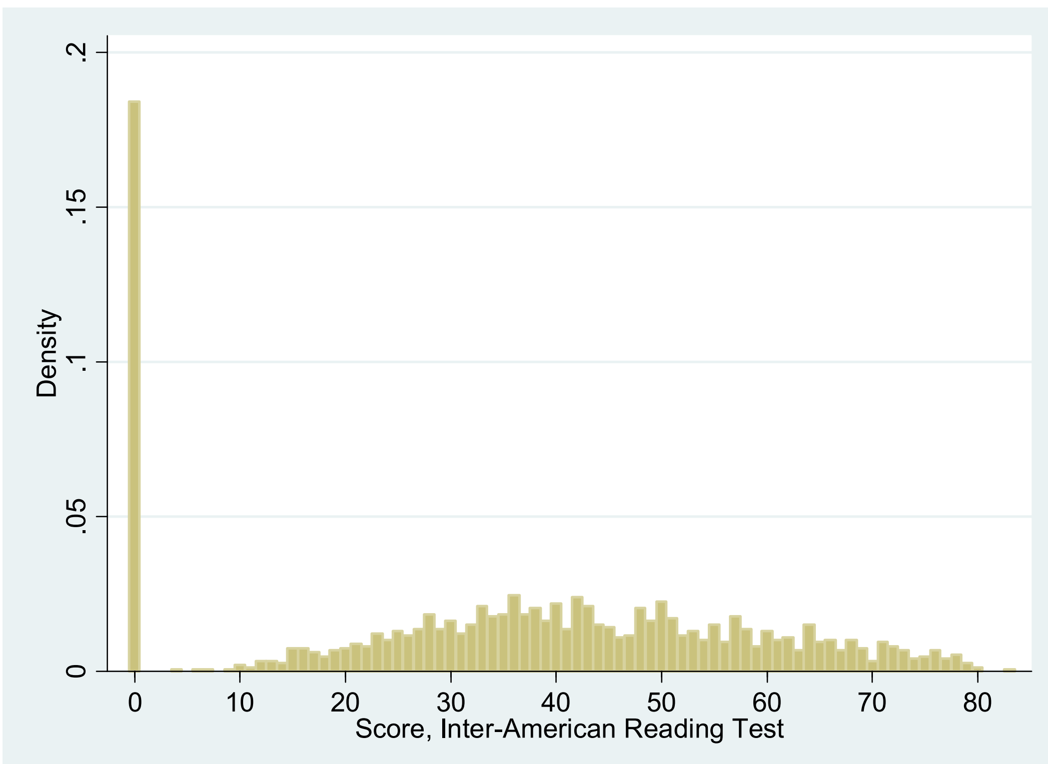
Figure 3—Distribution of scores, Ravens Progressive Matrices



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 i^{th} 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-

[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

[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

	Marginal effects of *atole* exposure (relative to *fresco*) from birth to 36 months		
Outcome	**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 (0.28) [N = 1,469]	0.411 (1.30) [N = 786]	-0.287 (0.84) [N = 683]
Grades passed / years attended school	0.046 (1.31) [N = 1,337]	0.105 (2.14)** [N = 715]	-0.025 (0.53) [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 (2.33)**	1.609 (1.79)*	1.728 (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

	Marginal effects of *atole* exposure (relative to *fresco*) from birth to 36 months		
Outcome	**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

	Marginal effects of *atole* exposure (relative to *fresco*) from birth to 36 months		
Outcome	**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

[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	1.113
	(1.79)*	(1.97)*
Grades passed / years attended school, women	0.104	0.139
	(1.66)*	(2.48)**
Raven's Progressive Matrices	1.488	1.511
	(1.44)	(1.81)*
Inter-American Reading Series	6.632	6.586
	(1.69)*	(2.00)**
Proportion in top half of distribution of scores on Inter-American Reading Series	0.148	0.168
	(1.49)	(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?

[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

	Model 1	Model 2
Covariates	(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.

References

Alderman, H., J. Hoddinott, and B. Kinsey. 2006. Long-term consequences of early childhood malnutrition. *Oxford Economic Papers*. Forthcoming.

Alderman, H., J. R. Behrman, V. Lavy, and R. Menon. 2001b. Child health and school enrollment: A longitudinal analysis. *Journal of Human Resources* 36 (1): 185–205.

Alderman, H., J. R. Behrman, D. Ross, and R. Sabot. 1996. The returns to endogenous human capital in Pakistan's rural wage labor market. *Oxford Bulletin of Economics and Statistics* 58 (1): 29–55.

Alderman, H., J. R. Behrman, H.-P. Kohler, J. A. Maluccio, and S. Cotts Watkins. 2001a. Attrition in longitudinal household survey data: Some tests for three developing country samples. *Demographic Research* [Online] 5 (4): 79–124. Available at <http://www.demographic-research.org>.

Angrist, J. D., and V. Lavy. 2002. *The effect of high school matriculation awards: Evidence from randomized trails*. Working Paper 9389. Cambridge, Mass., U.S.A.: National Bureau of Economic Research.

Baker, A. J. L., C. S. Piotrkowski, and J. Brooks-Gunn. 1998. The effects of home instruction program for preschool youngsters (HIPPY) on children's school performance at the end of the program and one year later. *Early Childhood Research Quarterly* 13 (4): 571-588.

Behrman, J. R. 1996. Impact of health and nutrition on education. *World Bank Research Observer* 11 (1): 23–37.

Behrman, J. R., and J. Hoddinott. 2005. Program evaluation with unobserved heterogeneity and selective implementation: The Mexican *Progresa* impact on child nutrition. *Oxford Bulletin of Economics and Statistics* 67 (2): 547–569.

Behrman, J. R., and M. R. Rosenzweig. 2002. Does increasing women's schooling raise the schooling of the next generation? *American Economic Review* 92 (1): 323–334.

________. 2005. Does increasing women's schooling raise the schooling of the next generation—Reply. *American Economic Review* 95 (5): 1745–1750.

Behrman, J. R., Y. Cheng, and P. Todd. 2004. Evaluating preschool programs when length of exposure to the program varies: A nonparametric approach. *Review of Economics and Statistics* 86 (1): 108–132.

Behrman, J. R., J. Hoddinott, J. A. Maluccio, E. Soler-Hampejsek, E. L. Behrman, R. Martorell, A. Quisumbing, M. Ramírez, and A. D. Stein. 2005. What determines adult skills? Impacts of preschool, school-years, and post-school experiences in Guatemala. University of Pennsylvania, Philadelphia, Penn., U.S.A. Photocopy.

Bergeron, G. 1992. Social and economic development in four ladino communities of eastern Guatemala: A comparative description. *Food and Nutrition Bulletin* 14 (3): 221–236.

Bertrand, M., E. Duflo, and S. Mullainathan. 2004. How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* 119 (1): 249–275.

Boissiere, M., J. B. Knight, and R. H. Sabot. 1985. Earnings, schooling, ability, and cognitive skills. *American Economic Review* 75 (5): 1016–1030.

Cunha, F., J. J. Heckman, L. Lochner, and D. V. Masterov. 2005. Interpreting the evidence on life cycle skill formation. In *Handbook of the economics of education*, ed. E. Hanushek and F. Welch. Amsterdam: North Holland.

Currie, J., and D. Thomas. 1995. Does Head Start make a difference? *American Economic Review* 85 (3): 341–364.

Deaton, A. 1997. *The analysis of household surveys: A microeconometric approach to development policy*. Baltimore, Md., U.S.A., and London: Johns Hopkins University Press for the World Bank.

Estudio 1360. 2002. Changes in the socioeconomic and cultural conditions that affect the formation of human capital and economic productivity. Final report presented to The Institute of Nutrition of Central America and Panama (INCAP), May 13. Photocopy.

Fitzgerald, J., P. Gottschalk, and R. Moffitt. 1998a. An analysis of sample attrition in panel data. *Journal of Human Resources* 33 (2): 251–299.

________. 1998b. The impact of attrition in the PSID on intergenerational analysis. *Journal of Human Resources* 33 (2): 300–344.

Garces, E., D. Thomas, and J. Currie. 2002. Longer-term effects of Head Start. *American Economic Review* 92 (4): 999–1012.

Glewwe, P., and E. M. King. 2001. The impact of early childhood nutrition status on cognitive achievement: Does the timing of malnutrition matter? *World Bank Economic Review* 15 (1): 81–114.

Glewwe, P., H. Jacoby, and E. M. King. 2000. Early childhood nutrition and academic achievement: A longitudinal analysis. *Journal of Public Economics* 81 (3): 345–368.

Grajeda, R., J. R. Behrman, R. Flores, J. A. Maluccio, R. Martorell, and A. D. Stein. 2005. The human capital study 2000–04: Tracking, data collection, coverage, and attrition. *Food and Nutrition Bulletin* 26 (2) (Supplement 1): S15–S23.

Grantham-McGregor, S. M., L. Fernald, and K. Sethuraman. 1999a. Effects of health and nutrition on cognitive and behavioral development in children in the first three years of life: Part 1: Low birth weight, breastfeeding, and protein-energy malnutrition. *Food and Nutrition Bulletin* 20 (1): 53–75.

________. 1999b. Effects of health and nutrition on cognitive and behavioral development in children in the first three years of life: Part 2: Infections and micronutrient deficiencies: Iodine, iron, and zinc. *Food and Nutrition Bulletin* 20 (1): 76–99.

Grantham-McGregor, S. M., C. Walker, S. Chang, and C. Powell. 1997. Effects of early childhood supplementation with and without stimulation on later development in stunted Jamaican children. *American Journal of Clinical Nutrition* 66 (2): 247–253.

Habicht, J.-P., and R. Martorell. 1992. Objectives, research design, and implementation of the INCAP longitudinal study. *Food and Nutrition Bulletin* 14 (3): 176–190.

Harcourt Assessment. 2005. Raven's progressive matrices. <http://www.harcourt-uk.com/product.aspx?n=1342&s=1346&skey=3068>, accessed September 12, 2005.

Hoddinott, J. 1996. Wages and unemployment in an urban African labour market. *Economic Journal* 106 (439): 1610–1626.

Hoddinott, J., J. R. Behrman, and R. Martorell. 2005. Labor force activities and income among young Guatemalan adults. *Food and Nutrition Bulletin* 26 (2) (Supplement 1): S98-S109.

Hruschka D. J., D. W. Sellen, A. D. Stein, and R. Martorell. 2003. Delayed onset of lactation and risk of ending full breast-feeding early in rural Guatemala. *Journal of Nutrition* 133 (8): 2592–2599.

Huber, P. 1967. The behavior of maximum likelihood estimates under non-standard conditions. In *Proceedings of the fifth Berkeley symposium in mathematical statistics and probability*, Vol. 1, 221–233. Berkeley, Calif., U.S.A.: University of California Press.

Johnston, F., S. Low, Y. de Baessa, and R. MacVean. 1987. Interaction of nutritional and socioeconomic status as determinants of cognitive achievement in disadvantaged urban Guatemalan children. *American Journal of Physical Anthropology* 73 (4): 501–506.

Kish, L. 1965. *Survey sampling*. New York: John Wiley & Sons.

Lasky, R., R. Klein, C. Yarborough, P. Engle, A. Lechtig, and R. Martorell. 1981. The relationship between physical growth and infant development in rural Guatemala. *Child Development* 52 (2): 219–226.

Li, H., H. X. Barnhart, A. D. Stein, and R. Martorell. 2003. Effects of early childhood supplementation on the educational achievement of women. *Pediatrics* 112 (5): 1156–1162.

Li, H., A. M. DiGirolamo, H. X. Barnhart, A. D. Stein, and R. Martorell. 2004. Relative importance of birth size and postnatal growth for women's educational achievement. *Early Human Development* 76 (1): 1–16.

Maluccio, J. A., R. Martorell, and L. Fernando Ramírez. 2005. Household expenditures and wealth among young Guatemalan adults. *Food and Nutrition Bulletin* 26 (2) (Supplement 1): S110–S119.

Maluccio, J. A., A. Murphy, and K. M. Yount. 2005. Research note: A socioeconomic index for the INCAP Longitudinal Study 1969–77. *Food and Nutrition Bulletin* 26 (2) (Supplement 1): S120–S124.

Maluccio, J. A., P. Melgar, H. Méndez, A. Murphy, and K. M. Yount. 2005. Social and economic development and change in four Guatemalan villages: Demographics, schooling, occupation, and assets. *Food and Nutrition Bulletin* 26 (2) (Supplement 1): S55–S67.

Manuel, H. T. 1967. *Technical reports, tests of general ability and tests of reading, Interamerican series*. San Antonio, Tex., U.S.A.: Guidance Testing Associates.

Martorell, R. 1997. Undernutrition during pregnancy and early childhood and its consequences for cognitive and behavioral development. In *Early child development: Investing in our children's future*, ed. M. E. Young, 39–83. Amsterdam: Elsevier.

Martorell R., J.-P. Habicht, and J. A. Rivera. 1995. History and design of the INCAP longitudinal study (1969-77) and its follow up (1988-89). *Journal of Nutrition* 125 (4S): 1027S–1041S.

Martorell, R., J. R. Behrman, R. Flores, and A. D. Stein. 2005. Rationale for a follow-up study focusing on economic productivity. *Food and Nutrition Bulletin* 26 (2) (Supplement 1): S5–S14.

Martorell, R., D. G. Schroeder, J. A. Rivera, and H. J. Kaplowitz. 1995. Patterns of linear growth in rural Guatemalan adolescents and children. *Journal of Nutrition* 125 (4S): 1060S–1067S.

Moulton, B. R. 1990. An illustration of a pitfall in estimating the effects of aggregate variables on micro units. *Review of Economics and Statistics* 72 (2): 334–338.

Pitt, M. M. 1997. Estimating the determinants of child health when fertility and mortality are selective. *Journal of Human Resources* 32 (1): 127–158.

Pitt, M., and M. R. Rosenzweig. 1989. *The selectivity of fertility and the determinants of human capital investments: Parametric and semi-parametric estimates*. Department of Economics Working Paper 89–30. Providence, R.I., U.S.A.: Brown University.

Pivaral, V. M. 1972. Características Económicas y Socioculturales de Cuatro Aldeas Ladinas de Guatemala. Ministerio de Educación Pública, Instituto Indigenista Nacional, Guatemala.

Pollitt, E., K. S. Gorman, P. Engle, R. Martorell, and J. A. Rivera. 1993. Early supplementary feeding and cognition: Effects over two decades. *Monographs of the Society for Research in Child Development, Serial No. 235*, 58 (7).

Ramey, C. T., F. A. Campbell, and C. Blair. 1998. Enhancing the life course for high-risk children. In *Social programs that work*, ed. J. Crane, 184-199. New York: Russell Sage Foundation.

Raven, J. C., J. H. Court, and J. Raven. 1984. *Manual for Raven's Progressive Matrices and Vocabulary Scales. Section 2: Coloured progressive matrices*. London: H. K. Lewis.

Read, M. S., and J.-P. Habicht. 1992. History of the INCAP longitudinal study on the effects of early nutrition supplementation in child growth and development. *Food and Nutrition Bulletin* 14 (3): 169–175.

Rose, D., R. Martorell, and J. A. Rivera. 1992. Infant mortality rates before, during, and after a nutrition and health intervention in rural Guatemalan villages. *Food and Nutrition Bulletin* 14 (3): 215–220.

Schroeder, D. G., H. J. Kaplowitz, and R. Martorell. 1992. Patterns and predictors of participation and consumption of supplement in an intervention study in rural Guatemala. *Food and Nutrition Bulletin* 14 (3): 191–200.

Schroeder, D. G., R. Martorell, J. A. Rivera, M. T. Ruel, and J.-P. Habicht. 1995. Age differences in the impact of nutritional supplementation on growth. *Journal of Nutrition* 125 (4S): 1051S–1059S.

Schweinhart, L. J., and D. P. Weikart. 1998. High/scope Perry Preschool Program effects at age twenty-seven. In *Social programs that work*, ed. J. Crane, 148-162. New York: Russell Sage Foundation.

Stein, A. D., J. Behrman, A. DiGirolamo, R. Grajeda, R. Martorell, A. Quisumbing, and U. Ramakrishnan. 2005. Schooling, educational achievement and cognitive functioning among young Guatemalan adults. *Food and Nutrition Bulletin* 26 (2) (Supplement 1): S46–S54.

Sternberg, R. J., and E. L. Grigorenko. 1997. Interventions for cognitive development in children 0-3 years old. In *Early child development: Investing in our children's future*, ed. M. E. Young, 127–156. Amsterdam: Elsevier Science.

Torun, B., A. D. Stein, D. G. Schroeder, R. Grajeda, A. J. Conlisk, M. Rodríguez, H. Méndez, and R. Martorell. 2002. Migration and cardiovascular disease risk factors in young Guatemalan adults. *International Journal of Epidemiology* 31 (1): 218–226.

UNICEF (United Nations Children's Fund). 1998. *The state of the world's children 1998*. New York: Oxford University Press.

White, H. 1980. A heteroscedasticity-consistent covariance matrix and a direct test for heteroscedasticity. *Econometrica* 48 (4): 817–838.

Wooldridge, J. M. 2003. Cluster-sample methods in applied econometrics. *American Economic Review Papers and Proceedings* 93 (2): 133–138.

World Bank. 2003. *Poverty in Guatemala*. Report No. 24221-GU. Washington, D.C.

Yaqub, S. 2002. Poor children grow into poor adults: Harmful mechanisms or over-deterministic theory. *Journal of International Development* 14 (8): 1081–1093.

FCND DISCUSSION PAPERS

206 *Conflict, Food Insecurity, and Globalization*, Ellen Messer and Marc J. Cohen, May 2006

205 *Insights from Poverty Maps for Development and Food Relief Program Targeting: An Application to Malawi*, Todd Benson, April 2006

204 *Nutrition Mapping in Tanzania: An Exploratory Analysis*, Kenneth R. Simler, March 2006

203 *Early Childhood Nutrition, Schooling, and Sibling Inequality in a Dynamic Context: Evidence from South Africa*, Futoshi Yamauchi, January 2006

202 *Has Economic Growth in Mozambique Been Pro-Poor?* Robert C. James, Channing Arndt, and Kenneth R. Simler, December 2005

201 *Community, Inequality, and Local Public Goods: Evidence from School Financing in South Africa*, Futoshi Yamauchi and Shinichi Nishiyama, September 2005

200 *Is Greater Decisionmaking Power of Women Associated with Reduced Gender Discrimination in South Asia?* Lisa C. Smith and Elizabeth M. Byron, August 2005

199 *Evaluating the Cost of Poverty Alleviation Transfer Programs: An Illustration Based on PROGRESA in Mexico*, David Coady, Raul Perez, and Hadid Vera-Ilamas, July 2005

198 *Why the Poor in Rural Malawi Are Where They Are: An Analysis of the Spatial Determinants of the Local Prevalence of Poverty*, Todd Benson, Jordan Chamberlin, and Ingrid Rhinehart, July 2005

194 *Livelihoods, Growth, and Links to Market Towns in 15 Ethiopian Villages*, Stefan Dercon and John Hoddinott, July 2005

193 *Livelihood Diversification and Rural-Urban Linkages in Vietnam's Red River Delta*, Hoang Xuan Thanh, Dang Nguyen Anh, and Ceclila Tacoli, June 2005

192 *Poverty, Inequality, and Geographic Targeting: Evidence from Small-Area Estimates in Mozambique*, Kenneth R. Simler and Virgulino Nhate, June 2005

191 *Program Participation Under Means-Testing and Self-Selection Targeting Methods*, David P. Coady and Susan W. Parker, April 2005

190 *Social Learning, Neighborhood Effects, and Investment in Human Capital: Evidence from Green-Revolution India*, Futoshi Yamauchi, April 2005

189 *Estimating Utility-Consistent Poverty Lines*, Channing Arndt and Kenneth R. Simler, March 2005

188 *Coping with the "Coffee Crisis" in Central America: The Role of the Nicaraguan* Red de Protección Social *(RPS)*, John A. Maluccio, February 2005

187 *The Use of Operations Research as a Tool for Monitoring and Managing Food-Assisted Maternal/Child Health and Nutrition (MCHN) Programs: An Example from Haiti*, Cornelia Loechl, Marie T. Ruel, Gretel Pelto, and Purnima Menon, February 2005

186 *Are Wealth Transfers Biased Against Girls? Gender Differences in Land Inheritance and Schooling Investment in Ghana's Western Region*, Agnes R. Quisumbing, Ellen M. Payongayong, and Keijiro Otsuka, August 2004

185 *Assets at Marriage in Rural Ethiopia*, Marcel Fafchamps and Agnes Quisumbing, August 2004

184 *Impact Evaluation of a Conditional Cash Transfer Program: The Nicaraguan* Red de Protección Social, John A. Maluccio and Rafael Flores, July 2004

183 *Poverty in Malawi, 1998*, Todd Benson, Charles Machinjili, and Lawrence Kachikopa, July 2004

182 *Race, Equity, and Public Schools in Post-Apartheid South Africa: Is Opportunity Equal for All Kids?* Futoshi Yamauchi, June 2004

181 *Scaling Up Community-Driven Development: A Synthesis of Experience*, Stuart Gillespie, June 2004

180 *Kudumbashree—Collective Action for Poverty Alleviation and Women's Employment*, Suneetha Kadiyala, May 2004

179 *Scaling Up HIV/AIDS Interventions Through Expanded Partnerships (STEPs) in Malawi*, Suneetha Kadiyala, May 2004

178 *Community-Driven Development and Scaling Up of Microfinance Services: Case Studies from Nepal and India*, Manohar P. Sharma, April 2004

177 *Community Empowerment and Scaling Up in Urban Areas: The Evolution of PUSH/PROSPECT in Zambia*, James Garrett, April 2004

176 *Why Is Child Malnutrition Lower in Urban than Rural Areas? Evidence from 36 Developing Countries*, Lisa C. Smith, Marie T. Ruel, and Aida Ndiaye, March 2004

175 *Consumption Smoothing and Vulnerability in the Zone Lacustre, Mali*, Sarah Harrower and John Hoddinott, March 2004

174 *The Cost of Poverty Alleviation Transfer Programs: A Comparative Analysis of Three Programs in Latin America*, Natàlia Caldés, David Coady, and John A. Maluccio, February 2004

173 *Food Aid Distribution in Bangladesh: Leakage and Operational Performance*, Akhter U. Ahmed, Shahidur Rashid, Manohar Sharma, and Sajjad Zohir in collaboration with Mohammed Khaliquzzaman, Sayedur Rahman, and the Data Analysis and Technical Assistance Limited, February 2004

172 *Designing and Evaluating Social Safety Nets: Theory, Evidence, and Policy Conclusions*, David P. Coady, January 2004

171 *Living Life: Overlooked Aspects of Urban Employment*, James Garrett, January 2004

170 *From Research to Program Design: Use of Formative Research in Haiti to Develop a Behavior Change Communication Program to Prevent Malnutrition*, Purnima Menon, Marie T. Ruel, Cornelia Loechl, and Gretel Pelto, December 2003

169 *Nonmarket Networks Among Migrants: Evidence from Metropolitan Bangkok, Thailand*, Futoshi Yamauchi and Sakiko Tanabe, December 2003

168 *Long-Term Consequences of Early Childhood Malnutrition*, Harold Alderman, John Hoddinott, and Bill Kinsey, December 2003

167 *Public Spending and Poverty in Mozambique*, Rasmus Heltberg, Kenneth Simler, and Finn Tarp, December 2003

166 *Are Experience and Schooling Complementary? Evidence from Migrants' Assimilation in the Bangkok Labor Market*, Futoshi Yamauchi, December 2003

165 *What Can Food Policy Do to Redirect the Diet Transition?* Lawrence Haddad, December 2003

164 *Impacts of Agricultural Research on Poverty: Findings of an Integrated Economic and Social Analysis*, Ruth Meinzen-Dick, Michelle Adato, Lawrence Haddad, and Peter Hazell, October 2003

163 *An Integrated Economic and Social Analysis to Assess the Impact of Vegetable and Fishpond Technologies on Poverty in Rural Bangladesh*, Kelly Hallman, David Lewis, and Suraiya Begum, October 2003

162 *The Impact of Improved Maize Germplasm on Poverty Alleviation: The Case of Tuxpeño-Derived Material in Mexico*, Mauricio R. Bellon, Michelle Adato, Javier Becerril, and Dubravka Mindek, October 2003

161 *Assessing the Impact of High-Yielding Varieties of Maize in Resettlement Areas of Zimbabwe*, Michael Bourdillon, Paul Hebinck, John Hoddinott, Bill Kinsey, John Marondo, Netsayi Mudege, and Trudy Owens, October 2003

160 *The Impact of Agroforestry-Based Soil Fertility Replenishment Practices on the Poor in Western Kenya*, Frank Place, Michelle Adato, Paul Hebinck, and Mary Omosa, October 2003

159 *Rethinking Food Aid to Fight HIV/AIDS*, Suneetha Kadiyala and Stuart Gillespie, October 2003

158 *Food Aid and Child Nutrition in Rural Ethiopia*, Agnes R. Quisumbing, September 2003

157 *HIV/AIDS, Food Security, and Rural Livelihoods: Understanding and Responding*, Michael Loevinsohn and Stuart Gillespie, September 2003

156 *Public Policy, Food Markets, and Household Coping Strategies in Bangladesh: Lessons from the 1998 Floods*, Carlo del Ninno, Paul A. Dorosh, and Lisa C. Smith, September 2003

155 *Consumption Insurance and Vulnerability to Poverty: A Synthesis of the Evidence from Bangladesh, Ethiopia, Mali, Mexico, and Russia*, Emmanuel Skoufias and Agnes R. Quisumbing, August 2003

154 *Cultivating Nutrition: A Survey of Viewpoints on Integrating Agriculture and Nutrition*, Carol E. Levin, Jennifer Long, Kenneth R. Simler, and Charlotte Johnson-Welch, July 2003

153 Maquiladoras *and Market Mamas: Women's Work and Childcare in Guatemala City and Accra*, Agnes R. Quisumbing, Kelly Hallman, and Marie T. Ruel, June 2003

152 *Income Diversification in Zimbabwe: Welfare Implications From Urban and Rural Areas*, Lire Ersado, June 2003

151 *Childcare and Work: Joint Decisions Among Women in Poor Neighborhoods of Guatemala City*, Kelly Hallman, Agnes R. Quisumbing, Marie T. Ruel, and Bénédicte de la Brière, June 2003

150 *The Impact of* PROGRESA *on Food Consumption*, John Hoddinott and Emmanuel Skoufias, May 2003

149 *Do Crowded Classrooms Crowd Out Learning? Evidence From the Food for Education Program in Bangladesh*, Akhter U. Ahmed and Mary Arends-Kuenning, May 2003

148 *Stunted Child-Overweight Mother Pairs: An Emerging Policy Concern?* James L. Garrett and Marie T. Ruel, April 2003

147 *Are Neighbors Equal? Estimating Local Inequality in Three Developing Countries*, Chris Elbers, Peter Lanjouw, Johan Mistiaen, Berk Özler, and Kenneth Simler, April 2003

146 *Moving Forward with Complementary Feeding: Indicators and Research Priorities*, Marie T. Ruel, Kenneth H. Brown, and Laura E. Caulfield, April 2003

145 *Child Labor and School Decisions in Urban and Rural Areas: Cross Country Evidence*, Lire Ersado, December 2002

144 *Targeting Outcomes Redux*, David Coady, Margaret Grosh, and John Hoddinott, December 2002

143 *Progress in Developing an Infant and Child Feeding Index: An Example Using the Ethiopia Demographic and Health Survey 2000*, Mary Arimond and Marie T. Ruel, December 2002

142 *Social Capital and Coping With Economic Shocks: An Analysis of Stunting of South African Children*, Michael R. Carter and John A. Maluccio, December 2002

141 *The Sensitivity of Calorie-Income Demand Elasticity to Price Changes: Evidence from Indonesia*, Emmanuel Skoufias, November 2002

140 *Is Dietary Diversity an Indicator of Food Security or Dietary Quality? A Review of Measurement Issues and Research Needs*, Marie T. Ruel, November 2002

139 *Can South Africa Afford to Become Africa's First Welfare State?* James Thurlow, October 2002

138 *The Food for Education Program in Bangladesh: An Evaluation of its Impact on Educational Attainment and Food Security*, Akhter U. Ahmed and Carlo del Ninno, September 2002

137 *Reducing Child Undernutrition: How Far Does Income Growth Take Us?* Lawrence Haddad, Harold Alderman, Simon Appleton, Lina Song, and Yisehac Yohannes, August 2002

136 *Dietary Diversity as a Food Security Indicator*, John Hoddinott and Yisehac Yohannes, June 2002

135 *Trust, Membership in Groups, and Household Welfare: Evidence from KwaZulu-Natal, South Africa*, Lawrence Haddad and John A. Maluccio, May 2002

134 *In-Kind Transfers and Household Food Consumption: Implications for Targeted Food Programs in Bangladesh*, Carlo del Ninno and Paul A. Dorosh, May 2002

133 *Avoiding Chronic and Transitory Poverty: Evidence From Egypt, 1997-99*, Lawrence Haddad and Akhter U. Ahmed, May 2002

132 *Weighing What's Practical: Proxy Means Tests for Targeting Food Subsidies in Egypt*, Akhter U. Ahmed and Howarth E. Bouis, May 2002

131 *Does Subsidized Childcare Help Poor Working Women in Urban Areas? Evaluation of a Government-Sponsored Program in Guatemala City*, Marie T. Ruel, Bénédicte de la Brière, Kelly Hallman, Agnes Quisumbing, and Nora Coj, April 2002

130 *Creating a Child Feeding Index Using the Demographic and Health Surveys: An Example from Latin America*, Marie T. Ruel and Purnima Menon, April 2002

129 *Labor Market Shocks and Their Impacts on Work and Schooling: Evidence from Urban Mexico*, Emmanuel Skoufias and Susan W. Parker, March 2002

128 *Assessing the Impact of Agricultural Research on Poverty Using the Sustainable Livelihoods Framework*, Michelle Adato and Ruth Meinzen-Dick, March 2002

127 *A Cost-Effectiveness Analysis of Demand- and Supply-Side Education Interventions: The Case of* PROGRESA *in Mexico*, David P. Coady and Susan W. Parker, March 2002

126 *Health Care Demand in Rural Mozambique: Evidence from the 1996/97 Household Survey*, Magnus Lindelow, February 2002

125 *Are the Welfare Losses from Imperfect Targeting Important?*, Emmanuel Skoufias and David Coady, January 2002

124 *The Robustness of Poverty Profiles Reconsidered*, Finn Tarp, Kenneth Simler, Cristina Matusse, Rasmus Heltberg, and Gabriel Dava, January 2002

123 *Conditional Cash Transfers and Their Impact on Child Work and Schooling: Evidence from the* PROGRESA *Program in Mexico*, Emmanuel Skoufias and Susan W. Parker, October 2001

122 *Strengthening Public Safety Nets: Can the Informal Sector Show the Way?*, Jonathan Morduch and Manohar Sharma, September 2001

121 *Targeting Poverty Through Community-Based Public Works Programs: A Cross-Disciplinary Assessment of Recent Experience in South Africa*, Michelle Adato and Lawrence Haddad, August 2001

120 *Control and Ownership of Assets Within Rural Ethiopian Households*, Marcel Fafchamps and Agnes R. Quisumbing, August 2001

119 *Assessing Care: Progress Towards the Measurement of Selected Childcare and Feeding Practices, and Implications for Programs*, Mary Arimond and Marie T. Ruel, August 2001

118 *Is* PROGRESA *Working? Summary of the Results of an Evaluation by IFPRI*, Emmanuel Skoufias and Bonnie McClafferty, July 2001

117 *Evaluation of the Distributional Power of* PROGRESA*'s Cash Transfers in Mexico*, David P. Coady, July 2001

116 *A Multiple-Method Approach to Studying Childcare in an Urban Environment: The Case of Accra, Ghana*, Marie T. Ruel, Margaret Armar-Klemesu, and Mary Arimond, June 2001

115 *Are Women Overrepresented Among the Poor? An Analysis of Poverty in Ten Developing Countries*, Agnes R. Quisumbing, Lawrence Haddad, and Christina Peña, June 2001

114 *Distribution, Growth, and Performance of Microfinance Institutions in Africa, Asia, and Latin America*, Cécile Lapenu and Manfred Zeller, June 2001

113 *Measuring Power,* Elizabeth Frankenberg and Duncan Thomas, June 2001

112 *Effective Food and Nutrition Policy Responses to HIV/AIDS: What We Know and What We Need to Know*, Lawrence Haddad and Stuart Gillespie, June 2001

111 *An Operational Tool for Evaluating Poverty Outreach of Development Policies and Projects*, Manfred Zeller, Manohar Sharma, Carla Henry, and Cécile Lapenu, June 2001

110 *Evaluating Transfer Programs Within a General Equilibrium Framework*, Dave Coady and Rebecca Lee Harris, June 2001

109 *Does Cash Crop Adoption Detract From Childcare Provision? Evidence From Rural Nepal*, Michael J. Paolisso, Kelly Hallman, Lawrence Haddad, and Shibesh Regmi, April 2001

108 *How Efficiently Do Employment Programs Transfer Benefits to the Poor? Evidence from South Africa*, Lawrence Haddad and Michelle Adato, April 2001

107 *Rapid Assessments in Urban Areas: Lessons from Bangladesh and Tanzania*, James L. Garrett and Jeanne Downen, April 2001

106 *Strengthening Capacity to Improve Nutrition*, Stuart Gillespie, March 2001

105 *The Nutritional Transition and Diet-Related Chronic Diseases in Asia: Implications for Prevention*, Barry M. Popkin, Sue Horton, and Soowon Kim, March 2001

104 *An Evaluation of the Impact of PROGRESA on Preschool Child Height*, Jere R. Behrman and John Hoddinott, March 2001

103 *Targeting the Poor in Mexico: An Evaluation of the Selection of Households for PROGRESA*, Emmanuel Skoufias, Benjamin Davis, and Sergio de la Vega, March 2001

102 *School Subsidies for the Poor: Evaluating a Mexican Strategy for Reducing Poverty*, T. Paul Schultz, March 2001

101 *Poverty, Inequality, and Spillover in Mexico's Education, Health, and Nutrition Program*, Sudhanshu Handa, Mari-Carmen Huerta, Raul Perez, and Beatriz Straffon, March 2001

100 *On the Targeting and Redistributive Efficiencies of Alternative Transfer Instruments*, David Coady and Emmanuel Skoufias, March 2001

99 *Cash Transfer Programs with Income Multipliers: PROCAMPO in Mexico*, Elisabeth Sadoulet, Alain de Janvry, and Benjamin Davis, January 2001

98 *Participation and Poverty Reduction: Issues, Theory, and New Evidence from South Africa*, John Hoddinott, Michelle Adato, Tim Besley, and Lawrence Haddad, January 2001

97 *Socioeconomic Differentials in Child Stunting Are Consistently Larger in Urban Than in Rural Areas*, Purnima Menon, Marie T. Ruel, and Saul S. Morris, December 2000

96 *Attrition in Longitudinal Household Survey Data: Some Tests for Three Developing-Country Samples*, Harold Alderman, Jere R. Behrman, Hans-Peter Kohler, John A. Maluccio, Susan Cotts Watkins, October 2000

95 *Attrition in the Kwazulu Natal Income Dynamics Study 1993-1998*, John Maluccio, October 2000

94 *Targeting Urban Malnutrition: A Multicity Analysis of the Spatial Distribution of Childhood Nutritional Status*, Saul Sutkover Morris, September 2000

93 *Mother-Father Resource Control, Marriage Payments, and Girl-Boy Health in Rural Bangladesh*, Kelly K. Hallman, September 2000

92 *Assessing the Potential for Food-Based Strategies to Reduce Vitamin A and Iron Deficiencies: A Review of Recent Evidence*, Marie T. Ruel and Carol E. Levin, July 2000

91 *Comparing Village Characteristics Derived From Rapid Appraisals and Household Surveys: A Tale From Northern Mali*, Luc Christiaensen, John Hoddinott, and Gilles Bergeron, July 2000

90 *Empirical Measurements of Households' Access to Credit and Credit Constraints in Developing Countries: Methodological Issues and Evidence*, Aliou Diagne, Manfred Zeller, and Manohar Sharma, July 2000

89 *The Role of the State in Promoting Microfinance Institutions*, Cécile Lapenu, June 2000

88 *The Determinants of Employment Status in Egypt*, Ragui Assaad, Fatma El-Hamidi, and Akhter U. Ahmed, June 2000

87 *Changes in Intrahousehold Labor Allocation to Environmental Goods Collection: A Case Study from Rural Nepal*, Priscilla A. Cooke, May 2000

86 *Women's Assets and Intrahousehold Allocation in Rural Bangladesh: Testing Measures of Bargaining Power*, Agnes R. Quisumbing and Bénédicte de la Brière, April 2000

85 *Intrahousehold Impact of Transfer of Modern Agricultural Technology: A Gender Perspective*, Ruchira Tabassum Naved, April 2000

84 *Intrahousehold Allocation and Gender Relations: New Empirical Evidence from Four Developing Countries*, Agnes R. Quisumbing and John A. Maluccio, April 2000

83 *Quality or Quantity? The Supply-Side Determinants of Primary Schooling in Rural Mozambique*, Sudhanshu Handa and Kenneth R. Simler, March 2000

82 *Pathways of Rural Development in Madagascar: An Empirical Investigation of the Critical Triangle of Environmental Sustainability, Economic Growth, and Poverty Alleviation*, Manfred Zeller, Cécile Lapenu, Bart Minten, Eliane Ralison, Désiré Randrianaivo, and Claude Randrianarisoa, March 2000

81 *The Constraints to Good Child Care Practices in Accra: Implications for Programs*, Margaret Armar-Klemesu, Marie T. Ruel, Daniel G. Maxwell, Carol E. Levin, and Saul S. Morris, February 2000

80 *Nontraditional Crops and Land Accumulation Among Guatemalan Smallholders: Is the Impact Sustainable?* Calogero Carletto, February 2000

79 *Adult Health in the Time of Drought*, John Hoddinott and Bill Kinsey, January 2000

78 *Determinants of Poverty in Mozambique: 1996-97*, Gaurav Datt, Kenneth Simler, Sanjukta Mukherjee, and Gabriel Dava, January 2000

77 *The Political Economy of Food Subsidy Reform in Egypt*, Tammi Gutner, November 1999.

76 *Raising Primary School Enrolment in Developing Countries: The Relative Importance of Supply and Demand*, Sudhanshu Handa, November 1999

75 *Determinants of Poverty in Egypt, 1997*, Gaurav *Datt* and Dean Jolliffe, October 1999

74 *Can Cash Transfer Programs Work in Resource-Poor Countries? The Experience in Mozambique*, Jan W. Low, James L. Garrett, and Vitória Ginja, October 1999

73 *Social Roles, Human Capital, and the Intrahousehold Division of Labor: Evidence from Pakistan*, Marcel Fafchamps and Agnes R. Quisumbing, October 1999

72 *Validity of Rapid Estimates of Household Wealth and Income for Health Surveys in Rural Africa*, Saul S. Morris, Calogero Carletto, John Hoddinott, and Luc J. M. Christiaensen, October 1999

71 *Social Capital and Income Generation in South Africa, 1993-98*, John Maluccio, Lawrence Haddad, and Julian May, September 1999

70 *Child Health Care Demand in a Developing Country: Unconditional Estimates from the Philippines*, Kelly Hallman, August 1999

69 *Supply Response of West African Agricultural Households: Implications of Intrahousehold Preference Heterogeneity*, Lisa C. Smith and Jean-Paul Chavas, July 1999

68 *Early Childhood Nutrition and Academic Achievement: A Longitudinal Analysis*, Paul Glewwe, Hanan Jacoby, and Elizabeth King, May 1999

67 *Determinants of Household Access to and Participation in Formal and Informal Credit Markets in Malawi*, Aliou Diagne, April 1999

66 *Working Women in an Urban Setting: Traders, Vendors, and Food Security in Accra*, Carol E. Levin, Daniel G. Maxwell, Margaret Armar-Klemesu, Marie T. Ruel, Saul S. Morris, and Clement Ahiadeke, April 1999

65 *Are Determinants of Rural and Urban Food Security and Nutritional Status Different? Some Insights from Mozambique*, James L. Garrett and Marie T. Ruel, April 1999

64 *Some Urban Facts of Life: Implications for Research and Policy*, Marie T. Ruel, Lawrence Haddad, and James L. Garrett, April 1999

63 *Are Urban Poverty and Undernutrition Growing? Some Newly Assembled Evidence*, Lawrence Haddad, Marie T. Ruel, and James L. Garrett, April 1999

62 *Good Care Practices Can Mitigate the Negative Effects of Poverty and Low Maternal Schooling on Children's Nutritional Status: Evidence from Accra*, Marie T. Ruel, Carol E. Levin, Margaret Armar-Klemesu, Daniel Maxwell, and Saul S. Morris, April 1999

61 *Does Geographic Targeting of Nutrition Interventions Make Sense in Cities? Evidence from Abidjan and Accra*, Saul S. Morris, Carol Levin, Margaret Armar-Klemesu, Daniel Maxwell, and Marie T. Ruel, April 1999

60 *Explaining Child Malnutrition in Developing Countries: A Cross-Country Analysis*, Lisa C. Smith and Lawrence Haddad, April 1999

59 *Placement and Outreach of Group-Based Credit Organizations: The Cases of ASA, BRAC, and PROSHIKA in Bangladesh*, Manohar Sharma and Manfred Zeller, March 1999

58 *Women's Land Rights in the Transition to Individualized Ownership: Implications for the Management of Tree Resources in Western Ghana*, Agnes Quisumbing, Ellen Payongayong, J. B. Aidoo, and Keijiro Otsuka, February 1999

57 *The Structure of Wages During the Economic Transition in Romania*, Emmanuel Skoufias, February 1999

56 *How Does the Human Rights Perspective Help to Shape the Food and Nutrition Policy Research Agenda?*, Lawrence Haddad and Arne Oshaug, February 1999

55 *Efficiency in Intrahousehold Resource Allocation*, Marcel Fafchamps, December 1998

54 *Endogeneity of Schooling in the Wage Function: Evidence from the Rural Philippines*, John Maluccio, November 1998

53 *Agricultural Wages and Food Prices in Egypt: A Governorate-Level Analysis for 1976-1993*, Gaurav Datt and Jennifer Olmsted, November 1998

52 *Testing Nash Bargaining Household Models With Time-Series Data*, John Hoddinott and Christopher Adam, November 1998

51 *Urban Challenges to Food and Nutrition Security: A Review of Food Security, Health, and Caregiving in the Cities*, Marie T. Ruel, James L. Garrett, Saul S. Morris, Daniel Maxwell, Arne Oshaug, Patrice Engle, Purnima Menon, Alison Slack, and Lawrence Haddad, October 1998

50 *Computational Tools for Poverty Measurement and Analysis*, Gaurav Datt, October 1998

49 *A Profile of Poverty in Egypt: 1997*, Gaurav Datt, Dean Jolliffe, and Manohar Sharma, August 1998.

48 *Human Capital, Productivity, and Labor Allocation in Rural Pakistan*, Marcel Fafchamps and Agnes R. Quisumbing, July 1998

47 *Poverty in India and Indian States: An Update*, Gaurav Datt, July 1998

46 *Impact of Access to Credit on Income and Food Security in Malawi*, Aliou Diagne, July 1998

45 *Does Urban Agriculture Help Prevent Malnutrition? Evidence from Kampala*, Daniel Maxwell, Carol Levin, and Joanne Csete, June 1998

44 *Can FAO's Measure of Chronic Undernourishment Be Strengthened?*, Lisa C. Smith, with a *Response* by Logan Naiken, May 1998

43 *How Reliable Are Group Informant Ratings? A Test of Food Security Rating in Honduras*, Gilles Bergeron, Saul Sutkover Morris, and Juan Manuel Medina Banegas, April 1998

42 *Farm Productivity and Rural Poverty in India*, Gaurav Datt and Martin Ravallion, March 1998

41 *The Political Economy of Urban Food Security in Sub-Saharan Africa*, Dan Maxwell, February 1998

40 *Can Qualitative and Quantitative Methods Serve Complementary Purposes for Policy Research? Evidence from Accra*, Dan Maxwell, January 1998

39 *Whose Education Matters in the Determination of Household Income: Evidence from a Developing Country*, Dean Jolliffe, November 1997

38 *Systematic Client Consultation in Development: The Case of Food Policy Research in Ghana, India, Kenya, and Mali*, Suresh Chandra Babu, Lynn R. Brown, and Bonnie McClafferty, November 1997

37 *Why Do Migrants Remit? An Analysis for the Dominican Sierra*, Bénédicte de la Brière, Alain de Janvry, Sylvie Lambert, and Elisabeth Sadoulet, October 1997

36 *The GAPVU Cash Transfer Program in Mozambique: An assessment*, Gaurav Datt, Ellen Payongayong, James L. Garrett, and Marie Ruel, October 1997

35 *Market Access by Smallholder Farmers in Malawi: Implications for Technology Adoption, Agricultural Productivity, and Crop Income*, Manfred Zeller, Aliou Diagne, and Charles Mataya, September 1997

34 *The Impact of Changes in Common Property Resource Management on Intrahousehold Allocation*, Philip Maggs and John Hoddinott, September 1997

33 *Human Milk—An Invisible Food Resource*, Anne Hatløy and Arne Oshaug, August 1997

32 *The Determinants of Demand for Micronutrients: An Analysis of Rural Households in Bangladesh*, Howarth E. Bouis and Mary Jane G. Novenario-Reese, August 1997

31 *Is There an Intrahousehold 'Flypaper Effect'? Evidence from a School Feeding Program*, Hanan Jacoby, August 1997

30 *Plant Breeding: A Long-Term Strategy for the Control of Zinc Deficiency in Vulnerable Populations*, Marie T. Ruel and Howarth E. Bouis, July 1997

29 *Gender, Property Rights, and Natural Resources*, Ruth Meinzen-Dick, Lynn R. Brown, Hilary Sims Feldstein, and Agnes R. Quisumbing, May 1997

28 *Developing a Research and Action Agenda for Examining Urbanization and Caregiving: Examples from Southern and Eastern Africa*, Patrice L. Engle, Purnima Menon, James L. Garrett, and Alison Slack, April 1997

27 *"Bargaining" and Gender Relations: Within and Beyond the Household*, Bina Agarwal, March 1997

26 *Why Have Some Indian States Performed Better Than Others at Reducing Rural Poverty?*, Gaurav Datt and Martin Ravallion, March 1997

25 *Water, Health, and Income: A Review*, John Hoddinott, February 1997

24 *Child Care Practices Associated with Positive and Negative Nutritional Outcomes for Children in Bangladesh: A Descriptive Analysis*, Shubh K. Kumar Range, Ruchira Naved, and Saroj Bhattarai, February 1997

23 *Better Rich, or Better There? Grandparent Wealth, Coresidence, and Intrahousehold Allocation*, Agnes R. Quisumbing, January 1997

22 *Alternative Approaches to Locating the Food Insecure: Qualitative and Quantitative Evidence from South India*, Kimberly Chung, Lawrence Haddad, Jayashree Ramakrishna, and Frank Riely, January 1997

21 *Livestock Income, Male/Female Animals, and Inequality in Rural Pakistan*, Richard H. Adams, Jr., November 1996

20 *Macroeconomic Crises and Poverty Monitoring: A Case Study for India*, Gaurav Datt and Martin Ravallion, November 1996

19 *Food Security and Nutrition Implications of Intrahousehold Bias: A Review of Literature*, Lawrence Haddad, Christine Peña, Chizuru Nishida, Agnes Quisumbing, and Alison Slack, September 1996

18 *Care and Nutrition: Concepts and Measurement*, Patrice L. Engle, Purnima Menon, and Lawrence Haddad, August 1996

17 *Remittances, Income Distribution, and Rural Asset Accumulation*, Richard H. Adams, Jr., August 1996

16 *How Can Safety Nets Do More with Less? General Issues with Some Evidence from Southern Africa*, Lawrence Haddad and Manfred Zeller, July 1996

15 *Repayment Performance in Group-Based credit Programs in Bangladesh: An Empirical Analysis*, Manohar Sharma and Manfred Zeller, July 1996

14 *Demand for High-Value Secondary Crops in Developing Countries: The Case of Potatoes in Bangladesh and Pakistan*, Howarth E. Bouis and Gregory Scott, May 1996

13 *Determinants of Repayment Performance in Credit Groups: The Role of Program Design, Intra-Group Risk Pooling, and Social Cohesion in Madagascar*, Manfred Zeller, May 1996

12 *Child Development: Vulnerability and Resilience*, Patrice L. Engle, Sarah Castle, and Purnima Menon, April 1996

11 *Rural Financial Policies for Food Security of the Poor: Methodologies for a Multicountry Research Project*, Manfred Zeller, Akhter Ahmed, Suresh Babu, Sumiter Broca, Aliou Diagne, and Manohar Sharma, April 1996

10 *Women's Economic Advancement Through Agricultural Change: A Review of Donor Experience*, Christine Peña, Patrick Webb, and Lawrence Haddad, February 1996

9 *Gender and Poverty: New Evidence from 10 Developing Countries*, Agnes R. Quisumbing, Lawrence Haddad, and Christine Peña, December 1995

8 *Measuring Food Insecurity: The Frequency and Severity of "Coping Strategies,"* Daniel G. Maxwell, December 1995

7 *A Food Demand System Based on Demand for Characteristics: If There Is "Curvature" in the Slutsky Matrix, What Do the Curves Look Like and Why?*, Howarth E. Bouis, December 1995

6 *Gender Differentials in Farm Productivity: Implications for Household Efficiency and Agricultural Policy*, Harold Alderman, John Hoddinott, Lawrence Haddad, and Christopher Udry, August 1995

5 *Gender Differences in Agricultural Productivity: A Survey of Empirical Evidence*, Agnes R. Quisumbing, July 1995

4 *Market Development and Food Demand in Rural China*, Jikun Huang and Scott Rozelle, June 1995

3 *The Extended Family and Intrahousehold Allocation: Inheritance and Investments in Children in the Rural Philippines*, Agnes R. Quisumbing, March 1995

2 *Determinants of Credit Rationing: A Study of Informal Lenders and Formal Credit Groups in Madagascar*, Manfred Zeller, October 1994

1 *Agricultural Technology and Food Policy to Combat Iron Deficiency in Developing Countries*, Howarth E. Bouis, August 1994