Fiscal Studies
The Journal of Applied Public Economics
Institute for Fiscal Studies

FISCAL STUDIES, vol. 33, no. 3, pp. 305–334 (2012) 0143-5671

The Impact of *Oportunidades* on Consumption, Savings and Transfers*

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Abstract

In this paper, we estimate the effect of the Mexican conditional cash transfer programme, *Oportunidades*, on transfers, savings and consumption for treated households. We find positive effects on consumption of non-durable and durable goods, an increase in savings coupled with a drop in the number and values of loans, and a reduction of in-kind transfers received by households in treatment areas. These results are consistent with the existing evidence that conditional cash transfer programmes have beneficial effects in both the short and medium term, but that they partly crowd out private transfers.

Policy points

- This paper provides new evidence on the effect of a conditional cash transfer (CCT) programme on consumption, savings and transfers in

*Submitted December 2009.

The authors would like to thank several staff at *Oportunidades*, who provided very useful answers to their questions about details of the data.

Keywords: consumption, savings, conditional cash transfers, impact evaluation.

JEL classification numbers: D12, I38, O15.

urban areas to complement the well-known results of its effects in rural areas.
- We find large positive effects on food consumption, an increase in savings coupled with a drop in the number and values of loans, and a reduction of in-kind transfers. This suggests that CCTs can be effective at reducing poverty in urban as well as rural areas, at least in the short run.
- In contrast to rural areas, where the transfer is used to finance productive activities (such as micro-enterprises and agricultural activities), in urban areas savings are primarily used to pay off debts. When this result is coupled with a reduction in transfers received by the beneficiary families, it suggests that CCTs in urban areas might be less successful in enabling productive activities than they are in rural areas.
- Participation is low in urban areas and only the poorest among eligible households participate. To increase the effectiveness of the programme in these areas, the amount and structure of the grant could be rethought. For example, the grant could be increased to allow households to save part of it.

I. Introduction

This paper studies the effect of the *Oportunidades* programme in urban areas in Mexico on consumption, savings, ownership of different assets, and transfers. *Oportunidades* is a conditional cash transfer which was originally targeted to the rural poor and was subsequently extended to the majority of Mexican poor families, including many living in urban areas. Conditional cash transfer programmes have received much attention because they have been perceived as effective in reducing poverty and inequality.

Studying the effects of a programme such as *Oportunidades* on consumption is important for a variety of reasons. Consumption is a synthetic indicator of household well-being, and therefore changes in consumption reflect more accurately than other variables the programme's effectiveness in reducing poverty. Previous work (Angelucci, Attanasio and Shaw, 2005; AAS05 from now on) analysed the effect of *Oportunidades* on consumption one year after the implementation of the programme. Here, we consider the effects on consumption up to two years after the programme first started, using the data collected in 2004 on the same households observed in the 2002 and 2003 evaluation surveys. The dynamics of consumption changes are important because they will reflect both the perception that individual households have of the programme and its sustainability and because they may reflect other changes in behaviour and changes the programme induces in sources of income that take time to adjust. Indeed, the evidence from the evaluations of the rural component of

Oportunidades has shown that the magnitude of the programme effect in the first year differs from the magnitude in later years, with consumption in the first year being particularly unresponsive. Therefore, if we want to have a better sense of the size of the change in consumption and the marginal propensity to consume the programme transfer, it is crucial to add at least a second year to the time span of our analysis.

In addition to overall consumption, we study how the grant is allocated between food and the rest of consumption. This is interesting for several reasons. One of the main justifications of cash transfers is the fact that poor households might have a better notion of their needs and, such needs being heterogeneous, might target the resources offered by the programme more effectively than, say, an in-kind transfer. It is therefore important to consider how the grant is spent.

Food is usually considered a necessity and thus one would expect its share to decrease with an increase in total consumption or, more generally, with living standards. This would imply that food consumption should increase proportionally less than total expenditure. However, in the case of many conditional cash transfers (CCTs), including the rural component of *Oportunidades*, it has been noted that food consumption increases in the same proportion as, if not more than, total expenditure.[1] It has been suggested that this effect might be driven by the fact that most CCTs are targeted to women and therefore change the balance of power within the household. This might shift expenditure shares to reflect the increased influence that women and their preferences might have as a consequence of the programme. It will be interesting to check whether similar effects are observed in the case of the urban component of *Oportunidades*.

The magnitude of the effect of *Oportunidades* on consumption and its components is far from obvious for many other reasons. The programme imposes a number of conditionalities, which might affect the pattern and level of consumption. Moreover, and more relevantly for this paper, the increase in resources induced by the cash transfer might lead to several changes in the household budget constraint. Income might change, because of changes in number of children or adults' labour supply. Transfers to and from other households might also change. A part of the grant might go towards the purchase of assets, which might change income in the future. It has been argued that CCTs might relax liquidity constraints and therefore allow poor households to invest in productive activities that were beyond their reach before the programme and, in that way, reduce poverty in the long run.

[1]See Angelucci and Attanasio (2011) and Attanasio and Lechene (2002 and 2011) for Mexico, Attanasio, Battistin and Mesnard (2012) for Colombia, Schady and Rosero (2008) for Ecuador and Macours, Schady and Vakis (2008) for Nicaragua.

For all these reasons, it is important and interesting to look at the possible effects of the programme on the various components of the budget constraint faced by the treated households and to establish how they were affected by the programme one and two years after its introduction. This exercise allows us to start from the grant received and match it to different components of the budget constraint. Of course, we do not expect an exact correspondence, both because the horizon covered by the interview is not the same as that of the grant and because several items of the individual budget constraint are affected by measurement error. However, we expect a rough correspondence. More importantly, the changes induced by the programme to different components of the budget constraint can be informative about the mechanisms that the programme triggers.

Therefore, in addition to consumption and its components, we study the impact that the transfer has on ownership of (and expenditure on) durable assets, some of which can be used for income-generating purposes. As the programme might facilitate poor households' access to the financial system while at the same time increasing their overall net worth, possibly reducing pre-existing debts, we estimate the impact that *Oportunidades* has on access to formal banking and on the level of financial assets and debts. We also estimate the effect of the programme on intrahousehold transfers to take into account the possibility that intrahousehold relationships may change.

The final contribution of our paper to the literature consists of studying the programme's effect on the urban poor. While there is abundant evidence on the effect of CCT programmes on the rural poor, much less is known about how they affect the well-being of their urban recipients.

The rest of the paper is organised as follows. We start in Section II with a very brief description of *Oportunidades* and of the samples used in estimation. We keep the description of the rules and parameters of the programme to a bare minimum, as information on these can be obtained from AAS05 and, in more detail, from Skoufias (2005). In Section III, we discuss the identification and estimation of treatment effects in the context of the non-experimental design of our sample. We present our results in Section IV, which contains our main contribution. Section V concludes the paper.

II. *Oportunidades*: programme and data characteristics

1. Programme features and evaluation design

Oportunidades is a conditional cash transfer programme that targets poor households in rural and urban areas and that consists of several components. As mentioned above, details of the programme's operation can be found in Skoufias (2005). Here we supply some basic information.

The programme was started under the Zedillo administration in 1998 under the name 'PROGRESA' in rural areas. *Oportunidades* constitutes a potentially important contribution to the income of eligible families. The most important elements of the programme are the nutrition, health and education components. The nutrition component consists of a cash grant for all treated households and an additional nutritional supplement for households with very young children and pregnant or lactating mothers. The cash transfer for food consumption was worth 155 pesos (or US$14[2]) per month in the second semester of 2003 and is only conditional on the family regularly attending health centres. It rose slightly over time, to 160 pesos for the first six months of 2004 and 165 pesos for the last six months of 2004 (i.e. around US$14). The educational grant is linked to regular attendance in school and depends on the grade and gender of the beneficiaries. As with the original programme PROGRESA, the education grant starts in the third grade of primary school and increases with the grade; it is higher for girls than for boys starting from the first grade of secondary school. Unlike PROGRESA, it does not stop at the last grade of secondary school but is also available during the three years of high school. In addition to monetary support, primary school children receive some school supplies at the beginning and in the middle of the academic year. Secondary and high school children receive a transfer for the acquisition of school supplies at the beginning of the academic year. No household can receive more than 1,445 pesos from a combination of grants for different children. In addition to the monetary transfers, during the last three years of secondary school, students accumulate funds that are redeemable (under certain conditions) upon graduation from high school. For students registered since their ninth grade, this additional amount is about 3,000 pesos.

The urban expansion of *Oportunidades* started in 2003. Before the beginning of the expansion, a data collection effort was started. Unlike with the evaluation of the rural programme in the late 1990s, the allocation of the programme across treatment and control areas was not random. Instead, as discussed in AAS05 and Todd et al. (2005), the programme was first offered in the blocks with the highest density of poor households. The control blocks – blocks that display similar characteristics to the treatment blocks where the programme was initially offered – were selected using a matching algorithm. That is, suppose that the dummy Z indicates whether a block is a treatment ($Z = 1$) or control ($Z = 0$) block. The programme evaluation team predicted the probability $P(Z{=}1|X)$ that a given block would be offered the programme as a function of block characteristics X. It did so by estimating a propensity score at the block level, $P(X) = P(Z{=}1|X)$. It then selected a representative sample of treatment blocks, matching them to a sample of control blocks

[2]The exchange rate is approximately 10 pesos = US$1.

with similar values of the propensity score. A final sample of 486 treatment blocks and 418 control blocks was obtained.

The data used in this paper consist of the three waves of the urban evaluation sample ENCELURB. The evaluation sample consists of 'treatment' and 'control' city blocks. The programme is offered to eligible households in treatment blocks only. The first data wave was collected in 904 blocks in 2002, before the start of the programme in urban areas. Because of the non-random allocation of the programme, the availability of a baseline survey collected before the start of the programme is crucial to control for systematic pre-existing differences in the outcomes of interest between the treatment and control samples. The second wave was collected in 2003, one year after the start of the programme in urban areas. The third wave was conducted in 2004, two years after the start of the programme.

Table 1 shows some features of the database. In this paper, we focus only on households that are eligible for the programme, which number 9,945 at the 2002 baseline. While we do not perform an in-depth analysis of attrition in our sample, we report here some information on how many households are lost between waves and on the rate of incomplete responses in the sample. In 2002, the rate of incomplete response among eligible households[3] is artificially low, as the sample does not include households that could not be classified as poor because they could not be interviewed or because they only provided incomplete information. In 2003 and 2004, a little over 1,000 households did not provide complete information, with the rates of incomplete response not being dramatically different between treatment and control groups. Only very few eligible households are lost in 2003, while the number is higher in 2004,[4] but reassuringly the rate of missing households is not substantially different between treatment and control groups. Our estimation sample is composed of 7,903 households for which we have data available, with complete responses, in all three waves. Finally, we test whether the probability that an eligible household could not be included in the final sample because of attrition is correlated with the poverty score in 2002, which is the score variable used to decide eligibility for the programme. In practice, we run a regression where the dependent variable is a dummy that takes the value 1 for the 7,903 households for which we have data available and complete for all three waves and takes the value 0 otherwise (that is, for the remaining 2,042 eligible households); the regressors include the poverty score and the full set of control variables as in Table 8. Importantly, we find that attrition is not correlated with the poverty

[3]This mainly refers to households that only responded to the first part of the questionnaire, which only contained basic demographic questions and, importantly, did not include the consumption module.

[4]In 2004, the definition of 'lost households' implies that they were present in both 2002 and 2003, but not in 2004.

TABLE 1
Database features[a]

	2002		2003		2004	
	C	*T*	*C*	*T*	*C*	*T*
Eligible (poor) households	9,945		9,934		9,192	
	3,634	6,311	3,628	6,306	3,409	5,783
With incomplete responses	88		1,178		1,046	
Percentage of eligible[b]	0.74%	0.97%	12.57%	11.45%	12.50%	10.72%
Lost in follow-ups[c]			11		742	
Percentage of eligible[d]			0.17%	0.08%	6.12%	8.29%
Households with data available in all 3 waves[e]	7,903					
	2,848	5,055				
p-value of poverty score in 'attrition' regression[f]	0.3970					

[a]C = control group and T = treatment group.
[b]Percentage of control and treatment group in each wave.
[c]The figure for 2003 refers to households present in 2002 but missing in 2003. The figure for 2004 refers to households present in 2002 and 2003 but missing in 2004.
[d]Percentage of eligible households in 2002 for values in 2003. Percentage of eligible households in 2003 for values in 2004.
[e]The figure excludes households with incomplete responses.
[f]The 'attrition' regression is a regression in which the dependent variable is a dummy taking the value 1 if the household has available and complete data in all three waves (that is, for 7,903 households) and 0 otherwise (that is, for 9,945–7,903 = 2,042 households). In addition to the poverty score, the regression includes a full set of controls as in Table 8. Results are robust to different specifications with different sets of controls.

score (we report the p-value of the poverty score coefficient in Table 1); therefore selection out of the sample should not be a concern for the potential bias and interpretation of our treatment effects.[5]

As discussed in AAS05, the treatment sample is not a representative one. In particular, participants in the programme were oversampled. Fortunately, it is possible to reconstruct the proportion of participants in the treatment areas using a census survey in the same areas, which was used as a screen to identify poor and participant households for the urban evaluation sample. These true proportions allow us to compute the appropriate weights to obtain the effect of the programme.[6] All the descriptive statistics and the estimated impacts that follow are computed using these weights.

In addition to the oversampling of participants in treatment blocks, an additional modification of the sampling frame was introduced. In some blocks, even after sampling all participants, it was perceived that the number of the latter was too small. This situation led to the inclusion in the sample of

[5]Full results of this regression are not shown but can be provided upon request.

[6]For details of the construction of these weights, see Appendix A, available online at http://www.ifs.org.uk/docs/fssep12_angeluccietal_appendices.pdf.

adjacent blocks, which are described as *barrido* or 'swept'. A problem with the *barrido* blocks is that there is no census sample for them. This implies that we cannot observe the proportion of participants among eligible households in these blocks. Indeed, only participating eligible households from *barrido* blocks were included. In computing the weights, we impute to each *barrido* block the participation rate of the adjacent regular blocks. To check robustness, we compute all our results including and excluding the *barrido* blocks. The full results are available upon request.

2. Data characteristics

In Table 2, we report the programme participation rate and the average amounts received by treated households, according to the administrative data, in 2003 and 2004. The participation rates are computed using administrative data, rather than self-reported participation. The first striking feature of this table is the relatively low participation rate, especially if compared with the rural programme, whose participation rate was greater than 90 per cent. Just over half the eligible households participate in the programme. Moreover, the proportion barely increases between 2003 and 2004. The distribution of payment is skewed, with the mean payment being above the median. It should be noted that the annual averages mask a substantial amount of variation over the year, as the educational grants are typically not paid when the school is in recess, from July to August.

Tables 3 to 7 report some descriptive statistics for our sample. All the results in these tables are computed weighting participants and non-participants differently so as to take into account the choice-based nature of our sample. We consider eligible households only – that is, those households with a sufficiently high poverty level to qualify for the programme. These households encompass programme participants and non-participants. The tables show household characteristics in 2002, unless otherwise specified, for households in treatment and control blocks. This year is our baseline, before the beginning of the programme. Table 3 shows the proportions of households with different education levels and employment statuses and

TABLE 2

Programme participation and amount of transfer

	2003	*2004*
Percentage of treated households in treatment areas	51.8	53.9
Mean amount received (monthly)	358	436
Median amount received (monthly)	275	339

Notes: Averages are weighted to account for the choice-based nature of the sample. The mean and median amounts received are for treated households only. The transfer value is in nominal pesos. The exchange rate is approximately 10 pesos = US$1.

their mean income.[7] Details of expenditures and asset ownership are given in Table 4, of savings in Table 5 and of transfers in Table 6. We also report, for each variable, a test of equality of means between treatment and control blocks.

TABLE 3

Eligible households' education, income and employment at baseline (2002)

	C	*T*	*p-value*
Proportion of households where			
head is literate	0.809	0.766	0.000
partner is literate	0.800	0.750	0.000
at least one child goes to school	0.924	0.914	0.183
at least one child works	0.074	0.125	0.000
Proportion of households with			
head with no education	0.179	0.234	0.000
head with primary education	0.550	0.529	0.125
head with secondary education	0.217	0.186	0.006
head with higher education	0.051	0.050	0.765
partner with no education	0.169	0.176	0.600
partner with primary education	0.585	0.584	0.978
partner with secondary education	0.208	0.197	0.431
partner with higher education	0.036	0.040	0.530
Mean income of			
household	3,686	3,137	0.008
household in 2001	2,303	2,266	0.602
household in 2000	2,109	1,978	0.091
household in 1999	2,181	1,762	0.000
head	1,918	1,860	0.379
partner	717	427	0.044
Proportion of households with			
institutional transfers in 2001	0.266	0.268	0.858
head employed	0.661	0.662	0.965
partner employed	0.172	0.221	0.024
head self-employed	0.200	0.213	0.349
partner self-employed	0.100	0.160	0.000
head or partner working in 2001	0.873	0.887	0.225

Notes: C = control blocks and T = treatment blocks. Averages are weighted to account for the choice-based nature of the sample. Monetary values are nominal.

[7]These statistics were computed trimming the bottom and top 1 per cent of income, to avoid the influence of extreme outliers.

TABLE 4

Eligible households' consumption and asset ownership at baseline (2002)

	C	*T*	*p-value*
Mean value of monthly non-durable expenditure for			
total	2,149	1,836	0.000
food	1,299	1,149	0.001
non-food	849	687	0.000
Proportion with zero expenditure for			
furniture	0.95	0.96	0.004
improvement to the house	0.95	0.93	0.006
home utensils	0.94	0.94	0.302
domestic appliances	0.97	0.96	0.001
vehicles	0.99	0.98	0.046
Mean value of annual expenditure for			
furniture	6.80	6.80	0.909
improvement to the house	11.40	13.20	0.223
home utensils	1.74	1.43	0.009
domestic appliances	7.53	8.68	0.199
vehicles	0.60	1.23	0.000
Proportion of households owning			
properties[a]	0.043	0.051	0.333
vehicles[b]	0.039	0.019	0.000
appliances[c]	0.75	0.735	0.562
electrics[d]	0.913	0.894	0.018
animals[e]	0.104	0.106	0.815

[a]Houses, land, etc. in addition to the house where the household lives.
[b]Cars, trucks, motorbikes, tractors and other motor vehicles.
[c]Fridges, heaters, washing and drying machines, boilers and tankers.
[d]TV sets, radios, VCRs and other devices such as PCs or microwave ovens.
[e]Used for work and/or consumption.
Notes: C = control blocks and T = treatment blocks. Averages are weighted to account for the choice-based nature of the sample. Monetary values are nominal. All expenditure values in the table include zeros.

The main conclusion from inspecting these tables is that households in treatment blocks are generally poorer and more vulnerable than households in control blocks. The difference between poverty and vulnerability is that, while poverty is an *ex-post* measure of household well-being, vulnerability is related to the likelihood of being poor in the future or to the effect of large negative income shocks. We provide more details consistent with these statements in the remainder of this section.

We consider the following proxies for poverty and vulnerability:[8] education, child labour, consumption, asset ownership and balance sheet. The higher poverty among households in treatment blocks is expected, as it

[8]Naturally, there is a degree of overlap between proxies for poverty and for vulnerability.

corresponds to the criterion for the selection of such blocks. For example, Table 3 shows that the proportion of literate household heads is more than 4 percentage points higher in control than in treatment areas. Child labour seems to be considerably more common in treatment areas. Total household income is significantly higher in control areas in 1999, in 2000 (marginally so) and in 2002. Moreover, spouses (partners) are more likely to work in treatment areas than in control areas, although they earn less in treatment areas.

Table 4 reports statistics for non-durable and durable expenditures and asset ownership.[9] Consistent with the findings from the previous table, control households exhibit considerably higher levels of consumption. Durable expenditures do not seem very informative, as hardly any households have made any purchases in the considered time span (which varies between 1 and 12 months for different commodities). It is more useful to compare the rates of asset ownership, which, when statistically different, tend to be higher for households in control blocks.

Table 5 looks at different types of savings. All the figures in this table refer to stocks and, in computing the averages, we include households with

TABLE 5

Eligible households' savings at baseline (2002)

	C	*T*	*p-value*
Proportion of households that			
contracted loans	0.124	0.240	0.000
have a bank account	0.007	0.010	0.032
have savings	0.018	0.035	0.000
have had savings in the last 12 months	0.021	0.044	0.000
Mean value of			
savings	53.6	64.6	0.585
debts	388	600	0.000
Proportion of loans (out of total loans solicited) solicited to			
savings bank	0.082	0.037	0.045
government programme	0.011	0.021	0.070
tanda[a]	0.002	0.009	0.000
moneylender	0.155	0.157	0.945
relative or friend	0.693	0.625	0.039
other	0.054	0.140	0.000

[a]This is a rotating credit association.

Notes: C = control blocks and T = treatment blocks. Averages are weighted to account for the choice-based nature of the sample. Monetary values are nominal.

[9]Non-durable consumption is defined as monthly expenditure on all the commodities on which we have information. As questions about different non-food commodities refer to different time horizons, before forming the non-food aggregate we convert all the figures into monthly flows.

TABLE 6

Eligible households' transfers at baseline (2002), excluding households with zero transfers

	C	*T*	*p-value*
Proportion of households that			
sent transfers[a]	0.041	0.063	0.000
sent monetary transfers	0.015	0.027	0.000
sent in-kind transfers	0.027	0.041	0.003
received transfers[a]	0.081	0.164	0.000
received monetary transfers	0.033	0.069	0.000
received in-kind transfers	0.056	0.119	0.000
Monetary transfers sent			
total	1,613	1,196	0.133
to the same municipality	275	523	0.020
out of the municipality	1,338	673	0.010
Monetary transfers received			
total	3,707	2,757	0.010
from the same municipality	303	675	0.029
from outside the municipality	3,404	2,082	0.005
from spouse	1,244	1,519	0.379
from offspring	1,551	551	0.014
from parent	120	97	0.563
from other relative	652	414	0.434
from non-relative	305	162	0.198
In-kind transfers sent			
total	384	263	0.442
to the same municipality	134	222	0.338
out of the municipality	250	41	0.226
In-kind transfers received			
total	430	340	0.198
from the same municipality	232	247	0.322
from outside the municipality	198	93	0.092
from spouse	1.5	10	0.000
from offspring	67	74	0.853
from parent	91	39	0.357
from other relative	278	201	0.340
from non-relative	197	222	0.438

[a]Monetary, in kind or both.

Notes: C = control blocks and T = treatment blocks. Averages are weighted to account for the choice-based nature of the sample. Monetary values are nominal.

TABLE 7

Eligible households' transfers at baseline (2002), including households with zero transfers

	C	*T*	*p-value*
Monetary transfers sent			
total	25	29	0.478
to the same municipality	4	12	0.000
out of the municipality	21	16	0.312
Monetary transfers received			
total	113	187	0.014
from the same municipality	9	39	0.000
from outside the municipality	104	148	0.150
from spouse	36	106	0.000
from offspring	45	40	0.766
from parent	3	5	0.189
from other relative	19	23	0.670
from non-relative	9	10	0.655
In-kind transfers sent			
total	7	8	0.851
to the same municipality	2	6	0.072
out of the municipality	5	1	0.360
In-kind transfers received			
total	23	38	0.012
from the same municipality	13	28	0.000
from outside the municipality	11	10	0.891
from spouse	0.05	0.57	0.003
from offspring	3	5	0.014
from parent	3	3	0.882
from other relative	10	14	0.487
from non-relative	7	19	0.003
Monetary net transfers[a] *(received–sent)*			
total	1,991	1,682	0.332
same municipality	367	837	0.146
from outside the municipality	2,711	2,210	0.188
In-kind net transfers[a] *(received–sent)*			
total	318	312	0.942
same municipality	290	272	0.737
from outside the municipality	325	382	0.792

[a]Does not include households with zero for both received and sent transfers.

Notes: C = control blocks and T = treatment blocks. Averages are weighted to account for the choice-based nature of the sample. Monetary values are nominal.

zero amounts. In particular, we consider the proportion of households that hold different types of assets (or liabilities) and the mean values of savings and of debts. Treatment households are more likely to hold debt, but also to have savings. The average level of debts is 600 pesos for treatment households and only 388 pesos for control households, and the average value of savings is also higher for treatment households (but not significantly so) and at very low levels (around 60 pesos). That is, households in treatment blocks have lower income and consumption, fewer assets and more liabilities.

We can use the data from Tables 4 and 5 to compare the ratio of assets to liabilities for the two groups of households. While we cannot actually compute this ratio, as we do not have the monetary value of the assets owned by households, it is likely that the ratio is higher for control households, as they own more assets, have higher income and hold fewer liabilities. This comparison suggests that households in treatment blocks are more vulnerable to negative shocks.

Tables 6 and 7 consider transfers to and from households, both monetary and in kind. Table 6 does not include households with zero transfers, while Table 7 does. Transfers refer to interpersonal transfers received or sent over the last 12 months, so they do not include the programme's transfers. Treatment households are considerably more likely both to send and to receive transfers (both monetary and in kind) than control households. When we consider total net transfers (see bottom part of Table 7), the differences between treatment and control households are less pronounced, whereas some differences arise in monetary net transfers both within the same municipality and outside the municipality.

III. Identification and estimation of programme impacts

1. Identification

We are interested in identifying two parameters: the average intention to treat (AIT) and the average treatment on the treated (ATT) effects. The AIT is a useful policy parameter because it measures the average programme effect on the subjects who are offered the treatment.

Our identification strategy relies on observing households living in two groups of similar blocks, only one of which is offered the treatment. Our key assumption is that, conditional on observables, block type is a valid instrument.

We define blocks where the programme is offered to poor households (Z = 1) as 'treatment blocks' and blocks where the programme is not implemented (Z = 0) as 'control blocks'. We observe outcomes for households in both block types at time t_1, almost one and two years after the implementation of *Oportunidades*, and at time t_0, prior to the start of the programme. The treatment consists of participation in *Oportunidades*. The variable Z is our instrument. Potential outcomes for household i at time t_1 are $Y_{it_1}(1)$ in the presence of the treatment ($D_{it_1}=1$) and $Y_{it_1}(0)$ without the treatment ($D_{it_1}=0$). The relationship between potential and observed outcomes is $Y_{it_1}=Y_{it_1}(1)D_{it_1}+Y_{it_1}(0)(1-D_{it_1})$. We express potential participation of household i at time t_1 as a function of the instrument: $D_{it_1}(1)$ is potential participation where the household lives in a treatment block and $D_{it_1}(0)$ is potential participation if living in a control block. Participation is zero by definition in control blocks, as the programme is not implemented there, i.e. $D_{it_1}(0)=0$. Therefore, the relationship between observed and potential outcomes is $D_{it_1}=D_{it_1}(1)Z_{it_1}+D_{it_1}(0)(1-Z_{it_1})=D_{it_1}(1)Z_{it_1}$.

Given this notation, the following equation defines the average treatment effect on the treated:

$$ATT=E\left[Y_{it_1}(1)-Y_{it_1}(0)\mid D_{it_1}(1)=1\right].$$

This notation implicitly assumes that potential outcomes for each subject are not affected by the treatment status of others, an assumption usually referred to in the literature as the stable unit treatment value assumption (SUTVA), formalised by Rubin (1980 and 1986). Our key identification assumption is that, conditional on a set of observable characteristics measured in a pre-programme time period $t = t_0$, X_{it_0}, the area of residence is independent of the potential treatment $D_{it_1}(1)$ and $D_{it_1}(0)$ and of the change in potential outcomes $\Delta Y_{it}(1)=Y_{it_1}(1)-Y_{it_0}(1)$ and $\Delta Y_{it}(0)=Y_{it_1}(0)-Y_{it_0}(0)$, i.e. $Z_i \perp \Delta Y_{it}(0),\Delta Y_{it}(1),D_{it_1}(0),D_{it_1}(1)\mid X_{it_0}$. That is, we allow residents of treatment and control blocks to have different levels of potential outcomes, but the differences are assumed to be time-invariant; therefore the differences disappear by taking their first difference.[10] Z has a positive causal effect on participation, i.e. $E\left[D_{it_1}(1)\right]>0$.

[10]One can express potential outcomes as two separate terms, one a function of X and the other of Z, with the latter term being time-invariant and constant across both potential outcomes: $Y_{it_1}(J)=Y_{it_1}(J,X)+U_i(Z)$, with J = $\{0,1\}$. $\Delta Y_{it}(J)=Y_{it_1}(J,X)-Y_{it_0}(J,X)$. Note that $Y_{it_0}(1,X)=Y_{it_0}(0,X)$ because the treatment has not started in t = t_0. Therefore, $Y_{it_1}(1)-Y_{it_1}(0)=Y_{it_1}(1,X)-Y_{it_1}(0,X)$ and $\Delta Y_{it}(1)-\Delta Y_{it}(0)=Y_{it_1}(1)-Y_{it_1}(0)$.

From the above assumptions, and dropping the subscripts for expositional ease, it follows that

$$
\begin{aligned}
&E\left[\Delta Y \mid Z=1, X\right]-E\left[\Delta Y \mid Z=0, X\right] \\
&\quad =E\left[\Delta Y(1) D(1)+\Delta Y(0)(1-D(1)) \mid Z=1, X\right]-E\left[\Delta Y(0) \mid Z=0, X\right] \\
&\quad =E\left[\Delta Y(1)-\Delta Y(0) \mid D(1)=1, X\right] P(D(1)=1 \mid X)+E\left[\Delta Y(0) \mid X\right]-E\left[\Delta Y(0) \mid X\right] \\
&\quad =E\left[Y(1)-Y(0) \mid D(1)=1, X\right] P(D=1 \mid Z=1, X).
\end{aligned}
$$

The last equality follows from SUTVA and from the conditional independence of Z from potential treatment, $P(D(1)=1|X) = P(D(1)=1|Z=1,X) = P(D=1|Z=1,X)$. Thus the ATT for individuals with characteristics X, ATT_X, can be estimated as the ratio between the expected difference in observed outcomes in treatment and control areas and the observed probability of participation in treatment areas. We can express this as a function of the propensity score $P(X) = P(Z=1|X)$:[11]

$$
\begin{aligned}
ATT_{P(X)} &= E\left[Y(1)-Y(0) \mid D(1)=1, P(X)\right] \\
&= \frac{E\left[\Delta Y \mid Z=1, P(X)\right]-E\left[\Delta Y \mid Z=0, P(X)\right]}{P(D=1 \mid Z=1, P(X))}.
\end{aligned}
$$

If we further assume common support, i.e. $P(Z=1|X) < 1$, then the ATT is

$$ATT = \int_p ATT_{P(X)=p}\, dF(p \mid D=1).$$

With this approach, one normally identifies the local average treatment effect (LATE), i.e. the average treatment effect for the set of agents who are induced to participate in the programme because of the instrument. In this particular case, though, our subjects consist only of ‘never-takers’ ($D(1)$ = $D(0)$ = 0) and ‘takers’ ($D(1)$ = 1 and $D(0)$ = 0), as we have neither ‘always-takers’ nor ‘defiers’ (Angrist, Imbens and Rubin, 1996). Therefore the subjects who are induced to participate in the programme because they are offered the treatment are all the treated subjects (Imbens and Angrist, 1994). This estimator is a conditional version of the Bloom estimator (Bloom, 1984; Heckman, 1996), where the availability of the treatment is not random, unlike in the other papers mentioned.

The numerator of $ATT_{P(X)}$ is the average intention to treat for individuals with a given value of the propensity score $P(X)$. The AIT measures the effect of the programme on eligible subjects, regardless of whether they participate

[11]Rosenbaum and Rubin, 1983.

in it. Since the policymaker often has little influence on participation, the AIT is one of the relevant parameters for policy analysis.

The AIT is also interesting because it provides a lower bound to the average treatment effect on the treated.[12] In addition, identifying the AIT requires less restrictive identification assumptions than needed for identifying the ATT, as it effectively ignores the issue of what determines participation in the programme.

In our case, the AIT is identified under the assumptions that the programme has no effect in control areas, that the changes in potential consumption in treatment and control areas are independent of areas of residence, conditional on observables, and that there is full common support, $P(Z=1|X) < 1$. Since only about half of the eligible households enrolled in the programme and spillover effects from participants to eligible non-participants are unlikely, we expect the AIT to be substantially smaller than the ATT. For example, if the programme effect were homogeneous, the AIT would be half the magnitude of the ATT in the absence of spillover effects.

Neither parameter is identified if the programme affects the consumption of poor households in control blocks. However, such effects are unlikely to occur, given the geographic distance from the *Oportunidades* blocks. To identify the ATT, we further require no indirect programme effect for eligible non-participants. While Angelucci and De Giorgi (2009) find a 10 per cent increase in consumption for non-participating households in treated rural villages, we believe that these effects are unlikely in urban areas for two reasons. First, the treated areas in rural Mexico are very small villages, with a median size of about 50 households, and most households are treated. Urban areas, on the contrary, are larger and the share of treated households is much lower. Therefore both the likelihood that treated households may share their transfers with eligible non-participants and the average amount shared are going to be much lower. Second, while the households that indirectly benefit from the programme in rural areas are not eligible for it, those in urban areas are actually eligible for the programme but do not participate. Thus, it is unlikely that they would receive transfers from treated households, given that they could enrol in the programme and receive the unconditional income support even if they chose to send no children to school. We also rule out any general equilibrium effects on prices, wages or labour supply, based on the evidence from rural areas, where there are no such effects.[13]

[12]The lower bound refers to a positive ATT, and further assumes that any effect of the treatment on eligible non-participants is smaller than the one on participants. See Hirano et al. (2000) for an application in which this latter assumption is violated.

[13]Angelucci and De Giorgi, 2009.

The other identification assumptions – the conditional independence assumption (CIA) and common support – depend on the set of conditioning variables. Therefore we will discuss them in the next subsection.

2. Estimation issues

Before estimating the programme effects on consumption, it is important to check whether, given the variables we use to estimate the propensity score, there is a sufficiently large number of control households for each treatment household and whether the CIA and SUTVA are credible.

We follow Angelucci and Attanasio (2009) to address the various estimation issues. In particular, they show that control and treatment blocks are not balanced geographically, and indeed the areas from which these blocks are sampled have different local business cycles. Therefore, it is especially important to control for pre-programme macroeconomic variables as well as individual ones.

The presence of common support is a testable assumption; therefore we proceed to see whether it is maintained in our data. We estimate the propensity score, $P(X) = P(Z=1|X)$, at the household level by probit using a wide set of observable characteristics in 2002 or earlier years. The dependent variable is a dummy indicating whether the household is resident in a block where the programme is offered ($Z = 1$) or not. The conditioning variables we use are meant to capture systematic differences between treatment and control blocks before the programme started. They include both individual- and household-level variables (such as family composition and education) and area-level variables. In particular, the variables we use are (using 2002 values, unless otherwise specified): household size dummies; number of children by age categories (0 to 5, 6 to 12, 13 to 15, and 16 to 20 years) and by age category and school attendance; wealth index as a second-order polynomial (programme eligibility is based on this index); income (as a second-order polynomial); savings (excluding those of domestic helpers and their relatives, and of individuals whose relationship to other family members is missing) and debt; transitory shocks in 2002 such as death or illness of a non-resident family member, job or business loss for a resident family member, and whether the household suffered a natural disaster; doctor visits in the previous four weeks for children, head and spouse (as three separate dummies); household head's and spouse's presence (including multiple heads), gender, education dummies (the categories are no qualification – the excluded category – incomplete primary, complete primary, incomplete secondary, complete secondary, higher education) and employment status in 2002 (employee or self-employed; the excluded category is unemployed); dummies for whether either head or spouse

TABLE 8

Probit estimates of the propensity score: marginal effects

	$P(Z=1\|X)$		$P(Z=1\|X)$
numkids0-5	0.017 (0.020)	ptremployee	0.107*** (0.027)
numkids6-12	–0.008 (0.027)	ptrse	0.117*** (0.031)
numkids13-15	–0.011 (0.022)	hhinc	0.006 (0.004)
numkids16-20	–0.024 (0.016)	hhincsq	–0.00001 (0.00001)
numsch0-5	0.004 (0.020)	hhinc01	0.003** (0.001)
numsch6-12	0.0004 (0.021)	hhinc00	–0.001 (0.003)
numsch13-15	0.002 (0.027)	hhinc99	–0.001 (0.001)
numsch16-20	–0.021 (0.027)	hhworked01	0.010 (0.031)
sfem	0.122*** (0.034)	hhworked00	0.005 (0.032)
nopartner	0.041 (0.040)	hhworked99	0.067** (0.034)
hoheduc-1	–0.055* (0.032)	savings	0.001 (0.006)
hoheduc-2	–0.107*** (0.039)	debt	0.002 (0.002)
hoheduc-3	–0.049 (0.044)	death	0.050* (0.027)
hoheduc-4	–0.115** (0.055)	unemp	0.127*** (0.032)
hoheduc-5	–0.179** (0.077)	bust	–0.193** (0.089)
ptreduc-1	0.136*** (0.037)	disaster	0.176*** (0.067)
ptreduc-2	0.120*** (0.039)	wealth	–0.137*** (0.045)
ptreduc-3	0.170*** (0.043)	wealthsq	0.006 (0.011)
ptreduc-4	0.123*** (0.037)	GDP00	–14.37*** (2.800)
ptreduc-5	0.173*** (0.043)	GDP01	0.260 (3.706)
hohemployee	0.082** (0.041)	GDP02	8.581*** (3.196)
hohse	0.073* (0.042)		

Area characteristics: No
Household size dummies: Yes
Doctor visit dummies: Yes
Income joint significance: 14.84***
Observations: 8,324

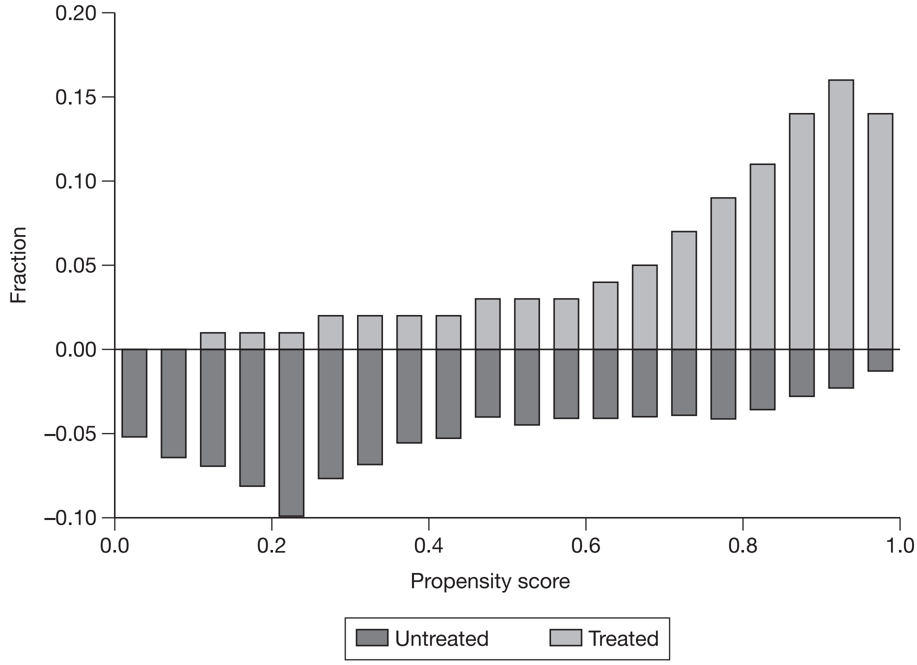
Notes: See next page.

Notes to Table 8
Robust standard errors are given in parentheses; clustering at the locality level. */**/*** means significant at 10/5/1 per cent level. hh = household; hoh = head of household; ptr = partner (e.g. hhworked01 is a dummy for whether the household head or spouse was employed in 2001). Unless otherwise specified, all variables are from 2002. A more detailed variable description is given in the online appendix, available at http://www.ifs.org.uk/docs/fssep12_angeluccietal_appendices.pdf.

worked in 1999, 2000 and 2001; and income of head and partner in 1999, 2000 and 2001 (as a linear term). Lastly, we add state annual GDP growth in 2000, 2001 and 2002 to control for differential trends between treatment and control blocks.

We show the estimated marginal effects of the propensity score in Table 8. These confirm that treatment blocks are poorer than control blocks, as the households living in treatment blocks have lower wealth, a larger share of uneducated household heads (the excluded category), and a higher likelihood of suffering from transitory shocks (except loss of business) and of being headed by females without a partner, normally associated with high indigence. Interestingly, though, residents of treatment blocks also have higher employment rates (both as employees and self-employed), higher education for the spouse of the household head and income no different from

FIGURE 1
The propensity score



that of residents of control blocks (with the exception of 2001 income, which is higher in treatment blocks), conditional on the other observable characteristics. Lastly, treatment and control blocks have different state GDP growth rates, confirming that they are not balanced at the geographic level. In sum, this table shows the need to rebalance the observables between treatment and control blocks.

Figure 1 shows that the common support is complete – that is, for each household in the *Oportunidades* blocks, we have a sufficiently high number of close matches from control blocks. Full common support ensures we can compute average treatment effects for the entire sample of eligible and treated households, respectively, and not only for non-random subgroups of families.

We now provide indirect evidence in favour of our conditional independence and absence of spillover effects assumptions (CIA and SUTVA). While these identification assumptions are not directly testable, the evidence provided below supports our conjecture that the CIA holds, given the chosen set of conditioning variables, and that there are no indirect effects of *Oportunidades* on non-participating households' consumption.

The main issue for the CIA validity is whether we have successfully controlled for differential trends between treatment and control blocks, since our difference-in-difference approach controls for time-invariant unobserved differences. The evaluation surveys contain retrospective information on income, covering several years. This allows us to check whether there are differential trends in income before the introduction of the programme between treatment and control areas.[14] While we do not report these results here, we identify some differences in pre-2003 income and female employment growth between treatment and control areas. We suspect that these differential trends depend on the lack of geographic balance between treatment and control blocks, which come from different states. To address this issue, our set of conditioning variables includes state GDP growth for 2000, 2001 and 2002.

Adding state GDP growth to the set of variables we use to estimate the propensity score has a sizeable effect on the estimated treatment effects. We show this by estimating the average treatment effect on the change in the log of consumption for the non-poor, alternatively adding and omitting pre-programme state GDP growth. Since these households are not eligible for the programme, we expect the treatment effect to be zero. This is exactly what we find when we condition on GDP growth: Table 9 shows that the effect of *Oportunidades* on the non-poor's log of consumption is –0.010 and not statistically significant (column 1). However, when we fail to control for the difference in GDP growth, we estimate a positive, significant and large

[14]See also Angelucci and Attanasio (2009).

TABLE 9

Average treatment effect on log-consumption for the non-poor

	Log-consumption	
	(1)	*(2)*
ATE	–0.010 (0.081)	0.148*** (0.056)
GDP growth	Yes	No
Observations	3,528	

Notes: Standard errors estimated with the block-bootstrap (1,000 replications). The block is the city block. */**/*** means significant at 10/5/1 per cent level.

treatment effect: consumption appears to be about 15 per cent higher for the non-poor in treatment areas (column 2). This exercise also indirectly validates the SUTVA: the estimate in column 1 suggests that, given the chosen set of conditioning variables, there are no spillover effects of the programme among the non-poor living in treatment blocks.

IV. The impact of *Oportunidades* on consumption, wealth and transfers

In this section, we first present the results obtained applying the methods described in the previous section to several different outcomes. We then briefly discuss our interpretation of the findings.

1. Results

According to the estimates in Table 10, the main effect of the programme on treated households is an increase in food consumption by 169 and 283 pesos in 2003 and 2004. The effect on non-durable, non-food consumption is

TABLE 10

Average treatment effects of Oportunidades *on non-durable consumption*

	Food		Non-food	
	2003	*2004*	*2003*	*2004*
AIT	44.06 (57.00)	86.12* (46.20)	–58.31 (64.82)	31.21 (57.96)
ATT	168.54* (108.87)	282.85*** (95.82)	–57.00 (120.88)	141.57 (110.58)
Observations	7,322	6,824	7,320	6,829

Notes: Local linear regression (llr) matching estimates. The estimates from llr are similar to the ones obtained using the five nearest neighbours with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 replications. The block is the city block. */**/*** means significant at 10/5/1 per cent level. Monetary values are nominal.

negative and insignificant in 2003, and positive and insignificant in 2004. Summing the estimated effects for food and non-food consumption in the first two years of the programme, the total amount spent on non-durable consumption is about 68 per cent of the average transfer size. The share of the transfer consumed, however, seems to vary over time. Indeed, while non-durable consumption is considerably smaller than the average transfer in 2003, in 2004 one cannot reject the hypothesis that all the transfer is consumed.

Table 11 shows the estimated treatment effects on durable expenditures. These effects are positive and significant, but small, averaging about 5 pesos per month. We interpret these results as evidence that most of the effect of the treatment on consumption is on non-durable, rather than durable, goods. This pattern of findings – the size of the effect on consumption growing over time, with the bulk of the effect being on non-durable and in particular food consumption – is similar to the results from the evaluation of the rural component of the programme.

Overall, the results on consumption suggest that the eligible households that participate in the programme may be saving part of the transfer in 2003, but they seem to be spending a larger amount in 2004. This result can be explained by the fact that in 2003 beneficiaries were not sure about the continuation of the programme. Moreover, at the programme's inception, payment might have been irregular and plagued by delays.

Notice that the ATT is not simply obtained by dividing the AIT by the participation rate. As we mentioned in Section III, we compute the AIT and the corresponding ATT for a given set of *X*s and then aggregate. This averaging explains why, for instance, the AIT on non-food non-durable consumption is –58 in 2003 while the ATT is –57 (although neither figure is significantly different from zero).

We now look at the effect of the programme on savings and loans. Table 12 shows programme effects on the probability of having savings and loans/debts, and the respective amounts, while Table 13 provides information on the probability of having a bank account and on the number of loans requested. Despite a small increase in the likelihood of having savings in both years, and positive effects on the likelihood of having a bank account, we find no effects on the amounts of savings in either year. Instead, while there is no change in the number of loans asked for (coefficients are significant but very small), we estimate a considerable decrease in debts, both in the proportion of households holding one and in the amounts. The decrease in loans for participants, of roughly 350 pesos in 2003 and 990 pesos in 2004, might be considered implausibly large. However, it should be stressed that the loan is a stock rather than a flow and that therefore the effect we are measuring should be compared not with the average monthly

TABLE 11

Effect of Oportunidades *on durable assets*

	Monthly expenditure		Ownership of property		Ownership of transport		Ownership of electrics		Ownership of appliances	
	2003	*2004*	*2003*	*2004*	*2003*	*2004*	*2003*	*2004*	*2003*	*2004*
AIT	2.54***	2.37**	0.006	–0.008	–0.003	–0.008	0.116***	0.063**	0.059**	0.030
	(0.71)	(1.06)	(0.018)	(0.015)	(0.009)	(0.010)	(0.025)	(0.027)	(0.027)	(0.027)
ATT	5.82***	5.49**	0.015	–0.022	–0.007	–0.021	0.269***	0.147**	0.136**	0.072
	(1.67)	(2.41)	(0.041)	(0.036)	(0.020)	(0.024)	(0.059)	(0.063)	(0.062)	(0.061)

Notes: Local linear regression (llr) matching estimates. The estimates from llr are similar to the ones obtained using the five nearest neighbours with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 replications. The block is the city block. */**/*** means significant at 10/5/1 per cent level. Monetary values are nominal. The number of observations is 7,890 for 2003 and 7,885 for 2004.

TABLE 12

Effect of Oportunidades *on savings and loans*

	Savings (proportion of households)		Savings (amount)		Debts (proportion of households)		Debts (amount)	
	2003	*2004*	*2003*	*2004*	*2003*	*2004*	*2003*	*2004*
AIT	0.034***	0.005	67.75	–13.92	–0.058***	–0.107***	–169.91	–518.33***
	(0.011)	(0.012)	(42.73)	(75.78)	(0.024)	(0.037)	(218.16)	(170.13)
ATT	0.130***	0.060**	111.51	–20.57	–0.115***	–0.217***	–347.93	–991.65***
	(0.031)	(0.029)	(76.52)	(138.04)	(0.049)	(0.066)	(414.12)	(307.51)

Notes: Local linear regression (llr) matching estimates. The estimates from llr are similar to the ones obtained using the five nearest neighbours with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 replications. The block is the city block. */**/*** means significant at 10/5/1 per cent level. Monetary values are nominal. The number of observations is 7,890 for 2003 and 7,885 for 2004.

transfer that we have mentioned so far, but with the total amount received up to 2003 and up to 2004. The average beneficiary family in 2003 had received 3,792 pesos, and in 2004 the cumulative average was 8,196 pesos, using 2002 prices. The declines of 350 and 990 pesos therefore imply that about 8–10 per cent of the grant was used to repay debts.

This number is not inconsistent with the evidence we have presented on consumption, where the point estimates indicated that part of the grant was saved in 2003 and that most of the grant was consumed in 2004.

TABLE 13

Effect of Oportunidades *on bank accounts and loans*

	Probability of having a bank account		Number of loans requested	
	2003	*2004*	*2003*	*2004*
AIT	0.075***	0.072***	–0.054**	–0.096***
	(0.023)	(0.021)	(0.023)	(0.029)
ATT	0.174*	0.169***	–0.128**	–0.224***
	(0.056)	(0.047)	(0.055)	(0.068)

Notes: Local linear regression (llr) matching estimates. The estimates from llr are similar to the ones obtained using the five nearest neighbours with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 replications. The block is the city block. */**/*** means significant at 10/5/1 per cent level. Monetary values are nominal. The number of observations is 7,890 for 2003 and 7,885 for 2004.

TABLE 14

Effect of Oportunidades *on transfers*

Proportion of households

	Monetary				*In kind*			
	Sent		Received		Sent		Received	
	2003	*2004*	*2003*	*2004*	*2003*	*2004*	*2003*	*2004*
AIT	0.006	–0.0002	–0.003	–0.004	–0.005	–0.018	–0.042*	–0.041*
	(0.009)	(0.008)	(0.013)	(0.013)	(0.014)	(0.015)	(0.022)	(0.024)
ATT	0.014	–0.001	–0.009	–0.011	–0.012	–0.042	–0.098**	–0.097*
	(0.022)	(0.019)	(0.029)	(0.030)	(0.033)	(0.034)	(0.050)	(0.055)

Average transfers (including zero)

	Monetary				*In kind*			
	Sent		Received		Sent		Received	
	2003	*2004*	*2003*	*2004*	*2003*	*2004*	*2003*	*2004*
AIT	2.34	–16.10	82.86	–234.16	2.46	4.42	20.63	–29.60**
	(20.91)	(12.85)	(166.46)	(219.62)	(7.91)	(7.38)	(16.83)	(14.69)
ATT	8.92	–41.21*	155.93	–424.90	4.93	11.40	21.94	–68.51***
	(46.02)	(28.31)	(326.26)	(397.77)	(15.30)	(13.55)	(33.51)	(27.81)

Notes: Local linear regression (llr) matching estimates. The estimates from llr are similar to the ones obtained using the five nearest neighbours with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 replications. The block is the city block. */**/*** means significant at 10/5/1 per cent level. Monetary values are nominal. The number of observations is 7,890 for 2003 and 7,885 for 2004.

An additional explanation for these observed lower loans is that they are partly a form of crowding out of private transfers, as informal loans from family and friends may be types of transfers used to insure against risk.

Table 14 provides estimates of the programme effect on transfers. We include in the sample households with both positive and zero transfers. While *Oportunidades* does not affect monetary transfers (with the exception of a weakly significant 41 peso drop in transfers sent in 2004), the programme causes a drop in the receipt of in-kind transfers in 2004: treated households are about 10 percentage points less likely to receive transfers in both years, and the amount of in-kind transfers received has a significant drop of 69 pesos in 2004. Like Albarran and Attanasio (2003) in the case of rural PROGRESA, we find some evidence of crowding out for private transfers. This reduction, however, is quite modest and limited to in-kind transfers.

2. Interpretation

The picture that emerges from these results is reasonably clear. Urban *Oportunidades* increases consumption. The increase is substantially larger in the second year than in the first. This evidence is consistent with the evidence from other programmes and probably reflects that in the first year households might have had doubts about the continuation of the programme and there might have been logistical difficulties that meant that grant payments were sometimes delayed.

As in most CCTs, most of the increase in consumption is on food. As we mentioned in the introduction, this might reflect a shift in the relative weights that husbands and wives have in the allocation of resources within the household. The fact that the share of food does not decrease (and if anything increases) with the increase in total expenditure conflicts with the notion that food is a necessity and it probably reflects a shift in household preferences.

In terms of magnitudes, we estimate that after two years, households are spending about 68 per cent of the grant. This is even lower than the 75 per cent spent in rural areas and implies that other components of the budget constraint have changed. The evidence we have reported here seems to indicate that *Oportunidades* households have considerably reduced their indebtedness. At the same time, they have increased their access to the formal financial system: we register a non-negligible increase in the proportion of households that have a bank account.

As for crowding out of private transfers, we find only limited evidence of a reduction in private transfers to the beneficiary households. Therefore there do not seem to be spillovers of the *Oportunidades* grant through this channel.

A final important issue that needs to be kept in mind is the limited participation of eligible households in the programme in urban areas. This feature, discussed at length in Angelucci and Attanasio (2011), from a statistical point of view means that there is a large difference between AIT and ATT. However, from a substantive point of view it is an indication of the fact that the programme is probably much less attractive to potential urban beneficiaries than to their rural counterparts. This difference in the attractiveness of the programme is probably also reflected in the use to which programme beneficiaries put the grant.

V. Conclusions

In this paper, we report some results on the effects of the *Oportunidades* programme on consumption, savings and transfers in urban areas. We make a distinction between the so-called average intention to treat, which measures the effect of the programme on the eligible population, and the average treatment on the treated, which measures the effect of the programme on recipients. The distinction between the two is very important in our context because of the low participation rates in the programme.

The main programme effects are: (1) an increase in food consumption of roughly half or two-thirds the size of the transfer in 2003 and 2004, respectively; (2) a small increase in expenditure on durable items accompanied by a small increase in the ownership of certain electric goods; (3) a small drop in received transfers, especially in kind; and (4) a reduction in the stock of debt that is roughly equivalent to 8–10 per cent of the monthly transfer.

Some aspects of the results are consistent with those obtained in the evaluation of the rural component of *Oportunidades*. For instance, both the large effect on food consumption and the crowding out of private transfers are consistent with the evidence from the rural component of the programme. Other aspects of the results, however, are different from those in rural areas. The most noticeable difference is the fact that while it seems that in rural areas beneficiary households are able to save a fraction of the grant or spend it on productive activities, the results we have reported for urban areas establish that savings are primarily used to pay off debts. How beneficial this is for the families depends on how costly this debt is. Taking into account the evidence of a reduction in transfers received by the beneficiary families, however, it seems unlikely that in the medium run the programme could generate the type of saving, and resulting acquisition of productive assets, observed in rural areas.

A possible explanation for these differences could be related to the low participation in the programme in urban areas. The evidence we have seems to indicate that those who choose to enrol into the programme are the poorest households among those eligible in urban areas. This might reflect that the size of the grant, kept at the same level as in rural areas, might be insufficient to induce participation by those households that would be most likely to save part of it. If the households that participate in urban areas are selected from the poorest among the eligible ones, as seems to be the case from the participation equation, it is possible that these households are less able to save even small fractions of the grant. Whether this is the case is an interesting area for further research. This intuition, if supported by additional investigations, would call for an increase, or at least a restructuring, of the grant in urban areas.

Another result of interest is the fact that, proportionally, the increase in food consumption is a very large share of the increase in total consumption. This contradicts the prediction of a simple Engel curve for food. If food is a necessity, one would expect that, in the face of an exogenous shift in income, food consumption would increase less than proportionally relative to other components of consumption. Instead, we observe that the increase in food consumption is of the same order of magnitude as that of other components of consumption. There are two (not mutually exclusive) explanations for this phenomenon. The first points out that food, as an aggregate, is extremely heterogeneous, including basic staples, such as rice and tortilla, and more expensive items, such as meat. We know from the results in Attanasio and Shaw (2005) that most of the increase in food is observed in meat and other similar commodities. It is therefore plausible that behind the increase in the amount spent on food there is an upgrading of quality. The second explanation refers to the fact that the *Oportunidades* transfer is given to the mothers, who might have different preferences from the fathers. If that is the case, the fact that women have greater control of the family budget would modify the pattern of consumption for each level of income. This is the thesis suggested and tested by Attanasio and Lechene (2002) and Angelucci and Attanasio (2011), among others.

The fact that the crowding out of private transfers is very limited is another interesting aspect of our findings. It means that the *Oportunidades* transfer reaches its intended beneficiaries. This is particularly important given the limited participation in the programme and evidence, documented in Angelucci and Attanasio (2011), that the participants are the poorest of the eligible households.

Finally, a very interesting and positive aspect of the results is the considerable reduction in debt and the increased participation in the formal financial system that we seem to find. While we do not observe the increase in investment in productive activities documented in Gertler, Martinez and Rubio-Codina (2011), or at least not one of the same magnitude, the fact that beneficiary households use almost 10 per cent of the grant to reduce their outstanding debt is suggestive that the impacts of the programme could be going in the same direction.

What do these results imply for the success of the programme? Obviously, our findings do not provide all the elements that would be necessary for a full cost–benefit analysis of *Oportunidades*. For such an analysis, it would be necessary to take into account all of the programme's impacts (including those on health and education) and all the consequences that these impacts have for the accumulation of human capital in the long run. This is well beyond the scope of this paper. However, in terms of the welfare of the current beneficiaries in the current period, what happens to consumption is probably most important. For one thing, consumption has a direct impact on welfare, nutritional status and satisfaction. And, more subtly, individual consumption decisions are informative of individual perceptions of available current and possibly future resources. Moreover, the analysis of the impacts of transfers, assets and access to the financial system that we have provided is informative of the mechanisms at play in generating the overall impacts of the programme. In this respect, although other papers have pointed out that the programme has had limited impacts on education and health outcomes in urban areas, the effects we have shown on consumption are important because they show that *Oportunidades* has been effective in reducing poverty, at least in the short run. This result is relevant in the current policy debate in which it has been pointed out that CCTs or, more generally, cash transfers might be relevant for developing countries mainly as effective redistributive tools.

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