INSURANCE AND INVESTMENT WITHIN FAMILY NETWORKS*

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Abstract

We study how family networks affect informal insurance and investment in poor villages. We use panel data from the randomized evaluation of *PROGRESA* in rural Mexico and exploit the information on surnames to identify extended families. Using exogenous income variations, we show that members of an extended family (connected) share risk with each other but not with households without relatives in the village (isolated). In addition, connected households invest more in their children's human capital when hit by a positive income shock, the *PROGRESA* transfer, and disinvest less when hit by a negative health shock. Such a higher level of investment is long-lasting, and increases long-term consumption. At the same time connected households achieve almost perfect insurance against idiosyncratic risk. These findings suggest that anti-poverty policies should take into account the familial structure of village economies.

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1 Introduction

Poor households in developing countries face substantial risk, yet typically have limited access to formal insurance and credit markets. This reduces their ability to smooth consumption and invest. To overcome these market failures, households resort to informal arrangements with each others. This paper looks at one specific informal institution: the village-based extended family, that is, the network of households that live in the same village and are related to each other. Since informal institutions may both provide insurance and favor investment, it is important to examine the role of extended families on consumption smoothness and investment jointly.

The absence of credit and insurance markets may cause a household to forego higher-return, but lumpier investment such as schooling, for lower-return, but less lumpy investment such as poultry.[1] For example, the lack of credit may prevent a household from sending its children to school. This causes future consumption to be lower than if the household had invested in education. In addition, the household may disinvest to keep consumption stable when facing a negative shock. For example, the lack of insurance may force a household whose head falls ill to take its children out of school, foregoing higher future consumption to smooth current consumption. In sum, the lack of credit and insurance reduces the level and increases the volatility of consumption (with a tradeoff between the two).

The presence of an informal resource-sharing network such as the extended family may increase the mean and reduce the variance of consumption. The increase in average consumption occurs through an increase in higher-return investment. The reduction in consumption variance occurs because the extended family can provide insurance against idiosyncratic risk. For example, the extended family may relax credit constraints by providing the capital to undertake an investment which a single household cannot finance on its own (e.g. by buying the books and uniform to send a child to school and cover for forgone earnings). Moreover, once the investment is made, the extended family's provision of insurance against idiosyncratic risk makes the investment less responsive to negative income fluctuations (so that the child can stay in school even when the father falls ill and cannot work). With multiple investment goods, the extended family may channel aggregate resources towards the higher-return investment and protect this investment against negative income shocks. This may result in higher future income and consumption for all its members.

The extended family relaxes credit constraints only when its aggregate resources are sufficiently high. Belonging to an extended family does not relax credit constraints if all its members have low income. Therefore, the beneficial role of this informal institution varies depending on the economic conditions. Households with and without geographically proximate families may have similar investment profiles when all the households are poor. Conversely, increases in aggregate resources may yield long-lasting differences in investment, earnings, and consumption, if only households with relatives living nearby can channel the available resources to the high-yielding investment goods.

[1] See Arrow (1971), Obstfeld (1994), Rosenzweig and Wolpin (1993), Kochar (1995) and Morduch (1995), Chetty and Looney (2006), and Kinnan and Townsend (2010).

We study the extended family as a resource-sharing institution because theory and evidence suggest that small networks can achieve high levels of insurance. The extended family may be an important resource-sharing institution, since its members likely know each other well, care for each other, and are able to monitor and punish deviating behavior by imposing sanctions.[2]

Our data, a panel of poor households from rural Mexico collected for the evaluation of the *PROGRESA* program, are a census of 506 villages in rural Mexico interviewed 8 times between 1997 and 2003 and provide information on surnames of household heads and their spouses. We use this information to identify 'connected' and 'isolated' households – those with and without close relatives (parents, offspring, siblings) in the village of residence. To the best of our knowledge, this is one of the most extensive mappings of village-level extended family structures that has been conducted in the social sciences.

Our data are a partial population experiment. Between 1998 and 1999, *PROGRESA* offered cash transfers in 320 randomly chosen treatment villages. We observe households eligible and ineligible for the program in both the control and treatment villages. Therefore, we have an exogenous income variation (rarely available in observational data), two sets of counterfactual households (eligibles and ineligibles in control villages), and a panel. These unique data characteristics let us test whether and how much an exogenous income shock (the *PROGRESA* grant) affects the consumption of ineligible household related and unrelated to the eligible households, as well as measure how the shock affects investment for connected and isolated eligible households.[3] Moreover, we can measure whether these differential effects on investment are long-lasting by comparing the medium-run change in investment for connected and isolated households. Importantly, we can estimate the causal effect of belonging to a village-based extended family on consumption and investment under the assumption that the time-invariant and time-varying unobserved determinants of network formation, that also affect insurance and investment, enter our model additively.

We test whether households share risk with their relatives, but not with their non-relatives in the village. After establishing that this is the case, we study how investment differs for connected and isolated households. Specifically, we conjecture that the presence of an extended family may relax credit constraints, enabling connected households to invest in schooling, a high-return but costly good, more than isolated households when they become eligible for *PROGRESA* transfers. We also test whether the extended family protects its members' schooling against negative shocks, compared to what happens to isolated households that suffer similar shocks. If investment in high-return, high-cost goods such as schooling responds to shocks differently for connected and isolated households, investment in lower-returns, lower-costs goods such as poultry also does. Since we find evidence of differential investment response to income shocks for connected and isolated households, we also investigate whether this differential effect of the program transfers on investment is long-

[2]See, e.g., Rosenzweig and Stark 1989, Altonji, Hayashi, and Kotlikoff, 1992; Hayashi, Altonji, and Kotlikoff, 1996; Foster and Rosenzweig, 2001; Genicot and Ray 2003; La Ferrara, 2003; Karlan, Mobius, Rosenblat, and Szeidl, 2009; Ambrus, Mobius, and Szeidl, 2009, Ligon 1998, Fafchamps and Lund 2003, De Weerdt and Dercon 2006, Dubois, Jullien, and Magnac 2008, Mazzocco and Saini 2009. For the relation between genetic distance and income see Spolaore and Wacziarg (2009).

[3]In this dimension, our paper is also related to the literature that uses field experiments to study insurance, such as Cole, Giné, Tobacman, Topalova, Townsend and Vickery (2008), Cai, Fang and Zhou (2009), Giné, Townsend and Vickery (2008).

lasting.

Our main empirical results are as follows. First, connected households - about 80% of all village residents - are similar to isolated households at baseline, suggesting that time and risk preferences do not vary systematically for these two groups. The lack of differences at baseline is consistent with the hypothesis that, when all households are sufficiently poor, belonging to a village-based extended family does not relax credit constraints to high-return investment.

Second, when some households become eligible for *PROGRESA* transfers, the consumption of their ineligible relatives significantly increases by about 13%, while the consumption of unrelated ineligibles does not. This is consistent with the hypothesis that connected households share risk within their family network but not with isolated households. Moreover, connected households are almost fully insured against idiosyncratic income variations. We deduce that the extended family is an important risk-sharing informal institution in our setting.

Third, when we switch the focus from consumption to investment, we find that similar resource shocks have different effects on investment for connected and isolated households. When households become entitled to the *PROGRESA* grants – a positive income shock – children's school enrollment increases only for connected eligible households, and not for isolated eligible households. Conversely, the stock of poultry increases more for isolated than for connected households. Isolated households use the *PROGRESA* grants to purchase poultry, a liquid asset, rather than increase schooling because the funds are not sufficiently high to overcome credit constraints (as they cover only a fraction of the full cost of secondary education). When the household head falls ill – a negative income shock – school enrollment decreases more for isolated than for connected households, while the stock of poultry increases for both groups.

Our final set of results explores the long run implications on investment of having extended family members in the village when the local economy experiences positive shocks. We find that, in the four to five years since the beginning of *PROGRESA*, the change in the stock of human capital and poultry is bigger for connected than isolated households. This is consistent with our hypothesis that the extended family makes a more effective use of the cash transfers by pooling resources and relaxing credit constraints. This will likely increase its long-term consumption more than for isolated households.

Our main conceptual point is to consider the joint effect of the extended family on insurance and investment. This translates into considering how this institution affects the level and volatility of consumption depending on the economic conditions. We study this joint effect in the context of a developing country, where formal markets are almost non-existent and household consumption is close to subsistence levels. Therefore, the scope for informal risk-sharing is much bigger than in industrialized countries, as studied in, e.g., Altonji *et al.* (1992), Hayashi *et al.* (1996), and Blundell, Pistaferri and Preston (2008).

The strength of this paper is that the unique features of our data enable us to measure these effects in new ways and using weaker identification assumptions. First, we can map the village-based, extended family network, and not only subsets of a social network, as typically done from observational data (e.g. Fafchamps and Lund 2003). Unlike other data sets that measure only the size of the full extended family (within and outside one's village), such as MxFLS, we actually observe the socioeconomic characteristics of the entire network, and we can therefore link income shocks of a household to the consumption of its relatives.

Second, we can exploit an exogenous income shock and a partial population experiment to measure who benefits indirectly from this income shock and by how much. This helps identify the resource-sharing network and measure how close to full insurance this network is. This is a novel approach to testing for risk sharing between households. In Altonji *et al.* (1992), Townsend (1994), Hayashi *et al.* (1996), and most of the papers that followed them, for example, the risk-sharing tests start by assuming that pairs or groups of households share risk with each other, and then measure the extent of risk sharing or whether the full risk sharing assumption is rejected. In contrast, in principle our data enable us to directly measure whose consumption increases when a given household experiences an exogenous, positive income change, without having to make any assumption on the identity of the risk-sharing members ex-ante (although in practice we use it to test the hypothesis that family members share resources within each other but not with unrelated households). Moreover, our test indirectly sheds light on one of the mechanisms through which households cope with risk - in our case by sharing resources within one's extended family, and not, for example, through self-insurance. The standard risk-sharing test, conversely, informs whether one's consumption is affected by one's income shocks without distinguishing between, e.g., self-insurance and resource sharing.

Our results are an example of how informal institutions interact with the economic conditions in affecting long-term outcomes. Households with close relatives in the village are affected differently by *PROGRESA* than households without close relatives in the village but which are otherwise observationally equivalent - the ineligible members of the extended family consume more and the eligible members increase their children's secondary school enrollment, unlike for ineligible and eligible isolated households. That is, the same treatment has substantially different impacts on similar households depending on the presence of family members, a piece of data which is normally not collected. Therefore, our paper shows that it is important to collect data on the extended family and to design policies that take these institutional differences into account.

This paper is a natural follow-up to Angelucci and De Giorgi (2009), who find that cash transfers to eligible households indirectly increase the consumption of the rest of the village and that such an effect operates through insurance and credit. Unlike them, however, we study the role of the extended family in providing resources to smooth consumption and undertake investment in the presence of imperfect credit and insurance markets. The paper is also related to Angelucci, De Giorgi, Rangel and Rasul (2010), who focus exclusively on the effect of connectedness on secondary school enrollment. We build on that earlier work by introducing a theory of how extended family affects its members' consumption and investment behavior and by testing the implications of the model.

The paper is organized as follows. Section two presents a stylized framework in which we derive empirical predictions on consumption and investment behaviors. Section three describes the *PROGRESA* data, program features, and research design. Section four presents descriptive evidence on our empirical setting, extended family networks, and correlates of whether households are connected or isolated. Sections five to seven present tests of the hypotheses on consumption, consumption volatility, and investment respectively. Section eight discusses the broader policy implications of our findings and directions for future work. The Appendix contains proofs of results, additional descriptive evidence, and robustness checks.

2 Theoretical Framework and Testable Hypotheses

The goal of this Section is to discuss how the extended family may affect both the smoothness and the level of consumption by providing insurance and credit to its members and how we can test these hypotheses given our data.

2.1 Insurance and investment

Informal institutions can reduce consumption variance by providing insurance against income fluctuations. If households can pool resources, making informal loans and transfers to each other, their consumption is protected against idiosyncratic shocks. To make this point more formally, consider two pairs of risk-averse households who live in an exchange economy with no storage technology or leisure. The two pairs of households have the same income process. However, one pair pools income, so that each household's consumption depends on aggregate resources only and all idiosyncratic risk is insured away. The other pair lives in autarky, i.e. does not pool income, so each household consumes its own income in each period and state of the world. To simplify the exposition, we use here the same terminology (and notation) we adopt in the empirical analysis. Therefore, we call the resource-pooling pair connected or K and the autarkic pair isolated or O. In this scenario, (i) the optimal consumption of each connected household (but not of the isolated households) is a function of the other household's endowment and (ii) consumption variance is lower for connected than for isolated households. See, e.g., Mace (1991), Cochrane (1991), or Townsend (1994). One can nest these two cases in a more general model of partial insurance with transaction costs, as in Schulhofer-Wohl, (2008). The above scenario is broadly consistent with our empirical evidence, as we will show below.

Consider now what happens if households can invest, as well as consume. Investment goods differ in many dimensions, for example on how lumpy and liquid they are, and in the timing and riskiness of their returns. Two common (relevant) investment assets in our context are poultry and schooling. Poultry is likely less lumpy and risky, more liquid, and with more immediate but lower returns than schooling.

By pooling income, the extended family favors investment in schooling in two ways. First, by attenuating the credit constraints associated with the lumpier investment, it makes a connected household more likely to invest in schooling than in poultry. For example, a household with insufficient funds to send its children to school may use the extended family's resources to purchase school materials and cover transportation and opportunity costs (forgone earnings). Second, by providing insurance against income risk, the extended family makes a household less likely to withdraw its children from school to face negative income shocks, conditional on having enrolled them. In turns, this increases the likelihood of investing in schooling to begin with. By favoring investment in high-return assets, the extended family increases its future resources and consumption.

We show these points using a simple model. To do so, consider again the connected (income-pooling) pair of households and the isolated (autarkic) one. The households in each pair are called j and l. Each pair has the same income process and time and risk preferences. We assume that the income process exhibits some idiosyncratic variation within the pair. We rule out transfers between non-relatives, the labor supply is fixed, and in this model the saving technology is given by the investment goods. Households can then either consume or invest their income. Moreover, the connected households may make transfers to each other. We assume two different assets, poultry and schooling. We suppose for simplicity that the two investment goods differ only in the convexity, lumpiness, and in the size of their returns. Returns on poultry are linear, continuous, and relatively low; the returns to schooling are more convex, lumpy, and relatively larger. We focus on lumpiness because this is one relevant characteristic of investment which fits our empirical analysis. In the presence of credit constraints, any investment good with a non-negligible upfront cost may be classified as lumpy. Credit constraints are binding for many households in our data (see the evidence in section 3). The main effect of *PROGRESA*'s cash injection is to loosen credit constraints, while the program does not change the other aspects of investment in any sizable way, nor does it change the income process in the time considered (Angelucci and De Giorgi 2009).

We sketch this model in the Appendix. Here, we use Figure 1 to show how aggregate investment for the connected and isolated pairs varies as a function of household j's income, y_j (on the horizontal axis), keeping household l's income constant (and low, i.e. $0 < y_l \ll t^K$) and given the returns on the investments. Panel A of Figure 1 shows the hypothetical returns for the two goods, which are as described above.[4]

Panels B and C in Figure 1 show the aggregate schooling and poultry investments of the connected pair (black solid lines) and the isolated one (grey dashed lines) as a function of household j's income. The investment level is measured by the vertical distance between the horizontal axis and the black and grey lines for schooling and poultry. Investment is a discontinuous function of own endowment for the isolated pair and of aggregate endowment for the connected pair. Consider the behavior of the isolated household j first, and in particular how it varies with its income. For low income levels, this household invests in poultry only. As soon as its income reaches the threshold t^O, household j switches from poultry to schooling. Only when the marginal returns are equalized, i.e. when the investment capital exceeds the level corresponding to the red dotted line, does it hold both investment goods. The isolated household with the fixed and low income, household l, can invest only in poultry. Therefore, while household j switches from poultry to schooling, the aggregate investment in poultry remains positive because of household l's behavior. In sum, the isolated pair always invests in poultry and sometimes also in schooling, when the income of household j is high enough.

Aggregate investment for the connected pair follows the same broad pattern. The pair invests only in poultry for low levels of y_j, then switches from poultry to schooling, then adds poultry again when income is sufficiently high. However, there is one important difference. Since the connected pair pools income, it can reach the minimum income required to invest in schooling at a lower level of y_j's income. This threshold is t^K.

[4] We do not specify the unit of the vertical axis because what matters in this example are the relative returns.

In the interval $y_j \in (0; t^K)$, therefore, both households invest in poultry only and aggregate investment is the same for the two pairs. Conversely, in the interval $y_j \in (t^K; t^O)$, the connected pair invests in schooling only, while the isolated pair cannot and invests in poultry.[5] For $y_l \geq t^O$ both pairs of households can invest in both goods. Given the returns functions we specified, they will invest in schooling up to the point in which the marginal returns on the two investments are equalized.[6] Beyond that point, they resume investing in poultry.

The differences in investment over the income support have three implications. First, the effect of the same income shocks on investment may be different for the two pairs of households. Conditional on both pairs of households not having children in school, i.e. when $y_j < t^K$, positive income shocks (s) are more likely to enable the connected households to invest in schooling than the isolated households. For example, when $y_j + s \in (t^K; t^O)$, the shock has a positive effect on schooling and a negative effect on poultry for the connected households and no effect on schooling and a positive effect on poultry for the isolated households. Moreover, conditional on both pairs of households having children in school, i.e. when $y_j > t^O$, negative income shocks are less likely to cause the connected households to take their children out of school than the isolated households. For example, when $y_j - s \in (t^K; t^O)$, the shock causes the isolated households to withdraw their children from school (and increase their stock of poultry) more than for the connected households.

Second, there are no differences in investment for low levels of aggregate income. When $y_j < t^K$, neither pair of households invests in the high-return good. Being part of an extended family, therefore, does not help relax credit constraints when the entire network is sufficiently poor. In this case, while belonging to the network is useful for insurance purposes, as the extended family helps smooth consumption, it does not help undertake high-return, lumpy investments. The extended family is equally irrelevant when income is high enough, that is, when there are no credit constraints to begin with. This scenario, which corresponds to the far right of the graphs in Figure 1, is unlikely to occur in the poor and marginalized villages in our data.

Third, aggregate consumption may be higher for the connected pair, depending on the level of aggregate resources. When aggregate resources are sufficiently high to enable the connected households, but not the isolated households, to finance schooling by pooling resources, the connected households enjoy higher returns and therefore a higher future consumption. Given the same endowments, the isolated households can invest only in poultry, and their future consumption is lower.

2.2 Testable hypotheses

To map these results into our empirical setting we need introduce some key features of our data and research design (described in detail in Section 3). We have a census of 506 villages, some of which are randomly allocated to receive the conditional cash transfer (*PROGRESA*) between 1997 and 1999 and some of which start receiving the transfers at the end of 1999. We call these two groups of village treatment and control

[5] The fact that the connected pair holds no poultry between the two thresholds depends on assuming away risk. With uncertain returns, there is scope for holding both goods if their returns are not perfectly positively correlated.

[6] By pooling resources, the connected pair reaches the optimal level of schooling before the isolated pair. The difference in the level of household j's income at which schooling is optimal is $t^K - t^O$.

villages. In each village, we observe households eligible and ineligible for the program. We can therefore group households according to their eligibility status (eligible and ineligible) and village of residence (treatment and control). Only eligible households in treatment villages receive the program transfers, an exogenous income increase, in its initial phase.

Consider two pairs of households j and l living in the same village. One pair is part of the same extended family, or connected (K), while the other pair is unrelated, or isolated (O). If the connected pair shares risk efficiently and the isolated pair does not share risk at all (as we discussed in the previous section), the optimal consumption of a connected household is a positive function of both households' endowments but the optimal consumption of an isolated household is a function of its own endowment only. Hence, an exogenous shock to household l's resources affects the consumption of household j only if these households are connected. That is,

$$H_1 : \frac{\partial c_j^K}{\partial y_l^K} > 0 \text{ and } \frac{\partial c_j^O}{\partial y_l^O} = 0$$

This hypothesis forms the basis of our risk-sharing test. We consider ineligible households and measure whose consumption increases when eligible households' income increases exogenously. This test departs from the standard full risk-sharing test (e.g. Cochrane, 1991; Mace, 1991; Townsend, 1994) in two dimensions. First, in principle one could implement this test for individual households (for the ineligibles only) - provided they have a valid counterfactual in control villages - without having to assume a risk-sharing group ex-ante. We would have to consider each ineligible household in treatment villages and measure whose consumption increases after the eligible households start receiving the program transfers.[7] Second, this test indirectly sheds light on the mechanism through which households share risk - through informal transfers or quasi-credit, and not, for example, through savings. The standard risk-sharing test, conversely, informs whether one's consumption is affected by one's income shocks without shedding light on the mechanisms through which this happens. As anticipated, we find evidence consistent with hypothesis H_1 in Section 5.

After establishing this evidence, we test the hypothesis that connected households are fully insured against idiosyncratic risk. To do that, we regress the growth in household consumption on the growth in aggregate consumption and household income using the following specification,

$$\Delta \ln c_{ht} = \beta_1 \Delta \overline{lnC_t} + \beta_2 \Delta lny_{ht} + u_{ht}, \tag{1}$$

where Δlny_{ht} is the endowment growth for household h at time t and $u_{ht} = \Delta\epsilon_{ht}$ is an error term derived from assuming a multiplicative error in the measurement of consumption, $c_{ht} = c_{ht}^* e^{\epsilon_{ht}}$. Under the null hypothesis that idiosyncratic risk is fully insured, consumption growth does not depend on the growth of the idiosyncratic

[7]This test is correct only in the absence of general equilibrium effects of the program, as discussed in Angelucci and De Giorgi (2009).

component of the endowment, that is:

$$H_2 : \beta_2 = 0$$

(see, e.g., Mace 1991 or Townsend 1994). We test this hypothesis in Section 6. As anticipated, we also find evidence consistent with hypothesis H_2 in our data.

We then study how investment differs for connected and isolated households. First, compared with the isolated, at baseline the connected should have (i) higher aggregate consumption, (ii) higher levels of high-return, lumpy capital, such as schooling, and (iii) lower levels of low-return capital, such as poultry. However, if all households are sufficiently poor, these differences may be negligible. This is because the financing potential of the extended family cannot be exploited if the aggregate resources are too low to invest in high-return goods. We verify that the characteristics of connected and isolated households at baseline match these predictions in Section 3.

Second, the effects of income shocks on investment should differ for connected and isolated households. Both households would want to invest in schooling when they have sufficient funds to undertake the investment and to protect it from negative income shocks. However, connected households can be more successful at this because they have two advantages over isolated households: they can obtain financing capital from their relatives when they are credit constrained but the aggregate network resources are high enough and they can insure their investment against negative shocks by pooling resources with their relatives. This will likely generate an asymmetric response to income shocks. With positive shocks, the connected are more likely to invest in schooling than the isolated. With negative shocks, the connected are less likely to withdraw their children from school than the isolated. The effect of income shocks on poultry are harder to predict, as they depend on both the shock and the investment in schooling. We test and find evidence consistent with the following hypothesis in Section 7:

$$H_3 : \frac{\Delta I_h^K}{\Delta y_h^K} \neq \frac{\Delta I_h^O}{\Delta y_h^O}, \quad \text{The same income shocks have different effects on } K, O \text{ households } h.$$

Third, since connected households can pool resources and finance the lumpy, high-return investment more frequently than isolated households (provided the aggregate resources are high enough), their future consumption, C, is likely higher than for the isolated. While our data are not ideal for this test, since our panel is relatively short and the changes in consumption are confounded by the *PROGRESA* transfers, we find evidence that the differential effect of the program transfers on investment is long-lasting in Section 8. Long-lasting differences in investment are likely to cause differences in future consumption levels.

3 Data

3.1 Key Features of *PROGRESA*

We use household panel data collected to evaluate the *PROGRESA* anti-poverty program in rural Mexico. This is an appropriate setting in which to test our hypotheses because households face substantial risk and credit and insurance markets are imperfect.[8] The data are a census of households in 506 villages from seven states in rural Mexico. Baseline data were collected in September 1997 and households were re-surveyed approximately every six months until November 2000, with a final wave in November 2003. We observe detailed information on food consumption, school enrollment, and poultry. Consumption and school enrollment are measured in six waves, assets and livestock are recorded in five waves. We have complete data for 22,500 households in each wave up to November 1998, 20,000 up to November 2000, and 19,000 in 2003.[9]

PROGRESA is a cash-transfer program that targets poor households. These transfers are substantial – 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 (Angelucci and De Giorgi 2009). About 75% of households are classified as eligible based on their poverty status as computed in September 1997.[10]

The transfer is in theory conditional, as households have to send their children to school and have periodic health checks to become entitled to the grant. However, these constraints are not binding for most households as the health checks are infrequent and almost all of the primary school children would have gone to school regardless of the program (around 95% of primary school kids go to school in control villages, see, Skoufias, 2005, and Angelucci *et al.*, 2010). In contrast, the transfer conditionality is actually binding for households whose eligible secondary school-age children would have not gone to school and worked full time 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 transfer, because the secondary school transfer amounts to only around two thirds of full-time child wage (Schultz, 2004). For such households, the transfer may be too low to increase their children's secondary school attendance. This distinction is key to assessing how eligibility differentially affects secondary school enrollment for connected and isolated households.

Of the 506 villages in the data, 320 were randomly assigned to receive *PROGRESA* transfers between May 1998 and November 1999. The remaining villages started receiving transfers at the end of 1999. The evaluation data contain information on the eligibility status of all households in treatment and control villages. Therefore, we can group households depending on where they live (treatment or control), their status (eligible

[8]Although unique in some aspects, *PROGRESA* is one of many programs targeting the poor in rural Mexico. Thus, we consider its introduction as one of the states of the world on which households write implicit contracts, rather than an entirely new occurrence.

[9]The attrition rate is similar in both treatment and control villages.

[10]The initial allocation of households between eligible and ineligible status was revised just before the program roll-out. However, in the first year of the program most of the re-classified households did not receive any grant because of an administrative error. While we use these re-classified households to construct the extended family links, we exclude them from our analysis. All monetary values are reported at November 1998 prices. 10 pesos correspond to around US$1 in the late 1990s.

and ineligible) and, as we detail in the next section, the presence of close relatives in the village (connected and isolated).

As Table A1 shows, there are two data waves pre-program, three waves during which a random subset of villages are treated, and three waves during which all villages are treated. The take-up rates for at least one component of the program among eligible households are over 90%.

In the sampled villages, agriculture is the main activity, incomes are volatile, and there is little crop diversification, suggesting a high need for insurance. Yet, there are few formal credit or insurance institutions (see Angelucci and De Giorgi 2009 for details). Credit constraints likely bind for many households and appear related to poverty and lack of collateral.[11]

Despite the lack of formal insurance and credit markets, consumption is more stable than income and idiosyncratic risk is not fully insured at the village level.[12] Hence it is worth exploring whether subgroups of households are better insured than others.

There are at least three good reasons to focus on family networks, rather than friendship networks, say. First, evolutionary biology suggests preferences are defined over the family dynasty. This literature and some of the anthropological literatures on this are discussed in Cox and Fafchamps (2008). Second, there are specific inter-generational investments that have no counterpart in friendship ties and provide long run incentives for family members to reciprocate in resource sharing arrangements. Finally, the transactions costs of sharing resources with non-family members may be higher because it is both more costly to observe outcomes outside the family network, and fewer mechanisms exist by which to punish non-family members that renege on such arrangements (La Ferrara 2003). Therefore, while households might share risk with non-family members, there are good reasons to focus on identifying resource-sharing arrangements among extended family networks, especially when such networks change only slowly. Our data enable us to do this while exploiting large and exogenous income changes.

3.2 Construction and Characteristics of Extended Family Networks

To identify the family links between households in the same village, we exploit information on surnames provided in the third wave of data, together with the naming convention in Mexico.[13] Mexicans use *two* surnames – the first is inherited from the father's paternal lineage and the second from the mother's paternal lineage. For example, former Mexican president Vicente Fox Quesada would be identified by his given name (Vicente), his father's paternal name (Fox) and his mother's paternal name (Quesada). In the evaluation

[11]For example, the share of households with loans in the most and least marginalized control villages (lowest and highest quartiles) are 1.8% and 3.6% in November 1998. In the same time period, land owners in control villages are 32% more likely to have loans than landless households. Investments with high returns but large upfront costs, such as international migrations, are very low and positively correlated with income (Angelucci, 2011).

[12]The longitudinal coefficient of variation of consumption is half that of income, suggesting households engage in informal resource-sharing activities. When we estimate equation (8) at the village level, the coefficient on the growth of household income, $\hat{\beta_2}$, is 0.025 and is significantly different from zero at conventional levels. With full insurance, this coefficient is zero.

[13]One concern is that the program may affect the migration of the household head or of his spouse. However, only .4% (.5%) of households in wave 3 (5) report having a migrant head or spouse. Moreover the share of households with such migrants does not differ across treatment and control villages.

data, respondents were asked to provide the – (i) given name; (ii) paternal surname; (iii) maternal surname, for each household member. Hence couple-headed households have four associated surnames – the paternal and maternal surnames of the head, and the paternal and maternal surnames of his wife. Figure 2 illustrates the matching algorithm. Consider household **A** at the root of the family tree. The head of the household has paternal and maternal surnames $F1$ and $f1$ respectively. His wife has paternal and maternal surnames $F2$ and $f2$ respectively.

The children of the couple in household **A** will adopt the paternal surnames of their father ($F1$) and mother ($F2$). Hence we assume there is a parent-son relationship between households **A** and **B** if – (i) the paternal surname of the head in household **B** is the same as the paternal surname of the head in household **A** ($F1$); (ii) the maternal surname of the head in household **B** is the same as the paternal surname of the spouse in household **A** ($F2$). Parent-daughter and intra-generational family ties between siblings can also be identified. We impose the following restrictions when defining family links – (i) inter-generational links exist when the relevant individuals have at least 15 years age difference, and no more than 60 years age difference between mother and child; (ii) intra-generational links exist when the individuals have at most 30 years age difference.

Our matching algorithm emphasizes close family ties, both in terms of consanguineous and geographic proximity. For example, two households headed by cousins are classified as belonging to the same extended family only if both sets of parents are present. Similarly, a household may have its extended family in the adjacent village, but still be classified as isolated.

Some forms of measurement error from using two surnames in our matching algorithm might still be present and would cause us to incorrectly label connected households as being isolated and vice versa. All else equal, this makes it harder to identify significant differences between connected and isolated households. The Appendix provides evidence of our matching algorithm's reliability and the potential incidence of some forms of measurement error.

About 80% of couple-headed households are connected and the remaining 20% isolated. There are no significant differences in the extended family links of eligibles and ineligibles between treatment and control villages. There are 1379 (817) family networks in treatment (control) villages covering 10559 (6471) households. On average there are around 7.8 households in each family, so the average village has around six family networks. Around 50% of extended family networks comprise a mix of eligible and ineligible households, and this rises to two thirds when we consider networks of more than two households. For more details, see Angelucci *et al.*. 2010.

Table 1 shows the mean and standard deviation of household socioeconomic variables and village characteristics, measured mostly at baseline (September 1997). Columns 1 and 2 split these variables between connected and isolated households, focusing on couple-headed households. Column 3 reports the p-values of the differences between the two, allowing for within-village clustering.

There are two broad results. First, connected and isolated households look remarkably similar and live in similar villages.[14] Out of the 38 variables we consider, only 5 differ at the 95% confidence level. For the variables that differ, the magnitude of the differences is generally rather small. In particular, labor income, food expenditures, and the wealth index – the basis for *PROGRESA* eligibility – are almost identical, as are adult and child labor, school enrollment, household composition, as well as ownership of most assets. 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 eight years old, and found no significant differences.[15] These similarities suggest that connected and isolated households do not have systematically different time and risk preferences.

Second, the few differences that we detect are consistent with our theoretical conjectures, as isolated households have lower rates of durable goods ownership and lower levels of lumpy, higher-risk, illiquid investment. In particular, isolated households are 16% less likely to have temporary U.S. labor migrants, 6% and 9% less likely to own stoves and TV sets. Conversely, they own 12.5% more poultry, the most liquid form of livestock in this environment.[16]

Taken together, the many similarities and few differences in the data are consistent with our conjecture that connected and isolated households do not differ considerably when aggregate resources are sufficiently low. While the extended family has the potential to finance higher levels of high-return, but lumpy, illiquid, high-risk investments such as migration, which in turns generates higher aggregate consumption, it requires a high enough aggregate income to do so. In sum, before the advent of *PROGRESA*, the behavior of connected and isolated households is similar to the one depicted in the left side of Figure 1.

We exploit the random assignment of *PROGRESA* across villages in a few of our tests. Hence, the remainder of Table 1 (Columns 4 and 5) reports p-values of the differences in observables between households in treatment and control villages. This is done separately for connected and isolated households. Given the randomization, most of the variables do not have statistically different means in control and treatment villages at the 95% confidence level. Indeed, out of 76 tests of mean equality, only 4 tests – about 5% of the total – have p-values lower than 0.05.

The few exceptions are the number of oxen and televisions owned by connected households, the first of which is lower and the second of which is higher in treatment villages, and the number of household members aged 19 and older, also higher in treatment villages. The magnitude of these differences is not large. Two more important differences are the ones in the school enrollment rate for children aged 11 to 16 for the isolated and the weekly hours of child labor for the same age group for the connected. The enrollment rate for the

[14] We choose three key village-level variables: village wealth, an index of food prices, and the village marginality index. The food price index is the unweighed average of the median village price of 36 food categories. The village marginality index, constructed by the Mexican administration, is a score that grows with both poverty and geographic isolation of the village. Unfortunately we do not know the exact location of each village and its remoteness. We believe these three variables represent important correlates of the economic conditions of a village.

[15] 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 (.12%), working for a family business without pay (4.4%), and *ejidatario* (2.5%).

[16] The 2.5 year age difference between household heads is probably caused by parents of older isolated households being less likely to be alive. The difference in their literacy rate is caused by a cohort effect. The differences in asset ownership and investment rates are significant also conditional on the age of the household head.

isolated and the hours of child labor for the connected are 9% and 12% lower in treatment villages. Since these latter two variables are important outcomes in some of our regressions, it is important to mention that we use double- and triple-difference estimators. Therefore, our key identification assumption is that connected and isolated households in treatment villages would have experiences the same trend as the one observed in control villages. Under this assumption, any additively-separable time-varying and time-invariant difference between connected and isolated households in control and treatment villages cancels out. Moreover, our specifications control for a series of households and village characteristics which improve the precision of the estimates of our parameters of interest.

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 networks, 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 would be less (more) than one. The computed ratios are centered around one, rejecting the hypothesis of assortative matching. This finding confirms one cannot easily predict whether a household is connected or isolated by looking at its observable characteristics.

4 Identification of the Effects and Potential Endogeneity of Family Networks

The process under which extended family networks are formed, and to what extent this correlates with our outcomes of interest, is unclear. While Jackson and Rogers (2007) show that the structure of social networks of friendships and romantic relationships is well explained by a process of random link formation, network formation may not be random in our setting. However, this process has multiple determinants beyond insurance and investment. For example, the structure of family networks is also shaped by the nature of household production and the operation of land markets, behavior within marriage markets, mortality outcomes, fertility choices, and the provision of bequests (Rosenzweig and Stark 1989, Foster 1993, Foster and Rosenzweig 2002, Banerjee, Duflo, Ghatak and Lafortune 2009, Munshi and Rosenzweig 2009). On a similar note, Fafchamps and Gubert (2007) present evidence on the formation of risk sharing networks in the Philippines. They find that proximity is a more important driver of the formation of such networks than matching across different occupations. Udry (1994) finds that geographic proximity and family ties account for 97% of the loans (cum insurance) in Nigerian villages.

Regardless of one's prior about the unobserved determinants of family formation, one of the strength of our research design is that we use panel data from a partial population experiment (Moffitt 2001). As Figure 3 shows, the program is initially implemented in a random set of villages and in each village we observe households potentially eligible and ineligible for the program. This design has two major advantages over observational data. First, it enables us to identify and estimate the causal effect of being eligible for the *PROGRESA* transfer on the behavior of both ineligible households (the basis of our test of hypothesis H_1) and eligible households (whose investment behavior we look at to test hypothesis H_3) under weak identification assumptions. Second, most of our tests are robust to the potential endogeneity of the family network, as long as it is additive. This is because the program transfers are randomly allocated in our data and we have multiple observations for the same household. The research design enables us to control for both time-invariant and time-varying unobservables correlates of family formation as long as they are additive.

We first lay down the notation and define our parameters of interest. We term our outcome of interest y, depending on the test this could be either consumption or investment. Define a dummy $P_h = 1$ if household h resides in a village where *PROGRESA* is implemented, and the treatment is the existence of *PROGRESA* grants to eligible households ($N_h = 1$) in treatment villages. Define y^f_{1h} and y^f_{0h} as the potential outcomes for household h of type $f = K, O$ in treatment villages in the presence and absence of the treatment. Δy^f_{1h} and Δy^f_{0h} are the after (t_1) minus before (t_0) the program starts differences in potential outcomes for the different household types, e.g. $\Delta y^f_{1h} = y^f_{1h,t_1} - y^f_{1h,t_0}$.

It is possible to write the potential outcomes in a general form as:

$$y^f_{1ht} = \mu^f_{1t} + u^f_{1ht}, \tag{2}$$

$$y^f_{0ht} = \mu^f_{0t} + u^f_{0ht}, \tag{3}$$

where the parameter μ is a common component within household type, while the heterogenous parts are the sum of θ^f, a household fixed effect, λ^f, an unobserved trend, and ϵ^f, a time-varying, household-specific shock: $u^f_{1ht} = \theta^f_{1h} + \lambda^f_{1t} + \epsilon^f_{1ht}$, $u^f_{0ht} = \theta^f_{0h} + \lambda^f_{0t} + \epsilon^f_{0ht}$. Importantly, connected and isolated households may differ in these three parameters (e.g. they may have different time or risk preferences, θ, different trends, λ, and different time-varying shocks, ϵ).

We refer to the average effect of the treatment on eligible households residing in treatment villages as the Average Treatment Effect on the Eligibles (ATE). Similarly, we refer to the average effect of the treatment on ineligible households as the Indirect Treatment Effect (ITE). We define these parameters as follows,

$$\begin{aligned} ATE(y)^f &= \Delta\mu^f_1 - \Delta\mu^f_0 = E(\Delta y^f_{1h}|P_h = 1, N_h = 0) - E(\Delta y^f_{0h}|P_h = 1, N_h = 0), \\ ITE(y)^f &= \Delta\mu^f_1 - \Delta\mu^f_0 = E(\Delta y^f_{1h}|P_h = 1, N_h = 1) - E(\Delta y^f_{0h}|P_h = 1, N_h = 1), \end{aligned} \tag{4}$$

where $f \in \{K, O\}$. Under the assumption of random assignment and no program spillovers into control villages, the expected value of the change in potential outcome in the absence of the treatment, Δy^f_0, is the same in both treatment and control villages, i.e. $E(\Delta y^f_{0h}|P_h = 1, N_h = j) = E(\Delta y_{0h}|P_h = 0, N_h = j)$, for $j \in \{0, 1\}$.

Therefore, the differences

$$ATE(y)^f = E(\Delta y_h^f | P_h = 1, N_h = 0) - E(\Delta y_h^f | P_h = 0, N_h = 0), \quad (5)$$

$$ITE(y)^f = E(\Delta y_h^f | P_h = 1, N_h = 1) - E(\Delta y_h^f | P_h = 0, N_h = 1), \quad (6)$$

identify the ATE^f and ITE^f.

Testing hypotheses H_1 and H_3 consists of identifying and estimating ITE's and ATE's. In order for these tests to be valid and the parameters identified, as shown in (5) and (6), we have to maintain that $E(\Delta u^f | P_h = 1, N_h = j) = E(\Delta u^f | P_h = 0, N_h = j)$. This holds as long as there is a common trend at the village level, i.e. the change in outcomes in treatment villages would have been the same as the observed change in outcome in the control villages in the absence of the treatment while still allowed to differ according to f. In this case, all additive time-varying and time-invariant unobservables in the outcome equations cancel out. That is as long as unobserved heterogeneity enters additively, our strategy gets rid of any endogenous determinant of family networks. Hypothesis H_2 does not involve a comparison of connected and isolated households. Therefore, this discussion does not apply to it. The assumptions under which we can identify and estimate causal effects when measuring the differential effects of the program transfers on investment are long-lasting are different. We discuss them in the relevant section.

Before describing our tests and their findings, note that different tests use different sub-samples in our data. To test hypothesis H_1, we will use information on connected and isolated households ineligible for PROGRESA. To test H_2, we use the sub-sample of all connected households. To test H_3, we use data on connected and isolated households eligible for PROGRESA. Lastly, to test whether the differential effects of the program transfers on investment are long-lasting we use data on all connected and isolated households in our sample. One implication of using these different sub-samples is that we have samples with different sizes. In addition, different outcomes are measured at different points in time in our data. Table A1 in the Appendix shows, by panel wave, the availability and definitions of the key variables we exploit. Table A2 shows the sample households, the data waves used, and the estimation method for each set of results.

5 Do Family Networks Share Resources?

The first step in the empirical analysis is to test whether connected and isolated ineligible households increase consumption when eligible households become entitled to *PROGRESA* transfers.

Our first hypothesis, H_1, states that for a given pair of households $h \in \{j, l\}$, the consumption of household j is a function of household l's income, $\frac{\partial c_j}{\partial y_l} > 0$, *only* if the households share resources. We hypothesize this occurs within extended family networks. This hypothesis is typically hard to test as plausibly exogenous variations in income are rare. We use this to first establish whether households share resources only with their extended family or with the entire village, under weak identifying assumptions.

Testing this hypothesis consists of estimating consumption ITEs for connected and isolated ineligible households. If connected households share risk only with family members, then when eligible households receive *PROGRESA* transfers, ineligibles' consumption will increase only for the connected, $ITE(c)^K > 0$, and not for the isolated, $ITE(c)^O = 0$. We identify $ITEs$ from the following regression, estimated for ineligible households h in time period t,

$$c_{ht} = \delta_0 + \delta_1 P_h + \delta_2 O_h + \delta_3 T_t + \delta_4 (P_h \times O_h) + \delta_5 (P_h \times T_t) + \delta_6 (O_h \times T_t) + \delta_7 (P_h \times O_h \times T_t) + \delta_8 x_h + u_{ht}, \quad (7)$$

where c_{ht} is food consumption per adult equivalent.[17] Using data from September 1997 until November 1999, we compute monthly food consumption per adult equivalent from seven-day recall data based on 36 food items. At baseline, food consumption accounts for about 72% of total non-durable consumption.[18]

P_h is a dummy equal to one if the household resides in a *PROGRESA* village, and equal to zero in control villages; T_t is a time dummy equal to one for post-program waves of data, and is zero for baseline data; O_h is a dummy equal to one if household h is isolated, and zero if it is connected; x_h includes the age, gender, and literacy of the household head; the number of household members in age bins; the household wealth index, livestock ownership, temporary U.S. migration, the village wealth and marginality indexes, all measured at baseline; time and region dummies; u_{ht} is a disturbance term. The set of x_h variables encompasses the covariates that differ across connected and isolated households; in this way, we control for correlates of consumption that depend on these covariates as well as for village characteristics that might be relevant for our analysis. However, note that, while adding these variables likely increases the precision of the estimate, it does not affect the estimation of our parameter of interest, as long as these covariates are not systematically different in treatment and control villages (as shown in Table 1). In the estimation we cluster the standard errors at the village level, which is the level at which PROGRESA operates, to capture any common shocks to consumption across households within the village.[19]

Under the previously discussed assumptions, the parameters δ_5 and $\delta_5 + \delta_7$ identify the ITEs for connected and isolated households and their difference may be interpreted as caused by the presence of the extended family network.

Table 2 presents the results. Column 1 shows the baseline estimate of (7). In line with H_1, the indirect effect of *PROGRESA* on monthly household consumption per adult equivalent is positive and significant for households with eligible relatives resident in the same village ($ITE(c)^K > 0$), its point estimate is 26.4 *pesos*, and there is no significant indirect effect on the consumption of isolated households ($ITE(c)^O = 0$). As reported at the foot of Table 2, the differences in ITEs in Column 1 is positive and significant. Hence

[17] The adult equivalence scale used for consumption, derived in Di Maro (2004), is 1 for members 18 or older, and 0.73 otherwise.

[18] For each item, the data contain information on the quantity consumed and whether it was purchased, donated, or produced by the household. We also observe (non-durable) non-food consumption. We do not focus on this latter variable because it is imprecisely measured: the recall period is longer than for food consumption, 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 households. We drop the first wave after *PROGRESA* started because Angelucci and De Giorgi (2009) show there are no significant ITEs a few months after the program starts. This is because *PROGRESA* had transferred very little money by November 1998.

[19] See Bertrand, Duflo and Mullainathan, 2004.

households that are not themselves directly eligible for *PROGRESA*, appear to have significant increases in consumption if they have extended family members in the same village. In contrast, ineligible households without any relative in the village have no significant change in consumption.

Column 2 explores the hypothesis that consumption for ineligible households increases only when they have eligible relatives, regardless of whether they are connected or isolated. To do so, we group ineligible households depending on whether they have at least one eligible family member in the village or not. The ITE is positive and significant only for ineligible households with eligible relatives in the village, for whom it amounts to 26.9 *pesos*. Again, the difference in the two ITEs is positive and significant.[20]

The average counterfactual consumption is about 200 *pesos*. Hence consumption for ineligible but connected households increases by about 13%.[21]

The Appendix provides two pieces of supportive evidence that extended families share risk in this setting. There we show that the ITE on consumption does not vary by two potential alternative sharing networks, defined by land ownership or ethnicity, and we discuss how household consumption responds to another resource shock in the network – the self-reported illnesses among other household heads in the family network.

Finding that consumption increases for ineligible relatives ($ITE^K > 0$), but not for ineligible non-relatives ($ITE^O > 0$) of eligible households is evidence that within-village risk sharing occurs among relatives only.

6 Family Networks and Income Risk

After having established that connected households share risk with each other, but not with isolated households, we are interested in testing whether connected households are fully insured against idiosyncratic risk. This is our hypothesis H_2. To test this hypothesis, we regress the growth in household h consumption, $\Delta \ln c_{ht}$, on the growth in aggregate consumption, $\Delta\overline{lnC_t}$, and household income, $\Delta \ln y_{ht}$, using the following specification,

$$\Delta \ln c_{ht} = \beta_1 \Delta \overline{\ln C_t} + \beta_2 \Delta \ln y_{ht} + u_{ht}, \tag{8}$$

where $u_{ht} = \Delta\epsilon_{ht}$ is an error term derived from assuming a multiplicative error in the measurement of consumption, $c_{ht} = c^*_{ht} e^{\epsilon_{ht}}$. Under the null hypothesis that households are fully insured against idiosyncratic risk, consumption growth does not depend on the growth of the idiosyncratic component of the endowment,

[20] An alternative explanation for the observed increase in consumption is that *PROGRESA* increases ineligible households' income through changes in local wages or an aggregate demand shock. However, with such general equilibrium effects, we would expect consumption to increase also for isolated ineligible households, which is not the case. Angelucci and De Giorgi (2009) present more detailed evidence that there are no indirect treatment effects on labor and goods income and that prices do not change differentially between treatment and control villages. For an analysis of price effects of cash and in-kind transfers see Cunha, De Giorgi and Jayachandran (2011).

[21] A caveat to the results in Column 2 to 4 is that 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, the estimated effects might capture the impact of some unobservables that correlate with the share of eligible in a family network and drive changes in food consumption over time. To ease such concerns we also 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 draw a consistent picture: among ineligibles, only households with eligible relatives increase their food consumption.

and $\beta_2 = 0$ (see, e.g., Mace 1991 or Townsend 1994).[22]

To test this hypothesis, we use data on all connected households in our sample, regardless of their eligibility status for *PROGRESA*. Table 3 presents the results for connected households using two alternative definitions for aggregate resources: network level consumption (Panel A); and network-by-wave dummies (Panel B), as suggested by Ravallion and Chaudhury (1997). Column 1 shows that for all connected households we reject the null of efficient risk sharing, $\beta_2 = 0$. However, while household consumption co-varies with own income, conditional on network resources, the magnitude of the effect is small: the estimate of β_2 is about 0.02-0.03 in both specifications. This is likely an upper bound to the true parameter, if risk preferences are heterogenous and income is endogenous (Schulhofer-Wohl 2008, Mazzocco and Saini 2009). Given these endogeneity concerns, we also estimate the above regressions using instrumental variables. We use the household head's self-reported illness during the previous month. And to deal with potential measurement error we use the (within transformation) lag of household income. We report the IV estimates of β_2 in columns 2 and 3. As expected, the IV estimates of β_2 are smaller than its OLS estimates. Moreover, they are not statistically different from zero, meaning that we cannot reject the null hypothesis that the extended family network achieves full insurance. In sum, the pattern of coefficients suggests that transaction costs among extended family members are rather small, if not negligible.

7 The Effect of Income Shocks on Investment

The next step in our analysis is to test whether income shocks affect investment differently for the connected and the isolated (hypothesis H_3). As emphasized earlier, positive income shocks are likely to relax credit constraints more for the connected. Therefore, these shocks likely increase schooling more and the stock of poultry less for the connected than the isolated. Conversely, negative income shocks hurt investment more for the isolated, who are less likely to receive informal insurance, and therefore decrease schooling more and probably increase the stock of poultry more than the connected.

We use *PROGRESA* transfers (eligibility) as positive income shocks. As mentioned, the experimental design ensures this shock is exogenous. We use the household head's self-reported illness during the previous month as negative income shock. The idiosyncratic nature of health shocks makes them well suited to study extended families' insurance mechanisms. While the household head's health status may not be exogenous, we find that it is uncorrelated with the household wealth index, once we control for the covariates x_h included in (7). Moreover, the incidence of illness for the household head is not statistically different between connected and isolated households, as shown in Table 1.[23]

To test hypothesis H_3 we need to estimate the average treatment effects (ATEs) of the two shocks on the investment behavior of connected and isolated households and measure whether they differ from each

[22] As we do not identify which households the isolated share risk with, if any, we cannot estimate the corresponding regressions for isolated households.

[23] We do not use weather shocks or natural disasters because these shocks are unlikely to be idiosyncratic within an extended family in a given village.

other. The first set of ATEs is the effect of *PROGRESA* eligibility. If we define illness the 'treatment' and we assume that it occurs at random, conditional on the covariates in (7), the second set of ATEs is the effect of household head illness. To do so, we estimate the following specification for household h in time period t,

$$\Delta y_{ht} = \theta_0 + \theta_1 \Delta S_{ht} + \theta_2 O_h + \theta_3(\Delta S_{ht} \times O_h) + \theta_4 x_h + \epsilon_{ht}, \tag{9}$$

where Δy_{ht} measures changes in physical and human capital and livestock, as described below; S_{ht} corresponds to the measure of the positive or negative income shock, (eligibility for *PROGRESA* and illness of the household head); O is a dummy equal to one if household h is isolated, and zero if it is connected; the other controls in x_h are the previously defined baseline variables.

If the identification assumptions discussed in section 4 hold, the parameters θ_1 and $\theta_1 + \theta_3$ identify the ATEs for connected and isolated households and their difference can be interpreted as the causal effect of belonging to an extended family network. In addition, this equation allows for differential trends in the investment outcomes for isolated and connected households, as well as differential trends for households with different observables.

Our samples are different in the two sets of regressions. To estimate the differential effects of positive shocks on investment we use only households eligible for *PROGRESA*, while to estimate the differential effects of negative shocks on investment we use the entire sample. Moreover, the specifications using *PROGRESA* transfers use data from September 1997 to November 1999 (using only one wave per school year when school enrollment is the outcome). The specifications using illness of the head use data from November 1998 to November 2003, with some exceptions (as we explain in Table 4).[24] As before, we cluster the standard errors at the village level.

Our outcomes (y_{ht}) are the stock of poultry and secondary school enrollment rates. Schooling is lumpier and has higher returns than holding poultry. A chicken costs 25 *pesos* (in November 1998 prices, the first available), 9% of monthly labor earnings per adult equivalent, and eggs are sold at 12 *pesos* per kilo. Secondary education entails higher upfront costs to purchase books, shoes, and a uniform, plus (and importantly) forgone earnings of up to two thirds of adult full time wages (Schultz, 2004) and a return 70% higher than the return to primary school (López-Acevedo 2001), provided an individual goes to school for at least 9 years (or for at least three additional years besides primary school). We measure school enrollment as the share of 11-16 year old children attending school.

At baseline, the secondary school enrollment rates are 65.7% and 64.4% and the number of chickens owned is 8 and 7 for isolated and connected households. Table 4 presents the estimates of the parameters θ_1 and $\theta_1 + \theta_3$ from equation (9), which are the ATE^K and ATE^O, as well as their difference. The upper panel shows that, when households become eligible for *PROGRESA* transfers, school enrollment increases by 7.8 percentage points for connected households and this effect is statistically significant (column 1). In contrast,

[24] See Table A2 for further details. The reported results are unchanged if we estimate the effect of health shocks on investment restricting the sample to the one used to estimate the effect of *PROGRESA*.

the effect on school attendance is negligible and not statistically significant at conventional levels for isolated households. Under this metric, the program has positive effects only for connected households and no effect for isolated households, despite offering both groups of households the same financial incentives to increase secondary school attendance. We also estimated the effect of the positive income shocks on child labor for the same children. Time spent working significantly decreases by about a quarter of a day per week among connected households, and has a negligible and statistically insignificant change for isolated households.

How do the isolated then spend the unconditional component of *PROGRESA* transfers? These households increase their stock of poultry more than the connected (by 0.27 and 0.16 respectively), although this difference is imprecisely estimated (column 2). As the return to poultry ownership is likely lower than the return to secondary education, these results are consistent with our conjecture that isolated and connected households respond differently to the same positive income shock because being part of an extended family helps relax credit constraints and use the available capital in the most effective way. Our interpretation of this finding is that, since the program transfer does not cover the full cost of secondary education, only connected households are able to significantly increase their children's school attendance, because they can pool resources within the family network.[25] Conversely, this partial school subsidy is not high enough to relax credit constraints for the isolated, who cannot pool resources with other village residents and use the subsidy to increase the stock of poultry. Consider Figure 1. The *PROGRESA* transfer is sufficiently large to move some connected households with income $y < t^K$ to the right of the first and second discontinuity, in which case both schooling and poultry increase, but not sufficiently large to move the isolated households with income $y < t^O$ to the right of the first discontinuity, in which case only the stock of poultry increases, and by more than the increase for the connected.

The lower panel of Table 4 shows that school enrollment is significantly more responsive to negative income shocks among isolated households than among connected households (column 1). While the enrollment of 11-16 years old decreases for all households with an ill head, the drop is only one percentage point for the connected and not statistically significant, but more than four times as large and significantly different from zero for the isolated.[26,27]

When the household head falls ill, the stock of poultry increases for both connected and isolated households (column 2). Consider again Figure 1. These findings are consistent with some households responding to the negative shock by moving from the right to the left of the first discontinuity, which causes a drop in schooling and an increase in poultry. However, this shift is more pronounced for isolated than connected households.[28]

[25] 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.* 2010). In unreported regressions we confirm that the effect of becoming eligible for the program vary depending on the amount of conditional versus unconditional transfers. As expected, the unconditional transfers produce income effects.

[26] We also checked whether the drop in enrollment associated with illness of the household head is correlated with child illness. We find no evidence in support of this, and therefore the results are unlikely to be picking up the effect of contagious diseases.

[27] The number of weekly workdays for the same set of children significantly increases by 0.077 for the connected and by 0.283 (more than a day of work per month) for the isolated, when the household head is hit by a health shock.

[28] In unreported regressions, we find that the stock of goats and pigs decreases in response to a health shock. This change in livestock composition is consistent with the hypothesis that households respond to negative shocks by re-allocating their livestock

In sum, these findings are consistent with our hypothesis that the extended family may favor investment by relaxing credit constraints and by protecting the investment against idiosyncratic shocks. The provision of credit and insurance enables connected households to invest in lumpy but high-return goods more than the isolated, when the extended family's aggregate resources are sufficiently high. These results echo Kinnan and Townsend's (2010) findings that kin networks in Thailand facilitate large investment expenditures through the relaxation of borrowing constraints by acting as an implicit collateral.

As we already mentioned, schooling and poultry differ in multiple dimensions besides their lumpiness and returns. For example, schooling is also less liquid and probably riskier than owning poultry, and the returns from education accrue far in the future. If isolated households were more risk-averse than connected households, or if they thought their children's optimal schooling level were lower than for connected households, they might choose to spend the *PROGRESA* transfer differently, favoring poultry over schooling. However, the evidence from Table 1 is inconsistent with these alternative explanations. If isolated households deemed schooling too risky or had a lower optimal schooling level, their baseline enrollment rate would be lower than for connected household, which is not the case. One alternative explanation might be that the existence of *PROGRESA* changes the riskiness of the environment. However, the program does not change the income process in the time period we consider when we look at schooling, up to November 1999 (Angelucci and De Giorgi, 2009).

8 Is the Difference in Investment Long-lasting?

The evidence from the previous section suggests that the eligibility to *PROGRESA* transfers increased investment in schooling more for connected than isolated households. In this section we test whether this difference is long-lasting. By 2003, eligible households have been receiving the *PROGRESA* transfers for up to five years in treatment villages and four in control villages. If these transfers relax credit constraints more for connected than isolated households, we would expect connected households to have a bigger increase in schooling, the high-return investment, than isolated households. This higher investment will eventually cause consumption to become higher. To shed light on this, we estimate the change in schooling and poultry for connected and isolated households between the first and last years in our data, 1997 and 2003. For completeness' sake, we also report the change in food consumption, although this comparison is not necessarily informative. It might take more than four or five years for the higher stock of human capital to cause permanent consumption increases. Moreover, the receipt of *PROGRESA* transfers, which in 2003 is still ongoing, might distort the effect of higher human capital on consumption (as, e.g., the households that invest less in secondary education may have a higher share of the transfer available for current consumption)

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 rates of return 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.

We use the following specification for household h in time period t,

$$y_{ht} = \kappa_0 + \kappa_1 O_h + \kappa_2 T_t + \kappa_3 (O_h \times T_t) + \kappa_4 x_h + u_{ht}, \tag{10}$$

where the subscript t refers to either 1997 or 2003, the first and last years of data. The outcomes, y_{ht}, are the share of household members 11 and older with at least 9th grade education, the stock of poultry, and monthly food consumption per adult equivalent measured at November 1998 pesos. T_t is a dummy equal one for 2003, and O_h and x_h are as previously defined so all pre-determined household characteristics that differ at baseline for connected and isolated households are controlled for, and u_{ht} is an error term.

Since we have two observations for each household, our identification strategy is robust to unobserved, time-invariant correlates of connectedness and of our dependent variables. That is, as long as all the unobserved heterogeneity is additive and time-invariant, we can interpret our estimates as the causal effect of being part of an extended family on the outcomes of interest. Nevertheless, we notice that this result is the weakest in the paper as the identification of the parameters of interest does not rely on the randomization.

We estimate this equation for all households first, and then for ineligible households that are either isolated or have no eligible relatives. If the changes in investment are long-lasting and driven by the *PROGRESA* transfers, we would expect to find a difference in the stock of human capital (and possibly poultry and consumption) for the entire sample, driven by the program recipients and their extended families, but not for the second group of households, which should not be affected by the program (but for general equilibrium effects, which, however, are absent by November 1999).

Table 5 reports the estimated change in outcomes between 1997 and 2003 for the connected ($\hat{\kappa_2}$) and the isolated ($\hat{\kappa_2} + \hat{\kappa_3}$), as well as their difference ($\hat{\kappa_3}$).[29] The top panel, which reports results for the entire sample, shows that the change in the share of household members who completed at least 9th grade grows by 4.9 and 3.8 percentage points for connected and isolated households, that is by 1.1 percentage points more for connected households - an increase of almost 30%. The stock of poultry decreases for both groups of households, but by 23% more so for the isolated. We note in passing that food consumption decreases for both groups, but by 57% more so for the isolated. These results are statistically significant at conventional levels (and weakly significant for consumption).

The lower panel, which reports results for ineligible households that are either isolated or have no eligible relatives in the village, shows a different pattern of results. First, the stock of human capital does not increase for either group: the estimates of the differences are 0.9 and 0.8 percentage points for connected and isolated households, a much smaller magnitude than for the entire sample and not statistically different from zero or from each other. The changes in the stock of poultry and consumption are also not different for connected and isolated households.

We interpret these results as consistent with the hypothesis that the differences in investment between the

[29]The estimates shown focus on households that are observed both at baseline and in 2003. The results are qualitatively unchanged if we include also the households observed at baseline but not in 2003.

connected and the isolated are driven by *PROGRESA*'s positive aggregate shock, as we do not observe similar changes for the households which are least likely to benefit from the program. These differences appear to be long-lasting: after 4 to 5 years since the program start, the changes in investment in human capital and poultry of isolated households considerably lag behind the changes for connected households. This long-lasting difference in the effect of similar incentives on human capital will likely affect long-term consumption.

9 Policy Implications and Conclusions

We have presented evidence that extended family networks are an important informal resource-sharing institution in our sample of rural Mexican villages. Under the assumption that any potential time-invariant and time-varying unobserved determinant of network formation that also affects insurance and investment is additive, we find that households with close relatives in the village (parents, offspring, siblings) behave differently from households without such relatives in the village: the former group shares resources within the network and is better able to smooth consumption and to undertake more human capital investments than the latter group when the network resources are sufficiently high. As a consequence of the insurance and investment services provided by the extended family, connected households may respond to similar exogenous income increases by accumulating more resources over time than their isolated, but otherwise broadly similar, neighbors. The extended family's role of provider of investment capital, however, may occur only when the network has sufficient aggregate resources. When all members are close to the subsistence level, the main role of the informal network is to provide insurance. Once there are sufficient resources circulating, the network has the additional role of channeling capital to its most profitable use.

Finding that transfers are shared within one's informal network has important implications for the design and the evaluation of public policies. Policies should be designed bearing in mind that: (i) their recipients have complex interactions with their peers, and such interactions might affect whether an intervention crowds the existing arrangements out (e.g. Attanasio and Rios-Rull, 2003) or in (e.g. the pooling of resources to finance lumpy investment); (ii) similar recipients will need transfers of different sizes to achieve the same policy goals, depending on the characteristics of their informal network (as shown by the lack of a significant increase in school enrollment among isolated households). On policy evaluation, treatment effects might be biased if evaluators do not take account of informal resource-sharing arrangements which affect the behavior and choices of some ineligible households (the extended family members, in our case). This affects the design of the experiment and the survey instrument. When possible, the design should be a partial population experiment (Moffitt, 2001). The survey should identify and collect data on the recipients' informal network.

This work has provided evidence on the importance of proximity - both geographic and consanguineous - for informal arrangements, as hypothesized also by Ambrus, Mobius, and Szeidl (2009). The next step in our research agenda is to investigate the nature of the informal arrangements within extended families. We are especially interested in understanding what features of the extended family enable its members to share resources and how these features are reflected in the network architecture.

A Appendix

A.1 Theory: Investment

Consider a two-period model in which two risk-averse households consume and invest. Households $h \in \{j, l\}$ receive exogenous endowments in each period $t \in \{1, 2\}$, $y_h^t(s)$.[30] Investment, I, has risk-free gross returns $f(I)$, with $f(I) = I^\gamma$, $\gamma \in (0, 1)$ and $I \in (0, 1)$. Given the endowments, the investment return function, and transaction costs, the central planner chooses investment and transfers to solve the following problem,

$$\max_{I_j^1(s), I_l^1(s), d^1(s), d^2(s)} U \equiv \sum\nolimits_s \pi(s) \sum\nolimits_h \sum\nolimits_t \delta^{t-1} \ln c_h^t(s), \tag{11}$$

subject to (i) the aggregate resource constraints, $\sum_h y_h^1(s) - \alpha d^1(s) = \sum_h c_h^1(s) + I_h^1(s)$ and $\sum_h y_h^2(s) + f[I_h^1(s)] - \alpha d^2(s) = \sum_h c_h^2(s)$, and (ii) the non-negativity constraints, $c_h^t(s) > 0$, $d^t(s) \geq 0$, $I_h^1(s) \geq 0$.

We choose an income realization such that household l receives a transfer in the first period and makes a transfer in the second. We focus on two cases: when the two households pool income and when they live in autarky, making no transfer to each other. This is consistent with our empirical evidence, which suggests that transaction costs are negligible for connected households (K), but so high that there is no sharing for isolated households (O). To simplify the exposition, we use here the same terms we adopt in the empirical analysis. Therefore, we call the resource-pooling pair connected or K and the autarkic pair isolated or O.[31]

We capture some features of our data by considering two types of investments. One is continuous and has low returns (assume $\gamma \to 1$). One such commodity is poultry. The other type of investment is lumpy and has larger returns. One such investment is secondary education. Our first objective is to study how income shocks affect these two investment goods for connected K and isolated O households. We show that these effects vary depending on whether the investment is continuous or lumpy.

The optimal continuous investment for connected and isolated households is the following (note that, to simplify the analytical solution, in the remainder of the section we assume that the exogenous endowment y is zero in the second period for both households):

$$I^{K*} = \frac{\delta\gamma(y_j^1 + y_l^1)}{2(1 + \delta\gamma)};$$

$$I^{O*} = \frac{\delta\gamma(y_h^1)}{(1 + \delta\gamma)}.$$

This generates the following result.

Result 1. *(1a) I_h^* is a positive function of own endowment for both isolated (O) and connected (K) house-*

[30]Assume that at least in some states of the world $y_j^t \neq y_l^t$.

[31]We assume investment returns are certain and observable for simplicity. In general, if investment returns are uncertain, or investment is fully or partially hidden, the differences in investment choices between resource-pooling households and autarkic households are even more pronounced than those described in the main text.

holds. (1b) Investment is less sensitive to contemporaneous changes in own income for connected households than for isolated ones, that is, $\frac{\partial I_h^{O*}}{\partial y_h} > \frac{\partial I_h^{K*}}{\partial y_h}$.

Connected households do not have to disinvest as much as isolated households when hit by a negative idiosyncratic shock because their shock is insured away. However, when connected households experience positive income shocks, they cannot invest as much as the isolated households because they share the income increase within the pair. We refer to this mechanism as the 'insurance motive' for investment.

Now suppose that investment is lumpy, i.e. $f(I) = 0$ for $I < \overline{y}$, and $f(.) = I^\gamma$ with $\gamma \in (0,1)$ for $I \geq \overline{y}$, with returns higher than the returns of the continuous investment good. When the aggregate resources are low, $y_j + y_l \leq \overline{y}$, both pairs of households do not invest. When aggregate resources are large enough, $y_j + y_l > \overline{y}$, at least one connected household can invest. The behavior of the isolated pair depends on how large the individual endowments are. If both endowments are sufficiently large, $\overline{y} < y_j \leq y_l$, all households can invest and behave in the way described in **Result 1**. If only one endowment is sufficiently large, e.g. $y_j \leq \overline{y} < y_l$, only one of the isolated households can invest. If neither endowment is large enough, e.g. $y_j \leq y_l < \overline{y}$, neither isolated household can invest.

Given the assumptions of the model we can also state the following result.

Result 2. *(2a)* I_h^* *is a discontinuous function of own endowment for connected and isolated households; it is zero until a certain threshold,* $t^{K,O}$*, and positive beyond it. The threshold for the connected pair,* t^K*, is a function of both households' endowments, while the threshold for the isolated pair,* t^O*, is a function of own endowment only (given the other parameters of the model). (2b) Given the endowment process, this threshold is lower for the connected pair,* $t^K < t^O$*. Therefore, in the interval* $y_h \in (t^K; t^O)$ *the effect of own endowment on investment is positive for the connected pair and zero for the isolated pair, i.e.* $\frac{\Delta I_h^{O*}}{\Delta y_h} < \frac{\Delta I_h^{K*}}{\Delta y_h}$.

Proof of Result 2: Consider the maximization problem where for simplicity we assume linear returns to investment after a given threshold of investment $\overline{y}$, $f(I) = \theta I$, with $\theta > 1$. $(2a)$ With lumpy investment there is a floor to investment, $\overline{y}$, below which no investment occurs. For example when $\overline{y} \geq \frac{\theta y_l^1 - y_l^2}{\theta} = t^O$ there is no investment in autarky, as $U_{I=0}^A > U_{I>0}^A$ for both households. However, if households can make transfers to each other, then there is positive investment even if $\overline{y} \geq \frac{\theta y_l^1 - y_l^2}{\theta}$, as long as $\overline{y}$ does not exceed a certain threshold. When $\delta = 1$ this threshold is $t^K = \frac{\theta Y^1 - Y^2}{\theta}$ and this proves $(3b)$ as well.

For example, consider the case in which $\overline{y} = y_l^1 > y_j^1$. Neither autarkic household will invest as consumption must be strictly positive. However, household l can invest if household j is willing to transfer some resources to it. Such resource sharing households are better off by adding resources in the first period and having positive investment if the return is sufficiently high. That is, $U_{I>0}^K > U_{I=0}^K$ if $\theta \geq \frac{Y^2}{y_l^1}$. To prove this, express total utility as $U = \ln(c_j^1 c_l^1 (c_j^2 c_l^2)^\delta)$. Suppose that in case of positive investment, household l invests the minimum amount, $I = y_l^1$, and the households consume $c_h^1 = \frac{y_j^1}{2}$ in the first period and $c_h^2 = \frac{\theta y_l^1 + Y^2}{2}$ in the second period. Then $U_{I>0}^K > U_{I=0}^K$ holds when $\ln((\frac{y_j^1}{2})^2 (\frac{\theta y_l^1 + Y^2}{2})^{2\delta}) > \ln((\frac{Y^1}{2})^2 (\frac{Y^2}{2})^{2\delta})$. This is true when $\theta \geq \frac{Y^2}{y_j^1}$. Note that as both households consume the same amount, if $U_{I>0}^K > U_{I=0}^K$ then it follows that $U_{h,I>0}^K > U_{h,I=0}^K$ for $h = \{j, l\}$.

For this particular example with a high floor to investment $\overline{y} = y_l^1$, the rate or return must be more than 100% for the connected households to invest and establish a share holder-investor relation. We can easily write other minimum levels of investment for which only a connected household would invest.■

The intuition for this result is that the extended family may act as a lender or a shareholder by pooling resources to finance an investment which a single household could not undertake by itself. As such, the social network has both insurance and financing motives. We refer to this latter mechanism as the 'credit motive' for investment.

In sum:

1. $y_j \leq y_l < \overline{y}$ and $y_j + y_l \leq \overline{y}$. No investment.

2. $\overline{y} < y_j \leq y_l$ and $y_j + y_l > \overline{y}$ All households invest following **Result 2**;

3. $y_j \leq y_l < \overline{y}$ and $y_j + y_l > \overline{y}$ Only connected households can invest;

4. $y_j \leq \overline{y} < y_l$ and $y_j + y_l > \overline{y}$ Both connected households and one isolated household can invest.

The final goal of this section is to study the conditions under which consumption is higher (in the aggregate) and smoother when there are no transaction costs. We have the following result.

Result 3. *(3a) Aggregate consumption is higher for connected than for isolated households when investment is continuous and its returns are concave and when investment is lumpy and $y_h \geq t^K$; aggregate consumption is equal for connected and isolated households when investment is lumpy and $y_h < t^K$. (3b) The difference between aggregate consumption levels between connected and isolated households is a function of aggregate resources. It is initially negligible, when aggregate resources are sufficiently low, and becomes larger and grows with aggregate resources when their level is sufficiently high.*

Proof of Result 3: $(3a)$ follows from the assumptions of concave returns for the continuous and lumpy investments and from results $2a$ and $2b$. $(3b)$ states that if aggregate endowments are low the financing potential of sharing households vanishes as $Y = y_l + y_j < t^K$, that means no high return investment is undertaken by any of the households (K, O). Coupled with low returns in the continuous investment, this simply states that $\sum_{h,t} c_h^{t,K} \simeq \sum_{h,t} c_h^{t,O}$ as the available resources are almost identical for both types of households.■

The intuition for *3a* is the following. For continuous investment, aggregate investment is the same for both types of households. However, given that the return is concave, aggregate returns are largest when both households (of a given type) invest the same amount. This occurs for the connected pair but not for the isolated pair, unless their endowments are identical. As a consequence, aggregate consumption is higher for the connected households. The difference in aggregate consumption, however, is smaller the smaller the returns on the investment. With lumpy investment goods, aggregate investment may be higher for the connected pair, which requires a lower level of aggregate resources to reach the threshold beyond which the lumpy investment has positive returns. While financing the lumpy investment requires foregoing consumption in the first period, the returns on the investment are sufficiently high that aggregate consumption in the two periods is higher for the connected pair.

The see the intuition for *3b*, consider the case in which households can invest in both goods, the continuous, low-return one – poultry, and the lumpy, high-return one – schooling. When aggregate resources are sufficiently low, neither pair of households can invest in schooling because of its lumpiness. Both pairs invest in poultry. Since poultry has low returns, the difference in aggregate consumption is negligible even if the connected pair has higher returns from investing in a more efficient way (from *3a*). Only when aggregate endowments exceed a certain threshold can the connected pair exploit the pair's financing potential. In that case, the connected households can pool resources and finance schooling, enjoying high returns and therefore a higher future consumption. Given the same endowments, the isolated households cannot finance schooling because they do not share resources. They can invest only in poultry, and their future consumption is lower.

B Not for Publication

B.1 Reliability of the Constructed Extended Family Links

We present three pieces of evidence related to the reliability of the algorithm we used to construct extended family links between households resident in the same village. These relate to the descriptive evidence on surnames, checking the incidence of some forms of potential measurement error in surnames, and providing external validity to the created links using data from the Mexican Family Life Survey.

B.1.1 Descriptive Evidence on Surnames

Table B1 provides descriptive evidence on each surname type. For both head and spouse, there are fewer paternal than maternal surnames reported. As Figure 2 shows, this reflects the fact that under the Hispanic naming convention, paternal surnames have a greater survival rate across generations. There are 1696 different paternal surnames reported by heads ($F1$), lower than for the other types of surname including those reported as the spouse's paternal surname ($F2$). This is both because the naming convention implies spouse's paternal surnames have lower survival rates across generations than those of male heads of household, and also be partly due to spouses moving into the 506 villages in the data from villages outside the evaluation sample.

The second row shows the majority of surnames are mentioned at least twice. For each surname type, the most frequent surname covers around 9% of households, and half the households have one of the 50 most frequent surnames for each surname type. The third row shows the probability of two randomly matched households having the same surname type is close to zero, and the expected number of households with the same head's paternal surname is 13.3. This is higher than the expected number of households with the same spouse's paternal surname, again suggestive of women moving into *PROGRESA* villages from other locations.[32]

The next two rows report the same information but at the village level. The probability (without replacement) of two randomly chosen households in the village having the same surname is orders of magnitude larger than in the population. Hence households are not randomly allocated by surname type into villages. On the other hand, the fact that the expected number of households in the village with the same surname is smaller than in the population implies households do not perfectly sort into villages by surname either.[33]

[32] These population values are calculated as follows for any given surname type. Let n_i denote the number of households with surname i and let N denote the number of households that report some surname of the given type. The probability, without replacement, that two randomly chosen households have surname i is then $P_i = \left(\frac{n_i}{N}\right).\left(\frac{n_i-1}{N-1}\right)$, and the expected number of households in the population with name i is $E_i = n_i.\left(\frac{N-1}{N}\right)$. The values reported in Table B1 are the averages of P_i and E_i over all surnames i.

[33] These village values are calculated as follows for any given surname type. Let n_{iv} denote the number of households with surname i in village v and n_v denote the number of households that report some surname of the type in village v. The probability, without replacement, that two randomly chosen households in the village have surname i is then $p_{iv} = \left(\frac{n_{iv}}{n_v}\right).\left(\frac{n_{iv}-1}{n_v-1}\right)$, and the expected number of households in the village with name i is $e_{iv} = n_{iv}.(\frac{n_v-1}{n_v})$. The values reported in Table B1 are the weighted averages of p_{iv} and e_{iv} over all villages v, where the weights are $\frac{n_{iv}}{n_v}$. These weights account for the same name being reported to different extents across villages. The expected number of matches in the village is based on only one surname, and so provides an upper bound on the total number of extended family links our matching algorithm actually defines.

B.1.2 Measurement Error in Extended Family Links

There are a number of potential forms of measurement error in the surnames data that can be checked for. The first arises from the convention that women change their paternal surname to their husband's paternal surname at the time of marriage. To address this concern, we note that the precise wording of the question specifically asks respondents to name the paternal and maternal surname of each household member. Furthermore, in only 5.8% of households is the spouse's maternal surname recorded to be the same as her husband's paternal surname. This provides an upper bound on the extent to which measurement error of this form is occurring. Second, if the male head is the respondent, he may not recall his wife's maternal surname and simply replace it with her paternal surname. This may occur because his children only inherit his wife's paternal surname. Reassuringly, this problem occurs in only 4.9% of households. A final circumspect case is households in which the paternal and maternal surnames of both the head and spouse are all reported to be the same. This occurs for 1.6% of households, although the figure drops to .5% if we exclude households with the most common surname in the data.[34]

Some forms of measurement error however cannot be addressed. The first arises from any remaining typos in surnames. Second, there may be two identical families in the village who share the same paternal and maternal surnames of head and spouse but are genuinely unrelated. The matching algorithm then assigns the number of family links to be double what they actually are. A check for the severity of this problem is based on the following intuition. By definition, household i cannot have parental links to more than two other households (the parent's of the head and the parent's of the spouse), conditional on the parents not being present within the household. This is true for 97% of households using our matching algorithm.

B.1.3 External Validity of the Extended Family Links: MxFLS Data

To provide external validity to the constructed data on extended family links in the *PROGRESA* data, we present similar information from an alternative data set that was collected in a comparable economic environment and time period. The *Mexican Family Life Survey* (MxFLS), collected in 2001, provides information on the number of each type of link, by head and spouse, that are still alive in *any* location, not just the same village. This provides an upper bound on what should be recorded as family links in the *PROGRESA* data, in which we only construct links in the same village. To make the MxFLS data comparable, we restrict the sample to couple headed households that reside in locations with less than 2500 inhabitants in states that are also covered in the *PROGRESA* data. There are 580 such households.[35]

Table B2 reports the findings from the MxFLS. The number of family links to parents, children and siblings outside the household and located anywhere, are greater than those we construct using surnames data within the village from the *PROGRESA* data. The fact that more parents of the spouse are alive is because spouses

[34] There are no differences in the incidence of these potential errors between treatment and control villages.

[35] One restriction on the matching algorithm used in the *PROGRESA* data is that we are unable to identify links to parental households if only one of the parents is alive. To ensure the MxFLS data is therefore comparable, we do not include information from couple headed households that report only having a single parent alive in another household.

are younger than their husbands. Moreover, the differences between husbands and spouses in the number of parents and siblings are less dramatic in the MxFLS data, presumably because these statistics refer to family links in any location and so are unaffected by the geographic mobility of women at the time of marriage.

C Robustness checks for hypothesis H_1.

This Appendix provides two pieces of supportive evidence that extended families share risk in this setting. The first exploits variation in the monetary value of *PROGRESA* transfers that become available to eligible households in the network. The second estimates the ATE of food consumption of eligible households, defined in (5), to compare what share of the transfer is consumed by eligible households with and without ineligible relatives.

The first set of results therefore directly tests whether ineligible household's consumption is higher in family networks that receive higher transfers per member, all else equal. We consider only treatment villages and regress, for household h in time period t, the log of household food consumption per adult equivalent (c_{ht}) on the log of transfers per network adult equivalent (g_{ht}) as follows,

$$\ln c_{ht} = \lambda_0 + \lambda_1 \ln g_{ht} + \lambda_2 x_h + u_{ht}. \tag{12}$$

The actual transfer received is endogenous because it depends on whether eligible households comply with *PROGRESA*'s schooling and health care requirements. We therefore use the *potential* transfer to which a household is entitled to instrument for the actual transfer received. Potential transfers are only a function of the household's demographic characteristics at baseline, thus a valid (and strong, $F - stat = 132$) instrument. The data waves used are May and November 1999, and we cluster the standard errors by village.[36]

Columns 1 and 2 of Table C1 present the results from both OLS and IV specifications. The IV estimate of the elasticity (Column 2) is .136, significantly different from zero, and larger than (but not statistically different from) the OLS estimate, which is .071 (Column 1). If log-grant is measured with error, the OLS estimates would be expected to be smaller than the IV estimates, as classical measurement error causes an attenuation bias.

Taken together, the evidence shows that an extra dollar transferred to the eligible relatives increases ineligible consumption by 13 cents in the average extended family network with both eligible and ineligible members. We also estimated reduced-form OLS regressions of the log-food consumption of ineligibles on potential rather than current log-transfers per adult equivalent in treatment and control villages (Columns 3 and 4).

[36] In this specification, we control for the same variable in x_h as before to control or any differences in observables between connected and isolated households at baseline. In addition, as we exploit comparisons across connected households and the intensity of the program, we additionally control for correlates of consumption that are also related to the program intensity. These are the number of eligible children in the network grouped by gender and primary versus secondary school level, the share of eligible households in the network, and the share of the network hit by adverse weather.

The reduced-form coefficients are positive and significant in treatment villages, with a point estimate of 0.093, but negative and not statistically different from zero in control villages, with an estimate of -0.032. This latter negative, though very imprecisely estimated coefficient, is evidence that networks with a larger share of eligible households or with more children are poorer (since poverty is positively correlated with fertility). Since extended family members share resources, the poorer the network, the lower its aggregate resources, the lower the consumption of all its members, including ineligible households. The results from Column 4 confirm that the positive correlation between network's potential transfer size and ineligible's consumption is not driven by unobserved correlates of consumption and network structure, in which case we would observe a positive correlation also in control villages.

Our second set of results checks whether eligible households with and without ineligible relatives consume different fractions of the transfers received. Intuitively, we expect eligible households with only eligible relatives to consume a higher share of their transfers, as they do not have to share part of their transfers with their ineligible relatives. To do so, we estimate ATEs for connected eligible households with and without ineligible relatives and compare it with the transfer received to compute the fraction of the transfer they consume.

Table C2 presents the results, again using data from March 1998 and May and November 1999. Eligible connected households that are related to ineligible households increase their food consumption by 28 pesos per adult equivalent per month out of a 64 peso transfer per adult equivalent network member (Column 1, Rows 1 and 2). In contrast, eligible households without ineligible relatives increase their food consumption by 31 pesos out of a 55 peso transfer (Column 2, Rows 1 and 2). Connected eligible households with ineligible relatives consume a significantly smaller share of their transfer – 43% versus 56% (and 56% versus 69% looking at total non-durable consumption) – than connected eligible households without ineligible relatives. Their ineligible relatives increase their consumption by 13 cents for each peso received in the network (Row 6).[37]

Table C3 presents some robustness checks on the baseline results. To begin with we check whether the indirect effects on consumption depend on land ownership or ethnicity, rather than on the presence of the extended family networks. As shown in Columns 1 and 2, we find no positive and significant ITEs in either case. Moreover, if we also allow for the ITE to vary with the presence of the family network, we fail to find an effect for land owners or ethnic minority households over and above the effect of family network.

Lastly, while we have used *PROGRESA* eligibility to proxy large, observable, and exogenous income shocks to households, it might be the case that households perceive *PROGRESA* as an atypical occurrence. Hence their behavioral response might not be an accurate reflection of how they respond to more typical resource shocks. To explore this we estimate whether households' consumption is correlated to self-reported illnesses of *other* household heads in the extended family network. 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. This negative correlation is imprecisely estimated.

[37] We obtain similar results when we consider nondurable consumption rather than only food consumption.

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Table 1: Characteristics of Couple Headed Connected and Isolated Households

	(1) Connected Households		(2) Isolated Households		(3) Difference	(4) Difference Between Treatment and Control, Connected Households	(5) Difference Between Treatment and Control, Isolated Households
	Mean	SD	Mean	SD	p-value	p-value	p-value
Household Head Characteristics:							
Age	44.7	15.1	47.2	15.8	[.000]	[.157]	[.587]
Literate (yes=1)	.73	.44	.67	.47	[.000]	[.124]	[.643]
Ethnicity (indigenous=1)	.34	.48	.38	.49	[.172]	[.539]	[.556]
Self reported illness	.112	.315	.120	.325	[.199]	[.868]	[.221]
Household Characteristics:							
Wealth index	728	141	729	141	[.912]	[.169]	[.691]
Labor income (pesos)	273	353	276	325	[.693]	[.152]	[.618]
Food expenditure (pesos)	153	127	152	136	[.829]	[.332]	[.168]
Progresa eligibility (yes=1)	.552	.497	.547	.498	[.693]	[.142]	[.857]
School enrollment rate (11-16 yr old)	.644	.410	.657	.410	[.287]	[.587]	[.003]
Temporary MX labor migrants	.137	.344	.124	.329	[.200]	[.117]	[.212]
Permanent MX migrants	.016	.124	.016	.124	[.988]	[.149]	[.610]
Temporary US labor migrants	.058	.366	.038	.192	[.001]	[.385]	[.535]
Permanent US migrants	.017	.128	.018	.133	[.601]	[.644]	[.301]
Land, livestock, other assets:							
Irrigated land size (hectares)	.12	1.01	.14	.75	[.292]	[.454]	[.317]
Other land size (hectares)	2.18	4.2	2.21	4.91	[.766]	[.572]	[.808]
Horses	.40	1.04	.39	.99	[.544]	[.300]	[.218]
Donkeys	.40	1.16	.39	.87	[.570]	[.317]	[.920]
Ox	.12	.74	.12	1.01	[.789]	[.013]	[.087]
Goat	1.58	5.93	1.40	5.09	[.168]	[.158]	[.055]
Cow	1.16	3.76	1.10	4.12	[.520]	[.431]	[.877]
Chicken	7.01	8.16	7.98	8.81	[.000]	[.202]	[.443]
Pig	1.20	2.92	1.18	3.11	[.782]	[.595]	[.657]
Fridge	.16	.36	.16	.37	[.591]	[.151]	[.904]
Stove	.32	.47	.29	.45	[.039]	[.263]	[.666]
Heater	.03	.17	.03	.18	[.433]	[.683]	[.815]
Radio	.65	.48	.64	.48	[.403]	[.073]	[.220]
Television	.49	.5	.46	.5	[.015]	[.020]	[.582]
Car	.02	.15	.03	.16	[.324]	[.398]	[.151]
Truck	.08	.28	.08	.27	[.464]	[.207]	[.377]
Household demographics:							
Aged 0-7	1.30	1.26	1.25	1.28	[.059]	[.721]	[.831]
Aged 8-14	1.14	1.21	1.15	1.22	[.817]	[.743]	[.206]
Aged 15-18	.54	.78	.55	.78	[.610]	[.350]	[.364]
Aged 19 or more	2.65	1.12	2.65	1.10	[.983]	[.045]	[.869]
Child labor, age 11-16:							
Engaged in labor market	.27	.44	.27	.44	[0.966]	[.146]	[.542]
Weekly days of child labor	.96	1.84	.94	1.82	[0.692]	[.069]	[.932]
Village characteristics:							
Price index	9.53	0.42	9.49	0.43	[0.379]	[0.716]	[0.674]
Wealth	726	86	734	86	[0.424]	[0.142]	[0.880]
Marginalization index	0.48	0.75	0.55	0.71	[0.383]	[0.951]	[0.480]

Notes: couple-headed households. The p-values on the differences are reported from the corresponding OLS regressions allowing standard errors to be clustered by village. All data is taken from October 1997 except for expenditures, which are recorded in March 1998, and illness of the household head, which is reported in November 1998. Income and expenditures are in per adult equivalents, measured at November 1998 prices. Temporary migration refers to the household having at least one member migrate and return in the past 12 months . Permanent migration encompasses migration to work, get married, and study. Village characteristics statistics use one observation per village.

Table 2: Do Family Networks Share Resources?

Dependent Variable: Food Consumption in Pesos, per Adult Equivalent

Difference-in-difference OLS estimates, standard errors clustered by village

	(1) Ineligibles	(2) Ineligibles With and Without Eligible Extended Family Members
ITE^K [connected]	26.4**	
	(11.1)	
ITE^O [isolated]	-15.5	
	(20.2)	
ITE [connected with eligible relatives]		26.9**
		(11.7)
ITE [isolated and connected w/o eligible relatives]		-4.4
		(16.2)
ΔITE	41.9**	31.4*
	(20.9)	(17.3)
Connected households (11/99)	2843	
Isolated households (11/99)	712	
Connected households with eligible relatives (11/99)		2482
Isolated and connected w/o eligible family (11/99)		1073
Observations	11015	11015

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The dependent variable is household food consumption per adult equivalent, measured in November 1998 pesos. Standard errors are clustered by village. The sample covers ineligible couple-headed households, the data waves used are March 1998, May 1999, and November 1999. The number of households reported in each column refers to November 1999. The following controls are included in each specification: household head's age, gender, ethnicity, and literacy; number of household members by age/gender groups; household wealth index, livestock ownership (size), temporary US migration, village poverty index, time and region dummies. All controls are measured at baseline. The adult equivalence scale used for consumption is one for members 18 or older, and 0.73 otherwise. The foot of each column also reports the difference in estimated indirect treatment effects and its corresponding standard error.

Table 3: Full Insurance Within Extended Family Networks

Dependent Variable: Growth in Food Consumption per Adult Equivalent, Nov. 1998 pesos
Standard errors are clustered by network

	(1) OLS	(2) IV (health shock)	(3) IV (lagged income)
	PANEL A: Aggregate resources as network consumption		
Δ Log aggregate resources (Y)	.973***	.970***	.967***
	(.005)	(.008)	(.009)
Δ Log household resources (y)	.021***	.007	.010
	(.001)	(.021)	(. 009)
	PANEL B: Aggregate resources as network by wave dummies		
Δ Log household resources (y)	.030***	.012	.016
	(.002)	(.034)	(.085)
Network by wave dummies	yes	yes	yes
Number of observations	76517	60012	56646

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The dependent variable is household food consumption per adult equivalent, measured in November 1998 pesos. Within group estimator. Standard errors are clustered by village. Clustering at the network level doesn't change the results. The sample covers eligible and ineligible couple-headed connected households. The data waves used are November 1998, May 1999, November 1999, November 2000, and November 2003, the health shock is not available in the last wave of our data. The adult equivalence scale used for consumption is one for members 18 or older, and 0.73 otherwise. In Column 2 the IV for household income is a dummy for the head of household being sick. In Column 3 the IV for household income is the first lag of household income. Extreme values are trimmed in the regressions.

Table 4: Investment and Resource Shocks

OLS regression estimates, standard errors are clustered by village

A. Positive Resource Shock: Progresa Eligibility

	(1) School Enrollment Rate (aged 11-16)	(2) Chickens
Connected Household (K)	.078***	.160***
	(.014)	(.057)
Isolated Household (O)	-.020	.274***
	(.023)	(.094)
Difference (K-O)	.098***	-.114
	(.026)	(.093)
Number of observations	24070	39810

B. Negative Resource Shock: Illness of Household Head

	(1) School Enrollment Rate (aged 11-16)	(2) Chickens
Connected Household (K)	-.010	.125***
	(.008)	(.041)
Isolated Household (O)	-.040***	.093
	(.015)	(.074)
Difference (K-O)	.030*	.032
	(.018)	(.084)
Number of observations	30812	31088

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. These double difference estimates use the maximum number of available data waves: In Panel A we use September 1997 to November 1999 data for the effect of *PROGRESA* (using one wave per school year only for effect on stock with at least secondary education); In Panel B we use November 1998 to November 2003 for illness of household head. Data on poultry are missing in November 2000. The sample is only based on couple-headed households. Standard errors clustered by village. The following controls are included in each specification: household head's age, gender, ethnicity, and literacy; number of household members by age/gender groups; household wealth index, temporary US migration, village poverty index, time and region dummies. All controls are measured at baseline. The results from panel B are qualitatively similar if we restrict the sample to households eligible for *PROGRESA* (as in Panel A).

Table 5: Extended Family Networks and Long Run Changes in Investment and Consumption

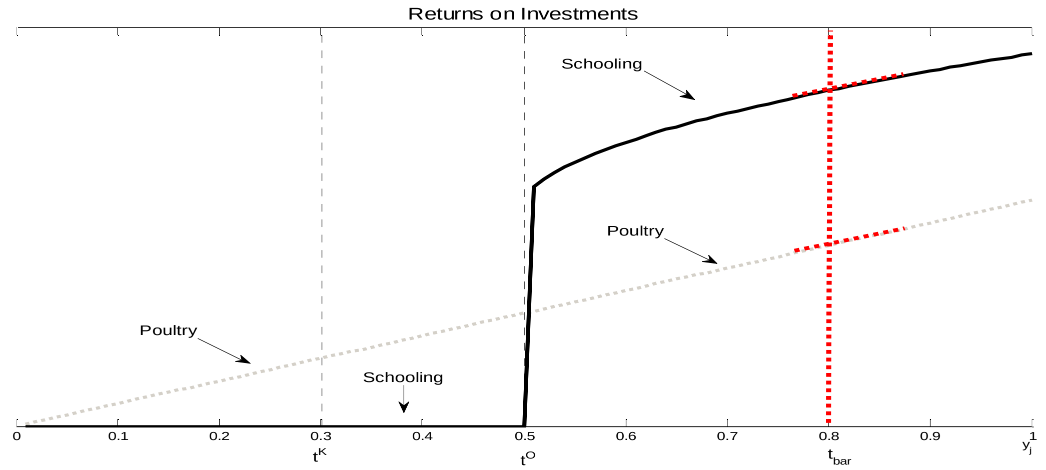
OLS regression estimates. Standard errors clustered by village.

	(1) Share of Household Members With at Least 9th Grade	(2) Chickens	(3) Food Consumption
All households			
Connected Household	0.049	-0.678	-8.937
	[0.003]***	[0.038]***	[2.278]***
Isolated Household	0.038	-0.833	-14.061
	[0.004]***	[0.057]***	[3.019]***
Difference	0.011	0.155	5.125
(Connected-Isolated)	[0.004]***	[0.057]***	[3.193]*
Number of observations	**32306**	32081	**32088**
Ineligible households			
Connected Household w/o	0.009	-1.349	-27.039
eligible relatives	[0.020]	[0.343]***	[9.558]***
Isolated Household	0.004	-1.239	-18.660
	[0.010]	[0.138]***	[9.261]**
Difference	0.004	-0.110	-8.379
(Connected-Isolated)	[0.020]	[0.344]	[12.158]
Number of observations	1544	1531	1532

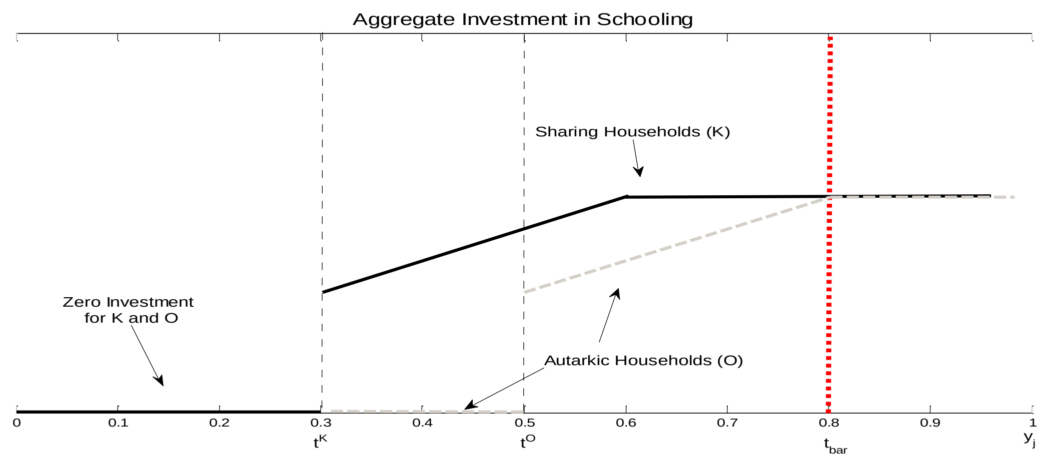
Notes: : *** denotes significance at 1%, ** at 5%, and * at 10%. Estimates are double differences in schooling, poultry, and consumption between November 2003 and September 1997 (March 1998 for consumption, as data is not available in 1997). Standard errors clustered by village. The sample is based on couple-headed households only. The following controls are included in each specification: household head's age, gender, ethnicity, and literacy; number of household members by age/gender groups; household wealth index, land ownership, village poverty index, time and region dummies. The adult equivalence scale used for consumption is one for members 18 or older, and 0.73 otherwise. The foot of each column also reports the difference in the change in outcomes for connected and isolated households and its corresponding standard error.

Figure 1: Investment

Panel A: Returns on Investments



Panel B: Aggregate Investment in Schooling



Panel C: Aggregate Investment in Poultry

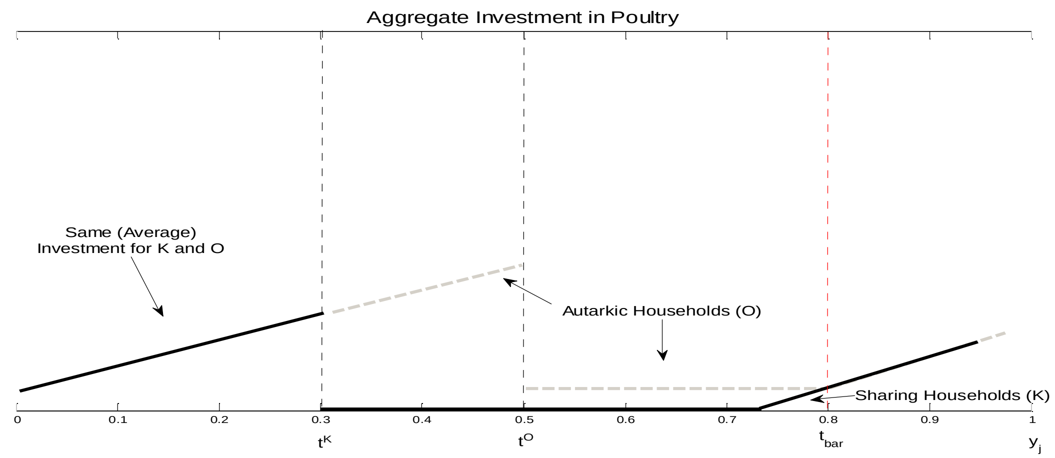
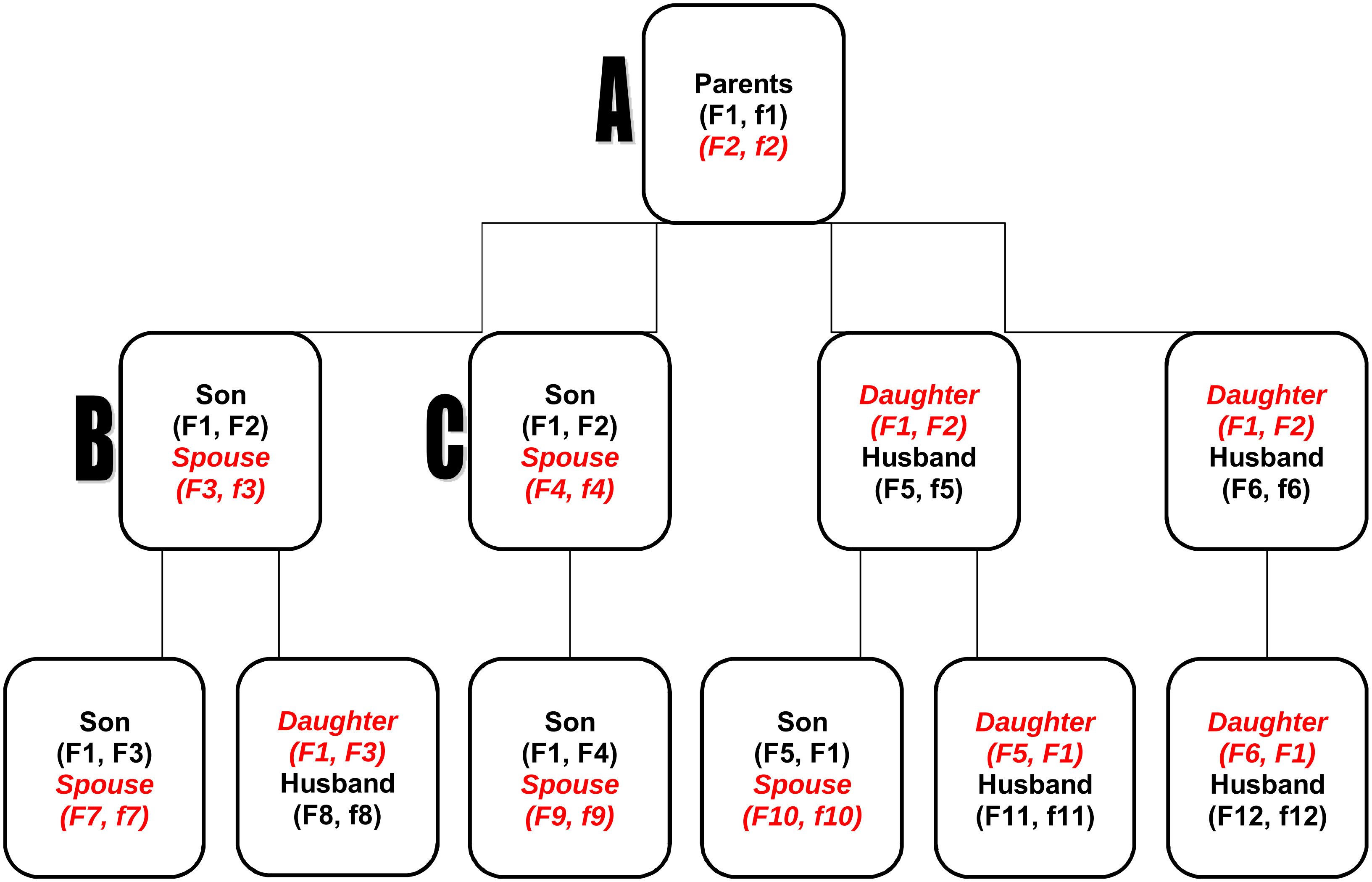
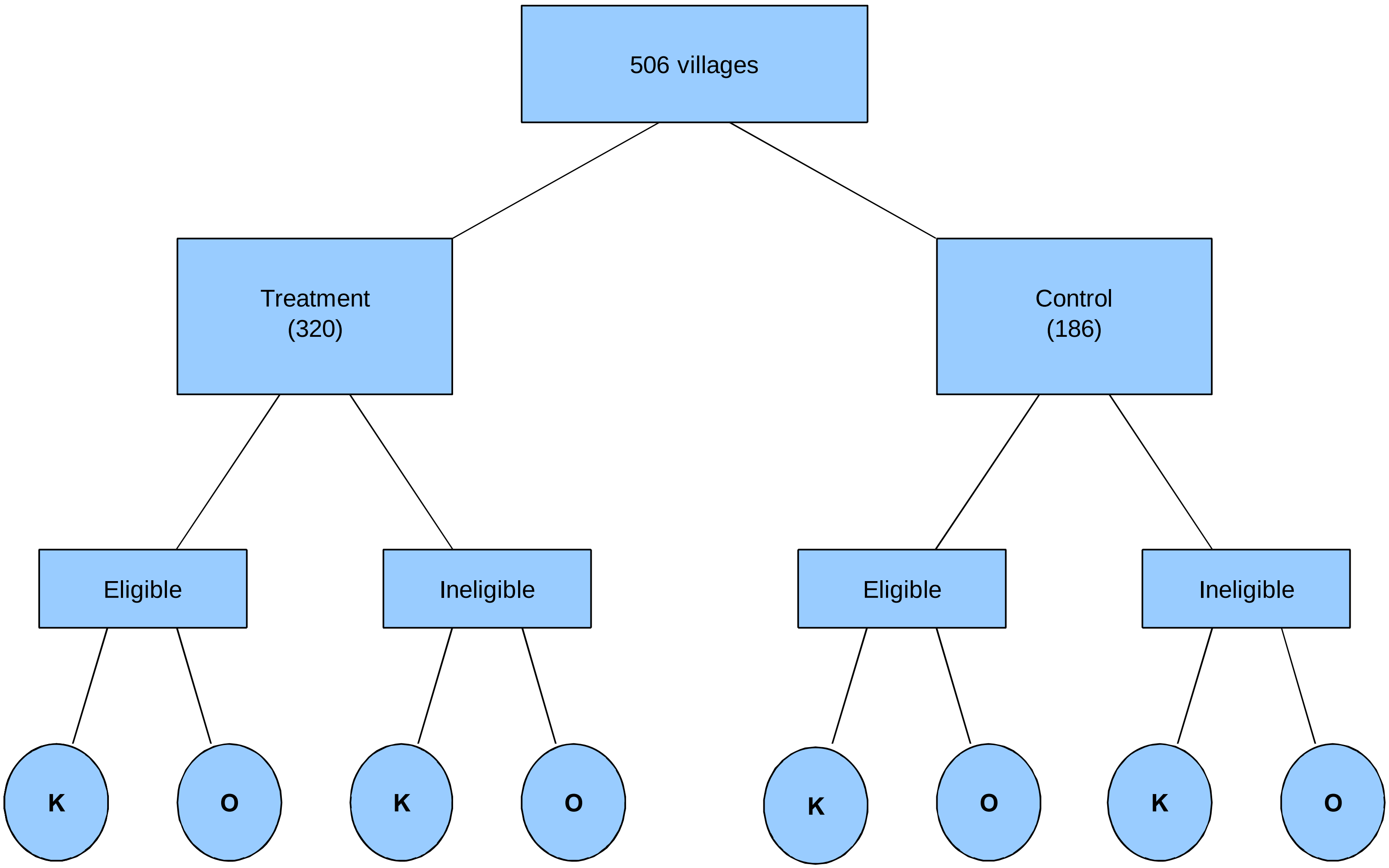


Figure 2: Family Tree



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 3: Data Structure



Notes: K and O indicate connected and isolated households. K households have at least one closely-related household in the village.

Table A1: Variable Descriptions and Data Availability

		Availability							
		Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6	Wave 7	Wave 8
Variable	**Description**	**October 1997**	**March 1998**	**November 1998**	**May 1999**	**November 1999**	**May 2000**	**November 2000**	**November 2003**
Food consumption	Food consumption per adult equivalent			X	X	X		x	x
	(<18=0.73; 18+=1; seven-day recall of 36 items; includes home production and gifts)			PH	PH	PH		H	H
Food expenditure	Seven-day recall for broad food categories		X						
			P						
Potential PROGRESA transfer	Built from the age and gender demographic structure of the household			X	X	X			
				P	P	P			
Actual PROGRESA transfer	From administrative records			X	X	X			
				P	P	P			
Household head ill	Self-reported health status during previous 30 days			X	X	X		X	X
	(in some waves this was deduced from days unable to work)			H	H	H		H	H
School enrolment rate	% of 11-16 year old children enrolled in school	X		X	X	X	X	X	X
		P		PH	PH	PH		H	H
Child labor	11-16 year old children's average days worked in previous week	X		X	X	X	X	X	X
		P		PH	PH	PH		H	H
Animal ownership	Number of animals owned	X		X	X	X	X		X
		P		PH	PH	PH			H
Agricultural expenses	Purchases of seeds, fertilizers, tools and machinery for agricultural production			X	X				
	(land rental excluded)			PH	PH				
Land used/owned	Hectares used (or owned) in the previous six months			X	X	X			
				PH	PH	PH			
Assets	Dummy variables if household owns fridge, stove, heater, radio, TV, car, truck	X		X	X			X	X
		P		PH	PH			H	H

Notes: 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 November 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 A2: Sample Households, Data Waves, and Estimators Used by Results Table

Table	Outcome	Households in Sample	Data Waves	Estimator	CS or DD
1	Various	CH, K and O	1	OLS	CS
2	Food consumption	CH, N, K and I	2, 4, 5	OLS	DD
3	Food consumption	N K	1,3,4,5,6,8	OLS, IV	CS
4	Investment	CH, K and O	1,2,3,4,5,6,8*	OLS	DD
5	Investment	CH, K and O	1,8	OLS	DD
A3	Extended family links	K and O	3	OLS	CS
B1	Surnames	K and O	3	-	-
B2	Extended family links	MxFLS data			
C1	Food consumption	N K	2, 4, 5	OLS, IV	DD
C2	Food consumption, all non-durable consumption	N K	4, 5	OLS	DD
C3	Food consumption	CH, N, K and I	2, 4, 5	OLS	DD

Notes: N=ineligible for Progresa; E=eligible for Progresa; CH=couple-headed; K=connected; I=isolated. CS=only use cross sectional variation; DD= use both cross sectional and longitudinal variation, controlling for time-invariant heterogeneity; *: the number of waves used for this table varies depending on the dependent variable used.

Table B1: Descriptive Statistics on Surnames, by Surname Type

Mean, standard errors in parentheses, percentages in brackets

	Head's Paternal Surname (F1)	Head's Maternal Surname (f1)	Spouse's Paternal Surname (F2)	Spouse's Maternal Surname (f2)
Number of surnames	1696	1996	1912	2025
Number [percentage] of surnames mentioned more than once	1064 [62.7]	1188 [59.5]	1088 [56.9]	1100 [54.3]
Probability of same surname in population	9.50×10^{-6}	7.54×10^{-6}	8.60×10^{-6}	8.33×10^{-6}
	(5.48×10^{-6})	(4.16×10^{-6})	(4.95×10^{-6})	(4.95×10^{-6})
Expected number of same surname matches in population	13.3	11.2	9.92	9.26
	(1.66)	(1.36)	(1.25)	(1.19)
Probability of same surname in the village	.042	.021	.022	.020
	(.0005)	(.0004)	(.0004)	(.0004)
Expected number of same surname matches in the village	7.55	5.31	5.42	4.98
	(.039)	(.036)	(.036)	(.040)

Notes: For the matching probabilities and expected number of same surname matches in the population, the standard errors are clustered by surname for each surname type. The sample is restricted to those households that can be tracked for the first and third waves of the *PROGRESA* data, namely in the baseline survey in October 1997 (wave 1) and the first post program survey in October 1998 (wave 3).

Table B2: The Number of Family Links, by Type of, as Reported in the Mexican Family Life Survey

Couple Headed Households

Mean, standard error in parentheses clustered by village

	Outside of the Household (ANY location)				
	Parent	**Children Aged 0-16**	**Adult Children**	**Siblings**	**All**
From head of household to:	.476	-	1.23	3.27	4.97
	(.035)		(.089)	(.116)	(.014)
From spouse of household to:	.669	-	1.23	3.50	5.39
	(.039)		(.089)	(.113)	(.148)

Notes: The sample is taken from the first wave of the Mexican Family Life Survey, 2001. Standard errors are clustered by village. We restrict this sample to the seven Mexican states that are also covered in the *Progresa* evaluation data, and to couple headed households, in locations with less than 2500 inhabitants. There are 580 such households. By construction, the number of family links to parental households is always conditional on two such family links existing. We do not therefore use information on households that have single parents in any location. By construction, the number of children of the couple are identical for the head and the spouse. The number of children outside of the household is restricted to be 17 and older (based on spouses' reports).

Table C1: Ineligibles' Log Food Consumption as a Function of the Transfer per Extended Family Network Member

Dependent Variable: Log Food consumption per Adult Equivalent (November 1998 pesos)

OLS and IV estimates of Log Current and Potential Transfer coefficients. Standard errors clustered by village

	Treatment Villages			Control Villages
	(1) OLS	**(2) IV**	**(3) OLS, reduced form**	**(4) OLS, reduced form**
Log actual transfer	.071***	.136***		
	(.024)	(.051)		
Log potential transfer			0.093	-.037
			(.036)**	(.052)
First stage IV significance		132		
Number of observations	3353	3353	3353	1230

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The dependent variable is log food consumption per adult equivalent, measured in November 1998 pesos. OLS and IV estimates using log-potential transfer as IV for log-actual transfer per network adult equivalent. Standard errors are clustered by village. The sample covers ineligible couple-headed households, the data waves used are May and November 1999. The following controls are included in each specification: household head's age, gender, ethnicity, and literacy; number of household members by age/gender groups; household wealth index, temporary US migration, village poverty index, time and region dummies; share of eligible households in the network; eligible schoolchildren by age/gender, as a share of total network adult equivalents. The adult equivalence scale used for consumption is one for members 18 or older, and 0.73 otherwise.

Table C2: ATE Estimates on the Food Consumption of Eligibles

OLS regression estimates, standard errors are clustered by village

Dependent Variable:	Change in Food Consumption per Adult Equivalent, Nov. 1998 pesos (May and Nov. 1999 – Mar 1998)		Change in All Non durable Consumption, Nov. 1998 pesos (May and Nov. 1999 – Mar 1998)	
	(1) Extended Family Networks With Eligible and Non-eligibles Members	(2) Extended Family Networks With Eligible Members Only	(3) Extended Family Networks With Eligible and Non-eligibles Members	(4) Extended Family Networks With Eligible Members Only
(1) ATE	28.1***	31.2***	34.4***	38.6***
	(5.86)	(7.42)	(7.65)	(10.4)
(2) Transfer per eligible household	64.0	55.5	64.0	55.5
(3) ATE Share (row 1/row 2)	.439	.562	.538	.695
(4) Number of observations	10883	5627	10883	5627
(5) ITE per eligible	8.51**	-	8.62*	-
	(3.52)		(4.77)	
(6) ITE Share (row 5/row 2)	.133		.135	
(7) Number of observations	5478		5472	

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The dependent variable in Columns 1 and 2 (3 and 4) is the difference household food consumption per adult equivalent (all non-durable consumption), measured in November 1998 pesos. Standard errors are clustered by village. The sample covers both eligible and ineligible couple-headed households, the data waves used are March 1998, May 1999, and November 1999. The following controls are included in each specification: household head's age, gender, ethnicity, and literacy; number of household members by age/gender groups; household wealth index, livestock ownership (size), temporary US migration, village poverty index, time and region dummies. All controls are measured at baseline. The adult equivalence scale used for consumption is one for members 18 or older, and 0.73 otherwise.

Table C3: Robustness Checks

Dependent Variable: Food Consumption in Pesos, per Adult Equivalent

Difference-in-difference OLS estimates, standard errors clustered by village

	(1) Land	(2) Ethnicity
ITE [land]	18.2	
	(12.4)	
ITE [no land]	13.7	
	(15.3)	
ITE [indigenous]		31.0
		(24.4)
ITE [hispanic]		14.1
		(12.0)
ΔITE	4.51	16.9
	(17.1)	(26.8)
Number of observations	13468	13468

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The dependent variable is the change between November 1999 and October 1997 in household food consumption per adult equivalent, as measured in November 1998 pesos. Standard errors are clustered by village. The sample covers couple headed households, and the number of households reported in each column refers to November 1999. The following controls are included in each specification: the age, gender, and literacy of household head; the number of household members in demographic bins; the household wealth index, livestock ownership, the village wealth index, time, and region dummies. The adult equivalence scale used for consumption is one for members 18 or older, and .73 otherwise. The foot of each column also reports the difference in estimated indirect treatment effects and its corresponding standard error.