INSURANCE AND INVESTMENT IN FAMILY NETWORKS*

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Abstract

We study how family networks affect informal insurance and investment in poor villages in rural Mexico. Related (or "connected") households can achieve higher insurance and investment than unrelated (or "isolated") households if the family network eases commitment and information issues. We find that: i. households share risk with their relatives almost to Pareto-efficient levels, but do not share risk with the isolated; ii. the connected disinvest less when hit by health shocks and invest more in human capital when offered a partial subsidy for schooling, compared with the isolated; iii. the connected have a smoother consumption and a bigger long-term increase in consumption, income, and investment than the isolated.

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1 Introduction

Poor households in developing countries face high income volatility but have limited access to formal insurance and credit, resorting to informal arrangements between groups of households. These resource-sharing networks require easy access to information on its members and effective means to reduce commitment problems. Networks that are small, (e.g. Genicot and Ray 2003, Ambrus, Mobius, and Szeidl, 2009)), have members who trust or care for each other (e.g. Altonji, Ayashi, and Kotlikoff, 1992; Foster and Rosenzweig, 2001, Karlan, Mobius, Rosenblat, and Szeidl, 2009), and can punish reneging members can achieve high levels of insurance (e.g. Deweerdt and Dercon, 2006).

The extended family may be an important resource-sharing institution (Fafchamps and Lund, 2003), since its family members know each other well, care for each other, and are able to monitor and punish deviating behavior by imposing sanctions (e.g. La Ferrara 2003). Altruism and evolutionary biology (e.g. the need to transmit and protect one's genes) may contribute to the enforcement of insurance agreements.

Besides providing insurance, the extended family may favor investment through three different channels. First, because there is a link between insurance and investment. Poor households may be willing to engage in costly activities to ensure their consumption never falls below subsistence levels, including reducing high-return investments. This need to disinvest to face short-term income fluctuations might have long-term negative consequences. For example, a household that is forced to withdraw its children from school and deplete its assets to cope with a negative income shock is deemed to live in permanent poverty. Conversely, a household that smoothes consumption by sharing resources within the extended family may not need to disinvest as much when hit by a negative shock.[1]

Second, the extended family may act as a shareholder if there are non-convexities in the investment good. In this case, a group of relatives may pool resources to finance one investment, sharing the returns. Third, the availability of insurance may enable agents to undertake riskier, more profitable investment (Arrow 1971; Obstfeld 1994).

Since informal institutions may both provide insurance and favor investment, one should look at both consumption smoothing and investment to assess the importance of extended families in the presence of imperfect capital and insurance markets. To do that, we begin by modelling an endowment economy where households can make costly transfers to each other

[1] On the other hand, uninsured households may invest more than insured households when they have a positive shock, for example because they don't have to share their resources with others.

to smooth consumption. The transaction cost is a reduced form term for all information and enforcement problems that may limit the degree of insurance. The lower the cost, the higher the degree of insurance against idiosyncratic risk, the smaller the consumption variance.

We then allow households to invest in a risk-free asset with positive returns. Aggregate investment is positively related to the level of insurance. Moreover, aggregate investment may be also higher when households can make costless transfers to each other. In this case, the resource-sharing network can pool resources to finance an investment that a single household is unable to undertake. As transaction costs are likely lower for relatives than non-relatives, members of extended families (or "connected" households) are likely better insured and able to invest more than unrelated (or "isolated") households.

We look at a set of poor villages in rural Mexico, using the experimental data collected for the evaluation of Progresa, Mexico's flagship cash transfer program, as well as subsequent waves after the experiment ended. This is a population with a high need for insurance but no access to formal credit and insurance markets, as typical of developing countries.

Our analysis exploits three unique features of the data. First, we have a census of 506 villages in rural Mexico. This is a panel of more than 20,000 households interviewed 8 times between 1997 and 2003, providing information on consumption, income, and health shocks, as well as other socio-demographic characteristics. Second, we can identify the extended family in each village by matching household heads' and spouses' last names, exploiting the two-surname Hispanic convention. Third, we observe an exogenous income variation: between 1998 and 1999 Progresa is offered only to a random group of 320 villages, in which poor households are eligible for sizeable cash transfers, while no public transfer is offered in the control villages.

We have the following key findings. First, connected households - about 80% of the village residents - look remarkably similar to isolated households. The few differences seem related to life cycle and cohort effects, rather than to different needs and preferences for insurance.

Second, our data suggest that connected households share risk within their family network but not with isolated households. When eligible households receive the Progresa grants, their ineligible relatives consume more, while ineligibles who have no eligible relative (or no relative at all) in the village do not.[2] Consumption for the ineligibles is a positive function of the size of the income shock per network members, i.e. the more money is injected in the network, the

[2] We also show that insurance is related the "connectedness" of the households, rather than to other characteristics of the households. That is, insurance does not seem related to ethnicity or land ownership, and these variables are not associated with higher degrees of insurance over and above the one associated with connectedness.

more its members will benefit, regardless of which household receives the extra income. On average, for each extra dollar received by an eligible individual, her non-durable consumption increases by 54 cents and the consumption of her ineligible relatives increases by 13 cents. The consumption of eligible households who have no ineligible relatives increases by 69 cents for each dollar received. The longitudinal coefficients of variation of consumption is significantly smaller for connected than isolated households.

Third, similar shocks have different effects on human capital investments for connected and isolated households. Negative health shocks decrease school enrollment and increase child labor more for the isolated than for the connected. Conversely, when offered a partial subsidy for schooling, only connected households increase school enrollment.

Fourth, while similar at baseline, investment, income, and consumption, increase more for the connected than for the isolated between 1997 and 2003, the first and last years in our data set. This preliminary evidence is consistent with our hypothesis that the extended family, by providing insurance, enables its members to undertake more investment, increasing their long-term income and consumption.

Understanding whether and to what extent members of extended family networks can share resources and how this affects insurance and investment has important implications for the design of optimal policies. For example, if the extended family is an important risk-sharing institution, the isolated may be especially vulnerable to idiosyncratic risk, given that the family structure changes slowly over time. In that case, the policy maker may want to provide insurance targeting the isolated first.

In our specific case, failure to consider that households share resources within the extended family biases the estimates of the treatment effects. For connected households, the effect on consumption is higher than its effect on the treated (as consumption increases both for the eligibles and the ineligibles), while the effect on school enrollment of 11 to 16 year old children is lower than its effect on the treated (as some ineligibles decrease their school enrollment to favor enrollment of eligible children, whose schooling is partly subsidized).

For isolated households, the program fails to have an effect on school enrollment for 11 to 16 year old children (the age at which children typically drop out of school).

To conclude, our work helps shed light on the role of informal institutions as means to overcome market imperfections in developing countries. While the existence of family-based institutions may be ridden with inefficiencies and hinder economic growth in some cases, our results suggest that, in the appropriate setting, they may help overcome market imperfections, increasing insurance and human capital investment, which may reduce poverty in the long run.

The paper has the following structure: Section 2 sketches a simple resource-sharing model with investment to derive the main testable hypotheses. Section 3 describes the data, the creation of family networks and their characteristics. Section 4 shows that risk-sharing occurs only within the extended family, and Section 5 that consumption is smoother for the connected and families are almost fully insured against idiosyncratic risk. Section 6 documents that investment is affected differently by income shocks for the isolated and the connected. Section 7 concludes.

2 A model of partial insurance and investment

The purpose of this section is to sketch a simple model of risk sharing and investment to understand under what conditions members of an extended family achieve a higher level of insurance than non-members, and in which case the presence of the extended family favors investment.

2.1 Partial insurance

Consider a simple exchange economy with no storage technology, no leisure, and two infinitely-lived, risk-averse households, $h \in \{1, 2\}$, with have instantaneous utility function $u_h = (1 - \delta)\ln(c_h)$ and a rate of inter-temporal preference $\delta < 1$. There are s finite states of the world, each state occurring with a probability $\pi(s)$, with $\sum_s \pi(s) = 1$. Households receive an exogenous endowment, $y_h^t(s)$, and then choose how much to transfer to each other to maximize their discounted expected utility over the perishable composite consumption good, c. Transferring resources is costly and may reduce the amount of informal insurance (as in Schulhofer-Wohl, 2008). This iceberg-type transaction cost, α, is such that part of the transferred resources is lost in the transfer. Thus, a transfer d to household 1 requires $(1 + \alpha)d$ transfer from household 2. This transaction cost captures all the potential reasons why full insurance may not be achieved - e.g. imperfect information or enforcement.

The social planner maximizes a weighted average of the households' utility functions. Assume for simplicity that the households have identical weights. Consider the case in which household 2 makes a transfer to household 1, that is $y_2(s) > y_1(s)$. The maximization problem for the social planner is (omitting the time subscript):

$$
\begin{aligned}
& max_{c_1,c_2,d} U = \sum_s \pi(s) \left[\ln c_1(s) + \ln c_2(s)\right] \\
& s.t. \\
& c_1(s) = y_1(s) + d(s) \\
& c_2(s) = y_2(s) - (1+\alpha)d(s) \\
& y_1(s) + y_2(s) = Y(s) + \alpha d(s) \\
& c_1(s), c_2(s) > 0; d(s) \geq 0
\end{aligned}
$$

From the first-order conditions we can derive the optimal consumption levels in each state. These are:

$$c_1^*(s) = \frac{1}{2(1+\alpha)}[Y(s) + \alpha y_1] \tag{1}$$

$$c_2^*(s) = \frac{1}{2}[(1+\alpha)Y(s) - \alpha y_2] \tag{2}$$

$$d^*(s) = \frac{y_2 - (1+\alpha)y_1}{2(1+\alpha)} \tag{3}$$

$$\tag{4}$$

where Y(s) is the sum of the endowments. The amount of insurance depends negatively on α, and is highest when $\alpha = 0$, in which case households are fully insured against idiosyncratic risk. The the longitudinal variance of consumption grows with α. As long as α does not exceed a certain threshold, there is some insurance: both households' consumption increases in response to a higher income Δy_2, but this increase in consumption is smaller than with full insurance and grows inversely with transaction costs, i.e. $\Delta c_1^* = \frac{1}{2(1+\alpha)}\Delta y_2$.

In autarky, i.e. when α_h is "large", $d^*(s) = 0$, each household consumes one's own endowment, and the longitudinal variance of one's consumption equals one's income variance. An increase in household 2's income Δy_2 results in an increase in her consumption for the same amount, $\Delta c_2^* = \Delta y_2$ and in no change in household 1's consumption.

Suppose transaction costs are lower among related, or connected (K) than unrelated, or isolated (S) households and isolated households' costs are sufficiently large to discourage any risk-sharing. This yields our first set of testable hypotheses. Consider two pairs of households,

h and j, which are related, and f and l, which are unrelated.

$$[HP1a]: \quad \frac{\partial c_h^K}{\partial y_j^K} > 0, \quad but \quad \frac{\partial c_f^S}{\partial y_f^S} = 0$$
$$[HP1b]: \quad Var(c^S) > Var(c^K).$$

That is, an exogenous shock to a household's income affects the consumption of its relatives, but not of its non-relatives (as only the former group shares risk). If isolated households do not share risk in the village, their longitudinal consumption variance, $Var(c^S)$, is larger than the longitudinal consumption variance for the connected, $Var(c^K)$. The first of these two hypotheses is very general and holds in the presence of heterogeneity in each o the model parameters (e.g. different income processes, inter-temporal time preferences, and degrees of risk aversion), as long as the incomes are not perfectly positively correlated. The second hypothesis, on the other hand, is true only if connected and isolated households have the same income process and time and risk preferences. We will discuss this in the empirical section.

2.2 Resource sharing and investment

The presence of an extended family network likely affects investment, through different channels. First, insurance and investment are related. That is, the existence of insurance stabilizes consumption, enabling households to invest more when hit by negative shock (but less when hit by a positive shock because they have to share it within their network). Second, connected households may invest more than if they had no family networks because their relatives may lend them money or become share-holders in the investment. This is especially important in the case of lumpy investment, which credit-constrained households cannot finance by themselves. Third, the availability of insurance may enable households to undertake riskier, more profitable investment (Arrow 1971; Obstfeld 1994). Therefore, to assess the value of the extended family as a resource-sharing institution we should consider its effect on both insurance and investment. We consider the first two cases only, as the third one has been sufficiently explored already.

Consider a two-period version of the previous model in which we add the possibility to invest. The investment, I, has gross returns, which are certain, given by the function $f(I)$, with $f'(.) > 0, f'' \leq 0$.[3] The central planner maximizes the following objective function:

[3] We assume the return is certain and observable for simplicity. In unreported results we considered the cases in which the investment is fully or partially hidden. The scope for investment is higher for insured households

$$
\begin{aligned}
&\max U \equiv \sum_{h=1}^{2}\sum_{t=1}^{2}\beta^{t-1}\ln c_h^t \\
&s.t. \\
&c_1^1 = y_1^1 + d^1 - I_1 \\
&c_2^1 = y_2^1 - (1+\alpha)d^1 - I_2 \\
&c_1^2 = y_1^2 - (1+\alpha)d^2 + f(I_1) \\
&c_2^2 = y_2^2 + d^2 + f(I_2) \\
&d^{1,2} \geq 0.
\end{aligned}
$$

As before, we model $y_1^1 < y_2^1$ and household 1 as having a low draw and receiving a transfer from household 2. The planner chooses the optimal transfers and investments given the returns, transaction costs, and endowments.

Assume the investment has decreasing marginal returns, e.g. $f(I_h) = I_h^{0.5}$ with $I_{1,2} \in (0,1)$, and no non-convexities. Further, consider only the extreme cases in which $\alpha = 0$ and α sufficiently large that there are no transfers, that is the households are in autarky and consume and invest their own resources only.[4] We show the optimal levels of consumption and investment in the Appendix.

With $\alpha = 0$, each agent chooses the same optimal consumption and investment because they have the same Pareto weights and face the same return to the investment. Investment for each agent is a positive function of *both* households' period 1 endowments:

$$
\frac{\partial I_h^{FI}}{\partial y_h^1} > 0, \quad \frac{\partial I_h^{FI}}{\partial y_j^1} > 0, j \neq h. \tag{5}
$$

In autarky, households 1 and 2 have different consumption and investment levels because their initial endowments differ and, unlike before, investment depends on *own* income only.

$$
\frac{\partial I_h^{1AUT}}{\partial y_h^1} > 0, \quad \frac{\partial I_h^{AUT}}{\partial y_j^1} = 0;
$$

when their investment is hidden.

[4]The model has an analytical solutions for these two cases. We simulate cases when $\alpha > 0$ and aggregate resources are constant in the two periods, and find that the level of investment is higher when the endowments are not too different.

Comparing the two sets of results shows that investment is less sensitive to changes in own income when $\alpha = 0$:

$$\frac{\partial I_h^{AUT}}{\partial y_h} > \frac{\partial I_h^{FI}}{\partial y_h}.$$

That is, while households that can share resources do not have to disinvest as much as autarkic households when hit by a negative idiosyncratic shock because their shock is (at least partly) insured away, on the other hand they cannot invest as much when hit by a positive shock, as they have to share it with their network.[5]

Lastly, we can show that, as long as X Y Z hold, aggregate investment is higher when there are no transaction costs than when the households are in autarky.

$$2 * I^{FI} - \left(I_1^A UT + I_2^A UT\right) \geq 0.$$

Another mechanism through which the extended family favors investment is in the presence of non-convexities. With lumpy investment, aggregate investment may be higher if households can share resources than if they do not. This occurs when there are borrowing constraints and the individual household in unable finance the investment by itself. That is, if the household is close to subsistence, foregoing some current consumption to invest is very costly, even if the return to the investment is high. However, if the household has a resource-sharing relative, and if the return to the investment, which the two households share in the second period, is sufficiently high, the relative may make a transfer to the first household in period 1, which enables the first household to invest. Both households consume more in the second period because the relative has become a shareholder to the investment.[6] In sum, when investment is lumpy, a marginal increase in either one's own or one's relative's endowment in the first period may increase investment for households who make transfers to each other but not for autarkic households. Therefore, aggregate investment may be higher for sharing than for autarkic households. We illustrate this case in the Appendix.

The two investment models we present here, with and without non-convexities, are quite different. The first one is an income pooling model in which related households share their endowments in order to maximize the returns to the joint investments, as well as smooth

[5] One can think of Progresa as the sum of an unconditional transfer and a partial subsidy to schooling. A model with a partial subsidy generates the same theoretical predictions as the ones discussed here. We will return to this in the empirical section.

[6] This is equivalent to having the relative make a loan with a sufficiently high interest rate to the first household.

idiosyncratic risk. The mechanism behind the model with lumpy investment departs from the classical insurance models. Here one of the two related households is acting as a shareholder, providing part of the capital to undertake the investment and then receiving part of the profit. In this case, both households are sacrificing a large part of current consumption in favor of future consumption. An alternative for the investing household would be to form a link with a household with a similar income process (in our case one with a high initial income draw, followed by a lower one). In this sense, there is a trade off between the insurance and the investment motive to form resource-sharing networks. We believe the former prevails for poor households in developing countries, for which the cost of a negative shock is potentially very high.

Suppose transaction costs are zero among connected (K) households and sufficiently high for isolated (O) households that they are in autarky. This yields our second set of testable hypotheses. Consider two pairs of households, h and j, which are connected, and f and l, which are isolated.

$$
\begin{aligned}
[HP2a]: &\quad \frac{\partial I_h^O}{\partial y_h^O} > \frac{\partial I_h^K}{\partial y_h^K}, \quad if \partial y < 0 \\
[HP2b]: &\quad \frac{\partial I_h^O}{\partial y_h^O} < \frac{\partial I_h^K}{\partial y_h^K}, \quad if \partial y > 0 \quad and \quad I \quad lumpy \\
[HP2c]: &\quad 2 * I^{FI} \geq \left(I_1^A UT + I_2^A UT \right)
\end{aligned}
$$

That is, investment is more sensitive to negative shocks in own income for the isolated than for the connected ($HP2a$); investment is more sensitive to positive shocks in own income for the connected than for the isolated if the presence of lumpiness in investment is sufficiently important to prevent autarkic households to undertake investments ($HP2b$); if the first two hypotheses hold, then aggregate investment may be higher for the connected than for the isolated ($HP2c$). We will explain how to test hypotheses $HP1a$ to $HP2c$ after describing the data.

3 Data characteristics

Our data have three important characteristics. First, they provide a complete census of 506 poor rural villages from seven states in central Mexico.[7] The data are longitudinal, with an initial wave collected in October 1997 and then approximately every 6 months until November

[7] Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San-Luis Potosi and Veracruz; mostly in central Mexico.

2000, and lastly in November 2003. We observe our key variables - consumption, illness of household head, livestock ownership, and school enrollment - 5 times between November 1998 and 2003 (6 including pre-program expenditure, collected in March 1998, and schooling, further observed in October 1997). We have complete data for about 22,500 households per wave up to November 1998, about 20,000 up to November 2000, and 19,000 in 2003. The attrition rate is similar in both treatment and control villages.[8]

Second, between May 1998 and November 1999, Progresa, a conditional cash transfer program which we describe below, is offered only in a random group of 320 villages. The remaining 186 villages receive their first grants only at the end of 1999. Therefore, we have three data waves, November 1998, May 1999, and November 1999, in which only a random subset of our sample of villages is treated.[9] We have information on current and potential eligibility, which depends on a time-invariant wealth measure, for all households in our 506 villages. Third, we observe people's last names, which we use to match related households, as we explain in the next section.

These three features enable us to group households according to their eligibility status, village of residence, and connectedness, as shown in Figure 2. This figure shows our data provide a partial-population experiment between November 1998 and 1999 (Moffitt, 2001), as the treatment is offered only to a subset of the villagers. Thus, for that time period we can measure the effect of Progresa on consumption of connected and isolated ineligible households.

3.1 Progresa and its evaluation

Progresa is an anti-poverty program that targets poor households in rural Mexico. The average grant is 200 pesos, equivalent to 22% of eligible households' monthly income (De Jainvry and Sadoulet, 2006) and to 25% of pre-program household's food consumption, which averages 160 pesos for adult (equivalent) in recipients households (Angelucci and De Giorgi, 2009). About 75% of households are classified as eligible based on their poverty status as computed in October 1997, although only a smaller share are treated initially.[10]

The grant is conditional on eligible members attending nutrition and health classes and having regular health checks. It has a fixed component of 100 *pesos* per month and a variable

[8] We provide further details of the the key variables and their availability in the Appendix.

[9] Some households in control control villages started to receive grants in November 1999. Thus for November 1999 we can only estimate lower bounds (in absolute value) to the true treatment effects.

[10] The initial allocations of households between eligible and ineligible was revised just before the program roll-out; however, in the first year of the program most of the re-classified households, typically the elderly, did not receive any grant for administrative reasons. We exclude these households from our analysis.

component conditional on children attending classes between 3rd and 9th grade. These scholarships vary between 140 and 510 *pesos* per child, increase with school grade and are larger for girls than for boys for grades 7 to 9. The grant is capped at 625 *pesos* per month (all these values are at November 1998 prices).

While nominally conditional, a substantial component of the grant is *de facto* unconditional. This is because pre-program enrollment rates up to 6th grade (corresponding to primary school) are higher than 90% and the health checks are infrequent for most eligible persons (e.g. they are annual for adults). This part of the grant has a pure income effect for its recipients.

Conversely, pre-program attendance to grades 7 to 9 (corresponding to secondary school) is about 65%. That is, the grant conditionality is actually binding for households whose eligible secondary school-age children would have not gone to school in the absence of the program. Some of these households may incur a net financial loss from sending their children to school despite receiving the program transfer, because the secondary school grant amounts to only about two thirds of full-time child wage (Schultz, 2004). For these families, the grant is a partial subsidy.

About one third of the eligible households have no child in the subsidized year/grades in November 1998 and receive only the fixed grant component. Out of the remaining two thirds that are entitled to a larger grant, most families have some primary school eligible children - about 87% compared to 57% of families with at least one secondary school child. On average, 30% of the total potential grant is associated with scholarships for grades 3 to 6.[11]

The partial grant conditionality and its relatively low monetary value, compared to the opportunity cost of secondary school attendance, suggests that the program may not provide some eligible families with enough resources to increase secondary school attendance, unless the unconditional component is large. This distinction is going to be important to assess how the program eligibility differentially affects secondary school attendance for connected and isolated households.

[11] We compute potential grants by looking at highest grade completed in the 1997-1998 academic year and assuming that all children enrolled in school grade x in the academic year t will progress to school grade $x+1$ the following year. That is, we assume a zero retention grade, while it is in fact positive. We use potential rather than actual grade completion because we want to compute a measure of potential grant that is exogenous to the program existence. Eligible households in treatment and control villages likely have different incentives to fail school grades.

3.2 Village characteristics

The villages in our sample are small: their average and median household numbers are 51 and 46.

Agriculture is the main activity and there is hardly any crop diversification: in October 1997 corn is the main (and often sole) crop in 88% of villages.

Income is volatile: the longitudinal coefficient of variation of income (standard deviation divided by mean) is approximately 0.70. The lack of crop diversification and income volatility suggest a high need for insurance. However, there are hardly any formal credit and insurance institutions. In November 1998, fewer than 1% of villages have credit or consumption cooperatives, and fewer than 3% have NGO's or production associations.

Despite the lack of formal insurance an credit markets, consumption is much more stable than income: the longitudinal coefficient of variation of consumption is half as large as the coefficient of variation of income, suggesting that households engage in informal risk-sharing activities.

Indeed, there is a high, yet not efficient degree of insurance against idiosyncratic income fluctuations at the village level. When we regress household consumption growth on the growth of village aggregate resources and on household income (as in, e.g. Mace 1991 and Townsend 1994), the coefficient of income is small, about 0.025, but significantly different from zero. Thus, it is worth exploring whether specific groups of households, in our case members of extended families, are better insured than others.

Mobility is low. In November 1998 and 1999 only 5% of the total number of individuals had left the household in the previous 5 years, 20% of whom lives in the same village as the household of origin. A consequence of this low mobility is that most families are related, as we will describe below.

3.3 Family networks in rural Mexico

3.3.1 Network creation

To construct extended family links between households in the same village we exploit information on surnames provided in the third wave of data. Mexicans use *two* surnames - the first is inherited from the father and the second from the mother. For example, former Mexican president Vicente Fox Quesada would be identified by his given name (Vicente), his father's (Fox) and his mother's (Quesada) paternal names. Hence a couple-headed household has four associated surnames.

We use these data to create within-village family networks, starting from each household head and spouse's parents, offspring, and siblings, and proceeding to more distant relatives (grand-parents, aunts and uncles, etc.). All family links are defined across households on the basis of *two* surname matches. We match related households based on their set of surnames as well as age gaps (e.g. two households cannot be "father-son" if the age gap between the parent and the offspring is only 10 years). We show the assumed links between different households in Figure 1.

The average family network is composed by 7.8 households. Networks are unlikely to span across more than three generations. About 50% of all networks (and two thirds of networks of more than two households) are a mix of eligible and ineligible households.

We believe our matching algorithm to be reliable. Even if they were not, and we were erroneously labeling connected households as isolated and viceversa, this would result in under-estimating our parameters of interest, as long as the mis-match is random.

We suspect that some single-headed households are mis-classified. For example, it is possible to classify a single-headed households that is in fact connected as isolated because we cannot observe the extended family links of the missing spouse. Since single-headed households are older and poorer than couple-headed ones, including them when comparing outcomes for connected and isolated households would bias our estimates and it is not obvious whether the bias would be upward or downward. Therefore, we deal with single-headed households in the following way. When our tests compare connected and isolated households, we drop the single-headed. However, when we compare outcomes *within* connected households we include both the couple-headed and the single-headed. We discuss the algorithm reliability and the rationale for this choice in the Appendix.

3.3.2 Correlates of Connectedness

Table 1 shows the means of key demographic and socioeconomic variables from the 1997 data. Once we drop single-headed households and compare both demographic and economic outcomes, which we do in the first 5 columns, connected and isolated households appear remarkably similar.

The main difference between the two groups is that isolated households heads are 2.5 years older, have fewer young children, and a 9% higher share of illiteracy than connected heads. The correlation between the age of the head and spouse of a household and the likelihood that their parents, adult children and siblings live in the same village in a separate household is somewhat mechanical. Angelucci et al. (2007) show that the rate at which a household head "loses" links as he ages (i.e. parents passing away) is faster than the rate at which he "acquires" new links (through children being born). This makes households with older heads more likely to be isolated. Thus, these difference seem overall attributable to life cycle and cohort effects: isolated households are older, therefore have fewer infants and a lower education level.

Most of the other variables do not differ between the connected and isolated. In particular, income, food expenditure, and wealth index are almost identical, as well as the amount of land owned or used, adult and child labor, and most assets and livestock. We also performed a Pearson's chi squared test of the difference in the distribution of unemployment and occupation for all members of connected and isolated households at least 8 years old. [12] If land ownership and employment patterns may reflect risk preferences or need for insurance, as well as income processes, this evidence suggests isolated and connected households may have similar preferences and income processes, hence a similar need for resource sharing.

One exception to this pattern is that the isolated own 12.5% more poultry and are 6% and 9% less likely to own stoves and TV sets. Poultry is the most liquid type of livestock these households hold. Therefore, an interpretation of this finding consistent with our conjecture that the connected are better insured than the isolated is that the latter group has to hold a larger stock of savings, as it cannot rely on informal risk-sharing as much as connected households (since the isolated have no relatives in the village with whom to share resources). The lower level of durable assets is also consistent with our theory. Since the isolated do not have access to resource sharing, they can invest less and their (durable) consumption is lower than the connected.

Since a number of the tests we run exploit the program randomization, the last three columns of Table 1 report the p-values of the differences in observables between households in treatment and control villages. We do that separately for the connected and the isolated.

Most of the variables do not have statistically different means in control and treatment villages, not surprising, given the randomization. A few notable exceptions for the isolated are the differences in the number of oxen and goats owned and the share of 11-16 year old children enrolled - all of which are lower in control villages. For the connected, the number of oxen owned is slightly lower in control villages. The share of households potentially eligible for Progresa

[12] The main labor force categories are unemployed (68%), daily worker (15%), non-agricultural manual worker (4.5%), self-employed without (4.3%) and with personnel (0.12%), working for a family business without pay (4.4%), and *ejidatario* (2.5%).

is also statistically different in control villages, unless we include the single-headed, in which case this difference is no longer significant at conventional significance levels. We address this issue by estimating treatment effects on these outcomes using difference-in-difference estimators. Moreover, we control for the set of variables that are unbalanced between treatment and control villages at baseline.

Comparing means for connected and isolated households is somewhat misleading if there is assortative matching. For example, all the wealthiest and poorest households may belong to distinct family network, but by aggregating all families we would not detect it. We test for assortative matching in wealth by computing the standard deviation of the wealth index used to determine program eligibility for each family network and each village. If there is positive (negative) assortative matching then the ratio of network over village standard deviation, $\frac{sd^n}{sd^v}$, would be less (more) than one. The computed ratios, available upon request, are centered around one, rejecting the hypothesis of assortative matching. This confirms the previous conclusion, namely that one cannot easily predict whether a household is connected or isolated by looking at its observable characteristics.

4 Do family networks share resources?

We test hypotheses $HP1a$ to establish whether the extended family is a resource-sharing institution in our villages and provide further evidence consistent with the notion that households share resources in the extended family.

4.1 Testing hypothesis HP1a

Consider two households, h and j. Hypothesis HP1a states that consumption of each of them is a function of *both* households' resources, that is, $\frac{\partial c_h^K}{\partial y_j^K} > 0$ only if the households share resources, absent any general equilibrium effect.

This hypothesis is normally hard to test without making some restrictive and often unrealistic assumptions, as it is difficult to find truly exogenous income variation. Our data, on the other and, benefit from the experimental income variation caused by Progresa. Therefore, we can exploit the large, observable, and exogenous income shock caused by the provision of Progresa grants to study whether households share resources with their relatives only or with the whole village under very general identification assumptions.

Define Y_{1h} and Y_{0h} as the potential outcomes for household h in treatment villages ($P_h = 1$)

in the presence and *in the absence* of the treatment, where the treatment is the existence of Progresa grants to eligible households ($N_h = 0$) in treatment villages ($P_h = 1$). We call the average effects of the program on 1) eligible households and 2) ineligible households ($N_h = 1$) living in treatment villages, Average Treatment Effect on the Eligibles (ATE) and Indirect Treatment Effect (ITE), and we define them as follows:

$$\begin{aligned} ATE(Y)^f &= E(Y_{1h}|P_h = 1, N_h = 0, f = 1) - E(Y_{0h}|P_h = 1, N_h = 0, f = 1) \\ ITE(Y)^f &= E(Y_{1h}|P_h = 1, N_h = 1, f = 1) - E(Y_{0h}|P_h = 1, N_h = 1, f = 1) \\ &\qquad f = \{K, I\} \end{aligned}$$

Under the assumption of random assignment and in the absence of program spillover effects to control villages, the expected value of the potential outcome in the absence of the treatment, Y_0, is the same in both treatment and control villages, i.e. $E(Y_{0i}|P_h = 1, N_h = j, f = 1) = E(Y_{0i}|P_h = 0, N_h = j, f = 1)$, for $j = \{0, 1\}$ and $f = \{K, O\}$. Therefore, the differences

$$ATE(Y)^f = E(Y_h|P_h = 1, N_h = 0, f = 1) - E(Y_h|P_h = 0, N_h = 0, f = 1) \tag{6}$$

$$ITE(Y)^f = E(Y_h|P_h = 1, N_h = 1, f = 1) - E(Y_h|P_h = 0, N_h = 1, f = 1) \tag{7}$$

identify the ATE and the ITE.

Testing hypothesis $HP1a$ consists of comparing ITEs for connected and isolated households. If the connected share risk only with family members and not with the isolated, only connected ineligible households increase consumption because of Progresa, i.e. $ITE(c)^K > 0$, but $ITE(c)^S = 0$, and their consumption increase is a positive function of available resources at the network level.

Although it has some unique features that distinguish it from other poverty alleviation programs, Progresa is just one of many programs targeting the poor in rural Mexico. The village residents are used to receiving government assistance of different forms.[13] Thus, we think of Progresa as one of the states of the world on which households write implicit contracts, rather than an entirely new occurrence.

We test whether the consumption ITEs are larger for connected than for isolated households

[13]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).

by estimating the following regression:

$$c_{ht} = \delta_0 + \delta_1 P_h + \delta_2 O_h + \delta_3 P_h * O_h + \delta_4 T_t + \delta_5 P_h * T_t + \delta_6 P_h * O_h * T_t + \delta_7 X_{ht} + u_{ht} \qquad (8)$$

where c stands for food consumption, P is a dummy for residents of Progresa ($P = 1$) and control ($P = 0$) villages, T is a time dummy for baseline ($T = 0$) and later data waves ($T = 1$), and I a dummy for isolated ($O = 1$) and connected ($O = 0$) households. X is a set of household and village characteristics measured at baseline and time dummies.[14]

We compute monthly food consumption per adult equivalent from seven-day recall data on 37 different food items. For each item, we know the quantity consumed and whether it was purchased, donated, and produced by the household.[15] In our sample, food consumption accounts for about 72% of total non-durable consumption.

The parameters δ_5 and $\delta_5 + \delta_6$ identify the ITEs for connected and isolated households. Our identification strategy is a triple-difference (comparing the treatment effects for the connected and the isolated using both the cross sectional and longitudinal variation in our data, as well as the randomization). Therefore, while being isolated or connected is to some extent a non-random event, we can control for additively-separable time-invariant and time-varying sources of heterogeneity.[16]

Table 2 presents the estimates of the treatment effects on monthly food consumption per adult equivalent in 1999 for ineligible households, grouped in different ways.[17] We group and compare households' treatment effects depending on whether: 1) they are connected or isolated (panel 1); 2) they have relatives eligible for Progresa or not (panel 2); the third panel has the same grouping as 2) but shows the ITE for households with at least 30% of eligible relatives. We estimates the ITEs by difference-in-difference and cluster the standard errors at the village level, the data sampling unit. We also report the number of unique households and the sample

[14] These variables, shown also in Table 1 are age, gender, and literacy of household head; number of household members in various age categories; household wealth index; livestock ownership; village poverty index; time and region dummies.

[15] We observe also non-food consumption, but choose to not use it because it is imprecisely measured: the recall period is longer - between one and six months - and there is a sizeable proportion of households with zero expenditure in several non-food items, as the purchase of those commodities is infrequent for indigent families. See Angelucci and De Giorgi (2009) for further details on the data creation.

[16] One empirical issue we face is whether to estimate the treatment effects on consumption by difference-in-difference or using only data from after the program start, since we observe only pre-program expenditures. We end up doing both. Since the results are qualitatively similar, we report the difference-in-difference estimates only.

[17] We drop the first wave after the beginning of the program, November 1998, because Angelucci and De Giorgi (2009) showed there are no significant ITEs a few months after the program starts. This is because Progresa had transferred very little money by November 1998.

size. Unlike for the first grouping, when we group households according to whether they have eligible relatives or not, their baseline characteristics are poorly balanced across treatment and control villages. Therefore, we are less confident of the reliability of the estimated treatment effects in panels 2 and 3.

Regardless of how we group the ineligibles, the indirect effect of Progresa on consumption is positive and significant only for those with eligible relatives, consistent with the model predictions. Food consumption increases significantly only for the connected (by 26 *pesos*) and not for the isolated, and only for households with eligible relatives (by 18 and 32 *pesos*), and not for households with no eligible relatives. Given an average counterfactual consumption level of about 200 *pesos*, consumption increases between 10 and 15%. The differences in ITEs is positive and significant in the three panels.

We experimented with using only a simple- and not a double-difference or with trimming the data in different ways. While the estimates of the ITEs vary somewhat across the different specifications, they all draw a consistent picture: among the ineligibles, only households with eligible relatives increase their food consumption.

There is an equivalent of assumption HP1a for investment; that is, investment for household h is a function of resources of both households h and j only if these two households share resources. However, we choose to focus on consumption to establish whether extended family members share resources with each other for several reasons. First, we believe that the main purpose for sharing resources is to smooth consumption; therefore, we expect the strongest effects for this variable. Second, consumption is measured more accurately than the other variables in our data. Third, while our model did not consider savings, the households in our sample are likely to save. The effect of a positive income shock on a relative's savings may be negative, as, since it is easier to borrow part of the relative's good income if hit by a negative shock, one can reduce its stock of savings. This would generate ambiguous predictions on assets like livestock, for example, which can be both investment and savings. Lastly, the increase in schooling for eligible children may actually crowd out ineligible children, for example if the increased class sizes sufficiently reduce school quality. We return to these two last points when we study the effect of income shocks on investment.

4.2 Further evidence

As further evidence in favor of relatives sharing resources, we test the implication of hypothesis $HP1A$ that the increase in consumption for household h is a positive function of the increase in income for its related household j, or, in other words, whether ineligibles' consumption is higher in networks that receive higher grants per capita, ceteris paribus. We consider only treated villages and regress household log-food consumption per adult equivalent on network log-grant per adult equivalent. Since the actual grant is endogenous, as its amount depends on whether eligible households comply with the program schooling and health requirements, we use potential grant as an instrument.[18]

Table 3 shows the relevant parameters. The IV estimates from its top panel show that the average grant elasticity of food consumption for the ineligibles is 0.13, that is, an extra dollar transferred to the eligible relatives increases ineligible consumption by 13 cents in the average network, and by 16 cents in networks in which at least 30% of the members are eligible. The estimates from the Table's middle panel are from a reduced-form regression of ineligible log-food consumption on potential rather than current log-grant in treated villages. The effects are positive and significant. The bottom panel of the Table shows the results from our validation exercise: ineligible households' consumption in control villages is not a function of the potential grant (i.e. of the number of potentially eligible children in the networks). Thus, the effects we estimated in treatment villages is caused by the program grants, rather than by a comparison of networks with different consumption level irrespective of the program.

Our final test consists of comparing the share of the grant consumed by connected eligibles with and without ineligible relatives. If eligible households share their grants with their ineligible relatives, then we would expect connected eligible households to consume a smaller fraction of their grant if they are related to some ineligibles, compared with connected eligibles who have no ineligible relatives.[19]

We estimate treatment effects on consumption for connected eligible households with and without ineligible relatives. We then compare these treatment effects with the grant size, computing the share of grant consumed for these two groups separately. As a further check, we compute consumption for their ineligible relatives as a fraction of the total network transfer. We present the results in Table 4. We estimate these effects first for food consumption only and then for all non-durable consumption, because, unlike the ineligibles, eligible households

[18] Besides the variables listed before, we control for number of eligible children in the network grouped by gender and primary versus secondary school level, share of eligible household in the network, and share of the network hit by adverse weather.

[19] The effect of Progresa on consumption depends on the grant size, the presence of ineligible households in the network and the Pareto weights. If the latter are proxied by relative wealth in the network, eligible households in an all-eligible family network should share a smaller part of their grant for two reasons: first, all network members are receiving the grant; second, the Pareto weights of eligible households are on average higher in an all-eligible network.

increase both food and non-food consumption in response to the program.

We find that eligible connected households increase their food and total consumption by 28 and 34 pesos per adult equivalent per month out of a 64 peso grant per network member (adult equivalent), if they are related to ineligible households. Eligible households that have only eligible relatives, on the other hand, increase their consumption by 31 and 38 pesos respectively, out of a 55 peso grant (these increases are roughly between 18 and 24% of the counterfactual consumption). Thus, connected eligible households consume a larger share of their transfer - 54% versus 69%, significant at the 99% level - if they have ineligible relatives with whom they supposedly share their extra income. These latter households increase their consumption by 13 cents for each dollar transferred to their eligible relatives.

4.3 Robustness checks

First, we check whether there is a differential effect on gifts and informal loans received by the ineligibles. Unlike the consumption data, the quality of the gift and loan data is questionable, as explained in Angelucci and De Giorgi (2009). Nevertheless, we consistently find that the ITE on gifts and loans is positive and significant only for the connected, and in particular for those with eligible relatives, and not for the other ineligibles. The magnitude of the estimates varies largely depending on how we trim the data, but it is never larger than the estimated effect on consumption.

Second, we test the hypothesis that the indirect effects on consumption depend on land ownership or ethnicity, rather than on extended family network. We find no positive and significant ITEs in either case. Moreover, we fail to find an effect for land owners or ethnic minority households over and above the effect of family network. We show the related evidence in the Appendix.

Third, while we use the program eligibility as the income shock to measure whether risk is shared within the extended family because the program transfers are big, observable, and random, households may perceive Progresa as a “special” occurrence, and apply different social norms to this temporary income shock. As a robustness check, therefore, we test whether households’ consumption is correlated to illness of related household heads. In line with our previous findings, we find that household i’s consumption is negatively correlated with the share of its related households with ill heads.

To conclude, an additional explanation for the observed increase in consumption is that the program may increase ineligible households’ income through increase of local wages or through its demand shock. However, Angelucci and De Giorgi (2009) show that there are no indirect treatment effect on labor and goods income and that prices do not change differentially between treatment and control villages. Moreover, if Progresa had also income effects we would expect consumption to increase also for isolated ineligibles, which is not the case.

5 Consumption smoothness for connected and isolated households

We have established that connected households share risk within the extended family, but not with isolated households. We now proceed to test hypothesis $HP1b$, comparing the longitudinal variance of food consumption for the connected an the isolated, which our model predicts to be smaller for the connected.

Unlike the previous test, which holds under very general cases, this second test holds only if connected and isolated households have similar income processes, as well as time and risk preferences. While we have no direct way to prove it, the evidence from our data Section suggests that isolated and connected households may have similar income processes and preferences.

If the presence of the extended family helps the connected invest more, then their future consumption will be higher, increasing consumption variance. To control for differential consumption paths, we compute the longitudinal coefficient of variation CV of consumption, dividing its standard deviation by its mean. Table 6 compares the log in food consumption CV, $lnCV(C)$, for connected and isolated households by regressing it on a dummy for isolated households, O.

Consistent with our hypothesis, consumption is 6% more volatile for the isolated than for the connected, regardless of whether we condition on the log CV of income.[20] While this difference in comparative consumption smoothness may not seem large, a small increase in its smoothness can cause a sizeable increase in welfare for households close to subsistence level (Chetty and Looney, 2006).

After having established that the connected 1) share risk within their extended family and 2) have a smoother consumption path than the isolated, it is important to establish whether the connected are fully insured against idiosyncratic risk. Consider the optimal consumption equation (1). Under the hypothesis that $\alpha = 0$, taking the log and the first difference yields a well-known relationship between the growth of individual and aggregate consumption for

[20] The coefficient of variation of food consumption is 2% significantly larger for the isolated when we drop single headed households.

household h at time t:

$$\Delta lnc_h^{t*} = \Delta ln\overline{Y_t}$$

We can regress the growth in household consumption over the growth in aggregate consumption and household income,

$$\Delta lnc_h^t = \beta_1 \Delta \overline{lnY_t} + \beta_2 \Delta lny_h^t + u_h^t \tag{9}$$

where Δlny_h^t is the endowment growth for household h at time t and $u_h^t = \Delta\epsilon_h^t$ is an error term derived from assuming a multiplicative error in the measurement of consumption, $c_h^t = c_h^{t*}e^{\epsilon_h^t}$. Under the null hypothesis that $\alpha = 0$ consumption growth does not depend on the growth of the idiosyncratic component of the endowment; therefore $\beta_2 = 0$.

Table 5 provides estimates of the coefficients of the growth in aggregate resources and household income from equation (9) for the connected.[21] We reject efficient risk sharing in the extended family when we estimate this equation by OLS (upper panel). However, while household consumption co-varies with own income, conditional on network resources, its coefficient is only 0.022. The idiosyncratic component has a small economic effect on the households. Indeed, if risk preferences are heterogenous it is likely that income is endogenous and its coefficient would then be overestimated (Schulhofer-Wohl 2008; Mazzocco and Saini 2008). Moreover, if income is measured with error, there is an additional source of bias. To address this latter issue we also estimate this equation by IV, using the second lag of total household income as instrument. In this case we cannot reject the null hypothesis that the extended network achieves full insurance. While we should interpret these results with caution, given that the IV standard errors are much larger than the OLS ones, the IV coefficient is also considerably smaller. In any case, these results confirm our conjecture that transaction costs among extended family members are rather small.[22]

6 Investment and family networks

While consumption is smoother for connected than isolated households, this relative smoothness may be an inadequate proxy for the value of being part of an extended family, as family

[21] We cannot estimate it for the isolated because we cannot compute aggregate resources for these households, since we do not know with whom they share risk.

[22] When we estimate equation (9) for the entire village by OLS, we also find a significant coefficient for income growth of 0.025. Similarly, the standard error increases when we estimate it by IV, although its point estimate remains close to 0.016.

networks affect investment as well. We pointed out how the presence of a resource-sharing extended family makes investment less sensitive to idiosyncratic income fluctuation, so that, for example, a household does not have to disinvest as much when hit by a negative shock because this shock is insured away. One exception to this general rule occurs in the presence of lumpy investment and credit constraints. In this, case, for a given positive shock the connected may invest more than the isolated, as the family network pools resources to partly finance an investment that a single household could not undertake. In sum, aggregate investment may be higher among the connected than the isolated.

This section tests whether investment for these two types of households responds differentially to income shocks. This helps us understand whether and to what extent 1) the investment models we discussed characterize the behavior of our households and 2) investment lumpiness and credit constraints are an important barrier to investment for isolated households. As a robustness check, we also test the model prediction that investment is a function of network, as well as own resources. Lastly, we consider the differential effect of the conditional and unconditional components of the Progresa grant.

6.1 Differential response to shocks

We now test hypotheses $HP2a$ and $HP2b$, that is, whether connected households are less sensitive to income shock, unless they are in the presence of lumpy investment, in which case the connected may be *more* sensitive to positive income shocks than the isolated:

$$[HP2a]: \quad \frac{\partial I_h^O}{\partial y_h^O} > \frac{\partial I_h^K}{\partial y_h^K}, \quad if \partial y < 0$$
$$[HP2b]: \quad \frac{\partial I_h^O}{\partial y_h^O} < \frac{\partial I_h^K}{\partial y_h^K}, \quad if \partial y > 0 \quad and \quad I \quad lumpy$$

As before, we need exogenous variations in income to see whether they affect investment differentially for the connected and the isolated. We exploit the exogenous income variation caused by Progresa for our measure of positive income shock. We use self-reported data on illness of the household head during the previous month as our measure of positive income shock. While the head's health status may not be random, unlike the program eligibility, it is uncorrelated with the household wealth index, once we control for the predetermined characteristics described in footnote 14. Moreover, the incidence of household head illness is not statistically different for connected and isolated households, as shown in the data section. In any case, what we really care about when comparing the effect of this shock for the connected and the isolated is that any non-randomness be the same for these two groups of households. The similar incidence of illness for the connected and the isolated suggests this may be the case.

We estimate the following equation:

$$\Delta Y_{ht} = \theta_0 + \theta_1 \Delta S_{ht} + \theta_2 K_h + \theta_3 \Delta S_{ht} K_h + \theta_4 X_h + \epsilon_{ht} \tag{10}$$

The variables Y, S, K, and X are the outcomes of interest (schooling, child labor, physical capital), the shock dummy (household head illness and program eligibility), the connected dummy, and the set of characteristics described in footnote 14. More specifically, we measure school enrollment as the share of 11-16 year old children attending school and child labor as the number of weekly days worked per child by 11-16 year old children (considering only households with at least one such child). Our measures of physical capital are livestock, counted as the number of animals owned, the size of land owned or cultivated, which we cannot separately measure in our data, and agricultural investment expenditures such as the purchase of seeds, pesticides, and fertilizers. This equation allows differential trends in the outcomes for isolated and connected households, as well as differential trends for households with different X values.[23] Since we establish that the family network is a relevant unit of analysis, from now on we cluster the standard errors at the network level.

The upper panel of Table 7 shows the differential responses of connected and isolated households to an illness of the household head, the primary breadwinner in the family, as well as their difference. Consistent with hypothesis $HP2a$, investment, in particular in human capital, is significantly more responsive to the health shock for the isolated than for the connected.

While enrollment of 11-16 years old decreases for all households, the drop is only 1 percentage point for the connected and not statistically significant, but more than four times as large and strongly significant for the isolated.[24] The changed patterns in school enrollment are consistent with a differential increase in child labor. The number of weekly workdays for the same set of children increases by 0.077 for the connected, and for 0.283 for the isolated, that is an increase in more than a day of work per month.

[23]We actually estimate the treatment effect of Progresa eligibility using the same equation as (4.1), with different outcomes.

[24]In unreported regressions we rejected the hypothesis that the drop in enrollment associated with illness of the household head is caused by child illness. We therefore rule out the possibility that we are picking up the effect of contagious diseases. We perform similar tests and reject this hypothesis in all the estimations involving direct and indirect effects of household head illness.

The composition of livestock also changes in response to the health shock, albeit not differentially for the connected and the isolated. However, unlike schooling, owning livestock serves a precautionary and consumption motive, as well as an investment one, so it is harder to interpret these results in light of our simple model. Our findings, nevertheless, shed light on the complex ways in which households respond to shocks. The stock of goats and pigs decreases (although it is imprecisely estimated), while the stock of poultry increases. The change away from swine and towards poultry probably suggests that households re-allocate their livestock portfolio towards more fungible, short-term, probably lower-yielding investments. For example, the return to swine ownership comes when the animal is slaughtered, and the largest profits are probably made from the sale of ham, lard, and sausages, rather than from the sale of the animal itself. Producing these goods involves substantial labor costs and requires specific skills, as well as physical strength. Conversely, poultry provides smaller but more uniform returns in the form of eggs, and maximizing the return on its sale does not involve large labor inputs. In addition, poultry is more easily traded or consumed than larger animals.

The size of land owned or cultivated also increases. However, while more land is used, there is no significant increase in the associated expenditures. Therefore, we deduce that the household is probably cultivating marginal land for subsistence purposes (e.g. the children who dropped out of school may tend to a little plot of land for household consumption).

The lower panel of Table 7 shows the differential responses of connected and isolated households to Progresa eligibility. The results for child schooling and labor are striking: while for the connected school enrollment increases by 7.7 percentage point and time spent working decreases by about a quarter of a day per week, school attendance and child labor do not change for the isolated, despite the fact these households are being offered money to send their children to school. Under this metric the program has positive effects only for the connected and no effect for the isolated.

While striking, these results are consistent with our model predictions. When investment is lumpy, as in the case of schooling, and the offered subsidy covers only part of its cost, it is possible that only connected households may be able to increase their schooling investment, as they can pool resources within the family network, unlike isolated households. Indeed, among the eligibles, the increase in school enrollment for 11 to 16 year old children is a positive function of the unconditional grant received by both the household itself and its entire family network (Angelucci *et al.*, 2007).

How do the isolated spend the program grant? Besides consuming more, these households increase their livestock and land ownership (or usage) more than the connected. The stock of poultry and land increase by 11 and 6.7 percentage points more for the isolated.

In sum, the response to Progresa's exogenous income increase is in favor of higher school attendance for the connected, consistent with hypothesis $HP2b$, and more oriented towards an increase in livestock for the isolated. Since the return to poultry ownership is likely lower than the return to secondary education - which in Mexico is about 70% higher than the return to primary school (López-Acevedo 2001) - these results suggest isolated and connected households respond differently to Progresa eligibility, with the isolated favoring investment in physical capital with short-term, low returns, and the connected increasing investment in human capital with long-term, high returns.

6.2 Aggregate investment

These findings suggest that aggregate investment may increase more for the connected than the isolated in the long run. This is consistent with hypothesis $HP2c$, which states that aggregate investment is higher under full insurance than in autarky (good approximations for the degree of risk-sharing achieved by connected and isolated households in the village). If investment causes long-lasting income increases, this will also be reflected in higher consumption. To test this hypothesis, we estimate the change in investment, consumption, and income for connected and isolated households between the first and last years in our data - 1997 and 2003.

$$Y_{ht} = \kappa_0 + \kappa_1 K_h + \kappa_2 T_t + \kappa_3 K_h * T_t + \kappa_4 X_h + u_{ht}$$

The subscript t refers to either 1997 or 2003. The variable Y is the outcome of interest (share of household members 11 and older with at least 9th grade education, livestock, dummies for machinery ownership, durable and non-durable consumption, labor income). These variables are measured at the monthly level. Income is computed per adult equivalent at November 1998 pesos, for durables and machinery we use dummies for whether the household owns any or not. The dummies K and T equal one for connected households and for 2003. We condition for all the predetermined household characteristics (X) that differ for these two groups, measured at baseline, as described in footnote 14.

Table 9 compares the difference in income, consumption, and investment levels for connected and isolated households between 1997 and 2003, conditional on 1997 levels. That is, we report the estimated change in outcomes for the connected ($\hat{\kappa_2} + \hat{\kappa_3}$), for the isolated ($\hat{\kappa_2}$), and their difference ($\hat{\kappa_3}$). The descriptive evidence is consistent with our conjecture: the change in investment - proxied by the share of household members with at least secondary education and by the stock of animals (which are both store of value, savings, and productive assets) and machinery is significantly higher for the connected than for the isolated. The higher stock of physical and human capital increases productivity for the connected, whose income increases more than for the isolated. The higher investment and income drive consumption up, which indeed is significantly higher for the connected, especially of durable assets.

6.3 Resource redistribution within the extended family

This section has two purposes. First, to perform a check that household investment is a function of their relatives' income shocks, as well as their own. Second, to understand how the conditional and unconditional components of the Progresa grant affect household behavior.

As explained, one can think of the fixed income support and the scholarships to grade 3 to 6 as an unconditional grant. Conversely, the scholarships to attend grades 7 to 9 can be thought of as a partial education subsidy for the children who would have worked full time in the absence of Progresa. That is, the program makes educating children entitled to the grant much cheaper than before, and cheaper than educating ineligible children. It is therefore possible that family networks with a mix of eligible and ineligible secondary school children may increase the enrollment of children whose education is subsidized and decrease the enrollment of ineligible children.

To estimate the effect of a health shock, we estimate the following equation for household h in network n at time t:

$$\Delta Y_{hnt} = \chi_0 + \chi_1 \Delta TS_{nt} + \chi_2 \Delta S_{ht} + \chi_3 \Delta NS_{nt} + \chi_4 X_h + \epsilon_{ht} \tag{11}$$

The variable Y represents the usual outcomes of interest (consumption and investment); the variable TS counts the number of ill household heads in the family network, excluding own illness, S, which is added as a separate control; the other controls are network size, NS, measured as the number of households in the extended family, and the usual set of controls, X, measured at baseline. We show the estimate of our parameter of interest, χ_1, in the top panel of Table 8.

To estimate the separate effects of the unconditional and conditional components of the Progresa grant, we group ineligible households depending on whether their eligible relatives receive more or less than 50% of their potential grant through scholarships for grades 3 to 6. If they do, the majority of the potential grant to which the family network is entitled is unconditional. If they do not, then the extended family likely has to change the time allocation of its children in order to receive a sizeable part of the grant. About 23% of ineligible households belong to networks in which at least half the potential grant is unconditional, according to the above definition. We create a dummy variable, G, that equals one if the potential grant is largely unconditional, i.e. if the share of the potential grant associated with scholarships for grade 3 to 6 is ≥ 0.5, and zero if this share is < 0.5.

To estimate the parameters of interests, we specify the following equations for ineligible (N) connected households:

$$\begin{aligned} Y_{ht} &= \eta_0 + \eta_1 N_h + \eta_2 T_t + \eta_3 N_h * G_h + \eta_4 N_h * T_t \\ &+ \eta_5 G_h * T_t + \eta_6 N_h * G_h * T_t + \eta_7 X_h + u_{hnt} \end{aligned}$$

The variable T is a dummy that equals 0 in October 1997, at baseline, and 1 between November 1998 and November 1999, the waves in which the program was offered only in random group of villages. The parameters $\eta_4 + \eta_6$ and η_4 identify the effect of the program on ineligible households with and without a mainly unconditional grant, which we show in the central panel of Table 8.

Lastly, we estimate the marginal effect of the potential unconditional and conditional grants for households entitled to different grant levels. To do that, we compute the potential monthly grants per network adult associated with primary (PP) and secondary (PS) school - that is grades 3-6 and 7-9 - and see how they correlate with investment of ineligible households by estimating the following regression:

$$Y_{hnt} = \gamma_0 + \gamma_1 PP_{nt} + \gamma_2 PS_{nt} + \gamma_3 X_{hn} + \nu_{ht}$$

We show the estimates of γ_1 and γ_2 in the bottom panel of the Table.

We now describe our results. The top panel of Table 8 shows that illness of related households affects investment in ways consistent with the model prediction: the higher the number of ill relatives, the lower school enrollment and oxen and equine ownership, and the higher the intensity of child labor. Consistent with the previous findings, higher network illness is associated with an increase in the use of land, but no increase in agriculture-related expenditures such as the purchase of seeds and fertilizers. Our conjecture, as mentioned before, is that some network members react to the negative health shock by cultivating some crop for own consumption.

The central panel shows the indirect effect of Progresa on the ineligible relatives of households entitled to the program grants. The number of small animals owned (poultry, goats, pigs) decreases. This is consistent with a precautionary motive for holding small livestock. Interestingly, the share of 11 to 16 year old ineligible children enrolled in school significantly decreases by 5.8 percentage points.[25]

The following two rows confirm that Progresa causes a re-allocation of the type of children attending school within networks of eligible and ineligible households. When most of the potential grant is conditional there is an 8.9 percentage point drop in enrollment for ineligible 11 to 16 year old children. Conversely, we find no evidence of a drop in enrollment for ineligible children, when most of the potential grant is unconditional. Moreover, the significant drop in livestock that we observed previously occurs only for this latter group, that is when most of the grant is unconditional, reinforcing our precautionary savings interpretation. These results are robust to conditioning on total potential monthly grant per network adult equivalent.

Importantly, the effect of Progresa on total enrollment of 11 to 16 year old children is positive and significant, averaging 4.1 percentage points between October 1997 and November 1999. Since the pre-program attendance rate was roughly 65%, the program increases overall school attendance by 6.3% for connected households but it changes the composition of enrolled children within the extended family. Failure to measure the network-wide program effect on schooling would result in an over-estimate of its true effect.

Our interpretation of this differential effect of school enrollment of older children is the following. Consider an extended family with both eligible and ineligible households that pool resources to maximize and smooth consumption, and to maximize the educational attainment of its members. This family has several children who could potentially go to secondary school but not enough resources to finance the education of all children, so only a subset of them is enrolled. The enrolled children are more likely to belong to the least poor households in the network, perhaps because of some unspecified social norm or parental preferences for own children's education over education of relatives' children. Progresa decreases the cost of secondary

[25] Angelucci et al. (2007) estimate a similar parameter for the ineligibles and also find a negative effect, though closer to zero and less precisely estimated. The main difference in their analysis is that they look at November 1999 school enrollment only. Their estimate is probably attenuated because by that month some households in control villages had begun being treated, so it is possible their ineligible households may have reduced their children's enrollment (as we find is the case in treatment villages). Moreover, they use a slightly different estimator. Their standard errors are also larger because they have a smaller sample, since they do not use data from November 1998 and May 1999. Bobonis and Finan (2009) find a positive effect on schooling for the poorest ineligible households, which is not incompatible with our findings.

education by a half to two thirds for the least wealthy households in the extended family. The extended family pools resources and chooses their optimal allocation between consumption and education. To maximize the number of children who go to school, the enrollment of ineligible children, now comparatively more expensive, decreases. Yet, total enrollment and consumption are higher than before.

These findings suggest that, for a given grant size, there may be differential effects on consumption and schooling depending on the grant conditionality. For a given grant, the higher its unconditional share, the bigger the effect on consumption and the smaller the drop in ineligible children's schooling. We check whether these conjectures are consistent with the data in Table 10, in which we estimate treatment effects on consumption and on 11 to 16 year old kids' schooling for eligible and ineligible households that are part of extended families. We group households depending on their potential grant composition, considering only extended families that are a mix of eligibles and ineligibles. We form three groups based on the share of the potential grant that is unconditional, i.e. that comes from scholarships for grades 3 to 6. The cutoff are up to 30%, 60%, and 100%. The first group's grant is mainly conditional, while the third group includes all households. We also report the average potential and current grants and the share of the eligible households in the network.

Consistent with the above conjecture, Table 10 has two key results. First, the share of actual grant consumed is only 50% when the grant is mainly conditional - the overall treatment effect on consumption is 18 *pesos* per month per adult equivalent, out of a 36 peso grant - but almost 100% when the grant becomes mainly unconditional - consumption increases by 32 *pesos* out of a 33 peso grant.

Second, the drop in ineligibles' schooling is highest in family networks with a larger conditional grant share. Ineligible households in networks with mostly conditional grants reduce their 11-16 year old children's school enrollment by 15.3 percentage points, while the average reduction when we include networks with higher shares of unconditional grants in 6.5 percentage points. This means all 11-16 year old eligibles' schooling is financed through a mix of partial subsidy (directly received from Progresa), unconditional grant (the subsidy to school attendance for younger children, who would have gone to school anyways), and through reallocation of secondary school attendance of ineligible children. The share of the second component increases, while the share of the third component increases for networks with more unconditional grants. Interestingly, both the schooling ATE and the average total increase in schooling are roughly constant across the three network groups - the former is between 0.071 and 0.068 the latter 0.037.

Not only are these findings consistent with our model; they also rule out some alternative explanations. For example, one may wonder whether the sharing of the Progresa grant with the ineligibles is a form of compensation for the ineligibles' drop in school attendance. This is not the case. If it were, we would expect ineligibles' consumption to increase inversely (relative to the eligibles' consumption increase) with their schooling reduction. That is, the estimated difference in treatment effects, $ITE^K - ATE^K$, should be biggest in the first column and smallest in the last one. On the contrary, not only is this difference constant across extended families with different shares of the conditional grant, but it also never statistically different from zero.[26,27]

7 Conclusion

This paper shows that informal institutions, i.e. family networks, affect both consumption and investment, and that, therefore, we should look at both outcomes to understand their role in developing countries. We consider extended family networks in rural Mexican villages and show that households are able to smooth consumption by sharing resources within the extended family. Consumption of households without relatives in the village, on the other hand, does not respond to positive income shock of other village members, consistent with the idea that the isolated do not share risk within the village. Consumption is smoother for the connected than for the isolated. Moreover, human capital investment decreases less for the connected than for the isolated, when hit by negative health shocks, and increases only for the connected, when both connected and isolated households are offered the same subsidies to send their children to school. These results show that the isolated engage in costlier consumption-smoothing behavior than the connected, that partial subsidies to schooling may be ineffective in the presence of credit constraints, and that informal institutions may have a beneficial role for both insurance and investment. The design and evaluation of economic policies should consider the presence of informal networks.

[26] Both the share of network households that are eligible and the grant per network adult equivalent are roughly constant across the three groups of households (both actually decline slightly as we include family networks with more and more unconditional grants, but this decline is always less than 10%). Therefore, if the different networks adopt similar sharing rules, we would expect to find similar relative treatment effects in consumption for the eligibles and the ineligibles across these networks, which we do.

[27] The estimated potential grant is always approximately 36 to 39% bigger than the actual grant. This is partly caused by eligibles households not sending all children to school, but largely because we compute our potential grant measure assuming a zero retention rate.

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A Appendix (section A.3 to A.5 not for publication)

A.1 Decreasing marginal returns to the investment

Consider the following central planner maximization problem:

$$\begin{aligned}
&\max U \equiv \sum_{h=1}^{2}\sum_{t=1}^{2}\beta^{t-1}\ln c_h^t \\
&s.t. \\
&c_1^1 = y_1^1 + d^1 - I_1 \\
&c_2^1 = y_2^1 - (1+\alpha)d^1 - I_2 \\
&c_1^2 = y_1^2 - (1+\alpha)d^2 + I_1^{0.5} \\
&c_2^2 = y_2^2 + d^2 + I_2^{0.5} \\
&d^{1,2} \geq 0, \quad I_{1,2} \in (0,1).
\end{aligned}$$

We model $y_1^1 < y_2^1$ and household 1 as receiving a transfer from household 2. The planner chooses the optimal transfers and investments given the returns, transaction costs, and endowments. Consider the cases in which $\alpha = 0$ and α sufficiently large that there are no transfers, that is the households are in autarky and consume and invest their own resources only.

With $\alpha = 0$, the optimal allocation of resources for each household h is:

$$I_h^{FI} = \frac{\beta^2 (Y^1)^2}{\left(Y^2 + \left(2\beta\left(2-\beta\right)Y^1 + (Y^2 2)^2\right)^{.5}\right)^2};$$

$$c_h^{1FI} = \frac{Y^1}{2} - \frac{\beta^2 (Y^1)^2}{\left(Y^2 + \left(2\beta\left(2-\beta\right)Y^1 + (Y^2)^2\right)^{.5}\right)^2};$$

$$c_h^{2FI} = \frac{Y^2}{2} + \frac{\beta Y^1}{Y^2 + 2\beta\left(2-\beta\right)Y^1 + (Y^2)^2}.$$

where Y^1, Y^2 are the aggregate endowments in period $1, 2$ and each agent chooses the same consumption and investment because they have the same Pareto weights and face the same return to the investment.

In autarky we have the following optimal allocation:

$$I_1^{AUT} = \frac{\beta^2 (y_1^1)^2}{\left(y_1^2 + \left(\beta\left(2+\beta\right)y_1^1 + (y_1^2)^2\right)^{.5}\right)^2};$$

$$I_2^{AUT} = \frac{\beta^2 (y_1^2)^2}{\left(y_2^2 + \left(\beta\left(2+\beta\right)y_2^1 + (y_2^2)^2\right)^{.5}\right)^2};$$

$$c_1^{1AUT} = y_1^1 - \frac{\beta^2 (y_1^1)^2}{\left(y_1^2 + \left(\beta\left(2+\beta\right)y_1^1 + (y_1^2)^2\right)^{.5}\right)^2};$$

$$c_2^{1AUT} = y_2^1 - \frac{\beta^2 (y_1^2)^2}{\left(y_2^2 + \left(\beta\left(2+\beta\right)y_2^1 + (y_2^2)^2\right)^{.5}\right)^2};$$

$$c_1^{2AUT} = y_1^2 - \frac{\beta y_1^1}{y_1^2 + \left(\beta\left(2+\beta\right)y_1^1 + (y_1^2)^2\right)};$$

$$c_2^{2AUT} = y_2^2 - \frac{\beta y_1^2}{y_2^2 + \left(\beta\left(2+\beta\right)y_2^1 + (y_2^2)^2\right)};$$

Households 1 and 2 have different levels of consumption and investment because their initial endowments differ.

Figure 3 shows the equilibrium allocations of consumption and investment for the two households for different transaction costs and specific values of the model parameters.[28] As

[28] A large number of scenarios have been simulated and are available upon request.

before, the transfer declines with the cost until no transfer takes place and both households consume and invest in isolation. Aggregate investment is positively correlated with the degree of insurance (which is inversely proportional to α). Our baseline scenario assumes $y = 150$ and $Y = 180$ (as to match our data). With perfect risk sharing ($\alpha = 0$) the optimal transfer is 15, each household has a post-transfer endowment of 165 and therefore makes the same optimal investment. Consider the opposite extreme, autarky. Now household 2, the household with the high endowment in the first period, invests more than the fully insured households for two reason: first, because it has more available resources, which are not shared with household 1; second, because now investment has the additional purpose of transferring wealth over time, as well as providing positive returns. The opposite argument applies to household 2, it has fewer incentives to invest, because it will receive the high endowment in the future, as well as fewer resources to invest. The aggregate investment in autarky is less than with full insurance because of the decreasing (marginal) returns: a transfer from household 2 to household 1 would increase aggregate investment. The remaining parts of Figure 3 show these results hold for a non-cyclical economy and with different discount rates and income values.

A.2 Lumpy investment

Consider the maximization problem from Section 2.2, with linear returns to the investment for simplicity. Suppose investment is lumpy, that is there is a level of I_{min} below which there is no investment. This level is lower in autarky than in risk-sharing, as agents cannot share resources. When $I_{min} \geq \frac{\gamma y_2^1 - y_2^2}{\gamma}$ there is no investment in autarky, as $U_{I=0}^{AUT} > U_{I>0}^{AUT}$ for both households, but positive investment when the agents can make transfers to each other, as long as I_{min} does not exceed a certain threshold (this threshold is $\frac{\gamma Y_1 - Y_2}{\gamma}$ when $\beta = 1$).

For example, consider the case in which $I_{min} = y_2^1 > y_1^1$. Neither of the autarkic agents will invest, as consumption must be strictly positive. However, household 2 can invest, if household 1 is willing to transfer same resources. Income pooling households are better off by adding resources in the first period and having positive investment if the return is sufficiently high. That is, $U_{I>0}^{FI} > U_{I=0}^{FI}$ if $\gamma \geq \frac{Y^2}{y_2^1}$.

To prove it, express total utility as $U = ln[c_1^1 c_2^1 (c_1^2 c_2^2)^{\beta}]$. Suppose that in case of positive investment, agent 2 invests the minimum, $I = y_2^1$ and the agents consume $c_h^1 = \frac{y_1^1}{2}$ in the first period and $c_h^1 = \frac{\gamma y_2^1 + Y^2}{2}$ in the second one. Then $U_{I>0}^{FI} > U_{I=0}^{FI}$ holds when $ln[(\frac{y_1^1}{2})^2 (\frac{\gamma y_2^1 + Y^2}{2})^{2\beta}] > ln[(\frac{Y^1}{2})^2 (\frac{Y^2}{2})^{2\beta}]$. This is true when $\gamma \geq \frac{Y^2}{y_1^1}$. Note that since both households consume the same amount, if $U_{I>0}^{FI} > U_{I=0}^{FI}$ then it follows that $U_{hI>0}^{FI} > U_{hI=0}^{FI}$ for $h = \{1, 2\}$.

The intuition is the following. The return to the investment, which the two households share in the second period, is sufficiently high to induce both of them to forgo current consumption in exchange for much higher future consumption. Household 1 makes a transfer to household 2, which makes an investment of at least y_2^1.[29]

One important difference when the conditionality is binding is that a marginal increase in endowment in the first period may increase investment for the agents who make transfers to each other. Consider the case in which $\beta = 1$, to keep the notation simple. Suppose $I_{min} = \frac{\gamma Y_1 - Y_2}{\gamma} + \epsilon$. In this case neither the autarkic agents nor the agents who can make transfers to each other will be better off by investing. For any small increase in first period endowment greater than ϵ, the latter group is better off by investing, unlike the autarkic agents, that is, $U_{I>0}^{FI} > U_{I=0}^{FI}$ but $U_{I>0}^{AUT} < U_{I=0}^{AUT}$.

A.3 Reliability of our matching algorithm and single-headed households

We believe our matching algorithm to be reliable, as confirmed by the following evidence. First, consider the possibility that names were measured with error. If this occurred, we might find spouses with four identical surnames. However, this happens only in 1.6% of the households. Further, 97% of the households are linked to no more than two parental households. More generally, comparing our data with the network data from the *Mexican Family Life Survey* (MxFLS), collected in 2001 shows that the links we identify in our data are indeed no larger than those measured in the MxFLS, which uses relatives from *any* location and not only from within the village. This applies also when we compare different types of link (e.g. the number of "sibling" links we identify is not larger than the reported number of siblings in the MxFLS).

Two further characteristics of our surnames support the view our matching algorithm is reliable. First, there are fewer paternal surnames reported by male household heads than for the other types of surname, including those reported as the wives' paternal surname. This is exactly what one would expect, because the patronymic naming convention implies wives' paternal surnames have lower survival rates across generations than those of male heads of household. Second, 85% of spouses that have their parents present in the village report remaining in the same village at the time of marriage. The corresponding figure for spouses that have no parental links in the village is only 59%. This can be explained by female marriage migration - i.e. moving to the husband's village upon marriage, which corresponds to anecdotal evidence

[29] While for this particularly high $I_{min} = y_2^1$, in which the investment must be at least as high as the highest endowment, the rate or return must be more than 100%, lower investment thresholds still result in positive investment for the risk-sharing households only with a lower return.

(as well as empirical evidence from other poor countries, e.g. (Rosenzweig and Stark, 1989).

To conclude this discussion, we consider the implications of having wrong matches (either erroneously matching two unrelated households or failing to match two related ones). First, as we explained above, the available evidence suggests this is not a serious issue in our data, once we omit single-headed households. Indeed, this group of families is the one for which mismatches are most likely. This happens because we do not observe the missing spouse's last names, so we cannot match the single-headed household to all its links. Therefore, some single-headed households may be erroneously classified as having no relatives in the village.

Call households with relatives in the village and without "connected" and "isolated". Suppose only related households share resources and we want to test this. That is, as household i's endowment changes, this will affect consumption and investment of its relatives only. Including the single-headed adds some connected households to the isolated pool. So, if, for example, we want to estimate the effect of a government grant to household i on the consumption and investment of its related and unrelated households, including the single-headed would result in over-estimating the effect of the grant for the unrelated. That is, the difference between the grant effect on the connected and the isolated is a lower bound of the true difference. In a way, then, this type of bias actually strengthens our results (if we find a significant difference anyways).

In general, if our networks data were contaminated by large measurement error we should not find any evidence of the proposed mechanism and in particular no significant differences in consumption smoothing and investment behavior between isolated and connected households. Angelucci, De Giorgi, Rangel, and Rasul (2006, 2007) discuss the creation of these family networks, the characteristics of the matching algorithm, and the possibility and extent of measurement error in greater details.

As we explained in section 3.3, we suspect that some single-headed households are misclassified. For example, it is possible to classify a single-headed households that is in fact connected as isolated because we cannot observe the extended family links of the missing spouse. Since single-headed households are older and poorer than couple-headed ones, including them when comparing outcomes for connected and isolated households would bias our estimates in two ways. First, they would cause an attenuation bias. Suppose the isolated invest less than the connected. Erroneously classifying some connected single-headed as isolated would reduce the estimate of the difference in investment between the connected and the isolated. Second, they may cause an amplification bias. For example, suppose the single-headed invest *less* than the couple-headed because they are poorer. By mis-classifying some single-headed as isolated when they are not we may obtain too big an estimate of the connected-isolated differential. This latter issue is a serious concern, as it may result in wrongly attributing some positive effects to connectedness. Therefore, when comparing the connected and the isolated, we drop single-headed households. Conversely, when we make comparisons *within* connected households, we include the single-headed.

Figure 4 top panel confirms our suspicions that some single-headed households are mis-classified. When we include these families, as we do in the left panel, the distribution of the propensity score for the probability of being isolated, $P(I|X)$, is not well balanced. However, this distribution becomes much more balanced once we drop the single-headed, as shown in the right panel.

The Figure's lower panel confirms that including or omitting single-headed households does not really affect how the characteristics of connected households are balanced between treatment and control villages. This is not surprising, since the treatment villages are randomly selected.

A.4 Consumption smoothing: is it really the family?

In this section we investigate whether we find a positive ITE on consumption and loans/transfers grouping households depending on land ownership or ethnicity, rather than on connectedness. Our data provide information on land ownership and language spoken including any indigenous language. About 60% of the households in our sample own any land, 35% belong to some ethnic minority, and these characteristics vary within family network.

We find no positive and significant ITEs in either case. Moreover, we fail to find an effect for landed or ethnic minority households over and above the effect of family network. Tables 11 and 12 provide the relevant estimates.

A.5 Samples, time periods, and estimators

Table 13 describes the key variables used and shows in which data waves they are available. Different variables are available in different waves. Since there is only one wave in which we observe all variables (November 1998), we choose to use as many data points as possible when looking at different outcomes. Therefore, different sets of estimates are associated with different sample sizes. We believe this strengthens our results, as we show that our estimates draw a consistent picture irrespective of the particular data waves we end up using in the various estimations.

A further complication arises from the fact that different types of tests use different households. For example, most of the tests that use Progresa as an exogenous shock use only ineligible households, while the tests that use health shocks use all households. To aid the reader, Table 14 summarizes the set of households, time periods, estimators, and types of variation in the data that we use to estimate our parameters of interest.

Table 1: Demographic and economic characteristics of connected and isolated households

	Compare connected (K) and isolated (I)					Compare K and I by village type		
	Connected		Isolated			Connected	Connected (all)	Isolated
Variable	Mean	SD	Mean	SD	p-value	p-value	p-value	p-value
Household size:								
age 07	1.30	(1.26)	1.25	(1.28)	[0.059]*	[0.721]	[0.620]	[0.831]
age 814	1.14	(1.21)	1.15	(1.22)	[0.817]	[0.743]	[0.963]	[0.206]
age 1518	0.54	(0.78)	0.55	(0.78)	[0.610]	[0.350]	[0.367]	[0.364]
age 1921	0.27	(0.54)	0.25	(0.52)	[0.062]*	[0.673]	[0.468]	[0.393]
age 22+	2.38	(0.97)	2.40	(0.93)	[0.333]	[0.023]**	[0.035]**	[0.487]
Household head:								
age	44.69	(15.11)	47.18	(15.84)	[0.000]***	[0.157]	[0.088]*	[0.587]
literacy	0.73	(0.44)	0.67	(0.47)	[0.000]***	[0.124]	[0.136]	[0.643]
sex	0.99	(0.1)	0.99	(0.1)	[0.654]	[0.760]	[0.293]	[0.693]
ethnicity	0.34	(0.48)	0.38	(0.49)	[0.172]	[0.539]	[0.549]	[0.043]**
Irrigated land size	0.12	(1.01)	0.14	(0.75)	[0.292]	[0.454]	[0.479]	[0.317]
Other land size	2.18	(4.2)	2.21	(4.91)	[0.766]	[0.572]	[0.522]	[0.808]
horse	0.40	(1.04)	0.39	(0.99)	[0.544]	[0.300]	[0.265]	[0.218]
donkey	0.40	(1.16)	0.39	(0.87)	[0.570]	[0.317]	[0.261]	[0.920]
ox	0.12	(0.74)	0.12	(1.01)	[0.789]	[0.005]***	[0.002]***	[0.039]**
goat	1.58	(5.93)	1.40	(5.09)	[0.168]	[0.158]	[0.119]	[0.002]***
cow	1.16	(3.76)	1.10	(4.12)	[0.520]	[0.431]	[0.349]	[0.877]
chicken	7.01	(8.16)	7.98	(8.81)	[0.000]***	[0.202]	[0.258]	[0.443]
pig	1.20	(2.92)	1.18	(3.11)	[0.782]	[0.595]	[0.741]	[0.657]
fridge	0.16	(0.36)	0.16	(0.37)	[0.591]	[0.087]*	[0.097]*	[0.904]
stove	0.32	(0.47)	0.29	(0.45)	[0.039]**	[0.263]	[0.229]	[0.666]
heater	0.03	(0.17)	0.03	(0.18)	[0.433]	[0.683]	[0.705]	[0.815]
radio	0.65	(0.48)	0.64	(0.48)	[0.403]	[0.033]**	[0.021]**	[0.220]
TV	0.49	(0.5)	0.46	(0.5)	[0.015]**	[0.006]***	[0.005]***	[0.582]
Car	0.02	(0.15)	0.03	(0.16)	[0.324]	[0.398]	[0.488]	[0.085]*
truck	0.08	(0.28)	0.08	(0.27)	[0.464]	[0.207]	[0.203]	[0.377]
Wealth index	728.43	(141.48)	728.9	(141.35)	[0.912]	[0.169]	[0.162]	[0.691]
Labor income	272.79	(352.64)	275.99	(325.17)	[0.693]	[0.152]	[0.151]	[0.618]
Food exp	153.05	(127.35)	152.27	(136.44)	[0.829]	[0.332]	[0.258]	[0.168]
Weekly child labor:								
Dummy age 8-14	0.12	(0.32)	0.13	(0.34)	[0.279]	[0.015]**	[0.017]**	[0.437]
Dummy age 15-18	0.48	(0.5)	0.48	(0.50)	[0.972]	[0.662]	[0.697]	[0.424]
Days 8-14	0.39	(1.26)	0.40	(1.25)	[0.784]	[0.746]	[0.673]	[0.863]
Days 15-18	2.20	(2.58)	2.20	(2.58)	[0.943]	[0.662]	[0.697]	[0.511]
Progresa eligible	0.552	(0.497)	0.547	(0.498)	[0.693]	[0.087]*	[0.101]	[0.857]
School enrollment (11-16 years old)	0.644	(0.410)	0.657	(0.410)	[0.287]	[0.587]	[0.589]	[0.000]***
Completed secondary school	0.075	(0.217)	0.078	(0.224)	[0.594]	[0.599]	[0.633]	[0.853]
Ill household head	0.112	(0.315)	0.120	(0.325)	[0.199]	[0.868]	[0.871]	[0.221]

P-values of differences from standard errors clustered at the village level. October 1997 data, except March 1998 expenditures and November 1998 illness rates. Income and expenditures per adult equivalent at November 1998 pesos. The values reported for land are size owned/used. We cannot separately identify land owned and cultivated in our data.

Table 2: Extended families and the indirect effect of Progresa on food consumption in 1999

1. Ineligible K and I		**2. Ineligibles without and with eligible relatives**		**3. Ineligibles without and with > 30% eligible relatives**	
ITE^{K}	26.54	ITE^{Erel}	18.40	$ITE^{>30\% Erel}$	31.96
	[11.20]**		[10.85]*		[13.14]**
No. households	2888	No. households	2688	No. households	2519
ITE^{I}	-14.73	ITE^{noErel}	-6.43	ITE^{noErel}	-6.43
	[20.14]		[13.69]		[13.69]
No. households	656	No. households	856	No. households	856
Obs	11054	Obs	11054	Obs.	9979
ΔITE	41.27	ΔITE	24.83	ΔITE	38.39
	[20.80]**		[14.05]*		[18.97]**

Difference-in-difference OLS estimates between October 1997 and November 1999, controlling for 1997 household characteristics, region and time effects. Standard errors clustered at the village level. Number of households in each category measured in November 1999. K=connected; I=isolated; $Erel$=all households with relatives eligible for Progresa; $noErel$=no relative eligible for Progresa. $ITE^{>30\% Erel}$ uses only connected households with at least 30% eligible relatives.

Table 3: Ineligible log-food consumption as a function of log-transfer per network member

	Treatment villages		
	OLS	IV	IV p>.30
ln(actual transfer)	0.075	0.136	0.161
	[0.024]***	[0.051]***	[0.095]*
1st stage IV signif.		131.74	91.02
Obs.	3353	3353	2409
		OLS	OLS p>.30
ln(potential transfer)		0.093	0.106
		[0.036]**	[0.063]*
Obs.		3353	2409
	Control villages		
		OLS	OLS p>.30
ln(potential transfer)		-0.032	-0.225
		[0.052]	[0.115]*
Obs.		1230	829

OLS and IV estimates using log-potential transfer as IV for log-actual transfer per network adult, controlling for 1997 household and network characteristics, state and time effects. p is the fraction of eligible households in the network. The consumption elasticity of the transfer is 0.37 sd 0.08 (0.38 sd 0.08) for all treated (for p>.30) households. Standard errors clustered at the village level.

Table 4: Treatment effects on monthly consumption per adult equivalent for connected eligible households, 1999.

	Food consumption	All consumption
	Networks with both eligibles and ineligibles	
ATE	28.104	34.355
	[5.860]***	[7.646]***
Obs	10883	10883
ITE per eligible	8.512	8.618
	[3.517]**	[4.768]*
Obs	5478	5472
Transfer per eligible	63.93	63.93
ATE share	**0.44**	**0.54**
ITE share	0.13	0.13
	Networks with eligibles only	
ATE	31.226	38.569
	[7.421]***	[10.358]***
Obs	5627	5627
Transfer per eligible	55.5	55.5
ATE share	**0.56**	**0.69**

OLS first-difference estimates of consumption using May 1999, and November 1999 and subtracting March 1998 consumption, controlling for 1997 household characteristics, state and time effects. Standard errors clustered at the village level.

Table 5: Full-insurance regressions in Extended Families $\Delta \ln c_{ht} = a + \alpha \Delta \ln Y_t + \beta \Delta \ln y_{ht}$.

	ALL HOUSEHOLDS	COUPLE HEADED HOUSEHOLDS
Total Income		
LnY	0.965	0.925
se	[0.007]***	[0.012]***
Lny	0.019	0.018
se	[0.002]***	[0.002]***
Obs.	55965	51055
Total Income IV (Lag 2 Income)		
LnY	0.965	0.929
se	[0.012]***	[0.016]***
Lny	-0.003	0.000
se	[0.020]	[0.021]
Obs.	40502	35552

Notes: The aggregate resources (Y) are consumption in the network. SE clustered at the village level. Extreme values trimmed in the regressions. ***, **, * significantly different from zero at 1, 5, 10%.

Table 6: Difference in the log of the coefficient of variation of food consumption and income for connected and isolated households

	Log of food consumption CV	
Isolated	0.060	0.054
SE	[0.010]***	[0.009]***
Constant	-1.14	-1.10
SE	[0.007]***	[0.006]***
Observations	22200	20384
Control for $lnCV(Y)$	No	Yes

Note: coefficients clustered at the network level. If high-CV(Y) households more likely to form family links, then the Isolated coefficient is downward-biased. ***, **, * significant at 1, 5, 10%.

Table 7: Effect of household head illness and Progresa on own human and physical capital investment

	School/work (11-16 y.o.)		Stock of animals owned				Agriculture	
Effect on:	% enrolled in school	Weekly workdays	Chicken	Cow	Ox/horse/ donkey	Goat/ pig	Land owned/ cultivated	Fertilizer/ seeds etc
Negative shock: average effect of own household head illness on all households, 1998-2003								
ATE^K	-0.010	0.077	0.092	0.029	0.015	-0.008	0.043	-0.455
	[0.008]	[0.036]**	[0.034]***	[0.044]	[0.018]	[0.034]	[0.014]***	[0.979]
ATE^I	-0.042	0.283	0.088	-0.007	0.002	-0.021	0.046	-0.739
	[0.016]***	[0.084]***	[0.063]	[0.033]	[0.016]	[0.037]	[0.028]*	[2.021]
ATE^K-ATE^I	0.032	-0.206	0.004	0.036	0.013	0.013	-0.003	0.283
	[0.018]*	[0.091]**	[0.072]	[0.055]	[0.024]	[0.050]	[0.031]	[2.245]
Obs.	30872	24026	45391	45408	45394	45392	31029	13285
Positive shock: average effect of Progresa on eligible households (randomly assigned), 1998-1999								
ATE^K	0.077	-0.232	0.158	0.002	0.021	0.026	0.016	0.050
	[0.013]***	[0.068]***	[0.051]***	[0.010]	[0.012]*	[0.031]	[0.015]	[0.475]
ATE^I	-0.022	0.005	0.269	-0.001	0.030	-0.023	0.082	-0.005
	[0.023]	[0.106]	[0.068]***	[0.030]	[0.028]	[0.043]	[0.024]***	[0.753]
ATE^K-ATE^I	0.100	-0.237	-0.112	0.003	-0.010	0.049	-0.067	0.055
	[0.027]***	[0.126]*	[0.086]	[0.032]	[0.031]	[0.051]	[0.028]**	[0.809]
Obs.	24122	24122	39897	39903	39893	39894	39839	18029

Note: double differences using the maximum number of available data waves. 1) Sept. 1997 to Nov. 1999 data for effect of Progresa (using one wave per school year only for effect on stock with at least secondary education). 2) Nov 1998 to Nov 2003 for illness of household head, with following exceptions: data on livestock missing in November 2000; data on child labor missing in 2003; data on size of land used missing from November 2000 onwards; data on agricultural expenditures missing from Nov. 1999 onwards. The results for the effect of health shocks are very similar even if we use fewer data waves (e.g. Up to Nov 2000 or Nov 1999 for all variables). Couple-headed households only. Standard errors clustered at the network level. *,**,*** = significant at the 1%, 5%, 10% level.

Table 8: effect of positive and negative shocks on consumption and investment of related households

Effect on:	School/work (11-16 y.o.) % enrolled in school	School/work (11-16 y.o.) Weekly workdays	Stock of animals owned Chicken	Stock of animals owned Cow	Stock of animals owned Ox/horse/ donkey	Stock of animals owned Goat/ pig	Agriculture Land owned/ cultivated	Agriculture Fertilizer/ seeds etc
Negative shock (1998-2003): marginal effect of relatives' household head sickness on all connected households (compare networks with different numbers of ill household heads)								
Illness	-0.003	0.009	-0.001	-0.00005	-0.0002	-0.0002	0.005	-0.074
of relatives	[0.001]***	[0.003]***	[0.001]	[0.0005]	[0.0001]*	[0.0002]	[0.002]***	[0.106]
Obs.	26860	20898	40588	40691	40734	40633	27852	11928
Positive shock (1998-1999): average effect of Progresa on eligibles' relatives (random assignment) (estimate separate effects for networks with mainly conditional (<.5pr) or unconditional (≥.5pr) grants								
ITE^K	-0.058	0.030	-0.050	-0.004	0.001	-0.013	0.006	-0.571
	[0.022]***	[0.117]	[0.021]**	[0.003]	[0.003]	[0.004]***	[0.027]	[0.734]
Obs.	5994	5994	13639	13675	12237	13686	13595	6162
ITE^K (mainly	-0.089	0.119	-0.030	-0.002	-0.002	-0.007	0.034	-0.604
conditional)	[0.027]***	[0.141]	[0.024]	[0.004]	[0.004]	[0.004]	[0.032]	[0.826]
ITE^K (mainly)	0.008	-0.167	-0.037	-0.004	0.007	-0.027	-0.007	1.408
(uncond.)	[0.039]	[0.243]	[0.016]**	[0.034]	[0.007]	[0.007]***	[0.060]	[1.547]
Difference	0.097	-0.287	-0.067	-0.006	0.009	-0.022	-0.025	2.012
(uncond.- cond.)	[0.047]**	[0.280]	[0.050]	[0.009]	[0.007]	[0.009]***	[0.065]	[1.697]
Obs.	5588	5588	12726	12755	12784	12768	12687	5758
Positive shock (1998-1999): marginal effect of Progresa's potential grant on eligibles' relatives (compare networks with different transfers - do not use randomization)								
Unconditional	0.009	-0.001	-0.006	0.0003	0.0007	0.0006	-0.002	0.253
grant	[0.003]**	[0.007]	[0.001]**	[0.0003]	[0.0002]	[0.0003]**	[0.002]	[0.117]**
Conditional	-0.001	-0.005	-.0.001	0.0002	-0.0001	0.0008	0.003	0.076
grant	[0.002]	[0.005]	[0.001]	[0.0002]	[0.0002]	[0.0002]***	[0.001]**	[0.094]
Obs.	2276	2276	5294	5294	5309	5302	5294	3278

Note: animal and agricultural investment variables are per adult equivalent. ITEs by share of conditional grant are robust to adding total potential grant and to using different cutoffs (e.g. 35% of the grant from primary school children rather than 50%). ITEs for animal and agricultural investment estimated by tobit. All other parameters in the table are estimated by OLS, clustering the standard errors at the network level. Double difference and first difference estimates using the maximum number of available data waves. 1) Sept. 1997 to Nov. 1999 data for effect of Progresa (using one wave per school year only for effect on stock with at least secondary education). 2) Nov 1998 to Nov 2003 for illness of household head, with following exceptions: data on livestock missing in November 2000; data on child labor missing in 2003; data on size of land used missing from November 2000 onwards; data on agricultural expenditures missing from Nov. 1999 onwards. The results for the effect of health shocks are very similar even if we use fewer data waves (e.g. Up to Nov 2000 or Nov 1999 for all variables). To compute the treatment effects on schooling we consider only households with members aged 11-16. Couple-headed households only. Standard errors clustered at the network level. *,**,*** = significant at the 1%, 5%, 10% level.

Table 9: Changes in investment, income, and consumption for connected and isolated households - Difference-in-difference estimation between 1997 and 2003.

	% members with	Investment. 1. Stock of animals:					2. % owning machinery:	
	$\geq$ 9th grade	Cow	Ox	Horse/donkey	Chicken	Goat/pig	Car	Tractor
K	0.044	-0.132	-0.005	-0.084	-0.685	-0.273	0.130	0.004
	[0.003]***	[0.010]***	[0.002]**	[0.005]***	[0.031]***	[0.018]***	[0.007]***	[0.001]***
I	0.034	-0.113	-0.011	-0.088	-0.805	-0.247	0.105	0.003
	[0.003]***	[0.018]***	[0.004]***	[0.006]***	[0.045]***	[0.028]***	[0.006]***	[0.002]**
K-I	0.010	-0.019	0.006	0.004	0.120	-0.026	0.025	0.001
	[0.004]**	[0.021]	[0.004]	[0.008]	[0.055]**	[0.033]	[0.009]***	[0.002]
Obs.	35661	35421	35410	35410	35424	35416	35656	35652

	Labor	Consumption. 1. Durable (% owning):					2. Non-durable:	
	income	Fridge	Stove	Heater	Radio	TV	Food	Non-food
K	30.215	0.175	0.081	0.014	-0.050	0.156	-11.551	57.214
	[4.785]***	[0.007]***	[0.006]***	[0.004]***	[0.009]***	[0.007]***	[2.119]***	[1.847]***
I	13.802	0.146	0.058	-0.004	-0.057	0.136	-16.976	52.369
	[6.285]**	[0.008]***	[0.008]***	[0.004]	[0.011]***	[0.010]***	[2.681]***	[2.255]***
K-I	16.413	0.028	0.023	0.018	0.007	0.020	5.425	4.845
	[7.967]**	[0.011]***	[0.010]**	[0.005]***	[0.015]	[0.012]*	[3.421]	[2.911]*
Obs.	35093	35642	35645	35598	35632	35641	35433	35416

Note: double difference in consumption (durable and non-durable) and machinery between Nov. 1998 and Nov. 2003, as data not available in 1997. Standard errors clustered at the village level. Couple-headed households only. Including also single-headed households does not change the results.

Table 10: Differential effect of Progresa on consumption and schooling by grant conditionality

	← Conditional	Unconditional →	
Share of grant that is unconditional:	Max 30%	Max 60%	Max 100%
		Consumption	
All	18.148	32.621	31.951
	[8.039]**	[6.080]***	[5.896]***
ITE^K	15.627	33.488	29.687
	[14.892]	[13.027]**	[12.251]**
ATE^K	18.553	31.588	32.040
	[8.242]**	[5.826]***	[5.663]***
ITE^K-ATE^K	-2.926	1.899	-2.353
	[14.849]	[12.984]	[12.308]
Obs.	12237	28665	30277
No. of households (Nov 1999):			
All	4171	9282	9735
Ineligible	855	2514	2808
Eligible	3316	6768	6927
Potential grant per network member	50.34	46.78	45.81
Current grant per network member	36.06	33.8	33.16
Share eligibles in network	0.62	0.59	0.58
		Schooling	
All	0.037	0.037	0.037
	[0.021]*	[0.013]***	[0.012]***
ITE^K	-0.153	-0.091	-0.065
	[0.049]***	[0.026]***	[0.024]***
ATE^K	0.070	0.071	0.068
	[0.021]***	[0.013]***	[0.013]***
Obs.	8743	21668	22871

Note: difference-in-difference OLS estimate with standard errors clustered at the network level. *,**,***= significant at the 1%, 5%, and 10% levels. We consider only extended families with a mix of eligible and ineligible households. We exclude the "densificados" to estimate the treatment effects, although we compute the transfers per network adult equivalent including all network members. To compute the treatment effects on schooling we consider only households with members aged 11-16. See table X for a list of the predetermined controls we add to the specification.

Table 11: Treatment effects on 1999 food consumption for ineligible households by land ownership and ethnicity.

Land ownership		Ethnicity	
ITE land	18.23	ITE Indigenous	30.98
	[12.40]		[24.43]
ITE no land	13.72	ITE Hispanic	14.05
	[15.25]		[11.95]
ΔITE	4.51	ΔITE	16.92
	[17.05]		[26.82]
Obs	13468	Obs	13468

Difference-in-difference OLS estimates. Standard errors clustered at the village level.

Table 12: treatment effects on 1999 food consumption for ineligible households by land ownership and ethnicity over and above effect of family network.

Land ownership		Ethnicity	
ITE^K_{L1}	28.69	ITE^K_{I1}	42.16
	[13.84]**		[26.41]
ITE^K_{L0}	23.38	ITE^K_{I0}	24.16
	[18.35]		[13.48]*
ITE^K_{L1}-ITE^K_{L0}	5.30	ITE^K_{I1}-ITE^K_{I0}	18.23
	[20.90]		[12.40]
ITE^I_{L1}	-17.27	ITE^I_{I1}	-2.42
	[21.73]		[36.62]
ITE^I_{L0}	-5.04	ITE^I_{I0}	-16.29
	[24.34]		[18.59]
ITE^I_{L1}-ITE^I_{L0}	-12.23	ITE^I_{L1}-ITE^I_{L0}	13.72
	[32.39]		[15.25]
Obs	13468	Obs	13468

Difference-in-difference OLS estimates. Standard errors clustered at the village level.

Table 13: Variable descriptions and data availability.

Variable	Description	Availability Wave 1 Oct.1997	Wave 2 Mar.1998	Wave 3 Nov.1998	Wave 4 May 1999	Wave 5 Nov.1999	Wave 6 May 2000	Wave 7 Nov.2000	Wave 8 Nov.2003
Food consumption	Food consumption per adult equivalent (<18=0.73; ≥18=1; 7-day recall of 36 items; includes home production and gifts.			x PH	x PH	x PH		x H	x H
Food expenditure	7 day recall for broad food categories		x P						
Transfers	Gifts from friends, relatives, neighbors, migrants.			x P		x P			
Loans	Informal loans received form friends, relatives, neighbors, moneylenders, TBC			x P	x P				
Potential grant	Built looking at the age by gender by education household demographic structure			x P	x P	x P			
Actual grant	From administrative records.			x P	x P	x P			
Household head ill	Self-reported health status during previous 30 days (in some waves deduced from days unable to work)			x H	x H	x H		x H	x H
School attendance	% of 11-16 year old children enrolled in school	x P		x PH	x PH	x PH	x	x H	x H
Child labor	11-16 year old children's average days worked in previous week	x P		x PH	x PH	x PH	x	x H	x H
Animal ownership	No. of animals owned	x P		x PH	x PH	x PH	x		x H
Agricultural expenses	Purchases of seeds, fertilizers, tools and machinery for agricultural production (land rental excluded)			x PH	x PH				
Land used/owned	Hectares used (or owned) in the previous 6 months			x PH	x PH	x PH			
Assets	dummies for household owning fridge, stove, heater, radio, TV, car, truck.	x P		x PH	x PH			x H	x H

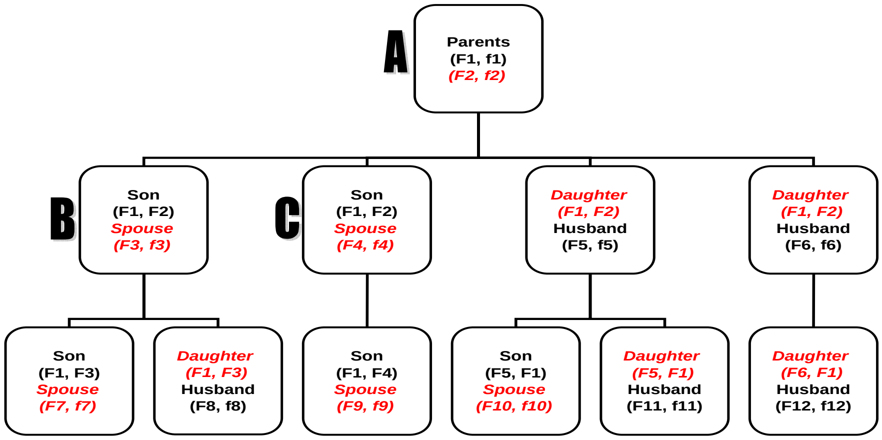
Note: x=available; P=used in regressions that exploit the exogenous eligibility to Progresa; H=used in regressions that exploit the household health shocks. All variables are monthly, unless otherwise specified. All monetary data are in November 1998 pesos. Potential and current grants available for all waves since Nov. 1998. However, we only use them for the 3 waves during which Progresa was offered in a random group of villages only. Therefore, all regressions exploiting the randomization use up to wave 5 data.

Table 14: Samples, time periods, and estimators.

Table	Outcome	Household type	Data waves	Estimator	CS or DD
1	Various	CH K and I	1	OLS	CS
2	Food consumption	CH N K and I	2,4, and 5	OLS	DD
3	Food consumption	All N K	2,4, and 5	OLS	DD
4	Food consumption	All N K	4 and 5	OLS and IV	CS
5	Food and all non-durable	All K	2,4, and 5	OLS	DD
5	consumption				
6	Loans and transfers	CH N K and I	3,4, and 5	OLS,Probit, Tobit	CS
7	Loans and transfers	All N K	3,4, and 5	OLS,Probit, Tobit	CS
8					
9					
10	Food consumption				DD
11	Consumption, investment, and income	CH K and I	1 and 8	OLS	DD
12 u	Investment	CH K and I	3 to 8 (with gaps)	OLS	DD
12 l	Investment	CH E K and I	1-2 to 5	OLS and Tobit	DD
13 u	Investment	All N K	3 to 8 (with gaps)	OLS	DD
13 m	Investment	All N K	1-2 to 5	OLS and Tobit	DD
13 l	Investment	All N K	1-2 to 5	OLS and Tobit	CS
14	Food consumption	All N K and I	2,4, and 5	OLS	DD
15	Food consumption	All N K and I	2,4, and 5	OLS	DD

Note: waves 1 to 8 correspond to; N=ineligible for Progresa; E=eligible for Progresa; CH=couple-headed; All=both couple- and single-headed; K=connected; I=isolated. CS=only use cross sectional variation; DD= use both cross sectional and longitudinal variation, controlling for time-invariant heterogeneity. u=upper, m=middle, l=lower.

Figure 1: Family Tree



Notes: We use the convention that the head's surnames are written in standard (black) font, and those of his wife are written in (red) italics. Paternal surnames are indicated in upper case (F1, *F2*) and maternal surnames are indicated in lower case (f1, *f2*). First names are not shown as they are not relevant for the construction of extended family ties. Each household in the family tree is assumed to be couple headed purely to ease the exposition.

Figure 1: Family Tree

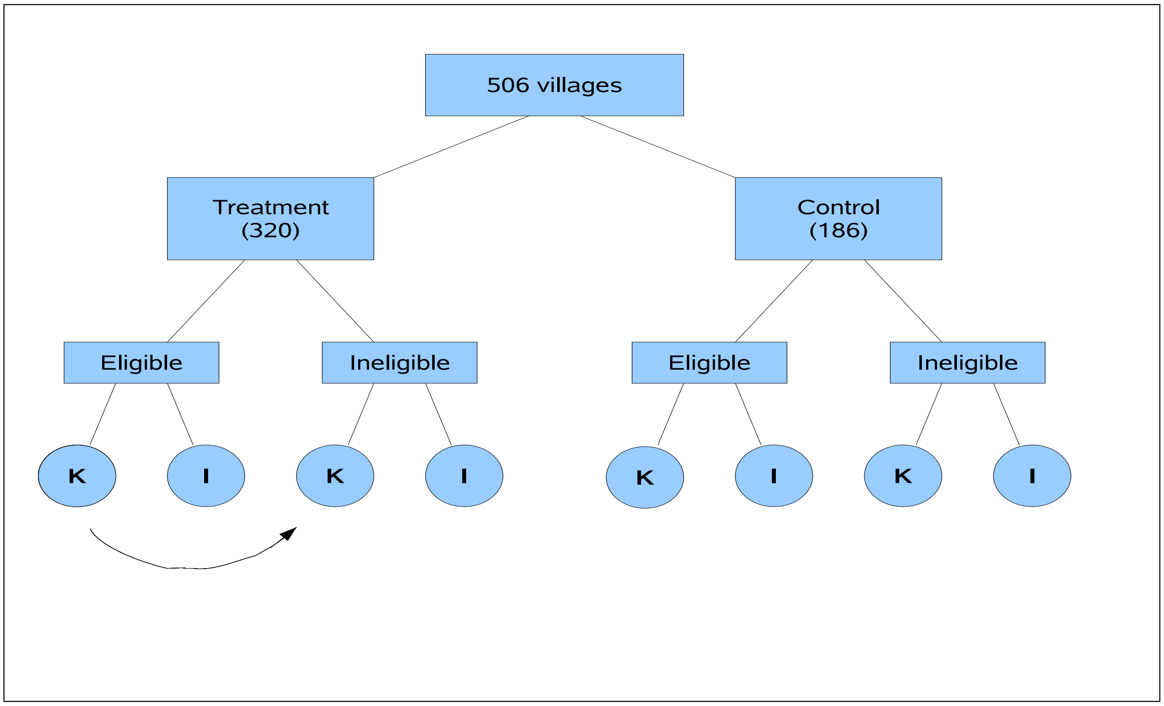


Figure 2: Data structure

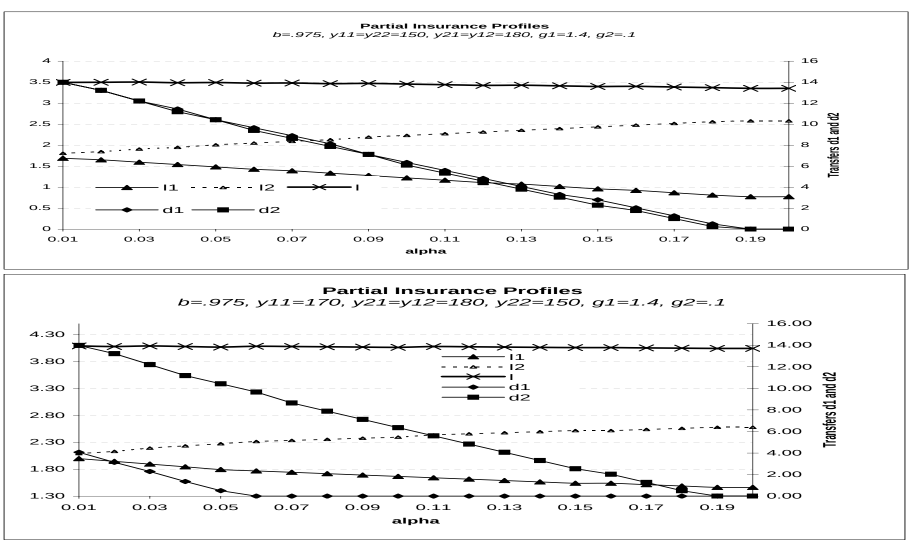


Figure 3: Optimal investment with decreasing marginal returns.

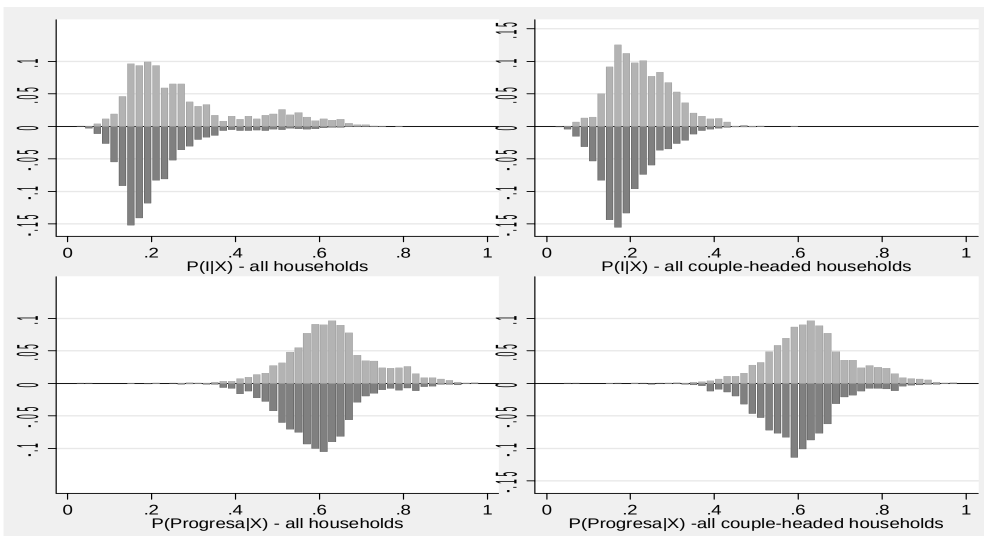


Figure 4: Propensity scores