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THE APPLICATION OF SOCIAL COST-BENEFIT ANALYSIS TO THE EVALUATION OF PROGRESA

David P. Coady

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

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The material presented below is taken directly from a number of sub-reports. The material in Sections 1-3 is taken from Coady (1999). The material in Section 4 is taken from Coady, Perez and Vera-Llamas (2000). The material from Section 5 is taken from Coady (2000). The material from Section 6 is taken from Coady and Parker (2000). This report has also benefited from numerous discussions with these co-authors. Of course, all errors specific to this report are mine alone.

EXECUTIVE SUMMARY

In August 1997 the Mexican government introduced a key component of its overall development and poverty alleviation strategy, the PROGRESA program, in the most marginal rural areas of the country. The expansion of the program across localities took place in phases. By the final phase11 of the program in early 2000, the program included nearly 2.6 million families in 72,345 localities in all 31 states. This constitutes around 40% of all rural families and one ninth of all families in Mexico. The total annual budget of the program in 1999 was around $777 million, equivalent to just under 20% of the Federal poverty alleviation budget or 0.2% of GDP.

The program gives cash transfers to mothers in households classified as "poor", these transfers being conditional on child attendance at school and regular visits by family members to health clinics for preventative check-ups. Households with young children also receive food supplements to improve their nutritional status. Although the program is essentially a demand-side intervention, an important dimension of the program involves the explicit recognition that, for such an intervention to be effective at achieving its ultimate objectives, co-ordination with the supply side is essential. To this end, the education and health ministries are expected to plan to allocate resources to areas where substantial demand increases are experienced in order to avoid deteriorating quality on the supply-side that may frustrate the achievement of program objectives.

In this report we are concerned with the application of a social cost-benefit analysis (SCBA) to PROGRESA. The application of SCBA to the evaluation of the program requires one to identify both the impacts and the costs of bringing about these impacts, and then to compare both of these to determine the overall welfare impact of the program and how effectively the program achieves these welfare impacts relative to alternative policy instruments. When monetary values can be attached to these impacts they are referred to as "program benefits" and the application of cost-benefit analysis (CBA) then involves determining whether benefits exceed costs and by how much. In the absence of a monetary valuation of impacts, one is restricted to the use of cost-effectiveness analysis (CEA), which identifies the cost of bringing about a given impact.

To date the evaluation of PROGRESA has essentially focused on program *impacts* (i.e. reductions in poverty levels, increased school enrollment and attendance, increased use of health services for preventative care, and improved nutritional status). While knowledge of program impacts is an essential component of any economic evaluation, in isolation it provides very little policy guidance. In general, there are a number of policy instruments that could be employed to generate a given impact and these may differ substantially in terms of cost. Choosing an individual instrument, or a combination of instruments, involves identifying those that can achieve the desired impacts at least cost. So it is important then not to lose sight of the cost side of the program (i.e. of the fact that we have to allocate resources to get these impacts) and the need to link costs to outcomes (i.e. to do cost-benefit analysis). This aspect of policy choice is obviously particularly important when budget allocations are tight.

It is important to point out that the objective of the report is not to provide definitive assessments of program design or performance. Rather, we wish to show, firstly, how the application of

SCBA techniques can provide a useful framework for understanding how the program as a whole can contribute towards the overall development objectives of the government, more particularly, the alleviation of poverty. In this spirit, we emphasize that PROGRESA should be viewed as only one component of a more comprehensive development strategy and that other crucial elements of that strategy must not be overlooked. Also, there are other aspects of the program and its design that are not discussed in any detail in this report but which are nonetheless extremely important in forming an overall assessment of the program. The myriad of social and political dimensions is just one obvious example. Secondly, we wish to show how such an analysis can provide valuable insights into how the various components of the program are working towards achieving the specific objectives of the program. Such insights can help to identify areas where reform of the existing program design may enhance the ability of the program to achieve its objectives.

The structure of the report is as follows. The first step in any economic evaluation of a public-sector program (or policy) is to address the underlying motivation for the government intervention. The answer to this question has implications for the most appropriate form of intervention and for program design. In Section 2 we therefore discuss the potential motivations for government intervention to influence the allocation of resources in various sectors of the economy. Conditional on program design, we then evaluate the different components of the program and how effectively they achieve specific program objectives. In Section 3 we provide a more detailed discussion of the objectives and design of the program and develop our approach to the evaluation of the program. Following this, we provide detailed evaluations of the cost structure and cost efficiency of the program (Section 4), of the distributional power of the program and its relative efficiency at alleviating poverty (Section 5) and, finally, of the cost-effectiveness of the program in increasing demand for education. Although in the present version of the report we do not evaluate the health and nutrition components in any detail, we hope to address these in future revisions.

The Economic Analysis of PROGRESA

In our discussion of the economic analysis of public policy we highlight the two main motivations for public interventions aimed at influencing the allocation of resources in the economy: (i) to bring about a more efficient allocation of resources, and (ii) to bring about a more equal distribution of resources. However, this purely economic approach is only one component of the overall public policy debate. Other valid approaches exist, e.g. those that focus on ensuring access to basic needs (including food, shelter and basic infrastructure) as well as those that focus on the wider notion of "capabilities". In addition, other social and political perspectives also play a crucial role in determining the design and implementation of an overall development and poverty alleviation strategy for Mexico. However, it is hoped that the economic analysis presented here will contribute to our understanding of what role PROGRESA plays within this overall strategy and its relative effectiveness at achieving its objectives. In the process, we hope to contribute to the debate on how various components of its design might be reformed so that it can achieve its objectives more effectively.

For the purposes of an economic evaluation, it is useful to interpret PROGRESA as a program with multiple objectives, namely:

- the alleviation of current poverty through targeted cash transfers, and
- the generation of a sustained decrease in poverty by conditioning these transfers on the accumulation of human capital (i.e. education and health status).

The targeting of cash transfers is motivated by the desire to maximize the impact on current poverty for a given program budget. The conditioning of these transfers transforms them into subsidies for human capital accumulation, i.e. households only receive the transfers if they invest in their family's education and health. Regarding the first objective, since the benefits are already in monetary terms the application of CBA is relatively straightforward. However, given the difficulties associated with attaching monetary values to the human capital components of the program, we confine ourselves to the application of cost-effectiveness analysis when evaluating these components of the program. Although the integrated nature of the program is viewed by policy makers as being a crucial determinant of its success, the fact that the program was delivered as a single "package" precludes the evaluation of this dimension. Also, the dynamic nature of the program means that our evaluation of the human capital components of the program must rely on the analysis of intermediate outputs in the human capital accumulation process, e.g. attendance at school and health clinics. In addition, although our evaluation is conditional on the program's operational performance, it is hoped that the analysis, in conjunction with the earlier evaluation of operations, will help identify areas where changes may enhance the program's overall effectiveness.

In summary then, it is important to be aware that we view PROGRESA as only one component of a more comprehensive development and poverty alleviation strategy. The program is, for the most part, a demand-side intervention that attempts to increase households' demand for human capital through increasing enrollment and attendance at school and increasing visits to health clinics for preventative health care. The ability of the program to contribute to the achievement of overall development objectives depends crucially on the presence of other components of the strategy. These are relevant for both dimensions of the program, i.e. current poverty alleviation and human capital accumulation. For example, because of the integrated design of the program (i.e. the reliance on targeted human capital subsidies as opposed to unconditional transfers), some very poor households have been excluded, namely, poor households that live in less marginal localities that were not incorporated into the program and households that were deemed not to have adequate access to the necessary supporting public infrastructure (i.e. schools and clinics). From the perspective of horizontal equity and social justice, it is important that other components of the poverty alleviation strategy compensate for this exclusion. In terms of the human capital objectives, once one gets household members into school and health clinics, it is important to ensure that they receive quality services. In the case of health care, this requires that the package of services they receive are appropriate for the types of health problems to which they are susceptible as well as ensuring that households have access to supporting public infrastructure (e.g. sanitation and sewage facilities). In the case of education, it requires that they achieve high educational standards, including the acquisition of skills that are demanded by potential employers. However, once households invest in and receive higher human capital, it is equally important that the economic environment is such that this greater supply of human capital can be absorbed without diminishing the returns to such capital. In this respect, the key role played by efficient macroeconomic management to promote economic growth and increasing job opportunities is obvious.

An Analysis of PROGRESA's Costs

Although cost analysis is a crucial ingredient to economic analysis, it is also of use in its own right as a management tool since, for example, a detailed analysis of costs can direct management to areas where improved operational efficiency may have a high return. In our cost analysis we start by giving a brief description of the evolution of the program and the various stages in its implementation. We then identify the various ingredients of cost analysis, emphasizing the need to look beyond program costs to include private and social costs. Finally, we present detailed calculations of the various cost components. More specifically, we evaluate the cost efficiency of the program, i.e. the cost of getting transfers into the hands of beneficiaries.

In order to deliver cash transfers to households, program resources must be allocated to a number of activities so that only a fraction of the budget is available for poverty alleviation. These costs can be classified as:

- *targeting costs* associated with the targeting of transfers to the most marginal localities as well as only to the poorest households within these localities
- *conditioning costs* associated with ensuring that households meet their responsibilities by ensuring attendance of children at school and household members at scheduled regular preventative check ups
- *operation costs* associated with the actual operation of the program

In order to receive transfers, households must also incur private costs, including the time and financial costs of traveling to schools and health clinics (i.e. due to the conditioning of the program) as well as to collect the transfers from distribution points. Although we identify time costs, we do not try to attach a value to these. Also, while information on the total private costs is a useful input into policy analysis, for the purposes of evaluation only the *incremental* costs due to the introduction of the program are relevant. In the report we provide details of both.

In order to qualify for the food transfer, household members must make a series of visits to health clinics for check-ups and health lectures. The average distance traveled to the clinic is 3.98km, rising to 5.12km when zeros are excluded (i.e. including only those that have to travel outside of their community to get health services). The average cost of a return trip is $3.95, rising to $12.95 when zeros are excluded. We calculate that the total annual travel cost is on average $95.7 per family. Households also incur time costs in traveling to and from the clinic, in waiting to be seen for a check-up, in attending the check-up itself, and in attending the health lectures. The average travel time to the clinic is just over 118 minutes per return trip. The average annual travel time per household is just over 48 hours, equivalent to roughly 4 hours each month, most of this (c. 68%) being accounted for by the mother's time. On average, households have to wait nearly 39 minutes to be seen for a check-up (or consultation). In total then, each month household members make on average 2.1 trips, each taking nearly two hours travel time, one of these being a health lecture, which takes up one hour, the other 1.1 trips incurring a 40 minute waiting time and a 20 minute consultation time. So, on average, household members incur around 6.3 hours in time costs in order to meet health-clinic attendance requirements.

Households receive $125 per month in food transfers, equal to a $1500 annual transfer. This means that households incur travel costs of $6.38 per $100 received. However, this is a substantial overestimate of the incremental private costs since this cost is only additional for the extra trips brought about by the program. According to Gertler (2000), the program brought about a 30%-50% increase in the number of trips. Using an estimate of a 40% increase, this implies that only 28.6% of total trips are additional. This in turn implies that the incremental private costs of receiving the food transfer are $1.82 per $100 received.

As with health visits, households incur both financial and time costs due to children having to travel to and from school, to attend school, and to undertake homework. For primary level it is assumed that all localities have a primary school so that travel time and money costs are approximately zero. We calculate that secondary school children spend on average nearly 65 minutes traveling to and from school, this rising to nearly 100 minutes when zeros are excluded. The average distance traveled to school is 2.54km, rising to 3.68km when zeros are excluded. This is consistent with many children having to travel substantial distances to attend secondary school. In the sample, 17.3% of localities, accounting for 31.1% of secondary school children, have a secondary school. The average cost of travel is $1.58 per return trip, rising to $9.9 when zeros are excluded. On average then, households incur $316 in school travel costs annually, rising to $1,980 when zeros are excluded.

If a household receives $217 per school month, this gives $2170 per year. Both these numbers imply that households on average incur $14.6 for every $100 of education grants received. However, as with health above, this is a substantial overestimate of the incremental cost due to the program since most of these travel costs would have been incurred in its absence. Based on Schultz (2000), we assume that the program brings about an 8.2% increase in enrolment levels at secondary school from a starting average enrolment rate of around 70% so that only 10% of travel costs are additional. Using this number we can then calculate that households spend, on average, only an additional cost of $1.5 per $100 received.

In principle, beneficiaries pick up their cash transfers once every two months. This implies both financial and time travel costs plus the time costs associated with waiting in line for the transfer. Only 1.13% of localities, accounting for 2.79% of households, have a distribution point located in them. The average distance to a distribution point is 9.1km, rising to 9.33km when zeros are excluded. We find that households on average spend around 138 minutes traveling to and from transfer distribution points at an average cost per return trip of $12.53 per return trip. This implies a household on average incurs $75 annually in travel costs, rising to $113 when zeros are excluded. We also find that nearly 40% of households make at least monthly trips to these locations.

Households on average receive monthly $219 in education grants (over 10 months) and $125 in food transfers, giving an average annual household transfer of $3900. This implies that households incur $1.9 per $100 received. Adjusting for the fact that these trips are additional for only 60% of households, this implies private cost of $1.2 per $100 received.

For the purposes of evaluation, it is useful to distinguish between two types of comparisons

- comparisons across different programs
- comparisons across different policy questions.

With regard to comparisons across different programs, one can think of a number of different program designs. The present program is a conditioned-targeted program. Transfers are made conditional on household members attending school and health checks, and are essentially subsidies for the acquisition of these forms of human capital investments. These transfers are also targeted to poor households in the localities chosen to participate in the program. One can thus consider alternative programs that do not condition transfers and/or do not target households. Since both conditioning and targeting require program resources (e.g. to monitor households behavior and to administratively select "poor" households within participating localities), the costs of implementing unconditioned or untargeted programs will be lower than their conditioned or targeted counterparts.

With regard to comparisons across different policy questions one can distinguish between the costs associated with implementing the program from scratch (i.e. the actual program), the costs associated with expanding the program to incorporate more localities (i.e. program expansion), and the costs associated with continuing the existing program unchanged (i.e. continuation of the program). The relevant costs are generally lower in moving from the actual program to program expansion to program continuation, reflecting the presence of sunk costs.

Focusing first on program costs, we calculate the program cost incurred in transferring monies to beneficiaries, i.e. a cost-benefit ratio (CBR). The CBR of 0.089 for the actual conditioned-targeted program tells us that every $100 transferred to beneficiaries cost the government $8.9. Or, in other words, of every $100 allocated to the program $8.2 is "absorbed" by administration costs. Given the complexity of the program, this level of program costs would appear to be quite small. It is definitely relatively low compared to the numbers given by Grosh (1994) for the LICONSA and TORTIVALES programs, which imply program costs of $40 and $14 per $100 transferred respectively.

By comparing the CBRs across the different programs to that for the actual conditioned-targeted program we can identify the relative importance of the different activity costs. The largest cost component is that associated with targeting at the household level; this accounting for nearly 30% of total program costs. This is followed by the costs associated with conditioning the program, which account for 26% of total costs. Dropping household targeting would thus reduce program costs to $6.2 per $100 transferred, while dropping conditioning would reduce the program cost to $6.6 per $100 transferred. Dropping both would reduce these costs to $3.9 per $100 transferred.

Certain costs are only relevant to particular policy questions. For example, the costs associated with targeting localities and households were incurred up-front and are therefore sunk: in other words, these will not be saved by suspending the program and, say, switching to another program. When these costs are taken out, the program costs reduce to $5.2 per $100 transferred.

We can add in the incremental private costs discussed above to get the total cost of transfers. We find that relevant total costs lie in the range 0.113-0.047, depending on the program type and policy question being addressed. Focusing on the actual conditioned-targeted program, private costs increase total costs from 0.089 (i.e. program costs) to 0.113, i.e. by about 27%. In this respect, ignoring private costs will obviously lead to a substantial underestimate of total costs. So, for every 100 pesos transferred to households $11.3 are incurred in administrative and private costs. This falls to $7.8 when conditioning is dropped, $8.6 when targeting is dropped and $5.1 when both conditioning and targeting are dropped.

In conclusion, the administrative costs employed in getting transfers to poor households appear to be small relative to the costs incurred in previous programs and for targeted programs in other countries. This is in spite of the program being quite a complex program, which involves both the targeting and conditioning of transfers and all the costs that such activities entail. In fact, both these dimensions are large proportions of the total administrative cost. It is important then that the resources spent on these activities generate the expected gains in terms of improving the distributional power of the program and in encouraging human capital accumulation by households. These aspects of the program are discussed below. It is also important to recognize that households incur financial (and time) costs in traveling to collect transfers and to meet the conditioning requirements. It turns out that these are a substantial in relation to program costs. This highlights the need to examine ways in which such costs can be reduced, e.g. through more distribution points or improved transport facilities.

A Cost-Benefit Analysis of Transfers

An important objective of the program is the alleviation of current poverty through cash transfers. Two dimensions of the program influence its distributional efficiency: (i) the fact that it is targeted, and (ii) the fact that the level of transfers a household receives depends in part on its demographic composition. The program is targeted in two respects. Firstly, it is targeted to the poorest (or most marginal) rural localities, i.e. it is geographically targeted. Secondly, it is targeted at "poor" households within these localities. Although geographic targeting is usually very effective at increasing the distributional power of a program (e.g. by ensuring that a relatively high percentage of the program budget gets to poor households), it has a serious shortcoming from the perspective of overall poverty alleviation, namely, it leaves out poor households that do not live in the poorest localities. As indicated earlier, this outcome is undesirable from the perspective of horizontal equity. Similar problems also arise from the fact that poor households deemed not to have access to adequate supporting infrastructure (i.e. schools and health clinics) are also excluded. However, in this report we do not evaluate these features of the program, except insofar as to point out that it is crucial that other components of the development and poverty alleviation strategy address this issue. Rather, here we focus on the second stage of targeting within localities. We also analyze the implications of the linking of the transfers to household composition.

The objectives of this analysis are threefold. We wish:

- to determine how the existing structure of the transfers compares to a range of alternatives;

- to understand how the different components of the transfer system contribute to the distributional power of the program; and

- to understand any trade-offs that exist between the poverty alleviation and human capital accumulation objectives of the program.

We are particularly interested in understanding the implications for the targeted and demographic features of the transfers. The estimated benefits received by households are essentially *theoretical* transfers, i.e. the transfers that would exist if there was 100% take-up by all eligible (i.e. poor) households. This hypothetical program acts as our reference for evaluation purposes and one would expect its benefits structure to resemble that of PROGRESA if the transfers were unconditional. We compare the welfare impact of such a program with the following alternatives:

(i) *Pre-Densification Transfers*: We compare the present post-densification pattern of transfers with that which existed prior to the increase of the income "cut-off line" determining participation.

(ii) *Uniform Targeted Transfers*: Instead of poor households receiving transfers linked to demographic characteristics, one can consider a uniform transfer to these households.

(iii) *Uniform Universal Transfers*: Same as (ii) but now all (i.e. poor and non-poor) households receive a uniform transfer.

(iv) *Non-Targeted Transfers*: A program without within-locality targeting, where all households in the selected localities receive the benefits.

(v) *Transfer Components*: We decompose the welfare impact of each program component (i.e. primary scholarships, secondary scholarships, school materials, and food transfer) in order to identify the contribution of each to the total welfare impact. This analysis will inform the issue of the welfare impact of a change in the structure of the transfers (e.g. reducing food transfers or primary scholarship levels to finance an increase in secondary scholarships in order to get a greater education effect).

(vi) *Intensive Expansion*: Rather than expanding the program across localities (i.e. *extensive* expansion), one could expand intensively by scaling up the benefits in the poorest localities.

(vii) *Actual Transfers*: This allows for some households not receiving the theoretical transfers because they decide not to take-up certain benefits or don't satisfy certain conditions. Households that do not undertake their scheduled visits to the health center do not receive

the food transfer. Neither do households in which children do not meet the 85% school attendance criterion receive transfers for these children. In addition, actual benefits may differ from hypothetical because of operational delays in collecting and processing school and health center attendance data as well as in sending out transfers.

In our analysis we have been concerned with evaluating the distributional power of PROGRESA, i.e. it's ability to get transfers to the most needy households in the program localities, relative to other potential transfer schemes. Our results based on a comparison of PROGRESA with these alternatives suggest the following:

- In spite of substantial leakage during the densification phase of the program, the distributional power of the program is still very high relative to alternatives. This reflects its effectiveness at identifying poor households, but particularly its effectiveness at getting a relatively high proportion of total transfers to the poorest of the poor. The latter in turn operates through the demographic structure of education transfers.

- Restructuring education transfers towards higher grants for secondary schooling in order to try to enhance the educational impact of the program has little effect on the distributional power of the program. Any adverse effect it has can be reversed through simultaneously adjusting the cap on transfers which is relatively more binding for the poorest of the poor.

- As expected, there is a potentially high return, in terms of reduced current poverty, from differentiating transfers across localities (e.g. higher transfers in the most marginal localities). However, this could possibly be at the expense of the educational and health impacts of the program.

- Although the average gains from household-level targeting are modest, these vary inversely with locality marginality. But to reap the gains from targeting as the program expands to include less marginal rural and urban localities, it is important that the targeting errors that occurred during the densification process be avoided.

- The initial stages of the recertification process should focus on correcting the targeting errors that have occurred.

The above conclusions are based on analysis that looks at the benefits side of the program and ignores costs. When we include the targeting costs identified earlier we find that the costs of targeting exceeded the benefits only for the most marginal localities. This is not surprising since very few households in these localities are left out of the program. Of course, this was not known ex-ante so that this information is more relevant for future programs and other countries considering targeting. With regard to PROGRESA, these targeting costs are now sunk and cannot be retrieved and should not now influence the decision on whether or not to continue targeting. But what remains clear is that as the program expands into less marginal rural and urban localities these gains are still substantial but, as above, this is conditional on eliminating the targeting errors made during the densification process. Finally, because of the presence of private costs, households may not take-up benefits if they are not sufficiently large. One might

expect this to differ across program components given the relatively low level of the transfer. We also have preliminary evidence that take-up has been lower among the moderately, as opposed to severely, poor. This tends to increase the distributional power of the program since a higher percentage of the actual budget goes to the poorest households when the moderately poor self-select themselves out of the program. But the fact that adjusting for take-up patterns do not have much affect on the distributional power of the program reflects the fact that take-up is very high, especially among the poorest households. More work needs to be undertaken on this issue (e.g. on the pattern of take-up for the different program components across income groups) and the insights provided from such an analysis may prove invaluable in trying to understand the potential for introducing an element of self-selection into the program. Such issues are extremely important in the ongoing debate regarding whether, when and how programs like PROGRESA should apply targeting rules.

A Cost-Effectiveness Analysis of Education

As indicated above, when evaluating the program it is important to understand the initial motivations for public intervention to influence the educational outcomes of the targeted population. To address this issue we first compare the education outcomes (i.e. enrollment rates) of children in poor and non-poor households and find some support for the targeting of education subsidies at poor households. But this applies only to subsidies for secondary education since children from poor households only have an educational disadvantage over these grades. The subsidies are also higher for females in secondary school in order to eliminate a gender bias in enrollment outcomes. Although we have shown that overall attainment of girls, as measured by years of completed schooling, was not lower than boys prior to program implementation, there is a clear gender bias against females in terms of enrollment rates in secondary school. These conflicting trends can largely be explained by boys having higher repetition rates and/or greater absences or dropout rates from school. Note that this is consistent with boys having a higher opportunity cost through participation in market work. With respect to the grant structure, the lower enrollment rate for girls suggests that grants should be higher for girls, yet the higher probability of grade repetition or dropout for boys implies that the grants should be higher for boys. Concern for human capital accumulation also suggests that grants should be conditioned on completion as opposed to attendance. The common empirical finding that the social returns to education (e.g. in terms of child health and nutrition) are relatively high for females compared to males also reinforces the argument for higher grants for girls. Anticipating our results, the program results in a higher enrollment increase for girls than for boys, sufficient to equalize enrollment rates for both groups. However, since we do not know if this could have been achieved with equal grants for boys and girls, there is nothing in this result that really informs the issue of the structure of grants. But in terms of maximizing the impact on educational outcomes the observed enrollment pattern provides some support for a greater emphasis on grants for secondary education. Because enrollment rates in primary school were already very high before the program one does not expect a substantial impact on these rates and, in the absence of much of an impact, primary school grants act more as a pure cash transfer rather than a subsidy. Therefore, what constitutes an appropriate grant structure depends on the relative weight of these effects and one should be very careful before translating results from our impact analysis into such detailed policy prescriptions. We hope to discuss these issues in more detail in a revised version of this report.

It is also important to recognize that the program is just one component of a more comprehensive development strategy. From an educational perspective, the main objective of the program is to get more children into school. In order to generate a sustained decrease in poverty, two other components must be added. Firstly, the amount of human capital acquired will depend on the quality of schooling children receive while in school. Although we address some elements of the extensive expansion of the program in the form of extra schools and student-teacher ratio, we have much less to say about many issues of intensive expansion that are crucial to ensuring children receive quality education. It is therefore important not to lose sight of this dimension of the education program and, more particularly, PROGRESA should not be view as a substitute for action on these fronts. Secondly, the ability of children to translate higher human capital into higher future incomes will depend crucially on good macroeconomic management, specifically the creation of sufficient new job opportunities to absorb the greater supply of human capital without depressing returns to this capital. Again, it is important not to lose sight of the importance of this dimension of the poverty alleviation strategy.

The main objective of this report is to undertake a cost-effectiveness analysis of the education component of PROGRESA. A full-blown cost-benefit analysis would require one to attach monetary values to the education impacts of the program (i.e. to extra years of education generated by the program) and to compare these to the costs of generating these impacts. Given the difficulty in attaching a monetary evaluation to the myriad of benefits thought to arise from educational investments (of which productivity gains are only one), for the most part in this report we focus on cost-effectiveness analysis in the form of identifying the cost of generating an extra year of education and comparing this across alternative policy instruments, namely, education subsidies for primary education, education subsidies for secondary education, and extensive expansion of the school system.

An important step in cost-effectiveness analysis is the identification of the impact of the program. The effectiveness indicator in our analysis is extra completed years of schooling generated by the program. One could measure this directly by focusing on the total years of completed years of schooling for each child. Alternatively, one can measure it indirectly by focusing on enrollment levels and making assumptions about completion rates. In this report, partly to ensure consistency with previous reports, we employ the indirect approach and assume that an extra year of enrollment leads to an extra year of completed schooling, with the exception of children who return to school and drop out soon after for whom we assume the impact is zero. We measure impacts using regression analysis to get so-called difference-in-difference estimates of the impact and also derive so-called difference estimates by comparing means across each grade level. In this respect, we build on the work of Schultz (2000). In order to facilitate comparisons between the effectiveness of education subsidies and extensive expansion of the supply side, we add supply-side variables to these regressions. This helps to separate the demand and supply side impacts on enrollment. We then estimate the costs incurred in generating these impacts and compare cost-effectiveness ratios (i.e. the cost of generating an additional year of schooling) across the alternative instruments.

We start by analyzing the supply side, i.e. the characteristics of schools attended by PROGRESA children. We have data on all the junior secondary schools in the seven evaluation states for the years 1997-1999. The increasing number of schools from 1997 to 1999 reflects an ongoing expansion program geared to improving access to schools. Over 70% of the children eligible for secondary grants attend schools outside of their locality and not in a program locality. Given the proximity of "control" and "treatment" localities and the fact that children often have to travel long distances to school, it is likely that many of the control and treatment samples attend the same schools. It is not surprising then that supply side characteristics are very similar across both sets of communities. Consistent with their incorporation into the program, we find that student-teacher ratios increase between 1997 and 1998 in treatment localities but fall back to the initial lower levels in 1999, consistent with increased enrollment due to the program and the supply side interventions occurring with a lag. However, there is evidence that the student-classroom ratio has increased steadily in treatment localities in spite of a decrease in the number of more than one-class classrooms.

To estimate the impact of the education subsidies and supply-side interventions on enrollment we generate difference-in-difference estimates using regression analysis. Our analysis differs from that of Schultz (2000) in three dimensions: (i) we use only the three November household surveys from 1997-1999 instead of the five rounds used in Schultz, (ii) we differentiate between the program impact on continuation rates and return rates, and (iii) we expand on the supply side of the analysis. For all regressions we use the sample of children that are eligible for school grants. For example, in secondary school this constitutes all children under 18 years of age that have completed grades 6 to 8 and live in households classified as poor. We test the robustness of our results to different sample compositions in two dimensions:

(i) Choosing three different samples based on households initially classified as *poor* (i.e. 52% of households in the treatment sample), being classified as poor after the so-called *densification* phase that added a further 25% of treatment households, and excluding some of the latter that were *not incorporated* due to operational errors;

(ii) Choosing two different samples based on whether an individual appeared in any of the three years (the *pooled* sample) or only on individuals living in households that appear in all three years (the smaller *panel* sample).

Our finding is that the impact estimates are not sensitive to the underlying sample. In our cost-effectiveness analysis we therefore focus on our preferred panel-sample estimates.

Our estimates of the program impact on enrollment are similar to those reported in Schultz (2000). The program increases enrollment by 8 percentage points in 1998, falling to 6 percentage points in 1999. Splitting the sample along gender lines, we find that the impact is substantially larger for girls. The impact on boys' enrollment decreases from 8 percentage points in 1998 to 5.6 percentage points in 1999, while the impact on girls' enrollment stays constant at around 11.8 percentage points.

In order to identify separately the enrollment impact attributable to supply-side changes occurring simultaneously to the introduction of the program, we add a number of supply-side

characteristics to the regression specification, including a variable representing the distance to the nearest junior secondary school. A decrease in the average distance captures extensive expansion on the supply-side (i.e. more schools). The fact that the program impact is not changed by the introduction of supply-side variables reflects the fact that these are similar for children living in both control and treatment localities. For example, the average distance to the nearest secondary school decreases by around 10% between 1997 and 1999 in both groups. As expected, distance is an important factor in explaining variation in enrollment decisions, especially for girls.

In attempt to explain the decreasing program impact on enrollment over time we try to identify separately the program impact on *continuation rates* and *return rates*. For this purpose we separate the sample into two groups, namely, those children that were enrolled prior to the introduction of the program and those who were not. The program impact on the former is interpreted as its impact on continuation rates and on the latter is interpreted as its impact on return rates. We find that the impact of the program on continuation rates is much larger and is sustained over time, at around 7-8.5 percentage points for boys and 11 percentage points for girls. However, return rates exhibit a completely different pattern. In 1998 the impact is 5.4 percentage points (but statistically insignificant from zero) for boys and 13.6 percentage points for girls (and highly statistically significant). But both these fall substantially in 1999, to 0.4 percentage points for boys (and highly statistically insignificant) and 5.7 percentage points (an just statistically insignificant at the 10% level) for girls. Our interpretation of this is that many of those that return to school after periods of absence do so because they are primarily motivated by the subsidy and not by any perceived private returns to the extra schooling received. These children may find it difficult to keep up with the curriculum and eventually drop out of school. This appears to be more prominent for boys than for girls, i.e. the program is more successful at getting females to return permanently to school than it is with boys. In our earlier regressions this was picked up by a lower program impact on boys' enrollment in 1999 compared to 1998. Since incomplete years most likely do not constitute extra human capital, we therefore view the lower 1999 estimates of program impact as better at capturing the human capital impacts of the program. These are the estimates used in our cost-effectiveness analysis.

Based on the above estimates, we calculate the impact of the program on extra years of schooling for a cohort of 1000 children. We compare this to the impact generated by the observed extensive expansion on the supply side. In both cases we focus on conditional enrollment rates, i.e. the enrollment rate of children who are eligible to enroll is a particular grade. For example, a conditional enrollment rate of 0.3 at grade 7 implies that only 30% of those children completing primary school actually enroll in grade 7 (the first year of junior secondary school). Focusing on the impacts suggested by the more conservative and preferred difference-in-difference estimates, we find that the primary education subsidies result in an extra 76 years of education for girls and 57 for boys. This gender bias in favor of females is sufficient to eliminate the initially small bias in enrollment rates in favor of boys: initial rates for boys and girls were 88% and 87% respectively.

The initial conditional enrollment rates for secondary school indicate that the big drop-out from school occurs in the transition from primary to secondary school, with conditional enrollment falling drastically in grade 7 to around 30%. Once enrolled, the vast majority go on to complete

junior secondary school with conditional enrollment rates rising to 86% and 90% in grades 8 and 9 respectively. The average conditional enrollment rate over the three secondary school years is 47% for girls and 56% for boys, providing evidence of a clear enrollment gender bias against girls. The impact of the program is to increase this average rate by 5.6 percentage points for boys and 11.9 percentage points for girls. Assuming that all this impact is concentrated in the transition year (i.e. grade 7), as is suggested by the data, this increases the conditional enrollment rate in grade 7 by 9.3 percentage points for boys and 17.9 percentage points for girls. The implied impact on extra years of schooling are 249 and 479 extra years of schooling for boys and girls respectively, the impact gender bias being sufficiently strong to virtually eliminate the initial gap in the average conditional enrollment rate over the three years. After the program these are nearly equalized at 59% and 61% for girls and boys respectively.

Focusing on the impact of extensive supply-side expansion, the data suggest that 12 new schools were built between 1997 and 1999 leading to a decrease in the average distance to the nearest secondary school from 2.02km in 1997 to 1.95km in 1999. Using the coefficients on distance (and its square) from the regression analysis, we predict that this resulted in an average enrollment impact of 0.75 percentage points for girls and 0.45 percentage points for boys. If one assumes that this impact is concentrated in the transition year to secondary school these imply increases in the conditional enrollment rate at grade 7 of 1.1 percentage points for girls and 0.7 percentage points for boys over the two years. This, in turn, leads to over 30 extra years of schooling for girls and over 26 extra years for boys over the same period.

The relative attractiveness of primary and secondary education subsidies and extensive expansion on the supply side depends on their relative cost-effectiveness ratios (CERs), i.e. the cost incurred in generating an extra year of schooling. Focusing on CERs based on difference-in-difference estimates, and assuming that the enrollment effects of extensive supply last for 40 years, we find a clear and robust ordering with the cost of generating an extra year of schooling being $10,000 using subsidies in secondary school, $55,483 using subsidies in primary school and $167,962 using extensive expansion through the building of more secondary schools thus decreasing average distances traveled to school. These results provide fairly persuasive evidence that, in terms of its objective of getting more children into school, PROGRESA's subsidies are a very attractive option from the perspective of cost effectiveness. This is particularly true of subsidies for secondary education given the already high enrollment rates in primary school.

In this report we have not attempted to undertake any cost-benefit analysis. This would involve attaching monetary values to the extra years of education generated by the program. Such values are usually based on observed market returns to education. These invariably tell us that returns to secondary education are higher than for primary education: this pattern just reinforces our ranking of primary versus secondary education subsidies. Also, the ranking of secondary school subsidies relative to extensive expansion of the supply side is insensitive to market valuations since each extra year will have the same valuation so that the ratio of CERs across these two instruments will always be the same as the ratio of CBRs. The fact that market values do not necessarily provide useful extra information for the present evaluation partly reflects the view that for other reasons (e.g. the existence of social benefits or a desire for greater equality of opportunity) we believe that educational outcomes (i.e. enrollment) are too low so that the important policy question relates to the most cost-effective way of raising these enrollment rates.

However, in conclusion, we do recognize that other potentially attractive policy instruments have not been considered in this report (e.g., improved transport facilities or different educational technologies) and that other dimensions of policy not considered here are equally important (e.g. high quality education and improved job opportunities). It is important that such issues are not lost sight of in the construction of a comprehensive poverty alleviation strategy. It is also the case that knowledge of market returns to education are particularly valuable in the design of these other policy dimensions. For example, low returns to education may be the result of poor quality of education (e.g. poor education performance by children or a mismatch between the skills employers require and those acquired by children through formal education) or from poor macroeconomic management leading to low growth and an inability of the economy to absorb the greater number of educated persons without depressing the returns to education.

THE APPLICATION OF SOCIAL COST-BENEFIT ANALYSIS TO THE EVALUATION OF PROGRESA

David P. Coady

1. INTRODUCTION

In August 1997 the Mexican government introduced the PROGRESA program in the most marginal rural areas of the country. This program gives cash transfers to mothers in households classified as "poor", these transfers being conditional on child attendance at school and regular visits by family members to health clinics for preventative check-ups. Households with young children also receive food supplements to improve their nutritional status. Although the program is essentially a demand-side intervention, an important dimension of the program involves the explicit recognition that, for such an intervention to be effective at achieving its ultimate objectives, co-ordination with the supply side is essential. To this end, the education and health ministries are expected to plan to allocate resources to areas where substantial demand increases are experienced in order to avoid deteriorating quality on the supply-side that may frustrate the achievement of program objectives.

In this report we are concerned with the application of a social cost-benefit analysis (SCBA) to PROGRESA. The application of SCBA to the evaluation of the program requires one to identify both the impacts and the cost of bringing about these impacts, and then to compare both of these to determine the overall welfare impact of the program and how well the program achieves these welfare impacts relative to alternative policy instruments. When monetary values can be attached to these impacts they are referred to as "program benefits" and the application of cost-benefit analysis then involves determining whether benefits exceed costs and by how much. In the absence of a monetary valuation of impacts, one is restricted to the use of cost-effectiveness analysis, which identifies the cost of bringing about a given impact.

To date the evaluation of PROGRESA has essentially focused on these program *impacts* (i.e. reductions in poverty levels, increased school enrollment and attendance, increased use of health services for preventative care, and improved nutritional status). While knowledge of program impacts is an essential component of any economic evaluation, in isolation it provides very little policy guidance. In general, there are a number of policy instruments that could be employed to generate a given impact and these may differ substantially in terms of cost. Choosing an individual instrument, or a combination of instruments, involves identifying those that can achieve the desired impacts at least cost. So it is important then not to lose sight of the cost side of the program (i.e. of the fact that we have to allocate resources to get these impacts) and the need to link costs to outcomes (i.e. to do cost-benefit analysis). This aspect of policy choice is obviously particularly important when budget allocations are tight.

For the purposes of an economic evaluation, it is useful to interpret PROGRESA as a program with multiple objectives, namely:

- the alleviation of current poverty through cash transfers, and
- the generation of a sustained decrease in poverty by conditioning these transfers on the accumulation of human capital (i.e. education and health status).

Regarding the first objective, since the benefits are already in monetary terms the application of CBA is relatively straightforward. However, given the difficulties associated with attaching monetary values to the human capital components of the program, we confine ourselves to the application of cost-effectiveness analysis when evaluating these components of the program.

It is important to point out that the objective of the report is not to provide definitive assessments of program design or performance. Rather, we wish to show, firstly, how the application of SCBA techniques can provide a useful framework for understanding how the program as a whole can contribute towards the overall development objectives of the government, more particularly, the alleviation of poverty. In this spirit, we emphasize that PROGRESA should be viewed as only one component of a more comprehensive development strategy and that other crucial elements of that strategy must not be overlooked. Also, there are other aspects of the program and its design that are not discussed in any detail in this report but which are nonetheless extremely important in forming an overall assessment of the program. The myriad of social and political dimensions is just one obvious example. Secondly, we wish to show how such an analysis can provide valuable insights into how the various components of the program are working towards achieving the specific objectives of the program. Such insights can help to identify areas where reform of the existing program design may enhance the ability of the program to achieve its objectives.

The first step in any economic evaluation of a public-sector program (or policy) is to address the underlying motivation for the government intervention. The answer to this question has implications for the most appropriate form of intervention and for program design. In Section 2 we therefore discuss the potential motivations for government intervention to influence the allocation of resources in various sectors of the economy. Conditional on program design, we then evaluate the different components of the program and how effectively they achieve specific program objectives. In Section 3 we provide a more detailed discussion of the objectives and design of the program and develop our approach to the evaluation of the program. Following this, we provide detailed evaluations of the cost structure and cost efficiency of the program (Section 4), of the distributional power of the program and its relative efficiency at alleviating poverty (Section 5) and, finally, of the cost-effectiveness of the program in increasing demand for education. Although in the present version of the report we do not evaluate the health and nutrition components in any detail, we hope to address these in future revisions.

2. THE ECONOMIC ANALYSIS OF PUBLIC POLICY

The evaluation of any public program (or policy) requires three crucial ingredients. Firstly, one needs to motivate the need for public-sector involvement. Secondly, one must explicitly specify the objectives of the program. Thirdly, one must calculate the contribution of the program to achieving these objectives (that is, the consequentialist approach). This requires (implicitly or explicitly) a model that enables us to identify the consequences of the program in terms of objectives. It is this third step that is often seen as being the most difficult. Where alternative forms of intervention exist, one should attempt to evaluate their relative merits. Below we discuss each step in turn. However, there will also be a number of non-economic issues which are relevant for policy design which are not adequately addressed within this framework (for example, those reflecting social and political constraints). But, economic evaluation of programs can often provide some useful insights on these issues (for example, on the distribution of benefits between various political constituencies).

2.1. The Role of Government

For the purposes of motivating a role for government, that is, establishing some justification for state intervention to change the allocation of resources, economists have found it useful to use as their point of departure the *laissez faire* (or "free-market") economy where individuals allocate resources and interact with each other without any government influence, control or interference. As early as the eighteenth century, Adam Smith argued that such unfettered market forces would, on the whole, result in socially desirable outcomes or resource allocations. That is, individuals pursuing solely their own self-interests would, as if guided by an "invisible hand," simultaneously promote those of society (Smith, 1776). The conditions under which this holds true were formally analyzed by Arrow (1951) and Debreu (1959) and are encapsulated within the two fundamental theorems of welfare economics (Atkinson and Stiglitz, 1980).

The first theorem states that if (a) households and firms act perfectly competitively taking prices as given, (b) there is a full set of markets, and (c) there is perfect information, then a perfectly competitive economy will result in a Pareto *efficient* allocation of resources from which no one can be made better off without someone being made worse off. Therefore, under such conditions, state intervention is not desirable on efficiency grounds. However, although efficient, the resulting distribution of resources may be extremely undesirable from a social perspective and this is where the second theorem comes in. It states that if, in addition to the above assumptions, (d) governments have access to costless instruments for lump-sum transfers, then any efficient allocation (for example, one representing a more desirable *distribution* of

resources) can be achieved through a competitive economy with the appropriate lump-sum transfers.[1]

Therefore, in the "first-best" economy satisfying conditions (a)-(d), the only role for government is in redistributing income in a lump-sum manner, that is, using policy instruments which do not distort economic incentives. Within this framework, additional arguments for government intervention must rely on departures in the real world from the strict conditions underlying the first-best economy, that is, some of the assumptions (a)-(d) do not hold, resulting in a socially inefficient allocation of resources. Failure of assumptions (a)-(c) are referred to as *market failures* while the absence of optimal lump-sum transfers provides a motivation for interventions on the grounds of *income redistribution*.

The market failures approach thus provides a number of efficiency motivations for public interventions which influence the allocation of resources, some of which are especially relevant to private investments in education, health and nutrition. Targeting of interventions towards the poor can be justified when these households are disproportionately affected by market failures. For example:

- *Absence of competitive markets*: Economies of scale and fixed costs associated with production in some sectors (for example, physical infrastructure) often result in a few producers having monopoly power over prices leading to output being too low and prices being set above marginal costs. This provides an argument for a potential role for government in controlling or regulating these industries. Such conditions may be more prevalent in poor rural areas with low levels of demand and high transaction costs. It may be, for example, that private providers of health and education operate inefficiently in that they do not exploit scale economies.[2] Scale economies and private transport costs also make it more efficient to locate schools and health centers in more densely populated

[1] Two extra conditions, that is, that households' preferences and firms' production sets are convex, are often added to ensure the existence of a competitive equilibrium. Convexity of preferences requires only that all households are never satiated in all commodities. However, convexity of technology rules out the existence of substantial fixed costs or increasing returns to scale, each of which is associated with decreasing marginal costs over a large range of output and is generally associated with market power and inefficient pricing. Assumption (a) encompasses these assumptions.

[2] In education, economies of scale may reflect economies from teacher subject specialization and from economies of scope due to the benefits to students from a broader education. At higher levels of health care (that is, hospital care) economies of scale may reflect returns from specialist care and economies of scope from having access to a range of specialist services. For primary health care, economies of scope are probably more important with referral procedures addressing complicated cases.

areas so that poor households located in more remote areas face high private costs (that is, of time and transport) resulting in lower consumption of these services.[3]

- *Imperfect information*: Producers or consumers may have poor information regarding products and prices and are thus not in a position to make informed decisions. For example, because of their low levels of education, the poor may not appreciate the returns to such investments or the relationship between their behavior and health and nutrition outcomes. Lack of education may also create barriers to acquiring such information.
- *Missing or imperfect markets*: Because of information and transaction costs, markets may not provide certain goods and services even when the cost of extra production is below consumers' willingness to pay. For example, imperfect credit markets (reflecting problems of imperfect or asymmetric information regarding the risk profile of households) result in high interest rates or lack of access to credit. Using existing assets as collateral can help to partially address this market failure, but this is often not an option for poor households whose main asset is their human capital. So the poor may therefore invest too little in education and health. Similar outcomes may result when the goods produced by the poor are subject to high tax rates which reduce returns to investment. Problems of asymmetric information also occur in insurance markets, and the absence of insurance possibilities creates particular problems for the poor who have lower "precautionary savings."
- *Externalities*: Production or consumption externalities associated with certain activities may lead to benefits (or costs) that accrue elsewhere without due compensation resulting in under-supply (over-supply) of these activities. Individual investments in education and health often have positive externalities for others in society, and these externalities are also thought to be more important at the lower levels of investment often associated with low incomes.
- *Public goods*: Pure public goods have two important characteristics. Firstly, the benefits are "non-excludable" or the costs of excluding those who will not pay are prohibitive. Therefore, the private sector is unlikely to produce enough of such goods and services. Secondly, benefits are "non-rival" so that one person consuming does not affect availability to others. Thus, once supplied, the marginal cost of consumption is near zero making positive pricing inefficient (even if feasible). The external benefits accruing from individual investments in education and health often have public-good characteristics, especially at low levels. Information also has public good characteristics suggesting a role for the public sector in providing and disseminating information.
- *Co-ordination failure*: Even where information is available, households and firms may adjust slowly reflecting difficulties in absorbing and acting on information due to education and organizational deficiencies. Public-sector co-ordination of actions may therefore increase efficiency. Similarly, where important inter-sectoral complementarities exist, transaction costs involved in coordinating actions across sectors may prevent them being adequately exploited. For example, there are thought to be important complementaries associated with investments in education, health and nutrition so that inadequate investment in one sector may substantially reduce the returns from

[3] This raises the interesting issues of the appropriate "technology" for delivering education and health services to such households.

investments in the other sectors. The ability or willingness of consumers (or relevant institutions) to co-ordinate actions may also be least at the low investment levels usually associated with low incomes.

The above market failures provide a potential justification for public intervention to bring about a more efficient allocation of resources. When and how to intervene should also be based on the following guiding principles. Firstly, justification for public policies requires that such interventions will lead to improvements sufficiently large to cover intervention costs. Where the latter are large, the best policy may be to leave well-enough alone, so that governments are well advised to concentrate public policy in areas where gains are thought to be substantial. Secondly, public policy should try to operate as directly as possible on the source of the inefficiency. For example, subsidies (taxes) on activities producing positive (negative) externalities, anti-trust regulation to promote competition, or public provision of information in a manner easily absorbed and acted upon by recipients. Indirect approaches operating through other markets (for example, manipulating international trade policies to serve environmental ends) introduce other "distortions" which off-set efficiency gains in the initially distorted market. The impact across markets may also be difficult to identify with much precision with obvious difficulties for the design of effective public policy. Thirdly, public intervention can be of a centralized or decentralized manner, where the former approach involves intervening directly in the reallocation of resources (for example, through public provision of a good or regulations) and the latter involves indirectly influencing the allocations of households and firms through manipulating the incentives they face (for example, through taxes or subsidies). Where households or firms have more information about the costs of responding to public policies, a decentralized (or market-based) approach tends to be more cost effective (for example, taxing environmental pollution as opposed to cruder environmental regulation). Where responses by households or firms are uncertain or slow, and the cost of wrong or slow responses is high, a centralized (or regulatory) approach may be more desirable (for example, vitamin fortification of foods). Since the costs of intervention can be expected to depend on the effectiveness of public institutions as well as on the nature of "market failures", the appropriate mix of policy instruments can also be expected to differ across sectors and countries. Fourthly, where subsidies are used it should be remembered that such subsidies are usually financed by distortionary taxation or through reductions elsewhere in the poverty alleviation budget, and that some programs (for example, education) often have relatively long gestation periods, so that budget allocations must be sustainable over at least the medium term.

Even in the absence of any of the market failures identified above there may be other justifiable reasons for public intervention. For example, the state may take a *paternalistic* approach, which rejects a purely individualistic view of society (upon which the market failures approach is based), and treat certain activities or goods as "merit" (for example, education) or "demerit" (for example, drugs or alcohol) goods and therefore intervene to promote appropriate consumption patterns or economic activities accordingly. For example, the state may reject household preferences over the welfare of individual members (for example, females or children) and promote increased consumption by and investment in these members. Such a rationale is often invoked to justify interventions that protect environmental resources for the benefit of future generations, interventions that are gender-biased, or interventions which target children. Similar concerns provide the basis for the argument for "specific egalitarianism" with respect to certain

goods such as education and health, a variation on the concept of "horizontal equity" which requires equal treatment of individuals that are deemed equal in all relevant respects. From an intra-household perspective, age and gender would not be regarded as relevant characteristics in determining the allocation of resources. The basis of the paternalistic approach is therefore that the decisions of households, even when based on full information regarding costs and benefits and in the absence of social externalities, may not adequately reflect social preferences dictated by views towards what constitutes a just society.[4]

In the absence of market failures or with the rejection of paternalistic motivations, public policy can be justified on the grounds of improving *income distribution*. The "first-best" policy response is to redistribute (from rich to poor) using lump-sum transfers that do not distort resource-allocation decisions. This requires transfers to be based on characteristics that are not under the control of individuals or households and that therefore cannot be manipulated in pursuit of higher transfers. The appropriate characteristic is thus some measure of *potential* income which is not observable and which households have no incentive to truthfully reveal. One then needs to select observable characteristics to which transfers can be linked. The choice of "screening" characteristics should take into account the following: (i) these characteristics are more or less under the control of households so that transfers based on these will invariably introduce distortions in resource allocation (for example, unemployment) and remove incentives for "self-help"; (ii) these characteristics are not perfectly correlated with those with which we are ultimately concerned so transfers will be imperfectly targeted; and (iii) there are administrative costs associated with observing these characteristics. In practice, one observes a variety of targeting methods including those based on income-related means tests (or administrative targeting), on household characteristics such as age or gender (so-called categorical targeting or tagging), or on a combination of both. Another alternative is the use of self-targeting mechanisms that screen on the basis of the private costs and benefits of program participation (e.g. workfare programs). In evaluating the relative efficiency of these targeting interventions, one needs to be explicit about the objectives of the policy, the range of policy instruments available to achieve these objectives, and the social and political constraints on instrument choice (Atkinson, 1995).

The distortionary nature of transfers introduces a trade-off between equity and efficiency: for a given aversion to inequality, the greater the distortion the lower the optimal level of transfers (that is, the higher the optimal level of inequality). Thus, distributional objectives become endogenous. It may be that a wider range of policy instruments can help to reduce the "deadweight loss" or inefficiency resulting from transfers. Also, although the use of a greater number of screening characteristics can

[4] But the "freedom to choose" is also often considered an important dimension of a just society. Libertarians tend to focus more on preventing the government from restricting free choice rather than on the equally important role of government in promoting such freedoms. These freedoms constitute an important component of individual "capabilities" (that is, the capability of turning "means," such as income, into "ends," such as health and nutrition status) as discussed in Sen (1992).

improve the relationship between the observed and unobserved characteristics, administrative costs may be increasing in the number of screening characteristics used. The inefficiency associated with the transfer may also increase with the size of any one transfer. Such issues will determine the degree to which one should specialize in a small number of screening devices or diversify over a larger number of devices.

The administrative, bureaucratic, political and social feasibility of transfers should also influence the design of transfer mechanisms. The choice of instruments (for example, the reliance on indirect rather then direct taxation) in developing countries is strongly influenced by administrative constraints, and this may change over time. Some very efficient transfer instruments may be politically less acceptable (for example, land taxes) or more acceptable (for example, universal subsidies). The theory of political economy also suggests that the nature of bureaucratic constraints is also important, for example, bureaucrats may derive power or esteem from control over a large budget and thus prevent moves to a more effective transfer mechanism that reduces the importance of this budget. So "government failure" may be just as important a factor as market failure when designing transfer systems. Social attitudes also matter. For example, the relatively low take-up rate of "means-tested" benefits compared to categorical benefits is often attributed to the social stigma associated with receipt of such benefits.

Increasingly in developing countries, many transfer programs are "targeted" using means testing based on actual income. For example, poverty alleviation programs often target households whose income falls below some specified "poverty line." The potential benefits from such targeting are obvious, for example, the poverty budget is concentrated on a smaller number of poorer households so that we get more "bang for our buck" in terms of poverty reduction and the distortionary costs associated with such programs may be smaller. However, the potential costs are less obvious. For example, as well as the administrative costs associated with targeting there are the costs associated with imperfect targeting commonly referred to as "leakage" (wrongly including non-poor households) and "under-coverage" (wrongly excluding poor households). When calculating the incremental value for the poor of switching to targeted programs and away from untargeted transfers (for example, universal subsidies for food), one needs to allow for these lost benefits. Similarly, some of the non-poor may be only marginally better off than the poor and these may lose from the withdrawal of non-targeted transfers. Such targeting programs also (as in the case of PROGRESA) have multiple objectives and the trade-off between these objectives must be explicitly addressed (for example, the costs of targeting to areas with schools and health centers in terms of forgone poverty reduction).

2.2. Objectives

The conventional approach of economists, often referred to as the *welfarist* approach, is to specify objectives in terms of money-metric utility. Programs are evaluated in terms of their impact on present and future income which finances the consumption of goods and services (including leisure) which in turn increase individual utility. Impacts on individuals are aggregated using some well-defined social welfare function which captures society's willingness (or otherwise) to trade-off efficiency (that is, higher total income) for equity (that is, a better distribution of income). Impacts across time are aggregated using discounting. This approach therefore involves all impacts being collapsed into a single monetary unit of account or

numeraire. The benefit of the program is then taken as the total impact of the program on this *numeraire*, and compared to the cost of the program to calculate the net benefit of the program. A positive (negative) value indicates that the program increases (decreases) social welfare. One can also compare net benefits across programs and choose the program(s) with the highest net benefits.

The calculation of a net benefit requires us to attach a monetary valuation to the impact. For outputs traded in a market we can use the observed market price as the *basis* of our valuation.[5] However, this possibility is not available where the output is not traded in a market. In such circumstances, we can use the market price of a close substitute or related good if one exists. If not, we could attempt to get users to reveal their (maximum) willingness to pay for the output. But if individuals are unwilling or unable to reveal their preferences in such a manner then it may be that no valuation can be attached to the impact so that we cannot determine by how much the program increases social welfare. Our decision to go ahead with the program then rests on whether we believe the total benefit to be greater than the total cost of the program. Where alternative program designs are available, one can then undertake cost-effectiveness analysis in order to determine the most cost-effective way of achieving a given impact or, equivalently, the design which achieves the biggest impact for a fixed budget. The assumption of a fixed budget operates essentially to make alternative uses of the budget mutually exclusive thus highlighting the fact that there is an opportunity cost associated with budget funds, that is, there are other ways of achieving the same objectives.

The calculation of net benefits also requires identification of costs as well as benefits. It is important to include not only budgetary or program costs but also private costs (often non-monetary costs such as time) incurred by participants. These may vary substantially with program design and can be an important part of program targeting: self-targeting programs are often designed to have higher private time costs, associated with the need for beneficiaries to allocate time for claiming and receiving benefits, since the poor are thought to have a lower valuation of time.

The above approach is by no means the only way to proceed. For example, one could legitimately take a rights-based approach or emphasize not the consequences of the program but the *process* by which these are achieved (Nozick, 1974). Two other approaches which have gained prominence over the last decade are the *basic needs* approach (Streeten *et al.*, 1981) and the *capabilities approach* (Sen, 1985 and 1987). Both of these approaches distinguish between income as a "means" or as an "end", and highlight the commonly observed lack of strong correlation between income and other outcomes that enter into one's concept of development. The basic needs approach focuses on human needs in terms of health, food, education, water, shelter and transport. Proponents of this approach argue that, because of the public-good characteristics of these (and other) sectors, the private sector will not supply adequate amounts, particularly in rural or sparsely populated areas which are often poor. As a result *ceteris paribus*

[5] Of course, even where market prices exist the need to be adjusted for the presence of taxes, externalities, market power and so on.

the utility (or *numeraire*) value of additional money income, which captures the ability to transform income into utility and social welfare, differs substantially across households.

The approach can be interpreted in terms of market failure using the concept of specific egalitarianism, which requires (a certain level of) equality in the distribution of certain commodities (as opposed to income). It may be that the range of commodities over which individuals should have access to some "minimally acceptable" level is more extensive than the list of "basic needs" conventionally used. However, market forces may achieve these levels for most commodities so that motivations for public intervention apply only to those that are not adequately met for some "poor" households. For example, certain public goods may be under-supplied in certain areas and therefore not available to some households at required levels. The ability of such households to transform income ("means") into utility ("ends") therefore also differs. Income transfers alone will not necessarily remedy this ("socially unjust") situation so that the satisfaction of their minimum requirements requires public intervention to correct the underlying market failures. The inclusion of, for example, food in the list of "basic needs" is possibly problematic from the above interpretation since it is likely that consumption is constrained by income as opposed to by inadequate market supplies (DrPze and Sen, 1989).

From the perspective of SCBA the problem can be seen as one of observed income or expenditure not being a good indicator of welfare. Therefore, since it is difficult to adjust actual income to reflect differential access to these resources (that is, by incorporating a valuation of consumption of rationed public goods into a measure of full income), one needs to determine not only the impact on money income but also to document information on access to these resources. Attempts to aggregate such information into a single index (for example, a "human development index") is often arbitrary and rarely based on the economic principles regarding the valuation of rationed public goods. Essentially, what this approach is saying is that investments in education, health and so on can be expected to have very high social returns because such resources are under-supplied and particularly so in poor areas. Therefore, budgets allocated to such programs may have a substantially higher social return compared to pure income-transfer schemes.

The capabilities approach rejects the welfarist paradigm in which utility (that is, actual consumption of goods and services) is taken as the sole metric of welfare. It views income as a means to the purchasing of commodities that are valued not only for the utility derived directly from their consumption but also because they expand one's capability to function as a valued member of society.[6] What matters is not only one's achievements but one's potential to achieve. Note that the relationship between capabilities and achievements is not unique but depends on preferences, so that evaluation from this perspective requires not only information on actual achievements (or "functionings") but also potential achievements (that is, on the set of functionings which one is capable of achieving). From the perspective of SCBA the main

[6] It is often argued that capabilities are highly correlated with income so focusing on income is not problematic. However, correlation and cause-and-effect are not the same and this may be crucial when it comes to policy selection and design. But one of the motivations for the capabilities and basic needs approaches appears to originate in the belief that there is not a strong empirical correlation in practice.

shortcoming of the capability approach is the difficulty in specifying what variables capture one's capabilities and how these can then be aggregated both into a single individual index and across individuals. Present attempts to achieve this appear arbitrary and extremely sensitive to weighting schemes. However, the approach does serve to highlight the fact that economic analysis is but one component of social analysis and that a wider range of "indicators" needs to be employed in empirical analysis of "development". The correlation and trade-offs between these indicators and income should be addressed. Just because we cannot easily capture important implications through a set of well-defined variables does not mean that these are not important. But it does present difficulties for their incorporation into the economic analysis of programs as conventionally practiced.

As mentioned earlier, the success or otherwise of public policies can also depend on issues of political economy as captured, for example, by the impacts on various vested interest groups. Public support for programs can influence how effectively they achieve their objectives and should therefore impinge on program design or the selection among alternative policies (for example, universal versus targeted programs). For example, it is often argued that universal programs are attractive because the substantial numbers who benefit constitute an important lobby group. Targeted programs may prove fragile when political power bases change. But targeted programs obviously minimize 'leakage' and thus may enable a larger impact from a given budget. Public support may also depend on how effective the program is implemented, for example, by minimizing leakage and wastage. Good program evaluation, by highlighting the substantial social benefits, can often help to de-politicize policy especially when individuals and communities develop a sense of participation and ownership.

A defining characteristic of social programs which attempt to increase household investments in "human capital" is the diverse or multi-dimensional set of benefits which accrue as a result of these investments, making identification and valuation of benefits difficult if not impossible in some cases. Firstly, such investments have both a direct impact on household welfare (for example, by increasing the marginal utility of consumption and leisure) and an indirect effect coming through higher productivity of household production activities.[7] Secondly, some of these production activities are marketed while others are non-marketed. This has important implications for cost-benefit analysis since, whereas higher productivity in marketed activities will show up in higher money incomes, higher productivity in non-marketed activities is not captured through direct monetary flows. Failure to recognize this may result in the real possibility that such valuable benefits will be ignored in cost-benefit analysis and thus also in program design and selection. Thirdly, even marketed benefits may be difficult to value since they can be quite diverse and work through complex channels. For example, greater human capital may not just affect productivities and incomes in existing production activities but may also result in improved job opportunities which may involve migration and which may also provide a less risky income profile. Fourthly, the dynamic of such investments may mean that benefits accrue (or increase) with a long lag so that evaluation at early stages of the program may have to rely on tracking inputs (or intermediary outcomes) such as impacts on attendance or

[7] In other words, such investments result in benefits accruing in terms both of more consumption and more production goods.

utilization rates. It is difficult to attach a monetary value to such indicators whereas, for example, effects in terms of extra years of education or better health status may be more easily linked with higher incomes. Additionally, many other unobserved factors may be influencing the accumulation of human capital making it difficult to disentangle the separate effect of the program. Finally, as indicated earlier, the benefits from these investments may accrue not only to all household members but to others in society (including future generations). These external benefits, in turn, may take the form of higher productivities or of "social" externalities such as more efficient operation of economic and political institutions. All of the above issues have implications for our ability to associate a monetary value to the benefits arising from human capital investments and thus influence our approach to cost-benefit analysis of such programs, for example, the choice between the use of cost-benefit analysis or cost-effectiveness analysis.

2.3. Model

The net impact of the program is the difference between the "world with" the program and the "world without" the program. For our purposes this involves identifying the impact of the program on, say, the individual components of human capital; we then try to evaluate the impact on welfare of this higher level of human capital. But it also requires us to identify the channels through which these impacts emerge and important interactions between various program inputs and outputs.[8] The fact that resources are scarce (that is, have a positive opportunity cost) provides our motivation for searching for less costly ways of achieving the same impacts or, similarly, for ways of achieving greater impacts from the same program budget.

The conventional approach to identifying program impacts is to use regression analysis to determine how program inputs are turned into human capital (or intermediary) outcomes, thus enabling one to evaluate the effectiveness of various programs which differ according to composition of inputs. The underlying model used to specify and interpret such regressions is one where households are seen as allocating resources (for example, money and time) over a range of competing uses including investments in human capital (see, for example, Behrman and Deolaliker, 1988; Schultz, 1988; and Strauss and Thomas, 1995). Observed outcomes or allocations will reflect both household preferences (which may reflect endowments, including initial endowments of human capital, and may also include gender preferences) and constraints (including household and community endowments and prices). Demands for education, health and nutrition inputs are made simultaneously and have important cross-effects on each other's productivity. The fact that households have choices over human capital inputs, and thus also outcomes, means that the former should be viewed as endogenous whenever they appear as explanatory variables. This has important implications for approaches to specification and estimation.

[8] One should recognize that our knowledge of the technical relations determining human capital outcomes and the nature of interactions and lags both within and between the various components of human capital is in many important respects quite incomplete.

One approach is to estimate reduced-form equations for inputs or outcomes with only exogenous variables (for example, endowments and prices) as explanatory variables. A dummy for participation in the program could be included as an extra explanatory variable. If participation in the program (either by individuals or communities) was randomly determined then this dummy can be viewed as exogenous and the equation estimated using classical OLS estimation techniques, possibly with some adjustments to standard errors to allow for departures from the classical assumptions regarding the structure of the error terms. The coefficient on the program dummy identifies the relevant average program impact. One can try to identify the distribution of the program impact across individuals or communities with different characteristics, or in different parts of the distribution, by interacting the dummy with the relevant characteristic (for example, education levels) or using quantile regression estimation techniques. Such extra information may be quite informative for program design. Additionally, instead of using a single program dummy one could use a set of dummies capturing varying program composition (for example, receipt of any single one, or combination, of the human capital components) where such variation exists. This will be crucial if one wishes to identify the returns from integrating human capital investments.[9] Of course, all the standard estimation concerns regarding omitted variable bias apply here, that is, if there is important unobserved heterogeneity among households or communities that is correlated with program participation then the estimated program impact will be biased.

Crucial to estimating the program effect using OLS estimation techniques is the assumption that program participation was determined randomly and can therefore be treated as exogenous. If households or communities had some influence over whether they participated in the program, or if those responsible for selecting beneficiaries did not allocate the program randomly, then this introduces the potential for sample selection bias if OLS estimation techniques are applied. For example, if those who select themselves into the program are also those who are most likely to benefit from the program then we will overestimate the (average) impact of the program on the population. In such circumstances, one needs to use other estimation approaches such as the Heckman (1974) sample selection approach or instrumental variable techniques, which requires information on variables that influence selection directly but do not influence the dependent variable (Pitt *et al.*, 1993; Rosenzweig and Wolpin, 1986).

The application of OLS techniques will also only give unbiased coefficient estimates if all explanatory variables are exogenous. However, sometimes one may wish to identify the impact of an endogenous variable, for example, the impact of an increase in household money income. From the perspective of cost-effectiveness analysis this may provide much useful information which can be used as a basis for preliminary comparisons across alternative policy instruments,

[9] Alternatively, it may be that one could use the variation in households eligibility (or propensity) to participate, which is dependent on household composition, to identify these effects. But sufficient variation may not exist and one also needs to address other reasons for (endogenous) non-participation.

where many of the explanatory variables are interpreted as potential policy instruments.[10] For example, when explaining variation in fertility outcomes one might include parents' education, parents' incomes and, say, a program dummy as explanatory variables. From the coefficients one can identify "equivalent" policy reforms in the sense that these will have the same impact on fertility. In particular, one can identify what might be called "equivalent direct income transfers" (or EDITs), which indicate how much income would have to be transferred to the household to get an impact equal to that of the program or some other potential policy instrument. These EDITs provide a starting point for effectiveness comparisons across alternative policy instruments or program designs but require relative cost data for more complete comparisons. For example, one can compare the cost of the program with its EDIT. If the program costs less than its EDIT then it is more cost effective than such an income transfer to the household. Of course, income transfers (and even programs) may differ substantially in regard to their impacts on other important outcomes. Note that in terms of informing public-sector resource allocation decisions the coefficients from regressions are not useful in themselves unless we can translate them into some sort of statement on cost-effectiveness analysis, which requires additional information on program and private costs.

Rather than just being interested in estimating reduced-form relationships, one may be particularly interested in identifying structural relationships. For example, one may be interested in identifying the parameters of production functions with the aim of attaching a monetary value to the program impact, say, on marketed outputs for which we observe market prices. To the extent that households are not rationed in resource allocations one may also then be able to use prices related to marketed activities to evaluate impacts on non-marketed activities. However, the fact that the variables typically entered as explanatory variables in such relationships are also household choice variables means that problems with simultaneity bias are pervasive in such studies. Falling back on estimating reduced-form relationships (for example, with endowments and prices as explanatory variables) may have an expensive trade-off in terms of knowledge of structural relationships that can provide useful insights into program design. But the information provided by production-function estimates may also be only one part of the story since these estimates capture only the direct effects of the program on the particular outcomes that are "produced" by the production functions that are estimated. The program (for example, the cash benefits) may also have indirect impacts that operate only indirectly through production functions, for example, through influencing the level of inputs into production. Therefore, the total effect of the program on "production" may be very different from the production-function effects. One therefore needs to explicitly address the channels through which the program is expected to have its intended impacts.

Poor knowledge regarding structural relationships also makes endogenous variables poor policy instruments due to the resulting uncertainty associated with the ultimate impacts of programs. Better knowledge of such relationships thus helps us to differentiate between alternative policy

[10] This approach is similar in spirit to that used in Summers (1994) who evaluates the benefits from female education in terms of such outcomes as fertility and mortality by asking how much it would have cost using other specific program instruments to achieve similar benefits.

instruments in terms of effectiveness in achieving objectives. For example, programs that transfer income to women or food to children may be frustrated in achieving their objectives by household responses when such resources are fungible within the household. Households may simply redistribute other household income or food in order to achieve their own objectives. In this instance, such programs make poor policy instruments. But households may also respond in a manner which reinforces policy interventions, for example, improving child health may encourage reduced fertility (that is, child quantity) and increase household investments in education (for example, child quality) since healthy children may perform better in school and live for longer periods over which they can reap the rewards from such investments. So a better understanding of the household decision-making process and structural relationships can greatly inform policy choice and program design.

However, if one is solely interested in identifying program reduced-form impacts then estimation difficulties resulting from the presence of simultaneity bias do not arise. If alternative designs have been randomly implemented then a great deal of cost-benefit analysis can be undertaken based solely on reduced-form estimates. This, of course, was a key motivation for the use of an experimental approach for the evaluation of PROGRESA (i.e. the collection of "before" and "after" data for both "control" and "treatment" localities).[11] Of course, where omitted variable bias is important this is equally relevant to the identification of reduced-form impacts. Also, although alternative modeling approaches (for example, viewing household decisions within a "unitary" or "bargaining" framework) suggest different reduced-forms which allow for the validity of various restrictions to be tested empirically (for example, by allowing the coefficients on alternative assets under the control of different household members to differ), they are arguably more susceptible to biases due to omitted variables (for example, related to preferences and productivities) and simultaneity. This makes the interpretation of such results for policy purposes very difficult.

The above approach identifies only direct impacts on beneficiary households. One should also try to identify possible indirect impacts, for example, increased over-crowding in schools, decreased quality due to usage pressures and other congestion-type costs, and prevention of contagious diseases. Such impacts fall on other households but may also have an effect on the behavior of beneficiary households, for example, regarding their decision to participate in the program. Interventions which provide more and better quality services may be crucial here and may have an important bearing on how successful the program is in changing the decisions of beneficiaries as well as in cultivating community support for the program.

[11] See, for example, Schultz (2000) for a more detailed discussion of this approach in the context of the evaluation of the education component of PROGRESA.

3. ECONOMIC ANALYSIS OF PROGRESA

In this section we discuss our approach to the application of social cost-benefit analysis (SCBA) to the economic analysis of PROGRESA. We start, in Section 3.1, by giving a brief description of the explicit objectives and the design of the program. This helps to identify key features of the objectives and design that create difficulties for the application of SCBA. These issues are taken up in Section 3.2 where we set out our approach to the economic evaluation of the program and where this evaluation fits in terms of an evaluation of the overall development and poverty alleviation strategy of Mexico.

3.1 Description of the Program

According to program documents, the aim of PROGRESA is to provide support for families living in conditions of extreme poverty in order to broaden their opportunities and capabilities to attain higher levels of well-being. The program consists of a series of targeted cash transfers made conditional on households taking decisions conducive to raising their living standards by improving opportunities for education, health and food. Specifically, PROGRESA has the following objectives:[12]

- To substantially improve the conditions of education, health and nutrition of poor families, particularly children and their mothers, by providing sufficient quality services in the areas of education and health, as well as providing monetary assistance and nutrition supplements;

- Integrate these actions so that educational achievement is not affected by poor health or malnutrition in children and young people, or because they carry out work that makes school attendance difficult;
- Ensure that households have sufficient means and resources available so that their children can complete their basic education;

- Encourage the responsibility and active participation of parents and all family members in improving the education, health and nutrition of children and young people; and

- Promote community participation and support for the actions of PROGRESA, so that educational and health services benefit all families in the localities where it operates, as well as uniting and promoting community efforts and initiatives in actions that are similar or complementary to the program.

[12] The statements regarding the specific objectives and components of PROGRESA are taken directly from Skoufias, Davis, and Behrman (1999).

In order to achieve these objectives, PROGRESA is made up of three components that are closely linked to each other:

(1) Educational grants to facilitate and encourage the educational aspirations of children and young people by fostering their enrollment and regular school attendance, and promoting parents' appreciation of the advantages of their children's education. At the same time, actions will be carried out to improve the quality of education;

(2) Basic health care for all members of the family and strengthening the quality of services as well as reorienting individuals and health services towards taking preventive actions towards health care and nutrition; and

(3) Monetary transfers and nutrition supplements to improve the food consumption and nutritional state of poor families, emphasizing that the purpose of this is to improve the family's food intake, particularly of children and women, who are generally the members of households who suffer most from nutritional deficiencies.

Therefore, the ultimate objective of PROGRESA is to improve the education, health, and nutritional status of poor households, especially for women and children, which are viewed as being among the most vulnerable groups in society. Each of these components can be viewed as a form of human capital, which enters directly into individual well-being (for example, enabling one to contribute to, and participate as a valued member of, the society in which one lives) but also indirectly in determining individual productivity and thus income-earning potential. The nature of the education-health-nutrition nexus is often seen as being the root of the vicious circle of poverty, whereby children born into poor families disproportionately experience health and nutritional problems which diminish their potential for benefiting from whatever education they receive. Public action is therefore thought to be required to transform this vicious circle into a virtuous one.

3.2 Evaluation of the Program

From the above one can see that PROGRESA is a complex program for a number of related reasons. Firstly, its conception appears to reflect a number of objectives involving income, basic needs and capabilities. Secondly, it is not simply an income transfer program but also contains important components on education, health and nutrition. As discussed earlier, the latter components present difficulties regarding identification and valuation of their benefits. Also, one needs to address the trade-offs between alternative objectives. Thirdly, it is dynamic in that the impacts of the program can be expected to evolve over time rather than instantaneously so we are more likely to observe inputs (for example, school attendance or visits to health centers) rather than outcomes (for example, health status or education levels achieved). Fourthly, the integrated nature of the program reflects the underlying belief that each component is not independent of the others in that the presence of each influences the effectiveness of the others. All of the above factors have important implications for how we conduct our economic analysis.

There are therefore three crucial assumptions underlying the focus and design of PROGRESA. First, investments in these dimensions of human capital are socially valuable, that is, have a high social return. Second, an integrated program is more efficient at achieving these objectives. Third, the underlying market failures are assumed to be more severe for poor households thus justifying the targeting of these programs from both efficiency and equity perspectives. Ideally our economic analysis should try to evaluate all three of these assumptions. However, although we can undertake a fairly rigorous analysis of the last of these assumptions, our analysis of the first two is necessarily limited. The nature of the benefits accruing from such investments restricts any economic analysis since it is difficult to attach a monetary valuation to many of these benefits. But, where possible, one should at least try to evaluate the cost effectiveness of such programs in achieving given impacts relative to other forms of intervention or alternative program designs. In addition, the fact that the program has been delivered as a single package (i.e. conditional on household composition, both the health and education components are always delivered together) prevents us from evaluating the returns integrating program components.

As indicated above, the integrated design of PROGRESA reflects the underlying belief that the effectiveness of each component depends strongly on the other sectoral investments. For example, educational performance is influenced strongly by nutritional and health status; one=s ability to absorb nutrients depends on one=s health; and susceptibility to disease is influenced by nutritional status. One therefore expects that an integrated program is substantially more effective in achieving objectives. However, it is important to recognize that these large programs are both complex to design (for example, since the operational performance of each component depends on that of the others and incentive structures are also possibly interlinked) and costly to implement and co-ordinate (for example, in terms of management and administration resources as well as household resources). Such large programs therefore require careful planning and continuous evaluation to facilitate learning by doing.[13] As well as feeding back into improved program design as the program is expanded within Mexico, such insights will have cross-country benefits since many other countries are beginning to implement similar poverty alleviation programs. The uncertainty associated with how such integrated programs actually work in practice also highlights the need to focus, not only on their eventual impact (that is, the observed technical relationship between inputs and outputs), but also on their operational performance. Indeed, such an "operations evaluation" forms a key part of our overall evaluation of PROGRESA (Coady, 1999; Adato, Coady and Ruel, 2000).

In order to design and implement a program which contributes to the achievement of the ultimate objectives and which can be evaluated, it is necessary to specify a set of *derived* objectives (or outputs) which help to make more explicit and to operationalize the overall objectives. These are listed in the first two columns of Table 1, which is taken from Coady (1999). Certain features of the design of the program are seen as being so crucial to its effectiveness that they can

[13] Of course, on the plus side, as our knowledge of how these program components combine increases it may be that we can interlink incentive structures and spread costs over a wider range of benefits. The size of the evaluation budget should thus be judged with reference to the expected program size and not the budget for the pilot schemes.

be viewed as "secondary" outputs. These essentially relate to households and communities participating in, and taking responsibility for, the success of the program, and are listed in Table 1 both separately, under an additional component category labelled "Other," and within each of the three main components (in column 2). For the purpose of this report we are concerned solely with evaluating the impact in terms of the "primary" objectives.

Having identified outputs one then needs to specify indicators that facilitate an impact evaluation: these are listed in the third column of Table 1. Operations evaluation, on the other hand, is concerned with specifying in detail the inputs (column 4) necessary for achieving these impacts and with evaluating the operational performance of the program in delivering in this regard (column 5). The main purpose of Table 1, then, is to help to structure our analysis of the program by being explicit about program objectives, how the program impact in terms of these objectives can be captured, which inputs are meant to ensure that the impacts come about, and how we intend to measure whether such inputs are being adequately delivered. In this respect, it is important to distinguish between two separate reasons for program failure. First, the program may be fundamentally unsound from an economic viewpoint. Second, as designed the program may be fundamentally sound but have been poorly implemented. The policy implications of each are obviously different. In this paper we are concerned primarily with evaluation of the program as it operates in the field, although the actual analysis should provide insights regarding implementation. In Adato, Coady and Ruel (2000) we present a detailed analysis of the operational performance of the program.

For the purposes of our economic evaluation, then, we interpret PROGRESA as a program with multiple objectives, namely:

(i) the alleviation of current poverty through targeted cash transfers, and

(ii) the generation of a sustained decrease in poverty by conditioning these transfers on the accumulation of human capital (i.e. education and health status) by households.

The targeting of cash transfers in motivated by the desire to maximize the impact on current poverty for a given program budget. The conditioning of these transfers transforms them into subsidies for human capital accumulation, i.e. households only receive the transfers if they invest in their family's education and health.

As pointed out above, this multiplicity of benefits generated by the program creates difficulties when it comes to economic analysis. Firstly, in the absence of being able to attach monetary valuations to the human-capital impacts generated by the program, one is unable to aggregate across the range of impacts in order to undertake unified cost-benefit analysis of the program. Secondly, on the cost side one faces the conceptually difficult problem of allocating joint costs to the various program components. In order to apply cost-benefit (or effectiveness) analysis to the evaluation of the program, one can consider two alternative approaches, each which essentially addresses a different policy issue:

- One can consider each component of the program (i.e. current poverty, education and health) as a *stand-alone* program, deal with each of the impacts separately and identify the costs that would have to be incurred in order to generate these impacts in isolation. For example, one can focus on the cost of transferring income to households through the program, or the cost of generating the observed human-capital impacts. All of these hypothetical programs will incur the joint costs but certain costs will be specific to individual components, e.g. the supply-side costs or the costs of monitoring attendance at schools and health centers. These can then be compared to the costs that would have to be incurred to generate the same impacts using an alternative instrument.

- One can interpret the program as a hierarchy of components, viewing one component as the *core* program and the others as *add-ons*. One can view the poverty alleviation component as the core program and the education, health, and nutrition components as the add-ons since the latter would not exist without the former. The core program then consists of unconditional cash transfers with the objective of reducing current poverty or improving income distribution. Adding-on any of the other components then involves incurring extra costs to make the transfers conditional. One can then identify the extra cost incurred in achieving these extra benefits and compare these with those incurred using alternative policy instruments. But one should be aware that the fact that conditioning the transfers implies additional program and private costs means that there is an inherent trade-off between the dual objectives of current poverty alleviation and human capital accumulation.

In our analysis we appeal to each approach and this also has particular implications for the way we disaggregate costs below.

In conclusion, it is important to be aware that we view PROGRESA as only one component of a more comprehensive development and poverty alleviation strategy. The program is, for the most part, a demand-side intervention that attempts to increase households' demand for human capital through increasing enrollment and attendance at school and increasing visits to health clinics for preventative health care. The ability of the program to contribute to the achievement of overall development objectives depends crucially on the presence of other components of the strategy. These are relevant for both dimensions of the program, i.e. current poverty alleviation and human capital accumulation. For example, because of the integrated design of the program (i.e. the reliance on targeted human capital subsidies), some very poor households have been excluded, namely, poor households that live in less marginal localities that were not incorporated into the program and households that were deemed not to have adequate access to the necessary supporting public infrastructure (i.e. schools and clinics). From the perspective of horizontal equity and social justice, it is important that other components of the poverty alleviation strategy compensate for this exclusion. In terms of the human capital objectives, once one gets household members into school and health clinics, it is important to ensure that they receive quality services. In the case of health care, this requires that the package of services they receive are appropriate for the types of health problems to which they are susceptible as well as supporting public infrastructure (e.g. sanitation and sewage facilities). In the case of education, it requires that they achieve high educational standards, including the acquisition of skills that

are demanded by potential employers. However, once households invest in and receive higher human capital, it is equally important that the economic environment is such that this greater supply of human capital can be absorbed without diminishing the returns to such capital. In this respect, the key role played by efficient macroeconomic management to promote economic growth and increasing job opportunities is obvious.

4. A COST ANALYSIS OF PROGRESA

In this section we provide a detailed analysis of the levels and structure of the various cost components of the demand-side of PROGRESA. Although cost analysis is a crucial ingredient to economic analysis, it is also of use in its own right as a management tool since, for example, a detailed analysis of costs can direct management to areas where improved operational efficiency may have a high return. In Section 4.1 we start by giving a brief description of the evolution of the program and the various stages in its implementation. We then identify the various ingredients of cost analysis in Section 4.2, emphasizing the need to look beyond program costs to include private and social costs. Finally, in Section 4.3, we present detailed calculations of the various cost components. More specifically, we evaluate the cost efficiency of the program, i.e. the cost of getting transfers into the hands of beneficiaries.

4.1 The Evolution of the PROGRESA

The implementation of PROGRESA has involved two distinct stages.[14] The first stage involved the identification of the most marginal rural localities using a specially constructed "marginality index" based mainly on data from the national census. Based on this marginality index, selected localities are visited to ensure that they do indeed have access to the required supporting infrastructure in the form of schools and health clinics. The second stage involved the selection of households within eligible localities, using locality census data to classify households as "poor" or "non-poor" based on a discriminant analysis of household income and other characteristics. Once beneficiary households are identified, a general assembly is held to incorporate households and inform them of their responsibilities and rights and, more generally, of the objectives and functioning of the program.

The expansion of the program across localities took place in phases. The data collection for the first and second phases of PROGRESA began in October 1996, and this data was used to develop the method for household targeting based on discriminant analysis. In phase one (Table 1), incorporation of households began in August 1997 when 140,544 households in 3,369 localities were incorporated, with the first transfers taking place over September-October 1997. Phase two of the program began in November 1997 when a further 160,161 households in 2,988 localities were incorporated, with the first transfers taking place in January 1998. The 506 control and treatment localities used for the program evaluation come from this second phase. For the most part, expansion of the program over time has been determined by budget

[14] See Skoufias, Davis, and Behrman (1999) for more detail on these stages.

allocations,[15] and the greatest expansion occurred in 1998 (i.e. phases 3-6) when nearly 1.63 million families in 43,485 localities were incorporated. By the final phase11 of the program in early 2000, the program included nearly 2.6 million families in 72,345 localities in all 31 states. This constitutes around 40% of all rural families and one ninth of all families in Mexico.

The total annual budget of the program in 1999 was around $777 million, equivalent to just under 20% of the Federal poverty alleviation budget or 0.2% of GDP. Table 2 presents the total amount of transfers over each year from 1997-2001.

4.2 The Ingredients of A Cost Analysis

Undertaking cost analysis requires one to identify the total costs incurred as a result of introducing the program, to disaggregate these costs into various categories, and also to identify changes over time. For the purposes of cost analysis it is useful to identify costs according to:

- Whether they are incurred in cash or in-kind: Costs can involve either *financial* costs (e.g. salaries, user-charges or travel costs) or *opportunity* costs (e.g. time or "unpaid" personnel costs). Often the latter are wrongly overlooked.

- Who incurs the cost: Costs can be categorized as *program*, *private*, or *social* costs.[16] Program costs are those financed out of the program budget (e.g. administrative salaries), private costs are costs borne by beneficiaries (e.g. travel costs), and social costs are those borne by others (e.g. through taxes). All of these can be incurred either as financial or as opportunity costs. Often too much attention is focused on program costs.

- The timing of costs: *Fixed* costs are usually incurred at the start of the program before any "output" is produced and thus do not vary as output varies. These costs are often irretrievable (i.e. "sunk") once incurred. The size of *variable* costs, on the other hand, depends on the level of output. These costs determine how total costs vary with the size of the program (i.e., *marginal* cost), and can thus be crucial in determining the appropriate scale of programs and how costs will change as a program is expanded or across programs of different sizes. Whereas fixed costs are typically incurred at the beginning of a program (or at discrete intervals), variable costs are an important component of what is often referred to as *recurrent* costs, which are incurred at regular intervals. This separation of costs is important given the use of discount rates since costs incurred later in time will look smaller in present value terms. The presence of up-front fixed costs means that the average costs of generating benefits will decrease as the program is expanded spatially or through time.

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[15] In future work we hope to examine the relationship between program expansion and such variables as the marginality index.

[16] For certain purposes, e.g. understanding the political economy of programs or for budgetary purposes, it is also useful to classify costs according to which ministry or social group incurs them.

Breaking costs down into the above categories is a very useful mechanism for ensuring that important costs are not ignored and for the transparency and accessibility of any evaluation. Also, categorizing costs is also important in that not all costs are relevant to all policy questions. Below we describe how we identify and disaggregate the various costs incurred as a result of the introduction of the program. We discuss program, private and social costs in turn and, within each of these categories, we distinguish between financial and opportunity costs, as well as fixed and variable costs.

4.2.1 Program Costs

In order to disaggregate program costs we first differentiate by *activities*, namely: selection of localities (using national census data to construct a locality "marginality index"), identification of beneficiaries (through community ENCASEH censuses), incorporation of families, certification of compliance, cash transfers, and program monitoring. A description of each activity is presented in Table 3. These activities have a natural sequential ordering. The first three (i.e. selection of localities, identification of beneficiaries, and incorporation of families) are activities that must be undertaken up-front before any cash transfers take place, and thus constitute a one-off cost. The selection of localities is also a fixed (and sunk) cost that does not vary with the total size of the program (i.e. number of beneficiary localities or households). Therefore, this component of average fixed cost per peso transferred (or per household) will decrease as the program expands to include more localities and households (or as the level of transfers increases). On the other hand, the identification and incorporation of families involves costs that increase with the number of localities and households included in the program, but are unrelated to the size of transfers. The last three activities (i.e. certification of compliance, cash transfers, and program monitoring) recur throughout the life of the program and increase with the number of beneficiary households.

The process of allocating costs to activities involves the following steps. For a given year, the total program cost *incurred* is disaggregated into to the categories in Table 4. Some of these costs are allocated directly to some activities, e.g. the cost of collecting surveys is allocated to activities (2) and (7), the former relating to the cost of the baseline ENCASEH survey and the latter to the cost of the ENCEL surveys (see notes to Table 4 for more details). We deduct these from total program costs to get "adjusted total" program costs. These costs are in turn adjusted by replacing capital purchases with a more appropriate cost of capital (or capital use). This procedure is explained in Appendix Table 1. These adjustments result in a total program cost "adjusted for capital". This total is then inflated to 2000 prices to get a total "adjusted for inflation", which in turn is allocated to activities using the time-allocation matrix described in

columns 1-4 of Table 5.[17] Columns 5-8 contain these personnel costs. The costs presented in columns 9-12 are the costs directly allocated to certain activities from Table 2, inflated to 2000 prices.

In Table 6 we present the sum of annual personnel and these other operative costs and calculate the net present values (NPVs) of these costs using alternative discount rates of 3%, 5% and 8%; this implicitly views the program as a three-year program. Focusing on the net present values also indirectly accounts for the fact that much of the survey analyses that facilitated locality and household targeting (e.g. construction of locality marginality indices or developing procedures to target households) were incurred up front in 1997. Below, using NPVs, we analyze both the level and composition of program costs focusing on cost per peso transferred.

4.2.2 Private Costs

In order to qualify for and collect transfers, households must incur private costs, including both financial and time costs. To qualify for the food transfer household members must make regular trips to health clinics for check-ups and *platicas*. So households incur both financial and time costs of travel as well as a time cost for attending the clinic. To qualify for the education grants, children must incur similar travel costs as well as any forgone earnings from attending school. Also, beneficiaries must make bi-monthly trips to collect the cash transfers. In this section we analyze these costs in more detail. Whereas the program costs can be gathered from sources within PROGRESA, private costs (e.g. time, travel, forgone earnings etc.) have to be estimated from survey data. We use a combination of data sources (e.g. ENCEL, operations data, ENIGH, time-allocation data, locality data etc.) to build-up a picture of the composition and level of private costs. For the most part, we focus on actual costs, but one should keep in mind that for the purpose of evaluating the program only incremental costs are relevant, i.e. we should only include costs that would not have been incurred in the absence of the program.

Food Transfers

In order to qualify for the food transfer, household members must make a series of visits to health clinics for check-ups and health lectures. The required schedule of visits for the various household members is presented in Table 7. From here we can see that private costs will depend on the size and age composition of the family.

[17] Notice that this approach involves two implicit assumptions: (i) that each person in each activity carries the same average wage, in turn implying an identical mix of personnel of different skill and salary levels, and (ii) that an input of each person also requires a fixed amount of other inputs (e.g. stationery and utensils, computer usage, transportation, furniture and other overheads). In practice, one expects that some activities are more intensive in high-wage personnel or other inputs than others. The disaggregation of time allocation across the different program divisions is presented in Appendix Table 2 and disaggregation across state and federal levels is presented in Appendix Table 3).

To calculate the financial cost of travel, for each household we calculate the annual number of return trips as follows. We assume that the mother must accompany all children less than 17 years old to the clinic, thus incurring extra travel costs. We further assume that children 0-2 years do not have to pay for the trip, that children 3-5 years pay half price, and that everyone over 5 years pays full price. These factors are used to transform the number of "actual trips" into the number of "effective trips" for each household. The financial cost of trips for each household is then calculated as the number of effective trips times the cost of a return trip. For each household, the cost of a trip is taken as the median cost for their locality. If there is a health clinic in the locality then these costs are assumed to be zero. From Table 8 we can see that nearly 92% of localities, accounting for nearly 86% of households, do not have a health clinic in their locality. Household members on average make just over 25 trips per year, most of these obviously being accounted for by the two monthly trips made by mothers. The average distance traveled to the clinic is 3.98km, rising to 5.12km when zeros are excluded. The average cost of a return trip is $3.95, rising to $12.95 when zeros are excluded. We calculate that the total annual travel cost is on average $95.7 per family.

Households also incur time costs in traveling to and from the clinic, in waiting to be seen for a check-up, in attending the check-up itself, and in attending the platicas. Time costs are derived as follows. The travel time costs for each household are taken as the median travel time for households in the locality; note that these are zero when there is a health clinic in the locality but also zero if household members walk to the nearest health clinic. Households also incur time costs both waiting to be attended to at the clinic and the time taken for the check-up. The former is taken as the median of the relevant locality values given in the operations survey and the latter is assumed to be constant at 20 minutes for each check-up. The platicas are assumed to last one hour, based on a talk time of 45 minutes. The average travel time to the clinic is just over 118 minutes per return trip. The average annual travel time per household is just over 48 hours, equivalent to roughly 4 hours each month, most of this (c. 68%) being accounted for by the mother's time. On average, households have to wait nearly 39 minutes to be seen for a check-up (or consultation). In total then, each month household members make on average 2.1 trips, each taking nearly two hours travel time, one of these being a platica which takes up one hour, the other 1.1 trips incurring a 40 minute waiting time and a 20 minute consultation time. So, on average, household members incur around 6.3 hours in time costs in order to meet health-clinic attendance requirements.

Education Grants

As with health visits, households incur both financial and time costs due to children having to travel to and from school, to attend school, and to undertake homework. The approach used to derive these costs are similar to those used for health visits. Each household is attributed the locality's median travel time and cost to the nearest locality with a school, both being zero if there is a school in the locality. Financial costs are also zero if children walk to school. These numbers are applied to all children enrolled in school. For primary level it is assumed that all localities have a primary school so that travel time and money costs are approximately zero. From Table 9 we see that secondary school children spend on average nearly 65 minutes traveling to and from school, this rising to nearly 100 minutes when zeros are excluded. The average distance traveled to school is 2.54km, rising to 3.68km when zeros are excluded. This is

consistent with many children having to travel substantial distances to attend secondary school. In the sample, 17.3% of localities, accounting for 31.1% of secondary school children, have a secondary school. The average cost of travel is $1.58 per return trip, rising to $9.9 when zeros are excluded. On average then, households incur $316 in school travel costs annually, rising to $1,980 when zeros are excluded.

Cash Transfers

In principle, beneficiaries pick up their cash transfers once every two months. This implies both financial and time travel costs plus the time costs associated with waiting in line for the transfer. The procedure for calculating these costs is the same as that described above for health and education. Each household is allocated their locality median time and financial costs and we assume 6 trips per year. Table 10 shows that only 1.13% of localities, accounting for 2.79% of households, have a distribution point located in them. The average distance to a distribution point is 9.1km, rising to 9.33km when zeros are excluded. We find that households on average spend around 138 minutes traveling to and from transfer distribution points at an average cost per return trip of $12.53 per return trip. This implies a household on average incurs $75 annual in travel costs, rising to $113 when zeros are excluded. We also find that nearly 40% of households make at least monthly trips to these locations.

4.2.3 Social Costs

In principle, there are other costs incurred (or saved) or benefits received by others in the economy as a result of the program. These costs include the following:

(i) Taxes: The program costs may include taxes (e.g. income taxes and VATs) that are not true social or resource costs, but constitute a transfer of resources from the PROGRESA budget to general revenues. These should therefore be offset by treating them as benefits in the "government account".

(ii) Supply-side costs incurred by Ministries of Education and Health.

(iii) Other Programs: Since, in principle, beneficiaries must forgo benefits from other programs, both the net transfer to households and net budgetary impact are smaller. It is also important to identify what has happened to the allocation of resources from other programs. For example, is there any evidence of other programs withdrawing from beneficiary localities? Is the increased allocation of education and health resources to beneficiary communities financed out of decreases elsewhere (i.e. a reallocation within an overall fixed budget) or from "extra" resources. One could also include any adjustments in private transfers here.

The magnitudes of these costs and benefits are not calculated in this paper.

4.3 An Analysis of PROGRESA's Costs

In this section we bring together the cost information described above in a manner that helps to inform the policy debate. For this purpose it is useful to distinguish between two types of comparisons: (i) comparisons across different programs, (ii) comparisons across different policy questions.

With regard to comparisons across different programs, one can think of a number of different program designs. The present program is a conditioned-targeted program. Transfers are made conditional on household members attending school and health checks, and are essentially subsidies for the acquisition of these forms of human capital investments. These transfers are also targeted to poor households in the localities chosen to participate in the program. One can thus consider alternative programs that do not condition transfers and/or do not target households. Since both conditioning and targeting require program resources (e.g. to monitor households behavior and to administratively select "poor" households within participating localities), the costs of implementing unconditioned or untargeted programs will be lower than their conditioned or targeted counterparts.

With regard to comparisons across different policy questions one can distinguish between the costs associated with implementing the program from scratch (i.e. the actual program), the costs associated with expanding the program to incorporate more localities (i.e. program expansion), and the costs associated with continuing the existing program unchanged (i.e. continuation of the program). The relevant costs are generally lower in moving from the actual program to program expansion to program continuation, reflecting the presence of sunk costs. We discuss all these issues for program, private and social costs below.

4.3.1 Program Costs

Table 11 presents the relationship between the relevant program costs for the alternative program designs and alternative policy questions. Since both conditioning and targeting involve resource costs, the conditioned-targeted actual program has the highest total cost. To determine the costs relevant to the various program designs and policy issue combinations we make a number of assumptions.

Regarding alternative program designs, we assume that the costs associated with the identification of beneficiaries (ID) are incurred only when targeting: in the absence of targeting we assume that there is no need to collect and analyze household data (i.e. the ENCASEH surveys) because the required information regarding the existence and composition of households would be available from the national census. We also assume that in the absence of conditioning one would not incur the costs of incorporating households (IN) or incur the costs of certifying that they satisfy the conditions (C): incorporation is seen as a process of providing households information regarding their responsibilities and how the program operates, and certification is not necessary in the absence of conditioning.

Regarding alternative policy questions, we assume that the costs associated with selecting localities to participate (S), i.e. of calculating a marginality index etc., are not relevant when

deciding whether to expand the program to incorporate new localities (i.e. when addressing the issue of program expansion). The relevant information is assumed to have been accumulated up-front prior to the incorporation of the first of the localities.[18] In addition, the costs associated with targeting poor households and incorporating them are not relevant when addressing the issue of continuation (or suspension) of the program: the only extra costs incurred (or saved) due to continuing (or suspending) the program are the recurrent costs associated with transferring benefits and monitoring the program operations.

Each of the above combinations is associated with a different program cost. For each we calculate a cost-benefit ratio (CBR), derived as the relevant cost divided by the total food and education transfers. Since costs and benefits are both in NPV terms, we have a CBR for each program combination and for each discount rate assumed. These are presented in Table 12. Notice first that the CBRs increase with the discount rate reflecting the fact that many program costs are incurred up-front early in the life of the program while program benefits are received evenly throughout the life of the program. The CBR of 0.089 for the actual conditioned-targeted program tells us that every $100 transferred to beneficiaries cost the government $8.9. Or, in other words, of every $100 allocated to the program $8.2 is "absorbed" by administration costs. Given the complexity of the program, this level of program costs would appear to be quite small. It is definitely relatively low compared to the numbers given by Grosh (1994) for the LICONSA and TORTIVALES programs, which imply program costs of $40 and $14 per $100 transferred respectively.[19]

By comparing the CBRs across the different programs to that for the actual conditioned-targeted program we can identify the relative importance of the different activity costs. By far the biggest cost is that associated with targeting at the household level;, this accounting for nearly 30% of total program costs. This is followed by the costs associated with conditioning the program, which account for 26% of total costs. Dropping household targeting would thus reduce program costs to $6.2 per $100 transferred, while dropping conditioning would reduce the program cost to $6.6 per $100 transferred. Dropping both would reduce these costs to $3.9 per $100 transferred.

Certain costs are only relevant to particular policy questions. For example, the costs associated with targeting localities and households were incurred up-front and are therefore sunk: in other words, these will not be saved by suspending the program and, say, switching to another program. When these costs are taken out, the program costs reduce to $45.2 per $100 transferred.

[18] This may not be strictly true since we observe some locality selection costs throughout the life of the program. To capture this we could attribute some portion of costs in first year to "up-front locality selection costs" viewing the rest as a variable cost.

[19] These programs are also smaller urban programs, which one expects would have lower costs than their equivalent rural programs.

4.3.2 Private Costs

As well as having to incur program costs to transfer money to households, households also incur private costs. The relevant time and financial costs were identified above. For the purpose of calculating these costs we focus exclusively on the financial cost of travel. This is equivalent to assuming that the opportunity cost of time is zero, consistent with the household being able to sufficiently substitute time between activities so that only the most unproductive tasks are not undertaken. We now discuss the relative magnitudes of these costs for health, education and the collection of transfers.

Health: From earlier, we know that the average cost of trips per family is $95.7 per annum. Households receive $125 per month in food transfers, equally to a $1500 annual transfer. This means that households incur travel costs of $6.38 per $100 received. However, this is a substantial overestimate of the incremental private costs since this cost is only additional for the extra trips brought about by the program. According to Gertler (2000), the program brought about a 30%-50% increase in the number of trips. Using an estimate of a 40% increase, this implies that only 28.6% of total trips are additional. This in turn implies that the incremental private costs of receiving the food transfer are $1.82 per $100 received.

Education: Based on the school calendar, the private costs discussed earlier imply an annual cost of around $316. If a household receives $217 per school month, this gives $2170 per year. Both these numbers imply that households on average incur $14.6 for every $100 of education grants received. However, as with health above, this is a substantial overestimate of the incremental cost due to the program since most of these travel costs would have been incurred in its absence. Based on Schultz (2000), we assume that the program brings about an 8.2% increase in enrolment levels at secondary school from a starting average enrolment rate of around 70% so that only 10% of travel costs are additional. Using this number we can then calculate that households spend, on average, only an additional cost of $1.5 per $100 received. This number implicitly assumes that the opportunity cost of children's time is, on average, zero, which is consistent with the findings of Parker and Skoufias (2000).

Cash Transfers: The average transport costs incurred to collect transfers were earlier calculated as $75.2 per annum. Households on average receive monthly $219 in education grants (over 10 months) and $125 in food transfers, giving an average annual household transfer of $3900. This implies that households incur $1.9 per $100 received. Adjusting for the fact that these trips are additional for only 60% of households, this implies private cost of $1.2 per $100 received.

4.3.3 Total Costs

In this section we aggregate across program and private costs. Whereas program costs are relevant to the total transfer a household receives, this is not so for all private costs since the private costs associated attending health clinics are relevant only for the consumption transfer and the private costs associate with secondary school attendance are relevant only for the secondary grants. We assume that private costs associated with primary school are zero. The private costs associated with collecting transfers are relevant to the total transfer. To aggregate program and private costs we use the following transfer shares: food/health 35% and secondary

schooling 41%. Also, the private health and schooling costs are relevant only for the conditioned program. So, for example, the total private costs per peso transferred for the actual conditioned-targeted program (Table 12) are: 0.113=0.089+(0.018*0.35)+(0.015*0.41)+0.012.

From Table 12 we see that relevant total costs lie in the range 0.113-0.047, depending on the program type and policy question being addressed. Focusing on the actual conditioned-targeted program, private costs increase total costs from 0.089 (i.e. program costs) to 0.113, i.e. by about 27%. In this respect, ignoring private costs will obviously lead to a substantial underestimate of total program costs. So, for every 100 pesos transferred to households 11.3 are incurred in administrative and private costs. This falls to 7.8 when conditioning is dropped, 8.6 when targeting is dropped and 5.1 when both conditioning and targeting are dropped. These are the costs we will use when we integrate the program benefits and costs into a cost-benefit analysis of the program below.

In conclusion, the administrative costs employed in getting transfers to poor households appear to be small relative to the costs incurred in previous programs and for targeted programs in other countries. This is in spite of the program being quite a complex program, which involves both the targeting and conditioning of transfers and all the costs that such activities entail. In fact, both these dimensions are large proportions of the total administrative cost. It is important then that the resources spent on these activities generate the expected gains in terms of improving the distributional power of the program and in encouraging human capital accumulation by households. These aspects of the program are discussed below. It is also important to recognize that households incur financial (and time) costs in traveling to collect transfers and to meet the conditioning requirements. It turns out that these are a substantial in relation to program costs. This highlights the need to examine ways in which such costs can be reduced, e.g. through more distribution points or improved transport facilities.

5. A COST-BENEFIT ANALYSIS OF PROGRESA'S CASH TRANSFERS

An important objective of the program is the alleviation of current poverty through cash transfers. In this section we evaluate the efficiency of the program in achieving this objective. Two dimensions of the program influence its distributional efficiency: (i) the fact that it is targeted, and (ii) the fact that the level of transfers a household receives depends in part on its demographic composition. The program is targeted in two respects. Firstly, it is targeted to the poorest (or most marginal) rural localities, i.e. it is geographically targeted. Secondly, it is targeted at "poor" households within these localities. Although geographic targeting is usually very effective at increasing the distributional power of a program (e.g. by ensuring that a relatively high percentage of the program budget gets to poor households), it has a serious shortcoming from the perspective of overall poverty alleviation, namely, it leaves out poor households that do not live in the poorest localities. As indicated earlier, this outcome is undesirable from the perspective of horizontal equity. Similar problems also arise from the fact that poor households deemed not to have access to adequate supporting infrastructure (i.e. schools and health clinics) are also excluded. However, in this report we do not evaluate these features of the program, except insofar as to point out that it is crucial that other components of

the development and poverty alleviation strategy address this issue. Rather, here we focus on the second stage of targeting within localities. We also analyze the implications of the linking of the transfers to household composition.

The objectives of this section are threefold. We wish:

- to understand how the different components of the transfer system contribute (or detract) from the distributional power of the program;
- to determine how the existing structure of the transfers compares to a range of alternatives; and
- to understand any trade-offs that exist between the poverty alleviation and human capital accumulation objectives of the program.

We are particularly interested in understanding the implications for the targeted and demographic features of the transfers.

In Section 5.1 we briefly explain the targeting process employed by the program and identify alternative program designs. This is followed by a discussion of the methodology used to measure the relative efficiency (or distributional power) of the program in Section 5.2. Our results are presented and discussed in Section 5.3 and Section 5.4 summarizes and concludes.

5.1 PROGRESA and Program Alternatives

The targeting process adopted by PROGRESA is essentially a two-stage process.[20] Using the national census data, an index of marginality (IML) is constructed for each locality. Based on this index, the most marginal localities are chosen to participate in the program. Once participating localities are identified, PROGRESA then undertake a locality census (ENCASEH) that includes data on household demographics, income and assets. Households are categorized as "poor" and "non-poor" based on income with reference to a standard food basket. Households are then reclassified using discriminant analysis and household characteristics other than income, e.g. dependency ratio, characteristics of household head (i.e. age, sex, occupation and schooling), and dwelling characteristics. This classification process appears to have changed over time: the initial classification (PRO) had just over 52% of treatment households classified as poor, but this increased to just over 78% with the "densification" process (PROD) which used a higher poverty line. The increase in the percentage of households classified as poor came about essentially due to a community participation process that suggested that the program selection mechanism had led to a substantial underestimate of the poverty rate.

Nationwide, by the end of 1999, the program was being implemented in nearly 50,000 rural localities in over 2,000 municipalities in 31 states. In all, approximately 2.6 million families, equivalent to 40% of all rural families and one ninth of all families, were receiving benefits. The

[20] See Skoufias, Davis, and Behrman (1999) for details.

total budget of the program was around $777 million, equivalent to just under 20% of the Federal poverty alleviation budget or 0.2% of GDP.

This report uses the ENCASEH data for the 14,856 households in the "treatment" sample. Using data on household composition, we first estimate the benefits received by households based on the payment schedule set out in Table 1. For all households identified as "poor" by PROGRESA, payments are linked to the number, age and gender of children. Therefore, one can view the program as involving a combination of poverty (or means) and demographic (or statistical) targeting. Later we compare the relative contribution of these two dimensions to the effectiveness of the program is distributing income to households in need. Table 2 presents average transfers by component separately for control and treatment localities. In treatment localities the transfers account for, on average, nearly 29% of total household consumption.

The estimated benefits received by households are essentially *theoretical* transfers, i.e. the transfers that would exist if there was 100% take-up by all eligible (i.e. poor) households. This hypothetical program acts as our reference for evaluation purposes and one would expect its benefits structure to resemble that of PROGRESA if the transfers were unconditional. We compare the welfare impact of such a program with the following alternatives:

i. *Pre-Densification Transfers* (PR): We compare the present post-densification pattern of transfers (PRD) with that which existed prior to the increase of the poverty line.

ii. *Uniform Targeted Transfers* (PRDU): Instead of poor households receiving transfers linked to demographic characteristics, one can consider a uniform transfer to these households.

iii. *Uniform Universal Transfers* (NoTgtU): Same as (ii) but now all (i.e. poor and non-poor) households receive a uniform transfer.

iv. *Non-Targeted Transfers* (NoTgt): A program without within-locality targeting, where all households in the selected localities receive the benefits.

v. *Transfer Components*: We decompose the welfare impact of each program component (i.e. primary scholarships, secondary scholarships, school materials, and food transfer) in order to identify the contribution of each to the total welfare impact. This analysis will inform the issue of the welfare impact of a change in the structure of the transfers (e.g. reducing food transfers or primary scholarship levels to finance an increase in secondary scholarships in order to get a greater education effect).

vi. *Intensive Expansion*: Rather than expanding the program across localities (i.e. *extensive* expansion), one could expand intensively by scaling up the benefits in the poorest localities.

vii. *Actual Transfers*: which allows for some households not receiving the theoretical transfers because they decide not to take-up certain benefits or don't satisfy certain

conditions.[21] Households that do not undertake their scheduled visits to the health center do not receive the food transfer. Neither do households in which children do not meet the 85% school attendance criterion receive transfers for these children. In addition, actual benefits may differ from hypothetical because of operational delays in collecting and processing school and health center attendance data as well as in sending out transfers.

Note that the reference program is the post-densification program. Where the total budgets of the programs differ from the actual post-densification budget, the benefits are effectively scaled up or down appropriately. All of the above programs will differ in terms of benefit structure and total costs. The differences in benefits are due to the fact that different households, e.g. differentiated according to initial welfare, receive benefits and in different amounts. The differences in costs reflect the presence or absence of targeting. For example, to target within localities one needs to undertake a survey of these households. These survey costs will presumably differ according to the amount of extra information required (e.g. demographic and income information) as well as the cost of getting to localities and the number of households to be surveyed. In this section we focus exclusively on the benefits side of the analysis, with costs being incorporated in the next section.

5.2 Methodology

In this section we motivate and discuss the methodology employed to compare across alternative allocations of the program budget. For this purpose it is useful to set out a very simple model of an economy with two groups, namely, households and the government.[22] The objective of public policy is taken to be an increase in social welfare, which in turn depends on household welfare. For our purposes, the objective of the "social planner" may then be specified as choosing the size of the transfer to or from each household so as to maximize social welfare subject to the government budget constraint that the total amount to be disbursed (i.e. budget available for fighting poverty) equals the total amount raised by lump-sum taxes. Specifically, social welfare is specified as a function of household welfare, V(**p**,m), where **p** is the vector of commodity and factor prices faced by the household and *m* is lump-sum transfers to or from the government. The Lagrangean function for the planner's problem can thus be written as choosing a set of values m^h for each household *h* so as to:

$$\max \Psi = W(...,V^h(p,m^h),...) + \lambda \sum_h m^h$$

where W(.) is the social welfare function and λ is the Lagrange multiplier associated with the budget constraint.[23] This specification is essentially the specification for the determination of the

[21] Also, in reality, scholarships are linked to grade not age.

[22] This section draws directly on Skoufias, Coady and Davis (2000). See also Coady and Skoufias (2000) and DrPze and Stern (1987) for a more rigorous discussion of the model.

[23] This formulation of the problem essentially assumes that cash transfers are non-distortionary lump-sum transfers and that no other distortions exist in the economy. Although restrictive, this simple formulation is adequate for our purposes. See DrPze and Stern (1987) for

optimal pattern of cash transfers that maximizes social welfare. As is well known (Atkinson and Stiglitz, 1980; Stiglitz, 1988), the solution to this optimization problem is determined from the first-order necessary conditions:

$$d\Psi = \frac{\partial W}{\partial V^h}\frac{\partial V^h}{\partial m^h}dm^h - \lambda dm^h = \beta^h dm^h - \lambda dm^h = 0\,,\ \forall h$$

which implies $\beta^h = \lambda$, for all *h*, where β^h is the social valuation of an extra unit of income to household *h*, the so-called "welfare weight" of household *h*. In other words, at the optimum, the pattern of transfers must be such that the social valuation of income at the margin is constant across all households. If all households are modeled as having the same welfare function then the optimum is characterized by an equal distribution of income.

The above essentially assumes that the poverty-alleviation budget is endogenously determined. In practice, however, there are economic, social and political constraints both on the size of budgets and their distribution. By summing across all households the above first-order conditions can be re-written as:

$$\lambda = \frac{\sum_h \beta^h dm^h}{\sum_h dm^h}\,.$$

In the absence of an optimal distribution of income, β^h will in general differ across households. One can interpret alternative income vectors $d\mathbf{m}=\{\ldots,dm^h,\ldots\}$ as representing alternative targeting schemes for a given budget. Therefore, λ will differ across targeting schemes both because β^h differs across households and the structure of $d\mathbf{m}$ differs across alternative programs.

Underlying our objective of poverty alleviation must be the view that extra income to low-income (or poor) households is more socially valuable than extra income to high-income (or non-poor) households. Making this view explicit essentially requires the specification of a set of "welfare weights" and we expect this weight to decrease with the (initial) consumption (or welfare) level of the household. The welfare weight for each household (β^h) can be derived as follows:

$$\beta^h = (y^k / y^h)^{\varepsilon}$$

where *y* refers to consumption (or "permanent income"), *h* superscript denotes the household in question and *k* superscript denotes a reference household, which always has a weight of unity (e.g. the household just on the poverty line, in which case $y^k=z$, where z is the poverty line).[24]

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details and Coady and Harris (2000) for an application where tax distortions already exist and must be manipulated to finance the program.

[24] Which household we use as the reference household to normalize welfare weights is irrelevant to our analysis. See, for example, Ahmad and Stern (1984; 1991, p129) for discussion on the choice of welfare weights.

The term ε captures one's "aversion to inequality" of income or consumption and determines how the welfare weights vary (i.e. decrease) with household income. For example, a value of ε=0 implies no aversion to inequality and all welfare weights take the value unity, i.e. an extra unit of income to households is viewed as being equally socially valuable regardless of initial consumption level. A value of ε=1 implies that if household *h* has twice (half) the income of household *k* then its welfare weight is 0.5 (2.0) as opposed to unity for *k*. A value of ε=2 similarly implies a welfare weight of 0.25 (4.0) for *h*. As ε approaches infinity the impact of the program on the welfare of the lowest-income group dominates any evaluation, consistent with a Rawlsian maxi-min social welfare perspective where one cares only about how much of the program benefits are received by the poorest of the poor. The welfare weights used in our simulations presented below use initial consumption as their welfare reference and we also evaluate the sensitivity of our findings to different sets of welfare weights based on different degrees of aversion to inequality of initial consumption (i.e. different values of ε). Consistent with the program objectives, we consider only values of ε>0.

Once we have specified welfare weights, we can then evaluate the welfare impact of a program (dW) as:

$$dW = \sum_h \beta^h dm^h$$

where the level of transfers is dm^h (which in general can be positive, negative or zero) for household *h*. A program that transfers more of the budget to poor households (i.e. households with relatively high β^h) will exhibit a higher dW, and thus will look increasingly attractive as an income redistribution mechanism the higher the value of ε. As indicated above, one can transform this statistic into a more conventional benefit-cost ratio by dividing by the overall poverty budget to get, for each program *j*:

$$\lambda_j \equiv \frac{\sum_h \beta^h dm_j^h}{\sum_h dm_j^h} \quad \text{.....(1)}$$

which can be interpreted as the marginal social value of a unit of revenue transferred to households through the program in question. This statistic is commonly referred to in the literature as the *distributional characteristic* of the program (or any policy instrument) and it captures the distributional power (or efficiency) of the program (Feldstein, 1974). It essentially captures how effective a given program is at getting the transfer budget to the most needy households. It can be used to compare the relative welfare impact of alternative programs or of reallocations of a budget between different programs. Notice that it is independent of the size of the budget, i.e. scaling up or down benefits and the budget of a program will not change its distributional characteristic. It can also be usefully re-written as:

$$\lambda_j \equiv \sum_h \beta^h \theta_j^h \quad \text{.....(2)}$$

where 2_j^h is the share of household *h* in the total budget of program *j*. Programs in which those receiving relatively high shares of the budget have relatively high welfare weights (i.e. are relatively needy) will obviously have relatively high welfare impacts.

In our empirical analysis presented below, we focus on λ_j as opposed to dW but, as indicated above, given the assumption of a fixed budget, both are equivalent for evaluation purposes.[25] The relative welfare impact of alternative programs can be gauged by comparing λ_j across these programs. When program budgets differ then it is also useful to think of the effect on welfare as:

$$dW = \lambda B \quad \text{.....(3)}$$

so that:

$$W^* = \lambda^* + B^* \quad \text{.....(4)}$$

where an asterisk denotes a proportional change. In other words, the proportional difference in the impact between two programs can be seen as the sum of the proportional difference in the welfare impact per unit of budget expenditure (i.e. the distributional characteristic) plus the proportionate difference in the total budget. For well-targeted programs one expects λ^* to be negative in the face of program expansion to include more households (i.e. extensive expansion due, for example, to an increase in the poverty line being used to select households). For intensive expansion, i.e. an increase in the transfer levels to existing beneficiaries, $\lambda^*=0$ so that $W^*=B^*$. In other words, the proportional difference in program budgets can be taken as the proportional difference in their welfare impacts only for an intensive expansion of the program.

One can also use the differences across λ_j to calculate the impact on welfare of transferring a unit of the program budget across programs, i.e. marginal reallocations of the budget. In addition, one can easily allow for other costs associated with identifying and transferring cash to households by including these costs (which may differ across programs) in the denominator, which is interpreted as the fixed program budget. The higher these costs, the less money that is available to be distributed to households. We return to this issue in the next section, which undertakes cost-benefit analysis.

Since the program is made up of a number of components (i.e. scholarships, food etc.), in our analysis below we will find it useful to decompose the welfare impact by component. By viewing *dm* as the sum of a number of components it is easy to show that:

$$\lambda = \sum_i \lambda_i \sigma_i \quad \text{.....(5)}$$

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[25] Note, however, that we are not comparing alternative programs to an "optimal transfer scheme" as in, for example, Chaudhuri and Ravallion (1994). The λ for such a program will obviously be the highest attainable, but is hypothetical in so far as other factors (e.g. the existence of multiple objectives, or information, social or political constraints) in practice determine the structure of benefits. Both Chaudhuri and Ravallion (1994) and Schady (1999) focus on the minimum cost of achieving a given poverty impact across a range of transfer schemes, including an optimal transfer scheme. In any case, our approach here can easily incorporate such a comparison.

where λ is the distributional characteristic for the full program, λ_i is the distribution characteristic for component *i* in isolation, and F_i is the share of the transfers allocated to component *i* in the total program budget. By comparing across λ_i one can determine the relative distributional power of the various components.

For our purposes below it is also useful to decompose the λ for each program into the sum of its *targeting efficiency* (λ_T) and its *redistributive efficiency* (λ_R) by adding and subtracting the *average* level of the transfer across all *beneficiaries* (i.e. across households with $dm^h>0$) to get (Coady and Skoufias, 2000):

$$\lambda = \frac{\sum_h \beta^h dm^*}{\sum_h dm^h} + \frac{\sum_h \beta^h (dm^h - dm^*)}{\sum_h dm^h} = \lambda_T + \lambda_R \quad \text{.....(6)}$$

where dm^* is the average level of the transfer. One can interpret λ_T as the welfare impact of a program which transfers the poverty alleviation budget to the same beneficiary (i.e. poor households in the case of PROGRESA) households but in equal amounts (i.e. poverty targeting), and λ_R as the adjustment that needs to be made to allow for the differentiation of the transfers across households in a more progressive ($\lambda_R>0$) or regressive ($\lambda_R<0$) manner (e.g. through demographic targeting).

5.3 Benefit Analysis

We now present the results from our empirical analysis. In general, we are interested in evaluating the distributional power of PROGRESA relative to alternative ways of distributing the program budget (i.e. alternative program designs). In this sense, we are interested here only in the current poverty alleviation objective of the program.

5.3.1 Comparison of Pre- and Post-Densification PROGRESA

As indicated above, the classification of households as "poor" and "non-poor" appears to have changed over time as a result of a "densification process". In order to evaluate this change, as well as to facilitate comparison to the earlier evaluation of PROGRESA's targeting (Skoufias *et al*, 1999), for both stages of the process we first compare the leakage (L) and undercoverage (U) rates, defined respectively as the percentage of poor households wrongly left out of the program (i.e. errors of exclusion) and the percentage of beneficiary households that are wrongly included (i.e. errors of inclusion). This requires that we establish an "ideal" welfare indicator and, in line with convention in economics (Ravallion, 1997; Deaton and Zaidi, 1999), we choose household per adult equivalent consumption.[26] The distribution of this variable (henceforth referred to

[26] We use an updated version of the measure used in Skoufias, Davis, and Behrman (1999), which was provided by Emmanuel Skoufias to whom we are very grateful. Based on this consumption measure and the transfer schedule in Table 1, the program budget was 22.5% of the poverty gap and transfer levels were on average equal to 21.1% of total household consumption.

simply as consumption) is presented in Figure 1 together with the pre- and post-densification poverty lines.

Using consumption as the reference welfare measure, we classify households as poor and non-poor and compare this with PROGRESA's classification, which was based on income. We find that pre-densification U and L were both 27% compared to post-densification where both were around 16%. This difference in part just reflects the fact that a higher percentage of households were included post-densification thus leaving less room for U and L errors.

However, what matters more for the welfare impact of each program is where these errors occur in the distribution of consumption (e.g. are they concentrated around the poverty line or spread out). For example, the welfare losses from mis-targeting will be relatively high if a high proportion of the poorest households are wrongly excluded and/or if a high proportion of the richest households are wrongly included. We can examine this by looking at "predicted error probability" (PEP) curve used in Skoufias, Coady and Davis (2000). We construct a binary variable where each household that is misclassified as poor or non-poor by PROGRESA's methodology is assigned a value of unity with all other (correctly classified) households being assigned a value of zero. We then simply plot the averages for the various consumption 5-percentiles (Figure 2). Notice that although pre-densification the curve is bell-shaped, with the percentage error decreasing the further one gets from the poverty line, post-densification this is not the case with over 45% of households in the top 5% consumption bracket being wrongly included in the program. This is suggestive of substantial welfare losses due to mis-targeting. This comparison also highlights the fact that comparing leakage rates across programs of different sizes can be very misleading; they are relatively low post-densification because, although a relatively high proportion of the non-poor are wrongly classified as poor, these are a much lower percentage of the total households receiving benefits given that nearly 80% receive benefits. Similarly with undercoverage, since the greater the proportion of total households included in the program the lower the potential for undercoverage.

We finish this section by plotting the percentage of households classified as poor in each locality ordered by marginality index and grouped into 5-percentiles (Figure 3). We do this for both the post- and pre-densification programs, but also for the post-densification program under the assumption of perfect targeting. Comparing the pre- and post-densification relationship between locality coverage and marginality, we see that the biggest increases in coverage occurred in the least marginal localities, i.e. those localities in the bottom quartile according to the marginality index. Since mean income is negatively correlated with marginality and given the observed high levels of leakage in the highest income groups, one expects that much of this increase is due to mistargeting. Comparing PROGRESA's post-densification coverage levels with those based on consumption (i.e. perfect targeting) it is clear that the inclusion of non-poor households in the least marginal localities came mainly at the expense of poor households in localities with marginal indices just above the least marginal localities, i.e. in the next to bottom quartile according to the marginality index. This suggests that one of the first tasks of the planned recertification process at the end of three years participation by households could be to correct for this mistargeting by removing non-poor households in localities in the bottom marginality quartile and replacing them with poor households in the next to bottom marginality quartile.

5.3.2 The PROGRESA Program versus Alternatives

In this section we compare the welfare impact of PROGRESA with that of the alternatives identified earlier. The welfare impacts of all these programs are presented in Table 3 and their performance relative to PROGRESA post-densification plotted in Figure 4. Focusing on Figure 4, the first thing to notice is that the distributional efficiency is higher pre-densification compared to post-densification. For a moderate concern for income distribution (ε=2) the welfare impact per unit expenditure is over 12% higher pre-densification compared to post-densification. This of course is not surprising since, for the most part, the densification process is about incorporating households that were previously deemed to lie above the poverty line and, for well-targeted programs, the distributional characteristic will always decrease as the program expands to include extra households. But in moving to the post-densification program the budget also increases by 37.5% which, using (4) above, implies an overall welfare increase of 25.5%. The question then becomes whether the decrease in the distributional power of the program could have been lower. For this one needs comparisons with other potential programs, a natural one being a program that targets optimally using household consumption levels.

What is somewhat surprising is that the post-densification program compares very favorably to the situation with optimal targeting. In fact, it dominates the latter for higher levels of inequality aversion. The intuition behind this at first counter-intuitive result lies in the realization that while the "optimal" program perfectly identifies poor households, the transfer *levels* are not optimal across households; this would have included higher transfers to the poorer households. What is happening is that those wrongly incorporated under the densification process as well as having relatively low welfare weights also receive relatively low transfers thus increasing the relative share of the total transfer budget received by poorer households, which have higher welfare weights. With consumption targeting the households correctly included are moderately poor and receive relatively high transfers thus decreasing the proportion of the budget going to the severely poor. Thus, the distributional efficiency of the program, which can be seen as a weighted average of the welfare weights of beneficiaries with the weights being the share of the overall budget they receive, decreases. In the terminology of Coady and Skoufias (2000), although the "targeting efficiency" (i.e. who you hit) of the densified program is lower than that of a perfectly targeted program, its "redistributive efficiency" is higher. Using the decomposition described in (6), we can see from Table 4 that for ,=2 although the targeting efficiency is higher for consumption targeting (λ_T=4.07 as against 3.74) the redistributive efficiency is relatively low (λ_R=0.97 as against 1.31).

The results in Table 3 and Figure 4 also indicate that the (average) gains from targeting households within localities is in the range 3%-14%, depending on one's aversion to inequality - below we look at the distribution of this gain across localities with different characteristics. There are also very sizeable gains both from differentiating payments by demographic composition as opposed to uniform transfers ranging from 9%-39% when the program targets poor households and 12%-49% without household targeting. Both these gains increase with the

level of aversion to inequality.[27] So, in conclusion, it is clear that in spite of the mistargeting inherent in the densification phase of the program, the distributional power of the program is still relatively high. PROGRESA is an extremely effective program in terms of getting transfers into the hands of the most needy.

5.3.3 Distributional Efficiency of Individual Program Components

The structure of PROGRESA's transfers reflects its underlying objectives of improving the current welfare of poor households while simultaneously encouraging households to invest in their human capital. One of the issues being discussed by policy makers is whether or not the structure of benefits should be changed, in particular whether a restructuring of scholarships so as to give higher grants to secondary school children and lower grants to primary school children is desirable. From an education point of view this would appear desirable (Schultz, 2000) since the biggest enrollment impact comes from this older age group where enrollment is still relatively low compared to primary enrollment levels. Here we are concerned with the trade-off in terms of current welfare inherent in such a restructuring of benefits.

To address the above issue we can interpret each of the separate components of the program as alternative transfer instruments. By calculating a distributional characteristic for each component using (5) we can identify the welfare impact of reallocating a unit of the budget between components. A crucial feature of the program, however, is that total monthly payments to households are capped at 550 pesos per household. Therefore, transfers of the budget between program components may have very little effect on the net transfers to capped households compared to uncapped households. This would appear to be particularly important for the distributional impact of the program given that from Figure 5 we can see that it is the poorest of the poor who are relatively constrained by the cap, with 70% of the poorest income group being capped. The difference between capped and uncapped transfers is also greatest for these households (Figure 6). We address this issue by analyzing the distributional impact of components with and without a transfer cap. We finish this section by analyzing the welfare impact of scaling down primary school transfers to finance a scaling up of secondary scholarships.

Table 5 and Figures 7a, b present the relative welfare impacts of the various program components. It is clear that the more concerned we are about those suffering from severe poverty the more attractive are the educational components compared to the food (or health) component from a distributional perspective. This reflects the fact that the former payments are linked to the number of children in a household whereas the latter is a uniform transfer across households, and the number of children in a household is positively correlated with household welfare. It is also clear that the capping of transfers reduces the redistributive power of transfers, consistent with the poorest households being more likely to be capped since they have more children. Also, in the absence of capping, primary and secondary transfers are equally

[27] In Coady and Skoufias (2000) we find that the gains from poverty (or means) targeting are generally relatively high. The relatively low gains found in the present analysis reflects the fact that we are ignoring the first stage of the targeting process, which targets the poorest localities.

redistributive. However, when transfers are capped, primary transfers appear more distributionally attractive the greater our concern for the poorest households: the welfare impact from increasing primary transfers compared to secondary transfers is nearly 17% at moderate levels of inequality aversion (ε=2), increasing to just over 26% for higher levels (e.g. ε=5).[28] This suggests, therefore, that in the presence of capping restructuring transfers in favor of secondary school children (in an attempt to increase the overall enrollment impact) may involve a trade-off in terms of a lower impact on current welfare for the poorest households.

The above analysis is strictly only valid for very small (i.e. marginal) changes in the payments structure since it assumes that those households that are capped do not become capped due the restructuring of the transfer system. For larger changes this will presumably not hold since households are constrained to different degrees (i.e. the benefits actually received are different distances below the uncapped levels) and a crude categorization of household into capped and uncapped may be misleading. We therefore conclude this section by evaluating the welfare impact of a specific restructuring of the transfer scheme: a 10% increase in secondary scholarships with the budget held constant by an appropriate decrease in primary grants. We also replicate a modified version of this with the cap also being rescaled up by 10%. Our results indicate that although welfare decreases when we increase secondary grants at the expense of primary grants, these welfare losses are always less than 1%. This lower welfare loss indicates that the restructuring enables the poorest households to get a greater share of the budget than is suggested by the marginal analysis, consistent with some of the higher income households becoming capped under the restructured transfer system. When we also scale up the cap by 10% welfare increases by a high of 1.9% when ε=5. These results confirm that the welfare losses from restructuring scholarships with the objective of enhancing the enrollment effect of the program are relatively small, but also that these could, if required, be offset by an increase in the maximum transfer allowed per household.

5.3.4 Intensive versus Extensive Expansion

The approach taken by PROGRESA has been to apply a given structure and level of benefits to all localities. One can interpret this as an *extensive* expansion strategy with the budget being exhausted by incorporating new localities keeping the level and structure of payments fixed. Under this scheme, everything else being equal (i.e. demographic composition), the poor in all localities receive the same level of benefits regardless of the depth of poverty in the locality. Alternatively, one can consider an *intensive* expansion strategy under which the same structure of benefits is applied to all localities but instead of increasing the number of localities incorporated into the program the *level* of benefits to existing localities could be increased. In order to identify the potential gains from such a strategy one can compare distributional characteristics across localities. This comparison could be used to identify either welfare improving reallocations of the fixed budget across localities (i.e. decreasing the level of transfers in localities with relatively low distributional characteristics to finance an increase in those with relatively high distributional characteristics) or identifying which localities would benefit most

[28] In the presence of capping we assume that capped households do not receive any extra funds allocated to either the primary or secondary budgets, while other households receive amounts that keep their relative shares of the budget constant.

from such an intensification strategy financed by a higher overall budget. Note also that such comparisons can be useful in identifying which control (or non-beneficiary) localities might be included first into the program, e.g. by giving priority to those with high distributional characteristics.

The variation in the distributional characteristics across localities captures the potential for welfare improving transfers between localities, i.e. the benefits of intensive as opposed to extensive expansion of the program. As one would expect, this variation increases with our aversion to inequality with the gini coefficient for locality distributional characteristics increasing substantially from 0.048 (for ϵ=0.5) to 0.169 (for ϵ=2) and to 0.312 (for ϵ=5). One expects the welfare impact of the program to be highest in localities with a lower mean consumption level and also, for a given mean consumption, to increase with the level of inequality in the locality capturing the potential for redistribution. This is borne out both by non-parametric and parametric analysis. Figures 8 plots the relationship between a locality's distributional characteristic and its mean consumption level for ϵ=2 and indicates a clear negative relationship – a similarly strong negative relationship was observed for ϵ=5. Regressing distributional characteristics on consumption we find that for ϵ=2 we get an income elasticity of –1.19, this elasticity increasing with inequality aversion (Table 7). Such a regression explains around 63% of the variation in the distributional characteristic; this decreasing with inequality aversion.[29] When we introduced the log of locality inequality as an extra independent variable this elasticity increased to –1.24 and the explained variation to 71%.[30] The inequality elasticity of the distributional characteristic comes out at 0.28 and is increasing in inequality aversion. To get an indication of the scope for welfare improving reforms, the mean of the distributional characteristic for ϵ=2 is 5.01 with a standard deviation of 1.49. Comparing reallocations one standard deviation either side of the mean, welfare could be increased by around 85%. Therefore, extensive rather that intensive expansion has large welfare costs from the perspective of current poverty alleviation.

Although the strong relationship between consumption and the distributional characteristic makes the former an obvious candidate around which to formulate a decision rule to inform movements towards intensification, this information is not available for all localities in the program. An obvious alternative candidate is the locality marginality index, which was used to select localities into the program. Figure 9 presents a plot of the relationship between this index and the distributional characteristic of the locality. Although there is a clear positive relationship between a locality's distributional characteristic and its index of marginality, our regression analysis indicates that the latter has much less explanatory power than locality consumption, indicating a lot of variation in the distributional characteristic for a given level (or range) of the index of marginality. But this does not mean that it is not a useful basis for a reallocation decision rule. For example, if we divide localities into deciles according to their marginality

[29] The explained variation was higher at 72% when we used a quadratic in place of the double log specification.

[30] The inequality measure used was one half of the square of the coefficient of variation, which is one of the commonly used measures and a member of the general entropy family of inequality indices. Using a measure more sensitive to inequality at the bottom of the distribution would probably increase the explained variation for higher degrees of inequality aversion.

index we find mean distributional characteristics for the top and bottom deciles of 6.54 and 3.45. Reallocating funds between these two sets of localities will thus result in a 90% increase in welfare per unit expenditure. However, it is likely that the human capital objectives of the program are a strong motivation behind the existing strategy of extensive expansion and thus also a strong counter-argument to the differentiation of transfer levels across localities.

5.3.5 The Benefits from Targeting

An important policy issue concerns the magnitude of the welfare gains from targeting poor households within localities. Our earlier discussion concentrated on the overall, or average, welfare gains from targeting; from Table 8 we can see that on average the welfare impact per unit expenditure increases by 2.9% for ϵ=0.5 to 10.7% for ϵ=2 to 15.8% for ϵ=5. In general, one expects the gains from targeting to depend on:

- *The proportion of households that are to be included in the program*: The greater the proportion of households to be included (or the lower the proportion to be excluded), the lower the gains from targeting. For example, the gains are obviously zero if all households are eventually classified as poor and included. This explains the fairly modest average gains from targeting indicated above which partly reflects the very high coverage rate of 78%.

- *The targeting efficiency of the program*: The better the targeting efficiency of the program the greater the gains from targeting. In general, for well-targeted programs, there are always gains from targeting. However, if programs use inefficient targeting mechanisms then the gains from targeting will be relatively small or even negative, for example, if the poorest households were incorrectly left out of the program.

The above factors imply that although the average gains from targeting are modest, these gains may differ substantially across localities both because the percentage of household included in the program differs across localities and because there is evidence that the degree of mistargeting also varies across localities. We therefore analyze the distribution of the gains from targeting for the current post-densification program, but also the potential distribution for the program if the current mistargeting were eliminated.

The distributions of the gains from targeting, relative to the mean gain, both for the current post-densification program and for this program corrected for mistargeting, are presented in Figures 10 and 11 for ϵ=2. Notice first that with the actual program the introduction of targeting *decreases* the distributional efficiency of the program in some localities, i.e. the gain in moving to targeting is negative (for 17 localities, 45 have gain=0). This is in part due to the fact that the actual targeting mechanism makes errors of inclusion and exclusion and dropping targeting may include some poor households previously excluded. As expected, these negatives do not exist for the perfectly targeted program. The maximum gains over both programs are 7.65 and 9.16 respectively (relative to a common mean), suggesting that the gains from targeting are relatively substantial in some localities and that these gains can be increased through improved targeting.

Understanding the pattern of gains across localities can further help to identify when gains are likely to be substantial. We therefore examine the relationship between the magnitude of targeting gains and three characteristics of localities: mean consumption, coverage rates and the marginality index. Looking at the relationship of gains to mean consumption, Figure 12 indicates that these gains are highest for the localities with the highest mean consumption. Also, the gains at the upper end of the income distribution are enhanced by improved targeting. The fact that the gains increase fairly monotonically (or continuously) with consumption makes it difficult to establish a consumption cut-off point as the basis of a decision rule for whether or not the gains from targeting are worthwhile (e.g. relative to any social costs arising from excluding some household localities). Similarly, the gains from targeting decrease fairly continuously with the percentage of households covered, again making it difficult to establish a decision rule based on coverage (Figures14 and 15).

Both consumption and coverage can only be potentially used as decision rules for targeting after one has incurred the costs of surveying households to collected consumption or income information. In this sense, a possibly more important variable is the locality marginality index, this variable being available prior to the collection of household data in participating localities. Figure 13 plots the relationship between absolute targeting gains and the marginality index. For the actual post-densification program, the gains first increase as one moves down the marginality index from the most marginal localities, but they suddenly begin to decrease again for localities in the top marginality quartile. This reflects the high degree of mistargeting at this part of the marginality distribution. However, if this mistargeting were to be reduced one would find a consistently negative relationship between targeting gains and marginality. This helps to highlight the fact that efficient targeting becomes especially important as the program expands to less marginal rural and semi-urban localities if one is to capture the potential gains from targeting poor households.

5.3.6 Conclusions

In this paper we have been concerned with evaluating the distributional power of PROGRESA, i.e. it's ability to get transfers to the most needy households in the program localities, relative to other potential transfer schemes. Our results suggest the following:

- In spite of substantial leakage during the densification phase of the program, the distributional power of the program is still very high relative to alternatives. This reflects its effectiveness at identifying poor households, but particularly its effectiveness at getting a relatively high proportion of total transfers to the poorest of the poor. The latter in turn operates through the demographic structure of education transfers.

- Restructuring education transfers towards higher grants for secondary schooling in order to try to enhance the educational impact of the program has little effect on the distributional power of the program. Any adverse effect it has can be reversed through simultaneously adjusting the cap on transfers which is relatively more binding for the poorest of the poor.

- As expected, there is a potentially high return, in terms of reduced current poverty, from differentiating transfers across localities (e.g. higher transfers in the most marginal localities). However, this could possibly be at the expense of the educational and health impacts of the program.

- Although the average gains from household-level targeting are modest, these vary inversely with locality marginality. But to reap the gains from targeting as the program expands to include less marginal rural and urban localities, it is important that the targeting errors that occurred during the densification process be avoided.

- The initial stages of the recertification process should focus on correcting the targeting errors that have occurred.

The next stage of the analysis is to incorporate the cost side of the program, i.e. the program and private costs associated with getting transfers to households, targeting these transfers and conditioning them on the accumulation of human capital. Such an analysis will also facilitate a more integrated approach to the evaluation of the program

5.4 Cost-Benefit Analysis

We now integrate both the cost and benefit sides of the analysis, using the results from the cost analysis in Section 4. As explained above, for the purposes of evaluating the poverty alleviation component of the program, the benefit of the program reflects the effectiveness with which it gets a relatively high proportion of the budget into the hands of the most needy households. The cost side reflects the budget cost of these transfers, but also the program costs associated with targeting the transfers, as well as the program and (incremental) private costs associated with conditioning the transfers. Above we essentially focused only on the budget costs. In this section we also take account of program and private costs. Integrating these costs into the analysis affects both the numerator and denominator of the distributional characteristic (i.e. the BCR of the program), so that the net effect on the program's BCR can, in principle, be either an increase or decrease.

With respect to the targeting the transfers, this is expected to improve the distributional power of the program but this comes at a cost since program funds must be diverted to activities that are necessary for targeting. With respect to the conditioning the transfers, this is expected to generate additional human capital benefits. However, the conditioning of the transfers can also affect the distributional power of the actual transfers since the receipt of transfers now involves households incurring private costs and this may affect the take-up of the program since those with high private costs (relative to transfer levels) may self-select out of the program. If take-up is relatively high among the moderately (severely) poor then this will decrease (increase) the distributional power of the program. Therefore, the conditioning of the transfers can affect the distributional power of the program either positively or negatively. In addition to private costs, conditioning also involves additional program costs that reduce the program's distributional power. Note that because incremental private costs depend on the program's impact on human capital (e.g. increasing attendance at school and health clinics), we cannot evaluate any alternative program that involves extending the conditioned program to the non-poor since, given

the evaluation design, we do not know how the program would affect human capital accumulation by these households and therefore cannot calculate incremental private costs.

5.4.1 Targeting

In the last section we focused on the benefits from targeting and found that the average gains were modest but that the gains were potentially more important as the program expanded into less marginal communities. We now incorporate the program costs into the analysis and analyze the relative distributional power of the unconditioned-targeted and unconditioned-untargeted programs. – remember that we cannot identify the private costs associated conditioning for the untargeted population. Our analysis of program costs indicated that the unconditioned-targeted program incurred 7.8 pesos in program costs for every 100 pesos transferred whereas an unconditioned-untargeted program would have incurred only 5.1 pesos. To incorporate such costs into the analysis we simply have to scale up the denominator, i.e. the program budget, by factors 1.078 and 1.051 respectively.

The distributional characteristic of the targeted program decreases from 5.05 when program costs are excluded to 4.64 when these costs are included. That for the untargeted program decreases from 4.56 to 4.34. Therefore, the average percentage gains from targeting decrease from nearly 11% to nearly 7%. But, as with our earlier results, this modest gain hides variation across localities. Figure 16 compares the distribution of gains for the actual program across localities according to their marginality index, this time including program costs. The top line presents the distribution of gains when program costs are ignored, the bottom the gains when program costs are accounted for. The latter indicates that the gains from targeting are near zero both for the most marginal localities (reflecting very high coverage rates) and the least marginal localities (reflecting high mistargeting). Figure 17 presents the corresponding pattern for a perfectly targeted program and helps to highlight that, even when targeting costs are included, the gains from targeting can be substantial on the margin but that this is dependent on cleaning up the mistargeting that occurred during the densification process. But the gains from targeting are still negligible among those localities in the bottom 15% of the distribution of the marginality index. In terms of the present program, these are sunk, so cannot be recovered and are therefore irrelevant to the issue of whether to continue targeting or not. But if the recertification process requires incurring similar targeting costs to determine which households remain eligible then, from a purely economic perspective, it appears that in these localities the returns to targeting are negligible. On the other hand, one could argue that because the net effect on the welfare impact of the program is negligible among these highly marginal localities and substantial in others, combined with the fact that the former tend to be relatively small in terms of number of households, it is not worthwhile deviating from the general principle of targeting. However, it is suggested elsewhere (Adato, 2000) that there may be important social costs associated with targeting and, if so, the nature and magnitude of these need to be considered. For example, if these "social conflicts" are more important in localities where only a relatively small proportion of households are excluded then it may be that the decision to target in the most marginal localities needs to be reconsidered. Whether to target or not in such localities is then more a socio-political decision rather than an economic one. But there is no escaping the fact that the gains from targeting are relatively substantial on the margin and will presumably increase as the program expands into even less marginal localities.

5.4.2 Conditioning

An important part of an analysis of the conditioning dimension of the program is the incorporation of information on take-up of the program. To identify take-up we make use of a data set that contains information on all the payments sent out to beneficiary households, which we merge with the household data set described above. We consider two different definitions of take-up:

- households that have received some payments for any bimester up to and including November-December 1999,
- households that received a payment for any one of the final two bimesters of 1999, i.e. for the months September-December.

Given that for the first few months of the program households received payments that were conditional only on enrollment at schools and registration at health clinics, regardless of attendance, the first definition can be interpreted as identifying households that decided to take-up the program at its inception. But some households will have dropped out of the program over time. Thus, the second definition treats the final two bimesters of 1999 as providing a snapshot of take-up some two years after the program was introduced in the treatment localities. Note that the first definition is subsumed within the second so that the latter is stricter and will therefore result in lower take-up rates. In our empirical analysis, for the most part we focus on the second definition.

The evaluation baseline dataset contains 24,407 households, which includes 14,994 households in "treatment" localities. We find that around 60% of treatment households had some money sent out to them since the start of the program, implying that these households were deemed to have met the conditions required for receiving transfers.[31] Focusing on the 14,994 treatment households we find that 78% (i.e. 11,761 households) of these were classified as poor post-densification, 53% being incorporated pre-densification and the remaining 25% being incorporated during the densification process. Out of the 11,761 beneficiary households, 77% had at least one positive payment sent out to them since the start of the program. These constitute 95% of the pre-densification poor but only 40% of the poor households incorporated during the densification process. So, 2681 poor households (i.e. 23% of poor households) never had a transfer sent out to them. If we add in the 170 households that did not have a transfer sent out to them for at least one of bimesters five or six in 1999, this increases to 2851 households. To this we add 43 households that did not receive a transfer in bimester six and for which the data indicating whether or not they picked up their payment for bimester five is missing. These adjustments bring to 2894 (i.e. nearly 25% of poor households) the number of households

[31] Conditional on registering in school and a health clinic, households can receive up to three bimester payments (i.e. 6 months) without further conditioning. Non-receipt of any transfers thus suggests that a household decided up-front not to participate. Below we will discuss how this number needs to be adjusted for mistakes during the incorporation process. The corresponding figure for the control group was 78%.

classified as not having transfers sent out (576, i.e. nearly 20%, that were incorporated pre-densification).

So, in our data it appears that whereas just over 7% of the poor households that were incorporated pre-densification did not receive transfers for the last two bimesters of 1999, the corresponding number increases dramatically to just over 60% for households incorporated during the densification process. However, this number includes a group of households that were identified as poor during the densification process but are from localities that were never in fact incorporated. For our purposes, we view these households as not being part of the program and eliminate them from the analysis completely. To identify these households we take all the households that were initially meant to have been introduced into the program during the densification process, that reside in localities where no such households ever received a transfer. This is consistent with the incorporation error being locality specific. In all, we identify 2163 such households (i.e. nearly 42% of the households that were meant to be incorporated during the densification process). When these households are dropped from the sample we are left with 9598 households in the treatment sample, of which 731 households never had a payment sent out since the start of the program *nor* did they receive a transfer in either bimester five or six. So nearly 8% of poor households that were incorporated can be deemed not to have taken up the program under our second measure of take-up.

As well as those households that by choice or default have not taken up the program, there are households for whom transfers were sent out but were never collected. This provides us with a third definition of take-up, i.e.:

- those households that both received and collected transfers for the months September-December 1999.

In our empirical analysis we focus on the second and third definitions of take-up. The percentages of households not taking up the program under both definitions are 7.6% and 12.4% respectively. The corresponding numbers for pre- and post-densified households are (7.3%, 12.1%) and (9.4%, 13.8%) respectively. So, even after adjusting for incorporation problems, take-up of the program appears to be substantially lower among those poor households incorporated during the densification process.

Table 9 describes the distributional power of the program with and without the operation errors at the incorporation stage, as well as the impact of take-up. The first thing to notice is that the distributional power of the program is substantially higher with the errors than without and that the difference between the two increases the greater out concern for the poorest of the poor. This is consistent with the fact that those not incorporated come from the densification stage, which incorporated the moderately poor. Leaving out some of the moderately poor results in a higher proportion of the actual transfer budget being concentrated in the hands of the poorest households. The final two columns present the distributional power of the program once we allow for some households not taking up the program. The appropriate comparison is with column two, i.e. assuming full take-up in the sample of households that were actually incorporated into the program (i.e. with operation errors). From such a comparison it is clear that the distributional power is slightly higher when take-up is allowed for indicating that a

higher proportion of the moderately poor (compared to the extremely poor) do not take-up the program. Non-collection also appears to be more concentrated among the moderately poor. This pattern is captured in Figure 18, which shows that the percentage increase in the distributional power of the program rises with our concern for the poorest of the poor. The impact of collection also exhibits the same pattern. The fact that the magnitudes of the differences are low (e.g. relative to the impact of linking the grants to the demographic structure of households) is due to the fact that take-up is very high, particularly among the poorest of the poor.

6. COST-EFFECTIVENESS ANALYSIS OF THE EDUCATION COMPONENT

In this section we focus on the economic evaluation of the education component of the program. As indicated earlier, the first step in any economic evaluation of a public-sector program (or policy) is to address the underlying motivation for the government intervention. The answer to this question has implications for the most appropriate form of intervention and for program design. In Section 2 we therefore discuss the potential motivations for government intervention to influence education outcomes and undertake a preliminary evaluation of the design of the education component. Conditional on program design one then wants to evaluate the economic impact of the program using cost-benefit analysis. The application of cost-benefit analysis requires one to identify the impacts of the program as well as the inputs required to bring about these impacts, attach monetary values to these impacts and inputs to get benefits and costs respectively, and finally to compare costs to benefits. In order to inform resource allocation decisions one should also compare the costs and benefits of the program to those for alternative uses of the program funds, in particular alternative ways of achieving the same program benefits. Where monetary values cannot be attached to impacts one can undertake cost-effectiveness analysis, which compares the costs of achieving the program impacts to those of alternative interventions that could achieve the same impacts. This report concentrates exclusively on cost-effectiveness analysis given the difficulties associated with attaching monetary values to education outcomes.

The analysis of program "effectiveness" requires the identification of the impact of the program on education outcomes. For this purpose, we take as our point of departure the Schultz (2000) report and extend this in two dimensions. Firstly, we build up the supply side of the analysis in an attempt to identify separately the impacts arising from conditional cash transfers (i.e. education subsidies) and those due to improvements on the supply side. Secondly, we differentiate between the program's impacts on "return rates" and "continuation rates". In Section 3 we discuss the supply data used in our analysis. In Section 4 we present the results from the extended impact analysis, identifying separately the impacts arising from the demand and supply side interventions. Section 5 then integrates these results into a cost-effectiveness analysis of the program. Section 6 summarizes and concludes.

6.1 Evaluation of Program Design

From an economic perspective, the main motivations for government intervention are on the grounds that such intervention will either increase the efficiency of resource allocation or that it

will reduce the inequality of resource distribution (Stern, 1989; Coady, 1999). The potential for efficiency improvements results from the existence of "market failures", e.g. the absence of perfectly competitive markets, the absence of perfect information, or the existence of public goods or externalities. Different market failures in general call for different forms of intervention. But even if markets are efficient, the resulting inequality in the distribution of market outcomes, e.g. of incomes, may not be desirable and may require public policies to influence this distribution. In this situation, government interventions should attempt to redistribute incomes without distorting incentives for the efficient allocation of resources: in the jargon of economics, any resource transfers between households should be as close to being "lump sum" as possible. Where the inequality of incomes results from the fact that market failures are worse for the poor, then there is a potential for public policy to bring about both a more efficient allocation of resources *and* a more equitable distribution of resources. Such a situation is often referred to as a win-win (or double dividend) situation. In any case, understanding the precise motivations for government intervention is crucial to identifying the most appropriate form of intervention, i.e. the policy instruments or programs that should be used and the detailed design of these policy instruments or programs.

In most countries one invariably observes the bulk of education supplied at zero charge by publicly regulated institutions. Yet education is not a pure public good since it is excludable (that is, it is possible to charge a positive price) and its marginal cost of supply is not zero (that is, it is desirable to charge from an efficiency perspective). Motivations for public-sector involvement must therefore come from elsewhere. Firstly, the perceived social externalities from greater aggregate productive efficiency and improved social cohesion provide a strong motivation for subsidizing education, particularly primary and secondary education. Secondly, imperfect capital markets also mean that subsidies result in efficiency gains since they can help to alleviate credit constraints. If low-income households are more credit constrained, then targeting subsidies is also desirable. The poor are, virtually by definition, more constrained in terms of access to credit (for example, due to low disposable income, savings, and lack of collateral) and their valuation of additional current income is higher. Thirdly, parents and individuals, particularly in poor households, often have poor information regarding the private benefits from investments in these sectors, and information itself also has public-good characteristics. Fourthly, sub-optimal allocation of public resources to one sector often leads to low private returns in the others due to important inter-sectoral linkages. For example, for poor households, investment in education may have a low private return if they regularly suffer from health and nutritional problems. Where the cost of coordinating such public investments are high this may lead to inefficient investment by the public sector (i.e. under-supply) and consequently by households. Fifthly, considerations of social justice provide another motivation for intervention along the lines that all children should have equal access to education regardless of parental preferences or income. Public supply without charge will therefore promote equality of opportunity and inter-generational mobility.

However, to the extent that the level of education achieved increases across income groups, uniform subsidies are regressive in that they disproportionately benefit the better off. Also, to the extent that low-income households rely more on income from children, paternalistic objectives achieved through compulsory education can again be regressive. Income distribution objectives therefore reinforce efficiency arguments for *targeted* subsidies. Thus the multiple

motivations for public intervention suggest that targeted conditional education grants are an attractive policy instrument since they encourage households to take more socially desirable education decisions and are also progressive.

The education component of the program involves both demand-side and supply-side dimensions. On the demand side, households receive educational grants for each child attending grades 4-6 of primary school or grades 7-9 of junior secondary school. To receive grants children must enroll and maintain sufficient attendance levels (i.e. no less than 85% attendance). There is also an additional transfer to cover the cost of school materials. These grants can be interpreted as a subsidy to education investments by households, i.e. households only receive these subsidies if they "consume" education. The crucial features of the grants structure are: (i) the grants increase by school grade, and (ii) the grants are higher for females in secondary school. The increasing grants structure (Table 1) is presumably motivated by the need to compensate households for earnings lost from sending children to school, these losses increasing with age. Higher grants for females reflect a perceived bias in educational outcomes in favor of males and are thus meant to compensate for gender biases both within and outside the household. The fact that grants apply only to primary and secondary education is also consistent with social externalities being relatively more important at lower levels of education. On the supply side, extra resources are to be 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. This reflects the recognition of the fact that the level of supply-side resources also influences enrolment and other educational outcomes.

Although the above motivations indicate a potential role for public-sector intervention in education one should recognize that, even in the absence of such interventions, households have a private incentive to educate their children since private returns are also high.[32] This is especially true at lower levels of education where the costs in terms of forgone earnings may be very low and possibly even negative if primary schooling provides cheap child-care services thus releasing mothers' time for other productive activities within and outside the home. Prior to intervening it is thus important to empirically motivate the need for intervention through analyzing private education decisions. Such an analysis can help one to determine whether grants are best targeted at the poor, at both primary and secondary education, at females, or across and within localities.

To address the issue of program design, we analyze information on education outcomes available in the baseline data collected by the program as part of the evaluation of the program, i.e. the ENCASEH 1997 census of 506 localities (186 "control" and 320 "treatment" localities). Figure 1 compares mean enrollment across different age groups for children classified as living in "poor" and "non-poor" households.[33] A clear pattern emerges with no differences in enrollment

[32] Parker (2000) estimates returns to education per year at the primary level to be about 5% and those above the primary level to be about 12%. Lopez-Acevedo et al. (1999) estimate rates of return to levels of schooling, estimating for instance about 18% wage gains for individuals completing primary school versus those with no education.

[33] This classification is based on the "pre-densification" variable *pobre_1*, which classified around 52% of households as poor.

over primary school ages (i.e. ages 5-11) but with "poor" households exhibiting lower enrollment rates from age 11 onwards.[34] This provides some justification for the targeting of secondary education grants to poor households but raises concerns about such targeting of primary education grants.[35]

Figure 2 presents mean enrollment rates by age and gender. A very similar pattern to above emerges. Firstly, enrollment at the primary level is already very high and one would not expect the program to have much impact on primary enrollment rates. However, enrollment rates fall off substantially in secondary school, again suggesting that the concentration of education grants on secondary schooling might result in larger enrollment impacts. Secondly, whereas no gender enrollment bias exists in primary school, over secondary school years girls exhibit substantially lower enrollment rates lending support to the decision to give higher grants to girls over these grades.

Nevertheless, Figure 2a presents years of completed schooling by age and gender. Presumably years of completed schooling is the more important output indicator of education (that is, we would not care about enrollment unless it were highly correlated with completing school years). Surprisingly, though this Figure shows no gender gap favoring boys in years of completed schooling. In fact, while quite small, the differences actually favor girls rather than boys up until the ages of 18 where differences favoring boys begin to appear. While this Figure might appear to be inconsistent with Figure 2, the two can be reconciled through three possible effects. The first may be that boys start school later than girls, so that at each age group, they have lower completed years of schooling. The second possibility is that boys have higher rates of grade repetition or absences from school so that their overall performance while in school is lower than girls. The third possibility is that of higher dropout of boys, which seems unlikely given the enrollment tendencies shown earlier.

Preliminary analysis shows support for the second hypothesis, that is that boys tend to have higher rates of grade repetition as well as frequent absences from school. There is no evidence that boys start school at higher ages than girls (on average, both boys and girls start school at the age of 6.15 years). Rather than late entry, it seems likely that these higher levels of schooling gap are reflective of a higher degree of grade repetition or intermittent attendance for boys than girls, which is consistent with boys having a higher opportunity cost through participation in market work.

What are the implications of this for the structure of the grants? With respect to the grant structure, the lower enrollment rate for girls suggests that grants should be higher for girls. Yet the higher probability of grade repetition of boys implies that the grants should be higher for boys. What constitutes an appropriate grant structure depends on the relative weight of these

[34] Although many children start late or lag behind, most children start primary school (i.e. grades 1-6) at age six and junior secondary school (i.e. grades 7-9) at age 12.

[35] It should be noted that the grants are also conditional on meeting attendance requirements, which may introduce some justification for targeting if the poor exhibit lower attendance levels. However, the results of Schultz (2000a) suggest that once enrolled most children have high attendance levels.

effects. Nevertheless, one possible policy option would be to link grants to completing grades, rather than enrollment and attendance. This brief discussion does however highlight the fact that great care needs to be taken in translating the results from our impact analysis into detailed policy prescriptions. For example, although the results discussed below indicate that the grants structure brings about a higher enrollment impact for girls than for boys, sufficient in fact to equalize enrollment rates, it is unclear whether or not such an impact could have been achieved with equal grants.

The observed enrollment patterns have additional implications for our evaluation of the design of the program. Firstly, given already high primary enrollment rates, it is unlikely that any significant increase in enrollment at this level can be achieved. In the absence of any enrollment impact, grants operate essentially as a pure (that is, unconditional) cash transfer and should be evaluated as such. For example, targeting transfers to areas with primary schools may not be the most efficient targeting mechanism available for alleviating current poverty. Secondly, in terms of education objectives, concentrating education grants on secondary enrollment may be a more desirable policy strategy.

Finally, it is important to view PROGRESA as just one component of a more comprehensive education and poverty alleviation strategy. Consistent with its objectives, it leaves out households that are deemed not to have "access" to schools. It also targets the program at the most marginal localities. Thus the program will undoubtedly exclude some very poor households living in communities without "access" to adequate public infrastructure (i.e. a school or health clinic) or living in less marginal localities from benefits. This feature is not desirable from the perspective of horizontal equity. Thus, it is important that other components of the overall strategy compensate for this exclusion. One can also view PROGRESA's main objective as being to get more children from poor households into the education system in order to generate a sustained decrease in poverty over time. Crucial to achieving the latter part of this objective are: (i) ensuring that once in school these children acquire a quality education, and (ii) ensuring that there is a high return to the acquisition of this human capital. Although in this report we analyze some supply-side characteristics, in particular extensive expansion in the form of more schools, we are unable to do adequate justice to the myriad of dimensions of education quality that are equally important. Neither do we dwell much on the obvious importance of creating a strong economic environment with growing investment and job opportunities. It is important not to lose sight of these crucial components of the overall strategy.

6.2 Supply Side

In this section we describe the supply-side data that we merge with the evaluation sample used in Schultz (2000). We begin first with data from the secondary schools, for which we have more detailed information. Table 2 presents the total number of junior secondary schools in the seven evaluation states for the years 1995-1999, identified separately by whether the school is in a treatment community, control community or in an "outside" community (i.e. schools outside of the control and treatment areas studied in the evaluation. Note that we do not distinguish here versus schools outside the evaluation communities which are nevertheless in other localities attended by PROGRESA versus schools in communities which are not receiving PROGRESA benefits). The increasing number of schools reflects an ongoing extensive expansion program

geared at improving access to schools. Through information provided by the Secretary of Public Education (SEP), for each of these schools we have information on the number of students enrolled in grades 7 through 9, the number of teachers, teachers' education level, the number of classrooms, the percentage of children who failed between 1 to 5 classes, the number of more than one-class classrooms, type of school, and source of funding.

In order to merge this school-level data set with the evaluation sample of households, we assume that each child attends the school which is closest to him/her (in terms of kilometers) e.g. that the available supply for this child is captured by the characteristics of the closest school. If a school is located within the community where the child lives, obviously this would be the school closest to him/her.. This enables us to link each child in a community to the supply-side characteristics of the nearest school.

Of the children in the evaluation sample that have a highest schooling grade achieved of between grades 6 and 8 (i.e. are eligible to enroll in grades 7-9), between 53-60% reside in households classified as poor by PROGRESA, thus being eligible for school transfers (Table 3). Of these, around 18% go to schools in treatment communities, 10% to schools in control communities, with the remaining 72% attending schools in "outside communities" (Table 4). Given the proximity of control and treatment communities and the fact that children often have to travel long distances to the nearest secondary school, it is likely that many children from both control and treatment communities attend the same schools. Therefore, extra resources to schools 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 a natural "control" group for supply-side interventions.

Just under 90% of PROGRESA children attend tele-secondary schools, the remainder attending technical secondary or other standard schools (Tables 5 and 6). This is also reflected in the source of funding, which is mostly from federal or state funds (Table 7). No television secondary is funded privately.

In 1997, the year before the program started in the evaluation communities, the average enrollment level in schools located in control and treatment communities was around 52 students (Table 8). This is substantially smaller than average enrollment levels in the outside communities, which average 84 students, presumably because many PROGRESA students attend schools in non-PROGRESA communities that are more likely to have larger technical secondary and standard schools (Table 9). We also observe a steady increase in enrollment in schools in treatment localities in 1998 and 1999, whereas enrolment does not increase in control communities until 1999. This is consistent with a growing enrollment motivated by benefits in the treatment communities and the fact that control communities are not incorporated into the program until 1999. But the number of teachers and classrooms also increases more steadily in treatment communities (Table 10). However, although schools in all three communities start off in 1997 with student-teacher ratios of around 22, we observe an increase in this ratio in 1998 in schools in treatment communities which falls back to initial levels by 1999. This suggests that the number of teachers increased with some lag but extra resources eventually were dedicated to compensate for the extra demand generated by the increased enrollment. The smaller increase in this ratio in control communities may reflect some anticipation of this program effect on demand

or the fact that this demand has not yet materialized in all control communities. But it is important to note that student-teacher ratios have increased over all three years in outside communities.

Although there seems to have been a tendency for teacher resources to increase in treatment localities, the student-classroom ratio (or class size) has been allowed to increase steadily. This is also the case in outside communities (Table 10). But the number of multiple classrooms (or more than one-class classrooms) has decreased in treatment areas, and also at least initially in outside communities, consistent with extra resources for building classrooms being targeted first at those schools with the worst initial conditions. However, it is also noticeable that the percentage of students failing 1-5 classes has also increased in treatment communities, although there has been a slight improvement in 1999 compared to 1998. This increase in fail rates is also beginning to appear in 1999 in control communities and has increased steadily in outside communities. The initial substantial increase in treatment communities just after the introduction of the program is potentially consistent with the program bringing back into the school the children of families motivated more by the subsidy than the perceived advantages of extra education. An alternative explanation is that children who are drawn back into school are children who, due to the fact they were previously out of school, have more educational difficulties in absorbing the basic educational material and are thus more likely to fail the school year. The improvement in 1999 is also consistent with some of these students deciding to withdraw from the program for similar reasons.

We now turn to our data on primary school quality. Unfortunately, the data we have access to is not as detailed as that of secondary schools, and allows us to only construct two variables related to supply and quality. The first refers to distance to the available primary school. Almost all localities have their own primary school, as shown in Tables 13 and 14. Of the 506 communities, only 45 did not have their own primary school. The number of communities without primary schools has decreased over time, such that by 1999, only 36 communities did not have their own school. This implies that 9 primary schools were constructed in the evaluation communities over time. The average distance to the closest primary school for children who did not have a primary school in their community also decreased over time, from 1.82 km in 1997 to 1.66 km. This suggests that even when over time a primary school was not built within a child's community, the distance to the primary school decreased because new schools which were constructed reduced the average distance for children living in communities without schools.

The other variable which we are able to construct is that of student teacher ratio (Table 15). This variable shows that the average student teacher ratio was approximately 25 students per teacher in control communities and 24 in treatment communities. This value has actually decreased slightly over time. Note that given the high rate of primary school attendance, it is not expected that the student teacher ratio would increase over time due to PROGRESA.

6.3 Identification of Program Impacts

The application of cost-effectiveness analysis to program evaluation requires one to identify the impact (or effectiveness) of the program and the costs incurred in generating these impacts. To complete the analysis, one then needs to identify alternative policy instruments that could be

used to generate the same impact, the costs associated with these instruments, and compare their cost-effectiveness with that of the program. In this section we focus exclusively on the impact side of the analysis; the next section deals with the cost side.

The first step in the analysis is to identify our measure of impact (or effectiveness). For this purpose one could focus on enrollment rates, attendance levels, completed years of education, and/or school performance. Given the data available, it has not proved possible to evaluate the impact on school performance (Behrman and Todd, 2000). Evaluation of school attendance have shown that there is little impact of PROGRESA on attendance rates, that is, once children are enrolled in school, they tend to attend regularly. Our focus, therefore, is on extra years of education. We can identify the program impact on extra years of education either directly or indirectly. To get a direct measure we can use survey information on each individual's "highest grade achieved". A more indirect route is to focus on enrollment and to translate program impacts on enrollment into extra years of education; this invariably involves making some assumptions about completion rates. Note that estimating the impacts of PROGRESA on highest grade achieved is somewhat problematic because children who re-enroll in school may be, as mentioned above, children who are more likely to have trouble completing the school year, that is, the composition of students who enroll in PROGRESA may change and using this sample to estimate the impacts of PROGRESA on years of completed schooling is likely to underestimate the impacts. The present version of the paper uses the latter approach and assumes once enrolled a child completes the year. We do, however, take into account that there are an important number of children who return to school initially and then drop out again. We assume these children do not complete the year, that is, we assume that the impacts of PROGRESA are effectively zero for these children. [36]

The two objectives of this section of the paper are:

(i) To identify program enrollment impacts in a way that enables us to identify the program impact on extra years of education;

(ii) To identify separately the impacts due to the education subsidy (i.e. conditional cash transfers) and to changes in the supply side (i.e. extensive expansion through the building of more schools and intensive expansion through improving the quality of education services in schools).

In this sense we are building on the earlier work of Schultz (2000). For this reason, we start this section by generating a baseline set of estimates of program impacts, which are comparable to those generated by this earlier work. We then explain how we expand on the supply-side of the analysis. This is followed by an analysis that separates the program impact between its impact on "continuation rates" (i.e. on those who were already enrolled prior to the program) and its impact on "return rates" (i.e. on those who were not previously enrolled).

[36] In future work we will also use the more direct approach, which focuses on the program's impact on the variable "highest grade achieved".

6.3.1 Baseline Estimates of Program Impact

In this section, we begin by providing estimates of the impact of PROGRESA on school enrollment using a model similar to Schultz (2000). As Schultz does, we construct double difference regression estimates of the impact of enrollment by using the ENCASEH97 survey as our baseline survey prior to program implementation and the subsequent ENCEL surveys to isolate the impact of the program. The form of our regression is to pool the ENCASEH97, the ENCEL98N, and the ENCEL99N surveys, so that we have three observations covering three different school years.[37] Note that this has the advantage that 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 post-program rounds, as might be the case if the program impacts decrease (or increase) over time. The regression equation that we estimate is thus the following:

$$S_{it} = \sum_{r=1}^{3} \alpha_{0t} + \alpha_1 T_i + \alpha^*_2 T_i R_2 + \alpha^*_3 T_i R_3 + \sum_{j=1}^{J} B_j X_{jit} + \varepsilon_{it}$$

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).

We estimate this equation for both primary and secondary school students eligible for the PROGRESA grants. The equation demonstrates that the impact of PROGRESA over the various rounds of the evaluation survey is estimated by including variables which interact 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 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 α terms capture the point that school enrollment may vary (for reasons unrelated to PROGRESA) over each round of the analysis.

Note that we focus in these regressions only on those eligible for the program and identify impact by the variable that measures whether a household resides in a control or a treatment community. Nevertheless, the definition of those who are eligible has some ambiguities which we now briefly describe.

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[37] Unlike Schultz (2000), we do not make use of the ENCEL surveys of March 1998 and May 1999, which we believe are more appropriate for studying attendance, rather than enrollment.

In our regressions, we focus only on the group of individuals (families) who are eligible for the program and we identify impact through including a dummy variable measuring whether a household resides in a control or a treatment community. Nevertheless, the definition of those who are eligible has some ambiguities which we now briefly describe.

The selection process of eligible households in the communities where PROGRESA operates consisted in the case of the early phases of the Program in two steps. Originally, a set of households were selected and incorporated into the program according to the discriminant analysis procedure (see Skoufias, Davis and de la Vega, 1999) for more description). In the evaluation sample, the percentage of households selected corresponds to approximately 52% of all households in the communities. Nevertheless, in order to correct perceived errors in leaving out households, in particular, elderly households, a second selection, called “densificacion” took place in which an additional 25% of households in treatment communities were identified as eligible to receive benefits. Incorporating these “densified” households required the return of personnel from PROGRESA and Sedesol to the communities to incorporate the newly selected household. However, it appears that some of these families experienced substantial delays in their incorporation. As of November, 1999, the date of our last survey used in this report, only 819 of the 3023 densified households had been incorporated, that is 2204 families had not yet begun to receive benefits from PROGRESA.

A final complication before discussing our measures of eligibility is that an additional 478 families chosen in the original selection mechanism as beneficiaries had also not received payments. This may be due to for instance, migration before the family was incorporated into the Program or to lack of takeup. This second explanation would have the obvious implication of making the PROGRESA participation variable a choice variable, or in other words, possibly endogenous to the impact indicators. Families who refuse benefits may be "different" from families which accept benefits in ways which are related to the impacts of the program, for instance they may be those most (or least) likely to be affected by the Program. Nevertheless, given their small overall number, they are unlikely to significantly alter the program impact estimations.

It is also worth noting that the control group was constructed through carrying out of the PROGRESA selection mechanism on the households located in the control communities. This was carried out twice, first using the discriminant selection method to select the initial eligible sample and a second time as well, to adjust the selection criteria to include the “densified” sample. Given that our interest here is to compare the impact of the Program using only households which are eligible for benefits, this point is important for insuring that the control group is comparable to the treatment group.

For the impact evaluation, this discussion brings out the issue of which sample of families is the most appropriate to use in the evaluation. Theoretically, the sample which includes both the initially chosen and the “densified” households corresponds to the actual selection mechanism used in PROGRESA to select eligible families and thus would seem the most attractive sample to use. Nevertheless, as described above, a substantial portion of these households did not receive benefits during our period of analysis (and were likely not even aware they had been chosen as eligible to receive PROGRESA benefits). Thus, using this sample would tend to

underestimate the impact of the program by including a significant number of households who supposedly are in the treatment group but have not received benefits due to operational difficulties which delayed their incorporation into PROGRESA.

This report takes the approach of constructing three different eligible samples, to first judge the extent to which these issues may or may not affect the estimated impact of the program. Our first group of eligibles corresponds to those initially identified as eligible to receive PROGRESA benefits. Our second group corresponds to the sum of those initially incorporated as well as those "chosen" to be incorporated through the densification process, whether or not they were incorporated or not. Our third sample of eligibles consists of all families identified to receive benefits, who were incorporated into the program and who had received at least one payment during our period of study. Note that this last definition of eligible may be endogenous to the extent that it includes the decision of households to participate. As mentioned earlier, take up rates in PROGRESA are quite high, nevertheless it is still an important issue, beyond the scope of this report, to examine how endogenizing program participation may affect impact estimates. Our report uses the strategy of comparing the different program impacts based on the different eligibility criteria to judge the possible bias in program impacts. One would expect impact estimates based on those initially classified as poor and those receiving at least one payment to provide higher impact estimates than the "densified" sample. Impact estimates based on those initially classified as poor and the densified sample are economically more exogenous, nevertheless if those based on those receiving at least one payment using are not particularly different, we may conclude that the bias based on endogeneity is not too severe.

The issue here in this paper is simply to measure the extent to which the estimates of program impact may or may not vary due to the incorporation problems described above. For our variables of interest (i.e. schooling of children), in practice it may not be important as the densification chose mainly elderly households, households which are unlikely to have children of school age residing in the household. These three sample, then, give us a range of estimates of program impact which we can be fairly confident reflect the true impacts of the program.

Within this same spirit, we also carry out analysis using the panel sample of children, e.g. the children who we are interviewed in each of our three datasets as well as the 'pooled" sample, which includes all children who were present in any of the three rounds of the analysis. While the panel sample may be conceptually preferable, as it is based on observing the behavior of the same individuals over time, it may suffer from some bias if, for instance, children who move out of the sample are likely to be children who are less effected by the sample, e.g. if migration is correlated with program impact.

As in Schultz, we include a number of other control variables, including a child's age, mother and father education levels, community agricultural wage, and distance to the nearest municipal center. Note that for now, we leave out variables relating to the supply and quality of schools, which we will include in the next section to see how they may alter the impact of the program.

Table 16a provides the first estimates of the impact of PROGRESA on secondary school enrollment. The results are reassuringly consistent for the different eligibility measures, varying only slightly. As expected, the estimates are smallest for those based on the measure of

pobreden, nevertheless, the differences from estimates based on using the other samples, correspond to only about 1 percentage point. The results are also similar whether one uses the panel sample or the pooled sample.

For boys, the results correspond to an increase in about 8 percentage points in the fall of 1998, and decrease to 6 percentage points in 1999. For girls, the impacts are somewhat higher; in both years corresponding to an increase of about 10 to 11 percentage points. That is, by 1999, the impacts of PROGRESA on secondary school enrollment for girls are almost double the level for boys. The possible reasons behind the decreasing impact for boys over time and the higher impact on girls will be explored in more detail below.

Turning to the effects of PROGRESA at the primary level (Table 16b), the results show that the impacts are much smaller, as expected given the high enrollment of children at the primary level. On average, the impacts for boys are approximately 1.8 percentage points both for boys and girls. From an average pre-program enrollment of about 90 percent, these impacts are fairly small, although significant in all cases. As with the estimated impacts at the secondary level, the impacts are similar depending on which eligibility indicator is used. The estimates do, however, tend to be a bit higher using the panel sample than the pooled sample. Nevertheless, the estimated impacts do not exceed 2.0 percentage points.

In summary, the results show fairly large impacts of PROGRESA on secondary school enrollment for boys and girls and smaller but still significant impacts on primary school. The impacts are also quite similar across different samples and using different eligibility criteria.

6.3.2 Adding Supply Measures to Baseline Impact Estimates

In this section, we consider how adding supply measure to our estimation equations may alter our estimated program impacts. It is important to recall that not only does the educational component of PROGRESA provide educational grants conditional on school attendance but it is also meant to be combined with improvements in the supply and quality of schooling, e.g. through higher teacher salaries, new school construction, and improved school supplies.

The regression framework used above, which estimates impact through the inclusion of a dummy variable measuring receipt or not of the program, cannot isolate the effect of these different components. That is, the estimated impact is a result of all of the different actions taken along with the introduction of PROGRESA. As is, therefore, we cannot argue that the impact represents the effect of the grants as opposed to the improvements in supply.

Nevertheless, once we add supply indicators of schooling, assuming that our data is 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. That is, if the effect of PROGRESA, as measured by the dummy variable, is reduced with the inclusion of the supply variables, this would imply that part of the enrollment impact attributed to the introduction of PROGRESA's cash transfers derives from improvements in the supply side in PROGRESA communities.

In this section, we add supply indicators to our regression framework so that our estimated equation becomes:

$$S_{it} = \sum_{r=1}^{3} \alpha_{0t} + \alpha_1 T_i + \alpha^*_2 T_i R_2 + \alpha^*_3 T_i R_3 + \sum_{j=1}^{J} B_j X_{jit} + \sum_{k}^{K} \mathrm{B}_k X_{kit} + \varepsilon_{it}$$

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, X_{jit} represents the vector of J control variables for individual i in time t and X_{kit} represents the vector of K variables measuring supply of schooling.

For secondary schools, the supply and quality measures that we include are the following.[38] First, we include distance to the closest secondary school and its square. This variable captures a number of aspects related to schooling. Distance, clearly, to some extent is a measure of cost and time needed to attend school. In this sense, it can be viewed as affecting the cost of attending school; a greater distance increases the private costs (such as transportation) of attending school. Nevertheless, distance is also a supply measure of schools in the sense that the only way that, for a given child, this distance can be reduced is through the construction of new schools. For some children, the distance may be so great as to make enrollment virtually impossible, at least without migrating to a community where the school is closer. [39]

The second school characteristic that we focus on is on the type of secondary school available. While there are five different types of secondary schools nationwide, in the rural communities that our analysis is based on, the dominant type of secondary school is the TV-secondary. Therefore, we analyze the impact on when the school available to a child is a TV-secondary versus any of the other types of secondary. That is, we analyze whether the type of secondary school available has differential effects on the probability of children enrolling in school.

The next variable we focus on is on the education level of the teacher. Our hypothesis is that teachers with higher levels of schooling (i.e. having greater level of human capital) may be better able to pass their knowledge on to their pupils. Our indicator of teacher's human capital is captured through the percentage of teachers with at least a high school education at the available secondary school.

Finally, we consider an indicator which measures the percentage of children who reported failing the previous year. We recognize that this indicator is somewhat ambiguous in that it is not clear whether one would expect it to be positively or negatively related with school enrollment. A

[38] One of our principal variables measuring school quality is that of student-teacher ratio. Nevertheless, in all specifications that we tried, this variable was generally not related to the probability of enrollment. In fact, in the only instances in which there was a significant relationship, the sign was opposite to that expected. While initially surprising, a review of the literature demonstrates that student-teacher ratio is generally not of great importance after the primary school level (REFS).

[39] In this preliminary report, we assume that children do not migrate to a community in order to attend a "better" school.

negative relationship would suggest that the fact that children are frequently failed in the class reflects to some extent a failure of the school and so might be viewed as a negative indicator of school quality. Nevertheless, a positive relationship might suggest that the school imposes stringent requirements on children in order to pass the year and thus could be viewed as being a more demanding or higher quality school.

At the primary level, our data set is more limited. In the future, we expect to have access to more detailed information at the primary level from the Secretary of Public Education. For the present, we use only 2 measures to measure quality and supply of primary schools. First, we include a dummy variable measuring whether a primary school is located within the community where the child lives. (Prior to program implementation, this is the case in about 91% of all communities). Second, we include a measure of the student/teacher ratio.

Table 17a and 17b reports the results of the impact estimates on secondary and primary school respectively including the measures of school quality and supply. We report here only the estimated impacts of PROGRESA and the supply measures.[40]

Beginning with the impact measures on secondary schooling, Table 17a shows that the impacts of PROGRESA remain similar to those estimated previously without the inclusion of supply measures. In fact, in some cases, the effect is slightly higher than previously although not significantly higher. What is the intuition behind these results that the impact of program participation is not reduced through the inclusion of variables measuring supply and quality of schools? Note that it does not necessarily imply that PROGRESA has not been accompanied by an improvement in supply in the communities where it operates. These results are consistent with a story in which supply improved in PROGRESA treatment communities but also in control communities. In fact, this is consistent with our earlier descriptive analysis, which showed some improvement in supply and quality of education both in PROGRESA communities as well as control communities. For instance, in both PROGRESA and control communities, average distance to the nearest secondary school has decreased by the order of 10% between 1997 and 1999.

It is perhaps not surprising that the supply and quality of educational services appears to have improved not just in treatment communities but in control communities as well. Given the nature of the experimental design, control communities are often in the same general geographical area as the treatment communities. Furthermore, at the secondary level, the majority of communities do not have a secondary school located in their community so students must travel elsewhere to attend secondary school. Given this, 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.

Finally, it is worth briefly commenting on the overall impacts of the supply/school quality variables we have included in our estimations of enrollment which are shown in Table 18a. In general, the results are consistent independent of the sample or measure of eligibility used. There are some differences, however, for boys and girls that we discuss below.

[40] The full regression estimates are available on request.

Distance to secondary school has a consistently negative effect on the probability of enrolling in secondary school for both boys and girls. The impact is, in general, much larger for girls than for boys. For girls, a reduction in distance to the nearest secondary school of 1km from the current mean of about 2 km would result in an increase in the probability of attending by approximately 10 percentage points, whereas for boys, the corresponding increase would be approximately 7 percentage points.

When the closest secondary school is a tele-secondary school, as opposed to a general or technical secondary school, this is also associated with a large reduction in the probability of attending school on the order of 10-14 percentage points. While this result requires further analysis, it may suggest that parents are less likely to send their children to school when it is a tele-secondary school, perhaps because they put less weight on the education children receive from this kind of schooling. On a cautionary note, this variable may be correlated with other omitted characteristics of the community, although we have controlled already in the regression for a number of community characteristics including level of margination, distance to municipal center, and average community wage level.
As expected, our measure of human capital of the teachers has a generally positive and significant effect on school enrollment.

We now turn to our estimates at the primary level with supply measures (Table 18b). As mentioned above, our information on supply measures is limited. The impact results on PROGRESA are quite similar to those presented for secondary school. Including the supply measures for primary school in some cases slightly reduces the estimated impact of PROGRESA on school enrollment.

With respect to the impacts of the supply measures at the primary level, it is interesting to note that having a primary school in the community has no significant impact on whether a child enrolls in school or not. Nevertheless, student teacher ratio is negative and significant in all cases, indicating that a greater number of students to teachers results in a lower school enrollment. The size of the impact is approximately the same for both boys and girls.

6.3.3 Decomposing the Impact of PROGRESA on Educational Enrollment

Part of the purpose of a cost-effectiveness or cost-benefit analysis is to provide useful information on how to maximize impact, keeping other factors constant. If the only purpose of PROGRESA were to maximize its impact on education, it would be useful to know which characteristics, both of individuals and communities may be associated to a greater impact of the program. In this section, we focus in more depth on whether there are specific characteristics of individuals that are correlated with PROGRESA having a bigger impact.[41] More specifically, we

[41] Note that we also analyzed whether the impact of PROGRESA varied according to community characteristics including the level of the margination index and distance to the nearest secondary school and parental characteristics, including mother's education level. Surprisingly, none of these interaction terms proved significant and we do not report these results here.

focus on whether the program impacts differently children who had previously dropped out of school versus those who were attending school prior to program implementation. Given the very high enrollment rates at the primary level prior to program implementation, this analysis we carry out using only the secondary school sample.

The structure of the grants that PROGRESA gives implies that eligibility, besides depending on age, also depends on the current level of attained schooling or the grade. Nevertheless, this implies that the sample of children who are potentially eligible to receive the grants is quite heterogenous. For instance, of all children who currently have 6 years of completed education and are thus eligible to enter into the first year of secondary school (that is, they have completed primary school) the actual age varies from 11 to 17 years. This group of eligible children cannot possibly be expected to have the same probability of enrolling in seventh grade. For instance, for those children aged 16 or over, it is likely that they completed primary school as much as 3 years before and then dropped out, whereas children aged 13 may have been attending school continuously overtime.

This discussion then suggests the division of the impacts of PROGRESA on school enrollment into two avenues. PROGRESA may increase school enrollment through increasing the *continuation rates* of children already enrolled in school or it may increase the impact of school enrollment through the *return* of children who had previously dropped out of school. One suspects that the program may have a much stronger and lasting effect on continuation rates as opposed to return rates. Those returning to school may have to give up previous earnings and may also find it more difficult to keep up with the curriculum. Both of these may be increasing in the length of time since last enrolled.

To the extent that PROGRESA continues to operate over the longer term, and to the extent it is successful in affecting the enrollment and timely completion of children in school, the differences between continuation rates and return may become less important. That is, in part the distinction between continuation and return is relevant here because, for a number of children, the program came at a time when they have already been out of the school system for one or more years. To the extent that the program becomes permanent, and each new generation of children entering school is affected by its grants, one would expect that PROGRESA would both increase enrollment, reduce dropout and reduce grade repetition. In that sense, the program's impact on return rates may be more of a short-run phenomenum.

In the following regressions we condition on whether children were previously enrolled or previously out of school at the time of program implementation. That is, we separate our analysis into two samples, one composed of children who were enrolled in school prior to program implementation and those who were not enrolled in school prior to implementation of the program in the 1997 ENCASEH. The impact on enrollment of children already enrolled in school, we will term continuation rates, whereas the impact of enrollment on children not in school, we will term return rates. Our regressions take the same structure as those in the previous section. That is, we include our supply and quality measures of schooling, although we do not report their impacts here.

Table 19 presents the results distinguishing between children previously enrolled in school versus those not enrolled. The results show that the impacts for children who were previously enrolled in school are much larger and, perhaps more importantly, more likely to be sustained over time. The impacts for children who were previously out of school are smaller and, for both boys and girls, become insignificant by 1999. This suggests that even for those children who may have returned to school as a result of PROGRESA, the results do not seem to be sustained. An obvious interpretation is that many of those who return were motivated solely by the subsidy as opposed to the benefits from extra schooling and thus were unable to keep up with the curriculum and eventually dropped out. Thus, one should not count their extra year of enrollment as an extra years of schooling.

6.4 Cost-Effectiveness Analysis

In order to carry out a complete social cost-benefit analysis of an education program one needs to identify both the expected benefits of the program as well as the costs associated with the program. We start this section by discussing the myriad of benefits conventionally thought to arise from education and argue that, since attaching monetary values to most of these benefits is an extremely hazardous task, one is often limited to the application of cost-effectiveness analysis in the place of cost-benefit analysis. We then describe how we calculate the effectiveness of PROGRESA and how we can compare it to the effectiveness of supply side changes in the form of an extensive expansion of education through building extra schools and thus decreasing the average distance to the nearest school. This is followed by a description of the costs incurred in generating these impacts. Finally, we bring both the effectiveness and cost sides of the analysis together by comparing the cost-effectiveness of the two alternatives considered.

6.4.1 Benefits From Education

There are a wide range of benefits thought to accrue from investments in education (Schultz, 1988), including:

(i) Increased productivity in marketed production activities (for example, agricultural production of traded crops);

(ii) Improved job opportunities and lower incidence and duration of employment spells (for example, access to jobs outside agriculture or in urban areas, and better matching);

(iii) Increased productivity in non-market production activities (for example, agricultural production of subsistence crops);

(iv) Increased productivity of household activities (for example, improved health and nutrition for given income and food consumption, or more control over fertility outcomes which also results in social gains when fertility rates are socially inefficient);

(v) Increased marginal utility of leisure (for example, ability to read and write and thus participate in social activities);

(vi) Increased productivity of others (for example, due to need for lower monitoring and instruction costs);

(vii) Faster diffusion of information on, and adoption of, new technologies and practices;

(viii) More rapid and efficient responses to shocks (for example, price shocks);

(ix) Increases in efficiency of public goods (for example, of economic and political institutions, including more efficient tax system);

(x) Improved income distribution and greater participation of poor in the development process and efficiency benefits from removal of any distortions created by poverty or inequality reducing policy instruments;

(xi) Beneficial socializing influences by enhancing adherence to social norms (for example, crime reduction or social cohesion) possibly also resulting in higher productivity and marginal utility leisure. Better education may also help individuals break away from socially undesirable "outdated" norms.

The existence of such a diverse and complex range of benefits has implications for our ability to apply cost-benefit techniques to the economic evaluation of education programs. Not only is it difficult to attach values to many of these benefits but the fact that households have choices over both how much investment to make as well as the channel through which they receive these benefits means that it is often very difficult to identify empirically the impacts (or benefits) of such investments. For example, in many cases the benefits of education may not accrue as money income or it is difficult to attach a monetary benefit to the impact. However, to the extent that households are unconstrained in their choices and participate in at least some marketed activities, one can use the fact that (unconstrained) optimization results in the equal valuation of alternative benefits (on the margin) to attribute an approximate monetary value to non-marketed activities. This is useful when the alternative forms of education benefits are mutually exclusive, but less so when these benefits have public good characteristics. To identify these impacts one therefore needs a wide range of information on household endowments and prices, but also on, for example, impacts on migration patterns and incomes accruing from off-farm and urban employment. Such employment may also have additional insurance benefits if its income is less risky and negatively correlated with farm incomes. To the extent that benefits accrue outside the household (that is, as social externalities), household information does not help us to identify or value such benefits. Also, the social benefits from higher marketed production also differ from private household benefits when market prices are distorted by taxes or price controls.

In terms of PROGRESA, identification and valuation of impacts is also restricted by the fact that the ultimate benefits (that is, years of quality-adjusted schooling) accrue with a lag so that one

will only observe intermediary impacts on inputs in our data. This means that our analysis will have to focus on such indicators as enrollment, attendance and dropout rates as well as test scores. It is difficult to predict the eventual impact of these indicators on years of quality-adjusted years of education and the resulting productivity and income effects. One therefore has to fall back on cost-effectiveness analysis of these indicators.

6.4.2 Effectiveness

In this section we calculate the effectiveness of the program in terms of extra years of schooling generated. We do this separately for the primary and secondary components of the program as well as for boys and girls. We also calculate the effectiveness of extensive expansion on the supply side, which builds more schools thus decreasing the distance to the nearest school and increasing enrollment. We adopt the indirect method of 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, excluding individuals who return to school but then drop out of school within one year.

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 extra years of schooling they would receive after the program (i.e. given the higher enrollment rates) compared to before the program. To be 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 junior secondary school. Focusing on this variable we use two methods for identifying the enrollment impact of the program:

- A *difference* estimator: which, for each grade, calculates the impact on enrollment as the difference between the mean conditional enrollment rates in the control and treatment samples in the 1998 ENCEL data. These are presented in Figures 3 and 4 for boys and girls separately.

- A *difference-in-difference* estimator: which is based on the regression estimates derived above. The regression estimate for secondary school gives an estimate of the impact of the program on the average conditional enrollment rate 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 receive grants. The average enrollment impact for junior secondary school can be calculated as:

$$E_S = \frac{R_7 + R_7 R_8 + R_7 R_8 R_9}{1 + R_7 + R_7 R_8}$$

  where number subscripts refer to grades. For primary school this is defined over grades 3-6, i.e. the grades for which grants are available. In primary school we assume that the enrollment impact is spread equally over all grades, i.e. each grade experiences an equi-proportionate increase in conditional enrollment. In secondary school we assume that the

enrollment impact is concentrated in the transition year from primary school (i.e. impacts only on grade 7). Both these assumptions are consistent with the pattern in the difference estimates shown in Figures 3 and 4. 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 higher impact estimates but also because the grants are differentiated across grades. With grants increasing with grades both these factors offset each other in the calculation of cost-effectiveness ratios.

Education Grants

Table 20 presents the results separately for boys and girls and for primary and secondary education. The first column gives enrollment rates before the program taken from the baseline data, i.e. ENCASEH 1997. The second column presents the program impact on enrollment rates using the difference between enrollments rates in the 1998 ENCEL and the baseline data. These differences are also presented in Figures 3 and 4 where one can see that the biggest enrollment impact is at the transition year from primary to secondary 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 the cohort of 1000 children starting in the first grades of primary and secondary school respectively.

We focus first on primary school. The conditional enrollment rates across grades imply an average conditional enrollment rate of nearly 88% and 87% for girls and boys respectively over all four grades. The difference estimates of program impact across grades is higher than those based on the difference-in-difference regression estimate of 0.0096 for girls and 0.0074 for boys (Schultz, 2000) – these different impacts are sufficient to eliminate the initial gender gap. The difference estimates suggest that a cohort of 1000 female children going through grades 3 to 6 gain 251 extra years of education as a result of the education grants, while their 1000 male counterparts gain only 155 extra years. This indicates a substantial relative increase for girls. The difference-in-difference estimates suggest extra years of 76 and 57 respectively indicating a similar gender bias.

Focusing on secondary school the conditional enrollment rates across grades show a clear pattern for both boys and girls: only 26%-34% of those finishing primary school enroll in junior secondary school but thereafter a very high percentage (i.e. 86%-90%) continue into the other two years. These rates imply an average enrollment rate over all three grades of 47% for girls and 56% for boys indicating a clear enrollment bias against girls. Using the difference impact estimates we find that similar 1000 person cohorts gain 285 (girls) and 293 (boys) extra years, a very slight bias towards males. The regression difference-in-difference estimates of 0.056 and 0.119 for boys and girls respectively translate into increases in conditional enrollment rates of 0.093 and 0.179 respectively when concentrated in grade 7, the transition year from primary school. Not only is the gender bias switched in favor of girls but the magnitude of the differential impacts is much larger. These estimates imply 249 and 479 extra years of schooling for boys and girls respectively, a clear bias in favor of girls sufficient to nearly equalize average conditional enrollment rates over the three grades, which are now 59% for girls and 61% for boys.

Extensive Supply Expansion

Simultaneous to the program 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. Therefore, extensive expansion brings about an increase in enrollments in both samples so we combine both for the purpose of our analysis.

Analysis of the distance variable indicates that the average distance has decreased from 2.02km in 1997 to 1.95km in 1998 and 1.94km in 1999. To estimate the impact of these decreases on enrollment rates we use the coefficients from the regressions presented earlier and calculate the change in the probability of enrollment (dE) as:

$$dE = -0.075 + (2*0.004)\ D \qquad \text{(for boys)}$$
$$dE = -0.112 + (2*0.006)\ D \qquad \text{(for girls)}$$

where D is the distance (in kms) to the nearest school in 1997. This is calculated for each individual in the sample and averaged to get the expected impact on enrollment. The average decrease in distance was 0.063km in 1998 and 0.049km in 1999, or 0.112km over the two years. This was larger for girls than for boys, for example, over the two years being 0.124 for girls and 0.109 for boys.

Also, as one would expect, the decreases were larger for those initially further from the nearest school.

Using the above formula we calculate the average increase in enrollment resulting from observed decreases in distance and apply these to the baseline enrollment rates over junior secondary school to calculate the resulting impact on enrollment. When the enrollment impacts are assumed to be equal over all grades (Table 21a), a cohort of 1000 girls entering grade 7 will receive 26 extra years of education in junior secondary school as a result of the combined decrease in distance from 1997-99. Reflecting the timing of distance decreases, the majority of this occurs in 1998 (i.e. 16 extra years). The corresponding numbers for boys are 21 extra years, with 12 of these occurring in 1998. When the distance effect is concentrated in the transition year the extra years gained by girls increases to 30 years and to boys to 26 years. In our cost-effectiveness discussion below we use the latter (i.e. higher) estimates of effectiveness.

6.4.3 Cost Effectiveness

We now address the issue of the cost of generating the above impacts. We calculate the cost per extra year of schooling generated by schooling subsidies and extensive supply expansion. For both we calculate this separately for boys and girls. For education subsidies we also look at primary and secondary grants separately.

Table 22a and Table 22b present the calculation of the cost of an extra year of schooling in the case of education subsidies. The cost is calculated using the cost of providing the education grants and the school supplies in current pesos. 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 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. As above, this is done using both the difference and difference-in-difference estimates.

Focusing first on primary school, the cost per extra year is substantially higher for boys than for girls, at $24,053 for boys and $15,714 for girls, reflecting the higher enrollment impact for girls. Although the corresponding numbers are much higher using the DIF estimates, i.e. $62,370 and $48,596 respectively, the gender pattern is the same. The higher DIF estimates reflect the relatively lower impact estimates discussed earlier.[42]

In secondary school a similar, although less pronounced, pattern is observed with a cost per extra year of $11,238 for boys and $10,779 for girls using the difference estimates, or $12,733 and $7335 respectively using the difference-in-difference estimates. Therefore, in secondary school the higher enrollment effect for girls offsets their higher subsidy rates.

Note also that when comparing the cost per extra year across primary and secondary schooling, the effect of higher subsidies in the latter is offset by a higher enrollment impact so that the average cost per extra year is higher in primary than in secondary school: averages of $19,884 versus $10,779 respectively using difference estimates. The difference-in-difference estimates give a much stronger bias towards secondary education with the cost per extra year of primary education being $55,483 compared to $10,034 for secondary.

We can compare the cost of generating an extra year of schooling using subsidies with that using an extensive supply expansion strategy. Using the merged school supply and household data set, we calculate that in both 1998 and 1999 six new schools were available to the students compared to the previous year (Table23). [43]The number of different types of schools in the sample is the number of separate schools attended by the sample children. All schools are those nearest to the child's locality. When a school drops from the sample we assume that this is due to the building of a new school nearer to the locality; but the old school still exists. A school added to the sample is considered to be a newly built school. In 1998, four of these were telesecondaries and

[42] Note that the fact that the difference-in-difference estimates are lower than the difference estimates suggests that the control sample had higher enrollment rates prior to the program.

[43] This calculation is based on observations of the number of schools which 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, of course, increase the estimated costs here so that our estimate of costs for reducing distance to school should be considered a minimum estimate.

two were technical secondaries. In 1999, all six were technical secondaries. The cost of building and operating such schools are presented in Table 24. 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 through extensive expansion of the school system is presented in Table 25a,b using both difference and difference-in-difference estimates, for boys and girls separately, and with and without discounting. A number of points emerge from the table:

- the difference-in-difference estimates suggest substantially lower costs of an extra year than do the difference estimates reflecting the higher impacts with the former

- the cost decreases the longer one assumes the extensive supply effect to last, reflecting the fact that up-front infrastructure costs are spread over a longer period

- the cost decreases with the discount rate, reflecting the fact that a greater proportion of the enrollment is distributed further in time relative to costs

- the cost is lower for girls than for boys, reflecting the larger effect of lower distances on girls enrollment relative to boys

- the cost decreases 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 (i.e. four out of six) whereas all six new schools in1999 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 very 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 using difference-in-difference estimates at just below $94,000 per extra year of schooling. The largest CER in the case of education subsidies was just over $12,700 for boys again using difference-in-difference estimates. Therefore, the conclusion that PROGRESA is a cost-effective way of getting more children into secondary school would seem to be robust.

6.4.4 Cost-Benefit Analysis

In order to move from a cost-effectiveness analysis to a cost-benefit analysis one would need to attempt to value the monetary return to an extra year of schooling. This could differ across boys and girls but also across primary and secondary. Returns tend to be much higher for secondary education than for primary: for Mexico, Parker (1999) estimates a 5% return to an additional year of primary education and a 12% return to an additional year of secondary education. To move from our CERs to corresponding cost-benefit ratios (CBRs) one just divides the former by

the net-present value (NPV) of the extra stream of income that accrues to the recipient of the extra year as a result of that year:

$$CBR_i = CER_i / NPV_i$$

where subscript *i* refers to the policy instrument being used to generate the extra year, in our case, subsidies to primary or secondary education or extensive expansion of the supply side of secondary education. However, in this report we do not attempt such an exercise mainly because it does not add much to the present evaluation. For example, it is quite clear that the return to an extra year of primary education is substantially lower than that for an extra year of secondary education. Although in deriving a corresponding NPV one must adjust for the additional income forgone in remaining in secondary education and outside of the labor force, it is undoubtedly the case that this pattern of returns just serves to reinforce our ranking of primary and secondary education subsidies. That is, given the higher return to secondary schooling, using CBRs in the place of CERs will imply an even stronger preference for subsidies to secondary schooling. It should also be clear that no matter what the return to secondary schooling, the ratio of CBRs for secondary schooling and extensive expansion of secondary education will always be the same as the ratio of their CERs. Therefore, information on returns to extra years of schooling will not necessarily inform the debate on the best way of generating higher enrollment rates.

Of course, the view that a move in the direction of cost-benefit analysis (i.e. incorporating information on the private returns to education) will not inform the present debate in part reflects a prior belief that such investments are desirable. In other words, we are assuming that we are starting from a position where the budget allocated to education is sub-optimal (i.e. too low). In this situation, all we need to know is the cheapest way of generating a given increase in enrollment and CERs contain all this information. The desire for greater enrollments may, for example, be motivated by the belief that education investments have substantial social returns. Also, to the extent that these social returns are thought to be more substantial over primary education, the above ranking may be affected. Also, private returns reflect market valuations and thus any biases that exist in the market. Therefore, allocating resources on this basis would only serve to reinforce such biases (e.g. the gender bias) and perpetuate initial economic disadvantage. In addition, both primary education and female education are obviously relatively desirable from the perspective of equality of opportunity as well as from the perspective of specific egalitarianism which promotes equality of education regardless of ability to pay or parental preferences.

However, this is not to say that knowledge of the private returns to education do not constitute useful information for the design of the overall poverty alleviation strategy. For example, if one observes that returns to education are low this could be interpreted to be a consequence either of: (i) an underlying poor quality of education where quality is given a broad interpretation to include the need to develop human capital that has a market value reflecting a demand by employers, or (ii) an underlying failure of macro-economic and job-creating policies resulting in low growth and an inability to absorb extra school graduates without pushing down education returns. Although a desire to improve educational outcomes has motivations other than private market returns, these market returns are still very important. But information on market returns

is more relevant to the other dimensions of the overall poverty alleviation strategy than it is for the motivation and evaluation of PROGRESA.

6.5 Summary and Conclusions

For the purpose of evaluation it is useful to view the program as a system of cash transfers targeted at poor households to alleviate current poverty and conditioned on investment in children's human capital in order to generate a sustained decrease in poverty over time. In evaluating the program it is important to understand the initial motivations for public intervention to influence the educational outcomes of the targeted population. To address this issue we first compare the education outcomes (i.e. enrollment rates) of children in poor and non-poor households and find some support for the need to target education subsidies at poor households. But this applies only to subsidies for secondary education since children from poor households only have an educational disadvantage over these grades. The subsidies are also higher for females in secondary school in order to eliminate a gender bias in enrollment outcomes, although we have shown that overall attainment of girls, as measured by years of completed schooling, was not lower than boys prior to program implementation. Therefore, in terms of maximizing the impact on educational outcomes the above provides some support for a greater emphasis on grants for secondary education. The issue of retaining a higher grant for females in secondary school requires further analysis and justification. It may be justified, for instance, on the point that social returns of women's education are higher than that of men's (See Schultz, 1993). Because enrollment rates in primary school were already very high before the program one does not expect a substantial impact on these rates and, in the absence of much of an impact, primary school grants act more as a pure cash transfer rather than a subsidy.

It is also important to recognize that the program is just one component of a more comprehensive poverty alleviation strategy. For example, the program undoubtedly leaves out some extremely poor households either because they are not deemed to have adequate access to the required supporting public infrastructure in the form of primary and secondary schools (or health clinics) or because they live in less marginal localities not included in the program. It is important that other components of the strategy address the poverty of such households. Also, the main objective of the program is to get children in to school. In order to generate a sustained decrease in poverty, two other components must be added. Firstly, the amount of human capital acquired will depend on the quality of schooling children receive while in school. Although we address some elements of the extensive expansion of the program in the form of extra schools and student-teacher ratio, we have much less to say about many issues of intensive expansion that are crucial to ensuring children receive quality education. It is therefore important not to lose sight of this dimension of the education program and, more particularly, PROGRESA should not be view as a substitute for action on these fronts. Secondly, the ability of children to translate higher human capital into higher future incomes will depend crucially on good macroeconomic management, specifically the creation of sufficient new job opportunities to absorb the greater supply of human capital without depressing returns to this capital. Again, it is important not to lose sight of the importance of this dimension of the poverty alleviation strategy.

The main objective of this report is to undertake a cost-benefit analysis of the education component of PROGRESA. This requires one to attach monetary values to the education

impacts of the program (i.e. to extra years of education generated by the program) and compare these to the costs of generating these impacts. Given the difficulty in attaching a monetary evaluation to the myriad benefits thought to arise from educational investments, for the most part in this report we focus on cost-effectiveness analysis in the form of identifying the cost of generating an extra year of education and comparing this across alternative policy instruments, namely, education subsidies for primary education, education subsidies for secondary education, and extensive expansion of the school system.

An important step in cost-effectiveness analysis is the identification of the impact of the program. The effectiveness indicator in our analysis is extra completed years of schooling generated by the program. One could measure this directly by focusing on the total years of completed years of schooling for each child. Alternatively, one can measure it indirectly by focusing on enrollment levels and making assumptions about completion rates. In this report, partly to ensure consistency with previous reports, we employ the indirect approach and assume that an extra year of enrollment leads to an extra year of completed schooling, with the exception of children who return to school and drop out soon after for whom we assume the impact is zero. We measure impacts using regression analysis to get so-called difference-in-difference estimates of the impact and also derive so-called difference estimates by comparing means across each grade level. In this respect, we build on the work of Schultz (2000). In order to facilitate comparisons between the effectiveness of education subsidies and extensive expansion of the supply side, we add supply-side variables to these regressions. This helps to separate the demand and supply side impacts on enrollment. We then estimate the costs incurred in generating these impacts and compare cost-effectiveness ratios (i.e. the cost of generating an additional year of schooling) across the alternative instruments. Finally, we calculate benefit-cost ratios (under some admittedly crude assumptions) in order to capture the market returns to such investments.

We start by analyzing the supply side, i.e. the characteristics of schools attended by PROGRESA children. We have data on all the junior secondary schools in the seven evaluation states for the years 1997-1999. The increasing number of schools from 1997 to 1999 reflects an ongoing expansion program geared to improving access to schools. Over 70% of the children eligible for secondary grants attend schools outside of their locality and not in a program locality. Given the proximity of "control" and "treatment" localities and the fact that children often have to travel long distances to school, it is likely that many of the control and treatment samples attend the same schools. It is not surprising then that supply side characteristics are very similar across both sets of communities. Consistent with their incorporation into the program, we find that student-teacher ratios increase between 1997 and 1998 in treatment localities but fall back to the initial lower levels in 1999, consistent with increased enrollment due to the program and the supply side interventions occurring with a lag. However, there is evidence that the student-classroom ratio has increased steadily in treatment localities in spite of a decrease in the number of more than one-class classrooms.

To estimate the impact of the education subsidies and supply-side interventions on enrollment we generate difference-in-difference estimates using regression analysis. Our analysis differs from that of Schultz (2000) in three dimensions: (i) we use only the three November household surveys from 1997-1999 instead of the five rounds used in Schultz, (ii) we differentiate between

the program impact on continuation rates and return rates, and (iii) we expand on the supply side of the analysis. For all regressions we use the sample of children that are eligible for school grants. For example, in secondary school this constitutes all children under 18 years of age that have completed grades 6 to 8 and live in households classified as poor. We test the robustness of our results to different sample compositions in two dimensions:

i. Choosing three different samples based on households initially classified as *poor* (i.e. 52% of households in the treatment sample), being classified as poor after the so-called *densification* phase that added a further 25% of treatment households, and excluding some of the latter that were *not incorporated* due to operational errors;

ii. Choosing two different samples based on whether an individual appeared in any of the three years (the *pooled* sample) or only on individuals living in households that appear in all three years (the smaller *panel* sample).

Our finding is that the impact estimates are not sensitive to the underlying sample. In our cost-effectiveness analysis we therefore focus on our preferred panel-sample estimates.

Our estimates of the program impact on enrollment are similar to those reported in Schultz (2000). The program increases enrollment by 8 percentage points in 1998, falling to 6 percentage points in 1999. Splitting the sample along gender lines, we find that the impact is substantially larger for girls. The impact on boys' enrollment decreases from 8 percentage points in 1998 to 5.6 percentage points in 1999, while the impact on girls' enrollment stays constant at around 11.8 percentage points.

In order to identify separately the enrollment impact attributable to supply-side changes simultaneously to the introduction of the program, we add a number of supply-side characteristics to the regression specification, including a variable representing the distance to the nearest junior secondary school. A decrease in the average distance captures extensive expansion on the supply-side (i.e. more schools). The fact that the program impact is not changed by the introduction of supply-side variables reflects the fact that these are similar for children living in both control and treatment localities. For example, the average distance to the nearest secondary school decreases by around 10% between 1997 and 1999 in both groups. As expected, distance is an important factor in explaining variation in enrollment decisions, especially for girls.

In attempt to explain the decreasing program impact on enrollment over time we try to identify separately the program impact on *continuation rates* and *return rates*. For this purpose we separate the sample into two groups, namely, those children that were enrolled prior to the introduction of the program and those who were not. The program impact on the former is interpreted as its impact on continuation rates and on the latter is interpreted as its impact on return rates. We find that the impact of the program on continuation rates is much larger and is sustained over time, at around 7-8.5 percentage points for boys and 11 percentage points for girls. However, return rates exhibit a completely different pattern. In 1998 the impact is 5.4 percentage points (but statistically insignificant from zero) for boys and 13.6 percentage points for girls (and highly statistically significant). But both these fall substantially in 1999, to 0.4

percentage points for boys (and highly statistically insignificant) and 5.7 percentage points (an just statistically insignificant at the 10% level) for girls. Our interpretation of this is that many of those that return to school after periods of absence do so because they are primarily motivated by the subsidy and not by any perceived private returns to the extra schooling received. These children may find it difficult to keep up with the curriculum and eventually drop out of school. This appears to be more prominent for boys than for girls, i.e. the program is more successful at getting females to return permanently to school than it is with boys. In our earlier regressions this was picked up by a lower program impact on boys' enrollment in 1999 compared to 1998. Since incomplete years most likely do not constitute extra human capital, we therefore view the lower 1999 estimates of program impact as better at capturing the human capital impacts of the program. These are the estimates used in our cost-effectiveness analysis.

Based on the above estimates, we calculate the impact of the program on extra years of schooling for a cohort of 1000 children. We compare this to the impact generated by the observed extensive expansion on the supply side. In both cases we focus on conditional enrollment rates, i.e. the enrollment rate of children who are eligible to enroll is a particular grade. For example, a conditional enrollment rate of 0.3 at grade 7 implies that only 30% of those children completing primary school actually enroll in grade 7 (the first year of junior secondary school). Focusing on the impacts suggested by the more conservative and preferred difference-in-difference estimates, we find that the primary education subsidies result in an extra 76 years of education for girls and 57 for boys.

This gender bias in favor of females is sufficient to eliminate the initially small bias in enrollment rates in favor of boys: initial rates for boys and girls were 88% and 87% respectively.

The initial conditional enrollment rates for secondary school indicate that the big drop-out from school occurs in the transition from primary to secondary school, with conditional enrollment falling drastically in grade 7 to around 30%. Once enrolled, the vast majority go on to complete junior secondary school with conditional enrollment rates rising to 86% and 90% in grades 8 and 9 respectively. The average conditional enrollment rate over the three secondary school years is 47% for girls and 56% for boys, providing evidence of a clear enrollment gender bias against girls. The impact of the program is to increase this average rate by 5.6 percentage points for boys and 11.9 percentage points for girls. Assuming that all this impact is concentrated in the transition year (i.e. grade 7), as is suggested by the data, this increases the conditional enrollment rate in grade 7 by 9.3 percentage points for boys and 17.9 percentage points for girls. The implied impact on extra years of schooling are 249 and 479 extra years of schooling for boys and girls respectively, the gender bias being sufficiently strong to virtually eliminate the initial gap in the average conditional enrollment rate over the three years. After the program these are nearly equalized at 59% and 61% for girls and boys respectively.

Focusing on the impact of extensive supply-side expansion, the data suggest that 12 new schools were built between 1997 and 1999 leading to a decrease in the average distance to the nearest secondary school from 2.02km in 1997 to 1.95km in 1999. Using the coefficients on distance (and its square) from the regression analysis, we predict that this resulted in an average enrollment impact of 0.75 percentage points for girls and 0.45 percentage points for boys. If one assumes that this impact is concentrated in the transition year to secondary school these imply

increases in the conditional enrollment rate at grade 7 of 1.1 percentage points for girls and 0.7 percentage points for boys over the two years. This, in turn, leads to over 30 extra years of schooling for girls and over 26 extra years for boys over the same period.

The relative attractiveness of primary and secondary education subsidies and extensive expansion on the supply side depends on their relative cost-effectiveness ratios (CERs), i.e. the cost incurred in generating an extra year of schooling. Focusing on CERs based on difference-in-difference estimates, and assuming that the enrollment effects of extensive supply last for 40 years, we find a clear and robust ordering with the cost of generating an extra year of schooling being $10,000 using subsidies in secondary school, $55,483 using subsidies in primary school and $167,962 using extensive expansion through the building of more secondary schools thus decreasing average distances traveled to school. These results provide fairly persuasive evidence that, in terms of its objective of getting more children into school, PROGRESAs subsidies are a very attractive option from the perspective of cost effectiveness. This is particularly true of subsidies for secondary education given the already high enrollment rates in primary school.

In this report we have not attempted to undertake any cost-benefit analysis. This would involve attaching monetary values to the extra years of education generated by the program. Such values are usually based on observed market returns to education. These invariably tell us that returns to secondary education are higher than for primary education and this just reinforces our ranking of primary versus secondary education subsidies. Also, the ranking of secondary school subsidies relative to extensive expansion of the supply side is insensitive to market valuations since each extra year will have the same valuation so that the ratio of CERs across these two instruments will always be the same as the ratio of CBRs. The fact that market values do not necessarily provide useful extra information for the present evaluation partly reflects the view that for other reasons (e.g. the existence of social benefits or a desire for greater equality of opportunity) we believe that educational outcomes (i.e. enrollment) are too low so that the important policy question relates to the most cost-effective way of raising these enrollment rates.

However, in conclusion, we do recognize that other potentially attractive policy instruments have not been considered in this report (e.g. improved transport facilities or different educational technologies) and that other dimensions of policy not considered here are equally important (e.g. high quality education and improved job opportunities). It is important that such issues are not lost sight of in the construction of a comprehensive poverty alleviation strategy. It is also the case that knowledge of market returns to education may be particularly valuable in the design of these other policy dimensions. For example, low returns to education may be the result of poor quality of education (e.g. poor education performance by children or a mismatch between the skills employers require and those acquired by children through formal education) or from poor macroeconomic management leading to low growth and an inability of the economy to absorb the greater number of educated persons without depressing the returns to education.

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SECTION 3: TABLES

Table 1CFramework for the Evaluation of PROGRESA

Program Objective: Improve Education, Health and Nutrition Status of Poor Households (Especially Females and Children).				
	Impact Assessment		**Process Assessment**	
Component (Objective)	**Output**	**Indicator**	**Inputs**	**Indicator**
Education (Improve quantity and quality of education for children and youths)	Improved attendance Increase years Cognitive achievement	Dropout/Re-entry levels Attendance levels Years of education Test scores	Grants for attendance Money for materials Performance incentives Improving educational services	Take-up of grants Receipt/use of money Loss of grant Improved school capacity, infrastructure, equipment; student/staff ratios; staff training and program development;
	Pre-school education	Education in home	Promoting non-formal pre-school education	Development and performance of adult education programs
	Parents= participation	Awareness of role and design	Adult education programs related to PROGRESA	

continued

Table 1C continued

Program Objective: Improve Education, Health and Nutrition Status of Poor Households (Especially Females and Children)				
	Impact Assessment		**Process Assessment**	
Component (Objective)	**Output**	**Indicator**	**Inputs**	**Indicator**
Health (To expand and improve coverage of primary-care services)	Incidence of premature mortality Incidence and duration of excess morbidity symptoms, days lost, physical limitations (respondent reported) Improved household and community preventative care Parents- participation	Maternal and infant mortality Incidence and treatment of diarrhea, respiratory illnesses, tuberculosis, infectious disease, contagious disease, intestinal disease, sexually transmitted disease, cervicouterine cancer, blood-pressure, diabetes, hearing, sight Vaccinations, anti-parasite treatment, health visits, injuries and first-aid, improved hygiene, self treatment, family planning Awareness of role and design	Improved health services which increase quality and use of public-health resources (including timetables for visits, reduced waiting times, appointment calendars, better opening hours, vaccinations) Medicine and materials supplies Integration with second- and third-level health care Improved health services (public and private) Health-care visits Education and training especially for pregnant and nursing mothers with small children Adult education programs related to PROGRESA	Receipt of resources (for example, equipment, medicines) Receipt of quality training Procedures for, and performance of, detection, appointments, referrals etc. Incentives for staff Number and quality of drugs and equipment Health-care-worker/patient ratios Attitudes/work habits of health-care-workers Health-care records Development and performance of adult education programs

continued

Table 1C continued

Program Objective: Improve Education, Health and Nutrition Status of Poor Households (Especially Females and Children).				
	Impact Assessment		**Process Assessment**	
Component (Objective)	**Output**	**Indicator**	**Inputs**	**Indicator**
Nutrition (Improve nutritional status)	Incidence of malnutrition Treatment of malnutrition Prevention of malnutrition Parents- participation	Child malnutrition Child height, age and weight Food, nutrient and calorie consumption (level and composition) Maternal anthropometry Awareness of role and design	Nutrition supplements for pregnant and lactating women and young children Finger-prick tests (?) Food grants Education and training Adult education programs related to PROGRESA	Receipt of supplements, grants and quality training Take-up of supplements and grants Follow-up procedures Development and performance of adult education programs

continued

Table 1C continued

Program Objective: Improve Education, Health and Nutrition Status of Poor Households (Especially Females and Children).				
	Impact Assessment		**Process Assessment**	
Component (Objective)	**Output**	**Indicator**	**Inputs**	**Indicator**
Other	Women empowerment and participation	Improved education, health and nutrition; stated ability to take decisions; activity responsibility; gender attitudes; actual control of resources; higher status within household and community	All aspects of human capital for mothers and daughters	As above
	Household Welfare	Family education, health and nutrition status; household consumption, durables and savings	Adult education programs related to PROGRESA	Development and performance of adult education programs
	Community Participation	Community awareness, facilitation and attitudes (transport, bribes, theft etc.) Requests for case review	Community meetings Community adjustment of criteria and beneficiaries	Community outreach worker, training and process
	Integration with other community and state programs	Existence and use of other programs, substitutes or complements	Information on and existence of other programs	Awareness and existence of other programs
	Targeting (poor, facilities)	Errors of exclusion and inclusion relative to other targeting mechanisms	Data collection and analysis; community input	Implications of analysis for design
	Incentives	Take-up of benefits; use of services; expenditure allocations; program attrition	Limits on grants; schooling years targeted; integration with other programs	Appropriate benefit levels received; attrition over time; nature of other programs

SECTION 4: TABLES

Table 1— Expansion of PROGRESA Over Time

	Phase 1	Phase 2	Phase 3	Phase 4	Phase 5	Phase 6	Phase 7	Phase 8	Phase 9	Phase 10	Phase 11
ENCASEH Survey	Oct-Dec 1996	Oct-Dec 1996, Oct-Dec. 1997	Oct-Dec. 1997	May-July 1998	Cleaning of Phases 1-3	Oct-Dec 1998	May-July 1999 (plus cleaning 6)	May-July 1999	May-July 1999	Oct-Dec. 1999	Completion of 1999 (plus Chiapias)
Incorporation Date	Aug/Sept. 1997	Nov/Dec. 1997	Feb/Mar, May1998	July-Sept. 1998	October 1998	Nov/Dec. 1998	May/June 1999	July/Aug. 1999	Sept/Oct. 1999	Nov/Dec. 1999	Mar/April 2000
Localities Incorpd.	3,369	2,988	4,334	25,568	5,432	8,151	3,290	9,758	2,801	6,523	131
Households Incorpd.	140,544	160,161	141,211	1,000,496	65,303	422,317	96,372	283,818	26,389	251,778	5,670
Cumulative Families	140,544	300,705	441,916	1,444,412	1,507,715	1,930,032	2,026,404	2,310,222	2,336,611	2,588,389	2,594,059
First Transfer	Sept/Oct. 1997	Jan/Feb. 1998	April-Aug 1998	Sept-Dec. 1998	Nov-Dec 1998	Jan-April 1999	July-Aug 1999	Sept-Oct. 1999	Nov-Dec. 1999	Jan-Mar. 2000	May-June 2000

Note: The treatment and control samples (i.e. 506 localities and 24,000 households) were taken from Phase 2. The control households were incorporated during phases 10 and 11.

Table 2 — The Level and Composition of Transfers to the Beneficary Families ('000 pesos, 2000 prices)

Transfer Components	1997	1998	1999	2000	2001/Predicted
Education transfers	131485.2833	916327.8844	2797760.566	4003000	4258632.93
Scholarships	127432.6547	na	2729380.162	3681600	3916708.217
Materials	4052.628594	na	68380.40426	321400	341924.7124
Food transfers	97210.55218	1029600.178	2985686.654	3753876.2	3993600
Health Component	112873.2112	1858438.478	1305834.483	1461700	1555044.655
Supplements	36923.94941	965809.8506	744256.284	721600	767681.6193
Other	75949.2618	892628.627	561578.1992	740100	787363.0356
Total Cash Transfers (Food+Education)	228695.8355	1945928.063	5783447.22	7756876.2	8252232.93
Food share	0.574935189	0.470895046	0.483753108	0.516058256	0.516058256
Scholarship share	0.425064811	0.529104954	0.516246892	0.483941744	0.483941744
Total Transfers	341569.0467	3804366.54	7089281.703	9218576.2	9807277.585

Note: The numbers for 2001 are predicted using the food transfers for 2001 and applying the ratios from 2000.

Table 3 — Description of Program Activities

(1) Selection of Localities These costs are incurred before any localities, or households within localities, are identified as being eligible for benefits, and are thus independent of the number of households in the program or the level of cash transfers.	(i) Co-ordination with education/health sectors	Collection and processing data from health and education ministries and the national census to determine which localities are eligible for the program, i.e. have access to a school and health clinic. A marginality index is constructed for each locality with the most marginal selected into the program
	(ii) Verification of access to, and capacity to provide, education and health services	Informing municipal governments which localities were selected, confirmation of their access to adequate facilities, and consideration of others that may qualify
	(iii) Regionalization of localities	Grouping of localities into "homogenous regions" later used to select beneficiary households
(2) Identification of Beneficiary Families These costs are also incurred prior to the incorporation of households from eligible localities into the program and are thus independent of the total number of households incorporated and the level of transfers. We associate these with household targeting.	(i) Collection of socio-economic information on households	Collection of baseline ENCASEH household surveys, including revisiting households to re-assess beneficiary status
	(ii) Entry and processing of survey data	This data is entered in PROGRESA's office
	(iii) Identification of households in extreme poverty	This involves the application of discriminant analysis to baseline ENCASEH and national census data

Table 3 — Description of Program Activities

(3) Incorporation of Families The magnitude of these costs is expected to increase with the number of localities and households incorporated into the program but is independent of the level of transfers. We associate these costs, for the most part, with the conditioning of the transfers.	(i) Printing and distribution of forms (and induction package)	Forms handed to beneficiary households during the general assembly
	(ii) Organization and operation of incorporation process of families	Advance preparation for, and carrying out of, general assemblies
	(iii) Collection of forms from households	Collected at first cash transfer and required as proof of registration at schools and health clinics
(4) Certification of Fulfillment of Co-responsibility Actions The magnitude of these costs increases with the total number of households but is independent of the level of transfers. We associate these with program conditioning.	(i) Confirmation of school registration	This involves collection of E2 and S2 forms from the various institutions. The schools and health clinics send these forms via the UAEPs
	(ii) Confirmation of school attendance	
	(iii) Confirmation of health registration	
	(iv) Confirmation of health attendance	
(5) Delivery of Cash Transfers These costs increase with the number of households in the program and the level of transfers.	(i) Cost of transferring cash transfers	Payment to Telecomm for distributing cash transfers (fixed % of transfers)
	(ii) Cash transfers: education, health and school materials	Actual transfers sent to households
	(iii) Verification/monitoring of cash transfers	Involves setting-up and running cash transfer
	(iv) Delivery and administration of holograms	Holograms are needed by households to identify themselves as beneficiaries
	(v) Administration of blank holograms	
(6) Follow-up Services These costs are expected to increase with the	(i) Promotora reports on education/health services	These are undertaken on a monthly basis

Table 3 — Description of Program Activities

number of localities and households in the program. We treat them as being incurred regardless of whether transfers are targeted to poor households or conditioned on human capital accumulation.	(ii) Organizing, collecting and processing reports	This is undertaken in PROGRESA
	(iii) Support to beneficiary families	Receipt of new applications for inclusion, changes in the beneficiary list, and other related operational activities
	(iv) Support to non-beneficiaries	Dealing with requests for inclusion from households and localities, processing and filing this information
(7) Evaluation of Program These costs are not treated as part of the program costs.	(i) Analysis of ENCEL and other surveys	Some undertaken in PROGRESA, some by IFPRI. Collection of ENCEL survey data for evaluation of program
Note: This classification of activities is based on "Esquema Gereral de Operacion de PROGRESA: 8. Capacitacion".		

Table 4 — Disaggregation of Annual Program costs (thousand of pesos)

Cost Components	1997	1998	1999	2000 e/
Honoraries and Commissions		26,431.75	43,910.43	37,900.00
Salaries		32,295.00	77,219.66	81,800.00
Materials and supplies		28,610.40	37,005.39	24,216.68
Basic services [1]	1,565.09	25,326.56	93,136.61	127,132.71
Rental and Lease Payments		1,836.94	2,206.70	1,490.87
Training, Informatics and Reports [2]		40,892.60	10,384.40	38,694.75
Commercial and Bank Services		6,853.28	5,185.62	4,987.43
Maintenance of Computer Equipment		97.22	679.28	748.31
Maintenance of other equipment and buildings		6,486.70	5,709.46	4,445.96
Dissemination of Information		13,763.97	17,882.38	15,140.07
Travel and Subsistence [3]		42,583.01	47,635.31	43,976.29
Official Services		9,909.00	3,421.16	6,442.31
Other Services		30.77	35.19	24.64
Other State Expenses [4]		7,885.30	0.00	0.00
Furniture and Real Estate [5]	328.50	2,623.40	2,915.61	2,200.00
Machinery and industrial and telecommunications equipment [5]	16,016.20	13,637.60	2,408.56	3,080.00
Vehicles [5]	2,681.40	73.00	12,354.05	3,520.00
Tools and parts		841.07	0.00	0.00
Surveys [6]	109,034.37	216,855.39	95,220.31	20,600.00
Encaseh	109,034.37	207,013.19	83,908.78	0.00
Evaluation Surveys		9,842.20	11,311.52	20,600.00
Total	**204,481.00**	**477,032.95**	**457,310.12**	**416,400.00**
Adjusted total [7]	**93,881.54**	**224,610.91**	**267,212.25**	**263,804.04**
Adjusted for Capital [8]	**81,314.80**	**220,765.83**	**268,966.71**	**276,193.84**
Adjusted for Inflation [9]	**122,051.37**	**287,471.78**	**296,168.31**	**276,193.84**

1 Postal and telegraphic costs amounting to $1,042 (1997/estimate), $22,996 (1998) $84,285 (1999), $124,100 (2000/budget) are allocated to activity (5(i)) in Table 3.

2 This includes costs related to training, capacity building, information services, and studies and investigations. These are allocated to activity (7) in Table 3. Also includes the cost for information services that corresponds to some contracts for entry and processing survey data that corresponds to activity (2(ii)) in Table 3: $8.623 (1998), $4945.19 (1999).

3 International travel and subsistence costs of $303 (1998), $208 (1999), $157 (2000/estimate) are allocated for activity (7) in Table 3.

4 This refers to costs incurred by states, which are treated in the same way as salaries and other personnel costs.

5 Capital equipment is assumed to depreciate at the following annual rates: Furniture and Real Estate(10%), Machinery and industrial and telecommunications equipment(30%) and Vehicles (25%). The calculation of cost of capital based on these rates and purchases of capital is presented in Appendix Table 1.

6 This refers to the cost of collecting the Encaseh surveys (which are allocated to activity 2(i) in Table 3). and Encel surveys (which are allocated to (7) in Table 3). For 2000 these only include the Encel Surveys.

7 The total cost adjusted for the costs directly allocate to activities as described above.

8 This makes a further adjustment by replacing capital purchases with the cost of capital, as described in footnote 6 and is specified in Appendix Table 1.

9 This inflates costs to a common base of March 2000 prices based on the General Price Index published by *Banco de México*. The inflation scale factors are: 1.5 (1997), 1.3 (1998), 1.1 (1999)

Note: For 1997 we do not have complete disaggregated cost data.

Table 5— Allocation of Annual Adjusted Total Program Costs to Activities

	Personal Time By Activities				Adjusted Total Personal Cost by Activities *				Adjusted Total Non-Personal Cost by Activities			
PROGRAM ACTIVITY	**1997**	**1998**	**1999**	**2000**	**1997**	**1998**	**1999**	**2000**	**1997**	**1998**	**1999**	**2000**
SELECTION OF LOCALITIES												
Co-ordination with health and education sectors	7.50	3.42	2.66	2.39	9,153.85	9,825.85	7,872.31	6,607.24				
Verification of access to, and capacity to provide, education and health services	4.20	2.28	2.22	1.99	5,126.16	6,550.56	6,560.26	5,506.03				
Regionalization of localities	2.10	1.14	1.11	1.00	2,563.08	3,275.28	3,280.13	2,753.02				
Education services												
Health services												
IDENTIFICATION OF BENEFICIARY FAMILIES												
Collection of socio-economic information of households	-	-	-	-	-	-	-	-	163,657.71	269,563.67	126,240.67	-
Into new localities												
Into Progresa localities												
Entry and processing of survey dat	-	-	-	-	-	-	-	-		8,623.80	4,945.19	
Identification of households in extreme poverty	9.45	5.70	5.54	4.98	11,533.85	16,376.41	16,400.64	13,765.08				
INCORPORATION OF FAMILIES												
Printing and distibution of forms	4.19	3.62	2.34	2.11	5,116.82	10,403.84	6,936.11	5,821.49				
Organization and operation of incorporation process of families	3.96	5.00	3.89	3.93	4,833.23	14,373.59	11,528.29	10,858.29				
Collection of formS from households	1.65	4.75	5.03	5.08	2,013.85	13,643.23	14,904.10	14,037.91				
CERTIFICACION OF FULFILLMENT OF CO-RESPONSIBILITY ACTIONS				-								
Confirmation of registration of students in primary and secondary schools	3.53	4.06	4.56	4.60	4,311.29	11,676.30	13,496.36	12,711.99				
Confirmation of child attendance at primary and secondary schools	6.60	10.58	12.37	12.50	8,049.65	30,413.90	36,648.62	34,518.70				
Confirmation of registration at health clinics	3.53	4.06	4.56	4.60	4,311.29	11,676.30	13,496.36	12,711.99				
Confirmation of attendance at health clinics	5.84	12.48	12.37	12.50	7,130.67	35,871.19	36,648.62	34,518.70				
DELIVERY OF CASH TRANSFERS												
Cost of trasferring of monies through Telecomm	9.41	6.70	5.54	4.98	11,487.19	19,266.37	16,400.64	13,765.08	2,349.16	29,944.75	92,809.56	124,100.00
Verification and monitoring cash transfer process by Progresa	7.53	5.36	5.54	6.09	9,189.75	15,413.09	16,400.64	16,823.98				
Delivery and administration of holograms	0.99	2.72	2.66	2.68	1,208.31	7,823.02	7,872.31	7,414.79				
Administration of blank holograms	-	0.54	1.11	1.22	-	1,541.31	3,280.13	3,364.80				
FOLLOWING-UP AND SERVICING												
Promotoras' community reports on education and health services												
Support to beneficiary families												
Receipt of new applications for inclusion	1.98	2.72	2.34	3.34	2,416.62	7,823.02	6,936.11	9,224.87				
Changes in beneficiary list	1.13	0.80	1.11	0.50	1,378.46	2,311.96	3,280.13	1,380.97				
Operational incidents	3.63	2.28	3.32	3.11	4,430.46	6,550.56	9,840.38	8,589.63				
Support to non-beneficiaries												
Dealing of requests of non-beneficiaries	3.46	2.23	1.78	3.04	4,222.98	6,404.49	5,267.34	8,396.29				
Dealing of requests of new localities	5.91	4.51	3.99	3.04	7,213.24	12,955.06	11,827.60	8,396.29				
Processing and filling	0.66	1.90	2.01	3.59	805.54	5,457.29	5,961.64	9,915.36				
Seguimiento de la operación	5.74	6.33	7.31	3.57	7,011.49	18,187.45	21,648.84	9,860.12				
Sistema de información de la DGCSR												
EVALUATION	7.00	6.84	6.65	9.14	8,543.60	19,651.69	19,680.77	25,251.22	-	57,834.91	19,173.64	59,451.76
COLUMN TOTAL	100	100	100	100	122,051.37	287,471.78	296,168.31	276,193.84	166,006.87	365,967.13	243,169.06	183,551.76

Table 6 — Net Present Value of Total Program Costs by Activity

	Adjusted Total Cost by Activities					Net Present Value				Net Present Value			
PROGRAM ACTIVITY	**1997**	**1998**	**1999**	**2000**	**2001**	**r = 0%**	**r = 3 %**	**r = 5%**	**r = 8%**	**r = 0%**	**r = 3 %**	**r = 5%**	**r = 8%**
SELECTION OF LOCALITIES													
Co-ordination with health and education sectors	9,153.85	9,825.85	7,872.31	6,607.24	7,029.18	33,459.24	32,160.47	31,359.80	30,246.13	40,488.42	38,405.80	37,142.72	35,412.79
Verification of access to, and capacity to provide, education and health services	5,126.16	6,550.56	6,560.26	5,506.03	5,857.65	23,743.01	22,708.40	22,071.45	21,186.72	29,600.66	27,912.84	26,890.55	25,492.27
Regionalization of localities	2,563.08	3,275.28	3,280.13	2,753.02	2,928.82	11,871.50	11,354.20	11,035.73	10,593.36	14,800.33	13,956.42	13,445.28	12,746.13
Education services													
Health services													
IDENTIFICATION OF BENEFICIARY FAMILIES													
Collection of socio-economic information of households	163,657.71	269,563.67	126,240.67	-	-	559,462.05	544,363.95	534,889.02	521,484.73	559,462.05	544,363.95	534,889.02	521,484.73
Into new localities													
Into Progresa localities													
Entry and processing of survey dat	-	8,623.80	4,945.19	-	-	13,568.99	13,033.93	12,698.57	12,224.70	13,568.99	13,033.93	12,698.57	12,224.70
Identification of households in extreme poverty	11,533.85	16,376.41	16,400.64	13,765.08	14,644.12	58,075.98	55,489.45	53,897.09	51,685.26	72,720.10	68,500.56	65,944.84	62,449.13
INCORPORATION OF FAMILIES													
Printing and distibution of forms	5,116.82	10,403.84	6,936.11	5,821.49	6,193.25	28,278.26	27,083.07	26,345.32	25,317.89	34,471.50	32,585.69	31,440.52	29,870.11
Organization and operation of incorporation process of families	4,833.23	14,373.59	11,528.29	10,858.29	11,551.71	41,593.40	39,591.57	38,358.67	36,645.43	53,145.11	49,855.11	47,862.28	45,136.28
Collection of formS from households	2,013.85	13,643.23	14,904.10	14,037.91	14,934.38	44,599.08	42,154.92	40,652.33	38,568.07	59,533.46	55,423.92	52,938.88	49,545.29
CERTIFICACION OF FULFILLMENT OF CO-RESPONSIBILITY ACTIONS													
Confirmation of registration of students in primary and secondary schools	4,311.29	11,676.30	13,496.36	12,711.99	13,523.78	42,195.94	40,002.39	38,654.26	36,784.82	55,719.72	52,018.09	49,780.31	46,725.20
Confirmation of child attendance at primary and secondary schools	8,049.65	30,413.90	36,648.62	34,518.70	36,723.08	109,630.87	103,712.05	100,075.20	95,033.01	146,353.95	136,340.03	130,287.37	122,025.57
Confirmation of registration at health clinics	4,311.29	11,676.30	13,496.36	12,711.99	13,523.78	42,195.94	40,002.39	38,654.26	36,784.82	55,719.72	52,018.09	49,780.31	46,725.20
Confirmation of attendance at health clinics	7,130.67	35,871.19	36,648.62	34,518.70	36,723.08	114,169.19	108,091.42	104,353.65	99,167.08	150,892.27	140,719.39	134,565.81	126,159.64
DELIVERY OF CASH TRANSFERS													
Cost of trasferring of monies through Telecomm	13,836.35	49,211.12	109,210.20	137,865.08	146,669.19	310,122.74	290,721.29	278,853.98	262,474.08	456,791.93	421,034.96	399,519.09	370,280.31
Cash transfers to Progresa families					-					-	-	-	-
Food transfers	97,210.55	1,029,600.18	2,985,686.65	3,753,876.20	3,993,600.00	7,866,373.58	7,346,446.90	7,028,627.36	6,590,237.08	11,859,973.58	10,894,708.77	10,314,171.96	9,525,652.30
Scholarships	127,432.65	916,327.88	2,729,380.16	3,681,600.00	3,916,708.22	7,454,740.70	6,958,959.49	6,656,058.65	6,238,460.75	11,371,448.92	10,438,904.01	9,878,344.19	9,117,358.21
School materials	4,052.63	-	68,380.40	321,400.00	341,924.71	393,833.03	362,634.25	343,713.08	317,815.49	735,757.75	666,429.93	625,015.38	569,140.36
Supplements	36,923.95	965,809.85	744,256.28	721,600.00	767,681.62	2,468,590.08	2,336,502.57	2,255,150.46	2,142,101.48	3,236,271.70	3,018,577.74	2,886,724.02	2,706,370.39
Verification and monitoring cash transfer process by Progresa	9,189.75	15,413.09	16,400.64	16,823.98	17,898.37	57,827.46	55,009.42	53,277.94	50,877.46	75,725.83	70,911.89	68,002.97	64,033.29
Delivery and administration of holograms	1,208.31	7,823.02	7,872.31	7,414.79	7,888.30	24,318.43	23,009.46	22,204.39	21,087.18	32,206.73	30,018.11	28,694.12	26,885.32
Administration of blank holograms	-	1,541.31	3,280.13	3,364.80	3,579.67	8,186.23	7,667.52	7,349.72	6,910.40	11,765.91	10,848.01	10,294.73	9,541.57
FOLLOWING-UP AND SERVICING													
Promotoras' community reports on education and health services													
Support to beneficiary families													
Receipt of new applications for inclusion	2,416.62	7,823.02	6,936.11	9,224.87	9,813.98	26,400.63	24,991.80	24,127.17	22,929.76	36,214.60	33,711.39	32,201.15	30,143.32
Changes in beneficiary list	1,378.46	2,311.96	3,280.13	1,380.97	1,469.16	8,351.52	7,978.71	7,748.44	7,427.61	9,820.68	9,284.03	8,957.12	8,507.48
Operational incidents	4,430.46	6,550.56	9,840.38	8,589.63	9,138.16	29,411.04	27,926.47	27,014.66	25,751.07	38,549.21	36,045.61	34,532.65	32,467.89
Support to non-beneficiaries													
Dealing of requests of non-beneficiaries	4,222.98	6,404.49	5,267.34	8,396.29	8,932.48	24,291.10	23,089.70	22,353.16	21,334.21	33,223.59	31,026.10	29,701.94	27,899.85
Dealing of requests of new localities	7,213.24	12,955.06	11,827.60	8,396.29	8,932.48	40,392.18	38,623.40	37,532.40	36,014.16	49,324.66	46,559.80	44,881.17	42,579.81
Processing and filling	805.54	5,457.29	5,961.64	9,915.36	10,548.56	22,139.83	20,797.25	19,975.60	18,840.86	32,688.38	30,169.51	28,653.92	26,594.37
Seguimiento de la operación	7,011.49	18,187.45	21,648.84	9,860.12	10,489.79	56,707.90	54,098.73	52,486.55	50,239.40	67,197.69	63,418.77	61,116.53	57,949.71
Sistema de información de la DGCSR													
EVALUATION	8,543.60	77,486.60	38,854.40	84,702.99	90,112.15	209,587.58	197,912.54	190,752.07	180,841.80	299,699.73	277,976.01	264,887.56	247,076.91
TOTAL (excluding evaluation and transfers)	279,514.64	575,952.30	500,482.97	375,042.61	398,992.96	1,730,992.53	1,653,661.95	1,605,969.36	1,539,608.21	2,129,985.49	2,008,162.03	1,934,221.86	1,832,879.94

Note: Total costs include personnel and non-personnel costs from Table 5. Net-present values include predicted costs for 2001.

Table 7— Family Health Attendance Requirements

Age Group	Frequency of Visits
Less than 4 months	3 visits: at 7 and 28 days, and at 2 months
From 4 to 24 months	8 visits for nutrition and immunization: at 4, 6, 9, 12, 15, 18, 21 and 24 months. In addition, one monthly visit to measure weight and height.
From 2 to 4 years	3 annual visits: 1 every 4 months
From 5 to 16 years	2 annual visits: 1 every 6 months
More than 17 years	1 visit a year

Source: CONPROGRESA (2000), Reglas Generales para la Operación del Programa de Educación, Salud y Alimentación, page 38.
Note: Since children between 0-2 years must make 25 visits, we assume 12.5 visits per year.

Table 8 — Private Costs associated With Health Component By State

		Guerrero		Hidalgo		Michoacán		Puebla		Queretaro		San Luis Potosí		Veracruz		Total	
		Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean
1	**Trips to health center (annual)**																
	Age group: 0 - 5 years old	1.86	3.63	2.03	3.33	1.98	3.56	1.95	3.51	2.15	3.71	2.01	3.51	1.80	3.30	1.94	3.46
	Age group: 6 - 12 years old	1.85	4.15	1.88	4.05	2.10	4.44	1.91	4.07	2.07	4.31	2.03	4.17	1.93	4.04	1.96	4.14
	Age group: 13 - 16 years old	1.18	2.82	1.20	2.93	1.40	3.29	1.24	2.95	1.27	3.15	1.26	3.07	1.18	2.91	1.25	2.99
	Age group: 17 years old and more	1.10	1.94	1.10	1.91	1.19	2.13	1.26	2.02	1.39	2.16	1.26	2.16	1.12	1.92	1.18	2.01
	Mothers	5.50	17.85	5.14	16.85	5.08	16.45	5.44	17.38	5.66	17.05	5.28	16.91	5.03	16.89	5.25	17.02
	Fathers	1.50	1.19	2.28	1.42	2.81	1.54	1.91	1.30	2.83	1.61	2.70	1.59	1.91	1.26	2.25	1.39
	Family	8.09	26.68	7.61	24.74	7.64	25.30	8.15	25.66	8.91	25.70	8.37	24.66	7.47	25.01	7.92	25.25
	Age group: 13 - 50 years old	8.20	19.50	7.85	18.13	7.51	18.54	8.09	18.77	8.88	18.07	8.38	18.12	7.52	18.61	7.94	18.54
2	**Actual annual trips (more than 5 years)**	7.25	24.72	6.80	23.23	6.86	23.73	7.25	23.90	7.84	24.13	7.50	23.16	6.70	23.48	7.07	23.64
3	**Effective annual trips by family (2-4 years= 0.5)**	7.61	25.70	7.14	23.98	7.19	24.51	7.64	24.78	8.32	24.91	7.88	23.91	7.03	24.24	7.44	24.45
4	**Annual travel time (include zeros)** *hours*																
	Age group: 0 - 5 years old	6.15	6.77	6.68	6.11	5.33	5.63	8.55	8.86	3.74	4.97	6.25	6.39	5.51	6.14	6.54	6.62
	Age group: 6 - 12 years old	7.07	7.76	7.11	7.53	6.32	7.20	8.23	10.04	4.03	6.04	7.05	7.49	5.90	7.15	6.92	7.78
	Age group: 13 - 16 years old	4.76	5.12	4.44	5.36	4.73	5.45	5.43	7.12	2.68	4.47	4.85	5.56	4.32	5.29	4.70	5.60
	Age group: 17 years old and more	3.38	3.43	3.42	3.53	3.12	3.51	4.90	5.27	2.56	3.13	3.56	3.82	3.48	3.53	3.71	3.82
	Mothers	24.98	32.67	25.80	31.12	19.52	26.42	33.74	44.06	12.79	23.70	24.61	30.93	23.60	31.33	26.09	32.58
	Fathers	4.16	2.21	6.63	2.69	4.84	2.43	7.20	3.48	4.66	2.27	5.28	2.75	4.70	2.35	5.62	2.65
	Family	37.76	48.97	38.15	45.75	30.73	40.72	49.04	64.92	19.75	35.80	36.15	44.78	34.54	46.12	38.48	48.17
	Age group: 13 - 50 years old	30.00	35.37	30.01	32.90	24.65	30.13	39.18	47.15	16.63	24.83	29.68	32.76	28.47	34.55	30.86	35.23
	Annual travel time (distance to clinic is > 0 kms, not include zeros) *hours*																
	Age group: 0 - 5 years old	6.49	8.34	7.40	7.35	5.51	6.10	8.66	8.96	3.89	4.97	6.02	6.72	5.72	6.65	6.84	7.30
	Age group: 6 - 12 years old	7.50	10.02	7.60	9.14	6.50	7.81	8.43	10.14	4.19	6.43	7.16	8.11	6.00	7.60	7.19	8.62
	Age group: 13 - 16 years old	5.13	6.81	4.48	6.34	4.88	5.86	5.52	7.19	2.82	4.68	4.82	6.07	4.45	5.80	4.83	6.24
	Age group: 17 years old and more	3.68	4.44	3.72	4.28	3.20	3.85	5.00	5.34	2.70	3.37	3.51	4.03	3.62	3.83	3.89	4.26
	Mothers	24.50	41.85	27.14	38.02	19.91	28.66	34.50	44.55	13.57	25.23	23.38	32.65	24.08	33.48	26.78	36.05
	Fathers	4.90	2.90	7.53	3.22	5.08	2.62	7.39	3.54	5.13	2.51	5.97	3.08	5.27	2.62	6.18	2.98
	Family	37.49	62.23	40.02	55.85	31.34	44.22	50.09	65.64	20.75	38.01	34.97	47.51	35.52	49.23	39.59	53.31
	Age group: 13 - 50 years old	30.86	44.98	32.07	39.77	25.20	32.73	39.96	47.57	17.70	26.29	28.93	34.25	29.42	36.84	31.97	38.83
5	Distance (kms) include zeros	2.96	3.20	3.58	3.62	3.44	4.62	3.36	4.70	2.70	2.51	4.17	4.41	3.96	3.72	3.68	3.98
	Distance (kms) not include zeros	2.42	4.69	3.12	5.35	3.15	5.30	3.25	4.98	2.67	3.14	3.70	6.10	3.85	4.93	3.40	5.12
6	Cost by trip (pesos)	6.47	4.56	7.22	3.35	7.39	6.88	4.98	2.01	4.52	1.46	10.02	7.09	4.89	2.39	7.09	3.95
7	Cost by trip (values >zero)	5.57	10.89	8.80	12.97	4.92	12.93	4.82	12.47	5.94	12.74	8.87	16.49	4.45	10.52	6.96	12.95
8	Cost by effective trips (annual)	178.94	118.15	180.28	79.84	194.65	169.16	142.06	54.39	120.72	37.02	236.66	162.85	120.48	55.37	178.65	95.70
9	Waiting time (mins)	24.22	29.78	15.19	34.71	14.62	45.37	18.56	31.97	9.88	22.06	18.49	51.83	24.81	41.45	21.18	38.73
10	Consult time (mins) constant		20		20		20		20		20		20		20		20
11	Pláticas time (mins) constant		60		60		60		60		60		60		60		60
12	% localities whitout clinic		85.7%		86.6%		91.1%		100.0%		85.7%		94.4%		91.4%		91.6%
13	%Families or households		10.2%		17.8%		12.4%		16.7%		4.3%		15.2%		23.4%		100.0%
14	% families without clinic		73.5%		71.6%		86.3%		100.0%		73.8%		92.4%		89.8%		85.9%

Sources: Encuestas de Seguimiento Operativo de 1999, surveys may (ESO99M) and november (ESO99N).

Notes: The average of persons by age group is: 0-5 (0.99), 6-12 (1.45), 13-16 (.74), 17 y+ (1.05), mother (1.06), father (0.91) and 13-50 (3.37). Total families 7799.

1 Constructed by applying Table 7 to Encel99N.

2 Equals trips by family ignoring persons less than 5 years old.

3 Equals trips by family but assuming 0-2 years constitute only half a trip.

4 Travel time in hours. Source is ESO99M, question 13, multiplying trave time by trips to the health center.

5 ESO99M, question 12: median of locality for one way trip.

6 ESO99N, question 2: median of locality for return trip.

7 ESO99N, question 2: median for locality return trip but excluding zero costs.

8 ESO99N, question 2: median cost of locality for return trip times effective trips (annual).

9 ESO99N, question 3: median of locality per trip.

10 Median of the "Encuesta de Recursos, Equipamiento y Condiciones de Slaud para Población Abierta, mayo de 1999", question 45.

11 Approximate time. Source: Personnel of Dirección General de Coordinación Sectorial, Conprogresa.

12 Encel99N and Encuesta de Recursos, Equipamiento y Condiciones de Salud para Población Abierta, mayo de 1999.

13 Encel99N.

14 Encel99N and Encuesta de Recursos, Equipamiento y Condiciones de Salud para Población Abierta, mayo de 1999.

Table 9 — Private Costs associated With Secondary Education By State

		Guerrero		Hidalgo		Michoacán		Puebla		Queretaro		San Luis Potosí		Veracruz		Total	
		Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean
1	Travel time to school (minutes) include zeros	82.00	59.06	98.15	66.55	77.98	54.89	108.73	84.36	61.64	75.41	42.94	24.59	92.47	81.19	89.22	64.75
	Travel time to school (minutes) not include zeros	86.29	92.51	105.44	112.69	82.60	76.40	111.25	127.29	61.64	75.41	44.76	71.64	92.74	103.09	93.67	99.57
2	Travel cost to school (constant pesos november of 1999)	-	-	6.00	2.09	5.57	2.17	5.81	2.42	4.98	3.14	4.02	0.62	3.33	1.41	4.79	1.58
	Travel cost to school (constant pesos november of 1999) not include zeros			9.12	13.01	8.11	9.87	6.00	13.72	4.67	8.38	9.64	23.15	4.43	6.00	7.85	9.90
3	Annual cost to travel secundary school (include zeros)	-	-	1,199.25	417.37	1,114.60	434.74	1,161.18	484.39	996.89	628.58	804.56	124.16	665.25	282.03	958.39	316.07
	Annual cost to travel secundary school (not include zeros)	-	-	1,823.85	2,601.31	1,622.06	1,974.45	1,199.65	2,744.87	934.94	1,676.21	1,927.82	4,629.52	886.25	1,200.34	1,570.68	1,979.50
4	Distance (kms) (include zeros)	2.79	2.80	2.52	2.13	2.78	2.99	2.31	2.40	2.39	3.80	2.44	1.49	3.01	3.02	2.73	2.54
	Distance (kms) (not include zeros)	2.47	4.06	2.39	3.48	2.47	4.08	2.07	3.35	2.39	3.80	2.57	3.69	2.94	3.66	2.56	3.68
5	% students		7.5		18.80		14.30		15.60		3.40		14.40		26.00		100.00
6	% students living in localities whit school (over total students in secundary of state)		33.2		38.80		26.90		28.00		0.00		59.20		18.50		31.30
7	% Localities whit secundary school		10.5		17.20		13.30		12.50		0.00		44.40		7.20		17.30

Notes: total students is 3247 and the total of the localities whit secundary school is 54. Is 200 days at year for go to school.

1 Encaseh, question 103. Go and return.

2 Encaseh, question 104. Constant pesos to november 1999. Go and return. Distance to secundary more near.

4 Geographic Information System, Dirección General de Planeación, Evaluación y Seguimiento. Only go. Lineal distance not real.

5 Encel99N, childs enrollment (inscritos) in the secundary school.

6 and 7 Encel99N. Information of the school in the locality our source sourvey is "Cuestionario de directores" SEP-Progresa, 1999.

Table 10 — Private Costs associated With Collection of Transfers By State

		Guerrero		Hidalgo		Michoacán		Puebla		Queretaro		San Luis Potosí		Veracruz		Total	
		Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean	Std. Desv.	Mean
1	Travel time (minutes)	109.39	127.94	85.43	160.61	57.72	98.91	137.51	172.38	75.10	107.56	70.12	125.36	80.28	133.48	95.56	137.59
2	Travel cost (pesos) include zeros	14.35	11.23	11.48	12.36	11.13	18.92	10.42	7.26	12.35	8.54	12.97	18.95	12.86	10.15	12.91	12.53
	Travel cost (pesos) not include zeros	14.62	16.98	9.61	17.83	10.81	19.46	10.35	15.14	12.13	18.40	11.02	22.40	12.05	18.64	11.52	18.77
3	Annual cost (include zeros)	86.11	67.37	68.86	74.16	66.76	113.49	62.52	43.56	74.09	51.22	77.79	113.70	77.14	60.90	77.48	75.21
	Annual cost (not include zeros)	87.74	101.90	57.67	107.00	64.85	116.74	62.12	90.82	72.79	110.42	66.11	134.39	72.32	111.83	69.11	112.63
4	Distance (kms) include zeros	4.03	6.67	4.83	7.45	5.75	10.09	4.68	6.31	4.78	4.97	8.44	13.08	7.24	11.02	6.65	9.10
	Distance (kms) not include zeros	4.03	6.67	4.52	8.04	5.75	10.09	4.68	6.31	4.53	6.32	8.38	13.21	7.22	11.07	6.57	9.33
5	How much goes to the locality? (Percent)																
	Daily	1.36		0.97		1.28		2.14		2.15		0.35		0.92		1.18	
	At week	39.43		19.90		27.58		19.36		20.43		17.73		16.45		21.63	
	Every 15 days	9.62		7.80		12.03		14.83		4.30		10.83		6.21		9.66	
	At month	6.37		6.99		10.33		5.27		2.51		8.21		5.98		6.85	
	By chance	16.26		25.10		15.23		12.52		11.47		16.16		22.14		18.20	
	Only by transfers	24.39		37.04		31.20		42.92		56.63		45.07		41.92		39.16	
	Not responding	2.57		2.19		2.34		2.97		2.51		1.66		6.38		3.31	
	Total	100.00		100.00		100.00		100.00		100.00		100.00		100.00		100.00	
6	% Persons whos live in pay points	0		1.38		0.00		0.00		1.17		0.14		0.09		2.79	
7	% Localities whit pay point	0		0.31		0.00		0.00		0.31		0.31		0.31		1.13	

Notes:

1 ESO99N, question 63. Median of the locality for return trip.

2 ESO99N, question 65. Median of locality for return trip.

3 Travel cost multiplied by six trips.

4 Geographic Information System, Dirección General de Planeación, Evaluación y Seguimiento. Linear distance one way.

5 ESO99N, question 62.

6 ESO99N.

7 Dirección General de Planeación, Evaluación y Seguimiento.

Table 11 — Program Costs For Alternative Program Designs and Policy Questions
(Costs that need to be deducted from total program costs because not incurred)

	Policy Questions		
Program Type	Actual Program	Program Expansion	Program Continuation
Conditioned/Targeted	None	Selection of Localities	Selection of Localities Identification of Households Incorporation of Households
Unconditioned/Targeted	Incorporation of Households Certification of Actions	Selection of Localities Incorporation of Households Certification of Actions	Selection of Localities Identification of Households Incorporation of Households Certification of Actions
Conditioned/Untargeted	Identification of Households	Selection of Localities Identification of Households	Selection of Localities Identification of Households Incorporation of Households
Unconditioned/Untargeted	Identification of Households Incorporation of Households Certification of Actions	Selection of Localities Identification of Households Incorporation of Households Certification of Actions	Selection of Localities Identification of Households Incorporation of Households Certification of Actions

Table 12 — Cost-Benefit Ratios (CBRs) for Alternative Program Designs and Policy Questions

	Net Present Value			
PROGRAM ACTIVITY	**r = 0%**	**r = 3 %**	**r = 5%**	**r = 8%**
PROGRAM COSTS (Net Present Values)				
SELECTION OF LOCALITIES (S)	84,889.40	80,275.07	77,478.55	73,651.19
IDENTIFICATION OF BENEFICIARY FAMILIES (ID)	645,751.14	625,898.45	613,532.44	596,158.56
INCORPORATION OF FAMILIES (IN)	147,150.08	137,864.72	132,241.68	124,551.67
CERTIFICACION OF FULFILLMENT OF CO-RESPONSIBILITY ACTIONS ©	408,685.66	381,095.61	364,413.81	341,635.62
DELIVERY OF CASH TRANSFERS	576,490.39	532,812.98	506,510.90	470,740.49
FOLLOWING-UP AND SERVICING	267,018.82	250,215.21	240,044.48	226,142.43
(1) TOTAL COST	2,129,985.49	2,008,162.03	1,934,221.86	1,832,879.94
(2) (1) - SELECTION OF LOCALITIES (S)	2,045,096.08	1,927,886.97	1,856,743.31	1,759,228.76
(3) (2) - IDENTIFICATION OF BENEFICIARY FAMILIES (ID)	1,399,344.95	1,301,988.52	1,243,210.87	1,163,070.20
(4) (3) - INCORPORATION OF FAMILIES (IN)	1,252,194.87	1,164,123.80	1,110,969.19	1,038,518.53
(5) (4) - CERTIFICACION OF FULFILLMENT OF CO-RESPONSIBILITY ACTIONS ©	843,509.21	783,028.19	746,555.38	696,882.91
TRANSFERS (Net Present Values)				
FOOD	11,859,973.58	10,894,708.77	10,314,171.96	9,525,652.30
SCHOLARSHIPS	11,371,448.92	10,438,904.01	9,878,344.19	9,117,358.21
SCHOOL MATERIALS	735,757.75	666,429.93	625,015.38	569,140.36
SUPLEMENTS	3,236,271.70	3,018,577.74	2,886,724.02	2,706,370.39
(1) TOTAL TRANSFERS	27,203,451.95	25,018,620.45	23,704,255.56	21,918,521.26
(2) (1) - SUPLEMENTS	23,967,180.25	22,000,042.70	20,817,531.54	19,212,150.87
COST - BENEFIT RATIOS				
Full	0.089	0.091	0.093	0.095
S	0.085	0.088	0.089	0.092
S/ID/IN	0.052	0.053	0.053	0.054
IN/C	0.066	0.068	0.069	0.071
S/IN/C	0.062	0.064	0.065	0.067
S/ID/IN/C	0.035	0.036	0.036	0.036
ID	0.062	0.063	0.063	0.064
S/ID	0.058	0.059	0.060	0.061
ID/IN/C	0.039	0.039	0.040	0.040

Table 13 — Total Program and Private Costs For Alternative Program Designs and Policy Questions

Program Type	Policy Questions		
	Actual Program	Program Expansion	Program Continuation
Conditioned/Targeted	Program=0.089 Private=0.018(H), 0.015(SE) 0.012(CT) Total=0.113	Program=0.085 Private=0.018(H), 0.015(SE), 0.012(CT) Total=0.109	Program=0.052 Private=0.018(H), 0.015(SE), 0.012(CT) Total=0.076
Unconditioned/Targeted	Program=0.066 Private=0.012(CT) Total=0.078	Program=0.062 Private=0.012(CT) Total=0.074	Program=0.035 Private=0.012(CT) Total=0.047
Conditioned/Untargeted	Program=0.062 Private=0.018(H), 0.015(E) 0.012(CT) Total=0.086	Program=0.058 Private=0.018(H), 0.015(E) 0.012(CT) Total=0.091	Program=0.052 Private=0.018(H), 0.015(E) 0.012(CT) Total=0.076
Unconditioned/Untargeted	Program=0.039 Private=0.012(CT) Total=0.051	Program=0.035 Private=0.012(CT) Total=0.047	Program=0.035 Private=0.012(CT) Total=0.047

Note: H, SE, and CT denote health/food, secondary education, and collection of cash transfers. To calculate total costs per peso received we use the following shares in total transfers: food/health (35%), utilities (6%), primary (22%) and secondary (37%).

SECTION 5: TABLES

Table 1 — Benefit Structure of PROGRESA, 1997 (Pesos/Month)

	Boys	Girls
Education Scholarships		
Primary		
8 years old	60	60
9 years old	70	70
10 years old	90	90
11-12 years old	120	120
Materials (annual)	110	110
Secondary		
13-14 years old	175	185
15 years old	185	205
16-17 years old	195	225
Materials (annual)	140	140
Food Transfer	90 per family	
Benefit Cap	550 per family	

Note: The benefits structure is meant to mimic as closely as possible that of the actual program which is linked to grades and not age: we choose age since this is consistent with an unconditional transfer program and, in any case, the data on maximum grade achieved is not very reliable. To be consistent with the figures for monthly per adult equivalent consumption, these numbers are deflated by a factor of 2.2868 to bring them to 1994 prices. The structure of scholarships is applied by age group but, in practice, is applied by grade and conditional on enrollment and attendance. All families receive the food transfer but, in practice, this is made conditional on regular visits to a health clinic. The cap on the total benefits a household can receive is applied only to the sum of the education scholarships and food transfer.

Table 2 — Distribution of Benefits Across Component for Treatment and Control Beneficiary Households

	Treatment Sample	Control Sample
Primary scholarships	76.1	75.9
Secondary scholarships	130.0	132.6
School materials	15.5	15.5
Food transfer	90.0	90.0
Total transfer	295.5	298.6
No. of households	14856	9221
Proportion poor:		
Pre-densification (PRO)	0.528	0.508
Post-densification (PROD)	0.782	0.778

Note: Transfers are in 1997 prices based on Table 1. The total transfer reflects the benefit cap.

Table 3 — Distributional Characteristic of Alternative Transfer Programs

	PROGRESA (PostDensf.)	PROGRESA (PreDensf.)	Consumption	NoTarget	PROGRESA Uniform	NoTarget Uniform
g=0.5	1.42	1.48	1.44	1.38	1.30	1.25
g=1.0	2.11	2.26	2.13	1.98	1.77	1.65
g=2.0	5.05	5.66	5.04	4.56	3.74	3.32
g=3.0	13.27	15.25	13.09	11.71	8.99	7.76
g=4.0	37.23	43.41	36.33	32.41	23.76	20.16
g=5.0	109.32	128.66	105.92	94.38	67.05	56.34
Average transfer	113	122	125	110	113	110
Number beneficiaries	11623	7837	11623	14856	11623	14856
Budget Scale Factor	-	0.73	1.11	1.25	1.00	1.00

Note: The budget scale factor is the factor is the ratio of the alternative program budgets to the post-densification budget; its inverse is the amount by which the post-densification transfers need to be scaled down or up in order to keep the total budget constant. Note that the distributional characteristic is independent of the size of the budget and is interpreted as the welfare impact of a unit of income being transferred to households through the various programs; in other words, the distributional power of the alternative programs.

Table 4 — Decomposition of Distributional Characteristic Into Its Targeting and Redistributive Efficiencies

	Post-Densification			Consumption			Pre-Densification		
	λ	λ_T	λ_R	λ	λ_T	λ_R	λ	λ_T	λ_R
e=0.5	1.43	1.30	0.13	1.44	1.36	0.08	1.48	1.38	0.10
e=1.0	2.11	1.77	0.34	2.13	1.90	0.23	2.26	1.98	0.28
e=2.0	5.05	3.74	1.31	5.04	4.07	0.97	5.66	4.45	1.21
e=5.0	109.32	67.05	42.27	105.92	71.89	34.03	128.66	85.50	43.16

Note: λ is the distributional characteristic, λ_T is the targeting efficiency and λ_R the redistributive efficiency. The targeting efficiency pre-densification is higher than that for consumption because it uses a lower poverty line to select households.

Table 5 — Welfare Impact of Program Components

	Uncapped Transfers					Capped Transfers			
	Total	Primary	Secondary	Utilities	Food	TotalCap	PrimCap	SecyCap	FoodCap
e=0.5	1.45	1.52	1.50	1.51	1.30	1.43	1.45	1.38	1.24
e=1	2.17	2.36	2.32	2.34	1.77	2.12	2.16	1.96	1.61
e=2	5.34	6.01	5.96	5.98	3.74	5.12	5.05	4.31	3.05
e=3	14.29	16.45	16.44	16.40	8.99	13.51	12.71	10.44	6.61
e=4	40.66	47.16	47.76	47.28	23.76	37.96	33.95	27.26	15.92
e=5	120.64	140.29	143.77	141.38	67.05	111.62	95.27	75.53	41.61

Table 6 — Distributional Characteristic For Restructured Benefits

Inequality Aversion	Cap =550	Cap=605
e=0.5	1.43	1.43
e=1.0	2.12	2.13
e=2.0	5.11	5.17
e=3.0	13.46	13.68
e=4.0	37.85	38.59
e=5.0	111.32	113.75

Note: The benefits are restructured by scaling up secondary scholarships by 10% and scaling down primary scholarships by a factor of 0.83, the latter keeping the budget constant. This is motivated by a desire to get a larger enrolment impact. The first set of results keeps the cap at 550 pesos while the second also scales this up by 10%.

Table 7 — Relationship of Distributional Characteristic to the Level and Inequality of Consumption

	(1) e=2	(2) e=0.5	(3) e=5	(4) e=2	(5) e=05
lpce	-1.195	-0.345	-2.314	-1.241	-0.357
	(22.21)**	(28.60)**	(15.46)**	(23.18)**	(30.99)**
lineq				0.276	0.071
				(7.46)**	(8.31)**
Constant	7.515	2.065	16.023	8.370	2.285
	(28.57)**	(34.90)**	(21.96)**	(27.86)**	(35.55)**
Observations	320	320	320	320	320
R-squared	0.63	0.71	0.50	0.71	0.78

Note: "lpce" is the log of adult equivalent consumption and "lineq" the log of its inequality, both at locality level. Absolute value of t-statistics in parentheses. * denotes significant at 5% level and ** significant at 1% level. When e=2 the mean of the distributional characteristic across localities is 5.07 with a standard deviation of 1.51 implying an 85% increase in welfare per unit expenditure in transferring income from one s.d below the mean to one s.d. above the mean. The equivalent number for e=5 is 248%.

Table 8 — Average Percentage Gains From Household Targeting

Inequality Aversion	Targeting	No Targeting	Targeting Gains
e=0.5	1.42	1.38	2.9%
e=2.0	5.05	4.56	10.7%
e=5.0	109.32	94.38	15.8%

Table 9 — Impact of Take-up On Distributional Power of Program

	Operational Errors		Definition of Take-up	
	Without	With	Second	Third
e=0.5	1.42	1.46	1.47	1.47
e=1.0	2.11	2.22	2.23	2.24
e=2.0	5.05	5.49	5.57	5.60
e=3.0	13.27	14.77	15.04	15.15
e=4.0	37.23	42.06	42.97	43.34
e=5.0	109.32	124.82	127.80	129.00

SECTION 6: TABLES

Table 1 — Monthly Education Subsidy Rates, July-Dec. 1999

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

Table 2—Number of Junior Secondary Schools In Evaluation States

	Treatment Communities	Control Communities	Other Communities	All Communities
YEAR				
1995	48	30	7689	7767
1996	51	31	7925	8007
1997	53	34	8260	8347
1998	55	35	8583	8673
1999	55	36	8851	8942

Table 3— Poverty Headcount Indices

	Non-Poor	Poor	Missing	Total
YEAR				
1997	6454	7551	0	14005
%	46.08	53.92	0.00	100.00
1998	3873	6174	410	10457
%	37.04	59.04	3.92	100.00
1999	4359	7455	792	12606
%	34.58	59.14	6.28	100.00

Source: Merged ENCASEH97 and ENCEL98/99 (Oct.-Nov.) samples.

Table 4 —Where PROGRESA Secondary School Children Go To School

	Treatment Communities	Control Communities	"Outside" Communities	All Communities
YEAR				
1997	1346	809	5351	7506
%	17.93	10.78	71.29	100.00
1998	1120	619	4402	6141
%	18.24	10.08	71.68	100.00
1999	1387	802	5213	7402
%	18.74	10.83	70.43	100.00

Source: Merged school-level data and panel of household-level data sets.

Table 5 —What Type of Secondary School Do PROGRESA Children Attend?

	Other	Technical Secondary	Television Secondary	Total
YEAR				
1997	333	459	6759	7551
%	4.41	6.08	89.51	100.00
1998	261	375	5538	6174
%	4.23	6.07	89.70	100.00
1999	316	531	6608	7455
%	4.24	7.12	88.64	100.00

Source: As for Table 4.

Table 6— Type of Secondary School In Treatment, Control and "Outside" Communities

	Treatment Communities	Control Communities	"Outside" Communities	Unmatched Children	Total
School Type					
Other	0	0	779	131	910
%	0.00	0.00	5.21	100.00	4.30
ST	198	0	1167	0	1365
%	5.14	0.00	7.80	0.00	6.44
TV	3655	2230	13020	0	18905
%	94.86	100.00	87.00	0.00	89.26
Total	3853	2230	14966	131	21180
%	100.00	100.00	100.00	100.00	100.00

Source: As for Table 4.

Table 7—Funding Source of PROGRESA Secondary Schools

	Federal	State	Private	Total
YEAR				
1997	3812	3561	133	7506
%	50.79	47.44	1.77	100.00
1998	3104	2963	74	6141
%	50.55	48.25	1.21	100.00
1999	3901	3501	0	7402
%	52.70	47.30	0.00	100.00

Source: As for Table 4.

Table 8— Enrolment Levels At PROGRESA Secondary Schools

	Treatment Communities	Control Communities	"Outside" Communities	Total
YEAR				
1997	52.2	52.3	84.2	75.0
1998	59.9	51.8	95.6	84.7
1999	67.9	59.3	110.3	96.8

Source: As for Table 4.

Table 9 — Enrollment Levels By Secondary School Type

	Treatment		Control	"Outside"		
	TV	ST	TV	TV	ST	Other
YEAR						
1997	51.38	68.00	52.27	65.08	208.99	224.58
1998	59.82	63.00	51.85	76.09	222.46	238.08
1999	68.72	57.00	59.27	78.23	251.40	424.92

Source: As for Table 4.

Table 10 — Characteristics of PROGRESA Schools

YEAR	Treatment Communities	Control Communities	"Outside" Communities	Total
Number of Teachers In PROGRESA Secondary Schools				
1997	2.3	2.3	4.0	3.5
1998	2.6	2.4	4.2	3.7
1999	3.0	2.5	4.6	4.1
Number of Classrooms In Use in PROGRESA Secondary Schools				
1997	2.5	2.4	3.4	3.1
1998	2.6	2.3	3.5	3.2
1999	2.7	2.6	3.8	3.5
Student-Teacher Ratios In Secondary Schools Attended By PROGRESA Children				
1997	22.73	22.62	22.40	22.48
1998	25.02	21.33	24.61	24.35
1999	22.25	23.61	25.00	24.34
Student-Classroom Ratio In Secondary Schools Attended By PROGRESA Children				
1997	20.97	22.61	22.95	22.56
1998	23.47	23.23	26.01	25.27
1999	25.16	22.66	26.69	25.96
Number of Multiple Classrooms in PROGRESA Secondary Schools				
1997	0.79	0.33	0.26	0.36
1998	0.45	0.42	0.14	0.23
1999	0.45	0.33	0.24	0.29
Percentage of Students that Fail 1-5 Classes in Secondary Schools				
1997	0.15	0.10	4.30	3.10
1998	1.07	0.03	6.08	4.56
1999	0.85	0.26	7.78	5.67

Table 11 — PROGRESA Evaluation Communities with Primary School within Community

	Evaluation communities	
	Within community	Outside community
YEAR		
1997	461	45
1998	464	42
1999	470	36

Table 12— Where PROGRESA Primary School Children Go To School

	Treatment communities		Control communities	
	Within community	Outside community	Within community	Outside community
YEAR				
1997	93.8%	6.2%	91.5%	8.4%
1998	93.8%	6.2%	92.4%	7.6%
1999	94.6%	5.4%	92.4%	7.6%

Source: School-level data merged to individual data.

Table 13— Student-Classroom Ratio In Primary Schools Attended By PROGRESA Children

	Treatment Communities	Control Communities	Total
YEAR			
1997	24.90	26.25	25.40
1998	23.63	25.87	24.50
1999	23.90	25.33	24.40

Table 14a— The Impact of PROGRESA on the School Enrollment in Secondary School

Difference in Difference Estimates (w.o. supply measures)

	Boys					Girls				
	Initial level	Impact				Initial level	Impact			
		Nov. 98		Nov. 99			Nov. 98		Nov. 99	
		coef.	t-stat	coef.	t-stat		coef.	t-stat	coef.	t-stat
Panel sample										
By sample of eligibles										
Initially classified-poor	0.653	0.079	3.120	0.053	1.830	0.528	0.117	4.450	0.120	3.700
Densified poor	0.668	0.070	3.240	0.052	2.000	0.549	0.103	4.330	0.100	3.480
Receiving payments	0.659	0.081	3.650	0.063	2.280	0.540	0.117	4.770	0.122	4.080
Pooled sample										
By sample of eligibles										
Initially poor	0.568	0.083	3.790	0.061	2.110	0.452	0.105	4.170	0.110	3.550
Densified poor	0.566	0.073	3.450	0.057	2.260	0.461	0.096	4.160	0.100	3.520
Receiving payments	0.562	0.074	3.370	0.059	2.200	0.451	0.110	4.540	0.115	3.920

Table 14b— The Impact of PROGRESA on the School Enrollment in Primary School

Difference in Difference Estimates (w.o. supply measures)

	Boys					Girls				
	Initial level	Impact				Initial level	Impact			
		Nov. 98		Nov. 99			Nov. 98		Nov. 99	
		coef.	t-stat	coef.	t-stat		coef.	t-stat	coef.	t-stat
Panel sample										
By sample of eligibles										
Initially classified-poor	0.919	0.019	2.330	0.022	2.820	0.927	0.021	2.740	0.018	2.220
Densified poor	0.924	0.018	2.350	0.021	2.990	0.927	0.018	2.350	0.017	2.210
Receiving payments	0.922	0.020	2.740	0.025	3.730	0.926	0.023	3.120	0.023	3.160
Pooled sample										
By sample of eligibles										
Initially poor	0.881	0.016	2.450	0.017	2.990	0.887	0.016	2.920	0.015	2.440
Densified poor	0.880	0.015	2.650	0.016	3.120	0.882	0.014	2.840	0.014	2.500
Receiving payments	0.880	0.016	2.840	0.019	3.880	0.881	0.017	3.480	0.017	3.620

Table 15a — The Impact of PROGRESA on the School Enrollment in Secondary School

Difference in difference estimates (with supply measures)

	Boys					Girls				
	Initial level	Impact				Initial level	Impact			
		Nov. 98		Nov. 99			Nov. 98		Nov. 99	
		coef.	t-stat	coef.	t-stat		coef.	t-stat	coef.	t-stat
Panel sample										
By sample of eligibles										
Initially classified-poor	0.653	0.082	3.610	0.056	1.930	0.528	0.124	4.640	0.131	3.940
Densified poor	0.668	0.071	3.390	0.054	2.060	0.549	0.108	4.540	0.106	3.540
Receiving payments	0.659	0.081	3.740	0.063	2.310	0.540	0.118	4.790	0.124	3.990
Pooled sample										
By sample of eligibles										
Initially poor	0.568	0.084	3.790	0.060	2.030	0.452	0.108	4.290	0.115	3.620
Densified poor	0.566	0.073	3.450	0.055	2.140	0.461	0.097	4.150	0.099	3.290
Receiving payments	0.562	0.076	3.480	0.062	2.330	0.451	0.109	4.480	0.114	3.670

Table 15b — The Impact of PROGRESA on the School Enrollment in Primary School

Difference in Difference Estimates (with supply measures)

	Boys					Girls				
	Initial level	Impact				Initial level	Impact			
		Nov. 98		Nov. 99			Nov. 98		Nov. 99	
		coef.	t-stat	coef.	t-stat		coef.	t-stat	coef.	t-stat
Panel sample										
By sample of eligibles										
Initially classified-poor	0.919	0.018	2.210	0.021	2.770	0.927	0.021	2.700	0.018	2.230
Densified poor	0.924	0.016	2.210	0.020	2.940	0.927	0.017	2.250	0.017	2.190
Receiving payments	0.922	0.019	2.610	0.024	3.720	0.926	0.022	3.060	0.022	3.200
Pooled sample										
By sample of eligibles										
Initially poor	0.881	0.015	2.370	0.018	3.010	0.887	0.016	2.880	0.014	2.440
Densified poor	0.880	0.014	2.560	0.016	3.160	0.882	0.014	2.750	0.014	2.490
Receiving payments	0.880	0.016	2.750	0.019	3.960	0.881	0.016	3.400	0.017	3.670

Table 16a — The Impact of Indicators of School Quality and Supply on Enrollment in Secondary School

	Boys						Girls					
	Pobre1		Pobreden		Pobre3		Pobre1		Pobreden		Pobre3	
Panel sample												
Distance to Sec. School (km)	-0.075	-6.660	-0.079	-7.350	-0.080	-7.320	-0.112	-8.130	-0.109	-8.470	-0.111	-8.280
Distance squared	0.004	3.560	0.004	3.740	0.004	3.650	0.006	3.330	0.006	3.230	0.006	3.140
Available school tele-secondary school	-0.106	-1.850	-0.108	-2.220	-0.110	-2.100	-0.142	-2.820	-0.149	-3.380	-0.154	-3.340
% of teachers with at least HS degree	0.026	0.360	0.029	0.480	0.050	0.760	0.174	2.470	0.157	2.290	0.185	2.560
% of students failing last year	0.024	0.130	-0.030	-0.190	0.022	0.130	-0.226	-1.270	-0.291	-1.710	-0.265	-1.470
Pooled sample												
Distance to sec. school	-0.085	-7.540	-0.087	-8.080	-0.090	-8.240	-0.113	-8.430	-0.110	-8.690	-0.112	-8.650
Distance squared	0.005	4.530	0.005	4.580	0.005	4.710	0.007	3.720	0.007	3.620	0.007	3.600
Available school tele-secondary school	-0.106	-1.800	-0.106	-2.090	-0.105	-2.010	-0.129	-2.580	-0.138	-3.180	-0.145	-3.160
% of teachers with at least HS degree	0.067	0.900	0.062	0.930	0.069	1.050	0.138	2.060	0.118	1.840	0.145	2.080
% of students failing last year	-0.013	-0.070	-0.030	-0.200	0.024	0.150	-0.130	-0.770	-0.247	-1.590	-0.233	-1.390

Table 16b — The Impact of Indicators of School Quality and Supply on Enrollment in Primary School

	Boys						Girls					
	Pobre1		Pobreden		Pobre3		Pobre1		Pobreden		Pobre3	
Panel sample												
Primary school located in community	-0.009	-0.640	-0.013	-1.070	-0.016	-1.250	-0.0024	-0.260	-0.004	-0.420	-0.0028	-0.030
Student teacher ratio	-0.0008	-2.470	-0.0008	-3.130	-0.0010	-3.640	-0.0006	-2.330	-0.0009	-3.140	-0.0009	-3.390
Pooled sample												
Primary school located in community	-0.006	-0.560	-0.077	-0.820	-0.090	-8.240	-0.007	-0.960	-0.009	-1.160	-0.112	-8.650
Student teacher ratio	-0.0005	-1.980	-0.0005	-2.590	-.009	-0.96	-0.0004	-1.980	-.0006	-2.77	-0.0006	-2.79

Table 17—The Impact of PROGRESA on Continuation Rates Versus Return Rates: Enrollment in Secondary School
(with supply measures)

	Boys Impact				Girls Impact			
	Nov. 98		Nov. 99		Nov. 98		Nov. 99	
	coef.	t-stat	coef.	t-stat	coef.	t-stat	coef.	t-stat
Panel sample								
Enrolled in school prior to program	**0.086**	**4.060**	**0.071**	**3.580**	**0.107**	**4.800**	**0.111**	**5.240**
Out of school prior to program	0.054	1.350	0.004	0.100	**0.136**	**3.450**	0.057	1.510
Pooled sample								
Enrolled in school prior to program	**0.081**	**3.920**	**0.066**	**3.290**	**0.107**	**4.870**	**0.114**	**5.370**
Out of school prior to program	**0.067**	**1.680**	0.013	0.360	**0.122**	**3.260**	0.051	1.340

Note: Impact estimates use pobre1 as indicator.

Table 18— Impact of Education Grants on Extra Years of Primary and Secondary Education For Boys and Girls

Difference Estimates		Boys Conditional Enrollment				Girls Conditional Enrollment			
		Before	Impact	After	ExtraYrs	Before	Impact	After	ExtraYrs
	Grade								
Primary	3	0.900	0.001	0.901	1.408	0.912	0.024	0.936	24.062
	4	0.852	0.040	0.892	37.461	0.872	0.025	0.897	44.277
	5	0.866	0.024	0.889	51.528	0.857	0.047	0.903	77.210
	6	0.850	0.029	0.878	64.271	0.850	0.053	0.903	105.768
	Total				154.667				251.316
Secondary	7	0.345	0.109	0.455	109.406	0.265	0.104	0.369	103.673
	8	0.903	-0.003	0.900	97.464	0.895	0.004	0.899	94.277
	9	0.866	0.004	0.869	85.845	0.879	0.014	0.893	87.504
	Total				292.714				285.453

Difference-In-Difference Estimates		Boys Conditional Enrollment				Girls Conditional Enrollment			
		Before	Impact	After	ExtraYrs	Before	Impact	After	ExtraYrs
	Grade								
Primary	3	0.900	0.008	0.908	7.663	0.912	0.010	0.922	9.999
	4	0.852	0.007	0.859	13.113	0.872	0.010	0.882	17.543
	5	0.866	0.007	0.873	17.099	0.857	0.009	0.866	22.669
	6	0.850	0.007	0.857	19.451	0.850	0.009	0.859	25.824
	Total				57.326				76.035
Secondary	7	0.345	0.093	0.438	92.806	0.265	0.179	0.444	178.779
	8	0.903	0.000	0.903	83.775	0.895	0.000	0.895	160.003
	9	0.866	0.000	0.866	72.543	0.879	0.000	0.879	140.712
	Total				249.124				479.493

Table 19a — Effect of Decreasing Distance on Enrollment (Allocated over All Grades)

		Enrollment			Extra Years of Education		
		Before	Impact98	Impact99	1997-8	1998-9	1997-9
	Grade						
Girls							
	7	0.265	0.00474	0.00276	4.74	2.76	7.50
	8	0.895	0.00474	0.00276	5.52	3.24	8.76
	9	0.879	0.00474	0.00276	6.01	3.54	9.55
Total					16.27	9.54	25.80
Boys							
	7	0.345	0.00202	0.00244	2.02	2.44	4.46
	8	0.903	0.00202	0.00244	4.82	3.16	7.99
	9	0.866	0.00202	0.00244	5.27	3.41	8.68
Total					12.12	9.01	21.13

Table 19b— Effect of Decreasing Distance on Enrollment (Allocated to Transition Year)

		Enrollment			Extra Years of Education		
		Before	Impact98	Impact99	1997-8	1998-9	1997-9
	Grade						
Girls							
	7	0.265	0.007	0.004	7.12	4.15	11.27
	8	0.895	0.000	0.000	6.37	3.71	10.08
	9	0.879	0.000	0.000	5.60	3.26	8.87
Total					19.10	11.12	30.22
Boys							
	7	0.345	0.003	0.004	3.35	4.04	7.39
	8	0.903	0.000	0.000	6.43	3.74	10.17
	9	0.866	0.000	0.000	5.52	3.21	8.73
Total					15.29	11.00	26.29

Table 20a—Cost of Extra Years of Education (Difference Estimates Using Means)

	Primary			Secondary		
	Boys	Girls	Average	Boys	Girls	Average
Total Enrollment	3049	3221	3135	1220	996	1108
Total Impact	155	251	203	293	285	289
Grants	3720237	3949229	3834733	3289657	2945725	3117691
Cost Per Year	24053	15714	19884	11238	10319	10779

Table 20b—Cost of Extra Years of Education (Using D-in-D Regression Estimates)

	Primary			Secondary		
	Boys	Girls	Average	Boys	Girls	Average
Total Enrollment	2951	3046	2999	1176	1190	1183
Total Impact	57	76	67	249	479	364
Grants	3575389	3694970	3635179	3172063	3517206	3344635
Cost Per Year	62370	48596	55483	12733	7335	10034

Table 21 —Number of New Schools in Evaluation Sample

School Type	Number of Secondary Schools			Number of New Schools	
	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
No. 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.

Table 22— Cost of School Construction (Pesos)

Item	Telesecondary	Technical Secondary
Personnel	169624	426356
Operating Costs	302	718
Furniture+Equipment	20576	44771
Infrastructure	1360000	2400000
Total	1550502	2871845

Table 23a — Cost Effectiveness Ratios For School Building (Difference Estimates)

	r=0%			r=5%		
	20 Years	30 Years	40 Years	20 Years	30 Years	40 Years
Girls 1997-8	126292	115624	110291	145648	135925	131590
Girls 1998-9	345648	320009	307189	392171	368802	358383
Girls 1997-9	207354	191154	183054	236750	221984	215401
Boys 1997-8	169577	155254	148092	195568	182512	176691
Boys 1998-9	365776	338644	325078	415008	390278	379252
Boys 1997-9	253260	233474	223581	289164	271130	263088
Avg. 1997-8	147934	135439	129191	170608	159219	154141
Avg. 1998-9	355712	329326	316133	403590	379540	368817
Avg. 1997-9	230307	212314	203317	262957	246557	239244

Table 23b — Cost Effectiveness Ratios For School Building (D-in-D Estimates)

	r=0%			r=5%		
	20 Years	30 Years	40 Years	20 Years	30 Years	40 Years
Girls 1997-8	107567	98482	93939	124054	115773	112080
Girls 1998-9	296365	274381	263390	336255	316218	307284
Girls 1997-9	177045	163213	156297	202144	189536	183915
Boys 1997-8	134323	122977	117304	154910	144569	139958
Boys 1998-9	299623	277398	266285	339951	319694	310662
Boys 1997-9	203474	187577	179628	232319	217830	211369
Avg. 1997-8	120945	110729	105621	139482	130171	126019
Avg. 1998-9	297994	275890	264837	338103	317956	308973
Avg. 1997-9	190259	175395	167962	217232	203683	197642

SECTION 5: FIGURES

Figure 1— Kernel Density For Monthly Per Adult Equivalent Consumption (1994 prices)

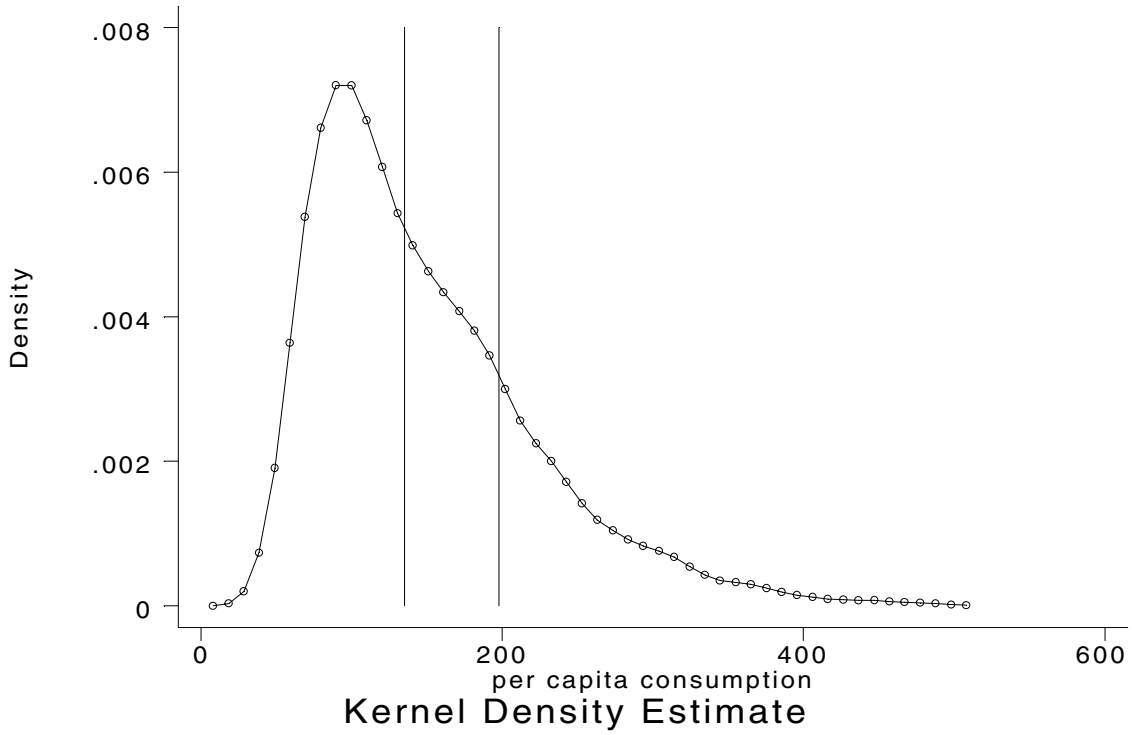


Figure 2 — Pre- and Post-Densification Targeting Errors

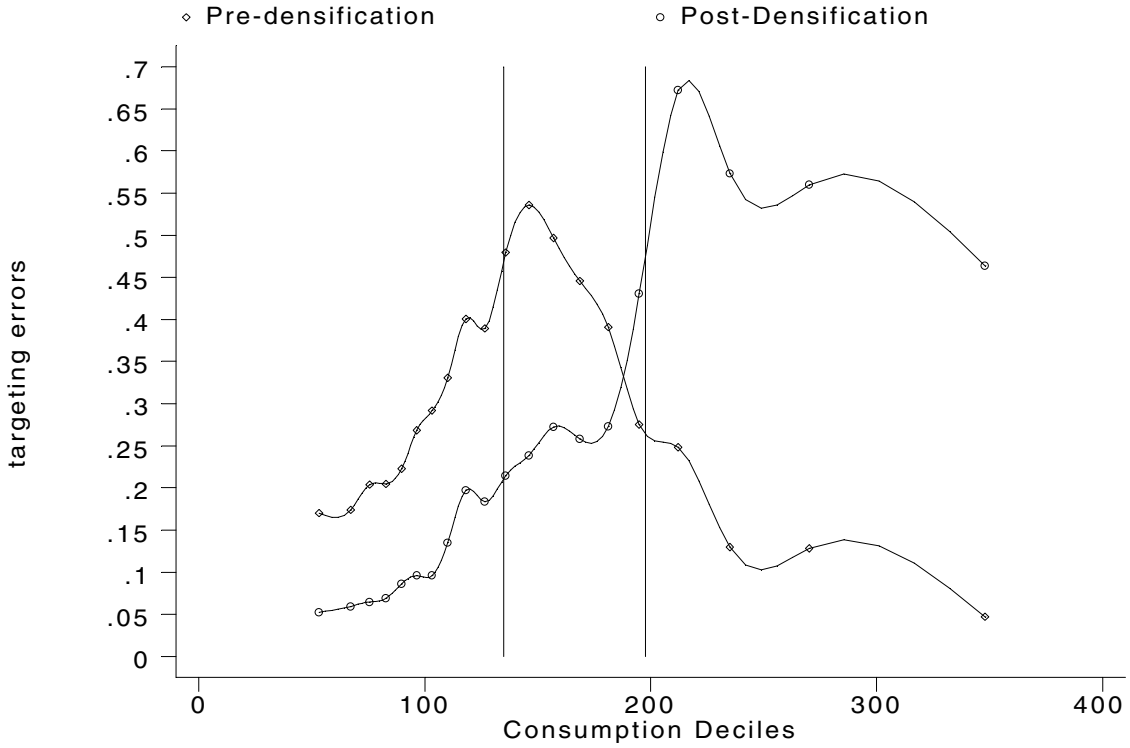
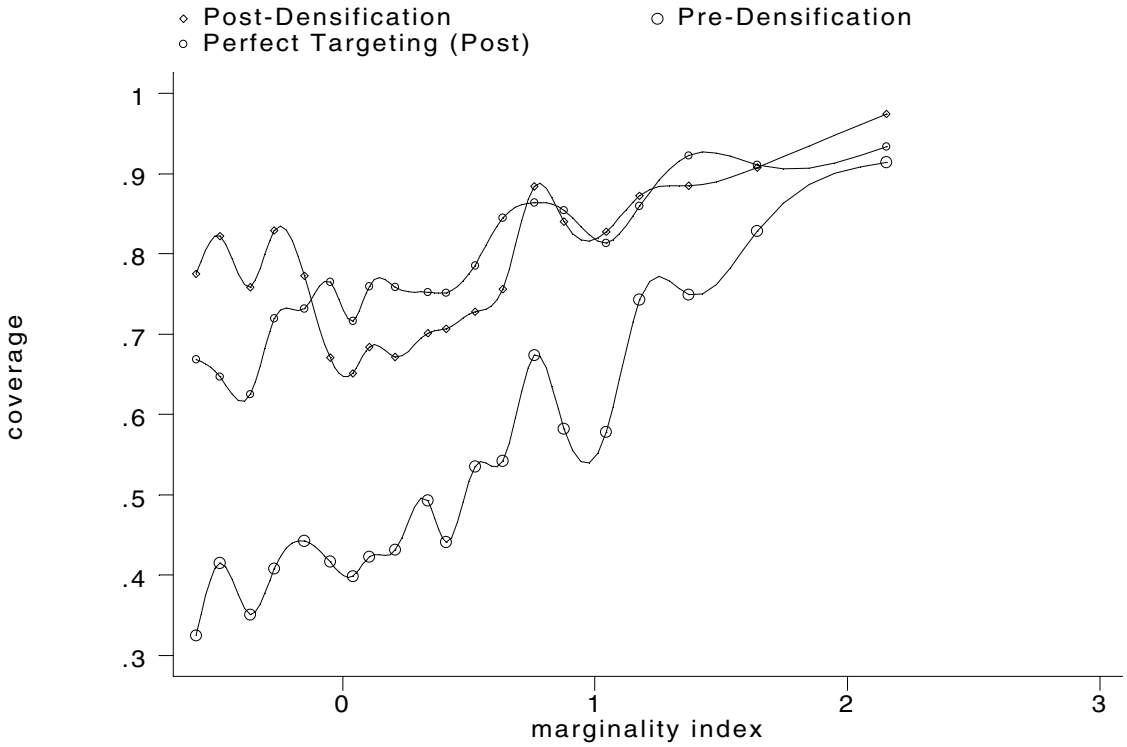


Figure 3 — Poverty Headcount Rates Across Localities



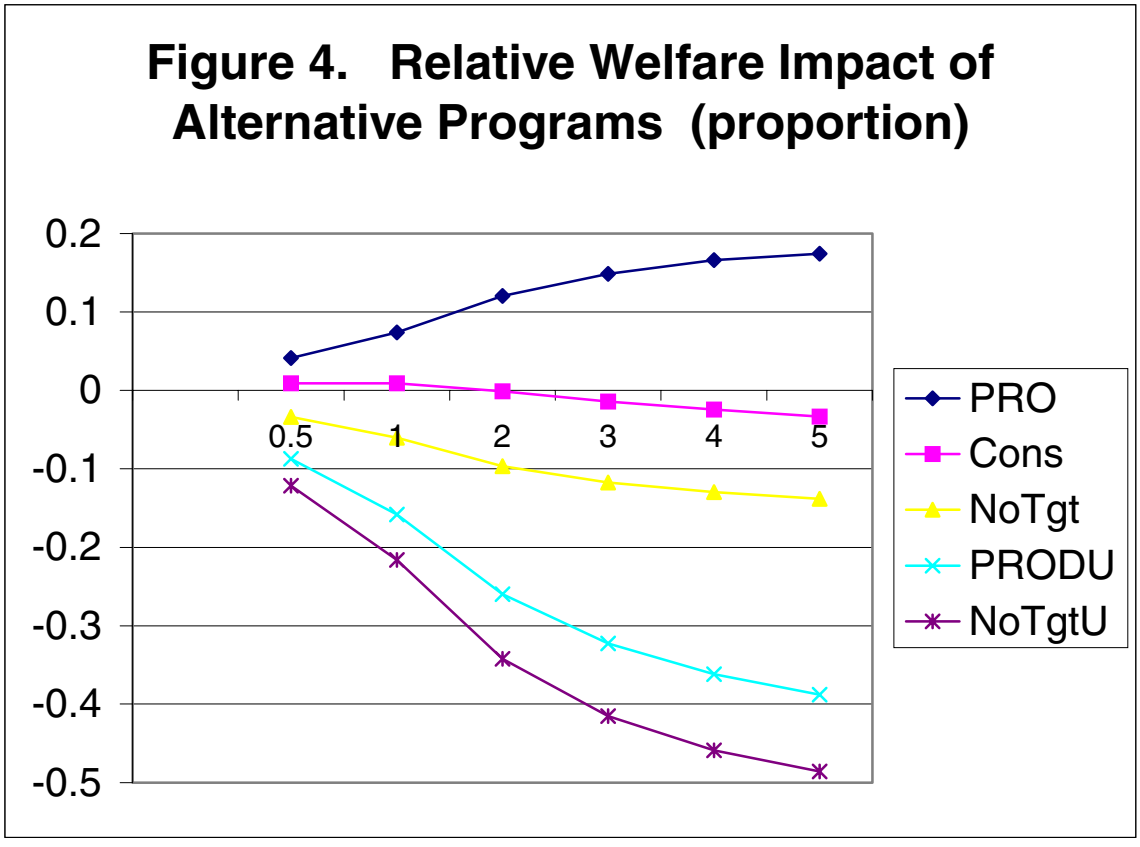


Figure 5 — Proportion of Capped Households By Consumption Group

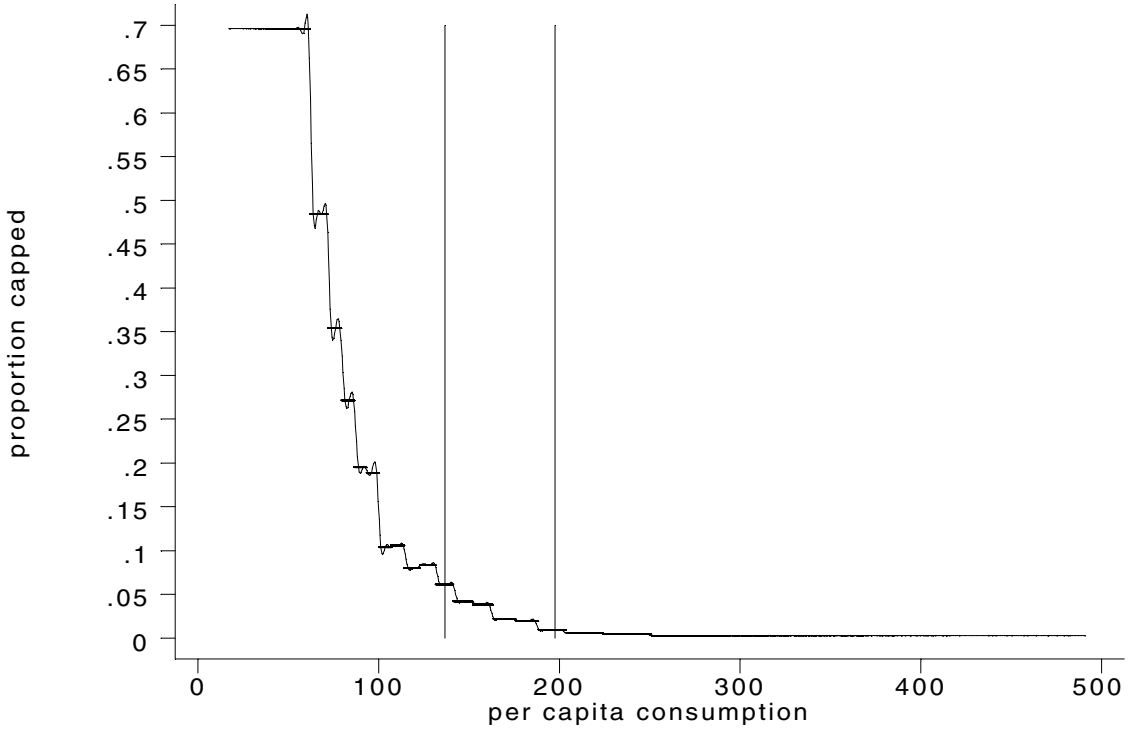


Figure 6— Average Level of Capped and Uncapped benefits By Consumption Group

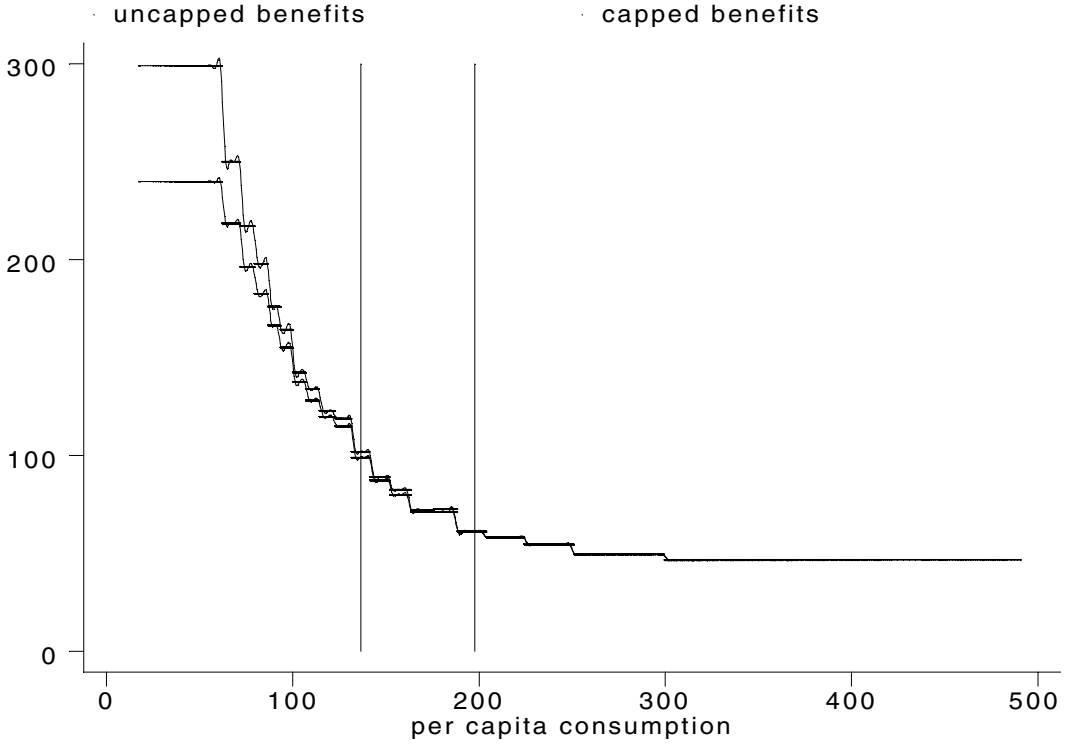


Figure 7a. Lambdas For Uncapped Components

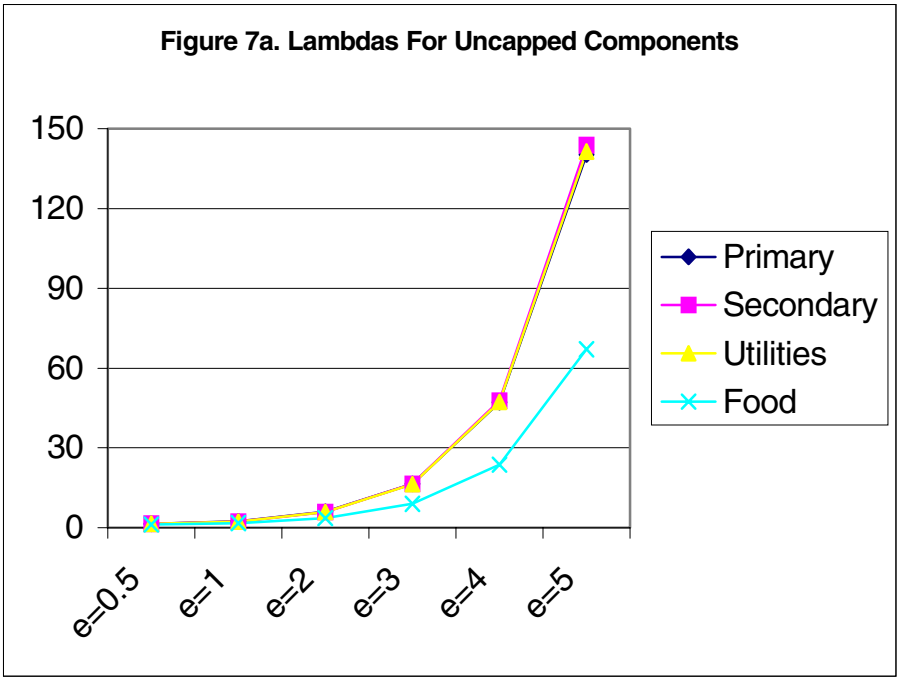


Figure 7b. Lambdas For Capped Components

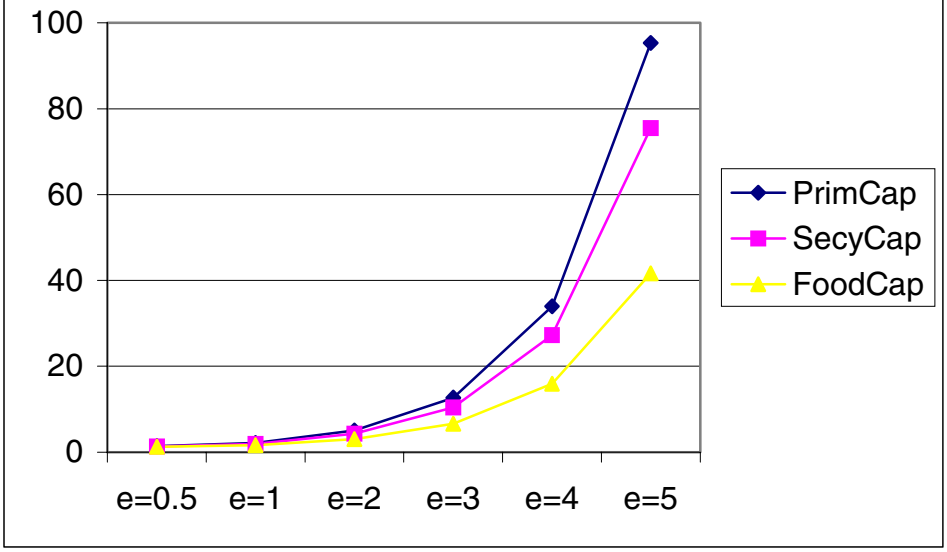


Figure 8 — Locality Distributional Characteristics and Consumption (e=2)

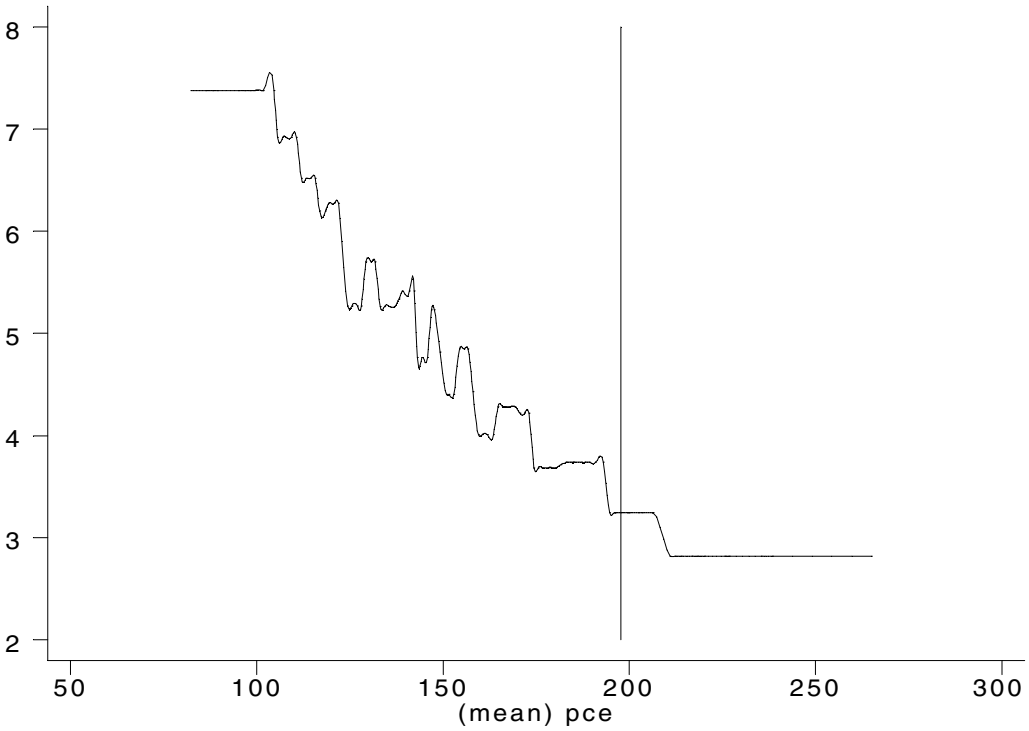


Figure 9 — Locality Distributional Characteristics (e=2) and Marginality Index

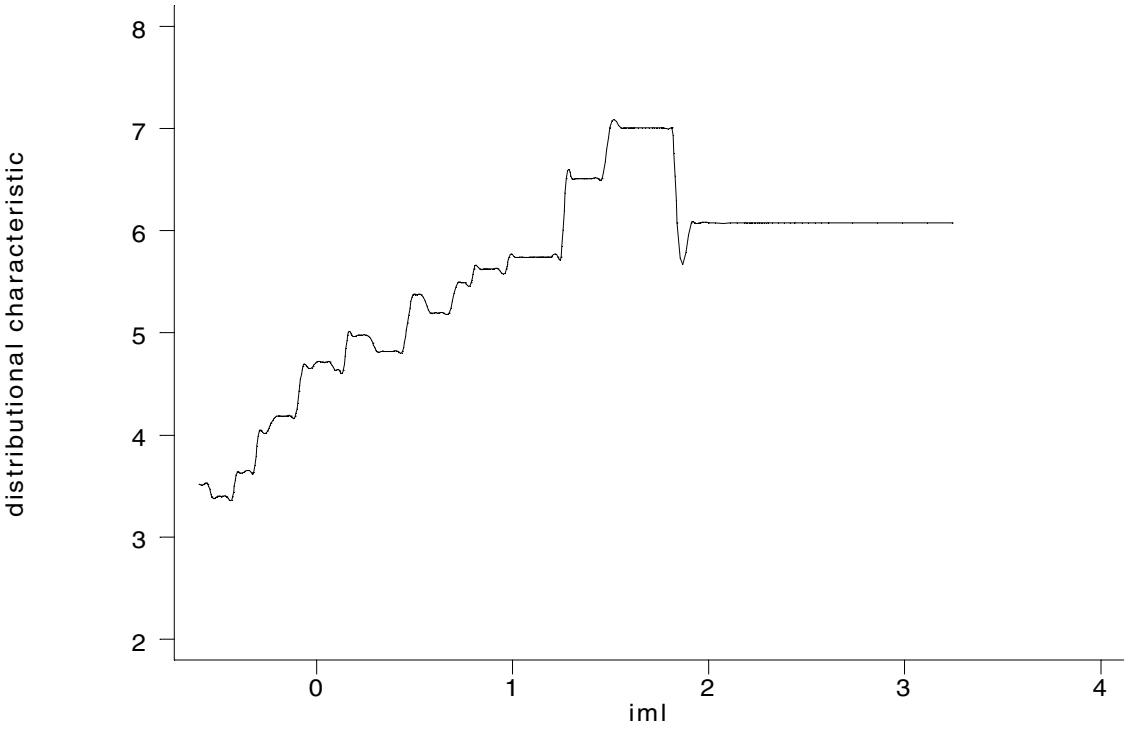


Figure 10.— Kernel Density of Gains From Targeting Across Localities (Actual, e=2)

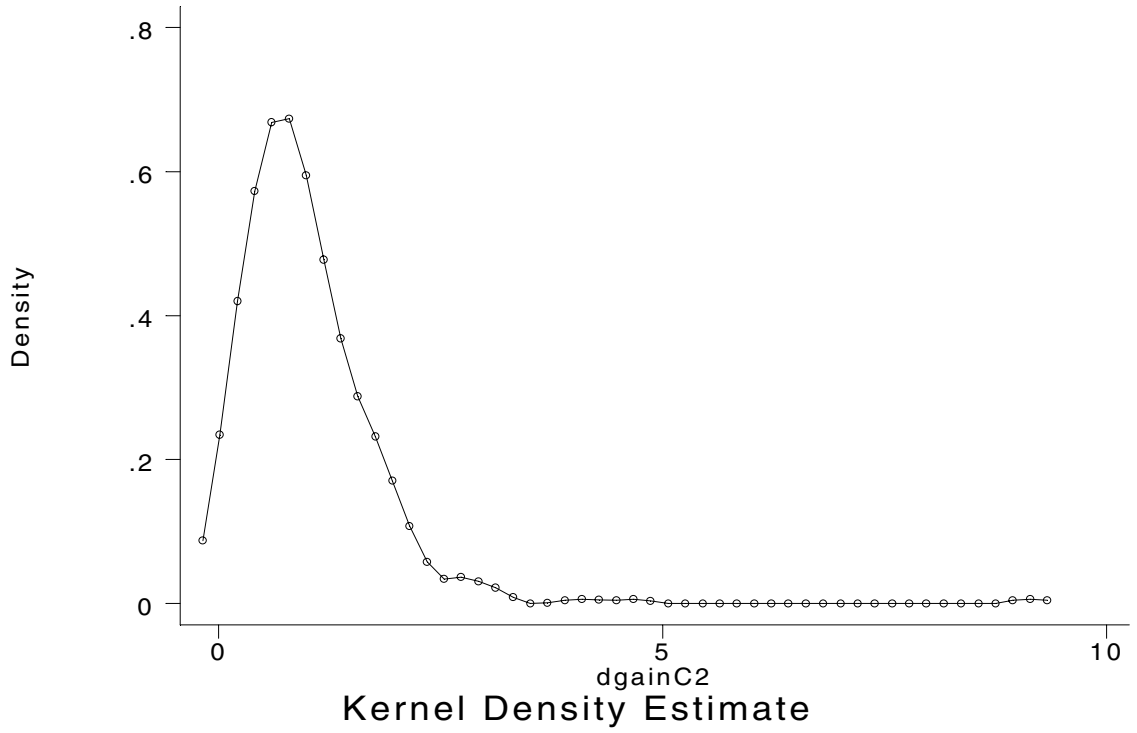


Figure 11 — Kernel Density of Gains From Targeting Across Localities (Consumption, e=2)

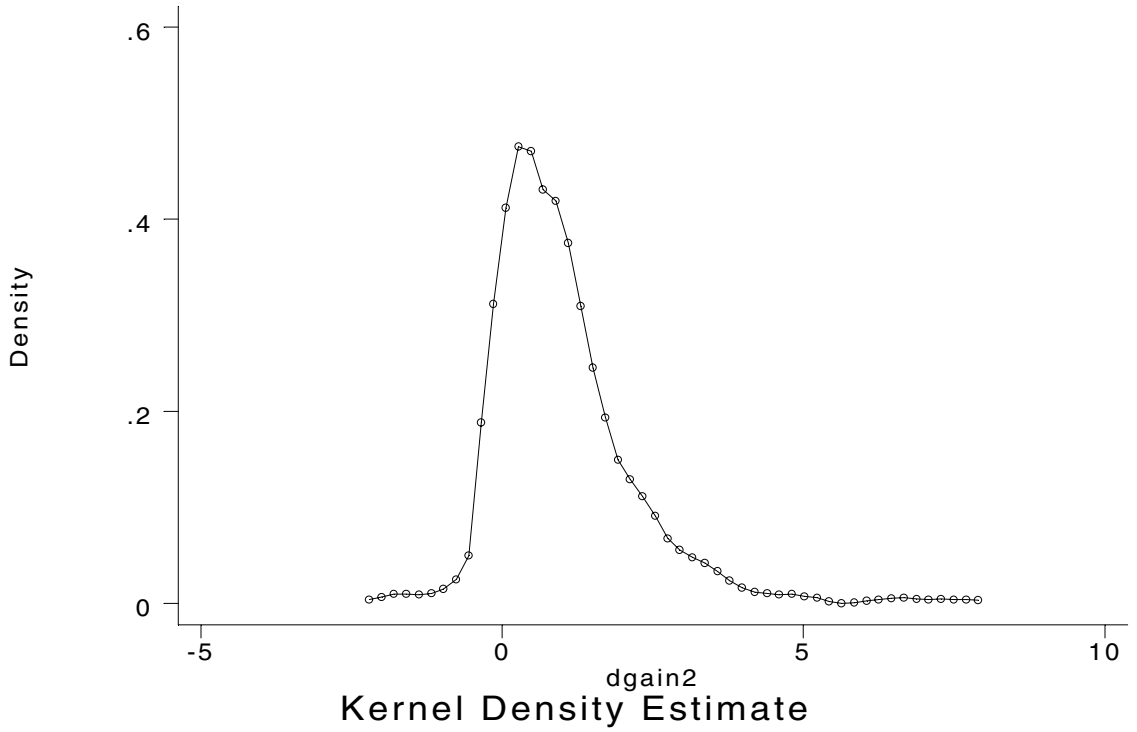


Figure 12—Welfare Gains From Household Targeting Across Locality Consumption (e=2)

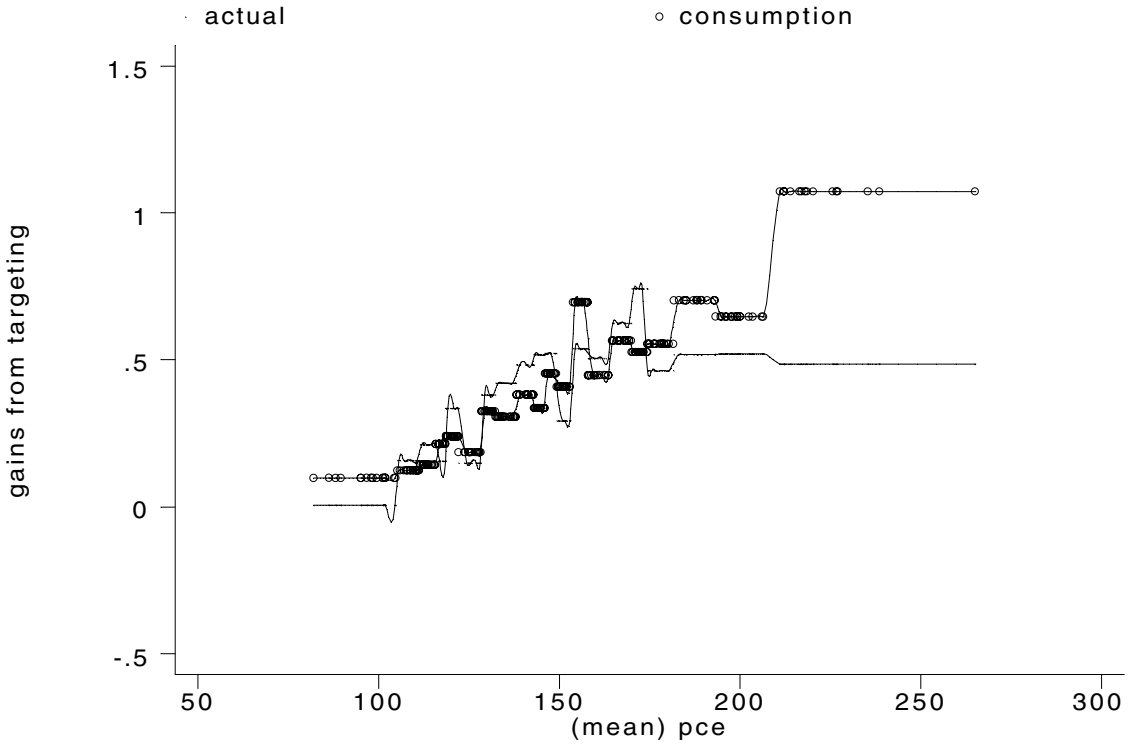


Figure 13— Welfare Gains From Household Targeting Across Locality Marginality (e=2)

Figure 14 — Welfare Gains From Household Targeting Across Locality Coverage (Actual, e=2)

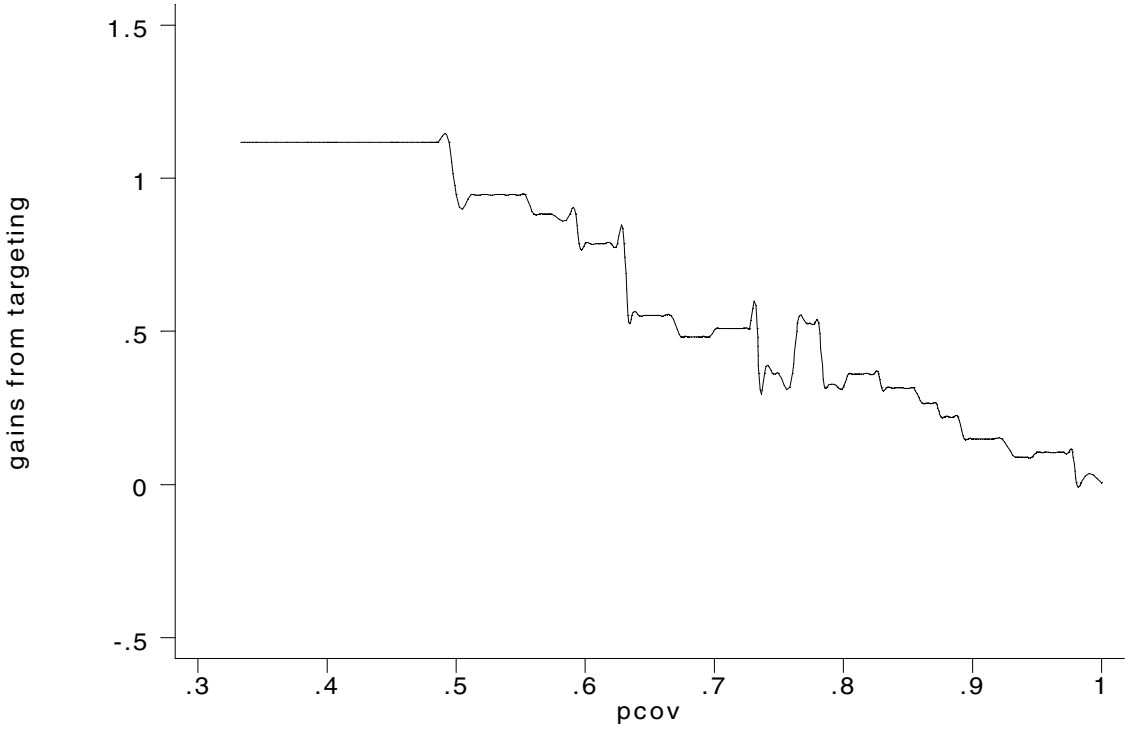


Figure 15— Welfare Gains From Household Targeting Across Locality Coverage (Consm, e=2)

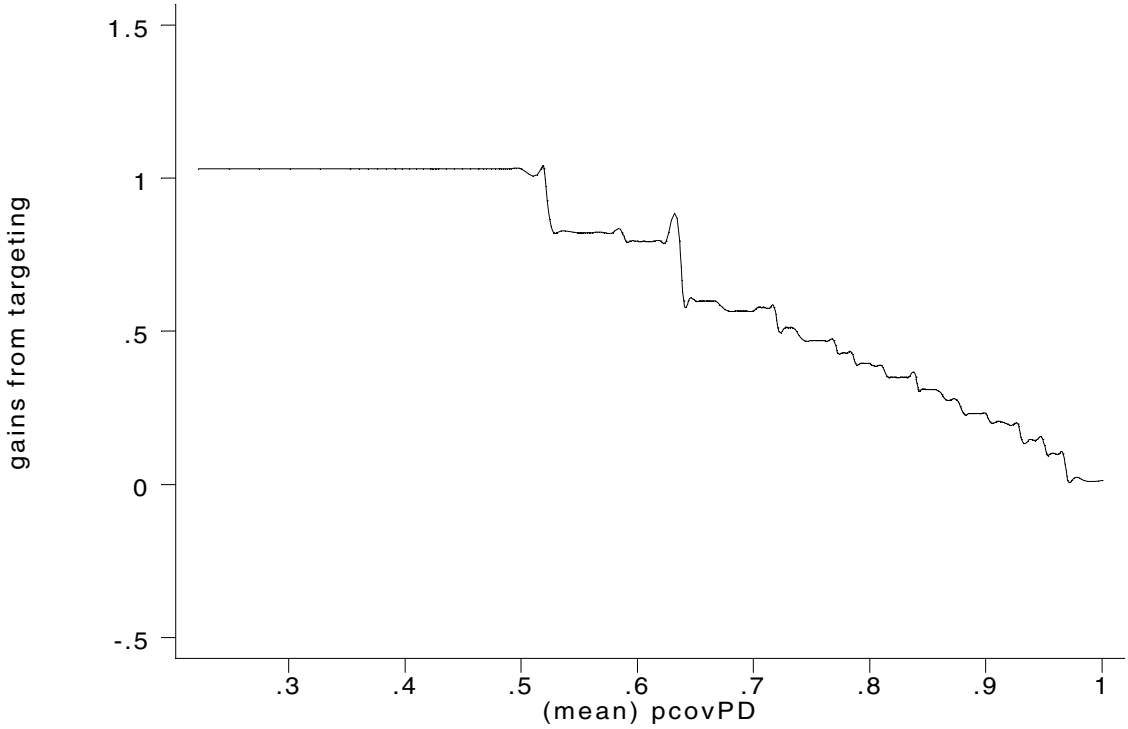


Figure 16—Welfare Gains From Targeting With/Out Program Costs (Actual Program)

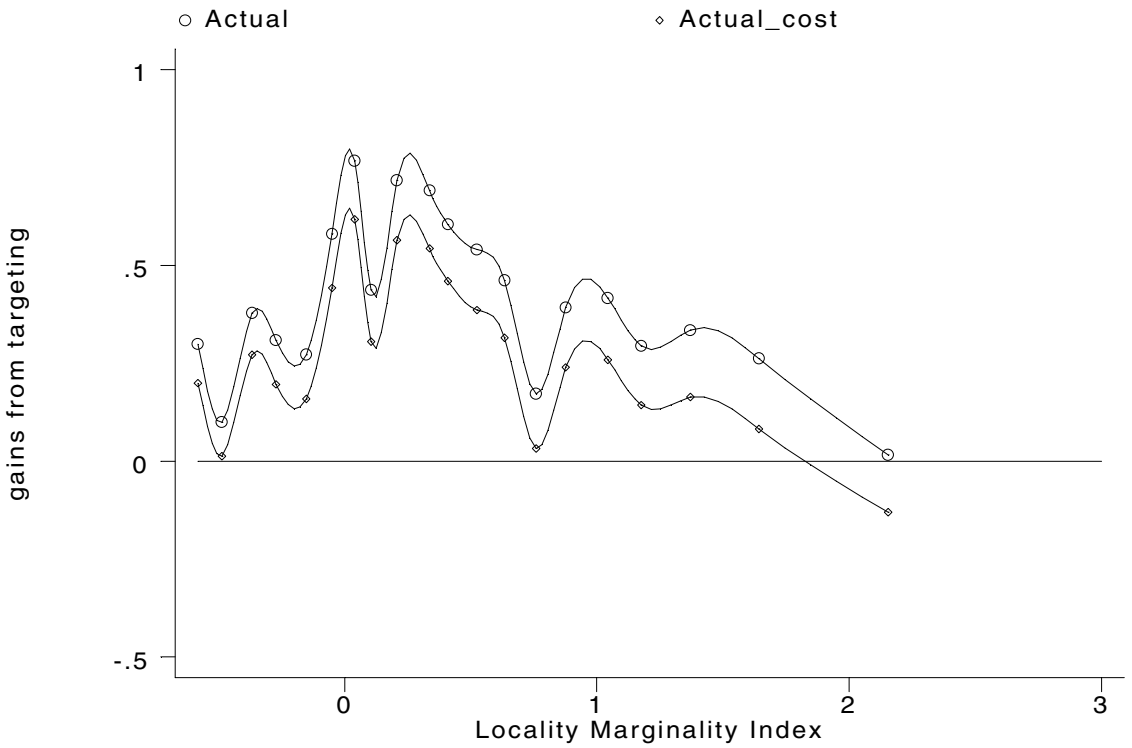


Figure 17— Welfare Gains From Targeting With/Out Program Costs (Perfect Targeting)

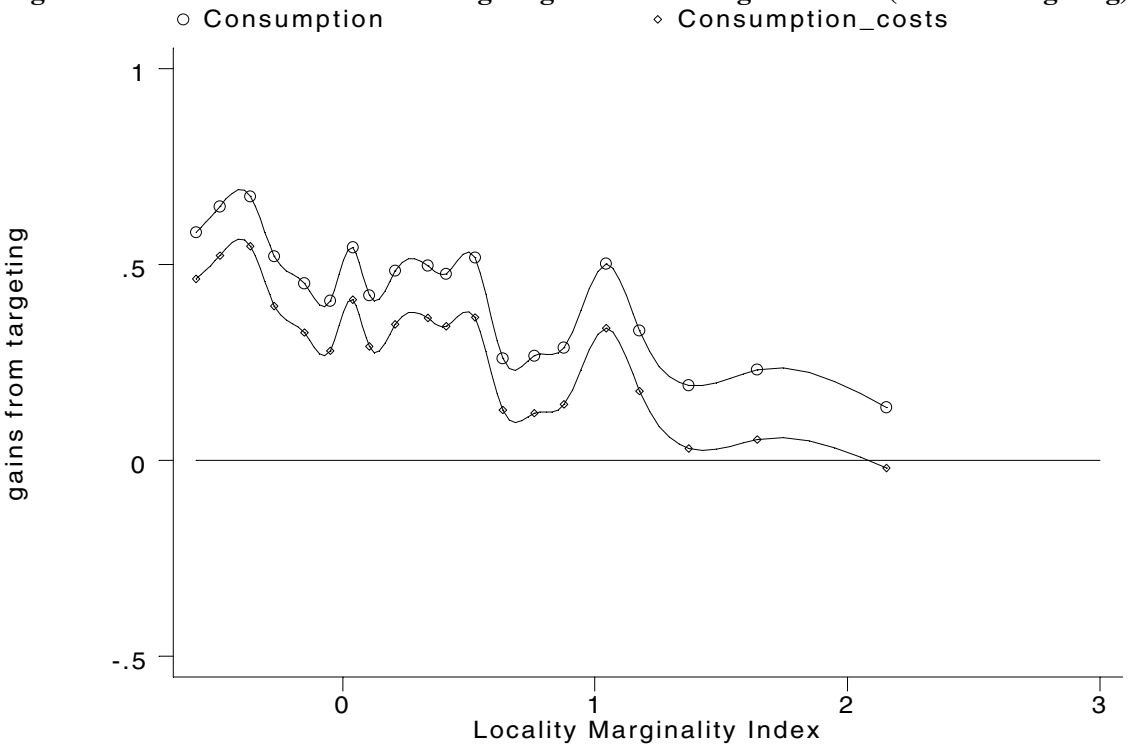
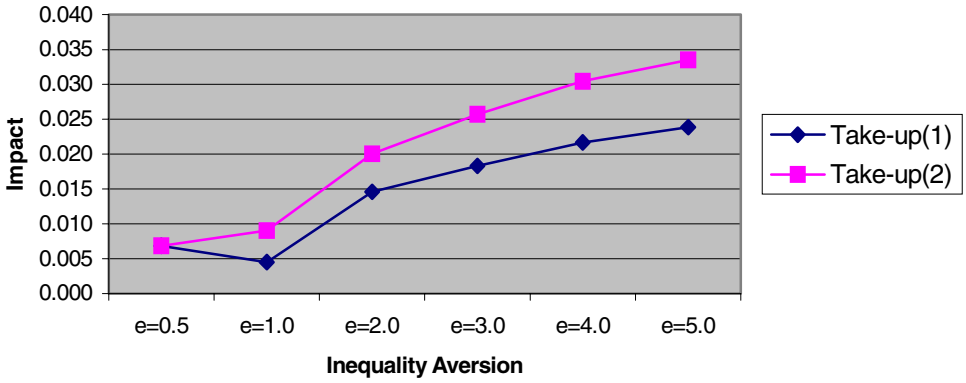


Figure 18. Impact of Take-up On Distributional Power of Program



SECTION 6: FIGURES

Figure 1. Enrollments Rates of Poor and Non-poor By Age Before Program

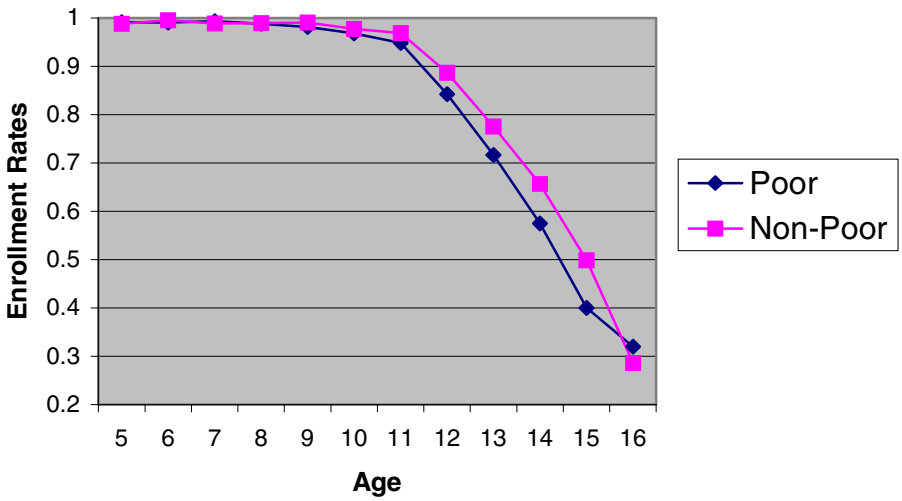


Figure 2. Enrollment Rates for Girls and boys Before Program By Age

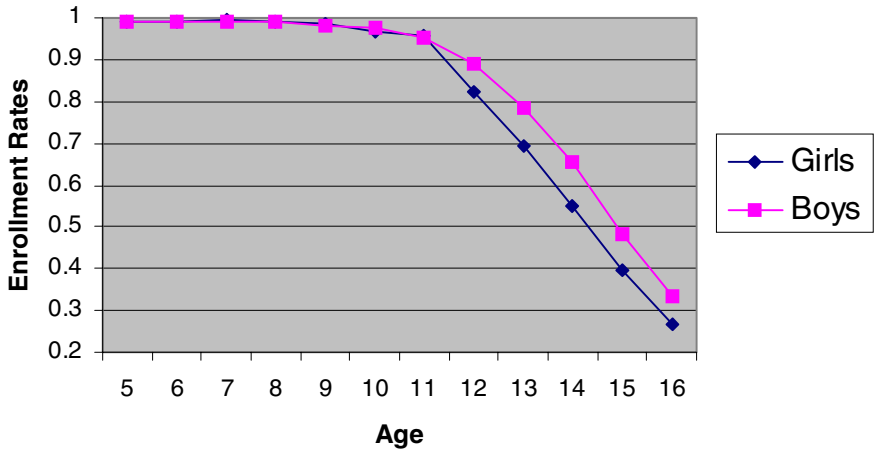


Figure 3. Enrollment Rates Treatment vs. Control By Grade For Girls 1998

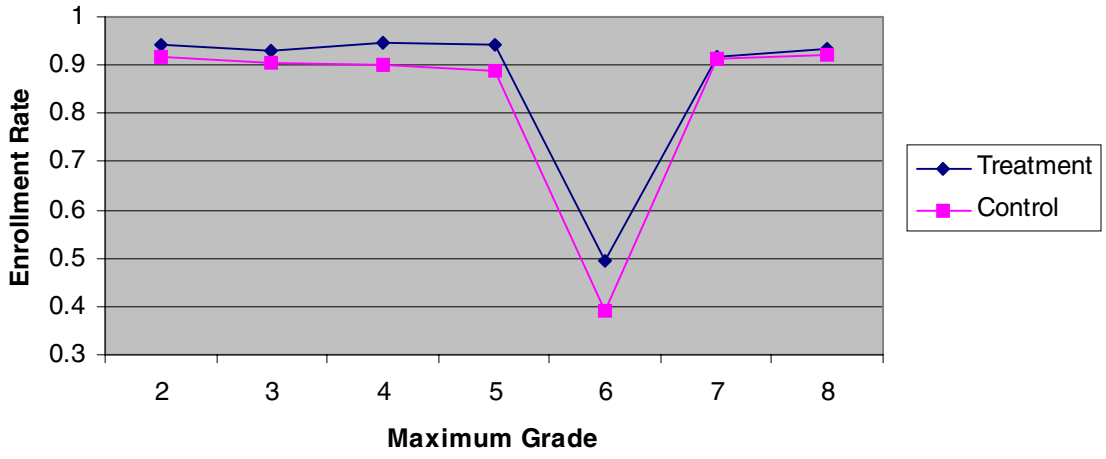
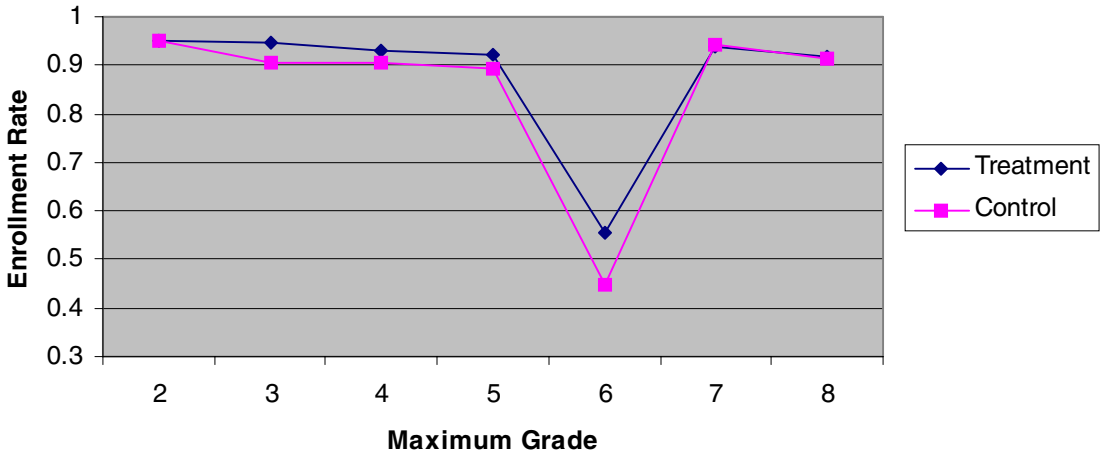


Figure 4. Enrollment Rates Treatment vs. Control By Grade For Boys 1998



Graph 2a: Years of completed schooling by age and gender: prior to program implementation