INSURANCE AND INVESTMENT WITHIN FAMILY NETWORKS*

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Abstract

We study how family networks affect informal insurance and investment. We use panel data from the randomized evaluation of PROGRESA in rural Mexico and the information on surnames of household heads and their spouses to identify extended families. Members of an extended family: 1) share risk with each other but not with households without relatives in the village; 2) invest more in their children's human capital when hit by positive income shocks, and disinvest less when hit by negative shock, and 3) have a higher long-term increase in capital, income, and consumption than households without relatives in the village.

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1 Introduction

Poor households in developing countries face substantial risk from multiple sources, yet typically have limited access to formal insurance and credit markets. They therefore have to resort to informal arrangements with other households. Understanding the nature of such arrangements helps clarify how policy interventions might substitute or complement pre-existing informal resource-sharing arrangements. This has implications for the optimal design of new polices as well as the accurate evaluation of existing policy interventions.

The theoretical literature suggests that small networks [e.g. Genicot and Ray 2003, Ambrus, Mobius, and Szeidl, 2009], with members who care for or trust each other and can punish reneging members can achieve high levels of insurance [e.g. Altonji, Ayashi, and Kotlikoff, 1992; Foster and Rosenzweig, 2001; Karlan, Mobius, Rosenblat, and Szeidl, 2009; La Ferrara, 2003]. The empirical evidence from a disparate set of developing countries is consistent with these predictions [Ligon 1998, Fafchamps and Lund 2003, De Weerdt and Dercon 2006, Dubois, Jullien, and Magnac 2008, Mazzocco and Saini 2009, Ambrus, Mobius, and Szeidl 2009]. 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.

Besides affecting risk-sharing, the extended family may favor investment. This may occur through two different channels. First, by providing insurance against idiosyncratic risk, the family network ensures investment is less responsive to negative income fluctuations. Absent risk sharing, risk-averse poor households may be willing to engage in costly activities to have a stable consumption, including reducing high-return investments [Chetty and Looney 2006]. This need to disinvest to face short-term income fluctuations might have long-term negative consequences. For example, a household that is forced to withdraw its children from school and deplete its assets to cope with a negative income shock may be deemed to live in permanent poverty. Conversely, a household that smooths consumption by sharing resources within the extended family may not need to disinvest as much when hit by a negative shock, enabling its children to complete school and access white-collar jobs. The availability of insurance may also enable agents to undertake riskier, more profitable investment [Arrow 1971, Obstfeld 1994].

Second, the extended family may act as a shareholder (or lender) in the presence of borrowing constraints and non-convexities in the investment good. In this case, a group of relatives may pool resources to finance one investment, sharing the returns, which a single household would not be able to undertake on its own. If the extended family protects investment against negative income shocks and attenuates credit constraints, its members may be able to invest more than households without close relatives.

Since informal institutions may both provide insurance and favor investment, one should look at consumption smoothness and investment jointly to assess the importance of extended families in the presence of imperfect financial markets. In this respect, our paper builds on and adds to Rosenzweig and Wolpin [1993], Kochar [1995], Morduch [1998], and Chetty and Looney [2006], among others.

While the contribution of our analysis is predominantly empirical, we first develop a simple model of resource-sharing within extended families to formally derive the hypotheses discussed above. We then test these hypotheses using household panel data from rural Mexico, a setting in which financial markets are imperfect and households face substantial and multiple sources of risk. We use data collected to evaluate the *PROGRESA* social assistance program, which has a randomized control trial research design. We exploit three unique features of the data. First, it represents a census of all households in 506 villages in rural Mexico. This represents a panel of more than 20,000 households interviewed eight times between 1997 and 2003, and provides information on food consumption, income, investment, asset holdings, health shocks, and other socioeconomic characteristics.

Second, we combine information on the paternal and maternal surnames of household heads and their spouses, with the double-surname Hispanic naming convention to build the inter and intra-generational extended family links between a household head and his spouse and all other household heads and their spouses within the same village. This allows us to identify 'connected' households – those that belong to an extended family network within the same village, and households that we classify as being 'isolated,' in the sense that none of their extended family members reside within the same village. To the best of our knowledge, this is one of the most extensive mappings of village level extended family structures that has been conducted.

Third, we observe an exogenous variation in household resources. Between 1998 and 1999 *PROGRESA* offered transfers in 320 randomly chosen villages out of 506 samples ones. There are two waves of the panel data collected pre-program, and six after *PROGRESA* started. Given the data covers a census of village households, we observe households that are both eligible for the program, namely those that are sufficiently poor at baseline, and those that are ineligible.

Therefore, our data enable us to observe whether and how much the consumption of a given household changes when its relatives receive an exogenous income shock (the *PROGRESA* grant) and compare this effect with the change in consumption for households that have no relatives eligible for *PROGRESA*. This test and similar ones, which also use information on health shocks to the household head, are the basis of our empirical analysis. More generally, our experimental design lets us compare changes in the behavior of households in treatment and control villages with similar extended family structures. To use such comparisons to identify the causal effect of resource shocks on the behavior of connected and isolated households relies on the twin assumptions of random assignment of villages to treatment and control status, and absence of spillover effects of *PROGRESA* to control villages. Since we observe connected and isolated households both before and after the program start, we can use triple-difference estimators to compare changes in consumption and investment behavior of connected and isolated households within the same village.

Our main empirical results are as follows. First, we document that 80% of households are connected, i.e. they belong to an extended family network in the village, and 20% are isolated in the sense that none of their close relatives live within the same village. Connected and isolated households are similar on many dimensions at baseline, such as consumption, income, employment, and land ownership. This suggests their needs and preferences for risk-sharing might not differ. However, connected and isolated households do differ according to two classes of characteristics. The first class relates to life cycle and cohort effects, so that for example, isolated households tend to be older and less educated. The second class relates to the extended family providing insurance and investment capital to its members. In particular, connected households experience more temporary migration, own more durable assets, and hold lower levels of precautionary savings. This is consistent with the conjecture that connected households have a lower need for precautionary savings than isolated households, as they can share risk with their extended family.

Second, we find that connected households share risk within their family network but not with isolated households. When households eligible for *PROGRESA* receive such cash transfers, the consumption of their ineligible relatives significantly increases, while the consumption of unrelated ineligibles does not. Consumption for ineligible households is a positive function of the size of the income shock in the network. Hence, the greater the (per member) injection of cash into the family network, the greater its members' consumption, regardless of which eligible household receives the positive income shock from *PROGRESA*. For each extra dollar received by an eligible household, her non-durable consumption increases by 54 cents, while the consumption of her ineligible relatives increases by 13 cents. On the other hand, the consumption of the subset of households that are eligible, connected, and all of their extended family members within the village are also eligible for *PROGRESA*, increases by a larger amount – 69 cents – for each dollar received of *PROGRESA* transfers.

Third, the longitudinal coefficient of variation of consumption is significantly smaller for connected than isolated households. If connected and isolated households have similar income processes and preferences (as suggested by our first result that connected and isolated household have similar consumption, income, employment, and land ownership at baseline), the difference in consumption smoothness indicates that connected household are better insured against idiosyncratic risk than isolated households.

Fourth, when connected and isolated households experience similar resource shocks, this has different effects on human capital investment. 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, when the household head falls ill – a negative income shock – school enrollment decreases more and child labor increases more for isolated than for connected households. These findings are consistent with our hypotheses that the extended family may both help finance lumpy investments such as child schooling and prevent households hit by negative shocks from disinvesting by providing insurance.

Fifth, between 1997 and 2003 (the first and last years in our data), assets, income, and consumption increase more for connected than isolated households. This is consistent with our hypothesis that the extended family enables its members to undertake more investment, and thereby increase its long-term income and consumption. This finding is also consistent with our first result that the connected have more investment, durable assets, and fewer precautionary savings than the isolated at baseline.

Our results suggest that the extended family improves insurance and investment. This conclusion is based on the assumption that our empirical analysis successfully controls for unobserved correlates of "connectedness" that affect also consumption and investment. The following considerations are consistent with this causal interpretation. First, while the extended family affects insurance and investment ex-post, it is not obvious whether and to what extent this is an ex-ante determinant of its formation. For example, the structure of family networks is also shaped by a host of other factors such as the net benefits of other services provided by the family network, the nature of household production and the operation of land markets, behavior within marriage markets, mortality outcomes, fertility choices, and the provision of bequests [Foster 1993, Foster and Rosenzweig 2002, Munshi and Rosenzweig 2005]. Moreover, 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.[1]

Second, even if the need to alleviate capital market imperfections is one likely determinant of family formation, the structure of family networks is also shaped by other factors, such as the value of services provided by social networks, bequests, and the nature of household production (e.g. Foster 1993, Foster and Rosenzweig 2002, Munshi and Rosenzweig 2005). Therefore, even if family formation is likely non-random, it is not clear whether insurance- and investment-based considerations are the primary determinants of its formation. Indeed, our descriptive evidence suggests that connected and isolated households do not seem to have different time and risk preferences.

Third, since we have both cross sectional experimental variation and a panel, we can control for additively-separable (time varying) unobserved sources of heterogeneity. Lastly, what strengthens our interpretations of the empirical findings is that they match the predictions of the theoretical model. Since one cannot experimentally create family networks, or force random subjects to trust each other, our exercise - using exogenous income variations to test theoretical predictions - is probably the best one can do to assess the value of this informal institution.

As stated at the outset, understanding whether and to what extent members of extended family networks share resources and how this affects insurance and investment has important implications for the design and evaluation of policy. On policy design, if, as our evidence suggests, extended families are an important resource-sharing institution in rural Mexico, isolated households might be especially vulnerable to idiosyncratic risk. This is especially the case as family structures typically change only slowly through time. Hence, it might be optimal for policy makers to initially target isolated households when intervening in financial markets in such rural economy settings.

On policy evaluation, failure to consider that households share resources within the family network biases the estimated treatment effects of *PROGRESA*. For example, in terms of consumption outcomes, the average effect on consumption in family networks is higher than its effect on eligible households alone, as consumption increases both for eligible and ineligibles. In terms of lumpy investment outcomes, only connected households increase secondary school enrolment in response to the program. Eligible but isolated households do not respond to the program in terms of changing their behavior with respect to this type of lumpy investment. More generally, this paper highlights how we need a theoretical basis to guide our empirical analysis. The heterogeneous treatment effects we have just discussed would not have been searched for nor measured without a theory of the role of extended family networks.

[1] 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.

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

We develop a stylized model of resource-sharing and investment tailored to our empirical setting and the measurable features in our data. The main purpose of the model is to derive testable implications on differential responses to resource shocks between connected and isolated households, in terms of their consumption and investment choices. We view the model as providing a parsimonious framework in which to interpret our results, although many subsets of the implications are consistent with other classical insurance models. In the final section of the paper we speculate on the how the analysis might be extended to distinguish between alternative theoretical frameworks.

2.1 Insurance

Consider a simple exchange economy with no storage technology, no leisure, and two infinitely-lived, risk-averse households, $h \in \{j, l\}$ with instantaneous utility function $u_h = (1-\delta)\ln(c_h)$, where c is a perishable composite consumption good, and $\delta < 1$ is the rate of intertemporal preference. There are s finite states of the world, each occurring with probability $\pi(s)$ so $\sum_s \pi(s) = 1$. Households receive an exogenous endowment in period t, $y_h^t(s)$, and $Y^t(s)$ denotes the sum of endowments in period t. We assume $var(y_j^t) = var(y_l^t) = \sigma^2$ and $cov(y_j^t, y_l^t) = \rho\sigma^2$, where ρ is correlation coefficient between the endowment processes.

Transferring resources across households is costly and model this using an iceberg-type transaction cost, α. Therefore if household j transfers an amount d to household l, household l actually receives $(1-\alpha)d$. The transaction cost captures in reduced form various mechanisms through which full insurance might not be achieved, such as imperfect information or enforcement costs. The transaction cost is assumed to be predetermined.

Consider the case in which household l makes a transfer to household j, that is $y_l(s) > y_j(s)$. For simplicity we omit time superscripts and assume households have the same Pareto weight. The social planner's problem is to choose each household's consumption to maximize the sum of discounted expected household utilities,

$$\underset{c_j, c_l}{max} U = \sum\nolimits_s \pi(s) \left[\ln c_j(s) + \ln c_l(s)\right], \tag{1}$$

subject to: (i) each household's budget constraint: $c_j(s) = y_j(s) + (1-\alpha)d(s)$, $c_l(s) = y_l(s) - d(s)$; (ii) the aggregate resource constraint: $\sum_h y_h(s) = Y(s) + \alpha d(s)$; (iii) non-negativity constraints $c_h(s) > 0$, $d(s) \geq 0$. The solution implies the following for insurance and consumption volatility,

Result 1 (Consumption): *(i) the amount of insurance decreases in* α for all α; *(ii)* $Var_t(c)$ *increases in* α *for* $\alpha \in (0, \overline{\alpha}]$.

To see part (i), note that absent transaction costs ($\alpha = 0$), the household with the higher endowment, household l makes a transfer to household j so both households consume half the total endowment and households are thus fully insured against idiosyncratic risk. With positive transaction costs, the transfer from household l to j decreases in the transaction cost, and the households are partially insured, a result in the same spirit as Schulhofer-Wohl [2008]. For $\alpha \geq \overline{\alpha}$, no transfers take place ($d^*(s) = 0$), each household therefore consumes its own endowment. That is, $\overline{\alpha}$ is a sufficiently high level of transaction costs such that households have no incentives to share risk with each other.

To see part (ii) we note first that the longitudinal variance of consumption $Var_t(c)$ is smallest in the absence of transaction costs, $\alpha = 0$, when households are perfectly insured. For $\alpha > 0$, the amount of insurance declines with α and $Var_t(c)$ increases, as shown in the Appendix. $Var_t(c)$ is highest when $\alpha \geq \overline{\alpha}$, in which case transfers are too costly and households revert to autarky, consuming their own resources. In this case $Var_t(c) = Var_t(y)$.

To map these results to our empirical setting, consider a pair of related, or connected household (K), and a pair of unrelated (O) households. If transaction costs are lower than alpha bar for the connected and greater or equal alpha bar for the isolated, two testable hypotheses follow.

First, Result 1(i) implies the optimal consumption of household j is a positive function of household l's endowment, and *vice versa*, only if the two households are related, as unrelated households do not make transfers to each other. Hence an exogenous shock to a household's resources affects the consumption of its relatives, but not of its non-relatives. Second, Result 1(ii) implies that as isolated households do not share risk in the village, their longitudinal consumption variance, $Var(c_h^O)$ is larger than the longitudinal consumption variance for connected households, $Var(c_h^K)$. To summarize,

$$\begin{aligned} [HC\text{-}1] &\quad : \quad \frac{\partial c_j^K}{\partial y_l^K} > 0, \quad \frac{\partial c_j^O}{\partial y_h^O} = 0 \\ [HC\text{-}2] &\quad : \quad Var(c_h^O) > Var(c_h^K). \end{aligned} \tag{2}$$

Our model can be extended in a number of directions. For example, we could model the process of extended family formation; we could explicitly account for limited commitment or moral hazard; we could model within-households bargaining or let agents have heterogeneous risk preferences or altruistic preferences for their relatives. These extensions are beyond the scope of our paper, as our key hypothesis, HC-1, would hold in all these cases. Hypothesis HC-2, on the other hand, may not, e.g. if connected and isolated households have different endowment processes or time and risk preferences.

2.2 Investment

The presence of a resource-sharing institution such as the extended family networks affects the investment opportunities of its members in at least two ways. First, by providing insurance, it insulates its members' investment from idiosyncratic shocks. Second it may act as a shareholder (or lender). In the presence of borrowing constraints and non-convexities in the investment good, a group of relatives might pool resources to finance one investment, sharing the returns, which a single household would not be able to undertake on its own. We model these insurance and financial effects on investment below.

2.2.1 Continuous Investment

We consider a two-period version of the previous model in which we add the possibility to invest. As before, households $h \in \{j, l\}$ receive exogenous endowments in each period $t \in \{1, 2\}$, $y_h^t(s)$. Investment, I, has risk-free gross returns $f(I)$, where $f(.)$ is assumed concave.[2] The central planner chooses investment, and transfers, taken as given endowments, the investment return function, and transaction costs, to solve the following problem,

$$\max_{I_j^1, I_l^1, d^1, d^2} U \equiv \sum\nolimits_s \pi(s) \sum\nolimits_h \sum\nolimits_t \beta^{t-1} \ln c_h^t(s), \tag{3}$$

subject to: (i) each household's budget constraint in each period: $c_j^1(s) = y_j^1(s) + (1-\alpha)d^1(s) - I_j(s)$, $c_l^1(s) = y_l^1(s) - (1+\alpha)d^1(s) - I_l(s)$, $c_j^2(s) = y_j^2(s) - (1+\alpha)d^2(s) + f(I_j(s))$, $c_l^2(s) = y_l^2(s) + d^2(s) + f(I_l(s))$; (iii) 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 $\sum_h y_h^2(s) + f(I_h^2(s)) - \alpha d^2(s) = \sum_h c_h^2(s)$; (iv) non-negativity constraints $c_h^t(s) > 0$, $d^t(s) \geq 0$, $I_h^1(s) \geq 0$.

Without loss of generality, we choose a realization of income processes such that household l receives a transfer in the first period and makes a transfer in the second. We focus mainly on two cases: when $\alpha = 0$ so households are fully insured against idiosyncratic risk, and when $\alpha \geq \bar{\alpha}$, where $\bar{\alpha}$ is sufficiently large so there are no transfers. In the Appendix we show the following claim holds in the two extreme cases, e.g. $\alpha = 0$ and $\alpha = \bar{\alpha}$ and provide numerical evidence for intermediate values of α (Figure A1),

Claim 1 (Continuous Investment): *(i) I_h^* is a positive function of own endowment for all α; (ii) investment is less sensitive to contemporaneous changes in own income when $\alpha = 0$.*

In other words, we claim that households in resource-sharing networks do not have to disinvest as much as autarkic households when hit by a negative idiosyncratic shocks precisely because their shock is partially insured against. However, when such households experience positive income shocks they cannot invest as much as autarkic households because they have to share some portion of their resource increase with their resource-sharing network. We refer to this mechanism as the 'insurance motive' for investment. In the empirical analysis, we exploit positive and negative shocks to household income to fully explore the asymmetric implications of this mechanism.

[2] We assume investment returns are certain and observable for simplicity. If investment returns are uncertain, or investment is fully or partially hidden, the differences in investment choices between risk-sharing connecting households and isolated households are even more pronounced than those described in the main text.

2.2.2 Lumpy Investment

For lumpy investments, $f(I)$ is non-convex because $f(I) = 0$ for $I \leq \overline{I}$, and $f(.)$ is concave for $I > \overline{I}$. If households face borrowing constraints and are unable to finance such investments by themselves, the presence of the extended family might enable a household to overcome such constraints and achieve higher levels of lumpy investments on average. In particular, one of the two related households can act as a shareholder, providing part of the capital to undertake the investment and then receiving part of the revenues. In this case, both households sacrifice current consumption in favor of future consumption. With such behavior we then have the following result,

Result 2 (Lumpy Investment): *(i) the partial effect of own endowment on investment is discontinuous: it is zero up to some threshold $\overline{y}$, after which it is positive; (ii) if relatives act as lender or shareholder, this lowers the threshold.*

Pulling together the implications of the insurance and credit motives for investment, if the insurance motive for investment dominates, then the effect of a given change in own endowment on optimal investment will be smaller for the two households if they are related (K) than if they unrelated (O). Conversely, if the credit motive prevails, the opposite is true. To summarize,

$$[HI\text{-}1]: \begin{cases} \frac{\partial I_h^O}{\partial y_h^O} > \frac{\partial I_h^K}{\partial y_h^K} \text{ if the insurance motive dominates,} \\ \frac{\partial I_h^O}{\partial y_h^O} < \frac{\partial I_h^K}{\partial y_h^K} \text{ if the credit motive dominates,} \end{cases} \tag{4}$$

In the empirical analysis, we exploit information on investments into continuous and lumpy investments to fully explore the implications for both types of investment highlighted by HI-1.[3] Irrespective of which motive dominates, we may expect households in resource-sharing networks to be able to invest more in the long run.

Claim 2 (Average Investment): *Average investment may be higher when there are no transaction costs than when the households are in autarky ($\alpha \geq \bar{\alpha}$), $E\left[I^{*\alpha=0}\right] - E\left[I^{*\alpha=\bar{\alpha}}\right] \geq 0$.*

Although this cannot be unambiguously proven even in this highly stylized framework, in the Appendix we present simulations to shed light on the conditions under which this result holds.

The remainder of the paper provides evidence related to the results and claims developed on consumption and investment behavior developed in this Section.

[3]We also note that the insurance motive leads to the formation of links between households with negatively correlated shocks, the investment motive favors links between households with positively correlated income processes. In this sense, there is a trade-off between the insurance and the investment motive to form resource-sharing networks. Intuition suggests the former should prevail for poor households in developing countries, for which the cost of negative shocks are potentially very high. While in our framework we take as given family networks, in the empirical analysis, we provide a more detailed discussion on the formation of family networks in this setting.

3 The *PROGRESA* Program

3.1 Data

We use household panel data that was collected to evaluate the *PROGRESA* social assistance program in rural Mexico. This is an appropriate setting in which to test the basic hypotheses developed because households face substantial risk and financial markets are imperfect.[4] The data represents a census of households in 506 villages from seven states in rural Mexico. Baseline data was collected in October 1997, and households were re-surveyed approximately every six months until November 2000, with a final wave in November 2003. The data provides detailed information on food consumption and investment. More precisely, there is data on less lumpy investments such as those into assets and livestock, and lumpier investments, such as those into secondary schooling. We consider schooling as a lumpy investment because: (i) it is only profitable if one attends classes regularly and for extended period; (ii) it has fixed costs such as out of pocket expenses on school materials and time costs of travel. Consumption and school enrolment 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. Appendix Table A1 shows, by panel wave, the availability and definitions of the key variables we exploit. Table A2 shows the sample households, data waves exploited, and estimation method, for each results table.[5]

The *PROGRESA* data we exploit has information on each household member's surnames. As explained below, we combine information on the paternal and maternal surnames of household heads and their spouses, with the double-surname Hispanic naming convention to build the inter and intra-generational extended family links between any pair of households within the same village. This allows us to classify households as being 'connected', if they have close relatives in the same village, or being 'isolated' if none of their extended family resides in the same village.

3.2 Program Features

PROGRESA is a cash-transfer anti-poverty 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 October 1997, although only a smaller share is treated initially.[6]

Eligible households receive a fixed unconditional transfer of 100 *pesos* per month. In addition, there are

[4]Although it has some unique features, *PROGRESA* is just 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.

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

[6]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.

conditional cash transfer components. One class of such transfers is conditional on eligible members attending nutrition and health classes and having regular health checks. A second variable component is conditional on children attending classes between 3rd and 9th grade. These transfers vary between 140 and 510 *pesos* per child, increase with school grade and are larger for girls than for boys for grades 7 to 9. The grant is capped at 625 *pesos* per month.

While nominally conditional, a substantial component of the schooling related transfer is *de facto* unconditional. This is because pre-program enrollment rates up to 6th grade, corresponding to primary school, are higher than 90% and the health checks are infrequent for most eligible persons [Skoufias 2005]. On average, 30% of the total potential grant is associated with scholarships for grades 3 to 6, so a substantial portion of *PROGRESA* transfers act as a pure income effect for recipient households. This closely matches the comparative static results developed previously on the effect of exogenous income increases on consumption and investment behaviors.[7]

In contrast, pre-program enrolment rates for children in grades 7 to 9, corresponding to secondary school, are far lower at around 65%. That is, 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, this partial transfer conditionality and its relatively low value, compared to the opportunity cost of secondary school attendance, suggest *PROGRESA* may not provide some eligible households with a sufficient potential increase in resources to raise the secondary school attendance of their children.[8]

This distinction is key to assessing how eligibility differentially affects secondary school enrolment for connected and isolated households. The framework developed highlights that if isolated households are less likely to receive resources from others in the village, their behavioral response to *PROGRESA* in terms of lumpy investments such as secondary school enrolment, should substantially differ from connected households that share-resources with their extended family within the village because of the credit motive for investment that the extended family provides.[9]

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

[8]One third of eligible households have no children in the subsidized grades in November 1998 and receive only the fixed transfer component. Of the remaining households, 87% have some primary school children. The figure for households with at least one secondary school child is 57%.

[9]The evaluation data was collected during a period when the program was being expanded throughout rural Mexico. Hence even if relatives of isolated households reside in close by villages outside of the valuation sample, these relatives are far less likely to receive the increase in resources caused by *PROGRESA*. Moreover, even if this were the case, isolated households would still experience higher transactions costs in resource transfers across villages, than for connected households who transfer resources within the same village, as emphasized in the model.

3.3 Evaluation Design

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. 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.[10]

The evaluation data contains information on the eligibility status of all households in treatment and control villages. In 1997 households were classified as either being eligible (poor) or ineligible (not poor) according to a household wealth index. This index is a weighted average of household income (excluding children), household size, durables, land and livestock, education, and other physical characteristics of the dwelling. The index is designed to give relatively greater weight to correlates of permanent income rather than current income. Households were informed that their eligibility status would not change at least until November 1999, irrespective of any variation in household income. A distinguishing feature of *PROGRESA* is that households were clearly informed about the program's introduction through village-wide assembly meetings. These meetings also ensured households agreed with their designated eligibility status. Hence take-up rates for at least one component of the program among eligibles are over 90% and we do not therefore distinguish between intent-to-treat and treatment effects.[11]

4 Descriptive Evidence

4.1 Village Characteristics

In rural Mexico households face substantial risk and financial markets are imperfect. The sampled villages are small, with a median size of 46 households. Agriculture is the main activity and there is little crop diversification. At baseline in October 1997 corn is the main, and often sole, crop in 88% of villages. Income is volatile – the longitudinal coefficient of variation of income is .70. This lack of crop diversification and high level of income volatility suggest a high need for insurance, yet there are few formal credit or insurance institutions. For example, in November 1998 fewer than 1% of villages have credit or consumption cooperatives, and fewer than 3% have NGOs or production associations.

Despite the lack of formal insurance and credit markets, consumption is more stable than income. The longitudinal coefficient of variation of consumption is half that of income, suggesting households engage in informal resource-sharing activities. Following Cochrane [1991], Mace [1991], and Townsend [1994], we regress household consumption growth on the growth of village aggregate resources and on household income. The coefficient on aggregate resources is .985, and is not significantly different from one. The coefficient on

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

[11]The monetary value of transfers that eligibles were entitled to was determined by the age and gender composition of the children resident in the household at baseline. There are therefore no incentives for eligibles to foster children from ineligibles with the aim of obtaining more transfers.

the growth of household income is .025 and is significantly different from zero. With full insurance, these coefficients should have been one and zero respectively. This preliminary evidence suggests there is a high, yet inefficient degree of insurance against idiosyncratic income fluctuations at the village level. Hence it is worth exploring whether subgroups of households are better insured than others.[12] 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 plausibly exogenous income changes.

4.2 Identifying 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, and in conjunction 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 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.[14] Figure 1 illustrates the matching algorithm. Each link is constructed using information on *two* of the four surnames. 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 define there to be 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

[12]For example, Townsend [1994] performs the analysis at the land-ownership level, while Mazzocco and Saini [2009] focus on the caste level, both using the ICRISAT data. Neither rejects full insurance.

[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.

[14]The precise wording of the question in Spanish is, "*Dígame por favor el nombre completo con todo y apellidos de todas las personas que viven en este hogar, empezando por (jefe del hogar) – (i) nombre; (ii) apellido paterno; (iii) apellido materno*". We cleaned the surnames data as follows – (i) we removed non-alphabetical characters, replaced "Sin Apellido" (no surname) with missing values, and corrected some obvious typos based on intra-household surname checks; (ii) we imputed a small number of missing female surnames from wave 2; (iii) we verified surnames using the same information from wave 5, and verified the relationship to the household head using wave 1 data. No information on surnames is available in the first wave of data. The head of household is originally defined to be the main income earner. In a very small number of cases the head of a couple headed household is reported to be a women. To keep clear the exposition, we redefine the head to be male in such cases.

difference.

There are three points to emphasize. First, 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 it is still classified as isolated. Our emphasis on within-village networks for insurance is also partially driven by the fact that many transfers between households typically take place in-kind, for example in the form of produced goods and productive inputs such as labor exchange, that would not, by their nature, cross village borders or be monetized very easily.

Second, a specific set of concerns arise for single-headed households. It is possible to classify a single-headed households that is in fact connected as isolated because we cannot observe the extended family members of the missing spouse. Since single-headed households are older and poorer than couple-headed ones, including them when comparing outcomes for connected and isolated households would bias our estimates, and the direction of bias is unclear. Therefore, when our tests compare connected and isolated households, we drop the single-headed. However, when we compare outcomes *within* connected households we include both the couple-headed and the single-headed. In the Appendix we provide evidence on the reliability of our matching algorithm.

Third, many forms of measurement error that stem from our matching algorithm based on information on two surnames, would cause us to incorrectly label households as being connected when they are truly isolated. All else equal, this makes it harder to identify significant behavioral differences between connected and isolated households. In the Appendix we provide evidence on the potential incidence of some forms of measurement error.

As Table A3 shows, we find that 80% of couple-headed households are connected, with the remaining 20% being defined to be isolated. We also note that links from the head are significantly more likely to exist than links from his spouse, and that intra-generational family ties are significantly more common than inter-generational ties. These patterns hold for eligibles and ineligibles in treatment and control villages. Reassuringly, in nearly all cases 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 seven family dynasties. Finally, around 50% of extended family networks comprise a mix of eligible and ineligible households, and this rises to thirds when we consider networks of more than two households.

4.3 Correlates of Household Connectedness

Table 1 shows the mean and standard deviation of a series of socioeconomic variables as measured mostly at baseline in October 1997. Columns 1 and 2 split these variables between connected and isolated households, focusing on couple-headed households throughout. Column 3 reports p-values on a test on significant differences between the two, allowing for within-village clustering.

Connected and isolated households appear remarkably similar on many observable dimensions. In particular, income, food expenditure, and the wealth index – the basis for *PROGRESA* eligibility – are almost identical, as are the amounts of land owned or used, adult and child labor, and most assets and livestock. We also performed a Pearson's chi-squared test of the difference in the distribution of unemployment and occupation for all members of connected and isolated households at least eight years old, and found no significant differences.[15] If land ownership and employment patterns reflect risk preferences or need for insurance, as well as income processes, this evidence suggests isolated and connected households may have similar preferences and income processes, hence a similar need for resource-sharing.

However, connected and isolated households do differ in two distinct classes of characteristics. The first class relates to life cycle and cohort effects. For example, isolated household heads are 2.5 years older, have fewer young children, and are 9% more likely to be illiterate than heads of connected households. The second class of differences relates to the predictions of the framework developed that connected households are better insured than the isolated, and have a greater set of investment opportunities available. There are three such pieces of suggestive evidence. First, isolated households own 12.5% more poultry, which is the most liquid form of livestock households in this environment can hold. Hence it appears that isolated households find it optimal to hold a larger stock of savings. Second, isolated households are 16% less likely to have temporary migrants. We might view temporary migration as a lumpy investment because such actions involve large up-front costs with potential benefits only to be realized in the future. Third, isolated households are less likely to own durable goods as stoves and TV sets (6% and 9% respectively), again consistent with them having fewer investment opportunities, resulting in lower permanent income and fewer durables.

Later in the paper our empirical method exploits the random assignment of *PROGRESA* across villages. 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 randomization, most of the variables do not have statistically different means in control and treatment villages.[16] To address concerns related to the differences that do exist, we note that – (i) we estimate treatment effects using difference-in-difference estimators; (ii) we control throughout for variables that are unbalanced between treatment and control villages at baseline.

Comparing means for connected and isolated households is somewhat misleading if there is assortative matching. For example, all the wealthiest and poorest households may belong to distinct family network, but by aggregating all families we would not detect it. We test for assortative matching in wealth by computing

[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]A few notable exceptions for the isolated are the differences in the number of oxen and goats owned, temporary migration, and the share of 11-16 year old children enrolled – all of which are lower in control villages. For the connected, the number of oxen owned is slightly lower in control villages. The share of households potentially eligible for *PROGRESA* is also statistically different (10% level) in control villages (the difference disappears if single-headed households are included).

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.

5 Do Family Networks Share Resources?

5.1 Method to Test Hypothesis HC-1

Our first hypothesis HC-1, is that for a given pair of households $h \in \{j, l\}$, the consumption of household j is a function of household l's income, $\frac{\partial c_j^K}{\partial y_l^K} > 0$, *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. However, a key feature of our data is that the *PROGRESA* program experimentally creates large and measurable variations in households income. The research design, summarized in Figure 2, is best described as a partial population experiment (Moffitt, 2001). We use this to first establish whether households share resources only with their extended family or with the entire village, under weak identifying assumptions.

We discuss the test of this hypothesis using the potential outcomes notation. We 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. We then define y_{1h} (y_{0h}) as the potential outcome for household h in treatment villages in the presence (absence) of the treatment. 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,

$$\begin{aligned} ATE(y)^f &= E(y_{1h}|P_h = 1, N_h = 0, f = 1) - E(y_{0h}|P_h = 1, N_h = 0, f = 1), \qquad (5) \\ ITE(y)^f &= E(y_{1h}|P_h = 1, N_h = 1, f = 1) - E(y_{0h}|P_h = 1, N_h = 1, f = 1), \end{aligned}$$

where $f \in \{K, O\}$. Under the assumption of random assignment and in the absence of program spillover effects to control villages, the expected value of the potential outcome in the absence of the treatment, y_0, is the same in both treatment and control villages, i.e. $E(y_{0i}|P_h = 1, N_h = j, f = 1) = E(y_{0i}|P_h = 0, N_h = j, f = 1)$, for $j \in \{0, 1\}$ and $f \in \{K, O\}$. Therefore, the differences,

$$ATE(y)^f = E(y_h|P_h = 1, N_h = 0, f = 1) - E(y_h|P_h = 0, N_h = 0, f = 1), \qquad (6)$$

$$ITE(y)^f = E(y_h|P_h = 1, N_h = 1, f = 1) - E(y_h|P_h = 0, N_h = 1, f = 1), \qquad (7)$$

identify the ATE and ITE. To test HC-1 we compare the signs of the ITEs for connected and isolated 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 estimate $ITEs$ from the following regression, estimated for ineligible households h in time period t,

$$c_{ht} = \delta_0 + \delta_1 P_h + \delta_2 O_h + \delta_3 [P_h \times O_h] + \delta_4 T_t + \delta_5 [P_h \times T_t] + \delta_6 [P_h \times O_h \times T_t] + \delta_7 x_{ht} + u_{ht}, \quad (8)$$

where c_{ht} is food consumption per adult equivalent. Using data from October 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.[17] 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_{ht} includes a set of household and village characteristics measured at baseline and time and region dummies, and u_{ht} is a disturbance term. 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.[18]

The parameters δ_5 and $\delta_5 + \delta_6$ identify the ITEs for connected and isolated households respectively. Our method therefore uses a triple-difference estimator, comparing the treatment effects for the connected and the isolated using both the cross sectional and longitudinal variation in our data, as well as the randomization. Hence, our estimates account for additively-separable time-invariant and time-varying sources of heterogeneity across households that might drive both consumption and whether the household is connected or isolated.[19]

5.2 Results

Table 2 presents the results. Column 1 shows the baseline estimate of (8). In line with HC-1, the indirect effect of *PROGRESA* on household consumption is positive and significant for households with eligible relatives resident in the same village ($ITE(c)^K > 0$), 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, so that $ITE(c)^K > ITE(c)^O = 0$. Hence households that are not themselves directly eligible for *PROGRESA*, appear to have significant increases in consumption if they have extended

[17]For each item, there is 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 use this 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.

[18]We also experimented with clustering by family network, which generally resulted in slightly smaller standard errors. x_{ht} includes 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. Finally, the adult equivalence scale used for consumption, derived in Di Maro [2004], is one for members 18 or older, and .73 otherwise.

[19]One issue is whether to estimate the treatment effects on consumption using difference-in-difference estimates, or only using data after *PROGRESA* initiates as we observe only pre-program expenditures. We have used both methods and found the results to be qualitatively similar.

family members in the same village. In contrast, consumption within ineligible and isolated households is not responsive to exogenous increases in resources to other households in the village caused by being randomly assigned to a *PROGRESA* village.

The remaining columns explore the conjecture that consumption for connected ineligible households increase only when their eligible relatives receive *PROGRESA* transfers. To do so, Column 2 estimates the ITE among connected households, distinguishing between connected households with at least one eligible family member in the village, and those that have no eligible family members. We find that the ITE is only positive and significant for connected households that have eligible extended family members in the village. In Column 3 we distinguish between connected households where at least 30% of their extended family network is eligible, relative to those that have no eligibles in their family network. The ITE is again only positive and significant for connected households that have eligible relations in the village, and it is of larger absolute value (32.0) relative to the estimate in Column 2 which pooled together all connected households with at least one eligible relative (18.4). As reported at the foot of Table 2, the differences in ITEs in each Column is positive and significant, so that $ITE(c)^K > ITE(c)^O = 0$.[20]

To quantify these effects, we note an average counterfactual consumption level of about 200 *pesos.* Hence consumption for ineligible but connected households increases by between 10 and 15% relative to this baseline, with the variation caused by the extent to which relatives are themselves eligible for *PROGRESA* transfers. Hence we find consumption increases of ineligibles to be a positive function of the change in available resources at the network level, in line with resource-sharing within family networks.[21]

In the Appendix we show that the ITE on consumption do not vary by land ownership or ethnicity, and discuss how household consumption responses to another resource shock in the network – the self-reported illnesses among other household heads in the family network.

5.3 Further Evidence

We now provide 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 (6), 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

[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.

[21] A caveat to the results in Columns 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.

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,

$$\ln c_{ht} = \alpha_0 + \alpha_1 \ln g_{ht} + \alpha_2 x_{ht} + u_{ht}. \tag{9}$$

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 a function of the household's demographic characteristics at baseline. The data waves used are May and November 1999, and we cluster the standard errors by village.[22]

Columns 1 and 2 of Table 3 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 the OLS estimate, which is .075 (Column 1). In Column 3 we repeat the exercise focusing on family networks in which at least 30% of members are eligible. The IV estimated is positive, significant, and the point estimate is larger than that reported in Column 2. 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, and by 16 cents in networks in which at least 30% of the members are eligible.[23]

One concern with these results is that there might exist unobservables driving both the formation of family networks and consumption over time. To address this, in Columns 4 and 5 we estimate using OLS analogous specifications for households in control villages and see that: (i) ineligible households' consumption in control villages is not a function of the potential transfer available that would have been available to the family network, which is a function of the demographic composition of eligible households; (ii) this result remains true in control villages among family networks in which at least 30% of households would have been eligible. This validation exercise suggests the results in Columns 2 and 3 are unlikely to be driven by unobservable correlates of consumption and network structure.

Our second set of results check whether eligible households with and without ineligible relatives consume different fractions of the transfers they receive. Intuitively, we expect households with (only) eligible relatives to consume a higher share of their transfers, because households with ineligible relatives 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

[22] In this specification, we control for the same variable in x_{ht} 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.

[23] We also estimated analogous reduced-form OLS regressions of the log-food consumption of ineligibles on potential rather than current log-transfers per adult equivalent in treated villages, as those reported in Columns 2 and 3. The reduced form estimates are .093 and .106 and both are positive and significant.

they consume.

Table 4 presents these results, again using data waves from May and November 1999. Eligible connected households that are related to ineligible households increase their food and total non-durable consumption by 28 and 34 pesos per adult equivalent per month out of a 64 peso transfer per adult equivalent network member (columns 1 and 3, rows 1 and 2). In contrast, eligible households without ineligible relatives increase their food and total consumption by 31 and 38 pesos out of a 55 peso transfer (columns 2 and 4, rows 1 and 2). Connected eligible households with ineligible relatives consume a significantly smaller share of their transfer - 56% versus 69% looking at total non-durables - 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)

6 Consumption Smoothness

6.1 Method to Test Hypothesis HC-2

We now test hypothesis HC-2 that consumption is smoother for connected than for isolated households. To do so, we compare the longitudinal coefficient of variation of food consumption for connected and isolated households. Unlike hypothesis HC-1 which holds under very general assumptions, HC-2 might not hold if connected and isolated households have different income processes. The descriptive evidence presented in Section 4 suggests this is not the case in this particular empirical setting. To present evidence related to HC-2, we run the following regression for household h using data from March 1998 to November 2000,

$$\ln CV_h(c) = \alpha + \beta O_h + u_h, \tag{10}$$

where $\ln CV_h(c)$ is the log of the coefficient of variation of food consumption for the household over time, O_h is a dummy equal to one if household h is isolated, and zero if it is connected, and we cluster the standard error by network (for the connected). The parameter of interest is β which captures whether isolated and connected households differ significantly in their consumption volatility.

6.2 Results

Estimating (10) we find that $\widehat{\beta} = .060$, with a standard error of .010, so we reject the null that $\beta = 0$ at the 1% significance level. Hence, consistent with hypothesis HC-2, the data suggests consumption is 6% more volatile for isolated than connected households. We find very similar results $\widehat{\beta} = .054$ with standard error .009 when we condition on the longitudinal coefficient of variation of household income. The similar magnitude of the two estimates suggests that income volatility alone cannot explain the more volatile consumption profiles for the isolated households. While the magnitude of $\widehat{\beta}$ may not seem large, even a small increase in consumption smoothness can cause a sizeable increase in welfare for households close to subsistence level [Chetty and Looney 2006]. Absent resource-sharing networks, risk-averse poor households might be willing to engage in costly activities to ensure a stable consumption path, including reducing high-return investments [Arrow 1971, Obstfeld 1994]. Obviously, this need to disinvest to better face short-run income fluctuations might also have negative long-run effects for the household, and be a driver of inequality between households with and without extended family members in the village. In the next Section we therefore compare the long-term changes in capital, income, and consumption for connected and isolated households.

6.3 Further Evidence

The previous empirical analysis has established that connected households' longitudinal consumption pattern is consistent with: (i) resource-sharing occurring within the extended family; (ii) their having smoother consumption paths than isolated households. These results then beg the question of whether connected households are *fully* insured against idiosyncratic risk. To present evidence on this, we note that from the maximization problem in (1), the optimal consumption level in the absence of transactions costs is a fixed share of aggregate resources, so that each household consumes half the total endowment and both households are fully insured. Taking the log of this expression and first differencing yields the well-known relationship between the growth of individual and aggregate consumption for household h at time t,

$$\Delta \ln c^*_{ht} = \Delta ln\overline{Y_t}. \tag{11}$$

We regress the growth in household consumption on the growth in aggregate consumption and household income using the following specification,

$$\Delta \ln c_{ht} = \beta_1 \Delta \overline{lnY_t} + \beta_2 \Delta lny_{ht} + u_{ht}, \tag{12}$$

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 $\alpha = 0$, consumption growth then does not depend on the growth of the idiosyncratic component of the endowment. Hence we should find that $\beta_2 = 0$. We measure aggregate resources in the network, Y_t, using the total consumption in the family network. As we do not identify which households the isolated share risk with, if any, we cannot estimate the corresponding regression for isolated households.

Table 5 presents the results for connected households. Column 1 shows that for all connected households included single-headed households, we reject the null of efficient risk sharing ($\widehat{\beta}_2 \neq 0$). Column 2 shows this remains the case if we only focus on couple-headed households. However, while household consumption co-varies with own income, conditional on network resources, the magnitude of the effect is small: $\widehat{\beta}_2$ is smaller than .02 in both specifications. Moreover, if risk preferences are heterogenous, income is likely to be endogenous and $\widehat{\beta}_2$ is upwards biased [Schulhofer-Wohl 2008, Mazzocco and Saini 2009].[24]

[24] If we estimate (12) using OLS for all village households, $\widehat{\beta}_2 = .025$ and is significantly different from zero. When estimated using IV, the standard error increases although its point estimate still falls to .016.

An additional source of bias exists if income is measured with error. To address this issue the last two columns present analogous estimates using IV, using the second lag of total household income as an instrument for current income. In these case we still do not reject the null hypothesis that the extended family network achieves full insurance. While these results should obviously be interpreted with some caution, the pattern of coefficients does suggest that transaction costs among extended family members are rather small.

7 Do Households in Family Networks Invest More?

The evidence presented thus far suggests consumption is smoother for connected than isolated households. This relative smoothness might still underestimate the true value to being within a resource-sharing extended family network if such networks also allow their members to increase their investment levels. As emphasized before, this can occur through two channels: (i) the presence of a resource-sharing family makes investment less sensitive to idiosyncratic income fluctuations, so connected households do not have to disinvest as much when hit by negative shocks – this is the insurance motive for investment; (ii) in the presence of imperfect financial markets, family networks might pool resources to partly finance an investment that a single household could not undertake – this is the credit motive for investment. To understand which of the insurance and credit motives dominates overall, we first directly test hypothesis HI-1, which follows from Result 2.

7.1 Method to Test Hypothesis HI-1

As before, we exploit the receipt of *PROGRESA* transfers as a positive income shock that is orthogonal to household's connectedness. Exploiting such transfers as a positive income shock may be particularly well-suited to study the credit motive of family networks as these represent a large income shock with a sizeable aggregate component, considering that multiple households are eligible for the transfers in many family networks.

To better detect if the insurance role of family networks is relevant, we also use households' self-reported data on illness of the household head during the previous month as a measure of a negative resource shock. The more idiosyncratic nature of health shocks make them better suited to study the insurance mechanism extended families might provide. While clearly the household head's health status may not be as exogenous as eligibility for *PROGRESA*, reassuringly, we find that it is uncorrelated with the household wealth index once we control for the standard set of covariates x_{ht} included in (8).[25] Finally, we note that the incidence of illness for the household head is not statistically different between connected and isolated households, as shown in Table 1. We then estimate the following specification for eligible household h in time period t,

$$\Delta y_{ht} = \theta_0 + \theta_1 \Delta S_{ht} + \theta_2 K_h + \theta_3 \Delta S_{ht} K_h + \theta_4 x_{ht} + \epsilon_{ht}, \tag{13}$$

where Δy_{ht} measures changes in either continuous or lumpy investments, as described below; S_{ht} corresponds

[25] In order to identify a differential effect for connected and isolated households we require either that any non-randomness be the same for these two groups, or that it be additively decomposable so that it cancels out in our triple-difference estimation.

to the measure of the positive or negative income shock, namely eligibility for *PROGRESA* in treatment villages, and the self-reported illness of the household head; K is a dummy equal to one if household h is connected, and zero if it is isolated; the other controls in x_{ht} are as previously defined. Given the earlier evidence that family networks share resources, we cluster the standard errors at the network level to capture common shocks to the network that might affect its investment choices. We note that this estimating equation allows differential trends in the investment outcomes for isolated and connected households, as well as differential trends for households with different observables.

For our outcomes (y_{ht}) we measure lumpy investments into human capital using measures of school enrolment and child labor. More precisely, we measure school enrollment as the share of 11-16 year old children attending school. We measure child labor as the number of weekly days worked per child by 11-16 year old children, considering only households with at least one such child. We view human capital investments as lumpy investments because: (i) such activities are only profitable if one attends regularly and for a number of years; (ii) the fixed costs of school attendance which take the form of out of pocket expenses on school materials, time costs of travel. It is also difficult for households to commit to finance the education of someone else's child, as well as to enforce sharing the returns on such investments. Therefore, financing of, and sharing the returns to, this investment type hinges on the group's ability to solve commitment problems. The extended family might be especially well placed to overcome such commitment problems.

We measure less lumpy investments into physical capital using the number of animals owned, the size of land used or owned (which cannot be separately measured), and agricultural investment expenditures such as the purchase of seeds, pesticides, and fertilizers.[26]

7.2 Results

Table 6 presents the results from estimating (13). The upper panel shows the responses of connected and isolated households to *PROGRESA* eligibility, ATE^K and ATE^I respectively, as well as their difference. On lumpy investments, the results in Columns 1 and 2 for child schooling and child labor are striking: when households experience an exogenous positive income shock, among connected households school enrollment increases by 7.7 percentage points and time spent working decreases by about a quarter of a day per week. In contrast, school attendance and child labor do not change for isolated households. Under this metric the program has positive effects only for connected households, and no effect for isolated households despite these households being eligible for transfers to enrol their children in school.

The lower panel shows the responses of connected and isolated households to an illness of the household head, as well as their difference. On lumpy investments, Column 1 shows that school enrolment is significantly more responsive to negative income shocks among isolated households than among connected households. More

[26]The specifications using *PROGRESA* transfers use data from all the waves from September 1997 to November 1999, using only one wave per school year when school enrolment is the outcome. For specifications using illness of the head, we use waves from November 1998 to November 2003 with some exceptions as are detailed in the Table. 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*.

precisely, while the enrollment of 11-16 years old decreases for all households when hit by a health shock to the household 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.[27] These results on school enrolment are reflected in those for child labor as shown in Column 2. The number of weekly workdays for the same set of children significantly increases by .077 for the connected and by .283 (more than a day of work per month) for the isolated, when the household is hit by a health shock.

The remaining Columns in both panels focus on the effects of income shocks on continuous investments. Here we find rather more mixed evidence of there being differential responses by connected and isolated households to positive or negative shocks. This is not altogether surprising given the difficulty in defining continuous investments. Some of those we use, such as livestock, might serve a precautionary and consumption motive, as well as an investment one. Clearly it is hard to interpret these results given the highly stylized framework developed earlier which abstracts from such motives.[28]

Our framework provides clearer guidance to interpret the results on lumpy human capital investments. For such investments, when the offered subsidy covers only part of its cost, only connected households are able to increase school attendance, because they can pool resources within the family network, unlike isolated households. Indeed, among the eligibles, the increase in school enrollment for 11 to 16 year old children is a positive function of the unconditional grant received by both the household itself and its entire family network [Angelucci, De Giorgi, Rangel and Rasul 2009].

How do the isolated then spend the unconditional component of *PROGRESA* transfers? Besides increasing their consumption as shown in Table 4, these households increase their livestock and land ownership (or usage) more than the connected. The stock of poultry and land increase by 11 and 6.7 percentage points more for the isolated, as shown in Table 6, although these differences are not precisely measured. As the return to poultry ownership is likely lower than the return to secondary education, which in Mexico is about 70% higher than the return to primary school [López-Acevedo 2001], these results suggest isolated and connected households respond differently to *PROGRESA* eligibility, with isolated households favoring investment in physical capital with short-term, low returns, and connected households increasing investment in human capital with long-term, higher returns.[29]

[27]We also checked whether the drop in enrollment associated with illness of the household head is caused by child illness. We find no evidence in support of this, and therefore rule out the possibility that we are picking up the effect of contagious diseases.

[28]With this caveat in mid, our findings, nevertheless, shed some light on the complex ways in which households respond to negative resource shocks. The stock of goats and pigs decreases, although not precisely estimated, while the stock of poultry increases. The move away from swine and towards poultry might suggest households re-allocate their livestock portfolio towards more fungible, short-term, probably lower-yielding investments. For example, the return to swine ownership comes when the animal is slaughtered, and the largest profits are probably made from the sale of ham, lard, and sausages, rather than from the sale of the animal itself. Producing these goods involves substantial labor costs and requires specific skills, as well as physical strength. Conversely, poultry provides smaller but more uniform 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. Bullocks do not appear to be used to smooth consumption, unlike in rural India [Rosenzweig and Wolpin 1993]. Indeed, few households own oxen.

[29]In unreported regressions we confirm that the responses of the households vary depending on the amount of conditional versus unconditional transfers. As expected the unconditional transfers produce income effects.

7.3 Further Evidence

The evidence suggests the capital stock held by connected households is larger than that held by isolated households. If such higher investment causes long-lasting income increases, this will also be reflected in higher consumption. Although we cannot prove this unambiguously even in our highly stylized model, *Claim 2* suggests it is likely to be true under a wide range of reasonable parameter values. To shed light on this, we therefore estimate the change in capital, consumption, and income for connected and isolated households between the first and last years in our data, 1997 and 2003, using the following specification for household h in time period t,

$$y_{ht} = \kappa_0 + \kappa_1 K_h + \kappa_2 T_t + \kappa_3 [K_h \times T_t] + \kappa_4 x_{ht} + u_{ht}, \tag{14}$$

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, stocks of various forms of livestock, dummies for machinery and asset ownership, consumption, and labor income. Income and consumption are measured at the monthly level, and are computed per adult equivalent at November 1998 pesos. T_t is a dummy equal one for 2003, and K_h and x_{ht} 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.

Table 7 reports the difference in the various outcome measures between connected and isolated households between 1997 and 2003, conditional on 1997 levels. That is, we report the estimated change in outcomes for the connected ($\hat{\kappa_2} + \hat{\kappa_3}$), for the isolated ($\hat{\kappa_2}$), and their difference ($\hat{\kappa_3}$). This descriptive evidence is consistent both with *Claim 2*, and the descriptive evidence presented in Table 1 on the cross sectional differences in assets and precautionary savings between connected and isolated households at baseline in October 1997.

In particular, the change in investment between 1997 and 2003 – proxied by the share of household members with at least secondary education, the stock of animals, and the stock of machinery, is significantly higher for connected than for isolated households. The higher stock of physical and human capital increases productivity for the connected, whose income increases more than for the isolated. The higher investment and income drive consumption up, which indeed is significantly higher for connected households, especially for durable consumption goods.

8 Discussion

We have presented evidence on whether extended family networks are an important informal resource-sharing (with features of quasi-credit, Udry 1994) arrangement in rural economies. Our analysis is guided by a theoretical framework emphasizing the role of extended families in providing consumption insurance and widening investment opportunities to their members. We use household panel data from the randomized evaluation of *PROGRESA* in rural Mexico. We exploit information on the paternal and maternal surnames of household heads and their spouses, with the double-surname Hispanic naming convention to build the inter and intra-generational extended family links between pairs of households within the same village. We also closely match the theory and evidence by exploiting exogenous and large changes in household income caused by *PROGRESA*'s introduction.

We find that households belonging to an extended family network share resources within the network and are better able to smooth consumption over time than their neighbors that have no close relatives within the village. We also find that such connected households are more able to undertake lumpy investments such as those into the human capital of their children. As a consequence of such access to the network services, connected households accumulate more resources over time than their isolated, but otherwise broadly similar, neighbors.

There are important implications of these findings, both in terms of the design and the evaluation of public policies. On policy design, consider a micro-insurance intervention, for example. Our results indicate that such a product is more likely to be demanded by isolated households, who do not have access to informal sources of insurance available to households that belong to an extended family. Our results suggest when geographically allocating resources to such interventions, governments might target areas in which family ties are less prevalent, and hence insurance alternatives most needed.

On policy evaluation, treatment effects might be biased if evaluators do not take account of: (i) informal resource-sharing arrangements within extended families so that ineligible and connected households might also benefit; (ii) the effect of resource transfers on a wide range of interlinked behaviors related to consumption, investment, and savings. There might also be either complementarities or substitutabilities between formal (government provided) and informal (network provided) services that need to be accounted for [Albarran and Attanasio 2003, Attanasio and Rios Rull 2003]. This is particularly true for interventions that attempt to address the absence or incompleteness of financial markets, and for conditional cash transfers attached to investment decisions such as those into human capital.

A richer model allowing for heterogenous risk preferences and endogenous bargaining weights in implicit contracts might help shed further light on the nature of informal arrangements within extended families.[30] Incorporating those channels would leave most of the predictions we have investigated unchanged, but at the same time open the possibility of richer empirical implications that could shed light on households' contracting behavior. Further, one might want to study the roles played in informal insurance agreements of particular members of the extended family network, or the households position within the network. We leave such extensions for future work.[31]

[30] We note that if the bargaining weights in informal arrangements were to change noticeably due to *PROGRESA*, the cross-sectional variance of consumption should be smaller in treated than in control villages, and more generally the consumption distribution should differ between the two groups of villages. Angelucci and De Giorgi (2009) do not find this to be the case in these data, in which the introduction of *PROGRESA* is evaluated over a relatively short period of time.

[31] For example, the rapidly expanding literature on the strategic formation of networks provides new theoretical insights on risk sharing and the potentially different roles played by households within the same network [Jackson 2008]. These insights have only begun to be explored empirically [De Weerdt and Fafchamps 2007].

A Appendix

A.1 Proofs

Proof of Result 1: From the first-order conditions of (1), with $y_l > y_j$ we can derive the optimal levels of consumption and transfer in each state,

$$c_j^* = \frac{1}{2(1+\alpha)}[Y + \alpha y_j], \tag{15}$$

$$c_l^* = \frac{1}{2}[(1+\alpha)Y - \alpha y_l], \tag{16}$$

$$d^* = \frac{y_l - (1+\alpha)y_j}{2(1+\alpha)}. \tag{17}$$

To see part (i) we note that $\frac{\partial d^*}{\partial \alpha} = -\frac{y_l}{2(1+\alpha)^2} < 0$. Eventually $d^* \to 0$ as $\alpha \to \bar{\alpha}$. To see part (ii) recall $Var(y_j) = Var(y_l) = \sigma^2$, and $Cov(y_j, y_l) = \sigma_{12} = \rho\sigma^2$, where ρ is correlation coefficient of the two endowment processes. Given the asymmetry in the consumption processes due to the transaction cost, one way to think about the longitudinal variance of consumption in partial insurance (PI) is to compute $Var(c_h^{PI}) = Var(c_j^{PI}) + Var(c_l^{PI})$. The above is just a simplification of an endowment process where the two realizations alternate evenly. Doing so, the variance of consumption in autarky is $V(c_h) = V(y_l) + V(y_j) = 2\sigma^2$, while in partial insurance it is $V(c_h^{PI}) = \frac{((1+\alpha)^2+1)((1+\alpha)^2+2\rho(1+\alpha)+1)}{4(1+\alpha)^2}\sigma^2$. It is then easy to show that $\frac{\partial V(c_h^{PI})}{\partial \alpha} = \frac{\alpha\sigma^2(2+\alpha)\left(\rho(1+\alpha)+(1+\alpha)^2+1\right)}{2(1+\alpha)^3} > 0$.■

Proof of Result 2: Consider the maximization problem (3) where for simplicity we assume linear returns to investment, $f(I) = \theta I$, with $\theta > 1$. With lumpy investment there is a floor to investment, I_{min}, below which no investment occurs. For example when $I_{min} \geq \frac{\theta y_l^1 - y_l^2}{\theta}$ 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 $I_{min} \geq \frac{\theta y_l^1 - y_l^2}{\theta}$, as along as I_{min} does not exceed a certain threshold. When $\delta = 1$ this threshold is $\frac{\theta Y^1 - Y^2}{\theta}$.

For example, consider the case in which $I_{min} = y_l^1 > y_j^1$. 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, $U_{I>0}^F > U_{I=0}^F$ 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 $U_{I>0}^F > U_{I=0}^F$ 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 $U_{I>0}^F > U_{I=0}^F$ then it follows that $U_{hI>0}^F > U_{hI=0}^F$ for $h = \{j, l\}$.

For this particular example with a high floor to investment $I_{min} = y_l^1$, 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.

A.2 Claims

Claim 1: Consider the maximization problem (3) where was assume $y_j^1 < y_l^1$ so that household j receives a transfer from household l, and consider a concave investment function of the form $f(I) = I^\gamma$, with $\gamma \in (0, 1)$. Further, to simplify, consider the case of no-endowment in the second period. We study two cases: $\alpha = 0$ and α sufficiently large that there are no transfers, that is the households are in autarky and consume and invest their own resources only.

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

$$\begin{aligned} I_h^{FI} &= \frac{\delta\gamma(y_j^1 + y_l^1)}{2(1+\delta\gamma)}, \qquad (18) \\ \frac{\partial I_h}{\partial y_h^1} &= \frac{\delta\gamma}{2(1+\delta\gamma)}. \end{aligned}$$

Each household chooses the same consumption and investment because they have the same Pareto weights and face the same return to the investment. We note that in the absence of transaction costs, investment then depends only on aggregate resources.

In autarky, so α is sufficiently large, we have the following optimal allocation,

$$I_h^A = \frac{\delta\gamma y_h^1}{1+\delta\gamma}, \qquad (19)$$

$$\frac{\partial I_h^A}{\partial y_h^1} = \frac{\delta\gamma}{(1+\delta\gamma)}. \qquad (20)$$

Hence we see that the optimal investment level is a positive function of the household's endowment for all α. We also see that investment is less sensitive to contemporaneous changes in own endowment when $\alpha = 0$. This proves Claim 1 under our assumptions. Although in more general settings the algebra is more cumbersome, the same result can continue to hold.

Claim 2: Figure A1 shows the equilibrium allocations of consumption and investment for the two households for two different scenarios for transaction costs and specific values of the model parameters. The transfer declines with the cost until no transfer takes place and both households consume and invest in isolation. Aggregate investment is positively correlated with the degree of insurance (which is inversely proportional to α). Our baseline scenario assumes $y_j^1 = 1.5 = y_l^2$ and $y_l^1 = 1.8 = y_j^2$, further to an investment function $f(I_h) = \gamma_1 I_h - \gamma_2 (I_h)^2$. With perfect risk sharing ($\alpha = 0$) the optimal transfer is .15, each household has a post-transfer endowment of 1.65 and therefore makes the same optimal consumption investment decision. Consider the opposite extreme, autarky. Now household l, the household with the high endowment in the first period, invests more than the fully insured households for two reasons: (i) it has more available resources, which are not shared with household j; (ii) now investment has the additional purpose of transferring wealth over time, as well as providing positive returns. The opposite argument applies to household l. The average investment in autarky is less than with full insurance because of the decreasing (marginal) returns – a transfer from household 2 to household 1 would increase average investment. Scenario 2 shows the same type of features of the model when aggregate resources fall and shows how aggregate investment stays almost constant between different degrees of insurance.

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 1 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 the 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.

[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 A2 are the averages of P_i and E_i over all surnames i.

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]

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,

[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 $p_{iv} = \left(\frac{n_{iv}}{n_v}\right) \cdot \left(\frac{n_{iv}-1}{n_v-1}\right)$, 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 A2 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.

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

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 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 Not for Publication

C.1 Robustness Checks

Table C1 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.

Second, 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.

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[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.

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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]	[.043]
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 eligible (yes=1)	.552	.497	.547	.498	[.693]	[.087]	[.857]
School enrolment rate (11-16 yr olds)	.644	.410	.657	.410	[.287]	[.587]	[.000]
Temporary migrants	.191	.393	.160	.366	[.000]	[.429]	[.008]
Permanent migrants	.031	.174	.033	.178	[.848]	[.127]	[.666]
Land:							
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]
Livestock:							
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]	[.005]	[.039]
Goat	1.58	5.93	1.40	5.09	[.168]	[.158]	[.002]
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]
Other Household Assets:							
Fridge	.16	.36	.16	.37	[.591]	[.087]	[.904]
Stove	.32	.47	.29	.45	[.039]	[.263]	[.666]
Heater	.03	.17	.03	.18	[.433]	[.683]	[.815]
Radio	.65	.48	.64	.48	[.403]	[.033]	[.220]
Television	.49	.5	.46	.5	[.015]	[.006]	[.582]
Car	.02	.15	.03	.16	[.324]	[.398]	[.085]
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 19-21	.27	.54	.25	.52	[.062]	[.673]	[.393]
Aged 22 or more	2.38	.97	2.40	.93	[.333]	[.023]	[.487]
Child labor:							
Engaged in labor market, aged 8-14	.12	.32	.13	.34	[.279]	[.015]	[.437]
Engaged in labor market, aged 15-18	.48	.5	.48	.50	[.972]	[.662]	[.424]
Days of child labor, aged 8-14	.39	1.26	.40	1.25	[.784]	[.746]	[.863]
Days of child labor, aged 15-18	2.20	2.58	2.20	2.58	[.943]	[.662]	[.511]

Notes: The sample includes 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 expect 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 in the past 12 months and have returned.

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) Non-eligibles	(2) Non-eligibles With and Without Eligible Extended Family Members	(3) Non-eligibles With and Without At Least 30% of Extended Family Members Being Eligible
ITE^K [connected]	26.5**		
	(11.2)		
ITE^O [isolated]	-14.7		
	(20.14)		
ITE [connected, eligible family]		18.4*	
		(10.9)	
ITE [connected, no eligible family]		-6.43	-6.43
		(13.7)	(13.7)
ITE [connected, at least 30% of family is eligible]			32.0**
			(13.1)
ΔITE	41.3**	24.8*	38.4**
	(20.8)	(14.1)	(19.0)
Number of connected households	2888	2688	2519
Number of isolated households	656	856	856
Number of observations	11054	11054	9979

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 sample size falls in Column 3 because we do not include connected households in family networks in which at least one member is eligible but less than 30% of members are eligible for *PROGRESA* transfers. 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, 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 estimated indirect treatment effects and its corresponding standard error.

Table 3: 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) IV, At Least 30% of Extended Family Members Being Eligible	(4) OLS	(5) OLS, At Least 30% of Extended Family Members Being Eligible
Log actual transfer	.075***	.136***	.161*		
	(.024)	(.051)	(.095)		
Log potential transfer				-.032	-.225*
				(.052)	(.115)
First stage IV significance		132	91.0		
Number of observations	3353	3353	2409	1230	829

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 sample size falls in Column 3 because we do not include connected households in family networks in which at least one member is eligible but less than 30% of members are eligible for *PROGRESA* transfers. 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, 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 4: 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, village poverty index, region dummies. The adult equivalence scale used for consumption is one for members 18 or older, and 0.73 otherwise.

Table 5: 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: Total Income, All Households	(2) OLS: Total Income, Couple Headed Households	(3) IV: Total Income, All Households	(4) IV: Total Income, Couple Headed Households
Δ Log aggregate resources (Y)	.965***	.925***	.965***	.929***
	(.007)	(.012)	(.012)	(.016)
Δ Log household resources (y)	.019***	.018***	-.003	.000
	(.002)	(.002)	(.020)	(.021)
Number of observations	55965	51055	40502	35552

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The dependent variable is the growth in 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 September 1997, November 1998, May 1999, November 1999, November 2000, and November 2003. The adult equivalence scale used for consumption is one for members 18 or older, and 0.73 otherwise. The aggregate resources (Y) are consumption in the network. In Columns 3 and 4 the IV for household income is the second lag of household income. Extreme values are trimmed in the regressions.

Table 6: Investment and Resource Shocks

OLS regression estimates, standard errors are clustered by extended family network

A. Positive Resource Shock: Progresa Eligibility

	Human Capital Investments		Livestock				Agriculture	
	(1) School Enrolment Rate (aged 11-16)	(2) Child Labor (weekly workdays)	(3) Chickens	(4) Cows	(5) Oxes/horses/ donkeys	(6) Goats/pigs	(7) Land owned/cultivated	(8) Fertilizer/seeds
Connected Household	.077***	-.232***	.158***	.002	.021*	.026	.016	.050
	(.013)	(.068)	(.051)	(.010)	(.012)	(.031)	(.015)	(.475)
Isolated Household	-.022	.005	.269***	-.001	.030	-.023	.082***	-.005
	(.023)	(.106)	(.068)	(.030)	(.028)	(.043)	(.024)	(.753)
Difference	.100***	-.237*	-.112	.003	-.010	.049	-.067**	.055
	(.027)	(.126)	(.086)	(.032)	(.031)	(.051)	(.028)	(.809)
Number of observations	24122	24122	39897	39903	39893	39894	39839	18029

B. Negative Resource Shock: Illness of Household Head

	Human Capital Investments		Livestock				Agriculture	
	(1) School Enrolment Rate (aged 11-16)	(2) Child Labor (weekly workdays)	(3) Chickens	(4) Cows	(5) Oxes/horses/ donkeys	(6) Goats/pigs	(7) Land owned/cultivated	(8) Fertilizer/seeds
Connected Household	-.010	.077**	.092***	.029	.015	-.008	.043***	-.455
	(.008)	(.036)	(.034)	(.044)	(.018)	(.034)	(.014)	(.979)
Isolated Household	-.042***	.283***	.088	-.007	.002	-.021	.046*	-.739
	(.016)	(.084)	(.063)	(.033)	(.016)	(.037)	(.028)	(2.02)
Difference	.032*	-.206**	.004	.036	.013	.013	-.003	.283
	(.018)	(.091)	(.072)	(.055)	(.024)	(.050)	(.031)	(2.25)
Number of observations	30872	24026	45391	45408	45394	45392	31029	13285

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. These double difference estimates use the maximum number of available data waves: In Column 1 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 Column 2 we use November 1998 to November 2003 for illness of household head. Data on livestock is missing in November 2000; data on child labor is missing in 2003; data on size of land used is missing from November 2000 onwards; data on agricultural expenditures is missing from November 1999 onwards. The sample is only based on couple-headed households. Standard errors clustered at the network level.

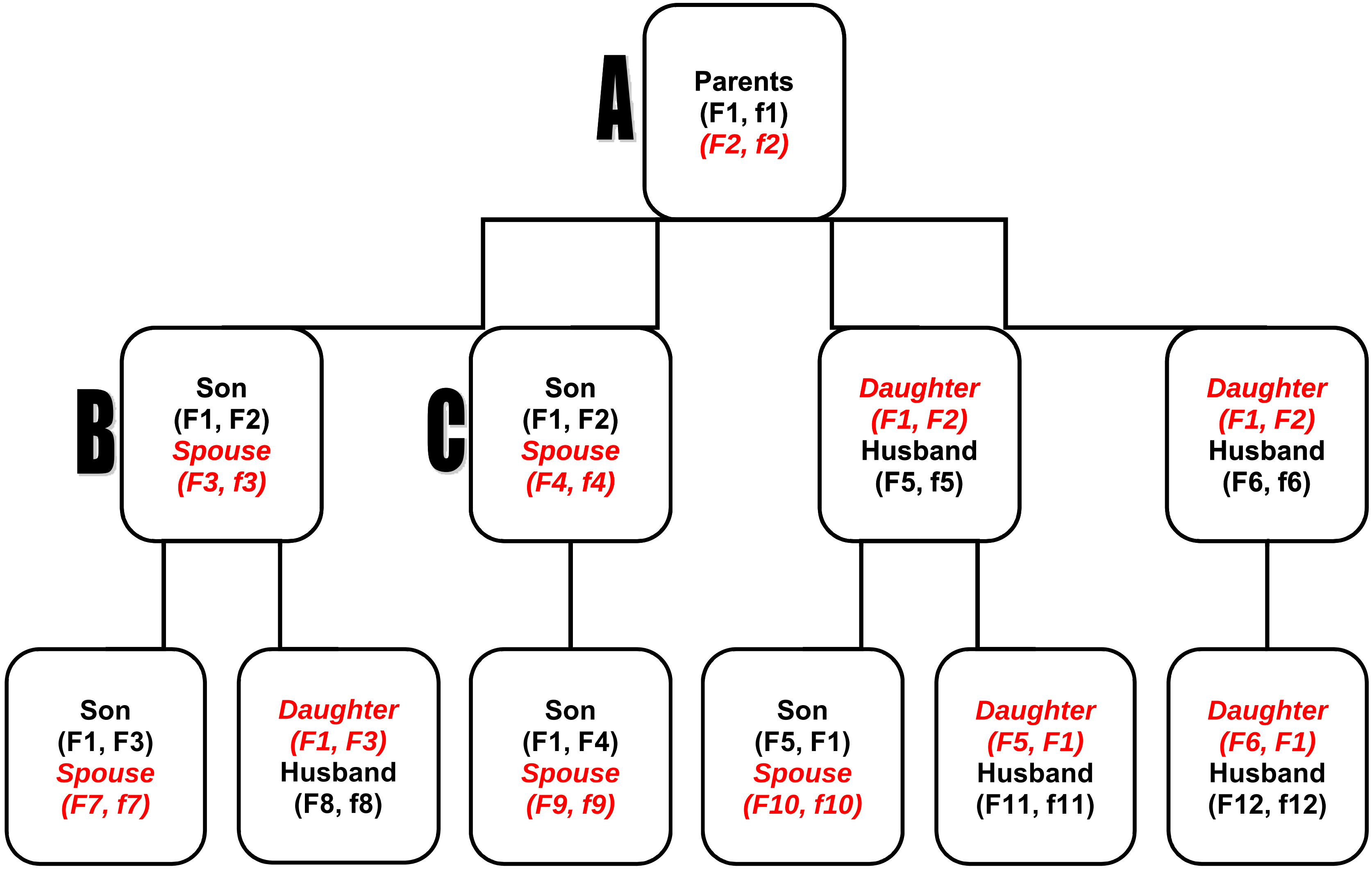
Table 7: Extended Family Networks and Long Run Changes in Investment, Income, and Consumption

OLS regression estimates, standard errors clustered by village level for isolated households and by network for connected households

	Human Capital	Livestock				Machinery		Income and Non-durable Consumption			Durable Consumption				
	(1) Share of Household Members With at Least Secondary Education	(2) Chickens	(3) Cows	(4) Oxes/horses/ donkeys	(5) Goats/pigs	(6) Car	(7) Tractor	(8) Labor Income	(9) Food Consumption	(10) Non Durable, Non-food Consumption	(11 Fridge	(12) Stove	(13) Heater	(14) Radio	(15) TV
Connected Household	.044***	-.685***	-.132***	-.007	-.273***	.130***	.004***	30.2***	-11.6***	57.2***	.175***	.081***	.014***	-.050***	.156***
	(.003)	(.031)	(.010)	(.010)	(.018)	(.007)	(.001)	(4.79)	(2.12)	(1.85)	(.007)	(.006)	(.004)	(.009)	(.007)
Isolated Household	.034***	-.805***	-.113***	-.017	-.247***	.105***	.003**	13.8**	-17.0***	52.4***	.146***	.058***	-.004	-.057***	.136***
	(.003)	(.045)	(.018)	(.013)	(.028)	(.006)	(.002)	(6.29)	(2.68)	(2.26)	(.008)	(.008)	(.004)	(.011)	(.010)
Difference	.010***	.120**	-.019	.010	-.026	.025***	.001	16.4**	5.43	4.85*	.028***	.023**	.018***	.007	.020*
	(.004)	(.055)	(.021)	(.009)	(.033)	(.009)	(.002)	(7.97)	(3.42)	(2.91)	(.011)	(.010)	(.005)	(.015)	(.012)
Number of observations	35661	35424	35421	35407	35416	35656	35652	35093	35433	35416	35642	35645	35598	35632	35641

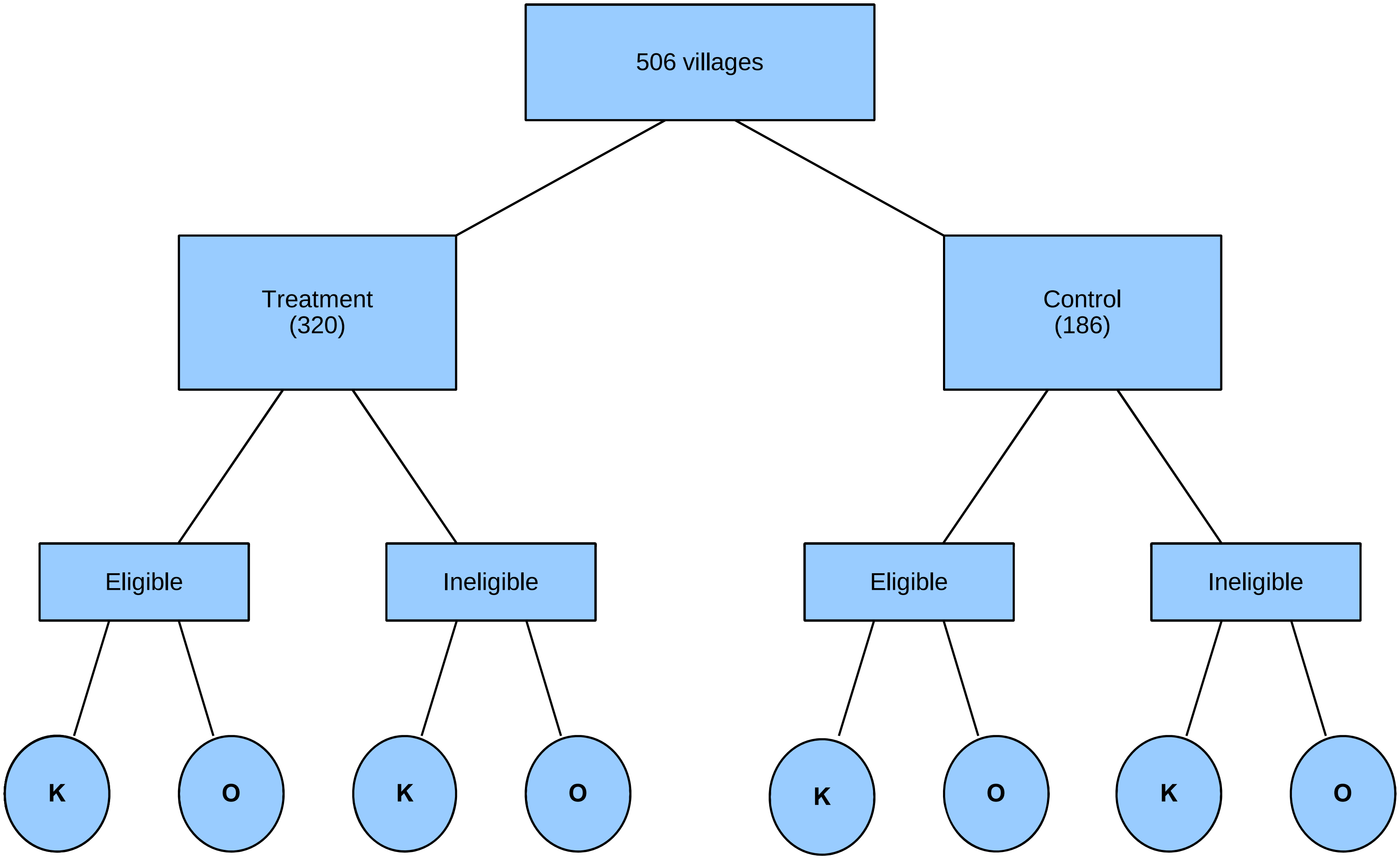
Notes: : *** denotes significance at 1%, ** at 5%, and * at 10%. Estimates are double differences in consumption (durable and non-durable), income, human and physical capital between November 1998 and September 1997 (March 1998 for consumption, as data is not available in 1997). Standard errors clustered by village level for isolated households and by network for connected households. The sample is based on couple-headed households only. Including also single-headed households does not change the results. 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, 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: 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 2: Data Structure



Notes: K and O stand for connected and isolated households respectively, K hoseholds are those with at least 1 relative household present in the village, as defined throughout the paper.

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

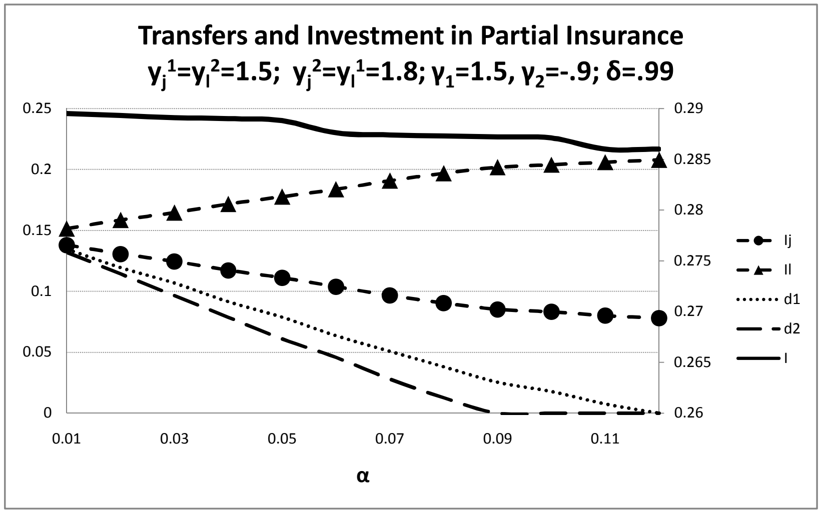
Table	Outcome	Households in Sample	Data Waves	Estimator	CS or DD
1	Various	CH, K and I	1	OLS	CS
2	Food consumption	CH, N, K and I	2, 4, 5	OLS	DD
3	Food consumption	All N K	2, 4, 5	OLS, IV	DD
4	Food consumption, all non-durable consumption	All N K	4, 5	OLS	DD
5	Food consumption	All N K	1,3,4,5,6,8	OLS, IV	CS
6	Investment	CH, K and O	1,2,3,4,5,6,8*	OLS	DD
7	Investment	CH, K and O	1,8	OLS	DD
A3	Extended family links	All K and I	3	OLS	CS
A4	Surnames	All K and I	3	-	-
A5	Extended family links	MxFLS data			
A6	Food consumption	CH, N, K and I	2, 4, 5	OLS	DD

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

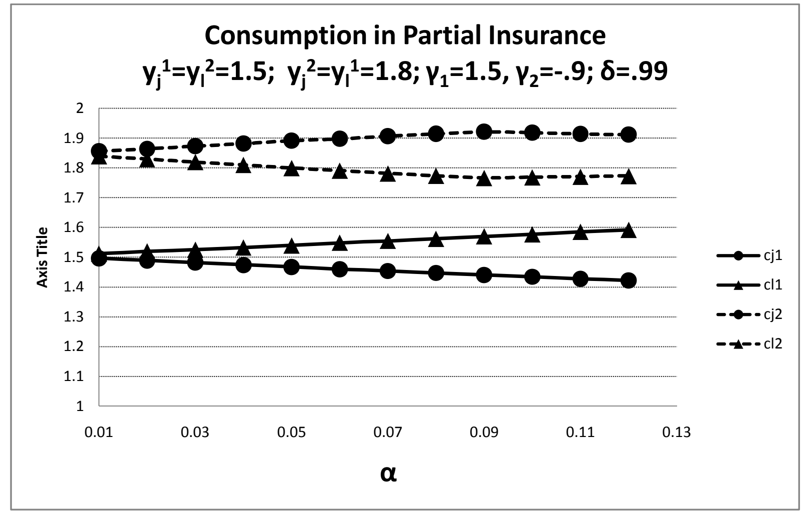
Figure A1: Claim 1 and 2 - Simulated Investment and Consumption

Scenario 1:

Investment

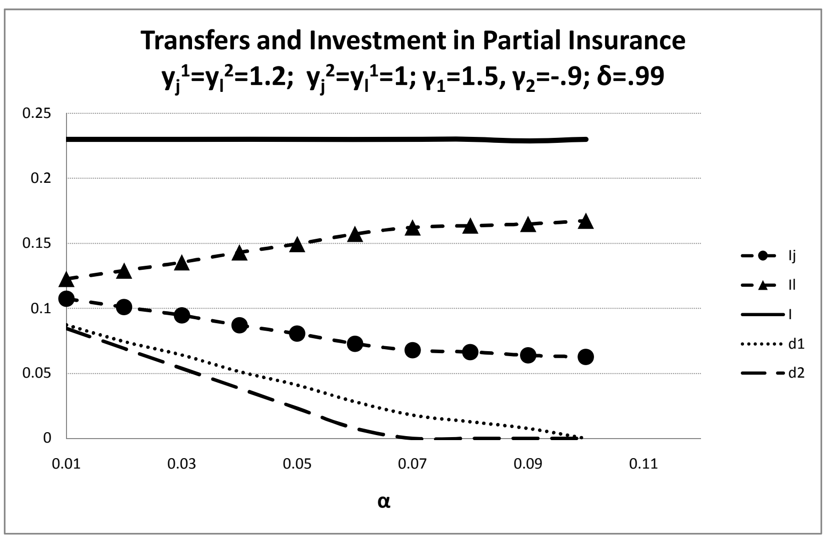


Consumption



Scenario 2:

Investment



Consumption

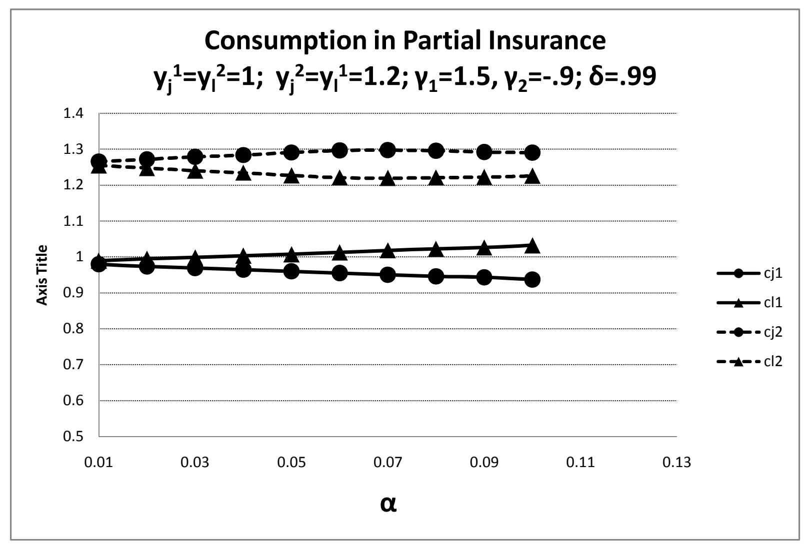


Table A3: Probability of an Extended Family Link

Couple Headed Households

Mean, standard errors in parentheses clustered by village

				Intra-generational Family Links				Inter-generational Family Links			
	Any Family Link (Connected)	Any Family Link of the Head	Any Family Link of the Spouse	Head to Head (Brothers)	Head to Spouse	Spouse to Head	Spouse to Spouse (Sisters)	Parents to Son	Parents to Daughter	Son to Parent	Daughter to Parent
Eligible Households											
Treatment	.817	.693	.550	.506	.351	.338	.306	.150	.077	.169	.108
	(.011)	(.012)	(.014)	(.013)	(.015)	(.014)	(.013)	(.009)	(.006)	(.008)	(.007)
Control	.800	.682	.541	.503	.364	.348	.314	.149	.079	.163	.097
	(.017)	(.020)	(.023)	(.023)	(.026)	(.026)	(.026)	(.014)	(.009)	(.011)	(.008)
Difference	.017	.012	.009	.003	-.013	-.010	-.008	.002	-.002	.006	.011
	(.020)	(.023)	(.028)	(.026)	(.030)	(.030)	(.030)	(.016)	(.010)	(.013)	(.011)
Non-eligible Households											
Treatment	.808	.712	.523	.532	.332	.258	.248	.198	.112	.142	.074
	(.016)	(.018)	(.021)	(.021)	(.018)	(.016)	(.017)	(.015)	(.010)	(.014)	(.008)
Control	.802	.694	.562	.481	.353	.313	.272	.226	.112	.126	.089
	(.019)	(.022)	(.024)	(.026)	(.028)	(.022)	(.022)	(.021)	(.016)	(.014)	(.010)
Difference	.006	.019	-.039	.051	-.021	-.055**	-.024	-.028	-.000	.016	-.015
	(.025)	(.028)	(.032)	(.034)	(.034)	(.027)	(.028)	(.026)	(.019)	(.020)	(.013)

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by village. The sample is restricted to couple headed households that can be tracked over the first and third *Progresa* waves. Means and differences are reported for those households that have secondary school age children (aged 11 to 16) in the baseline survey of October 1997. The standard errors on the differences are calculated from running a corresponding OLS regression, which allows for the error terms to be clustered by village.

Table B1: Descriptive Statistics on Surnames, by Surname Type

Mean, standard errors in parentheses, percentages in brackets

	Head's Paternal Surname	Head's Maternal Surname	Spouse's Paternal Surname	Spouse's Maternal Surname
	(F1)	**(f1)**	**(F2)**	**(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: 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.