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"Program Evaluation with Unobserved Heterogeneity and Selective Implementation: The Mexican *Progresa* Impact on Child Nutrition"

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PROGRAM EVALUATION WITH UNOBSERVED HETEROGENEITY AND SELECTIVE IMPLEMENTATION: THE MEXICAN *PROGRESA* IMPACT ON CHILD NUTRITION

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ABSTRACT

The assessment of the impact of social programs is the subject of lively, sometimes heated debate over whether program evaluation is best conducted *either* by comparing mean outcomes from a randomized intervention *or* by using econometric techniques with nonrandom samples. This paper contributes to this debate through an examination of *PROGRESA*, a Mexican anti-poverty and human resource program, on child nutritional status. *PROGRESA* was randomly assigned at the locality level. However, a shortage in the availability of one component– a nutritional supplement provided to preschool children –led local administrators to exercise discretion in its delivery, systematically favoring those children with poorer nutritional status. While comparisons of mean outcomes suggest that *PROGRESA* had *no* or a *negative* effect on nutritional status, estimates that control for this heterogeneity using child specific fixed effects find that *PROGRESA* had significant and substantial *positive* impacts in increasing stature. The long-term consequences of these improvements are non-trivial; its impact working through adult height alone may result in a 2.9% increase in lifetime earnings.

JEL classification: H43, I12, I38, O15
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1. INTRODUCTION

A major concern of policy makers is determining whether interventions such as social programs work as intended. Such knowledge is invaluable because only limited resources are available to advance social and policy goals, particularly in many developing countries. However, program evaluation that does not incorporate unobserved heterogeneity at various levels may lead to misunderstanding of program effectiveness (Rosenzweig and Wolpin, 1986). If program resources are allocated to favor those with poorer (better) unobserved characteristics and these characteristics are not controlled, program impact is likely to be under (over) estimated because program resources proxy in part in the estimation for the correlated unobserved characteristics. Several previous studies have reported estimates consistent with such effects being large in various contexts, both because of individual and community unobserved heterogeneities.[1]

Given this important concern, the methods for making such assessments are the subject of a lively debate. Grossman (1994, p. 177) writes, "The general consensus is that random assignment is the evaluation technique that produces the most defensible results." Newman, Rawlings and Gertler (1994) and Burtless (1995) provide further supporting arguments for "the case for randomized field trials on economic and policy research". In contrast, Heckman and Smith (1995, p. 108) remark, "While the existing regime of self-contained black-box experimental evaluations designed to produce only mean-difference estimates of program impact supports a healthy contract research industry, it contributes next to nothing to the cumulative body of social science knowledge." For this and other reasons, Heckman and Smith (1995) forcefully argue in favor of continued econometric evaluation of interventions. A danger in this sometimes heated debate is to see program evaluation in either-or terms; that is, evaluation is conducted either by comparing mean outcomes from a purported randomized intervention or by using econometric techniques to control for bias introduced by program design and behaviors with nonrandom data, without considering sufficiently what actually occurs in the specific program implementation being evaluated.

This paper provides a salutary example of some of the issues related to this debate. It considers the case of *PROGRESA*, a Mexican anti-poverty and human resource program with the aim of improving the educational, health and nutritional status of poor families. *PROGRESA* was designed to be randomly assigned to localities; a randomization design that was confirmed in subsequent analysis and utilized in the evaluation of a

[1] See Pitt, Rosenzweig and Gibbons (1993) and Gertler and Molyneaux (1994).

number of components of the PROGRESA program.[2] However, a shortage in the availability of one component of this intervention – a nutritional supplement provided to preschool children – appears to have led local administrators to exercise discretion in the delivery of this intervention, systematically favoring those children with poorer nutritional status. Consequently, when comparing outcomes expressed in terms of differences in means between treatment and control groups, the estimates indicate that *PROGRESA* had no effect on preschool child nutritional status. However, when we use child specific fixed effects regressions to control for these selection effects, we find that *PROGRESA* had significant and substantial positive impacts in increasing growth in stature by about a sixth and in reducing the probability of a child being stunted for children in the age range of 12 to 36 months, with somewhat larger effects for children in poorer communities but also those who have more educated mothers.[3]

While the methods used to evaluate this component of *PROGRESA* provide a valuable cautionary tale for program evaluation based on random assignment of communities to treatment and control groups without concern about possible selection within communities, our substantive results are of considerable interest in themselves because of the critical importance of early childhood nutrition on outcomes over the life cycle and the widespread prevalence of malnourishment among children in developing countries. Hundreds of millions of children are estimated to be malnourished, particularly in developing countries (United Nations ACC/SCN, 2000). Further, the nutrition of preschool children is widely perceived to have substantial persistent impact on their subsequent physical and mental development and on their health status as adults. These, in turn, shape their lifetime options through affecting their schooling success and their post-schooling productivity. Improvements in the nutritional status of currently malnourished infants and small children, thus, potentially may have important payoffs over decades. We show that the long-term consequences of these improvements are non-trivial. Using adult anthropometric-earnings relations from elsewhere in Latin America, we estimate that the

[2] These are summarized in Skoufias (2001).

[3] Malnutrition can take many forms. Longer-run macro or protein-energy malnutrition (PEM) is manifested in being short for one's age and sex relative to standards established for healthy populations. Shorter-run PEM often is measured by low weight-for-height, low weight for one's age and sex, or a low Body-Mass-Index. Micro nutrient deficiencies can be identified by various observational and clinical measures, depending on the exact nature of the deficiency. In Mexico, as in much of Central America (Johnston, et al., 1987, Martorell, 1995, 1999 and, Martorell, et al., 1989, 1994) the dominant form of PEM is stunting, and so in this paper, we focus on the impact of PROGRESA on stunting. We have undertaken parallel explorations in the determinants of wasting and anemia. These do not indicate that PROGRESA has had significant impact. But nutritional status is much better with regard to these indicators in the population of interest, so the lack of a significant impact of PROGRESA on them is not a matter of concern.

effects of childhood nutritional supplements found here working through adult height alone may result in a 2.9% increase in lifetime earnings.

2. PROGRESA AND PRESCHOOL CHILD NUTRITION

a) Overview

In 1997, the Federal Government of Mexico introduced the Programa de Educación, Salud y Alimentación (the Education, Health, and Nutrition Program), known by its Spanish acronym, *PROGRESA*, as part of an effort to break the intergenerational transmission of poverty. The program has a multiplicity of objectives, primarily aimed at improving the educational, health and nutritional status of poor families, and particularly of children and their mothers, in poor rural communities. *PROGRESA* provides cash transfers some of which are conditional on children's enrollment and regular school attendance and on family clinic attendance and in-kind health benefits and nutritional supplements for children up to age five and pregnant and lactating women.

The identification of households eligible to receive *PROGRESA* benefits had two stages. In Stage 1, a "marginality index" with five categories was developed for all rural communities (except those in Chiapas because of civil unrest there) using principal components for human resources, access to basic services, housing quality, and occupational structure. Those with high or very high degree of marginality were considered priorities for inclusion in *PROGRESA*. The 3,369 localities from nine states initially selected also satisfied the following criteria: (i) access to primary and secondary schools and clinics because *PROGRESA* benefits were tied in part to use of these services; (ii) population between 50 and 2,500; and (iii) and not being "extremely isolated". In Stage 2, household survey data were used to construct an index of household welfare that in turn was used to determine eligibility. Individual households could petition that they had been inappropriately classified.

The expansion of *PROGRESA* across localities and over time was determined by a planned strategy that involved the annual budget allocations and logistical complexities associated with the operation of the program in very small and remote rural communities. In phase one that began in August 1997, 140,544 households in 3,369 localities were incorporated. By phase eleven, the final phase of the original program in early 2000, the program included nearly 2.6 million families in 72,345 localities in all 31 states.[4] This constitutes around 40% of all rural families and one ninth of all families in Mexico. The total annual budget of the program in 1999 was around $777 million, equivalent to just under 20% of the Federal poverty alleviation budget or 0.2% of GDP.

b) PROGRESA and preschool child nutrition

There are at least four pathways by which participation in *PROGRESA* might affect child nutrition:

(1) Nutrition supplement or "*papilla*": The nutritional component of *PROGRESA* includes the provision of food supplements to pregnant and lactating women and to children between the ages of four months and two years and to children between two and five years if any signs of malnutrition are detected. It is important to note that these supplements also may be given to non-*PROGRESA* households if any signs of malnutrition are detected, which has the potential to bias downward the estimated impact of *PROGRESA* because some control children may be receiving this part of the treatment. These supplements are distributed to health centers through DICONSA, an operational arm of the Ministry of Social Development. The supplements have a shelf life of about one year. Mothers visit the clinic at least once a month to pick up six packets of supplements per child per month with each pack containing five doses, enough for one dose per day. The supplements constitute 20% of calorie requirements and 100% of all necessary micronutrients and have presentational and flavor characteristics that resulted in high levels of acceptability and intake (Rosado 1999 and Rosado, *et al*. 2000).

(2) Cash transfers: Some monetary transfers in the PROGESA program are motivated, as noted, by the desire to improve peoples' nutrition, particularly young children's and mothers' nutrition. There has been considerable controversy in the literature over the extent to which increased income translates into increased nutrient consumption.[5] Estimates for the *PROGRESA* sample indicate that a 10% increase in income translates into a 3 to 4.5% increase in caloric availability, with some of the rest of the incremental income used to purchase better food, perhaps including food that is richer in micro nutrients (Hoddinott, Skoufias and Washburn 2000). While there is not direct evidence on the intrahousehold distribution of nutrients in the *PROGRESA* population, studies

[4] For more details see Skoufias (2001, Section 4) and Coady (2000, Table 1).

[5] See Alderman (1986, 1993), Behrman and Deolalikar (1987, 1988), Behrman, Foster and Rosenzweig (1997), Bouis (1994), Bouis and Haddad (1992), Strauss and Thomas (1995, 1998) and Subramanian and Deaton (1996).

on other poor populations have concluded that larger shares of resources that go to mothers are directed toward child health and nutrition than of resources directed to fathers and in part for this reason *PROGRESA* directs resources to mothers.[6]

(3) Growth monitoring: A prerequisite for receiving nutrition supplements is ongoing growth monitoring of preschool children. Conventional wisdom holds that there is a high payoff to such growth monitoring because it increases substantially the probability that parents or other caregivers become aware of nutritional problems before longer-run damage occurs.

(4) Participation in the *platicas*: *PROGRESA* participants are required to attend regularly meetings at which, *inter alia*, health and nutrition issues and practices are discussed. These sessions are conducted by physicians and nurses trained in these specific topics (Rivera, et al 2000). If these meetings improve knowledge and practices related to child nutrition and health, they may increase child growth.

3. CONCEPTUAL FRAMEWORK

We conceptualize parental decisions to devote resources to improving child health as being motivated both by immediate concern about the welfare of the children and by longer-run concerns about investing in the human capital of their children, as in Becker (1967), conditional on a number of factors including related program placement and intensity. These parental concerns arise out of some mixture of altruism about their children and the possibility of sharing in some of the returns from human capital investments in their children when those children become adults. Parents may not have identical preferences regarding the use of family resources, but engage in (perhaps implicit) bargaining about such allocations, in which the strength of the bargaining position of each individual may depend on her/his access to resources including those provided by social networks and policies. Decisions that parents make, whether through bargaining or some other mechanism, about devoting resources to the children's nutrition and health are made under constraints imposed by resources that the parents have and expect to have in the future, prices in markets that they face and expect to face in the future, and community resources including governmental programs to which they have and expect to have access in the future. Expectations about the future are important because, for example, the return to investments in the health

[6] See Alderman, *et al*. (1995), Behrman (1997), Haddad and Hoddinott (1994), Haddad, *et al*. (1997), Hoddinott and Haddad (1995), Strauss and Thomas (1995) and Thomas (1990, 1993, 1994).

and nutrition of small children may not be realized for many years and the extent of those returns will depend upon what will be the future value in labor markets and other markets of increased productivity.

These concerns can be formalized by assuming maximization of intertemporally separable preference functions subject to intrahousehold decision rules, resource, market and community and policy constraints (including the allocation of resources across communities such as that described for *PROGRESA*). This process leads to reduced-form dynamic decision rules or demand relations that give some behavioral outcome in the current period as dependent on all predetermined prices and resources and on the parameters in the underlying production functions and preferences. These demand functions can be written with a vector of behavioral outcomes (Z) dependent on a vector of prices (P) and resources (R). If there are uncertainties regarding relevant future prices, policies and shocks, then the characteristics known at the time of the decision of interest regarding the distributions of those outcomes should be included instead of their realized values. A linear approximation to the demand function for a family facing prices PF and with resources RF and a vector of stochastic terms (V) is:

(1A) $Z_f = b_{PF}PF + b_{RF}RF + V$,

where the b's are the parameters to be estimated and indicate the impact of the variables for which they are coefficients on the demands for Z_f. The stochastic term in each relation includes all the effects of the stochastic terms in all of the production activities in which the family is engaged, plus other chance events that affect household decisions. One of the behavioral outcomes determined in this process is children's height. Relevant resources include characteristics of each individual in the household (e.g., innate robustness of the child under consideration), the household (e.g., overall resources of the household and household size), the community (e.g., nature of governmental programs), and past shocks (e.g., a child having had contagious diseases).

Both prices and resources may be observed or unobserved in the data, so it is useful to indicate that distinction by using superscripts ^{o} and ^{u} respectively. There is one such demand relation (or one element in the vector Z_f) for every behavioral outcome of the family, including all human resource investments and all behavioral inputs that affect human resource investments through production relations. Each of these demand relations conceptually includes the same identical right-side predetermined variables, reflecting that there may be important cross-effects.

For the particular human resource of interest in this paper, the health/height of the ith child (H_{ijt}) in the t^{th} period, this relation can be written as:

(1B) $H_{ift} = b_{PFO}PF_t^O + b_{PFU}PF_t^U + b_{RFO}RF_t^O + b_{RFU}RF_t^U + cPROG_t + V_t$,

where PROG refers to *PROGRESA* and the subscript t on the right-side variables refers to the vectors of past, current, and expected future values of the respective variables as of time t. The basic estimation problem is that there are likely to be many unobserved variables that affect child height within this framework and that may be correlated with whether a particular child in a particular household in a particular community participates in *PROGRESA*. A few examples include that households that have the option of accessing child nutrition supplements through *PROGRESA* may be more likely to do so if the child is innately less healthy or if the parents have greater concern about their children's welfare and future prospects or if the parents perceive that the future returns to human capital investments are higher or if there are not good market alternatives or social services through which human capital investments in children could be financed or if the local environment is relatively unhealthy. Likewise, *PROGRESA* may be more likely to be present in communities in which there are less good conditions for child development that are not observed in our data, given the anti-poverty emphasis in the program. If there is no control for such factors, the estimated c will be biased.

Note, however, that if *PROGRESA* were randomly assigned to children, this correlation would disappear. Alternatively, provided there are at least two observations on child health/height, and provided that access to *PROGRESA* varied over time, child-level fixed effects estimation controls for the first-order effects of all time invariant unobserved child, parental and household, market and community/policy characteristics, including those correlated with access to *PROGRESA*. Under the assumption that all the unobserved factors are fixed and that t-1 is a pre-program period, this leads to:

(1C) $H_{ift} - H_{ift-1} = b_{PFO}(PF_t^O - PF_{t-1}^O) + b_{RFO}(RF_t^O - RF_{t-1}^O) + cPROG_t + V_t - V_{t-1}$.

In this child fixed effects regression, the dependent variable becomes child growth between measurements or survey rounds. The first two right-side variables (vectors) are the changes in the transitory components of prices and of resources. These are changes in the transitory components because the permanent or longer-run components are those that are fixed over time, so they are differenced out in (1C). The coefficient of $PROG_t$ is the estimated impact of *PROGRESA* on child growth where access to this program is initiated after t-1 and varies across children. $V_t - V_{t-1}$ is the difference in stochastic shocks, and does not cause any biases. Under the assumptions to obtain (1C), the estimates obtained of the impact of *PROGRESA* are unbiased. Additionally, the logic of the model underlying (1C) includes the possibility that the *PROGRESA* impact on child growth may differ depending on the nature of the child (e.g., be bigger for innately more sickly children), the circumstances of the household (e.g., be bigger for families with more education that enables them to exploit more quickly and more effectively the new options available because of *PROGRESA*). Therefore we also explore the possibility that in (1C) the parameter c depends on individual child, parental and household and community characteristics.

4. DATA

From its inception, *PROGRESA* included a serious data collection, monitoring and evaluation component. Taking advantage of the sequential expansion of the program, in the third phase of *PROGRESA*, localities in seven south-central states (Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San Luis Potosi and Veracruz) were randomly assigned either to receive benefits starting in mid 1998 ("treatment localities") or to receive identical benefits a year or so later ("control localities"). A series of household surveys, called "ENCEL", were implemented to assess the impact of *PROGRESA* on education attainments, consumption, time allocation and gender relations as well as to provide operational feedback to *PROGRESA* staff. Based on the initial rounds of data collected, Behrman and Todd (1999) conclude that assignment was random at the community level, but that at the household level there are somewhat more rejections of random assignment than would be expected by chance. However, they also note that the magnitude of the differences in household proportions and means between treatment and control groups that are statistically significant tend to be very small.

The ENCEL surveys did not include basic nutritional data. These were collected separately by Mexico's Instituto Nacional de Salud Publica (INSP) in two rounds, August-September 1998 and October-December 1999, in six of these seven states.[7] Survey design, sampling, sample size calculations and other aspects of the collection of these data are summarized in INSP (1998). The INSP survey was designed to be a longitudinal rotating child-based sample that partially overlapped the ENCEL surveys. Only 1639 children measured in 1998 were re-measured in 1999 and only 663 children were measured in both years and can be

[7] Michoacan was excluded from the INSP survey.

linked to a household included in the ENCEL surveys, fairly evenly split between those listed in treatment versus control households.

All children in the sample, both in the treatment and in the control sub-samples, tend to live in poor households located in poor localities. Their parents are characterized by having generally low schooling, undertaking primarily agricultural work, living mostly (70%) in formal marriages, and by speaking (in 30% of the children considered here) indigenous languages. Households average over seven members who tend to live in fairly crowded houses the majority of which have dirt floors and no access to piped water. The communities in which they live have varied social services, infrastructure and transportation links with almost all having primary schools but the majority not having access to drainage or health clinics.

Table 1 lists the number of children observed in treatment and control households by age group and indicates what proportion of children listed to receive treatment actually received these supplements at least once. Note that for the age group 0-24 months – the initial intended beneficiaries of *PROGRESA*, only 61-64 per cent of eligible children actually received the supplement. By contrast, roughly half of children in the age categories 24-36 and 36-48 months received the supplement. Amongst children in these treatment households, there was no significant difference in access to the other three components of *PROGRESA*. Comparing treatment households that received the supplement, with those treatment households who did not receive this supplement, we find that there was an equal, and high, probability (0.96) that mothers took their children for growth monitoring as scheduled, equal attendance at the monthly *platicas*, and roughly equal monetary transfers.

One reason for this pattern of lowered coverage relative to what was planned related to problems in distributing these supplements. Adato et al. (2000) report that local health institutions and PROGRESA field staff raised concerns regarding their physical availability. For this reason, it is important to consider whether the allocation of these supplements was indeed random. With this in mind, Table 2a compares mean values of height-for-age Z scores (which give the number of standard deviations from the means of the NCHS/CDC/WHO reference group) prior to the intervention between children who were listed for treatment and those children who were listed and actually received these supplements. These tell a clear story, namely that children older than 12 months who received the supplement had considerably poorer nutritional status than those children who did not receive the supplement.[8] To confirm that this was the case, we estimated a probit in which we regressed child height-for-age Z scores measured prior to the introduction of the program on whether the child received this supplement (as well as other child, maternal, paternal, household and community characteristics). Table 2b shows that conditional on the characteristics noted in the parentheses, children aged 12-36 months were significantly less likely to receive these nutritional supplements.

The results of Tables 2a and 2b are consistent with a scenario in which local program administrators, facing shortages of supplements, randomly reduced availability to very young children and re-directed these supplements to slightly older preschoolers who were observed to have poor nutritional status as measured by height given age. It is also consistent with what mothers reported in the ENCEL surveys. When asked why a child aged 2-4 years was receiving these supplements, 59.7% replied that it was because the child was malnourished and 29.9% replied that it was because the doctor had determined that the child should receive it. Additionally, we note that 5% of the children age 12-36 months in eligible households in the control localities also reported having received supplements.

Being cognizant of this selective coverage is, thus, important for correctly assessing the impact of *PROGRESA* on child nutritional status. Based on program documentation, together with the initial assessment of randomization at the community and household level, a reasonable prior expectation would be that a simple comparison between control and treatment households should be sufficient to uncover the total impact of *PROGRESA*. However, because of the shortages of supplements – together with allocations to older preschoolers with low initial height given age and to a few children in control localities, such a simple comparison will tend to <u>under</u>state the total impact of *PROGRESA*. It also underscores the importance of distinguishing between children who were "listed" for treatment (preschool children in "treatment" localities who received growth monitoring and whose mothers attended *platicas* and received monetary transfers but who did receive supplements) and those children who received these supplements in addition to growth monitoring, attendance by their mothers at *platicas* and receipt of monetary transfers.

5.ESTIMATES OF *PROGRESA* IMPACT

[8] We also considered a wide range of additional child, maternal and household characteristics. There were no significant differences between groups when we compared values of weight-for-height, child age and sex, maternal age, education and height, paternal education, household land holdings and housing characteristics.

a) Estimated Impact on Child Height

We begin with estimates of relation (1B), exploring whether children's heights in October-December 1999 – a full year after the *PROGRESA* had ostensibly commenced providing supplements - were affected by whether some children had access to benefits provided by *PROGRESA*. We focus on information on whether children have received supplements, but also consider whether they are in treatment households, returning to the importance of distinguishing between being "listed" and "receiving" treatment below.

Table 3 presents three OLS cross-sectional estimates for 1999 for children 12-36 months of age in August 1998. We focus on this age group, rather than the 0-24 month age range for three reasons. First, as described in Section 4 above, it would appear that resources were shifted to this slightly older age group by local program administrators. Second, the supplements would have no direct effect on children who are exclusively breastfeeding, as are many children who are less than one year old. Third, studies in the epidemiological and nutrition literatures emphasize that children in the 12-36 month age range are especially vulnerable to malnutrition (Martorell, 1997, 1999). These children tend to have been weaned (or, if not, tend to have breastfeeding supplemented by other food) and have high nutritional requirements, but the diets commonly available to young children in developing countries after weaning have poor energy and nutrient concentrations. Young children are also very susceptible to infections because their immature immune systems fail to protect them adequately. In poor countries, foods and liquids are often contaminated and are thus key sources of frequent infections. As a result of frequent infections and poor diets, young children easily succumb to malnutrition.

The first column of Table 3 includes only whether the children in treatment households actually received treatment in the form of nutritional supplements. The second adds the logarithm of child age. The third includes a wide variety of child, maternal, household, community, and state characteristics. Across all three specifications, the estimated *PROGRESA* treatment effects are negative, though significantly nonzero at the 5% level only in the first estimate and at the 10% level in the second. If indeed children had been randomly assigned to treatment rather than to the control group, these estimates taken literally suggest that *PROGRESA* had a *negative* effect on child nutritional status.

The pattern across the estimates indicates, however, that when there are additional controls the estimates become smaller in absolute magnitude though still are negative. The relatively large and significant negative coefficient estimate in the first column, thus, may reflect that treatment is positively correlated with characteristics that are associated with less child growth, so the coefficient estimate for treatment is biased downward in the first relative to the third column.

Table 4 gives three alternative specifications, controlling in turn for community, household and individual child fixed effects, the last being akin to relation (1C). By doing so, the estimates are purged of biases arising from our finding that children were not assigned randomly to treatment versus control groups in terms of observed characteristics, importantly including their initial health and nutrition status as well as possibly other characteristics that are not directly observed in the data. These estimates also include controls for fluctuations in household consumption expenditure per household member and food prices. It should be remembered that the coefficients estimates of these variables capture the impact of possible transitory changes in these variables – which are not necessarily equivalent to the impact of changes in the value of their 'permanent' or long-run levels.

The community fixed effects are statistically significant, but controlling for community level fixed characteristics leads to little change in the estimates of no impact. However, adding controls for household fixed characteristics not only indicates that such fixed effects are significant, but reverses the sign on our representation of *PROGRESA* treatment. Most strikingly, child fixed effects regressions indicate that receiving treatment has a significant positive estimated impact on child growth - in sharp contrast to the estimates in Table 3.[9] Conditional on the individual unobserved child fixed effects that prevail in the estimates in Table 4 being the true estimates, the OLS cross-sectional estimates in Table 3 are biased downwards to the point of being negative rather than positive by negative correlations between receiving treatment and unobserved determinants of child growth.

The estimates in the last column of Table 4 indicate that those children receiving treatment experienced growth per year of about one cm greater than those who did not. This is about a sixth of the mean growth per year and about a third of the standard deviation in that growth per year that would have been experienced by

[9] It has been suggested to us that children who initially had poorer nutritional status might grow faster than initially better-nourished children. Such children are more likely to have received these supplements and so this catch-up growth becomes conflated with our representation of PROGRESA treatment. But note that "growth potential" is a child unobservable that, over the time period considered here, is swept out in our child fixed effects estimates.

those in this sample in the absence of *PROGRESA*.[10] In results not reported here – but available on request – we find that the estimated effects are about the same (a) whether there are controls for transitory fluctuations in the log of consumption expenditure per household member and in prices that as a group do not have effects that are significantly nonzero and (b) whether the treatment variable is for all children who received supplements including the 5% of the control sample who received them rather than just for those in treatment households.

b) PROGRESA's impact on different age groups

As noted above, there are *a priori* reasons based on the age of weaning and on the nature of the implementation of *PROGRESA* to focus on children 12-36 months. But it is of interest to know what happens for other age ranges. Table 5 summarizes some important aspects of alternative estimates for which the specification is identical to the right-hand column of Table 4 but the sample is limited to different age ranges for the children.

The first column replicates the results found in Table 4 for reference. The next three columns provide estimates of the impact of *PROGRESA* for the age group that was originally targeted. Particularly striking is the result for the 0-24 month age group – the parameter estimate for *PROGRESA* is half that of the 12-36 month age group and is not statistically significant. The impact on the 24-36 month age group is the largest in magnitude, and there is no impact on children initially aged more than 36 months. The last three columns give estimates for the 12-36 month age range with one or the other limit of this age range changed by six or 12 months. These estimates suggest somewhat smaller estimated impacts of *PROGRESA*, with point estimates from 0.62 to 0.84 as compared with 1.02 for the 12-36 month range.

c) Alternative representations of PROGRESA

A second question concerns the representation of the *PROGRESA* treatment variable. Table 6 distinguishes between four alternative representations:

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[10] This calculation is based on subtracting an average of 1 cm from the annual growth for every child in the sample who received treatment and then calculating the summary statistics for annual growth for the sample: mean of 7.9 cm, median of 7.4 cm, standard deviation of 3.1 cm, and range from 0.3 to 21.3 cm.

- “received treatment”, defined by being eligible for treatment and being in a treatment community and whether the household respondent indicated that the child had received supplements at least once. This is the representation used in the tables above.
- “listed treatment”, defined as residing in a household eligible for *PROGRESA* and being located in a treatment community.
- “received treatment adjusted for intensity (1)”, defined by the product of being eligible for treatment, being in a treatment community, and the number of months the child is reported in the 1999 survey as having received treatment (normalized to range from 0 to 1.0).
- “received treatment adjusted for intensity (2)”, defined by the product of being eligible for treatment, being in a treatment community, and the number of months the child is reported in the 1999 survey as having received treatment up to a maximum of 12 months (some children are reported to have received the supplement for more than 12 months) again normalized to range from 0 to 1.0.

The three “received treatment” measures are fairly highly correlated (with r at least equal to 0.9), but are much more weakly correlated with the “listed treatment” measure r between 0.53 and 0.58. Table 6 gives estimates, using a specification identical to the right-hand column of Table 4 for the age range 12-36 months, but with these four alternative representations of *PROGRESA* treatment. Recall that the difference between being “listed for treatment” and “receiving treatment” lies in differences in access to the supplement – access to the other components of *PROGRESA* was basically identical. The “listed treatment” representation is much more imprecisely estimated than the “received treatment” representations and is less that half the magnitude of the “received treatment” representation. Because “listed treatment” is a noisy representation of actually receiving *full* treatment, its use leads to a bias towards zero of the true effect. Put another way, the distinction between being “listed for treatment” and “actually receiving treatment” is critical in assessing *PROGRESA*’s impact. These results also imply that the dominant part of the *PROGRESA* treatment related to child nutrition is the provision of the supplements, with the other three components that are discussed in Section 2 much less important.

d) Interactions between PROGRESA treatment in the form of supplements and observed characteristics

The estimates that are discussed to this point assume that the impact of *PROGRESA* is the same on all children who were in the 12-36 age range in August 1998. However, our conceptual framework suggests that impact may vary depending on characteristics of the child, his/her family and the community in which s/he lives. Accordingly, we present estimates that allow the impacts of receiving the *PROGRESA* treatment in the form of supplements to vary by child, parental and household, community and state characteristics by including interactions between such characteristics and having received treatment, in addition to the direct effect of having received treatment itself.

Our first step is to include interaction terms, one variable at a time, for the child, parental and locality characteristics that were used in Table 3. The subset of these estimates in which the interaction has a coefficient estimate that is significantly nonzero at least at the 10% level is included in Panel A of Table 7. We then explore what subset of these interactions remains significantly nonzero when they are included in combination, as is summarized in Panel B of Table 7. These latter estimates are more interesting because they are more robust to the inclusion of other controls.

Three characteristics affect the magnitude of the *PROGRESA* treatment differentially. The positive estimates for two of the household characteristics - speaking an indigenous language and whether the community has a DIF food program – are associated with poverty. [11] Mean household per capita expenditure and per capita caloric consumption both are lower in communities with a DIF food program than in those without such a program.[12] By contrast, children whose mothers with at least five years of schooling – corresponding to the attainment of functional literacy – achieve greater gains in height. It is plausible that such mothers are better able to process the necessary information to benefit more from *PROGRESA*. Specifically, although use of these supplements is explained at health clinics and at *platicas*, it may be the case that more

[11] These are only suggestive because there is no significant association with many indicators of household income and wealth such as household consumption expenditure per household member, housing characteristics, etc. (and similarly with regard to many community indicators).

[12] DIF is a municipality level social program that operates in poor areas. Households cannot receive benefits from both DIF and PROGRESA. In this sample, the means for children's households from communities with versus without DIF food programs are 158 versus 173 pesos for consumption expenditure per household member (significantly different at the 15% level) and 1789 versus 2057 calories per household member (significantly different at the 5% level).

literate mothers can rely on the instructions printed on the supplement packet and so are more likely to administer the supplement correctly.

e) Estimated Impact on the Probability of Stunting

A further question of interest is whether children who had the poorest nutritional status - those who are more than two standard deviations below international norms and are thus considered stunted – benefited from PROGRESA. Among the children age 12-36 months in August 1998 who were from households eligible for *PROGRESA*, 44% were stunted. A year later 41% were stunted, including 76% of those who were stunted in 1998.

Such data suggests that *PROGRESA* may have had some small impact on reducing stunting, but it is desirable to go further than such a summary by investigating the probability of being stunted parallel to the investigation above of the determination of child growth. The available data limits the extent to which such an exploration is possible, however. In particular, for the fixed effects logit the only observations that affect the estimates are those in which children change from being stunted to nonstunted or from nonstunted to stunted between the two rounds. The numbers who did so are very small. Among the children age 12-36 months in August 1998 who were from households eligible for *PROGRESA*, 24% of those stunted in 1998 were not stunted in 1999 and 14% who were nonstunted in 1998 were stunted in 1999.

Tables 8 and 9 are parallel to Tables 3 and 4, but present fixed effects logits for stunting instead of estimates for child height. The parallel holds not only for the organization of the tables, but also for the implications of the estimates. The cross-sectional logit estimates for 1999 in Table 8 provide no support for the proposition that *PROGRESA* supplements reduced child stunting - in fact all the coefficient estimates of the received treatment variable are the wrong sign (though not significantly different from zero). If we only control for community fixed effects, we reverse this sign, so that *PROGRESA* supplements reduce the likelihood of stunting, though the impact is poorly measured. If, however, we control for household or child fixed effects, we obtain a much larger estimated impact and one that is much more precisely measured. The magnitude of this effect in the child fixed effects results is large – evaluating all coefficients at their means, receiving supplements produces a predicted probability of stunting that is only one-third that of comparable children who do not receive the supplements. These explorations suggest that (a) the cross- sectional results are misleading because there are important unobserved fixed effects and (b) once there is control for child fixed effects *PROGRESA* treatment appears to have had a significant effect on reducing child stunting as well as on increasing average child growth.

6. LONGER-RUN IMPACT

To this point we have focused on estimating the impact of *PROGRESA* nutritional supplements on child growth. But this impact is of interest in part, as noted in the introduction, because it may relate to longer-run health and nutrition status and productivity. There are at least four channels through which any component of the *PROGRESA* program that affects child health/height can affect lifetime earnings: (1) by increasing cognitive skills as an adult (conditional on grades of schooling completed) that directly affect earnings, (2) by increasing physical stature as an adult that directly affects earnings, (3) by increasing the grades of completed schooling that directly affect earnings and the age of school completion and (4) by changing the age of school completion without changing the grades of schooling completed.[13]

There is piecemeal empirical evidence of significant effects through all four of these channels for other developing countries. Alderman *et al.* (1996), Boissiere, Knight and Sabot (1985), Glewwe (1996), and Lavy, Spratt and Leboucher (1997) find positive impacts of adult cognitive achievement on wages. Behrman and Deolalikar (1989), Deolalikar (1988), Haddad and Bouis (1991), Strauss (1986), and Thomas and Strauss (1997) find positive impacts of adult height on wages and/or productivity. Grantham-McGregor, *et al.* (1997, 1999), Martorell (1995), Martorell, Rivera and Kaplowitz (1989), Haas, *et al.* (1996), Martorell (1999) and Martorell, Khan and Schroeder (1994) and report the positive impact of early childhood nutrition and cognitive development on adult nutritional status and cognitive achievement. There are hundreds of studies on the impact of grades of schooling completed on wages -- many of which are surveyed in Psacharopoulos (1994) and Rosenzweig (1995). Jamison (1986), Moock and Leslie (1986), Alderman, Hoddinott and Kinsey (2001), Behrman (1993), Leslie and Jamison (1990) and Pollitt (1990) report positive impacts of better child nutrition on progress through schooling. Lastly, Alderman *et al.* (2001), Alderman, Hoddinott and Kinsey (2001),

[13] Reductions in the age at which a given grade of school is completed increase the benefits because they permit obtaining post-schooling benefits sooner and longer. Such reductions may occur because of entry into school when younger and/or because of higher progression rates through grades while in school.

Glewwe and Jacoby (1995) and Glewwe, Jacoby and King (2001) all find that better preschool child nutrition is associated with starting school at an earlier age.

As we do not have data that would permit direct estimation of these links, for illustrative simulations we use for channel (2) estimates from Thomas and Strauss (1997) who analyzed the relationship between adult earnings and height and completed grades of schooling for male workers in another Latin American country, Brazil. They find that a one percent increase in height leads to a 2.4 percent increase in adult male earnings in a regression of log hourly wages on height and completed grades of schooling, controlling for selectivity into employment. Our estimates imply that *PROGRESA* nutritional supplements increase recipient children's height by about 1.2%.[14] Under the assumption that there is strong persistence of changes in small children's anthropometric development as argued in some of the nutritional literature (e.g., Martorell 1999, Martorell *et al.* 1989) so that the percentage changes for adults equal those that we estimate for children, the impact from this effect alone would be a 2.9% increase in lifetime earnings. Under the assumption that there is less persistence of changes in small children's anthropometric development so that the percentage changes for adults equal half of those that we estimate for children, the impact from this effect alone would be a 1.4% increase in lifetime earnings. In addition to the effect through channel (2), there is evidence from the studies noted of significant positive effects through the other channels. So these estimates, conditional on the extent of persistence from childhood to adults in anthropometric measures, probably are lower bounds on the full effects that would be obtained if all four channels were considered.

7. SUMMARY AND CONCLUSIONS

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[14] The mean height for children in the primary sample used was 80.0 cm in 1998 and 88.2 cm in 1999 and the estimated impact of PROGRESA is about 1.0 cm.

It is widely perceived in the literature on program evaluation that randomization provides the mechanism by which the most robust results of program effectiveness can be obtained. In this paper, we have considered the impact of *PROGRESA*, a large Mexican anti-poverty program. A feature of *PROGRESA* was the random assignment of localities in certain states to either receive benefits starting in 1998, or to receive these benefits one or more years later. Assessment at the locality and level basically confirms that such random assignment had taken place.

When we examine the impact of *PROGRESA* based on the presumption of randomized allocations to treatment or control groups, we find that *PROGRESA* had a negative impact on child nutrition. However, when we more closely examine how *PROGRESA* actually operated, we find several important deviations from the initial evaluation design. Not all children designated to receive nutritional supplements actually did so. Children were more likely to receive supplements if they had poor initial nutritional status and some supplements were given to slightly older preschool children. Our preferred estimates -- child fixed effects estimates that control for unobserved heterogeneity and secular trends -- indicate a significantly positive and fairly substantial program effect of the nutritional supplements. They imply an increase of about a sixth in mean growth per year for these children, a lower probability of stunting, and effects which may be somewhat larger for children from poorer communities but whose mothers are functionally literate, In addition we find that some of the other related measurement and specification concerns about the details of the evaluation estimates have substantial effects on those estimates. One important example is the difference between the impact for those who were supposed to receive treatment ("listed treatment") and those who reported receiving treatment ("received treatment"). If the former is used instead of the latter in our otherwise preferred specification, the estimated impact is cut in half and no longer statistically significant. A second important example pertains to specifying the age range for the children affected, given the biological evidence that such effects are much greater for children under 36 months than for older children and the behavioral data that most children under 12 months in rural Mexico are breastfeed and thereby not likely to be affected by the supplements. Again, the age range used for the estimates has a considerable impact. If only children under 12 months or between 36 and 60 months are used, for example, the point estimates are much smaller (negative in the latter case) and insignificant rather than positively significant and substantial with the 12-36 month age range.

These results are of considerable interest not only because of interest in the immediate welfare of those children, but also because their nutrition in this formative stage of life is widely perceived to have substantial persistent impact on their physical and mental development and on their health status as adults. Their physical and mental development, in turn, shapes their lifetime options through affecting their schooling success and their post-schooling productivity. Improvements in the nutritional status of currently malnourished infants and small children, thus, potentially may have important payoffs over decades. Even these conservative estimates may have important long-run consequences. Under the assumptions that (1) there is strong persistence of changes in small children's anthropometric development so that the percentage changes for adults equal those (are half of those) that we estimate for children and (2) that adult anthropometric-earnings relations from elsewhere in Latin America apply to the labor markets in which these children will be working as adults, the impact from this effect alone would be a 2.9% (1.4%) increase in lifetime earnings. In addition there are likely to be other effects through increased cognitive development, increased schooling, and lowered age of completing given levels of schooling through starting when younger and passing successfully grades at a higher rate. While these estimates of necessity are fairly speculative, they suggest that *PROGRESA* may be having fairly substantial effects on lifetime productivities and earnings of currently young children in poor households.

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Table 1: Sample sizes by age groups and proportion of children in PROGRESA treatment households who receive nutritional supplements

Age Group in Months in August 1998	Number of children in PROGRESA control households	Number of children in PROGRESA treatment households	Proportion of children in PROGRESA treatment households actually receiving nutritional supplements
0-12	102	108	0.64
12-24	75	76	0.61
24-36	86	83	0.52
36-48	62	69	0.57

Table 2a: Comparing height-for-age for children in PROGRESA treatment households by age group

Age group in months as of August 1998	Mean HAZ for children in PROGRESA treatment households not receiving supplement	Mean HAZ for children in PROGRESA treatment households receiving supplement	Absolute value of t statistic on differences in means
0-12	-1.32	-1.24	0.22
12-24	-1.46	-1.89	1.24
24-36	-1.76	-2.35	1.96**
12-36	-1.64	-2.10	2.04**
36-48	-1.57	-2.01	1.39

Notes: * Significant at the 10% level; ** significant at the 5% level.

Table 2b: Probit estimates of the impact of initial height-for-age Z score on likelihood child receives PROGRESA treatment by age group

Age group in months as of August 1998	Impact of initial height-for-age Z score on likelihood child receives PROGRESA treatment	Marginal impact of initial height-for-age Z score on likelihood child receives PROGRESA treatment
0-12	-0.012	-0.004
	(0.12)	
12-24	-0.364	-0.141
	(1.78)*	
24-36	-0.349	-0.135
	(2.68)**	
12-36	-0.281	-0.112
	(2.94)**	
36-48	-0.404	-0.160
	(1.65)*	

Notes:
1. Dependent variable is whether child receives *Papilla* nutritional supplement.
2. Absolute value of z statistics in parentheses. Standard errors are robust to sampling (cluster) effects.
3. * Significant at the 10% level; ** significant at the 5% level.
4. Additional variables included in these probit regressions are child age and sex; household and parental characteristics are maternal age and education, household head's age, education, and ethnicity, housing characteristics, whether community has access to a DIF food program and state dummies.
5. Sample restricted to children 0-48 months living in PROGRESA treatment households.

Table 3: OLS estimates of determinants of height in 1999 for children aged 12-36 months in 1998

Specification	(1)	(2)	(3)
Received Treatment	-1.824 (2.31)**	-1.102 (1.84)*	-0.799 (1.13)
Ln child age		18.176 (13.56)**	19.415 (15.36)*
Constant	88.65 (191.69)**	23.43 (4.95)**	13.86 (1.20)
F test on all child coefficients	-	-	120.39**
F test on for all household and parental coefficients	-	-	4.58**
F test on all price coefficients	-	-	1.21
F test on all community coefficients	-	-	1.09
F test on all state coefficients	-	-	2.62**
F test on all coefficients	5.41**	90.10**	5.88**
R squared (adjusted)	0.014	0.358	0.377

Notes:
1. Dependent variable is child height in centimeters measured in October-December 1999, that is, one year after program implementation.
2. Absolute value of t statistics in parentheses. Standard errors are robust to sampling (cluster) effects.
3. * Significant at the 10% level; ** significant at the 5% level.
4. Sample size is 320.
5. In specification (3), child characteristics are age and sex; household and parental characteristics are maternal age, education and height, household head's age, education, job type, ethnicity, marital status, housing characteristics and log per capita consumption; prices are locality median prices for tomatoes, onions, potatoes, tortillas, rice, beans, chicken, eggs, milk, sugar, cooking oil; community characteristics are whether community has access to a DIF food program, piped water or a highway.

Table 4: Fixed effects estimates of determinants of height for children aged 12-36 months in 1998

	Community fixed effects	Household fixed effects	Child fixed effects
Received Treatment	-0.112	0.739	1.016
	(0.20)	(1.60)	(2.55)**
Ln child age	16.536	14.559	6.800
	(23.32)**	(12.99)**	(4.37)**
Ln consumption per household member	-0.239	0.349	0.374
	(0.63)	(0.98)	(1.23)
Trend	1.560	2.349	5.448
	(1.51)	(2.68)**	(6.08)**
F test on all price coefficients	0.60	1.08	1.28
F test on community or household or child fixed effects	3.36**	5.70**	8.18**
R squared (overall)	0.565	0.589	0.501

Notes:
1. Dependent variable is child height in centimeters.
2. Absolute value of t statistics in parentheses.
3. * Significant at the 10% level; ** significant at the 5% level.
4. Sample size is 640.
5. In community fixed effects, additional controls are child age and sex; household and parental characteristics are maternal age, education and height, household head's age, education, job type, ethnicity, marital status, housing characteristics and log per capita consumption; prices are locality median prices for tomatoes, onions, potatoes, tortillas, rice, beans, chicken, eggs, milk, sugar, cooking oil.
6. In household fixed effects, additional controls are child age and sex, locality median prices for tomatoes, onions, potatoes, tortillas, rice, beans, chicken, eggs, milk, sugar, and cooking oil.
7. In child fixed effects, additional controls are locality median prices for tomatoes, onions, potatoes, tortillas, rice, beans, chicken, eggs, milk, sugar, and cooking oil.

Table 5: Child fixed effects estimates of determinants of height for alternative age ranges

	Age range as of August 1998 (in months)								
	12-36	0-24	0-12	12-24	24-36	>36	6-36	12-42	12-48
Received Treatment	1.02 (2.55)**	0.47 (1.38)	0.26 (0.61)	0.78 (1.29)	1.22 (2.05)**	-0.35 (0.66)	0.84 (2.55)**	0.69 (2.07)**	0.62 (1.97)**
F test on child fixed effects	8.18**	7.80**	5.19**	5.55**	9.41**	9.62**	7.48**	9.33**	9.22**
Number of observations	640	722	420	302	338	262	880	794	902

Notes:
1. Dependent variable is child height in centimeters.
2. Absolute value of t statistics in parentheses.
3. * Significant at the 10% level; ** significant at the 5% level.
4. Specification is identical to Table 4, column (2).

Table 6: Child fixed effects estimates of determinants of height for children aged 12-36 months in 1998 with alternative representations of PROGRESA treatment

	Alternative Representations of PROGRESA Treatment			
	Received Treatment	Listed Treatment	Received treatment adjusted for intensity (1)	Received treatment adjusted for intensity (2)
Treatment Coefficient	1.02 (2.55)**	0.46 (1.27)	0.68 (1.95)**	1.00 (2.20)**
F test on child fixed effects	8.18**	8.07**	7.90**	7.91**

Notes:
1. Dependent variable is child height in centimeters.
2. Absolute value of t statistics in parentheses.
3. * Significant at the 10% level; ** significant at the 5% level.
4. Specification is identical to Table 4, column (2).
5. “Received treatment adjusted for intensity are normalized so that they range from 0 to 1, thus their coefficient estimates are not directly comparable to those for the first two representations

Table 7: Summary of Interaction Effects Between Receiving Treatment and Child, Household, Community and State Characteristics

Panel A. Adding One Interaction at a Time

Variable Interacted with Received Treatment	Coefficient Estimate for Received Treatment	Coefficient Estimate for Interaction
None (same as in Table 4)	**1.016** **(2.55)****	**-**
Mother has more than 5 years of schooling	0.538 (1.12)	1.276 (1.77)*
Household head speaks indigenous language	0.605 (1.40)	1.988 (2.32)**
Household head primarily agricultural worker	-0.077 (0.13)	1.761 (2.41)**
Household head primarily self-employed	1.173 (2.87)**	-2.374 (1.68)*
Years of school, household head	-0.494 (0.82)	0.466 (3.29)*
Community has DIF food program	0.658 (1.56)	2.269 (2.40)**
Community has highway	1.426 (3.20)**	-1.678 (2.01)**
Community has piped water	1.733 (3.59)**	-1.899 (2.58)**
Hidalgo state	1.507 (3.21)**	-1.519 (1.96)**
Queretaro state	0.765 (1.87)	3.191 (2.44)**

Panel B. Adding All Interactions That Remain Significant in Combination

Variable Interacted with Received Treatment	Coefficient Estimate
Constant (i.e., simply received treatment)	-1.049 (1.13)
Mother has more than 5 years of schooling	1.767 (2.02)**
Household head speaks indigenous language	2.452 (2.94)**
Household head primarily agricultural worker	1.046 (1.31)
Household head primarily self-employed	-0.629 (0.43)
Years of school, household head	0.221 (1.39)
Community has DIF food program	2.876 (2.66)**
Community has highway	0.170 (0.17)
Community has piped water	-1.171 (1.55)
Hidalgo state	-1.162 (1.20)
Queretaro state	1.042 (0.72)

Notes:
1. Dependent variable is child height in centimeters.
2. Absolute value of t statistics in parentheses.
3. * Significant at the 10% level; ** significant at the 5% level.
4. Specification and age group is identical to Table 4, column (2) except that, one at a time, interactions with "received treatment" are added for child, parental, household, community and state variables listed in Table 4 above in Panel A (and a group of such interactions are added in Panel B). Only the interactions that are statistically significant at least at the 10% level are presented.

Table 8: Logit estimates of determinants of stunting in 1999 for children aged 12-36 Months in 1998

Specification	(1)	(2)	(3)
Received Treatment	0.282 (1.05)	0.333 (1.23)	0.066 (0.16)
Ln child age		1.189 (2.00)**	1.148 (1.49)
Constant	-0.436 (3.10)**	-4.708 (2.20)*	7.634 (1.11)
Joint test on all child coefficients	-	-	10.40**
Joint test on all household and parental coefficients	-	-	38.81**
Joint test on all price coefficients	-	-	23.75**
Joint test on all community coefficients	-	-	3.09
Joint test on all state coefficients	-	-	9.45*
Chi squared test on all coefficients	1.10	5.37	122.16**
Pseudo R2	0.003	0.012	0.285

Notes:
1. Dependent variable equals one if child is stunted when measured in October-December 1999, zero otherwise.
2. Absolute value of z statistics in parentheses. Standard errors are robust to sampling (cluster) effects.
3. * Significant at the 10% level; ** significant at the 5% level.
4. Sample size is 320.
5. In specification (3), child characteristics are age and sex; household and parental characteristics are maternal age, education and height, household head's age, education, job type, ethnicity, marital status, housing characteristics and log per capita consumption; prices are locality median prices for tomatoes, onions, potatoes, tortillas, rice, beans, chicken, eggs, milk, sugar, cooking oil; community characteristics are whether community has access to a DIF food program, piped water or a highway.

Table 9: Fixed effects logits of determinants of stunting for children aged 12-36 months in 1998

	Community fixed effects	Household fixed effects	Child fixed effects
Received Treatment	-0.803 (1.66)*	-3.082 (2.79)**	-3.537 (2.57)**
Ln child age	0.035 (0.06)	1.516 (0.90)	1.931 (0.51)
Ln consumption per household member	0.209 (0.58)	-0.153 (0.23)	-0.023 (0.03)
Trend	-0.069 (0.08)	0.779 (0.41)	1.598 (0.56)
Joint test for price coefficients	8.64	7.64	6.37
Chi squared test on all coefficients	96.73**	35.80**	33.48**

Notes:
1. Dependent variable equals one if child is stunted.
2. Absolute value of t statistics in parentheses.
3. * Significant at the 10% level; ** significant at the 5% level.
4. Sample sizes are 396, 138 and 118 for community, household and child fixed effects respectively.
5. In community fixed effects, additional controls are child age and sex; household and parental characteristics are maternal age, education and height, household head's age, education, job type, ethnicity, marital status, housing characteristics and log per capita consumption; prices are locality median prices for tomatoes, onions, potatoes, tortillas, rice, beans, chicken, eggs, milk, sugar, cooking oil.
6. In household fixed effects, additional controls are child age and sex, locality median prices for tomatoes, onions, potatoes, tortillas, rice, beans, chicken, eggs, milk, sugar, and cooking oil.
7. In child fixed effects, additional controls are locality median prices for tomatoes, onions, potatoes, tortillas, rice, beans, chicken, eggs, milk, sugar, and cooking oil.