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Investing Cash Transfers to Raise Long-Term Living Standards

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In this paper we test whether poor households use cash transfers to invest in income generating activities that they otherwise would not have been able to do. Using data from a controlled randomized experiment, we find that transfers from the OPORTUNIDADES program to households in rural Mexico resulted in increased investment in micro-enterprise and agricultural activities. For each peso transferred, beneficiary households used 88 cents to purchase consumption goods and services, and invested the rest. The investments improved the household's ability to generate income with an estimated rate of return of 17.55%, suggesting that these households were both liquidity and credit constrained. By investing transfers to raise income, beneficiary households were able to increase their consumption by 34% after five and a half years in the program. These results suggest that cash transfers to the poor may raise long-term living standards, which are maintained after program benefits end.

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0. Introduction

> *Five years ago when my oldest daughter was in school and we received money from PROGRESA, we saved 600 pesos to buy wood and the other materials for building a chicken coup, and with what was left we bought a few chickens. Since then, we have raised many chickens which we sometimes sell, and we collect 10 to 15 eggs per week that we eat ourselves.*
>
> *OPORTUNIDADES (PROGRESA) beneficiary in rural Mexico (August 2004)*[1]

Cash transfer programs are important policy tools for fighting poverty in both developed and developing countries. A major concern in developed countries has been the extent to which beneficiary populations become dependent on government assistance for maintaining current consumption levels[2]. In contrast, we argue that beneficiary families in developing countries may invest part of their cash transfer in productive enterprises that boost their income generating ability and consequently raise living standards permanently. We test this hypothesis using a controlled randomized experiment to identify the extent to which beneficiary households from Mexico's OPORTUNIDADES[3] program invested cash transfers in income generating activities, and the extent to which those investments increased long-term household consumption.

In developing countries there are two primary pathways by which transfers correct market failures that limit investment in productive activities. First, transfers alleviate liquidity and credit constraints that contribute to poverty traps, whereby poor households are able to afford the startup costs associated with entrepreneurial activities[4]. The households studied here, with under US $1 per day in per capita consumption, are likely to be liquidity constrained, and therefore unlikely to be able to afford the "low" levels of capital investment needed to start micro-entrepreneurial activities[5]. Second, if transfers are perceived as a secure source of income, risk adverse households will be more willing to increase ownership of risky assets, even in the presence of risk. In these

[1] From a qualitative survey of agricultural production and micro-enterprise activity in rural Mexico, conducted by the authors. Interview from August 3, 2004.
[2] See Moffit (1992) for a review of the US case.
[3] Skoufias (2005) provides a description of the program.
[4] Models along these lines are developed by authors including Banerjee and Newman (1993), Aghion and Bolton (1997), Lindh and Ohlsson (1998), Lloyd-Ellis and Bernhardt (2000), and Banerjee (2004).
[5] McKenzie and Woodruff (2006) report that it takes $100 USD to start a micro-enterprise in Mexico.

ways, monetary assistance from OPORTUNIDADES provides a secure income stream that may help alleviate liquidity constraints and grant access to capital, leading to increased investment[6].

This study uses the randomized experiment from the rural evaluation of the OPORTUNIDADES conditional cash transfer program to estimate the impact of an exogenous increase in unearned income on investment and long-term living standards. We find that beneficiaries invest in production and draft animals, and that previously landless beneficiary households obtain land for agricultural production. Furthermore, there are significant increases in the number of households that operate micro-enterprises. Our estimates indicate that for each peso transferred, beneficiary households consume 88 cents directly, and invest the rest. The aggregate effect of the investments yields a 1.8 cent increase in consumption for each peso of transfers received. These estimates indicate an estimated rate of return on investment of between 15.52% and 17.55%. Through investments in productive activities, beneficiary households increased their consumption by 34% after five and a half years in the program. Because investments are made in productive assets and income-generating activities, the gains in living standards will likely be maintained even after program benefits end.

Our study contributes to the relatively small literature that examines the potential impact of cash transfer programs on long-term living standards. Ravallion and Chen (2005) show that the lion's share of temporary cash transfers given by an anti-poverty program in China were saved. Sadoulet, de Janvry and Davis (2001) argue that payments from the Mexican agricultural support program PROCAMPO are invested in farm production, and estimate an income multiplier in the range of 1.5 to 2.6. While not related directly to cash transfer programs, studies by Yang (2005) and Woodruff and Zenteno (2001), show that remittances are used as capital to invest in micro-enterprises in the Philippines and Mexico, respectively.

The paper is organized as follows. Sections 1 and 2 describe the OPORTUNIDADES human development program and review the program's experimental design and data. In section 3,

[6] Investment will also depend on expectations about the transfer's temporal horizon and life-cycle considerations (Modigliani and Brumberg 1954; Friedman 1957 and Hall 1978).

we present evidence of the causal effect of OPORTUNIDADES on increased investments in agricultural and micro-enterprise activities. Having established that cash transfers lead to more investment, section 4 develops a basic model of investment to illustrate the relationship between consumption, transfers and investment that we then estimate empirically. Section 5 provides a series of robustness tests to support our main findings and section 6 concludes.

1. The Rural OPORTUNIDADES Program

The Mexican Government established OPORTUNIDADES (originally called PROGRESA) in 1997. The program was designed to alleviate current poverty and break its intergenerational transmission by inducing parents to invest in the human capital of children. In this sense, OPORTUNIDADES was conceived as a temporary program (i.e. over the course of three to four decades), which would become obsolete once the current generation of beneficiary children reaches adulthood.

Cash transfers from OPORTUNIDADES are given to the female head of household[7], and are conditional on children attending school, family members obtaining preventive medical care through clinic visits and attending "pláticas" or education talks on health related topics. OPORTUNIDADES is the largest conditional cash transfer program of its kind. It distributed approximately 3 billion US dollars to some 5 million beneficiary households in 2004[8].

The cash transfers come in two forms. The first is a bimonthly fixed food stipend conditional on family members obtaining preventive medical care and is intended for families to spend on more and better nutrition. The second type of transfer comes in the form of educational scholarships and is given conditional on children attending school a minimum of 85% of the time and on not repeating a grade more than twice. Specifically, OPORTUNIDADES provides bimonthly cash scholarships for each child less than 18 years old enrolled in school between the third grade of primary school and the third grade (last) of junior high. High school scholarships are

[7] Scholarships for beneficiaries in upper-secondary school can be received by the youth themselves.
[8] www.oportunidades.gob.mx

granted to all beneficiaries younger than 21 years old who are enrolled in school. The educational stipend varies by grade and gender. It rises substantially after graduation from primary school and is higher for girls than boys during junior high and high school. Beneficiary children also receive money for school supplies once or twice a year. There is an upper limit in the total transfer received per household. Table 1 details transfer amounts in October 1997 prices.

When OPORTUNIDADES was first rolled out in rural areas starting in 1997, program eligibility was determined in two stages (Skoufias et al. 2001). First, the program identified underserved or marginalized communities and then identified low-income households within those communities. Selection criteria for marginalized communities were based on the proportion of households living in very poor conditions, identified by using data from the 1995 census (*Conteo de Población y Vivienda*). For the selection of eligible households within marginalized communities, OPORTUNIDADES conducted a socio-economic survey, the *Encuesta de Características Socioeconómicas de los Hogares* (ENCASEH). This census of households was used to classify households as eligible for treatment ("poor") or ineligible ("non-poor") using a proxy means test (PMT). Using the 1997 ENCASEH, the original classification scheme designated approximately 52% of households as eligible ("poor")[9].

All eligible households living in treatment localities were offered OPORTUNIDADES and a majority (90%) enrolled in the program. Once enrolled, households received benefits for a three-year period conditional on meeting the program requirements. New households were not able to enroll until the next certification period which prevented migration into treatment communities for OPORTUNIDADES benefits. Households in rural areas were "recertified" (re-assessed with a proxy means test) after three years on the program to determine future eligibility. If a household was recertified as eligible, it would continue receiving benefits. If not recertified, the household was guaranteed three more years of support followed by three years of transitional support. Thus,

[9] Later the Government decided that a subset of the "non-poor" households had been unduly excluded. They expanded the eligibility criteria to include a set of slightly wealthier households in a process called "densification" (Hoddinott and Skoufias 2004).

households could expect a minimum of nine years of benefits upon enrolling in the program (OPORTUNIDADES 2003).

In order to transfer the cash, OPORTUNIDADES had to verify that households actually completed the required health care visits by having medical providers from the public clinics who administered the services certify that the household had completed the requirements. A similar procedure was followed for the cash transfer associated with school attendance. About 1 percent of households were denied the cash transfer for non-compliance.

2. Experimental Design and Data

Our analysis takes advantage of the controlled-randomized evaluation design implemented by the Mexican Government to conduct a rigorous impact evaluation of OPORTUNIDADES. Due to budgetary and logistical constraints, the Government was unable to enroll all eligible families simultaneously. Rather, it needed to phase in enrollment over a period of time. For ease of implementation, the Government decided that it would enroll whole communities at a time and that it would enroll them as fast as possible so that no eligible household would be kept out of the program. As a result of this process, the government randomly chose 320 treatment and 186 control communities in seven states for a total of 506 experimental communities. Eligible households in treatment communities began receiving benefits in April of 1998, and eligible households in control communities were not incorporated until November of 1999 nor were they informed that OPORTUNIDADES would provide benefits to them until two months before incorporation.

The data used in this paper comes from household evaluation surveys and administrative records of the amount of money transferred to households. Detailed information on a host of topics was collected in a series of rural evaluation surveys, the *Encuesta de Evaluación de los Hogares Rurales* (ENCEL). The ENCELs survey all eligible and ineligible households in treatment and control communities. The sub-sample used for the main analysis in this paper is restricted to eligible households classified as poor, although we also use ineligible (non-poor) households for

robustness checks. There are three rounds of data during which eligible households in control communities did not receive transfers (October 1998, May 1999 and November 1999), and a straightforward comparison of treatments and controls is possible. Once control communities were incorporated into the program at the end of 1999, an additional three rounds of data were collected during which eligible households in both types of communities were receiving benefits (May 2000, November 2000 and November 2003). In addition, baseline data from the 1997 pre-intervention ENCASEH census is used for a total of seven rounds of data between 1997 and 2003.

Our sample consists of 7,658 poor households that were eligible for the program in the 320 treatment communities and 4,644 poor households that were eligible for the program in the 186 control communities (Table 2). We refer to this sample as the "intent to treat" or ITT sample. Baseline data for this sample are described in detail in Table 3A. The sample is well balanced across control and treatment groups. We report the test of difference in means across control and treatment groups, finding no statistically significant differences for 33 out of 35 characteristics measured at baseline. This suggests that the randomization was effective in generating truly exogenous variation in the treatment.

We also conduct the analysis with a second sample designed to estimate the effect of treatment on the treated (TOT). In these analyses, we use the approximately 90 percent of eligible households that actually took up the program, 6,819 treatment households and 4,159 control households that took up the program when offered in late 1999. A potential source of bias arises in the TOT sample if observed and unobserved time varying characteristics are driving the household's decision to participate in the program. We assume the group of controls that took up the program when offered in late 1999 would have taken up the program if offered earlier in 1998. The fact that take up rates amongst treatment and controls are almost identical (90%) supports this hypothesis (Table 2 and Graph 1). Furthermore, the TOT sample baseline observables are well balanced, with only 1 statistically significant difference out of 35 characteristics tested (Table 3B).

Given this evidence, we conclude that working with the actual beneficiaries does not introduce selection bias coming from heterogeneous take up responses.

3. Investment

In this section, we estimate the effect of OPORTUNIDADES on investments in agriculture and micro-enterprise activities. We show that beneficiary households increase agricultural assets and participation in micro-enterprise activities, suggesting that some of the cash transfer was used for investment in productive assets. Section 3.1 presents the empirical specification based on the randomized design of the OPORTUNIDADES evaluation sample, section 3.2 presents comparisons of means, and sections 3.3 and 3.4 discuss the main investment results.

3.1. Specification and Identification

Our analysis exploits the exogenous variation introduced by random assignment of treatment to estimate the effect of OPORTUNIDADES on investment in agricultural production and micro-enterprise activity, the two most common domestic income generating activities in this population. We estimate two types of models: one exploiting the community randomization to obtain average treatment effects, and a second which exploits the variation in transfer amounts to estimate the effect of the size of the transfer on investments.

To estimate the first specification we use the exogenous variation generated by the randomized phasing in of the program. Beneficiary households in treatment communities received transfers for just over a year and a half longer than beneficiary households in control areas. Given the timing of the ENCEL surveys, we first observe treatment households in October 1998, approximately 6 months following the first transfer payment. Treatment households would have received three bimonthly transfer payments by the time of the first follow up survey. Since control communities did not begin receiving payments until November-December of 1999, there are three rounds of data (October 1998, May 1999 and November 1999) where a simple comparison of eligible households in treatment and control communities is possible.

For these three rounds, we estimate the following reduced form:

$$A_{ijt} = \alpha_o + \alpha_1 T_{ij} + \sum_t \alpha_{2t} WAVE_t + \sum_k \beta_k X_{ij,97} + u_j + \varepsilon_{ijt}\,, \qquad (1)$$

where A_{ijt} denotes productive assets of household i in community j in period t; T_{ij} is a binary indicator equal to 1 if eligible household i lives in a treatment community in the ITT specification, or if the household has received benefits in the TOT specification; $WAVE_t$ takes on the value 1 in period t, and the $X_{ij,97}$ are household and community characteristics measured at baseline. The error term has two components: an idiosyncratic disturbance, ε_{ijt}, and a community random effect, u_j, that accounts for the correlation within communities over time. Note that the dependent variable in the analysis is the level of assets and not investment during the period. If the randomization successfully balanced the treatment and control groups, the level of pre-treatment assets would have been the same in treatment and control households (we explicitly test this assumption in section 3.2). Under this assumption, the coefficient α_1 is the average treatment effect on productive assets.

The second specification takes advantage of the variation in total cumulative transfers introduced both by the randomized phasing in of the program and the variation in the transfer amount based on the demographic structure of the household. The randomization meant that treatment households had been accumulating transfers for about 18 months longer than control households (Graph 1). In addition, households with more children in school and enrolled in higher grades (more female children in higher grades in particular) had higher transfer amounts, and therefore accumulated transfers faster than similar households with fewer children in school or with more male children. Graph 2 illustrates the variation in household demographic structure, plotting shares of households by number of children enrolled in any grade between the third year of primary and the third year of junior high[10].

[10] Households with teenagers enrolled in high school are excluded because high school stipends were granted beginning in the 2001-02 school year (following the experimental period), and because of the low high school enrollment rates (only 33% of 15 to 21 year olds report being enrolled in high school).

The second specification uses all six rounds of data from 1998 through 2003 to estimate the differential effects of cumulative transfers on productive assets. Cumulative transfer quintiles are constructed for each household-wave observation based on the total transfer amount a household has accumulated at that point in time. These new variables are constructed as indicator variables for the quintile a household belongs to in each round. It is important to note that they are constructed on the distribution of transfers across all waves of data and not within each round. We then use the cumulative transfer quintile dummy variables to estimate the following specification:

$$A_{ijt} = \alpha_o + \sum_{p=1}^{5} \alpha_{1p} Q_{ijt,p} + \sum_t \alpha_{2t} WAVE_t + \sum_k \beta_k X_{ij,97} + u_j + \varepsilon_{ijt} \qquad (2)$$

where $Q_{ijt,p}$ equals 1 if household i in community j in period t falls in the p-th quintile of the total cumulative transfer distribution. We specify the effect of cumulative transfers non-linearly so as to allow for jumps in investment due to the potential lumpiness of certain types of investment.

One potential concern with the specification in equation (2) is that actual total cumulative transfers are determined in part by household decisions to send their children to school. If households decide their children should work instead of going to school, then part of the income generated from work may be invested. In this case, we would obtain a biased estimate of the impact of the transfer amount on investment.

In order to correct for this potential endogeneity we use the potential transfer the household would have received if all eligible children were enrolled in school. To compute potential transfers, we take household composition and children's enrollment status at baseline and apply the program rules, assuming the child progresses one grade per year with no school drop outs and no repetition. Our IV is therefore constructed from pre-intervention household demographic structure interacted with timing of incorporation into the program (which was determined randomly). Since the potential transfer amounts include no household behavioral response but are highly correlated with actual transfers, it is a valid instrument of the actual transfer amounts. We also control for the

number of household members and share of children directly, so that the IV is not confounded with family composition effects.

Graphs 3a and 3b plot the distributions of the actual and potential current and accumulated transfers, respectively. As shown, the distributions of the potential transfers follow those of the actual transfers very closely. As expected, the potential transfers are an over-estimate of the actual transfers (given non-compliance, administrative delays in payments, etc). The simple correlation amongst them is 0.89. If we control for time effects and baseline covariates, the potential transfer explains 55.7% of the total transfer and 65.9% of the variation of the cumulative transfer. Graph 4 plots mean cumulative transfer by quintile for the distributions of actual and potential cumulative transfers.

3.2. Comparison of Means

The dependent variables for agricultural assets come from the six rounds of the ENCEL that contain information on animal ownership and amount of land in use over the 12 months preceding the interview (hectares of land for agriculture, grazing and/or forestry purposes). We define production animals to be those whose meat and/or by-products (milk, cheese, eggs, etc.) are sold and consumed. These include goats and sheep, cattle (cows), poultry (chickens, hens and turkeys), pigs and rabbits. We define draft animals to be those traditionally used for farming (plowing) and/or for transportation purposes. These include donkeys, mules, horses and oxen. We transform all of these animals into "cow equivalents" using the ratio of the price of the animal divided by the price of a cow. Therefore, we create cow-equivalent indices of production and draft animals. Land is measured in hectares and includes all plots used by the household[11].

The dependent variables for (non-agricultural) micro-enterprises come from five rounds of the ENCEL surveys (October 1998 to December 2000). A set of questions ask the household head whether somebody in that household had engaged in a "self-motivated" non-agricultural activity which generates income during the month before the interview. The list of activities includes

[11] A detailed description of the sample and variables is available upon request.

sewing clothes, making food for sale, carpentry and construction, sale of non-food items such as handcrafts, transportation of people or goods in own vehicle, repair of artifacts or machinery, domestic service (wash, iron or cook for a fee), or other activities done on your own. We define micro-enterprise as participation in any of these activities. No micro-enterprise activities were collected in the 1997 baseline survey, and because of lack of comparability (changes in the categories and time frame considered), we do not include the 2003 round.

For the purpose of analysis, we classify households according to the amount of land used and agricultural asset ownership at baseline. We distinguish between: (i) households with no agricultural assets, (ii) landless farms (households reporting no land use but animal ownership), (iii) smaller landed farms (households using at least 3 ha of land for agricultural, grazing or forestry purposes, regardless of animal ownership), and (iv) bigger farms (households using more than 3 ha, regardless of animal ownership). Around one tenth of the sample has no agricultural assets, 31% of households are landless but have some animals, 45% have smaller farms and 13% have bigger farms. We check for balance between treatment and control households within farm size categories in Table 3A in the Data Appendix.

With random assignment of communities to treatment and control groups, we would expect that ownership of farm assets at baseline would be equal between the two groups. Then, any subsequent changes could be attributed to the program. In effect, when we compare agricultural assets of treatment and control households in the TOT sample prior to intervention, we find no statistically significant differences in mean values (Table 4 Panel A).

During the experimental period (October 1998 through November 1999), a simple comparison of the mean values for each dependent variable between households in treatment and control communities gives us an estimate of the program impact (see Table 4 Panel B). We report the results both for whether the household had any of the asset type and for the amount of the asset. We observe significant increases in the likelihood of draft and production animal ownership for all households and in particular for households with no agricultural assets (increases in production

animal ownership only), landless households (increases in draft animal ownership only) and bigger farms (increases in both types of animal ownership). There are significant (at the 10% level) increases in land use for landless households (including households with no agricultural assets) and significant increases in the number of production animals for landless and smaller farms. Production animals generally require lower capital investments than draft animals. Thus, it is plausible that households with no farm assets initiate animal production by buying small animals that yield relatively quick returns (for example eggs and meat from chickens or turkeys).

For micro-enterprises, the comparison of means between treatment and control households during the experimental period suggests important program effects on the likelihood of engaging in micro-entrepreneurial activity. Specifically, when activities traditionally done by men (construction/carpentry and machinery repair, which are 92% and 77% male respectively) are excluded from the dependent variable, households in the treatment group have 58 percent more micro-enterprises than those in the control group. Results hold for landless and smaller farms.

3.3. Estimated Treatment Effects

We estimate the effect of OPORTUNIDADES on both the probability of owning an asset and the amount of the asset owned for draft animals, production animals and land. For the probability of having an asset type, we first report the results for the whole sample and then for a sample that is restricted to households that had none of the asset at baseline. Analogously, for the amount of the asset type, we first report the estimate on the whole sample and then restrict the sample to those households with positive levels of the asset at baseline. The probability of owning an asset conditional on having none at baseline provides insight on whether the program induced households to start a farm business (variation at the extensive margin), whereas the estimated impact of the amount of an asset conditional on having some of that asset at baseline tells us about how the program influenced households to expand their already existing farm assets (variation at the intensive margin).

The results of these analyses are presented in Table 5. In Panel I we report the estimated impact of the program on asset ownership (and land use). Each cell reports the estimated impact of the program on an asset type (columns) using a particular model specification (rows). For each of the asset types listed in the columns, we first report the results using the whole sample and then for the sample restricted to those that did not own any of the assets at baseline. The rows report the empirical specification used. Models A and B in the first two rows report the estimated ITT and TOT impacts, respectively, without controls. Models C and D add baseline household and community controls. The list of controls includes household head and spouse's ages and education, head's ethnicity, household demographic composition and size, baseline assets (home ownership, dirt floor and electricity), and community characteristics (male agricultural wage in the community, distance to large urban center and presence of community work associations). The descriptive statistics for the controls are presented in Tables 3A and 3B.

In Panel I of Table 5 (Model D - TOT with controls), we observe that the probability of draft animal ownership is 2.2 percentage points higher in treatment households than in control households, conditional on no draft animal ownership in 1997. Similarly, treatment households are 6.2 percentage points more likely to own production animals, conditional on no production animal ownership at baseline. Given the average levels of asset ownership for control households in the estimation sample, these coefficients imply that treatment households are 20.2% more likely to own draft animals and 13.4% more likely to own production animals than control households. Results are robust to the exclusion of covariates (Model B) and/or inclusion of all intent to treat households (Models A and C). Finally, the 5.2 percentage point (15.7%) increase in the conditional probability of land use suggests that landless households are acquiring land.

In Model E, we interact the treatment indicator with the level of agricultural assets prior to the intervention. We hypothesize that households with existing assets are more likely to invest more as they have already paid the fixed costs of startup. The estimates from Model E are in line with this hypothesis. Only households owning animals at baseline significantly increase draft animal

ownership under treatment. Landless households and small farms increase draft animal ownership by 3.5 and 2.9 percentage points respectively (conditional model). On the other hand, treatment households with no agricultural assets at baseline are 9.4 percentage points more likely to increase production animal ownership than control households. Households with no baseline agricultural assets and landless households increase land use by 4.9 to 6.5 percentage points respectively (10% significance).

Panel II in Table 5 presents the estimated impacts of the program on the amount owned of each asset. Again, there is no difference between the ITT and TOT models with or without controls. Model D shows that conditional on having draft animals at baseline, there is an increase in the number of cow equivalent draft animals of 0.062 cows (or equivalently, 0.15 horses or 0.41 mules) for all households (10% significance). Households with small farms increase the number of cow equivalent draft animals by 0.078 cows (or equivalently, 0.19 horses or 0.51 mules), conditional on having draft animals at baseline. Production animals increase by 0.125 cow equivalents (significant at the 10% level), equal to approximately 0.70 goats, 0.79 pigs or 6.76 chickens and/or turkeys. Effects are larger in magnitude for households with big farms at baseline. For big farms in the conditional sample, OPORTUNIDADES increases the number of draft animals by 0.112 cow equivalents (0.27 horses or 0.73 mules), and increases the number of production animals by 0.291 cow equivalents (1.63 goats, 1.84 pigs or 15.75 chickens and/or turkeys). Finally, big farms increase land usage by 0.162 hectares on the unconditional sub-sample[12].

The effect of OPORTUNIDADES on micro-enterprise participation is estimated with a set of probit regressions. Results are presented in Table 6. Models A and B present the average treatment effect when no controls are included, for the ITT and TOT sub-samples, respectively. Models C and D incorporate household and community controls. For this period, treated households

[12] For the October 1998, May 1999 and November 2003 rounds of the ENCEL, we have information on land ownership which allows us to classify land into "owned land", if any households member is reported to be the owner; and "non-owned land", if the plot is reported to be rented, borrowed or in tenancy. Results (available upon request) show an increase of approximately 0.034 ha (340m^2) in the use of non-owned land for beneficiary households.

have a 2.5 to 2.8 percentage point higher participation in micro-enterprise activities than control households. Given a mean participation rate in micro-enterprise of 5.9% by control households over the sample period, treatment households are approximately 42.3% to 47.7% more likely to engage in micro-enterprise activity. The last column in Table 6 shows the same set of regressions for predominantly female micro-enterprise activity participation. We find a positive treatment effect of 2.8 percentage points without controls, and 3.0 percentage points with controls, or a 58.9% increase in the likelihood of having a micro-enterprise[13].

3.4. Estimated Transfer Effects

In Table 7, we present the estimates of the intensity of treatment as measured by the quintile of the accumulated transfer distribution (equation (2)). The results are reported in six panels. The first three panels report the estimated effects of the amount of the transfer on the probability of owning the asset type and the second group of three panels reports the results for the impact on the amount of the asset owned. In all of the panels, Model A shows estimates using the quintiles of the cumulative potential transfer, which can be compared to estimates using the cumulative actual transfer in Model B. Note the similarity in magnitude and significance between estimates in models A and B. Model C repeats Model B using the conditional sample; either not owning the asset at baseline for the probability of ownership models or owning some of the asset at baseline for the amount models. Model D further interacts treatment with baseline farm size.

A noteworthy pattern emerging from Table 7 is that households receiving higher accumulated transfers per wave have a higher likelihood of investing in agricultural assets. Point estimates on each quintile, while positive and increasing with the quintile, are generally significant only at the top quintiles, suggesting a possible threshold in the amount of accumulated transfers needed to start investing. For production animals, significant increases are achieved at lower

[13] We have further analyzed female micro-enterprises by activity type (results available upon request). Interestingly, we find large and significant differences in handcraft enterprises amongst treatment and control households, but no differences in domestic services. Handcraft manufacture requires a larger initial capital expenditure than domestic services (which might only imply transportation costs if the jobs are in distant locations) and thus are more likely to be restricted by liquidity constraints.

quintiles. This is not surprising since smaller amounts of money are required to purchase a production animal (a turkey, a goat, etc) than a draft animal (a horse or an ox). The effect per quintile on the probability of land use follows the same general pattern. Effects are larger on the conditional estimates, again suggesting that initially landless households are acquiring more land (Model C). The number of hectares used is positive and increasing with the quintile of cumulative transfers except for the top quintile. One possible explanation for this result is that households might be afraid to lose their eligibility status if they accumulate too many easily verifiable assets. Results are similar when conditioning on baseline farm size (Models D in Table 7).

4. Consumption

Section 3 presented evidence that OPORTUNIDADES beneficiaries increased farm production and micro-enterprise activity. In this section, we turn our attention to the effect of these investments on living standards. Specifically, we are interested in the effect of the program on income and consumption. One of the limiting factors in this analysis is that we do not have income data, but rather only consumption. Therefore we need to infer the key structural parameters for a consumption model. The parameters of interest are the marginal propensity to consume and to invest (or save), and the return on these investments. We begin with a discussion of the specification and identification in the context of a simple inter-temporal consumption and investment model[14]. We then describe the estimation and results. We test a number of important identifying assumptions in the next section.

4.1 Model and Empirical Specification

In this section we derive the empirical specification for the consumption equation from the set of budget and productive constraints the household faces in the context of an inter-temporal utility maximization model. Consider a setting where household income is derived from a family business and government transfers, and the household is credit constrained. In each period, the household

[14] Note that it is beyond the scope of this paper to estimate a full household structural savings and investment behavioral model. Instead, we sketch a simple statistical model which allows us to interpret the reduced form estimates obtained in the empirical analysis.

decides how much of its income is consumed and how much is invested in the family farm or micro-enterprise so that its budget constraint is:

$$C_t = Y_t + PT_t - I_t \tag{3}$$

where C_t is consumption in period t, Y_t is income from the household's productive activities, PT_t are public transfers, and I_t denotes investment.

For simplicity, we will assume that the family business production is a function of business assets, A_t, and family labor, L:

$$Y_t = \gamma(L)A_t + \varepsilon_t \tag{4}$$

where ε_t is a zero mean random productivity shock such as weather or illness. We assume that only family labor is used for production, which is consistent with the data. Moreover, we are not allowing family labor time allocations to be a choice in this model.

Let δ be the depreciation rate. Then, the household can increase its stock of productive assets through investment, and accumulates assets as follows,

$$A_t = \delta A_{t-1} + I_{t-1} \tag{5}$$

We assume that households are credit and liquidity constrained. This implies that the maximum amount households can invest in a single period is their income in that period and that investment cannot be negative; i.e.

$$I_t \geq 0 \tag{6}$$

The household chooses consumption and investment to maximize the discounted present value of consumption. The resulting Euler equation at t is:

$$U'(C_t) = \beta\gamma(L)U'(C_{t+1}) + \lambda_t(I_t) \tag{7}$$

where $\lambda_t(I_t)$ is the Lagrange multiplier associated with constraint (6). The *optimal* levels of consumption and therefore investment equate the marginal utility of current consumption to the marginal utility of future consumption plus the marginal cost of the liquidity constraint on

investment. The solution in (7) provides us with the optimal marginal propensity to consume out of income, which we denote α_t.

Consider the first two periods that households receive transfers from OPORTUNIDADES. In this case, the optimal consumption level in the second period after transfers begin is:

$$C_{t+1} = \alpha\gamma(L)\delta[\delta + \gamma(L)(1-\alpha)]A_{t-1} + \alpha PT_{t+1} + [\alpha\gamma(L)(1-\alpha)]PT_t + \alpha\varepsilon_{t+1} + [\alpha\gamma(L)(1-\alpha)]\varepsilon_t \quad (8)$$

where α is the marginal propensity to consume and $(1 - \alpha)$ is the marginal propensity to invest[15]. Consumption is a function of initial assets and family labor, current transfers, lagged transfers and productivity shocks. The coefficient on current transfers is just the marginal propensity to consume. The coefficient on lagged transfer consists of three structural parameters. Working backwards, the first is the marginal propensity to invest *(1-*α), which when multiplied times transfers yields the amount of the transfer invested. The second parameter is the marginal productivity of investment, *γ(L)*, which when multiplied times investment yields income earned from the investment. The third parameter is the marginal propensity to consume, α, which when multiplied times income yields the amount of income earned from the investment that is consumed.

For each round of data from 1998 through 2003 where there is consumption data available, we estimate (8) as the following reduced form:

$$C_{ijt} = \phi_o + \phi_1 PT_{ijt} + \phi_2 \sum_{s=1}^{t} PT_{ijt-s} + \sum_t \phi_{3t} WAVE_t + \sum_k \pi_k X_{ij,97} + \nu_j + \xi_{ijt} \quad (9)$$

where C_{ijt} is monthly per capita adult equivalent consumption[16] of household *i* living in community *j* at time *t*; PT_{ijt} is the current monthly OPORTUNIDADES transfer per capita; and $\sum_{s=1}^{t} PT_{ijt-s}$ are 6-months lagged total per capita cumulative transfers, i.e. the total transfer amount the household had accumulated until six months prior to the current round of the ENCEL survey (*t*). Since

[15] For simplicity, we assumed that the marginal propensity to consume is constant over time. However, if OPORTUNIDADES significantly relaxed the liquidity constraint, the marginal propensity to consume might fall over time. We tested and rejected this assumption against the alternative of a constant marginal propensity to consume.

[16] We define adult equivalent household size as the number of children age 12 or younger times 0.5 plus the number family members older than 12.

consumption is also a function of initial assets and household labor endowments, we include baseline household demographics, the community wage rate and head and spouse characteristics, i.e. the $X_{ij,97}$ [17]. As before, the error term is composed of an idiosyncratic disturbance, ξ_{ijt}, and a community random effect, v_j.

The specification in (9) allows us to identify the marginal propensity to consume and the increase in consumption from investment. The marginal propensity to consume, α, is just the coefficient on current transfers, ϕ_1. The rate of return in terms of increased income, γ, is $\phi_2/\phi_1(1-\phi_1)$, where ϕ_2 is the coefficient on lagged transfers, and the rate of return in terms of consumption is $\phi_2/(1-\phi_1)$.

4.2 Estimated Treatment Effects

We now turn to the analysis of the effects of current and lagged cumulative transfers on consumption. Our aim here is to determine the proportion of the cash transfer that is consumed directly out of current transfers, with the remainder being saved or invested. We then capture the long run effects of the program on consumption through the total cumulative transfer amount lagged by 6 months, and argue that any increased long run consumption is achieved through productive investments.

We estimate (9) for the 4 rounds of the ENCEL for which we have detailed consumption information, including home produced consumption. As has been previously discussed, because reception of the transfer is conditional on school enrollment, the transfer amount received is endogenous to household behavior. Thus, we instrument current and lagged actual transfers with their "potential" counterparts[18]. The standard errors are clustered at the community level to control for the correlation of the productivity shocks within communities over time.

[17] The full list of controls is kept the same as for the investment runs with the exception that baseline household size is replaced with baseline household size adult equivalent for the consumption runs.

[18] Separate identification of lagged cumulative transfers from current transfers is possible because treatment households were randomly assigned benefits a year and a half before control households.

The consumption measurement used for the analysis is constructed as total household expenditures on food and non-food items, plus home produced consumption[19]. Expenditures and home produced consumption are separately identifiable in the data. Households are asked about the quantity and expenditures of goods purchased over the week before the interview, as well as how much of their own production of that good was consumed. We use community level prices to impute a value for household production. Previous research on the OPORTUNIDADES program estimates that monetary transfers from the program represent approximately 20% of pre-program consumption (Hoddinott and Skoufias 2004; Skoufias 2005). The summary statistics presented in Table 8 show a similar result, with the mean transfer amount in treatment households equal to 21% of consumption in control households. Comparing the consumption levels of households in treatment and control communities, we observe that treatment households consume approximately 22 pesos more per capita per month, representing 61% of the average per capita monthly transfer of 36 pesos. This simple comparison of consumption levels in treatment and control households suggests that households do not consume the entire transfer amount each month, and that some of the transfer is saved or invested.

Table 9 presents least squares (LS) and two stage least squares (2SLS) estimates of equation (9) for the entire sub-sample of households. Instrumental variable estimates (including controls) on monthly transfers per capita indicate that, on average, 0.884 pesos of each peso transferred are consumed. The coefficient on cumulative transfers is $\hat{\phi}_2$=0.018. These results indicate that households consume over four fifths of the transfers directly. The household invests the remaining 0.116 pesos of each peso transferred, and obtains a return of 0.018 on each peso transferred in terms per capita consumption terms. The return on each peso invested ($\hat{\gamma}$) is 15.52% in terms of consumption and 17.55% in terms of income.

[19] Alternative consumption measures were imputed using quantity consumed times median community prices, and yield comparable results. The consumption measurement used here sums reported expenditures and the imputed value of home production for food and non-food items in a detailed consumption module.

The implied long term effects of the transfers on living standards through the investment pathways are dramatic. In 2003, after five and one-half years on the program, households in the treatment group received an average cumulative transfer of $3,444 pesos per capita. Through the increased income from invested transfers, the average beneficiary consumed 62 pesos more per month[20]. Thus, after five and a half years, transfers increased consumption by 34% through the investment pathway[21]. Since the 34% increase in consumption is through investment in income generating activities, it should continue even if the household dropped out of the program.

These results also suggested that households were liquidity constrained. The estimated rate of return on investment, 17.55%, is substantially higher than the average annual real interest rate in the economy for the entire period (1997 to 2003), which was approximately 6.1%[22]. We take this result as evidence that these households face imperfect credit markets, since obtaining investments in productive activities would yield net gains. In fact, access to credit for the poor rural households in our sample is very limited. In the two rounds of the evaluation survey (May and November 1999) where questions on access to credit are asked, only 2.4% of households report using credit.

4.3 Heterogeneous Treatment Effects

Table 10 presents 2SLS estimates of the effects of current and lagged transfers interacted with farm size in 1997, the age of the head of household, head's education, and distance (in Km) to the closest urban center. The purpose of such interactions is to determine whether household heterogeneity affects preferences (the marginal propensity to consume) and/or productivity (the rate of return).

Non-agricultural households (households with no agricultural assets at baseline) have the highest marginal propensity to consume, while the other three types of households have a similar

[20] "Permanent" rise in consumption estimated as average cumulative transfers per capita for beneficiary households in the treatment group times the coefficient on returns from investment, $\hat{\phi}_2$, estimated as 0.018.

[21] The percentage increase is measured relative to first round of data where consumption data are available. In this survey eligible households in control communities have a real per-capita expenditure of $183 pesos.

[22] The annual real interest rate (computed on the 28-day Inter-Bank Equilibrium Interest Rate) was 5.5% in 1997 and 2.8% in 2003. The average annual real interest rate for the whole period is 6.1% (source: Bank of Mexico and National Institute of Geography and Statistics, INEGI). We take this rate as our benchmark since investments are likely to have occurred over the whole period.

marginal propensity to consume, not significantly different from 0.867. There is no significant difference in the effect of cumulative transfers on consumption either, with a constant return of 0.018 pesos per peso accumulated. Column 3 in Table 10 indicates that the older the head of the household, the lower the estimated marginal propensity to consume, resulting in a larger remainder left for saving or investment (result not statistically significant). However, this reduction in the marginal propensity to consume becomes smaller with age (positive coefficient on the age squared term). As one expects, there is a larger effect on consumption coming from lagged transfers for households with older heads, and once again, this increase becomes smaller over time. Thus, the transfer amount interacted with the age of the head of the household fits a concave function, which is consistent with a life-cycle model.

Column 4 of Table 10 shows that having a positive amount of education reduces the per capita marginal propensity to consume out of current transfers and increases per capita consumption coming from lagged (invested) transfers (the omitted category is no education). Thus, years of education seem to enhance productive activities slightly. Such results are coherent with the effect on age if we assume that basic education has expanded over time in rural communities so that younger people are likely to be more educated than older people. The interaction between distance to an urban center and lagged transfers (Column 5 in Table 10) is negative and significant. Households in more isolated communities may have overall lower return on investments, likely due to the fact that input and output markets are either difficult to access or completely unavailable. The final column of table 10 shows that all the effects are robust to jointly including all interactions in the same specification.

5. Robustness Tests

Despite the identification strategy adopted here, our estimates of the marginal propensities to consume are subject to a number of potential biases or alternative interpretations. Even in a perfect randomized setting, general equilibrium effects that arise as a result of the program may

drive the changes in household consumption and in saving/investment behavior. In what follows, we discuss a series of potential biases, the reasons why we should worry about them, and how they are resolved in this context.

5.1. Macro Income and Price Effects

OPORTUNIDADES infused small rural communities with large amounts of transfers to over half the residents. The transfers could have had multiplier effects on income and cause price inflation in treatment communities. These community macro effects could have caused prices to rise in treatment areas faster than control areas. Therefore, our estimated effects on consumption could be increases in prices rather than in living standards. In addition, it is possible that the observed increases in micro-enterprise and farm activities could be driven by a community macro income effect derived from the presence of OPORTUNIDADES in the community, rather than by increased liquidity from the transfers at the household level.

We address this concern in two ways. First, we compare agricultural wages for males, females and children between treatment and control communities. Second, we compare the consumption levels and investments in micro-enterprise and farm activities of ineligible households in treatment and control communities. Table 11 reports the comparisons of means for community wages between treatment and control communities, and the comparisons of mean consumption and investment levels for ineligible households after the implementation of the program. We find no significant differences between control and treatment areas for any of the variables. Moreover, Hoddinot and Skoufias (2004) find no differences for food prices between treatment and control communities, giving more support to the inexistence of local price and macro income effects.

5.2. Labor Supply Effects

OPORTUNIDADES transfers could be taken in leisure by reducing labor supply. If so, income in treatment households would be reduced over time, as would consumption. Everything else held constant, treatment households would experience lower increases in consumption with respect to control households, the larger the transfer received. In this case, our estimate of the

marginal propensity to consume would be downward biased and so the upper bound for the "real" rate of return will be lower than the one we have considered so far. Parker and Skoufias (2000) study the program effects on time allocation, concluding that OPORTUNIDADES had no effect on adult labor supply. Furthermore, they find no evidence that the program increased leisure time amongst men and women in beneficiary households.

However, OPORTUNIDADES should affect the labor supply of school-age children as it is designed to release them from work so that they can attend school. In fact, Parker and Skoufias (2000; 2001) do find evidence of changes in children's labor force participation in salaried and non-salaried activities, especially for boys; and increases in school attendance. Schultz (2004) also finds positive effects on primary and secondary school enrollment for boys and girls. As explained earlier, we control for this potential bias by instrumenting actual transfers with the potential transfers.

5.3. Fertility Effects

Fertility decisions may also be affected by program participation. The program might generate perverse fertility incentives in order to maximize future transfers received. It might also alter the costs and/or benefits of having a child, as well as reduce the cost of fertility control (or increase its knowledge and availability). The effect on consumption is thus ambiguous since it will depend on household size. Moreover, larger households that receive larger transfer amounts might increase consumption through the income effect. Skoufias (2005) summarizes the findings of a series of evaluations on OPORTUNIDADES until the end of 1999. He reports no record of statistical evidence on fertility rates. In any case, any endogenous fertility responses will be accounted for by the use of the potential transfer (computed on baseline household demographics) as an instrument for the actual transfer amount received.

5.4. Public and Private Transfers

OPORTUNIDADES transfers might also crowd out private transfers. The reduction in these transfers and therefore consumption implies that we would underestimate marginal propensity

to consume. Indeed there is some evidence that supports this potential bias. Using only the October 1998 cross-section collected six months after the intervention, Albarran and Attanasio (2002) find that both the likelihood of receiving a private transfer and the amount received -conditional on receiving private transfers, are significantly and negatively affected by the program.

To assess the extent of the potential bias, we have replicated Albarran and Attanasio's results for the 3 periods for which data on private transfers are available (October 1998, November 1999 and November 2000). First, the amount of private transfers is very small as only 7% of households receive a private transfer in any given period. The estimation results are presented in Table 12. Panel AI reports the effect of OPORTUNDADES on receiving any private transfer and Panel AII reports the estimated effects on the amount of the private transfer received. We estimate four specifications of the OPORTUNIDADES treatment: in the first row we report the results with a OPORTUNIDADES treatment dummy, in the second row we replace the treatment dummy with the potential OPORTUNIDADES transfer amount, in the third row we add potential transfers squared, and in the fourth row we instrument actual OPORTUNIDADES transfers and actual transfers squared with potential transfers and potential transfers squared. We find that the transferred squared models have significant effects, with the marginal effect size falling with the transfer amount.

The same argument could be made for other public transfers. To test this hypothesis, we estimated the effects of OPORTUNIDADES transfers on other public transfers and report the results in Panels BI and BII in Table 12. We find results similar to the private transfer effects.

To test the robustness of our main results to the effect of OPORTUNIDADES on private and other public transfers, we estimate a variant of the consumption equation (9) by adding private and other public transfers to OPORTUNIDADES transfers. If the coefficients on current transfers and lagged cumulative transfers do not change by adding private and other transfers, then we will conclude that there is no bias. The results are presented in Panels A and B of Table 13, for private and other public transfers respectively. The first column in Panel A, estimates the original

specification with only OPORTUNIDADES transfers in equation (9) using the data only from the waves that collected both private transfers and other public transfers using potential transfers as the instrument. The second column reports the results when private transfers are added to OPORTUNIDADES transfers. There is no difference between the estimated coefficients in the two models. Column 3 reports the results when we additionally add other public transfers to the OPORTUNIDADES and private transfers. Again there are no differences. In Panel B we re-estimate the specifications in Panel A using only other public transfers. The first column reports the base results using only OPORTUNIDADES transfers on data from the waves that collected other public transfers. The second column adds in other public transfers. There is no difference between columns 1 and 2 in Panel B. Finally, column 3 controls for whether the family also receives a public food basket. Again, the estimated coefficients do not change. These results imply that our estimates are not biased due to the exclusion of public and private transfers from the analysis.

5.5. Health Effects

We also test the possibility that beneficiary productivity and consumption increased through improvements in health brought on by participation in OPORTUNIDADES. Panel CI of Table 12 estimates the effect of treatment on health, measured by activities of daily living (ADL)[23]. We show that the program does have small positive effect on health. Given this potential pathway, we again estimate our consumption model controlling for health status (ADL) of the head of household on a limited sub-sample where health data are available. The results are presented in Panel C of Table 13, and again we find that the estimated coefficients on current and cumulative lagged transfers do not change controlling for ADLs. This suggests that our main results are not biased by not controlling for health.

5.6. Length of Benefits Expectations

[23] Activity of Daily Living measurements are based on an individuals' self-rating of ability to engage in normal daily activities, including ability to carry out vigorous and moderate activities, ability to carry groceries, ability to lift a piece of paper off the ground, ability to walk more than 2km and ability to use the bathroom and bathe unassisted.

Friedman's (1957) Permanent Income Hypothesis (PIH) contended that permanent income and not current income was the relevant determinant of consumption, and that permanent consumption was proportional to permanent income (the proportionality hypothesis, which implies that the permanent income elasticity is unity)[24]. Such a model would suggest that the marginal propensity to consume out of permanent income should be close to 1, and the marginal propensity to consume from transitory income should be close to 0. The implication of the PIH for our results is that if beneficiary households view the transfers as transitory, then they would save most of them; if they view them as permanent, then they would consume most of them.

The proportionality hypothesis and its major implications regarding growth and equity in developing countries have been tested many times, with ambiguous results. For example, Bhalla (1979) finds an estimated marginal propensity to consume out of permanent income of 0.61 and an elasticity of 0.79 (and significantly different from 1) using data from rural India. Wolpin (1982) estimates permanent income elasticities in the range of 0.91 to 1.02 for India as well and concludes that "whether or not it is unity, the permanent income elasticity of consumption may be closer to unity than suggested by previous research". Musgrove (1979) estimates 0.881 for urban Colombia, 0.896 for urban Ecuador, and, 0.776 for urban Peru; all significantly different from 1. Finally, Paxson (1992) estimates marginal propensities to consume from 0.56 to 0.84 out of permanent income, being significantly different from 1; and from 0.17 to 0.27 out of transitory income for rice farmers in Thailand.

Our estimate of the marginal propensity to consume out of current transfers is 0.88. In view of the estimated magnitudes of marginal propensities to consume in the development literature, we are inclined to think that the rural Mexican households studied here perceive the OPORTUNIDADES transfer as a source of permanent income. Indeed, households know that once they start receiving the benefit they will continue to receive it for at least 9 years provided they

[24] The Modigliani-Brumberg life cycle hypothesis (LCH) reaches analogous conclusions.

comply with the program requirements. Thus, their expectations regarding the length of the benefits are likely to be rather long term.

5.7. Precautionary Savings

We have argued that the evidence on farming and micro-enterprise investment responds to an alleviation of risk aversion and liquidity and credit constraints by the program. However, in an uncertain environment, one might expect households to accumulate wealth to smooth consumption over unexpected shocks. This motive is stronger the higher the uncertainty faced by consumers (Carroll and Kimball 1996). Moreover, if their banking services are scarce, savings are likely to take the form of assets such as land and livestock. For instance, Ravallion and Chen (2005) show that much of the impact of an anti-poverty program in Southwest China did not occur through consumption, but rather through savings, since participants treated income from the program as temporary. If the increase in farm asset ownership, farming and micro-enterprise we observe here is due primarily to precautionary savings rather than investment behavior, the expected increases in long run living standards may not be sustained if households stop saving when the transfer is removed.

To rule this possibility out, we test for the existence of a precautionary motive and how the existence of OPORTUNIDADES affected precautionary savings. We estimate agricultural assets (animal ownership and land usage) in 2003 as a function of adverse shocks in the community and the accumulated actual transfer amount received by the household. We construct the history of shocks from natural causes (principally droughts and plagues) from early 1997 to late 2000 and categorize a community as a "high risk area" if it has suffered more than eight shocks during this period[25]. If there is a precautionary motive, then households in riskier areas (with a larger shock history) should have larger investments in agricultural assets in 2003. A dummy for high risk area is interacted with the accumulated transfer amount. If beneficiary households are using part of the

[25] The number of shocks per community in this period varies from 0 to 13, with a median of 5 shocks and an average of 5.41 shocks. The proportion of treatment and control communities in high risk areas is the same as the proportion of treatment and control communities in the sample. However, control communities tend to report a larger number of shocks than treatment communities.

OPORTUNIDADES transfer for precautionary savings the estimated coefficient on this term should be positive. The coefficient on accumulated actual transfers, which we instrument with its potential counterpart, is simply the "per peso" treatment effect.

Results are shown in Panel A of Table 14 for a linear specification in transfers and in Panel B for a quadratic specification. Production animals and land are the channels of wealth accumulation used by these households to ensure their future income. We find that households living in riskier areas are more likely to use land and have production animals, suggesting a precautionary savings motive. However, none of the OPORTUNIDADES money is used with precautionary motives. In fact, the interaction term is negative and significant for land and production animals suggesting that beneficiary households in riskier areas lower their precautionary savings. We find a similar effect for draft animals once we introduce a quadratic term for the transfer amount. Households in treatment areas may be better insured against future adversities because households view the transfer as a permanent source of income. Thus, this analysis rejects the precautionary savings hypothesis as an alternative explanation for the investments results.

6. Conclusion

The analysis conducted in this study provides evidence that cash transfer payments from the OPORTUNIDADES program increase consumption not only through direct expenditures out of current transfers, but also through the income generated from investing part of the transfers in farms and micro-enterprises. Beneficiary households experience large increases in participation in micro-enterprise activities and increased investments in farm assets and agricultural activities. Furthermore, households that receive the largest transfers are most likely to invest.

Conditional cash transfer programs such as OPORTUNIDADES are designed to alleviate short-term poverty while investing in the human capital of the next generation. The positive benefits of this type of intervention are well established, including increased caloric intake, better health and nutrition, and higher school enrollment for children. This increased human capital is expected to play an important role in breaking the cycle of poverty for younger generations.

However, this paper shows that there are also more immediate implications of cash transfers for alleviating long-term poverty for the current generation of beneficiary households. Cash transfers have the potential to increase income permanently by facilitating investments in productive activities. Furthermore, we argue that the primary mechanisms for achieving this result are through reduced liquidity constraints and decreased risk aversion.

Although we do not argue that cash transfer programs are necessarily the most desirable policy for promoting micro-enterprise or farm investments for poor households in rural areas, it is clear that increased entrepreneurial activity brought on by cash transfers have increased the potential for self-sufficiency. The results presented here suggest that a permanent rise in consumption through increased economic activity may ultimately reduce long-run welfare dependence and permit beneficiary households to attain a higher standard of living that can be sustained even in the absence of the transfer program. Further understanding of the mechanisms through which cash transfers boost productive investments (softening of liquidity and/or credit constraints, reduction of risk aversion, insurance role) is crucially important in the determination and design of future policies to be undertaken, such as micro-lending, business incentives and advising or the provision of insurance schemes, for the alleviation of poverty.

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APPENDIX 1 –TABLES

Table 1: OPORTUNIDADES Monthly Transfer Amounts at Baseline (October 1997)

Transfer Component	Level	Grade	Boys	Girls
Education Stipend	Primary School	3rd year	60	60
		4th year	70	70
		5th year	90	90
		6th year	120	120
	Junior High School	1st year	175	185
		2nd year	185	205
		3rd year	195	225
	High School[1]	1st year	470	540
		2nd year	505	575
		3rd year	535	610
School Supplies Stipend	Primary, 1rst payment		80	80
	Primary, 2nd payment		40	40
	Junior High School		150	150
	High School[1]		240	240
Nutritional Stipend (per family)			90	
Transfer Cap I (per family)[2]			550	
Transfer Cap II (per family)[3]			700	

Source: OPORTUNIDADES (www.oportunidades.gob.mx). Transfer amounts adjusted for inflation every semester according to the Consumer Price Index published by the Bank of Mexico.

[1]High school stipends only granted beginning in the second semester of 2001 (July 2001).

[2]Transfer Cap I is the maximum transfer amount awarded for basic education (primary school and junior high) and nutrition.

[3]Transfer Cap II is the maximum transfer amount given for high school and nutrition.

Table 2: Sample Sizes and Take-Up Rates[1]

	Treatment		Control		All	
	N	%	N	%	N	%
Sample of Eligible (Poor) Households						
Number Non Take-Up Households	839	10.96	485	10.44	1324	10.76
Number Take-Up Households (Actually Treated -TOT)	6,819	89.04	4,159	89.56	10,978	89.24
Total Number of Households (Intent to Treat -ITT)	7,658		4,644		12,302	
Number of Communities	320	63.24	186	36.76	506	

[1]Take-up control households have received at least one bimonthly payment by the time all eligible households should have been incorporated (November 2000). Take-up treatment households must have received their first payment before any eligible control household is phased into the program (November 1999).

[2]We drop 116 households that receive a total transfer amount higher than the maximum they could have potentially received according to their household demographics and treatment status. Households with heads and/or head's spouses younger than 13 or older than 90 also dropped. Drop outs are balanced between the treatment and control samples.

Table 3A: Test of Equality of Means between Potential Treatments and Potential Controls Prior to Program Implementation
Sub-Sample of Original Poor at Baseline (October 1997) -Intent to Treat (ITT)

	Treatment Group			Control Group			
	N	Mean	SD	N	Mean	SD	t-stat
Intent to Treat Sub-Sample (ITT) -Explanatory Variables							
Head's Characteristics							
Age Household Head	7655	41.95	14.133	4643	42.33	14.521	-0.943
Female Head =1	7658	8.16	0.274	4644	8.40	0.277	-0.377
Indigenous Head =1	7646	41.90	0.493	4636	43.70	0.496	-0.324
Head's Education (Years)	5249	4.06	2.306	3143	3.95	2.250	1.148
Never Attended School (Head of Household) =1	7634	31.44	0.464	4637	32.65	0.469	-0.541
Primary School Not Completed (Head of Household) =1	7634	46.28	0.499	4637	46.75	0.499	-0.275
Primary School Completed (Head of Household) =1	7634	16.94	0.375	4637	15.59	0.363	0.988
More than Primary School (Head of Household) =1	7634	5.34	0.225	4637	5.00	0.218	0.516
Spouse's Characteristics							
Age Spouse of Head	6713	36.27	12.225	4074	36.17	12.223	0.296
Spouse's Education (Years)	4291	4.16	2.124	2551	4.21	2.243	-0.603
Never Attended School (Head's Spouse) =1	6700	36.18	0.481	4067	37.35	0.484	-0.411
Primary School Not Completed (Head's Spouse) =1	6700	41.64	0.493	4067	40.69	0.491	0.477
Primary School Completed (Head's Spouse) =1	6700	18.37	0.387	4067	16.97	0.375	0.986
More than Primary School (Head's Spouse) =1	6700	3.81	0.191	4067	4.99	0.218	**-1.953+**
Main Entrepreneur's Characteristics							
Age Main Entrepreneur in the Household	823	0.53	0.499	338	0.54	0.499	-0.178
Education Years Main Entrepreneur in the Household	818	0.35	0.477	335	0.36	0.480	-0.208
Female Main Entrepreneur in the Household =1	818	2.78	2.771	335	2.61	2.582	0.676
Main Entrepreneur is the (likely) Beneficiary Mother =1	822	40.29	13.919	338	40.25	14.299	0.040
Household Characteristics							
Presence of Children 0 to 7 =1	7645	75.36	0.431	4639	77.04	0.421	-1.396
Presence of Children 8 to 17 =1	7645	69.01	0.462	4639	69.54	0.460	-0.472
Presence of Adult Men 18 to 54 =1	7645	83.69	0.369	4639	83.75	0.369	-0.057
Presence of Adult Female 18 to 54 =1	7645	90.26	0.297	4639	90.80	0.289	-0.699
Presence of Adults Older than 55 =1	7644	27.00	0.444	4639	27.20	0.445	-0.177
Household Size	7658	5.91	2.433	4644	5.94	2.413	-0.457
Home Ownership =1	7655	93.39	0.248	4643	92.10	0.270	1.488
Dirt Floor =1	7641	73.17	0.443	4630	75.72	0.429	-1.015
Electricity =1	7653	57.65	0.494	4642	60.58	0.489	-0.684
No Agricultural Assets =1	7634	10.40	0.305	4629	11.67	0.321	-1.047
Landless Farms =1	7634	10.40	0.305	4629	11.67	0.321	-1.047
Small Landed Farms =1	7634	30.59	0.461	4629	32.45	0.468	-0.828
Big Farms =1	7634	46.86	0.499	4629	40.94	0.492	**2.229***
	7634	12.16	0.327	4629	14.95	0.357	-1.876
Community Characteristics							
Village Associations (Community Work)	7658	87.84	0.327	4644	87.75	0.328	0.021
Minimum Distance to Large Urban Centre (Km)	7658	107.73	41.361	4644	105.11	43.721	0.522
Monthly Community Agricultural Male Wage	4426	573.86	172.005	2781	585.23	172.115	-0.463

Notes: +significant at 10%, *significant at 5%, **significant at 1%.T-stat of differences in means computed clustering SE at the community level. Mean of dichotomous variables expressed in percentages.

Table 3B: Test of Equality of Means between Actual Treatments and Actual Controls Prior to Program Implementation
Sub-Sample of Original Poor at Baseline (October 1997) -Treatment on the Treated (TOT)

	Treatment Group			Control Group			
	N	Mean	SD	N	Mean	SD	t-stat
Treatment on the Treated Sub-Sample (TOT) -Explanatory Variables							
Head's Characteristics							
Age Household Head	6818	42.01	13.898	4158	42.49	14.381	-1.140
Female Head =1	6819	7.86	0.269	4159	7.89	0.270	-0.042
Indigenous Head =1	6809	41.72	0.493	4151	43.97	0.496	-0.403
Head's Education (Years)	4745	4.03	2.268	2841	3.91	2.206	1.275
Never Attended School (Head of Household) =1	6797	30.38	0.460	4154	32.04	0.467	-0.796
Primary School Not Completed (Head of Household) =1	6797	47.21	0.499	4154	47.64	0.500	-0.257
Primary School Completed (Head of Household) =1	6797	17.18	0.377	4154	15.53	0.362	1.172
More than Primary School (Head of Household) =1	6797	5.22	0.223	4154	4.79	0.214	0.668
Spouse's Characteristics							
Age Spouse of Head	6037	36.43	12.114	3693	36.47	12.120	-0.111
Spouse's Education (Years)	3917	4.12	2.090	2322	4.15	2.160	-0.287
Never Attended School (Head's Spouse) =1	6027	35.26	0.478	3687	37.08	0.483	-0.669
Primary School Not Completed (Head's Spouse) =1	6027	42.49	0.494	3687	41.58	0.493	0.464
Primary School Completed (Head's Spouse) =1	6027	18.57	0.389	3687	16.79	0.374	1.235
More than Primary School (Head's Spouse) =1	6027	3.68	0.188	3687	4.56	0.209	-1.451
Main Entrepreneur's Characteristics							
Age Main Entrepreneur in the Household	760	40.38	13.964	318	40.42	14.405	-0.035
Education Years Main Entrepreneur in the Household	757	2.77	2.764	315	2.64	2.555	0.519
Female Main Entrepreneur in the Household =1	761	53.35	0.499	318	53.77	0.499	-0.061
Main Entrepreneur is the (likely) Beneficiary Mother =1	757	35.54	0.479	315	35.87	0.480	-0.069
Household Characteristics							
Presence of Children 0 to 7 =1	6810	75.89	0.428	4157	77.24	0.419	-1.101
Presence of Children 8 to 17 =1	6810	70.47	0.456	4157	70.99	0.454	-0.435
Presence of Adult Men 18 to 54 =1	6810	84.33	0.364	4157	84.44	0.363	-0.103
Presence of Adult Female 18 to 54 =1	6810	91.17	0.284	4157	91.46	0.280	-0.378
Presence of Adults Older than 55 =1	6810	26.77	0.443	4157	27.54	0.447	-0.662
Household Size	6819	6.00	2.416	4159	6.04	2.405	-0.583
Home Ownership =1	6816	93.87	0.240	4158	92.83	0.258	1.199
Dirt Floor =1	6803	72.59	0.446	4145	75.49	0.430	-1.116
Electricity =1	6815	58.94	0.492	4158	61.21	0.487	-0.529
No Agricultural Assets =1	6798	9.55	0.294	4145	10.78	0.310	-1.012
Landless Farms =1	6798	30.77	0.462	4145	32.06	0.467	-0.570
Small Landed Farms =1	6798	47.16	0.499	4145	42.17	0.494	**1.84+**
Big Farms =1	6798	12.52	0.331	4145	14.98	0.357	-1.685
Community Characteristics							
Village Associations (Community Work)	6819	89.18	0.311	4159	87.21	0.334	0.432
Minimum Distance to Large Urban Centre (Km)	6819	107.53	41.857	4159	105.06	43.780	0.486
Monthly Community Agricultural Male Wage	4006	573.79	170.117	2498	579.80	163.320	-0.249

Notes: +significant at 10%, *significant at 5%, **significant at 1%.T-stat of differences in means computed clustering SE at the community level. Mean of dichotomous variables expressed in percentages.

Table 4: Exogeneity Test and Average Treatment Effect -Test of Equality of Means between Actual Treatments and Actual Controls
Sub-Sample of Original Poor -Treatment on the Treated (TOT)

	Treatment Group			Control Group			
	N	Mean	SD	N	Mean	SD	t-stat
A. Exogeneity Test -Dependent Variables at Baseline (1997)							
AI. Entire Sub-Sample							
Draft Animals Ownership	6819	35.15	0.477	4159	32.53	0.469	1.109
Production Animals Ownership	6817	81.90	0.385	4158	82.56	0.379	-0.376
Land Use	6819	59.77	0.490	4159	57.30	0.495	0.856
Number Draft Animals†	2391	0.98	1.999	1350	0.89	1.422	0.874
Number Production Animals†	5576	1.25	2.281	3425	1.23	2.356	0.236
Number Hectares Used†	4057	2.68	2.759	2370	2.94	2.851	-1.564
AIII. Landless Households in 97 =1							
Draft Animals Ownership	2092	26.00	0.439	1329	22.72	0.419	1.285
Production Animals Ownership	2092	96.99	0.171	1329	97.07	0.169	-0.097
Number Draft Animals†	543	0.75	2.714	301	0.56	0.913	1.437
Number Production Animals†	2026	0.76	1.494	1287	0.70	1.448	0.796
AIV. Smaller Farms in 97 =1							
Draft Animals Ownership	3206	41.08	0.492	1748	39.59	0.489	0.452
Production Animals Ownership	3206	85.90	0.348	1748	88.39	0.320	-1.288
Number Draft Animals†	1313	0.97	1.604	691	0.89	1.484	0.689
Number Production Animals†	2752	1.20	1.976	1541	1.18	2.086	0.168
Number Hectares Used†	3206	1.60	0.749	1748	1.66	0.749	-1.472
AV. Bigger Farms in 97 =1							
Draft Animals Ownership	851	61.46	0.487	621	56.52	0.496	1.191
Production Animals Ownership	851	91.89	0.273	621	94.36	0.231	-1.602
Number Draft Animals†	522	1.22	1.998	351	1.16	1.562	0.330
Number Production Animals†	780	2.68	3.760	585	2.41	3.579	0.851
Number Hectares Used†	851	6.74	3.654	621	6.53	3.454	0.664
B. Average Treatment Effect -Dependent Variables Oct 98 to Nov 99							
BI. Entire Sub-Sample							
Draft Animals Ownership =1	19406	29.19	0.455	11805	24.88	0.432	**2.013***
Production Animals Ownership =1	19406	74.81	0.434	11805	70.99	0.454	**2.115***
Land Use =1	19409	61.62	0.486	11805	59.06	0.492	0.998
Number of Draft Animals†	5641	0.66	1.070	2928	0.62	0.994	0.658
Number of Production Animals†	14490	1.02	1.951	8357	0.90	1.829	1.437
Number of Hectares Used†	11886	2.10	2.182	6928	2.10	2.171	-0.009
Household has a Micro-Enterprise =1	19409	8.48	0.279	11805	5.85	0.235	**1.855+**
Household has a Female Micro-Enterprise =1	19409	8.11	0.273	11805	5.13	0.221	**2.217***
BII. No Agricultural Assets Households in 97 =1							
Draft Animals Ownership =1	1814	7.55	0.264	1212	9.16	0.289	-0.937
Production Animals Ownership =1	1814	50.88	0.500	1212	42.16	0.494	**2.862****
Land Use =1	1816	27.75	0.448	1212	22.03	0.415	**1.719+**
Number of Draft Animals†	137	0.41	0.580	110	0.34	0.266	1.008
Number of Production Animals†	921	0.41	0.901	510	0.37	0.803	0.453
Number of Hectares Used†	503	1.25	1.403	264	1.36	1.369	-0.854
Household has a Micro-Enterprise =1	1816	7.21	0.259	1212	4.37	0.205	1.203
Household has a Female Micro-Enterprise =1	1816	6.77	0.251	1212	3.63	0.187	1.395
BIII. Landless Households in 97 =1							
Draft Animals Ownership =1	5908	24.68	0.431	3768	19.03	0.393	**2.438****
Production Animals Ownership =1	5908	75.00	0.433	3768	72.03	0.449	1.496
Land Use =1	5908	41.22	0.492	3768	36.73	0.482	**1.683+**
Number of Draft Animals†	1451	0.49	0.888	714	0.44	0.606	1.166
Number of Production Animals†	4426	0.77	1.454	2706	0.64	1.316	**1.767+**
Number of Hectares Used†	2425	1.61	1.860	1379	1.55	1.580	0.653
Household has a Micro-Enterprise =1	5908	8.26	0.275	3768	5.52	0.228	**1.761+**
Household has a Female Micro-Enterprise =1	5908	7.97	0.271	3768	4.99	0.218	**1.949***
BIV. Smaller Farms in 97 =1							
Draft Animals Ownership =1	9171	31.21	0.463	5004	27.26	0.445	1.369
Production Animals Ownership =1	9171	76.91	0.421	5004	73.90	0.439	1.343
Land Use =1	9172	75.44	0.430	5004	76.08	0.427	-0.321
Number of Draft Animals†	2847	0.68	1.126	1359	0.58	0.792	**1.669+**
Number of Production Animals†	7046	1.03	1.847	3688	0.87	1.660	**1.666+**
Number of Hectares Used	6887	1.90	1.903	3798	1.81	1.595	1.121
Household has a Micro-Enterprise =1	9172	8.93	0.285	5004	6.10	0.239	1.579
Household has a Female Micro-Enterprise =1	9172	8.50	0.279	5004	5.14	0.221	**1.996***
BV. Bigger Farms in 97 =1							
Draft Animals Ownership =1	2451	48.51	0.500	1784	40.81	0.492	**1.994***
Production Animals Ownership =1	2451	83.92	0.367	1784	79.88	0.401	**1.804+**
Land Use =1	2451	83.80	0.369	1784	83.02	0.376	0.379
Number of Draft Animals†	1187	0.83	1.103	728	0.87	1.351	-0.307
Number of Production Animals†	2043	1.77	2.953	1423	1.62	2.770	0.690
Number of Hectares Used	2027	3.56	2.832	1460	3.50	3.178	0.213
Household has a Micro-Enterprise =1	2451	8.08	0.273	1784	7.01	0.255	0.891
Household has a Female Micro-Enterprise =1	2451	7.79	0.268	1784	6.56	0.248	1.041

Notes: +significant at 10%, *significant at 5%, **significant at 1%. T-stat of differences in means computed clustering SE at the community level. Mean of dichotomous variables expressed in percentages. Smaller farms are landed households using, at least, 3 Ha. of land; bigger farms use more than 3 Ha. Number of draft or production animals are expressed in equivalent cow units. †Continuous variables conditional on being positive.

Table 5: Effect of OPORTUNIDADES on Agricultural Investments -Treatment and Control Variation
Sub-sample of Original Poor Households in October 1998, May 1999 and November 1999 -Intent to Treat (ITT) and Actual Treatments (TOT)

PANEL I: DICHOTOMOUS OUTCOMES	**Draft Animal Ownership (PROBIT)**	**Draft Animal Ownership $p(y\|y_{97}=0)$ (PROBIT)**	**Production Animal Ownership (PROBIT)**	**Production Animal Ownership $p(y\|y_{97}=0)$ (PROBIT)**	**Land Use (PROBIT)**	**Land Use $p(y\|y_{97}=0)$ (PROBIT)**
Model A: ITT -no controls						
Treatment Status	0.043*	0.025+	0.038*	0.065*	0.029	0.053*
	(0.021)	(0.013)	(0.018)	(0.027)	(0.025)	(0.026)
Model B: TOT -no controls						
Treatment Status	0.043*	0.024+	0.039*	0.062*	0.026	0.050+
	(0.021)	(0.013)	(0.018)	(0.029)	(0.026)	(0.027)
Model C: ITT -controls						
Treatment Status	0.046**	0.023*	0.037*	0.061*	0.028	0.053*
	(0.018)	(0.011)	(0.016)	(0.027)	(0.025)	(0.025)
Model D: TOT -controls						
Treatment Status	0.044*	0.022*	0.037*	0.062*	0.028	0.052*
	(0.017)	(0.011)	(0.017)	(0.028)	(0.025)	(0.025)
Model E: TOT by Farm Size at Baseline (1997) -controls						
Treatment Status * No Agricultural Assets	-0.009	-0.008	0.075**	0.094**	0.063+	0.065+
	(0.034)	(0.019)	(0.022)	(0.033)	(0.038)	(0.039)
Treatment Status * Landless	0.065*	0.035*	0.029+	0.096	0.045+	0.049+
	(0.026)	(0.017)	(0.018)	(0.075)	(0.025)	(0.025)
Treatment Status * Smaller Farm	0.038+	0.029+	0.028	-0.003	-0.002	-
	(0.022)	(0.016)	(0.020)	(0.041)	(0.025)	-
Treatment Status * Bigger Farm	0.046	0.000	0.035	0.007	0.026	-
	(0.028)	(0.022)	(0.023)	(0.083)	(0.032)	-
Observations ITT Sub-Sample (Model A and C)	33313	21899	33313	5964	33316	13641
Mean Dependent Variable ITT Sub-Sample (All)	0.275	0.124	0.729	0.494	0.605	0.360
Mean Dependent Variable ITT Sub-Sample (Controls)	0.248	0.108	0.706	0.453	0.588	0.329
Observations TOT Sub-Sample (Models B, D, E)	31211	20477	31211	5457	31214	12708
Mean Dependent Variable TOT Sub-Sample (All)	0.276	0.123	0.734	0.500	0.606	0.361
Mean Dependent Variable TOT Sub-Sample (Controls)	0.248	0.109	0.710	0.461	0.591	0.332

PANEL II: CONTINUOUS OUTCOMES	**Number of Equivalent Draft Animals (LS)**	**Number of Equivalent Draft Animals $p(y\|y_{97}>0)$ (LS)**	**Number of Equivalent Production Animals (LS)**	**Number of Equivalent Production Animals $p(y\|y_{97}>0)$ (LS)**	**Number of Hectares Used (LS)**	**Number of Hectares Used $p(y\|y_{97}>0)$ (LS)**
Model A: ITT -no controls						
Treatment Status	0.030	0.056	0.086	0.103	0.031	0.008
	(0.025)	(0.035)	(0.075)	(0.080)	(0.089)	(0.115)
Model B: TOT -no controls						
Treatment Status	0.029	0.058	0.090	0.103	0.036	0.023
	(0.025)	(0.037)	(0.072)	(0.076)	(0.091)	(0.117)
Model C: ITT -controls						
Treatment Status	0.030	0.060+	0.084	0.099	0.026	0.014
	(0.022)	(0.033)	(0.067)	(0.071)	(0.084)	(0.112)
Model D: TOT -controls						
Treatment Status	0.031	0.062+	0.092	0.100	0.038	0.031
	(0.023)	(0.034)	(0.065)	(0.068)	(0.086)	(0.113)
Model E: TOT by Farm Size at Baseline (1997) -controls						
Treatment Status * No Agricultural Assets	0.005	-	0.022	-	0.047	-
	(0.030)	-	(0.082)	-	(0.090)	-
Treatment Status * Landless	0.019	0.017	0.012	0.007	0.060	-
	(0.024)	(0.047)	(0.066)	(0.070)	(0.070)	-
Treatment Status * Smaller Farm	0.071**	0.078*	0.119+	0.125+	0.036	0.055
	(0.023)	(0.038)	(0.064)	(0.069)	(0.068)	(0.097)
Treatment Status * Bigger Farm	0.088**	0.112*	0.234**	0.291**	0.162*	0.182
	(0.028)	(0.046)	(0.076)	(0.083)	(0.082)	(0.111)
Observations ITT Sub-Sample (Model A and C)	33313	11385	33313	27301	33193	19573
Mean Dependent Variable ITT Sub-Sample (All)	0.176	0.403	0.722	0.825	1.265	1.763
Mean Dependent Variable ITT Sub-Sample (Controls)	0.152	0.367	0.651	0.741	1.239	1.790
Observations TOT Sub-Sample (Models B, D, E)	31211	10707	31211	25708	31097	18406
Mean Dependent Variable TOT Sub-Sample (All)	0.152	0.367	0.639	0.725	1.240	1.780
Mean Dependent Variable TOT Sub-Sample (Controls)	0.176	0.403	0.715	0.815	1.273	1.767

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parantheses. For the probit models, marginal effects and robust standard errors reported, clustered at the community level. LS regressions include community level random effects. All regressions include wave dummies. Controls in models C to E include: head's and spouse's age, age squared and education level dummies, head's ethnicity (language), household size, dummies controlling for household demographics, baseline assets (home ownership, dirt floor and electricity) and community characteristics (community organizations, distance to urban center and wages). Model E controls for farm size in October 1997 (baseline). Smaller farms use up to 3 Ha. of land; bigger farms use more than 3 Ha.

Table 6: Effect of OPORTUNIDADES on Micro-Entepreneurial Investments -Treatment and Control Variation. Sub-sample of Original Poor Households in October 1998, May 1999 and November 1999 -Intent to Treat (ITT) and Actual Treatments (TOT)

	Micro-Enterprise (PROBIT)	Female Micro-Enterprise (PROBIT)
Model A: ITT -no controls		
Treatment Status	0.026+	0.030*
	(0.014)	(0.013)
Model B: TOT -no controls		
Treatment Status	0.025+	0.028*
	(0.014)	(0.013)
Model C: ITT -controls		
Treatment Status	0.029*	0.031**
	(0.012)	(0.012)
Model D: TOT -controls		
Treatment Status	0.028*	0.030**
	(0.012)	(0.011)
Observations ITT Sub-Sample (Model A and C)	33316	33316
Mean Dependent Variable ITT Sub-Sample (All)	0.076	0.071
Mean Dependent Variable ITT Sub-Sample (Controls)	0.058	0.051
Observations TOT Sub-Sample (Models B and D)	31214	31214
Mean Dependent Variable TOT Sub-Sample (All)	0.075	0.070
Mean Dependent Variable TOT Sub-Sample (Controls)	0.059	0.051

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses. Marginal effects and robust standard errors reported, clustered at the community level. All regressions include wave dummies. Controls in models C and D include head's and spouse's age, age squared and education level dummies, head's ethnicity (language), household size, dummies controlling for household demographics, baseline assets (home ownership, dirt floor and electricity) and community characteristics (community organizations, distance to urban center and wages).

Table 7: Effect of OPORTUNIDADES on Agricultural Investments by Quintiles of Cumulative Transfer (Potential and Actual)
Sub-sample of Original Poor Households from October 1998 through November 2003 -Treatment on the Treated (TOT)

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5	Number of Obs.	Mean Dep. Var.
I. DRAFT ANIMALS OWNERSHIP (PROBIT)							
Model A TOT -Controls, Potential Transfer	0.023	0.035+	0.041+	0.057*	0.066+	51327	0.286
Treatment Status * Quintile	(0.015)	(0.020)	(0.022)	(0.028)	(0.034)		
Model B: TOT -Controls, Actual Transfer							
Treatment Status * Quintile	0.014	0.044*	0.045*	0.055*	0.076*	51327	0.286
	(0.015)	(0.020)	(0.023)	(0.027)	(0.034)		
Model C: TOT -Controls, Actual Transfer p(y|y $_{gz}$ =0)							
Treatment Status * Quintile	0.008	0.019	0.026	0.036+	0.058*	33649	0.136
	(0.010)	(0.014)	(0.017)	(0.020)	(0.027)		
Model D: TOT by Farm Size in 97 -Controls, Actual Transfer p(y|y $_{gz}$ =0)							
Treatment Status * Quintile * No Agricultural Assets	-0.008	-0.011	-0.005	0.019	0.053	33649	0.136
	(0.016)	(0.021)	(0.028)	(0.029)	(0.035)		
Treatment Status * Quintile * Landless	0.020	0.031+	0.051*	0.055*	0.062+		
	(0.014)	(0.019)	(0.024)	(0.025)	(0.033)		
Treatment Status * Quintile * Smaller Farm	0.010	0.028	0.024	0.038+	0.064*		
	(0.014)	(0.018)	(0.020)	(0.023)	(0.031)		
Treatment Status * Quintile * Bigger Farm	-0.002	-0.008	0.000	0.005	0.050		
	(0.018)	(0.024)	(0.026)	(0.027)	(0.035)		
II. PRODUCTION ANIMALS OWNERSHIP (PROBIT)							
Model A: TOT -Controls, Potential Transfer							
Treatment Status * Quintile	-0.015	0.015	0.030+	0.057**	0.065**	51333	0.741
	(0.013)	(0.016)	(0.017)	(0.021)	(0.023)		
Model B: TOT -Controls, Actual Transfer							
Treatment Status * Quintile	-0.016	0.019	0.027	0.054**	0.079**	51333	0.741
	(0.013)	(0.016)	(0.019)	(0.021)	(0.023)		
Model C: TOT -Controls, Actual Transfer p(y|y $_{gz}$ =0)							
Treatment Status * Quintile	0.018	0.057*	0.062	0.111*	0.166**	8924	0.524
	(0.024)	(0.029)	(0.038)	(0.048)	(0.057)		
Model D: TOT by Farm Size in 97 -Controls, Actual Transfer p(y|y $_{gz}$ =0)							
Treatment Status * Quintile * No Agricultural Assets	0.037	0.098**	0.070+	0.163**	0.197**	8924	0.524
	(0.026)	(0.034)	(0.041)	(0.045)	(0.051)		
Treatment Status * Quintile * Landless	-0.051	0.004	0.048	0.127	0.085		
	(0.077)	(0.079)	(0.104)	(0.088)	(0.121)		
Treatment Status * Quintile * Smaller Farm	-0.011	0.002	0.022	0.021	0.131*		
	(0.036)	(0.041)	(0.052)	(0.062)	(0.063)		
Treatment Status * Quintile * Bigger Farm	-0.010	-0.059	0.119	0.071	0.139		
	(0.078)	(0.082)	(0.094)	(0.097)	(0.118)		
III. LAND USE (PROBIT)							
Model A: TOT -Controls, Potential Transfer							
Treatment Status * Quintile	0.006	0.023	0.024	0.033	0.040	51390	0.619
	(0.020)	(0.025)	(0.027)	(0.031)	(0.035)		
Model B: TOT -Controls, Actual Transfer							
Treatment Status * Quintile	0.009	0.015	0.027	0.039	0.062+	51390	0.619
	(0.020)	(0.025)	(0.028)	(0.030)	(0.033)		
Model C: TOT -Controls, Actual Transfer p(y|y $_{gz}$ =0)							
Treatment Status * Quintile	0.033	0.045	0.064+	0.064+	0.098*	20868	0.387
	(0.021)	(0.028)	(0.033)	(0.036)	(0.044)		
Model D: TOT by Farm Size in 97 -Controls, Actual Transfer p(y|y $_{gz}$ =0)							
Treatment Status * Quintile * No Agricultural Assets	0.049	0.058	0.093*	0.043	0.116*	20868	0.387
	(0.032)	(0.040)	(0.046)	(0.046)	(0.054)		
Treatment Status * Quintile * Landless	0.031	0.041	0.056+	0.065+	0.090*		
	(0.022)	(0.028)	(0.034)	(0.036)	(0.044)		
Treatment Status * Quintile * Smaller Farm	-	-	-	-	-		
	-	-	-	-	-		
Treatment Status * Quintile * Bigger Farm	-	-	-	-	-		
	-	-	-	-	-		

Table 7 -continued

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5	Number of Obs.	Mean Dep. Var.
IV. NUMBER OF EQUIVALENT DRAFT ANIMALS (LS)							
Model A: TOT -Controls, Potential Transfer							
Treatment Status * Quintile	0.008	0.002	0.003	0.003	0.034	51327	0.217
	(0.015)	(0.017)	(0.019)	(0.022)	(0.028)		
Model B: TOT -Controls, Actual Transfer							
Treatment Status * Quintile	0.007	0.007	0.025	0.013	0.058*	51327	0.217
	(0.015)	(0.017)	(0.020)	(0.022)	(0.027)		
Model C: TOT -Controls, Actual Transfer p(y|y $_{97}$ >0)							
Treatment Status * Quintile	0.019	0.019	0.040	0.048	0.137*	17638	0.467
	(0.032)	(0.035)	(0.039)	(0.043)	(0.055)		
Model D: TOT by Farm Size in 97 -Controls, Actual Transfer p(y|y $_{97}$ >0)							
Treatment Status * Quintile * No Agricultural Assets	-	-	-	-	-	17638	0.467
	-	-	-	-	-		
Treatment Status * Quintile * Landless	0.002	0.002	0.000	-0.039	0.110		
	(0.059)	(0.063)	(0.068)	(0.068)	(0.075)		
Treatment Status * Quintile * Smaller Farm	0.012	0.013	0.042	0.061	0.110+		
	(0.041)	(0.044)	(0.048)	(0.050)	(0.060)		
Treatment Status * Quintile * Bigger Farm	0.040	0.037	0.085	0.093	0.175*		
	(0.058)	(0.062)	(0.066)	(0.065)	(0.071)		
V. NUMBER OF EQUIVALENT PRODUCTION ANIMALS (LS)							
Model A: TOT -Controls, Potential Transfer							
Treatment Status * Quintile	0.017	0.063+	0.072*	0.147**	0.164**	51333	0.740
	(0.028)	(0.033)	(0.036)	(0.043)	(0.053)		
Model B: TOT -Controls, Actual Transfer							
Treatment Status * Quintile	0.029	0.032	0.104**	0.181**	0.193**	51333	0.740
	(0.028)	(0.033)	(0.038)	(0.042)	(0.052)		
Model C: TOT -Controls, Actual Transfer p(y|y $_{97}$ >0)							
Treatment Status * Quintile	0.038	0.039	0.113**	0.187**	0.187**	42335	0.837
	(0.032)	(0.038)	(0.043)	(0.048)	(0.060)		
Model D: TOT by Farm Size in 97 -Controls, Actual Transfer p(y|y $_{97}$ >0)							
Treatment Status * Quintile * No Agricultural Assets	-	-	-	-	-	42335	0.837
	-	-	-	-	-		
Treatment Status * Quintile * Landless	0.009	0.005	0.049	0.073	0.109		
	(0.047)	(0.053)	(0.059)	(0.061)	(0.070)		
Treatment Status * Quintile * Smaller Farm	0.049	0.025	0.127*	0.196**	0.185**		
	(0.043)	(0.048)	(0.052)	(0.056)	(0.065)		
Treatment Status * Quintile * Bigger Farm	0.010	0.172*	0.237**	0.410**	0.344**		
	(0.070)	(0.076)	(0.080)	(0.078)	(0.084)		
VI. NUMBER OF HECTARES (LS)							
Model A: TOT -Controls, Potential Transfer							
Treatment Status * Quintile	0.121*	0.155*	0.136*	0.205**	0.048	51103	1.481
	(0.053)	(0.062)	(0.067)	(0.079)	(0.099)		
Model B: TOT -Controls, Actual Transfer							
Treatment Status * Quintile	0.135**	0.125*	0.140*	0.247**	0.114	51103	1.481
	(0.052)	(0.062)	(0.070)	(0.078)	(0.097)		
Model C: TOT -Controls, Actual Transfer p(y|y $_{97}$ >0)							
Treatment Status * Quintile	0.154*	0.127	0.137	0.260**	0.093	30314	1.965
	(0.070)	(0.081)	(0.091)	(0.100)	(0.125)		
Model D: TOT by Farm Size in 97 -Controls, Actual Transfer p(y|y $_{97}$ >0)							
Treatment Status * Quintile * No Agricultural Assets	-	-	-	-	-	30314	1.965
	-	-	-	-	-		
Treatment Status * Quintile * Landless	-	-	-	-	-		
	-	-	-	-	-		
Treatment Status * Quintile * Smaller Farm	0.124	0.123	0.140	0.224*	-0.015		
	(0.076)	(0.086)	(0.095)	(0.104)	(0.126)		
Treatment Status * Quintile * Bigger Farm	0.223+	0.094	0.113	0.262+	0.323*		
	(0.126)	(0.137)	(0.145)	(0.144)	(0.158)		

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses. For the probit models, marginal effects and robust standard errors reported, clustered at the community level. LS regressions include community level random effects. All regressions include wave dummies and controls for head's and spouse's age, age squared and education level dummies, head's ethnicity (language), household size, dummies controlling for household demographics, baseline assets (home ownership, dirt floor and electricity) and community characteristics (community organizations, distance to urban center and wages).Data are for the five waves for which we have data on the dependent variables: October 1998, May 1999, November 1999, May 2000 and November 2003. Smaller farms use up to 3 Ha. of land; bigger farms use more than 3 Ha.

Table 8: Consumption and Transfer Amounts -Test of Equality of Means between Actual Treatments and Actual Controls. Sub-Sample of Original Poor from October 1998 to May 1999[1] -Treatment on the Treated (TOT)

	Treatment Group			Control Group			
	N	Mean	SD	N	Mean	SD	t-stat
I. Entire Sub-Sample							
Monthly Consumption per Capita (Home Production Included)	12667	194.09	96.597	7710	171.91	93.004	4.900
Actual Monthly Transfer per Capita	12667	36.35	21.528	-	-	-	-
6-month Lagged Actual Cumulated Transfer per Capita	12667	141.33	123.927	-	-	-	-
Potential Monthly Transfer per Capita	12667	48.61	26.256	-	-	-	-
6-month Lagged Potential Cumulated Transfer per Capita	12667	216.64	187.569	-	-	-	-
II. No Agricultural Assets Households in 97 =1							
Monthly Consumption per Capita (Home Production Included)	1187	215.02	106.803	785	190.92	102.052	3.203
III. Landless Households in 97 =1							
Monthly Consumption per Capita (Home Production Included)	3864	199.84	95.667	2463	182.24	93.550	3.268
IV. Smaller Farms in 97 =1							
Monthly Consumption per Capita (Home Production Included)	5986	187.69	94.076	3284	160.99	90.104	5.213
V. Bigger Farms in 97 =1							
Monthly Consumption per Capita (Home Production Included)	1592	189.29	97.530	1154	167.67	88.754	3.046

Notes: T-stat of differences in means computed clustering SE at the community level. All consumption values are expressed in per capita adult equivalent units. Smaller farms are landed households using less than 3 Ha. of land; bigger farms use more than 3 Ha.

[1]Data on home production is only available for two rounds of data, namely October 1998 and May 1999.

Table 9: Effect of Current Transfers (Marginal Propensity to Consume) and Lagged Cumulative Transfers on Consumption Sub-sample of Original Poor Households from October 1998 through November 2003[1]-Actual Treatments (TOT)

	Consumption Linear I (LS)	Consumption Linear II (LS)	Consumption Linear III (2SLS)	Consumption Linear IV (2SLS)
Actual Transfer (monthly pc ae) -Preferences	0.649**	0.510**	0.733**	0.884**
	(0.020)	(0.022)	(0.034)	(0.035)
Actual Cumulative Transfer (6 months lag pc ae) -Productivity		0.011**	0.012**	0.018**
		(0.001)	(0.001)	(0.001)
Full Set of Controls	N	N	N	Y
Number of Observations	39529	39529	39529	39529
Mean of Dependent Variable (All)	186.598	186.598	186.598	186.598
Mean of Dependent Variable (Controls)	176.070	176.070	176.070	176.070

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses. All regressions include random effects at the community level and wave dummies. The full list of controls additionally includes: head's and spouse's age, education level dummies, head's ethnicity (language), household size adult equivalent and household demographic variables, baseline assets (home ownership, dirt floor and electricity), farm size at baseline and community characteristics (community organizations, distance to urban center and wages). The excluded instruments in the 2SLS models are the monthly pc ae potential transfer and the 6-month lag cumulative potential pc ae transfer. The first stages show significant effects of the excluded instruments on the endogeneous explanatory variables and are available upon request. Consumption is expenditures expressed in per capita adult equivalent units and includes home production. Outliers trimmed at the top and bottom 1% of both the consumption and transfer distributions. pc means "per capita"; ae means "adult equivalent".

[1]Because of the inclusion of home production, we only have data on consumption for the following rounds: October 1998, May 1999, Nov 2000 and Nov 2003.

Table 10: Consumption -Heterogeneous Treatment Effects
Sub-sample of Original Poor Households from October 1998 through November 2003[1]-Actual Treatments (TOT)

	Consumption Linear IV (2SLS)	Consumption by Farm Size (2SLS)	Consumption by Head's Age (2SLS)	Consumption by Head's Education (2SLS)	Consumption by Distance (2SLS)	Consumption by Head and Household Characteristics
Actual Transfer (monthly pc ae) -Preferences						
Linear (Alone)	0.886**	0.868**	1.094**	1.073**	0.872**	1.277**
	(0.035)	(0.038)	(0.261)	(0.055)	(0.034)	(0.288)
Interacted with Non Agricultural Household at Baseline (1997) =1		0.181*				0.166+
		(0.090)				(0.091)
Interacted with Bigger Farm at Baseline (1997) =1		0.020				0.0001
		(0.078)				(0.078)
Interacted with Head's Age			-0.017			-0.018+
			(0.011)			(0.011)
Interacted with Head's Age Squared			0.0002*			0.0002*
			(0.0001)			(0.0001)
Interacted with Head has not completed primary =1				-0.251**		-0.170*
				(0.064)		(0.069)
Interacted with Head has completed primary or more =1				-0.304**		-0.194*
				(0.077)		(0.090)
Actual Cumulative Transfer (6 months lag pc ae) -Productivity						
Linear (Alone)	0.018**	0.018**	0.009*	0.016**	0.028**	0.015**
	(0.001)	(0.001)	(0.004)	(0.001)	(0.002)	(0.005)
Interacted with Non Agricultural Household at Baseline (1997) =1		-0.002				-0.003
		(0.002)				(0.002)
Interacted with Bigger Farm at Baseline (1997) =1		0.002				0.002
		(0.001)				(0.001)
Interacted with Head's Age			0.0004**			0.0004**
			(0.0001)			(0.0001)
Interacted with Head's Age Squared			-5.35*10-6**			-5.30*10-6**
			(0.0001)			(0.0001)
Interacted with Head has Not Completed Primary =1				0.003**		0.003*
				(0.001)		(0.001)
Interacted with Head has Completed Primary or More =1				0.001		0.001
				(0.001)		(0.002)
Interacted with Min Distance to Urban Center (/100)					-0.009**	-0.009**
					(0.001)	(0.001)
Full Set of Controls	Y	Y	Y	Y	Y	Y
Number of Observations	39529	39529	39529	39529	39529	39529
Mean of Dependent Variable (All)	186.598	186.598	186.598	186.598	186.598	186.598
Mean of Dependent Variable (Controls)	176.070	176.070	176.070	176.070	176.070	176.070

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses. All regressions include random effects at the community level and the following controls: wave dummies, head's and spouse's age, education level dummies, head's ethnicity (language), household size adult equivalent and household demographic variables, baseline assets (home ownership, dirt floor and electricity), farm size at baseline and community characteristics (community organizations, distance to urban center and wages). Main effects of the controls added for those specifications with interactions. The excluded instruments are the monthly pc ae potential transfer, the 6-month lag cumulative potential pc ae transfer and its respectives interactions, depending on the specification. The first stages show significant effects of the excluded instruments on the endogeneous explanatory variables and are available upon request. Consumption is expenditures expressed in per capita adult equivalent units and includes home production. Outliers trimmed at the top and bottom 1% of both the consumption and transfer distributions. pc means "per capita"; ae means "adult equivalent". Landless and smaller farms are the omitted farm size categories; no education is the omitted education category. [1]Because of the inclusion of home production, we only have data on consumption for the following rounds: October 1998, May 1999, Nov 2000 and Nov 2003.

Table 11: Are there Price Effects? Testing for Macro Income and Price Effects
Sub-sample of Ineligibles (Non-Poor) Households in October 1998, May 1999 and November 1999 -Intent to Treat (ITT)

	Treatment Group			Control Group			
	N	Mean	SD	N	Mean	SD	t-stat
A. Raw Means -no controls							
***AI.* Agricultural Investments**							
Draft Animal Ownership	9053	37.23	0.483	5805	35.47	0.478	0.654
Number of Equivalent Draft Animals†	3341	0.90	1.523	2047	0.87	1.252	0.320
Production Animal Ownership	9054	75.99	0.427	5805	75.23	0.432	0.447
Number of Production Animals†	6858	1.98	3.334	4345	1.98	3.389	0.005
Land Ownership	9054	65.66	0.475	5806	64.74	0.478	0.423
Hectars of Land Used†	5819	2.98	3.000	3703	3.19	3.005	-0.975
AII. Micro-Enterprise Investments							
Micro-Enterprise	9054	9.21	0.289	5806	8.01	0.271	1.068
Female Micro-Enterprise	9054	8.92	0.285	5806	7.34	0.261	1.477
AIII. Consumption[1]							
Monthly Consumption per Capita (Home Production Included)†	6155	261.74	197.939	3874	255.97	198.147	0.729
AIV. Community Wages							
Monthly Male Agricultural Wage†	860	605.13	202.668	488	613.94	195.336	-0.520
Monthly Female Agricultural Wage†	364	534.53	191.883	197	516.00	188.584	0.884
Monthly Children Agricultural Wage†	318	437.42	183.389	160	421.03	162.437	0.811

Notes: T-stat of differences in means computed clustering SE at the community level. All consumption values are expressed in per capita adult equivalent units. †Continuous outcomes conditional on being positive

[1]Data on home production is only available for two rounds of data, namely October 1998 and May 1999.

Table 12: Effect of OPORTUNIDADES on Private Transfers (Panels AI, AII), Other Public Transfers (Panels BI, BII) and Activitities of Daily Living (Panel CI) Sub-sample of Original Poor Households -Actual Treatments (TOT)

	Treatment Status	Potential Transfer (monthly)	Potential Transfer Squared (monthly)	Cumulative Transfer (6 month lag)	Actual Transfer (monthly)	Actual Transfer Squared (monthly)	Actual Cumulative Transfer (6 month lag)	Number of Obs.	Mean Dep. Var.
AI. Private Transfers[1] =1									
Model AI.1 -PROBIT (Oct 98, Nov 99)	-0.002							20847	0.070
	(0.005)								
Model AI.2 -PROBIT (Oct 98, Nov 99, Nov 00)		-0.019						31071	0.067
		(0.014)							
Model AI.3 -PROBIT (Oct 98, Nov 99, Nov 00)		-0.087*	0.148*					31071	0.067
		(0.034)	(0.059)						
Model AI.4 -2SLS (Oct 98, Nov 99, Nov 00)					-0.223**	0.531**		31083	0.067
					(0.067)	(0.193)			
AII. Private Transfers Amount (monthly)†									
Model AII.1 -LS (Oct 98, Nov 99)	-54.798+								
	(29.780)								
Model AII.2 -LS (Oct 98, Nov 99, Nov 00)		-0.089						1512	337.618
		(0.096)							
Model AII.3 -LS (Oct 98, Nov 99, Nov 00)		-0.587*	0.001*					1512	337.618
		(0.234)	(0.0004)						
Model AII.4 -2SLS (Oct 98, Nov 99, Nov 00)					-1.235*	0.003*		1512	337.618
					(0.498)	(0.001)			
BI. PublicTransfers[2] =1									
Model BI.1 -PROBIT (Oct 98 - Nov 99)	-0.115**							30659	0.369
	(0.022)								
Model BI.2 -PROBIT (Oct 98 - Nov 00)		-0.243**						50208	0.346
		(0.047)							
Model BI.3 -PROBIT (Oct 98 - Nov 00)		-0.537**	0.611**					50208	0.346
		(0.126)	(0.190)						
Model BI.4 -2SLS (Oct 98 - Nov 00)					-0.611**	0.820**		50208	0.346
					(0.084)	(0.202)			
BII. Public Transfers Amount (monthly)†									
Model BII.1 -LS (Oct 98 - Nov 99)	-9.534**							8732	90.544
	(3.472)								
Model BII.2 -LS (Oct 98 - Nov 00)		-0.002						13881	91.991
		(0.006)							
Model BII.3 -LS (Oct 98 - Nov 00)		-0.036*	6.55*10-5*					13881	91.991
		(0.017)	(2.99*10-5)						
Model BII.4 -2SLS (Oct 98 - Nov 00)					-0.073*	1.69*10-4*		13881	91.991
					(0.032)	(7.74*10-5)			
CI. Activity of Dalily Living[3] (ADL)									
Model CI.1 -LS (May 99 - Nov 99)	0.003+							40491	0.978
	(0.002)								
Model CI.2 -LS (May 99, Nov 99, Nov 00)		0.073**						59907	0.979
		(0.019)							
Model CI.3 -LS (May 99, Nov 99, Nov 00)		0.065**		0.001				59907	0.979
		(0.021)		(0.001)					
Model CI.4 -2SLS (May 99, Nov 99, Nov 00)					0.107**			59907	0.979
					(0.038)				
Model CI.4 -2SLS (May 99, Nov 99, Nov 00)					0.112*		-0.0001	59907	0.979
					(0.048)		(0.003)		

Notes: +significant at 10%, *significant at 5%, **significant at 1%. For the probit models, marginal effects and robust standard errors clustered at the community level reported. LS and 2SLS regressions include community level random effects. All regressions include the following controls: wave dummies, head's and spouse's age, age squared and education level dummies, head's ethnicity (language), household size adult equivalent, dummies controlling for household demographics, baseline assets (home ownership, dirt floor and electricity), farm size at baseline and community characteristics (community organizations, distance to urban center and wages). Outliers trimmed at the top and bottom 0.5% of the OPORTUNDADES (public) and the private transfers distributions. For other public transfers, outliers trimmed at the top 5% of the distribution. †Conditional on being positive.

[1]Private transfers include both transfers from neighbours, relatives and friends living in the community and transfers received from migrants (conditional on a household member having migrated during the 5 years before the interview). Estimation limited to rounds: Oct 1998, Nov 1999 and Nov 2000 due to non-existent or non-comparable data.

[2] Public transfers =1 if someone in the household receives help from Procrampo, Niños con Solidaridad and/or DIF food package. Public transfer amount includes the total household monetary value of the transfers received from Procampo and Niños con Solidaridad. Estimation limited to rounds Oct 1998 to Nov 00.

[3]Estimation limited to the sub-sample of prime age adults (18 to 49 years old) in May 1999, Nov 1999 and Nov 2000 due to non-existent data in other rounds. OPORTUNIDADES actual and potential transfer amounts are expressed in per capita adult equivalent units for this set of regressions.

In Panels AI, BI and CI the OPORTUNIDADES potential and actual transfers (and its squared term) have been divided by 1000.

Table 13: Potential Biases in the Consumption Equation: Private Transfers, Public Transfers and Increased Productivity. Sub-sample of Original Poor Households in October 1998 through November 2003 -Actual Treatments (TOT)

	Consumption (2SLS)	Consumption (2SLS)	Consumption (2SLS)
A. Private Transfers [1] -controls			
Actual Transfer - monthly pc ae	1.055**		
	(0.049)		
Total Actual Transfers (OPORTUNIDADES + Private) -monthly pc ae		1.035**	
		(0.048)	
Total Actual Transfers (OPORTUNIDADES + Other Public + Private) -monthly pc ae			1.013**
			(0.050)
Actual Cumulative Transfer -6 months lag pc ae	0.016**	0.015**	0.010**
	(0.002)	(0.002)	(0.002)
Number of Observations	20282	20282	20267
Mean Consumption	181.476	181.476	181.985
B. Public Transfers [2] -controls			
Actual Transfer - monthly pc ae	0.913**		
	(0.035)		
Total Actual Transfers (OPORTUNIDADES + Other Public) -monthly pc ae		0.961**	0.966**
		(0.038)	(0.038)
Actual Cumulative Transfer -6 months lag pc ae	0.020**	0.018**	0.018**
	(0.002)	(0.002)	(0.002)
Receive Food Package DIF =1			7.454**
			(2.302)
Number of Observations	30126	30126	30126
Mean Consumption	181.916	181.916	181.916
C. Increased Productivity: Activity Daily Living (ADL) [3] -controls			
Actual Transfer - monthly pc ae	0.685**	0.685**	-
	(0.046)	(0.046)	-
Actual Cumulative Transfer -6 months lag pc ae	0.015**	0.015**	-
	(0.003)	(0.003)	-
Activity of Daily Living (ADL) Household's Head		-1.500	-
		(7.706)	-
Number of Observations	13179	13179	-
Mean Consumption	175.236	175.236	-

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses. All regressions include random effects at the community level and the following controls: wave dummies, head's and spouse's age, age squared and education level dummies, head's ethnicity (language), household size adult equivalent and household demographic variables, baseline assets (home ownership, dirt floor and electricity), farm size at baseline and community characteristics (community organizations, distance to urban center and wages). Consumption is expenditures expressed in per capita adult equivalent units and includes home production. Outliers trimmed at the top and bottom1% of the consumption and the total transfer distributions. pc means "per capita"; ae means "adult equivalent".

[1]Because of the inclusion of home production and private transfers, estimation is limited to the following rounds: October 1998 and November 2000. Private transfers include both transfers from neighbours, relatives and friends living in the community and transfers received from migrants (conditional on a household member having migrated during the 5 years before the interview).

[2]Because of the inclusion of home production, estimation is limited to the rounds of October 1998, May 1999 and November 2000. Public transfers include the total household monetary value of the transfers received from Procampo and Niños con Solidaridad. DIF =1 if someone in the househod has received a subsidized food package during the past month.

[3]Because of the inclusion of home production and ADLs, estimation is limited to the following rounds: May 1999 and November 2000.

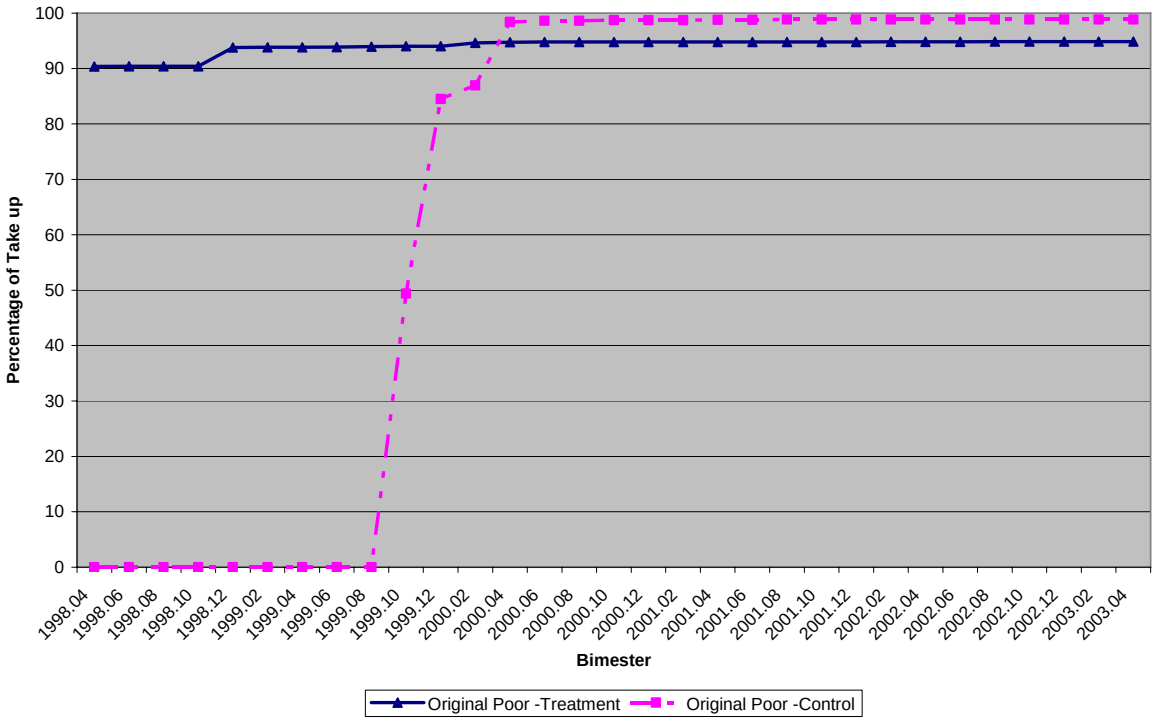
Table 14: Investment or Precautionary Savings?
Sub-sample of Original Poor Households in November 2003 -Actual Treatments (TOT)

	Draft Animal Ownership (2SLS)	Number of Equivalent Draft Animals p(y|y >0) (2SLS)	Production Animal Ownership (2SLS)	Number of Equivalent Production Animals p(y|y >0) (2SLS)	Land Use (2SLS)	Number of Hectares Used p(y|y >0) (2SLS)
Model A: TOT -controls						
High Risk Area	0.020	0.086	0.117**	0.097	0.147**	-1.545
	(0.049)	(0.300)	(0.036)	(0.221)	(0.048)	(0.970)
Total Household Actual Cumulative Transfer (/1000)	0.004*	-0.004	0.004*	0.002	0.002	-0.008
	(0.002)	(0.013)	(0.002)	(0.008)	(0.002)	(0.027)
High Risk Area * Total HH Actual Cumulative Transfer (/1000)	0.000	-0.020	-0.006**	0.001	-0.008**	0.081
	(0.003)	(0.016)	(0.002)	(0.014)	(0.003)	(0.056)
Model B: TOT -controls						
High Risk Area	0.009	-0.806+	0.094	-0.288	0.037	-0.509
	(0.101)	(0.421)	(0.078)	(0.398)	(0.098)	(1.870)
Total Household Actual Cumulative Transfer (/1000)	-0.001	-0.073	0.003	-0.057*	0.006	-0.197+
	(0.006)	(0.046)	(0.005)	(0.028)	(0.006)	(0.113)
Total Household Actual Cumulative Transfer Squared (/1000)	0.000	0.002	0.000	0.002*	0.012	0.006+
	(0.000)	(0.001)	(0.000)	(0.001)	(0.015)	(0.003)
High Risk Area * Total HH Actual Cumulative Transfer (/1000)	0.002	0.132+	-0.002	0.068	-0.000	-0.099
	(0.018)	(0.071)	(0.012)	(0.072)	(0.000)	(0.324)
High Risk Area * Total HH Actual Cumulative Transfer Squared (/1000)	-0.000	-0.005*	-0.000	-0.002	-0.001	0.006
	(0.001)	(0.002)	(0.000)	(0.002)	(0.000)	(0.012)
Observations TOT Sub-Sample	9953	2968	9957	7366	10009	6342
Mean Dependent Variable TOT Sub-Sample	0.303	1.147	0.746	1.066	0.681	3.694
Proportion Households in High Risk Areas	0.093	0.104	0.093	0.097	0.093	0.094

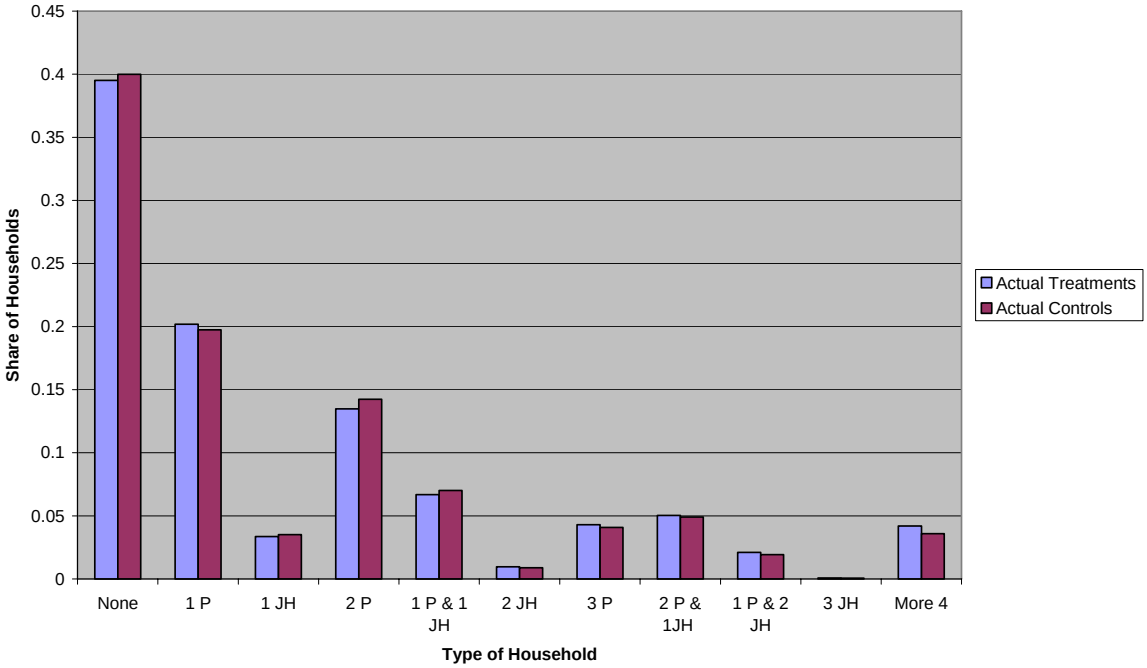
Notes: +significant at 10%, *significant at 5%, **significant at 1%. Community level clustered SE in parantheses. All regressions include the following controls: wave dummies, head's and spouse's age, age squared and education level dummies, head's ethnicity (language), household size, dummies controlling for household demographics, baseline assets (home ownership, dirt floor and electricity) and community characteristics (community organizations, distance to urban center and wages). High risk areas are communities that have suffered more than 8 weather shocks between early 1997 and late 2000. The number of shocks per community in these years varies from 0 to 13, with a median of 5 shocks and an average of 5.41 shocks.

APPENDIX 2 -GRAPHS

Graph 1: OPORTUNIDADES Program Take up Rates Over Time

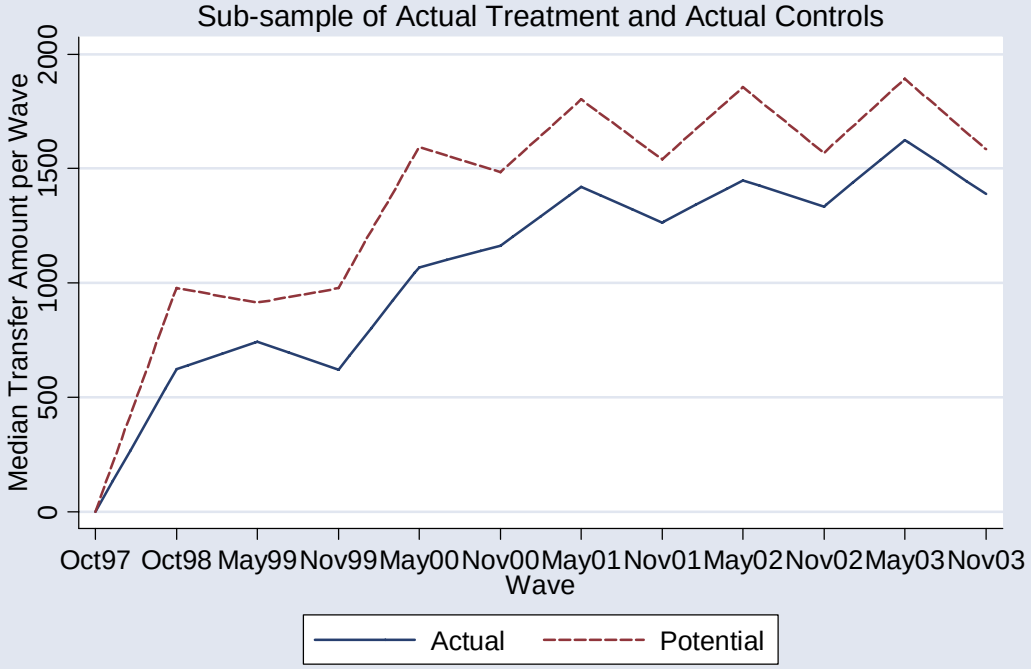


Graph 2: Share of Households by Number of Children Enrolled from 3rd Grade of Primary to 3rd Grade of Junior High School at Baseline (October 1997).

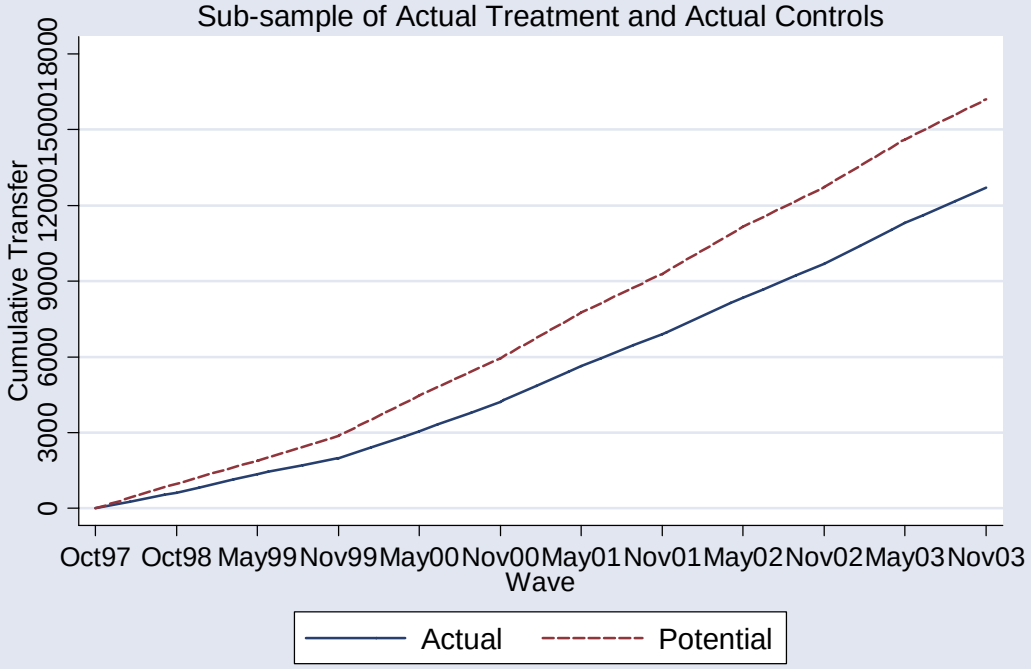


Note: P =Primary School; JH =Junior High School

Graph 3a: Actual and Potential Transfer Distributions
Sub-sample of Actual Treatment and Actual Controls



Graph 3b: Cumulative Actual and Potential Transfer Distributions
Sub-sample of Actual Treatment and Actual Controls



Graph 4: Mean Actual and Potential Cumulative Transfer Amount by Quintile