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AN EVALUATION OF THE IMPACT OF PROGRESA ON PRESCHOOL CHILD HEIGHT

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ABSTRACT

The nutrition of preschool children is of considerable interest not only because of concern over their immediate welfare, 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. Children's physical and mental development shapes their later lives by affecting their schooling success and post-schooling productivity. Improving the nutritional status of currently malnourished preschoolers may, therefore, have important payoffs over the long term. Within rural Mexico, stunting, or short height relative to standards established for healthy populations, is the major form of protein-energy malnutrition (PEM). Low weight for height, or wasting, is much less of a problem. But stunting is symptomatic of longer-term effects of early childhood malnutrition.

One of the major components of the PROGRESA program has been directed toward improving the nutritional status of children in poor rural communities in Mexico. Cross-sectional comparisons of height for children who received PROGRESA treatment versus others who were in PROGRESA-eligible households but who did not receive treatment suggest no positive effect of PROGRESA, either on average child height or on the proportion of children who are stunted, i.e., more than two standard deviations below recognized norms. But these comparisons may be misleading because of the failure to control for unobserved child, parental and household, and market and community characteristics that may be correlated with children receiving the PROGRESA treatment,

or because of the failure to control for systematic initial differences. For example, on average, the children in the control sample tended to have better anthropometric status than children in the treatment sample.

The preferred estimates used in this study control for these factors. PROGRESA treatment is represented by those who reportedly received nutritional supplements in the treatment group (less than 60 percent of children in the treatment group) for children in the critical age range of 12 to 36 months. These estimates find significant effects of receiving PROGRESA treatment in increasing child growth and reducing the probability of child stunting. These estimates imply an increase of about one-sixth in mean growth per year for these children, and perhaps somewhat greater for children from poorer households and poorer communities but whose household heads are more educated. This is a potentially important effect: 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 percent (1.4 percent) 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 remain fairly speculative, they suggest that PROGRESA may have substantial effects on lifetime productivity and earnings of preschool children in poor households.

CONTENTS

TABLES

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1. INTRODUCTION

The nutrition of preschool children is of considerable interest not only because of concern over their immediate welfare, 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 by affecting their schooling success and post-schooling productivity. Improving the nutritional status of malnourished infants and small children may, therefore, have important payoffs over the long term.[1]

Malnutrition can take many forms. Longer-run basic macro or protein-energy malnutrition (PEM) usually is manifested in stunting, i.e., being short for one's age and sex relative to standards established for healthy populations. Shorter-run PEM often is measured by wasting (low weight-for-height), low weight for one's age and sex, or a low body-mass-index (BMI, weight-per-height squared). Micronutrient deficiencies can be identified by various observational and clinical measures, depending on the nature of the deficiency. Blood tests, for example, can identify iron deficiencies.

Table 1 summarizes information on some of the major indicators of PEM for children ages 12–36 months who are in households eligible for PROGRESA in August

[1] A number of aspects of the literature on child health and nutrition and on the impact of health and nutrition on productivity in school and subsequent to school are discussed in Adair (1999), Alderman et al. (2000), Behrman (1993, 1996), Behrman and Deolalikar (1988), Glewwe, Jacoby, and King (2000), Golden (1994), Grantham-McGregor et al. (1997), Grantham-McGregor, Fernald, and Sethuraman (1999), Johnston et al. (1987), Leslie and Jamison (1990), Martorell (1995, 1999), Martorell, Rivera, and Kaplowitz (1989), Martorell, Khan, and Schroeder (1994), Pollitt (1990), Pollitt et al. (1993), Strauss and Thomas (1995, 1998), and Young (1995). Also, see Section 6 below.

1998 (before the infant and small child nutrition component of the program was initiated in the sample from which these data are drawn).[2] The first three rows give information on Z-scores for height-for-age, weight-for-age, and weight-for-height. Z-scores give the number of standard deviations from the mean score for the same anthropometric measure conditional on age in a healthy population—in particular the NCHS/WHO standards based on a healthy U.S. population that are widely used in the literature. The last two rows give the proportions stunted and wasted, defined here as being more than two standard deviations below the mean for the standards.

Table 1—Nutrition status indicators for children aged 12–36 months in households eligible for PROGRESA in INSP 1998 August survey

Nutrition Status Indicator	Mean	Standard Deviation	95% Confidence interval		Sample size
Z score for height-for-age[a]	-1.76	1.34	-1.91	-1.62	316
Z score for weight-for-age[a]	-1.40	1.16	-1.53	-1.27	316
Z score for weight-for-height[a]	-0.41	1.09	-0.53	-0.29	316
Proportion stunted[b]	0.44	0.50	0.38	0.49	320
Proportion wasted[b]	0.06	0.23	0.03	0.08	320

[a] The Z scores give the number of standard deviations from the means for healthy children as defined in the NCS standards for the United States. The sample sizes differ slightly between the z scores and the proportions stunted and wasted because of a few outliers with very high z scores for height and weight (i.e., more that six standard deviations above the mean) that are excluded from these calculations for the z scores.

[b] These are the proportions that are more than two standard deviations below the norms (i.e., with Z scores < -2).

[2] The data are discussed below. As also is discussed below, we focus on this group of children in this study.

Note that for the PROGRESA target population in this age range, the dominant form of PEM is stunting rather than wasting. On average, these children are 1.76 standard deviations below the norm in terms of height-for-age, and 44 percent are more than 2.0 standard deviations below the norm. By contrast, they average only 0.41 standard deviations below the norm with regard to weight-for-height, and only 6 percent are more than 2.0 standard deviations below the norm (and thus wasted). Since there is a substantial incidence of stunting with its potentially damaging long-run effects, but not much wasting,[3] this report focuses on the impact of PROGRESA on stunting.[4]

After this introduction, Section 2 summarizes important aspects of the PROGRESA intervention in general and with regard to infant and small child nutrition. Section 3 presents a conceptual framework that guides the empirical exploration. Section 4 presents the data and considers bivariate patterns in nutritional status and comparisons between the treatment and control communities. Section 5 discusses our basic estimates of the impact of PROGRESA on child growth and stunting and considers some variants of those estimates. Section 6 uses our basic estimates to simulate the impact on lifetime productivity and income. Finally, Section 7 summarizes and concludes.

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[3] These same patterns, of course, are reported in the INSP (1998) summary of these data. They also are common in many other populations in the region, including for example, those in Central America by Johnston, *et al*. (1987), Martorell (1995, 1999), Martorell, Khan, and Schroeder (1994), Martorell, Rivera, and Kaplowitz (1989), and others.

[4] 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 the same concern as the question of the impact on stunting.

2. PROGRESA INTERVENTION

PROGRESA aims to provide support for families living in extreme poverty in small communities in rural Mexico. To broaden opportunities and capabilities to attain higher levels of well-being, the program attempts to raise living standards by improving opportunities to gain access to education, health, and food. Specifically, PROGRESA has the following objectives:[5]

A. To improve substantially the conditions of education, health, and nutrition of poor families, particularly children and their mothers, by providing sufficient quality services in the areas of education and health, as well as providing monetary assistance and nutrition supplements.
B. To integrate these actions so that educational achievement is not affected by poor health or malnutrition in children and young people, or because they carry out work that makes school attendance difficult.
C. To ensure that households have sufficient means and resources available so that their children can complete their basic education.
D. To encourage the responsibility and active participation of parents and all family members in improving the education, health and nutrition of children and young people.
E. To promote community participation and support for the actions of PROGRESA, so that educational and health services benefit all families in the localities where it operates, as well as uniting and promoting community

[5] The statements regarding the specific objectives and components of PROGRESA are based directly on Coady (1999) and Skoufias, Davis, and Behrman (1999).

efforts and initiatives in actions that are similar or complementary to the program.

PROGRESA comprises three components that are closely linked to each other:

(1) Educational grants to facilitate and encourage the educational aspirations of children and young people by fostering their enrollment and regular school attendance, and promoting parents' appreciation of the advantages of their children's education. At the same time, the quality of education is improved.
(2) Basic health care for all members of the family and strengthening the quality of services as well as reorienting individuals and health services toward taking preventive actions toward health care and nutrition.
(3) Monetary transfers and nutritional supplements to improve the food consumption and nutritional state of poor families, emphasizing that the purpose of this is to improve the family's food intake, particularly of children and women, who are generally the household members who suffer most from nutritional deficiencies.

Each of these components can be viewed as a form of human capital that enters directly into individual well-being, e.g., enabling one to contribute to and participate in society, but also indirectly in determining an individual's productivity and thus income-earning potential. The nature of the education-health-nutrition nexus is often seen as the root of the vicious cycle of poverty, whereby children born into poor families disproportionately experience health and nutritional problems, which diminish their potential for benefiting from whatever education they receive. Public action is necessary if this vicious cycle is to be transformed into a virtuous one.

This paper measures the impact of PROGRESA on growth of small children who, as noted in Table 1, often suffer long-run malnourishment and stunting in the target population with possible long-term negative effects on their health and productivity. There are at least four pathways by which participation in PROGRESA might affect child growth:

(1) ***Cash Transfers***. Some monetary transfers are motivated, as noted, by the desire to improve nutrition, particularly that of young children and mothers. There has been considerable controversy in the literature over the extent to which increased income translates into increased nutrient consumption.[6] Estimates for the PROGRESA sample indicate that a 10 percent increase in income translates into a 3 to 4.5 percent increase in caloric availability, with some of the rest of the incremental income used to purchase "better" food, i.e., richer in micronutrients (Hoddinott and Skoufias 2000). There remains, of course, the question of intrahousehold distribution and how much of such increments in nutrients goes to small children. While there is no direct evidence on the intrahousehold distribution of nutrients in the PROGRESA

[6] See, for example, 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).

population, studies 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, which is one reason, of course, why PROGRESA directs resources to mothers.[7]

(2) ***Growth Monitoring***. A prerequisite for receiving the nutritional supplements (see (4) below) is ongoing growth monitoring of the relevant children. Conventional wisdom holds that there is a high payoff to such monitoring because it increases substantially the probability that parents (or other caretakers) become aware of PEM and some other nutritional problems being experienced by their children at an earlier stage.

(3) ***Participation in Meetings with Health Care Specialists***. PROGRESA participants are required to attend regular meetings at which health and nutrition issues and practices are discussed, among other things. 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.

(4) ***Nutritional Supplements***. 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

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

> between two and five years if any signs of malnutrition are detected. The food supplements are produced at a production plant devoted solely to this task and then distributed to health centers through DICONSA, an operational arm of the Ministry of Social Development (SEDESOL) and the largest distributor of food in rural areas. There are about 18,000 DICONSA stores in rural areas. Mothers visit the clinic at least once a month (more if they are pregnant or have small children) 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 percent of caloric requirements and 100 percent of all necessary micronutrients. Rosado et al. (1999) found that these supplements had presentational and flavor characteristics that resulted in high levels of acceptability and intake and that intake of the supplement averaged about 87 percent of the ration.

While this paper attempts to estimate the total impact of PROGRESA, it cannot identify the separate impact of these four possible pathways. For our preferred representation of having PROGRESA treatment, we use information on whether children have received supplements as well as whether they are in treatment households. The estimates presented in Section 5 indicate that this measure of having "received treatment" is more consistent with reported child growth than is "listed treatment" (see Section 4).

3. CONCEPTUAL FRAMEWORK

This study assumes that parental decisions to devote resources to improving the health and nutritional status of their children are motivated both by immediate concern about their children's welfare and longer-term concern about investing in the human capital of their children (see Becker 1967). Parents presumably have this concern because of some mixture of altruistic concern about their children and the possibility of sharing in some of the returns from these human capital investments, e.g., being provided for in old age. Parents may not have identical preferences regarding the use of family resources, but may engage in (perhaps implicit) bargaining about such allocations, in which the strength of the bargaining position of individuals may depend on their access to resources, including those provided by social networks and policies. Decisions that parents make about devoting resources to the children's nutrition and health—whether arrived at through bargaining or some other mechanism—are under constraints imposed by resources that parents have and expect to have in the future, prices in markets they face and expect to face in the future, and community resources that they have access to and expect to have access to in the future. Expectations are important because, for example, the return to investments in the health and nutrition of small children will not all be realized for many years, and the extent of those returns will depend upon what will be the value in labor and other markets of increased productivity in the future.

These concerns can be formalized in a tractable manner by assuming maximization of intertemporally separable preference functions subject to intrahousehold decision rules, resource, market, and community constraints. 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 (from the point of view of the entity—family, or whatever—making the decisions) prices and resources and on the parameters in the underlying production functions and preferences.

On a general level, demand functions can be written as a vector of behavioral outcomes (*Z*) dependent on a vector of prices broadly-defined (*P*) and a vector of resources (*R*)—with the relevant prices and resources depending on what entity is the demand function. If there are uncertainties regarding relevant future prices, policies, and shocks, the characteristics known at the time of the decision of interest regarding the distribution 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 all the stochastic terms in all of the production activities in which the family is engaged, plus perhaps other chance events. 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), characteristics of the household (e.g., overall household resources and household size), characteristics of the community (e.g., nature of household services), and past shocks (e.g., a child having had some contagious diseases). Both prices and resources may be observed or unobserved in the data, so it is useful to indicate that distinction (using superscripts "o" and "u"). 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 (e.g., the nature of health services may affect schooling demand, and vice versa) so that a nutrition or health aspect of *PROGRESA* may affect educational (and other) outcomes, and vice versa. That means that any predetermined variable that affects any one behavioral outcome may affect any or all other behavioral outcomes.

For the particular human resource of interest in this paper, the health/height of the *i*th child (*Hijt*) 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, one of the possible household resources, that has been singled out because of its central interest for this paper, 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*. We want to obtain an estimate of the impact of *PROG* on child health/height, i.e., a good estimate of the parameter "*c*." 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. For example, a household with access to nutritional supplements through PROGRESA may be more likely to take advantage of the program if the child is innately less healthy, if the parents have greater concern about their children's welfare and future prospects, if the parents perceive that the future returns to human capital investments are higher, 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. As a result, if there is not control for such factors, the estimated "*c*" will be contaminated by omitted variable bias and may differ substantially and possibly even in sign from the true value.

The estimation strategy used in this study controls for all time invariant unobserved child, parental and household, and market and community characteristics through fixed effects estimates. Given that there are two rounds of INSP data on child nutrition—one before access to the PROGRESA nutrition components was initiated and one about a year after they were initiated, this is equivalent to estimating the first difference of relation (1*B*). Under the assumption that all the unobserved factors are fixed, this leads to

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

The dependent variable is child growth between the interview rounds. The first two right-side variables are the changes in the transitory components of prices and of resources.

These are changes in the transitory components because the permanent or longer-term components are basically fixed over time, so they are differenced out in (1*C*) (or—equivalently—controlled for in the fixed effects estimates). It is not clear that these transitory components are likely to have a significant effect on longer-run investments such as in children's human capital, but that is an empirical question explored below. The coefficient of $PROG_t$ is the estimated impact of PROGRESA on child growth (recall that $PROG_{t-1}$ is zero because the baseline survey was supposed to be conducted prior to the intervention commencing). $V_t - V_{t-1}$ is the difference in stochastic shocks, and does not cause any biases. Under the assumptions to obtain (1*C*), the estimates obtained of the impact of PROGRESA are unbiased.

Thus, this estimation strategy focuses on the fixed effects equivalent of relation (1*C*) as preferred estimates. But (1*B*) is also estimated to explore whether control for unobserved fixed factors makes any difference—and finds that it makes a considerable difference. Of further note is that the logic of the model underlying (1*C*) 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 that are poorer and have less access to markets and other public services or for families with more education—which enables them to exploit more quickly and more effectively the new options available because of PROGRESA). Therefore this paper explores the possibility that in (1*C*) the parameter "*c*" depends on individual child, parental and household, and community characteristics. Section 5 presents these estimates.

4. DATA

The basic data source for the dependent variables of interest and for some of the right-side variables is the two rounds of data collected by the *Instituto Nacional de Salud Publica* (INSP) in August–September 1998 and October–December 1999. Survey design, sampling, sample size calculations, and other aspects of the collection of these data are summarized in INSP (1998). The 1998 survey collected basic anthropometric information on approximately 4,000 children residing in six states, Guerrero, Hidalgo, Puebla, Querétero, San Luis Potosi, and Veracruz.

For this analysis, it was necessary to have usable data on children measured in both 1998 and 1999. In this context, usable means that it must be possible to link these children to their mothers (e.g., to determine whether the impact of PROGRESA varies across children whose mothers have differing levels of education), their households (e.g., to determine if the impact of PROGRESA varies across indigenous and nonindigenous parents) and their communities (e.g., to control for food prices as possible determinants of child health). Meeting all these requirements results in a data set with substantially fewer observations than that found in the raw INSP data sets.

Rivera et al. (2000) report that the 1998 survey round was designed to contain approximately 4,480 children. However, 831 children reside in localities not surveyed as part of the ENCEL surveys, which provide much of this study’s information on household characteristics. A further 380 children reside in households not in the ENCEL surveys, although they do reside in localities that were part of the ENCEL surveys. It proved impossible to match 586 children to their mothers and a further 83 children had ages in excess of 60 months or could not be uniquely identified within the household. This left a sample of 2,597 children measured in 1998 with usable data.

Next, the data were merged with the data collected in 1999. The 1999 survey round collected anthropometric data on approximately 5,000 children. However, according to INSP records (after dropping observations with missing heights, where location was miscoded, and where the child resided in Morelos state, which is not part of the ENCEL surveys), only 1,639 children measured in 1998 were remeasured in 1999. These data were merged into the 1998 sample using information on state, municipality, locality, the household identifier number (folio), and child's birth date to ensure that a child measured in 1998 was correctly matched to data collected in 1999. This produced a sample of 693 children.

Accordingly, the next step was to check why so many children were "lost" between 1998 and 1999. To start, each record of children appearing in 1998 but not in 1999 was examined. The working assumption was that information on child's residence (state, municipality, locality, and folio) was entered correctly, but there were errors in entering birth dates. This proved to be most informative. Three errors emerged: (1) minor errors in entering birth dates. For example, a birth date was recorded as September 1, 1995, in the 1998 round and September 2, 1995, in 1999; (2) reversing dates and months. For example, a child recorded as being born on September 1, 1995 (09/01/95) in 1998 is recorded as being born on January 9, 1995 (01/09/95) in the 1999 round; and (3) differences in the year of birth recorded in different survey rounds. For example, a child might have a birth date recorded as September 1, 1995 in 1998 and September 1, 1996 in 1999. Such cases became obvious upon examination of the height-for-age Z-scores across the two survey rounds. This work produced an additional 220 children for this merged data set.

In addition, some exploration was undertaken regarding the remaining lost children. It appears that a major reason for the loss of these children is age and location related. For example, of the 1,089 children aged over 36 months at the time of the 1998 survey round, only 182 were remeasured in 1999. Table 2 summarizes the percentage of children appearing in the 1998. Further, remeasured rates are far less common in

Table 2—Percentage of children appearing in both 1998 and 1999 survey rounds, by age, state, and household eligibility for PROGRESA

	Percentage of children measured in 1998 and 1999	Chi Squared Test for independence of likelihood of remeasurement by selected categories
Child age in 1998		
< 6 months	65.8%	
6-12 months	66.2	
12-18 months	38.6	
18-24 months	47.6	
24-36 months	39.6	
>36 months	16.7	
		Chi squared (d.f.=5) = 378.3*
Child resides in		
Guerrero	33.9%	
Hidalgo	35.8	
Puebla	38.7	
Queretaro	41.0	
San Luis Potosi	39.4	
Veracruz	25.5	
		Chi squared (d.f.=5) = 30.3*
Child resides in household		
Eligible for PROGRESA	21.5%	
Not eligible for PROGRESA	26.1	
		Chi squared (d.f.=1) = 0.6

Note: * differences are significant at the 5 percent level.

Veracruz than in other states. Although the likelihood of being remeasured does not vary by PROGRESA eligibility, it does vary by child height-for-age. In particular, as Table 3 indicates, children less than 36 months who were measured in both 1998 and 1999 typically had poorer height-for-age Z-scores than children who were not remeasured. This feature of the data further strengthens the importance of an estimation strategy that controls for such characteristics.

Table 3—Mean height-for-age Z-scores of children measured only in 1998 and measured in 1998 and 1999, by age group

Age group	Height-for-age Z-score, children measured only in 1998	Height-for-age Z-score, children measured in 1998 and 1999	T statistic on difference in means
6-12 months	-0.24	-0.71	3.20*
12-36 months	-1.48	-1.60	1.26
>36 months	-1.80	-1.83	0.25

Note: * Means significantly different at the 5 percent level.

Distribution of children in INSP longitudinal sample among treatment-control and eligible-noneligible categories. Table 4 summarizes the distribution of children who are in both rounds of the INSP sample among treatment versus control and eligible for PROGRESA and not eligible categories. The top panel refers to all children, of which there are 663 in households eligible for PROGRESA, fairly evenly split between those listed in treatment versus control households. The bottom panel refers to children ages 12–36 months in August 1998, which is the sample on which this analysis focuses (see below), so it is highlighted in bold. There are 320 such children, divided almost evenly between those listed to be in treatment versus those in control households.

Table 4—INSP longitudinal sample for 1998 and 1999: Numbers of children less than 60 months of age in August 1998 in PROGRESA control and treatment communities who are eligible and not eligible for PROGRESA

	Treatment	Control	Total
Full samp le (children ages 0-60 months in August 1998)			
Eligible	337	326	663
row percentage	51%	49%	100%
column percentage	*86%*	*75%*	*80%*
Not eligible	56	109	165
row percentage	34%	66%	100%
column percentage	*14%*	*25%*	*20%*
Total	393	435	828
row percentage	47%	53%	100%
column percentage	*100%*	*100%*	*100%*
Children ages 12-36 months in August 1998			
Eligible	**159**	**161**	**320**
row percentage	**50%**	**50%**	**100%**
column percentage	***90%***	***78%***	***84%***
Not eligible	17	46	63
row percentage	27%	73%	100%
column percentage	*10%*	*22%*	*16%*
Total	176	207	383
row percentage	46%	54%	100%
column percentage	*100%*	*100%*	*100%*

Representation of PROGRESA treatment. As noted in Section 2, for the preferred representation of having PROGRESA treatment, we use information on whether children have received supplements as well as whether they are in treatment households. The estimates presented in Section 5 indicate that this measure of having received treatment is more consistent with reported child growth than is listed treatment. Three important characteristics of the reports on having received treatment are now noted. First, as will be discussed in Section 5, by this definition, 50–60 percent of children in the 12–36 month age range in eligible households that are listed as receiving PROGRESA treatment (below referred to as “listed treatment”) are reported actually to have "received treatment." Second, 5 percent of the children ages 12–36 months in eligible households in the control localities report having received supplements, so there is a small downward bias in any representation of the PROGRESA treatment that is conditional on being designated as a member of a listed treatment household. Third, the proportion of children in eligible treatment households that have received supplements in fact does not seem to be concentrated in the 4–24 month age range, despite the stated intention to do so (see Section 2). The first column in Table 5 indicates that there is a slightly higher incidence of receiving supplements for children less than 24 months of age, but not a very large difference.

Age range for preferred estimates. The last two columns in Table 5 indicate that the INSP sample is concentrated on the first three to four years of life, perhaps because of the PROGRESA emphasis on this age range. Given this concentration, the stated orientation of the program toward children of less than 24 months of age, that many children less than 12 months are not yet receiving other foods to supplement breastfeeding (and those that are likely to be a selected sample for which breastfeeding is difficult), and that the previous literature suggests that the risk of faltering permanently behind in terms of long-term negative impacts of early childhood malnutrition is thought to continue until about age three (e.g., Martorell 1995, 1999), this investigation concentrates on children 12–36 months of age. (Section 5, however, explores how sensitive these estimates are to changing this age range.)

Table 5—Proportion of children in eligible treatment households who received nutrition supplements and number of children in INSP sample of eligible households, by age groups

Age group in months in August 1998	Proportion of children in eligible treatment households who received nutrition supplements	Number of children in INSP sample of eligible households	
		Treatment households	Treatment and control households
0-12	0.64	108	210
12-24	0.61	76	151
24-36	0.52	83	169
36-48	0.57	69	131
48-60	--	1	2

Basic characteristics of variables used in the analysis. The first column in Table 6 gives the means and standard deviations for all children less than 60 months of age in the INSP sample who are in PROGRESA-eligible households for groups of variables related to child characteristics, parental and household characteristics (further subdivided into mother's characteristics, household head characteristics, household and housing characteristics, resources—per-capita consumption and land owned, whether household received food help, prices, community characteristics, and states of residence. For most of the variables included, there is considerable within-sample variation. As noted in Section 1, the children tend to be stunted much more than wasted. Their parents are characterized by having generally low schooling, undertaking primarily agricultural work, living mostly (70 percent) in formal marriages, and by speaking (in 30 percent of the children considered here) indigenous languages. Households' average over seven

Table 6—Means and standard deviations for children eligible for PROGRESA less than 60 months of age and variables for which the means differ significantly between eligible treatment and control households in August 1998 for all children < 60 months and for those 12-36 months

Variables	Mean and standard deviation for all children < 60 months of age eligible for PROGRESA	Whether mean for treatment (T) significantly greater or less than that for control (C) and significance of difference[b]			
		All children < 60 months eligible for PROGRESA		Children between 12 and 36 months eligible for PROGRESA	
		Listed treatment versus control	Received treatment versus others	Listed treatment versus control	Received treatment versus others
Child characteristics					
Blood count	11.2 (2.9)				
Height (centimeters)	77.2 (11.2)		C**	C**	C*
Weight (kilograms)	9.9 (2.8)		C**		C*
Body mass index	16.5 (4.1)				
Height-for-age Z-score	-1.5 (1.4)	C**	C*		C*
Weight-for-age Z-score	-1.2 (1.3)	C***	C**		
Weight-for-height Z-score	-0.2 (1.2)				T***
Birth weight (kilograms)	3.1 (0.6)	C***	C***	C**	
Reported birth size	3.0 (1.0)				
Stunted	0.36 (0.48)	T*	T*	T*	T*
Wasted	0.05 (0.22)			C**	
Age (months)	22.2 (13.4)				C***
Sex (female)	0.52 (0.50)				
Parental and household characteristics					
Mother's Characteristics					
Blood count	12.5 (2.6)				
Height (centimeters)	148.2 (5.8)				
Weight (kilograms)	53.3 (10.5)				
Body mass index	24.2 (4.3)				
Age (years)	29.8 (6.6)	T***			
Schooling (grades)	3.6 (2.7)	C*	C*	C**	
Household head characteristics					
Sex male	0.97 (0.17)				
Marital status "open union"	0.27 (0.44)				
Marital status "married"	0.69 (0.46)			C**	
Marital status single	0.005 (0.067)				
Indigenous (speaks dialect or indigenous language)	0.30 (0.46)	C*	C*	C*	C*
Agricultural work primarily	0.67 (0.47)				
Nonagricultural worker	0.11 (0.31)				
Self-employed	0.08 (0.26)				
Age (years)	37.8 (11.3)		C*		C**
Literate	0.78 (0.41)				
Schooling (grades)	3.5 (2.6)				
Household and housing characteristics					
Household size	7.3 (2.5)				
Dirt floors	0.67 (0.47)				T**
Piped water access	0.34 (0.47)	T*	T*	T*	T*
Electricity	0.59 (0.49)	C*	C*	C*	C*
Rooms per household member	0.22 (0.11)				
Ln Per capita consumption, land owned					
Ln per capita consumption	5.01 (0.57)	T*	T*	T*	

Variables	Mean and standard deviation for all children < 60 months of age eligible for PROGRESA	Whether mean for treatment (T) significantly greater or less than that for control (C) and significance of difference[b]			
		All children < 60 months eligible for PROGRESA		Children between 12 and 36 months eligible for PROGRESA	
		Listed treatment versus control	Received treatment versus others	Listed treatment versus control	Received treatment versus others
Land owned (area)	2.9 (25.0)	T**			
Whether household received food help					
Liconsa (subsidized milk)	0.95 (0.22)	T**			
DIF (school breakfasts, food support & community kitchens)	0.89 (0.31)	T*	T*	T*	T*
Free tortillas	0.995 (0.068)				
Prices					
Tomatoes	43.2 (5.4)	C*			
Onions	30.8 (5.9)	C**		C***	
Potatoes	30.9 (7.8)				
Tortillas	12.5 (2.6)	C*	C*	C*	C*
Rice	30.7 (4.0)				
Beans	48.6 (6.2)	C*			
Chickens	94.1 (21.1)	C***		C*	
Eggs	45.1 (4.6)	C**			
Milk	30.1 (34.7)	C*	C*	C*	
Sugar	25.3 (1.8)	T*	T*	T**	
Oil	47.5 (3.4)				
Community characteristics					
DICONSA shop	0.20 (0.40)				
DIF health center	0.00 (0.00)				
DIF food	0.45 (0.50)	C*	C*	C*	C*
DIF kitchens	0.10 (0.30)			C**	
FIDELIST tortilla	0.03 (0.17)	C*	C**	C*	
IMSS clinic	0.07 (0.25)	T*			
INI shelters	0.002 (0.04)				
LICONSA milk	0.15 (0.36)	T*	T*	T*	T*
Primary school	0.94 (0.23)				
Sec de Salud clinic	0.33 (0.47)	C**		C**	
Drainage	0.06 (0.23)		T**		
Electricity	0.72 (0.45)	C*	C*	C**	C*
Piped water	0.50 (0.50)				
Highway	0.28 (0.45)				
Main road	0.90 (0.30)	T*	T**	T*	
Paved road	0.04 (0.20)	C**		C*	C**
Public transportation	0.41 (0.49)				C*
States					
Puebla	0.26 (0.44)	T*		T*	
Queretaro	0.08 (0.28)			T*	
San Luis	0.17 (0.37)				
Veracruz	0.16 (0.37)	C*	C*	C*	C*

[a] The number of observations varies among the variables because of missing observations. The maximum number of children who are eligible for PROGRESA in the sample is 663 for all children under 60 months and 320 for children between 12 and 36 months.

[b] "T" indicates that mean for treatment >mean for control (and vice versa for "C"). Significance levels are * = 5 percent, **= 10 percent and *** = 15 percent for two-sample t-tests with equal variances. A shaded box indicates no significant difference in means at the 15 percent or lower confidence level.

members who tend to live in fairly crowded houses, the majority of which have dirt floors and no access to piped water. The communities have varied social services, infrastructure, and transportation links; while almost all having primary schools, the majority do not have drainage or health clinics.

Assignment to treatment versus control samples. If households were assigned randomly to treatment versus control samples with regard to all relevant observed and unobserved determinants of child health/height, simple comparisons of child health/height would reveal the treatment impacts. PROGRESA attempted to assign communities randomly to treatment versus control communities (localities) in its basic evaluation samples. Based on the initial rounds of data collected, Behrman and Todd (1999) conclude that assignment was basically random at the community level, but that at the household level there are more rejections of random assignment than would be expected by chance. As discussed above, the INSP longitudinal data that are central for the present study are a subsample of the larger PROGRESA evaluation sample, so it is not clear to what extent these results carry over to the INSP data.

The last four columns in Table 6 summarize whether means for all the variables in the INSP sample differ significantly between the listed treatment and control children and between the received treatment and others, separately for all children less than 60 months and for children 12–36 months of age in August 1998. For many of the variables, there are not significant differences. But there are significant differences more often than would occur by chance, suggesting that assignment was not random. Moreover, in the fairly high proportion of cases in which there are significant differences for child nutrition indicators, the children in the control households almost always were initially better off than those in the treatment households. This means that a simple comparison between control and treatment households in the second round would tend to *understate* the impact of PROGRESA. For this reason it would appear that such simple cross-sectional comparisons are not always reliable. By taking the approach outlined at the end of Section 2 for the preferred estimates, this analysis will control for the basic observed and unobserved differences between the treatment and control children, including that the control sample apparently tended to start with somewhat better health and nutritional status than did the treatment sample.

5. ESTIMATES OF PROGRESA IMPACT

ESTIMATED IMPACT ON CHILD HEIGHT

OLS Cross-Sectional Estimates

If the individual children in the sample were selected randomly for the treatment versus control group, then a simple comparison of their heights in 1999 would reveal the impact of the PROGRESA treatment. The exploration in Section 4 suggests that assignment of children between treatment and control probably was not random. Nevertheless, it is instructive to see what cross-sectional comparisons suggest for children who received treatment versus those who did not. Table 7 presents three OLS cross-sectional estimates for 1999 for children 12–36 months of age in August 1998 in

Table 7—OLS estimates of impact of treatment on height in 1999 for children 12-36 months of age in August 1998 who are in households eligible for PROGRESA

Received treatment	-1.82 (2.3)*	-1.10 (1.7)**	-0.85 (1.2)
Ln child age		18.2 (13.1)*	19.1 (14.3)*
Constant	88.7 (228.9)*	23.4 (4.7)*	-32.8 (0.9)
Joint test for all child coefficients			F(4, 250) = 54.35 Prob > F = 0.0000
Joint test for all household and parental coefficients			F(23, 250) = 1.90 Prob > F = 0.0093
Joint test for all price coefficients			F(11, 250) = 0.96 Prob > F = 0.4799
Joint test for all community coefficients			F(12, 250) = 1.42 Prob > F = 0.1581
Joint test for all state coefficients			F(5, 250) = 1.59 Prob > F = 0.1642
Joint test for all coefficients	F(1, 318) = 5.41 Prob > F = 0.0207	F(2, 317) = 90.10 Prob > F = 0.0000	F(69, 250) = 5.88 Prob > F = 0.0000
Number of observations	320	320	320
R squared (adjusted)	0.0136	0.3584	0.3774
Root Mean Squared Error	6.03	4.86	4.79

Notes: Absolute t values in parentheses to right of point estimates. * indicates significance at 5 percent level, ** at 10 percent level, and *** at 15 percent level. For the estimates in the right column dummy variables are used for missing prices or ln consumption per household member (which effectively controls for the means for the missing values for these variables). Prices are median locality prices of tomatoes, onions, potatoes, tortillas, rice, beans, chicken, milk, vegetable oil, eggs and sugar.

households eligible for PROGRESA of relation (1B).[8] The first includes only whether the children received treatment. The second adds the logarithm of child age. The third

[8] Below we explore the robustness of our fixed-effects estimates to changes in the ages of children considered, to alternative representation of the PROGRESA treatment, and to alternative treatment of missing observations. Here we report on the OLS estimates using the choices that we prefer for each.

includes the childbirth weight and sex and all of the household and parental background, community, and state variables that are discussed in Section 4 (Table 4).

What do these estimates suggest about the impact of PROGRESA on child growth? At a first glance, the answer is not encouraging. For all three estimates, ranging from no to a large number of controls, the estimated PROGRESA treatment effects are negative, though significantly nonzero at the 5 percent level only in the first estimate and at the 10 percent level in the second. If indeed the children are randomly assigned to treatment rather than to the control group, these estimates suggest that, if anything, PROGRESA had a negative effect on child growth.

The pattern across the estimates indicates, however, that when there are additional controls the estimates become smaller in absolute magnitude and less precise, though the estimate is still negative in the last column that includes a large number of controls. 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. Among the controls included in the estimates in the third column, only the groups for the child and for parental and household characteristics are statistically significant at the standard 5 percent level, not prices or community or state characteristics. The particular child and parental and household characteristics that have significant coefficient estimates at least at the 10 percent level, despite the substantial multicollinearity that results from including so many right side variables, are positive ones for child age, child being a boy, child birth weight, mother's height, rooms per household member, and not having a dirt floor. A logit estimate of receiving treatment on these characteristics yields a relation that is significant at the 5 percent level and that has negative coefficient estimates on all these characteristics.[9]

Basic Individual Child Fixed Effects Estimates

The examination of the data in Section 4 suggests, however, that children may not have been assigned randomly to treatment versus control groups in terms of observed characteristics, importantly including their initial health and nutrition status. This also suggests that they might not have been so assigned in terms of characteristics that are not observed in the data. If either of these possibilities holds, then cross-sectional estimates of the treatment effects, such as these, are biased.

Table 8 gives two individual child-fixed effects estimates, akin to relation (1C), using the panel data on individual children in 1998 and 1999. The estimates in the first column use a simple specification that controls for the observed nonlinear child age effect and a possible secular trend, and adjusts for differing lengths of time between measurements. The estimates in the second column also control for fluctuations in household consumption expenditure per household member and in food prices, as discussed in Section 3 above. It should be remembered that these coefficients capture the

9 $Chi^2(8)=15.73$, prob>chi^2 = 0.0464. Coefficient estimates (absolute z statistics) are –0.29 (1.0) for the child being a boy, –1.27 (1.9) for ln child age in months, –0.55 (1.8) for child birth weight in kg, –0.26 (1.8) for relative birth size, –1.24 (1.0) for rooms per household member, and –0.57 (1.8) for not having a dirt floor.

Table 8—Individual child fixed effects estimates of impact of treatment on height for children 12-36 months of age in August 1998 who are in households eligible for PROGRESA

Received Treatment	0.95 (2.4)*	1.02 (2.6)*
Ln child age	6.3 (4.1)*	6.8 (4.4)*
Ln consumption per household member		0.37 (1.2)
Trend	5.3 (7.9)*	5.49 (6.1)*
Mean fixed effect	60.2 (12.4)*	55.0 (8.2)*
Joint test for price coefficients		F(11, 299) = 1.28 Prob > F = 0.2340
Joint test for all coefficients	F(1, 317) = 804.41 Prob > F = 0.0000	F(21, 299) = 117.60 Prob > F = 0.0000
Number of observations	640	640
R squared: within	0.8839	0.8920
between	0.4276	0.3377
overall	0.5056	0.5005
sigma_u	5.01	5.04
sigma_e	2.12	2.10
rho (fraction of variance due to u_i)	0.85	0.85
F test that all u_i=0:	F(319, 317) = 8.53 Prob > F = 0.0000	F(319, 299) = 8.18 Prob > F = 0.0000
Hausman specification test	chi2(3)=49.04 Prob>chi2 = 0.0000	chi2(21)=136.89 Prob>chi2 = 0.0000

Notes: Absolute t values in parentheses to right of point estimates. * indicates significance at 5 percent level, ** at 10 percent level, and *** at 15 percent level. For the estimates in the right column dummy variables are used for missing prices or ln consumption per household member (which effectively controls at the means for the missing values for these variables). Prices are median locality prices of tomatoes, onions, potatoes, tortillas, rice, beans, chicken, milk, vegetable oil, eggs and sugar.

impact of possible transitory changes in these variables. This is not necessarily equivalent to a change in the value of their "permanent" or long-term level. Further, to the extent to which these regressors are measured with error, their coefficients will be biased downward toward zero. Both estimates control for unobserved fixed child (or child's family, household or community) characteristics. The F-tests in the penultimate row suggest that for all of these specifications the probability is very low (less than 0.0000) that these estimates would be obtained were all the individual fixed effects zero. The Hausman specification test results in the last row indicate that the probabilities also are very low that the unobserved effects are random rather than correlated with the right-side variables. The individual fixed effects account for about 85 percent of the variance in the disturbance terms and for the majority of the explained variance (which overall is above half).

The striking result is that in these estimates receiving treatment has a significant *positive* estimated impact on child growth—in sharp contrast to the OLS cross-sectional estimates in Table 7. Conditional on the individual unobserved child-fixed effects that prevail in the estimates in Table 8, the OLS cross-sectional estimates in Table 7 are biased downward to the point of being negative rather than positive by negative correlations between receiving treatment and unobserved determinants of child growth.

The estimates in Table 8 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 those in this sample in the absence of PROGRESA.[10] These estimates, thus suggest that PROGRESA had an

[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 centimeters, median of 7.4 centimeters, standard deviation of 3.1 centimeters, and range from 0.3 to 21.3 centimeters.

important impact on the children who received treatment in the critical 12–36 month age range. The estimated effects are about the same whether there are controls for transitory fluctuations in the log of consumption expenditure per household member and in prices—which as a group do not have effects that are significantly nonzero.

As noted in Section 3, the fixed effects estimates control for all fixed effects, whether they are observed in the data or not. An interesting question is the extent to which the fixed effects implied by the estimates in Table 8 are related to observed characteristics of children in the sample, their parents and households in which they live and the prices and other characteristics of the communities and states in which they live.

Table 9 provides some insight into this question. The dependent variable in this regression is the estimated child-specific fixed effect that was obtained when estimating the results reported in Table 8. Because there are so many possible correlates of these fixed effects, Table 9 only reports significance tests for groups of variables and for coefficient estimates for individual variables for which t-tests indicate significance at least at the 15 percent level (as well as the names of such variables for which there is not such significance). These indicate that the child specific fixed effect is associated at least at the 1 percent level with each of the major groups of variables that are included—child characteristics, household and parental characteristics, prices, and community and state characteristics. Despite the presence of substantial multicollinearity among these regressors (due to the many characteristics included in this regression), a number of the individual coefficient estimates are significantly nonzero at least the 15 percent level.

Table 9—Relation between estimated individual child growth fixed effects from Table 8 and observed fixed child, household and parental, price, community and state characteristics for children 12-36 months in August 1998 and eligible for PROGRESA

Child characteristics F(4, 251) = 759.94, Prob > F = 0.0000			
Birth weight (kilograms)		ln age (months)	6.6 (54.7)*
Reported birth size		Sex (female)	
Parental and household characteristics F(23, 251) = 2.64, Prob > F = 0.0001			
Mother's characteristics			
Height (centimeters)		Age (years)	
Weight (kilograms)		Schooling (grades)	
Body mass index			
Household head characteristics			
Sex male		Nonagricultural worker	
Marital status "open union"	-0.29 (1.5)***	Self-employed	
Marital status "married"		Age (years)	-.0068 (2.4)*
Marital status single		Literate	
Indigenous (speaks dialect or indigenous language)		Schooling (grades)	
Agricultural work primarily			
Household and housing characteristics			
Household size		Electricity	-0.17 (2.6)*
Dirt floors		Piped water access	0.16 (2.4)*
Rooms per household member			
Ln Per capita consumption, land owned			
ln per capita consumption	0.31 (4.7)*	Land owned (area)	
Prices F(11, 251) = 22.37, Prob > F = 0.0000			
Tomatoes	0.027 (3.8)*	Chickens	
Onions	0.053 (7.0)*	Eggs	0.025 (2.9)*
Potatoes	-0.023 (3.1)*	Milk	-0.005 (7.4)*
Tortillas	-0.019 (6.6)*	Sugar	
Rice		Oil	
Beans	0.070 (9.2)*		
Community characteristics F(12, 251) = 3.19, Prob > F = 0.0003			
DICONSA shop		Sec de Salud clinic	
DIF health center		Drainage	0.35 (1.9)**
DIF food	-0.28 (3.9)*	Electricity	
DIF kitchens		Piped water	-0.20 (2.6)*
FIDELIST tortilla		Highway	0.11(1.6)***
IMSS clinic	-0.27 (1.9)**	Main road	
INI shelters		Paved road	
LICONSA milk		Public transportation	
Primary school			
States F(5, 251) = 3.37, Prob > F = 0.0058			
Guerrero	-0.26 (1.4)***	Queretaro	
Hidalgo		San Luis	
Puebla		Veracruz	-0.35 (3.4)*
Number of observations = 320		Adjusted R-squared = 0.9408	
F(68, 251) = 75.49, Probability > F = 0.0000		Root Mean Squared Error = 0.385	

Notes: Dependent variable is fixed effect from estimates in right-hand column in Table 8. In addition to the variables included in this table there also are variables to control for the mean of missing observations for some variables. Only coefficient estimates that are significantly nonzero at at least the 15 percent level are included. Absolute values of t-tests are in parentheses to the right of the point estimates. Significance levels are * = 5 percent, **= 10 percent and *** = 15 percent for two-sample t-tests with equal variances.

Positive associations are reported for the log of child's age, household access to piped water, log consumption expenditures per household member, some prices (for tomatoes, onions, beans, and eggs), and drainage and a highway at the community level. Negative coefficients are reported for the household head being in an "open" martial union rather than married, for older household heads, for the household having electricity, for some prices (for potatoes, tortillas, and milk), for some community characteristics (having a DIF food program, an IMSS clinic, and piped water), and for being in Guerrero or Veracruz rather than in the other four states included in the sample. Despite the difficulties in interpreting such reduced-form relations (e.g., the prices responses may include effects of being producers as well as consumers of these items), these patterns generally seem to be plausible.[11] All in all, the observed characteristics included in Table 9 are consistent with a considerable portion of the variance in the individual child-fixed effects, with an adjusted R^2 of 0.94. The residual in this estimate, while strongly positively correlated with receiving PROGRESA treatment, is negatively correlated with child height—so the unobserved part of the child fixed effects may be underlying the sign reversal between the OLS cross-sectional estimates in Table 7 and the fixed effects estimates in Table 8 (even though the observed characteristics are associated with most of the variance in the fixed effects).

[11] Two possible exceptions are the negative coefficient estimate for the household having electricity and the opposite signs for the household versus the community having piped water.

Some Aspects of the Robustness of the Fixed Effects Estimates

We consider questions relating to three aspects of our estimates—missing observations for right-side variables, the age range of children, and the empirical representation of the PROGRESA treatment.

One question is how to address missing observations on individual variables, particularly on prices. In alternative estimates, we have explored the option of simply dropping from the estimates all of the observations in which the value for any included variable is missing. If we do so, the estimated coefficient on the PROGRESA treatment increases by almost 60 percent in the estimates in the right-hand column of Table 8. But this apparently reflects sample selection because there is a similar increase of almost 40 percent in the estimated PROGRESA treatment effect for the estimates in the left-hand side relation in Table 8 if it is limited to observations with complete information on prices. And there is a fairly substantial reduction in observations from 640 (320 children with information on both rounds) to 529. Therefore, we adopt the alternative of not dropping any observations because of missing data on individual variables, but controlling for the mean of such missing data through dummy variables.

A second question concerns the age range of children who are included in our basic estimates. As we discuss in Section 2 above, there are *a priori* reasons based on the age of weaning and on the nature of the PROGRESA program to focus on roughly the 12–36 month age range. But it is of interest to know what happens for other age ranges. Table 10 summarizes some important aspects of alternative estimates for which the specification is identical to that in the right-hand side of Table 8, but the sample is limited to different age ranges for the children.

The first three columns in Table 10 give estimates for 0–12, 12–36 and 36–60 months, respectively. The column for the 12–36 month age range, on which this paper focuses, is in bold for easy reference. These three columns suggest that the impact of PROGRESA indeed is centered on the 12–36 age range; only for this age range, among

Table 10—Individual child fixed effects estimates of impact of treatment on height for children in August 1998 who are in households eligible for PROGRESA for alternative age ranges[a]

	Age range (months)							
	0-12	**12-36**	36-60	6-36	12-30	12-24	12-42	12-48
Received treatment	0.26 (0.6)	**1.02** **(2.6)***	-0.36 (0.7)	0.84 (2.5)*	0.98 (2.2)*	0.78 (1.3)	0.70 (2.1)*	0.62 (2.0)*
Ln child age	4.1 (13.6)*	**6.8** **(4.4)***	-7.4 (0.5)	4.3 (6.5)*	7.2 (3.6)*	6.8 (2.1)*	7.3 (5.8)*	6.9 (6.0)*
Joint test for all coefficients (Prob > F = 0.0000 for all cases)	F(22, 188)= 182.16	**F(21, 299)= 117.60**	F(20, 113)= 51.96	F(22, 418)= 172.57	F(21, 204)=9 6.52	F(21, 130)= 59.74	F(21, 376)= 149.84	F(21, 430)= 165.70
Number of observations	420	**640**	266	880	450	302	794	902
R squared: within	0.9952	**0.8920**	0.9019	0.9008	0.9086	0.9061	0.8933	0.8900
between	0.6143	**0.3377**	0.0370	0.5987	0.2491	0.1921	0.4990	0.5587
overall	0.7566	**0.5005**	0.3038	0.5250	0.5315	0.5160	0.5295	0.5299
sigma_u	3.75	**5.04**	4.87	5.79	4.42	4.54	5.38	5.68
sigma_e	1.91	**2.10**	1.83	2.10	2.02	2.19	2.02	2.02
rho[a]	0.29	**0.85**	0.88	0.88	0.83	0.81	0.88	0.89
F test that all u_i=0 (Probability > F = 0.0000 for all columns)	F(209, 188) = 5.19	**F(319, 299) = 8.18**	F(132, 113)= 9.92	F(439, 418)= 7.48	F(224, 204)= 7.18	F(150, 130)= 5.55	F(396, 376)= 9.33	F(450, 430)= 9.22

Notes: The specifications for all columns are identical to that in the right-side column in Table 8 (which is reproduced here in bold) but the age range for children in the sample varies as indicated. Absolute t values in parentheses to right of point estimates. * indicates significance at 5 percent level, ** at 10 percent level, and *** at 15 percent level.

[a] rho is the fraction of variance due to u_i.

the three considered, is the estimated effect significantly nonzero. The last five columns in Table 10 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 tend to suggest somewhat smaller estimated impacts of PROGRESA, with point estimates from 0.62 to 0.98 as compared with 1.02 for the 12–36 month range. For all the different age ranges considered in this table, the overall relations and the child fixed effects in particular are significantly nonzero at high levels (with probabilities > than 0.0000).

A third question concerns the representation of the PROGRESA treatment variable. Tables 11a and 11b consider four representations of the PROGRESA treatment variable, all for the same 12–36 month age range:

(1) *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 benefits in at least one month. This is the preferred representation used in the tables above.

(2) *Listed treatment:* defined as residing in a household eligible for PROGRESA and being located in a treatment community.

(3) *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).

(4) *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. This too is normalized to range from 0 to 1.0.

Table 11a gives the means and standard deviations for each of these four representations and the correlations among them. Only about quarter-to-a-third of the sample (0.24 to 0.31) received treatment in the sense defined by the first representation, which is about 50–60 percent of the number indicated for “listed treatment.” 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). Therefore, “listed treatment” may be only a moderately useful proxy for having received treatment. Table 11b gives estimates of the specification in the right-most column in Table 8 for the same age range of 12–36 months, but with the four alternative representations of PROGRESA treatment. In most respects the summary statistics for the four estimates look very similar, with high levels of significance for all the coefficient estimates and for the fixed effects and quite similar R’s squared and decompositions of the disturbances between the fixed effects and the random term. The one important difference, however, is that the “listed treatment” representation is much

Table 11a—Means, standard deviations, and correlations for four alternative representations of PROGRESA treatment for children in eligible households who were 12-36 months of age in August 1998

			Correlations			
			1	2	3	4
	Mean	Standard Deviation	Received treatment	Listed treatment	Received treatment adjusted for intensity (1)	Received treatment adjusted for intensity (2)
1. Received treatment	0.24-0.31[a]	0.43	1.00	0.58	0.90	0.96
2. Listed treatment	0.50	0.50		1.00	0.53	0.57
3. Received treatment adjusted for intensity (1)[b]	0.10	0.19			1.00	0.95
4. Received treatment adjusted for intensity (2)[b]	0.23	0.40				1.00

[a] This range reflects the range of possibly assumptions for the treatment of missing observations for the question regarding months of usage (of which there are 21 among the 320 children used for our basic analysis). Whether we treat these individuals as missing or as receiving no benefits changes the coefficient estimate for received treatment in Table 8 only in the third digit (by less than .03).
[b] These are normalized so that their ranges go from 0 to 1, so their means and standard deviations are not comparable to those for the first two representations.

more imprecisely estimated than the "received treatment" representations and, in the case in which comparisons of the coefficient estimates make sense, is less that half the magnitude of the "received treatment" representation (i.e., option 1 versus 2). It would appear that the "listed treatment" variable is a noisy representation of actually receiving treatment with the result that the use of "listed treatment" is likely to lead to a bias toward 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.

Table 11b—Individual child fixed effects estimates of impact of treatment on height for children 12-36 months of age in August 1998 who are in households eligible for PROGRESA for alternative representations of PROGRESA treatment[a]

	Alternative representations of PROGRESA treatment			
	1. Received treatment	2. Listed treatment	3. Received treatment adjusted for intensity (1)[b]	4. Received treatment adjusted for intensity (2)[b]
Treatment coefficient	1.02 (2.6)*	0.46 (1.3)	0.24 (2.0)*	1.00 (2.2)*
Joint F(21, 299) test for all coefficients	117.60 Prob > F = 0.0000	115.49 Prob > F = 0.0000	103.96 Prob > F = 0.0000	104.39 Prob > F = 0.0000
R squared: within	0.8920	0.8903	0.8870	0.8875
between	0.3377	0.3802	0.3535	0.3443
overall	0.5005	0.5112	0.5015	0.4977
sigma_u	5.04	4.98	5.01	5.03
sigma_e	2.10	2.12	2.16	2.16
rho[c]	0.85	0.85	0.85	0.84
F(319, 299) test that all u_i=0:	8.18 Prob > F = 0.0000	8.07 Prob > F = 0.0000	7.90 Prob > F = 0.0000	7.91 Prob > F = 0.0000

[a] Specification and sample is same as for right-most column in Table 8 and the first column here, highlighted in bold, is the same as that column. Absolute t values in parentheses to right of point estimates. * indicates significance at 5 percent level, ** at 10 percent level, and *** at 15 percent level. For the estimates in the right column dummy variables are used for missing prices or ln consumption per household member (which effectively controls at the means for these variables).

[b] These are normalized so that their ranges go from 0 to 1, so their means and thus their coefficient estimates are not directly comparable to those for the first two representations.

[c] rho is the fraction of variance due to u_i.

Interactions Between PROGRESA Treatment and Observed Characteristics

The estimates discussed to this point assume that, controlling for unobserved fixed effects, transitory fluctuations in consumption expenditure per household member and in prices, and child's age, the impact of PROGRESA is the same on all children who were in the 12–36 age range in August 1998. But the conceptual framework in Section 3 suggests that the impact may vary depending on characteristics of the child, his or her family, and the community in which he or she lives.

Accordingly, we present estimates that allow the impacts of receiving the PROGRESA treatment to vary by child, parental and household, and community and state characteristics. We do so by including in the specification interactions between such characteristics and having received treatment, in addition to the direct effect of having received treatment itself. We first have explored such interactions, one variable at a time, for most of the variables that are included in Table 4.[12] The subset of these estimates in which the interaction has a coefficient estimate that is significantly nonzero at least at the 15 percent level is included in Panel A of Table 12. We then explore what subset of these interactions remains significantly nonzero when they are included in combination, as summarized in Panel B of Table 12. These latter estimates are more interesting because they are more robust to the inclusion of other controls.

The significant estimates in Panel B of Table 12 suggest that three household characteristics and two community characteristics affect the magnitude of the PROGRESA treatment differentially. The positive estimates for two of the household characteristics, speaking an indigenous language and household size, may reflect that poorer households that are more likely to speak an indigenous language and to be larger are more constrained by market imperfections. Therefore, their children benefit more

[12] We have not included the child characteristics that refer to anthropometric status subsequent to birth because these would seem clearly to reflect behavioral choices of the types under investigation, nor do we include prices.

Table 12—Summary of interaction effects between receiving treatment and child, household, community and state characteristics[a]

Panel A. Adding one interaction at a time			Panel B. Adding all interactions that remain significant in combination	
Variable interacted with received treatment	Coefficient estimate for received treatment	Coefficient estimate for interaction	Variable interacted with received treatment	Coefficient estimate
None (same as in Table 8)	1.02 (2.6)*		Constant (i.e., simply received treatment)	-2.89 (2.0)*
Child birth weight	0.27 (0.4)	0.37 (1.5)***	Child birth weight	
Mother's schooling	0.41 (0.7)	0.19 (1.4)***	Mother's schooling	
Household head speaks indigenous language	0.61 (1.4)	1.99 (2.3)*	Household head speaks indigenous language	2.29 (2.8)*
Household head primarily agricultural worker	-0.08 (0.1)	1.76 (2.4)*	Household head primarily agricultural worker	
Household head primarily self-employed	1.17 (2.9)*	-2.37 (1.7)**	Household head primarily self-employed	
Household head's schooling	-0.49 (0.8)	0.47 (3.3)*	Household head's schooling	0.45 (3.1)*
Household size	-0.61 (0.5)	0.24 (1.5)***	Household size	0.35 (2.3)*
Community has DIF food program	0.66 (1.6)***	2.27 (2.4)*	Community has DIF food program	3.05 (3.1)*
Community has piped water	1.73 (3.6)*	-1.89(2.6)*	Community has piped water	-2.39 (2.0)*
Community has highway	1.42 (3.2)*	-1.68 (2.1)*	Community has highway	
Hidalgo state	1.51 (3.2)*	-1.52 (2.0)*	Hidalgo state	
Queretaro state	0.76 (1.9)**	3.19 (2.4)*	Queretaro state	

[a] The specification or each estimate is identical to that in the right-most column of Table 8 for the same 12-36 month age group (for which the estimate is presented in the first row of this table) except that, one at a time, interactions with "received treatment" are added for child age, sex, and birth weight and for all the parental and household, community and state variables that are 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 15 percent level are presented here. Absolute t values in parentheses to right of point estimates. * indicates significance at 5 percent level, ** at 10 percent level, and *** at 15 percent level.

from identical options presented by PROGRESA than do children from initially somewhat better off households. The positive estimate for household head's schooling, on the other hand, tends to suggest that somewhat better-off households have greater gains, perhaps because they are better able to process the necessary information to benefit more from PROGRESA. It is interesting to note that it is the household head's schooling that is significant and not the schooling of the children's mothers even though 97 percent of the household heads in this sample are male. This contrasts with widely-held perceptions that mothers' schooling is particularly important for child development (e.g., Strauss and Thomas 1995).

With regard to the community characteristics, impacts are larger in communities without access to piped water. This is consistent with PROGRESA, having greater effects not only among poorer households but in communities that are poorer with regard to the general health and sanitation environment. The other community variable that had a significant interaction—whether the community has a DIF food program—also may be representing poorer communities if indeed poorer communities are more likely to have such programs. There is some evidence that indeed these programs do tend to be placed in poorer and more malnourished communities. 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.[13]

Thus these estimates suggest that, among children who receive PROGRESA treatment, the effects tend to be larger for poorer households in poorer communities but

[13] For the sample on which we focus, 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 percent level) and 1,789 versus 2,057 calories per household member (significantly different at the 5 percent level).

that children in households in which household heads have more schooling also tend to benefit more.[14]

ESTIMATED IMPACT ON THE PROBABILITY OF STUNTING

The analysis in most of the previous section pertains to the average impact of PROGRESA on child growth, thought the last part of the section investigates possible differences among children in treatment effects. But of particular interest is what are the effects on the children who are faring most poorly—those who are more than two standard deviations below the norms or are stunted. Among the children age 12–36 months in August 1998 who were from households eligible for PROGRESA, 44 percent were stunted. A year later, 41 percent were stunted, including 76 percent of those who were stunted in 1998.

Such data suggest that PROGRESA may have had some small impact on stunting, but it is desirable to go further than such a summary by investigating the probability of being stunted parallel to the investigation of the determination of child growth in the previous section. The available data limit the extent to which such an exploration is possible. 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.

[14] These are suggestive only 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).

Among the children age 12–36 months in August 1998 who were from households eligible for PROGRESA, 24 percent of those stunted in 1998 were not stunted in 1999 (34 children) and 14 percent who were nonstunted in 1998 were stunted in 1999 (25 children).

Tables 13 and 14 are parallel to Tables 7 and 8, but present 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 13 provide no support for the proposition that PROGRESA reduced child stunting. In fact, all the coefficient estimates of the received treatment variable are the wrong sign (though not significantly different from zero). The fixed effects logit estimates in Table 14, in sharp contrast, indicate significant negative effects of PROGRESA treatment on child stunting. Thus, though qualifications are necessary because of the small number of observations noted above (which may be related to the fairly different values of the point estimates with changes in specification in Table 14), these explorations suggest that (1) the cross-sectional results are basically misleading because there are important unobserved fixed effects and (2) once there is control for the fixed effects, PROGRESA treatment appears to have had a significant effect on reducing child stunting as well as on increasing child growth on the average.

Table 13—Logit estimates of impact of treatment on stunting in 1999 for children 12-36 months of age in August 1998 who are in households eligible for PROGRESA[a]

Received treatment	0.28 (1.1)	0.33 (1.3)	0.14 (0.3)
Ln child age		1.19 (2.0)*	1.00 (1.2)
Constant	-0.44 (3.3)*	-4.71 (2.2)*	15.6 (0.7)
Joint test for all child coefficients			chi2(4) = 14.76 Prob > F = 0.0052
Joint test for all household and parental coefficients			chi2(22) = 42.80 Prob > F = 0.0050
Joint test for all price coefficients			chi2(11) = 21.93 Prob > F = 0.0249
Joint test for all community coefficients			chi2(12) = 13.89 Prob > F = 0.3075
Joint test for all state coefficients			chi2(5) = 7.99 Prob > F = 0.1569
Joint test for all coefficients	LR chi2(1) = 1.15 Prob > F = 0.0027	LR chi2(2) = 5.24 Prob > F = 0.0727	LR chi2(65) = 151.48 Prob > F = 0.0000
Number of observations	320	320	314
Psuedo R squared	0.0027	0.0121	0.3581

[a] Absolute z values in parentheses to right of point estimates. * indicates significance at 5 percent level, ** at 10 percent level, and *** at 15 percent level. Prices are median locality prices of tomatoes, onions, potatoes, tortillas, rice, beans, chicken, milk, vegetable oil, eggs and sugar. For the estimates in the right column dummy variables are used for missing prices or ln consumption per household member (which effectively controls at the means for the missing values for these variables).

Table 14—Individual child fixed effects Logit estimates of impact of treatment on stunting for children 12-36 months of age in August 1998 who are in households eligible for PROGRESA[a]

Received Treatment	-1.50 (2.5)*	-3.54 (2.6)*
Ln child age	-0.02 (0.0)	1.9 (0.5)
Ln consumption per household member		-0.02 (0.0)
Trend	0.23 (0.2)	1.60 (0.6)
Joint test for price coefficients		chi2(11) = 6.37 Prob > F = 0.8476
Joint test for all coefficients	LR chi2(3) = 8.25 Prob > F = 0.0412	LR chi2(21) = 33.48 Prob > F = 0.0411
Number of observations	118	118

[a] Absolute t values in parentheses to right of point estimates. * indicates significance at 5 percent level, ** at 10 percent level, and *** at 15 percent level. Prices are median locality prices of tomatoes, onions, potatoes, tortillas, rice, beans, chicken, milk, vegetable oil, eggs and sugar. For the estimates in the right column dummy variables are used for missing prices or ln consumption per household member (which effectively controls for the means for the missing values for these variables).

6. LONGER-TERM IMPACT

This study has focused only on estimating the impact of PROGRESA on child growth. But this impact is of interest, as noted in the introduction, because it may relate to longer-term health and nutritional status and productivity. There are at least four channels through which the component of the PROGRESA program that affects child health/height can affect lifetime earnings: (1) by increasing cognitive skills as an adult (conditional on years of schooling completed) that directly affect earnings, (2) by increasing physical stature as an adult that directly affects earnings, (3) by increasing the years 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.[15] For the program to have impact through channels (3) and (4), we are assuming that improved cognitive skills and nutrition as a child facilitates earlier entry into school, lower repetition rates, and more years of schooling completed.

There is piecemeal empirical evidence of significant effects through all four of these channels for other developing countries:

[15] 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. These may be quite important. For example, in another developing country context, Behrman and Knowles (1999) find that the strongest association between parental family income and schooling success is with the progression rate through school, neither with the grades completed nor the cognitive achievement conditional on grades completed.

(1) on the impact of adult cognitive achievement on wages: Alderman et al. (1996) for rural Pakistan; Boissiere, Knight and Sabot (1985) for urban Kenya and Tanzania; Glewwe (1996) for Ghana; and Lavy, Spratt and Leboucher (1997) for Morocco.

(2) on the impact of adult height on wages and/or productivity: Behrman and Deolalikar (1989) and Deolalikar (1988) for rural India; Haddad and Bouis (1991) for rural Philippines; Strauss (1986) for Côte d'Ivoire; Thomas and Strauss (1997) for Brazil; and Behrman (1993) for the more general experience in developing countries.

(1) and (2) on the impact of early childhood nutrition and cognitive development on adult nutritional status and cognitive achievement: Grantham-McGregor et al. (1997) and Grantham-McGregor, Fernald, and Sethuraman (1999) for Jamaica; Martorell (1995) and Martorell, Rivera, and Kaplowitz (1989) for rural Guatemala; and Haas et al. (1996), Martorell (1999), and Martorell, Khan, and Schroeder (1994) for the more general experience in developing countries.

(3) on the impact of grades of schooling completed on wages—hundreds of studies, many of which are surveyed in Psacharopoulos (1994) and Rosenzweig (1995);

(3) and (4) on the impact of better child nutrition on progress through schooling: Jamison (1986) for China; Moock and Leslie (1986) for Nepal; and Behrman (1993), Leslie and Jamison (1990), and Pollitt (1990) for the more general experience in developing countries.

(4) on the impact of preschool child nutrition on age of starting school: Alderman et al. (2000) for rural Pakistan; Glewwe and Jacoby (1995) for Ghana; and Glewwe, Jacoby, and King (2000) for the Philippines.

Unfortunately, the PROGRESA data and analysis to date do not include direct estimates of these links. So for illustrative simulations we use estimates from Thomas and Strauss (1997) for (2). Strauss and Thomas (1998) analyze the relationship between adult earnings and height and completed years of schooling for male workers in another Latin American country, Brazil. They find that a 1 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 in Section 5.1 imply that PROGRESA increases target children's height by about 1.2 percent.[16] Under the assumption that there is strong persistence of changes in small children's anthropometric development 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 percent 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 percent increase in lifetime earnings. In addition to the effect through channel (2), there is evidence from the studies noted of significant positive

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

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

The nutrition of infants and small children is 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 life-time options by 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 the long term. Within rural Mexico, stunting, or short height relative to standards established for healthy populations, is the major form of PEM. Wasting is much less of a problem.

One of the major components of the PROGRESA program has been directed toward improving the nutritional status of small children in poor rural communities in Mexico. Cross-sectional comparisons of health/height for children who received this PROGRESA treatment versus others who were in PROGRESA eligible households but who did not receive this treatment suggest no positive effect of PROGRESA either on child height on the average or on reducing the proportion of children who are stunted. But these comparisons may be misleading because of the failure to control for unobserved child, parental and household, market and community characteristics that may be correlated with children receiving the PROGRESA treatment and because of the failure to control for systematic initial differences in that, on the average, the children in the control sample tended to have better anthropometric status than children in the treatment sample.

Our preferred estimates control for these factors, as well as secular trends and transitory fluctuations in prices and household consumption, by using fixed effects estimates. PROGRESA treatment is represented by those who reportedly received the nutritional supplements in the treatment group (less than 60 percent of those children in the treatment group) for children in the critical age range of 12–36 months. These estimates find significant impacts of receiving PROGRESA treatment in increasing child growth and in reducing the probability of child stunting. They imply an increase of about a sixth in mean growth per year for these children, which may be somewhat larger for children from poorer households and poorer communities but who come from households with more educated household heads. Furthermore, such estimates may be conservative because some children in the control localities have received these supplements; some households report receiving the supplement for more than 12 months; the supplement may not be fully consumed; and the INSP data indicate that in many households, the supplement is shared among family members.

However, 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 percent (1.4 percent) 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 small children in poor households.

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