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Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption?

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Cash transfers to eligible households indirectly increase the consumption of ineligible households living in the same villages. This effect operates through insurance and credit markets: ineligible households benefit from the transfers by receiving more gifts and loans and by reducing their savings. Thus, the transfers benefit the local economy at large; looking only at the effect on the treated underestimates the impact. One should analyze the effects of this class of programs on the entire local economy, rather than on the treated only, and use a village-level randomization, rather than selecting treatment and control subjects from the same community.

Policy interventions in developing countries are likely to affect all the residents of the areas where they are implemented. The program evaluation literature is mainly focused, however, on estimating the program effects on the treated, rather than the effects on the nontreated or locality-wide effects. These indirect effects may be large in communities where the lack of formal markets and institutions creates strong interactions among small groups of households. This paper estimates the indirect effects of the flagship Mexican welfare program, Progresa, on the consumption of ineligible households, and studies the mechanisms through which these indirect effects occur.

Started in 1997 and still ongoing, Progresa's aim is improving the education, health, and nutrition levels of poor households through sizeable cash transfers. In our sample of rural villages, more than half of the households are treated. The targeted villages are small and agriculture is the main, and often sole, economic activity. The exposure to natural disasters, the absence of formal credit and insurance institutions, and extensive within-village kinship relationships create incentives to engage in informal risk-sharing activities. If this is the case, treated households will share part of their higher income with members of their social network through gifts or loans. Therefore, the entire village will benefit from the program.

Understanding the program's indirect effects and their causes is important for three reasons. First, this type of program has become very popular and therefore a careful evaluation is needed. Second, the study of indirect effects has implications for the design of policies and of the experiments to evaluate them. Third, and more broadly, this exercise enables us to see how a positive income shock is transmitted through the local economy.

We can estimate these effects under fairly weak identification assumptions because of the unique design of the experimental trial and evaluation data. There is a village-level randomization and we have a census of all households, irrespective of eligibility for the treatment. Thus, we

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have information on four groups: eligible and ineligible households in treatment and control villages. Ineligible households in control villages provide a valid counterfactual for the ineligibles in treatment villages, under the assumptions that assignment is truly random and control villages are not indirectly affected by the program. The identification relies on the fact that only a subgroup of households in the village is eligible for a particular policy (Robert A. Moffitt 2001). A comparison of ineligible households' consumption, loans, and transfers in treatment and control villages enables us to identify the indirect effect of the program on these outcomes.

If villagers share risk, Progresa will cause an increase in consumption, loans, and transfers for ineligible families. Consistent with those predictions, food consumption for the ineligibles in treated villages increases by about 10 percent per month per adult equivalent in May and November 1999. This effect is roughly 50 percent of the average increase in food consumption for eligible adults since November 1998; failure to consider this indirect effect results in a 12 percent underestimate of the treatment impact. Ineligible households in treatment villages consume more by borrowing more money (mainly from family and friends), by receiving more transfers, and, to a small extent, by reducing their stock of grains and animals at the beginning of the program.

We rule out alternative potential causes for the observed consumption increase, including changes in labor earnings and increases in both goods prices and income from higher sales caused by a higher demand. Therefore, we conclude that the indirect program effect on consumption is not generated by an increase in earnings.

A limitation of the program evaluation literature is that there is often a sizeable difference between the experimental estimates of treatment effects and the effect of the policy on the population. This normally occurs for two reasons. Usually one can estimate the treatment effect only on the treated or the eligibles, and not, as in our case, its indirect effect on the ineligibles. Second, the experiment normally involves a small fraction of the relevant population; when a program is rolled out nationwide, it may have general equilibrium effects that offset the partial equilibrium effects estimated from experimental data. Our analysis does not suffer from this limitation because we observe the treatment effect on the ineligibles. Further, in our case this effect is not a function of the number of treated villages, but of the existence of informal risk-sharing networks. As long as informal networks are an important tool to insure against risk, we can predict positive indirect effects on consumption, irrespective of the number of localities that receive Progresa assistance. Thus, contrary to many active labor market programs, in this class of policies the indirect treatment effects reinforce the direct effects.

We contribute to the program evaluation literature in several ways. First, we show that a class of widely implemented aid policies has large, positive, indirect effects on consumption. Second, we establish how these indirect effects operate, and that they are a feature of the nationwide program, rather than of the evaluation sample only. Third, we point out that the unit of analysis to evaluate this class of policies is the entire local economy, rather than only the treated. The implication for the design of policy evaluations is that the experimental data should be randomized at the village level, as done in the Progresa evaluation, rather than within a given locality, as is often the case.

We also add to the literature that studies consumption smoothing in low-income economies by showing how a cash injection into a group of households affects all families living in the same village. Consistent with the predictions of a simple risk-sharing model, we find that ineligible households living in treated villages receive more informal loans (e.g., Mark R. Rosenzweig 1988b; Robert Townsend 1995; Christopher Udry 1994), receive more transfers from family and friends (e.g., Rosenzweig 1988a; Rosenzweig and Oded Stark 1989; Marcel Fafchamps and Susan Lund 2003), and reduce their livestock and grains (e.g., Angus S. Deaton 1992; Rosenzweig and Kenneth I. Wolpin 1993; Udry 1995; Youngjae Lim and Townsend 1998). In addition, unlike most of the empirical literature, we can identify to what extent a household's positive income shock benefits the other village members: for every 100 pesos transferred by Progresa to the eligible households, the consumption of ineligible households increases by approximately 11 pesos.

The paper is organized as follows: Section I describes the structure of the program, the data collected for its evaluation, and the village characteristics indicating there is scope for risk-sharing. In Section II we use the predictions from a simple risk-sharing model to derive a set of testable hypotheses; Section III discusses the identification of the parameters of interest, and Section IV estimates and interprets these parameters, showing there is a positive indirect treatment effect on consumption that occurs through higher loans and transfers. Section V rules out alternative explanations for this indirect effect, and Section VI checks that the estimated effects are consistent with each other. Section VII concludes.

I. The Data and Village Characteristics

A. *Program Structure and Data Characteristics*

Progresa (currently renamed Oportunidades) is an ongoing Mexican poverty alleviation program that targets poor households, providing grants to improve education, health, and nutrition. Started in 1997 and with transfers beginning around March 1998, this program had about 5 million recipient households in more than 92,000 localities by the end of 2006. The program provides grants in the form of nutritional subsidies, as well as scholarships for children attending third to ninth grade. The recipients of the transfers are women. The grants, paid bimonthly, are conditional upon family visits to health centers, women's participation in informal workshops on health and nutrition issues, and verification that children attended classes at least 85 percent of the time (Santiago Levy 2006).

Scholarships are larger for higher school grades and for girls going to secondary school. The monthly amounts range from 70 pesos for all third graders to 225 pesos for males and 255 pesos for females in ninth grade.[1, 2] These payments correspond to approximately one-half to two-thirds of the wage a child would earn by working full time (Paul T. Schultz 2004), and cannot exceed a monthly total of 625 pesos per household.[3] The actual monthly grants up to November 1999 are sizeable, averaging 200 pesos per household, or 32.5 pesos per adult equivalent. This is about 23 percent and 16 percent of the average food consumption per adult equivalent for the poor and nonpoor in control villages (which are, respectively, 140 and 200 pesos).

The experimental data for the evaluation of Progresa contain information on households from 506 poor rural villages in seven different states. Because of the program's geographic phase-in, 186 villages are randomized out and receive the treatment only at the end of 1999. Program eligibility depends on poverty status, and households are classified as being eligible or ineligible according to an assessment of their permanent income from information collected in the September 1997 Census of localities.[4] There were two rounds of selection of eligible households in Progresa; 52 percent of households were classified as eligible in 1997. A few months later, but

[1] These are the amounts of the scholarships in November 1998, the first postprogram wave for which we have data. Unless otherwise specified, all our monetary data are in November 1998 prices.

[2] The exchange rate is approximately ten pesos for one US dollar.

[3] The scholarships were smaller in 1998 and were later adjusted to keep their real value constant.

[4] We use the terms nonpoor and ineligible, or poor and eligible, interchangeably, as each pair identifies the same group of households. For a detailed discussion of the selection criteria for both villages and households, see Emmanuel Skoufias, Benjamin Davis, and Jere Behrman (1999); and Skoufias, Davis, and Sergio de la Vega (1999).

before the beginning of the program, 54 percent of the households initially classified as ineligible were added to the beneficiary group. However, about 60 percent of these households did not receive the transfers because of administrative problems, irrespective of their compliance with the eligibility rules. Thus, this group of reclassified households is in practice a mix of treated households and eligible but nontreated households that may actually expect transfers and behave accordingly. Because their behavior and incentives are unclear, we drop them from our data and keep only households initially classified as poor, and nonpoor families whose status was not revised.[5] These reclassified households are both in treatment and control villages, in equal share because of the village randomization. Therefore, the characteristics of the nonpoor in treatment and control villages are not systematically different.

The households are informed that, after they are classified as eligible or ineligible, their status will not change until November 1999, irrespective of any income variation. Thus, households have no incentive to reduce their labor supply or lie about their income. Furthermore, the current income is not used to compute the poverty index that determines program eligibility. In practice, there were hardly any status changes at the end of 1999.

After the start of the program, all residents of control and treatment villages are first interviewed in November 1998—about a semester after the beginning of the payments—and then in May and November 1999. This provides information from three different points in time after the beginning of the program. We also have preprogram data, collected in September 1997 and March 1998, which we use in the empirical analysis whenever possible.

The data can be divided into four groups: poor and nonpoor households in treatment and control villages. Only poor households in treatment areas receive the Progresa transfers. Poor households in control villages know they will be included in the program at the end of 1999, provided they are still eligible and the program is still in place.[6] Figure 1 shows the structure of the data and experimental design.

The sample size for the ineligibles varies across the three data waves: we observe 5,280, 4,443, and 4,502 households in November 1998, May 1999, and November 1999. The sample size changes in the same way for the poor. These differences may be due to household dissolution or death, to temporary or permanent migration, or to household members being unavailable for interviews. To confirm there are no differential attrition rates by village type for the nonpoor, we checked whether the ratio of ineligible residents in treatment and control villages is stable across the three waves, which it is: the share of nonpoor living in treatment villages is 61 percent in the first two semesters and 60 percent in the third semester.

B. *The Need for Risk-Sharing*

Consumption smoothing is especially important in developing countries, since, when income is low, a negative shock might have catastrophic consequences. This section provides evidence on the need for informal insurance in the sampled villages, in which there is hardly any income diversification and formal insurance is absent.

The September 1997 data show that agriculture is the main activity in 97 percent of villages, and the sole activity in 56 percent of localities. (Out of the remaining 44 percent of villages with

[5] For example, the reclassified households may initially change their children's school enrollment, expecting a transfer, or increase consumption by borrowing against their future transfers, which they know they will receive by the end of 1999 at the latest.

[6] The existence of the program could not be guaranteed beyond 1999 because Progresa may have been discontinued by the new administration, after the 2000 general election. Each new administration in Mexico generally begins its own programs, rather than continuing those of their predecessor (Levy 2006).

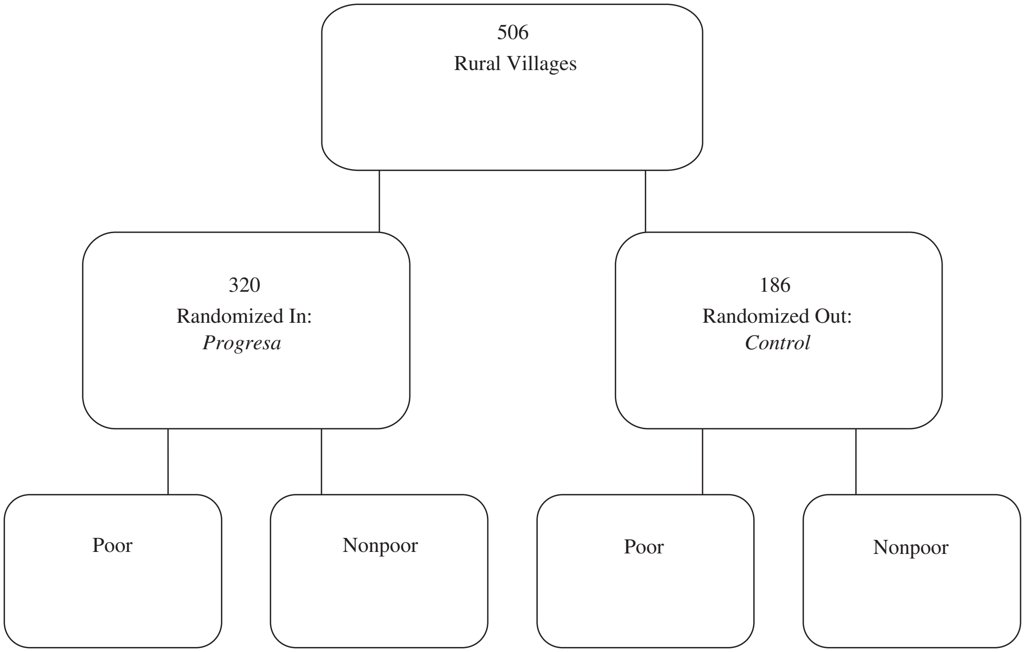

Figure 1. The Experimental Design

other activities, 50 percent engage in cattle farming and 28 percent in trade.) Corn is reported as the main (and often sole) crop by 88 percent of villages, while beans are the secondary crop in 60 percent of localities. Only 42 percent of villages cultivate three different crops. Thus, crop diversification does not play an important role in income smoothing.

These rural economies are subject to natural disasters: on average, 39, 57, and 30 percent of village residents suffered from at least one calamity in the 6 months prior to November 1998, May 1999, and November 1999. Water shortages, frost, and floods, all of which vary within village, are the most typical shocks, hitting a total of 30, 9, and 5 percent of households in the three periods. Other natural disasters such as earthquakes, hurricanes, fires, or pests are less frequent. Income also varies substantially both between households (the cross-sectional coefficient of variation, CV, is 1.5 in 1998 for the control villages), as well as within households over time (the longitudinal income, CV, is 1 for the average household in control villages).

Despite the need for insurance, formal credit and insurance institutions are virtually absent. In November 1998, fewer than 1 percent of villages had credit or consumption cooperatives, and fewer than 3 percent had nongovernmental organizations (NGOs) or production associations. On the other hand, informal institutions abound: 89 percent of villages engage in communal activities or chores, 85 percent of villages have a community assembly, 87 percent a parent association, and 38 percent a religious organization. Further, these villages are small: the average number of households per locality is 51 and the median is 46. In addition, mobility is low. In November 1998 and 1999, only 5 percent of the total number of individuals had left the household in the previous 5 years, 20 percent of whom live in the same village as the household of origin. A consequence of this low mobility is that most households are related. Angelucci et al. (forthcoming) report that 80 percent of the households have at least one related family member in the village, that the average size of this extended family network is 7.7 households, and that 52 percent of its members are eligible for the program, so extended families are composed of both poor and nonpoor households.[7]

Altogether, the high income risk, the absence of formal risk-sharing institutions, and the abundance of long-lasting relationships among village members strongly suggest that villagers engage in risk-sharing activities.

II. The Effect of Progresa in the Presence of Risk-Sharing

In this section we discuss the potential effect of Progresa for ineligible households if village members share risk. Consider a risk-sharing model in which agents fully insure against idiosyncratic risk by pooling resources and consuming a fixed share of total income, so that, conditional on aggregate resources, their consumption is independent of their individual income.[8] One of the implications of this model is that, given a pair of agents 1 and 2, an increase in agent 1's income will increase aggregate resources, resulting in higher consumption for both agents. This efficient resource allocation is achieved through a series of informal loans and transfers. Therefore, the higher income for agent 1 will also result in an increase in net transfers to agent 2.

Suppose agents 1 and 2 represent eligible and ineligible households in Progresa villages. As the program increases eligible households' income while leaving ineligible households' income unchanged, the consumption of both eligible and ineligible families will increase, and so will the net transfers to the ineligibles. These results generate our testable hypotheses:

HYPOTHESIS 1: *Progresa increases the consumption of ineligible households in treatment villages.*

HYPOTHESIS 2: *Progresa increases net transfers to ineligible households in treatment villages.*

One could object to our stylized model for a number of reasons. First, Progresa may represent an unprecedented event for the recipients, altering their income process. This, in turn, may reduce the amount of risk-sharing between villagers. As an extreme case, the treated may decide to stop insuring the ineligibles now that their income is higher, since these informal agreements cannot be legally enforced.[9] We believe this is not happening in our data for the following reasons. To begin, Progresa is not an unprecedented event in our villages, as their residents are used to receiving social assistance in many different forms.[10] Therefore, from the villagers' perspective, Progresa is just one of the many existing social assistance programs. Further, it is unlikely that Progresa changes the income process substantially because the program transfer

[7] Repeated interactions between a small number of households are important to address information and enforcement problems (see, e.g., Fafchamps and Lund 2003; Francis Bloch, Garance Genicot, and Debraj Ray 2005; Markus Mobius and Adam Szeidl 2007).

[8] We sketch this model in the online Appendix (http://www.aeaweb.org/articles.php?doi=10.1257/aer.99.1.486); see also, e.g., Barbara J. Mace (1991) or Townsend (1994).

[9] See, e.g., Stephen Coate and Martin Ravallion (1993), David K. Levine and Timothy J. Kehoe (1993), Narayana Kocherlakota (1996), and Ethan Ligon, Jonathan Thomas, and Tim Worrall (2002) for a more formal treatment of limited commitment models.

[10] For example, at the time Progresa is implemented, qualifying households receive basic consumer goods at subsidized prices (DICONSA), free tortillas (TORTIBONO), free breakfast for children (DIF), food packages (PASAF), free school supplies (CONAFE), lodging and education grants for indigenous students (INI), other school grants for all poor children (Ninos de Solidaridad), financing of productive projects (FONAES), temporary employment (PET), training scholarships for the unemployed (PROBECAT), and cash transfers to farmers producing specific crops (PROCAMPO) (Skoufias 2005).

is initially guaranteed only for less than two years and it is mainly in the form of scholarships, which stop as soon as the eligible children complete the subsidized school grades. On the other hand, the cost of not reciprocating may be the exclusion from future mutual insurance or other punitive sanctions, especially since the receipt of this transfer is publicly observed. For these reasons, we expect the cost of future exclusion from the insurance network to more than offset the benefit of not sharing the transfers. Consistent with this conclusion, we find no difference in the longitudinal variation of consumption in treatment and control villages, which would be the case if Progresa changed the amount of risk-sharing. The difference in the coefficients of variation is -0.002, with a standard error of 0.004. We also compared their distributions, which are almost identical.

Second, the discussion above abstracts from the conditionality of the program. The design of Progresa requires the recipients to have health checks and send children to school to receive the income transfers. Since complying with these requirements may be costly for the treated, the net value of the Progresa transfer may be small and the change in aggregate resources negligible. However, the transfers are, in practice, unconditional for most of the recipients. This is because most eligible children were already going to school before the program started (in 1997, primary and secondary school enrollment rates for the eligibles were 90 percent and 60 percent). Moreover, compliance with the health requirements is not very time consuming. For example, adults are asked to have only annual health checks.[11] In addition, most households would have had health checks even in the absence of the program (e.g., in November 1998, 72 percent of households in control villages had at least one health check during the previous 6 months). In sum, complying with the program rules is likely not very costly for most recipients.

Third, while we consider insurance within the village, risk-sharing may cross village boundaries. If risk-sharing occurs both within and between villages, however, the net financial transfers toward treatment villages should decrease, as Progresa may crowd out private transfers (Pedro Albarran and Orazio P. Attanasio 2004). If this were the case, our estimates would be lower bounds of the true program effects on ineligibles' consumption and transfers.

III. Identification and Estimation

Our data consist of a partial-population experiment (Moffitt 2001): the program is offered only to poor households living in a set of randomly chosen villages, and the data provide information on all village residents, eligible and ineligible, living in both treatment and control villages. This experimental design enables us to identify how offering Progresa to the poor affects the behavior of the nonpoor under fairly weak assumptions.

Define Y_{1i} as the potential outcome for nonpoor ($NP_i = 1$) in treatment villages ($T_i = 1$) *in the presence of the treatment*. The potential outcome for nonpoor ($NP_i = 1$) in treatment villages ($T_i = 1$) *in the absence of the treatment* is represented by Y_{0i}. The observed outcome is: $Y_i = Y_{0i} + T_i(Y_{1i} - Y_{0i})$. The treatment is the availability of Progresa for poor households ($NP_i = 0$) in treatment villages ($T_i = 1$). The average effect of the program on nonpoor households living in treatment villages, which we call the indirect treatment effect (ITE), is then

$$ITE = E(Y_{1i} \mid T_i = 1, NP_i = 1) - E(Y_{0i} \mid T_i = 1, NP_i = 1).$$

Under the assumptions of random assignment, the expected value of Y_0, the potential outcome in the absence of the treatment, is the same in both treatment and control villages, i.e.,

[11] The checks are more frequent for infants, young children, and pregnant and lactating women.

$E(Y_{0i}|T_i = 1, NP_i = 1) = E(Y_{0i}|T_i = 0, NP_i = 1)$. If there are no program spillover effects to control villages, the difference,

$$E(Y_i|T_i = 1, NP_i = 1) - E(Y_i|T_i = 0, NP_i = 1), \tag{1}$$

identifies the ITE. Despite the randomization, equation (1) does not identify an average ITE if nonpoor households in control villages are indirectly affected by the program. However, if there are indirect program effects for nonpoor households in both treatment and control villages, the sign of these effects is likely the same for the two groups. In this case, the parameter above identifies a lower bound to the ITE. For example, suppose that the increase in school enrollment of treated children reduces child labor. This decrease in labor supply may result in higher employment and earnings for ineligible households in *both* treatment and control villages.

We obtain estimates of the ITEs comparing mean-observed outcomes for the nonpoor in treatment and control villages. If we do the same for poor households, we estimate the average treatment effect (ATE) on the eligibles under the assumption that $E(Y_{0i}|T_i = 1, NP_i = 0) = E(Y_{0i}|T_i = 0, NP_i = 0)$. In practice, the difference between the ATE and the average treatment on the treated effect is negligible, because about 97 percent of eligible households participate in the program.[12]

IV. Indirect Treatment Effect on Consumption: Estimates and Causes

Now we can express our two testable hypotheses in terms of treatment effects:

HYPOTHESIS 1: *Progresa increases the consumption (C) of ineligible households in treatment villages, i.e.,* $ITE^C > 0$.

HYPOTHESIS 2: *Progresa increases net transfers (L) to the ineligibles in treatment villages, i.e.,* $ITE^L > 0$.

A. *Effect on Consumption*

Table 1 shows food consumption averages, as well as estimates of treatment effects for both ineligible and eligible households. We compute monthly food consumption per adult equivalent to ease the comparison between poor and nonpoor households, since their sizes differ. (For example, in November 1999 the average household sizes are 5.8 and 5 adult equivalents for the poor and the nonpoor.) We use an equivalence scale estimated from these data by Vincenzo Di Maro (2004) and November 1998 prices. The online Appendix provides further details on the creation of these variables.

Table 1 shows that, as expected, the ineligibles consume about 40 percent more than the eligibles in control villages. However, nonpoor households are not very well off; their average food consumption in control areas is only 200 pesos, that is, 20 US dollars per adult equivalent per month. Consumption is higher in treated areas for both sets of households: while the program has

[12] Gustavo Bobonis and Frederico Finan (2008) and Rafael Lalive and Alejandra Cattaneo (2008) use the same data to estimate peer effects on schooling. Our approaches are similar because we all exploit the partial-population experiment to identify indirect treatment effects. However, unlike these other papers, we do not attempt to separately identify contextual and endogenous social interactions.

Table 1—Average Monthly Food Consumption per Adult Equivalent—Levels and Differences

	Ineligibles				Eligibles		
	Nov. 1998	May 1999	Nov. 1999		Nov. 1998	May 1999	Nov. 1999
Control	222.61	213.69	206.71		159.96	159.92	153.7
	[179.76]	[212.19]	[232.56]		[112.19]	[158.33]	[126.72]
Treatment	216.38	233.06	224.08		175.80	185.66	184.31
	[166.82]	[303.79]	[285.61]		[136.59]	[193.81]	[172.25]
No controls:							
ITE	−6.24	19.37	17.36	ATE	15.84	25.74	30.61
	[7.58]	[10.50]*	[9.70]*		[4.86]***	[5.80]***	[5.15]***
Observations	4,643	3,855	4,285		10,973	9,659	10,554
Controls:							
ITE	−5.20	20.72	18.84	ATE	15.49	24.42	29.86
	[7.47]	[10.19]**	[9.42]**		[4.75]***	[5.64]***	[4.79]***
Observations	4,624	3,838	4,266		10,936	9,630	10,518

Notes: Monthly pesos per adult equivalent at November 1998 prices; the exchange rate is roughly ten pesos per US dollar. We report the standard deviations of the means and the standard errors, in brackets, of the treatment effects. The latter are clustered at the village level. The set of conditioning variables we add to the regressions in the left panel are: household poverty index, land size, head of household gender, age, whether speak indigenous language, literacy; at the locality level, poverty index and number of households. All variables are at 1997 values.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

no indirect effect in November 1998, a few months after the program transfers began, the ITE on food consumption is significantly higher, by 19.3 and 17.3 pesos per adult equivalent in May and November 1999. The estimated effects are 20.7 and 18.8 when we add conditioning variables. This is approximately a 10 percent increase over the average consumption in control villages.

The program effect on food consumption for the poor is positive and significant in all three periods and grows over time, consistent with the existing evidence (John Hoddinott, Skoufias, and Ryan Washburn 2000; Paul Gertler, Sebastian Martinez, and Marta Rubio-Codina 2006); it amounts to 15.8, 25.7, and 30.6 pesos per adult equivalent in the three waves we observe.

The estimated ITEs are robust to a variety of checks. First, by checking the administrative records, we verify that the nonpoor are not erroneously receiving the program transfers. Second, we find that the estimated effects are not caused by a disproportionate increase in the consumption of a few families. To test this hypothesis, we estimate average consumption for treatment and control households, grouping them according to their poverty level. Figure 2 provides kernel estimates of these averages, and shows that consumption is higher in treatment villages for all poverty levels. We also regress consumption on the welfare index interacted with the treatment dummy and find the interaction term is positive and significant. We further compare the densities of consumption for the nonpoor in treatment and control villages. We find that low consumption is less frequent and high consumption more frequent in treatment villages.

Third, in Table 2 we estimate treatment effects on the caloric content of food consumed, rather than on its monetary value. This exercise is a useful robustness check because, to compute the value of home-produced food, we had to impute prices from purchased goods. If the imputed prices were inaccurate, this would provide imprecise consumption data. We find a significant increase in daily caloric intake of 178 kilocalories per adult equivalent for the ineligibles and 340 for the eligibles. The estimates are obtained by pooling consumption data for May and November

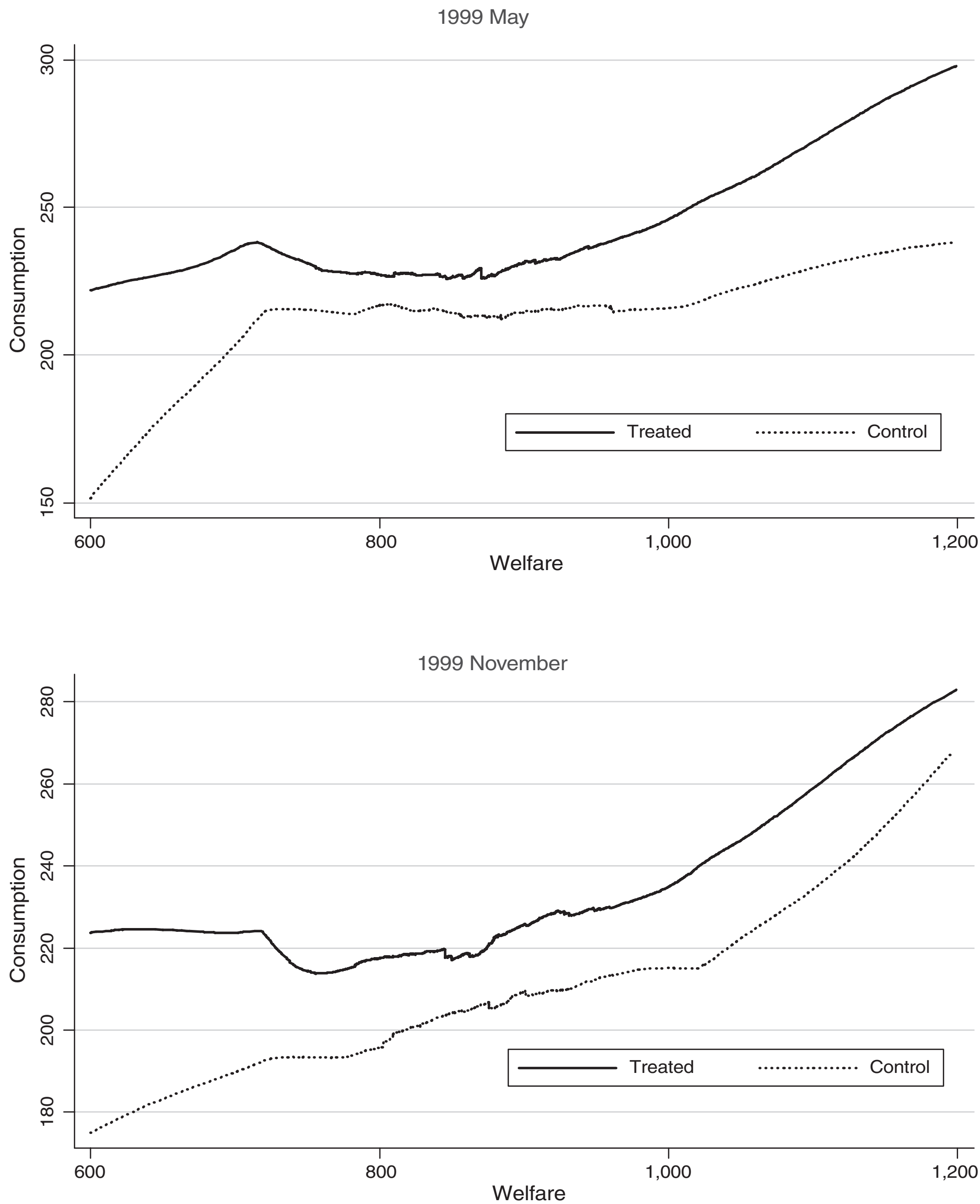

FIGURE 2. MONTHLY FOOD CONSUMPTION, PER ADULT EQUIVALENT, FOR INELIGIBLE HOUSEHOLDS BY WEALTH LEVEL

Note: Monthly peso value of food consumption per adult equivalent (at November 1998 prices).

Table 2—ITEs on 1999 Caloric Intake and Food Quantities

	Kcals	Tomatoes	Carrots	Greens	Oranges	Chicken
ITE	178.36	0.08	0.11	0.3	−0.04	0.07
	[50.68]***	[0.03]**	[0.07]*	[0.18]*	[0.41]	[0.04]*
Observations	8,746	8,125	811	707	2,454	4,402
	Meat	Eggs	Milk	Corn	Rice	Beans
ITE	0.14	0.02	0.43	1.15	0.04	0.06
	[0.05]***	[0.08]	[0.27]*	[0.59]*	[0.04]	[0.05]
Observations	5,177	7,182	2,403	3,558	5,564	8,217

Notes: Monthly quantity consumed (in kilos, liters, or pieces depending on food type) or kilocalories per adult equivalent. Standard errors in brackets clustered at the village level. We add the same set of conditioning variables described in Table 1.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

1999 and are both significant at the 99 percent level. We also compute the quantity of food consumed in 1999 for several types of aliments, and find significant increases in the consumption of tomatoes, carrots, meat, eggs, corn, and rice for ineligible households, as shown in the rest of Table 2.[13]

The parameters we estimated so far exploit only the cross-sectional variation in our data. We also use preprogram food expenditure (observed in March 1998) to estimate the ITE either using difference-in-difference estimators, or adding preprogram expenditure as a conditioning variable. We do not present difference-in-difference estimates as our key results because the March 1998 data provide information on expenditures only, so we do not observe preprogram consumption quantities. Expenditure and consumption may differ considerably if consumption of home-produced goods is a sizeable fraction of total food consumption, which is likely among indigent families. Moreover, rather than asking detailed item-by-item questions, as in the later data waves, the March 1998 data report expenditures only by food group, probably understating true expenditures. In any case, the difference in preprogram food expenditure between the nonpoor in treatment and control villages is either 0.57 pesos (with a standard error of 10.37), or –2.86 (and a standard error of 9.00), according to which of two available measures we use.[14] We report a subset of the estimated effects in Table A2 of the online Appendix, where we also experiment with different ways to deal with outliers and with adding a set of covariates at baseline values: the significance of the effects is largely unchanged.

In unreported regressions, we estimate the ITE on total nonfood consumption, but find no set of consistently significant effects across different specifications: the point estimates are positive, especially in May 1999, but not always significant. This is probably not surprising, because our nonfood consumption data are not as accurately measured as food consumption (e.g., the recall period is much longer) and nonfood consumption is lumpier. However, in 1999 the treatment

[13] We also estimate positive and significant ITEs on the log monetary value of food consumption for different food categories, i.e., fruits and vegetables, grains, meat, and fish, in Table A1 of the online Appendix.

[14] We obtain the first measure from total weekly food expenditure data, and the second one aggregating weekly expenditures for the following food categories: vegetables and fruits; grains and cereal; meat, fish, and dairy products; industrial products.

effect on nonfood consumption is positive and significant for the poor and it amounts to 6.1 and 5.3 pesos per adult equivalent per month in May and November.

B. *Effect on Loans and Transfers*

We now proceed to test the hypothesis of positive ITEs on loans and transfers. Unfortunately we have no direct information on the identity and location of network members, so it is not clear how to define a social network. However, the data presented above suggest that neighbors, relatives, and friends who live in the village may be an important part of it. Moreover, the evidence from the existing literature confirms that village-level networks are important. For example, Townsend (1994) and many others find a very high level of risk-sharing between villagers in various developing countries; Udry (1994) reports that almost no loan in his sample of northern Nigerian villages crosses the village boundary, and he argues geographic proximity generates informational advantages.

We have information on the receipt of loans in the previous six months, and of monetary and in-kind transfers from family and friends during the previous month. Credit is informal: 70 percent of loans occur among friends or relatives (and a further 9 percent occur through local moneylenders).

Our data suffer from the following limitations. First, we do not observe the identity of lenders and donors, nor whether they belong to poor or nonpoor households. Second, while in principle we also have data on transfers given, this variable is unreliable; hence we cannot build a good net transfers variable. For example, in the November 1999 data, 319 households report they *received* transfers from families living in the same village, while only 41 households appear to have *made* a transfer to a family in the same village, implying that on average each donor makes transfers to 8 different families. We think this is unlikely, and rather suspect that the poor may be afraid to admit they are sharing the Progresa grants with the nonpoor. Third, we observe both loans and transfers only in November 1998, when very little money had been transferred to treated households. In the remaining waves, we observe loans in May 1999 and transfers in November 1999.

In Table 3 we report means, standard deviations, and proportion of households receiving loans or transfers. About 12 percent of the ineligibles and 8 percent of the eligibles receive either loans, transfers, or remittances (which we call "total credit resources") in November 1998. The average monthly receipt amounts to some 400 pesos for the ineligibles, and to 220 pesos for the eligibles. Interestingly, this pattern is common for all variables and semesters: a higher proportion of the ineligibles receives transfers or loans, compared to the eligibles, and their average receipt is larger, both in treatment and in control villages. This could be a scale effect: the ineligibles are wealthier than the eligibles; therefore they earn, consume, and get higher transfers. Further, loans are larger in size than monetary transfers. This is consistent with the evidence for the Philippines (see Fafchamps and Lund 2003), i.e., that risk is shared through informal loans, rather than through transfers. On the other hand, however, the respondents are likely to underreport the true extent of in-kind gifts they received. For example, they may not consider a meal consumed at a friend's place as a transfer. Irrespective of this potential underreporting, both the proportion of recipients and the size of the receipt are larger in treatment than in control areas for the nonpoor (with a couple of exceptions for monetary and in-kind transfers), while the pattern is more mixed for the poor.

To test our prediction that the program results in more loans and transfers for the ineligibles, we estimate treatment effects on the probability of receipt and on the size of loans and transfers. These results are in Table 4. We report both OLS and Tobit estimates of the effects on the levels, since Tobit is inconsistent in the presence of heteroskedasticity (although the estimator

TABLE 3—CREDIT RESOURCES: MEAN, SHARE OF RECIPIENTS, AND AVERAGE AMOUNT OBTAINED PER ADULT EQUIVALENT BY HOUSEHOLD TYPE AND SEMESTER

	November 1998			May 1999			November 1999		
	Mean	%	Average receipt	Mean	%	Average receipt	Mean	%	Average receipt
Panel A: Total credit resources									
NP control	40.78	0.11	371.04						
	[216.45]		[552.14]						
NP treatment	50.05	0.12	422.24						
	[249.97]		[608.91]						
P control	17.69	0.08	222.02						
	[121.72]		[375.42]						
P treatment	17.74	0.08	219.64						
	[107.51]		[314.47]						
Panel B: Loans									
NP control	11.95	0.03	405.18	16.52	0.04	428.50			
	[111.81]		[518.81]	[150.62]		[646.5]			
NP treatment	19.56	0.03	607.62	27.69	0.05	530.15			
	[254.99]		[1295.77]	[233.33]		[883.85]			
P control	5.33	0.03	190.03	9.66	0.05	197.65			
	[58.82]		[298.2]	[97.8]		[398.99]			
P treatment	5.72	0.03	205.18	11.35	0.05	242.74			
	[57.16]		[276.7]	[133.05]		[568.62]			
Panel C: Monetary transfers from family and friends									
NP control	5.95	0.04	164.01				5.48	0.02	225.09
	[42.95]		[159.0]				[68.8]		[384.97]
NP treatment	11.02	0.04	247.04				9.31	0.04	244.77
	[79.81]		[291.75]				[81.44]		[343.29]
P control	2.83	0.03	108.24				1.68	0.01	125.46
	[26.48]		[124.52]				[35.06]		[279.14]
P treatment	3.20	0.03	124.56				1.98	0.02	119.77
	[32.81]		[164.04]				[22.85]		[132.66]
Panel D: In-kind transfers from family and friends									
NP control		0.01						0.02	
NP treatment		0.02						0.01	
P control		0.01						0.02	
P treatment		0.01						0.01	

Notes: Monthly pesos per adult equivalent at November 1998 prices; the exchange rate is roughly ten pesos per US dollar. Standard deviations in brackets. Top 1 percent trimmed in the computation of the quantities but not for the proportions. Total credit resources computed as the sum of loans, transfers, and remittances.

performs well under moderate departures from the homoskedasticity assumption). Since only a small share of households receives loans or transfers, the estimated effects on loan and transfer size are very sensitive to outliers. For this reason, we consider the probit estimates as the most reliable of the set.[15]

[15] Note that the nonresponse rates, which vary between 0 and 5.4 percent for nonpoor households, do not differ between treatment and control areas. This may have been an important issue, owing to the relatively small number of households reporting loans or transfers.

Table 4—Program Effects on Credit Resources

	November 1998			May 1999			November 1999		
	Probit	OLS	Tobit	Probit	OLS	Tobit	Probit	OLS	Tobit
Panel A: Loans—Ineligibles									
ITE	0.003	7.613	3.123	0.014	11.168	9.723			
	[0.01]	[5.362]	[3.948]	[0.01]*	[6.621]*	[4.51]**			
Observations	4,913	4,912	4,912	4,432	4,431	4,431			
Panel B: Monetary transfers from family and friends—Ineligibles									
ITE	0.007	3.739	2.562				0.014	3.825	4.137
	[0.009]	[3.289]	[1.716]				[0.008]*	[3.005]	[1.727]**
Observations	4,837	4,836	4,836				4,447	4,447	4,447
Panel C: In-kind transfers from family and friends—Ineligibles									
ITE	0.008						−0.007		
	[0.004]**						[0.004]*		
Observations	5,280						4,502		
Panel D: Loans—Eligible									
ATE	0.000	0.394	0.039	−0.002	1.686	−0.272			
	[0.005]	[1.431]	[0.785]	[0.008]	[2.794]	[1.482]			
Observations	11,805	11,805	11,805	11,019	11,019	11,019			
Panel E: Monetary transfers from family and friends—Eligible									
ATE	0.000	0.367	0.006				0.003	0.307	0.469
	[0.004]	[0.651]	[0.436]				[0.003]	[0.628]	[0.382]
Observations	11,630	11,630	11,630				10,823	10,823	10,823
Panel F: In-kind transfers from family and friends—Eligible									
ATE	−0.001						−0.006		
	[0.003]						[0.003]**		
Observations	12,519						10,967		

Notes: Monthly pesos per adult equivalent at November 1998 prices; the exchange rate is roughly ten pesos per US dollar. Standard errors in brackets clustered at the village level in OLS and Probit. Top 1 percent trimmed in the OLS and Tobit regressions. The results are qualitatively unchanged adding conditioning variables.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

The ineligibles in treatment villages may receive more net resources from both the treated, whose income has increased, and other ineligibles, who may shift resources away from the treated to the ineligibles within their network, as the former group has become less needy. Some ineligible households will also have good income shocks. However, because of the randomization, the distribution of these shocks does not differ between treatment and control villages, and is differenced out in the computation of treatment effects.

The main conclusion from this exercise is that the nonpoor receive more transfers and loans: the estimated ITEs are positive in all waves and significant especially in 1999, when the poor received more Progresa money. The effects are sizeable: for example, in May 1999 the likelihood of receiving loans increases by 1.4 percentage points, or 38 percent, and its size by roughly 10 pesos, that is, 50 percent of the observed consumption increase in the same month. In November 1999 the likelihood of receiving monetary transfers increases by 1.4 percentage points, or roughly 50 percent, and its level grows by about 4 pesos, or 23 percent of the observed consumption increase in the same period. Thus, the magnitude of the estimated effects is consistent with the size of the consumption increase, and suggests that the effect on the credit market is an important determinant of the estimated consumption increase.

As a minor point, the ITE for in-kind transfers is significant both in 1998 and 1999, but positive first and then negative. This may suggest that households transfer more food or clothes when there is little extra cash in the treated localities, while they shift the composition of transfers toward money when there is more currency in the local economy.

In-kind transfers to the poor decrease in 1999, but we find no other significant decrease in loans and transfers to eligible households. This is counterintuitive, as the treated should receive fewer transfers, since the program makes them better off (Albarran and Attanasio 2004). This effect is probably offset by eligible households' increased ability to borrow using their Progresa entitlement as collateral. The public transfers do crowd out private transfers to the poor, but not from other villagers: migrant remittances to eligible households decrease by about 144 pesos per month in November 1999 (with a standard error of 78), a 30 percent decrease compared to the level in control villages. The likelihood of receiving remittances does not change, nor is there any significant effect for the ineligibles.

In unreported robustness checks, we estimate the ITE on net transfers. The results are broadly unchanged, although the estimates are less precise, consistent with our suspicion that we are mainly adding noise to our dependent variable because the donation data are unreliable. For example, the OLS estimates are 3 pesos in 1998 and 3.3 in November 1999 (the standard errors are 1.86 and 3.2).

V. Alternative Channels

There are alternative mechanisms that might cause a consumption increase for the ineligibles. The estimated consumption increase (C) may be caused by higher labor (Y^l) and goods market (Y^g) incomes, lower savings (S) and investment (I), besides higher loans and transfers (L), as summarized in the following household accounting identity:

$$\Delta Y^l + \Delta Y^g + \Delta L = \Delta C + \Delta S + \Delta I. \tag{2}$$

The symbol Δ represents the indirect program effect for each variable.[16]

A. *Labor Market*

Labor earnings for the nonpoor may increase if the program affects the poor labor supply. In theory, Progresa may have the following effects on the treated: it may decrease child labor, as some treated children switch from employment to schooling, and reduce adult labor supply through an income effect. This may result in higher labor income for the nonpoor through higher wages and increases in their labor supply. In practice we do not expect to find any sizeable effect for the following reasons. First, Susan Parker and Skoufias (2000) estimate a 2.5 to 3 percentage point reduction in child labor for boys and 1.2 percentage points for girls. Since child labor is only a small fraction of total labor, the overall reduction in labor supply is probably not large enough to generate sizeable general equilibrium effects. Second, the program income effect is

[16] We also test whether the ineligibles start receiving more transfers through alternative welfare programs, or nutrition supplements for malnourished children initially intended only for eligible households, but we find negligible effects.

Table 5—Program Effect on Monthly Adult Equivalent Labor Earnings

	November 1998	May 1999	November 1999
ITE	−0.39	−4.52	7.91
	[18.20]	[17.09]	[19.28]
Observations	18,939		
ATE	8.54	4.2	10.2
	[6.63]	[6.44]	[7.64]
Observations	45,883		

Notes: Monthly pesos per adult equivalent at November 1998 prices; the exchange rate is roughly ten pesos per US dollar. Standard errors in brackets clustered at the village level. Difference-in-difference estimates. The sample size is from pooling the September 1997 data with the November 1998, May 1999, and November 1999 data. The results are unchanged if we add conditioning variables, with the exception of the ATE estimate for November 1999, which is now significant at the 10 percent level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

likely small, given the extreme poverty of treated households and the limited duration of guaranteed existence of the program.

We investigate whether Progresa changes labor income for the ineligibles by testing whether their labor earnings differ in treatment and control villages. We compute monthly labor earnings per adult equivalent as the sum of income from primary and secondary occupations, using reported wages and hours worked, and earnings from informal work activities (provision of transportation, cooking, sewing, repairs, carpentry, and various other paid services). Table 5 reports estimates of the treatment effects for both the nonpoor and the poor. These effects are never statistically different from zero. In unreported regressions, we tested for differences in hours of work, which never change for the nonpoor. Thus, we find no evidence that the ineligibles' increase in consumption is caused by higher labor income.

B. *Goods Market*

Progresa may affect the goods market through at least two channels. First, poor households' higher expenditures may increase goods prices in treatment villages. Second, the nonpoor may increase sales to the poor (e.g., if the nonpoor are land owners who sell produce and meat to the poor). In practice we do not expect sizeable effects, since this market is fairly integrated. Chicken, meat, and medicines are sold in fewer than 10 percent of the villages, and even staples such as corn, flour, and milk are not sold in 53 percent of the sampled villages.[17] If one store serves a cluster of treated and control villages, which is the case if, e.g., the stores and farmers markets are located in the municipal capital, any potential effect on prices and earnings caused by the program will equally affect all villages in the cluster, with no differential effect in treatment villages.

To test for effects on the goods market, we first compare prices in treatment and control localities. To do so, we consider village prices by good over time. We provide details on the creation of the price variables in the online Appendix, and estimates of the price differences between

[17] The Progresa demand shock may not affect prices of tradeable goods, but it increases prices of nontradeables. However, we showed above that labor earnings, which include earnings from services, do not increase.

Table 6—Net Sales of Agricultural Products and Animals

	November 1998 net sales level	May 1999 net sales level
Panel A: Agricultural sales		
ITE	−4.953	−5.37
	[3.25]	[4.52]
Observations	4,287	4,026
ATE	−0.639	−0.741
	[0.33]*	[0.61]
Observations	10,249	9,973
Panel B: Animals		
ITE	0.429	0.249
	[0.33]	[0.21]
Observations	4,803	4,338
ATE	0.008	−0.03
	[0.05]	[0.03]
Observations	11,546	10,797

Notes: Monthly pesos per adult equivalent at November 1998 prices. Standard errors in brackets clustered at the village level. Treatment effects on the levels estimated by OLS. The results are unchanged if we add conditioning variables.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

treatment and control villages in Tables A3 and A4. While we find a small positive effect on 5 out of 36 food prices in November 1998, prices of staples such as rice, beans, corn, and chicken do not change. Therefore, we do not expect any substantial increase in the cost of the food basket. Moreover, we find no food price change in the later waves, nor evidence of changes for nonfood prices. The evidence presented here is consistent with earlier work by Hoddinott, Skoufias, and Washburn (2000).

We further test whether there is a program effect on net sales of agricultural products and animals for poor and nonpoor households.[18] Table 6 shows estimates of the ITEs and ATEs for these variables. The main result for the ineligibles is that their income from net sales is not increasing: in fact, in 1998 their agricultural sales fall slightly, while livestock net sales do not change. Agricultural net sales drop by 0.6 pesos for treated poor in 1998. From those results we conclude that changes in the goods market are very unlikely to cause the observed increase in consumption for the ineligibles.

C. *Savings and Investment*

The households in our sample hold livestock and grains, which they might use as a buffer against income fluctuations. We investigate whether Progresa affects the stock of animals and grains. In Table 7 we compare the changes in the stock of horses, donkeys, oxen, cows, poultry, pigs, goats, and rabbits, in treatment and control villages. For the ineligibles, the stocks of oxen,

[18] We can perform this exercise only for the first two data waves, as no data are available in November 1999.

Table 7—Treatment Effects on the Average Monthly Change in Animal Stock

	Ineligibles			Eligibles		
	Nov. 98	May 99	Nov. 99	Nov. 98	May 99	Nov. 99
Horse	−0.001	0.001	0.001	0.001	0.001	**0.001**
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	**[0.0004]****
Observations	5,219	4,410	3,979	12,484	11,019	10,176
Donkey	−0.001	−0.001	0.001	0.001	0.001	0.001
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	5,233	4,410	3,990	12,429	10,981	10,181
Ox	**−0.001**	0.001	0.001	0.001	0.001	0.000
	[0.001]**	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	5,264	4,439	4,002	12,491	11,032	10,203
Goat	**−0.010**	0.005	0.002	**−0.004**	**0.004**	0.002
	[0.005]**	[0.006]	[0.006]	**[0.002]***	**[0.002]***	[0.002]
Observations	5,255	4,427	3,986	12,491	11,024	10,185
Cow	−0.002	−0.003	**0.011**	0.001	0.001	0.002
	[0.004]	[0.004]	**[0.004]*****	[0.001]	[0.001]	[0.001]
Observations	5,204	4,402	3,974	12,493	11,034	10,196
Poultry	**−0.024**	0.007	0.017	**0.010**	0.002	0.002
	[0.012]**	[0.011]	[0.010]	**[0.005]****	[0.006]	[0.006]
Observations	5,109	4,323	3,897	12,389	10,892	10,061
Pig	−0.001	−0.003	0.001	0.002	0.001	−0.001
	[0.002]	[0.002]	[0.003]	[0.002]	[0.001]	[0.002]
Observations	5,215	4,411	3,959	12,452	10,975	10,140
Rabbit	0.001	−0.001	0.001	0.001	0.001	0.001
	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	5,274	4,438	4,000	12,506	11,042	10,206

Notes: Number of animals per adult equivalent. Monthly averages computed dividing the change in stock between two data waves by the number of months between them. Standard errors in brackets clustered at the village level. First difference estimation. The results are unchanged if we add conditioning variables.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

goats, and poultry decreases between September 1997 and November 1998, and is stable in later waves (with the exception of the stock of cows, which grows between May and November 1999).

We check for similar patterns in the stock of corn and beans, the two most commonly produced crops. While we do not observe the stock of grains, we know how much was produced and sold, as well as the amount of home-produced grains that the household consumed. Therefore, we can infer the change in the stock by comparing the difference between net sales and consumption of home-produced grains. This comparison hinges on the assumption that the preprogram stock does not differ between households in treatment and control villages because of the randomization. Table 8 shows that, while net sales do not change, in May 1999 the ineligibles in treatment villages increase consumption of own corn by about one kilo per month per adult equivalent, worth approximately 1.7 pesos. This suggests the nonpoor are depleting their stock of corn. We find no significant changes in the stock of beans.

Table 8—Difference between Production and Sales, Value of Consumption of Own Production of Grains

	November 1998			May 1999		
	I Production-sales	II Consumption	III Value consumption	I Production-sales	II Consumption	III Value consumption
Panel A: Corn—Ineligibles						
ITE	3.773	−0.177	−0.367	9.264	0.947	1.733
	[17.201]	[0.325]	[0.494]	[27.688]	[0.603]	[1.036]*
Observations		5,280			4,443	
Panel B: Corn—Eligibles						
ATE	−2.169	0.112	0.227	13.074	0.508	1.124
	[6.045]	[0.223]	[0.385]	[15.678]	[0.435]	[0.725]
Observations		12,519			11,044	
Panel C: Beans—Ineligibles						
ITE	3.361	0.039	0.159	−1.914	0.065	0.312
	[4.514]	[0.212]	[0.977]	[9.711]	[0.048]	[0.252]
Observations		5,280			4,443	
Panel D: Beans—Eligibles						
ATE	1.482	0.070	0.384	3.136	0.015	0.183
	[2.063]	[0.027]**	[0.141]***	[1.906]	[0.034]	[0.185]
Observations		12,519			11,044	

Notes: Monthly adult equivalent in kilograms in columns I and II, pesos at November 1998 prices in column III. Standard errors in brackets clustered at the village level. The results are unchanged if we add conditioning variables.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

These results suggest that the program relaxes borrowing constraints for the nonpoor, who can now receive extra resources from the poor if they are hit by a negative income shock (Deaton 1991). Further, the size of the stock may be smaller because the program's beneficial effect on health may reduce both the likelihood and the size of future income drops (Christopher Carroll 1997). Better nutrition and an increased knowledge of basic health facts for all villagers, coupled with more frequent health checks for the poor, improve the health conditions of the entire village, both directly and through a lower probability of contagion from infectious diseases (as shown by Edward Miguel and Micheal Kremer 2004). Gertler (2000) and Skoufias (2005), among others, find sizeable beneficial health effects of the program on recipients. We found positive effects also for the nonpoor: when asked about the health effects on their jobs, the ineligibles in treatment villages had fewer days out of work due to health reasons. More specifically, during the previous four weeks, there is a significant reduction of 0.17, 0.13, and 0.12 in the number of days their health: (1) interfered with such daily activities as household chores, employment, and schooling, (2) prevented them from undertaking such activities, and (3) caused them to stay in bed. These changes amount to a 22, 20, and 25 percent reduction from the levels in control villages.

The poor's stock of poultry increases between September 1997 and November 1998, and is stable later on. This suggests that the poor are transferring part of their current higher income to the future. Their consumption out of their stock of corn increases by 70 grams per adult per month, for a value of 0.38 pesos.

Table 9—Investments: Costs of Agricultural Production and Animals

	November 1998		May 1999	
	Level	Probability	Level	Probability
Panel A: Agricultural expenditures—Costs				
ITE	−1.947	−0.0004	−2.309	0.007
	[1.819]	[0.0275]	[3.506]	[0.029]
Observations	4,381	4,784	4,080	4,119
ATE	0.618	0.051	0.311	0.008
	[0.358]*	[0.028]*	[0.526]	[0.026]
Observations	10,408	11,223	10,096	10,197
Panel B: Animals—Purchases				
ITE	0.215	0.01	0.019	0.008
	[0.117]*	[0.008]	[0.078]	[0.008]
Observations	4,854	5,263	4,387	4,431
ATE	−0.021	0.006	0.042	0.015
	[0.034]	[0.005]	[0.022]*	[0.005]***
Observations	11,671	12,499	10,915	11,025

Notes: Monthly pesos per adult equivalent at November 1998 prices. Standard errors in brackets clustered at the village level. Treatment effects on the levels estimated by OLS. Probit estimates for the probability. The results are unchanged if we add conditioning variables.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We also investigate whether poor and nonpoor villagers are changing their investment behavior, although it is difficult to empirically distinguish savings from investment. For example, the poor's purchase of livestock may be for investment purposes, as animals are productive assets both for the sale of meat, cheese, and eggs, and for farming.

Table 9 tests for differences in the likelihood and the value of agricultural-related expenditures (e.g., to purchase seeds, fertilizers, and machinery) and purchases of animals. The evidence for the ineligibles is not conclusive. While we find an increase in the purchase of animals in November 1998, worth 0.22 pesos, we know their stock of animals is decreasing. Therefore, probably they are both buying and selling more animals, as well as consuming part of their livestock, since Table 6 showed no change in net sales.

For the eligibles, we find evidence of increased investment, consistent with Gertler, Martinez, and Rubio-Codina (2006). The likelihood of having agriculture-related expenditures increases by 5 percentage points in November 1998, i.e., by about 9 percent, and the overall level of these costs rises by 0.6 pesos per adult equivalent, or 15 percent. Their purchase of animals also increases in May 1999: its likelihood is 1.5 percentage points, or 62 percent higher, while its overall level rises by 0.04 pesos per adult equivalent, i.e., by approximately 46 percent. This is consistent with our previous findings that the program may be increasing the stock of animals for eligible households.

VI. Results: Internal Consistency and Implications

The magnitudes of the estimated effects are consistent: in May 1999 the ITE on monthly consumption per adult equivalent is 19 pesos, financed through a 10 peso increase in loans, a likely increase in transfers, and through the consumption of part of the stock of grains. In November 1999 the ITE on consumption is 17 pesos, financed through a 4 peso increase in monetary transfers, and a change in loans of unknown size. If the increase in loans and transfers is roughly constant in 1999, then ineligible households in treatment villages receive 14 extra pesos overall.

As a further check, we compare the magnitude of the indirect effects on loans and transfers with the Progresa grant size. While the villages are not closed economies, we expect the bulk of the effect to operate through changes at the village level. The average monthly transfer for the poor is 200 pesos per household, of which 88 percent is consumed (Gertler, Martinez, and Rubio-Codina 2006).[19] Therefore, the average Progresa cash available to each poor household for savings, transfers, and loans to the nonpoor is about 24 pesos per month. Given that there are 2.5 times as many poor as nonpoor, this amounts to 60 pesos for each nonpoor household. This magnitude is consistent with the estimated 50 pesos and 20 pesos that each nonpoor household receives in May and November 1999.[20, 21]

Our findings imply that failing to consider these indirect effects would underestimate the true average treatment effect on consumption for the treated villages. Consider the following back-of-the-envelope calculation of the benefit of the program for its first 20 months of implementation, i.e., between March 1998 and November 1999, using November 1998 prices. Assume that the estimated effects are stable in months preceding the observation (e.g., what we estimate for November 1998 holds for the previous 8 months, while estimates for May 1999 and November 1999 hold for the previous 6 months). For every 200 pesos transferred each month, the recipient consumes 176 pesos (Gertler, Martinez, and Rubio-Codina 2006). The 20-month ATE on consumption for the eligibles is, therefore, $176 \times 20 = 3{,}520$ pesos. Consumption for the ineligibles increases by 95 and 85 pesos per household per month in May and November 1999 (19 and 17 pesos multiplied by 5, the number of adult equivalents per household). Given that there are 2.5 times as many poor as nonpoor, this amounts to 38 and 34 pesos for every 200 pesos transferred, resulting in an extra increase in consumption of $(38 + 34) \times 6 = 432$ pesos. This is the 20-month ITE. Thus, for every 100 pesos transferred by Progresa, nonpoor consumption increases by about 11 pesos. Considering eligible households only, there is an average treatment effect of 3,520 pesos out of a transfer of 4,000 pesos. Including the ineligibles increases the average treatment effect by 432 additional pesos. Therefore, failure to consider the effect on the ineligibles would result in a 12 percent underestimate of the average treatment effect on consumption.[22]

The finding that the nonpoor in treated villages are affected by the program has implications for the design of future experiments: since the entire village is affected, directly or indirectly, by the treatment, it is essential to randomize at the village level, as occurred for the evaluation of Progresa. The common practice of selecting the treatment and control groups from the same community would have two shortcomings. First, it would bias the estimates of the treatment on the treated effect if the control group indirectly benefits from the program. Second, by not estimating these indirect treatment effects, it would fail to capture the full policy impact. In a similar setting to the one considered here, this would result in a double underestimation of the treatment effect.

[19] Total income of eligible households including Progresa transfers significantly increases by approximately 230 pesos per month per household.

[20] We obtained household-level estimates of loans and transfers by multiplying the estimated ITEs from Table 4 by five, the average number of adult equivalents in nonpoor households.

[21] The lower bound to what each household likely receives on average may actually be 50 and 20 pesos, as we do not observe loans and transfers at the same time.

[22] This back-of-the-envelope calculation does not consider the treatment effect on reclassified households.

VII. Conclusions

Using the unique design of the experimental data for the evaluation of Progresa, we show that the program benefits ineligible households that live in treatment villages by increasing their food consumption level by about 10 percent, approximately half the size of the increase in food consumption for eligible households. This consumption increase is financed through higher loans and transfers from family and friends, and through a reduction in savings. These results show how a positive income shock for a group of households benefits the entire village, consistent with our knowledge of informal credit and insurance markets in developing countries.

This type of program has positive indirect effects for the entire set of villages in which it is implemented, rather than for treated households only. These effects are large, and, if neglected, result in a 12 percent underestimate of the average treatment effect of consumption for the treated villages. This finding has implications for the design of experiments: if the treatment affects the entire village, it is essential to randomize at the village level, as occurred for the evaluation of Progresa.

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