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The Impact of Conditional Cash Transfers on Marriage and Divorce

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Abstract: A growing number of less-developed countries have introduced conditional cash transfer programs in which funds are targeted to women. Economic models of the family suggest that these transfer programs may lead to marital turnover among program beneficiaries. We use data from the experimental evaluation of the PROGRESA program in Mexico to provide new evidence on the short-run impacts of targeted transfers on couples' union dissolution and individuals' new union formation decisions. We find that, although the overall share of women in union does not change as a result of the program, marital turnover increases. Intact families eligible for the transfers experienced a modest (0.32 percentage points) increase in separation rates, with most of the effect concentrated among young and relatively educated women households. In contrast, young single women with low educational attainment levels experienced a substantial increase in new union formation rates. The marital transition patterns are consistent with the workhorse economic model of the marriage market – individuals with the greatest prospects to start new unions and those who may become more attractive in the marriage market are more likely to transition out of existing relationships and form new ones.

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1. Introduction

Conditional cash transfer (CCT) programs – poverty alleviation programs which provide funds to recipients in exchange for certain actions such as children's school attendance, school performance, and preventive health care visits – are an increasingly popular instrument of social policy in less developed countries (Rawlings and Rubio 2003; Maluccio and Flores 2004). These programs have been shown to be successful not only in alleviating poverty, but also in promoting child school enrollment, grade promotion, preventive health care, and overall child and adult physical health (e.g., Schultz 2004; Gertler 2004; Lagarde, Haines, and Palmer 2007).

A common feature of these programs is that funds are usually targeted to adult women, mothers of the target child population. The basis for this gender-specific targeting is a growing consensus among scholars and policymakers that targeting resources to women may have a myriad of benefits, from promoting gender equity and female 'empowerment' within the household and in the community, to disproportionately improving children's human development (e.g. Thomas 1990; Schultz 1990; Duflo 2003; World Bank 2001; 2003; 2007). However, despite the burgeoning literature on these programs' impacts, and the gender-based targeting of these transfers in particular, on the levels of female empowerment, the allocation of household resources towards investments in children, and child well-being, little is known about whether these programs affect marriage and divorce (Attanasio and Lechene 2002; Djebbari 2005; Bobonis, Castro, and González-Brenes 2007; Bobonis 2009). The effects of these programs on marital transitions have important policy implications, since although marital dissolution may or may not be beneficial to adult females, there is growing evidence that parental divorce and growing-up in a single-parent household may have adverse impacts on children's long-run education levels, mental health, and socio-economic status during adulthood (e.g., Gruber 2004).

Economic theory provides ambiguous predictions of the expected impact of CCT programs (with or without gender-based targeting) on marriage and marital dissolution. On one hand, marital dissolutions could increase as a result of women's greater economic independence, or due to potential conflict among partners over the uses of the public transfers or households members' actions required as part of the conditionality requirements. Additionally, single and potentially single women may become more attractive in the marriage market due to their increase in unearned income, which would lead to greater rates of marital dissolution and the formation of new unions. On the other hand, increases in household income could aid in the reduction of stress and violent conflict related to the allocation of resources within the household, thus reducing marital dissolution rates. Although this ambiguity regarding the impact of government transfer programs on marital transitions is well-recognized in the academic literature on welfare policy across developed countries, it has not been thoroughly examined in the context of these recent social policy experiments in middle and low-income countries (Moffitt 2002; Grogger and Karoly 2005).

The present study provides the first piece of evidence on the short-run impacts of a conditional cash transfer program on marital transitions among low-income households. We 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 the formation of new unions. The PROGRESA program, one of the first large-scale conditional cash transfer programs in existence, established by the Mexican government in 1997, provides transfers to poor households in rural (as well as urban) areas. The transfers are paid to women contingent on certain requirements, children's school attendance and family-level visits to health services being prominent among them. The program initially involved an evaluation component: five hundred and six communities were selected to participate in a randomized evaluation. Communities were randomly assigned into two groups – the treatment group being phased-in to the program in March-April 1998 and the control group in November-December 1999. Using detailed survey data for all community members collected throughout the 1997-1999 evaluation period allows us to clearly estimate the short-term program impacts on marital transitions.

Comparing eligible treatment and control families in the evaluation sample, we find that the overall share of women in a marital union did not change as a result of the program; the village-level proportion of women in a marital or cohabiting union stayed at around 95 percent during the two-year follow-up period. These results suggests that, according to the model of gains to marriage by Choo and Siow (2006), the aggregate gains to marriage did not vary significantly as a result of the phase-in of the program. However, the program led to an increase in marital turnover. Intact families at baseline who were eligible for the program experienced a small but significant increase in marital dissolution rates: the absolute size of the effect is modest (0.32 percentage points in two years), but large relative to the underlying separation rate in the control group. We also find evidence that program-eligible mothers in no relationship at baseline also increase their cohabitation or marital union formation rates.

The results have important positive implications for the design of social welfare policies in less-developed countries. The study complements a growing literature which indicates that the potential unintended negative consequences of conditional cash transfer programs – PROGRESA in particular – in terms of generating work disincentives or increasing fertility rates, are indeed very limited (Skoufias and Di Maro 2006; Stecklov et al. 2006).[1] Specifically, the PROGRESA program's short-run impacts on marital turnover are limited. Our results thus contribute to the growing literature which suggests that conditional cash transfer programs may be conducive to alleviating poverty and promoting the human development of the next generation while avoiding the pitfalls of welfare policies experienced in developed country contexts.

Our findings contrast with existing evidence from various programs in the United States. For instance, Groeneveld, Tuma, and Hannan (1980) studied the consequences on marital stability of the Seattle and Denver negative income tax experiment programs of the 1970s – the Seattle-Denver Income Maintenance Experiments (SIME/DIME) – which randomly assigned low-income households to receive an income guarantee and income tax reimbursement as a substitute for the traditional welfare (Assistance to Families with Dependent Children, or AFDC) program. They found, contrary to initial expectations, that the experimentally-induced reform induced a significant increase in couples' likelihood of marital dissolution.[2] Another notable example is the U.S. federal welfare reform of 1996. Prior to the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, the welfare system in the U.S. was widely regarded as providing disincentives to marriage because it allocated benefits primarily to single women with children (see Bitler et al. (2004) for a survey of the literature). Under the increasing popular view that marriage is an important route to exiting welfare and poverty (Horn and Sawhill 2001; Lichter, Graefe, and Brown 2003; Murray 2001), the U.S. federal government created the Temporary Assistance for Needy Families (TANF) program, whose primary goal was to end the dependence of needy parents on government benefits by promoting marriage, job preparation, and work. However, empirical studies have shown mixed evidence regarding the impacts of these reforms on marital transitions.[3]

The paper is structured as follows. Section 2 provides a brief discussion of the PROGRESA program and its evaluation component, as well as the data used in the analysis. In Section 3, we briefly discuss the theoretical framework which allows us to generate hypotheses regarding the effects of PROGRESA on marital transitions. In

[1] Stecklov et al. (2006) find an exception to the common observation that fertility rates drop as a result of the introduction of CCT programs in the case of Honduras, a program which applied a different incentive payment scheme (i.e. no cap on transfers as a function of number of children) which did not discourage fertility.
[2] Their effects were concentrated among eligible whites and African-Americans, but found no effects among Mexican-American ('Chicano') households. 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 internal and external validity of this relationship (see Bishop (1980) and references therein).
[3] See the non-experimental evidence in Schoeni and Blank (2000); Bitler, Gelbach, and Hoynes (2002); Ellwood (2000); Rosenbaum (2000); and evidence from smaller-scale experimental studies in Grogger and Karoly (2005), Fraker et al. (2002).

Section 4, we present a simple empirical model to detect program impacts and our research design. The overall and heterogeneous program impacts, followed by sensitivity tests, are reported in Section 5, and Section 6 concludes.

2. PROGRESA Program, Data, and Social Context

2.1 Overview of PROGRESA Program

In 1998, the Mexican government initiated a conditional cash transfer program, the PROGRESA Program, aimed at alleviating poverty and improving the human development of children among low-income households in rural Mexico. The program targets the poor in marginal rural communities, and provides cash transfers to the mothers of over 2.6 million children conditional on school attendance, family health checks and adult women's participation in health clinics. The education component of PROGRESA consists of subsidies provided to mothers, contingent on their children's regular attendance to school.[4] The cash transfers are available for each child attending school in grades three to nine of primary/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 pesos per month per family in 1998).[5] The health/nutrition components consist of transfers of approximately 12 pesos per month and nutrition supplements targeted at children between the ages of 4 months and 2 years pregnant and breastfeeding women, and children ages 2-5 years who exhibit signs of malnutrition (Gómez de León and Parker 2000). These are contingent on attendance at a health clinic for preventive health checks. Overall, the program transfers represent on average ten percent of average expenditures of eligible families in the sample (Hoddinott, Skoufias, and Washburn 2000).

A distinguishing characteristic of the program is that it included an evaluation component from its inception. The program was implemented following an experimental design in a subset of 506 communities located across seven states. 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 proxy-means test was constructed using household income data collected in a baseline survey in both treatment and

[4] Receipt of the education-specific benefits is contingent on children attending school at least 85% of the time, which is verified by school personnel.
[5] The U.S. dollar/Mexican peso exchange rate during this period was approximately 1/10.

control communities in 1997.[6] While household eligibility was determined within all treatment and comparison group communities, only households classified as eligible and within the treatment villages became program beneficiaries during the evaluation period, which started in March/April 2008. Comparison village households started receiving program benefits by December 1999.

2.2 Data and Measurement

Extensive biannual interviews were conducted during October 1997 (the baseline survey), October 1998, May/June 1999, and November 1999, on approximately 24,000 households living in the 506 communities. Each survey is a community-wide census containing detailed information on household demographics, income, expenditures and consumption, and individual socio-economic status, health and schooling behavior and outcomes. More specifically, the surveys in October 1997, October 1998, May/June 1999, and November 1999 collected information on the marital status of each person in the household 8 years old and older. From this data, we construct two types of marital status variables: (1) a variable indicating whether the partners (e.g. children's parents) were separated or divorced at each survey round, and (2) a variable indicating whether mothers not in a union (separated, divorced, widowed, or single) at baseline entered a new union, either a cohabiting relationship or a marriage. We restrict the sample to eligible households with mothers between the ages of 16 and 55 years.[7] This results in a sample of 15,376 households.

We choose this sample for various reasons. First, we focus the analysis on the marital decisions of women who directly receive cash transfers as mothers and who have not benefitted from the program as eligible children (i.e., who have not been eligible to receive the PROGRESA scholarships as secondary school children). The reason for doing so is that we want to examine the effects of the program on adult individuals' marital decisions (including women 16 year of age and older) whose specific socio-economic characteristics – such as their schooling attainment levels – may not change as a result of the program. Since the scholarships were offered to households with children until the third grade of secondary school (i.e., eighth grade) during the evaluation period, and given the moderately high late school entry, drop-out, and repetition rates for this population, excluding women ages 15 and younger

[6] In addition to capturing the multidimensionality of poverty, another advantage of a welfare index is that it permits the classification of new households according to their socio-economic characteristics, other than income. See Skoufias et al. (2001) for a more detailed description of the targeting process.

[7] The age restrictions in the sample lead us to drop 1,691 observations – 1,258 couples and 433 women in no union at baseline.

allows us to minimize the extent to which these women are likely to have their skills or educational outcomes modified as a result of the program while including women who bear children at a young age.[8]

We exclude women 56 years and older for two reasons. First, elderly individuals were eligible to receive conditional cash transfers based on old-age support, independently of whether they had eligible children. Because we want to focus the analysis on the marital decisions of women who directly receive cash transfers intended to influence the human development of their children, we choose an age threshold that minimizes the extent of contamination for this reason. Second, because the competing risk of spousal death increases significantly after age 55 and this complicates the analysis significantly, we minimize it by excluding this older age group.

Table 1 reports the stock of marital unions at each survey round by treatment group, pooled for the overall population and stratified by the baseline marital status (in union versus not in union). Marital union ratios are quite high in this population, as approximately 90 percent of women are in a union during any one of the three periods under scrutiny (Table 1, Panel A). Also, the differences in marital ratios across treatment and control groups are quite small – overall union ratios are 0.20 percentage points (0.18 percent) higher at baseline in the PROGRESA villages, 0.50 percentage points (0.50 percent) higher in the first year of the intervention, and 0.10 percentage points (0.09 percent) lower in the second year of the intervention. These statistics also highlight the main results of the paper – although the overall share of women in union did not change as a result of the program, marital turnover increased. Intact families eligible for the transfers experienced a modest decrease in their marital union ratios of 0.32 percentage points (Table 1, Panel B), and the union formation ratios of women not in a union at baseline increased by 2.2 percentage points (Table 1, Panel C).

The table also depicts the extent of attrition in the overall sample – 6.7 percent in the first year of the intervention and 14.6 percent by the second year of the evaluation period (Panel A, column 6). These also tend to be moderately higher in the PROGRESA villages relative to the comparison group – a difference of 3.6 percentage points in the overall sample, of 3.8 percentage points in the couples' sub-sample, and of 2.1 percentage points in the sample of women not-in-union. Since the incidence of marital transitions is relatively rare in this population, attrition can lead to a significant extent of bias if it is asymmetric across treatment groups. We thus address this issue in our empirical analysis (see Sections 4 and 5).

[8] See Schultz (2004), Behrman, Sengupta and Todd (2005), and Bobonis and Finan (2005) for details of school entry, enrollment and progression among the PROGRESA households.

Table 2 presents the mean of various baseline (October 1997) individual and household-level characteristics for eligible intact couples as well as the mean differences between the treatment and control groups. Individuals in this sample come from poor socio-economic status households, since PROGRESA is targeted to poor individuals in marginalized rural communities. Among the intact household sample, approximately 70 percent of women and their partners have not completed primary school (Panels A and B, column 1). Most female partners do not earn wage labor income: only seven percent are either wage laborers or self-employed (Panel A). A large share (39 percent) of women in the sample report speaking an indigenous language, which is highly correlated with low socio-economic status in Mexico. Most male partners belong to the same age groups as their female partners and have similar school attainment levels (Panel B, column 1). Seventy percent of male partners work as wage laborers, and another 12 percent report to be self-employed. Eligible households had, on average, 3.67 children living in the household at baseline (Panel C).

Cohabitation is a common type of marital relationship in rural Mexico, since trial marriages are a prevalent phenomenon. This can affect the likelihood of marital dissolution, because their stability more strongly depends on the early economic and social well-being of the relationship. Among the sample of eligible couples, approximately one quarter (26 percent) report being in a cohabiting relationship rather than in a marital union.[9] Moreover, Mindek (2003) – based on a survey of the ethnographic literature – remarks that most marital dissolutions are in the form of separations rather than official divorces.[10] In the empirical analysis, we address this idiosyncrasy by pooling separations and divorces as one category; the incidence of dissolutions in the data is driven by the former.

The individual and household-level characteristics for program-eligible single (i.e., separated, divorced, widowed or never married) women are somewhat dissimilar to those of the group in union. Eligible women in this group tend to be older (60 percent are in the 36-55 years age group), and have lower educational attainment levels: 77 percent of them had not completed primary school (Panel A, column 5). These women also tend to have fewer children, consistent with them being at lower risk of childbearing due to the fact that some have not had or have ended marital relationships (Panel C, column 5). Interestingly, they are much more likely to be wage laborers (20

[9] The frequency of dissolution varies across groups (Mixtecs, Zapotecs, Nahuas, and others [Otomíes, Triquis, and Tzotziles] experience high [low] dissolution rates) partly due to the incidence of arranged marriages (Mindek 2003).
[10] Norms of family support for women and their children in the event of dissolution are similar across ethnic groups in Mexico. Upon 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 (Chiñas 1992). Also, across many indigenous groups, parents of one gender retain custody over children of the opposite gender, except young children, who always remain under the custody of the mother irrespective of their gender (Mindek 2003).

percent) or self-employed at baseline (11 percent): 31 percent report of them report receiving income from one of these activities (Panel A, column 5). These differences suggest that, as is well known in other contexts, there is a high degree of selection as to the types of unions which are dissolved, and the types of economic activities that single, separated and divorced women undertake.

We also compare mean attributes at baseline across treatment and control villages to evaluate the randomization of our sample (Table 2, columns 3-4, 7-8). As one would hope from the random assignment, there are no statistically significant differences in the observed characteristics of these individuals in most dimensions, with the important exceptions that (i) the proportion of couples who report being in a cohabiting relationship is lower in the treatment group and that (ii) women in no union in treatment villages report having more children than those in control villages.[11] Since these differences may impact our estimation of program impacts, we could condition on these observable characteristics in all our regression models to address potential selection bias.

3. Expected Effects of the Program: Theoretical Framework

The neoclassical economic theory of marriage and divorce assumes that marital union or dissolution is an optimal response; partners will choose to form or dissolve a union if partners are better off in the event of marriage or divorce (Becker 1973). The typical utility maximizing model assumes that an individual's (indirect) utility from being married depends on the individual's earned income if married, the spouse's income, other income, as well as other individual and household characteristics. In contrast, the individual's utility from being single depends on the individual's earned and unearned income if he/she decided to dissolve the union, as well as his/her individual and household characteristics (possibly including the ex-spouse's characteristics if the couple has children). An individual then chooses the utility maximizing state: marriage or remaining single.[12]

As argued by Becker et al. (1977), the neoclassical model hinges on the assumption that spouses can bargain efficiently and it is thus the combined gains to marriage that determine the marital union and dissolution decision. However, a growing body of empirical literature for the United States suggests that this assumption may be inappropriate. Under the neoclassical model, changes in divorce laws from a mutual consent to a unilateral divorce regime, which essentially give the individual the right to dissolve the union without the other's consent, should not

[11] 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.

[12] The model can also incorporate decisions to cohabit as opposed to entering a marital union. See Clarkberg, Stolzenberg, and Waite (1995), Brien, Lillard, and Waite (1999), and references therein.

lead to changes in the dissolution decisions because the partner who may want to divorce can be compensated by the other if the households' gains to marriage were positive. This now vast empirical literature suggests that marriage and divorce rates have been affected by the changes in these divorce laws (e.g., Friedberg 1998; Gruber 2004; Wolfers 2006), which is inconsistent with spouses being able to reach efficient bargains, as assumed in unitary or collective models of household behavior (Becker 1981; Chiappori 1992). A more recent theoretical literature has emerged to explain these patterns in the data, including search and learning in marriage markets and allowing for marriage to serve as a commitment device in collective action problems among partners within the household (e.g. Rasul 2006; Matouschek and Rasul 2008).

Nonetheless, the predictions of these alternative models of the impact of a CCT program (with or without gender-based targeting) on marriage and marital dissolution are ambiguous. This is the case, for instance, because higher incomes raise the individual's utility in both the married and the single states. On one hand, marital dissolutions could increase as a result of an 'economic independence' effect – because an increase in the woman's unearned income may make being single more attractive and discourage marriage (Becker 1973; Becker et al. 1977) – or due to conflict among partners over the uses of the public transfers or households members' actions required as part of the conditionality requirements. Additionally, single and potentially single women may become more attractive in the remarriage market due to their increase in unearned income, which would lead to greater rates of marital dissolution and the formation of new unions in the short run. On the other hand, increases in household income could have a stabilizing effect, reducing stress and conflict related to the allocation of resources within the household, thus reducing marital dissolution rates (e.g., Bobonis et al. 2007). Since the net effect of the CCT programs on marital transitions is ambiguous, this remains an empirical question which will be tackled in the empirical analysis.

4. Empirical Methods

4.1 Estimation Strategy

The random assignment of communities to the treatment or comparison group allows us to interpret mean differences in post-treatment outcomes among eligible couples/individuals as the average causal effects of the program. Since the assignment to the program is carried out at the village/locality level, we aggregate the marital dissolution, new union formation, individual and household control variables at the village-period level, and estimate the following models:

$$y_{ct} = \alpha + \theta_{98} T_c Year_{98} + \theta_{99} T_c Year_{99} + X_{c,97}\beta + \gamma Year_{99} + \varepsilon_{ct} \qquad (1).$$

The dependent variables, y_{ct}, denote (1) the overall proportion of women in a marital relationship in survey-round t; (2) the proportion of marital dissolutions among intact program-eligible couples at baseline; or alternatively, (3) the proportion of new unions among single program-eligible mothers at baseline, in village c at time t. The explanatory variables of interest, $T_c Year_{98}$ and $T_c Year_{99}$, respectively denote the PROGRESA treatment village indicator variable (T_c), interacted with October 1998 and November 1999 survey round indicators. We include averages of characteristics of individuals/families at baseline ($X_{c,97}$) to control for factors that influence marital transitions; $Year_{99}$ is a November 1999 survey round control; and ε_{ct} is a disturbance term which is allowed to be correlated over time within villages to account for auto-correlation of the error terms (Bertrand, Duflo, and Mullainathan 2004). We estimate these models by ordinary least squares; regressions are weighted by the population of families or individuals used in the denominator of the union dissolution or union formation ratios.[13]

In this specification, θ_{98} and θ_{99} represent the intent-to-treat program impacts on individuals' marital transitions approximately 6 and 18 months following the phase-in of the program. These are the main coefficients of interest; according to the previous discussion, the vector of θ coefficients could be positive or negative since the program could lead to increases or decreases in the probability of marital transitions. Equation (1) can also be estimated for multiple subgroups to account for heterogeneous mean impacts, which allows us to test for differences in the behavioral responses depending on individuals' attributes, such as their ethnic background, age group, and

[13] There are two motivations for doing the analysis at the village-period level. The first comes purely from the objective of estimating standard errors in a conservative manner (that is, to avoid estimating downward-biased standard errors). The empirical strategy follows the advice of Bertrand, Duflo, and Mullainathan (2004) to compute standard errors to address both within village-year correlation across observations, as well as serial correlation within villages across time, for cases with a small number of treatment groups. Since the exogenous variation is at the village-level and the timing of the treatment coincides across villages, we estimate models aggregating the data into (at most) 502 village-year cells (two cells per village) and estimate the models with the group averages of the variables defined above. Disturbance terms are allowed to be correlated at the village-level and the regressions are weighted by the size of the sample in each village, so that villages with larger populations have more influence on the regressions.

A second reason is that, for some subgroup specifications, a set of controls perfectly predicts the marital events, and thus removes those observations in the estimation of the probit and logit models. We have also estimated the regressions at the household/individual level employing linear probability, probit, and logit models, with standard errors clustered at the village level. Not surprisingly, the results are generally somewhat more precisely estimated. Estimates are available from the author upon request.

level of educational attainment. These decompositions will be useful to assess whether marital transitions are consistent with the workhorse model of the marriage market.

Multiple authors have shown that the PROGRESA randomization was successful in achieving a balanced support of baseline characteristics across the two experimental groups (e.g., Behrman and Todd 1999). However, since our focus is on changes in marital status, the significantly lower proportion of cohabiting couples in program (relative to control) villages at baseline for eligible households may limit our ability to estimate consistent treatment effects on marital dissolution. Nonetheless, all estimates of the program's impacts are robust to including controls for couples' baseline cohabitation status.

4.2 Sample Size and Attrition

A final issue in the analysis is the extent of attrition. If being out-of-sample is correlated with the likelihood of receiving treatment, this could lead to bias in the estimates of program impacts. As mentioned above, attrition rates rise to 14.6 percent by the second year of the intervention, and these are marginally higher among women in the treatment group (16.0 percent vs. 12.4 percent), both in the sample of women in union (14.8 vs. 11.0 percent) and those not in union at baseline (25.4 percent vs. 23.3 percent). Although we do not have data on the cause of attrition, it is sensible to think that household residential mobility is the main proximate cause.

To examine the extent of attrition on observables, we report estimates of the baseline correlates of attrition for women in union and not-in-union. Table 3 present estimates from OLS regressions that use the attrition indicator as dependent variable, and include a series of baseline individual and household characteristics, the treatment village indicator, and the interactions between these characteristics and the program village indicator. Overall attrition in the control group is higher among younger, wage laborer women, who are less likely to own a home, to have children, and whose partners are less likely to be wage or agricultural workers (Table 3, columns 1 and 3). In contrast, the interaction term – the differential correlation treatment groups in attrition as a function of baseline characteristics – is generally of the opposite sign and mutes this correlation (columns 2 and 4). This suggests that there is no selection on observable characteristics for women in the treatment group.[14] In sum, this analysis suggests that women from

[14] If anything, couples who leave the sample in the treatment group are somewhat negatively selected. Women who leave the sample are less likely to have completed primary schooling, are less likely to be indigenous, their partners are disproportionately younger and report being indigenous, are less likely to own their home and live in a home with a dirt floor. Joint tests of significance of the correlation for each characteristic for treatment group women (the sum of the coefficients on the observable characteristic and its interaction with the treatment group indicator) indicate that the correlations are significantly different from

the control group who leave the sample ("attriters") are 'positively' selected relative to those who stayed along observable characteristics, whereas this selection is muted among women in the treatment group.

This evidence of non-random attrition on observables nonetheless makes it difficult to characterize the direction of any potential biases. For instance, with regards to the couples at baseline, if women of higher socio-economic status are more likely to leave the sample in the control group and these are more independent women who may be more likely to dissolve the union, our estimated program impacts on marital dissolution would be biased downwards. If in contrast these higher socio-economic status households have more stable marriages, then our estimated impacts on marital dissolution may be biased upwards.

We thus remain agnostic as to the direction of the potential attrition bias and employ two methods in an attempt to address non-random sample attrition. First, baseline individual and household characteristics that could be determinants of attrition are included as explanatory variables (Alderman et al. 2001). We also employ the non-parametric method in Lee (2008) to place extreme bounds on program effects in the presence of differential attrition across the treatment and control groups. This method sums up to (i) identifying the excess number of individuals or couples who are induced to leave the sample because of the treatment, and then (ii) trimming the size of the control group individuals or couples by this number, which yields a set of best and worst-case scenario bounds (Lee 2008).[15] It allows us to estimate bounds on the program impacts among the sub-population of households who would not have left the sample as a result of the intervention. As shown below, given the relatively similar attrition rates across groups, this method yields reasonably tight bounds.[16]

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zero (the F-test statistic [p-values] for the couples sample is $F(34,501)= 6.24$ [<0.001], and the value of the test for the not-in-union women's sample is $F(22,432) = 4.39$ [<0.001]).

[15] The Lee (2008) method allows us to estimate bounds for the average treatment effect among the sub-population of individuals whose outcomes will be observed, irrespective of the assignment to treatment. This brief explanation is based on the exposition of the method in Lee (2002). Denote Y_1* and Y_0* the potential outcomes from assignment to the treatment group ($D = 1$) and the control group ($D = 0$), respectively, and Y the observed outcome. Also, denote S_1 and S_0 as indicator variables for whether the outcome is observed given assignment to the treatment and control groups, respectively. The average treatment effect among this sub-population is $E[Y_1*-Y_0* \mid S_0=1, S_1=1]$.

The lower bound is computed by reducing the fraction of observations with $Y = 0$ by the proportion in the excess group (in our case, observations in the control group; those with $S_0 = 1$, $S_1 = 0$), which effectively assumes that the trimmed group had $Y_1* = 0$, and reweighting by a factor of $(Pr[S=1|D=1]/Pr[S=1|D=0])$, reflecting that the denominator for computing the fraction $Y = 1$ has diminished due to the trimming. This quantity will equal one if all of the zeros in the group are trimmed. The upper bound is analogously computed by reducing the fraction of observations with $Y = 1$ by the proportion in the excess group, which assumes that the trimmed group had $Y_1* = 0$ and requires reweighting the proportion $Pr[Y=1|D=0,S=1]$ by $(Pr[S=1|D=1]/Pr[S=1|D=0])$, to reflect that the denominator for computing the fraction $Y = 1$ has diminished due to the trimming. This quantity will equal zero if all the ones are trimmed.

[16] The Lee (2008) approach relies on a monotonicity assumption in attrition: it assumes that every treatment group individual who reported a marital outcome would have reported an outcome if she had been assigned to the control group (or vice versa). This assumption is restrictive enough to generate a testable restriction. Essentially, the assumption is inconsistent with non-response being positively (negatively) correlated with treatment group individuals' characteristics and negatively (positively) correlated with control group individuals' characteristics. As shown in Table 3, the associations of observables with attrition are

5. Results

5.1 Program Impacts on Overall Union/Marriage Shares

Estimates of the overall program impacts on the share of women in a union are small and insignificantly different from zero. The point estimates suggest that the proportion of women in union among both eligible and ineligible women is 0.50 percentage points higher in the program villages relative to women in comparison villages during the first survey round (October 1998) and is 0.002 percentage points lower by the second survey round (Table 4, column 1). However, neither of these estimates is significantly different from zero. The results are robust to the inclusion of the baseline village-level average of individual and household-level characteristics (column 2).

The overall response among eligible households is also negligible: the program's estimated impacts on the share of eligible women in a marital union are 0.45 percentage points (0.50 percent) and -0.08 percentage points (-0.09 percent) during the first and second-years of the program, respectively (column 3). These estimates are again robust to the inclusion of the controls, and quite precisely estimated: they suggest a small increase of 0.50 percentage points in the proportion of married women during the first year of the program, and no difference during the second year (column 4). None of these estimates is significantly different from zero at conventional confidence levels. Also, the lack of evidence of changes in marriage shares is robust to specifications which include municipality fixed effects (not reported in the tables). These results suggests that, according to the Choo and Siow (2006) tractable version of the Becker (1973) theory of marital matching, the overall gains to marriage or a union do not vary significantly as a result of the phase-in of the program. However, the overall estimates of union/marriage shares may hide modest short-run increases in marital transitions which occurred, events which we highlight in the following subsections.

5.2 Program Impacts on Union Dissolution

We start the discussion with a graphical analysis, since it will shed light on the patterns in the data. Current separation rates for eligible couples in union at baseline increase over time in program villages and remain greater than comparison group rates throughout the evaluation period. The two-year separation rate is approximately 0.80

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not of opposite signs across the treatment groups, and thus this evidence is consistent with the assumption. We believe the robustness of our results to these varied methods of dealing with attrition provides confidence that they are not mainly driven by selective attrition bias.

percent for eligible partners in the treatment group and 0.47 percent for eligible control group couples; the treatment group's higher dissolution rate of approximately 0.32 percentage points can be attributed to the impact of the program (Figure 1, Panel A).

The parametric linear probability estimates of the program impacts on union dissolution rates mirror the results depicted in Figure 1. Using the complete sample of eligible couples in union at baseline, the main cross-sectional reduced-form effects from CCT eligibility imply an average 0.32 percentage point increase in current marital dissolution rates by the end of the two-year evaluation (Table 5, Panel A, column 1). The results are robust to including the baseline village-level averages of individual and household characteristics (column 2). The estimates also indicate that marital dissolution rates grow gradually, from 0.08 percentage points (28 percent) in October 1998, to the aforementioned 0.32 percentage points (67 percent) by the last survey period (November 1999). To further check robustness, we estimate the treatment effects using a specification with municipality fixed effects and find quantitatively larger estimates of program impacts, with estimated first and second period dissolution rate increases of 0.47 and 0.69 percentage points (164 and 144 percent), respectively (column 3). Although these differences are large in relative terms given the low counterfactual separation rates, the overall effects are quite modest. Finally, the Lee (2008) treatment effect bounds on marital dissolution rates are -0.08 and 0.08 percentage points during the first year of the program, and 0.31 and 0.80 percentage points during the second year (Table 5, Panel B, column 1). These suggest that sample attrition does not significantly bias our estimates of the program impacts by the second year of the intervention.

There are significant differential effects on union dissolution by women's indigenous background (Table 6, Panel A, columns 1-2), with a substantial increase among indigenous women households (0.62 percentage points, significant at 95 percent confidence), and no effect among non-indigenous women households. This result is consistent with the argument that, because indigenous women households are poorer on average, the gains to marriage for this group may be lower than those for non-indigenous households; therefore, the transfers may more likely lead to dissolution. However, it may also be the case that the transfers are disproportionately larger as a share of overall income among these households, thus leading to larger dissolution effects given the same income-dissolution 'elasticity'.

The effects on union dissolution are also larger for couples in which women have higher schooling attainment levels relative to those in which women have not completed primary schooling, although the differential impact is not significantly different from zero (Panel A, columns 3-4). This difference is especially significant if the partner's school attainment levels are low (results not shown in the tables).[17] Since it is generally assumed that spouses' schooling levels are complements in the marital matching function (e.g. Becker 1973; Boulier and Rosenzweig 1984), this evidence may be consistent with the possibility that the gains to *current* marriages may be relatively small among more educated women with less educated partners, perhaps because the prospects of remarriage with a higher-educated individual, or socio-economic conditions following the dissolution given the same amount of transfers, may be comparatively higher for these.

Since remarriage prospects are arguably higher for younger women, and these households may have invested less in marital-specific capital, we should expect gains to marriage to be smaller among this group, and thus to observe higher dissolution rates as a result of the program. Although there is also no statistically significant differential program effect among women of different age groups (women ages 16-35 versus ages 36-55) (Panel A, columns 5-6), the estimated impact for couples with 16-35 year old women is large and significant (0.41 percentage points, significant at 95 percent confidence) and small (0.18 percentage points) and insignificantly different from zero for older women households.

As robustness checks, we also estimate all subgroup specifications including municipality fixed effects: the results are qualitatively and quantitatively similar.[18] Finally, to further check the robustness of the results to potential attrition bias, we estimate Lee (2008) bounds and find that these are reasonably tight around the point estimate. For instance, the treatment effects bounds among all indigenous women households are 0.54 and 0.83 percentage points; those among households with relatively educated women are 0.58 and 1.00 percentage points.[19] In summary, the overall and subgroup analysis provides us with confidence that the dissolution patterns observed as a result of the program are broadly consistent with the workhorse economic model of union dissolution – couples with the lowest gains to current unions and with relatively better remarriage prospects are more likely to dissolve.

5.3 Program Impacts on New Union Formation

[17] The estimates of the second year impacts for couples whose husbands have schooling attainment levels below primary school and of primary school or above are -0.04 percentage points (standard error = 0.28 percentage points) and 0.42 percentage points (standard error = 0.17 percentage points), respectively. The difference across these households is even starker among the subset of households in which women have schooling attainment levels of primary or above (0.26 percentage points (standard error = 0.32 percentage points), versus 0.85 percentage points (standard error = 0.46 percentage points)).

[18] These estimates are available from the author upon request.

[19] The point estimate excluding baseline controls for the overall sample of households with relatively educated women is 0.58 percentage points (standard error = 0.29 percentage points; significant at 95 percent confidence). Therefore, the point estimates lies within the range of the Lee (2008) bounds.

Examining divorce rates among married women at baseline provides a partial view of the full effects of the program on marital transitions, since new marital formation effects should lead to lower overall shares of the population outside of a relationship. However, marital matching theory does not offer overly clear predictions regarding the impact of the conditional transfers on the likelihood that women will choose to match with a new partner. On one hand, women's higher incomes may induce, as a result of the economic independence effect, women outside of a relationship to choose to remain single. On the other hand, women with higher incomes may become more attractive in the marriage market, leading to greater new union formation rates among this group.

To assess which alternative effect dominates, we compare the treatment groups' trends in new union formation rates among women outside of a marital union at baseline. The graphical evidence shows that overall new union formation ratios for eligible women increase over time in program villages and remain greater than comparison group ratios throughout the evaluation period (Figure 1, Panel B). The difference in the extent of new union formation is approximately 3.1 percentage points (27 percent) in the first year and 2.2 percentage points (20 percent) by the second year. The analogous linear probability model estimates of the program's union formation rates effects mirror the results depicted in the figure, but the treatment group differences are imprecisely estimated and statistically insignificantly different from zero.[20]

These overall trends pool single never married, separated or divorced, and widowed women, and may be misleading to the extent that these women may experience substantially different marital matching opportunities. We also consider heterogeneity in the type of relationship being formed – cohabitation as opposed to remarriage. This is especially important in this context, since it is common among many couples to form trial marriages, and since the costs of entering a marital relationship may be higher than those of entering a cohabiting union, this may induce individuals to choose to cohabit. We thus present estimates of marital union formation effects separately for divorced and separated women, or those never married (single) at baseline, and also estimate cohabitation and remarriage-specific union formation effects for each group (Table 7).

We find evidence of a response in the formation of new cohabiting unions, as opposed to remarriage, among separated or divorced women at baseline. The estimates that exclude and include baseline controls

[20] Using the overall sample of eligible single women at baseline, the main intent-to-treat estimates excluding [including] baseline controls suggest increases in current union formation ratios of 3.13 [2.32] percentage points (28 [21] percent) six months following the phase-in of the program and an increase of 2.15 [0.87] percentage points (20.4 [8.3] percent) during the second year of the intervention (none is significantly different from zero; not reported in the tables). The estimates of zero effects are robust to the inclusion of municipality fixed effects: estimates for the first and second follow-up survey periods are -2.61 percentage points (standard error = 2.51) and -4.36 percentage points (standard error = 2.59), respectively (not reported in the tables).

respectively indicate that the program led to a 2.4-2.7 percentage points (128-140 percent) increase in the formation of new cohabiting relationships in the first survey round, and the impacts are maintained at 2.6-2.9 percentage points (149-169 percent) during the second year of the intervention (all estimates are significant at 90 percent confidence; Table 7, Panel A, columns 3-4).[21] In contrast, the program impact estimates on remarriage rates are negative, although small in magnitude and imprecisely estimated (Panel A, columns 5-6).

We also find evidence that single women opt for legal marital unions in response to the program as a substitute for cohabiting relationships. Although overall marital union ratios do not significantly increase among this subgroup, the point estimates suggest that marital union ratios increase by 8.7-13.3 percentage points (65-100 percent) by the second year of the intervention, whereas the formation of cohabiting unions decreases by between 4.3 and 6.6 percentage points (28-43 percent) (Panel B, columns 1-6). These patterns may be partially explained by the fact that, since the costs of entering a marital union may be higher than those of entering a cohabiting relationship, the additional income from the program may help induce individuals to expend the required legal costs to form a marital relationship.

We again explore heterogeneous responses in union formation and new cohabitation effects by educational attainment and age subgroups to further examine heterogeneity in program responses (Table 8).[22] Single, separated, and divorced women of low educational attainment levels are those who are more likely to form a new union as a response to the conditional cash transfers. The point estimates for cohabiting union formation ratios among separated or divorced women at baseline who had not completed primary schooling indicate an increase in the likelihood of this event occurring of 2.9 percentage points in the first year and 4.3 percentage points in the second year of the intervention, (Table 8, column 1). In contrast, the estimated impact for women with higher schooling levels is generally smaller and indistinguishable from zero by the second survey round (column 2). Analogous results, although less precisely estimated, are found for the remarriage ratios among single women at baseline (columns 5-6). This heterogeneous response by schooling attainment levels is consistent with less attractive women (i.e. those with lower education) becoming more attractive in the marriage market as a result of their higher incomes, an effect which may dominate the economic independence effect for these women.

[21] Including municipality fixed effects does not improve the precision of the estimates: first and second round impact estimates are 2.56 percentage points (standard error = 1.61; p-value=0.112) and 2.21 percentage points (standard error = 1.54; p-value=0.153) (not reported in the tables).

[22] We do not find definitive evidence of heterogeneity in new union formation responses by the woman's indigenous status, and thus omit these results for the sake of conciseness.

Finally, perhaps not surprisingly, we find that the type-specific new union formation effects are concentrated among relatively young (i.e. 16-35 years old) single women. The cohabitation effects estimates among separated/divorced women are large and significant (3.4 and 3.9 percentage points in the respective survey rounds, significant at least at 90 percent confidence), whereas the estimated impacts are smaller (2.2 and 2.1 percentage points, respectively) and insignificantly different from zero for older program-eligible women (columns 3-4). We find analogous results in the formation of new marriages among single women at baseline, although the estimates are less precisely estimated (columns 7-8). Again, to the extent that remarriage/cohabitation prospects are higher for younger women due to their greater attractiveness in the marriage market, we should expect greater gains in this group as a result of the program. In summary, the overall and subgroup analysis provides us with greater confidence that the new union formation and cohabitation patterns observed as a result of the program are broadly consistent with the workhorse economic model of the marriage market. Women with the greatest prospects to start new unions (i.e. younger women), and those that may become differentially more attractive in the marriage market and for whom independence effects may be weaker (i.e. the less educated) are more likely to form new marital relationships.

6. Conclusion

This paper contributes to the empirical literature on households' responses to conditional cash transfer programs by showing evidence of the effects of the PROGRESA program on individuals' union dissolution and formation decisions. It provides the first piece of evidence on these short-run impacts among low-income households. We find that although overall marital/union shares do not vary, marital transitions do increase as a result of the program. Intact families at baseline who were eligible for the program experienced a small but significant increase in marital dissolution rates: the absolute size of the effect is modest (0.3 percentage points in two years), but large relative to the underlying separation rate in the control group. We also find evidence that program-eligible mothers in no relationship at baseline also increase their marital union formation rates. This set of findings is consistent with the standard economic model of the marriage market, which predicts that unexpected changes in the income of a partner lead to changes in the perceived quality of the current match relative to prospective matches. By increasing the attractiveness of female program beneficiaries in the marriage market, unions may dissolve if the gains to marriage become negative for the current match (Becker et al. 1977; Weiss and Willis 1997) and union formation rates may increase for those women who become more attractive in the marriage market.

These results have important implications for the design of social welfare policies in less-developed countries. The study complements a growing literature indicating that the unintended negative consequences of conditional cash transfer programs – and PROGRESA in particular – in terms of generating work disincentives or increasing fertility rates, are indeed very limited (Skoufias and Di Maro 2006; Stecklov et al. 2006). Specifically, the PROGRESA program's short-run impacts on marital turnover are limited, a finding which contrasts strongly with existing evidence for various welfare programs, particularly in the United States.

The results also contribute to a complementary literature on the design of policy interventions intended to promote the empowerment of women in the household and in the community. Social programs that aim 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 degree of female empowerment may be limited to contexts in which women's power and/or status within the household is responsive to the gender-based targeting of transfers and in which marital turnover may not be as responsive to this policy – to the extent that turnover in the marriage market can undo women's short-term gains within the household (Lundberg and Pollak 1993; Chiappori, Iyigun, and Weiss 2006). In addition, responses in terms of marital turnover 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.

Finally, the conclusions from the present study require some qualification. We identify and estimate short-run impacts of the PROGRESA CCT program on marital turnover, results which may contrast greatly with longer-term impacts of the program. For instance, in the recent debate regarding the impact of unilateral divorce legislation on marital turnover in the U.S., studies have found that marital dissolution rates increased in the first eight years following the policy change, and decrease substantially afterwards (Friedberg 1998; Gruber 2004; Wolfers 2006). Complementary to this pattern, Rasul (2006) argues that, as a result of pipeline ('stock') and selection effects, marriage rates should decrease following the unilateral divorce legislation and the match quality of new marriages improve. The limited sample of new unions in our study does not allow us to credibly assess whether the quality of matches improves substantially following the program-induced increases in marital turnover.

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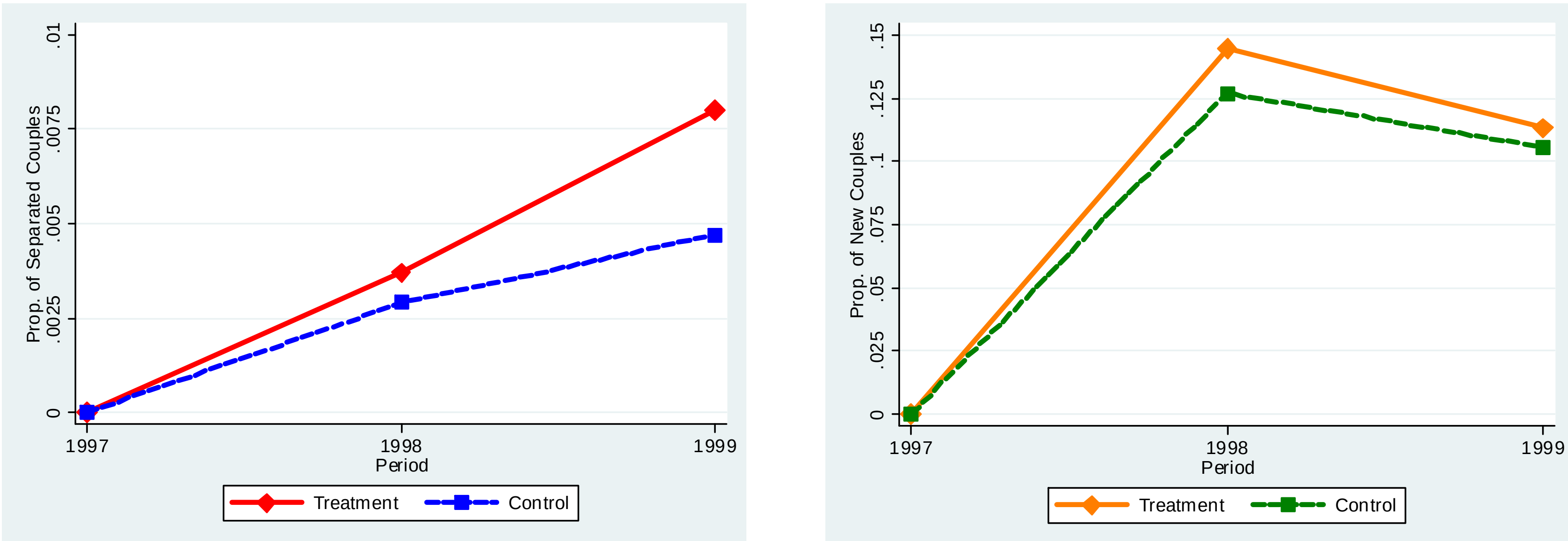
Figure 1: Marital Union and Dissolution Ratios through Time

Panel A: Couples in union at baseline

Panel B: Women not in union at baseline



Notes: Panel A - the (red) solid line with diamonds denotes the proportion of currently separated couples (given that the couple was in union at baseline) in PROGRESA communities, and the (blue) dashed line with squares denotes couples in comparison communities. Panel B - the (orange) solid line with diamonds denotes the proportion of couples newly formed (i.e. cohabiting) (given that the woman was single, separated, divorced, or widowed at baseline) in PROGRESA communities, and the (green) dashed line with squares denote newly-formed couples in comparison communities.

Table 1: Marital Union and Dissolution Descriptive Statistics

		In union (1)	Not in union (2)	Attriters (3)	Total (4)	Prop. in union (among non-attriters) (5)	Attrition ratio (6)
Panel A: All Women							
Treatment Group							
October 1997	(Year 0)	8,395	1,078	-	9,473	0.8862	-
October 1998	(Year 1)	7,978	863	632	9,473	0.9024	0.067
November 1999	(Year 2)	7,196	759	1,518	9,473	0.9046	0.160
Control Group							
October 1997		5,222	681	-	5,903	0.8846	-
October 1998		4,941	562	400	5,903	0.8979	0.068
November 1999		4,680	489	734	5,903	0.9054	0.124
Difference							
October 1997						0.0016	-
October 1998						0.0045	-0.001
November 1999						-0.0008	0.036
Panel B: Women in union (at baseline)							
Treatment Group							
October 1998	(Year 1)	7,837	29	529	8,395	0.9963	0.063
November 1999	(Year 2)	7,094	57	1,244	8,395	0.9920	0.148
Control Group							
October 1998		4,871	14	337	5,222	0.9971	0.065
November 1999		4,625	22	575	5,222	0.9953	0.110
Difference							
October 1998						-0.0008	-0.002
November 1999						-0.0032	0.038
Panel C: Women not in union (at baseline)							
Treatment Group							
October 1998	(Year 1)	141	834	103	1,078	0.145	0.096
November 1999	(Year 2)	102	702	274	1,078	0.127	0.254
Control Group							
October 1998		70	548	63	681	0.113	0.093
November 1999		55	467	159	681	0.105	0.233
Difference							
October 1998						0.031	0.003
November 1999						0.022	0.021

Notes: Cell totals and means by groups are presented.

Table 2: Individual and Household Baseline (October 1997) Characteristics

	All women in union at baseline				All women not in union at baseline			
	Overall Mean (1)	Std. Dev. (2)	Program Mean (3)	Control Mean (4)	Overall Mean (5)	Std. Dev. (6)	Program Mean (7)	Control Mean (8)
Panel A: Mothers' Characteristics								
Age 16-25 years	0.22	(0.42)	0.22	0.22	0.19	(0.39)	0.20	0.17
Age 26-35 years	0.34	(0.48)	0.35	0.34	0.22	(0.41)	0.22	0.21
Age 36-45 years	0.28	(0.45)	0.28	0.28	0.28	(0.45)	0.29	0.28
Age 46-55 years	0.16	(0.36)	0.15	0.16	0.31	(0.46)	0.30	0.34
Schooling < Primary	0.73	(0.44)	0.73	0.74	0.77	(0.42)	0.78	0.76
Indigenous woman	0.39	(0.49)	0.40	0.39	0.41	(0.49)	0.40	0.42
Wage laborer	0.03	(0.18)	0.04	0.03	0.20	(0.40)	0.18	0.24
Self-employed	0.03	(0.18)	0.04	0.03	0.11	(0.31)	0.13	0.07
Panel B: Partners' Characteristics								
Age 16-25 years	0.12	(0.32)	0.12	0.12	-		-	-
Age 26-35 years	0.32	(0.47)	0.33	0.31	-		-	-
Age 36-45 years	0.29	(0.45)	0.29	0.29	-		-	-
Age 46-55 years	0.19	(0.40)	0.19	0.19	-		-	-
Age 56-65 years	0.07	(0.25)	0.07	0.07	-		-	-
Schooling < Primary	0.71	(0.45)	0.71	0.72	-		-	-
Indigenous partner	0.40	(0.49)	0.41	0.40	-		-	-
Wage laborer	0.70	(0.46)	**0.68**	**0.73**	-		-	-
Self-employed	0.12	(0.33)	0.13	0.11	-		-	-
Panel C: Household Characteristics								
Number of children	3.67	(2.01)	3.38	3.34	2.75	(1.85)	**2.87**	**2.57**
Number of 10-14 year old children	0.90	(0.98)	1.40	1.41	0.69	(0.88)	**0.74**	**0.63**
Cohabiting couple	0.26	(0.44)	**0.24**	**0.29**	-		-	-
Dirt floor	0.66	(0.47)	0.65	0.67	0.69	(0.46)	0.69	0.70
Own house	0.94	(0.24)	0.94	0.93	0.96	(0.20)	0.96	0.95
Total Agricultural Land	1.67	(3.04)	1.61	1.76	1.73	(3.63)	1.79	1.65

Notes: Mean of characteristics by groups and standard deviations (in parentheses) are presented. The figures in bold represent statistically significant differences in means across experimental groups at 5 percent significance levels.

Table 3: Relationship between attrition and baseline characteristics

	Dependent variable: Out-of-sample (1/0)			
Sample	In union at baseline, Years 1 and 2		Not in union at baseline, Years 1 and 2	
	Main effect	Interaction with Treatment indicator	Main effect	Interaction with Treatment indicator
	(1)		(2)	
Treatment indicator	-0.0342		-0.0417	
	(0.0450)		(0.0906)	
Woman's age 26-35 years	-0.0306	0.0258	-0.0728	0.0449
	(0.0135)**	(0.0172)	(0.0435)*	(0.0523)
Woman's age 36-45 years	-0.0538	0.0476	-0.1497	0.1050
	(0.0159)***	(0.0214)**	(0.0441)***	(0.0553)*
Woman's age 46-55 years	-0.0633	0.0303	-0.1519	0.1039
	(0.0193)***	(0.0269)	(0.0458)***	(0.0563)*
Partner's age 26-35 years	-0.0210	-0.0231		
	(0.0161)	(0.0207)		
Partner's age 36-45 years	-0.0162	-0.0498		
	(0.0192)	(0.0247)**		
Partner's age 46-55 years	-0.0141	-0.0376		
	(0.0205)	(0.0273)		
Partner's age 56-65 years	-0.0301	-0.0384		
	(0.0212)	(0.0295)		
Partner's age 66+ years	-0.0350	-0.0175		
	(0.0247)	(0.0390)		
Woman's schooling < Primary	0.0028	0.0155	0.0021	-0.0388
	(0.0102)	(0.0139)	(0.0321)	(0.0401)
Partner's schooling < Primary	0.0111	0.0048		
	(0.0093)	(0.0127)		
Non-indigenous woman	-0.0154	0.0201	-0.0318	-0.0015
	(0.0238)	(0.0308)	(0.0273)	(0.0349)
Non-indigenous partner	0.0190	-0.0192		
	(0.0260)	(0.0321)		
Cohabiting couple	0.0190	-0.0118		
	(0.0121)	(0.0147)		
Home with dirt floor	-0.0049	-0.0011	0.0040	-0.0342
	(0.0087)	(0.0110)	(0.0246)	(0.0323)
Own home	-0.0604	0.0075	-0.1225	0.0585
	(0.0164)***	(0.0220)	(0.0546)**	(0.0732)
Woman - wage laborer	0.0618	-0.0584	0.0233	0.0458
	(0.0310)**	(0.0352)*	(0.0226)	(0.0322)
Woman - self employed	0.0057	0.0152	0.0151	0.0171
	(0.0270)	(0.0335)	(0.0392)	(0.0463)
Partner - wage laborer	-0.0487	0.0565		
	(0.0256)*	(0.0295)*		
Partner - self employed	-0.0362	0.0425		
	(0.0256)	(0.0318)		

Table 3: Relationship between attrition and baseline characteristics (cont.)

	Dependent variable: Out-of-sample (1/0)			
Sample	In union at baseline, Years 1 and 2		Not in union at baseline, Years 1 and 2	
	Main effect	Interaction with Treatment indicator	Main effect	Interaction with Treatment indicator
	(1)		(2)	
Partner - agricultural worker	-0.0540	0.0295		
	(0.0284)*	(0.0330)		
Partner - non-wage laborer	-0.0669	0.0929		
	(0.0282)**	(0.0394)**		
Total agricultural land	-0.0011	0.0029	0.0078	-0.0093
	(0.0011)	(0.0017)*	(0.0044)*	(0.0046)**
Num. of boys age 0-5 years	-0.0006	-0.0067	-0.0152	-0.0020
	(0.0047)	(0.0062)	(0.0269)	(0.0312)
Num. of boys age 6-7 years	-0.0054	-0.0123	-0.0406	-0.0079
	(0.0075)	(0.0097)	(0.0349)	(0.0415)
Num. of boys age 8-9 years	-0.0184	-0.0009	-0.0220	-0.0040
	(0.0075)**	(0.0096)	(0.0298)	(0.0382)
Num. of boys age 10-11 years	0.0032	-0.0152	0.0109	-0.0457
	(0.0068)	(0.0092)	(0.0331)	(0.0367)
Num. of boys age 12-14 years	-0.0177	0.0057	0.0208	-0.0119
	(0.0061)***	(0.0078)	(0.0310)	(0.0323)
Num. of boys age 15-18 years	-0.0071	-0.0028	-0.0225	0.0117
	(0.0059)	(0.0077)	(0.0186)	(0.0236)
Num. of girls age 0-5 years	0.0004	-0.0008	-0.0297	0.0555
	(0.0054)	(0.0066)	(0.0251)	(0.0311)*
Num. of girls age 6-7 years	-0.0113	-0.0063	0.0379	-0.0501
	(0.0078)	(0.0102)	(0.0379)	(0.0463)
Num. of girls age 8-9 years	-0.0074	-0.0245	-0.0192	-0.0531
	(0.0064)	(0.0084)***	(0.0398)	(0.0462)
Num. of girls age 10-11 years	-0.0076	-0.0058	0.0155	-0.0102
	(0.0069)	(0.0093)	(0.0342)	(0.0408)
Num. of girls age 12-14 years	-0.0211	0.0232	-0.0320	-0.0033
	(0.0064)***	(0.0084)***	(0.0311)	(0.0319)
Num. of girls age 15-18 years	0.0013	-0.0190	-0.0024	-0.0491
	(0.0061)	(0.0081)**	(0.0232)	(0.0290)
Year 1999 (1/0)	0.07		0.1518	
	(0.0071)***		(0.0123)***	
Constant	0.2114		0.3173	
	(0.0391)***		(0.0687)***	
F-test: Interaction terms = 0	1.82		1.32	
[p-value]	[0.004]		[0.149]	
Observations	27234		3518	

Notes: Robust standard errors in parentheses; disturbance terms are clustered at the village level; statistically significant at 90% (*), 95% (*), *99% (***) confidence levels. Column pairs present results from one regression with main covariate correlation in first and covariates interacted with the treatment indicator in the second.

Table 4: Program Impacts on Overall Marriage Ratios

	Dependent variable: Proportion of unions in village					
Sample	Eligible and Ineligible Women		Eligible Women		Ineligible Women	
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Panel A: Point estimates						
Treatment indicator, Year 1	0.0050	0.0055	0.0045	0.0050	0.0060	0.0080
	(0.0062)	(0.0058)	(0.0065)	(0.0059)	(0.0136)	(0.0123)
Treatment indicator, Year 2	-0.0002	0.0004	-0.0008	0.0000	0.0008	0.0030
	(0.0062)	(0.0059)	(0.0064)	(0.0060)	(0.0141)	(0.0130)
Individual and HH Controls	No	Yes	No	Yes	No	Yes
Municipality Fixed Effects	No	No	No	No	No	No
Observations	1004	1004	1004	1004	1004	1004
Prop. women in union in Control Group						
Year 1	0.895	0.895	0.898	0.898	0.885	0.885
Year 2	0.901	0.901	0.905	0.905	0.883	0.883
Panel B: Attrition/Non-response bounds						
Year 1			[0.0044, 0.0055]			
Year 2			[-0.0395, 0.0032]			

Notes: Coefficient estimates from OLS regressions, weighted by the number of observations in the village in each sample, are presented. 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 baseline village-level proportions of women in age groups 16-25, 26-35, 36-45, and 46-55 years; the proportion of women with none or less than primary schooling educational attainment; the proportion of women of an indigenous background; the proportion of women who are wage laborers and the proportion who are self-employed; the proportion of households having a dirt floor, and the proportion of households owning the residence. The sample is composed of all eligible women ages 16-55 years with children.

<u>Table 5</u>: Eligible Couples' Union Dissolution Effects, Overall and Restricted Samples

	Dependent variable: Proportion of unions dissoved in village		
Sample	All unions at baseline		
	(1) OLS	(2) OLS	(3) OLS
Panel A: Point estimates			
Treatment indicator, Year 1	0.0008	0.0008	0.0047**
	(0.0011)	(0.0012)	(0.0018)
Treatment indicator, Year 2	0.0032**	0.0033**	0.0069***
	(0.0016)	(0.0016)	(0.0021)
Individual and HH Controls	No	Yes	Yes
Municipality Fixed Effects	No	No	Yes
Observations	998	998	998
Prop. dissolutions in Control Group			
Year 1	0.0029	0.0029	0.0029
Year 2	0.0048	0.0048	0.0048
Panel B: Attrition/Non-response bounds			
Year 1	[-0.0008, 0.0008]		
Year 2	[0.0031, 0.0080]		

Notes: Coefficient estimates from OLS regressions, weighted by the number of observations in the village in each sample, are presented. 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 baseline village-level proportions of women in age groups 16-25, 26-35, 36-45, and 46-55 years; the proportion of women with none or less than primary schooling educational attainment; the proportion of women of an indigenous background; the proportion of women who are wage laborers and the proportion who are self-employed; analogous measures at baseline for their partners; the proportion of partners who are agricultural workers; the proportion of households having a dirt floor, and the proportion of households owning the residence. The sample is composed of all eligible women ages 16-55 with children who are in a union at baseline.

Table 6: Eligible Couples' Union Dissolution Effects, by Subgroup

Sample	Indigenous women	Non-indigenous women	Women's Schooling ≥ Primary	Women's Schooling < Primary	Women's Age ≤ 35 years	Women's Age > 35 years
	Dependent variable: Proportion of unions dissoved in village					
	All Unions at Baseline					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Panel A: Point Estimates						
Treatment indicator, Year 1	0.0026*	0.0003	-0.0001	0.0009	-0.0001	0.0018
	(0.0015)	(0.0016)	(0.0023)	(0.0012)	(0.0015)	(0.0015)
Treatment indicator, Year 2	0.0062**	0.0019	0.0053*	0.0021	0.0041**	0.0018
	(0.0026)	(0.0021)	(0.0028)	(0.0019)	(0.0019)	(0.0024)
Individual and HH Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	451	872	922	996	991	990
Prop. dissolutions in Control Group						
Year 1	0.0010	0.0041	0.0038	0.0025	0.0032	0.0024
Year 2	0.0028	0.0060	0.0041	0.0050	0.0038	0.0059
Panel B: Attrition/Non-Response Bounds						
Year 1	[-0.0010, 0.0019]	[0.0002, 0.0013]	[-0.0037, 0.0004]	[0.0002, 0.0010]	[-0.0032, 0.0001]	[0.0018, 0.0042]
Year 2	[0.0054, 0.0083]	[0.0016, 0.0079]	[0.0058, 0.0100]	[0.0021, 0.0073]	[0.0042, 0.0082]	[0.0015, 0.0078]

Notes: Coefficient estimates from OLS regressions, weighted by the number of observations in the village in each sample, are presented. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. See notes to Table 5 for a list of control variables. The sample is composed of all eligible women with children who are in a union at baseline in the appropriate subgroup.

<u>Table 7</u>: New Union Formation Effects, Overall Sample

	Dependent variables: Prop. of women in a new [...]					
	Union		Cohabiting union		Marital union	
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Panel A: Separated or divorced women						
Treatment indicator, Year 1	0.0202	0.0182	0.0243*	0.0266*	-0.0041	-0.0064
	(0.0267)	(0.0271)	(0.0141)	(0.0141)	(0.0231)	(0.0231)
Treatment indicator, Year 2	0.0092	0.0078	0.0260*	0.0294*	-0.0046	-0.0059
	(0.0309)	(0.0315)	(0.0153)	(0.0166)	(0.0266)	(0.0260)
Controls	No	Yes	No	Yes	No	Yes
Observations	516	516	516	516	516	516
Prop. Control Group, Year 1	0.081	0.081	0.019	0.019	0.062	0.062
Prop. Control Group, Year 2	0.081	0.081	0.017	0.017	0.070	0.070
Panel B: Single women						
Treatment indicator, Year 1	0.0741	0.0128	0.0385	0.0177	0.0356	-0.0091
	(0.0756)	(0.0615)	(0.0446)	(0.0419)	(0.0654)	(0.0573)
Treatment indicator, Year 2	0.0361	-0.0305	-0.0429	-0.0659+	0.1334**	0.0867*
	(0.0764)	(0.0667)	(0.0479)	(0.0441)	(0.0650)	(0.0578)
Controls	No	Yes	No	Yes	No	Yes
Observations	364	364	364	364	364	364
Prop. Control Group, Year 1	0.294	0.294	0.098	0.098	0.196	0.196
Prop. Control Group, Year 2	0.286	0.286	0.152	0.152	0.134	0.134

Notes: Coefficient estimates from OLS regressions, weighted by the number of observations in the village in each sample, are presented. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; statistically significant at (+) 85%; (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. See notes to Table 4 for a list of control variables. The sample is composed of all eligible women ages 16-55 years with children and in no marital union at baseline.

Table 8: New Union Formation Sub-Group Effects, Overall Sample

	Dependent variables:							
	Prop. of women in a new cohabiting relationship				Prop. of women in a new marriage			
Samples:	Separated or divorced women at baseline				Single women at baseline			
	Women's Schooling < Primary	Women's Schooling ≥ Primary	Women's Age ≤ 35 years	Women's Age > 35 years	Women's Schooling < Primary	Women's Schooling ≥ Primary	Women's Age ≤ 35 years	Women's Age > 35 years
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) OLS	(8) OLS
Treatment indicator, Year 1	0.0285+	0.0354+	0.0344**	0.0221	-0.0338	0.0457	0.0152	-0.0256
	(0.0182)	(0.0220)	(0.0154)	(0.0268)	(0.0758)	(0.0589)	(0.0525)	(0.1085)
Treatment indicator, Year 2	0.0428**	0.0138	0.0386*	0.0207	0.1198+	0.0777	0.1373**	0.0327
	(0.0187)	(0.0323)	(0.0199)	(0.0274)	(0.0735)	(0.0557)	(0.0532)	(0.1241)
Individual and HH Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	419	254	391	290	244	210	318	117
Prop. Control Group, Year 1	0.022	0.013	0.008	0.034	0.282	0.111	0.145	0.364
Prop. Control Group, Year 2	0.009	0.035	0.011	0.026	0.204	0.069	0.049	0.367

Notes: Coefficient estimates from OLS regressions, weighted by the number of observations in the village in each sample, are presented. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages; statistically significant at (+) 85%; (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. See notes to Table 4 for a list of control variables. The sample is composed of all eligible women with children in the appropriate subsample and in no marital union at baseline.