NÚMERO 341

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A Cost-Effectiveness Analysis of Demand-and Supply-Side Education Interventions: The Case of PROGRESA in Mexico

NOVIEMBRE 2005

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Acknowledgments
Both authors would like to thank John Maluccio, an anonymous referee, and staff and colleagues at PROGRESA, CIDE and IFPRI for helpful comments. Special thanks to Daniela Sotres and Mari Carmen Huerta for help will collecting the supply-side data.
JEL Classification Numbers: H52, I2, O15
Abbreviations: PROGRESA, SEP, ENCASEH, ENCEL, GIS, CER.
Number of Figures: 1 Number of Tables: 7
Date: First sent, June 14, 2001; Resent, December 19, 2001.

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Abstract

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 (i) extensive expansion of the school system (i.e., bringing education to the poor) and (ii) subsidizing investment in education by the poor (i.e., bringing the poor to the education system). To this end, we evaluate PROGRESA, a large poverty alleviation program recently introduced in Mexico, which 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.

Resumen

En este artículo abordamos la manera más rentable de mejorar el acceso a la educación para los hogares pobres en países en vías de desarrollo. Consideramos dos alternativas: (i) extensa ampliación del sistema educativo (llevar la educación a los pobres) y (ii) subsidiar la inversión de los pobres en educación (llevar a los pobres al sistema educativo). Con esta finalidad evaluamos PROGRESA, un amplio programa de alivio de la pobreza introducido recientemente en México, que subvenciona la educación. Usando los estimadores de regresión de doble-diferencia para la información recolectada antes y después el programa en hogares de "control" y de "tratamiento" seleccionados aleatoriamente, estimamos los impactos relativos del lado de la demanda —y la oferta— de los componentes del programa. Combinando estas estimaciones con la información de costo, encontramos que los subsidios del lado de la demanda son sustancialmente más rentables que del lado de la oferta.

Introduction

There is a vast body of literature that identifies the expansion of formal education as a key component of successful development strategies. However, there is still much disagreement about how best to allocate scarce public resources within the education sector. The policy debate is typically couched in terms of the relative importance of improved school quality *vis-à-vis* improved school access, with different researchers drawing very different policy conclusions from the same body of empirical evidence (Hanushek, 1995; Kremer, 1995).

The quality versus access debate addresses the issue of the most cost-effective way of achieving a given total years of education. Yet concerns for equity (i.e., the distribution of this 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, who 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 the poor.

In this paper we are specifically 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, (i) extensive expansion of the school system (i.e., bringing education to the poor), and (ii) subsidizing investment in education by the poor (i.e., bringing the poor to the education system). To our knowledge, this is one of the first studies that rigorously analyses the relative cost-effectiveness of demand-side subsidies versus expansion of supply within the context of a developing country.

Our analysis is based on an evaluation of a relatively unique and large program, (i.e., Programa de Educación, Salud y Alimentación; PROGRESA hereafter), which was introduced by the Mexican government in 1997 and subsidizes investment in human capital by poor households by conditioning cash transfers to families on enrolling their children in school and making regular health clinic visits.

There is also a supply-side component to the program with resources allocated towards improving school quality and access (i.e., more teachers, health clinic staff, higher salaries and extensive expansion

We analyze the cost effectiveness of the secondary education component of PROGRESA, focusing on the program goal of increasing school enrollment at this level (grades 7 to 9).[1] In the poor communities where PROGRESA operates, only about half of all children continue to secondary school after completing primary school (grades 1 to 6). This paper compares the cost effectiveness of the 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 impacts and cost each part accordingly. We show that the demand-side component is a much more cost-effective way of increasing enrollment levels relative to building additional schools.

1.- Program Design

PROGRESA is a large poverty alleviation program implemented in Mexico in 1997, which targets its benefits directly to the population in extreme poverty in rural areas. The program is made up of three closely linked components (namely, education, health and nutrition) based on the belief that there are positive interactions between the three components. Our analysis concentrates solely on the education component.

Under the education component, the program provides monetary education grants for each child under 18 years of age enrolled in school between the third grade of primary and the third grade of secondary school. In order to compensate for the forgone income from children, the grants increase with grades. Additionally, at the secondary school level, 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[2] in the third grade of primary to $265 pesos for boys and $305 pesos for girls in the third year of secondary school.

In order to provide incentives for human-capital accumulation, benefits are contingent on the fulfillment of certain obligations by the beneficiary families. Grants are linked to school attendance of children so that if a child misses more than 15% of school days in a month (for unjustified reasons), 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% of the monthly income of beneficiary families. 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.

2.- 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 both household-level data generated from a natural experiment designed for the evaluation of PROGRESA and school-level data collected separately from the SEP, we estimate the separate impacts of demand-side subsidies and of increased supply on school enrollment. We combine these estimated impacts with 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.

2.1.- Household-level Data from PROGRESA *Evaluation*

For the purposes of program evaluation, PROGRESA carried out a social experiment in which a random sample of 506 eligible communities were selected from the 7 Mexican states where the program was first implemented. Communities were randomly assigned to a treatment group (i.e., 320 communities that received transfers) and a control group (i.e., 186 communities that did not). All of the 24,077 households in both the treatment and control communities were surveyed prior to implementation of the program. This baseline household census, containing information on households' socio-economic characteristics, was collected in November 1997 (ENCASEH97). Households in the treatment group began to receive benefits in March of 1998. Periodic surveys were carried out after program implementation, approximately every six months. In our analysis, we use the ENCASEH97 and two post-program rounds (ENCEL98, ENCEL99) collected in October 1998 and November 1999.

2.2.- Supply Data

As we noted earlier, program transfers were accompanied by extensive expansion of supply aimed at avoiding deterioration in the quality of schooling. Without this component, it might be expected that overall school quality would 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.

Using GIS software, we identify the nearest secondary school to each community and match its characteristics to each individual child, including the distance to the school. We thus assume that the available supply side 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 km. Less than a third of our sample of children have a secondary school inside their community. 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 1 to 5 classes during the previous year, number of classrooms with more than one grade, type of school, and source of funding.

The data 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. Overall, mean distance decreases from about 2.2 to 2.0 kilometers over the two years, both in treatment and control communities. Given the close proximity of control and treatment communities, it is likely that many children from both attend the same schools. Therefore, extra resources to schools are likely to benefit children in both sets of communities. This will have implications below for how we identify demand- and supply- side effects of the program, given the absence of an explicit "control" group for supply-side interventions.

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 ratio and the student-classroom ratio increase only slightly over time, while the number of multiple classes (classrooms where more than one grade is being taught) decreases, all being 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.

3.- Identification of Program Impacts

Previous studies of the impact of PROGRESA on education outcomes have measured 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). This approach we adopt in this paper allows us to determine which part of the impact might be attributed to the education grants versus improvements in supply made by the program. We start this section by generating a reference set of estimates of total program impact, comparable to those generated by 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, which we then translate into an indicator of extra years of education due to the program.[3]

3.1.- 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 pre-program differences between the treatment and control groups are eliminated in the estimation of impacts. Under the assumption that any unobserved heterogeneity between 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 surveys, 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:

$$S_{it} = \sum_{t=1}^{3} \alpha_{0t} + \alpha_1 T_i + \alpha_2 T_i R_2 + \alpha_3 T_i R_3 + \sum_{j=1}^{J} \beta_j X_{jit} + \varepsilon_{it}$$

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 total 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. Note that α_1 is expected to be insignificantly different from zero (that is, pre-program 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, α_{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.

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. However, once we add supply indicators of schooling 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.

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 that this distance can be reduced is through the construction of new schools.

Second, we have information on the type of secondary school available. In the rural communities that we analyze, the dominant type of secondary school is the tele-secondary.[4] Third, we include a variable capturing the education level of the teacher, measured by the percentage of teachers with at least a high-school education at the available secondary school. Fourth, 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 DrΠze 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 communities. We therefore include the potential (as opposed to actual) student-teacher ratio, defined as the number of children under 17 years who have completed primary education divided by the number of teachers.

3.2.- Impact Results

Table 1 presents the estimates of the total program impact on secondary school enrollment. From an average enrollment for boys in secondary school of 65% 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%, the impacts are somewhat higher; both years exhibiting an increase of about 11 to 12 percentage points, roughly double the 1999 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 (Coady and Parker, 2001).

Table 1 also reports the results when we add the supply-side characteristics. Perhaps surprisingly, we find that the estimated coefficients on the program dummy 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. The lack of sensitivity in the impact estimates reflects the proximity of control and treatment communities, which simultaneously benefit from supply-side improvements. Below we focus on the lower estimates that may better reflect the extra years of education resulting from the program.

We now turn to the estimated impacts of the supply-side variables we have included in our regressions (Table 1). 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 km from the current mean of about 2 km 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.

When the closest secondary school is a tele-secondary 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% level). Nevertheless, this may be an overestimate if tele-secondary 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% level) only for boys.

4.- 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.

4.1.- 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 1000 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% of those children who complete primary school (i.e., the first six grades) continue in school and enroll in 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:

$$S = (R_7 + R_7R_8 + R_7R_8R_9) \ / \ (1 + R_7 + R_7R_8)$$

where R_i is the conditional enrollment rate for grade *i*. To be consistent with the data, we assume that the enrollment impact is concentrated in the transition year from primary school (i.e., impacts only on grade 7).[5]

Education Grants

Table 2 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 is 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 1000 children starting in the first grade of secondary school.

The conditional enrollment rates across grades show a clear pattern for both boys and girls: in 1997 only 27% of girls and 35% of boys who finish primary school go on to enroll in junior secondary school but thereafter a very high percentage (i.e., 86%-90%) continue into the other two years. The regression estimates of 0.057 and 0.132 for boys and girls respectively 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 1000 boys and 1000 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% for girls and 62% 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 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 km in 1997 to 2.1 km in 1998 and 2.00 km 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 1 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 kms) 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 3), a cohort of 1000 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 1997-99. Reflecting the timing of school constructions (and thus decreases in distance), the majority of this impact occurs in 1998 (i.e., 17 extra years). The corresponding number for boys is 25 extra years, with 14 of these occurring in 1998.

4.2.- 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 4 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 1000 children by the appropriate subsidy rate. 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.[6] The cost per extra year of schooling is $12,557 for boys and $6,904 for girls, so that the higher enrollment effect for girls easily offsets their higher grant levels.[7]

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 data set, we calculate that in both 1998 and 1999 six new schools were built compared to the previous year. 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 a newly built school, although we assume the old school still exists. In 1998, four of these were tele-secondaries and two were technical secondaries. In 1999, all six new schools were technical secondaries.

The costs of building and operating such schools are as follows. Infrastructure and equipment costs are about $1.38 million pesos for tele-secondary schools and about $2.4 million for technical secondary schools. Personnel and operating costs are about $170,000 per year for tele-secondary 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 5 for boys 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 types of secondary schools were constructed.

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 forty 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.

Conclusions

Our analysis shows that demand-side subsidies were a substantially more cost-effective method than school building in terms of increasing secondary school enrollments in the poorest areas of rural Mexico. However, 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 that might be relatively more cost-effective in specific environments. For example, given the importance of distance (especially for girls), improving transport conditions to and from secondary schools may be an attractive policy option. Also, whether or not expanding enrollment is a sensible policy will obviously depend both on the quality of education they receive as well as on having appropriate macroeconomic policies that enable the absorption of this educated labor without decreasing returns. Further analyses of the type conducted here should be pursued using alternative indicators and in other contexts to analyze the extent to which our conclusions may be more generalizable. But 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.

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[1] See Schultz (2000) and Behrman, Sengupta and Todd (2001) for an analysis of program impacts on enrollment, progression and return rates. A more detailed discussion of the program and our empirical results are available in Coady and Parker (2001, 2002). Reports addressing a wider range of impacts can be downloaded from http://www.ifpri.org/themes/PROGRESA.htm.

[2] We use $ to denote Mexican pesos. The exchange rate in 1999 was approximately 10 pesos per US dollar.

[3] We use an indirect approach (estimating years of extra schooling from enrollment impacts) rather than a more direct approach of directly estimating 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 which measures highest grade completed. 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).

[4] About 90% of children attend tele-secondary schools, which tend to be more basic than the larger technical secondary schools. Tele-secondary 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.

[5] Specifically, using conditional enrollment rates before the program, we calculate the total number of years of education for a cohort of 1000 children (Y_0) and use this to calculate an average conditional enrollment rate before the program as $S_0=(Y_0/1000)$. 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.

[6] 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, regardless of whether they would have done so in the absence of grants, receive grants tends to increase the CER.

[7] We also made the same calculation for primary school grants and find higher cost-effectiveness ratios of $22,552 for boys and $26,331 for girls.

Table 1

Program impact on enrollment in secondary school for boys and girls

	Boys			Girls		
	Initial 1997	Nov. 1998	Nov. 1999	Initial	Nov. 1998	Nov. 1999
Secondary Enrollment	0.653			0.528		
Without Supply Side						
Program Dummy		0.079	0.053		0.117	0.12
		-3.12	-1.83		-4.45	-3.7
With Supply Side						
Program dummy		0.085	0.057		0.126	0.132
		-3.7	-1.95		-4.75	-3.98
Distance to school (km)		-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	
		-1.7			-2.74	
% teachers with HS degree		0.3			0.176	
		-0.4			-2.53	
% students failing		-0.02			-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.

Table 2
Impact of Education Grants on Extra Years of Secondary Education For Boys and Girls

	Boys Conditional Enrollment				Girls Conditional Enrollment			
	Before	Impact	After	ExtraYrs	Before	Impact	After	ExtraYrs
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

Table 3
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

Table 4
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

Table 5
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
Avg. 1997-98	129,966	118,989	113,500	149,886	139,880	135,419
Avg. 1998-99	317,466	293,917	282,143	360,196	338,732	329,162
Avg. 1997-99	203,610	187,703	179,749	232,476	217,976	211,511

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