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 program, *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 programs have beneficial effects in both the short and medium term, but that they partly crowd out private transfers.

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1 Introduction

This paper studies the effect of the Oportunidades program 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 rural poor and subsequently was extended to the majority of Mexican poor families, including many living in urban areas. These programs have received much attention because they have been perceived as effective in reducing poverty and inequality.

Studying the effects of program such as Oportunidades on consumption is important for a variety of reasons. Consumption is a synthetic indicator of household wellbeing and therefore changes in consumption reflect more accurately than other variables the effectiveness of the program in reducing poverty. Unlike previous work (Angelucci, Attanasio and Shaw, 2004; AAS04, from now on), which analyzed the effect of Oportunidades on consumption one year after the implementation of the program, here we consider the effects on consumption up to two years after the program was 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 is important because will reflect both the perception that individual households have of the program and its sustainability and because it may reflect other changes in behavior and in sources of income induced by the program that take time to adjust. Indeed, the evidence from the evaluations of the rural component of Oportunidades has shown that the magnitude of the program 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 program transfer, it is crucial to add at least a second year to the time span of our analysis.

In addition to overall consumption, we also 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, being such needs heterogeneous, might target the resources offered by the program more effectively that, say, an in-kind transfer. It is therefore important to consider how the grant is spent.

Food is usually considered a necessity and, therefore, one would expect its share 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, including the rural component of Oportunidades, it has been noted that food consumption increases in the same proportion if not more than total expenditure (see Angelucci and

Attanasio, (2011) and , Attanasio and Lechene, (2002, 2011), for Mexico, Attanasio, Battistin and Mesnard (2011) for Colombia, Schady and Rosero (2008) for Ecuador, Macours et al. (2008) for Nicaragua). 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 have an effect in shifting expenditure shares to reflect the increased influence that women and their preferences might have as a consequence of the program. It is therefore 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 program 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 children or adults labor 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 CCT might relax liquidity constraints and therefore allow poor households to invest into productive activities which were beyond their reach before the program and, in that way, reduce poverty in the long run.

For all these reasons, it is important and interesting to look at the possible effects of the program on the various components of the budget constraint faced by the treated households and to establish how they were affected by the program one and two years after its introduction. This exercise allows us to start from the grant received and match into 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 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 program to different components of the budget constraint can be informative about the mechanisms that the program 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 program might facilitate the access of poor households to the financial system while at the same time increasing their overall net worth, possibly reducing pre-existing debts, we estimate the impact that the program has on the access to formal banking and on the level of financial assets and debts. Finally, as it has been argued that when considering the effect of a transfer program (conditional or unconditional) one has to consider the possibility that intrahousehold relationships change, we also estimate the impact of the program on intrahousehold transfers.

The final contribution of our paper to the literature consists of studying the program's effect on the urban poor. While there is abundant evidence on the effect of CCT program on the rural poor, much less is known on how this class of programs affects the well-being of its urban recipients.

The rest of the paper is organized as follows. We start with a very brief description of the program and of the samples used in estimation, in Section 2. We keep the description of the rules and parameters of the program details at a bare minimum, as these can be obtained from AAS04, and, in more detail, in Skoufias (2005). In section 3, we discuss the identification and estimation of treatment effects in the context of the non-experimental design of our sample. In section 4, we present our results. This section contains the main contribution of our paper. Section 5 concludes the paper.

2 *Oportunidades*: program and data characteristics

2.1 Program features and evaluation design

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

The program was started under the Zedillo administration in 1998 under the name of PROGRESA in rural areas. The most important elements of the program are the nutrition, health and education components. The nutrition component consists of a cash grant for all treated households and an additional a nutritional supplement for households with very young children and pregnant or lactating mothers. The educational grant is linked to regular attendance in school and starts on the third grade of primary school and continues until the last grade of secondary school (Preparatoria). Oportunidades constitutes a potentially important contribution to the income of eligible families.

The cash transfer for food consumption was worth 155 pesos (or 14 US$) per month in the second semester of 2003 and it is only conditional on regular attendance of the family to health centres, while the educational grant depends on the grade and gender of the beneficiaries. As with the original program PROGRESA, the education grant increases with the grade and is higher for girls than for boys starting from the 1st grade of secondary school. Unlike its predecessor PROGRESA, it does not stop at the 3rd 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, also. Each household cannot receive, by combining grants for different children, more than 1445 pesos. In addition to the monetary transfers, during the last 3 years of secondary school (preparatoria) students accumulate funds that are redeemable (under certain conditions) upon graduation from high school. For students registered since their 9th grade, this additional amount is about 3,000 pesos.

The urban expansion of Oportunidades was started in 2003. Before the beginning of the expansion, a data collection effort was started. Unlike with the evaluation of the rural program in the late 1990s, the allocation of the program across treatment and control areas was not random. Instead, as discussed in AAS04, and Todd et al. (2004), the program was first offered in the blocks with the highest density of poor households. The process of selection of the control blocks - blocks that display similar characteristics as the treatment blocks where the program is initially offered, occurred through a matching algorithm. That is, suppose that the dummy Z indicates whether a block is a treatment ($Z = 1$) or control block. The program evaluation team predicted the probability $P(Z = 1|X)$ that a given block is offered the program 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 with similar values of the propensity score. It obtained a final sample of 486 treatment blocks and 418 control blocks.

The data used in this paper consists of the three waves of the urban evaluation sample ENCELURB. The evaluation sample is made of 'treatment' and 'control' city blocks. The program is offered to eligible households in treatment blocks only. The first data wave was collected in 2002 in 904 blocks, before the start of the program in urban areas.

Because of the non-random allocation of the program, the availability of a baseline survey, collected before the start of the program (2002), 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 program in urban areas. The third wave was conducted in 2004, two years after the start of the program.

Table 1 shows some features of the database. In this paper we focus only on households eligible (poor) for the program, which at the 2002 baseline are 9945. While we do not perform an in-depth analysys of attrition in our sample, we report here some information on how many households are lost between waves and rate of incomplete responses in the sample. In 2002 the rate of incomplete responses of eligible households (this mainly refers to households who only responded to the first part of the questionnaire, which only included basic demographic questions and, importantly, did not include the consumption module) is artificially low, as the sample does not include households who could not be classified as poor, because they could not be localized or only provided incomplete information. In 2003 and 2004 a bit more than 1000 households did not provide complete information, with rates of incomplete response which are not dramatically different between treatment and control groups. Only very few eligible households are lost in 2003, while the number is higher in 2004[1], but reassuringly the rate of missing households is not substantially different between treatment and control groups. Our estimation sample is composed of 7903 households for which we have data available, with complete responses, in all 3 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 program. In practice, we run a regression where the dependent variable is a dummy that takes value 1 for the 7903 households for which we data available and complete for all the 3 waves and 0 otherwise (that is, for the remaining 9945-7903=2042 eligible households) and 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 score (we report the pvalue of 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 .[2]

As discussed in AAS04, the treatment sample is not a representative one. In particular, participants into the program were over-sampled. Fortunately, it is possible to reconstruct the proportion of participants in the treatment areas using a census survey in the same areas that was used as a screen to identify poor and participant households for the urban evaluation sample. These true proportions allowed us to compute the appropriate weights to obtain the effect of the program (see Appendix A for details of the construction of these weights). All the descriptive statistics and the estimated impacts that follow are computed using these weights.

In addition to the over-sampling 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 adjacent blocks, which are labelled as *barridos*, 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 participant 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, all our results were computed including and excluding the *barrido* blocks.

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

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

2.2 Data characteristics

In Table 2, we report the program 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 program. Just over half the eligible households participate into the program. Moreover, the proportion does not increase and, if anything, declines between 2003 and 2004. The distribution of payment is skewed, with the mean payment being above the median. It should be noticed 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. The cash transfer for food consumption changed slightly over time: in 2003 it was 155 pesos, it was raised to 160 for the first six months of 2004 and 165 for the last six months of 2004 (that is around 15US$).

The following tables report some descriptive statistics of our samples. All the results in these tables are computed weighting participants and non participants differently so 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 program. These households encompass program participants and non-participants.

Tables 3 to 7 show household characteristics in 2002, unless otherwise specified. This is our baseline, before the beginning of the program. We show the means of education, income, employment, expenditures and asset ownership, savings, and transfers for households in treatment and control blocks.[3] We also report, for each variable, a test of equality of means between these two groups.

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.[4] We provide more details consistent with these statements in the remainder of this section.

We consider the following proxies for poverty and vulnerability: education, child labor, consumption, asset ownership, and balance sheet. The higher poverty among households in treatment blocks is expected, as it corresponds to the criterion for the selection of such blocks. For example, Table 4 shows that the

[3]In the case of earnings and income, we consider both means and medians. These statistics were computed trimming the bottom and top 1% of income, to avoid the influence of extreme outliers.

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

proportion of literate household heads is more than four percentage points higher in control than treatment areas. Child labor seems to be considerably more common in treatment areas. Total household income is significantly higher in control areas in 1999, (marginally so) in 2000, 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.[5] 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 household has made any purchase in the considered time span (which ranges 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 zero amounts.[6] In particular, we consider the proportion of households that holds different types of assets (or liabilities) and the mean values of the same. 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 for control households, while the average value of savings is higher (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 and liabilities for the two groups of households. While we cannot actually compute these ratios, as we do not have the monetary value of the assets owned by households, it is likely that this 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.

Lastly, Tables 6 and 7 consider transfers to and from the households, both monetary and in kind. Table 6 does not include households with zero transfers, while Table 7 does. Transfers refer to interpersonal transfers (therefore they do not include the program's transfers) received (or sent) over the last twelve months. Treatment households are considerably more likely to both send and receive transfers (both monetary and in-kind) than control households. When we consider total net transfers (see bottom part

[5]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.

[6]If one is interested in averages conditional on ownership, one can obtain them by dividing the amounts in the bottom panel by the fractions in the top panel.

of Table 7) the differences between treatment and control households are less pronounced, whereas some differences arise in monetary net transfers both in the same municipality or out of the municipality.

3 Identification and estimation of program impacts

3.1 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 program 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.

Define blocks where the program is offered to poor households ($Z = 1$) "treatment blocks" and blocks where the program is not implemented ($Z = 0$) "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 program start. The treatment consists of participation to *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 $Y_{it_1} = Y_{it_1}(1)D_{it_1} + Y_{it_1}(0)(1 - D_{it_1})$. Express potential participation of a household i at time t_1 as a function of the instrument: $D_{it_1}(1)$ is potential participation where the household to live in a treatment block and $D_{it_1}(0)$ is potential participation if living in control blocks. Participation is zero by definition in control blocks, as the program 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:

$$ATT = E[Y_{it_1}(1) - Y_{it_1}(0)|D_{it_1}(1) = 1]$$

This notation implicitly assumes that potential outcomes for each subject are not affected by the treatment status of others an assumption usually referred in the literature as the Stable Unit Treatment Value Assumption (SUTVA), formalized by Rubin (1980, 1986). Our key identification assumption is that, conditional on a set of observable characteristics measured in a pre-program time period $t = t_0$, X_{it_0}, 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 $\Delta Y_{it}(1) = Y(1)_{it_1} - Y(1)_{it_0}$ and $\Delta Y_{it}(0) = Y(0)_{it_1} - Y(0)_{it_0}$, i.e. $Z_i \perp \Delta Y_{it}(0), \Delta Y_{it}(1), D_{it_1}(0), D_{it_1}(1)|X_{it_0}$.

That is, we allow residents of program and control blocks to have different levels of potential outcomes, but the differences are assumed to be time-invariant, therefore they disappear by taking their first difference.[7] Z has a positive causal effect on participation, that is $E[D_{it_1}(1)] > 0$.

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

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

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 $P(X) = P(Z=1|X)$ (Rosenbaum and Rubin 1983):

$$ATT_{P(X)} = E[Y(1) - Y(0)|D(1)=1,P(X)] = \frac{E[\Delta Y|Z=1,P(X)] - E[\Delta Y|Z=0,P(X)]}{P(D=1|Z=1,P(X))}$$

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

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

With this approach one normally identifies the LATE, i.e. the average treatment effect for the set of agents who are induced to participate in the program 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 program because they are offered the treatment are all the treated subjects (Angrist and Imbens 1994). This estimator is a conditional version of the Bloom estimator (Bloom 1984, and 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 intent to treat (AIT) for individuals with a given value of the propensity score $P(X)$. The AIT measures the effect of the program on eligible subjects, regardless

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

of whether they participate in the program or not. Since often the policy maker has little influence on participation, the AIT is one relevant parameters for policy analysis.

The AIT is also interesting because it provides a lower bound to the ATT under the assumption that the program effect on non participants in the treatment group is lower than its effect on participants.[8] In addition, identifying the AIT requires less restrictive identification assumptions than for the ATT, as it effectively ignores the issue of what determines participation in the program.

In our case, the AIT is identified under the assumptions that the program 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 program 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 program 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 program affects the consumption of poor households in control blocks. However, such effects are unlikely to occur, given the geographic distance with the *Oportunidades* blocks. To identify the ATT we further require no indirect program effect for eligible non-participants. While Angelucci and De Giorgi (2009) find a 10% 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. Further, while the households who indirectly benefit from the program in rural areas are not eligible for the program, those in urban areas are actually eligible for the program, but do not participate. Thus, it is unlikely they will receive transfer from treated households while they could enroll in the programs 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 labor supply based on the evidence from rural areas, where there are no such effects (Angelucci and De Giorgi 2009).

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

[8]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.

3.2 Estimation issues

Before estimating the program 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 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 and 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-program macro-economic 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 of blocks where the program is offered ($Z = 1$) or not. The conditioning variables we use are meant to capture systematic differences between treatment and control blocks before the program was 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) grouped according to their status (working, going to school, or neither), poverty index as a second-order polynomial (program eligibility is based on this index), income (as a second-order polynomial), savings (excluding domestic helpers and their relatives, and individuals whose relationship to other family members is missing) and debt, transitory shocks in 2002 such as death or illness of non-resident family member, job or business loss for 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, literacy, education dummies (the categories are: no qualification, incomplete primary, complete primary, incomplete secondary, complete secondary, higher education), employment status in 2002 (employee or self-employed, the excluded category is unemployed), dummies for whether either head or spouse worked in 1999, 2000, and 2001, and income of head and partner in 2001, 2000, 1999 (as a linear term). Lastly, we add state annual GDP growth between 2000 and 2002 to control for differential trends between treatment and control blocks.

We show the coefficients of the propensity score in Table 8. The estimated coefficients 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, 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 have also higher employment rates (both as employees and self-employed), and no different income from residents of control blocks (with the exception of 2001 income, which is higher in treatment blocks), conditional on the other observable characteristics and higher education for the spouse of the household head. Lastly, treatment and control blocks have different state GDP growth rates, confirming they are not balanced at the geographic level. In sum, this Table shows the need to re-balance the observables between treatment and control blocks.

Figure 1 shows that the common support is complete, that is for each household in *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 favor 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 surveys also 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 program between treatment and control areas (see also, Angelucci and Attanasio, 2009). 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 of treatment and control blocks, which come from different states. To address this issue, our set of conditioning variables includes state GDP growth between 2000 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 Average Intent to Treat Effect on the change in log-consumption for the non-poor alternatively adding and omitting pre-program state GDP growth. Since these households are not eligible for the program, 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 non-poor's log-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 treatment effect: consumption appears to be about 14% 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 effect of the program among the non-poor living in treatment blocks.

4 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.

4.1 Results

According to the estimates in Table 10, the main effect of the program on treated households is an increase in food consumption by 168 and 282 pesos in 2003 and 2004. The effect on non-durable, non-food consumption is negative and insignificant in 2003, and positive and insignificant in 2004. If one sums the estimated effects for food and non-food consumption in the first two years of the program implementation, the total amount spent on non-durable consumption is about 73% 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 on durable goods. This pattern of findings - the size of the effect on consumption growing over time, the bulk of the effect being on non-durable, and in particular on food consumption - is similar to the results from the evaluation of the rural component of the program.

Overall, the results on consumption suggest that the eligible households that participate into the program 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 continuity of the program. Moreover, at the program's inception, payment might have been irregular and plagued by delays.

Notice that the ATT is not simply obtained dividing the AIT by the participation rate. As we mentioned in Section 3, 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).

In the following two Tables, we look at the effect of the program on savings and loans: Table 12 shows program effects on the probability of having savings and loans, and the respective amounts; Table 13 instead provides information on the probability of having a bank account and on the number of loans held. Despite a small increase in the likelihood of having savings in 2003, 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, we estimate a considerable decrease in the amount of debts, both in the percentage of households holding one and in the amounts. The decrease in loans for the participants, of roughly 650 and 1455 pesos in 2003 and 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 to the average monthly transfer that we have mentioned so far, but to the total amount received up to 2003 and up to 2004. The average beneficiary family in 2003 had received 3792 pesos and in 2004 the cumulate average was 8196 pesos, using 2002 prices. The decline of 650 and 1455 pesos implies, therefore, that about 17% 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 most of the grant was consumed in 2004.

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

Table 14 provides estimates of the program 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 pesos drop in transfers sent in 2004), the program 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 68 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.

4.2 Interpretation

The picture that emerges from these results is reasonably clear. Urban Oportunidades increases consumption. The increase in the second year is substantially larger than in the first year. This evidence is consistent with the evidence from other programs and probably reflects both the fact that in the first years households might have doubts about the continuity of the program and logistic difficulties that might have implied that grant payments were sometimes delayed.

As in most CCTs, most of the increase in consumption is in 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 probably reflects a shift in household preferences.

In terms magnitudes, we argued that after two years, households are spending about 73% of the grant. This is even lower than the amount 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 indebtness. 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 who have a saving 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 does not seem to be spill-overs of the Oportunidades grant thorugh this channel.

A final important issue that needs to be kept in mind is the limited participation of eligible households to the program. 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 is an indication of the fact that the program is probably much less attractive to potential urban beneficiaries than to their rural counterpart. This difference in the attractiveness of the program is probably also reflected in the use to which program beneficiaries put the grant.

5 Conclusions

In this paper we report some results on the effects of the Oportunidades program on consumption, savings, and transfers in urban areas. We make a distinction between the so called Average Intent to Treat (which measures the effect of the program on the eligible population) from the Average Treatment on the Treated, which measures the effect of the program on recipients. The distinction between the two is very important in our context because of the very low participation rates to the program.

The main program effects are: 1) an increase in food consumption of roughly half the size to two thirds of the transfer in 2003 and 2004, respectively; 2) a small increase in the 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; 4) a reduction in the stock of debt that is roughly equivalent to 17% 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 program. 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 in 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 also the evidence of a reduction in transfers received by the beneficiary families, however, it seems unlikely that in the medium run the program 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 into the program in urban areas. The evidence we have seems to indicate that those who choose to enrol into the program 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 one, 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 component 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, like meat. We know from the results in AAS04 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 in food there is an upgrading of quality. The second explanation refers to the fact that the Oportunidades transfer is given to the mothers that might have different preferences relative to fathers. If that is the case, the fact that women have a larger 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 transfers reaches its intended beneficiaries. This is particularly important given the limited participation into the program 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 (2011), or at least not of the same magnitude, the fact that beneficiaries households use almost 20% of the grant to reduce their outstanding debt is suggestive that the impacts of the program could be going into the same direction.

What do these results imply for the success of the program? 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 the impacts of the program (including those on health and education) and all the consequences that these impacts have for the accumulation of human capital in the long run. Obviously 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 the most important thing. 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, net worth and access to the financial system that we have provided is informative of the mechanisms at play in generating the overall impacts of the program. In this respect, although other papers have pointed out that the program 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 CCT or more generally cash transfers might be relevant for developing countries mainly as effective redistributive tools.

6 References

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A Weight creation

As described in AA04, the method used to sample households for the socio-economic questionnaire meant that disproportionately many poor households participating in *Oportunidades* appear in the ENCELURB socio-economic database and disproportionately few non-participating poor households. To correct for this, the data had to be weighted. Here, we describe what we did.

Weights were created, following Manski and Lerman (1977), by taking the ratio between the proportion of the *population* who participated in the program (Q_i) and the observed proportion in our *sample* who participated (H_i):

$$w_i = \frac{Q_i}{H_i}$$

It was straightforward to calculate this for households in original treatment blocks (i.e. non-barrido treatment blocks), because all of them appear in the census conducted by the evaluation team in a subset of treatment and control blocks. For each household in a given block, Q_i is the proportion of households in that block in the census who are enrolled onto *Oportunidades* - so this is the true proportion of program participants. H_i is calculated using the socio-economic survey[9] For each household in a given block, it is the observed proportion of households enrolled on *Oportunidades.*

Households in barrido blocks do not have a census interview, so we have to impute the value of Q_i . Since all localities (defined by the variables entidad, municip and localid) that contain a barrido household

[9]Note, however, H_i was calculated using our final sample which only includes households who appear in both the 2002 2003 and 2004 data.

also contain a non-barrido treatment household, we impute Q_i by attributing to all barrido households in a given locality the true proportion of non-barrido households in that locality who are program participants according to the census. H_i is calculated in the same way as for households in the original treatment blocks.

Since poor households in control blocks were neither over- nor under-sampled, the weight for these households is always 1.

B Variables description (not for publication)

Consumption. Food expenditure was calculated for each category in 2002 and 2003, excluding households with more than 18 (out of a total of 37) missing food consumption responses.

Food includes:

- Starch - Maize tortillas, bread, pasta, kidney beans, rice, potato chips, flour, corn, other cereals, etc.
- Protein - Red meat, chicken, pork, canned fish, fresh fish, seafood, eggs, milk, cheese, other animal products.
- Fruit and vegetables - Tomatoes, onions, potatoes, chile peppers, carrots, pumpkin, bananas, apples, oranges, other fruits and vegetables.
- Junk food - Sugar, soft drinks, water purification tablets, coffee, vegetable oil, fried potatoes, fried pig skin, other manufactured food.
- Food eaten outside the home.

Non food includes:

- Household services (excluding rent) - Fuel, DIY/repairs, electricity, water, rubbish collection, phone, gas.
- Public transport - Transport in buses, taxis, lorries, vans.
- Personal care - Personal care items (toothpaste, toilet paper, deodorant, shampoo, etc), baby care items, beauty services (eg manicure).
- Household non-durables - Matches and lighters, candles, cleaning products.
- Education - School fees, transport to school, educational materials.

- Children's clothing - Children's clothes and shoes.
- Adult clothing - Adult clothes and shoes.
- Health - Health expenditure including medical appointments, contraceptives.
- Alcohol and tobacco.
- Household durables - Furniture, cooking utensils, crockery, sheets, white goods.
- Entertainment - Newspapers, magazines, leisure, toys, books, music.
- Vehicles -Cars, motorbikes and bicycles.
- Miscellaneous - Jewellery, securities, insurance, holidays, lottery tickets, etc.

Household Income. We add incomes for the main and second job of all household members and other sources such as other jobs, pensions, and compensations. Incomes from domestic helpers and their relatives, and individuals whose relationship to other family members is missing, are not included.

Variables used to calculate our propensity score. With the exception of past employment and earnings information, all variables relate to 2002.

hhinc; hhincsq - *Household Income; squared(see above)*
saving - *Household saving excluding domestic helpers and their relatives, and individuals whose relationship to other family members is missing.*
Dmissav - *Dummy for missing household saving*
Dtothh1-5 - *Size of household dummies for households with between 1 and 5 members.*
Dnohhsize - *Dummy for missing size of household*
Dnot1hoh - *Dummy for households recorded as having more than one head.*
nokids0-5, 6-12, 13-15, 16-20 - *Number of children in the household in the specified age range.*
sfem -*Dummy for female headed household*
Dnopartner-*Dummy for households where there is no partner.*
hohlit - *Head of household is literate*
ptrlit - *Partner is literate.*
hoheduc - *Head of household education level (included as a series of dummies). The categories are: no qualification, incomplete primary, complete primary, incomplete secondary, complete secondary, higher education.*

ptreduc - *Partner education level (included as a series of dummies, same categories as for head).*

hohdoc - *Head of household went to a conventional doctor in last four weeks.*

ptrdoc - *Partner went to a conventional doctor in last four weeks.*

nosch0-5, 6-12, 13-15, 16-20 - *Number of children in the household in the given age range attending school.*

kiddoc - *At least one child in household went to conventional doctor in last four weeks.*

hohemployee - *Head of household is an employee.*

ptremployee - *Partner is an employee.*

hohse - *Head of household is self-employed.*

ptrse1 - *Partner is self-employed*

misshohemp - *Head of household employment status is missing.*

missptremp - *Partner employment status is missing.*

hhworked01, 00 ,99 - *Either head of household or partner worked in 2001, 2000, 1999.*

hhinc01, 00, 99 - *Income of head and partner in 2001, 2000, 1999.*

misinc01, 00, 99 - *Dummy for missing income in 2001, 2000, 1999.*

death - *Household suffered death or illness of non-resident family member in the last year.*

unemp - *A resident family member was unemployed in the last year.*

bust - *A resident family member lost a business in the last year.*

disaster - *Household suffered a natural disaster in the last year.*

wealth (and its square) - *Socio-economic household-specific wealth index (program eligibility is based on this index);*

GDP - *State GDP growth.*

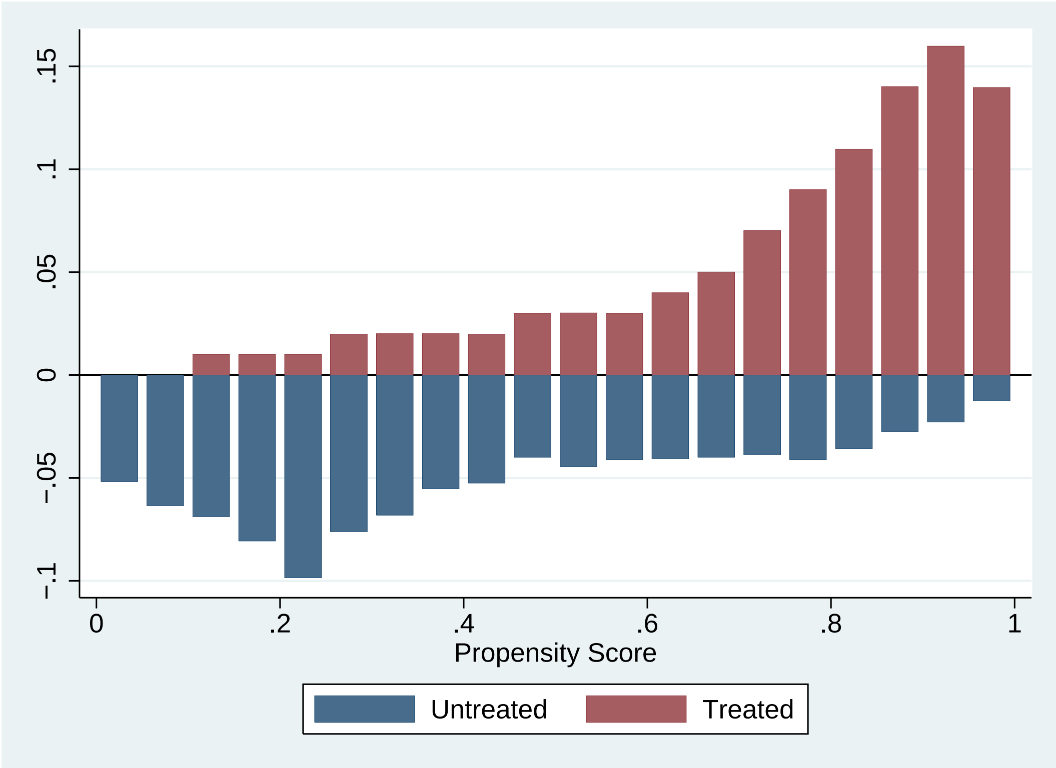

Figure 1: The propensity score.

Table 1: Database features

	2002		2003		2004	
	C	T	C	T	C	T
Eligible (poor) households	9945		9934		9192	
	3634	6311	3628	6306	3409	5783
With incomplete responses	88		1178		1046	
Percentage of eligible*	0.74%	0.97%	12.57%	11.45%	12.50%	10.72%
Lost in follow-ups**			11		745	
Percentage of eligible***			0.17%	0.08%	6.12%	8.29%
Households with data available	7903					
...in all 3 waves****	2848	5055				
pvalue of poverty score	0.3970					
...in "attrition" regression*****						

T= Treatment group, C=Control group (as by locality)

***Percentage of control and treatment group in each wave. **The figure for 2003 refers to households present in 2002 which are missing in 2003. The figure for 2004 refers to households present in 2002 and 2003 which are missing in 2004. ***Percentage of eligible in 2002 for values in 2003 and for eligible in 2003 for values in 2004. ****The figure excludes households with incomplete responses. ****The "attrition" regression is a regression in which the dependent variable is a dummy taking 1 if household has available and complete data in all 3 waves (that is 7903 households) and 0 otherwise (that is in 9945(total eligible households)-7903=2042 households). In addition to the poverty score, the regression includes a full set of controls as in Table 8. Results are robust to different specification with different sets of controls.*

Table 2: Program Participation and Amount of Transfer

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

Weighted averages to account for choice-based nature of the sample. The average and median amounts received are for treated households only. The transfer value is in nominal pesos. The exchange rate is approximately 10 pesos = 1USD.

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	0.8	0.75	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 household with			
head with no education	0.179	0.234	0.000
head with primary education	0.55	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	3686	3137	0.008
household 2001	2303	2266	0.602
household 2000	2109	1978	0.091
household 1999	2181	1762	0.000
head	1918	1860	0.379
partner	717	427	0.044
Proportion of households with			
institutional transfers (last year)	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	0.1	0.16	0.000
head or partner working in 2001	0.873	0.887	0.225

T= Treatment blocks, C=Control blocks. Weighted averages to account for choice-based nature of the sample. Monetary values are nominal.

Table 4: Eligible households' consumption and asset ownership at baseline (2002)

	C	T	p-value
Mean value of monthly non-durable expenditure for			
total	2149	1836	0.000
food	1299	1149	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	6.8	6.8	0.909
improvement to the house	11.4	13.2	0.223
home utensils	1.74	1.43	0.009
domestic appliances	7.53	8.68	0.199
vehicles	0.6	1.23	0.000
Proportion of households owning			
properties*	0.043	0.051	0.333
vehicles**	0.039	0.019	0.000
appliances****	0.75	0.735	0.562
electrics***	0.913	0.894	0.018
animals*****	0.104	0.106	0.815

** houses, land, etc. in addition to the house where the household lives*

*** cars, trucks, motorbikes, tractors and other motor vehicles*

**** TV sets, radios, VCRs and other devices such as PCs or microwave ovens*

***** fridges, heaters, washing and drying machines, boilers and tankers*

****** used for work and/or consumption*

T= Treatment blocks, C=Control blocks. Weighted averages to account for choice-based nature of the sample. Monetary values are nominal.

Table 5: Eligible households' savings at baseline (2002)

	C	T	p-value
Proportion of households that			
contracted loans	0.124	0.24	0.000
has a bank account	0.007	0.01	0.032
has savings	0.018	0.035	0.000
has 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 program	0.011	0.021	0.070
tanda	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	0.14	0.000

T= Treatment blocks, C=Control blocks. Weighted averages to account for choice-based nature of the sample. Monetary values are nominal.

Table 6: Eligible households' transfers (A) at baseline (2002)

Sample does not include households with zero transfers			
	C	T	p-value
Proportion of households that			
sent transfers*	0.041	0.063	0.000
sent monetary transfers	0.015	0.027	
sent in-kind transfers	0.027	0.041	0.003
received transfers*	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	1613	1196	0.133
to the same municipality	275	523	0.020
to out of the municipality	1338	673	0.010
Monetary transfers received			
total	3707	2757	0.010
from the same municipality	303	675	0.029
from out of the municipality	3404	2082	0.005
from spouse	1244	1519	0.379
from offspring	1551	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
to 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 out of 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

** either monetary or in-kind(or both)*

T= Treatment blocks, C=Control blocks. Weighted averages to account for choice-based nature of the sample. Monetary values are nominal.

Table 7: Eligible households' transfers (B) ownership at baseline (2002)

Sample includes households with zero transfers	C	T	p-value
Monetary transfers sent			
total	25	29	0.478
to the same municipality	4	12	0.000
to 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 out of 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
to 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 out of 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* (received-sent)			
total	1991	1682	0.332
same municipality	367	837	0.146
out of the municipality	2711	2210	0.188
In-kind net transfers* (received-sent)			
total	318	312	0.942
same municipality	290	272	0.737
out of the municipality	325	382	0.792

*T= Treatment blocks, C=Control blocks. Weighted averages to account for choice-based nature of the sample. *Net transfers do not include household with zero for both received and sent transfers. Monetary values are nominal.*

Table 8: Probit estimates of the propensity score - marginal effects

	$P(Z=1\|X)$		$P(Z=1\|X)$
nokids0-5	0.017	ptremploy-1	0.107
	[0.020]		[0.027]***
nokids6-12	-0.008	ptrse-1	0.117
	[0.027]		[0.031]***
nokids13-15	-0.011	hhinc	0.006
	[0.022]		[0.004]
nokids16-20	-0.024	hhinc^2	-0.00001
	[0.016]		[0.00001]
nosch0-5	0.004	hhinc01	0.003
	[0.020]		[0.001]**
nosch6-12	0.0004	hhinc00	-0.001
	[0.021]		[0.003]
nosch13-15	0.002	hhinc99	-0.001
	[0.027]		[0.001]
nosch16-20	-0.021	hhworked01	0.010
	[0.027]		[0.031]
sfem	0.122	hhworked00	0.005
	[0.034]***		[0.032]
nopartner	0.041	hhworked99	0.067
	[0.040]		[0.034]**
hoheduc-1	-0.055	saving	0.001
	[0.032]*		[0.006]
hoheduc-2	-0.107	debt	0.002
	[0.039]***		[0.002]
hoheduc-3	-0.049	death	0.050
	[0.044]		[0.027]*
hoheduc-4	-0.115	unemp	0.127
	[0.055]**		[0.032]***
hoheduc-5	-0.179	bust	-0.193
	[0.077]**		[0.089]**
ptreduc-1	0.136	disaster	0.176
	[0.037]***		[0.067]***
ptreduc-2	0.12	wealth	-0.137
	[0.039]***		[0.045]***
ptreduc-3	0.17	wealth2	0.006
	[0.043]***		[0.011]
ptreduc-4	0.123	GDP2000	-14.37
	[0.037]***		[2.800]***
ptreduc-5	0.173	GDP2001	0.26
	[0.043]***		[3.706]
hohemploy-1	0.082	GDP2002	8.581
	[0.041]**		[3.196]***
hohse-1	0.073		
	[0.042]*		
Area characteristics	No		
Household size dummies	Yes		
Doctor visit dummies	Yes		
Income joint significance	14.84***		
Observations	8324		

Robust standard errors in brackets; clustering at the locality level. Note: hh=household; hoh=head of household; ptr=partner (e.g. hhworked01=dummy for whether household head was employed in year 2001). Unless otherwise specified, all variables are from 2002. We provide a more detailed variable description in the Appendix. *, **, *** mean significant at 10%, 5%, 1%.

Table 9: Average treatment effect on log-consumption for the non-poor.

	Log-consumption	
	1	2
ATE	-0.010	0.148
	[0.081]	[0.056]***
GDP growth	Yes	No
Observations	3528	

Standard errors estimated with the block-bootstrap (1000 reps). The block is the city block. ***,**,* = significant at 10, 5, 1% level.

Table 10: Average Treatment Effects of *Oportunidades* on non-durable consumption

	food		non-food	
	2003	2004	2003	2004
AIT	44.06	86.12	-58.31	31.21
	[57.00]	[46.20]*	[64.82]	[57.96]
ATT	168.54	282.85	-57.00	141.57
	[108.87]*	[95.82]***	[120.88]	[110.58]
Obs.	7322	6824	7320	6829

Local linear regression (llr) matching estimates. The estimates from llr are similar to the ones obtained using the 5 nearest neighbors with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 reps, the block is the locality. *,**,*** = significant at 10, 5, 1% level. Monetary values are nominal.

Table 11: Effect of *Oportunidades* on durable assets

	Monthly expenditure		Ownership of: Property		Transport		Electrics		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)

Local linear regression matching estimates. The estimates from llr are similar to the ones obtained using the 5 nearest neighbors with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 reps, the block is the locality. Monetary values are nominal.

Table 12: Effect of *Oportunidades* on savings and loans

	Savings				Debts			
	%		Amount		%		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	0.13	0.06	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)***

Local linear regression matching estimates. The estimates from llr are similar to the ones obtained using the 5 nearest neighbors with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 reps, the block is the locality. Monetary values are nominal.

Table 13: Effect of *Oportunidades* on bank accounts and loans

	Probability of having a bank account		Number of loans asked	
	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)***

Local linear regression matching estimates. The estimates from llr are similar to the ones obtained using the 5 nearest neighbors with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 reps, the block is the locality. Monetary values are nominal.

Table 14: Effect of *Oportunidades* on transfers

	Monetary				In kind			
	Sent		Received		Sent		Received	
	2003	2004	2003	2004	2003	2004	2003	2004
Proportion								
AIT	0.006	-0.0002	-0.003	-0.004	-0.005	-0.180	-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 0)								
AIT	21.81	11.89	-3.76	111.2	-2.27	8.44	-7.66	-13.94
	(15.02)	(14.92)	(52.29)	(85.88)	(5.13)	(5.21)*	(16.3)	(17.34)
ATT	49.87	26.41	-3.68	268.27	-5.06	19.56	-18.96	-32.24
	(34.65)	(34.16)	(121.6)	(203.17)	(11.68)	(11.83)*	(37.77)	(39.65)

Local linear regression matching estimates. The estimates from llr are similar to the ones obtained using the 5 nearest neighbors with replacement. The AIT effects are estimated using standard propensity score matching. Standard errors obtained from the block-bootstrap with 500 reps, the block is the locality. Monetary values are nominal.