IFPRI

FCND DP No. 127

FCND DISCUSSION PAPER NO. 127

A COST-EFFECTIVENESS ANALYSIS OF DEMAND- AND SUPPLY-SIDE EDUCATION INTERVENTIONS: THE CASE OF *PROGRESA* IN MEXICO

David P. Coady and Susan W. Parker

Food Consumption and Nutrition Division

International Food Policy Research Institute
2033 K Street, N.W.
Washington, D.C. 20006 U.S.A.
(202) 862–5600
Fax: (202) 467–4439

March 2002

ABSTRACT

This paper is concerned with the issue of the most cost-effective way of improving access to education for poor households in developing countries. We consider two alternatives: (1) extensive expansion of the school system (i.e., bringing education to the poor) and (2) subsidizing investment in education by the poor (i.e., bringing the poor to the education system). To this end, we evaluate the Programa Nacional de Educación, Salud y Alimentación(PROGRESA), a large poverty alleviation program recently introduced in Mexico that subsidizes education. Using double-difference regression estimators on data collected before and after the program for randomly selected control and treatment households, we estimate the relative impacts of the demand- and supply-side program components. Combining these estimates with cost information, we find that the demand-side subsidies are substantially more cost-effective than supply-side expansions.

CONTENTS

TABLES

FIGURES

ACKNOWLEDGMENTS

The authors thank staff and colleagues at the Programa Nacional de Educación, Salud y Alimentación (PROGRESA), the Centro de Investigación y Docencia Económicas A.C. (CIDE), and the International Food Policy Research Institute (IFPRI) for helpful comments. All errors remain our own.

David P. Coady
International Food Policy Research Institute

Susan W. Parker
Centro de Investigación y Docencia Económicas A.C. (CIDE)

1. INTRODUCTION

There is a vast body of literature that identifies the expansion of formal education as a key component of successful development strategies (Schultz 1988; Psacharopoulos 1994; Barro and Sala-i-Martin 1995). In spite of this general consensus, there is still much disagreement about how best to allocate scarce public resources within the education sector. In a recent survey of the empirical literature on education, Hanushek (1995) identified school quality as the important constraint toward increasing education levels. But, in a reply based on the same empirical literature, Kremer (1995) argues that, while quality is undoubtedly important, there is no evidence that improving quality is more important than opening new schools in isolated areas or subsidizing the cost of schooling to allow more people to attend. Thus, this debate regarding the relative importance of improved school quality vis-à-vis improved school access appears to be far from settled.

The quality versus access debate is about the issue of the most cost-effective way of achieving a given total years of education. Yet concerns for equity—the distribution of education across different income groups—is a strong motivating factor underlying government intervention in the education sector. Since economies of scale imply that it is generally more cost-effective to locate schools in relatively densely populated areas, poorer households, which tend to be disproportionately located in remote areas, may face substantially higher private costs and, as a result, tend to acquire lower education levels.

This may be further exacerbated by the relative importance of credit market failures for poorer households.

In this paper we are concerned with the issue of the most cost-effective way of improving access to education for poor households in developing countries. We consider two alternatives, namely, (1) extensive expansion of the school system (bringing education to the poor), and (2) subsidizing investment in education by the poor (bringing the poor into the education system). To this end, we evaluate a relatively unique and large program recently introduced in Mexico that subsidizes education. To our knowledge, this is one of the first studies that rigorously analyzes the relative cost-effectiveness of demand- versus supply-side subsidies in the context of a developing country.

The program we analyze, the Programa Nacional de Educación, Salud y Alimentación (PROGRESA), was introduced by the Mexican government in 1997. The program subsidizes investment in human capital by poor households by conditioning cash transfers to families on their enrolling their children in school and making regular trips to health clinics. There is also a supply-side component to the program with resources allocated toward improving school quality and access (e.g., more teachers, health clinic staff, higher salaries, and extensive expansion). PROGRESA has grown rapidly, and by the end of 2000, the program was providing benefits to 2.6 million of the poorest families in rural Mexico, corresponding to about 40 percent of all rural families and nearly 12 percent of all families in Mexico. The idea of linking monetary transfers to human capital investment has become a model for other countries: similar programs are underway in

Bangladesh, Honduras, and Nicaragua and are in the planning stages in Argentina, Colombia, and Jamaica.

We analyze the cost effectiveness of the secondary education component of PROGRESA, based on the program goal of increasing school enrollment at the secondary level (grades 7–9).[1] In the poor communities where PROGRESA operates, only about half of all children continues to secondary school after primary (grades 1–6). This paper compares the cost-effectiveness of the PROGRESA transfers (educational grants) to the policy of constructing new schools. We use household-level data as well as data on supply and costs to separate the supply-side from the demand-side impact and derive the cost of each part accordingly. We show that the demand-side component is a much more cost-effective way of increasing education levels relative to building additional schools.

Our evidence is derived from unique panel data of children in poor rural communities in Mexico. The communities formed part of a social experiment where communities were allocated between "control" and "treatment" groups to receive PROGRESA benefits. Baseline and follow-up data were collected from households in both sets of communities, but the program was implemented only in the treatment localities during the period this information was collected. We combine this data with information on the cost of transfers as well as data from the Secretary of Public

[1] Two previous studies (Schultz 2000; Behrman, Sengupta and Todd 2001) focused on identifying the overall impact of PROGRESA on educational outcomes, including enrollment, progression, and return rates. Although such impact analyses constitute a crucial input into any economic evaluation of the program, knowledge of impact by itself may be insufficient for policymakers concerned with allocating scarce public resources between competing alternatives. There may be many alternative ways of achieving a given impact, but with costs differing substantially across these alternatives.

Education on the cost of building schools. These data allow us to both identify program impacts precisely as well as carry out a comprehensive cost-effectiveness analysis.

The format of the paper is as follows. Section 2 describes the program design. Section 3 describes the strategy for estimation as well as the data. Section 4 estimates the program impact on enrollment, differentiating between the demand- and supply-side components. Section 5 presents the cost-effectiveness analysis and Section 6 summarizes and qualifies the results.

2. PROGRAM DESIGN

PROGRESA, a large poverty alleviation program in Mexico begun in 1997, targets its benefits directly to the population in extreme poverty in rural areas.[2] It currently operates in over 50,000 localities in 31 states, with a budget of nearly $1.3 billion for 2001. The program is made up of three closely linked components (education, health, and nutrition) based on the belief that there are positive interactions between the three. Our analysis concentrates on the education component, which we now briefly describe.

Under the education component, the program provides monetary education grants for each child less than 18 years of age enrolled in school between the third grade of

[2] Beneficiaries are selected through a three-stage targeting mechanism. First, using national census data, geographic targeting is applied to select the most marginal communities. Second, socioeconomic data are collected from all households in the most marginal communities. Using income and other data (e.g., education, housing conditions, and durable goods), discriminant analysis is used to identify "poor" households. Finally, community feedback is used to reclassify households. See Skoufias, Davis, and de la Vega (2001) for details.

primary and the third grade of secondary school (Table 1). In order to compensate for the forgone income that children would otherwise contribute to their families if they were working, the grant amounts increase as children progress to higher grades. Additionally, at the secondary school level (junior high), the grants are slightly higher for girls than for boys. In the second half of 1999, the amounts of the monthly educational grants ranged from $80 (Mexican pesos[3]) in the third grade of primary to $265 for boys and $305 for girls in the third year of secondary school.

Table 1—Monthly education subsidy rates (pesos), July–December 1999

	Males	**Females**
Primary		
- Grade 3	80	80
- Grade 4	95	95
- Grade 5	125	125
- Grade 6	165	165
- Supplies	100 (per semester)	100 (per semester)
Secondary		
- Grade 7	240	250
- Grade 8	250	280
- Grade 9	265	305
- Supplies	190 (per semester)	190 (per semester)

Note: The maximum monthly transfer that households can receive is $750. Subsidy rates are indexed to inflation every six months.

In order to provide incentives for human-capital accumulation, benefits are contingent on fulfillment of certain obligations by the beneficiary families. Grants are

[3] We use the symbol $ to denote Mexican pesos. The exchange rate in 1999 was approximately 10 pesos per U.S. dollar.

linked to school attendance of children: if a child unjustifiably misses more than 15 percent of school days in a month, the family will not receive the grant that month. All of the benefits are given directly to the mother of the family, with a maximum monthly limit of $750 per family. Average monthly benefits are currently $255, equivalent to about 22 percent of the monthly income of beneficiary families. After three years, families may renew their status as beneficiaries, subject to a reevaluation of their socioeconomic conditions. On the supply side, extra resources are made available to schools serving the beneficiary communities to compensate for the expected increase in demand generated by the program, thus helping to avoid negative congestion externalities.

3. EMPIRICAL STRATEGY AND DATA

The empirical analysis in this paper has several parts. First, we estimate the overall impact of the program (i.e., the combined demand- and supply-side components) on secondary school enrollment. Then, using two sources of data, (1) household-level data generated from a natural experiment designed for the evaluation of PROGRESA, and (2) school-level data collected separately from the Secretary of Public Education, we estimate the separate impacts of demand-side subsidies and of increased supply on school enrollment. We combine these estimated impacts with an analysis of program costs to evaluate the cost-effectiveness of grants versus construction of secondary schools as alternative strategies for promoting secondary school enrollment. We now briefly describe the data sources.

HOUSEHOLD-LEVEL DATA FROM PROGRESA EVALUATION

Specifically for the purposes of program evaluation, PROGRESA carried out a social experiment in which a random sample of 506 eligible communities was selected from the seven states where the program was first implemented. Communities were randomly assigned to a treatment group (320 communities that received transfers) and a control group (186 communities that would receive benefits about two years later). All of the 24,077 households in both treatment and control communities were surveyed prior to implementation of the program. This baseline household census, containing information on households' socioeconomic characteristics, was collected in November 1997 (ENCASEH97: *Encuesta de Características Socio-económicos de los Hogares*). Households in the treatment group began to receive benefits in March 1998. Periodic follow-up surveys (ENCEL-*Encuesta de Evaluación*) were carried out after program implementation approximately every six months. These surveys include information on numerous topics, including education, health utilization, household expenditure, women's status, and community indicators. In our analysis, we use the ENCASEH and two post-program rounds of the ENCEL, namely the October 1998 and November 1999 rounds. Behrman and Todd (1999) evaluate the success of the randomization and find that characteristics do not systematically differ at the community level.

SUPPLY DATA

As we noted earlier, concomitant with the monetary transfers of PROGRESA, there was an extensive expansion of supply aimed at improving (or at least avoiding a deterioration in) the quality of schooling. Without this component, it might be expected that overall school quality might decrease, given that increasing enrollment due to the program would likely increase variables such as the student-teacher ratio. In this section, we describe the relevant supply variables across control and treatment communities for each of the three sample years. Data on school characteristics come from the Secretary of Public Education (SEP), which collects information on all schools nationwide.

Using GIS software, we identify the nearest secondary school to each community and match its characteristics to each child, including the distance to the school (in kilometers). We thus assume that the available supply for this child can be captured by the characteristics of the closest school. If a school is located within the community where the child lives, this distance is registered as 0 kilometers. Less than a third of our sample of children have a secondary school inside their community.[4] For each school we have the following information: number of students enrolled in grades 7 through 9, number of teachers, teachers' average education level, number of classrooms, percentage of children who failed between one and five classes during the previous year, number of classrooms with more than one grade, type of school, and source of funding.

Table 2 shows a clear decrease in distance to the nearest school in both control and treatment communities over time, consistent with school construction occurring over our time period of analysis. The year 1997 represents the situation before program

[4] Note that the closest school to the child is not necessarily the school attended by the child, although this is the case in most instances. However, we believe that using characteristics of the closest school rather than the actual school attended is less problematic from the perspective of endogeneity.

implementation, whereas 1998 and 1999 represent the situation after program implementation. Overall, mean distance decreases from about 2.2 to 2.0 kilometers, both in treatment and control communities. Given the proximity of many control and treatment communities, it is likely that many children from both control and treatment communities attend the same schools. Therefore, extra resources to schools, to the extent they are given, are likely to benefit children in both sets of communities. This will have implications for how we identify demand- and supply-side effects of the program below, given the absence of an explicit "control" group for supply-side interventions.

Table 2—Summary of supply-side data (means)

	Treatment localities			Control localities		
Secondary school	**1997**	**1998**	**1999**	**1997**	**1998**	**1999**
Distance to nearest school	2.21	2.13	2.04	2.22	2.17	1.98
Telesecondary	0.88	0.88	0.88	0.91	0.92	0.90
School enrollment	75.80	82.26	97.60	72.01	80.96	91.90
Student-teacher ratio	22.06	23.57	24.17	22.91	23.51	25.23
Student-classroom ratio	21.76	24.12	25.61	22.44	24.86	25.71
Multiple classrooms	0.55	0.23	0.38	0.21	0.20	0.14
Percent students failing	0.02	0.03	0.03	0.02	0.03	0.02
Percent teachers with higher education	0.96	0.93	0.94	0.96	0.95	0.94

Note: The numbers in the table are variable means and based on the panel sample of children on which we have information for all three years. Children are attributed the supply characteristics of the nearest school.

Consistent with the presence of the program, we observe larger increases in school enrollment levels in treatment communities than in control communities. In spite of this, both the student-teacher and student-classroom ratios increase only slightly over time, while the number of multi-grade classrooms (classrooms where more than one grade is being taught) decreases, all consistent with supply-side resources increasing to compensate for increases in demand. We also observe only very slight changes in the indicators of average educational attainment of teachers and the percentage of students reported as failing at least one class. All in all, the general picture is one of increasing demand being compensated for by matching supply-side resources.

4. IDENTIFICATION OF PROGRAM IMPACTS

Previous studies of PROGRESA have measured educational impact through simple mean comparisons between the treatment and control group or through regression analysis using a dummy variable to capture program eligibility (Schultz 2000). Note, however, that this method does not allow us to determine which part of the impact might be attributed to the education grants versus the improvements in supply made by the program. Our empirical strategy allows us to separate these effects. By including indicators of the supply of schooling over time in our sample, we should pick up the program impact that occurs through changing supply-side characteristics. If, in fact, part of the program impact on schooling results from supply-side changes, controlling for supply-side variables should result in a decrease in the estimated coefficient on the dummy variable for treatment-control compared to the regression without supply-side variables.

We start this section by generating a reference set of estimates of total program impact; these are comparable to those generated by the earlier work of Schultz (2000). We then separate out the total program impact into its supply- and demand-side impacts. Our estimations focus on the variable school enrollment,[5] which we then translate into an indicator of extra years of education due to the program.[6]

EMPIRICAL SPECIFICATION OF PROGRAM IMPACT

To estimate the program impact on school enrollment, we construct double-difference regression estimates using the ENCASEH97 survey as our baseline survey prior to program implementation and the subsequent ENCEL surveys. These estimators are based on comparing differences between the treatment and control groups before and after the program. Note that double-difference estimators have the advantage that any preprogram differences between the treatment and control groups are eliminated in the estimation of impacts. Under the assumption that any unobserved heterogeneity between

---

[5] Other potential indicators are attendance levels and/or school performance. The available data have thus far shown little impact of PROGRESA on student test scores (Behrman, Sengupta, and Todd 2000). Evaluation of school attendance has also shown little impact of PROGRESA on attendance rates; that is, once children are enrolled in school, they tend to attend regularly.

[6] We use an indirect approach (estimating years of extra schooling from enrollment impacts) rather than a more direct approach of directly estimating PROGRESA's impact on years of completed schooling for two basic reasons. First, years of completed schooling is a longer-term measure of schooling achievement and its effect is likely to be underestimated using our data, which contains data for only 18 months after program implementation. Second, we have found substantial inconsistencies in the variable that measures highest grade completed. Whereas, between any two given school years, children should have either the same years of schooling or one additional year, the data show that a large fraction of the sample has improbable progression patterns. Using enrollment rates to derive years of schooling invariably involves making some assumptions about completion rates. We assume that, once enrolled, a child completes the year, both in the treatment and control group. Note that this is likely to actually *underestimate* the impact of the program since PROGRESA has had some effect on increasing completion rates (Behrman, Sengupta, and Todd 2001).

the treatment and control groups is fixed over time, the double-difference estimator eliminates differences attributable to this heterogeneity. The empirical specification we use also contains a number of control variables, which may be useful for reducing any remaining statistical bias.

Estimating the Total Program Impact

We pool the three November surveys (ENCASEH97, ENCEL98N, and ENCEL99N), giving us three observations covering three different school years. Each round was carried out in the fall of each school year, that is, at the beginning of each school cycle. In our impact analysis, we allow the effect of the program to be different in each of the two post-program rounds, as might be the case if the program impacts decrease (or increase) over time. The regression equation that we estimate is the following:

$$S_{it} = \sum_{t=1}^{3} a_{0t} + a_1 T_i + a_2 T_i R_2 + a_3 T_i R_3 + \sum_{j=1}^{J} b_j X_{jit} + e_{it} \quad ,$$

where S_{it} represents whether the child i is enrolled in school in period t, T_i represents a binary variable equal to 1 if individual i lives in a treatment community and 0 otherwise, R is the round of the corresponding ENCEL survey, and X_{jit} represents the vector of J control variables for individual i in time t (described below).

Under this specification, the program impact over the various rounds of the evaluation survey is estimated by interacting the treatment dummy T_i with the round of the analysis R (round 1 represents the baseline observation before implementation of the program whereas rounds 2 and 3 represent after-program rounds corresponding to the ENCEL of November 1998 and November 1999). Note that a_1 is expected to be insignificantly different from zero (that is, preprogram differences prior to program implementation are expected to be zero) and the interaction terms represent the impact of being in a treatment community on school enrollment after program implementation. The intercept terms, a_{0t}, capture the fact that school enrollment may vary (for reasons unrelated to the program) over each round of the analysis. We include a number of other control variables, including a child's age, mother and father education levels, marginality level of the community, community agricultural wage, and distance to the nearest municipal center.[7]

Adding Supply-Side Variables

The regression framework used above, which estimates impact through the inclusion of a dummy variable measuring receipt or not of the program, cannot separate the effects of the demand- and supply-side components. As is, therefore, we cannot argue that the identified impact represents the effect of the subsidies as opposed to the improvements in supply. However, once we add supply indicators of schooling

[7] Our results (available on request) are robust to various eligibility definitions and to using pooled (everyone in the sample at some point) as opposed to panel data (only those in all years).

(assuming that our data are of sufficient quality to, in fact, adequately capture supply-side changes), we should be able to isolate the effect of any improvements in supply over our period of analysis. If the effect of the program as measured by the dummy variable is reduced with the inclusion of the supply-side variables, this would imply that part of the enrollment impact attributed to the introduction of the program derives from improvements in the supply side in treatment relative to control communities.

Adding supply indicators to our regression framework, our estimated equation becomes

$$S_{it} = \sum_{t=1}^{3} a_{0t} + a_1 T_i + a_2 T_i R_2 + a_3 T_i R_3 + \sum_{j=1}^{J} b_j X_{jit} + \sum_{k}^{K} b_k X_{kit} + e_{it} \quad ,$$

where X_{kit} represents the vector of *K* variables measuring supply of schooling and other variables are as before.

The supply-side variables that we include are the following. First, we include distance to the closest secondary school and its square. This variable captures a number of aspects related to schooling. Distance clearly is a measure of both private financial and time costs incurred in attending school; a greater distance increases the private costs of attending school. But distance is also a supply measure of schools in the sense that the only way (excluding migration) that, for a given child, this distance can be reduced is through the construction of new schools.

We include other supply-side variables that we hope will serve as proxies for the quality of education received. Since it is very difficult to specify with much confidence how these variables combine with each other (or, indeed, with unobserved quality characteristics), we avoid focusing on specific coefficients. We therefore view these quality variables as *jointly* controlling for quality differences.

The variables used to capture quality are as follows. We use information on the type of secondary school available. In the rural communities we analyze, the dominant type of secondary school is the "telesecondary."[8] Therefore, we consider the enrollment impact of having a telesecondary as the nearest secondary school versus the alternative of other types of secondary schools (mainly technical). Nevertheless, there is likely a problem of endogenous school placement here; for instance, telesecondary schools may be found precisely in areas that tend to have low school enrollment caused by factors that are unobservable to the researcher (Rosenzweig and Wolpin 1986). This would tend to bias the estimated impact and thus our results should be interpreted as only suggestive. A variable capturing the education level of the teacher is also included, measured by the percentage of teachers with at least a high school education at the available secondary school. We also include an indicator that measures the percentage of children reported as failing at least one class in the previous year.

Finally, we consider the impact of the student-teacher ratio on school enrollment. As DrPze and Kingdon (2001) have noted, it is inappropriate to assume that the student-teacher ratio is exogenous as this will clearly be affected by the enrollment decisions in

---

[8] About 90 percent of children attend telesecondary schools, which tend to be more basic than the larger technical secondary schools. Telesecondary schools are thought to be a cost-effective manner to bring secondary schooling to rural areas. These are generally small buildings with a television, which shows (by satellite) daily videos on each subject matter (e.g., math and Spanish). Instead of a teacher, there is an assistant to help children with exercises performed after seeing the videos.

communities. We use two strategies to address this issue. First, as in DrPze and Kingdon (2001), we include the potential student-teacher ratio (instead of the actual student-teacher ratio), defined as the number of children under 17 years who have completed primary education. Second, we instrument the actual student-teacher ratio using the potential student-teacher ratio. As both approaches gave very similar results, we only report estimations based on the first strategy.

IMPACT RESULTS

Table 3 presents the estimates of the total program impact of PROGRESA on secondary school enrollment.[9] From an average enrollment for boys in secondary school of 65 percent prior to the program, the results indicate an increase of about 8 percentage points in the fall of 1998, and are lower in 1999 at 5 percentage points. For girls, who had an initial secondary school enrollment of nearly 53 percent, the impacts are somewhat higher, with both years exhibiting an increase of about 11 to 12 percentage points. That is, by 1999, the program impact on secondary school enrollment for girls is around double the level for boys. The decrease in program impact for boys reflects the fact that many of those initially returning to school because of the grants subsequently drop out the following year.

Table 3 also reports the results when we add the supply-side characteristics. Perhaps surprisingly, we find that the estimated coefficients on the program dummy

---

[9] We do not include the full regression results; these are available upon request.

remain similar to those estimated previously without the inclusion of supply-side characteristics. In fact, in all cases, the program impact is slightly higher than previously, although not substantially higher. For the purpose of our cost-effectiveness analysis below, we focus on the lower estimates, since these may better reflect the extra years of education resulting from the program.

Table 3—Program impact on enrollment in secondary school, for boys and girls

	Boys			Girls		
	Initial 1997	November 1998	November 1999	Initial	November 1998	November 1999
Secondary enrollment	0.653			0.528		
Without supply side						
Program dummy		0.079	0.053		0.117	0.120
		(3.12)	(1.83)		(4.45)	(3.70)
With supply side						
Program dummy		0.085	0.057		0.126	0.132
		(3.70)	(1.95)		(4.75)	(3.98)
Distance to school (kilometers)		-0.079			-0.114	
		(6.68)			(7.83)	
Distance squared		0.004			0.007	
		(3.73)			(3.35)	
School is telesecondary		-0.098			-0.138	
		(1.70)			(2.74)	
Percent teachers with high school degree		0.30			0.176	
		(0.40)			(2.53)	
Percent students failing		-0.020			-0.243	
		(0.11)			(1.38)	
Child/teacher ratio		-0.002			-0.0007	
		(1.71)			(0.63)	

Note: These estimates are generated by double-difference regression analysis of individual-level data.

What is the intuition behind the result that the impact of program participation is not reduced through the inclusion of supply-side variables? Note that it does not necessarily imply that the program has not been accompanied by an improvement in supply in the communities where it operates. In fact, the results suggest a story in which supply improved in treatment communities but also in control communities. This is supported by our earlier descriptive analysis, which showed some improvement in supply-side characteristics in both treatment and control communities. As previously shown, in both control and treatment communities, average distance to the nearest secondary school has decreased by 10 percent between 1997 and 1999. Given the proximity of treatment and control communities, it would in fact be difficult to improve services in treatment communities without improving services for control students, because in many cases, they are attending the same schools.

Table 3 reports the estimated impacts of the supply-side variables we have included in our regressions. Most importantly, for both boys and girls, distance to secondary school has a consistently large and negative effect on the probability of enrolling in secondary school. The impact is, in general, much larger for girls than for boys. For girls, a reduction in distance to the nearest secondary school of 1 kilometer from the current mean of about 2 kilometers would result in an increase in the probability of attending by approximately 8.6 percentage points, whereas for boys, the corresponding increase would be approximately 6.3 percentage points.[10]

[10] Based on the baseline ENCASEH97 data, just over 30 percent of children under 18 years old (17 percent of localities) who completed primary school (and are thus eligible to go attend secondary school) have a secondary school in their community. Among those without a school in their community, the average distance traveled to and from school each day was 3.7km, taking on average nearly 100 minutes and costing nearly $10. The average annual travel cost was nearly $316, or nearly 15 percent of the average education subsidy received by households.

When the closest secondary school is a telesecondary school, as opposed to a general or technical secondary school, this is associated with a large reduction in the probability of attending school of the order of 10–14 percentage points (although, for boys, the coefficient is barely significant at the 10 percent level). Nevertheless, this may be an overestimate if telesecondary schools are placed precisely in areas with poor enrollment and attendance rates. As mentioned earlier, this variable may also be correlated with other omitted characteristics of the community. Our measure of human capital of the teachers has a positive and significant effect on school enrollment for girls only. Finally, with respect to the potential student-teacher ratio, this has a negative and significant effect (at the 10 percent level) only for boys.

In summary, our impact analysis has shown large impacts of PROGRESA on secondary school enrollment, particularly for girls. By including supply variables in our regression analysis, we can interpret these impacts as largely reflecting the impact of the educational grants, rather than improvements on the supply side. With regard to the supply-side variables, the analysis has shown that the most consistent and important determinant of school enrollment at the secondary school level is distance, with larger negative effects on girls than boys. Our results on the impact of other school quality variables show mixed results, with few variables significant at more than the 10 percent level (quite weak, given our number of observations) and rarely affecting enrollment levels. In the rest of the paper, we concentrate on a comparison of the cost-effectiveness of education grants with the policy of reducing distance by constructing new schools.

5. COST-EFFECTIVENESS ANALYSIS

We now present the results of our cost-effectiveness analysis, which integrates the impact analysis with the cost side. We start by translating our impact estimates into extra years of schooling generated by the program. We then combine the effectiveness measures with costs to calculate the cost of achieving an extra year of schooling, which we compare across the demand- and supply-side components of the program.

EFFECTIVENESS

We measure the effectiveness of the education grants in terms of extra years of schooling generated, separately for boys and girls. We also calculate the effectiveness of the construction of new schools, which decreases the distance to the nearest school and thereby increases enrollment. As discussed earlier, we adopt an indirect method for calculating extra years of schooling, i.e., we use the impact on the enrollment rate and assume that an extra year of enrollment is equivalent to an extra year of education.

In order to identify the impact of the program on years of schooling, we ask how many extra years of schooling a cohort of 1,000 children would receive. This is derived as the difference between the total years of schooling they would receive after the program (i.e., given the higher enrollment rates) compared to before the program. Consistent with the regression analysis, we focus on *conditional* enrollment rates, i.e., the enrollment rates conditional on having reached a certain grade level. For example, a conditional enrollment rate of 0.3 in grade 7 implies that 30 percent of those children who complete primary school (i.e., the first six grades) continue in school and enroll in junior secondary school.

Our measure of effectiveness is based on the impact estimates derived above. The regression coefficient on the program dummy gives an estimate of the impact of the program on the average conditional enrollment rate (S) in the sample of children whose maximum grades achieved lie between grades 6 and 8 so that they are eligible to enroll in grades 7–9 (i.e., junior secondary school) and thus to receive transfers. This can be calculated as

$$S = \frac{R_7 + R_7 R_8 + R_7 R_8 R_9}{1 + R_7 + R_7 R_8},$$

where R_i is the conditional enrollment rate for grade i. We assume that the enrollment impact is concentrated in the transition year from primary school (i.e., impacts only on grade 7), consistent with the pattern shown in Figures 1 and 2 comparing conditional enrollment rates in both control and treatment localities (for boys and girls separately) based on ENCEL98.[11] Where in the grade structure one allocates the impact is important, both because allocating it earlier means that the effect lasts for more years, thus giving

[11] Specifically, using conditional enrollment rates before the program, we calculate the total number of years of education for a cohort of 1,000 children (Y_0) and use this to calculate an average conditional enrollment rate before the program as $S_0 = (Y_0/1{,}000)$. The average conditional rate after the program is then calculated as $S_1 = S_0 + P$, where P is estimated program impact. We then calculate the total number of years of education after the program as $Y_1 = Y_0(S_1/S_0)$ and allocate these to grade 7 to arrive at a new conditional enrollment rate of $R^*_7 = (Y_1 - Y_0)/1000$. The results were not significantly altered by alternatively assuming that the impact is distributed evenly throughout the three years of secondary school.

Figure 1—Enrollment rates treatment versus control by grade, for girls 1998

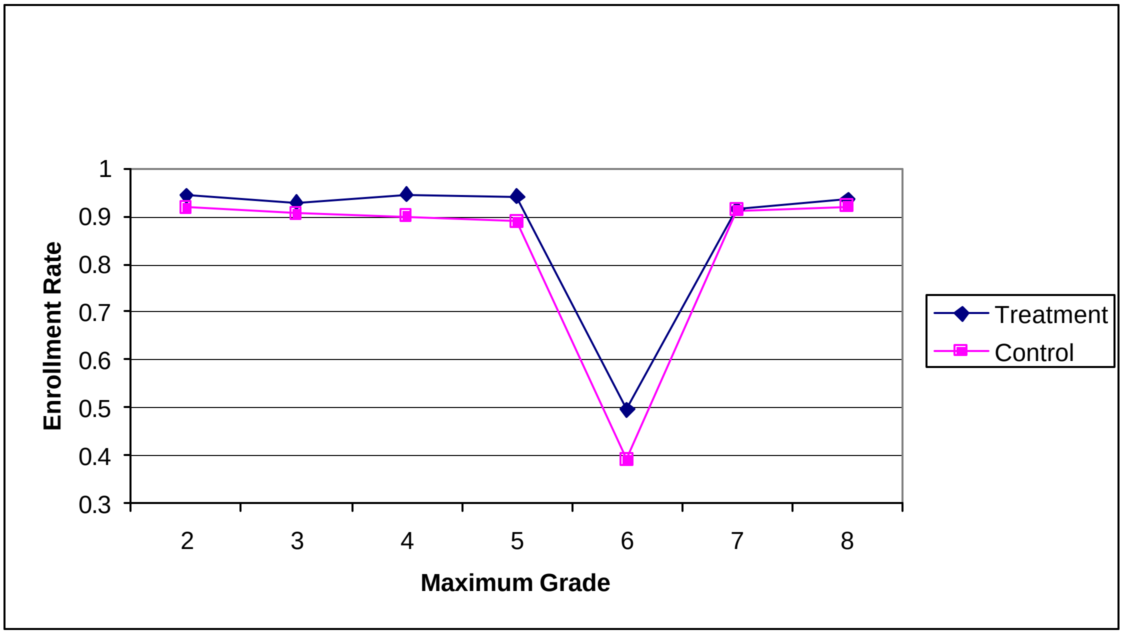
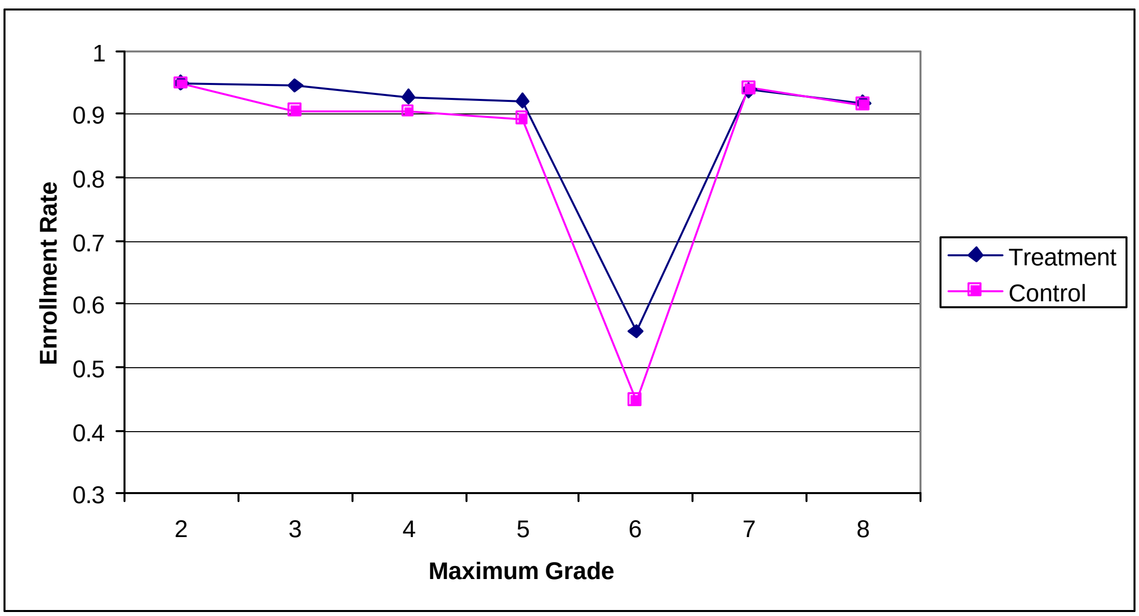


Figure 2—Enrollment rates treatment versus control by grade, for boys 1998



higher impact estimates, but also because the grant amounts differ by grade level. With grants increasing by grade, both these factors offset each other in the calculation of cost-effectiveness ratios.

EDUCATION GRANTS

Table 4 presents the results separately for boys (first four columns) and girls (second four columns). The first column gives enrollment rates before the program, taken from the baseline data. The second column presents the program impact on enrollment rates based on our regression estimates, adjusted so that all of the effect is concentrated in the transition year from primary school. The third column presents the enrollment rates after the program, which are simply the sum of the first two columns. The final column calculates the extra years of schooling attributed to the program as the difference between the third and first columns applied to a cohort of 1,000 children starting in the first grade of secondary school.

Table 4—Impact of education grants on extra years of secondary education, for boys and girls

	Boys conditional enrollment				**Girls conditional enrollment**			
	Before	**Impact**	**After**	**Extra years**	**Before**	**Impact**	**After**	**Extra years**
Grade								
7	0.345	0.094	0.440	94.5	0.265	0.198	0.463	198.3
8	0.903	0.000	0.903	85.3	0.895	0.000	0.895	177.5
9	0.866	0.000	0.866	73.8	0.879	0.000	0.879	156.1
Total				253.8				531.9

The conditional enrollment rates across grades show a clear pattern for both boys and girls: only 27 percent of girls and 35 percent of boys who finish primary school go on to enroll in junior secondary school, but thereafter a very high percentage (86–90 percent) continue into the other two years. The regression estimates of 0.057 and 0.132 for boys and girls, respectively,[12] translate into increases in conditional enrollment rates of 0.094 and 0.198, respectively, when concentrated in grade 7, the transition year from primary school. For a representative cohort of 1,000 boys and 1,000 girls, these estimates imply 254 and 532 extra years of schooling for boys and girls, respectively, a clear bias in favor of girls and sufficient to nearly equalize average conditional enrollment rates in secondary school, which after the program are 61 percent for girls and 62 percent for boys.

SUPPLY EXPANSION

Simultaneous to the program transfers, there has been an expansion of the supply side of education. Here we are specifically concerned with expansion on the extensive margin (i.e., more schools) rather than on the intensive margin (i.e., improvements in the quality of education). The former manifests itself through a decline in the distance to the

[12] We use the program impact estimates from 1999, which are substantially smaller for boys and slightly larger for girls compared to those in 1998. For boys, this may be an overestimate if one expects this impact to fall even further over time. However, as progression rates to secondary school improve due to the program, impact may increase over time. For example, take a 14-year-old boy who leaves school after grade 6 (primary completion) and so is three years out of school when the program is implemented. Because of his age relative to most of those in grade 6 (14 versus 12 years old), he may decide not to take up the program. The program will reduce these age gaps over time and so one expects more 14 year olds to enroll over time.

nearest school. As indicated earlier, since children from both control and treatment localities very often attend the same schools, we find that both groups experience similar declines in the average distance to the nearest school over our sample period. We use the entire sample (both treatment and control group) for the purpose of our analysis.

Analysis of the distance variable indicates that the average distance has decreased from about 2.2 kilometers in 1997 to 2.1 kilometers in 1998 and 2.00 kilometers in 1999. To estimate the impact of these decreases on enrollment rates, we use the coefficients on distance (and its square) from the regressions presented earlier in Table 3 and calculate the change in the probability of enrollment (*dS*) as

$$dS = -0.079 + (2*0.004)\, D \qquad \text{(for boys)}\ ,$$

$$dS = -0.114 + (2*0.007)\, D \qquad \text{(for girls)}\ ,$$

where *D* is the distance (in kilometers) to the nearest school in 1997. Then, *dS* is multiplied by the actual change in distance to get the change in enrollment due to extensive expansion. This is calculated for each individual in the sample and averaged to get the expected impact on enrollment. When the enrollment impacts are concentrated on the transition year (Table 5), a cohort of 1,000 girls entering grade 7 will receive 27 extra years of education in junior secondary school as a result of the combined decrease in distance from 1997–1999. Reflecting the timing of school constructions (and thus decreases in distance), the majority of this impact occurs in 1998 (17 extra years). The corresponding numbers for boys are 25 extra years, with 14 of these occurring in 1998.

Table 5—Effect of decreasing distance on enrollment (allocated to transition year)

	Grade	Enrollment			Extra years of education		
		Before	**Impact98**	**Impact99**	**1997-8**	**1998-9**	**1997-9**
Girls	7	0.265	0.006	0.004	6.46	3.76	10.22
	8	0.895	0.000	0.000	5.78	3.36	9.14
	9	0.879	0.000	0.000	5.08	2.96	8.04
Total					17.33	10.07	27.40
Boys	7	0.345	0.004	0.004	3.70	4.41	8.10
	8	0.903	0.000	0.000	6.83	3.39	9.22
	9	0.866	0.000	0.000	5.01	2.91	7.92
Total					14.53	10.71	25.24

COST-EFFECTIVENESS

We now address the issue of the cost of generating the above impacts. We calculate separately the cost per extra year of schooling generated by schooling subsidies and school construction for both boys and girls. Table 6 presents the calculation of the cost of an extra year of schooling in the case of education subsidies. Since the education subsidy is paid to all those that enroll, we calculate the total cost of generating the total impacts identified above by multiplying the total enrollment by grade after the program for the cohort of 1,000 children by the appropriate subsidy rate as presented in Table 1. We then sum across the appropriate grades. This number is then divided by the *extra* years of schooling generated by the subsidies to get the cost per extra year of schooling.[13]

[13] Notice that there are two forces pulling cost-effectiveness ratios (CERs) for grants in opposing directions. On the one hand, the fact that children only receive the grant if they attend school tends to reduce the CER. On the other, the fact that all children attending school receive grants, regardless of whether they would have done so in the absence of grants, tends to increase the CER.

The cost per extra year of schooling is $12,557 for boys and $6,904 for girls.[14] Note that the higher enrollment effect for girls easily offsets their higher grant levels.

Table 6—Cost of extra years of education through secondary grants

	Secondary		
	Boys	**Girls**	**Average**
Total enrollment	1,181	1,243	1,212
Total impact	254	532	393
Grants	3,184,059	3,671,964	3,428,012
Cost per year	12,557	6,904	9,730

We can now compare the cost of generating an extra year of schooling using subsidies with that of building new schools. Using the merged school supply and household dataset, we calculate that in both 1998 and 1999, six new schools were built compared to the previous year (Table 7).[15] The number of different types of schools in the sample is the number of separate schools attended by the sample children. When the school located closest to the community changes, we assume this is due to the building of a new school nearer to the locality. A school added to the sample is thus considered to be

[14] We also made the same calculation for primary school grants and find higher CERs of $22,552 for boys and $26,331 for girls.

[15] This calculation is based on observations of the number of schools that were constructed within the evaluation communities. It is also possible that distance to secondary school was reduced by construction of schools outside of the evaluation communities. This would increase the estimated costs (but not affect impact) so that our estimate of costs for reducing distance to school should be considered a lower-bound estimate.

a newly built school, although we assume the old school still exists. In 1998, four of these were telesecondaries and two were technical secondaries. In 1999, all six new schools were technical secondaries.

Table 7—Number of new schools in evaluation sample

	Number of secondary schools			Number of new schools	
School type	**1997**	**1998**	**1999**	**1998**	**1999**
General secondary	18	16	16	-2	0
Workers' secondary	2	2	1	0	-1
Technical secondary	27	29	35	+2	+6
Telesecondary	434	438	436	+4	-2
Number of new schools				6	6

Note: Technical secondary includes a category "alternative types." The number of secondary schools is the number of the different types attended by children in the sample. When a school disappears from the sample, it is assumed to be because children now go to another school (possibly a new school). So we count only the schools added to the sample.

The cost of building and operating such schools is presented in Table 8. Infrastructure and equipment costs are about $1.38 million for telesecondary schools and about $2.4 million for technical secondary schools. Personnel and operating costs are $170,000 per year for telesecondary schools versus $427,000 for technical secondary schools. Personnel and operating costs are assumed to recur every year, while furniture and equipment and infrastructure are assumed to be fixed, up-front costs.

The cost of generating an extra year of education (i.e., the cost-effectiveness ratio, CER) through extensive expansion of the school system is presented in Table 9 for boys

Table 8—Cost of school construction (1999 pesos)

Item	Telesecondary	Technical secondary
Personnel	169,624	426,356
Operating costs	302	718
Furniture and equipment	20,576	44,771
Infrastructure	1,360,000	2,400,000
Total	1,550,502	2,871,845

Table 9—Cost-effectiveness ratios for school building

	r = 0%			r = 5%		
	20 Years	30 Years	40 Years	20 Years	30 Years	40 Years
Girls 1997-98	118,575	108,560	103,552	136,749	127,620	123,550
Girls 1998-99	327,174	302,905	290,771	371,211	349,090	339,228
Girls 1997-99	195,268	180,013	172,385	222,951	209,046	202,846
Boys 1997-98	141,357	129,417	123,447	163,023	152,140	147,287
Boys 1998-99	307,758	284,930	273,515	349,181	328,374	319,097
Boys 1997-99	211,952	195,393	187,113	242,000	226,907	220,177
Average 1997-98	129,966	118,989	113,500	149,886	139,880	135,419
Average 1998-99	317,466	293,917	282,143	360,196	338,732	329,162
Average 1997-99	203,610	187,703	179,749	232,476	217,976	211,511

and girls separately and with and without discounting. We also consider different scenarios with respect to how long the school will "last" before requiring additional investment. The table presents estimates for both years, which differ according to how many and which type of secondary schools was constructed. A number of points emerge from the table. First, the cost decreases the longer one assumes that the extensive supply effect to last, reflecting the fact that up-front infrastructure costs are spread over a longer period. Second, the cost decreases as the discount rate increases, reflecting the fact that a greater proportion of the enrollment is distributed further in time relative to costs. Third, the cost is lower for girls than for boys, reflecting the larger effect of lower distances on girls' enrollment relative to boys'. Fourth, the cost increases over time, reflecting the fact that telesecondary schools are cheaper to build relative to technical secondaries and the majority of new schools in 1998 were telesecondaries (four of six), whereas all six new schools in 1999 were technical secondaries. Also, the effect of new schools on average distance is lower in 1999 relative to 1998.

Comparing the cost-effectiveness of education subsidies with that of extensive expansion, it is clear that education subsidies are a substantially more cost-effective method of increasing the number of children enrolled in school. The lowest CER for extensive expansion is for a 40-year period of impact on girls' enrollment with zero discounting at just below $103,600 per extra year of schooling. The largest CER in the case of secondary education subsidies was just over $12,600 for boys. Therefore, when combined with the fact that the parameters we have used were, if anything, biased against the demand-side, our conclusion that the demand-side program is a cost-effective way of getting more children into secondary school would seem to be quite robust.

6. CONCLUDING REMARKS

In this paper we have been concerned with evaluating the relative cost-effectiveness of two policy instruments aimed at increasing enrollment rates in junior secondary school in poor communities in rural Mexico. The two policy instruments are (1) demand-side subsidies in the form of monetary transfers conditioned on children's enrollment in school and (2) supply-side expansion through building more schools. The former has its effect through increasing the private benefit from schooling, while the latter has its effect through decreasing the private cost of schooling associated with the time and money costs of traveling to and from school. We have presented results that show that, in this context, demand-side policies are a much more cost-effective instrument than the alternative of expansion on the supply side. The large differences in cost-effectiveness ratios between grants versus school construction suggest that this result is likely to be fairly robust.

We are aware that we have focused only on two very specific alternatives, which furthermore represent the policies actually pursued by the government and not necessarily the optimal policy (e.g., perhaps schools were built in the "wrong" locations). Therefore, our results should not be broadly interpreted to mean that demand-side interventions are the only attractive alternative in terms of increasing enrollment rates. Other more focused instruments may exist on the supply side that might be cost-effective in specific environments. For example, given the importance of distance in secondary school, especially for girls, improving transport conditions to and from secondary schools may be an attractive policy option. Further analyses of this type should be pursued using alternative indicators and in other contexts to analyze the extent to which our conclusions may be more generalizable. The analysis done here does, however, provide a useful model of the type that should be a prerequisite to the allocation of scarce resources in the important area of education.

REFERENCES

Barro, R., and X. Sala-I-Martin. 1995. *Economic growth*. New York: McGraw-Hill.

Behrman, J., and P. Todd. 1999. Randomness in the experimental samples of PROGRESA (education, health and nutrition program). Report submitted to PROGRESA. International Food Policy Research Institute, Washington, D.C.

Behrman, J., P. Sengupta, and P. Todd. 2000. The impact of PROGRESA on achievement test scores in the first year. Report submitted to PROGRESA. International Food Policy Research Institute, Washington, D.C..

Behrman, J., P. Sengupta, and P. Todd. 2001. Progressing through PROGRESA: An impact assessment of Mexico's school subsidy experiment. International Food Policy Research Institute, Washington, D.C.

Drèze, J., and G. Kingdon. 2001. School participation in rural India. *Review of Development Economics* 5 (1): 1–24.

Hanushek, E. 1995. Interpreting recent research on schooling in developing countries. *World Bank Research Observer* 10 (2): 227–246.

Kremer, M. 1995. Research on schooling: What we know and what we don't (A Comment on Hanushek). *World Bank Research Observer* 10 (2): 247–254.

Psacharopoulos, G. (1994): Returns to investment in education: A global update. *World Development* 22 (9): 1325–1343.

Rosenzweig, M., and K. Wolpin. 1986. Evaluating the effects of optimally distributed public programs: Child health and family planning interventions. *American Economic Review* 76 (3): 470–482.

Schultz, T. P. 1988. Education investments and returns. In *Handbook of development economics*, vol. I, ed. H. Chenery and T. N. Srinivasan. Amsterdam: Elsevier Science Publisher B. V.

Schultz, T. P. 2000. School subsidies for the poor: Evaluating a Mexican strategy for reducing poverty. Report submitted to PROGRESA. International Food Policy Research Institute, Washington, D.C.

Skoufias, E., B. Davis, and S. de la Vega. 2001. Targeting the poor in Mexico: An evaluation of the selection of households into PROGRESA. *World Development* 29 (10): 1769-1784.

FCND DISCUSSION PAPERS

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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