

Medium-run effects of *Oportunidades* on  
Consumption in rural areas

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## Executive Summary

This paper attempts to study the short and medium run effect of the *Oportunidades* program on consumption and its components in rural areas. To do so, it uses the new rural evaluation sample collected by *Oportunidades* (formerly called PROGRESA) in 2003. The program aims to foster the accumulation of human capital and therefore reduce poverty in the long run, while at the same time alleviating poverty in the short run. The intervention has health, nutrition and education components and is targeted to poor household living in rural area. Distinctive characteristics of the program include: the conditionality of payments, direct household-level targeting, multisectoral focus and provision of benefits exclusively to mothers. A major objective of the program is to improve the diet of poor rural households.

PROGRESA had been the object of a major evaluation effort which involved the collection of 6 waves of data in 506 villages. The distinctive feature of the original evaluation data set was the fact that the program had been started later in a randomly selected subset of 186 villages. The last of these waves was collected in 2000, at just about the time the control group was incorporated in the program. In this paper we use a new wave of the data, collected in 2003. The 2003 sample is made of three groups of localities. The first group is made of the 320 localities in which the program was started in 1998, the second group of the 186 localities where the programme was started in 2000. These first two groups are the same as in the original sample. As both groups were in the programme in 2003, it was decided to add a new group of localities. The third group is made of 151 localities where the programme does not operate as of 2003. Obviously the allocation of the programme between the first two groups and the third is not random. Indeed, even though the third group of localities was judiciously chosen among rural localities where the program was not operating so to find localities that were reasonably similar to those that composed the original evaluation sample, the very targeting mechanism that governed the expansion of the programme made the task of finding 'similar' control localities very arduous.

The differences between the original sample and the new additional controls are compounded by the fact that data were not collected for these new localities in previous years (for which instead information does exist for the original sample). While for many variables, including important outcomes such as school enrolment it is possible to use retrospective information to reconstruct 'baseline' information for the new control

households, this is not feasible for consumption. This makes the comparison between the new and original sample very arduous.

In the paper we perform three exercises. First, we compare the new control with the original sample (including the communities where the programme started in 1998 and those where it started in 2000) in terms of several observable (background) variables. The evidence that emerges from this analysis is that while for locality level variables there are not many significant differences (with some noticeable exception), for individual level variables it seems that the new control is made of households that are, on average, better off than those in the original sample. While the fact that we do not find large differences among village level variables is probably a consequence of the effort of selecting comparable localities, the large differences at the individual level are consistent with the nature of the targeting process of PROGRESA and then *Oportunidades*.

The second exercise we perform is to compare consumption expenditure and its components between the 'treatment' and 'control' sample, where in the former we include both the set of localities in which the programme started in 1998 and those in which it started in 2000. In doing this comparison, we control for differences in observable variables in various ways. In particular, we control for a large number of individual specific and village level variables by Propensity Score Matching methods and by full interacted linear matching. The results we obtain with this exercise are somewhat implausible. Taken at face value, the estimates yield a negative impact albeit not significantly different from zero. Such results contrast sharply with the results of the first evaluation of PROGRESA and are clearly questionable. The most credible interpretation of these results is that, in the absence of a baseline survey that could be used to control for unobservable pre-existing differences between treatment and control communities, it is impossible to conclude much from the simple comparison at a point in time in terms of the effectiveness of the programme.

The third exercise we perform consists in evaluating the medium run effect of the programme on consumption and its components. To do this we do two things. First, we compare eligible households in villages where the programme was initiated in 1998 to eligible households living in communities where it was initiated in 2000. That is we compare households that have been exposed to the programme for 5.5 years to households that have been exposed for three years. Given that the initial allocation between the two groups was random, the comparison is relatively straightforward. We control for observables in a linear fashion in an attempt to improve efficiency. This

comparison gives rise to very small differences: the only significant ones are observed for education and children clothing (positive) and alcohol and tobacco (negative). The absence of large differences for food is consistent with the fact that the effect on these items is immediate and does not change much with exposure to the programme. We also check whether we find any significant additional exposure effects on durables and assets and we do not observe much, with the noticeable exception of availability of running water and on the probability of owning a car.

The second part of the third exercise consists in exploiting the remarkable variability in the number and amounts of payments received by the households in the sample. After showing some simple evidence that relates the outcomes of interest to a polynomial in the number of payments (or in the amounts) we take into account the fact that much of the variability in payments might be endogenous, as it is related to household choices. We therefore use an Instrumental Variable (IV) approach in which we instrument the amounts paid using the randomization between early and late starters. We find that for some of the outcomes of interests, notably education and children clothing, greater exposure leads to larger effects.

## 1. Introduction

The *Oportunidades* programme, formerly known as PROGRESA, gives, under certain conditions, such as children's enrolment and regular attendance in school and young children enrolment in growth and development check ups and attendance to informal courses by the mothers, a relatively large cash transfer to poor families. While in some situations *Oportunidades* replaces pre-existing transfers (for instance, see Skoufias, 2001 p. 8), it might be expected to affect consumption levels and composition. However, what one should expect is not completely obvious. While the beneficiaries of the program are very poor and are not saving much, so that one would expect them to consume most of the subsidy, the conditions imposed by the program itself make the effect on disposable income not completely obvious. As we detail below, the education subsidy might lead to a reduction of income coming from child labour. It is therefore important to establish the extent to which the subsidy is translated into an amount on household consumption. Moreover, by its very nature, the programme is likely to affect the composition of consumption. First and foremost, the conditionalities imposed by the programme imply some expenditures (namely on education and transport). Second, even ignoring the conditionalities, an increase in total expenditure is likely to be allocated differently across different commodities. Lastly (but by no means the least) the fact that the grant is to a large extent controlled by the woman in the family, might have an effect on the way it is spent. A second important research topic, therefore, is the extent to which the programme changes the pattern of consumption expenditure. This is extremely important in evaluating the overall effectiveness of the programme. As *Oportunidades* is supposed to foster the accumulation of human capital through health, nutrition and education, it is important to check how the additional resources received by poor households are spent, whether children do end up consuming a large fraction of them and whether some of them leak towards the consumption of adult commodities.

In this paper we study the consumption patterns induced by the *Oportunidades* program in the rural communities where it operates. Our main aim is to identify the medium run effects of the program on consumption and its components. However, the task is not an easy one because of the structure of the available data.

The evaluation of *Oportunidades*, previously known as PROGRESA, was started when the program started in 1997. The well known evaluation exploited the expansion phase of the program and the fact that the timetable for its start in some localities was randomized for the purpose of the evaluation. In particular, between 1997 and 1998 a data collection

exercise was started in 506 localities targeted by the programme. Of these, in 320 randomly chosen localities, the programme was started in early 1998, while the remaining 186 were put, so to speak, at a later stage of the expansion phase so that the program did not start until 2000. After the initial survey in August 1997 (the pre-program Survey of Rural Household Socioeconomic Characteristics, so-called ENCASEH), which collected some basic information on all the households living in these communities. After this, the administration collected 6 waves of a much richer evaluation household survey (known as the ENCEL), in March and October 1998, March and November 1999, April and November 2000. In 2003, a new data collection was started in the same 506 villages. However, given that all the 506 localities had been incorporated in the programme at this stage, it was decided to supplement the original sample with a number of localities in which the program had not yet started<sup>1</sup>. While this new sample is certainly valuable, because it offers information on a group that can potentially be used as a control, its use also poses some important methodological problems. The main difficulty, of course, arises from the fact that the allocation of localities to the program or to the control group is not random. Not only, but these localities are still left out of *PROGRESA/Oportunidades* because of the targeting mechanism. This implies differences in outcomes cannot necessarily be attributed to the effect of the programme. Moreover, we have almost no information on the households living in the new control group referring to the pre-programme period. This lack of data prevents the use of the difference in difference method. In section 3, we discuss the methodological problems relevant for this evaluation and the approaches we take. On the one hand, we try to control in a flexible way for observable differences between treatment and control localities. On the other, we also try to exploit the difference in ‘intensity’ of treatment to identify its effects. The latter exercise, that can exploit the randomization imbedded in the original evaluation, is, in our opinion, more credible.

Before delving into the methodological problem specific to our evaluation exercise, in section 2, we describe the main features of the program and discuss in detail the structure of the survey. In section 4, we describe the main features of our data and, in particular, the observed patterns of consumption. We estimate simple relations between the shares of different components of consumptions and total consumption expenditure. This exercise serves two purposes. On the one hand, gives a good idea of the pattern of

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<sup>1</sup> Due to the planning of expansion, incorporation of localities has been done in different phases. Therefore potential control areas were those programmed to be incorporated to the Program in year 2004.

consumption among the households that compose our sample. On the other, they can be useful to predict how the structure of consumption will change with an increase in total consumption.

If one assumes that the program induces an increase in consumption, one can then use the Engel curve results to predict, to a certain extent, the effect of the program on, say, the share of food or the share of education and so on. These predictions, however, need to be taken with some care, for at least two reasons. First, the program effectively reduced substantially the cost of education (and arguably makes it negative in some situations). This also implies a change in the *relative price* of education (relative to other commodities), while the model estimated implicitly assumes constant relative prices. This could invalidate the predictions. Second, the fact that the subsidy is given to mothers,<sup>2</sup> might change the way in which resources are allocated, as the programme changes the fraction of resources controlled by each individual. Indeed there is some evidence from PROGRESA that points to an increase in the share of children clothing and food as a consequence of the increase in the share of resources controlled by mothers (see Attanasio and Lechene, 2000).

Because of the limited use that a descriptive tool such as the Engel curves we report in Section 4 has to predict the effect of the programme, in Section 6 we report the results of our evaluation exercise based on the comparison between ‘treatment’ and ‘control’ localities. As of 2003, we have two types of ‘treated’ localities in the sample, those that started the treatment in 1998 and those that started it in 2000 and one type of control group (those that are not receiving the programme). As we have mentioned above, for the pure control group, we only have data for 2003.<sup>3</sup> In particular, we have no data referring to the period before 1998 or before 2000, when the two treatment group were *not* receiving the programme. This implies that we can only perform a cross sectional comparison (controlling for observable variables) but we cannot deurate our results from the biases arising from pre-existing differences in the outcomes of interest. We document the differences in a number of background variables between the treatment and control groups in Section 5.

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<sup>2</sup> The recently added scholarships for high school can be received by the youth themselves and constitute an exception.

<sup>3</sup> There exists a questionnaire called “Cuestionario de Características Socioeconómicas del hogar en 1997”, which aims to collect retrospective information for the new control households. However it does not collect information on consumption since it would be not realistic. Actually this questionnaire is very similar to the ENCASEH 97.

In addition to the simple comparison between the two samples, we also compare the two treatment groups. This comparison identifies the differential effect that the programme might have after 3 vs. 5.5 years. While for some outcomes, such as nutritional status or children cognitive achievements, one can think cumulate effects to be likely, we would not expect these differences to be expected in the case of consumption or its components, especially for a group of households with very high level of poverty and very low saving (and therefore intertemporal transfers of resources). In Section 7, we perform two types of exercises. On the one hand, we directly compare the two treatments and on the other we exploit the variability in the number and quantity of payments.

We summarize our results and discuss future research on these topics in Section 8.

## 2. The *Oportunidades* programme in rural areas and its evaluation

The *Oportunidades* program was initiated by the Mexican government in 1997, under the name of PROGRESA, to foster the accumulation of human capital in rural areas. PROGRESA and now *Oportunidades*, is a conditional cash transfers programme where poor individuals receive cash in exchange for specific types of behaviour (or conditionalities). *Oportunidades* has three main components. The first and probably the largest from a monetary point of view, is the education one: poor mothers receive subsidies on the condition that children are enrolled in school and attend at least 80% of classes. Children need to be certified by the school and can only repeat a grade once to maintain the grant. The grant starts with the third year of primary school and, now, continues until grade 12 (high school), while before year 2001, was paid only until grade 9 (i.e. the third year of secondary school). The size of the grant increases with grades and, after primary school, is slightly larger for girls than for boys. The reason for this difference is the higher attrition rate of girls. We report the structure of the grant in Table 2.1.

<b>Grade</b>	<b>Boys</b>	<b>Girls</b>
<b>Primary</b>		
3rd year	105	105
4th year	120	120
5th year	155	155

6th year	210	210
<b>Secondary</b>		
1st year	305	320
2nd year	320	355
3rd year	335	390
<b>High School</b>		
1st year	510	585
2nd year	545	625
3rd year	580	660
<i>Source: Oportunidades</i>		

In addition to the monetary support, secondary and High school children receive a transfer for the acquisition of school supplies at the beginning of the academic year. Primary school children receive some school supplies at the beginning and in the middle of the academic year. Students in the 3<sup>rd</sup> grade of secondary school and High School students accumulate funds that are redeemable (under certain conditions) upon graduation from High School. For a student registering for the program since the 3<sup>rd</sup> year of secondary school, this additional amount is about 3,000 pesos.

The health component of *Oportunidades* includes, among other things, a vaccination programme and growth and development check ups for children, as well as some health promotions talks that mothers have to attend to remain in the program, and in particular to be entitled to the nutritional component. The latter consists of a monetary supplement and a nutritional supplement supplied to infants under 2 years old<sup>4</sup> and lactating mothers. The monetary food transfer in 2003 was equal to 155 pesos (or 14 US\$) per month in 2003. For a complete description of the health and nutrition components see, for instance, Skoufias (2001).

Rural *Oportunidades* was targeted first at the community level. In particular, localities with a high level of ‘marginality’ (defined by the combination of a number of variables) were chosen to be targeted by the programme provided they had access to sufficient health and education infrastructure (within a certain distance) so to allow the conditionalities to be satisfied. Within localities, individual households were targeted using a proxy means testing. *Oportunidades* administers a census within each targeted locality and collect information in a detailed survey labelled ENCASEH. Some of the variables in this data base are used to compute a score which is then used to define beneficiary households.<sup>5</sup>

<sup>4</sup> 2-4 years old children are offered a nutritional supplement in case they are identified as malnourished in the growth and development check ups.

<sup>5</sup> The score is the first principal component of a number of poverty indicators. In the case of the localities in the evaluation sample the ENCASEH collected in 1997 was used to compute the score and classify

Provided they comply with the conditions of the program, beneficiaries families receive the payments (at least in the absence of administrative delays and errors).

PROGRESA/*Oportunidades* has become a model program that has been imitated in many parts of the world. One of the reasons behind its success has been the fact that the programme was, since its very start, subject to a rigorous, original and important evaluation. The evaluation was started with the program itself and exploited the logistics of the expansion of the programme to the more than 80,000 localities that it now covers. As a complete and simultaneous expansion of the programme was clearly not feasible, the administration, after selecting 506 localities in which to start a data collection for evaluation purposes, decided to randomly allocate 186 of these localities to the end of the expansion timetable. The data collection started in 1997, before beneficiary households started receiving their transfers and by the end of 2000 (at which time the 'control' communities were incorporated into the program, 6 waves of a longitudinal data base were collected, including around 25,000 households. These evaluation surveys (ENCEL) were censuses of the included communities and were much richer than the original ENCASEH surveys since they include questions regarding diverse themes. The unique feature of this data base, for evaluation purposes, was the fact that 186 randomly selected communities were not receiving, during the period from 1998 to 2000, the programme. The fact that the allocation of the programme was random, implied that these communities could be used as a comparison group. The evaluation of the impact of PROGRESA was done by International Food Policy Research Institute (IFPRI) in a series of reports that are summarized in Skoufias (2000). Hoddinott, Skoufias and Washburn (2000) studied the effect of the rural PROGRESA on consumption and, in particular, on food consumption. The main results they found were the following: (i) total consumption increased by 14% or 151 pesos per month. This figure has to be compared with an average subsidy of 197 pesos per month; (ii) food consumption increased, but by slightly less. At the median, the increase was about 10% (but larger at lower quintiles); (iii) among food consumption, the larger increases were registered in fruit and vegetables and in proteins; (iv) overall, PROGRESA beneficiaries seem to have acquired better diets.

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households as potential beneficiaries and non-beneficiaries (poor and non-poor). There is limited information on the exact procedure used to compute this index. Moreover, in March 1998 a number of non-poor families were reclassified as poor (both in treatment and control localities) in a process known as *densificación*. It turned out that many of the newly defined 'poor' did not receive the program for some time. All this introduces a substantial amount of variation in the number of payments received by different households in different localities.

The comparison between our results and those reported in Hoddinott et al. (2000) is obviously important. However, the very evolution of *Oportunidades* makes this task very complicated. When the collection of a new wave of the ENCEL survey was done in 2003, it was felt that, as all 506 localities had been by the end of 2000 incorporated into the program, the new survey should include a set of new localities in which the program did not operate yet. However, given that the programme had been expanding since its inception in 1998 quite rapidly, not only the new localities to be added to the evaluation sample as ‘controls’ were not selected randomly, but given the very nature of the targeting of the program, turned out to be very different from the localities that make up the original evaluation sample. As we will see, these localities are less poor than those included in the first evaluation of PROGRESA. The methods we will be using will try to take this into account as much as possible.

To summarize, the new evaluation sample, on which the ENCEL questionnaire was administered in 2003, includes three different types of localities (those receiving the program from 1998, those who started receiving it in 2000 and those who do not receive it) and two different groups of individuals: eligible and non eligible. Among the non eligibles it might also be interesting to consider individuals that are ‘almost’ eligible. Finally, we will be considering different outcomes: food consumption, total consumption and assets.

### **3. Methodological issues**

In this section, we will discuss the main methodological issues that are relevant in our context. The estimators we will be using are dictated by the available data. We discuss the conditions under which they provide reliable estimates of the impact of the program.

#### *3.1 Selection on observables*

As we mentioned above, the main problem we face is the fact that the set of new localities added to the original evaluation sample is not randomly selected. Moreover, because of the targeting and expansion pattern of the program is likely to be made of localities that are systematically different from those included in the program. Finally, and probably most problematic, we have no information on the outcome of interest in the

new control localities, before 2003. This prevents us from using difference in difference methods.<sup>6</sup> The main set of methods we use are matching techniques.

The basic assumption that is necessary to make to obtain consistent estimates of the impact of the program using matching methods is that, conditional on a number of observable variables, the allocation to the program between treatment and control localities is, effectively, random. If that is the case, one can use the outcome in the control localities to proxy for the outcome that would have obtained in the treatment localities in the absence of the program.

Taking for granted the ‘selection on observable’ assumption, one then has available a variety of possible estimators and can allow the program to affect individual households in flexible ways. In particular, one can allow for heterogeneity in effects both in terms of observables and unobservables. In what follows we report several sets of results.

1. We report results obtained by Ordinary Least Squares (OLS) and controlling linearly for a large set of observable variables at the individual and locality level. This estimator assumes the linearity of the controls, ignores possible common support problems, that is the possibility that the treatment and control areas have very different values for the conditioning  $x$ 's, and does not allow for rich heterogeneity of effects but is very simple and can be one of the most efficient ways to impose a simple structure on the problem so to improve the efficiency of the estimates.
2. We report results obtained by Propensity Score Matching (PSM). The estimator is derived in two steps. In the first one estimates the propensity score, that is the probability that a given observation is treated as a function of a number of observable variables. In the second step each treated observation is compared to observations with ‘similar’ propensity scores. We experimented with nearest neighbour, local linear regressions and kernel matching methods, but we report only the results obtained by nearest neighbour.
3. Finally we report results obtained with full interacted linear matching. These estimator consists in running an OLS regression of the outcome of interest on a treatment dummy interacted with all control variables. Such an estimator, which allows for heterogenous effects in a relatively flexible way (provided a large

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<sup>6</sup> The main idea behind the difference in difference estimator is to use the variation (between pre- and post- treatment period) in the untreated comparison group to disentangle the impact on the outcome variable due to treatment from the effect due merely to the temporal variation. For some outcomes, such as income, retrospective information was collected and could potentially be used. Of course it would have been unrealistic to hope to collect retrospective information on consumption.

enough set of controls) has been observed to yield, in many situations, estimates similar to those obtained by Propensity Score Matching.

All these results should be interpreted with caution, as their validity depends on an assumption (selection on observable) that might be particularly strong in the present context.

### 3.2 *Regression discontinuity design estimators*

Given the problems one might have with the selection on observable assumption behind the matching estimator, one might also want to try a completely different approach. Within each locality in the original survey, there are a number of individual households who do not receive the program. As we mentioned above, the allocation of the program was done on the basis of a score that can be constructed on the basis of a number of observable variables. Of course, the individuals that get the program are systematically different from those who do not get it. However, one can think of comparing the individuals who have a score just above the cut off level that defines eligible households to those that are just below the same cut off level. This procedure, in the present context, has two advantages and two disadvantages. The first advantage is the fact that one can apply this procedure on the original evaluation sample on individuals who live in the same locality. Therefore there are no area differences of the type discussed above. Moreover, one can, in principle, use pre-program information to implement a difference in difference version of such an estimator and therefore remove unobserved constant differences between the two groups. The two disadvantages are the fact that one measures only the effect on the households near the cut off point and the fact that one ignores the possibility that non-beneficiary households might, within a locality, be affected by the program (for instance through interpersonal transfers). We leave the use of this type of estimator to future research.

### 3.3 *Medium run effects and differential intensity*

It is possible that the effect of the programme on the variables of interest, in our case consumption, depends on the number of years during which the program has operated (or the number of payments received). This is particularly relevant given that, in our data, for reasons that are sometimes not obvious, many eligible households do not receive

payments or receive less than what they were supposed to receive. To investigate this possibility we compare extensively the two groups of treatment localities that, by design, have received a different number of payments, as the first group entered the program 1.5 years earlier than the second group. This exercise is considerably easier than the previous ones because we can safely assume that the two sets of localities did not differ systematically before the inception of the programme, as the localities were assigned to the two groups (late starters and early starters) randomly. Therefore, comparing the outcomes of interests in the two groups of localities gives us the difference between the effect of the programme *after three years* relative to the effect of the programme *after five and a half years*.

This analysis can be expanded to take into account explicitly the number of payments and the amounts paid. It should be stressed, however, that these variables can conceivably be considered as endogenous (the amount paid depends on household behaviour and the number of payments can identify regions where the programme works best and these factors might affect independently the outcome). To deal with this problem, one can use the randomization as an instrument for the number of payments or the amounts received.

#### 4. Consumption patterns in the new evaluation sample

We start describing the main features of our sample. In Table 4.1, we report the average, median and standard deviation of various variables of interest. Family size, number of children, age of household head and wife, education of household head and wife. In Table 4.2, we report the average level of consumption income and the shares of various consumption items.

**Table 4.1**

**Average, median and standard deviation of various variables of interest**

Variable	Average	Median	Standard deviation
Family size	5.06	5	2.86
Number of children	3.10	3	1.78
Age of household head	48.11	46	16.3
Age of household wife	42.76	40	16.47

Education of household head	2.52	3	2.18
Education of household wife	2.60	3	2.26

*Statistics are obtained from all the observation in the sample (treatment + control sample)*

**Table 4.2**

**Average level of consumption, expenditure and shares of consumption items**

Variable	Average	Standard deviation
Value of monthly food consumption	1196.11	8382.7
Total monthly expenditure	1883.29	8469.9
Food share	0.641	0.202
Alcohol and tobacco share	0.006	0.037
Education share	0.023	0.062
Health share	0.041	0.102
Children share	0.004	0.010
Clothes share	0.011	0.012
Energy share (heating and fuel)	0.064	0.077
Homewares share	0.006	0.022

*Statistics are obtained from all the observation in the sample (treatment + control sample)*

We continue the analysis with the estimation of Engel curves in our sample. In particular, we focus on the relationship between expenditure shares (that is the expenditure on a given group of commodities over total consumption expenditure) and total consumption expenditure for each of several commodities. Such a relationship is useful for several reasons. In our context, it is particularly interesting because it can be used to predict the effect of the program on the structure of consumption. Following the literature, for each expenditure share, we estimate the following relationship:

$$(1) \quad w_i^j = \beta^j z_i + \gamma_1 \log(x_i) + \gamma_2 \log(x_i)^2 + u_i$$

where  $w_i^j$  is the share of expenditure on (group of) commodity  $j$  by household  $i$ ,  $z_i$  is a vector of control variables including variables capturing the demographic composition of household  $i$ ,  $x$  is total consumption expenditure and  $u$  a random term. Equation (1) describes how expenditure shares vary with total expenditure. If, over a certain interval, the share increases with total expenditure, the commodity can be considered as a luxury, while if it decreases it can be considered as a necessity. Equation (1) can be derived from

a standard demand system with fixed relative prices. As we only consider one period, we can ignore price effects.<sup>7</sup> Banks, Blundell and Lewbel (1999) have stressed the importance of considering quadratic terms in systems like (1) to allow for the possibility of shares having a non-monotonic pattern.

Equation (1) can (and traditionally has been) estimated either by OLS or by Instrumental Variables (IV). There are two reasons to instrument total expenditure in (1). First it is likely that total expenditure is affected by measurement error, therefore introducing a bias in the estimates of the coefficient of (1) and second, total expenditure might be affected by factors that are correlated with  $u$ , the random term. This would be the case if, for instance, preferences are non-separable over time or if differences in discount factors across individuals are correlated with taste differences. In what follows we instrument total expenditure both with total income and labour income and with average incomes in the locality. The latter set of instrument is probably the most robust to the presence of measurement error.

Estimates of equation (1) can be used both as descriptive tools of the pattern of consumption in our sample and as a tool to predict the effects of the program on the structure of consumption. We report some of the estimates we obtained from the Engel curves in Appendix A. In this appendix we graph the polynomial in total log consumption for several commodities and consider both the estimates obtained by OLS and those by IV. It is apparent that instrumenting can make a large difference. It is instructive to consider, for instance, the Engel curve for food obtained with the two techniques. In the case of the OLS results, the share of food declines monotonically with total consumption expenditure, indicating that food is a necessity. However, in the case of the IV results, we see that for very low level of total consumption, the share of food is increasing, maybe because of an improvement in the quality of diet.

## **5. Comparison between treatment and ‘new’ control sample**

As we discussed above, much of the analysis in this study is based on the comparison between treatment and control groups. In this section we discuss the differences between the treatment villages (including those where the treatment started in 1998 and those where it started in 2000) and the control villages. When considering this analysis, it should be remembered that the two groups of villages included in the original evaluation

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<sup>7</sup>However, it might be argued that prices in different localities are different and that these differences should be considered explicitly, either using price information or village level effects in our regressions.

sample did not differ systematically in most observed characteristics in 1997/1998, as is to be expected given the randomization of the programme in the first phase of its expansion (see Behrman and Todd, 1999).

**Table 5.1**

**Differences between treatment and control villages: locality variables**

	Treatment villages	Control villages	Difference (p-value)
Number of poor households	54	44	10 (0.013)*
% of poor households	0.884	0.855	0.029 (0.034)*
% sec. school present	0.243	0.193	0.050 (0.205)
Distance from closest bank (km)	34.8	28.5	6.3 (0.134)
Distance from closest sec. school (km)	4.01	2.53	1.48 (0.65)
Distance from closest sec. school (including tele) (km)	3.44	2.16	1.28 (0.70)
% of villages with water piped	0.130	0.127	0.003 (0.90)
% with waste recollection	0.108	0.140	-0.032 (0.29)
Public telephone post	0.492	0.333	0.159 (0.001)**
Public market or other market in village	0.816	0.636	0.180 (0.000)**
Health clinic present	0.970	0.973	0.003 (0.890)
Main activity is not agriculture	0.048	0.107	-0.059 (0.008)**

*Regressions performed on 657 localities, of which 506 treated and 151 control.*

The p-value presented in each table are derived from a statistical test for equality between treatment and control areas. They tells us how likely is the difference between treatment and control areas, if the true (population) values are equal. If the p-value is smaller than 0.05, then the difference between treatment and control areas is significant at the 5 per cent level (\*); if it is smaller than 0.01 then the difference is significant at the 1 per cent level (\*\*). These p-values have been calculated using robust estimates of standard errors, clustered at the locality level

We start, in Table 5.1, by looking at village level variables. In particular, we consider the size of the village, the access to credit institutions, to schools, to health infrastructure as well as the other infrastructure. The control localities were matched to the localities already in the evaluation sample using a large range of observable characteristics at the village level. It is therefore not surprising to find out that, for many variables, there are

no strong significant differences. Some differences, however, do emerge. The treatment localities have a slightly larger number of poor households, which is statistically significant. This is driven in part by the fact that these localities are larger in terms of population and in part by a larger proportion of poor households (as reported in the second row of Table 5.1). Among the other variables we consider, we notice no significant differences in school or health infrastructure. Public telephone posts seem to be more frequent in treated villages, and banks seems to be marginally further away from these villages. For most of these localities, agriculture is the main activity. However, the proportion of villages for which this is not the case is much lower (and significantly so) among the treated villages. Finally treated villages seem to have more frequently a public market.

In Table 5.2, we consider individual level variables. We consider the age of the household head and spouse, several variables referring to the education level of the head and family size variables, although some of these variables (such as the number of young children) are conceivably affected by the program.

While most of the village level variables considered were not statistically different between treatment and control localities, at the individual level we find very strong differences. In particular, we find that households in the control localities have fewer adults and children aged 6 to 21. Interestingly, the number of children less than 5 is the only variable for which we do not find a significant difference. Even more strikingly, households in treatment localities seem to be systematically less educated than households in control localities. The percentage of indigenous households is considerably higher in treatment localities.

The overall picture that emerges from Table 5.2 is that treatment communities were, on averages, poorer than 'control' localities. This is not surprising as PROGRESA did target explicitly the poorest villages. However, it will create problems for our evaluation strategy especially when, as with consumption, it will not be possible to reconstruct pre-programme outcomes to use a difference in difference strategy.<sup>8</sup>

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<sup>8</sup> For other variables, notably education, retrospective information (back to 1997) is available.

	Treatment villages	Control villages	Difference (p-value)
Age of household head	47.79	46.67	1.12 (0.049)**
Age spouse	42.95	41.24	1.71 (0.001)**
Number of males	3.09	2.83	0.26 (0.000)**
Number of females	3.08	2.81	0.27 (0.000)**
Children 0-5	0.083	0.83	-0.747 (0.94)
Children 6-21	2.43	2.17	0.26 (0.000)**
Adult 21-39	1.73	1.51	0.22 (0.000)**
Adult 40-59	0.77	0.71	0.06 (0.000)**
% of heads who can read and write	0.679	0.722	-0.043 (0.011)**
% of heads with no formal education	0.304	0.258	0.046 (0.000)**
% of heads with more than primary	0.097	0.118	-0.021 (0.041)**
% of indigenous heads	0.386	0.268	0.118 (0.029)**
Poverty index	2.416	2.286	0.130 (0.121)

*Regressions performed on poor households in treatment and control localities. Standard errors are clustered at the locality level. \*=significant at 5 per cent level, \*\*=significant at 1 per cent level. For details on the p-value see the bottom of table 5.1*

## 6. Impact effects: what can be estimated

In this and in the next section we report the results we obtain applying various techniques to estimate the impact of the *Oportunidades* program on consumption and its components. In this section, we will focus on the comparison between the entire treatment group (that is both the communities where the programme was started in 1998 and those where it was started in 2000) and the control group of 151 localities where the programme has not started yet. As we will see these results will need to be taken with caution, given the large differences in observable variables discussed in Section 5 and the impossibility of constructing difference in difference estimates that take into account pre-existing differences. In the next section, instead, we will focus on estimating the

cumulative impact of the programme on consumption. That is, instead of comparing localities with the programme to localities without the programme, we compare localities that had the programme for a long time to localities that had it for a shorter time.

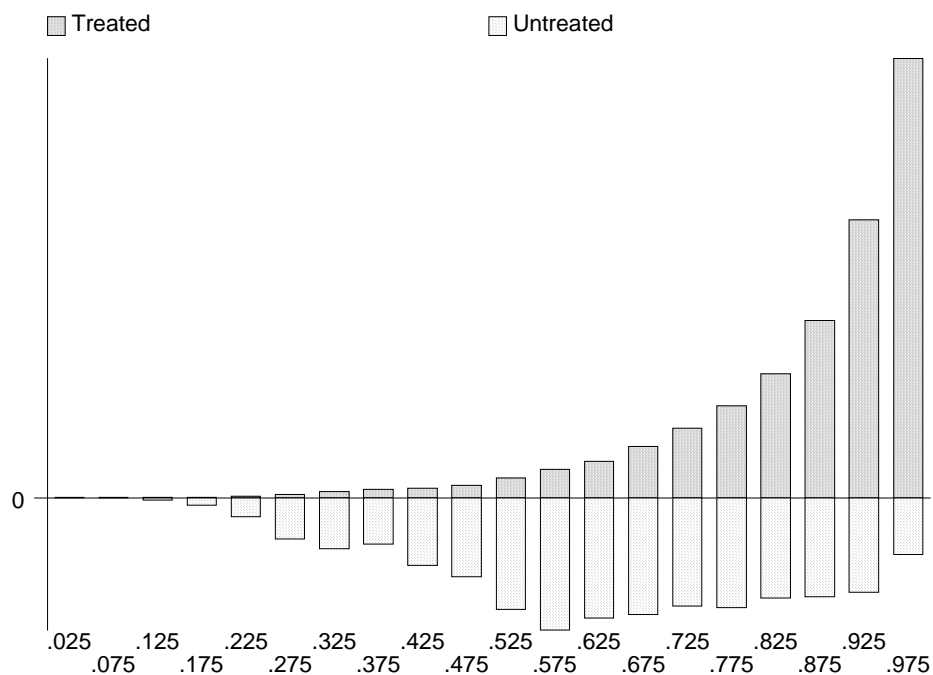
We have used several matching techniques and will report the results obtained with two of them. The first technique matches treated observations using the propensity score estimated via a Probit model. The second uses fully linear interactions.

### 6.1 Propensity score matching (PSM)

In Table 6.1, which is printed at the end of the paper, we report the results we obtain by estimating a Probit model on the sample of poor households in treatment and control communities. Such a model, which summarizes the differences in observable variables between treatment and control localities, includes a large set of variables, ranging from household level to locality level variables. As can be seen from the Table, most of these variables are strongly significant. The differences between treatment and control localities seem to be pervasive. In Figure 1, we report the density distribution of the estimated propensity score in treatment and control areas. As can be seen, while there are no large support problems, the distributions, as to be expected, are quite different.

**Figure 1**

**Density distribution of the estimated propensity score in treatment and control areas**



We report the results obtained by Propensity Score Matching for several variables of interest in Table 6.2. If one believes the assumptions behind PSM, one can estimate the effect of the program from the differences between treatment and control. While we have investigated several versions of PSM, we report the results obtained by *nearest neighbours*<sup>9</sup>. We focus on the following variables: total log consumption, total consumption, food consumption, log food, education, vices, health, adult clothing, children clothing. We also look at the share of food in total consumption. These results were obtained eliminating the top and bottom one percent of observations.

For none of the variables we consider we find a significant difference between treatment and control villages. The results were very robust to the use of different matching techniques, to the treatment of outliers, to the variables chosen to control for the non random selection process. Nowhere we found positive effects of consumption. Indeed, and most puzzling, the point estimates are, for most variables, estimated to be negative. This result is clearly implausible and casts serious doubts about the ability of controlling for the non-random assignment of the program.

**Table 6.2**

**Impact of *Oportunidades* PSM. Treatment (1998+2000) vs Control (2003).**

Variable	Average Treatment on the Treated	Standard error	95% confidence interval
Log consumption	-0.081	0.076	-0.232, 0.069
Level of consumption	-201.12	129.1	-456.1, 53.8
Log food	-0.058	0.072	-0.199, 0.084
Level of food	-70.2	68.1	-204.52, 64.14
Food share	0.013	0.017	-0.021, 0.048
Alcohol and tobacco	-6.79	4.02	-14.73, 1.14
Education	1.26	4.61	-7.82, 10.35
Health	-38.67	23.97	-85.93, 8.59
Children clothing	0.093	1.32	-2.50, 2.69

*Standard errors are obtained from 200 bootstraps with cluster effects at the locality level. Propensity score matching performed with nearest neighbours (5).*

<sup>9</sup> Local linear regressions and kernel matching give similar results.

## 6.2 Fully interacted linear matching (FILM)

Given the disappointing results we obtained by PSM, we investigated several alternatives. In particular, in this subsection we report estimates of the impacts on the same variables reported in Table 6.2, but estimated by full interacted linear matching. In the first column of the table we report the OLS estimates, while in columns 2 and 3 we report the Average Treatment on the Treated (ATT) and the Average Treatment Effect (ATE). Consistently with what we find with the PSM estimator, no effect is significant, with the exception of some of the OLS estimates. Even though in some cases (such as for total consumption) the effect is economically sizeable, the estimates are not statistically significant and zero is always within the 95% confidence interval. This results confirm the evidence in Table 6.2 and stress the difficulty in accounting for the unobservable differences between the villages that were originally targeted by PROGRESA [Treatment (1998+2000)] and those in which the programme were still not operating (Control 2003).

**Table 6.3**

**Impact of *Oportunidades* FILM. Treatment (1998+2000) vs Control (2003).**

Variable	OLS	Average Treatment on the Treated	Average Treatment Effect
Log consumption	-0.069 (0.031)	-0.032 (0.041)	-0.039 (0.036)
Level of consumption	-805.1 (740.5)	-1583 (1685)	-1417 (1484)
Log food	-0.050 (0.032)	-0.015 (0.043)	-0.021 (0.039)
Level of food	-705.5 (739.7)	-1507 (1684)	-1336 (1483)
Food share	0.012 (0.007)	0.012 (0.010)	0.012 (0.009)
Alcohol and tobacco	-4.357 (3.211)	-2.149 (3.577)	-2.900 (3.257)
Education	-4.982 (2.707)	2.976 (3.16)	1.40 (2.76)
Health	-21.97 (5.43)	-7.03 (8.28)	-9.90 (7.11)
Children clothing	-1.218 (0.82)	0.170 (0.98)	-0.066 (0.87)

*Standard errors are computed with cluster effects at the locality level. Control variables include all those for the Probit model in Table 6.1 .*

Given the evidence in Tables 6.1 and 6.2, we decided not to explore the effect of the program on other important outcomes, such as the ownership of durables (television, refrigerator and so on) as well as other assets (animals, land). It would be difficult to disentangle the effects of the programme from pre-existing differences.

## 7. Medium run effects and intensity effects.

We now move to the consideration of medium run effects. As we mentioned in Section 3, we will estimate these in two different ways. By simply comparing the average outcome in 2003 in the two groups of towns (treatment 1998 and 2000) we will infer the overall effect of the programme in the two groups of towns and, therefore, infer the difference in the impact after 5.5 years of treatment and 3 years of treatment.<sup>10</sup> We then try to exploit the variability in the number and amounts of payments.

### 7.1 *Comparing Treatment 1998 to Treatment 2000*

The results of the first exercise are reported in Table 7.1 for total and food consumption (both in logs and levels) for the share of foods and for some important categories (alcohol and tobacco, education, health, children clothing). In the table we report three different specifications. In the first column we report the simple difference in means, in the second column we condition on the individual controls that we used to estimate the probit model in Table 6.1 and in column 3 we also condition on town level controls. As the allocation of the localities between the two groups was random, controlling for observable variables should not make any difference to the estimates of the mean, but could affect the precision of the estimates and therefore the power of the test.

Perhaps not surprisingly, most of the effects we measure are not significantly different from zero. The only exceptions are the expenditure on education and on children clothing, where the effects are significant at the 10% level, even though their magnitude is miniscule (early intervention households spend around 2.5 and 1.5 pesos more than late intervention families for, respectively, education and children clothing). The reason why we find these result not particularly surprising is that, especially for poor households who have very low saving, it is unlikely to find differential effects of a subsidy that has been received for five and a half years versus a subsidy that has been received for three

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<sup>10</sup> Here we are ignoring a potentially important issue. While towns were allocated to early starters and late starters randomly in 1998, we are comparing outcomes in 2003. It might be that the programme, and its different duration in the two sets of town, over the years, has caused different non-random migration so that the composition of the households in the two sets of town could be slightly different.

years. Especially for non durable items (and if the subsidy is perceived as permanent), such as consumption of food (which accounts for a substantial fraction of these households budget) it is likely that upon receiving the subsidy the household increases its consumption to a new level and stays there. Interestingly, the only (marginally) significant effects that we find are directly related to the objectives of the programme and have an ‘investment’ element. At the same time, the only negative effect (albeit small, early intervention households consume around 4 pesos less than late intervention families) we find is on the consumption of alcohol and tobacco. This is consistent with the evidence reported by Attanasio and Lechene (2002) who interpret this as resulting from a shift in the control of resources from the male to female in the household within the framework of a more formal demand analysis than that reported here.

**Table 7.1**  
**Long run impacts of *Oportunidades* on consumption and its components.**  
**Treatment (2000) vs Treatment (1998).**

Variable	Mean comparison	+ individual controls	+ individual and town controls
Log consumption	0.011 [0.033]	0.028 [0.022]	0.030 [0.024]
Level of consumption	16.2 [60.0]	34.6 [40.5]	24.7 [42.5]
Log food	0.013 [0.030]	0.035 [0.026]	0.031 [0.029]
Level of food	9.15 [16.3]	25.4 [35.2]	9.58 [37.3]
Food share	0.002 [0.009]	0.004 [0.007]	0.002 [0.007]
Alcohol and tobacco	-3.42 [1.65]**	-4.64 [1.83]**	-4.52 [1.69]**
Education	0.752 [2.445]	1.475 [1.753]	2.625 [1.920]*
Health	-1.57 [5.92]	-1.05 [4.39]	-2.13 [4.25]
Children clothing	1.054 [0.786]	1.059 [0.558]	1.377 [0.546]*

*Standard errors in parentheses allow for cluster effects at the locality level. \* = significant at 10 per cent level, \*\* = significant at 5 per cent level*

The evidence of (slight) positive effects on education and children clothing induced us to look at other variables on which the programme might have effects only after a few years, so that being in it for 5.5 rather than three years might make a difference. Therefore, in addition to the outcomes considered in Table 7.1, we also consider ownership of several durables and other assets, such as animals and land. We report the results of this exercise in Table 7.2.

**Table 7.2**  
**Long run impacts of *Oportunidades* on durables and assets.**  
**Treatment (2000) vs Treatment (1998).**

Variable	Mean comparison	+ individual controls	+ individual and town controls
House	-0.013 [0.008]	-0.009 [0.007]	-0.006 [0.007]
Television	0.032 [0.026]	0.013 [0.016]	0.005 [0.015]
Automobile	0.011 [0.010]	0.012 [0.007]	0.012 [0.007]*
Refrigerator	0.017 [0.018]	0.009 [0.011]	0.015 [0.011]
Own animals	-0.020 [0.017]	-0.028 [0.015]*	-0.024 [0.014]*
Own land	-0.021 [0.021]	-0.029 [0.019]	-0.013 [0.018]
Agricultural machinery	-0.001 [0.012]	0.005 [0.011]	0.003 [0.011]
Running water	0.075 [0.042]*	0.060 [0.037]*	0.021 [0.034]
Rooms number	0.001 [0.041]	-0.041 [0.037]	-0.035 [0.036]
Dirt floor	0.024 [0.031]	0.015 [0.016]	0.019 [0.014]
Savings	0.001 [0.003]	0.003 [0.003]	0.001 [0.003]

*Standard errors in parentheses allow for cluster effects at the locality level. \*=significant at 10 per cent level*

Once again, and somewhat disappointingly, we do not find many significant effects. The most interesting effect is the one on the presence of running water, which shows a positive and (marginally) significant effect of the additional presence in the program. This effect is quantitatively important: the percentage of households with running water increases from 50% to 57.5% when moving from late to early intervention localities. We also find a positive effect on the probability of owning a car. Somewhat puzzling is the only other significant effect observed: that on the probability of owning animals, which is negative.

## 7.2 *Number and amount of payments*

In the last part of this section we analyze the effect of different amounts and number of payments received by beneficiary households from the sample. There is a considerable amount of variability in the number of payments and the amounts received by beneficiary households. While a large fraction of it is explained by the fact that 186 of the 506 localities in the original sample entered the programme much later, there is an additional amount of variability. This can be due to two very different reasons. First, there were administrative adjustments. Second, households need to participate in the programme and comply to its conditionalities to receive their correspondent payments. Moreover, how much they receive depends on the grade and gender composition of children attending school. The most obvious example of the first reason is what happened to the so-called ‘densificados’ households. As mentioned above, beneficiary households were first classified as eligible at the end of 1997. A certain number of households, however, were added to the list of eligibles in early 1998. These were called ‘densificados’. The ‘densificados’ households are systematically different from those that were originally classified as eligible so that one has to be careful in measuring the impacts.<sup>11</sup> Typically are slightly less poor and have fewer children. It turns out that, even in localities where the programme started in 1998, many of the ‘densificados’ households did not receive any payments<sup>12</sup> (and this was probably not by choice on the part of households). As for the second source of variability, that is definitely endogenous, as it is

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<sup>11</sup> Fortunately it is possible to identify ‘densificados’ households in the localities where the programme did not start until 2000.

<sup>12</sup> According to the evidence in Hoddinott, Skoufias and Washburn (2000) 27% of all eligible population (as in May 1998) had not received any benefits by March 2000. They find that the 85.7% of this zero-payment group was never incorporated into the program. All these “forgotten” households were “densificados”.

directly linked to households' behaviour and choices. We discuss below the implications of this endogeneity.

In Table 7.3, we report the distribution of payments and amounts in 'early' and 'late' intervention localities. The data on the number and amount of payments (or transfers) was obtained directly from the administrative records of *Oportunidades* which also provided the variables to match these data to the rural evaluation data base<sup>13</sup>. In the first row, we report the percentage of eligible households who did not receive any payment until 2003. Surprisingly, according to the administrative data sources, 30% of the eligible households in the early intervention sample and 33% of those in the late intervention sample received no payments from the programme. The average number of payments in the early treatment areas is 18.43, while in the later treatment areas is almost 15. This difference is less large than one would expect from the fact that the early treatment sample should have received the payment for 1.5 additional years. The same applies to the cumulative amounts: households in late intervention areas have received roughly 4/5.5 of households in early intervention areas (rather than 3/5.5). If the differential in payments were caused by administrative adjustments, this might indicate that such adjustments were more severe in the early years of the implementation of the programme.

	Treatment 1998	Treatment 2000	Difference
Percentage of zero payments	30.2	32.9	-2.7 (1.8)
Number of payments	18.43	14.95	3.48 (0.183)
Amounts paid	20,544.81	16,352.47	4,192.34 (730.30)

*Standard errors in parentheses allow for cluster effects at the locality level*

<sup>13</sup> Some problems arose when matching the transfer data to the rural evaluation database. There are around 7000 households that are not found in the transfer records even if they are eligible and in treatment localities. Some of these households belongs to the original sample (ENCASEH) while others were included in the sample during the following survey rounds (ENCEL 1998-2000 and 2003 rural survey). We believe that a consistent strategy is to consider zero payments for all these households, as in Table 7.3. However, it is important to make clear that results regarding zero payments can vary substantially under other strategies. In particular, if only household from the original sample are considered (that is, we exclude households incorporated in the sample in the following rounds) then we have only 19.8% and 18.8% of the households, respectively in the 1998 and 2000 intervention sample, not receiving any transfer.

Going back to the issue of the different reasons behind the variability in the intensity of the programme, we compute the coefficient of variation within each locality of the number of payments and the amounts. The former at 0.85 is relatively less variable within each village than the latter (1.16), perhaps indicating that it is more driven by administrative adjustments than by household behaviour.

In what follows, to take into account that the programme might have had differential effects depending on its intensity, we start by assuming that such effects can be modelled as a polynomial in either the number of payments or the amount paid. This functional form allows to study the sign of the program effect as well as whether this effect increases or decreases as the number (or the amount) of payments increases. To estimate such effects we estimate the following regression:

$$(2) \quad y_i = \alpha_0 + \alpha_1 x_i + \alpha_2 x_i^2 + \alpha_3 x_i^3 + \theta' Z_i + u_i$$

where  $y$  is the variable of interest,  $x$  is either the number of payments or the amounts paid,  $Z$  is a vector of control variables, including individual and town variables,  $u$  is a residual term and the  $\alpha$ 's and  $\theta$  are parameters to be estimated.

Estimating equation (2) by Ordinary Least Squares (OLS) does not take into account the fact that, as we have discussed, the magnitude and variability of the  $x$  variables is linked to household behaviour and is, therefore, likely to be correlated with  $u$ . OLS would therefore yield biased estimates. To tackle this problem we use a control function approach, using as an instrument a variable which determines the number of payments (and the amounts paid) but does not affect the outcome of interest due to the original randomization.

We use a control function approach (rather than a standard IV approach) because we have a single instrument and three parameters (albeit on a non-linear function of one variable) to estimate. In practice we run a regression of the  $x$  on the same controls that enter equation (2) and a dummy that indicates early intervention and add to equation (2) a polynomial in the estimated residuals of such an equation. This procedure, under standard assumptions, should give us consistent estimates of the parameters of interest.

We report some of our results in Figures 2 to 5. In each of the six panels of the Figures we plot the polynomial  $\alpha_1 x_i + \alpha_2 x_i^2 + \alpha_3 x_i^3$  either against the relevant  $x$  (number of payments or amounts) for six outcomes of interests the log and the level of total consumption, the log and the level of food consumption, the expenditure on education and the expenditure on children clothing. These are the most interesting results (a complete set is available upon request). In Figures 2 and 3 we report the OLS and

control function results for the number of payments, while in Figures 4 and 5 we report the equivalent results for the amounts paid. In each figures we report the test for the joint significance of the three coefficient of the polynomial.

The picture that emerges is that, while the OLS estimates are often significant, those by the control function are often much less so. Often the shape of the functional form changes substantially, once we take into account the endogeneity of payments. However, in some cases, we find some significant effects of the exposure to the program. For instance, both for education and children clothing, we find significant and consistent increases in expenditure, both as a function of the number of payments and as a function of the amounts paid (for example figure 3 shows that 20 payments result in an increase of around 7 pesos in education spending and 2 pesos in children clothing). In the case of total food and total consumption we find no significant effects when we look at the level and an effect that increases very quickly and then stabilizes when we look at logs. This is particularly so for the number of payments. Interestingly, in the case of amounts, the point estimates in the control function specification are much larger but they are also very imprecise.

Figure 2

Predicted outcome  $(\hat{\alpha}_1 \text{paynum}_i + \hat{\alpha}_2 \text{paynum}_i^2 + \hat{\alpha}_3 \text{paynum}_i^3)$  against number of payments (OLS)

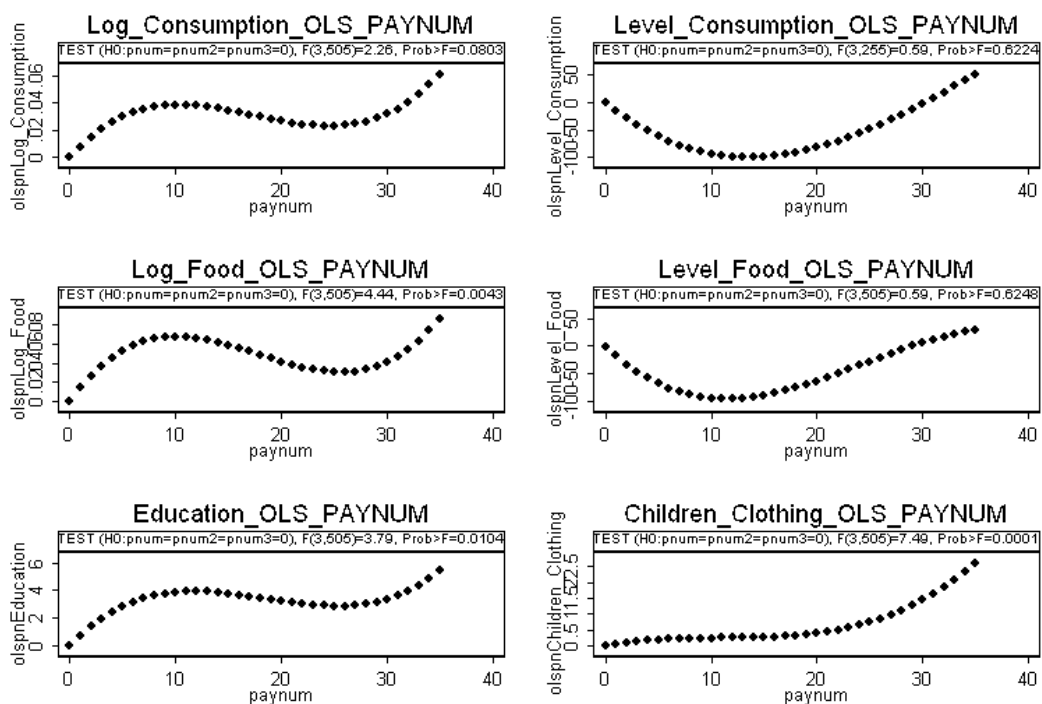
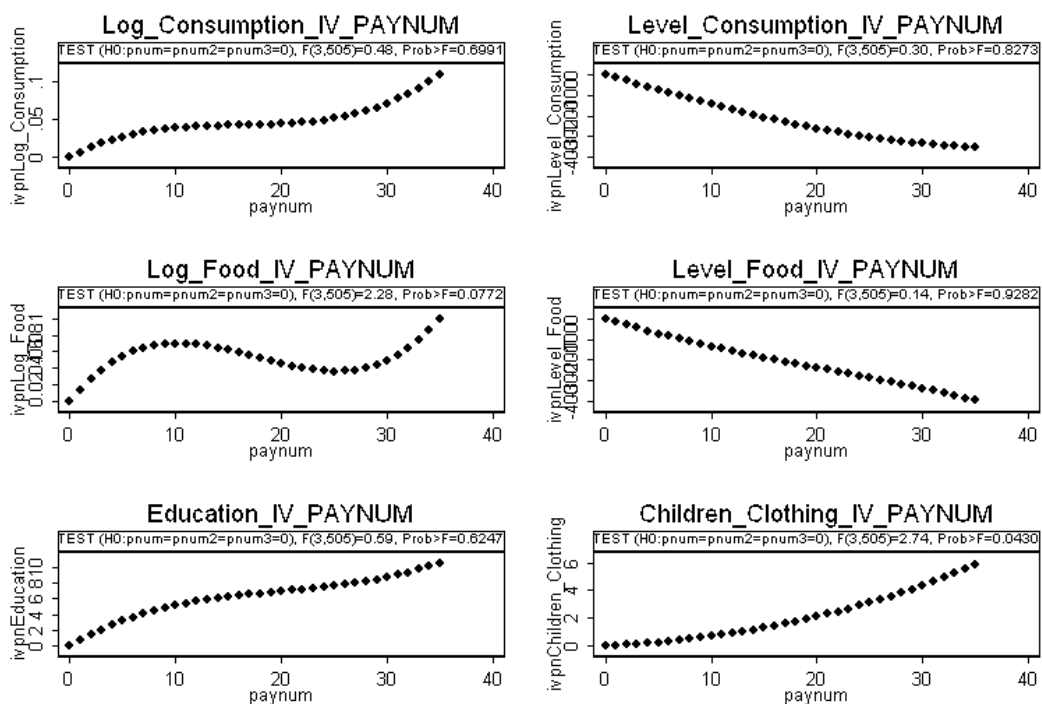
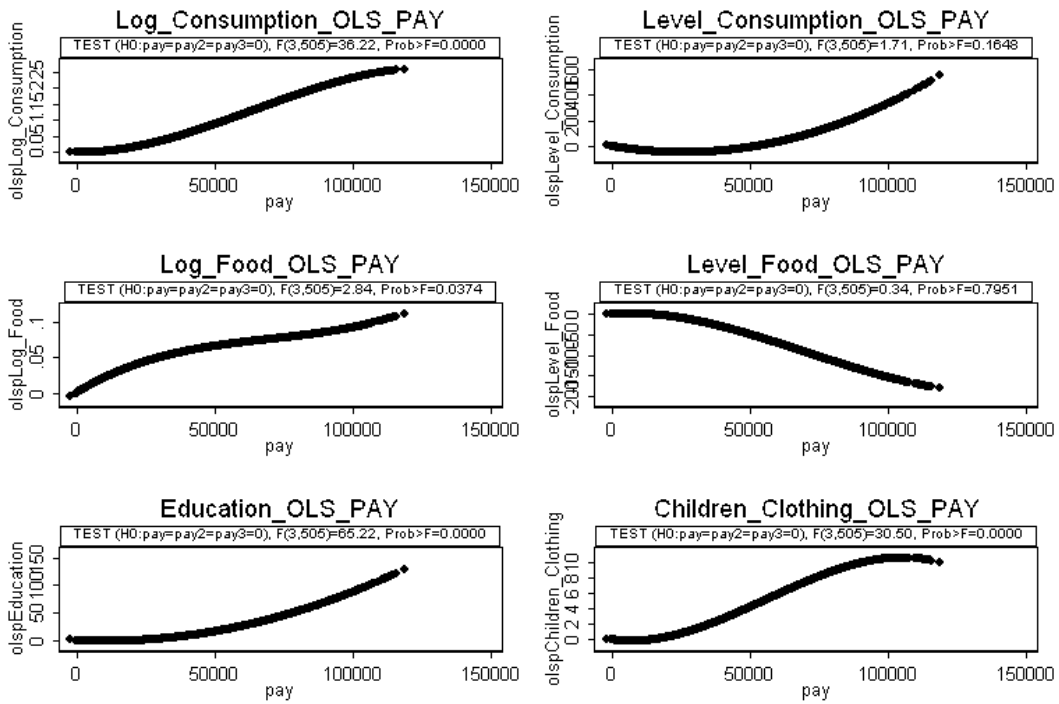


Figure 3

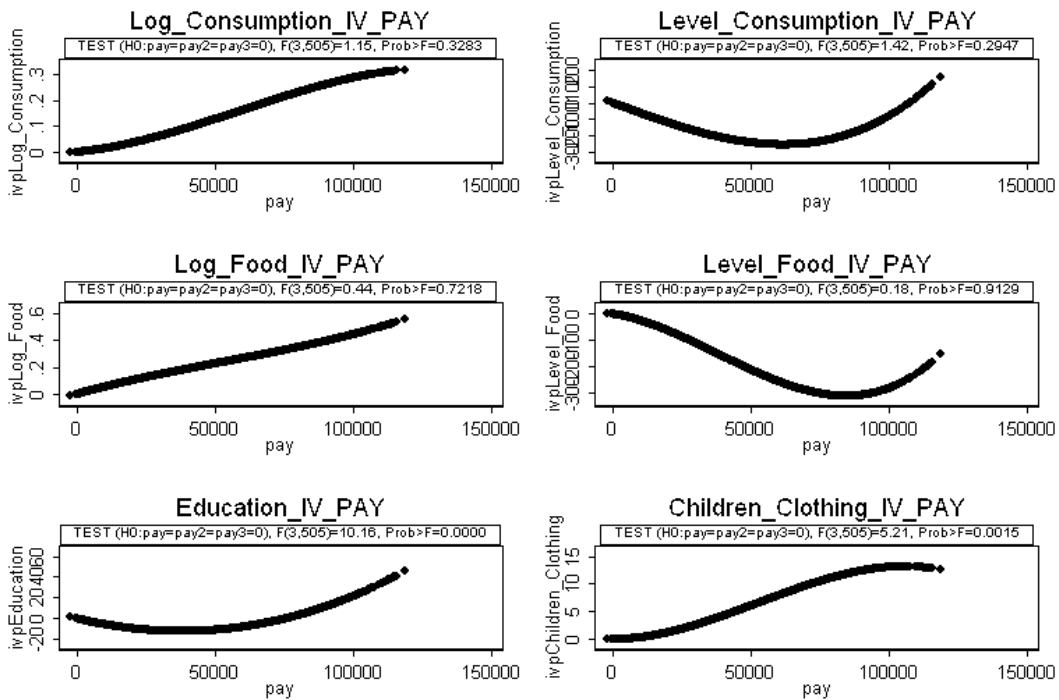
Predicted outcome  $(\hat{\alpha}_1 \text{paynum}_i + \hat{\alpha}_2 \text{paynum}_i^2 + \hat{\alpha}_3 \text{paynum}_i^3)$  against number of payments (IV)



**Figure 4**  
 Predicted outcome  $(\hat{\alpha}_1 pay_i + \hat{\alpha}_2 pay_i^2 + \hat{\alpha}_3 pay_i^3)$  against amount of payment (OLS)



**Figure 5**  
 Predicted outcome  $(\hat{\alpha}_1 pay_i + \hat{\alpha}_2 pay_i^2 + \hat{\alpha}_3 pay_i^3)$  against amount of payment (IV)



## 8. Conclusions

In this paper, we have assessed the effect of *Oportunidades* on consumption in rural areas five and a half years after the first implementation of the programme. We have compared (1) the treatment group (including those who started to receive the program in 1998 and those who did in 2000) to the new control sample (formed in 2003), and (2) compared the early (1998) and late (2000) incorporation sample, that is households who have received the programme for 5.5 years to households that have received it for three years. The outcome of the first exercise is disappointing. A careful analysis of the two samples reveals that the set of new localities used as control is too different from the treatment set to be meaningfully used as a control in the absence of retrospective data that allow to control for pre-existing differences with the ‘treatment sample’. As mentioned above this is a problem that is relevant for the analysis of consumption. Fortunately, for other variables, retrospective information is available.

The second exercise does not reveal big differences between early starters and later starters in most of the variables analyzed. The most notable exceptions are education expenditures and children clothing. This evidence is confirmed both by the simple comparison of early and late starters and by an analysis that tries to exploit the variability in the number and amounts of payments among beneficiary households. Other minor differences arise. First, we find a negative effect (albeit small, early intervention households consume around 4 pesos less than late intervention families) on the consumption of alcohol and tobacco. In addition, a positive effect (from 50% to 57% when moving from late to early intervention localities), only marginally significant, is found for the presence of running water. Finally, we also find a positive effect (again marginally significant) on the probability of owning a car.

The analysis of asset ownership and use at first sight contrasts with that recently presented by Gertler et al. (2005) that find substantive and positive effects of *Oportunidades* on these variables. The contrast, however, is only apparent. Gertler et al.’s analysis is based on the comparison between early intervention and later intervention’s samples *before* the late intervention was started (year 2000). It should also be remembered that the early vs late allocation of the program between these two groups of localities was random. Our analysis complements that of Gertler et al. (2005) as it is based on the comparison in 2003, that is three years after the program started in late intervention areas. Our results and those of Gertler et al. therefore say that the effects of the program

on these variables are reasonably quick, as after three years we do not find many significant between early and late intervention areas.

This type of conclusion also hold for consumption and its patterns: as shown in Hoddinott, Skoufias and Washburn (2000), the program has important effects on consumption and its composition. However, these effects, for the most part, establish themselves very quickly so that it is not possible to distinguish localities where the programme has operated for three years from localities where it has operated for five. For consumption and its structure this finding might not be too surprising. It is more interesting for the investment activities considered by Gertler et al. and in this paper. It indicates that the programme, even in this dimension, has fast effects.

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**Table 6.1: Probit estimates for propensity score**

Probit estimates Number of obs = 24485  
Wald chi2(66) = 401.58  
Prob > chi2 = 0.0000  
Log pseudo-likelihood = -8991.0943 Pseudo R2 = 0.2234

(standard errors adjusted for clustering on town)

treat	Coef.	Robust Std. Err.	z	P> z	[95% Conf. Interval]
Ientidad_13	.7058624	.3466712	2.04	0.042	.0263993 1.385326
_Ientidad_16	.4136047	.3973998	1.04	0.298	-.3652847 1.192494
_Ientidad_21	-.0997831	.3337846	-0.30	0.765	-.7539888 .5544226
_Ientidad_22	.4250823	.4943362	0.86	0.390	-.5437988 1.393963
_Ientidad_24	.2485	.3689928	0.67	0.501	-.4747126 .9717126
_Ientidad_30	-.5174151	.3216373	-1.61	0.108	-1.147813 .1129825
publicaagu~d	-.0805625	.2206047	-0.37	0.715	-.5129398 .3518148
publicabas~a	-.7128918	.2265687	-3.15	0.002	-1.156958 -.2688254
publicaalu~o	.2797879	.1824699	1.53	0.125	-.0778464 .6374223
publicadre~e	-.0295003	.3591202	-0.08	0.935	-.733363 .6743624
publicate~no	.0329129	.1458818	0.23	0.822	-.2530102 .318836
publicapos~e	-.3598811	.5496218	-0.65	0.513	-1.43712 .7173578
financiase~e	.3917002	.1639612	2.39	0.017	.0703422 .7130583
bankdistance	.003669	.0025813	1.42	0.155	-.0013903 .0087282
bankmiss	-.1159175	.3123561	-0.37	0.711	-.7281242 .4962893
credudista~e	-.0030417	.0028369	-1.07	0.284	-.0086019 .0025185
credumiss	-.1998917	.2390398	-0.84	0.403	-.6684012 .2686178
ahorrrdista~e	-.0019831	.0032331	-0.61	0.540	-.0083199 .0043536
ahorrmis	-.0079134	.2866387	-0.03	0.978	-.5697149 .5538882
lenderdist~e	-.0017029	.0025062	-0.68	0.497	-.006615 .0032091
lendmiss	.127483	.2116165	0.60	0.547	-.2872777 .5422437
schoolpres	.1577283	.2104827	0.75	0.454	-.2548102 .5702668
healthclin~a	.5102536	.3699949	1.38	0.168	-.214923 1.23543
healthnotc~a	.2063805	.1642706	1.26	0.209	-.1155839 .528345
npop	.0089179	.0042241	2.11	0.035	.0006389 .0171969
npop2	-.0000391	.0000133	-2.95	0.003	-.0000651 -.0000131
healthserv~s	.1768049	.166712	1.06	0.289	-.1499447 .5035544
salariomin	-.0004096	.0043556	-0.09	0.925	-.0089463 .0081272
salario~bres	-.0013006	.0042928	-0.30	0.762	-.0097144 .0071131
salario~eres	-.0129331	.0058658	-2.20	0.027	-.02443 -.0014363
salariomin~s	.1013366	.3323242	0.30	0.760	-.5500068 .7526801
salariohom~s	.2470492	.420791	0.59	0.557	-.577686 1.071784
salariomuj~s	-.7643382	.3632408	-2.10	0.035	-1.476277 -.0523993
diconsadisp	-.0133361	.193446	-0.07	0.945	-.3924833 .3658111
lugar	.2285443	.2243733	1.02	0.308	-.2112192 .6683078
mercado	-.0712774	.171231	-0.42	0.677	-.406884 .2643292
grupos	.7821988	.181179	4.32	0.000	.4270945 1.137303
noaccessed~n	-.2716682	.4816335	-0.56	0.573	-1.215653 .6723161
noaccesssa~d	-.1182771	.3566584	-0.33	0.740	-.8173147 .5807605

noaccessagua		.6152366	.4669882	1.32	0.188	-.3000435	1.530517
activityno~c		-.1044617	.2823833	-0.37	0.711	-.6579228	.4489994
mpcalif		.3760802	.0747057	5.03	0.000	.2296597	.5225007
mpcalif2		-.0515684	.0124303	-4.15	0.000	-.0759314	-.0272054
_Ieduhead_1		-.0565602	.0481381	-1.17	0.240	-.1509093	.0377888
_Ieduhead_2		-.0099219	.0726256	-0.14	0.891	-.1522654	.1324216
_Ieduhead_3		.0644118	.0795779	0.81	0.418	-.091558	.2203817
_Ieduhead_4		-.0147496	.092494	-0.16	0.873	-.1960346	.1665353
alfajefe		.014083	.0437966	0.32	0.748	-.0717568	.0999228
agejefe		.0015651	.0072719	0.22	0.830	-.0126876	.0158178
agejefe2		-.000049	.0000601	-0.81	0.415	-.0001668	.0000688
agewife		-.0018536	.0058801	-0.32	0.753	-.0133783	.0096711
agewife2		.000051	.0000473	1.08	0.280	-.0000416	.0001437
males		-.0399762	.0267527	-1.49	0.135	-.0924105	.0124581
females		-.0233953	.0261913	-0.89	0.372	-.0747293	.0279387
size		-.0358336	.0067053	-5.34	0.000	-.0489757	-.0226915
indigenajefe		.3839516	.1510684	2.54	0.011	.087863	.6800402
children5		.0368559	.0283306	1.30	0.193	-.018671	.0923828
children621		.0541442	.0269408	2.01	0.044	.0013412	.1069471
children1315		.0331856	.0216633	1.53	0.126	-.0092737	.0756448
children1620		.0709268	.020215	3.51	0.000	.0313061	.1105475
women2039		.1397351	.0297957	4.69	0.000	.0813366	.1981335
women4059		.075472	.0402257	1.88	0.061	-.003369	.154313
women60plus		.1676342	.0416826	4.02	0.000	.0859377	.2493306
men2039		.1865774	.0296852	6.29	0.000	.1283955	.2447594
men4059		.1060191	.0394026	2.69	0.007	.0287914	.1832469
men60plus		.091109	.0463486	1.97	0.049	.0002674	.1819506
_cons		-1.317302	.6428341	-2.05	0.040	-2.577233	-.0573699