# Effect of a nutrition intervention during early childhood on economic productivity in Guatemalan adults

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## Summary

**Background** **Substantial, but indirect, evidence suggests that improving nutrition in early childhood in developing countries is a long-term economic investment. We investigated the direct effect of a nutrition intervention in early childhood on adult economic productivity.**

**Methods** **We obtained economic data from 1424 Guatemalan individuals (aged 25–42 years) between 2002 and 2004. They accounted for 60% of the 2392 children (aged 0–7 years) who had been enrolled in a nutrition intervention study during 1969–77. In this initial study, two villages were randomly assigned a nutritious supplement (atole) for all children and two villages a less nutritious one (fresco). We estimated annual income, hours worked, and average hourly wages from all economic activities. We used linear regression models, adjusting for potentially confounding factors, to assess the relation between economic variables and exposure to atole or fresco at specific ages between birth and 7 years.**

**Findings** **Exposure to atole before, but not after, age 3 years was associated with higher hourly wages, but only for men. For exposure to atole from 0 to 2 years, the increase was US\$0·67 per hour (95% CI 0·16–1·17), which meant a 46% increase in average wages. There was a non-significant tendency for hours worked to be reduced and for annual incomes to be greater for those exposed to atole from 0 to 2 years.**

**Interpretation** **Improving nutrition in early childhood led to substantial increases in wage rates for men, which suggests that investments in early childhood nutrition can be long-term drivers of economic growth.**

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## Introduction

The World Bank's 2006 report Repositioning Nutrition as Central to Development: a Strategy for Large Scale Action argued for urgent and effective national programmes to prevent child malnutrition by targeting pregnancy and the first 2 years of life. This period of a child's life is thought to be the time when nutrition has the greatest effect on child health, growth, and development.[1] One argument for increasing investment in such programmes is that they drive long-term economic growth by leading to healthier and more productive adults. Several reports support this "productivity" hypothesis. In a review of five cohort studies, including the Guatemalan one in this report, Victora and colleagues[2] concluded there was evidence to link small size at birth and childhood stunting with short adult stature, reduced lean body mass, less schooling, diminished intellectual functioning, and reduced earnings.

About 200 million children in developing countries do not reach their developmental potential and are likely to do poorly in school;[3] documented risk factors for loss of potential include stunting, iodine deficiency, iron-deficiency anaemia, and inadequate cognitive stimulation.[4] Measures of human capital, such as adult stature and schooling, have been shown to be positively related to income and wealth.[5,6] Results from Guatemala show that improved nutrition in early childhood leads to better adult human capital including larger body size,[7] improved physical work capacity,[8] more schooling, and better cognitive skills.[9,10]

We are unaware of any study that has assessed the direct effects of nutrition interventions in early childhood on incomes in adulthood. Thus, we aimed to address this gap by analysis of data from Guatemala to estimate the effect of exposure to nutritional supplements in early childhood on incomes more than 25 years later.

## Methods

### Study participants and procedures

Between 1969 and 1977, the Institute of Nutrition of Central America and Panama (INCAP) undertook a study of the effect of improved protein intakes on physical and mental development of children from four villages of mixed Spanish-Amerindian ethnic origin in Guatemala.[11] 300 rural communities of appropriate size were screened to identify villages of appropriate compactness, ethnicity and language, diet, access to health-care facilities, demographic characteristics, nutritional status, and degree of physical isolation. From these, four villages were selected for the study.

Two villages, one from each pair matched on population size, were randomly assigned in March 1969 to receive a nutritious supplement called atole. Atole is a gruel-like drink made from Incaparina (a vegetable protein mixture), dry skimmed milk, and sugar that provided 6·4 g protein and 380 kJ (91 kcal) energy per 100 mL. In the other two villages, residents were given fresco, a drink that contains no protein, and 138 kJ (33 kcal) per 100 mL from sugar. From October, 1971, both supplements were fortified with micronutrients in equal concentrations by volume, sharpening the contrast in protein content. The supplements were available to all villagers twice daily throughout the study at a central location in each village. INCAP established and maintained medical services for each village.

In children younger than 7 years, participation (defined as any attendance during specified age intervals) was between 65% and 85% and varied little by village or age.[12] However, daily attendance for specified age intervals and the daily average volume of supplement consumed were higher in villages assigned to atole than in those assigned to fresco, for children younger than 3 years.[12] Protein, energy, and micronutrient intake from the supplements were also higher in atole villages.[13]

Home dietary intakes were measured at specified ages so that we could estimate the net dietary impact of the nutrition intervention via comparisons between atole and fresco villages in total nutrient intakes—ie, contributions from home diet and supplement consumption. Between the ages of 15 months and 36 months, home dietary intakes were estimated from eight 24 h surveys; children who were offered atole had a net increase of 8·7 g protein and 393 kJ (94 kcal) per day (representing 29% and 10% of total average intake, respectively) compared with children who were offered fresco.[13] The atole supplement improved child growth rates (for both sexes equally) only during the first 3 years of life.[14]

The effect of atole on growth to 3 years of age was assessed by use of the village as the unit of analysis.[13,15] Body length was similar in both groups in 1968, before the supplementation programme began. For children exposed to supplementation until 3 years of age, the mean increase in length over baseline was 2·9 cm in atole villages and 0·5 cm in fresco villages with a net change or double difference of 2·4 cm in favour of atole villages ($p<0·005$).[15] About 45% of 3-year-old children in atole and fresco villages were severely stunted (<–3 *Z* scores of the WHO National Center for Health Statistics reference) in 1969, the first year of the study. By the end of the study, the prevalence of severe stunting was reduced to less than 20% in atole villages but was unchanged in fresco villages.[13]

We resurveyed these individuals in 2002–04.[16,17] 2392 individuals had been enrolled in the original study; at follow-up, they would be aged between 25 and 42 years: 1855 (78%) were living in Guatemala; 272 (11%) had died—with most deaths from illnesses or infectious diseases in early childhood; 162 (7%) had migrated internationally; and 101 (4%) could not be traced. Of the 1855 individuals eligible for interview in 2002–04, 1113 (60%) lived in the original villages, 155 (8·4%) lived in nearby villages, 419 (22·6%) lived in or near Guatemala City, and 168 (9%) lived elsewhere in Guatemala.[16] For the 1855 eligible individuals living in Guatemala, 1571 (85%) completed at least one interview and 1424 (77%) completed the interview about economic activities. 284 (15%) did not complete an interview; two-thirds of them were known to be alive and in Guatemala, but current addresses could not be obtained. The refusal rate for at least partial participation among those contacted was low, at 5%.[16]

With a conservative definition of attrition or loss-to-follow-up that regards deaths as cases lost-to-follow-up, attrition for the collection of economic data was 40·5% (40·6% in atole villages, 40·3% from fresco villages). If instead, we assume that deaths were traced and thus part of the numerator, an approach followed by some researchers,[18] the attrition rate was 29·0% (28·2% for atole and 29·9% for fresco). These rates are similar to those reported in other cohort studies in developing countries, most of which had shorter follow-up periods.[2] Data cleaning reduced further the number of respondents included in the analyses, though the similarity of proportions in atole and fresco villages remained.

Double data entry was done with Epi-Info (version 6·04). All participants provided informed consent and the study was approved by Emory University's Institutional Review Board. We used a linear regression model to estimate the

| | Supplementation exposure during 0–24 months (n=297) | | No supplementation exposure during 0–24 months (n=305) | |
|---|---|---|---|---|
| | Atole villages (n=152) mean (SD) | Fresco villages (n=145) mean (SD) | Atole villages (n=169) mean (SD) | Fresco villages (n=136) mean (SD) |
| Age of respondent at interview (years) | 30·7 (1·71) | 30·8 (1·77) | 33·2 (5·12) | 33·7 (5·40) |
| Age of mother when respondent was born (years) | 27·4 (6·71) | 26·6 (7·16) | 27·2 (7·21) | 27·8 (6·31) |
| Maternal height (cm) | 149·2 (4·9) | 149·2 (4·4) | 149·1 (4·5) | 148·6 (4·6) |
| Grades of schooling, mother | 1·23 (1·53) | 1·63 (1·98) | 1·34 (1·56) | 1·61 (1·98) |
| Grades of schooling, father | 1·34 (2·13) | 2·34 (2·19)* | 1·15 (1·76) | 2·04 (2·25)* |
| Household wealth index score in 1967 | –3·30 (0·51) | –3·45 (0·52)* | –3·35 (0·43) | –3·46 (0·57) |
| Annual earned income (US$) | 3334 (2886) | 3117 (2532) | 3468 (3371) | 3178 (2803) |
| Annual hours worked | 2305 (858) | 2445 (887) | 2408 (957) | 2338 (897) |
| Wage rate (income earned per hour worked, US$) | 1·45 (1·14) | 1·27 (0·91) | 1·55 (1·87) | 1·52 (1·67) |

*Means were significantly different between atole and fresco subsets (within exposed or non-exposed groups).

***Table 1:*** **Characteristics of male participants**

impact of exposure to atole on incomes; the analyses also controlled for potentially confounding variables.

## Characterisation of supplement exposure and type

The original study enrolled all children under the age of 7 years at study launch (1969) and newborn infants from birth, until the study ended (1977). Children were followed up until they were 7 years old or until the study ended, whichever came first. Supplementation was provided from March 1, 1969, to Feb 28, 1977. Thus, different children were exposed to supplementation (atole or fresco) at different ages and for different periods of time. For example, only children born during or after 1969 and before February, 1974 could have been offered their village's supplementation continuously from birth to 36 months of age.

We characterised exposure to supplementation on the basis of the age of the child and the dates of operation of supplementation. Our primary interest was for exposure during 0–24 months, 0–36 months, and 36–72 months, but we also considered other windows of exposure within the 0–36 month range. The rationale for selecting 0–24 months was consensus that this is the priority target age for nutrition programmes.[1] We focused on children aged 0–36 months because this was the interval during which atole supplementation improved linear growth, and on those aged 36–72 months because atole had no effect on linear growth in this interval.[14]

For each child, we created a variable ("exposure to supplementation") that was given a value of either 1 when the respondent was offered any form of supplementation during the age range of interest (eg, 0–24 months), or 0. We also created a variable ("supplement type") that was given a value of 1 if the child lived in one of the two atole villages, or 0 if not. The interaction term between the two variables ("exposure to atole" from 0–24 months) represents the differential effect of exposure to atole compared with fresco at age 0–24 months, after subtraction of the difference between individuals exposed to atole versus fresco at other ages (ie, those coded "0" for "exposure to supplementation" from 0–24 months). The interaction term, therefore, provides an estimate of the double-difference effect of atole compared with fresco for a given exposure period, which we refer to here as the effect of exposure to atole. This same approach was followed in testing all other windows of exposure.

## Characterisation of outcomes: income, hours worked, and wage rates

We obtained information about all income-generating activities through a questionnaire that included the following topics: wage labour activities (type of work; hours, days, and months worked; and wages and fringe benefits received); agricultural activities (amount of land cultivated; crops grown; production levels and value; use of inputs; and hours, days, and months worked); and non-agricultural own-business activities (type of activity; value of goods or services provided; capital stock held; and hours, days, and months worked).[19] How these are used to calculate annual earned income, hours worked in the last year, and their quotient, the average wage rate (ie, income earned per hour worked) has been described elsewhere.[19] All monetary outcomes analysed were in US\$; to convert to US\$ we used the exchange rate prevailing during the survey period—7·9 Quetzales per US\$1—and adjusted the resulting sums to constant 2004 US\$. Virtually all (99%) men interviewed participated in at least one income-generating activity, with 80% working for a wage, 43% working on their own farms, and 28% operating their own businesses. A smaller proportion (70%) of women interviewed participated in at least one income-generating activity, with 33% working for wages, 21% working on their own farms, and 34% operating their own businesses.

We excluded from the analyses respondents (12 men and 238 women) who were not engaged in economic activities (ie, not participating in the labour market) since an hourly wage rate could not be calculated. We also excluded 41 men and 26 women who reported an extreme number of hours worked (ie, more than 12 h per day for all 365 days of the year) because of the concern that these implausibly high values would bias estimates of wage rates towards a lower value. The final samples analysed had 602 men and 505 women. For men, this sample represents 48·9% of the 1230 original male participants enrolled in the study (49·2% and 48·7% in atole and fresco villages, respectively). For women, the corresponding proportion is 43·2% (41·6% and 45·6 % in atole and fresco villages, respectively).

## Potentially confounding variables

To improve the validity, as well as the precision, of our estimates,[20] we controlled for individual, family, and community characteristics. The villages also underwent substantial socioeconomic changes during the 25 years that elapsed between the original intervention and the survey in 2002. For this reason, we also controlled for variables that capture changes and events in the community that might have affected income-generating activities and income-earning potential. The individual characteristics included sex and age, and family characteristics included the logarithm of the mother's age when the child was born, mother's and father's completed grades of schooling, the logarithm of mother's height, and an index of household wealth just before the intervention, all from the original 1969–77 study.

Village fixed effects were represented by dummy variables for three of the four villages, capturing all fixed characteristics of these localities that might affect wages and incomes. Using census and village histories, we documented key demographic, social, and economic changes.[21] From these community developmental histories, we constructed and then incorporated several "historical" variables into our analyses to control for community change, relating them to each individual's age. Proxy measures for schooling availability and quality included were a binary indicator of the availability of a permanent, cement-block structure for the primary school and student-teacher ratios in primary schools, in both cases when the individual was 7 years old. Several variables were created with age 18 years as the reference point, when most youths were entering the labour market. These included binary indicators for whether electricity was available, which could affect returns to labour in the village; the occurrence of events that might have reduced incomes such as earthquakes or other natural disasters sufficiently large to result in food aid being supplied to the village; and whether the village had increased demand for specific agricultural products (vegetables and yuquilla, a starchy tuber). Also included were the producer price for maize, the main agricultural commodity in the region, and the level of wages in the construction sector, at age 18 years, to assess the demand for labour.

### Statistical analyses

Because all three outcome variables are continuous, we used a linear regression model to assess the associations between the variables and exposure to the atole supplement, controlling for potentially confounding variables. Separate models were created for men and women. Since there were siblings in the sample, standard errors were calculated with allowance for clustering at the mother level.

We also explored whether the effect of the nutrition intervention varied by important family characteristics, such as parents' education or household wealth in 1967, just before the supplementation programme started.

The study design meant that the four villages, and not the individuals within them, were randomised to have the option of receiving the atole or fresco supplementation. The small number of villages randomised did not provide enough statistical power to estimate the effect of exposure to atole at the village level. Thus our models use individuals and not villages as the unit of analysis, since the duration and timing of exposure to the intervention for particular children depended on village of residence and year of birth. Further, our analyses controlled for potentially confounding factors.

See Online for webappendix

We include a webappendix with additional analyses. Since the analyses are based on only about half of the original sample, we implemented the correction procedure for attrition outlined by Fitzgerald and colleagues.[22,23] Also, we undertook several robustness checks, which included model specifications that are robust to outliers in the data (taking natural logarithms of the dependent variables and estimating median least absolute deviation regressions) and alternative ways of calculating the regression standard errors (the Huber-White method; clustering based on the village-birth-year cohorts; and block bootstrapping).

Data were analysed with Stata (version 9.2).


### Role of the funding source

The US National Institutes of Health and National Science Foundation, the sponsors of the study, had no role in study design, data collection, data analysis, data interpretation, or writing of the report. The corresponding author had full access to all the data in the study and had final responsibility for the decision to submit for publication.


## Results

In table 1, we show selected individual and family characteristics and measures of income-generating capacity for men according to the exposure of nutritional supplementation at 0–24 months of age. Parents from fresco-supplemented villages had more grades of schooling than those from atole villages; some of these differences were significant, underscoring the importance of controlling for such factors. The mean annual earned income and wage rates of men exposed to either supplement during 0–24 months were \$3334 and \$1·45, respectively in atole villages, compared with \$3117 and \$1·27, respectively, for the fresco villages, but these differences were not significant. Annual hours worked for those exposed to supplementation from 0–24 months were lower in atole villages than in fresco villages, but these differences also were not significant. Those not exposed to any supplementation at 0–24 months of age had higher incomes and wage rates than those exposed to supplementation at 0–24 months, perhaps because a large proportion of them were older and thus had higher earnings from longer work experience. We show results for effects of exposure to atole in women in the appendix, since they were non-significant.

Estimates from the linear regression models for men are shown in table 2 for three age intervals, 0–24 months, 0–36 months, and 36–72 months. The coefficients are the effect of atole supplementation (compared with fresco) on the income measure, after subtracting atole/fresco differences for those not exposed during this time, and controlling for individual, parental, and community characteristics.

Although point estimates suggested that atole supplementation during the first 2–3 years of life substantially increased total annual income by \$600–900, none of the estimates was significant (table 2). There was also no consistent evidence of an effect of atole supplementation on hours worked, though there was a tendency for the coefficient sign to be negative (ie, for the atole group to work fewer hours than the fresco group). The only significant effect on hours worked was for the age range 0–36 months (–421 h/year or –8 h/week; 95% CI –766 to –76). Exposure to atole before 3 years of age significantly raised wage rates by US\$0·62–0·67 per h. Exposure to atole during 36–72 months, however, was not significantly related to any of the three economic outcomes.

The webappendix contains additional results. We identified no evidence that effects of exposure to atole were linked to parental or family characteristics. Adjustment for attrition did not appreciably change the coefficients or the 95% CIs in table 2. Robustness checks that assessed the influence of outliers in the data and that estimated regression standard errors in several ways also produced results similar to those in table 2.

| | Difference in annual earned income in US$ (95% CI) | p | Difference in annual hours worked (95% CI) | p | Difference in wage rate, US$/h (95% CI) | p |
|---|---|---|---|---|---|---|
| 0–24 months | 870 (–216 to 1955) | 0·116 | –222 (–572 to 128) | 0·214 | 0·665 (0·16 to 1·17) | 0·009 |
| 0–36 months | 578 (–458 to 1613) | 0·273 | –421 (–766 to –76) | 0·017 | 0·622 (0·17 to 1·07) | 0·007 |
| 36–72 months | 329 (–836 to 1494) | 0·579 | –203 (–583 to 178) | 0·295 | 0·215 (–0·29 to 0·72) | 0·406 |

*Table 2:* **Difference between effect of atole compared with fresco supplementation during different age ranges on income measures**

## Discussion

In our study, exposure to the atole supplement during 0–36 months of age, but not during 36–72 months, significantly increased hourly wage rates in men, though not in women. For atole supplementation during 0–24 months, the corresponding increase in the hourly wage rate was US$0·67 per h, representing an increase of 46% over average wages in the sample. However, atole supplementation in early childhood did not consistently affect the number of hours men worked, although these men tended to work fewer hours. Increases in annual income for men given atole supplementation before 3 years of age could have been substantial, but were much less precisely estimated (and were not significant) than those for wage rates.

Interventions such as food supplementation could have differential effects on different portions of the population, depending on participation by specific groups as well as on the potential to benefit these groups. For example, participation might be more beneficial for households with few resources. Alternatively, those with large resources, or those with better educated parents, might appreciate more fully the potential benefits or be better able to take advantage of them. However, we did not identify any association between the effects of the supplements on income measures and parental education or socioeconomic indicators, which suggests that family background neither complemented the effect of the atole supplement nor acted as a substitute for it.

Our study had several limitations. One is that we were unable to distinguish different effects of different exposure windows within the first 36 months of life because of overlap in the study participants across the exposure windows.

A second limitation was the substantial degree of attrition. Owing to attrition or loss to follow-up (including deaths, mostly in the neonatal period and during infancy) and losses through data cleaning, our economic analyses for men were based on data from 49% of those originally enrolled in the study, with similar rates of attrition across atole and fresco villages. The linear regressions used to estimate influence of improved nutrition control for a large number of covariates, many of which, in addition to potentially affecting income, were also associated with attrition.[16] Therefore, these results already control in part for possible selectivity effects of attrition on the sample, because they control for many of the factors underlying attrition.[23] In addition, as reported in the appendix, we implemented Fitzgerald and colleagues' weighting method for addressing possible attrition bias. Our main results changed little when we did so. We interpret these findings to mean that, as found in other contexts with high attrition,[23,24] our results do not seem to have been biased by attrition.

A third limitation is that we could not control for all relevant village characteristics correlated with atole supplementation even though randomisation was at the village level. Our estimates do, however, include: a covariate for supplementation (for which we varied the exposure window across models), which captured all unmeasured events common to individuals who were similarly exposed to the supplementation programme; village-level fixed effects, which controlled for all unmeasured events that did not change over time and that were common to individuals within the same village; time-varying village characteristics measured at crucial ages for individual development (7 years and 18 years of age); and covariates for age to control for cohort effects, which captured unmeasured events common to all individuals of a given age. Thus, although there could have been other time-varying village characteristics that were correlated with atole supplementation for which we did not control, we perceive that the probability of significant bias is small.

Strengths of our study were that the original study included a nutrition intervention that was proven to have improved nutrient intakes and physical growth in children younger than 3 years, follow-up that was longer than 25 years, the high quality of the data on income, and the use of appropriate and robust statistical methods. Our analytical design used data from all birth cohorts (as opposed to only comparing participants exposed to atole and fresco during 0–24 months, for example) and it also incorporated several potentially confounding factors, many reflecting community development changes after the original intervention, to increase the validity and precision of the estimates.

The results we show are consistent with previously published findings from our follow-up study; atole supplementation during 0–24 months increased schooling by 1·2 grades for women, and in both sexes it increased reading comprehension scores and performance in the Raven's test of progressive matrices by about 17% and 8%, respectively. Why, then, did we not see effects on income measures in women? Perhaps this is due to differences between the sexes in labour force participation and in work activities. The vast majority of women in the sample engaged in low-productivity activities such as agricultural processing (indeed, for these reasons it would have been inappropriate to model men and women together) and this could explain the absence of effects.

Future research should investigate the pathways of nutritional supplementation on wage rates through physical strength, perhaps as mediated by height or lean body mass, and cognitive ability. Preliminary explorations suggest that the primary pathway is through cognitive skills, not through physical strength.[25]

Finally, our findings underscore the importance of further investigations in other settings of the long-term effects of improving early childhood nutrition on income. Although follow-up studies of nutrition interventions will be rare, other types of studies, such as follow-up studies of natural experiments, might be possible. One such study assessed the long-term effects of China's 1959–61 famine on the health and economic status of the survivors.[26] Exposure to the famine in early life (in people born between 1959 and 62) was associated with a reduction in height of 3 cm and with lower incomes and wealth. Our results from Guatemala and those from China provide the strongest confirmation available of the assertion that improving nutrition in early childhood is a long-term driver of economic growth.

#### Contributors

All authors contributed to study design, development of standard operating procedures and analytical protocols, and critical review and approval of this manuscript. JH, JRB, JAM, and RM contributed to the writing of the manuscript. JH, JRB, and JAM designed the survey instruments that collected the income data; JAM oversaw their implementation. JH and JAM undertook the statistical analysis. JRB was responsible for the specification of the representation of the intervention, and RF for coordination of the data management. RM was a researcher in the original longitudinal study, and supervised the 2002–04 follow-up study and its analyses.

#### Conflict of interest statement

We declare that we have no conflict of interest.

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## Effect of a nutrition intervention during early childhood on economic productivity in Guatemalan adults

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### Basic results for men

In table 1, we show the full results for the base specification used to explore the effect of the early childhood experimental nutritional intervention a quarter century later on annual income (US$), annual hours worked, and wages (income earned per hour worked, US$) for men. We used a linear regression model estimator and calculated standard errors that allow for clustering at the mother level. As described in the paper, we controlled for a number of potentially confounding variables, increasing the validity and precision of the estimates. These included individual characteristics, family background, place of birth, dimensions of school quality, and labour market shocks, as shown in table 1. With a 0–24 month exposure window, these results correspond to those presented in the first row of table 2 in the main paper. For example, the coefficient (869·6) on "(1) if exposed to supplementation between 0–24 m×atole," in the second row of the "Annual earned income" column of table 1, corresponds to the coefficient (870, rounded off) presented in the first row of the "Annual earned income" column of table 2 in the paper.

The variable "supplement type" (1 if atole, 0 if fresco) was functionally included as a main effect in the models in table 1 and in other tables (this in addition to being included in the interaction term); however, "supplement

| | Annual earned income (US$) | p | Annual hours worked | p | Wage rate (US$/h) | p |
|---|---|---|---|---|---|---|
| (1) if exposed to supplementation between 0–24 months | –389·5 | 0·461 | 143·5 | 0·397 | –0·4166 | 0·135 |
| (1) if exposed to supplementation between 0–24 months×atole | 869·6 | 0·116 | –221·8 | 0·214 | 0·6650 | 0·009 |
| Age | 5986 | 0·001 | 737·3 | 0·276 | 2·212 | 0·027 |
| Age$^2$ | –79·97 | 0·001 | –9·745 | 0·282 | –0·0299 | 0·022 |
| Logarithm of age of mother when child was born | –944·4 | 0·055 | –494·4 | 0·002 | –0·1045 | 0·680 |
| Mother's grades of schooling completed | 254·7 | 0·009 | 77·84 | 0·001 | 0·07267 | 0·170 |
| Logarithm of mother's height (cm) | 1190 | 0·749 | –3180 | 0·018 | 2·482 | 0·227 |
| Father's grades of schooling completed | 104·9 | 0·069 | –17·36 | 0·337 | 0·05483 | 0·056 |
| (1) if father died before child was aged 18 years | –1061 | 0·001 | 73·11 | 0·678 | –0·4114 | 0·062 |
| Household wealth index score in 1967 | 233·0 | 0·443 | 118·0 | 0·134 | 0·07386 | 0·622 |
| (1) if lived in San Juan | 1372 | 0·039 | 133·0 | 0·536 | 0·3943 | 0·281 |
| (1) if lived in Conacaste | 800·6 | 0·193 | 248·9 | 0·278 | 0·1408 | 0·654 |
| (1) if lived in Espírtu Santo | 799·5 | 0·214 | 11·61 | 0·956 | 0·4253 | 0·234 |
| (1) if village school permanent structure when child aged 7 years | 505·0 | 0·288 | –70·52 | 0·689 | 0·2888 | 0·264 |
| Village student-teacher ratio when child was aged 7 years | –21·80 | 0·144 | 1·568 | 0·785 | –0·0130 | 0·067 |
| (1) if electricity available in village when child was aged 18 years | –314·8 | 0·617 | 107·5 | 0·587 | –0·4773 | 0·200 |
| (1) if village was receiving food aid when child was aged 18 years | –1083 | 0·051 | –83·80 | 0·679 | –0·4120 | 0·132 |
| (1) if village experienced boom in vegetable prices when child was aged 18 years | –81·26 | 0·886 | –252·0 | 0·253 | 0·1074 | 0·711 |
| (1) if village experienced boom in demand for yuquilla when child was aged 18 years | 364·60 | 0·714 | –259·0 | 0·272 | 0·7519 | 0·205 |
| Producer price of maize when child was aged 18 years (in Quetzales) | 0·5412 | 0·682 | –0·1144 | 0·782 | 0·0003 | 0·614 |
| Wage in construction sector when child was aged 18 years (in Quetzales) | 1·028 | 0·010 | 0·1491 | 0·252 | 0·0004 | 0·112 |
| Constant | –111 660 | 0·005 | 6042 | 0·664 | –51·18 | 0·017 |
| Observations | 602 | | 602 | | 602 | |
| $R^2$ | 0·095 | | 0·078 | | 0·071 | |

Additional covariates included but not reported are dummy variables for observations with missing data on each of the following: maternal age, maternal schooling, maternal height, paternal schooling, and the 1967 household wealth index. p values were calculated allowing for clustering at the mother level.

***Table 1:*** **Effect of atole supplementation on annual income, hours worked, and wage rates for men: 0–24 month window of exposure to supplementation (n=602)**

type" was not represented by the single variable used in specifying the interaction but by two dummy variables corresponding to the two atole villages Conacaste (1 if Conacaste, 0 otherwise) and San Juan (1 if San Juan, 0 otherwise). To illustrate what happens when we use a single dummy to represent atole, we ran a regression model for annual income earned for men in which a single variable "supplement type" was substituted for the two atole village dummy variables. The resulting coefficient for income earned per h for the interaction term for atole*exposure 0–24 months was 0·6621 (0·010), nearly identical to that shown in table 1 but estimated with less precision. Although we get similar answers through these two specifications, we prefer to use the village dummy approach because it is analytically better since it also incorporates village fixed effects (not just fixed effects aggregated by atole/fresco).

In table 2, we show the effects of varying the length of the window of exposure in men, extending the results shown in table 2 in the main paper to include three additional exposure windows, 6–24 months, 0–30 months, and 6–36 months. Slight variations in the window of exposure during the first 3 years of life do not alter the principal finding of the paper, that early childhood nutrition led to a large and significant increase in wages for men.

| | Difference in annual earned income in US$ (95% CI) | p | Difference in annual hours worked (95% CI) | p | Difference in wage rate, US$/h (95% CI) | p |
|---|---|---|---|---|---|---|
| 0–24 months | 870 (–216 to 1955) | 0·116 | –222 (–572 to 128) | 0·214 | 0·665 (0·16 to 1·17) | 0·009 |
| 6–24 months | 773 (–339 to 1886) | 0·172 | –72 (–406 to 261) | 0·670 | 0·553 (0·02 to 1·08) | 0·041 |
| 0–30 months | 681 (–359 to 1720) | 0·199 | –276 (–606 to –54) | 0·100 0·295 | 0·562 (0·09 to 1·03) | 0·020 |
| 0–36 months | 578 (–458 to 1613) | 0·273 | –421 (–766 to –76) | 0·017 | 0·622 (0·17 to 1·07) | 0·007 |
| 6–36 months | 491 (–590 to 1572) | 0·373 | –276 (–611 to –59) | 0·106 | 0·517 (0·03 to 1·00) | 0·037 |

*Table 2:* **Difference between effect of atole supplementation, compared to fresco, for different windows of exposure within the 0–36 month interval on annual income, hours worked, and wage rates for men (n=602)**

| | Supplementation exposure during 0–24 m (n=225) | | No supplementation exposure during 0–24 m (n=280) | |
|---|---|---|---|---|
| | Atole villages (n=119) mean (SD) | Fresco villages (n=106) mean (SD) | Atole villages (n=137) mean (SD) | Fresco villages (n=143) mean (SD) |
| Age of respondent at interview (years) | 30·9 (1·91) | 30·5 (1·93) | 34·4 (4·93) | 33·8 (5·17) |
| Age of mother when respondent was born (years) | 27·6 (6·92) | 26·9 (7·19) | 27·6 (7·09) | 27·6 (7·03) |
| Maternal height (cm) | 149·2 (5·10) | 148·9 (5·06) | 149·3 (4·53) | 149·0 (4·71) |
| Grades of schooling, mother | 1·15 (1·54) | 1·27(1·62) | 0·98(1·46) | 1·61 (1·85)* |
| Grades of schooling, father | 1·50(1·99) | 2·01(2·20) | 0·99(1·54) | 2·31 (2·23)* |
| Household wealth index score in 1967 | –3·37(0·43) | –3·50(0·52) | –3·35(0·51) | –3·46 (0·54) |
| Student-teacher ratio when child was aged 7 years | 41·2 (6·26) | 34·7 (5·27)* | 43·6 (8·1) | 40·2 (12·96)* |
| Annual earned income (US$) | 1376 (1637) | 1347 (1692 | 1298 (2116) | 1316 (1552) |
| Annual hours worked | 1314 (1132) | 1415 (1181) | 1348 (1185) | 1561 (1214) |
| Wage rate (income earned per hour worked, US) | 1·23 (1·23) | 1·23(2·30) | 1·15(2·02) | 0·97 (1·09) |

*Means were significantly different across atole and fresco (within exposed or non-exposed groups).

*Table 3:* **Characteristics of female participants**

## Basic results for women

In the main report, we indicated that we noted no significant effects of exposure on annual income, hours worked, or wage rates for women. In tables 3–5, we show these results. There were 2392 children in the original study, 1162 of whom were female. Of these, 769 (66%) were interviewed for the economic parts of the survey in 2002–04 and 281 (24%) were not interviewed (with 112 [10%] having died by then, most in early childhood). The results reported here are based on the sample of 505 (66% of the 769 interviewed) women who reported working for wage or in-kind income (or were not dropped from the analysis for reasons described in the paper); unsurprisingly, labour force participation was much lower for women than for men, as reported elsewhere.[1] The sample of women represents 43·5% (41·6% and 45·6%, respectively, in atole and fresco villages) of the original sample. Descriptive statistics for these women are shown in table 3. In table 4, we present results for women parallel to those in table 1 (for the 0–24 month exposure window) and in table 5, results for women using other windows of exposure. We noted no significant effects for women of exposure to atole on these outcomes for any of the windows of exposure considered.

## Extensions to basic results for men

In table 6, we present the results for men when we included simultaneously two different exposure periods; exposure to the supplementation between 0–36 months and the interaction term between this variable and supplement type and exposure to the supplementation between ages 36–72 months and the interaction term between this variable and supplement type. This specification assesses whether there is a benefit to providing atole from 36–72 months with the effect of atole exposure at 0–36 months held constant, and vice versa. There is no evidence that conditional on exposure to atole before 36 months, exposure from 36–72 months affected income, hours worked, or wage rates.

In table 7, we present results interacting exposure to atole from 0–24 months (as defined in the paper: the interaction between being exposed to any type of supplementation from 0–24 months×exposed to atole from 0–24 months) with familial characteristics for men. Interventions such as the food supplementation programme examined here may have differential effects for different parts of the population, depending on take-up as well as on the potential to benefit different groups. For example, they may prove more beneficial for households with relatively fewer resources. Alternatively, those with greater resources, or those with better-educated parents, might better appreciate the potential benefits or be better able to take advantage of them. We explored such possible

| | Annual earned income (US$) | p | Annual hours worked | p | Wage rate (US$/h) | p |
|---|---|---|---|---|---|---|
| (1) if exposed to supplementation between 0–24 months | 490·4 | 0·051 | 75·19 | 0·684 | 0·6413 | 0·041 |
| (1) if exposed to supplementation between 0–24 months×atole | –396·6 | 0·409 | –291·4 | 0·285 | –0·0394 | 0·901 |
| Age | 1002 | 0·371 | –629·3 | 0·515 | 1·200 | 0·176 |
| Age² | –11·13 | 0·466 | 9·012 | 0·419 | –0·0133 | 0·253 |
| Logarithm of age of mother when child was born | 283·5 | 0·309 | 385·2 | 0·062 | 0·1142 | 0·589 |
| Mother's grades of schooling completed | 51·75 | 0·353 | 11·93 | 0·708 | 0·0505 | 0·343 |
| Logarithm of mother's height (cm) | 950·1 | 0·724 | 241·9 | 0·888 | 0·0830 | 0·969 |
| Father's grades of schooling completed | 115·6 | 0·031 | 61·55 | 0·042 | 0·0522 | 0·313 |
| (1) If father died before child was aged 18 years | –442·3 | 0·083 | 87·72 | 0·759 | 0·0181 | 0·977 |
| Household wealth index score in 1967 | 151·9 | 0·476 | 145·7 | 0·361 | 0·0262 | 0·868 |
| (1) If lived in San Juan | 739·4 | 0·146 | 153·2 | 0·592 | 0·4985 | 0·217 |
| (1) If lived in Conacaste | –169·9 | 0·661 | –211·7 | 0·507 | –0·0768 | 0·795 |
| (1) If lived in Espírtu Santo | –280·1 | 0·508 | 61·60 | 0·826 | –0·3980 | 0·182 |
| (1) If village school permanent structure when child was aged 7 years | 583·6 | 0·128 | 39·83 | 0·861 | 0·5919 | 0·068 |
| Village student-teacher ratio when child was aged 7 years | –9·926 | 0·283 | –4·534 | 0·504 | –0·0037 | 0·611 |
| (1) If electricity available in village when child was aged 18 years | 627·4 | 0·122 | 242·9 | 0·365 | 0·3951 | 0·333 |
| (1) If village was receiving food aid when child was aged 18 years | 588·6 | 0·148 | 416·7 | 0·148 | –0·1060 | 0·742 |
| (1) If village experienced boom in vegetable prices when child was aged 18 years | 644·5 | 0·180 | 283·1 | 0·376 | 0·5140 | 0·242 |
| (1) If village experienced boom in demand for yuquilla when child was aged 18 years | –1173 | 0·064 | –730·1 | 0·015 | –0·2127 | 0·639 |
| Producer price of maize when child was aged 19 years (in Quetzales) | 0·4685 | 0·626 | –0·6502 | 0·334 | 0·0007 | 0·372 |
| Wage in construction sector when child was aged 18 years (in Quetzales) | 0·2488 | 0·201 | –0·0119 | 0·943 | 0·0003 | 0·112 |
| Constant | –27 357 | 0·302 | 10 086 | 0·624 | –27·08 | 0·192 |
| Observations | 505 | | 505 | | 505 | |
| $R^2$ | 0·073 | | 0·057 | | 0·050 | |

Additional covariates included but not reported are dummy variables for observations with missing data on each of the following: maternal age, maternal schooling, maternal height, paternal schooling, and the 1967 household wealth index.

*Table 4:* **Effect of atole supplementation on annual income, hours worked, and wage rates for women: 0–24 month window of exposure to supplementation (n=505)**

| | Difference in annual earned income (US$) (95% CI) | p | Difference in annual hours worked (95% CI) | p | Difference in wage rate (US$/h) (95% CI) | p |
|---|---|---|---|---|---|---|
| 0–24 months | –397 (–1340 to 546) | 0·409 | –291 (–827 to 244) | 0·285 | –0·039 (–0·66 to 0·58) | 0·901 |
| 6–24 months | –407 (–1379 to 566) | 0·411 | –216 (–743 to 311) | 0·422 | –0·045 (–0·67 to 0·58) | 0·888 |
| 0–30 months | –279 (–1162 to 604) | 0·534 | –190 (–701 to 321) | 0·465 | 0·130 (–0·49 to 0·75) | 0·862 |
| 0–36 months | –265 (–1177 to 647) | 0·568 | –33 (–571 to 505) | 0·903 | –0·021 (0·67 to 0·64) | 0·951 |
| 6–36 months | –287 (–1195 to 624) | 0·535 | 45 (–475 to 564) | 0·866 | –0·064 (–0·71 to 0·58) | 0·846 |

See table 2 and table 4 for full results for the three regressions in the first row.

*Table 5:* **Difference between effect of atole supplementation, compared with fresco, during different windows of exposure within the 0–36 month interval on annual income, hours worked, and wage rates for women (n=505)**

| | Difference in annual earned income (US$) (95% CI) | p | Difference in annual hours worked (95% CI) | p | Difference in wage rate (US$/h) (95% CI) | p |
|---|---|---|---|---|---|---|
| 0–36 months | 543 (–526 to 1612) | 0·318 | –404 (–750 to –59) | 0·022 | 0·601 (0·14 to 1·06) | 0·011 |
| 36–72 months | 318 (–849 to 1486) | 0·592 | –179 (–555 to 197) | 0·350 | 0·183 (–0·32 to 0·69) | 0·477 |

Specification as per table 1 but with exposure from 0–36 months and 36–72 months, and their interactions with atole (presented in the table) included in the regression.

*Table 6:* **Effect of atole supplementation on annual income, hours worked, and wage rates in analyses that include two windows of exposure in the model (0–36 and 36–72 months) for men (n=602)**

heterogeneous effects with respect to three variables likely to influence programme effectiveness: maternal schooling, paternal schooling, and household wealth at the time of the intervention. We did this by interacting these characteristics with both the exposure to supplementation variable and its interaction with atole, leaving all other aspects of the estimation the same. Only in the case of hours worked are the interaction terms jointly significant (p=0·046), but even in this case none of the individual interactions are statistically significant. We conclude that there is little evidence of such heterogeneous effects.

## Robustness checks

### Outliers

A concern in any regression analysis is whether the results are robust to outliers in the data. Our concern for such outliers is what led us to exclude from the main analyses those individuals reporting hours of work averaging more than 12 h per day for the past year. We considered various other approaches to address this concern. The first approach was to estimate the impact of exposure to atole when the dependent variables are transformed as natural logarithms. Doing so produced qualitatively similar estimates to those reported in the

| | Coefficient for annual earned income (US$) (95% CI) | Coefficient for annual hours worked (95% CI) | Coefficient for wage rate (US$/h) (95% CI) |
|---|---|---|---|
| **Exposure to atole** | | | |
| 0–24 months | 3391 (–1541 to 8323) | 262 (–1052 to 1575) | 1·78 (–0·23 to 3·79) |
| **Exposure to atole between 0–24 months ×** | | | |
| Mother's grades of schooling completed | –74 (–467 to 320) | 87 (–34 to 208) | –0·093 (–0·23 to 0·05) |
| Father's grades of schooling completed | –31 (–317 to 254) | –16 (–109 to 77) | –0·020 (–0·13 to 0·09) |
| Household wealth index score in 1967 | 652 (–679 to 1983) | 129 (–231 to 488) | 0·300 (–0·24 to 0·84) |
| F statistic on joint significance of all interaction terms | 0·64 | 2·16* | 0·61 |

*Significant p value. Specification as per table 1 with six additional covariates: an interaction between (1) if exposed to supplementation between 0–24 months and, separately, (1) if exposed to supplementation between 0–24 months×atole and each of the following: mother's grades of schooling completed, father's grades of schooling completed, and the household wealth index score in 1967.

***Table 7:*** **Effect of exposure to atole on annual income, hours worked, and wage rates for men, with interaction terms between exposure to atole and parental characteristics included as additional controls (n=602)**

main paper (see table 8): a large but insignificant positive effect on income and a smaller and insignificant negative effect on hours worked. When the outcome variable is expressed in logarithmic form, the parameter estimate approximates the percentage change in the outcome variable. In table 8, the parameter estimate of 0·372 implies a 37% increase in wages, similar to the percentage changes calculated from the information found in table 2 of the paper.

The second approach we used to assess whether the results are robust to outliers in the data was to estimate a median regression, also known as least-absolute deviations or quantile regression at the 50th percentile.[2,3] We report those results in table 9; in this case there is some evidence (p=0·075) of a positive effect on income, as well as confirmatory evidence of the effect on wage rates.

The final approach was to replicate the basic analyses, and the above two variations including the 41 men who reported excessive hours of work. While less precise, consistent with the possibility that these observations were measured with error, the qualitative findings were similar (results not shown).

All of the approaches reproduced the main finding of the paper: a positive and statistically significant impact of exposure to atole on wage rates for men.

## Calculation of the standard errors

Another robustness analysis pertains to the calculation of the standard errors. The p values (and 95% CIs) reported in the paper are based on standard errors that are calculated with allowance for clustering at the mother level.[2,4] In table 10, we compared this approach with three other methods for calculating the standard errors. First, we reported p values based on the Huber-White method, which allows for heteroscedasticity of unknown form, but not for clustering.[3,5,6] Second, we reported results based on standard errors that allow for correlations within village-birth-year cohorts. There are 64 clusters (four villages×16 different birth-year cohorts). Finally, work by Angrist and Lavy[7] and Wooldridge[4] indicates that standard corrections used here for clustering are valid

| | Logarithm of annual earned income (US$) (95% CI) | p | Logarithm of annual hours worked (95% CI) | p | Logarithm of wage rate (US$/h) (95% CI) | p |
|---|---|---|---|---|---|---|
| 0–24 months | 0·256 (–0·08 to 0·59) | 0·137 | –0·116 (–0·30 to 0·07) | 0·215 | 0·372 (–0·07 to 0·68) | 0·017 |

***Table 8:*** **The effect of atole supplementation during 0–24 months on logarithmic annual income, logarithmic annual hours worked, and logarithmic wage rates for men (n=602)**

| | Difference in annual earned income (US$) (95% CI) | p | Difference in annual hours worked (95% CI) | p | Diference in wage rate (US$/h) (95% CI) | p |
|---|---|---|---|---|---|---|
| 0–24 months | 921 (–93 to 1935) | 0·075 | –92 (–532 to 349) | 0·682 | 0·602 (0·11 to 1·09) | 0·016 |

***Table 9:*** **Difference between effect of atole supplementation, compared with fresco, from 0–24 months on annual income, annual hours worked, and wage rates for men (n=602) using a median regression with bootstrapped standard errors (1000 repetitions)**

| | Annual earned income (US$) | Annual hours worked | Wage rate (US$) |
|---|---|---|---|
| Standard errors calculated with clustering at mother level (values reported in table 2) | 0·116 | 0·214 | 0·009 |
| Standard errors calculated using Huber–White method | 0·116 | 0·236 | 0·008 |
| Standard errors calculated with clustering at birth year-village level | 0·055 | 0·196 | 0·002 |
| Standard errors calculated using block bootstrap on birth-year-village clusters (1000 repetitions) | 0·102 | 0·270 | 0·010 |

Data are p values of coefficient on exposure to atole. Specification as per table 1.

***Table 10:*** **The significance of the effect of atole supplementation from 0–24 months on annual income, hours worked, and wage rates for men using alternative standard error calculations (n=602)**

| | Annual earned income (US$) (95% CI) | p | Annual hours worked (95% CI) | p | Wage rate (US$) (95% CI) | p |
|---|---|---|---|---|---|---|
| 0–24 months | 803 (–721 to 2327) | 0·292 | –233 (–885 to 420) | 0·475 | 0·611 (–0·05 to 1·27) | 0·068 |
| 0–36 months | 717 (–732 to 2167) | 0·322 | –360 (–912 to 193) | 0·195 | 0·580 (–0·04 to 1·20) | 0·064 |
| 36–72 months | –897 (–2510 to 715) | 0·267 | –220 (–727 to 287) | 0·385 | –0·322 (–1·11 to 0·47) | 0·414 |

Specification as per table 1 but using birth year-village cell means for all variables.

***Table 11:*** **Difference between effect of atole supplementation, compared with fresco, for different exposure windows on annual income, hours worked, and wage rates for men, using birth year-village cell means as observations (n=64)**

only when the number of groups or clusters is large. (For this reason, we do not cluster at the village level since the number of clusters would equal four.)

Therefore, as a further check, we report results using block bootstrapped standard errors.[8] We constructed a bootstrap sample by drawing with replacement 64 matrices consisting of outcomes and regressors, one observation from each village-birth-year cohort. We ran the regressions on this sample, obtained the estimated coefficients, and then replicated this procedure 1000 times, calculating the standard errors based on the empirical distribution of the 1000 coefficient estimates. For both total income and wage rates, all three approaches yielded smaller standard errors (and thus smaller p values) than those reported in the paper; consequently, the effect on wage rates remains significant, the effect on annual income approaches $p<0\cdot1$, and there remains little evidence of a significant impact on hours worked.

A fourth approach to assessing the robustness of our results to various methods of calculating the standard errors is to aggregate all covariates up to their group means and carry out estimation on the average data.[9,4] The cost of this approach is a considerable loss in degrees of freedom as the sample size drops from 611 to 64 observations. Parameter estimates for the 0–24 month and 0–36 month windows shown in table 11 are similar to those in table 2; parameter estimates for the 36–72 month window differed in the case of estimates for annual earned income and wages. Despite the massive drop in degrees of freedom, the coefficient for wages was significant at the 10% level for the 0–24 month ($p=0\cdot068$) and 0–36 month ($p=0\cdot064$) windows.

We conclude that the significance of the estimate of impact of the exposure to atole on average wages for men is robust to use of other methods of calculating standard errors.

| | Model 1 (1) if in sample | | Model 2 (1) if in sample | |
|---|---|---|---|---|
| | Coefficient | p | Coefficient | p |
| (1) If exposed to supplementation between 0–24 months | 0·01294 | 0·849 | 0·04117 | 0·570 |
| (1) If exposed to supplementation between 0–24 months×atole | –0·06471 | 0·380 | –0·06703 | 0·376 |
| Age | –0·19152 | 0·500 | –0·20708 | 0·483 |
| Age$^2$ | 0·00273 | 0·471 | 0·00306 | 0·438 |
| Logarithm of age of mother when child was born | –0·19437 | 0·002 | –0·13882 | 0·031 |
| Mother's grades of schooling completed | 0·018238 | 0·095 | 0·01573 | 0·156 |
| Logarithm of mother's height (cm) | –0·46812 | 0·429 | –0·44100 | 0·452 |
| Father's grades of schooling completed | –0·00718 | 0·349 | –0·00653 | 0·425 |
| (1) If father died before child was aged 18 years | 0·16309 | 0·034 | 0·20916 | 0·024 |
| Household wealth index score in 1967 | –0·01453 | 0·743 | –0·03282 | 0·476 |
| (1) If lived in San Juan | 0·04956 | 0·559 | 0·02215 | 0·804 |
| (1) If lived in Conacaste | –0·08479 | 0·315 | –0·09269 | 0·299 |
| (1) If lived in Espírtu Santo | 0·02084 | 0·803 | –0·03772 | 0·666 |
| (1) If village school permanent structure when child was aged 7 years | –0·00664 | 0·923 | –0·00264 | 0·970 |
| Village student-teacher ratio when child was aged 7 years | 0·00135 | 0·547 | 0·00106 | 0·648 |
| (1) If electricity available in village when child was aged 18 years | 0·04272 | 0·610 | 0·04279 | 0·628 |
| (1) If village was receiving food aid when child was aged 18 years | –0·02219 | 0·779 | 0·01915 | 0·815 |
| (1) If village experienced boom in vegetable prices when child was aged 18 years | 0·15561 | 0·066 | 0·15090 | 0·090 |
| (1) If village experienced boom in demand for yuquilla when child was aged 18 years | –0·06511 | 0·548 | –0·04002 | 0·716 |
| Producer price of maize when child was aged 18 years (in Quetzales) | 0·00018 | 0·300 | 0·00018 | 0·327 |
| Wage in construction sector when child was aged 18 years (in Quetzales) | –0·00005 | 0·351 | –0·00005 | 0·381 |
| (1) If mother alive when child was aged 7 years | .. | .. | –0·30783 | 0·077 |
| (1) If father alive when child was aged 7 years | .. | .. | 0·02717 | 0·857 |
| (1) If lived with both mother and father in 1975 | .. | .. | –0·02655 | 0·621 |
| (1) If lived with both mother and father in 1987 | .. | .. | 0·09422 | 0·030 |
| (1) If mother alive in 2002 | .. | .. | 0·15633 | 0·006 |
| (1) If father alive in 2002 | .. | .. | 0·09396 | 0·062 |
| (1) If mother living in original village in 2002 | .. | .. | 0·10215 | 0·088 |
| (1) If father living in original village in 2002 | .. | .. | 0·00796 | 0·877 |
| Logarithm of number of siblings in survey | .. | .. | –0·31743 | <0·001 |
| (1) If any sibling re-interviewed in 2002–04 | .. | .. | 0·45428 | <0·001 |
| Chi-square statistic on variables in model 2 only | .. | .. | 97·5 | <0·001 |
| Model $\chi^2$ statistic | 105·3 | <0·001 | 217·2 | <0·001 |
| Observations | 1230 | | 1230 | |
| Pseudo $R^2$ | 0·098 | | 0·165 | |

Sample has all 1230 male individuals who were exposed to the INCAP supplementation between 1969 and 1977. Derivatives evaluated at the mean (*dP/dx*) presented. p values were calculated allowing for clustering at the mother level.

***Table 12:*** **Attrition probits to construct weights for men used in table 13**

## Attrition bias

A possible concern in our analyses is attrition bias. As shown in Grajeda and colleagues,[10] the overall attrition in the sample is associated with several initial conditions including age, sex, and wealth levels before the intervention. Although the proportion of respondents included in the analyses is essentially the same across atole and fresco groups, the processes leading to who remained in the sample could still be different. What is of ultimate concern in this analysis is not the rate of attrition, however, but whether, and to what extent, the attrition invalidates the inferences we make from the subsample used in the analysis. To explore this, we implemented the correction procedure for attrition outlined by Fitzgerald and colleagues.[11,12] First, we estimated the probability of appearing in our analytic sample (602 men and 505 women) using information on individuals, taken from the 1969–77 study, as well as from follow-up censuses that have occurred in the villages approximately every decade, and then used those estimated probabilities to reweigh our regression results. This correction procedure incorporates adjustment for true loss to follow-up (respondents not interviewed) as well as for losses dues to data cleaning; for simplicity, we use the term "attrition-weighted estimates" when referring to these results.

| | Basic estimates | | | Attrition-weighted estimates | | |
|---|---|---|---|---|---|---|
| | Difference in annual earned income (US$) (95% CI) | Difference in annual hours worked (95% CI) | Difference in wage rate (US$/h) (95% CI) | Difference in annual earned income (US$) (95% CI) | Difference in annual hours worked (95% CI) | Difference in wage rate (US$/h) (95% CI) |
| 0–24 months | 870 (–216 to 1955) | –222 (–572 to 128) | 0·665 (0·16 to 1·17)* | 827 (–314 to 1968) | –274 (–620 to 72) | 0·674 (0·15 to 1·20)* |
| 0–36 months | 578 (–458 to 1613) | –421 (–766 to –76)* | 0·622 (0·17 to 1·07)* | 447 (–634 to 1528) | –472 (–810 to –135)* | 0·598 (0·12 to 1·08)* |
| 36–72 months | 329 (–836 to 1494) | –203 (–583 to 178) | 0·215 (–0·29 to 0·72) | 467 (–741 to 1675) | –227 (–611 to 158) | 0·306 (–0·25 to 0·86) |

*Significant p value.

***Table 13:*** **Difference between effect of atole supplementation, compared with fresco, during different exposure windows on annual income, hours worked, and wage rates using individual-level data for men (n=602)**

More specifically, we first estimated an "attrition" probit on all individuals, assigning them a "1" if they were in the analytical sample and "0" otherwise, and conditioning on all the independent variables considered in the main models, as well as an additional set of variables potentially associated with attrition, taken from the 1969–77 study as well as later study-related village censuses that occurred each decade.[13] We included several variables that reflect family structure in previous years, since these are likely to be associated with migration status—indicators of whether the parents were alive when each sample member was 7 years old and whether the sample members lived with both their parents in 1975 or in 1987. During the fieldwork in 2002–04, locating sample members was typically facilitated by having access to other family members from whom the field team could gather information. Therefore, we also included several 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 survey, and the natural logarithm of the number of siblings in the sample in each family. We do not formally have adjustments to correct for selection on unmeasured characteristics, but by including the measured characteristics indicated above, which are likely to be correlated with unmeasured characteristics, we expect that we are reducing the scope for attrition bias due to unmeasured characteristics as well. The factors described above were highly significant in predicting attrition, above and beyond the conditioning variables already included in the models (see table 12). They led to weights between 0·6 and 2·1 for men and 0·3 to 2·1 for women in the analysis samples.

Following Fitzgerald and colleagues,[11] we reweighted the estimates for men shown in table 2 in the paper; these results are shown in table 13. When we weight the corresponding regressions for women, results are similar to those in table 3 (results not shown). We interpreted these findings for men and women to mean that, as found in other contexts with high attrition[12,14] our results do not appear to be driven by attrition biases.

## Conclusions

Through consideration of a series of additional specifications under varying assumptions, we have demonstrated the stability of the results reported in the paper. Exposure to a nutritious supplement before but not after age 3 years of age had a significant, positive effect on wage rates for men but not for women.

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