

CESifo Area Conference

on

Employment and Social Protection



23-24 May 2008 CESifo Conference Centre, Munich

Family Networks and School Enrolment: Evidence from a Randomized Social Experiment

*Manuela Angelucci, Giancomo de Giorgi,
Marcos A.Rangel and Imran Rasul*

CESifo GmbH
Poschingerstr. 5
81679 Munich
Germany

Phone: +49 (0) 89 9224-1410
Fax: +49 (0) 89 9224-1409
E-mail: office@cesifo.de
Web: www.cesifo.de

FAMILY NETWORKS AND SCHOOL ENROLMENT: EVIDENCE FROM A RANDOMIZED SOCIAL EXPERIMENT*

MANUELA ANGELUCCI[†] GIACOMO DE GIORGI[‡]
MARCOS A.RANGEL[§] IMRAN RASUL[¶]

MAY 2008

Abstract

We present evidence on whether and how a household's behavior is influenced by the presence and characteristics of its extended family. Using household panel data from the *Progresa* program in rural Mexico, we use information on the paternal and maternal surnames of heads and spouses in conjunction with the patronymic naming convention to identify the inter and intra generational family links of each household to others in the same village. We then exploit the randomized research design of the *Progresa* evaluation data to identify whether the treatment effects of *Progresa* transfers on secondary school enrolment vary according to the presence and characteristics of extended family. We find that *Progresa* only raises secondary enrolment among households that are embedded in a family network. Eligible but isolated households do not respond. The mechanism through which the extended family influences household schooling choices is the redistribution of resources within the family network from eligibles that receive *de facto* pure cash transfers from *Progresa*, towards eligibles on the margin of enrolling their children into secondary school.

Keywords: extended family network, *Progresa*, resource sharing, schooling.

JEL Classification: I21, J12, O12.

*This research was supported by an IRB from the University of Chicago. The paper has been screened to ensure no confidential information is revealed. We thank Orazio Attanasio, Oriana Bandiera, Jere Behrman, Timothy Besley, Martina Bjorkman, Pedro Carneiro, Pierre-Andre Chiappori, Hilary Hoynes, Sylvie Lambert, Steve Machin, Ofer Malamud, Alan Manning, Guy Michaels, Debraj Ray, Jean-Marc Robin, Yoram Weiss, and seminar participants at Chicago, Columbia, IFS, INRA, LSE, Maryland, Northwestern, Stanford, Toulouse, UCL, and the NBER Education Program Meeting (Boston 2008) for comments. All errors remain our own.

[†]Department of Economics, University of Arizona [angelucm@eller.arizona.edu].

[‡]Department of Economics, Stanford University [degiorgi@stanford.edu].

[§]Harris School of Public Policy, University of Chicago [rangelm@uchicago.edu].

[¶]Department of Economics, University College London [i.rasul@ucl.ac.uk].

1 Introduction

Economists usually focus on the nuclear family as the unit of analysis from which to study household behavior. Indeed, standard models of household decision making, such as the unitary model [Becker 1981], bargaining models [Manser and Brown 1980, McElroy and Horney 1981], and collective choice models [Chiappori 1988] all emphasize the characteristics of household members in shaping outcomes. Moreover, it is typically not possible to empirically identify the precise familial ties between surveyed households in most data sets.

However, every household is actually embedded within an extended family network. If extended families shape the objectives and constraints relevant for households within them, then neglecting the role of this network leads to an incomplete understanding of household behaviors. The extended family may be especially relevant for household behavior in economic environments characterized by missing markets, correlated shocks, informal institutions of contract enforcement, and large scale policy interventions that affect many households in the local economy.

All these features apply to the empirical context of this paper, which uses household panel data from the *Progresa* social assistance program in rural Mexico. *Progresa* provides cash transfers to households conditional on their childrens' school attendance. We first exploit information on the paternal and maternal surnames of heads and spouses in conjunction with the patronymic naming convention in Mexico to identify the inter and intra generational family links of each household to others in the village. We then exploit the randomized design of the *Progresa* evaluation data to identify whether the treatment effects of *Progresa* transfers on school enrolment vary according to the presence and characteristics of extended family members.

Two key intuitions underlie our analysis. First, *Progresa* exogenously shifts the resources available to individual households *and* to their family network as a whole. Second, families are able to enforce implicit contracts of resource sharing. Hence *Progresa* can induce differential responses between connected households – namely those embedded within family networks, and those that are socially isolated in the sense that none of their extended family live in close proximity and are not therefore subject to the change in resources that *Progresa* provides.¹

While undoubtedly other households outside the extended family network also influence behavior, there are good reasons to focus on family networks, rather than friendship networks say, as being the core group within which resources are shared. First, evolutionary biology suggests

¹Our analysis relates to the literature on risk pooling arrangements across households. While such *ex post* risk pooling mechanisms have been documented in developing country settings, formal tests of the Pareto efficient allocation of risk being achieved at the village level are typically rejected [Townsend 1994, Ligon 1998, Dercon and Krishnan 2000]. There is stronger evidence of risk pooling within ethnic groups [Deaton 1992, Udry 1994], sub-castes [Munshi and Rosenzweig 2005], and family and friends [Rosenzweig 1988, Fafchamps and Lund 2003, La Ferrara 2003, Cox and Fafchamps 2007].

preferences are defined over the family dynasty, as is commonly modelled within an overlapping generations framework. Moreover there are specific inter-generational investments – such as those into children’s education – that have no counterpart in friendship ties, and provide long run incentives for family members to reciprocate in resource sharing arrangements. Finally, the transactions costs of sharing resources with non-family members may be higher because it is both more costly to observe outcomes outside the family network, and fewer mechanisms exist by which to punish non-family members that renege on such arrangements [La Ferrara 2003].²

We exploit three key data features. First, we combine information on the paternal and maternal surnames of household heads and their spouses, with the Mexican patronymic naming convention to build two types of extended family link within the same village – (i) inter-generational links, such as those from the head and spouse to their parents, and to their adult sons and daughters; (ii) intra-generational links, such as those from the head and spouse to their brothers and sisters. Combined with information from household rosters that identify extended family members that co-reside in the household, this provides an almost complete mapping of extended family structures across 506 villages, covering around 22,000 households and over 130,000 individuals.

Second, we exploit the multiple components of *Progresa*, each of which provides cash transfers conditional on a different household behavior. One component provides cash transfers conditional on the school attendance of children in primary and secondary school. However, as pre-program primary school enrolment rates are above 90%, transfers provided for this purpose represent a *de facto* unconditional cash transfer for households with primary school aged children. In contrast pre-program secondary enrolment rates are 65% so that for many households cash transfers will be obtained only if a change in behavior is induced. This is important because as the value of transfers only corresponds to between one half to two thirds of the full time child wage in the survey villages [Schultz 2004], and so do not fully compensate for foregone earnings of secondary school aged children employed full time in the labor market.

Hence *Progresa*’s effect on secondary enrolment may be a function of the presence of primary school aged children, who receive *de facto* unconditional transfers. In particular, households can use these transfers to supplement those specifically conditioned on secondary school enrolment, thus fully offsetting the opportunity costs of enrolling children into full time secondary school. This channel affects both connected and isolated households. In addition, if families share resources, and in particular they share the unconditional transfers obtained from the primary school component of *Progresa*, the response of connected households will also depend on the demographic composition

²We therefore also contribute to the literature on the effects of extended family on household behavior, such as for consumption [Altonji *et al* 1992]; inter-generational transfers [Cox and Jakubson 1995, Altonji *et al* 1997, La Ferrara 2003, Behrman and Rosenzweig 2006]; childrens’ education choices [Loury 2006]; and non-resident parental investments into children [Weiss and Willis 1985].

of eligible households within their family network. This drives a wedge between the program responses of connected and isolated households in terms of secondary enrolment.

Third, we exploit the randomized research design used to evaluate *Progresa*. Of the 506 sampled villages, 320 were randomly assigned to be a treatment group, namely villages where *Progresa* would be implemented, and 186 villages were controls. In each village, household data was collected on a panel of around 22,000 households every six months over the pre and post-program periods. Within each village the baseline survey provides a complete census of all eligible and all non-eligible households.

Under standard assumptions this design identifies the average treatment effect of *Progresa* from a comparison of eligibles in treatment and control villages. Our analysis then identifies whether this treatment effect varies between connected and isolated households by comparing eligibles with identical extended family characteristics across treatment and control villages.

Our main descriptive findings are as follows. First, 20% of couple headed households are isolated in the sense that none of their extended family reside within the village, while 80% are connected and so are embedded within an extended family network. As a point of comparison, we note that 15% of households are single headed. Second, there are no significant pre-program differences between isolated and connected households in terms of many of their observables including enrolment rates and overall poverty levels. Third, among connected households, the extended family of the head is more likely to reside in the same village than the extended family of his spouse, and this relates to higher levels of female migration at the time of marriage.³

Our main empirical findings are as follows. First, despite baseline enrolment rates being similar in connected and isolated households, only connected households respond to the program in terms of secondary school enrolment. The average treatment effect of *Progresa* on them is significantly different from zero at around 9%. In contrast, eligible but isolated households do not respond – their treatment effect varies between -.2% and .9% and is never significantly different from zero.⁴

Our second set of results provide direct evidence on the interplay between the design features of *Progresa*, whether a household is connected or isolated, and its response to the program. In particular, we find that the marginal response of connected households in terms of secondary enrolment, depends on the demographic composition of their own household, and the demographic composition of eligible households within their extended family. The particular pattern of responses we document, that are a function of primary school aged children both in the household and the eligible portion of the extended family network as a whole, point to resources being redistributed within family networks towards households that are on the margin of enrolling children

³This is similar to the findings of Rosenzweig and Stark [1989] who examine marital arrangements in rural India.

⁴We build on earlier findings on the effects of *Progresa* on enrolment [Schultz 2004, Attanasio *et al* 2005, Todd and Wolpin 2006]. This literature has not emphasized the influence of extended families on schooling choices.

into secondary school. These are those households that themselves receive effectively unconditional *Progresa* transfers for the primary school enrolment of their children.

Finally, we complement our main results by considering which specific family members share resources and why. Among sibling links, household responses are sensitive to the eligibility status of their sibling, and in particular, whether eligible siblings obtain unconditional transfers from having primary school aged children. Households are less sensitive to their sibling having secondary school aged children *per se*, suggesting there are not strong within sibling peer effects specifically related to secondary schooling. Among parent-adult child links, parental households are again sensitive to the eligibility status of their adult children and the presence of younger children in their households.

Taken together, the results on the influence of specific family links consistently point to resources being redistributed within the network from eligible households that receive largely unconditional transfers for the primary school enrolment of their children, towards eligible households that are on the margin of enrolling children into secondary school. The pattern of results also help rule out other mechanisms behind why families matter, such as information sharing or providing access to communal land. This is because if families mattered through these other channels, we would not expect household responses to *Progresa* in terms of secondary school enrolment to depend on the extent to which other households in the family network were eligible, or the extent to which these eligible households received transfers specific to their primary school aged children.

The paper is organized as follows. Section 2 describes the *Progresa* program and data. Section 3 discusses how we construct family links and provides descriptive evidence on them. Section 4 presents the empirical method and supportive evidence on the underlying identifying assumptions. Section 5 presents our baseline results on whether and why being connected influences program responses. Section 6 presents evidence on how specific family links shape schooling decisions. Section 7 concludes. Additional results and robustness checks are in the Appendix.

2 The Progresa Program and Evaluation Data

2.1 Transfers

Progresa is an ongoing publicly funded social assistance program in Mexico. It is designed to alleviate poverty by fostering human capital accumulation through two channels – (i) the provision of cash transfers to households conditional on children’s attendance in primary and secondary school grades; (ii) the provision of cash transfers and nutritional supplements conditional on attendance at local health facilities. This component of *Progresa* targets households with pregnant

or lactating women, and with children aged less than five.⁵

Central to the analysis in this paper is that the extended family members of any given household can be eligible for transfers even if they have no secondary school age children. For example, households with primary aged children, very young children and those with no children but a pregnant women in them, can be eligible for the primary school, health, and nutritional components respectively. Hence if families share resources, the response to *Progresa* of households with secondary school age children will be partly determined by the demographic composition of *all* eligible family members, not just those that also have secondary school aged children.

Transfers for school enrolment are paid bimonthly and conditional on the child attending classes at least 85% of the previous 60 days. Two points are of note. First, transfers are larger for higher school grades, and higher for girls within any given grade. The average monthly transfer to beneficiary households is non-negligible, corresponding to 20% of the value of monthly consumption expenditures pre-program [Skoufias 2005].⁶ However, the value of transfers only corresponds to between one half to two thirds of the full time child wage in the survey villages [Schultz 2004]. Hence the most credit constrained households should not be induced to change their schooling choices by such transfers alone.

Second, although both the education and health components are conditioned on household behavior, the extent to which households need to change behavior to obtain the transfers depends on their pre-program choices. As documented in Section 4, pre-program primary enrolment rates are above 90%. Hence transfers provided to enrol children into primary school effectively represent a pure income effect on households. In contrast pre-program secondary enrolment rates are closer to 65%. Hence given the value of transfers conditional on secondary enrolment do not compensate for the losses of income earned by children in the labor market, the likelihood the program increases secondary enrolment rates will depend partly on the demographic composition of children in the household. In addition, if families share resources, the response of connected households will in general depend on the demographic composition of eligible households within their entire family network. This channel drives a wedge between the behavioral responses of connected and isolated households to the program in terms of the secondary school enrolment.

Finally, the health and nutrition components of the program require the periodic attendance of mothers at local health clinics. Households are likely to view this component as somewhere

⁵The program was initially offered to 140,544 households in 1997, expanding to more than 2.6 million recipient households throughout rural Mexico by the end of 1999. This constitutes around 40% of all rural families and one ninth of all families in Mexico. The total annual budget of the program in 1999 was equivalent to just under 20% of the federal poverty alleviation budget or .2% of GDP [Skoufias 2005]. The program was expanded to urban areas in 2003 under the name of *Oportunidades*. We do not exploit this expansion into urban areas for our analysis.

⁶By November 1999 the bimonthly transfer ranged from 160 *pesos* for third grade, to 530 (610) *pesos* for boys (girls) in ninth grade. The total amount received bimonthly by a household cannot however exceed 1500 *pesos*.

between the purely unconditional transfers obtained for primary schooling, and the conditional transfers obtained for secondary schooling. Again it will be the case that the response in terms of secondary school enrolment will in general be influenced by the presence of young children in the household to whom this component is targeted, and for connected households, the presence of young children in eligible households within the family network as a whole.⁷

2.2 Eligibility and The Evaluation Data

In 1997 households were classified as either being eligible (poor) or non-eligible (not poor) for *Progresa* transfers according to a household poverty 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.

Around half the households were classified as poor and therefore eligible for *Progresa*. Households were informed that their eligibility status would not change at least until November 1999, irrespective of any variation in household income. Moreover, 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 non-eligibles in the hope of obtaining greater transfers. Finally, a distinguishing feature of *Progresa* is that households were clearly informed of their eligibility status, and so take-up rates are over 90%. We do not therefore distinguish between intent-to-treat and treatment effects.⁸

To evaluate *Progresa*, an experimental research design was implemented and household data collected on a panel of around 22,000 households every six months in 506 villages between March 1998 and November 1999.⁹ Of the 506 villages, 320 were randomly assigned to the treatment group, namely locations where *Progresa* would be later implemented in May 1998, and 186 villages were

⁷The required frequency of attendance varies depending on the age of the individual. For example, children younger than four months are required to attend three check ups, those aged between 4 and 24 months attend eight check ups, children aged between 2 and 4 attend every four months, children aged between 5 and 16 attend every six months, pregnant women attend five check ups during their prenatal period, lactating women attend two check ups, and adults aged 17 and above are required to attend an annual check up [Skoufias 2005].

⁸A group of households – referred to as *densificados* – had their eligibility status reclassified from non-eligible to eligible in October 1998. A non-random subset of them began receiving *Progresa* transfers in treatment villages prior to November 1999. As no precise algorithm exists to determine which *densificados* received transfers in treatment villages, no counterfactual set of households exists for them in control villages. As we can define extended family links to and from these households, all the reported descriptive statistics on extended families include links to and from *densificados*. We do not consider changes in enrolment among *densificados* in our analysis.

⁹Villages were selected on the basis of a marginality index constructed from information on the share of illiterate adults in the village; share of dwellings without water, drainage systems, electricity, and with floors of dirt; average number of occupants per room in village households; share of population working in the primary sector; distance from other villages, and health and school infrastructures in the village.

assigned to be control villages. The control group therefore contains 36.8% of all villages in the evaluation data, corresponding to 38.2% of all households.

The first two waves of data were collected pre-program (October 1997, March 1998). Transfers were first distributed in May 1998, hence the remaining waves (October 1998, May 1999, November 1999) correspond to the post-program period. To understand whether household behavior is influenced by the presence and characteristics of extended family members of both the head and spouse, we focus attention on the 85% of households that are couple headed throughout.¹⁰

3 Constructing Extended Family Links

3.1 Surnames and the Matching Algorithm

To identify both the intra-generational and the inter-generational family links between any two households in the same village we exploit information on surnames provided in the third wave of data, and in conjunction with the patronymic naming convention in Mexico.¹¹

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; and, (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.¹²

¹⁰Control villages began receiving *Progresá* transfers in December 1999. In 1997, eligible households in control villages were informed they would become part of the program at the end of 1999 conditional on them still being eligible and the program continuing.

¹¹Two concerns arise from the surnames data being measured in the first wave of post-program data. First, households may endogenously respond to the program by changing household structures, in particular, by artificially forming new households in order to increase the number of eligibles in the family. This concern is ameliorated by the fact that the register of eligible households was drawn up at baseline, and only households recorded to be eligible at that point were later entitled to receive transfers. Moreover, although there is an increase in the number of households from the baseline to October 1998, this increase is proportionately the same in both treatment and control villages. A second 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.

¹²The 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

Figure 1 illustrates the matching algorithm. To define each family link, we use 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.¹³

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** ($F1$); and, (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 relationships can be similarly defined. Moreover, intra-generational family ties between siblings can also be identified. For example, the heads of households **B** and **C** are identified to be brothers if they share the same paternal and maternal surnames.

Figure 1 shows all households to be couple headed solely to ease the exposition. To deal with the 15% of households that are single headed we use information on the gender of the head to accurately define family links. Finally, we impose the following restrictions when defining family links – (i) inter-generational links exist when the relevant individuals have at least 15 years age difference, and no more than 60 years age difference between mother and child; (ii) intra-generational links exist when the individuals have at most 30 years age difference.

However, there are limits to which information on surnames can be used to construct family ties. Consider links from household i to a single headed household j . As Figure 1 shows, the fact that household j is single headed does not affect the construction of links from the head and spouse of household i either to their children or to their siblings. However, links from the head (spouse) of household i to the household of his (her) parents can only be identified if *both* his (her) parents are alive and resident together. This is because this particular family link is identified using information from household j on the paternal surnames of both the head and spouse.¹⁴

We only define family links within villages because *Progresa* is implemented at the village level. Hence geographically proximate family members are those most relevant to understand household behavior if families share resources. Isolated households may well have extended family elsewhere and share resources with them, but their family is unlikely to experience any resource changes due to *Progresa* because the data covers a period when the program was still being rolled out.

household is reported to be a women. To keep clear the exposition, we redefine the head to be male in such cases.

¹³Paternal (maternal) surnames are indicated in upper (lower) case. First names are not shown as they are irrelevant for the matching algorithm. In Anglo Saxon countries, $F1$ corresponds to the family name and $F2$ corresponds to the spouse's maiden name.

¹⁴However this is unlikely to be a major issue. For example we note that female widows aged above 40 are 37% more likely to live as a dependent within a household, rather than head their own household, relative to a similar married woman. These single parents are then recorded in the household roster.

The Appendix provides more descriptive evidence on surnames in this setting, and discusses various forms of measurement error in the family links, some of which can be quantified. However, it is important to note that sources of measurement error that imply households are incorrectly labelled as connected when in fact they are isolated makes it less likely that any behavioral differences are found between households we label as connected and isolated. In addition, the econometric analysis only exploits information on whether such family links exist for a household, not the number of such links. We also show the robustness of our main results to dropping households with potential measurement error in their surnames, and to limiting the sample to smaller villages where spurious links between households are less likely to be defined.

3.2 The Number of Extended Family Links

Table 1 shows the number of family links each household has to others in the village. The columns of the upper panel split family links into – (i) inter-generational links to parents and adult children who head their own households; (ii) intra-generational links to siblings who head their own households. We report each type of link from the head and spouse separately. The lower panel reports the number of corresponding family links that co-reside inside the household as measured from the household roster in wave 3. Using information on the relationship to the household head, we decompose these links into those from the head and spouse.

Each type of link is reported separately for connected and isolated households. By definition, the upper panel reports that isolated households have no links to family members within the same village. We first note that 20% of couple headed households are isolated. The incidence of not having extended family members geographically proximate in the same village is therefore at least as high as the incidence of single headedness, which affects 15% of households.

On links to extended family, Table 1 shows parents are many times more likely to reside outside rather than inside the household of their adult children. The number of parents present is higher for the head than for his spouse, and this is true for parents inside and outside the household. This is consistent with either the spouse migrating to the village, or women moving in with their husband’s family within the same village. To shed more light on this we exploit data on spouse’s marital history. Wives were asked about where they went to live after marriage – 49.3% stated that they went to live with their in-laws after marriage, and only 6.5% report living with their own parents. The key difference between spouses with and without parents resident in the village is that 85% of spouses that have their parents present in the village report remaining in the same village at the time of marriage. The figure for spouses that have no parental links in the village is only 61%. Along other margins, women in connected households with and without parents in the village are similar. For example, they do not differ significantly in their ages at marriage, nor in

the proportions that report their in-laws originally proposing the marriage (56%).

The number of links to adult children are, by construction, identical for head and spouse. Given respondents' ages, there are many more young children inside than adult children outside the household. Sibling links are more likely to be outside the household, which is as expected given respondents' ages. Heads have more siblings links than their spouses, and this is again true for links both inside and outside the household. Overall, the upper panel shows that the average household has family ties to just over five other households in the same village, and that the majority of these family links are those of male heads of household.¹⁵

A concern is that isolated households may simply be those in which all family members reside under the same roof. The lower panel of Table 1 shows this is not true – the average household has around seven members and this is not different between connected and isolated households. Another explanation for why isolated households exist relates to geographic mobility. On this issue we note that while isolated and connected households are equally likely to report being resident in the same state of birth, unfortunately, the data does not contain information on how long individuals have been resident in the village. However, we note that among isolated households, only 52% of spouses report living in same village as at the time of marriage, which is lower than that for spouses in connected households as reported above.

Finally, we note that connected and isolated households are as likely to report receiving remittances from family members that have permanently migrated away in the five years prior to baseline. Hence isolated households *do* have family located somewhere, and may share resources with them.¹⁶ The key difference for our analysis is that their families are far less likely to receive *Progres*a resources since they live in different villages, and the program is being scaled up during the period of study. This drives a wedge between the program response of isolated and connected households, even if all families share resources.¹⁷

To provide external validity to the links, the Appendix presents similar information from the *Mexican Family Life Survey*, that was collected in rural areas over a comparable time period.

¹⁵In October 1998 the average age of heads (spouses) among couple headed households is 45.0 (40.5). The total number of siblings of the head is on average 2.23, implying his parents have 3.23 children that reside within the same village. In contrast, heads have on average 4.70 of their own children somewhere in the village. Again this is as expected given that siblings are older than adult children and so will be more likely to have migrated.

¹⁶There is however no information on pre-program transfers from other family members outside the village, including temporary migrants or family members in the place of birth if the household has itself migrated. Hence it is not possible to make inferences about whether the transactions costs of across village transfers are higher than within village transfers, due to asymmetric information say. It is the case that the transactions cost of other forms of exchange – such as labor transfers – are weakly higher across villages than within them, all else equal.

¹⁷Munshi and Rosenzweig [2005] provide evidence from India that those who migrate away from their sub-caste lose the services of that network, including mutual insurance arrangements. The model they develop and test implies the wealthiest households are those with incentives to withdraw from such arrangements. However, in general, the relationship between wealth and exit is theoretically ambiguous [Banerjee and Newman 1998].

4 Empirical Method

4.1 Preliminaries

The empirical analysis estimates the response of eligible to *Progresa* in terms of secondary enrolment rates, and explores whether these responses vary depending on the presence and characteristics of extended family members. To begin with, we denote the secondary enrolment rate of household h in village v in survey wave t as Y_{hvt} . This is defined as the fraction of children aged between 11 and 16 resident in the household that are enrolled in school on survey day in wave t . The behavioral response we focus on is the change in secondary enrolment within the same household over time, between November 1999 (wave 5) and October 1997 (wave 1), denoted ΔY_{hvt} . This corresponds to more than one academic year after *Progresa* is implemented.¹⁸

We define three dummy variables – (i) $D_h = 1$ if household h is eligible, and zero otherwise; (ii) $P_{vt} = 1$ if *Progresa* is in place in village v in wave t , and zero otherwise, so $P_{vt} = 0$ ($P_{vt} = 1$) in the pre (post) program waves in treatment villages, and $P_{vt} = 0$ in control villages for all t ; (iii) $L_{jh} = 1$ if household h has family link- j in the village, and zero otherwise. Although in the long run we expect family networks to endogenously adjust to the permanent presence of *Progresa*, our analysis treats networks as fixed over the time period considered.¹⁹

We then run the following first differenced OLS regression for eligibles ($D_h = 1$) where differences correspond to those between November 1999 and October 1997,

$$\Delta Y_{hvt} = \alpha + \beta_1 \Delta P_{vt} + \beta_2 (\Delta P_{vt} \times L_{jh}) + \Delta u_{hvt}, \quad (4.1)$$

and Δu_{hvt} captures unobserved time varying household characteristics, and standard errors are clustered by village. Time invariant household determinants of enrolment – such as household preferences or ability – are differenced out in this specification.

Consider the simplest case in which $L_{jh} = 1$ if the household has *any* extended family links present and $L_{jh} = 0$ if the household is isolated in that no members of its extended family live in the same village. When (4.1) is estimated, $\widehat{\beta}_1 + \widehat{\beta}_2$ identifies the average treatment effect of *Progresa* on eligibles in treatment villages that are embedded within extended family networks

¹⁸The enrolment rate is constructed from individual child observations that have complete information on the child’s age, gender, and residence. Waves 1 and 5 are collected during the school year which runs from September to July. This eases concerns that households mis-report enrolment status either because children are on school vacation or have dropped out part way through the academic year. We later also use primary enrolment rates for some of the analysis – these are analogously defined for 6 to 10 year olds.

¹⁹In support of this, we reiterate the share of households reporting a migrant head or spouse by is only .4% (.5%) in wave 3 (5), and that the share of households with such migrants does not differ across treatment and controls. In the Appendix we show our main results to be robust to redefining family networks on the basis of additional information collected in May 1999.

($L_{jh} = 1$). We denote this parameter as TTE^1 . This difference-in-difference (DD) in secondary enrolment is identified from a comparison of the average changes in enrolment among eligibles in treatment villages with extended family present, relative to eligibles in control villages who also have extended family present. Similarly, $\widehat{\beta}_1$ identifies the average response to the program among eligible but isolated households ($L_{jh} = 0$), denoted TTE^0 , from the DD in enrolment of isolated households in treatment and control villages. If families share resources, there exists a wedge between the behavioral responses of connected and isolated households to the program in terms of their secondary school enrolment, so that $TTE^1 > TTE^0$.

If (4.1) is estimated only for non-eligibles, $\widehat{\beta}_1 + \widehat{\beta}_2$ identifies the average indirect treatment effect of *Progresa* on non-eligibles in treatment villages that are embedded within extended family networks. We denote this parameter as ITE^1 . Finally, when (4.1) is estimated for non-eligibles, $\widehat{\beta}_1$ identifies the average indirect treatment effect of the program among non-eligible and isolated households, ITE^0 . These *ITEs* shed light on whether there exist within village spillovers of *Progresa* on secondary school enrolment. Spillovers onto non-eligibles may arise either because extended family networks span eligibles and non-eligibles and resource transfers take place between all households in the network, or because of general equilibrium effects of *Progresa*.

4.2 Unconditional Levels and Changes in Enrolment Rates

The left hand panel in Table 2 provides descriptive evidence on the pre-program levels of secondary school enrolment rates, and how they vary over time in treatment and control villages by eligibility status. While enrolment rates among eligibles at baseline are similar across treatment and control villages, by November 1999 eligibles in treatment villages have 6.9% higher enrolment rates than eligibles in control villages. This DD is positive and significant for both boys and girls enrolment, and consistent with previous studies, the proportionate increase is greater for girls.²⁰

The central panel of Table 2 splits the sample between connected and isolated households. This reveals that eligible isolated and connected households have similar baseline levels of enrolment, implying that determinants of the *levels* of enrolment – such as credit constraints, information on the costs and benefits of schooling, or demand for child labor in the home – do not, on average, explain baseline differences in the schooling choices between isolated and connected households. Hence isolated households are at least as well off – in terms of secondary enrolment – as connected households, and should not therefore be viewed as more vulnerable. This idea is reinforced by

²⁰Recall that the program design is such that households have no incentives to foster children in order to obtain greater transfers. Indeed, we note these changes in enrolment are driven by changes in the number of children enrolled in school, and not by the number of children resident in the household. The pre-program differences in enrolment rates of non-eligibles are reduced if the *densificados* – those households that changed eligibility status in October 1998 – are also used to calculate enrolment at baseline.

the fact that isolated and connected households do not differ in their poverty index at baseline, a measure of permanent income, as shown in Figure A2.

Despite similarities at baseline, there are significant differences in the program response of isolated and connected households. The DD in enrolment rates among eligible and connected households is 8.3% and significantly different from zero, while the DD for isolated households is close to zero. In short, the previously documented positive effects of *Progresa* on secondary enrolment are a combination of a large and significant effect on the 80% of connected households, and a close to zero effect on the 20% of isolated households. In line with this, we note that take-up rates for school related transfers – either secondary or primary – are consistently higher among connected households. Immediately after the initiating of *Progresa*, connected households have take-up rates that are .8% higher, and this difference rises to 4.0% by November 1999.²¹

The last panel in Table 2 shows the baseline levels and changes over time in primary enrolment rates for connected and isolated households. The baseline levels of primary enrolment are very high to begin with, and neither types of eligible household increase their enrolment rates relative to analogous households in control villages. This confirms that transfers from this component of *Progresa* are obtained without inducing any changes in behavior, and therefore essentially act as a pure income effect. This opens up the possibility that, if resources are shared within family networks, and these resources are channelled towards members with secondary school aged children, there is a wedge between the response of connected and isolated households in terms of secondary school enrolment, as suggested by the evidence in Table 2.

4.3 Identification

Equation (4.1) makes precise the assumptions required for the *TTE* and *ITE* parameters to be identified. first we require the standard twin assumptions of no cross village spillovers and random assignment of villages into treatment and control villages. Together these assumptions imply the change in enrolment for eligibles and non-eligibles in the absence of treatment in treatment villages would have been, on average, identical to the change in enrolment in the absence of the program in control villages. In other words, eligibles (non-eligibles) in control villages provide a valid counterfactual for eligibles (non-eligibles) in treatment villages.²²

One concern is that, given random assignment, any omitted time varying factors that would

²¹Among non-eligibles, neither connected nor isolated households in treated villages have any significant changes in enrolment vis-à-vis analogous households in control villages (not shown).

²²In support of the first identifying assumption, we note that villages were, in part, included in the evaluation data because they were geographically remote. In terms of the second identifying assumption, it has been previously documented that household and village characteristics do not significantly differ in treatment and control villages suggesting random assignment [Schultz 2004, Behrman *et al* 2005].

differentially drive enrolment rates in treatment and control villages will be orthogonal to ΔP_{vt} but not necessarily to L_{jh} . To address this we always control for a series of characteristics of the head, spouse, household, and village. Moreover we allow there to be a direct impact of the family link- j on secondary enrolment to allow for the effect of having family present in the village to change over time, say because of changes in other public assistance programs. We therefore estimate the following specification for eligibles and non-eligibles,

$$\Delta Y_{hvt} = \alpha + \beta_1 \Delta P_{vt} + \beta_2 (\Delta P_{vt} \times L_{jh}) + \beta_3 L_{jh} + \boldsymbol{\lambda}' \mathbf{X}_{hv} + \Delta u_{hvt}, \quad (4.2)$$

where \mathbf{X}_{hv} includes characteristics of the head, spouse, household, and village.²³

To then identify whether the *TTEs* and *ITEs* vary with regards the presence of extended family links of type- j , namely to consistently estimate β_2 , requires an additional assumption that the presence of link type- j is uncorrelated with unobservables that drive the response to *Progresa* ($Cov(L_{jh}, \Delta u_h) = 0$). However a central concern is that β_2 captures two effects – (i) the response to the *Progresa* program differs according to whether a household is isolated or embedded within a family network; (ii) connected and isolated households differ in characteristics that drive responses to *Progresa* and are correlated to the presence of the extended family.

To isolate the impact of the extended family *per se*, we need to purge the estimates of factors that both drive the presence of extended family and responses to *Progresa*. To do this we proceed as follows. First, we estimate what are the correlates of extended family link of type- j being present in the village or not, as detailed in the Appendix. These results, reported in Table A3, highlight the following characteristics are robust predictors of whether extended family members reside in the same village or not – whether the head’s (spouse’s) age is above or below the median among couple headed households, whether the head (spouse) is literate, whether the household owns land, and whether household size at baseline is above or below the median among couple headed households. Using this analysis to guide our approach we then allow for a household’s response to *Progresa* to be heterogeneous along each of these dimensions. We then estimate the following specification for eligibles and non-eligibles,

$$\Delta Y_{hvt} = \alpha + \beta_1 \Delta P_{vt} + \beta_2 (\Delta P_{vt} \times L_{jh}) + \beta_3 L_{jh} + \boldsymbol{\lambda}' \mathbf{X}_{hv} + \sum_i \gamma_{2i} Z_i + \sum_i \gamma_{3i} (\Delta P_{vt} \times Z_i) + \Delta u_{hvt}, \quad (4.3)$$

²³These controls are the head and spouse’s ages, literacy status, whether (s)he speaks an indigenous language, the household poverty index, whether the household owns any land, and household size at baseline. At the village level we control for the number of households in the village to capture any scale effects, the share of households that are eligible to capture any aggregate income effects, the marginality index for the village, and the village level enrolment rate at baseline among eligible and non-eligibles. This may correlate to distances to school facilities for example. Hence we do not exploit data from villages that have all eligible or all non-eligible households so the *TTEs* and *ITEs* are identified from the same set of villages. As randomization into treatment and control groups takes place within region, we control for regional fixed effects throughout.

where Z_i refers to each dimension along which we allow the response to *Progresa* to be heterogeneous, in addition to allowing for heterogeneous responses with the presence of the extended family link type- j . We additionally allow Z_i to include the average village level enrolment rates among eligible and non-eligibles, as measured at baseline, to pick up any differential trends in enrolment by eligibility, across treatment and control villages. Comparing the estimates from specifications (4.2) and (4.3) then indicates whether there are likely to exist unobserved characteristics, not captured in Z_i , that drive the differential responses of connected and isolated households.²⁴

The remaining econometric concerns relate to unobservables that – (i) are correlated to the existence of extended family; (ii) cause heterogeneous responses to *Progresa*; and, (iii) do not drive the baseline levels of enrolment to be different across connected and isolated households (Table 2). In the Appendix we present a series of robustness checks that allow program responses to vary with more village level characteristics, we explore the robustness of the results in alternative subsamples of villages, and address concerns over unobserved household level characteristics such as economic shocks driving enrolment changes.²⁵

4.4 Evidence in Support of the Identifying Assumptions

We provide three pieces of evidence related to whether connected and isolated households are likely to differ on unobservables. First, we estimated a propensity score for each type of household based on the variables in \mathbf{X}_{hv} , village fixed effects, and a full set of household demographic characteristics. Figure A1 shows the distribution of the propensity score by extended family links, reveals connected and isolated households to be, overall, balanced in household observables. Second, Figure 2A shows the distribution of potential transfers available to isolated and connected households to be very similar, again because the demographic characteristics of children do not differ across these household types at baseline. Third, we reiterate that connected and isolated households have similar baseline enrolment rates, and similar levels and distributions of permanent income, as measured by the household poverty index, as shown in Figure A2. Finally, we reiterate that the empirical analysis allows household responses to *Progresa* to vary along a number of dimensions that differ between connected and isolated households (Table A3). Hence we shed light on whether there exists a differential effect of being embedded within a family network or not, over and above potentially heterogeneous effects of characteristics that predict the existence of family links.

²⁴As the effects of *Progresa* are allowed to be heterogeneous along other dimensions Z_i , the *TTEs* and *ITEs* are evaluated at the mean value of each variable in Z_i .

²⁵A concern over the interpretation of the results stems from *Progresa* having a direct effect on fertility. If so the opportunity cost of female time will change and this may in turn drive schooling responses. Todd and Wolpin [2006] develop and test a dynamic structural model of schooling and fertility choices, and find that fertility rates are insensitive to the value of *Progresa* transfers, a result confirmed using a reduced form approach [Schultz 2004].

Table 3 shows how the probability of having an extended family link of type- j varies by eligibility across treatment and control villages. This sheds light on whether another of the identifying assumptions, $Cov(\Delta P_{vt}, L_{jh}) = 0$, is likely to hold in the data. Consistent with the earlier evidence – (i) 80% of households are connected; (ii) links from the head are significantly more likely to exist than links from his spouse; (iii) intra-generational links are significantly more common than inter-generational links. These patterns hold for eligibles and non-eligibles in treatment and control villages. Reassuringly, in nearly all cases there are no significant differences in the extended family links of eligibles and non-eligibles between treatment and control villages.²⁶

Table 4 provides descriptive evidence on family network characteristics as a whole, by treatment and control villages.²⁷ There are 1379 (817) family networks in treatment (control) villages covering 10559 (6471) households. In treatment villages, the first column shows that on average there are around 7.6 households in each family. The average village has around seven family dynasties so each dynasty encompasses 16% of all village households. The third column provides information on the diameter of the network – the largest distance between any two households in the network. This is around 2.4, implying family networks are unlikely to span more than three generations.

The remaining columns document how family networks vary. We consider characteristics that relate specifically to the design of *Progresá*, as discussed in Section 2.1, that we exploit. We first see that networks span eligibility status – there are an almost equal number of eligibles and non-eligibles in the average family. Hence there is considerable scope for *Progresá* transfers to be redistributed within the family. Family networks also vary in the share of households within them that have primary and secondary school aged children. Hence, as shown in the final column, the potential value of transfers each household is eligible for and hence the amount of resources to be redistributed within the family, varies considerably between networks.²⁸ Finally, Table 4 also decomposes the variation in each statistic into that arising between family networks across different villages, and across family networks within the same village. In nearly all cases, there is more variation within networks in the same village than across villages. This is important given that identification arises from an across village comparison of eligibles (non-eligibles) with a given set of family characteristics.

²⁶The one exception is the proportion of non-eligibles with links from the spouse to her brother being significantly higher in controls. Our later analysis however focuses on the influence of same gender siblings.

²⁷Two households i and j are defined to be within the same family network if the distance between them is finite. Isolated households do not contribute to these network statistics.

²⁸These figures relate to the potential value of transfers to all eligible households in the network. The values for the subset of eligible households with secondary school aged children are higher.

5 Does The Presence of Extended Family Matter?

5.1 Baseline Results

To begin with, Column 1 of Table 5 benchmarks our estimates against the existing literature by reporting standard TTE and ITE estimates of *Progresa* from specification (4.2), by pooling eligibles and non-eligibles and interacting ΔP_{vt} , L_{jh} , and $(\Delta P_{vt} \times L_{jh})$ with the dummy for eligibility status, D_h . These are averaged across households irrespective of their extended family structure. The TTE estimate implies eligibles have a 7.8% increase in their secondary enrolment rate compared to eligibles in control villages, which is similar in magnitude to the previously documented program effects [Schultz 2004]. The ITE estimate implies non-eligibles in treatment and control villages have similar changes in their secondary enrolment. This suggests that, on average, there are no significant spillover effects of *Progresa* on the secondary enrolment of non-eligibles.

Column 2a estimates all four parameters of interest from a single regression. The result confirms that the TTE of *Progresa* is actually comprised of a large and significant effect on eligibles that have extended family members present in the village ($TTE^1 > 0$), and a negligible and non significant effect among isolated households ($TTE^0 = 0$).

There is no evidence of any $ITEs$ on average for either households in family networks or isolated households. The fact that the ITE^1 is close to zero itself implies there are no within family spillovers in secondary enrolment rates. If recipient households redistribute some fraction of their transfers to non-eligibles within the same family network, then either – (i) the amount transferred is too small for the average ITE^1 to be positive and significant; or, (ii) recipient households do not themselves use such resources to increase secondary enrolment, but rather use them to change behavior along other margins such as consumption, say.²⁹

At the foot of the table we report the triple differences, $\Delta TTE = TTE^1 - TTE^0$, and $\Delta ITE = ITE^1 - ITE^0$. The former captures the differential effect of *Progresa* between eligible connected and isolated households within treatment villages, and similarly, the latter captures the differential effect of *Progresa* between non-eligible connected and isolated households within treatment villages. Column 2a shows that connected eligibles experience a 8.4% increase in their secondary enrolment rate relative to eligible but isolated households within treatment villages. Moreover, the ΔITE estimate implies non-eligibles with and without extended family present

²⁹Two further points are of note. First, the estimated parameters of interest are very similar whether they are estimated from separate regressions for connected and isolated households, or from a pooled regression. This implies the marginal effects of the other controls do not differ between connected and isolated households. Second, the coefficient on L_{jh} , β_3 , which recall is not used to estimate any of our parameters of interest, is not significantly different from zero. This suggests the effect of having an extended family on the level of enrolment in the absence of *Progresa*, is not changing over this time period.

have almost identical changes in enrolment from the baseline period. Given these results on the *ITEs*, we now focus on the *TTEs* and return to discuss the *ITEs* in Section 7.³⁰

Column 2b estimates (4.3) and allows responses to *Progresa* to vary along a number of additional dimensions that are correlated with the presence of extended family (Table A3), and also allows the response to *Progresa* to vary with the baseline village enrolment rates among eligibles and non-eligibles. We find the previous pattern of coefficients is robust to the inclusion of these interactions. Connected eligibles have a – (i) 9.2% increase in their enrolment rate relative to connected eligibles in control villages; (ii) 8.6% increase in their enrolment rate relative to isolated eligibles in treatment villages. The previous estimates do not appear to merely pick up heterogeneous responses along observable dimensions that are correlated with the existence of extended family links.³¹

While we can never rule out with certainty that there exists some unobserved characteristic that drives the results, the stability of the estimates across specifications (4.2) and (4.3) is reassuring, and the requirements for such a variable to explain the data are stringent. In particular the unobserved characteristic should – (i) be correlated to the presence of extended family in the village; (ii) drive responses to *Progresa*; (iii) not drive the baseline levels of enrolment to be different across connected and isolated households.

Estimating (4.3) allows us to benchmark the magnitude of the effect of being embedded in a family network vis-à-vis other observable characteristics. We find that – (i) the magnitude of TTE^1 is larger than the *TTE* of most other observable characteristics; (ii) there are few observable dimensions along which household behavior is as heterogeneous as with regards to whether members of the extended family are present or not. For example, the *TTE* of the household owning land is .082, the *TTE* of the household not owning land is .065. While both these *TTEs* are significant at the 1% level, the difference between them is not significantly different from zero.³²

³⁰As ΔITE corresponds to coefficient on the interaction term, $L_{jh}.D_h$, the results suggest there is no differential effect of having extended family by eligibility status. Reassuringly, this is in line with the presence of extended family not proxying for some unobservable that varies with eligibility status and drives enrolment over time.

³¹Three simple explanations of this result would be that – (i) isolated households have higher enrolment rates to begin with and so have less scope to respond to the program; (ii) trends in enrolment rates among isolated households are different to others households over this time period; (iii) isolated households are not aware of the program, while connected households may simply share information rather than resources within their family network. None of these explanations is supported by the data. Although isolated households do have slightly higher enrolment rates than households embedded in family networks at baseline, these enrolment rates are overtaken by November 1999 by the other households. Second, the pattern of coefficients on the *ITEs* rule out the explanation that isolated households are naturally changing their enrolment rates over this time period. Third, villages in the evaluation sample are small – the average number of households in each is 45. It is hard to conceive of a significant proportion of households remaining unaware of such a large scale policy intervention.

³²The *TTEs* of the head being above (below) the median age among all couple headed households is .090 (.062), the spouse being above (below) the median age is .070 (.082), the head being literate (illiterate) is .061 (.118), the spouse being literate (illiterate) is .061 (.099), and the household size at baseline being above (below) the median of

We next repeat the analysis for boy’s and girl’s secondary enrolment separately. Columns 3a and 3b estimate (4.3) and show that, first, connected eligibles significantly increase the enrolment of boys and girls more than analogous households in control villages. Second, eligible but isolated households do not respond to the program in terms of boys enrolment, although the TTE^0 estimate is imprecisely estimated. As a result, the ΔTTE shows that the null hypothesis that within treated villages connected and isolated households have the same response cannot be rejected. Third, eligible but isolated households do not respond to the program in terms of girls enrolment, despite the monetary value of conditional cash transfers being higher for girls’ enrolment, and the baseline level of girls’s enrolment being lower.

Having established the importance of being embedded within a family network for a households response to *Progresa*, we now begin to unpack why this is the case. To begin with, a necessary condition behind why connected and isolated households respond differentially to the program, is that the level of transfers conditional on secondary enrolment is not sufficient to compensate households for the opportunity costs of child labor. To provide direct evidence on this we use cross sectional variation in adult wage rates in October 1998 to identify villages with above and below the median level of adult wages.

The results show that in villages where wages are sufficiently high – (i) in terms of boys secondary enrolment, neither connected nor isolated households respond to *Progresa* (Column 4a); (ii) in terms of girls secondary enrolment, it remains the case that only connected households respond to the program (Column 4b). If children are substitutes for adults in the local labor market so that their wages are positively correlated with adult wages, and if boys are more likely to be engaged in labor market activities relative to girls, then the results suggest the magnitude of transfers is not large enough to induce many households to change their behavior and increase the school enrolment of boys. Moreover, for connected households, there exist limits to the resources family networks can redistribute towards members with secondary school aged children.³³

A second important factor underlying why connected and isolated households may respond differentially to the program is that extended families share resources. We therefore check whether family wealth is correlated to secondary enrolment rates at baseline for connected households. To

all couple headed households is .041 (.096). These $TTEs$ are all significant at the 1% level. The difference in $TTEs$ are not significantly different from zero except along two margins – the head being literate or not ($\Delta TTE = .057$ and is significant at the 10% level), and household size at baseline being above or below the median ($\Delta TTE = -.054$ and is significant at the 5% level).

³³Using wages as measured in October 1998 may be problematic if in villages where children withdraw from the labor market as a result of the program, are those that experience the greatest adult wage increases. However, October 1998 corresponds to only five months after the introduction of the program. Hence wages are unlikely to have adjusted to such an extent so as to change the cross sectional ranking of villages by adult wages. Moreover we note that the correlation over time in adult wages in the 212 villages in which wages are available both at baseline and in October 1998, is .749.

do so, we regress the baseline enrollment rate against the set of controls described above, and additionally control for the average poverty index among households in the extended family. The result in Column 5 shows that wealthier households have significantly higher enrolment rates at baseline. The coefficient implies that if there were to be a one standard deviation increase in the poverty index of the average family member, the households secondary enrolment rate would rise by 2%, relative to a baseline enrolment of 65%.

In the Appendix we present a series of robustness checks on our main result that only connected households respond to *Progresa*. The first series of checks relate to concerns over the surnames information and matching algorithm. In particular we show the robustness of the baseline results to – (i) potential measurement error in surnames; (ii) limiting the sample to smaller villages where there is less likelihood of spurious family links being defined; (iii) the fact that our matching algorithm may measure the intrinsic value of surnames rather than having anything inherently to do with extended family links. The second series of checks address – (i) the concern that there are unobserved village level characteristics that drive both the presence of isolated households and their differential response to *Progresa*; (ii) the concern that there may be unobserved time varying household characteristics that drive their program responses such as whether they are subject to economic shocks post-program; (iii) the underlying identifying assumption that there are no spillover effects from treatment to control villages; (iv) the underlying assumption that extended family networks are not changing over time.³⁴

5.2 Understanding Why Families Matter

To understand why connected and isolated households respond differently, we exploit the design feature of *Progresa* that transfers provided conditional on primary enrolment effectively act as a pure income effect on households. This implies, first, households with and without primary school aged children should respond differentially to *Progresa* in terms of secondary enrolment. Second, if families share resources, then the differential response between connected and isolated households depends on the presence of primary school aged children in eligible households within the extended family.

On the first mechanism, we estimate how the average treatment effect varies with the value of transfers received by the household, and the presence of primary school aged children in the household. On the second mechanism, we estimate how the average treatment effect varies with

³⁴An additional concern is that if children can flexibly supply any amount of hours to the labor market, then children may enrol in school 85% of their time – just sufficient to obtain *Progresa* transfers – and then devote the remaining 15% of their time to continue earning in the labor market. This hypothesis is not supported by the data. Attendance rates in November 1999 are over 95% for both boys and girls, suggesting indivisibilities or fixed costs in supplying labor to the market.

the average value of transfers received by eligibles in the extended family of connected households, and the presence of primary school aged children in the family network. As transfers are provided conditional on household behavior, the actual transfers received are endogenously determined. We therefore proxy actual transfer receipts with the potential value of transfers the household could have received, as measured in October 1998. This is determined by the demographic characteristics of children in the household at baseline.

Figure 2A shows that the distribution of potential transfers available to isolated and connected households is very similar, because the demographic characteristics of children in isolated and connected households do not differ at baseline. Figure 2B plots the potential transfers each eligible connected household with secondary school age children receives against the average potential transfer eligible households in its family network are entitled to. The correlation between them is only -.01, because, as documented in Table 4, there exists considerable variation in the demographic characteristics of children across households within the same family network.

We then use the following specification to estimate how, for eligibles, the response to *Progresa* varies with the potential transfers received by household h itself (T_h^O), and with the average value of transfers received by eligibles among the extended family (T_h^F),

$$\begin{aligned} \Delta Y_{hvt} = & \alpha + \beta_1^O (\Delta P_{vt} \times T_h^O) + \beta_1^F (\Delta P_{vt} \times T_h^F) + \beta_2^O T_h^O + \beta_2^F T_h^F \\ & + \boldsymbol{\lambda}' \mathbf{X}_{hv} + \sum_i \gamma_{2i} Z_i + \sum_i \gamma_{3i} (\Delta P_{vt} \times Z_i) + \Delta u_{hvt}, \end{aligned} \quad (5.1)$$

where all other controls are as previously defined, and standard errors are clustered by village. The parameter of interest is how the response to *Progresa* varies with the intensity of treatment,

$$\frac{\partial \Delta Y_{hvt}}{\partial \Delta P_{vt}} = \beta_1^O T_h^O + \beta_1^F T_h^F + \sum_i \gamma_{3i} Z_i. \quad (5.2)$$

As potential transfers are defined for eligibles in both treatment and control villages, this is identified from a comparison of eligibles in treatment villages with a given value of potential transfers, relative to eligibles in control villages that have an identical value of potential transfers. Both sets of households also have the same family characteristics (connected or isolated). By controlling for the level of potential transfers the household and average family member are eligible for in (5.1), we capture any direct effects the demographic composition of the household and its extended family have on changes in secondary enrolment.³⁵

Table 6 presents the results. The upper (lower) panel shows the results for households with

³⁵For example, families with a larger share of primary school aged children may be learning more quickly about the net benefits of secondary education for their older children, other things equal.

(without) primary school aged children in them at baseline. This allows us to assess any interplay between the own and family transfers and whether the household is on the margin of enrolling children into secondary school because it receives unconditional transfers for primary school aged children. Throughout we evaluate (5.2) at the mean values of T_h^O , T_h^F , and each Z_i .

Column 1 estimates the program response among isolated households. We see that the *TTE* is not significantly different from zero for isolated households, irrespective of whether they have any primary school aged children in them or not. Repeating the exercise for connected households in Column 2 shows that, on average, there is a positive and significant *TTE* for households both with and without primary school aged children.

The remaining Columns shed light on how these *TTEs* vary with the demographic composition of the eligible family network. Column 3 focuses on families in which the minority of members are both eligible and have primary school aged children. In such networks, *Progresa* leads to a relatively small increase in available resources per household. The upper panel shows that households with primary school aged children in such families significantly respond to the program. In contrast, the lower panel shows that connected households that receive no unconditional transfers themselves are not significantly influenced by the flow of resources into their household, or into the households of eligible family members.

Hence there exists a subset of connected households who do not respond to *Progresa* – namely those that receive no unconditional transfers themselves, and are part of family networks that potentially receive only a small cash injection from *Progresa*. This is consistent with resource sharing within family networks if resources are first redistributed towards those with primary school children who are on the margin of being able to enrol their children into secondary school.

A useful thought experiment is to ask what would have been a household’s response to the program if it were the *only* household in its family network to obtain transfers? To answer this, Column 3 reports (5.2) evaluated at $T_h^F = 0$. The estimate implies if a household is the only member of its extended family to obtain transfers, it does not significantly respond to the program in terms of secondary school enrolment. As shown in the upper panel, this is true even for those households who have primary school aged children and are closer to being able to fully offset the opportunity costs of secondary school enrolment. This pattern of behavior – that connected households do not respond to the program if others in their network are non-eligible – is also found throughout the next Section, where we consider how program responses vary with the presence and eligibility status of particular extended family members.

Column 4 focuses on the case in which the majority of family members are both eligible and have primary school aged children. In these networks, *Progresa* leads to a relatively large increase in resources per household. In such families, connected households with and without primary

school aged children are sensitive on the margin to the total inflow of resources to the family network. To see how responses to the program vary with the extent of the inflow of resources to the family network, Column 4 also reports the *TTE* where we evaluate (5.2) at the mean of T_h^O and the 75th percentile of T_h^F . The estimates imply that the response of households to *Progresa* in family networks that experience a large injection of transfers, is increasing in the magnitude of these transfers. Comparing the *TTEs* in Column 4 we note that in such families, households without primary school aged children are more sensitive on the margin to the flow of resources into the family network than are households with primary school aged children.

To summarize, if a household is the sole recipient of transfers within the family network, they have a response to *Progresa* close to zero, irrespective of whether they have primary school aged children or not (Column 3). For those with primary school aged children, their response is more sensitive to the inflow of resources into the extended family when these amounts are small (Column 3, upper panel), and they are less sensitive to them when inflows increase (Column 4, upper panel). In contrast, those without primary school aged children only become sensitive to the inflow of resources to their family when the magnitude of this inflow is sufficiently large (Column 4, lower panel). This pattern is consistent with families first redistributing resources towards extended family members with primary school aged children, namely those that receive unconditional transfers and are on the margin of enrolling children into secondary school.

The results help rule out other mechanisms behind why families matter, such as information sharing within families, or the presence of extended family being correlated to membership of *ejidos* which grant access to communal land, an institution that can help enforce interlinked labour-credit contracts. This is because if families mattered through these other channels, we would not expect household responses to *Progresa* in terms of secondary school enrolment to depend on the extent to which other households in the family network were eligible, or the extent to which these eligible households received transfers specific to their primary school aged children.

6 Which Family Members Matter and Why?

We now exploit the full richness of the constructed data on extended family links to shed light on which family members matter for connected households' response, and why. To do so, we estimate (4.2) for households with family link type- j and show how their response to *Progresa* varies with two characteristics of the link – the eligibility status of the link, and the demographic composition of children in linked households.³⁶

³⁶For example, to understand whether the program response of household h varies with the eligibility status of the brother of the head of household, we define our link variable, L_{jh} , to be equal to one if the head of household h has brothers present in the village and at least one brother is himself eligible for *Progresa* transfers, and zero if

6.1 Intra-generational Family Links

Column 1a of Table 7 estimates how the treatment effects of *Progresa* vary with the presence and eligibility status of the same gender siblings of the head and spouse of household h – namely the brother of the head of household, and the sister of his spouse.³⁷ Column 1a shows that eligible households in treatment villages that have an eligible brother of the head present, namely the uncle of the children in household h , increase secondary enrolment rates significantly more than analogous eligible households with eligible brothers in control villages ($TTE^1 > 0$). In contrast, there is no program response among eligible households with non-eligible brothers ($TTE^0 = 0$). Column 1b shows a similar pattern of responses among households by the eligibility status of the sister of the spouse, namely the aunt of the children in household h . The fact that households respond only if both they and their sibling are eligible for *Progresa* transfers is consistent with there being few resources to redistribute within the family when other members of the extended family are not eligible for *Progresa*.

The next columns explore whether for a given eligibility status of the sibling, the demographic composition of children in the sibling’s household affects the response of household h to the program. This pins down whether there is a redistribution of unconditionally received transfers from households with primary school aged children to their eligible siblings. We therefore define the link variable, L_{jh} , to be equal to one if the head of household h has an eligible brother that has secondary school aged children, and equal to zero if the head of household h has eligible brothers that only have primary school aged children.

Column 2a shows households with eligible brothers of the head present significantly increase secondary enrolment relative to analogous households in control villages. Importantly, the magnitude of the response is more than twice as large if siblings receive largely unconditional transfers through only having primary school aged children. Column 2b shows a similar pattern of results when we consider how the response of households varies with the demographic composition of children in the eligible household of the sister of the spouse.

One concern is that households whose siblings have younger children may differ on unobservables to those whose siblings have older children, and such unobservables drive program responses. This is addressed in Columns 3a and 3b that show, conditional on the sibling being non-eligible, the demographic composition of the children of siblings does not influence the response of house-

brothers are present in the village but all are non-eligible.

³⁷We focus on same gender siblings because, first, these household pairs are likely to be at similar stages of the life cycle and so more similar on observables than cross-gender sibling pairs still resident in the village. In particular secondary and primary school aged children are likely to be observed in both households. Second, as shown in Table 3, the proportion of households with same gender siblings is the same across treatment and control villages by eligibility – the same is not true for cross-gender sibling pairs.

hold h . It is not therefore the case that households that have siblings with young children respond differently to the program *per se*. Rather it is the interaction of those siblings being eligible for *Progres*a, with siblings receiving unconditional transfers for the primary school enrolment of their children, that allows household h to itself respond to *Progres*a. This is consistent with within family resource transfers taking place from eligible households with primary school aged children to the households of their eligible siblings with secondary school aged children. Such receiving households are likely to be on the margin of enrolling their children into secondary school.

Finally, the results also show the presence of siblings with secondary school aged children *only* influences program responses if those siblings are themselves eligible for *Progres*a. This evidence contradicts the hypotheses that siblings share any fixed costs of secondary school, or that behavior is driven by conformity or peer effects within siblings based on secondary school outcomes.

6.2 Inter-generational Family Links

We now explore how household behavior is shaped by the characteristics of those in the family network that are inter-generationally linked. We first consider the links between the head and spouse of household h to their adult sons or daughters that head their own household j in the village. These individuals correspond to the adult siblings of the children actually resident in household h . Given the age structure of these links, we consider how the program response of household h in terms of secondary school enrolment rates varies with the eligibility status and the presence of young children in household j . The presence of young children matters if the resources available to be shared are those transfers received on a largely unconditional basis, such as those for primary school enrolment, or the attendance of children aged under five to local health clinics.

As with sibling links, Column 1a of Table 8 shows eligible households only respond to *Progres*a if their adult son is also eligible ($TTE^1 > 0$, $TTE^0 = 0$). Column 1b confirms this applies equally to adult daughters resident in the village. To pin down whether these responses reflect within family resource transfers from adult children to their parents, we note first that the adult son is himself unlikely to have secondary school aged children and so the transfers he receives relate either to his primary school aged children, or the attendance of his young children or pregnant wife to health clinics. Such transfers are likely to be viewed as a pure income effect to his household.

We therefore estimate how the treatment effects vary in household h with the demographic composition of children in the eligible households of their adult sons and daughters. Given the age profile of such households, we consider the influence of eligible adult child households that have primary school aged children versus those that have no primary school aged children and are thus eligible either because of the presence of very young children or a pregnant wife. Columns 2a and 2b show the response of households, in terms of secondary school enrolment, increases conditional

on their adult sons and daughters being eligible, and irrespective of the demographic composition of children in their adult son and daughter’s households.

This suggests adult sons and daughters view both components – transfers conditional on the primary school enrolment of their children and those conditional on their attendance to health clinics – as pure income transfers that can be redistributed within the family network.

Finally, we consider inter-generational links from the head and spouse to their parents – namely, the paternal and maternal grandparents of the children in household h . Such elderly households can be eligible for a health component of the program where those aged over 60 are required to attend a clinic once per year. The results in Columns 3a and 3b show the response of households to the program are generally weaker in the presence of the paternal or maternal grandparents of the children. Unlike the other family links considered, this is the case irrespective of the eligibility status of the household in which the paternal or maternal grandparents reside. This may reflect the low monetary value of transfers such elderly households are eligible for [Skoufias 2005].³⁸

7 Discussion

We have presented evidence from the *Progresa* social assistance program on whether and how household behavior is influenced by the presence and characteristics of its extended family. Our central finding is that *Progresa* raises secondary enrolment only among households that are embedded in extended family networks. Eligible but isolated households do not respond. Our results suggest a key channel through which the extended family influences household schooling choices relates to the redistribution of resources among family members to enable eligible family members to fully overcome the opportunity costs of enrolling their children into secondary school.

There are a number of common themes running throughout our analysis that we now bring to the fore. The first stems from the design feature of *Progresa* that transfers are provided directly to women. If this increases females’ bargaining power, household behavior may be differentially influenced by the characteristics of the extended family of the wife relative to that of her husband. However, throughout the paper we find no evidence that the family links of the spouse are more influential than are the family links of her husband. The mechanism through which extended family affects household schooling behavior – resource sharing – is qualitatively the same for both the head and spouse’s family links.³⁹

³⁸The evaluation data contains limited information on pre-program transfers. We note that those households defined to be of older generations are more likely to report receiving transfers at baseline. This is consistent with evidence from the Mexican Family Life Survey that transfers tend to flow from younger generations to older generations in the family network.

³⁹This result does not however imply that female bargaining power or the distribution of resources *within* the household is unaltered by *Progresa* [Attanasio and Lechene 2002].

A second common theme is that there is little evidence of any indirect treatment effect of *Progresa*, namely that secondary enrolment rates among non-eligibles in the family network do not increase on average. This suggests either – (i) resources are not transferred from eligibles to non-eligibles in the network; (ii) the amounts transferred are small; (iii) non-eligibles do not themselves use these resources to increase secondary enrolment.⁴⁰

To shed light on this, we consider program responses along another margin, consumption. We therefore estimate the average treatment and indirect treatment effects of *Progresa* on household consumption, and explore how these vary with whether the household is connected or isolated. Our sample covers all couple headed households, not just those with secondary school aged children, and we use two measures of consumption – (i) the change in weekly consumption of meat, a luxury good; (ii) the proportionate change in household non-food expenditures.⁴¹

The results in Table 9 show that eligibles – whether connected or isolated – significantly increase both types of consumption. Reassuringly, this confirms isolated households are not dysfunctional – they are both aware of the program and take-up *Progresa* transfers. Hence connected and isolated households do not differ in terms of the information they have on *Progresa*. Isolated households respond on this margin because the mere receipt of transfers allows consumption to increase. In contrast, the value of transfers given to any one household for the secondary school enrolment of their children, are not by themselves sufficient to offset the opportunity costs of sending children to school. Hence it is the transmission of resources within family networks that makes connected and isolated households respond differently in terms of secondary school, not differences in the

⁴⁰Note that we do not find robust evidence of an effect of the program on the school enrolment of the *average* non-eligible. Spillover effects in schooling have been reported by Bobonis and Finan [2006] and Cattaneo and Lalive [2006]. These alternative findings may stem from methodological differences. For example, Bobonis and Finan [2006] and Cattaneo and Lalive [2006] match children over the waves of the data and consider changes in each individual’s enrolment status. Cattaneo and Lalive [2006] focus on the effects of the program in its first year up to October 1998 (wave 3), and consider the enrolment among primary school aged children and those transiting from primary to secondary school. Bobonis and Finan [2006] consider changes in enrolment of those children that have completed primary school at baseline, and find such spillover effects predominantly among those in non-eligible households on the margin of being eligible. Taken together, these results suggest there may be heterogeneous spillover effects on non-eligibles, and heterogeneous effects on the children within non-eligibles. Whether extended families influence the nature of such spillovers remains a question for future research.

⁴¹Consumption data is not available at baseline so we consider changes in these measures from the second pre-program wave of data (March 1998) until November 1999. For meat consumption, households were asked the number of times they consumed meat in the week prior to survey day. For non-food expenditures we normalize changes over time by their initial values in March 1998. We remove the top and bottom 2.5% of values in the proportionate change in non-food expenditures so that the results are not driven by outliers. Although there may be seasonal changes in consumption, these should affect isolated and connected households equally. Moreover, the qualitative results are similar when changes between March 1998 and March 1999 are considered instead. Non-food expenditures include those on clothing, adult goods such as alcohol and tobacco, hygiene products, medical products, kitchen utensils, and children’s toys. We do not include school related expenditures in this measure. All non-food expenditures are valued in pesos. Angelucci and De Giorgi [2006] and Gertler *et al* [2006] provide evidence that local food prices do not change significantly over time between treatment and control villages.

transmission of information inside and outside family networks.

Furthermore, there are indirect treatment effects on meat consumption for both connected and isolated households. The fact that the *ITE*'s are both positive suggests there might be – (i) redistribution of resources within families from eligibles to non-eligibles; (ii) redistribution of resources to isolated households; or, (iii) village-wide effects of the program that alter the consumption bundles of all households in treated villages. The fact that there are no *ITE*s for non-food consumption may reflect that on the margin, households prefer to use additional resources for the consumption of luxury food rather than non-food items.⁴²

In ongoing work, we develop and test a formal model of risk sharing in villages. Preliminary results suggest – (i) connected households share risk only within the extended family, and not with unrelated villagers; (ii) isolated households are as well insured against weather and health shocks as connected households, and do not rely more on self-insurance. An explanation for the latter result is that isolated households have family elsewhere, so that the existence of extended family is what matters to insure against shocks, irrespective of whether they live in the village or not.

The final theme running throughout our analysis has been the interplay between the design of conditional cash transfer programs, the presence of extended family members, and household responses to the program. There are three policy implications of our findings for the design of such programs. First, if families share resources, how connected households respond to such policies on any given margin, will generally depend on the eligibility status of others in their network. The particular design features of *Progresa* also lead to the behavior of connected households to depend on the demographic composition of others in the family network. More generally, our findings highlight that ignoring the presence and characteristics of the extended family can lead to an incomplete understanding of the forces driving the behavioral responses of households to large scale policy interventions in developing country settings.

Second, in this setting the value of transfers conditional on secondary school enrolment is not sufficiently high to offset any potential loss in children's labor market earnings if they were to enrol into full time secondary education. Our results suggest that the resources spent on *Progresa* could therefore be more efficiently targeted if the policy aim is to increase secondary school enrolment. In particular, if the program's entire budget were to be channelled into transfers conditional on secondary school enrolment – with no component conditioned on primary enrolment, and such

⁴²These results are in line with those of Angelucci and De Giorgi [2006] who find there to be indirect effects on the consumption of non-eligibles in treatment villages. They present evidence that this occurs through the insurance and credit markets – households indirectly benefit from their neighbors higher income by receiving more transfers, by borrowing more when hit by a negative idiosyncratic shock, and by reducing their precautionary savings. They find a limited impact of the program on food prices directly, a result confirmed in Gertler *et al* [2006], suggesting the $ITE^0 > 0$ result in Table 9 reflects the redistribution of resources through insurance networks in the village rather than general equilibrium effects of *Progresa*.

that the value of transfers offset children’s labor market earnings – then we would expect *both* isolated and connected households to increase their secondary enrolment rates.⁴³

Finally, our analysis has general implications for understanding the role of family networks in developing economies. Using data from over 22,000 households in 506 villages in rural Mexico, we have documented that 20% of couple headed households are isolated in that none of their extended family members are geographically proximate in the same village. The incidence of this type of isolatedness is therefore at least as high as the incidence of single headedness, which affects 15% of households in our data. While there exists a large literature on the effects of single headedness on household welfare, our data and results suggest the importance of designing future surveys to identify isolated households in other settings, and more generally, to establish the social ties between households in survey data. Such information can then be used to understand the effects of isolation and being part of geographically proximate kinship networks on household behavior and welfare. This forms the basis of a broad and challenging research agenda.⁴⁴

References

- [1] AKRESH.R (2005) Risk, Network Quality, and Family Structure: Child Fostering Decisions in Burkina Faso, mimeo Yale University.
- [2] ALTONJI.J.G, F.HAYASHI, AND L.J.KOTLIKOFF (1992) “Is the Extended Family Altruistically Linked? Direct Tests Using Micro Data”, *American Economic Review* 85: 1177-98.
- [3] ALTONJI.J.G, F.HAYASHI, AND L.J.KOTLIKOFF (1997) “Parental Altruism and Inter Vivos Transfers: Theory and Evidence”, *Journal of Political Economy* 105: 1121-66.
- [4] ANGELUCCI.M AND G.DE GIORGI (2006) Indirect Effects of an Aid Program: How Do Liquidity Injections Affect Non-Eligibles’ Consumption?, mimeo Stanford University.
- [5] ATTANASIO.O AND V.LECHENE (2002) “Tests of Income Pooling in Household Decisions”, *Review of Economic Dynamics* 5: 720-48.

⁴³Todd and Wolpin [2006] develop and test a structural dynamic model of the schooling and fertility effects of *Progresa*. They find the policy as currently designed increases years of schooling on average by .5 years. They then estimate the effect of a revenue neutral reform that eliminates transfers to children in grades 3 to 5 and increasing transfers to those in grades 6 to 9 by around 50%. They find such a reform would increase average completed schooling by an additional .1 years. Attanasio *et al* [2005] conduct a similar experiment and also find larger effects on secondary enrolment of such revenue neutral policy alternatives.

⁴⁴Several recent contributions have collected detailed information on the structure of family and other networks in village economy settings. These studies have, for example, provided important insights on the role of social networks in learning about agriculture in Ghana [Conley and Udry 2005], and child fostering in Burkina Faso [Akresh 2005].

- [6] ATTANASIO.O, C.MEGHIR, AND A.SANTIAGO (2005), Education Choices in Mexico: Using a Structural Model and a Randomised Experiment to Evaluate *Progresa*, IFS Working Paper.
- [7] BANERJEE.A AND A.NEWMAN (1998) “Information, the Dual Economy, and Development”, *Review of Economic Studies* 65: 631-53.
- [8] BECKER.G.S (1981) *A Treatise on the Family*, Cambridge: Harvard University Press.
- [9] BEHRMAN.J.R AND M.ROSENZWEIG (2006) “Parental Wealth and Adult Children’s Welfare in Marriage”, *Review of Economics and Statistics* 88: 496-509.
- [10] BEHRMAN.J.R, P.SENGUPTA, AND P.TODD (2005), “Progressing Through *Progresa*: An Impact Assessment of a School Subsidy Experiment in Rural Mexico”, *Economic Development and Cultural Change* 54: 237-76.
- [11] BOBONIS.G AND F.FINAN (2006) Endogenous Peer Effects in School Participation, mimeo, University of Toronto.
- [12] CATTANEO.A AND R.LALIVE (2006) Social Interactions in Schooling Decisions, CEPR Working Paper 5816.
- [13] CHIAPPORI.P.A (1988) “Rational Household Labor Supply”, *Econometrica* 56: 63-90.
- [14] CONLEY.T AND C.UDRY (2005) Learning About a New Technology: Pineapple in Ghana, mimeo Yale University.
- [15] COX.D AND G.JAKUBSON (1995) “The Connection Between Public Transfers and Private Interfamily Transfers”, *Journal of Public Economics* 57: 129-67.
- [16] DEATON.A (1992) “Saving and Income Smoothing in Côte D’Ivoire”, *Journal of African Economies* 1: 1-24.
- [17] DERCON.S AND P.KRISHNAN (2000) “In Sickness and in Health: Risk Sharing Within Households in Rural Ethiopia”, *Journal of Political Economy* 108: 688-727.
- [18] FAFCHAMPS.M AND S.LUND (2003) “Risk-sharing Networks in Rural Philippines”, *Journal of Development Economics* 71: 261-87.
- [19] GERTLER.P, S.MARTINEZ, AND M.RUBIO-CODINA (2006) Investing Cash Transfers to Raise Long-Term Living Standards, mimeo University of California Berkeley.

- [20] LA FERRARA.E (2003) “Kin groups and Reciprocity: A Model of Credit Transactions in Ghana”, *American Economic Review* 93: 1730-51.
- [21] LIGON.E (1998) “Risk-Sharing and Information in Village Economies”, *Review of Economic Studies* 65: 847-64.
- [22] LOURY.L.D (2006) “All in the Extended Family: Effects of Grandparents, Aunts, and Uncles on Educational Attainment”, *American Economic Review Papers and Proceedings* 96: 275-8.
- [23] MANSER.M AND M.BROWN (1980) “Marriage and Household Decision Theory - A Bargaining Analysis”, *International Economic Review* 21: 21-34.
- [24] MCELROY.M AND M.HORNEY (1981) “Nash-bargained Decisions: Towards a Generalization of the Theory of Demand”, *International Economic Review* 22: 333-49.
- [25] MUNSHI.K AND ROSENZWEIG.M (2005) Why is Mobility in India so Low? Social Insurance, Inequality, and Growth, mimeo Brown University.
- [26] ROSENZWEIG.M.R (1988) “Risk, Implicit Contracts and the Family in Rural Areas of Low Income Countries”, *Economic Journal* 98: 1148-70.
- [27] ROSENZWEIG.M.R AND O.STARK (1989) “Consumption Smoothing, Migration, and Marriage: Evidence From Rural India”, *Journal of Political Economy* 97: 905-26.
- [28] SCHULTZ.T.P (2004) “School Subsidies for the Poor: Evaluating the Mexican *Progresa* Poverty Program”, *Journal of Development Economics* 74: 199-250.
- [29] SKOUFIAS.E (2005) *Progresa and Its Impacts on the Human Capital and Welfare of Households in Rural Mexico: A Synthesis of the Results of an Evaluation by IFPRI*, Research Report 139, Washington D.C.: International Food Policy Research Institute.
- [30] TODD.P AND K.I.WOLPIN (2006) “Assessing the Impact of a School Subsidy Program in Mexico: Using a Social Experiment to Validate a Dynamic Behavioral Model of Child Schooling and Fertility”, forthcoming *American Economic Review*.
- [31] TOWNSEND.R (1994) “Risk and Insurance in Village India”, *Econometrica* 62: 539-91.
- [32] UDRY.C (1994) ‘Risk and Insurance in a Rural Credit Market: An Empirical Investigation in Northern Nigeria’, *Review of Economic Studies* 61: 495-526.
- [33] WEISS.Y AND R.J.WILLIS (1985) “Children as Collective Goods and Divorce Settlements”, *Journal of Labor Economics* 3: 268-92.

Table 1: The Number of Extended Family Links, by Type of Link

Couple Headed Households

Mean, standard error in parentheses clustered by village

	Outside the Household and in the Village									
	<u>Parent</u>		<u>Children Aged 0-16</u>		<u>Adult Children</u>		<u>Siblings</u>		<u>All</u>	
	Connected	Isolated	Connected	Isolated	Connected	Isolated	Connected	Isolated	Connected	Isolated
From head of household to:	.461	-	-	-	.652	-	2.23	-	3.34	-
	(.010)				(.066)		(.111)		(.164)	
From spouse of household to:	.250	-	-	-	.652	-	1.63	-	2.54	-
	(.007)				(.066)		(.103)		(.160)	

	Inside the Household									
	<u>Parent</u>		<u>Children Aged 0-16</u>		<u>Adult Children</u>		<u>Siblings</u>		<u>All</u>	
	Connected	Isolated	Connected	Isolated	Connected	Isolated	Connected	Isolated	Connected	Isolated
From head of household to:	.062	.079	3.23	3.10	.821	.787	.033	.052	4.15	4.02
	(.003)	(.006)	(.027)	(.043)	(.015)	(.021)	(.002)	(.007)	(.035)	(.056)
From spouse of household to:	.018	.021	3.23	3.10	.821	.787	.013	.017	4.09	3.93
	(.001)	(.003)	(.027)	(.043)	(.015)	(.021)	(.001)	(.003)	(.034)	(.054)

Notes: The sample is restricted to couple headed households in the baseline survey. Standard errors are clustered by village. Of the 22553 households that can be tracked in the first and third waves of *Progres*a, 84.2% report to be couple headed in October 1997 (wave 1). We define the head of the household to be the male among the couple. By construction, the number of family links to parental households is always two conditional on such a family link existing. By construction, the number of children of the couple inside and outside the household are identical for the head and the spouse. Adult children are defined to be at least 17 years of age.

Table 2: Descriptive Evidence on Enrolment Rates

Couple Headed Households

Mean, standard errors in parentheses clustered by village

	Secondary School Enrolment Rates (children aged 11 to 16)							Primary School Enrolment Rates (children aged 6 to 10)			
	<u>Eligibles, by Village Type</u>			<u>Eligibles, by Family Link Type</u>				<u>Eligibles, by Family Link Type</u>			
	Control	Treatment	All Households	Connected	Isolated	Connected	Isolated	Connected	Isolated	Connected	Isolated
	October 1997	October 1997	Difference in Difference	October 1997	October 1997	Difference in Difference	Difference in Difference	October 1997	October 1997	Difference in Difference	Difference in Difference
All children	.651	.654	.069***	.653	.654	.083***	.001	.927	.890	.013	-.011
	(.016)	(.012)	(.016)	(.011)	(.015)	(.017)	(.031)	(.005)	(.013)	(.012)	(.021)
Boys	.681	.685	.043**	.681	.698	.044*	.040	.932	.887	.020	-.001
	(.017)	(.013)	(.022)	(.012)	(.016)	(.023)	(.047)	(.006)	(.014)	(.015)	(.031)
Girls	.603	.612	.102***	.608	.610	.131***	-.030	.925	.901	.005	-.015
	(.018)	(.014)	(.022)	(.012)	(.019)	(.024)	(.044)	(.007)	(.012)	(.015)	(.026)

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The sample is restricted to couple headed households that can be tracked over the first and third *Progres*a waves. A household's secondary school enrolment rate is defined to be the fraction of children aged 11 to 16 resident in the household that are full-time enrolled in secondary school at the time of the survey. A household's primary school enrolment rate is defined to be the fraction of children aged 6 to 10 resident in the household that are full-time enrolled in primary school at the time of the survey. In the left hand panel, the difference in difference is defined to be the difference in enrolment rates between households in treatment and control villages in November 1999, minus the corresponding difference at baseline in October 1997. In the centre and right hand panels, this difference in difference is reported for connected and isolated households separately. Standard errors on the differences are derived from an OLS regression, estimated on eligible and non-eligibles separately, of school enrolment rates on a dummy equal to one for *Progres*a villages and zero otherwise. Standard errors are clustered by village.

Table 3: Probability of an Extended Family Link

Couple Headed Households

Mean, standard errors in parentheses clustered by village

	Any Family Link (Connected)	Any Family Link of the Head	Any Family Link of the Spouse	Intra-generational Family Links			Inter-generational Family Links				
				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 (.011)	.693 (.012)	.550 (.014)	.506 (.013)	.351 (.015)	.338 (.014)	.306 (.013)	.150 (.009)	.077 (.006)	.169 (.008)	.108 (.007)
Control	.800 (.017)	.682 (.020)	.541 (.023)	.503 (.023)	.364 (.026)	.348 (.026)	.314 (.026)	.149 (.014)	.079 (.009)	.163 (.011)	.097 (.008)
Difference	.017 (.020)	.012 (.023)	.009 (.028)	.003 (.026)	-.013 (.030)	-.010 (.030)	-.008 (.030)	.002 (.016)	-.002 (.010)	.006 (.013)	.011 (.011)
Non-eligible Households											
Treatment	.808 (.016)	.712 (.018)	.523 (.021)	.532 (.021)	.332 (.018)	.258 (.016)	.248 (.017)	.198 (.015)	.112 (.010)	.142 (.014)	.074 (.008)
Control	.802 (.019)	.694 (.022)	.562 (.024)	.481 (.026)	.353 (.028)	.313 (.022)	.272 (.022)	.226 (.021)	.112 (.016)	.126 (.014)	.089 (.010)
Difference	.006 (.025)	.019 (.028)	-.039 (.032)	.051 (.034)	-.021 (.034)	-.055** (.027)	-.024 (.028)	-.028 (.026)	-.000 (.019)	.016 (.020)	-.015 (.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 4: Family Network Descriptives

Means, standard deviation between villages in parentheses, standard deviation within villages in brackets

Treatment Villages	Size of Global Family Network	Network Size/Number of Households in Village	Diameter	Share that are Eligible	Share With Primary School Aged Children at Baseline	Share With Secondary School Aged Children at Baseline	Average Value of Potential Transfers Households in Network are Eligible for (March 1998, pesos)
Mean	7.66	.169	2.42	.518	.480	.480	746
Standard deviation between villages	(.249)	(.153)	(1.20)	(.233)	(.150)	(.139)	(181)
Standard deviation within villages	[.153]	[.153]	[2.14]	[.259]	[.262]	[.263]	[281]
Control Villages	Size of Global Family Network	Network Size/Number of Households in Village	Diameter	Share that are Eligible	Share With Primary School Aged Children at Baseline	Share With Secondary School Aged Children at Baseline	Average Value of Potential Transfers Households in Network are Eligible for (March 1998, pesos)
Mean	7.92	.163	2.51	.525	.484	.491	756
Standard deviation between villages	(.230)	(.141)	(1.14)	(.249)	(.145)	(.139)	(188)
Standard deviation within villages	[.155]	[.155]	[2.07]	[.229]	[.252]	[.258]	[297]

Notes: The sample is restricted to households that can be tracked over the first and third *Progreso* waves. There is one observation per family network so that each network has the same weight irrespective of the number of households within it. There are 1379 family networks in treatment villages covering 10559 households. There are 817 family networks in control villages covering 6471 households. The size of the network is the number of households in the network. The diameter of the networks is the longest distance between two households that exists in a network. We define two households that are directly connected to be of distance one to each other. Primary school aged children are defined to be those aged 6 to 10 and resident in the household. Secondary school aged children are defined to be those aged 11 to 16 and resident in the household. The average value of potential transfers households in the network are eligible for, are calculated among eligible households only. The standard deviations between and within villages take account of the fact that there are an unequal number of family networks in each village.

Table 5: Extended Families and the Response to *Progresa*, Baseline Specification

Dependent Variable (Columns 1-5b): Change in Household's Secondary School Enrolment Rate (November 1999 - October 1997)

Dependent Variable (Columns 6): Household's Secondary School Enrolment Rate at Baseline (October 1997)

OLS regression estimates, standard errors are clustered by village

	All Children			Boys	Girls	Boys	Girls	All Children
	Standard	Family Links	Interactions	Interactions	Interactions	High Adult Wage Villages		Secondary Enrolment
	(1)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	Rate at Baseline (October 1997) (5)
TTE	.078*** (.016)							
ITE	-.015 (.026)							
TTE [connected]		.093*** (.017)	.092*** (.018)	.055** (.025)	.126*** (.026)	.040 (.028)	.095*** (.029)	
TTE [isolated]		.009 (.032)	.006 (.032)	.041 (.048)	-.035 (.046)	.038 (.056)	-.013 (.050)	
ITE [connected]		-.016 (.028)	-.027 (.030)	.008 (.042)	-.035 (.046)	.005 (.049)	-.020 (.050)	
ITE [isolated]		-.007 (.061)	-.012 (.061)	.020 (.083)	-.006 (.077)	.056 (.089)	.019 (.080)	
Average poverty index of households in the extended family / 100								.018*** (.006)
ΔTTE		.084** (.035)	.086** (.036)	.013 (.052)	.161*** (.052)	.002 (.061)	.107* (.057)	
ΔITE		-.009 (.065)	-.015 (.065)	-.012 (.093)	-.029 (.085)	-.052 (.102)	-.039 (.091)	
Observations	6227	6227	6227	3947	3760	2844	2765	5919

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The sample is restricted to couple headed households that can be tracked over the first and third *Progresa* waves. Standard errors are clustered by village. A household's secondary school enrolment rate is defined to be the fraction of children aged 11 to 16 resident in the household that are full-time enrolled in school at the time of the survey. In Columns 2a onwards the link variable is defined to be equal to one if household h has any family links in the village, and zero otherwise. All specifications also control for the following - the husband's age, literacy, whether he speaks an indigenous language, the spouse's age, literacy, whether she speaks an indigenous language, the household poverty index, whether the household owns any land, the number of individuals in the household at baseline, the number of households in the village, the share of households in the village that are eligible, the marginality index for the village, regional fixed effects, and the village level enrolment rate at baseline among eligible and non-eligible households. In Columns 2b to 4b the effects of the following controls are also allowed to vary with eligibility status, *Progresa*, and the interaction of the two - whether the head's (spouse's) age is above or below the median among couple headed households, whether the head (spouse) is literate, whether the household owns land, whether the household size at baseline is above or below the median among couple headed households, and the village level enrolment rates at the baseline among eligible and non-eligible households. The samples in Columns 4a and 4b are restricted to villages that have above the median level of adult wages as recorded in October 1998. In Column 5 the sample is restricted to connected households that have at least one secondary school aged child at baseline (October 1997), the dependent variable is the household's secondary enrolment rate at baseline, and the same set of controls as in Column 1a is included. The household poverty index increases as the household has higher permanent income.

Table 6: Extended Families, Transfers, and the Response to *Progresa*

Dependent Variable: Change in Household's Secondary School Enrolment Rate (November 1999 - October 1997)

OLS regression estimates, standard errors are clustered by village

	Isolated	Connected	Connected Households, Varying Characteristics of the Local Family Network	
	Own Transfer	Own and Family Transfer	Minority of Family is Eligible and Has Primary School Aged Children	Majority of Family is Eligible and Has Primary School Aged Children
	(1)	(2)	(3)	(4)
Households With Primary School Aged Children				
TTE [evaluated at mean of own transfer]	-.010 (.043)			
TTE [evaluated at mean of own + family transfer]		.093*** (.021)	.133*** (.035)	.076** (.030)
Evaluated at mean of own transfer, family transfer = 0			.045 (.030)	
Evaluated at mean of own transfer, family transfer at 75th percentile				.083*** (.031)
Observations	659	3180	1622	1558
Households With No Primary School Aged Children				
TTE [evaluated at mean of own transfer]	.077 (.086)			
TTE [evaluated at mean of own + family transfer]		.119*** (.044)	.053 (.079)	.116* (.067)
Evaluated at mean of own transfer, family transfer = 0			.096 (.077)	
Evaluated at mean of own transfer, family transfer at 75th percentile				.142** (.064)
Observations	194	814	457	357

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The sample is restricted to eligible couple headed households that can be tracked over the first and third *Progresa* waves. Standard errors are clustered by village. A household's secondary school enrolment rate is defined to be the fraction of children aged 11 to 16 resident in the household that are full-time enrolled in school at the time of the survey. For the own transfer effects, the value of potential transfers to the households is evaluated at the mean of the distribution of such transfers in October 1998 among all eligible couple headed households that have secondary school aged children. In Column 2 onwards, the family refers to the local family network at degree one to the household. The value of potential transfers to the local family network that is eligible is evaluated at the mean of the distribution of such transfers in October 1998 among the local family network (excluding the household itself) of all connected eligible couple headed households that have secondary school aged children. All specifications also control for the following - the husband's age, years of schooling, literacy, whether he speaks an indigenous language, the spouse's age, years of schooling, literacy, whether she speaks an indigenous language, the household poverty index, the number of individuals in the household at baseline, the number of households in the village, the share of households in the village that are eligible, the marginality index for the village, regional fixed effects, and the village level enrolment rate at baseline among eligible and non-eligible households. In all columns the effects of the following controls are also allowed to vary with *Progresa* - whether the head's (spouse's) age is above or below the median among couple headed households, whether the head (spouse) is literate, whether the household owns land, whether the household size at baseline is above or below the median among couple headed households, and the village level enrolment rates at the baseline among eligible and non-eligible households.

Table 7: Schooling and Eligibility Status of Intra-generational Family Links

Dependent Variable: Change in Household's Secondary School Enrolment Rate (November 1999 - October 1997)

OLS regression estimates, standard errors are clustered by village

Inter-generational Family Link Type:	Brothers		Sisters		Brothers		Sisters	
	Father to Uncle	Mother to Aunt	Father to Uncle	Mother to Aunt	Father to Uncle	Mother to Aunt	Father to Uncle	Mother to Aunt
Relationship to secondary school age children in household h:	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)		
TTE [linked household is eligible]	.134*** (.038)	.106*** (.040)						
TTE [linked household is non-eligible]	-.010 (.046)	.061 (.058)						
TTE [linked household is eligible and has secondary school age children]			.096* (.052)	.139*** (.053)				
TTE [linked household is eligible and has only primary school age children]			.215*** (.075)	.183** (.085)				
TTE [linked household is non-eligible and has secondary school age children]					.008 (.056)	.040 (.078)		
TTE [linked household is non-eligible and has only primary school age children]					.106 (.176)	-.025 (.152)		
Observations	1366	1064	631	524	369	302		

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The sample is restricted to couple headed households that can be tracked over the first and third *Progres*a waves. Standard errors are clustered by village. A household's secondary school enrolment rate is defined to be the fraction of children aged 11 to 16 resident in the household that are full-time enrolled in school at the time of the survey. All specifications also control for the following - the husband's age, years of schooling, literacy, whether he speaks an indigenous language, the spouse's age, years of schooling, literacy, whether she speaks an indigenous language, the household poverty index, the number of individuals in the household at baseline, the number of households in the village, the share of households in the village that are eligible, the marginality index for the village, regional fixed effects, and the village level enrolment rate at baseline among eligible and non-eligible households.

Table 8: Eligibility Status of Inter-generational Family Links

Dependent Variable: Change in Household's Secondary School Enrolment Rate (November 1999 - October 1997)

OLS regression estimates, standard errors are clustered by village

Inter-generational Link Type: Relationship to secondary school age children in household h:	Parents to Son		Parents to Daughter		Son to Parents	Daughter to Parents
	Father to Adult Brother	Mother to Adult Sister	Father to Adult Brother	Mother to Adult Sister	Father to Paternal Grandparents	Mother to Maternal Grandparents
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
TTE [linked household is eligible]	.206*** (.045)	.173*** (.054)			.085 (.053)	.102* (.060)
TTE [linked household is non-eligible]	.078 (.077)	.088 (.120)			.001 (.037)	.077* (.046)
TTE [linked household is eligible and has primary school aged children]			.148** (.063)	.171** (.078)		
TTE [linked household is eligible and has no primary school aged children]			.269*** (.060)	.196** (.083)		
Observations	965	499	690	384	1028	649

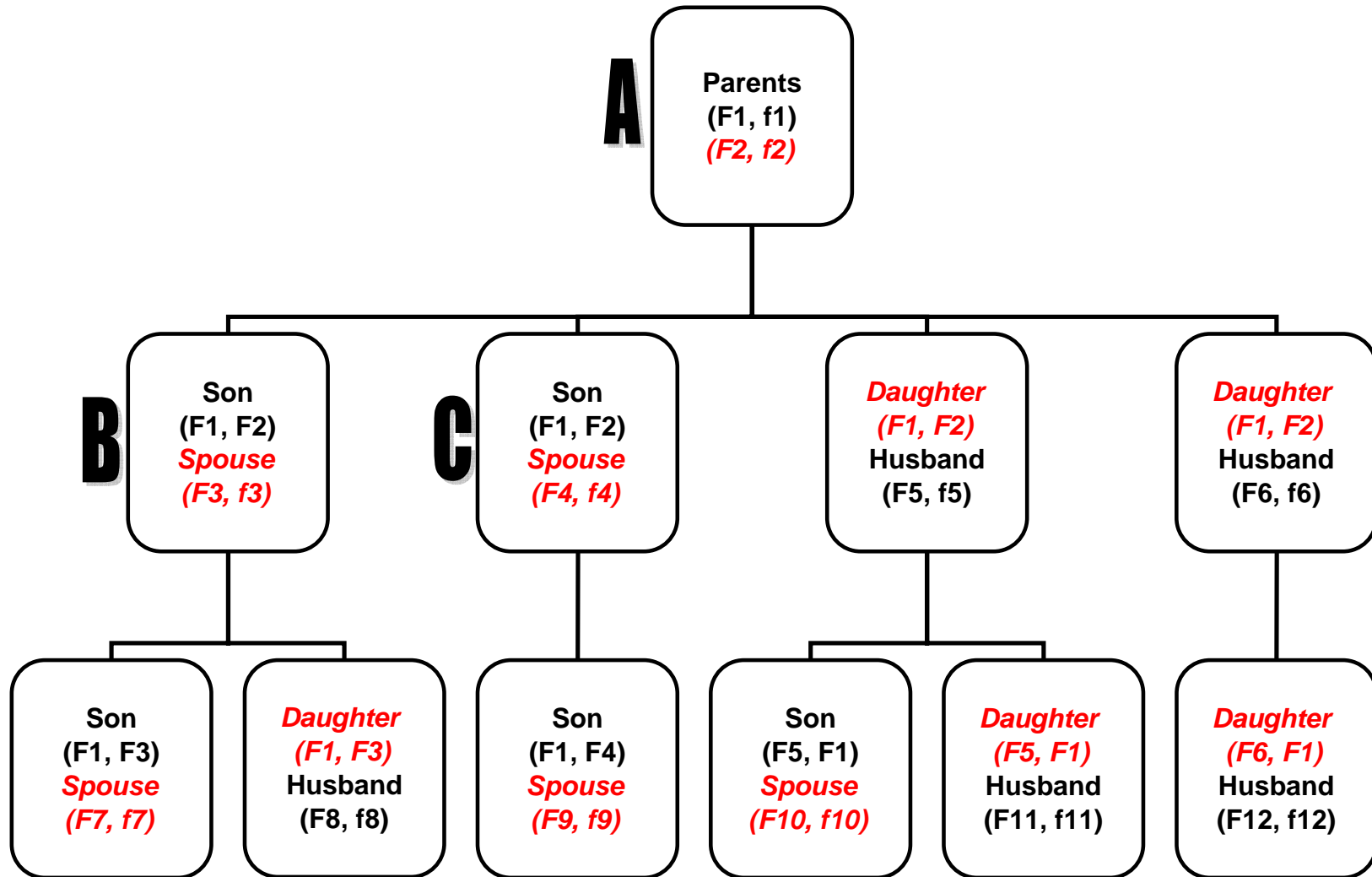
Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The sample is restricted to couple headed households that can be tracked over the first and third *Progres*a waves. Standard errors are clustered by village. A household's secondary school enrolment rate is defined to be the fraction of children aged 11 to 16 resident in the household that are full-time enrolled in school at the time of the survey. All specifications also control for the following - the husband's age, years of schooling, literacy, whether he speaks an indigenous language, the spouse's age, years of schooling, literacy, whether she speaks an indigenous language, the household poverty index, the number of individuals in the household at baseline, the number of households in the village, the share of households in the village that are eligible, the marginality index for the village, regional fixed effects, and the village level enrolment rate at baseline among eligible and non-eligible households.

Table 9: Extended Families and the Response to *Progresa* in Consumption
OLS regression estimates, standard errors are clustered by village

Dependent Variable:	Change in Weekly Consumption of Meat (Nov 1999 - Mar 1998) (1)	Proportionate Change in Non Food Expenditures (Nov 1999 - Mar 1998) (2)
TTE [connected]	.409*** (.055)	.159*** (.048)
TTE [isolated]	.312*** (.087)	.221*** (.071)
ITE [connected]	.228*** (.076)	.027 (.072)
ITE [isolated]	.298*** (.113)	.114 (.111)
Δ TTE	.097 (.084)	-.062 (.076)
Δ ITE	-.070 (.110)	-.087 (.116)
Mean (sd) of consumption variable [March 1998]		
Connected households	.867 (.867)	362 (336)
Isolated households	.922 (.901)	367 (335)
Observations	11681	10844

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The sample is restricted to couple headed households that can be tracked over the first and third *Progresa* waves. Standard errors are clustered by village. The dependent variable in Column 1 is the change in the number of days in a week the household reports consuming meat between November 1999 and March 1998. The dependent variable in Column 2 is the proportionate change in monthly non food expenditures, measured in pesos, between November 1999 and March 1998. This is defined as the change in non food expenditures over this time period, divided by the level of non food expenditures in March 1998. This does not include any schooling related expenditures. All specifications also control for the following - the husband's age, years of schooling, literacy, whether he speaks an indigenous language, the spouse's age, years of schooling, literacy, whether she speaks an indigenous language, the household poverty index, the number of individuals in the household at baseline, the number of households in the village, the share of households in the village that are eligible, the marginality index for the village, regional fixed effects, and the village level enrolment rate at baseline among eligible and non-eligible households. In all columns the effects of the following controls are also allowed to vary with eligibility status, *Progresa*, and the interaction of the two - whether the head's (spouse's) age is above or below the median among couple headed households, whether the head (spouse) is literate, whether the household owns land, and whether the household size at baseline is above or below the median among couple headed households.

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 2a: Potential Transfers in Eligible Isolated and Connected Households

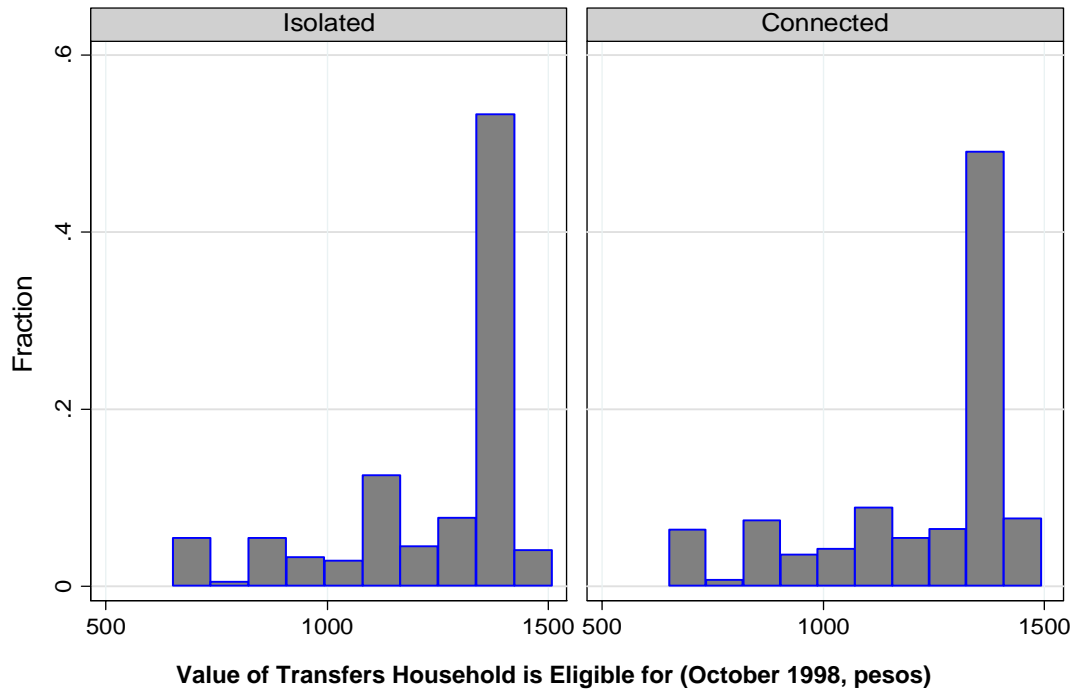
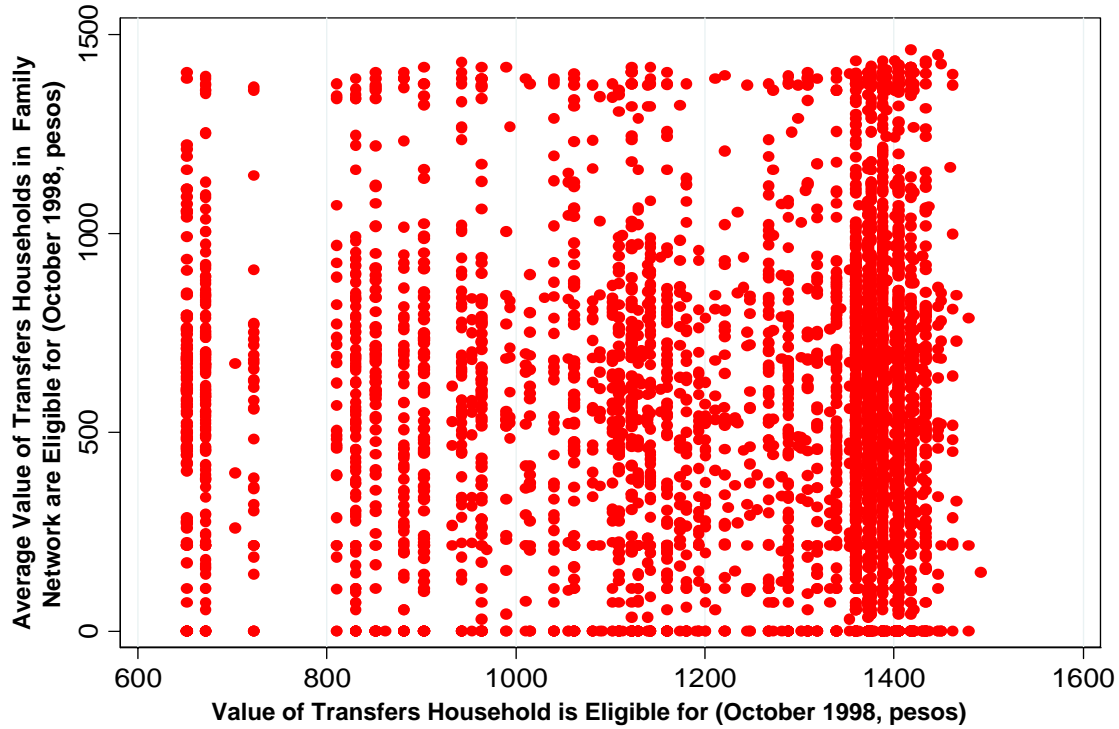


Figure 2b: Potential Transfers in Eligible Households and Their Family Network



Notes: The samples for both figures are based on eligible couple headed households that can be tracked over the first and third *Progres*a waves. Figure 2a plots the potential transfers each eligible connected household with secondary school aged children may receive, as recorded in October 1998, split for isolated and connected households. Figure 2b plots the potential transfers each eligible connected household with secondary school aged children may receive against the average potential transfer eligible households in its family network of degree 1, are entitled to, as recorded in October 1998. The correlation between these two potential sources of income for connected households is $-.01$.