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Investing Cash Transfers to Raise Long-Term Living Standards[†]

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Using data from a randomized experiment, we find that poor rural Mexican households invested part of their cash transfers from the Oportunidades program in productive assets, increasing agricultural income by almost 10 percent after 18 months of benefits. We estimate that for each peso transferred, households consume 74 cents and invest the rest, permanently increasing long-term consumption by about 1.6 cents. Results suggest that cash transfers can achieve long-term increases in consumption through investment in productive activities, thereby permitting beneficiary households to attain higher living standards that are sustained even after transitioning off the program. (JEL D14, H23, I38, O12*)*

> *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 coop, and with what was left we bought a few chickens. Since then, we have raised many chickens that we sometimes sell, and we collect 10 to 15 eggs per week that we eat ourselves.*
>
> —*Oportunidades* beneficiary in rural Mexico, August 2004[1]

Cash transfer programs are increasingly common tools for fighting poverty in developing countries. These programs are concerned with providing relief through income support to those living in extreme poverty, and in the case of conditional cash transfer (CCT) programs, with providing incentives for parents to invest in the human capital of children. A major concern for policymakers and development practitioners has been the extent to which households become dependent on cash transfers to maintain current living standards, and when transitioned off of these programs, whether they will revert to preprogram poverty levels.

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[†] To comment on this article in the online discussion forum, or to view additional materials, visit the article page at http://dx.doi.org/10.1257/app.4.1.164.

[1] Quote is from a survey of beneficiaries conducted by the authors in the summer of 2004.

In this paper, we investigate the hypothesis that poor families use some of their cash transfers to invest in productive entrepreneurial activities that raise their living standards in the long run. There are two primary reasons why cash transfers may lead to increased investments by poor households. First, cash transfers alleviate liquidity and credit constraints.[2] An infusion of cash through the transfer may help households afford the startup costs associated with entrepreneurial activities (McKenzie and Woodruff 2006). Second, if cash transfers are perceived as a secure and steady source of income over time, risk-adverse households will be more willing to invest in riskier but higher return activities. With a secure stream of income from the cash transfer program that is large enough to alleviate liquidity constraints, we would expect to see poor households making more investments in productive assets and activities. If returns on investments are maintained over time, poor households will achieve improvements in long-term living standards that persist even in the absence of the cash transfer program.

We test this hypothesis using data from a controlled randomized experiment of the Oportunidades CCT program in Mexico. We find that beneficiary households increased ownership of productive farm assets, such as farm animals and land for agricultural production, significantly faster than nonbeneficiary households; that agricultural production in terms of both crops and animal products increased faster for beneficiary households than nonbeneficiary households; and that this resulted in significantly higher agricultural income. In fact, we estimate that an 18-month exposure to the program resulted in a 9.6 percent increase in agricultural income. Beneficiary households also started substantially more nonagricultural microenterprises, mainly production of handcrafts for sale, compared to nonbeneficiary households.

We then explore whether returns on these investments persist over time and raise long-term living standards as measured by consumption. We find that even 4 years after households in the control group were incorporated into the program, consumption levels for the original treatment households were 5.6 percent higher than for the original control households. This result suggests that returns on investments made by treatment households during the initial 18-month experimental period did in fact translate into improvements in long-term living standards.

Finally, we identify two parameters that require additional assumptions beyond the randomized experimental design. Specifically, we estimate the marginal propensity to consume and invest out of transfers and the marginal investment effect, i.e., the impact of one peso transferred on long-term consumption via the investment pathway. We then use these estimated parameters to calculate the effect of the program on long-term living standards.

Our analysis shows that for each peso transferred, beneficiary households consume 74 cents directly and invest the rest. The aggregate effect of the investment yields about a 1.6 cent increase in long-term consumption for each peso of transfers received. Using these estimates to predict the effect of investments on long-term consumption, we estimate that after 5 ½ years (the period of observation in our data), beneficiary households increase consumption by 41.9 pesos per capita per month.

[2]Liquidity constraints and poverty have been explored in Banerjee and Newman (1993); Aghion and Bolton (1997); Lindh and Ohlsson (1998); Lloyd-Ellis and Bernhardt (2000); and Banerjee (2004), amongst others.

After 9 years (the minimum expected duration of the program) this amount increases to 53.9 pesos, a substantial amount relative to the 159.8 pesos of baseline per capita consumption for equivalent households that do not participate in the program. As a result, if removed from the program, beneficiary households would most likely not revert to pre-program poverty levels.

To our knowledge, this paper is the first to consider the long-term poverty implications of productive investments made as a result of CCT programs. Our paper is related to a small literature on uses of transfers for nonconsumption purposes. Sadoulet, de Janvry, and Davis (2001) estimate the effect of payments from the Mexican agricultural assistance program, PROCAMPO, on consumption, and find a multiplier greater than 1 consistent with an interpretation of productive investment. Ravallion and Chen (2005) show that the lion's share of a temporary public cash transfer program in China was saved. Woodruff and Zenteno (2007) and Yang (2008) show that private remittances were used as capital to invest in microenterprises in Mexico and the Philippines, respectively. Our work also contributes to the literature on the effects of CCT programs on household consumption (Hoddinot and Skoufias 2004; Attanasio and Mesnard 2006; Angelucci and Attanasio 2009). However, these papers do not explore how transfers affect long-term consumption via the investment pathway.

The remainder of the paper is organized as follows. Sections I and II describe the Oportunidades program and its experimental evaluation design and data. In Section III, we show the causal link between cash transfers and investments in productive assets and activities; and in Section IV, we explore the effect of these investments on income and productivity. Section V then tests for sustained long-run effects of the investments on household consumption. Section VI provides a number of falsification tests to validate the impacts observed in Sections III, IV, and V. In Section VII, we move beyond the purely experimental data and estimate the marginal propensity to consume and invest transfers, and the marginal investment effect. Section VIII concludes.

I. The Rural Oportunidades Program[3]

The Mexican government established the Oportunidades CCT program in 1997. The primary objective was to break the intergenerational transmission of poverty by alleviating current poverty while investing in the human capital of the next generation. To this end, the program provides financial incentives (cash) to parents to invest in the human capital of their children, encouraging investments in health, nutrition, and education. The program is one of the largest CCT interventions in the world, with approximately $4.5 billion US distributed to some 5.8 million beneficiary households in 2010 (Oportunidades 2011).

The rural Oportunidades program operates in small credit constrained communities with between 50 to 2,500 inhabitants in isolated and marginalized rural areas.

[3] The Oportunidades program was known originally as PROGRESA. The name was changed from PROGRESA to Oportunidades during the Vicente Fox administration in 2000. The data used here are from the rural PROGRESA evaluation. However, we refer to the program under its current name, Oportunidades, throughout the text.

Approximately 89 percent of households in these communities engage in farming activities, such as raising crops and farm animals; and 7 percent of households report having a small business or microenterprise. A large number of households are also engaged in the local labor market. Indeed, 65 percent of the households have at least one household member employed in a paid activity, mostly as agricultural day laborers. Credit opportunities are very limited, with only 1.2 percent of communities having a formal credit institution, typically in the form of a credit cooperative (*caja de ahorro o cooperativa de crédito*). Not surprisingly, only 2.8 percent of households report to have active loans, and from those, almost all are informal loans from friends, neighbors, and relatives.[4]

A. *Eligibility and Duration of Benefits*

When Oportunidades began rolling out to rural areas in 1997, program eligibility was determined in two stages (Skoufias, Davis, and de la Vega 2001). The program first identified marginalized communities and then identified low-income households within those communities.[5] Thus, a household's eligibility depends on whether it is in an eligible village and on its own poverty level based on a proxy means test. All eligible households living in treatment communities were offered Oportunidades and the vast majority of them (90 percent) 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. After the initial three-year enrollment period, households in rural areas were "recertified" (reassessed with a proxy means test) to determine future eligibility. If a household was recertified as eligible, it would continue to receive benefits for another three years until the next recertification. If a household was not recertified, then the household was granted six more years of support before being phased off the program. Thus, households could expect a minimum of nine years of benefits upon enrollment.[6]

B. *Benefit Structure*

Cash transfers from Oportunidades are given to the female head of the household and are conditional on children attending school, family members obtaining preventive medical care, and attending "pláticas" or education talks on health related

[4] Authors' calculations using data on control households from the Oportunidades household and community questionnaires.

[5] 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 socioeconomic survey of all households, the *Encuesta de Características Socioeconómicas de los Hogares* (ENCASEH). The Government used the ENCASEH to construct a proxy means index and classify households as eligible for treatment ("poor") or ineligible ("nonpoor"). The original classification scheme designated approximately 52 percent of households as eligible (poor). Subsequently, the government decided that a subset of the nonpoor households had been unduly excluded and expanded the eligibility criteria to include a set of slightly wealthier households in a process called "densification" (Hoddinott and Skoufias 2004).

[6] See Oportunidades (2003) for the rules that lay out eligibility and other operational aspects of the program.

topics.[7] The cash transfers come bimonthly in two forms. The first, received by all beneficiary households, is a fixed cash stipend of 90 pesos per month (in 1997 prices) 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 percent of the time and on not repeating a grade more than twice. The educational stipend is provided for each child less than 18 years old who is enrolled in school between the third grade of primary and the third grade (last) of junior high. The educational scholarship varies by grade and gender. It rises substantially after graduation from primary school and is higher for girls than boys during junior high school. The rates vary from 60 pesos per month for children enrolled in third grade to 225 pesos per month for females enrolled in the third year of junior high school. Hence, households with more female children enrolled in higher grades are eligible for larger transfers compared to similar households with male children enrolled, or children enrolled in lower grades. Beneficiary children also receive money for school supplies once a year during junior high school, and twice a year during primary school.

Only children who were living in the household at the time of incorporation are eligible for the school transfers in order to prevent individual migration into the household. While children fostered into the household at a later date are not eligible for the education transfers, children born into the household will be eligible for future educational transfers once they reach nine years old and enter the third grade of primary school.

Finally, total transfers for any given household are capped at a predetermined upper limit of 550 pesos per month. The cap implies that a household cannot receive an unlimited amount by increasing the number of children enrolled in school. It also implicitly implies that enrolling more than three older children (per household) in school will not increase the amount of transfers received by the household. There appears to have been no effect of the program on fertility rates or family structure (Stecklov et al. 2007).

II. Experimental Design and Data

The identification in this paper relies on experimental variation in program treatment, generated through the Oportunidades randomized evaluation. The evaluation sample includes all households in 506 rural communities in 7 states. Communities were randomly assigned to treatment (320 communities) and control (186 communities) groups, which were phased into the program at different points in time as part of the program's national scale-up.[8] Eligible households in treatment communities

[7] If a female head of household was not present or able to receive payments, an alternate household member is designated for receipt of payment. Compliance with conditions was verified through the clinics and schools, who certified whether households actually completed the required health care visits, and whether kids attended schools. About 1 percent of households were denied the cash transfer for noncompliance.

[8] Given budgetary and logistical constraints, the government could not enroll all eligible families in the country simultaneously and had to phase in enrollment over time. To make implementation feasible, the government decided that whole communities would be enrolled at a time, and that they would be enrolled as fast as possible so that no eligible household would be kept out of the program.

began receiving benefits starting in March/April of 1998, while eligible households in control communities were incorporated in November/December of 1999. In order to minimize anticipation effects, households in control communities were not informed that Oportunidades would provide benefits to them until two months before incorporation. Behrman and Todd (1999) confirm that the original randomization balanced the control and treatment communities; and Attanasio, Meghir, and Santiago (forthcoming) explicitly test, but find no evidence of, anticipation effects amongst control households.

The data used in this paper are primarily from the Oportunidades rural evaluation surveys, the *Encuesta de Evaluación de los Hogares Rurales* (ENCEL).[9] The ENCEL interviewed all households—eligible and ineligible—living in the 506 evaluation communities every 6 months between March 1998 and November 2000, and again in November 2003.[10] In addition, we use data from the 1997 ENCASEH census for baseline (pre-intervention) information and administrative records on transfer of payments to households in the evaluation sample. The ENCASEH contains the basic socioeconomic household information that was used to classify households in selected communities as eligible or ineligible for treatment.

For the core of the analysis, we work with the sample of eligible households originally classified as poor.[11] We also use ineligible (nonpoor) households for falsification robustness checks. The treatment group includes 7,658 eligible households in treatment communities that were offered to begin the program in March/April of 1998, and the control group includes 4,644 eligible households in control communities that were offered to begin the program in November/December of 1999.[12]

Sample attrition for the 6-year panel is relatively low, at 10.32 percent for treatment households and 9.86 percent for control households. Because attrition rates are not significantly different for the two groups, we believe that neither attrition nor selective migration is a major concern for our analysis. First, a 10 percent attrition rate over a 6-year period implies an average yearly rate between rounds as low as 1.6 percent. Second, if the program was expected to reduce migration by providing monetary incentives to remain in the community, then we would expect a higher attrition rate in treatment areas than in control areas, whereas in reality the opposite occurred. Finally, statistical tests on the subsample of nonattriter households show that treatment and controls are well balanced in terms of their baseline observables (results available upon request).

[9] These data are publicly available at www.oportunidades.gob.mx

[10] ENCEL follow-up surveys were collected in March 1998, October 1998, May 1999, November 1999, May 2000, and November 2000. We do not use the March 1998 ENCEL survey as it excludes many of the variables of interest.

[11] We do not use the "densified" households—i.e., the set of wealthier and older households that were deemed eligible later—because their process of incorporation in the program is less well documented. Moreover, many of these households suffered substantial administrative delays in the receipt of benefits (Hoddinott and Skoufias 2004). Because households were categorized as "densified" in both treatment and control communities, and under the same criteria, excluding them does not compromise the internal consistency of our estimates.

[12] We drop 53 households that report a head or a head's spouse younger than 13 or older than 90. The proportion of households we drop for this reason is balanced between treatment and control groups.

III. Investment

The central question explored in this paper is whether poor households invest part of their cash transfers in productive enterprises that lead to improvements in long-term living standards. In this section, we address the first part of this question and estimate the intent-to-treat impact of Oportunidades on investments in productive agricultural assets, such as animals and land, and nonagricultural microenterprises. To this end, we first use the three rounds of data spanning the purely experimental period, i.e., before the randomized control group was incorporated into the program: October 1998, May 1999, and November 1999. In the final subsection, we include data from November 2003, once both treatment and controls are receiving benefits, to test for sustained program impacts over time.

A. *Dependent Variables*

The dependent variables for investment outcomes are constructed from information on animal ownership at the time of the survey, the amount of land in use over the 12 months preceding the interview, and nonagricultural microenterprise activity over the month before the interview. We define production animals to be those whose meat and/or byproducts (milk, cheese, eggs, etc.) are sold and consumed. These include goats and sheep, cows, poultry (chickens, hens, and turkeys), pigs, and rabbits. We define draft animals to be those traditionally used for plowing the fields and for transportation purposes. These include donkeys, mules, horses, and oxen.[13] We aggregate all animals into the total value of production animals and the total value of draft animals using price data on animal sales available in the October 1998 and May 1999 rounds (deflated to 1997 prices).[14] Land is measured in hectares and includes all plots used by the household for agriculture, grazing, and forestry purposes.

We also look at the impact of the program on operating nonagricultural microenterprises. The ENCELs ask about household involvement in self-owned nonagricultural activities that generated income during the month preceding the interview. We define the indicator "microenterprise" as participation in any of the following activities: sewing clothes; making food for sale; carpentry and construction; sale of nonfood items, such as handcrafts, repair of artifacts, or machinery; or other activities done in the context of self-employment.

[13] Table A1 in the online Appendix provides additional details on the construction of the dependent variables and the survey rounds data is available for.

[14] We take household reports on animal sales prices and construct the median community sale price per animal type. As we discuss later, Table A2 in the online Appendix shows no differences in average animal prices in treatment and control communities during the experimental period. We also checked household reports on prices against data we collected in the summer of 2004. Animal prices and other information collected during our fieldwork are available upon request.

B. *Baseline Balance*

We first validate the Oportunidades experimental design by comparing the mean pre-intervention characteristics of households in the treatment and control groups (Table 1).

We find no statistically significant differences in productive assets (panel A), with the exception of the number of hectares of land used. Control households were using about one-third of a hectare more land than treatment households at baseline. We also find no significant differences in baseline characteristics for 28 of the 29 socio-demographic variables tested (see panels B–F in Table 1).[15] As expected from a randomized experiment, these tests confirm that there are no systematic pre-intervention differences between the treatment and control groups that might otherwise affect the estimates of program impact.[16]

C. *Methods*

We estimate the program treatment effect on investments as follows:

$$(1) \quad A_{ijt} = \alpha_0 + \alpha_1 T_{ij} + \sum_t \alpha_{2t} Wave_t + \sum_k \beta_k X_{ijk,97} + \sum_l \gamma_l A_{ijl,97} + \varepsilon_{ijt},$$

where A_{ijt} denotes productive activity of household i in community j in period t; T_{ij} is a binary indicator equal to one for households in treatment communities; $WAVE_t$ is an indicator for each period $t = \{$October 1998, May 1999, and November 1999$\}$; $X_{ijk,97}$ are k household and community characteristics measured at baseline, included in the regressions to reduce residual variance.[17] Through $A_{ijl,97}$ we also explicitly control for the level of baseline agricultural assets, namely the number of draft and production animals and the number of hectares of land used in 1997. Hence, the model can be interpreted as the increment to asset A since baseline rather than the level in period t. We estimate these models using OLS and cluster the standard errors at the community level to control for correlation within the community (the primary sampling unit) and over time.

We estimate the effect of Oportunidades on both the probability of owning an asset and the value (or amount) of the asset owned for draft animals, production animals and land; and on the probability of operating a nonagricultural microenterprise. For the probability of having an asset type, we first report results for the whole sample (unconditional sample) and then for the restricted group of households that had

[15] All nominal variables used in the analysis are expressed in October 1997 (baseline) prices. We have adjusted values for inflation every semester using the Consumer Price Index published by the Bank of Mexico: http://www.banxico.org.mx/politica-monetaria-e-inflacion/estadisticas/inflacion/indices-precios.html

[16] Note that no microenterprise data was collected at baseline.

[17] Baseline controls include: household demographics (household size and composition); head and spouse characteristics (age, education and ethnicity); household assets (binary indicator variables for home ownership, dirt floor, electricity, ownership of any agricultural assets—i.e., agricultural household—and ownership of more than three ha of productive land); and community characteristics (male agricultural wage in the community, distance to large urban center and presence of community work associations). Results are robust to the exclusion of these baseline controls (results available upon request).

Table 1—Test of Equality of Means at Baseline (*October 1997*)

	Treatment group			Control group			
	N	Mean	SD	*N*	Mean	SD	*t*-stat
Panel A. Agricultural assets							
Draft animals ownership = 1	7,658	34.49	0.475	4,644	31.76	0.466	1.201
Production animals ownership = 1	7,656	80.72	0.395	4,643	81.46	0.389	−0.422
Land use = 1	7,658	59.11	0.492	4,644	56.03	0.496	1.072
Monetary value draft animals (in pesos)[a]	2,633	1748.51	2681.141	1,472	1641.44	2270.887	0.768
Monetary value production animals (in pesos)[a]	6,172	2264.08	4438.938	3,773	2225.85	4480.324	0.190
Number hectares used[a]	4,432	2.69	2.780	2,543	3.04	2.955	−2.010**
Panel B. Head of household characteristics							
Age	7,655	41.95	14.133	4,643	42.33	14.521	−0.943
Female = 1	7,658	8.16	0.274	4,644	8.40	0.277	−0.377
Indigenous = 1	7,646	41.90	0.493	4,636	43.70	0.496	−0.324
Never attended school = 1	7,634	31.44	0.464	4,637	32.65	0.469	−0.541
Primary school not completed = 1	7,634	46.28	0.499	4,637	46.75	0.499	−0.275
Primary school completed = 1	7,634	16.94	0.375	4,637	15.59	0.363	0.988
More than primary school = 1	7,634	5.34	0.225	4,637	5.00	0.218	0.516
Panel C. Spouse of head characteristics							
Age	6,713	36.27	12.225	4,074	36.17	12.223	0.296
Never attended school = 1	6,700	36.18	0.481	4,067	37.35	0.484	−0.411
Primary school not completed = 1	6,700	41.64	0.493	4,067	40.69	0.491	0.477
Primary school completed = 1	6,700	18.37	0.387	4,067	16.97	0.375	0.986
More than primary school = 1	6,700	3.81	0.191	4,067	4.99	0.218	−1.954*
Panel D. Main entrepreneur characteristics							
Age	823	40.24	13.981	338	40.25	14.299	0.064
Education (in years)	818	2.78	2.771	335	2.61	2.582	0.676
Female = 1	823	53.22	0.499	338	54.44	0.499	−0.178
Main entrepreneur is the (likely) beneficiary mother = 1	818	34.84	0.477	335	35.82	0.480	−0.208
Panel E. Household characteristics							
Children 0 to 7 = 1	7,645	75.36	0.431	4,639	77.04	0.421	−1.396
Children 8 to 17 = 1	7,645	69.01	0.462	4,639	69.54	0.460	−0.472
Adults 18 to 54 = 1	7,645	94.69	0.224	4,639	95.24	0.213	−1.079
Adults older than 55 = 1	7,645	27.00	0.444	4,639	27.20	0.445	−0.180
Household size	7,658	5.91	2.433	4,644	5.94	2.413	−0.457
Home ownership = 1	7,655	93.39	0.248	4,643	92.10	0.270	1.488
Dirt floor = 1	7,641	73.17	0.443	4,630	75.72	0.429	−1.015
Electricity = 1	7,653	57.65	0.494	4,642	60.58	0.489	−0.684
No agricultural assets = 1	7,635	10.64	0.308	4,630	11.88	0.324	−1.003
Households with more than 3 ha of (productive) Land = 1	7,635	19.45	0.396	4,630	21.94	0.414	−1.337
Panel F. Community characteristics							
Village associations (community work) = 1	7,658	87.84	0.327	4,644	87.75	0.328	0.021
Minimum distance to large urban center (km)	7,658	107.73	41.361	4,644	105.11	43.721	0.522
Monthly community agricultural male wage (pesos)	4,426	573.86	172.005	2,781	585.23	172.115	−0.463

Notes: *t*-stat of differences in means computed clustering standard errors at the community level. Mean of dichotomous variables expressed in percentages.

[a] Conditional on being positive.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

none of the asset at baseline (conditional sample). This provides insight on whether the program induced households to start a farm business (variation at the extensive margin). Analogously, for the value or amount of the asset type, we first report the estimate for all households (unconditional sample), and then restrict the sample to households with positive levels of the asset at baseline (conditional sample).

This tells us about how the program influenced households to expand their already existing farm assets (variation at the intensive margin).

D. *Results*

Panel A in Table 2 presents the effects of Oportunidades on agricultural and microenterprise activities. Each coefficient (cell) in the first row of panels A1 and A2 is estimated from a separate regression for equation (1) using data collected before the incorporation of the control group (October 1998, May 1999, and November 1999 waves). The last two rows in these panels report the mean of the dependent variable for the control group over the three waves of data, and *p*-values for the hypothesis test that results are equal over time—i.e., equal across waves.[18] These tests show that we accept the null of equal effects over time in all but one case.[19] In addition, wave dummies in all regressions control for any time trends or potential cyclicality in the accumulation of livestock or land.

We observe large and significant effects of Oportunidades on agriculture for the full unconditional sample (panel A1). Specifically, treatment households are 17.1 percent more likely to own draft animals and 5.1 percent more likely to own production animals compared to control households. Investments in farm animals are also reflected through the value of its stock. On average, treatment households increase the value of draft animals owned by 21.4 percent and the value of production animals owned by 16.6 percent.

The first three columns of panel A2 estimate the impact of Oportunidades on the probability of asset ownership, conditional on not having the asset at baseline. These models can be interpreted as the effect of the program on *initiating* a given productive activity. Amongst households with no baseline agricultural assets, we find a 24.3 percent increase in the probability of acquiring draft animals, a 12.4 percent increase in the probability of acquiring production animals, and a 15.3 percent increase in the use of land for agricultural purposes given treatment.

Columns 4–6 of panel A2 report the effects of the program on *expanding* existing activities conditional on owning the asset at baseline. Amongst the conditional subsamples, we find that treatment households expand the total value of draft animals by 14.1 percent and the total value of production animals by 16.3 percent, relative to control households. However, there is no significant impact on the number of hectares of land used.

Finally, in Table 3, we observe a positive impact on participation in nonagricultural microenterprises of 3.3 percentage points, which translates into a 67.3 percent increase in the number of households operating such businesses. Effects are driven primarily by increased production of handcrafts for sale.[20]

[18] These are obtained after running a variation of equation (1) that includes a set of treatment × wave dummies.

[19] We cannot accept the null of equal effects at the 10 percent level in the conditional model for the value of draft animals.

[20] The survey asked who in the household was responsible for the business. Males were almost exclusively responsible for carpentry and construction (92 percent) and machinery repair (77 percent), and women were almost exclusively responsible for the other types of activities, such as sewing clothes, making food for sale, sale of nonfood items, and domestic service. We drop domestic service from the list of activities since this is primarily related to labor market participation. The fraction of households undertaking domestic services is only 1 percent.

TABLE 2— OPORTUNIDADES IMPACT ON AGRICULTURAL ASSETS

	Draft animal ownership = 1 (OLS) (1)	Production animal ownership = 1 (OLS) (2)	Land use = 1 (OLS) (3)	Value of draft animals (in pesos) (OLS) (4)	Value of production animals (in pesos) (OLS) (5)	Number of hectares used (OLS) (6)
Panel A. Sample: October 1998 to November 1999						
A1. Unconditional sample ($N = 33{,}103$)						
Treatment = 1	0.042***	0.036**	0.031	65.124**	186.830***	0.062
	(0.014)	(0.015)	(0.019)	(25.291)	(66.995)	(0.059)
Mean dependent variable (control households)	0.246	0.705	0.585	304.066	1,125.756	1.235
Probability same effect for all waves (p-value)[a]	0.929	0.375	0.632	0.125	0.103	0.749
A2. Conditional sample[b]						
Treatment = 1	0.026**	0.056**	0.050**	104.947*	208.420***	0.031
	(0.011)	(0.026)	(0.023)	(56.695)	(73.479)	(0.087)
Observations	21,796	5,948	13,612	11,279	27,107	19,171
Mean dependent variable (control households)	0.107	0.453	0.327	741.923	1,279.164	1.802
Probability same effect for all waves (p-value)[a]	0.818	0.454	0.937	0.060	0.112	0.920
Panel B. Sample: November 2003						
B1. Unconditional sample ($n = 10{,}244$)						
Treatment = 1	0.031	0.020	0.027	138.700**	72.491	−0.039
	(0.020)	(0.018)	(0.017)	(64.847)	(86.687)	(0.167)
Mean dependent variable (control households)	0.292	0.738	0.670	629.055	1,418.382	2.477
B2. Conditional sample[b]						
Treatment = 1	0.039*	0.047	0.038	185.141*	75.025	−0.282
	(0.019)	(0.035)	(0.027)	(111.156)	(97.832)	(0.210)
Observations	6,725	1,772	4,108	3,511	8,460	5,939
Mean dependent variable (control households)	0.152	0.545	0.446	1,306.504	1,574.968	3.189
Panel C. Sample: Oct 1998 to Nov 1999 and Nov 2003						
C1. Unconditional sample ($n = 43{,}344$)						
Treatment = 1	0.042***	0.035**	0.032	65.954***	179.405***	0.076
	(0.014)	(0.015)	(0.019)	(25.477)	(67.067)	(0.060)
Treatment × November 2003 = 1	−0.014	−0.017	−0.005	62.405	−110.088	−0.170
	(0.015)	(0.018)	(0.017)	(61.748)	(81.144)	(0.166)
Mean dependent variable (control households)	0.257	0.713	0.605	381.200	1,195.237	1.528
C2. Conditional sample[b]						
Treatment = 1	0.027**	0.056**	0.051**	110.201*	201.611***	0.051
	(0.011)	(0.026)	(0.023)	(57.486)	(73.557)	(0.087)
Treatment × November 2003 = 1	0.009	−0.008	−0.012	44.162	−135.772	−0.395*
	(0.014)	(0.031)	(0.024)	(102.855)	(91.922)	(0.208)
Observations	28,520	7,720	17,720	14,788	35,564	25,107
Mean dependent variable (control households)	0.118	0.474	0.355	878.403	1,349.875	2.133

Notes: Standard errors clustered at the community level in parentheses. Outliers trimmed at the top 1 percent of the value of animals and land ha distributions by wave. Subsample of households with complete information on all dependent variables. Values of draft and production animals are expressed in October 1997 pesos. All regressions include the following controls: wave dummies, head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draft and production animals, ha of land, farm size, home ownership, dirt floor and electricity), and baseline community characteristics (community organizations, distance to urban center and wages).

[a] p-value of the test of equal treatment effects by wave, obtained from regressing each outcome variable on a set of treatment × wave dummies (and all regressors above).

[b] Models A2, B2 and C2 are conditional on not having any assets at baseline ($y_{97} = 0$) for dummy variables (probability of having an asset type); and conditional on having a positive level of the asset at baseline ($y_{97} > 0$) for continuous variables (amount of asset type).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 3—Oportunidades Impact on Microenterprise Activity

	Micro enterprise = 1 (OLS)	Handcrafts = 1 (OLS)	Carpentry and construction = 1 (OLS)	Sewing clothes = 1 (OLS)	Making food for sale = 1 (OLS)	Repair artifacts or machinery = 1 (OLS)	Other activities = 1 (OLS)
Sample: October 1998 to November 1999 (N = 33,103)							
Treatment = 1	0.033**	0.029**	−0.002	0.000	0.001	−0.000	0.007*
	(0.015)	(0.012)	(0.004)	(0.001)	(0.001)	(0.000)	(0.004)
Mean dependent variable (Control households)	0.049	0.016	0.007	0.006	0.003	0.000	0.017
Probability same effect for all waves (p-value)[a]	0.800	0.494	0.833	0.761	0.244	0.845	0.551

Notes: Standard errors clustered at the community level in parentheses. Same subsample of households as in Table 2. All regressions include the following controls: wave dummies, head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draft and production animals, ha of land, farm size, home ownership, dirt floor and electricity), and baseline community characteristics (community organizations, distance to urban center and wages).

[a] p-value of the test of equal treatment effects by wave, obtained from regressing each outcome variable on a set of treatment × wave dummies (and all regressors above).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

E. *Long-Term Effects*

We next test for sustained program impacts from the initial investments made by treatment households during the experimental period and discussed above. To this end, we estimate equation (1) for the cross-section of data from 2003, almost four years after control households were incorporated in the program. If differences in initial investments made by treatment households almost four years earlier (during the experimental period) are sustained over time, then we would expect the coefficients on treatment to be similar to those reported in panel A of Table 2. On the other hand, if the investment effect is temporary and control households "catch up" in their level of investment, then the difference in investment levels between treatment and control households should become smaller over time, and eventually offset the previous investment effect. In this case, we would expect no differences between the original treatment and control groups four years after controls were incorporated.

Results are reported in Table 2, panel B. We observe significant differences in draft animal ownership and the value of draft animals even four years after the incorporation of controls. For a majority of the other outcomes, the estimated coefficients are large, though we lose precision and do not have statistical significance. In fact, the magnitudes of most long-term effects are very similar to the short-run effects estimated in panel A. For some dependent variables, they are even larger. For example, the probability of draft animal ownership on the conditional sample is 25.6 percent higher for original treatment households compared to original controls in 2003. Draft animals may be particularly important for agricultural productivity.

As a robustness check, and to increase precision, we test whether the estimated long-term effects in panel B are significantly lower than the short-term estimates in panel A. We implement this test by adjusting the specification in equation (1) to

include all rounds of data and an interaction of the treatment indicator with the 2003 year indicator as follows:

$$(2) \qquad A_{ijt} = \alpha_0 + \alpha_1 T_{ij} + \alpha_2 T_{ij} \times Wave_{2003} + \sum_t \alpha_{2t} Wave_t + \sum_k \beta_k X_{ijk,97} + \sum_l \gamma_l A_{ijl,97} + \varepsilon_{ijt}$$

over $t = \{$October 1998, May 1999, November 1999, and November 2003$\}$, and where $Wave_{2003}$ is a dummy variable that equals 1 if the year is 2003. The coefficient on the interaction $T_{ij} \times Wave_{2003}$, α_2, is a test of whether the treatment effects are different in 2003 from the earlier experimental rounds.

Results are reported in panel C of Table 2.[21] We observe that the interaction terms are mostly small and negative, but insignificant except for one. These findings further suggest that the initial period of investment from 1998 to 1999 produced increases in productive assets for the original treatment group that are sustained over time.

IV. Agricultural Productivity

In Section III, we established the causal relationship between participation in Oportunidades and increased investments in agriculture. In this section, we explore the impact on agricultural production and income.[22] Specifically, we examine program impacts on agricultural income from crop and animal sales, planting of corn and beans (the two most common crops), whether the household sells animal products, and loans for production. Productive loans are defined as loans used to engage in agricultural work, buy animals, buy tools and equipment, and to set up a business.

We have information on all of these outcomes for two waves of the ENCEL data: October 1998 and November 1999. In addition, the October 1998 data round contains information on income from selling animal derivative products. As a result, we consider two measures of farm income: one with and one without income from animal products. Both income variables include the value of home produced consumption and are expressed in October 1997 (baseline) prices, per capita adult equivalent.[23] We use the same empirical specification to estimate program impacts as in equation (1).

The results are reported in Table 4. We estimate a 9.6 or 12.2 percent increase in income, depending on the measure used. We also find positive effects—significantly different from zero at the 10 percent level—on the probability of planting beans (25.3 percent increase relative to controls). Treatment households also have a 14.2 percent increase in the probability of selling animal products relative to

[21] The information on microenterprises available in 2003 is not comparable to that used in previous rounds.

[22] We do not have complete data on income from microenterprise activities.

[23] As will be further discussed in the next section, we use community level prices to impute the value of home production. The number of adult equivalents per household is computed using the equivalence scale of 0.73 for household members 18 years and younger, as estimated by Di Maro (2004) for this population. Results are robust to the use of a more standard adult equivalence scale—namely, counting each child younger than 12 as half an adult. Results are also preserved albeit with somewhat lower precision when no adult equivalence scale is used (see Table A3 in the online Appendix).

TABLE 4— OPORTUNIDADES IMPACT ON AGRICULTURAL INCOME, AGRICULTURAL PRODUCTION, AND CREDIT

	Total agricultural income A[a] (OLS)	Total agricultural income B[a] (OLS)	Produce corn = 1 (OLS)	Produce beans = 1 (OLS)	Animal products sold = 1 (OLS)	Productive loans = 1 (OLS)
Sample: October 1998 and May 1999						
Treatment = 1	2.358**	2.808*	0.023	0.020*	0.017*	0.004**
	(1.150)	(1.635)	(0.021)	(0.012)	(0.009)	(0.002)
Observations	21,414	10,458	22,369	22,369	22,369	22,350
Mean dependent variable (Control households)	24.624	22.980	0.476	0.079	0.120	0.006
Standard deviation dependent variable (Control households)	28.763	28.214	0.499	0.269	0.325	0.080

Notes: Standard errors clustered at the community level in parentheses. Same subsample of households as in Table 2. All regressions include the following controls: wave dummies, head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draft and production animals, ha of land, farm size, home ownership, dirt floor and electricity), and baseline community characteristics (community organizations, distance to urban center and wages).

[a] Total agricultural income A is expressed in monthly per capita adult equivalent (1 child = 0.73 adults) units, and is measured as the sum of total crop sale revenues, the value of animal sales, and total home production. Total agricultural income B additionally includes the value of sales of animal derivatives, which is only available for October 1998. All income values are expressed in October 1997 pesos. Outliers trimmed at the top 5 percent of the distribution of the dependent variable.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

controls. Finally, while the proportion of households taking loans for productive purposes is very low (0.6 percent of control households), participation in the program has a significant effect of 0.4 percentage points (66.7 percent) on the probability of taking this type of loan. We take these findings as indicative that the increased investment in productive assets documented in the previous section resulted in an increase in agricultural income.

V. Long-Term Living Standards

Having shown that Oportunidades beneficiaries invest some of the transfers in productive activities and that income from agricultural activities increases in the short run, we now turn to asking whether these investments improve long-run living standards. Specifically, the returns on investments should yield a higher stream of income and consequently increase consumption in the future, such that improved living standards are maintained even after the transfers stop.

We examine the long-run impact of the program on living standards using data from the 2003 follow-up survey, and test whether the consumption level of households in the original treatment group is higher than in the original control group, four years after control households were incorporated into the program. If increased consumption is financed exclusively from the Oportunidades transfers, then the consumption level of the control group should rise to be equal to the treatment group once the control group begins to receive benefits. If on the other hand, treatment households' investments in income generating activities raised long-term income (that leads to increased consumption), then these households would continue to have

higher incomes even after the incorporation of control households to the program. We also examine a number of alternative pathways that could explain higher treatment group consumption in 2003—including increases in unearned income from public and private transfers, earned income from labor supply, formal and informal credit flows, and improved health—to rule out other plausible pathways.

A. *Methods*

Our measure of living standards is household consumption constructed as the sum of expenditures on 36 food and 24 nonfood items, plus the value of home produced consumption. Included in consumption are monetary transfers to others outside the household. Households are asked about the quantity and expenditures of goods purchased, as well as how much of their own production of that good was consumed. We use community level prices to impute the value of household production.[24] We drop the top and bottom 1 percent of aggregate consumption values from the analysis. We get similar results if we trimmed the top 0.5 and the top and bottom 0.5 percent of values (see Table A4 in the online Appendix).

We also use monthly unearned and wage income per adult equivalent as dependent variables. Unearned income includes income from four different sources: Oportunidades transfers; transfers from PROCAMPO (a farm subsidy); net transfers from private sources including friends, neighbors, and relatives living outside the household; and the equivalent monthly amount from the total money borrowed over the past year, both to formal and informal money lenders. We measure total household wage income as earnings from working in a nonfamily business for all household members. All consumption and income variables are expressed in October 1997 prices and in per capita adult equivalent units, computed using an equivalence scale of 0.73 for nonadult household members, as defined earlier.

We estimate the program treatment impact on consumption, unearned income, and wage income using the following specification:

$$Y_{ij,2003} = \alpha_0 + \alpha_1 T_{ij,1999} + \sum_k \beta_k X_{ijk,1997} + \varepsilon_{ijt}, \tag{3}$$

where $Y_{ij,2003}$ denotes outcome of household i in community j in 2003; $T_{ij,1999}$ is a binary indicator equal to one if household i lived in a treatment community in 1998/1999 and was eligible for Oportunidades. The $X_{ij,97}$ are the same baseline household and community characteristics as in model (1) of Section III, and are included in the regressions to reduce residual variance.[25] As before, the model is estimated using OLS and clusters the standard errors at the community level to control for correlation within the community and over time.

[24] We constructed an alternative consumption measure by multiplying the quantity for each item times median community prices. This measure yielded comparable results to the one used in the paper.

[25] Results are robust to the exclusion of these baseline controls (available upon request).

Table 5—Long-Term Consumption and Income Sources

	Consumption (OLS) (1)	Home production (OLS) (2)	Oportunidades transfers (OLS) (3)	PROCAMPO transfers (OLS) (4)	Total private transfers (OLS) (5)	Amount borrowed (OLS) (6)	Outside wages (OLS) (7)
Sample: November 2003							
Original treatment household = 1	10.836** (4.832)	2.038* (1.091)	−1.724 (1.328)	0.153 (0.400)	−0.006 (0.340)	0.066 (0.138)	4.068 (6.215)
Observations	10,166	9,984	9,977	9,441	9,673	9,632	8,754
Mean dependent variable (control households)	193.715	12.398	49.448	4.575	3.213	1.070	154.411
Standard deviation dependent variable (control households)	118.345	22.531	30.794	11.781	20.344	9.637	134.508
Proportion dependent variable > 0	—	0.510	0.906	0.281	0.154	0.090	0.660

Notes: Standard errors clustered at the community level in parentheses. Consumption is expenditures and includes home production. PROCAMPO is a public farm subsidy. Total private transfers include the total household amount received from neighbors, relatives, and friends. Amount borrowed includes money borrowed over past 12 months from all formal and informal loans, regardless of whether loan has been repaid. Outside wages include wages from working in a nonfamily business for all household members. All income variables are monthly, and expressed in October 1997 pesos and in per capita adult equivalent (1 child = 0.73 adults) units. Households in the top and bottom 1 percent of the consumption distribution have been excluded. All regressions include the following controls: wave dummies, head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size adult equivalent, dummies controlling for household demographics at baseline, baseline assets (number of draft and production animals, ha of land, farm size, home ownership, dirt floor and electricity), and baseline community characteristics (community organizations, distance to urban center and wages). Outliers of income variables trimmed at the top 2 percent (top 5 percent for outside wages).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

B. *Consumption Results*

We find that household per capita consumption in 2003 is 10.84 pesos higher for original treatment households, and this difference is statistically significant (first column in Table 5). This impact amounts to a 5.6 percent increase in consumption for treatment households, even 4 years after controls started receiving program benefits. While we do not have agricultural production for the 2003 survey round, we do have home-produced consumption. We also find a significant increase in home-produced consumption (significantly different from 0 at the 10 percent level), which is consistent with a sustained increase in agricultural productivity (second column in Table 5).

C. *Alternative Income Pathways*

In columns 3–7 of Table 5, we explore alternative income pathways, above and beyond investments, that could explain the difference in 2003 consumption levels. We find no differences in 2003 Oportunidades transfers, income from the PROCAMPO farm subsidy, net private transfers, income borrowed, and wage income between original treatment and control households.

Related literature also suggests that these other pathways are unlikely, as the program at best reduced income from these sources. In fact, while there is no evidence that the CCT program affected labor supply of adults during the experimental period,

Table 6—Oportunidades Impact on Credit by Type of Loan

	Loans = 1 (OLS)	Consumption loans = 1 (OLS)	Productive loans = 1 (OLS)	Transfers from friends = 1 (OLS)	Transfers to friends = 1 (OLS)
Sample: October 1998 to November 1999					
Treatment = 1	0.000	−0.001	0.004**	−0.005	−0.001
	(0.005)	(0.003)	(0.002)	(0.006)	(0.001)
Waves: October 1998 and May 1999	Y	Y	Y	—	—
Waves: October 1998 and November 1999	—	—	—	Y	Y
Observations	22,357	22,350	22,350	22,245	22,243
Mean dependent variable (control households)	0.039	0.017	0.006	0.073	0.004
Standard deviation dependent variable (control households)	0.193	0.130	0.080	0.260	0.066

Notes: Standard errors clustered at the community level in parentheses. Same subsample of households as in Table 2. All regressions include the following controls: wave dummies, head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draft and production animals, ha of land, farm size, home ownership, dirt floor and electricity), and baseline community characteristics (community organizations, distance to urban center and wages).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Parker and Skoufias (2000) and Skoufias an Parker (2001) show that it reduced the labor supply of school-age children in salaried and nonsalaried activities, so that they could attend school. Moreover, there is evidence that Oportunidades transfers crowded out some private transfers from relatives and friends during the first year of program exposure (Albarran and Attanasio 2005).

D. *Formal and Informal Credit Pathways*

In Table 6, we investigate whether Oportunidades has had any impact on formal and informal credit arrangements amongst treatment households, including soft loans to and from friends and relatives, using data from the experimental period. We do not find any impact of the program on private transfers and formal loans *received* by the household, nor on transfers *sent out*, suggesting that there was no money to be repaid at later dates that could increase long-term consumption of treatment households above the level of control households.

E. *Health Pathways*

We also explore a further pathway that may explain the increased levels in household consumption and agricultural income observed amongst treatment households, namely increased productivity resulting from improved long-run health. While Gertler and Boyce (2001) show short-term positive health effects of Oportunidades on adults; in later work, Fernald, Gertler, and Hou (2008) show negative health effects of the program in terms of increased weight and hypertension. Thus, existing evidence on health is inconclusive and could affect effective labor supply positively or negatively.

In Table 7, we test for long-run health differences using 2003 data and the same econometric specification outlined in equation (3), but estimated at the individual

Table 7—Oportunidades Impact on Long-Term Health of Prime Age Adults (*18 to 64 Years Old*)

	Sick = 1 (OLS)	Number days sick (OLS)	Inactivity = 1 (OLS)	Number days inactivity (OLS)	Number km walk without getting tired (OLS)	Ability to perform moderate ADLs (OLS)	Ability to perform vigorous ADLs (OLS)
Sample: November 2003							
Original treatment household = 1	0.007	0.113	0.001	−0.074	−0.013	−0.005	−0.007
	(0.009)	(0.080)	(0.005)	(0.050)	(0.150)	(0.008)	(0.010)
Observations	24,679	24,679	24,679	24,679	24,253	24,689	24,685
Mean dependent variable (control households)	0.131	7.227	0.057	8.138	5.408	1.131	1.194

Notes: Standard errors clustered at the community level in parentheses. Same subsample of households as in Table 5. All regressions include the following controls: wave dummies, head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size ae, dummies controlling for household demographics at baseline, baseline assets (number of draft and production animals, ha of land, farm size, home ownership, dirt floor and electricity), and baseline community characteristics (community organizations, distance to urban center and wages). Information for all dependent variables refers to last month.

level and controlling for relevant individual characteristics, such as age, age squared and gender. Dependent variables include the self-reported health status of all prime age adults (ages 18–64) in the household, including whether the individual was sick over the last month, whether she was inactive, and various measures of Activity of Daily Living (ADLs). We find no differences in the long-term health of individuals in the original treatment and control groups, and thus exclude health as a likely pathway toward increased investment and consumption.

Based on this, we conclude that the most plausible remaining source of income to finance the additional consumption is income generated through the Oportunidades financed investment in family farm and microenterprises that households in the original treatment group made during the 18-month experimental period from 1998–1999.

F. *Marginal Propensity to Consume Transfers*

To interpret the long-run differences in consumption as the result of increased investment early on in the program, we must assume that the long-term marginal propensity to consume (MPC) out of transfers was the same for treatment and control households by 2003. If not, treatment households might have higher consumption simply by consuming more of the 2003 current transfers compared to controls. Despite the random assignment and baseline balance, we might observe different MPCs out of transfers if the MPC changes with duration on the program. However, one would expect that after some initial period of adjustment, the MPC would stabilize. Hence, it is likely that in 2003, four years after the control group was enrolled, the MPC of the control group would have converged to the MPC of the treatment group.

While we cannot test this directly, we can observe how the MPC changed over time for treatment households during the experimental period and test whether it stabilized during this period. To do so, we use data from the first three rounds collected during the experimental period (October 1998, May 1999, and November 1999). We regress monthly household consumption expenditures against a treatment dummy

Table 8—Oportunidades Impact on Consumption and Marginal Propensity to Consume (MPC) by Wave during Experimental Period (*October 1998–November 1999*)

	Average treatment effect on consumption per capita adult equivalent	Actual transfer per capita adult equivalent (treatment households)	Estimated monthly MPC
Sample: October 1998 to November 1999			
October 1998	17.613***	24.196	0.728***
	(2.899)	(16.128)	(0.250)
May 1999	16.033***	38.691	0.414***
	(2.662)	(25.027)	(0.077)
November 1999	14.596***	31.141	0.469***
	(2.874)	(21.207)	(0.110)

Notes: Estimates in column 1 come from regressing treament on per capita adult equivalent (1 child = 0.73 adults) units for the three experimental rounds ($n = 32{,}124$). The measure of consumption used is total expenditures and does not include home production. As such, it is defined for three data rounds: October 1998, May 1999, and November 1999. In all regressions standard errors are clustered at the community level and include the following controls: wave, municipality and wave × municipality dummies, head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draft and production animals, ha of land, farm size, home ownership, dirt floor and electricity), and baseline community characteristics (community organizations and distance to urban center). All values are expressed in October 1997 pesos. Outliers trimmed at the top and bottom 1 percent of the consumption distribution.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

interacted with time dummies, time dummies, and the same baseline covariates used in the 2003 consumption regression above.

We report the coefficients of treatment by wave dummies in the first column of Table 8. These coefficients are the average difference in consumption expenditures between the treatment and control groups for each of the survey rounds. In the next column, we report the average monthly transfer per adult equivalent in each survey round and in the last column, the MPC out of transfers calculated as the coefficient in the first column divided by the average transfer reported in the second column. We show that while the MPC goes down between October 1998 and May 1999, it then levels out and remains constant between May and November 1999. Assuming that control households react in a similar fashion, with the MPC stabilizing soon after initiating participation on the program, all households would have a similar MPC by 2003, and we can rule out different MPCs between treatment and control households as a potential explanation for differences in long-run consumption.

VI. Falsification Tests

In this section, we conduct falsification tests that lend additional confidence in the validity of our results in Sections III–V. Since the randomization of treatment balanced the evaluation sample of eligible households at baseline (Table 1), the primary threat to our results is the possible existence of other time varying changes that were different in treatment versus control areas and that could explain the differences observed in asset ownership and consumption levels in the post period. For example, asset prices and economic growth could have changed differentially

in treatment and control communities, or other public programs could have been introduced in the treatment areas and not in the control areas.

A specific concern is that the program generated general equilibrium effects that drive changes in investment behavior. Oportunidades infused small rural communities with large amounts of transfers to over half their residents, potentially increasing prices in treatment communities. In this scenario, the effects of transfers on increased microenterprise and farm activities shown could simply reflect increases in local prices. However, there is no evidence that differential prices are driving our results. Hoddinot and Skoufias (2004) and Angelucci and De Giorgi (2009) find no differences in food prices in treatment and control communities. In addition, we find no differences in community-level wage rates or animal prices during the experimental period (Table A2 in the online Appendix).

We also test for the possibility of differential unobserved time varying factors using the sample of households that were ineligible to participate in Oportunidades based on wealth. If differential time varying changes in unobservable community characteristics explain the differential level of assets and consumption observed in the post-intervention period, then we would also expect to see that ineligible households living in treatment communities have higher asset and consumption levels than ineligible households living in control communities. Hence, we replicate the main analysis in the paper, so far using the sample of ineligibles to help rule out general equilibrium effects.[26] Results reported in the online Appendix, show no effect on agricultural assets and microenterprises (Table A6), no effect on agricultural production or income (Table A7), and no effect on consumption in 2003 (Table A8). However, we do find an effect on productive loans (sixth column in Table A7), which is consistent with results reported in Angelucci and De Giorgi (2009).

VII. Marginal Propensity to Invest and Long-Run Effects

In this section, we estimate how much of the transfer is consumed versus invested—i.e., the marginal propensity to consume (MPC), and the return on transfers in terms of long-term consumption via the investment pathway, which we call the marginal investment effect (MIE). The MPC and the MIE characterize two principal pathways through which transfers affect living standards. First, households can increase living standards in the short run by spending part of the current cash transfer. This amount is just the marginal propensity to consume transfers. Transfers not consumed are saved or invested, so that the marginal propensity to invest transfers is equivalent to (1–MPC). Second, if households indeed invest part of the transfer in productive activities as shown in the previous sections, the return on that investment should yield higher income and consequently increase consumption in future periods. The first pathway represents a temporary increase in living standards that disappears when transfers stop, while the second pathway represents a long-term

[26] The subsample of ineligible households includes 3,212 households in treatment communities, and 2,038 households in control communities. To verify that ineligible households were also balanced at baseline, we compare their baseline characteristics, and find no statistical differences in 32 of the 35 key dependent and independent variables considered (see Table A5 in the online Appendix).

improvement in living standards that may be maintained even after the transfers come to an end.

A. *Empirical Specification*

The basic insight that we use to obtain an empirical specification is that we should be able to extract the MPC and MIE from a model that relates current consumption as a function of current and lagged transfers. We argue that contemporaneous transfers affect current consumption if they are used to purchase goods and services, whereas current transfers not used to purchase goods or services in the present are invested, and any increase in income from this investment is realized at a later point in time. In this setting, lagged or past transfers can only affect current consumption if they were invested in productive activities that generate income today.

Since our objective is limited to decomposing the effect of transfers on consumption into the MPC and MIE effects as opposed to welfare analysis, we do not build the empirical specification up from first principles nor do we try to identify structural parameters. Rather, we build the specification from a set of stylized relationships. We start with an analog to the classic consumption function (e.g., Friedman 1957; Hall 1978; Musgrove 1979; Paxson 1992) that specifies current consumption as a function of current income. We assume that consumption can be expressed as a linear function of income,

$$C_{ijt} = \varphi + \phi Y_{ijt}, \tag{4}$$

where C_{ijt} is consumption per adult equivalent for household i living in community j in period t; Y_{ijt} is income per adult equivalent for household i living in community j in period t; and ϕ is the MPC. Then, $(1 - \phi)$ is the marginal propensity to invest (MPI), or the share of income invested.

Income comes from earned income, Y^e_{ijt}, and from unearned transfers, T_{ijt}. Earned income comes either through wages generated from participating in the labor market or through enterprise income generated by using capital and labor in production. Hence, earned income is a function of capital assets and labor market wages. We formalize this in a linear relationship,

$$Y^e_{ijt} = \alpha_0 + \alpha_a A_{ijt} + \alpha_w W_{jt} + \varepsilon_{ijt}, \tag{5}$$

where A_{ijt} is household i's assets used in the enterprise in period t; W_{jt} is the local labor market j's wage rate in period t; and ε_{ijt} is a random productivity shock at both individual and community level—i.e., illness or a weather shock.

Assets are accumulated over time through investing part of current income. Specifically, today's assets are equal to last period's investment in assets, i.e., the share of income invested in the previous period plus the depreciated value of last period's assets:

$$A_{ijt} = (1 - \phi)(Y^e_{ij,t-1} + T_{ij,t-1}) + (1 - \delta)A_{ij,t-1}, \tag{6}$$

where δ is the one-period depreciation rate of assets. We solve the asset accumulation equation recursively to get

$$A_{ijt} = \gamma_0 + \sum_{s=0}^{t-1} \gamma_{1s} T_{ijs} + \gamma_2 A_{ij0} + \sum_{s=0}^{t-1} \gamma_{4s} W_{js} + \sum_{s=0}^{t-1} \eta_{ijs}, \tag{7}$$

where current assets are a function of lagged transfers, endowments of capital, the history of wage rates (and other input prices), and the history of productive random shocks.

The coefficients on each of these terms are a function of the production function parameters, the MPC, and the depreciation rate. They change over time, becoming more complex functions as the number of lagged periods grows and up to some point where they stabilize. This is because assets depreciate, and hence there is a point in time where the lagged values of transfers, wage rates, and shocks eventually have no influence on current income and investment or assets. Indeed, the number of lagged periods in this equation depends on the depreciation rate of assets—the higher the depreciation rate, the lower the number of lags.

Finally, we substitute the asset equation (7) into the income equation (5), and that into the consumption function in equation (4), to obtain the specification that we estimate:

$$C_{ijt} = \rho_0 + \phi T_{ijt} + \sum_{s=0}^{t-1} \rho_{1s} T_{ijs} + \rho_2 A_{ij0} + \sum_{s=0}^{t} \rho_{3s} W_{js} + \sum_{s=0}^{t} \mu_{ijs}. \tag{8}$$

Equation (8) specifies current consumption as a function of current transfers, the history of lagged transfers, endowments of capital, the history of wage rates (and other input prices), and an error term that is a function of preferences, and the history of productivity and labor market shocks. In this specification, the estimated coefficient on current transfers, $\hat{\phi}$, is the MPC; and the estimated coefficients on lagged transfers, $\hat{\rho}_{1s}$, are the MIE—i.e., the effect of one peso of transfers on consumption via the investment pathway.

B. *Data and Identification*

We estimate equation (8) using data from the four waves of the ENCEL surveys that collected detailed information on both purchased and home produced goods—namely October 1998, May 1999, November 2000, and November 2003. In addition, we link the ENCEL to administrative records of transfer payments that provide information on the amount and timing of money transferred to each household. We use these data to construct current transfers and lagged cumulative transfers, the new variables needed for the estimation of equation (8).

As discussed at length in Section IB and shown in Table 9, program rules stipulate that differently composed households receive different transfer amounts. The actual transfer amount received by a household in a given period depends on how many of the household's eligible children do attend school. The actual bimonthly

Table 9—Oportunidades Monthly Transfer Amounts in October 1997 Pesos

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
School supplies stipend	Primary, first payment		80	80
	Primary, second payment		40	40
	Junior high school		150	150
Nutritional stipend (per family)			90	
Transfer cap (per family)			550	

Notes: Transfer amounts are adjusted for inflation every semester according to the consumer price index published by the Bank of Mexico (http://www.banxico.org.mx/politica-monetaria-e-inflacion/estadisticas/inflacion/indices-precios.html). Transfer cap is the maximum transfer amount awarded for basic education (primary school and junior high) and nutrition.

Source: Oportunidades (www.oportunidades.gob.mx).

transfer for household i in community j at each time t, T_{ijt}, can be calculated by applying the values from Table 9 to the following formula:

$$T_{ijt} = \min\left(T_{\max}, BT_t + \sum_c ST_{ct} K_{cijt}\right), \tag{9}$$

where $T_{\max}$ is the maximum potential transfer possible in time t (recall that there is a cap to the total transfer amount a household can receive at any point in time); BT_t is the basic nutrition transfer amount to a household in time t; ST_{ct} is the transfer amount for a child of type c (i.e., based on grade and sex) who attends school; and K_{cijt} is the number of children of type c in household i who attend school in period t. In the analysis, we use the precise amount of transfers paid to each household in each period, as reported in the administrative data, but we use the above formula to confirm that the government followed the rules.

In addition to the current transfer amounts in a specific period, we calculate the total past transfers received and accumulated by a household up to time $t-1$. Past cumulative transfers are the product of how long the family has been on the program and the transfer amount per period. Formally, for family i living in community j, cumulative transfers through period $t-1$ are

$$\sum_{s=0}^{t-1} T_{ijs} = R_{98}\left(\sum_{s=4/98}^{t-1} T_{ijs}\right) + (1 - R_{98})\left(\sum_{s=11/99}^{t-1} T_{ijs}\right), \tag{10}$$

where $\sum_{s=0}^{t-1} T_{ijs}$ are cumulative transfers received by household i through period $t-1$; R_{98} is equal to one if the household was randomized to start the program in April 1998 (treatment households); $(1 - R_{98})$ equals one if the household was randomized to start the program 18 months later (control households); and T_{ijs} is household i's actual current transfer in period s as calculated by equation (9).

In equation (8), the coefficient on past cumulative transfers changes the longer in the past the transfer was made. Hence, we do not enter total cumulative past transfers as a single variable. Rather, we enter past transfers, $\sum_{s=0}^{t-1} T_{ijs}$, in three lagged variables ($s = 1, 2, 3$): the amount of transfers the household received in the 12 months prior to the current transfers ($s = 1$), the amount of transfers the household received from the thirteenth through the twenty-fourth month prior to the current transfers ($s = 2$), and the amount of transfers the household received more than 24 months prior to the current transfer ($s = 3$).

One potential concern with this specification is that the current and cumulative transfer amounts that the household actually receives are determined in part by whether children attend school. If a household sends their children to work instead of going to school, then the family would have lower transfers but higher income from the child's work. This would imply a downward biased estimate of the MPC and an upward biased estimate of the MPI. In reality, this is a concern for our estimates given that Parker and Skoufias (2000) and Skoufias and Parker (2001) find that the program reduces child labor and increases enrollment in junior high (secondary) schools as the opportunity cost of these children being in the labor force is now higher. Schultz (2004) also finds positive effects for primary school and, more notably, junior high school enrollment for boys and girls.

We solve this problem by instrumenting current transfers and past cumulative transfers with the maximum potential current and maximum potential past cumulative transfers, respectively, that a family could achieve if the maximum number of eligible children in the household were enrolled in school.[27] At each time t, we compute a family's maximum potential transfer using a modified version of the formula in (9) and assuming that all eligible children that were enrolled at baseline have advanced a grade per year. Because of the cap on total benefits and because the transfers are zero for the first three years of school, potential transfers are a nonlinear function of the number of children at baseline who could be enrolled in school in period t.

Maximum potential transfers and the three lagged maximum potential cumulative transfers are likely to be valid instruments for three reasons. First, they are strong predictors of the actual transfers and the three actual cumulative lagged transfer variables. Indeed, and as expected, the distribution of potential transfers follows that of the actual transfers very closely, albeit potential transfers are an overestimate of the actual transfers (given noncompliance, administrative delays in payments, etc.). The simple correlation amongst both variables is 0.89. After controlling for time effects and baseline covariates, 55.7 and 65.9 percent of the variation in current and cumulative lagged transfers are explained by their potential counterparts.

Second, they are unlikely to be correlated with consumption via other pathways, such as other income sources. Indeed, they are uncorrelated with changes in children's labor supply due to the program as they are computed assuming that all eligible children enrolled at *baseline* are still in school and have advanced one school

[27] Note that separate identification of current transfers and the three lagged cumulative transfers is possible because cumulative transfers are the product of a randomized variable and a nonlinear function of the number of eligible children attending school, whereas the current transfer only depends on the latter.

grade per school year. Nonetheless, the transfers could also be taken in leisure by reducing adult labor supply, which would reduce household income and therefore household consumption. Everything else held constant, this would imply a downward biased estimate of the MPC. Parker and Skoufias (2000) show that there is no effect of the program on adult labor supply, and we can thus safely assume that the transfer variables are not correlated with other earned sources of income.

Regarding program impacts on unearned income, the crowding out effect of private transfers found in Albarran and Attanasio (2005) and discussed earlier, would suggest that our estimated MPC is underestimated. However, private transfers are unlikely to explain our results as the crowding out effects are small in size and, on average, approximately 7.3 percent of eligible households report receiving private transfers over the experimental period (fourth column in Table 6). This proportion doubles to 15.4 percent in November 2003 (see the fifth column in Table 5) suggesting that the crowding effects are not sustained over time.

Finally, there is no bias from omitted family demographic structure as we directly control for family structure in the regression models. In fact, maximum potential transfers are not strongly correlated with the number of children in the household because of the nonlinear allocation rule. Let's imagine the following extreme situations: a household with three girls in grade 2 of primary school, and a household with three girls in grade 2 of junior high school. Both households have three female children, but while the first household will receive no school transfers, the latter household will receive a large monthly transfer. In addition, families with four or more children in junior high school would receive the same transfer amount as the latter household because the cap on total benefits would be binding. Indeed, the data shows low correlations between transfers and the number of children under 17 ($r = 0.14$), or with the number of siblings 15–17 years old ($r = 0.42$), 12–14 years old ($r = 0.22$), 6–11 years old ($r = -0.12$), and 0–5 years old ($r = -0.10$). Thus, we are able to explicitly control for household size and the number of children in the household in the empirical specification, which allows for identification of the potential transfer variable.

We measure human and physical capital endowments at baseline (1997) before treatment households start receiving transfers. Specifically, we control for head's and spouse's age, education, and ethnicity, as well as adult equivalent household size and composition. Note that the latter variables also adjust for any economies of scale in consumption. We also control for pre-intervention home ownership, dirt floor, electricity, the number of draft and production animals, and hectares of land in use. Finally, we control for wages and inputs prices using municipality dummies interacted with wave dummies.

C. *Results*

We estimate equation (8) by two-stage least squares (2SLS) clustering the standard errors at the community level to control for the correlation of shocks within communities over time. The first column of Table 10 presents the 2SLS estimates of the MPC and the MIE. For the sake of comparison, we report OLS estimates in the second column, which, as expected, are substantially lower than their 2SLS counterparts.

Table 10—Marginal Propensity to Consume (MPC) and Marginal Investment Effect (MIE) on Consumption

	Consumption per capita adult equivalent (2SLS)	Consumption per capita adult equivalent (OLS)
Actual current transfer (*monthly per capita adult equivalent*)—*MPC*		
Transfer amount (average MPC $= \hat{\phi}$)	0.740***	0.487***
	(0.097)	(0.059)
Past accumulated transfers (*monthly per capita adult equivalent*)—*MIE*		
Transfers accumlated 12 months before current transfer ($\hat{\rho}_{11}$)	0.003	0.007
	(0.011)	(0.006)
Transfers accumlated 13–24 months before current transfer ($\hat{\rho}_{12}$)	0.018***	0.008*
	(0.006)	(0.004)
Transfers accumlated more than 24 months before current transfer ($\hat{\rho}_{13}$)	0.016***	0.015***
	(0.002)	(0.002)
$p(\hat{\phi}) = 1$	0.008	0.000
$p(\hat{\rho}_{12} = \hat{\rho}_{13})$	0.805	0.147
Municipality, wave and municipality × wave dummies	Y	Y
Observations	42,139	42,139
Mean monthly consumption per capita adult equivalent (control households)	164.778	164.778
Mean monthly actual transfer per capita adult equivalent	31.136	31.136

Notes: Standard errors clustered at the community level in parentheses. All regressions include the following set of controls: wave, municipality and wave × municipality dummies, head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draft and production animals, ha of land, farm size, home ownership, dirt floor and electricity), and baseline community characteristics (community organizations and distance to urban center). The excluded instruments in the 2SLS models are the monthly per capita adult equivalent (1 child = 0.73 adults) potential transfers and the three lagged cumulative potential transfers. The first stages show significant effects of the excluded instruments on the endogeneous explanatory variables and are available in Table A10 (online Appendix). Consumption is expenditures expressed in per capita adult equivalent units, and includes home production. All values are expressed in October 1997 pesos. Data is available for the following rounds: October 1998, May 1999, November 2000, and November 2003. Outliers trimmed at the top and bottom 1 percent of the consumption and transfers distributions.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

We estimate the MPC to be 0.74, implying that approximately three-quarters of the transfers are directly consumed and one-quarter are invested. The estimated MPC is significantly different from 1 at conventional significance levels, as the test in the fifth row in Table 6 reports (p-value $= 0.008$). We also tested, and could not reject (p-value of 0.956), the hypothesis that the MPC is the same for all four rounds of the data used (results available upon request).

Note that our estimate of the MPC is consistent with other estimates in developing countries. For example, Musgrove (1979) estimates MPCs of 0.881 for urban Colombia, 0.896 for urban Ecuador, and, 0.776 for urban Peru; and Bhalla (1979) reports an MPC of 0.61 for rural India. Moreover, its value is relatively high, which is suggestive that beneficiaries are perceiving the program as a permanent—as opposed to transitory—source of income: Paxson (1992) reports MPCs out of permanent income from 0.56 to 0.84, and MPCs out of transitory income ranging from 0.17 to 0.27 for a sample of rice farmers in Thailand.

The next three rows in Table 10 report the estimated coefficients on the three lagged cumulative transfer variables and are the estimates of the MIE. The first coefficient, $\hat{\rho}_{11}$, represents the effect on consumption from transfers made in the 12 months prior to the current transfer. This coefficient is small and not significantly different from zero. This is consistent with the notion that investments take time before they become productive. Indeed, investments made within the last 12 months do not seem to generate enough income to affect consumption. The second coefficient, $\hat{\rho}_{12}$, represents the effect on consumption of transfers made in the second 12-month period prior to the current transfers. This coefficient is positive and significantly different from zero. We estimate that for every peso transferred during this period, current consumption increased by 1.8 cents. The third coefficient represents the effect on consumption of transfers made in the period prior to 24 months before the current transfer period. This coefficient is positive and significantly different from zero, and implies that for every peso transferred during this period, current consumption increased by 1.6 cents. While this coefficient is slightly lower than the second lag, the two are not significantly different from one another, as reported in the sixth row of Table 10 (p-value $= 0.805$). This suggests at best a slight drop off in the MIE due to depreciation.

Using these estimates of MPC and MIE, we run a back-of-the-envelope calculation to predict the long-term effects of the cash transfers on living standards, simply multiplying the MIE by total cumulative transfers. By November 2003, after 5½ years on the program, households in the treatment group had received a total of 2,624 pesos per capita, on average.[28] Using our more conservative estimate of the MIE of 0.016, this implies an increase in consumption of 41.9 pesos per capita per month through the investment pathway. This amounts to an approximately 21.8 percent increase in consumption. If these increases in consumption are, as argued here, derived from productive investments made by beneficiary households thanks to the cash transfers, then to the extent that the investments are long term, these increases in living standards are expected to be sustained over time even after the household is no longer receiving cash transfers from Oportunidades.

VIII. Conclusions

Conditional cash transfer programs such as Mexico's Oportunidades are designed to alleviate short-term poverty through cash while incentivizing families to invest in the human capital of the next generation. The positive benefits of this type of program are well established—including increased caloric intake, better health and nutrition, and higher school enrollment for children (see Fiszbein and Schady 2009 for a review). This increased human capital is expected to play an important role in breaking the cycle of poverty for younger generations. 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 appear to increase living standards permanently by facilitating investments in productive activities.

[28] Author's calculations using transfer payments records.

Using data from a controlled randomized experiment, we provide evidence that beneficiaries invested part of their payments in productive activities and that these investments increased income streams. We also find that the stream of income generated from these investments is sustained over time, thereby raising long-term living standards as measured by consumption. Specifically, we find that four years after the control groups were incorporated into the program, consumption levels for the original treatment households were 5.6 percent higher than for the original control households. This result suggests that returns on investments made by treatment households in the initial 18-month experimental period did in fact translate into improvements in long-term living standards. As a result, if removed from the program, beneficiary households would most likely not revert to pre-program poverty levels. Given that households are granted a minimum of nine years of program benefits, the investment of Oportunidades cash transfers is likely to have a large and significant effect on raising long-term living standards.

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