Income Transfers, Marital Dissolution and Intra-Household Resource Allocation: Evidence from Rural Mexico*

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Abstract: A growing number of countries have introduced transfer programs in which funds are specifically targeted to mothers. Alternative economic models of the family suggest that such programs may or may not affect the spending patterns of families that remain intact. Some models also suggest that providing income to mothers may lead to an increase in family dissolution. I use data from the experimental evaluation of the Progresa program in Mexico to provide new evidence on the effects of targeted transfers on marital dissolution and intra-familial spending decisions. Families that were eligible for the transfer experienced a significant increase in separation rates, with most of the effect concentrated among indigenous households. The absolute size of the effect is modest (0.7 percentage points), but large relative to the underlying separation rate in the control group. To evaluate the effects on spending decisions of the families, I combine the randomized variation generated by the evaluation with income variation attributable to localized rainfall shocks. Using these two variables as instruments for the overall level of family spending and the amount of outside income received by the mother, I find strong evidence against the hypothesis of strict income-pooling, particularly among indigenous households. A review of both quantitative and qualitative evidence suggests that social norms and property rights that favor women in indigenous communities may help explain the observed differences. The results suggest that targeting transfers to mothers leads to a significant rise in the fraction of the income spent on clearly identifiable children's goods.

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1. Introduction

Conflict over resource allocation within the household is common in both less developed and developed countries, and the outcome of this process may have major welfare implications. However, the traditional unitary model of intra-household resource allocation assumes that, through consensus in the household decision-making process (Samuelson, 1956) or through the emergence of a household dictator (Becker, 1991), household allocation preferences can be summarized by a representative-individual utility function, eliminating issues of bargaining and conflict in the intra-household resource allocation process. As a response to this theory, many researchers have argued that improving women's status in the household benefits household members, especially children, and this is often advanced as an argument in favor of social policies that target women. Consistent with this argument, empirical evidence suggests that households in which women have higher levels of unearned income have children with better health conditional on total household income (Thomas, 1990; Schultz, 1990; Duflo, 2003). These tests of the income-pooling hypothesis provide evidence which is inconsistent with the unitary household model of resource allocation.

The objective of this paper is two-fold. First, I show evidence that unexpected changes in women's income affect households' marital dissolution decisions. Using experimental variation in women's non-labor income, I find that this behavior is prevalent among poor households in rural Mexico. The Progresa program, a large-scale human development program initiated by the Mexican government in 1997, provides cash transfers for marginalized households in rural areas.[1] The transfer is paid *to mothers* contingent on certain requirements in terms of children's school attendance and family-level visits to health services. Five hundred and six communities were selected to participate in an experimental evaluation of the program; the communities were randomly divided into two groups, the treatment group being phased into the program in March-April 1998 and the control group in November-December 1999.

[1] Progresa was renamed "Oportunidades" under the Fox Administration. For consistency, I will refer to the program as Progresa throughout.

This randomized design allows for clear estimation of the effect of changes in women's non-labor income on marital dissolution decisions.

Comparing treatment and control families in the experiment, I find that families that were eligible for the transfer experienced a significant increase in separation rates, with most of the effect concentrated among indigenous households. The absolute size of the effect is modest (0.7 percentage points), but large relative to the underlying separation rate in the control group. The findings are consistent with Becker et al (1977)'s model of the marriage market. This type of model, which assumes the existence of a unitary household making family structure decisions, predicts that an *unexpected* change in the income of a partner leads to a change in the perceived quality of the current match relative to other prospective matches. As a result, households may decide to dissolve if the gains to marriage become negative for the current match (Becker et al, 1977; Weiss and Willis, 1997).

However, this identification strategy does not allow for a test of the income-pooling hypothesis, since there is no transfer of income from the husband to the wife in these households. The ideal experimental design to test the income-pooling hypothesis requires the random assignment of income shares to partners in the household holding total income fixed; however, a design of this sort may be infeasible with existing data. As a second contribution of the paper, I propose a novel quasi-experimental design that uses exogenous variation in two factors that manipulate men's and women's income, which approximates this ideal design. I combine the randomized variation generated by the evaluation with income variation attributable to localized rainfall shocks. Using these two variables as instruments for the overall level of family spending and the amount of outside income received by the mother, I find strong evidence against the hypothesis of strict income-pooling, particularly among indigenous households. I show that the rainfall shocks are uncorrelated with observed time-variant and time-invariant characteristics of households, and that the distribution of total spending is not significantly affected by the combination of program treatment and the shocks. Therefore, this research design allows me to identify changes in the effective share of income in the household earned by women, while total household income is unchanged.

Using expenditure shares in children's clothing as clearly identifiable measures of children's goods, I find a 40 percent increase in these shares among intact households where women received cash transfers and suffered a rainfall shock. This is a substantial change in intra-familial spending decisions shifted towards children relative to the comparison group of households with equivalent income levels (that neither received the transfer nor suffered the rainfall shock). Similar results are found when using instrumental variables methods to control for the total expenditures effects and matching households on pre- and post- treatment expenditure levels to minimize bias due to potential income effects.

In a third main empirical result, I find that behavioral responses vary substantially between indigenous and non-indigenous households. Marital union dissolution rates increased by 1.2 percentage points (150 percent increase) over a two-year period among indigenous households, relative to the comparison group, whereas there is no significant effect among non-indigenous households. In addition, expenditure shares in children's clothing among indigenous women increased by 60 percent for intact cash transfers recipient households who suffered a rainfall shock; these differences across subgroups are substantial and statistically significant. While the available data prevent a formal test, a review of the existing ethnographic literature and the author's field work in Mexico suggests that social norms and property rights that favor women in indigenous communities may help explain these observed differences. Moreover, claims in the ethnographic literature are validated with the use of survey qualitative measures of ownership rights and decision-making patterns among indigenous and non-indigenous households in Mexico. In summary, the evidence suggests that communities with lower degrees of social (e.g. stigma) and economic costs will lead to greater renegotiation and a higher incidence of movement in the marriage market.

Finally, the study provides empirical evidence of how changing women's outside options affect both intra-household resource allocation and marriage dissolution decisions within the same population. Although researchers have used various identification strategies to test the income-pooling hypothesis using both observational data and quasi-experimental designs, these tests may suffer identification problems such as omitted variables bias and reverse causality. For example, Thomas (1990) finds a correlation between the share of women's unearned income and children's health among Brazilian households, controlling for total household income. To the extent that individual unearned incomes are a result of variation in prices and wages faced by households, or other possibly unobserved factors affecting household resource allocation decisions, this strategy may lead to omitted variables bias due to heterogeneity in tastes across households (see Lundberg et al (1997) and Behrman (1997) for detailed criticisms to this literature). Duflo (2003), on the other hand, uses potentially exogenous variation in income levels from the expansion of the South Africa pension program in the early 1990s, finding that unearned income under control of the grandmother improves granddaughters' health, whereas the income earned by grandfathers does not have any effects on children's health. However, since the household's demographic composition may have affected the likelihood of receiving a pension, and households with an eligible male were more likely to also have an eligible female (and therefore have higher pension incomes), this research design may lead to confounding of the intra-household redistribution effect from a pure household income effect.[2]

Recent papers on the Mexican Progresa program use the same evaluation but different identification strategies to study the effects of the program on intra-household allocations. They present evidence suggesting that households in which women gain an increase in unearned income spend a larger share of the budget on children's educational and clothing expenditures, and expenditure changes in the type of foods consumed (Attanasio and Lechene, 2002; Rubalcava, Teruel, and Thomas, 2003). However, these studies fail to properly account for the endogeneity of program take-up, and base their identification of the income redistribution effect on potentially invalid instrumental variables and functional form assumptions, respectively.

This study is also related to another strand of the empirical literature, which studies the causes of marital dissolution in developed countries. Various empirical studies have attempted to identify the

[2] Other studies include Lundberg, Pollak, and Wales (1997), Rubalcava and Thomas (2000), and Rangel (2004). Lundberg et al (1977) uses a 'natural experiment', a policy change in the UK that transferred a substantial child allowance to wives in the late 1970s and find that this change in household income shares lead to shifts towards greater expenditures on women's and children's clothing relative to men's clothing. However, their design is limited by the lack of a valid comparison group.

effects of unexpected changes in income on the likelihood of divorce; however, problems of identification due to omitted variables bias and reverse causality plague this literature (Becker et al, 1977; Weiss and Willis, 1997; criticisms in Charles and Stephens, 2004). The most influential evidence of this pattern is for low-income households in the United States. Groeneveld et al (1980) show experimental evidence that government transfers to households from the Seattle-Denver Income-Maintenance Experiment (SIME/DIME) increased the probability of separation of eligible whites and African-Americans, but find no effect for Mexican-American households. However, other studies of the SIME/DIME program (Cain and Wissoker, 1990; Hannan and Tuma, 1990) and evidence from other U.S. income-maintenance experiments have questioned the generalizability of this relationship (see Bishop (1980) and references therein).

These findings outlined in this paper have important policy implications. Conditional cash transfer programs are currently one of the main poverty-alleviation tools in Latin America and the Caribbean, with programs providing transfers to mothers in Brazil, Colombia, Honduras, Jamaica, and Nicaragua (Rawlings and Rubio, 2003; Maluccio and Flores, 2004). The results suggest that targeting transfers to mothers leads to a significant rise in the fraction of the income spent on clearly identifiable children's goods. However, the extent of intra-household redistribution effects may be limited to households of indigenous background, which may have similar social norms as the households studied in the Mexican context; women empowerment efforts may be futile in other contexts.[3] Additionally, although women's empowerment is one of the programs' objectives, divorce may be an unintended consequence. The theory and empirical evidence suggest that income transfers to women in union may improve their well-being in *both* married and divorced states, but may increase marital separation in the process. These in turn may affect their children's well-being upon divorce; but, this latter question cannot

[3] The percentage of indigenous individuals in various Latin American countries is, respectively: Colombia (4%); Ecuador (25%); El Salvador (1%); Guatemala (43%); Honduras (7%); Mexico (30%); Nicaragua (5%). (Central Intelligence Agency, 2004). Note that some of these figures, such as those for El Salvador and Honduras, may be underestimates of the actual indigenous population due to the stigmatization of indigenous cultures in some of these countries.

be answered given the present research design.[4] To the extent that dissolution rates caused by the program are moderate in this context, it does not lead to large disruptive effects in the population, and the extent of dissolution may well be positive for both women and children in this context.

The paper is structured as follows: Section 2 provides the theoretical framework and predictions of the efficient dissolution and intra-household allocation models, followed by their main testable implications. Section 3 gives a brief overview of the Progresa conditional cash-transfer program, the data used in the analysis, and the ethnographic evidence on social norms heterogeneity. Section 4 discusses the empirical strategy and the estimates of the program's impacts on marital dissolution. Section 5 presents the main identifying assumptions of the empirical intra-household allocation model, followed by a discussion of the evidence, and Section 6 concludes.

2. Theoretical Framework and Derivation of the Tests

Although research into the causes of marital dissolution in other social sciences usually allows for the existence of conflict within marriage as a cause of marital dissolution, neoclassical economic theory claims that dissolution is a joint optimal response of the couple to new information in the marriage (see Becker, 1991). I will first present a brief discussion of the classes of models of intra-household allocation, which will then permit a discussion of the assumptions of the efficient bargaining dissolution model. Lastly, I will show the predictions of the theory in terms of the effects of changes in the partners' income share on marital dissolution and intra-household resource allocation decisions.

2.1 Models of Intra-Household Resource Allocation[5]

Assume that households allocate resources in a union according to the unitary or consensus model of resource allocation. A household with two partners will maximize the aggregate welfare function

$$W\left[u_F(c,q;\mu,\xi),u_M(c,q;\mu,\xi)\right] \qquad (1)$$

[4] Although it is not possible to identify the effects of divorce on children's well-being in this context, Reyes (2003) finds that marital dissolution in the U.S. as a result of changes in the divorce legislation of the 1970s increased teenagers' suicide rates. In the case of poor rural Mexican households, positive or adverse effects on human capital accumulation may be substantial.

[5] This subsection draws on Rubalcava, Teruel, and Thomas (2003).

where $u_F(c,q;\mu,\xi)$ is the female partner's utility and $u_M(c,q;\mu,\xi)$ is the male partner's utility over the private consumption vector c and household public goods vector q; child expenditures can be considered a component of this public goods vector. The vectors μ and ξ respectively represent observed and unobserved heterogeneity in preferences, which affect individual utility. Households maximize aggregate welfare subject to the household budget constraint:

$$p_c^{'}c + p_q^{'}q = \sum_i \left[p_{li}(T - c_{li}) + y_i\right] + y_0 \qquad (2)$$

where p_c, p_q are price vectors of private and public consumption goods, p_{li}, c_{li} are wages and leisure of individual i ($i = M, F$); y_i is the non-labor income of individual i; y_0 is all income held jointly by household members; and T is the total time endowment of each individual.

The unitary model of the household can be stated based on two different assumptions. The first interpretation assumes that partners have equivalent preferences, so that utility functions are identical. A second interpretation assumes that there is one household member who makes all resource allocation decisions; therefore, the aggregate welfare function is equivalent to the utility of that household member, the dictator. The latter interpretation assumes that heterogeneity in preferences does not affect household resource allocation decisions. As a result, demand for household private and public goods consumption depends only on prices, total household non-labor income $y = \sum_i y_i + y_0$, and both observed and unobserved household characteristics:

$$c = c(p, y, \mu, \xi) \qquad (3)$$

$$q = q(p, y, \mu, \xi) \qquad (4)$$

Note that, according to the unitary model, changes in the partners' share of non-labor income does not affect demand functions (and therefore allocation decisions), since it does not involve changes in total household non-labor income and therefore does not affect the household budget constraint (Thomas, 1990; Schultz, 1990).

An alternative class of models of intra-household resource allocation relaxes the assumption that the household can be treated as a unitary entity, and treat individuals as the decision-making agents. A general class of these models, the collective model, takes an axiomatic approach for determining intra-household allocations (Chiappori, 1992; Browning and Chiappori, 1998). The model assumes that allocations satisfy the following conditions: (i) efficiency – the outcome of the household decision process is Pareto efficient, and (ii) uniqueness – there exists a differentiable, zero-homogeneous function $\lambda(p, y_F, y_M, \mu, \xi)$ (the distribution function) such that for any (p, y_F, y_M, μ, ξ) the vector (c, q) is a solution to the maximization of the aggregate welfare function:

$$\sum_i \lambda_i u_i(c, q; \mu, \xi) \tag{5}$$

subject to the budget constraint presented in equation (2).

Chiappori (1992) provides an alternative interpretation for the distribution function, the "sharing rule" interpretation. Under the assumption that allocations are Pareto efficient, the decision program can be converted into a two-stage process. Partners first divide total non-labor income received by the household between them, according to some predetermined but unknown sharing rule. Once income has been allocated, both members face an individual budget constraint, and choose individual allocations subject to the budget constraint based on their respective share of household non-labor income. Therefore, the income sharing rule (and individual utility) is related to the distribution function λ. Note that in the special case in which individuals have the same preferences, we are back in the conventional "unitary" framework, where households have common utility. Also, cooperative household bargaining models (Manser and Brown, 1980) are nested within the collective framework.

The collective model implies that households will have demand functions for goods as functions of prices, total non-labor income, household characteristics, and the distribution function, which is a function of the model's parameters:

$$c = c(p, y, \lambda(p, y_F, y_M, \mu, \xi), \mu, \xi) \tag{6}$$

$$q = q(p, y, \lambda(p, y_F, y_M, \mu, \xi), \mu, \xi) \tag{7}$$

In the collective model case, changes in the partners' share of non-labor income may affect demand functions (and therefore allocation decisions), since these changes may affect the household income sharing rule. A comparison of demand functions (6) and (7) with equations (3) and (4) suggests a test of the income-pooling hypothesis: under the unitary framework, changes in non-labor income of the partners which leave total household income constant should not lead to demand changes. Under the collective framework, changes in partners' income shares will lead to shifts in the sharing rule, and these will lead to changes in the allocation decision. Section 5 below discusses the specific design of the test of the income-pooling hypothesis conducted.

2.2 Model of Efficient Marital Dissolution with Children

Efficient marital dissolution theory predicts that marital dissolution is a joint optimal response of the couple to new information in the marriage; partners will choose to dissolve a union if both partners are better off in the event of divorce (see Becker, 1991). This section will briefly state the main assumptions of the efficient bargaining dissolution model, with an extension for the voluntary provision to household public goods, and show predictions of the theory as a result of changes in household income received by women and changes in partners' income shares leaving total income constant.

Let $V(p, y, \mu, \xi)$ be the expected household indirect utility function based on the unitary household model discussed in Section 2.1. In the case of dissolution, partners make allocation decisions separately, taking into account that children are public goods with respect to parents. In addition, partners' outside options also take into account expected remarriage prospects, which depend on individual characteristics, such as income. Lastly, the outside options will also take into account the individual partner's dissolution cost, which entail the legal costs of dissolution, the costs of dividing household assets, the degree of social stigma from dissolution, etc., which will depend on the exogenous parameters of the model. We can subsequently define expected indirect utility functions based on the outside options of each partner *i*, $A_i(p, y, y_F, y_M, \mu, \xi)$, which depend on the potential ex-partners' non-labor incomes.

Becker et al (1977)'s theory of marital dissolution argues that divorce will occur if and only if both partners will expect to be better off divorced. That is, unions will dissolve if and only if the following condition holds:

$$V(p, y, \mu, \xi) < A_F(p, y, y_F, y_M, \mu, \xi) + A_M(p, y, y_F, y_M, \mu, \xi) \qquad (8)$$

Underlying condition (8) are assumptions that partners can costlessly bargain over household resources, such that the joint welfare is maximized, and that utility is perfectly transferable between partners. To the extent that the female partner may be better off divorced, the male can 'bribe' her not to seek a divorce by offering her a greater share of their married output. But, he will do so only to the extent that he is still better off married given his new share (see Becker (1991) for a discussion). In summary, divorce will occur if and only if the aggregate gains from marriage are negative.

According to the model, it is the unanticipated changes in partners' characteristics that may lead to divorce; for example, an unexpected change in woman's income has an ambiguous effect on the household's dissolution decision. On one hand, this 'shock' increases the utility that the family currently receives and expects to receive in the future, or $\partial V / \partial y_i > 0$. But the shock also changes the expected utility of partners given their alternative options: $\partial A_F / \partial y_F > 0$ and $\partial A_M / \partial y_F \geq 0$, where the latter condition holds due to the public goods condition of children. Therefore, the dissolution decision will depend on the changes in magnitude of the left- and right-hand sides of condition (8) (Becker et al, 1977; Weiss and Willis, 1997).

What would happen in the case in which there is a change in partners' income shares that favors women in two-parent households? In the married state, there should be no changes in aggregate utility due to income-pooling ($V(p, y, \mu, \xi)$ remains constant). However, taking into account the partners' remarriage prospects could lead to changes in the aggregate outside options in this model. Since the female has a higher level of non-labor income, her value in the remarriage market increases, whereas the male's value in the remarriage market decreases due to his lower non-labor income. As a result, the woman's expected utility upon dissolution will increase, while the man's will decrease (note that these changes in expected utility given remarriage opportunities are not necessarily of the same magnitude). Formally, it is possible that $A_F(p, y, y_F, y_M \mu, \xi)$ and $A_M(p, y, y_F, y_M \mu, \xi)$ should change in such a way that the sum of partners' outside options increases; therefore, these income share changes could lead to changes in marital dissolution.[6] I show evidence consistent with this analysis in Section 5.

As discussed in the Section 3.3 below, quantitative and qualitative evidence suggests substantial differences in social norms and women's property rights among indigenous and non-indigenous communities. This could be taken into account by understanding how these factors affect $V(p, y, \mu, \xi)$ and $A_i(p, y, y_F, y_M, \mu, \xi)$, in both the marital dissolution and intra-household allocation models. Although I do not attempt a formal discussion of these differences in the models, I suggest two potential mechanisms. First, social stigma upon dissolution may decrease A_F. It is arguable that stigma has a direct effect on the woman's happiness upon divorce; but, it is also possible that households which face lower social costs of divorce will invest less in marital-specific capital due to the higher potential divorce threat, further reducing the gains to marriage from specialization (see Weiss (1997) for a discussion).

Second, women's property rights over household assets increases A_F while it decreases A_M. If households remain in union, the assignment of property rights to different partners should not matter, due to income-pooling. However, in the case of marital dissolution, communities which restrict women's property rights place a non-market restriction on her potential non-labor income. Moreover, their asset ownership may affect both female and males' remarriage prospects. Both characteristics, which differ across indigenous and non-indigenous groups, lead to lower gains to marriage for indigenous couples relative to non-indigenous ones. Since the probability of divorce due to unexpected changes in income

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[6] If remarriage were not an option in the model, it could be possible that changes in partners' income shares would not lead to changes in dissolution rates. A Nash equilibrium of a non-cooperative game in which parents voluntarily provide a positive amount q_i^* to the public good has a striking 'neutrality' result: redistributions of income among partners such that neither of them loses more income than his/her original contribution to the public good do not change the private and public goods allocation decisions of either individual (see Bergstrom, Blume and Varian (1986) for a thorough discussion). Therefore, the utility of both parents remains the same, and therefore, there should be no change in marital dissolution decisions.

will be largest amongst couples who already have lower gains to marriage (i.e., indigenous background households), we should expect to observe greater degrees of separation amongst this group. Moreover, since there are potential differences in the outside options across the two groups, this could lead to differences in intra-familial spending patterns, according to non-unitary household models.[7] These are presumably important factors, and are taken into account in the empirical analysis. The following section discusses the Progresa program, the data used in the study, and the degree of heterogeneity in social norms regarding household decision-marking processes and marital formation and dissolution in rural Mexico, based on a survey of ethnographic evidence and available quantitative evidence on the questions.

3. Progresa Program, Data, and Social Context

3.1 Overview of Progresa Program

In 1997, the Mexican government initiated a large-scale Education, Health, and Nutrition Program (Progresa) aimed at improving the human development among children in marginalized rural areas in Mexico. The program targets the poor in marginal rural communities, where 40 percent of the children from poor households left school after the primary level. The program provides cash transfers to the mothers of over 2.6 million children conditional on school attendance, health checks and health clinics participation, at an annual cost of approximately one billion dollars, or 0.2 percent of Mexico's GDP. The education component of Progresa consists of subsidies provided to mothers, contingent on their children's regular attendance to school. These cash transfers are available for each child attending school in grades three to nine of primary and lower secondary school, and range from $70 to $255 pesos per month, depending on the gender and grade level the child is attending (with a maximum of $625 per month per family in 1998). Overall, the program transfers are important, representing 10 percent of the average expenditures of beneficiary families in the sample.

[7] For example, the Nash cooperative household bargaining model assumes that the allocation process strictly depends on the outside options of the partners, since the objective function is of the form:
$W[u_F(c,q;\mu,\xi) - A_F(c,q;\mu,\xi), u_M(c,q;\mu,\xi) - A_M(c,q;\mu,\xi)]$.
The less restrictive collective model assumes that distribution factors affecting partners' outside options may enter the intra-household distribution function (Chiappori et al, 2002).

A distinguishing characteristic of Progresa is that it included a program evaluation component from its inception. Progresa was implemented following an experimental design in a subset of 506 communities located across seven states: Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí, and Veracruz. Among these communities, 320 were randomly assigned into a treatment group, with the remaining 186 communities serving as a control group, thus providing an opportunity to apply experimental design methods to measure its impact on various outcomes. In addition, within these selected communities, a poverty indicator was constructed using the household income data collected from the baseline survey in 1997. A discriminant analysis was then separately applied in each of the seven regions in order to identify the household characteristics that best allow them to be classified as poor and non-poor households. Eligible households were identified on the basis of this welfare index (see Skoufias et al (2001) for a more detailed description of the targeting process). While household eligibility was determined within all communities, only households below a welfare threshold and within the treatment villages became program beneficiaries.

3.2 Data

After the baseline census in October 1997, the program conducted extensive biannual interviews on the 24,000 households of the 506 communities during March 1998, October 1998, May/June 1999, and November 1999, at the time of the implementation of the experimental phase of the program. Each survey is a community-wide census containing detailed information on household demographics and household income, expenditures and consumption. The surveys in October 1997, October 1998, May/June 1999, and November 1999 (numbered in the paper as survey rounds 1, 2, 3, and 4, respectively) also collected information on the marital status of each person in the household 8 years old and older. From this data, I construct two types of marital dissolution variables: (i) an indicator variable determining whether the mother of children in the program was currently separated or divorced at each survey round [currently separated indicator], and (ii) an indicator variable determining whether the woman ever separated or divorced since the baseline period [ever separated indicator]. In addition, based on the detailed expenditures and consumption modules conducted at each round, I construct measures of total household expenditures and the share of total expenditures on children's clothing, in aggregate and by children's gender. These latter measures arguably represent expenditures on child-specific goods, and comprise an important component of (non-food) total child expenditures. To the extent that women have stronger preferences than men for children's welfare, these measures would allow us to infer that changes in partner's income shares that favor women would imply a shift in household expenditures towards female-preferred goods (see Browning and Chiappori (1998) for a discussion of identification and measurement of individual-specific goods in intra-household resource allocation models).

Since we are interested in identifying the effects of income share changes on marital dissolution and intra-household resource allocation outcomes, using the complete sample of households may confound the income effect and the conditionality effects of the program (i.e., the fact that households only received cash if children were in school). Schultz (2004) presents evidence that school enrollment rates were close to 100 percent for primary school children among both Progresa and comparison village children, and therefore the program had no impacts on primary school enrollment. Since conditionality constraints are not likely to be binding for households with primary school children, (based on this evidence) and in order to minimize the confounding with the program conditionality effects, I restrict the sample to intact eligible households with children ages 9 years and younger at baseline, who will never be old enough to attend secondary school throughout the period. I further restrict the sample to households with mothers between the ages of 16 and 55 years. These restrictions result in a sample of approximately 3,000 households.

Given the random assignment of villages to either treatment or control groups, the groups' characteristics should not be systematically imbalanced. Mean levels of the baseline observable characteristics used in the empirical analysis by treatment group, are reported; as hoped, there are no statistically significant differences in the observed characteristics of these individuals in most dimensions (Table 1, columns 1 and 2).[8]

3.3 Descriptive Statistics and Ethnographic Evidence on Social Norms

Individuals in this sample come from poor socio-economic status households, since Progresa is targeted to poor individuals in marginalized rural communities in Mexico (Table 1). Approximately half of them have not completed primary school, and currently have 2.2 children living in the household, on average (Panels A and C). Most women do not earn cash income; only 6 percent are either wage laborers or self-employed. A large share (35 percent) of the women in the sample come from an indigenous background (65 percent do not speak an indigenous language). 92 percent of the mothers in the selected sample at baseline are in the 14-35 years age group, an expected fact given that the sample selects mothers with children 9 years old and younger (Panel A).

Most male partners of these women belong to the same age group (83 percent in the 14–35 years age group), and have similar schooling attainment (Panel B). 77 percent of partners work as wage laborers. In terms of household characteristics, households spend on average 870 Mexican pesos per month, or approximately 87 USD (Panel C). In addition, expenditure shares in children's clothing represent 3.3 percent of total household expenditures, which suggests that these households are quite poor (based on the expected Engel curves). Interestingly, approximately 35 percent of partners live in an unmarried cohabiting union, a common observation in rural Mexico.

In the case of rural Mexico, it will be important to take into account the substantial degree of heterogeneity in social norms regarding household decision-marking processes and marital formation and dissolution across ethnic groups. A body of ethnographic evidence suggests that variation in gender inequality and 'power' between indigenous and non-indigenous communities is evident. Whereas in the latter, the role of *machismo* enforces highly unequal gender disparities within the household, social scientists have found a striking lack of machismo in traditional indigenous cultures (Chiñas, 1992;

[8] Behrman and Todd (1999) conduct an exhaustive analysis of the degree of success of the random assignment of villages in the Progresa Program, and conclude that the randomization was successful.

Elmendorf, 1972; Wolf, 1959, 1964).[9] Others such as Mindek (2003a) argue however that greater degrees of gender inequality in non-indigenous households may not be a reality in these communities. In her observations in a Mixtec village in Southern Puebla, Mindek finds women are very active in negotiations over household decisions, and many control a substantial amount of partners and children's earned incomes; she argues that apparent gender differences may in part be a result of differences in ideological discourses across ethnic communities.

One striking difference is that indigenous communities tend to have less restrictive marital norms. Trial marriages are a prevalent phenomenon among Nahua households in the state of Puebla; for instance, the stability of the marriage depends on the early economic and social well-being of the relationship (Carpena-Méndez, 2004; personal communication). Based on a survey of ethnographic literature, Mindek (2003b) remarks that most dissolutions are in the form of separations rather than official divorces. In addition, there exists substantial variation in the frequency of dissolution among indigenous groups, with Mixtecs, Zapotecs, Nahuas, and others having high dissolution rates whereas the Otomíes, Triquis, and Tzotziles have somewhat lower dissolution rates. For example, according to the sources reviewed by Mindek (2003b), Mixtecs as well as Zapotecs suffer especially high dissolution rates due to the high incidence of arranged marriages in their communities.

Norms of family support for women and their children in the event of dissolution are similar across ethnic groups in Mexico, however. For example, Chiñas (1992) comments that, upon marital dissolution, Zapotec women in the Isthmus of Tehuantepec (Guerrero) keep custody over children and are expected to go back to their parents or siblings' household. Most indigenous groups surveyed by Mindek (2003b) have the custom that parents of one gender retain custody over children of the opposite gender (i.e., mothers take care of sons, and fathers take care of daughters), except young children, who always remain under the custody of the mother irrespective of their gender.

[9] A formal definition of *machismo* refers to "a sense of exaggerated masculinity or a cult of virility whose chief characteristics are extreme "aggressiveness and intransigence in male-to-male interpersonal relationships and arrogance and sexual aggression in male-to-female relationship" (Stevens, 1972, p.315)" (Elmendorf, 1977, p.141).

Finally, Robichaux (1995, 1997) argues that patterns of land inheritance include female participation in his study of a Nahua village in the state of Tlaxcala, and throughout indigenous communities in Mesoamerica, based on a survey of ethnographic evidence: "in the case studied and seemingly throughout most of Mesoamerica land is usually divided equally among all males, with some female participation in the inheritance. It appears that no ethnic group in Mesoamerica espouses the principle of impartible inheritance" (Robichaux, 1997). Although there is still no consensus among ethnologists on some of these dimensions, the bottom line is that evidence is consistent with a higher status within the household and lower social (e.g. stigma) and economic costs for women upon marital dissolution among indigenous ethnic groups in Mexico, leading to greater outside options.

Turning to the available quantitative evidence on the question, indigenous households have lower average socio-economic status than non-indigenous households (Table 1, columns 3 and 4). 62 percent of indigenous mothers in a union at baseline have not completed primary schooling, whereas only 47 percent of the non-indigenous have not done so (this difference is statistically significant at 95 percent confidence). Furthermore, indigenous households tend to be worse-off in terms of household income and wealth. Although they work larger agricultural plots (1.6 vs. 1.2 hectares), mean household expenditures in indigenous women households are lower than in non-indigenous women households by 140 pesos per month (15 percent; significant at 95 percent). Moreover, expenditure shares on children's clothing are 0.4 percentage points smaller in indigenous households (significant at 95 percent confidence). Given these socio-economic and cultural differences, the indigenous versus non-indigenous distinction will be prominent in the empirical work.

Subsequently, I use qualitative survey data to ascertain (to the extent possible) whether the ethnographic evidence is representative of the sample under study (Table 2). Women responded to a series of questions regarding which household member (i.e., husband, wife, or joint-decision) usually makes certain types of decisions concerning various household activities (for the exact questions, see Table 2). Additionally, women responded to a series of questions on who is entitled to own livestock and household plots, based on similar options as above. I constructed measures of female decision-making power and rights to own property; specifically, I constructed indicator variables determining whether a certain decision was made jointly, or mainly by the woman.

Since we are interested in the partial difference in the set of beliefs or ideology of indigenous relative to non-indigenous women, I run a set of regressions where the dependent variables are the indicator variables discussed above, on a set of explanatory variables, which include an indigenous woman indicator, and women, partner, and household controls. Coefficient estimates on the indigenous woman indicator are reported in Table 2. Interestingly, I do *not* find an association between the woman's indigenous background and the extent of woman or joint decisions with respect to children's school participation and use of health services, expenditures of wife's "extra money", etc., which would suggest that there is no greater extent of gender equality among indigenous households. However, women of indigenous background are 4.8 percentage points (10 percent) and 4.2 percentage points (12 percent) more likely to have joint or single ownership rights of livestock and household plots, respectively. This latter piece of evidence suggests differences in indigenous women's economic options outside of a union, consistent with the ethnographic evidence discussed above. This evidence will be exploited in the empirical strategy, as will be discussed in the following sections.

4. Program Impacts on Separation Decisions of Households In-Union at Baseline

4.1 Identification Strategy

First, I show evidence that unexpected changes in women's income affect households' marital dissolution decisions. Using the experimental variation in women's non-labor income from the Progresa cash transfers, I find that this behavior is prevalent among poor households in rural Mexico. The random assignment of communities to treatment and comparison groups allows us to interpret mean differences in outcomes as causal effects of the program.

As discussed in Section 2.2, condition (8) for marital dissolution predicts that unexpected changes in women's non-labor income could lead to changes in marital dissolution. I first estimate the following reduced form model:

$$D_{ict} = \pi_0 + \pi_1 T_{ct}^{PR} + X_{ic}\pi_2 + P_t\pi_3 + \varepsilon_{ict} \quad (9)$$

where D_{ict} is an indicator variable for whether couple *i* has separated or not in village *c* at time *t*; T_{ct}^{PR} is the Progresa treatment village indicator variable; X_{ic} is a set of baseline woman, partner, and household (including detailed demographic) controls; P_t is a vector of time controls; and ε_{ict} is a disturbance term, which is allowed to be correlated across households at the community level. In this specification, π_1 represents the intent-to-treat program impact on the household's marital dissolution decision. This is the main coefficient of interest; according to the theoretical discussion, π_1 could be positive or negative since unexpected changes in individual non-labor income of household members could increase or decrease the probability of dissolution. Equation (9) can be easily expanded to account for time-varying effects and heterogeneous average effects for different sub-groups, which allows us to test for differences in the behavioral responses depending on female attributes, such as their ethnic background in our case. This will be an important decomposition as suggested by the discussion of ethnic differences in Section 3.3.

We are also interested in recovering the female income-separation elasticity among these households. A parametric model of the marital dissolution equation can be specified as follows:

$$D_{ict} = \alpha + \theta_1 y_{ict}^{PR} + X_{ic}\beta + P_t\delta + \varepsilon_{ict} \quad (10)$$

where y_{ict}^{PR} is the amount of cash transfer received by the woman as a result of the program; and the rest of the variables are defined as above. In this specification, θ_1 represents the average effect of women's unearned income on the household's marital dissolution decision. This is another coefficient of interest, since it represents the average female income-separation elasticity among these households. However, since the program take-up decision may be correlated with unobserved factors influencing marital dissolution, OLS estimation may lead to biased estimates of the true relationship between women's cash transfer income and dissolution rates. Therefore, I use the Progresa treatment village indicator variable (T_{ct}^{PR}) as an instrumental variable (IV) for the cash transfer, and also estimate the following first-stage equation:

$$y_{ict}^{PR} = \pi_{01} + \pi_{11} T_{ct}^{PR} + X_{ic} \pi_{31} + P_t \pi_{41} + \upsilon_{1,ict} \qquad (11)$$

Under the IV conditions of (i) robust partial correlations between the instrumental variable and the endogenous regressor $(\pi_{11} \neq 0)$, and (ii) lack of correlation between the IVs and the disturbance term in equation (10) $\left(E\left[T_{ct}^{PR} \varepsilon_{ict}\right] = 0\right)$, 2SLS estimation is a consistent estimator of parameter θ_1. Condition (i) can be tested in the data, and results will be discussed in Section 4.2. Condition (ii) is not directly testable and is maintained as an assumption of the model; however, the random assignment of the program across villages should ensure that this condition holds (see discussion in Section 3.2).

4.2 Program Impacts Estimates

In this section, I present evidence of the program's impact on the household's probability of separation two-years after the start of the program. I start the discussion with a graphical analysis, since it will shed light on the patterns in the data. Figure 1 presents a series of graphs representing the time pattern of separation rates for women in union at baseline across treatment groups. These are presented using the two alternative separation measures (currently separated and ever separated women), and stratified by the women's indigenous background. Overall current separation rates increase over time for treatment group households and remain greater than comparison group rates throughout the evaluation period; the two-year separation rate is approximately 0.9 percentage points larger for the treatment group. In addition, a classification by women's indigenous background shows great heterogeneity in the separation effects: separation rates for indigenous women households consistently increase over time, reaching a difference of 1.4 percentage points after two years, whereas for non-indigenous women the two-year separation rate is 0.70 percentage points. Figures based on the alternative measure of ever-separated rates imply even larger differences across indigenous groups (2.0 and -0.2 two-year separation rates).

Using the complete sample of households (with children ages 9 years and younger at baseline), the main reduced-form effects from cash transfer eligibility, conditional on rainfall shock effects, imply a 0.73 percentage point increase in current marital dissolution rates (Table 6, regression 1). Given mean separation rates of approximately 0.66 percentage points, this constitutes an increase in separation rates of roughly 110 percent. There are significantly differential effects on dissolution by women's indigenous background (Table 6, regression 2), with indigenous women showing substantial increases (1.17 percentage points, statistically significant at 95 percent confidence), while there is essentially no effect for non-indigenous women (0.49 percentage points, not statistically significant). This result is consistent with the claims that indigenous households have, on average, lower gains to marriage than non-indigenous households; therefore, unexpected income shocks are more likely to lead to dissolution.

There are somewhat larger effects on marital dissolution for households with primary or higher schooled women than for lower educated women (Table 6, regressions 2-3, although not statistically significant) for both indigenous and non-indigenous women households. It appears that the gains to marriage are smaller in households with higher-educated women; however, the theory is ambiguous with respect to the effects of women's education on the gains to marriage.

Since remarriage prospects are arguably higher for younger women, and these households have invested less in marital-specific capital, we should expect gains to marriage to be smaller among this group, and therefore higher dissolution rates given the shock. Although there is also no statistically significant differential program effect among women of different age groups (women ages 14-25 versus ages 26-55) (regressions 4-5), there are once again negative point estimates on the interaction terms with the treatment indicator for both indigenous and non-indigenous women (differences across age groups of 1.31 and 0.95 percentage points, respectively; both significant at 16 percent significance level). The effect by age group for the pooled sample implies the same pattern: a 1.23 percentage point increase for women ages 14-25 years (statistically significant at 99 percent confidence; not reported in the table), and a 0.19 percentage points estimate for women in the 26-55 years age group (statistically insignificant; not reported in the table); in this case, the difference of 1.04 percentage points is significant at 90 percent confidence (not reported in the table). Interestingly, the estimates suggest that the marital dissolution effects are substantial for particular subgroups: indigenous women in young ages, and with relatively high schooling, who experience separation rates of approximately 1.9 percentage points over this two-year period (coefficients are significant at 95 percent confidence).

To further check the robustness of the results, I use the ever-separated indicator as dependent variable, which does not take into account possible reconciliation effects (and is a common measure used in other empirical studies of marital dissolution). Intent-to-treat program estimates, although imprecisely estimated, imply a cumulative 0.51 percentage point increase in marital dissolution after two-years (regression 7). Effects by indigenous background are even starker: indigenous women ever-separation rate effects are 1.59 percentage points (statistically significant at 99 percent confidence), a massive increase of 136 percent in marital dissolution rates (regression 8). On the other hand, non-indigenous women ever-separated rates are -0.08 percentage points and insignificantly different from zero; their difference is statistically significant at 95 percent confidence.

5. Test of Unitary Household Model and Intra-Household Allocation Effects

The previous identification strategy does not allow for a test of the income-pooling hypothesis, since there is no transfer of income from the husband to the wife in these households. The ideal experimental design to test the income-pooling hypotheses would be the random assignment of income shares to partners in the household holding total income fixed; however, a design of this sort may be infeasible given existing data. In this paper, I propose a quasi-experimental design that uses exogenous variation in two factors that manipulate women's and men's income such that we observe variation in income shares of household members holding total income fixed. Using rainfall variation, which affects the agricultural production of poor households in rural Mexico, and increases in women's unearned income as a result of the experimentally-designed conditional cash transfer program, I show that average total household income does not vary in these two groups of matched households, but the income share under the woman's control is (arguably) exogenously increased as a result of the cash transfer.

The empirical strategy is divided in three subsections. First, I discuss the empirical implementation of the unitary household model test, and the assumptions necessary for econometric identification. In summary, the research design relies on the validity of the exclusion restrictions of rainfall shocks and village treatment indicators from the second stage equation, and on the invariability of total household income given these exogenous income changes. Therefore, I present evidence that supports the use of rainfall variation as an instrumental variable in the subsequent analysis, and show that the use of both rainfall variation and the experimental variation in income transfers from the Progresa program satisfy the condition that total household income remains fixed.

5.1 Econometric Specification of the Test

As discussed in Section 2.1, demand equations (3) and (4) derived from the unitary household model predict that changes in partner's non-labor incomes that favor women, keeping total household income constant, should not lead to changes in intra-household resource allocation decisions. A parametric model of the dissolution decision and intra-household demand equations can be specified as follows:

$$D_{ict} = \alpha + \tilde{\theta}_1 y_{ict}^{PR} + \beta_1 Y_{ict} + X_{ic}\beta_2 + P_t\delta + \varepsilon_{ict} \qquad (10')$$

where D_{ict} is the outcome of interest (e.g., marital dissolution, intra-household resource allocation) for household *i* in village *c* at time *t*; Y_{ict} are total household expenditures; and $y_{ict}^{PR}, X_{ic}, P_t, \varepsilon_{ict}$ are as defined above. In this specification, $\tilde{\theta}_1$ represents the average effect of women's unearned income on the household's marital dissolution or intra-household allocation decision, conditional on the total household budget. According to the theoretical discussion, $\tilde{\theta}_1$ should be equal to zero in the demand equation, since, conditional on total household income, individual non-labor income of household members should not affect dissolution.

However, since program take-up and total consumption are endogenous variables in equation (10'), I use the Progresa treatment village indicator variable (T_{ct}^{PR}) and a *household-level* rainfall shock indicator (R_{ict}) as instrumental variables for the former explanatory variables, and estimate the following set of first-stage equations:

$$y_{ict}^{PR} = \pi_{01} + \pi_{11} T_{ct}^{PR} + \pi_{21} R_{ict} + X_{ic}\pi_{31} + P_t \pi_{41} + \upsilon_{1,ict} \qquad (11')$$

$$Y_{ict} = \pi_{02} + \pi_{12} T_{ct}^{PR} + \pi_{22} R_{ict} + X_{ic}\pi_{32} + P_t \pi_{42} + \upsilon_{2,ict} \qquad (11'')$$

Under IV conditions (i') $(\pi_{11} \neq 0, \pi_{21} \neq 0, \pi_{12} \neq 0, \pi_{22} \neq 0)$, and (ii') $(E[T_{ct}^{PR}\varepsilon_{ict}] = E[R_{ict}\varepsilon_{ict}] = 0)$, 2SLS estimation provides consistent estimates of the parameters $\tilde{\theta}_1$ and β_1. Again, condition (i) can be tested in the data (see Sections 5.4 and 5.5), and I present evidence below that suggests that condition (ii) holds for both instruments (see Section 5.2).

An additional concern for the estimation may be the mis-specification of structural equation $(10')$. If, for example, the relationship between marital dissolution and total household expenditures is non-linear, a linear control for total household expenditures could lead to bias. Therefore, I also present results that allow for non-linear relationships between income and marital dissolution, and include interactions of the rainfall shock with total (baseline) household agricultural land used as other instrumental variables. Also, since we are using the complete sample of households who either suffer or do not suffer rainfall shocks in both treatment groups to estimate equation $(10')$, I am implicitly making the assumption that local average effects of women's income are constant among shock and no-shock households. As a result, tests based on equation $(10')$ may be biased to the extent that these estimates are capturing an income effect for the no-shock group. Therefore, I propose an alternative identification strategy which is robust to this concern.

This second approach employs a matching method in order to control properly for changes in overall household income. I restrict the sample to household in two groups: Progresa treatment village households who suffered a rainfall shock in the past six months before the survey ("double-treatments"), and Progresa control village households who have not suffered a rainfall shock ("double-controls"). As is discussed in Section 5.3 below, these two shocks have the effect of leaving the distribution of total household expenditures roughly unchanged, but increase women's income share in the "double-treatment" households. Estimation of equation $(10')$ for this sub-sample of households by 2SLS, using the $R_{ict}T_{ct}^{PR}$ interaction as an IV for y_{ict}^{PR}, provides alternative estimates of the women's income shares effect on household marital decisions. In this case, the Y_{ict} term would not be necessary in the specification, given the quasi-experimental nature of the design; however, results are robust to the inclusion of this term in the model. As a further robustness check, I implement a semi-parametric matching estimator, where I match "double-treatment" and "double-control" households on observable levels of pre-treatment and post-treatment household expenditure levels and indigenous background and regression-adjust for remaining differences in controls (Rubin, 1979). The results are robust to both instrumental variables and matching estimator methods.

Note that in the empirical test in terms of intra-household allocations, I will estimate post-treatment equation $(10')$ for the sub-sample of households who remain in union only. Therefore, there is a potential degree of selection bias in this estimation, due to the marital dissolution decision. To the extent that unions that would experience the largest [smallest] changes in income share allocations are more likely to dissolve, it would lead to a downward [upward] bias in $\tilde{\theta}_1$. Therefore, I take this potential source of bias into consideration by estimating Lee (2002) treatment effect bounds, due to this quasi-experimentally driven non-random selection.

An issue in the empirical analysis is the extent of sample attrition. If being out-of-sample is correlated with the likelihood of receiving treatment, then this could lead to bias in the coefficient estimates. Sample attrition rates through the four survey rounds are approximately 10 percent for the samples of women in union at baseline (Table A1, regression 1). Although attrition rates are balanced across treatment groups, the likelihood of attrition is highly correlated with individuals' observable characteristics (regressions 2-4). Therefore, to reduce the extent of potential attrition bias, I control for baseline women, partner, and household's characteristics in all specifications.

5.2 Validity of Rainfall Shock as Instrumental Variable

Although the assignment to treatment and control villages is uncorrelated with unobserved household characteristics due to the random assignment of villages, it remains theoretically possible that the rainfall shocks do not satisfy the exclusion restriction. In other words, rainfall shocks could be correlated with unobservable characteristics of the household that determine partners' marital dissolution decisions or their intra-household resource allocation. In that case, IV condition (ii) $\left(E\left[R_{ict}\varepsilon_{ict}\right]=0\right)$ may not hold and 2SLS estimates of parameters $\tilde{\theta}_1$ and β_1 will not be consistent. Although this assumption cannot be directly tested, I show evidence that mean baseline characteristics of households in the sample do not differ systematically among those which ever received a rainfall shock in Periods 2-4 and those that do not, and those who received a rainfall shock strictly in Period 4 (see Table 3). Only differences in mean expenditure shares in children's clothing and the proportion of cohabiting couples are statistically significant (at 95 percent confidence levels) among these groups. Therefore, these rainfall shocks groups look comparable at baseline.

In addition, I test whether pre-rainfall shock trends in the dependent variables of interest differ among these groups, by estimating the following model:

$$D_{ic,t-k} - D_{ic,t-k-1} = \pi_1 + \pi_2 R_{ict} + \upsilon_{ic,t-k} - \upsilon_{ic,t-k-1} \qquad (12)$$

where $x_{ic,t-k} - x_{ic,t-k-1}$ is the difference in the outcome variable k periods before the oncoming of the rainfall shock in period t. Due to the unavailability of multiple pre-program survey periods, I can only test for first-differences in the period 2 – period 1 outcome variables for rainfall shocks in periods 3 and 4 jointly (Table 4, Panel A), period 2 – period 1 differences for rainfall shocks in period 3 (Panel B), and period 2 – period 1 and period 3 – period 2 differences for rainfall shocks in period 4 (Panel C). Hypotheses tests on pre-shock differential trends $(\pi_2 = 0)$ fail to reject that differences are significantly different from zero at 95 percent confidence levels, except for current separation rates in one period. In summary, these tests show that observable baseline and time-variant characteristics of households are not correlated with the oncoming of a rainfall shock, and this provides further confidence that 2SLS estimates are meaningful.

5.3 Instrumental Variables Combination Maintains Total Household Expenditures Fixed

In this section, I present evidence that the negative impact of rainfall shocks and the positive impact of the randomly-assigned income transfers from the Progresa program cancel out and satisfy the condition that total household income remains fixed. However, a caveat of the study is that the exact household-level rainfall shock-induced reduction in income cannot be observed; therefore, we must use statistical methods to estimate mean and other distributional impacts of the shock on household income. We use two different methods to address this concern, using the complete sample and the restricted sample of 'double-treatment' and 'double-control' households (defined in Section 5.1), which are analogous to the two identification strategies proposed in Section 5.1.

Using the complete sample of women, I first estimate the following equation:

$$Y_{ict} = \alpha + \theta_1 y_{ict}^{PR} + \theta_2 R_{ict} + X_{ic}\beta + P_t^{'}\delta + \eta_{ict} \qquad (13)$$

where all variables are defined as above, and η_{ict}, the disturbance term, is allowed to be correlated within villages. Since y_{ict}^{PR} may suffer from omitted variables and endogeneity biases in this equation, I estimate it by 2SLS, using T_{ct}^{PR}, the treatment village indicator, as an IV for the endogenous regressor. This model allows for the estimation of mean differences in total household expenditures from both exogenous shocks relative to households who suffer no shocks using the complete sample of households. Estimated mean differences in income are represented by:

$$E[Y_{ict} \mid y_{ict}^{PR} = \bar{y}_{ict}^{PR}, R_{ict} = 1] - E[Y_{ict} \mid y_{ict}^{PR} = 0, R_{ict} = 0] = \theta_1 \bar{y}_{ict}^{PR} + \theta_2 \qquad (14)$$

Coefficient estimates and estimated mean differences in household income from both exogenous shocks are reported in Table 5 for all post-treatment periods pooled, and individually for Period 4. Estimates of equation (13) imply mean differences in household expenditures of 2.02 pesos (standard error 36.16) per month on average throughout Periods 2-4, and -3.33 (standard error 39.41) pesos per month on average during Period 4 (Panel B); we cannot reject that these differences are significantly different from zero at conservative confidence levels. Allowing for heterogeneity in the effect of the shocks by indigenous/non-indigenous ethnicity status, we fail to reject that the change in income is zero (Panel B, columns 2 and 4), and moreover, fail to reject that these differences are significantly different between ethnic groups.[10] It is evident from these estimates that mean differences in total household income are insignificantly different from zero given these exogenous shocks.

We can compare the distributions of total household income between the "double-treatments" and "double-control" subgroups as an alternative test based on the matching identification strategy. Figure 2 shows density estimates of total household expenditures by treatment-rainfall shock combination group, for all post-treatment periods pooled and for Period 4 (Figure 2, Panels A and B). Inspection of the density estimates suggests a substantial increase in total household expenditures on average, but a small increase in total household expenditures in Period 4. In addition, we conduct equality of means and standard deviations tests of the household expenditure distributions, and Kolmogorov-Smirnoff tests of equality of distributions, and consistently fail to reject significant differences in the distributions and its first two central moments (reported in Figure 2).[11] In combination, these results provide suggestive evidence that, although these two exogenous shocks arguably change income levels of particular household members, the distribution of total household expenditures is not affected overall. Therefore, we can treat these instruments as shifting partners' household income shares while maintaining total household income fixed. Given the validity of these tests, I now turn to the main tests of the model.

5.4 Income Shares Effects on Households' Separation Decisions

In this section, I present evidence of the impacts of women's non-labor income on the household's probability of separation two-years after the start of the program. Since the previous analysis does not allow us to disentangle the income effect from the partner income share changes as a result of the income transfers, we make use of the econometric techniques discussed above to do so.

Estimates of the causal effect of women's non-labor income on marital dissolution (structural parameter $\tilde{\theta}_1$), conditional on equivalent income levels of the household (structural parameter β_1), based

[10] Mean difference in income changes between indigenous and non-indigenous groups are 72.76 (standard error 62.92) for periods 2-4 pooled, and 70.22 (standard error 74.45) for period 4.

[11] I conduct Levene (1960)'s robust test for equality of variances, which relaxes the assumption of normality of the distributions.

on the instrumental variables framework discussed above are presented. Although the IV-2SLS regression is imprecisely estimated, it suggests that an increase in income under control of women of 100 pesos leads to an increase in dissolution rates of 0.58 percentage points, or approximately 90 percent (Table 7, regression 1). Moreover, the point estimates suggest substantial differences in the behavioral response of households depending on women's indigenous background; mean indigenous households' responses of 1.06 percentage point increases relative to the 0.30 percentage point increases of non-indigenous women households, although neither significantly different from zero, suggest substantial heterogeneity in the responses (regression 2).

Alternative models, which relax the restriction of a linear income effect, give similar results; the analogous IV-2SLS estimate of parameter $\widetilde{\theta}_1$ is 0.70 percentage points (regression 3). In these, I include a quadratic term on total household expenditures and use as additional IVs the interaction of the flood indicator with total agricultural land used at baseline and its quadratic, relaxing the restriction of linear-additive rainfall shock effects on household expenditures in equation $(11'')$. Alternative estimates using the ever-separated indicator as dependent variable, estimates imply a cumulative 0.73 percentage point increase in marital dissolution after two-years (regression 5). Effects by indigenous background are analogous to the reduced form results: indigenous women ever-separation rate effects are 2.22 percentage points (statistically significant at 90 percent confidence), an enormous increase of 190 percent in marital dissolution rates (regression 6), whereas non-indigenous women ever-separated rates are -0.08 percentage points and insignificantly different from zero. The mean effect difference across the two groups is statistically significant at 95 percent confidence.[12]

Why are there differences between indigenous and non-indigenous women in their response to the program? Note that indigenous women receive similar amounts of cash transfers, on average; the

[12] I report linear probability model estimates because, since I have a zero number of indigenous households separating among the indigenous, it is impossible to estimate probit or logit regressions allowing for heterogeneity by indigenous background. However, probit marginal effects estimates are very similar to linear probability model estimates; for example, the main currently-separated probit marginal effect estimate is 0.620 percentage points (standard error 0.246, significant at 99 percent confidence). Other results are available from the author upon request.

coefficients on the income transfers first-stage regressions have similar magnitudes for all women and indigenous women.[13] Therefore, it should be that, (i) either the transfers represent a larger share of the women's income for the former, or (ii) there are differences in the relative costs of dissolution between the two groups, as discussed above. To test whether the difference is driven by the former mechanism, I estimate regression $(10'')$ using the share of women's non-labor income $\left(y_{ict}^{PR}/Y_{ict}\right)$ instead of the level as explanatory variable. Women's non-labor income share effect estimates for indigenous and non-indigenous women households are 10.1 and 7.5 (standard errors 3.6 and 3.5, respectively; not reported in the table); although the difference is insignificant. Since the increase in women's non-labor income shares for these two subgroups is 13.2 percent and 10.5 percent, these imply total differential effects of 1.33 and 0.79 percentage points, respectively. This evidence suggests that, although the former hypothesis might be influential, the differential effects by indigenous group are quite large, although statistically insignificant.

Total expenditure levels effects on dissolution decisions are consistently positive and of similar magnitude to those of the women's non-labor income effects. Main estimates imply that households with total expenditure levels 100 pesos higher are 0.53 percentage points more likely to separate, although this relationship is imprecisely estimated as well.

Weak instruments are not a main concern in the estimation. F-test statistics of the significance of the rainfall shock indicator and of the joint significance of all instruments in equation $(11')$ are 7.66 and 7.93, respectively (both significant at 99 percent confidence) (Table 7, column 1). In the heterogeneous women non-labor income effects by indigenous background specification, which uses the interaction of the treatment village indicator and a non-indigenous woman indicator as another IV, the F-test statistic of joint significance of the IVs in the total household expenditures equation is 5.46 (rejects lack of correlation at 99 percent confidence) (column 2). Tests of robustness of IVs in equation (8) are robust to

[13] Mean monthly income transfers to indigenous and non-indigenous women households are 68.2 pesos and 64.7 pesos, respectively. The mean difference of 3.6 pesos (standard error 4.66) is statistically insignificant at standard confidence levels.

the use of alternative specifications (columns 3-6), and analogous tests for the income transfers first-stage equation (equation (7)) are very robust, clearly due to Progresa's experimental design.[14]

In summary, this evidence is consistent with the hypothesis that changes in outside option of women relative to men that lead to efficient dissolution, and these differences are significantly different depending on women's inherent social and economic outside options, as implied by the evidence for household of differing ethnic background.

5.5 Income Shares Effects on Children's Clothing Expenditures of Intact Households

In this section, I present evidence of the effects of women's non-labor income on intra-household resource allocation of households who remain intact as a result of the change in income shares. I again start the discussion with a graphical analysis, since it will shed light on the patterns in the data. Figure 3 presents a series of graphs representing the differences across Progresa treatment groups over time in expenditure shares in children's clothing, overall and by gender, for households in union at baseline. These are shown for all households, and also stratified by the women's indigenous background. Expenditure shares on children's clothing are equivalent across treatment and control groups pre-treatment (Period 1 in figures), and we can observe an increase over time for treatment group households relative to control group households throughout the evaluation period; the two-year difference reaches 0.53 percentage points, or 17 percent. In this case, stratification by women's indigenous background shows less heterogeneity. Mean differences in children's clothing expenditure shares are similar during periods 2 and 3 across these households; differences appear to be substantial only during period 4 (0.29 percentage points, or a 9 percent difference). Differences by children's gender suggest a very similar pattern (Panels A and B).

IV estimates of the causal effect of women's non-labor income on expenditure share on children's clothing ($\tilde{\theta}_1$), overall and by children's gender, are reported in Table 8. The main effect implies that a

[14] The F-statistic (2,478) of the significance of rainfall shocks and the Progresa treatment village indicator in the income transfers first-stage regressions (specification 1) is 447.9 (significant at 99 percent confidence). The individual F-statistics for each IV are 3.88 (significant at 95 percent confidence) and 883.41 (significant at 99 percent confidence), respectively.

100 pesos shift in non-labor income towards women results in an increase in children's clothing expenditure shares of 1.63 percentage points (statistically significant at 99 percent confidence; Table 8, regression 1). Moreover, the point estimates suggest substantial differences in the households' behavioral responses depending on women's indigenous background; mean indigenous households' responses of a 2.14 percentage point increase (significant at 99 percent confidence) relative to a 1.35 percentage point increases (statistically insignificant) per 100 pesos shift in income of non-indigenous women households (regression 2).

As further robustness checks, I estimate analogous models to those presented for separation decisions, which relax the restriction of a linear income effect (adding a quadratic term on total household expenditures and use as additional IVs the interaction of the flood indicator with total agricultural land used at baseline). These give similar results; the analogous IV-2SLS estimate of parameter $\tilde{\theta}_1$ is 1.26 percentage points (regression 3). However, the effects by indigenous background are not as robust; the point estimates suggest a 0.89 percentage point and 1.50 percentage points increase per 100 pesos income shift for indigenous and non-indigenous background women households (although both statistically insignificant).

However, a concern in these specifications is the robustness of the instruments; the flood indicator variable and its interaction with agricultural land used at baseline are weakly correlated with household expenditures throughout the three post-periods; the F-statistics of the partial correlations with HH expenditures are at most marginally significant (see Table 8); this could lead to bias of the 2SLS estimates towards OLS. To address this concern, I make the strong assumption of exogeneity of total household expenditures, and assume all instruments affect children's clothing expenditures by shifting women's cash transfer income only. The resulting estimates suggest a stronger effect for indigenous women households (1.24 percentage points; significant at 99 percent confidence) than for non-indigenous women households (0.79 percentage points, statistically insignificant) (regression 5). Similar estimates which assume a more flexible (cubic) exogenous relationship of household expenditures give equivalent estimates (1.22 percentage points, significant at 99 percent confidence, and 0.79 percentage points, statistically insignificant; not reported in the table).

Another potential concern might be that, since indigenous households are poorer on average and the amount of transfer is similar among the two groups, the transfer reflects a larger shift in income shares for the former group, and due to this we observe larger effects for indigenous women households. To address this, I estimate equations $(10'-11'')$ allowing for differential effects of household expenditures by indigenous group, and use the flood indicator and non-indigenous woman interaction as an additional instrumental variable. The point estimates of the income shift effects are substantially larger: 2.53 percentage points for indigenous women households and 1.25 percentage points for non-indigenous households (although both insignificant; not reported in the table). Note that in this case, the flood shock and its interaction are quite weak instruments in the household expenditures regressions, therefore, we do not emphasize these results.

Finally, we decompose effects of the income share changes on child clothing shares by children's gender. The effects are equivalent for girls' and boys' clothing: a 100 pesos shift in non-labor income towards women results in an increase of 0.83 percentage points (56 percent) for the former and 0.76 percentage points (46 percent) for the latter (both significant at 95 percent confidence; regressions 6 and 8). Point estimates of the differences by indigenous background suggest equivalent patterns as discussed above, although the coefficients are again insignificant.

5.6 Matching Estimates of Separation Decisions and Intra-Household Allocations

Estimates based on the alternative identification strategy, which uses the restricted matched (or complete) sample of "double-treatment" and "double-control" households and uses the $R_{ict}T_{ct}^{PR}$ interaction as an IV for y_{ict}^{PR}, give similar results, but the restricted sample size for the complete (1171 observations) and matched (291 observations) samples limit the power of hypotheses tests. Estimates using the complete sample give similar results as the Rubin (1979) semi-parametric matching estimates, and I focus on the latter. IV-2SLS estimates of the main women non-labor income effect imply an effect of 0.55 percentage points (not statistically significant) increase in current separation rates from 100 pesos increase in income, and a two-year ever-separated effect of 2.05 percentage points (significant at 90 percent confidence) (Table 9, regressions 1 and 3). Moreover, although the current separation rates estimates by indigenous background suggest substantially larger effects for non-indigenous women (0.96 percentage points increase versus a 0.14 percentage point decrease for indigenous women; regression 2), the evidence from the ever-separated effect is consistent with results for the complete sample: 2.61 percentage points per 100 pesos increase for indigenous women, and a -0.87 decrease in ever-separated rates for non-indigenous women households (regression 4).

Effects on the intra-household allocation decisions of intact households are quite robust. Matching estimates imply a 0.91 percentage point increase in children's clothing expenditure shares (significant at 95 percent confidence; regression 5). The Lee (2002) bounds, which take into account selective marital dissolution, range from 0.97 to 0.72 percentage points (not reported in the table). In this case, these bounds are quite tight around the point estimate, which indicates that the selection due to marital dissolution do not lead to substantial amounts of bias. Differential effects for indigenous women households are 1.92 percentage points (54 percent; significant at 95 percent confidence), whereas the estimate of 0.39 percentage points for non-indigenous women households is insignificant at standard confidence levels; their difference is significant at 90 percent confidence (regression 6).

Effects on clothing expenditure shares by children's gender suggest slightly different patterns; effects on girls' clothing are significant, but do not vary by indigenous women's background; the point estimate of the main girls' clothing effect implies a 0.57 percentage point increase per 100 pesos, or a 33 percent increase (significant at 95 percent confidence; regressions 7). On the other hand, there are large and significant differences in expenditure shares in boys' clothing across indigenous and non-indigenous households (regressions 9-10). Although the main effect of 0.34 percentage points is not statistically significant, the effects for indigenous background households of a 74 percent increase in the expenditure share (point estimate is 1.35 percentage points; significant at 99 percent confidence) is significantly different at 99 percent confidence from the effect for non-indigenous households (-0.18 percentage points and statistically insignificant). Moreover, non-parametric Lee (2002) bounds on the boys' clothing share effect estimates range from 1.19 to 1.54 percentage points per 100 pesos for indigenous households, and from -0.21 to -0.18 for non-indigenous households (not reported in the table). In summary, these alternative estimates are consistent with estimates using the IV methods for the complete sample of households.

6. Conclusion

This paper contributes to the empirical literature on marital dissolution and intra-household resource allocation by showing evidence of the effects of unexpected changes in income on partners' marital dissolution decisions, and on how conditional cash transfer programs of this sort affect intra-household resource allocation decisions. Additionally, I show that behavioral responses are substantially larger in indigenous households than in non-indigenous households, possibly due to differences in social norms and property rights that favor women in indigenous communities.

These results have important policy implications: targeting transfers to mothers leads to a significant rise in the fraction of the income spent on clearly identifiable children's goods. These results suggest that transfer programs for patriarchal households with no targeted recipient could be less effective in improving children's well-being. For example, in the special case that transfers to households do not lead to changes in partners' income shares, changes in allocations to child goods would be strictly the result of income effects, and could be substantially smaller than those from female-targeted transfers.

In addition, these results imply that social programs that intend to improve women's status within the household should take into account existing environments as a point of departure in their design. As discussed above, conditional cash transfer programs similar to Progresa are employed as one of the main poverty-alleviation tools in Latin America and the Caribbean, with very positive results in terms of children's human development. However, the extent of intra-household redistribution may be limited to households in certain social contexts or among certain ethnic groups, which are more responsive with respect to household bargaining. In addition, responses in terms of marital dissolution may be either beneficial or detrimental to both women and their children in these contexts; examining this remains an important extension to consider for future research.

In the indigenous background dimension, future work should focus attention in assessing to what extent the norms discussed are common across indigenous groups in Central and South America. Evidence of these differences is not only existent across Mexico. Hawkins (1984), for example, presents a comparative case study of non-indigenous (Ladino) and indigenous Maya households in Northwestern Guatemala, and finds that the extent of household norms and negotiation and marital dissolution vary substantially across these groups. This type of research would improve our understanding of household responses programs that promote women empowerment. Moreover, this type of interdisciplinary approach to household economics is fruitful in terms of both enriching the theory and allowing for improved empirical tests, as evidenced in work by Duflo and Udry (2003) and Luke and Munshi (2004).

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Table 1: Mothers, Partners, and Household Baseline Characteristics, October 1997

	All Women		**All Women**		**Indigenous Women**		**Non-Indigenous Women**	
	Progresa	Control	Indigenous	Non-indig	Progresa	Control	Progresa	Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Mother's characteristics								
Mother's Age 14-25	0.54	0.55	0.55	0.54	0.54	0.57	0.54	0.54
Mother's Age 26-35	0.39	0.37	0.37	0.39	0.37	0.35	0.40	0.38
Mother's Age 36-45	0.06	0.06	0.07	0.05	0.07	0.07	0.05	0.05
Mother's Age 46-55	0.01	0.02	0.01	0.02	0.01	0.02	0.02	0.02
Mother <Primary Schooling	0.52	0.53	**0.62**	**0.47**	0.63	0.62	0.47	0.48
Non-indigenous woman	0.65	0.65	-	-	-	-	-	-
Wage laborer	0.04	0.03	0.03	0.04	0.03	0.03	0.04	0.03
Self-employed	0.03	0.02	0.03	0.02	0.03	0.04	**0.03**	**0.01**
Panel B: Partner's characteristics								
Partner's Age 14-25	0.29	0.30	0.31	0.29	0.29	0.35	0.30	0.27
Partner's Age 26-35	0.56	0.52	0.52	0.56	**0.56**	**0.46**	0.56	0.56
Partner's Age 36-45	0.11	0.13	0.12	0.11	0.11	0.13	0.11	0.12
Partner's Age 46-55	0.03	0.04	0.03	0.03	**0.03**	**0.05**	0.03	0.03
Partner's Age 56-65	0.01	0.01	0.01	0.01	0.01	0.01	**0.00**	**0.02**
Partner <Primary Schooling	0.51	0.54	0.56	0.50	0.54	0.58	0.50	0.51
Non-indigenous partner	0.63	0.64	**0.04**	**0.96**	0.04	0.04	0.96	0.96
Wage laborer	0.74	0.79	0.78	0.75	0.77	0.79	**0.72**	**0.78**
Self-employed	0.11	0.09	0.11	0.10	0.12	0.11	0.10	0.09
Agricultural worker	0.06	0.06	**0.03**	**0.08**	0.03	0.04	0.08	0.07
Non-wage laborer	**0.05**	**0.03**	0.05	0.04	0.07	0.03	0.05	0.03
Panel C: Household characteristics								
Total HH Expenditures*	870.0	871.1	**780.2**	**919.6**	807.6	734.4	904.2	944.6
Exp. Share in Girls' Clothing*	1.59	1.60	**1.47**	**1.66**	1.52	1.40	1.64	1.71
Exp. Share in Boys' Clothing*	1.74	1.60	**1.52**	**1.78**	1.57	1.41	1.83	1.69
Exp. Share in Children's Clothing*	3.34	3.20	**3.00**	**3.43**	3.10	2.82	3.47	3.38
Cohabiting couple	0.33	0.36	0.38	0.33	0.36	0.41	0.32	0.33
Dirt floor	0.63	0.65	**0.83**	**0.53**	0.80	0.87	0.53	0.53
Own house	0.90	0.90	**0.94**	**0.87**	0.94	0.96	0.88	0.87
Total Agricultural Land-Hectares	1.31	1.44	**1.56**	**1.25**	1.43	1.77	1.24	1.26
Number of boys ages 0-5 years	0.68	0.66	**0.71**	**0.65**	0.74	0.68	0.65	0.65
Number of boys ages 6-7 years	0.26	0.25	0.25	0.26	0.25	0.24	0.26	0.26
Number of boys ages 8-9 years	**0.19**	**0.16**	0.18	0.18	0.19	0.16	**0.19**	**0.16**
Number of girls ages 0-5 years	0.65	0.68	**0.71**	**0.63**	0.72	0.70	0.61	0.66
Number of girls ages 6-7 years	0.25	0.25	0.26	0.25	0.27	0.23	0.23	0.26
Number of girls ages 8-9 years	0.15	0.16	0.16	0.15	0.16	0.17	0.14	0.16

Notes: Mean of characteristics by groups are presented. Figures in bold represent statistically significant differences at least at 95 percent confidence levels; robust standard errors; disturbance terms are allowed to be correlated within villages. The sample is composed of 3486 women, of which 1230 are indigenous and 2256 non-indigenous. Baseline data from the October 1997 survey, except for variables marked with an *, which are unavailable in the October 1997 survey and were measured in the March 1998 survey.

Table 2: Baseline Differences in Decision-Making Patterns across Indigenous and Non-indigenous Women

	Coefficient estimate on Indigenous woman indicator (s.e.) (1) OLS	Mean of dependent variable (2)
Dependent variable: Proportion who respond **should have joint or woman's ownership of following goods**		
Livestock	0.048^{+}	0.469
	(0.025)	
Household plots	0.042^{*}	0.341
	(0.021)	
Dependent variable: Proportion who respond **joint decision-making or woman's decision**		
To send a sick child to the doctor?	-0.011	0.906
	(0.014)	
A child has to go to school when he/she does not want to?	-0.014	0.898
	(0.015)	
How to spend wife's extra money?	0.010	0.820
	(0.020)	
About the household's important expenditures?	0.017	0.693
	(0.023)	
About expenditures for children's clothing?	-0.002	0.758
	(0.021)	
Women's characteristics	Yes	
State indicator variables	Yes	
Partner's characteristics	Yes	
Household characteristics	Yes	

Notes: Each coefficient estimate is from a separate regression. OLS coefficient estimates are presented. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; statistically significant at (+) 10 percent, (*) 5 percent, and (**) 1 percent significance levels. Women's characteristics are mother age group indicators, completed less than primary school indicator, self-employed indicator and wage laborer indicators; partner characteristics are age group indicators, partner completed less than primary school indicator, indigenous partner indicator, and occupation type indicators; household characteristics are a cohabitation indicator, dirt floor, house owned, and number of children by age group and gender variables. Control variables are measured at baseline; decision-making variables are measured at start of program (March 1998). Sample sizes vary between 12,474 and 12,542 observations, due to non-response.

Table 3: Baseline Observable Characteristics of Households with and without Rainfall Shocks

	Household ever affected by flood in Periods 2-4				Household affected by flood in Period 4			
	Ever Flood	No Flood	Difference	S.E.	Flood	No Flood	Difference	S.E.
Panel A: Mother's characteristics								
Mother's Age 14-25	0.51	0.54	-0.032	(0.027)	0.50	0.53	-0.032	-0.032
Mother's Age 26-35	0.40	0.38	0.018	(0.029)	0.40	0.39	0.008	(0.036)
Mother's Age 36-45	0.07	0.06	0.012	(0.014)	0.08	0.06	0.019	(0.018)
Mother's Age 46-55	0.02	0.02	0.002	(0.008)	0.02	0.02	0.004	(0.011)
Mother <Primary Schooling	0.54	0.52	0.018	(0.031)	0.54	0.52	0.016	(0.035)
Non-indigenous woman	0.63	0.65	-0.014	(0.042)	0.61	0.65	-0.037	(0.053)
Wage laborer	0.04	0.03	0.002	(0.011)	0.04	0.03	0.010	(0.015)
Self-employed	0.01	0.03	-0.015^{+}	(0.009)	0.01	0.03	-0.014	(0.011)
Panel B: Partner's characteristics								
Partner's Age 14-25	0.28	0.305	-0.016	(0.027)	0.27	0.29	-0.013	(0.031)
Partner's Age 26-35	0.55	0.55	-0.001	(0.029)	0.53	0.55	-0.024	(0.037)
Partner's Age 36-45	0.13	0.11	0.017	(0.017)	0.14	0.12	0.021	(0.022)
Partner's Age 46-55	0.02	0.03	-0.007	(0.008)	0.04	0.03	0.004	(0.011)
Partner's Age 56-65	0.02	0.01	0.009	(0.008)	0.02	0.01	0.013	(0.011)
Partner <Primary Schooling	0.52	0.53	-0.012	(0.033)	0.51	0.53	-0.014	(0.038)
Non-indigenous partner	0.63	0.63	0.000	(0.045)	0.62	0.64	-0.016	(0.054)
Wage laborer	0.77	0.76	0.018	(0.030)	0.77	0.75	0.018	(0.032)
Self-employed	0.10	0.10	-0.004	(0.018)	0.11	0.10	0.011	(0.022)
Agricultural worker	0.05	0.06	-0.012	(0.019)	0.04	0.07	-0.031*	(0.015)
Non-wage laborer	0.04	0.04	0.000	(0.014)	0.05	0.04	0.008	(0.018)
Panel C: Household characteristics								
Total HH Expenditures	871.7	870.5	1.201	(42.203)	872.0	883.6	-11.54	(54.08)
Share of Exp. in Girls' Clothing	1.42	1.62	-0.192	(0.118)	1.38	1.64	-0.254^{+}	(0.145)
Share of Exp. in Boys' Clothing	1.46	1.73	-0.265^{+}	(0.138)	1.46	1.75	-0.281^{+}	(0.169)
Share of Exp. in Children's Clothing	2.89	3.34	-0.453*	(0.206)	2.85	3.38	-0.530*	(0.251)
Treatment village	0.67	0.62	0.054	(0.045)	0.67	0.61	0.061	(0.055)
Cohabiting couple	0.43	0.33	0.096**	(0.028)	0.44	0.33	0.112**	(0.036)
Dirt floor	0.64	0.64	0.001	(0.034)	0.67	0.64	0.030	(0.038)
Own house	0.89	0.90	-0.005	(0.018)	0.88	0.91	-0.026	(0.020)
Total Agricultural Land	1.34	1.36	-0.026	(0.148)	1.24	1.42	-0.177	(0.174)
Number of boys ages 0-5 years	0.73	0.71	0.019	(0.073)	0.86	0.71	0.150^{+}	(0.086)
Number of boys ages 6-7 years	0.22	0.25	-0.032	(0.040)	0.23	0.25	-0.018	(0.043)
Number of boys ages 8-9 years	0.24	0.17	0.065^{+}	(0.036)	0.26	0.18	0.081^{+}	(0.043)
Number of girls ages 0-5 years	0.68	0.72	-0.045	(0.090)	0.56	0.71	-0.158^{+}	(0.083)
Number of girls ages 6-7 years	0.21	0.27	-0.062	(0.039)	0.17	0.28	-0.108**	(0.037)
Number of girls ages 8-9 years	0.14	0.18	-0.023	(0.032)	0.16	0.17	-0.011	(0.035)

Notes: Mean baseline pre-rainfall shock variables (in levels) and coefficient estimates from OLS regresions of the baseline (period 1) pre-shock difference on the period t shock are presented. Robust standard errors in parentheses; statistically significant at (+) 90%, (*) 95%, and (**) 99% confidence levels. No controls are included in the regressions. Sample of all women with children 9 years old or younger at baseline.

<u>Table 4:</u> Pre-Rainfall Shock Trends in Dependent Variables

	Flood	No Flood	Difference	Std. Error	N
<u>Panel A:</u> Flood in Periods 3 or 4					
Δ HH Expenditures	-188.9	-188.6	-0.33	(54.45)	3373
Δ Exp. Share in Girls' Clothing	-0.248	-0.45	0.21	(0.16)	2983
Δ Exp. Share in Boys' Clothing	-0.20	-0.44	0.25	(0.17)	2990
Δ Exp. Share in Children's Clothing	-0.42	-0.91	0.50^{+}	(0.27)	2971
Δ Currently separated woman	0.0041	0.0038	0.0003	(0.0042)	3173
<u>Panel B:</u> Flood in Period 3					
Δ HH Expenditures	-105.6	-199.0	93.4	(115.2)	3008
Δ Exp. Share in Girls' Clothing	-0.23	-0.47	0.25	(0.50)	2717
Δ Exp. Share in Boys' Clothing	0.67	-0.44	1.1	(0.83)	2724
Δ Exp. Share in Children's Clothing	0.44	-0.93	1.38	(1.09)	2708
Δ Currently separated woman	0.0000	0.0032	-0.0032**	(0.0011)	2850
<u>Panel C:</u> Flood in Period 4					
Δ HH Expenditures	-44.0	-70.7	26.7	(32.7)	5787
Δ Exp. Share in Girls' Clothing	0.13	0.02	0.11	(0.11)	5390
Δ Exp. Share in Boys' Clothing	0.11	0.05	0.06	(0.12)	5397
Δ Exp. Share in Children's Clothing	0.25	0.07	0.18	(0.18)	5378
Δ Currently separated woman	0.0070	0.0036	0.0034	(0.0040)	5387

<u>Notes:</u> Mean pre-rainfall shock changes in variables and coefficient estimates from OLS regresions of the pre-shock difference (in period t-p; p>0) on the period t shock are presented. Robust standard errors in parentheses; statistically significant at (+) 90%, (*) 95%, and (**) 99% confidence levels. No controls are included in the regressions. Sample of all women with children 9 years old or younger at baseline.

Table 5: Differences in Total Expenditures from Cash Transfers and Rainfall Shocks

Panel A: Regression Coefficient Estimates

	Dependent variable: Total household expenditures			
	(1)	(2)	(3)	(4)
	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS
	Periods 2-4	Periods 2-4	Period 4	Period 4
Actual transfer amount	0.864**	1.125*	0.975**	1.021*
	(0.298)	(0.448)	(0.337)	(0.478)
Actual transfer amount * Non-indigenous indicator		-0.415		-0.073
		(0.571)		(0.658)
Flood indicator	-61.561*	-35.832	-85.564**	-50.783
	(29.856)	(37.776)	(30.256)	(37.038)
Flood indicator * Non-indigenous indicator		(39.876)		(56.288)
		(52.348)		(55.022)
Non-indigenous indicator	75.118*	99.796*	44.804	55.854
	(31.040)	(44.374)	(34.780)	(58.885)
Controls	Yes	Yes	Yes	Yes
Observations	8903	8903	2859	2859
R-squared	0.06	0.06	0.07	0.07
Mean of dependent variable	713.528	713.528	681.708	681.708

Notes: Panel A presents coefficient estimates from IV-2SLS regressions. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; statistically significant at (+) 90%; (*) 95%; (**) 99% confidence levels, respectively. Controls include indicators for mother's and partner's age group (26-35 years, 36-45 years, 46-55 years); indicator variables for none or less than primary schooling, indigenous language indicator for both women and their partners; wage laborer and self-employed indicators for women; wage laborer, self-employed and agricultural worker indicators for partners; cohabitation status, total household agricultural land,, having a dirt floor, owning the residence; demographic controls include number of children by gender and age group categories (0-5 years, 6-7 years, 8-9 years). Instrumental variables for actual transfer amount and its interaction with the non-indigenous indicator are a treatment village indicator and its interaction with the non-indigenous indicator. Sample in the regressions are women with children 9 years old or younger at baseline.

Panel B: Tests of Estimated Mean Differences in Total Expenditures from Transfer and Rainfall Shock Combination

	(1)	(2)	(3)	(4)
	Periods 2-4	Periods 2-4	Period 4	Period 4
All Women				
Mean transfer amount received | Progresa $\left(\bar{y}_{ict}^{PR}\right)$	73.62		84.38	
Estimated Mean Difference in Total Expenditures	**2.02**		**-3.33**	
	(36.16)		**(39.41)**	
Indigenous Women				
Mean transfer amount received | Progresa $\left(\bar{y}_{ict}^{PR}\right)$		75.13		89.43
Estimated Mean Difference in Total Expenditures		**48.70**		**40.53**
		(43.31)		**(56.71)**
Non-indigenous Women				
Mean transfer amount received | Progresa $\left(\bar{y}_{ict}^{PR}\right)$		72.79		81.62
Estimated Mean Difference in Total Expenditures		**-24.04**		**-29.69**
		(48.96)		**(51.19)**

Notes: Panel B reports tests of mean changes in total household expenditures as a result of the cash transfer and the rainfall shock. The estimate is a linear test of the following: $E[Y_{ict} \mid y_{ict}^{PR} = \bar{y}_{ict}^{PR}, R_{ict} = 1] - E[Y_{ict} \mid y_{ict}^{PR} = 0, R_{ict} = 0] = \theta_1 \bar{y}_{ict}^{PR} + \theta_2$. Robust standard errors of the estimates in parentheses.

Table 6: Program Impacts on Separation Rates, Mothers with Children 9 Years and Younger

	Dependent variables:							
	Currently separated indicator						Ever separated indicator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Women	All Women	Indigenous	Non-indigenous	Indigenous	Non-indigenous	All Women	All Women
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treatment indicator	0.0073**	0.0117*	0.0190*	0.0059	0.0188*	0.0088*	0.0051	0.0159**
	(0.0027)	(0.0045)	(0.0096)	(0.0049)	(0.0082)	(0.0039)	(0.0038)	(0.0061)
Treatment * Non-indigenous woman		-0.0068						-0.0167*
		(0.0056)						(0.0080)
Treatment * Woman's schooling < Primary			-0.0104	-0.0034				
			(0.0113)	(0.0069)				
Treatment * Woman's Age 26-55 Years					-0.0131	-0.0095		
					(0.0093)	(0.0067)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2859	2859	1015	1844	1015	1844	2906	2906
Mean of dependent variable	0.0066	0.0066	0.0069	0.0065	0.0069	0.0065	0.0117	0.0117

Notes: Coefficient estimates from OLS regressions and are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; effects statistically significant at (+) 90 percent, (*) 95 percent, (**) 99 percent confidence levels, respectively. Controls include indicators for mother's and partner's age group (26-35 years, 36-45 years, 46-55 years); indicator variables for none or less than primary schooling, indigenous language indicator for both women and their partners; wage laborer and self-employed indicators for women; wage laborer, self-employed and agricultural worker indicators for partners; cohabitation status, total household agricultural land,, having a dirt floor, owning the residence; demographic controls include number of children by gender and age group categories (0-5 years, 6-7 years, 8-9 years). Sample in the regressions are women with children 9 years old or younger at baseline.

Table 7: Effects of Income Transfer and Rainfall Shocks on Separation Rates

	Dependent variables:						
	Currently separated indicator					Ever separated indicator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS
Actual transfer amount (100's pesos)	0.0058	0.0106	0.0070	-0.0587	0.0164^{**}	0.0073	0.0222^{+}
	(0.0073)	(0.0083)	(0.0219)	(0.3604)	(0.0062)	(0.0109)	(0.0120)
Actual transfer amount * Non-indigenous woman		-0.0076		0.1041	-0.0088		-0.0230^{*}
		(0.0087)		(0.5615)	(0.0081)		(0.0117)
Total HH Expenditures (100's pesos)	0.0001	0.0001	0.0010	0.0015	-0.0007	0.0053	-0.0004
	(0.0001)	(0.0001)	(0.0029)	(0.0072)	(0.0005)	(0.0064)	(0.0105)
(Total HH Expenditures)^2 * 100			0.0000	0.0000	0.0000		
			(0.0000)	(0.0000)	(0.0000)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2859	2859	2859	2859	2859	2906	2906
Mean of dependent variable	0.0066	0.0066	0.0066	0.0066	0.0066	0.0117	0.0117
F-statistic of Flood indicator, (and Flood * Ag.Land Interactions) in HH Exp Regression	7.66^{**}	7.59^{**}	4.22^{*}	4.18^{*}	-	5.89^{*}	5.83^{*}
F-statistic of Flood indicator and Flood * Ag.Land Interactions in (HH Exp)^2 Regression	-	-	5.40^{**}	4.18^{*}	-	-	-
F-Test statistic of Instruments in HH Exp Regression	7.93^{**}	5.46^{**}	4.37^{*}	4.36^{*}	-	6.88^{**}	4.72^{**}
F-Test statistic of Instruments in (HH Exp)^2 Regression	-	-	3.00^{*}	3.24^{*}	-	-	-

Notes: Coefficient estimates from IV-2SLS regressions are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; effects statistically significant at (+) 90 percent, (*) 95 percent, (**) 99 percent confidence levels, respectively. Controls include indicators for mother's and partner's age group (26-35 years, 36-45 years, 46-55 years); indicator variables for none or less than primary schooling, indigenous language indicator for both women and their partners; wage laborer and self-employed indicators for women; wage laborer, self-employed and agricultural worker indicators for partners; cohabitation status, total household agricultural land,, having a dirt floor, owning the residence; demographic controls include number of children by gender and age group categories (0-5 years, 6-7 years, 8-9 years). F-statistics of significance of instruments in the HH Expenditures and (HH Expenditures)2 first stage regression are reported. Sample in the regressions are women with children 9 years old or younger at baseline.

Table 8: Effects of Income Transfer and Rainfall Shocks on Expenditure Shares in Children's Clothing

	Dependent variables:								
	Expenditure Shares in Children's Clothing					Exp. Shares in Girls' Clothing		Exp. Shares in Boys' Clothing	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS
Actual transfer amount (100's pesos)	1.63^{**}	2.14^{**}	1.26^{+}	0.89	1.24^{**}	0.83^{*}	1.08^{*}	0.76^{*}	0.97^{*}
	(0.60)	(0.76)	(0.75)	(2.00)	(0.30)	(0.33)	(0.42)	(0.33)	(0.42)
Actual transfer amount * Non-indigenous woman		-0.79		0.61	-0.45		-0.40		-0.32
		(0.66)		(2.35)	(0.39)		(0.37)		(0.32)
Household Expenditures (100's pesos)	-0.82	-0.82	1.68	1.53	-0.01	0.47	-0.48	-0.32	-0.32
	(0.56)	(0.56)	(4.34)	(3.83)	(0.02)	(0.31)	(0.31)	(0.32)	(0.32)
(HH Expenditures (100's))^2 * 100			-0.01	-0.08	-0.00^{+}				
			(0.16)	(0.14)	(0.00)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8834	8834	8834	8834	8834	8838	8838	8837	8837
Mean of dependent variable	3.14	3.14	3.14	3.14	3.14	1.47	1.47	1.67	1.67
F-statistic of Flood indicator, (and Flood * Ag.Land Interactions) in HH Exp Regression	3.53^{+}	3.52^{+}	1.98	1.97	-	3.53^{+}	3.53^{+}	3.60^{+}	3.60^{+}
F-statistic of Flood indicator and Flood * Ag.Land Interactions in (HH Exp)^2 Regression	-	-	3.39^{*}	3.83^{*}	-	-	-	-	-
F-statistic of IVs in HH Exp Regression	6.02^{**}	4.17^{**}	4.30^{**}	3.33^{*}	-	6.07^{**}	4.21^{**}	6.06^{**}	4.20^{**}
F-statistic of IVs in (HH Exp)^2 Regression	-	-	3.94^{**}	3.33^{*}	-	-	-	-	-

Notes: Coefficient estimates from IV-2SLS regressions are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; effects statistically significant at (+) 90 percent, (*) 95 percent, (**) 99 percent confidence levels, respectively. Controls include indicators for mother's and partner's age group (26-35 years, 36-45 years, 46-55 years); indicator variables for none or less than primary schooling, indigenous language indicator for both women and their partners; wage laborer and self-employed indicators for women; wage laborer, self-employed and agricultural worker indicators for partners; cohabitation status, total household agricultural land,, having a dirt floor, owning the residence; demographic controls include number of children by gender and age group categories (0-5 years, 6-7 years, 8-9 years). F-statistics of significance of instruments in the HH Expenditures and (HH Expenditures)2 first stage regression are reported. Sample in the regressions are women with children 9 years old or younger at baseline.

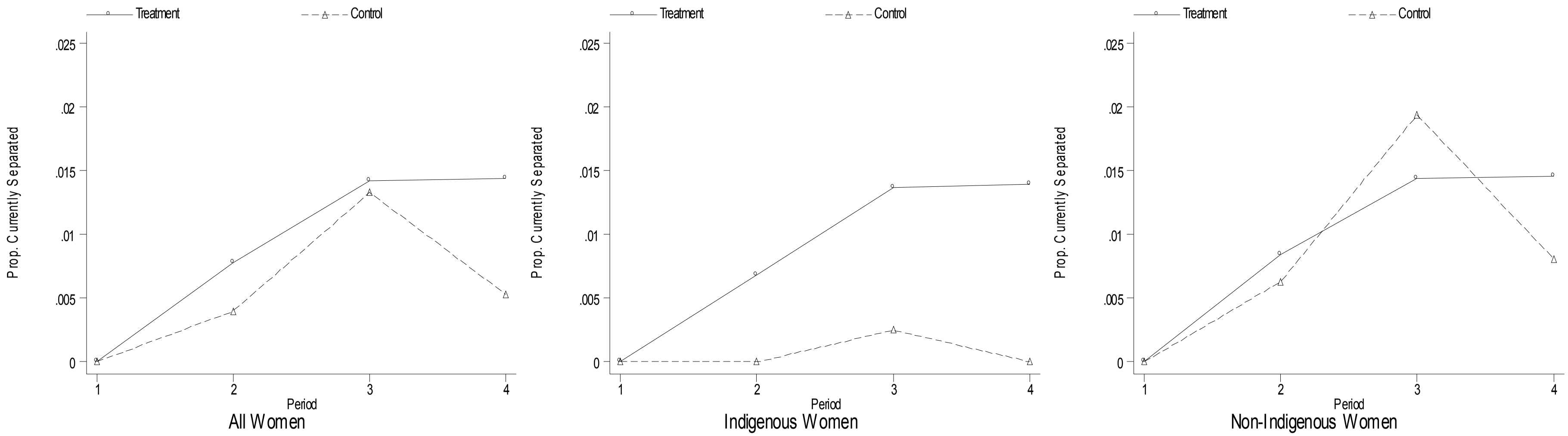
Table 9: Effect of Change in Women's Income Shares on Marital Dissolution Rates and Exp. Shares in Children's Clothing

	Dependent variables:									
	Currently separated		Ever separated		Expenditure Share in Children's clothing		Expenditure Share in Girls' clothing		Expenditure Share in Boys' clothing	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS
Actual transfer amount (100's Pesos)	0.0055	-0.0014	0.0205^{+}	0.0261	0.91^{*}	1.92^{*}	0.57^{*}	0.57	0.34	1.35^{**}
	(0.0057)	(0.0023)	(0.0116)	(0.0177)	(0.44)	(0.71)	(0.23)	(0.37)	(0.28)	(0.51)
Actual transfer amount * Non-indigenous		0.0110		-0.0087		-1.53^{+}		0.01		-1.53^{**}
		(0.0112)		(0.0166)		(0.83)		(0.47)		(0.58)
Total Household Expenditures	0.0001	0.0001	0.0001	0.0001	0.00	0.00	0.00	0.00	0.00	0.00
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Controls and Match Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	291	291	291	291	458	458	458	458	459	459
Period data	Period 4	Period 4	Period 4	Period 4	Periods 2-4	Periods 2-4	Periods 2-4	Periods 2-4	Periods 2-4	Periods 2-4
Mean of dependent variable	0.0066	0.0066	0.0117	0.0117	3.54	3.54	1.72	1.72	1.82	1.82
F-statistic of IV in Transfer 1st Stage	186.7^{**}	100.2^{**}	186.7^{**}	100.2^{**}	310.6^{**}	167.3^{**}	310.6^{**}	167.3^{**}	309.3^{**}	166.8^{**}
F-statistic of IV in Transfer * Non-Indigenous 1st Stage		96.4^{**}		96.4^{**}		102.2^{**}		102.2^{**}		101.5^{**}

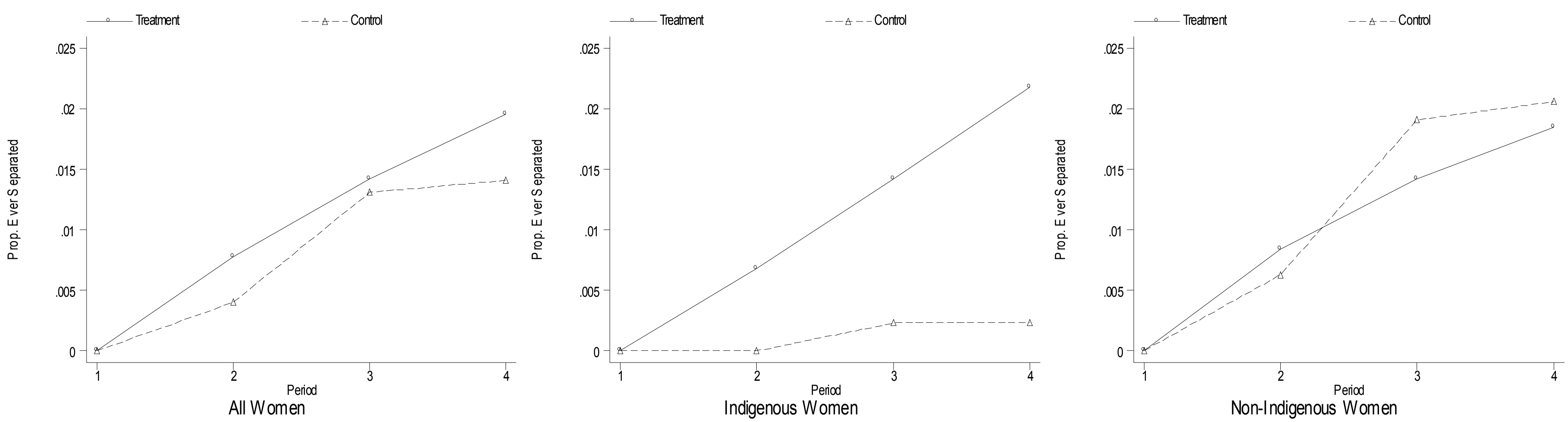
Notes: Coefficient estimates from 2SLS regressions are presented. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; statistically significant at (+) 90 percent, (*) 95 percent, and (**) 99 percent confidence levels. Estimates reported from matching estimators where households in the double-treatment are matched to the closest 'double-control' group household in terms of pre-program and current period household expenditures and indigenous identity of the woman, based on Rubin (1979). Instrumental variables are treatment village indicator and rainfall shock indicator interactions, and its interaction with non-indigenous woman indicator. Controls in both sets of regressions include indicators for mother's and partner's age group (26-35 years, 36-45 years, 46-55 years); indicator variables for none or less than primary schooling, indigenous language indicator for both women and their partners; wage laborer and self-employed indicators for women; wage laborer, self-employed and agricultural worker indicators for partners; cohabitation status, total household agricultural land,, having a dirt floor, owning the residence; demographic controls include number of children by gender and age group categories (0-5 years, 6-7 years, 8-9 years). Sample in the regressions are women with children 9 years old or younger at baseline.

Figure 1: Separation Rates over Two-Year Period, Women in Union at Baseline

Panel A: Currently Separated Rates



Panel B: Ever-Separated Rates

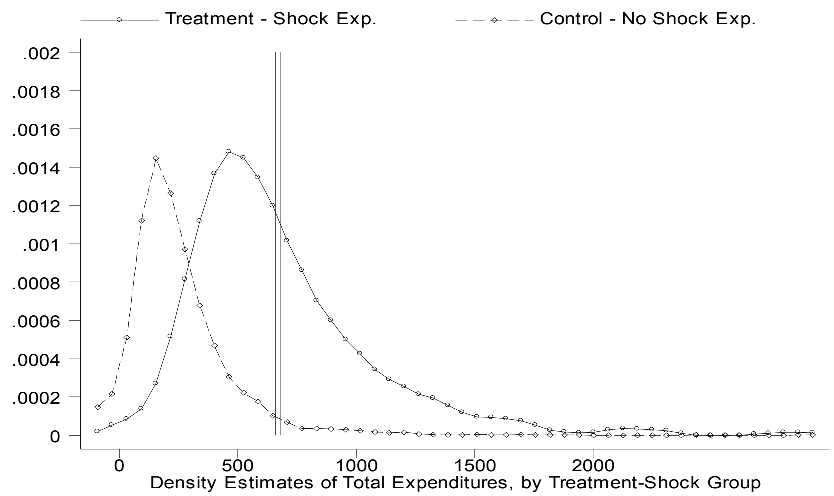


Notes: Currently separated and ever-separated rates for households by Progresa experimental treatment group through time are presented. Sample of women with children 9 years old and younger.

Figure 2: Tests of Equality of Summary Statistics and Distributions of Total HH Expenditures, by Treatment-Rainfall Group

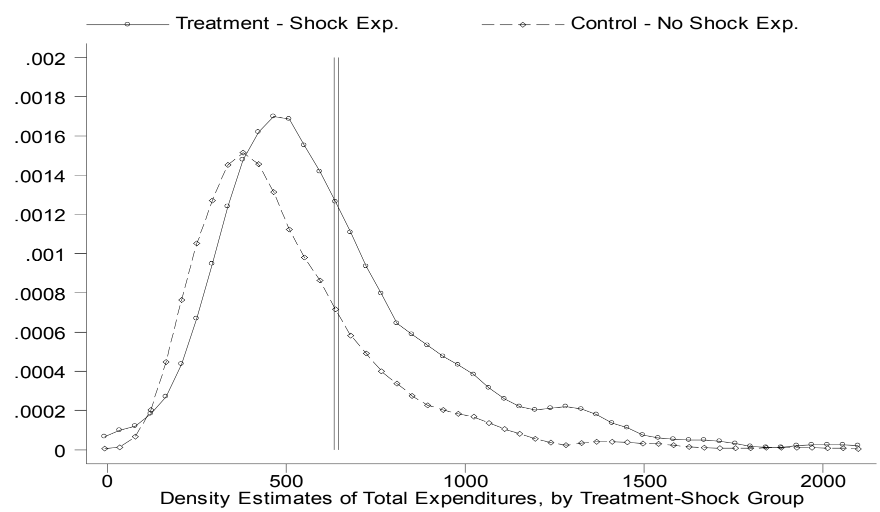
Panel A: Periods 2-4 Tests and Density Estimates

	Treatment – Shock	Control – No Shock	P-value
Mean	677.42	658.33	[0.516]
Standard Deviation	387.72	448.78	[0.241]
Kolmogorov-Smirnoff Test			[0.104]



Panel B: Period 4 Tests and Density Estimates

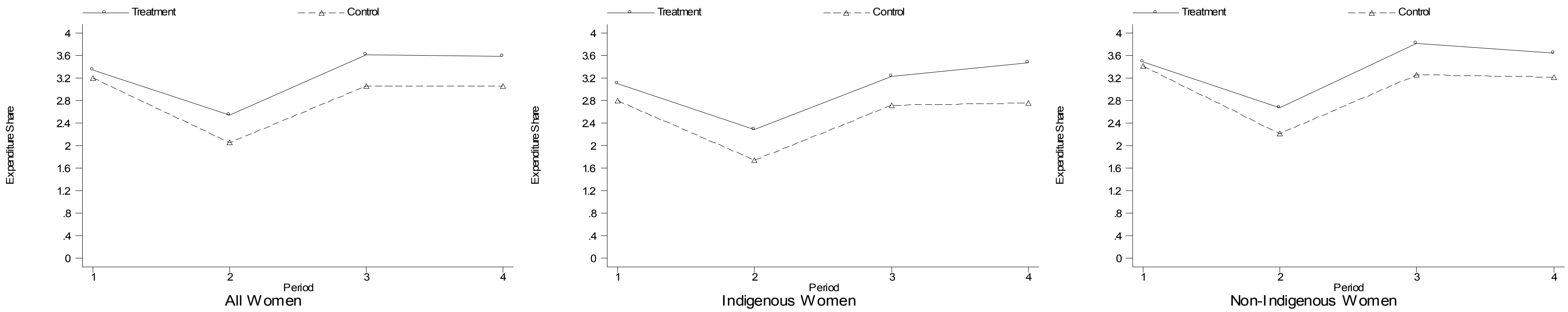
	Treatment – Shock	Control – No Shock	P-value
Mean	631.08	645.69	[0.641]
Standard Deviation	322.31	369.78	[0.149]
Kolmogorov-Smirnoff Test			[0.377]



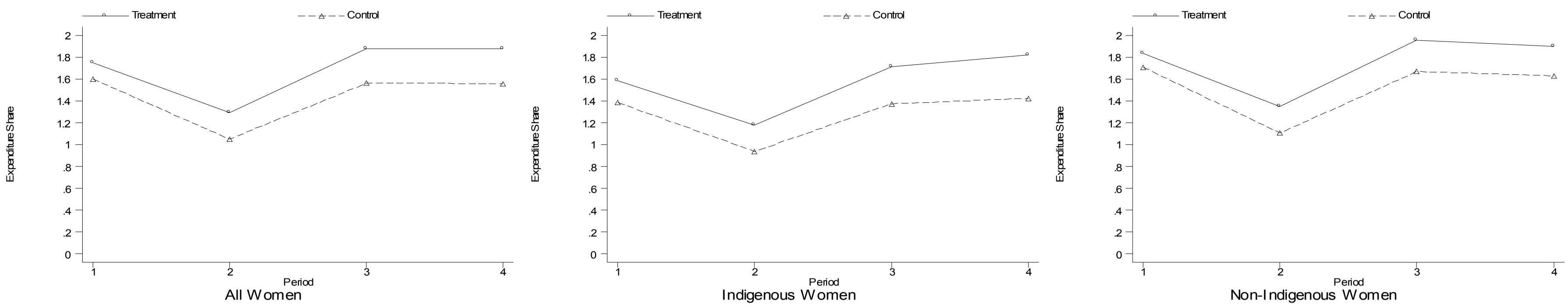
Notes: Mean and standard deviations for each Treatment-Rainfall shock (TR) group and Control-No Rainfall Shock (CNR) group are presented. P-values from t-tests of equality of means, F-tests of Levene (1960)'s equality of standard deviations test, and from Kolmogorov-Smirnoff tests of equality of distributions are reported in brackets. Estimates of density distribution functions by group are also presented. Sample sizes of TR group (Periods 2-4), CNR group (Periods2-4) are 245 and 3506; TR group (Period 4), CNR group (Period 4) are 155 and 1084, respectively.

Figure 3: Expenditure Shares in Children's Clothing over Two-Year Period, by Treatment Group and Indigenous Status

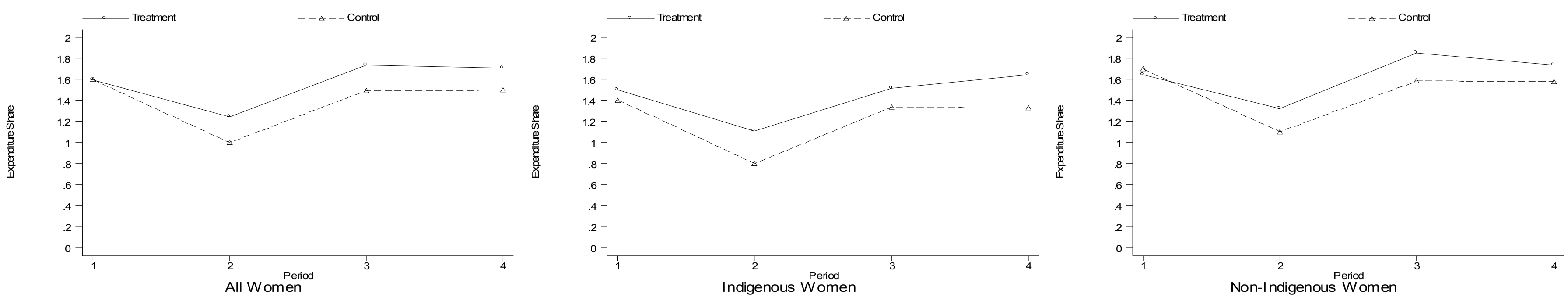
Panel A: Expenditure Share in Children’s Clothing



Panel B: Expenditure Share in Girls’ Clothing



Panel C: Expenditure Share in Boys’ Clothing



Notes: Expenditure shares in children’s, girls’ and boys’ clothing by experimental treatment group through time are presented. Sample of women with children 9 years old and younger who remain in a union.

Table A1: Relationship between attrition and characteristics of subjects at baseline

	Dependent variable: Attrition indicator			
	Treatment	Correlates	Main effect of Correlates	Interaction of Correlates with Treatment
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Treatment indicator	0.013	-	-0.053	-
	(0.010)		(0.062)	
Mother's age		-0.001**	-0.002**	0.001
		(0.000)	(0.001)	(0.001)
Mother's years of schooling		-0.003*	-0.001	-0.002
		(0.001)	(0.002)	(0.003)
Mother indigenous		-0.006	-0.001	-0.007
		(0.015)	(0.024)	(0.030)
Mother wage laborer		0.028+	0.072*	-0.068*
		(0.015)	(0.030)	(0.034)
Mother self-employed		0.014	0.013	0.000
		(0.013)	(0.023)	(0.027)
Partner's age		-0.001*	-0.001	0.000
		0.000	(0.001)	(0.001)
Partner's years of schooling		-0.002+	-0.002	-0.001
		(0.001)	(0.002)	(0.002)
Partner indigenous		-0.004	-0.011	0.010
		(0.015)	(0.025)	(0.031)
Partner wage laborer		-0.018	-0.062*	0.072*
		(0.014)	(0.028)	(0.031)
Partner self-employed		-0.014	-0.050+	0.056+
		(0.015)	(0.026)	(0.032)
Partner agricultural laborer		-0.021	-0.050+	0.047
		(0.016)	(0.029)	(0.034)
Partner non-wage laborer		0.023	-0.083**	0.149**
		(0.033)	(0.029)	(0.049)
Number of children		-0.009**	-0.005*	-0.007*
		(0.001)	(0.002)	(0.003)
N. children 10-14 years old		-0.006*	-0.009*	0.005
		(0.003)	(0.004)	(0.006)
Cohabitation		0.007	0.013	-0.008
		(0.007)	(0.012)	(0.015)
Dirt floor		-0.010+	-0.008	-0.003
		(0.005)	(0.009)	(0.011)
Own house		-0.047**	-0.051**	0.007
		(0.011)	(0.016)	(0.021)
Toilet		-0.024**	-0.019*	-0.008
		(0.006)	(0.009)	(0.013)
Agricultural Land		-0.008	-0.023+	0.024+
		(0.007)	(0.013)	(0.015)
Animals		-0.010+	0.001	-0.017+
		(0.005)	(0.007)	(0.010)
Constant	0.101		0.364**	-
	(0.007)		(0.054)	
Observations	40515	40515	40515	-
R-squared	0.00	0.02	0.02	-
F(Treatment Interactions)	-	-	-	1.76
P-value	-	-	-	0.02

Notes for Table A1: Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages, but not across villages. Significantly different than zero at 90% (+), 95% (*), 99% (**) confidence.. Columns 3 and 4 (and columns 7 and 8) presents results from one regression with main effects (col.3) (col.7) and all covariates interacted with treatment (col.4) (col.8).