The Effect of a Conditional Cash Transfer on Child Marriage: Evidence from Mexico

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Abstract

I study the effect of a conditional cash transfer program in Mexico on early marriage. The program provided monetary benefits to households, conditional on children's school attendance. Leveraging on the staggered implementation of the program, I find that exposure to the conditional cash transfer *increased* girls' probability of marriage. After five years of exposure to the program, beneficiary girls were, on average, 7 p.p more likely to be married than the control group. I find no effect for boys. These findings contrast with the previously documented positive effects of the program on education, which is usually associated with decreases in child marriage. I reconcile the simultaneous increase in marriage and education in a conceptual framework wherein agents treat marriage as a normal good. Finally, I test whether marriage responds positively to income by exploiting the variation in household composition and find that non-eligible children in beneficiary households - who were only exposed to the increase in household income - were between 10 and 18p.p more likely to be married than their counterparts in non-treated villages.

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1 Introduction

Child marriage has long been recognised as a violation of human rights, mostly prevalent in developing countries. It is mentioned as both the result and the cause of poverty due to its association with educational abandonment and lower participation in formal labour markets.[1] Child marriage disproportionately affects girls, and it is associated with a larger risk of early childbearing, violence, abuse and lack of autonomy.[2] In 2021, around 20% of the world's women aged 20 to 24 years old were first married or in a union before the age of 18 (UNICEF). Child marriage is more common in societies with large gender inequality, with conservative social norms, and where there is conflict, insecurity and acute poverty. Depending on the underlying causes of child marriage, policies might have different degrees of success in reducing it. Policies can aim to change norms and countries' legal systems, improve female education, social networks and labour market opportunities, or provide economic support. Cash transfers conditioned on school attendance or payment of school fees have been mentioned as one of the most promising strategies (Kalamar et al., 2016).

In this paper, I study one of the world's largest conditional cash transfer programs, Progresa/Oportunidades, and show that it led to increases in child marriage. The program gives monetary transfers to poor households, conditional primarily on children's school attendance. Therefore, there are two main channels through which the program can affect marriage. First, higher educational attainment may delay marriage. In a model of household specialisation, increasing labour market returns make home production less attractive, thus lowering the propensity to marry and delaying it (Becker, 1974). Furthermore, there is evidence that education increases autonomy and knowledge, thereby decreasing girls' need to rely on marriage as a safety net, empowering them concerning their partner's choice, increasing their bargaining power in the relationship and changing their fertility preferences (Ferré, 2009). The program's positive effects on schooling (Behrman et al., 2005, 2009; Dubois et al., 2012), should, then, lead

[1] See Thomson (2003) and Sperling and Winthrop (2015).

[2] On education and labor market, see Adebowale et al. (2012) and Kalamar et al. (2016). On violence and decision-making power, see Kırdar et al. (2018), Jejeebhoy et al. (1995) and Amin et al. (2017). On fertility choices and children outcomes, see Dahl (2010), Duflo et al. (2015) and Behrman (2015).

to a decrease in child marriage if there is human capital accumulation and returns from education in the labour market. Second, there might be an income effect on marriage, which is ambiguous. On the one hand, more income could lead households to rely less on marriage as a safety net or insurance mechanism (Amin et al., 2016). However, on the other hand, it could increase the marriage market value of the beneficiaries or facilitate the formation of a new household by making marriage expenditures more affordable.

First, I study this question empirically and show that the program caused an increase in child marriage. Then, I explain these effects in a model in which increases in disposable income, conditional on school attendance, can lead to increases in education and marriage. Finally, I provide empirical evidence that the monetary transfer might be an important mechanism in this context by showing that marriage is a normal good in this population.

Progresa/Oportunidades was implemented in Mexico to reduce poverty and its inter-generational persistence in rural Mexico. Beneficiary households receive a monetary transfer and basic healthcare, provided children regularly attend school and health centres. Progresa was implemented in 1998 and, out of 506 villages, 320 were randomly selected to receive the program and 186 to be the control group. Households in the latter group started receiving benefits in 2000 when the program was renamed Oportunidades. Until 2001, households with children enrolled in primary and secondary school were eligible for the benefit. From that year onward, the benefits were extended to high school. In 2003 the evaluation team surveyed a new set of villages that were selected as the new control group using propensity score matching. The staggered implementation of the program and the rich panel structure of the data allow me to obtain dynamic causal treatment effects by comparing the three groups for 6 years through a staggered differences in differences strategy. As the third group is created by propensity score matching, I use a doubly robust estimator that uses both outcome regression and inverse probability weighting to improve the comparability between the groups.

Exposure to the program increased the probability of marrying before 18 years old. One year after the start of the program, the effect was small, of 0.8p.p, although statistically different from zero (CI=[0.002, 0.015]). After five years of exposure to the program, beneficiaries were 3.5p.p (CI=[0.01, 0.06]) more likely to be married than the control group. This corresponds to more than doubling the marriage probability for treated individuals. These effects were largely driven by treated girls, who, after 5 years of program exposure, were 7 p.p (CI=[0.01, 0.11]) more likely to be married than non-treated girls. This effect is indistinguishable from zero for boys (the point estimate after 5 years is 0.005, with the 95% confidence interval ranging from -0.02 to 0.02).

For girls, I also investigate whether the treatment effect varies across age groups by looking separately at three cohorts: (i) girls between 6 and 7 years old at baseline, in 1997; (ii) girls aged 9 to 11 in 1997; and (iii) girls between 12 and 14 at baseline. The point estimate is positive across all years for all age groups and is increasing in age. From 2001 onward, this effect is large and statistically significant for girls in the second and third age cohorts. For each age cohort, I also observe that the program had a similar effect for girls in T1998 (first group being treated) and for those in T2000 (second group being treated). This suggests that being exposed longer to Progresa/Oportunidades did not change the magnitude of the program's impact on marriage. This finding is consistent with Behrman et al. (2005), who, among other outcomes, compare T1998 and T2000's marriage status and found no difference. In this paper, I add a pure control group and look at the long-run effects of the program. I show that if girls were in treated villages at ages at which marriage risk is high, then the program enabled marriage. For example, I find that girls 16 or 17 years old in 2001 were around 7p.p more likely to be married in both treated groups compared to similar girls in control villages.

Looking separately at each treated cohort, I observe that the program's effect on marriage is larger in magnitude from 2001 onward. This observation suggests that the changes in the program in the same year, which included expanding the benefit to high school years, might be relevant to explain the effect. I test this hypothesis by comparing individuals who are likely to be in high school from 2001 to 2003 to those who are younger, thus unlikely to be in high school. I observe that the first group primarily explains the program's positive effect. The effects are positive but not statistically significant for those who were not in high school in those years but were beneficiaries of the program. I reject the null that the effects are equal for these two groups. Since marriage probability is increasing in age, I further estimate the program's effect on those individuals who were likely in high school but before the changes in the program. I also find no effect for these. This evidence suggests that receiving the benefit at an age in which marriage risk is high - most early marriages in Mexico happen between 15 and 17 years old - enables that decision for girls in treated villages.

I build a conceptual framework that reconciles the empirical findings if marriage is a normal good. Individuals derive utility from consumption and marriage. In each period, single individuals draw the quality of their potential match and decide whether to marry that partner in the next period, and whether to go to school. I assume positive returns to education in both labour and marriage markets. First, I solve the model numerically, calibrating it to moments of the data before the program's introduction. Then, I compare the model's predictions regarding the proportion of individuals in each state in a world with and without school subsidies. The program's introduction leads to an increase in the number of individuals in school and married. I observe the same pattern in the data, where more individuals in treated villages transition from single to married status, particularly to married and in school simultaneously.

I test empirically whether marriage is a normal good and I show that, in this context, marriage probability increases with income. Since all beneficiaries who received the program had to comply with the education requirements, in this context, increases in schooling cannot be isolated from the effect of the monetary transfer. Therefore, I restrict the analysis to all children (female and male) who are no longer eligible for the benefits themselves at the beginning of the program but who live with an eligible member. I find that the program led to increases in marriage that are substantively large. Since these individuals are exposed to an increase in income only, and not to the condition, this is evidence that when the budget constraint is relaxed, agents are financially enabled to marry. [3]

This paper offers three main contributions. First, I evaluate a large education-conditioned cash transfer and show that the program increased child marriage for girls. Second, I study the income effect of the program on the marriage market by restricting the analysis to a population exposed to increases in household income but not to increases in education. I find that, in this population, income increases led to increases in marriage.[4] This paper shows that in a context

[3] I test whether there are spillover effects on education in this population, and I observe that they do not get more education than their counterparts in the control group.

[4] My findings are consistent with Bobonis (2011) who finds that the program increased marriage for young

where marriage is a normal good and returns from education are low, education-conditioned cash transfers may lead to increases in child marriage. Finally, I add to the understanding of child marriage and its causes in a setting where there are no widespread marriage arrangements nor monetary transfers between families due to the union and where children are the decision-makers. These results are important for the design of large-scale programs. In particular, they highlight how context-specific features may determine the intensities of opposing mechanisms, such as income and education effects, and how these can generate unintended consequences.

This paper contributes to the broad literature on child marriage and its causes. In particular, it adds to the literature on income effects on marriage. The results contrast with the ones reported by Baird et al. (2011), who found that an unconditional cash transfer reduced the likelihood of marriage in Malawi. In contrast, a conditional cash transfer program did not. Contrary to what I find in Mexico, the authors show that, in Malawi, increasing disposable income leads to marriage delays. How income affects marriage decisions appears to be a determinant for the success of these programs regarding child marriage.

This paper also contributes to the literature on the effect of education programs on child marriage. I show that a program praised for its success in educational outcomes led to increases in marriage. My results contrast with Angrist et al. (2002) and Hallfors et al. (2015). Both studies find that two programs that decreased the cost of education in Colombia and Zimbabwe led to an increase in years of education and a decrease in the probability of marriage or cohabitation. An explanation for the different effects of education on marriage can be the lack of returns from education in Mexican rural labour markets (Attanasio et al., 2012). If education does increase the opportunity cost of marriage, thus not counteracting the positive effect income, the program will lead to higher rates of child marriage. In urban Mexico, for example, where returns from education are positive Gulemetova-Swan (2009) shows that Oportunidades led to a small but positive delay in the age at first marriage of 1 to 4 months. However, given how different rural and urban Mexico are and the differences in the program implementation across villages in the two settings, one cannot exclude other sources of heterogeneity. Another important difference is that in rural populations marriage has a higher social value and child marriage is vastly more

single women with low educational attainment.

common.[5] Furthermore, in 2002, Oportunidades added mandatory attendance to sexual and reproductive education sessions, family planning, gender and health and domestic violence for girls in high school in urban areas. Increasing education on these topics might have directly influenced marriage and fertility decisions. An alternative explanation could be that education also increases marriage, contrary to the theory's predictions. Ashraf et al. (2016), for example, shows how the marriage effect of education is context-dependent. They compare communities with bride price (husband's family pays to wife's family) and dowries (the other way around) in Zambia and Indonesia and find that the amount transferred increases in female education only in bride price groups. These results contrast with the ones found by Agarwal et al. (2022), who show that in India - where dowries are a common practice - education and youth are valuable in the marriage market, leading to young educated girls marrying earlier than less educated ones. Further investigation is necessary to derive a conclusion regarding where the Mexican case falls.

The rest of this article is organised as follows. Section 2 presents the context, where I introduce child marriage in the context of Mexico and describe in detail the conditional cash transfer program. Section 3 introduces the data used in this project and some relevant summary statistics, and Section 4 explains and motivates the empirical strategy used to estimate the program's effect on child marriage. In Section 5 I present the results and discuss them in Section 6, using a toy model and an empirical exercise testing the main model prediction. Finally, section 7 concludes this paper.

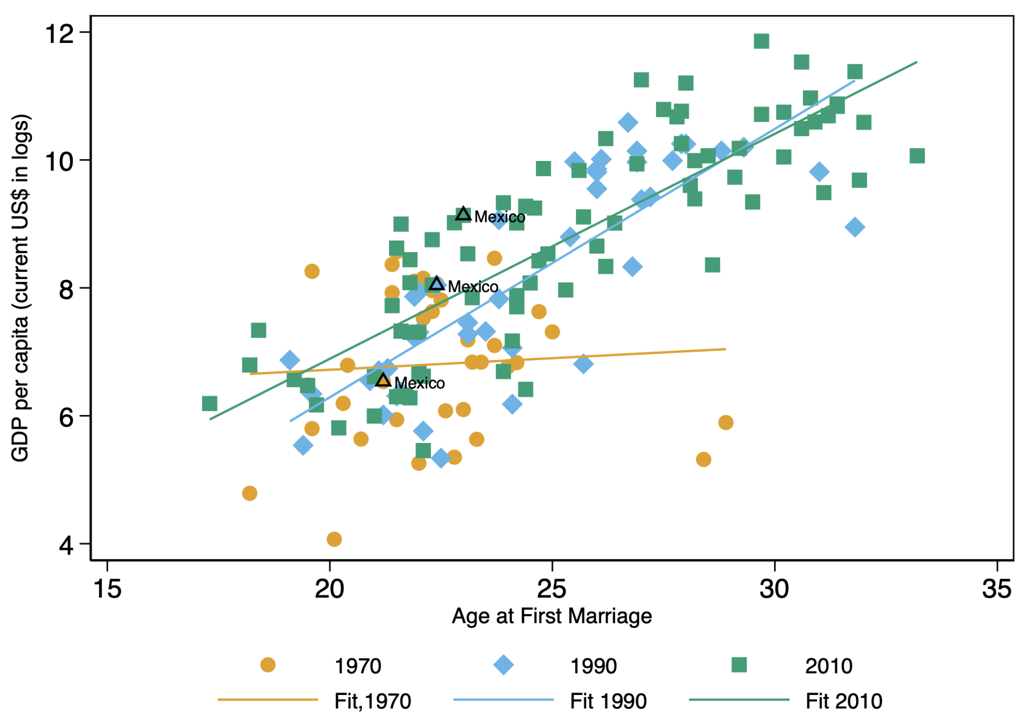
2 Context

In the past decades, Mexico has witnessed rapid and prosperous socioeconomic change. Usually, age at marriage correlates positively with economic progress, and child marriage is more prevalent in poorer societies. In Mexico, however, the marriage age has only increased slightly despite economic growth in the past decades. Figure 1 shows the positive correlation between

[5] According to the INSAD Report on Early Unions, in 15 out of 32 states in Mexico, child marriage rates in rural areas are higher than 30%, and no urban population exhibits such a high rate.

the average age at first marriage for women, and the country's GDP per capita, for several countries for 3 different years, 1970, 1990 and 2010. Except in 1970, in the most recent years, one would predict a lower per capita GDP for Mexico, given its average age at first marriage. For example, GDP per capita increased from 690 US$ in 1970 to 9.270 US$ in 2010, whereas women's average age at first marriage increased from 21.2 to 23 years old.

Figure 1: Correlation between Age at First Marriage and GDP per capita



Note: This graph presents a correlation between a country's GDP and age at marriage for several countries for three years. Each data point correcponds to a country in a given year. The lines represent the prediction for GDP from a linear regression of GDP on age at marriage. Data is from the World Bank Data Gender Portal. Data displayed in yellow circles correspond to 1970, in blue diamonds to 1990 and in green squares to 2010.

The witnessed economic development was particularly important for women. In Mexico, between 1970 and 2020, the percentage of women with secondary and tertiary education increased from 5 to 38% and 1 to 22%, respectively, and female labour market participation from 13 to 47% during the same period (Bhalotra and Fernández, 2021). The percentage of women aged 20-29 in consensual unions has decreased from 60 to 55% (World Bank), but child marriage rates have remained relatively constant, around 23% (UN Women). The adolescent fertility rate (births per 1000 women ages 15-19) was 114 in 1970 and halved in 2020 and overall fertility has been steadily decreasing from 6.6 live births in 1970 to 2.1 in 2020 (World Bank). Most early marriages occur as informal unions. Around 75% of the girls between 15 and 17 years old who were ever married or in a union report being in an informal union (Girls Not Brides). Since law enforcement is harder to implement, tackling this problem through legislative changes might not be efficient. A change in the state laws between 2014 and 2018 forbidding completely legal marriages under 18 years old led to a decrease in legal marriages that was offset by an increase in informal unions (Bellés-Obrero and Lombardi, 2020).

In Mexico, there are no widespread practices of dowries and price brides. Arranged marriages are rare; in most cases, children decide whether or not to get married rather than their parents. Given gender inequality and discriminatory social norms, the role of women in society does not focus on their occupation but on their ability to create and sustain a family. By becoming wives and mothers, they are better accepted in the community and gain respect from others. Gender disparity and conservative norms also play a role through constraints in girls' sexual lives (Brides, 2017; Taylor et al., 2019). Through marriage, girls are not subject to their family rules and restrictions on their sexuality and avoid the social stigma associated with out-of-lock pregnancy. It is also a mechanism to escape violent households and protect themselves from exploitative groups in areas with extreme violence. Finally, marriage might also offer economic stability, as formal insurance and labour market opportunities are limited (UNICEF, 2019; Parrado and Zenteno, 2002). According to the survey 'Lo que dicen los pobres', ran by the Secretary for Social Development in Mexico (SEDESOL) in 2003, 70% of the respondents resort to family first when facing problems regarding lack of money, and almost 60% seek family help first to improve housing conditions, around 65% count on family in case of an accident and 43% when they need a job. Family is a social institution in these countries; the wider it is, the better insurance it provides.

2.1 Progresa/Oportunidades

The positive correlation between poverty and child marriage has been documented worldwide, as well as the negative correlation between education and child marriage. In Mexico, a conditional cash transfer program, Progresa/Oportunidades, was implemented in 1998 to reduce poverty and its inter-generational cycle in rural areas through increased education. It does so through three sets of action: (i) offering basic health care to all family members; (ii) providing a fixed monetary transfer to be spent on food consumption and nutritional supplements, targeting children under two years old, malnourished children under five years old and pregnant and breast-feeding women; and (iii) monetary transfers to families with children in school, between the third grade of primary school and the third grade of secondary school. The benefits scheme for 1998 is in Table 1. They are increasing in grade and slightly higher for girls than boys in secondary school. Transfers consist, on average, of approximately 14% of eligible households' income (1400 pesos, equivalent to 173 USD in 1998). In 2001 the program underwent some changes, including its expansion, and was renamed Oportunidades. Significant changes for this analysis are the extension of benefits to high school (*preparatoria*) students and the provision of bonuses in case students passed grades [6]. I evaluate the effect of the program from its start, 1998, to 2003.

Table 1: 1998 Monthly Benefit (pesos)

Primary School	Boys	Girls
3rd Year	60	
4th Year	70	
5th Year	90	
6th Year	120	

Secondary School	Boys	Girls
1st Year	175	185
2nd Year	185	205
3rd Year	195	225

Note: This table presents the benefits scheme of Progresa in its first year of implementation. Children are eligible from the 3rd year of primary school until the third and last year of secondary school. Monetary benefits are increasing in schooling level and slightly higher for girls than boys in secondary school.

To receive these transfers, families must comply with a set of conditions. They must attend

[6] Oportunidades introduced *Jovenes con Oportunidades*, a component of the program that awards a monetary prize to those students who completed high school in less than 4 years and before turning 22 years old.

scheduled medical visits and at least 85% of classes/school activities. Primary and secondary school education has been mandatory since 1992, and although primary education had an enrolment rate of close to 90% in 1997, in secondary school, the rate was 65%.

The program was implemented in 1998 in 320 rural localities randomly chosen. Other 186 localities were randomly assigned to the control group. I refer to the first treated localities as T1998. All these localities fulfilled a set of geographic and socioeconomic criteria: they had to be highly deprived but with access to elementary school, middle school and a health clinic (Abúndez et al., 2006). In December 1999, the control group started receiving the program. I refer to this group as T2000. [7].

After the expansion of the program, in order to evaluate its long-term effects in 2003, the evaluation team selected a new control group of localities via propensity score matching. These are from the same states as the original 506 communities (except for one, for which the neighbour state was used). The matching was performed on aggregated locality aspects using individual data from the Census 1995 and 2000. These include housing and demographic characteristics, poverty level, labour force participation and ownership of durable goods. Besides, localities had to fulfil the program's eligibility criteria with respect to distance to schools and health clinics. I refer to this set of localities as C2000, the pure control group.

Inside each locality, eligible households were identified through socioeconomic data collected in 1997, assessing their poverty degree. On average, 78% of the households in the treatment group were eligible for the program and 97% accepted being beneficiaries (Dubois et al., 2012). Therefore, within each locality, I consider only those households eligible for the benefit (poor households).

The design of the program allows for comparisons across the three groups: (i) T1998 is the group of treated localities in 1998; (ii) T2000, the set of villages that started receiving the program only from 2000 onward; and (iii) C2000, the group of villages that did not receive the program until 2003.

[7] The last survey T2000 answered as a control group was set in November 1999, before the program's introduction. Therefore, for simplicity, I name this group T2000.

3 Data

The data used in this paper consists of a sample of households in both control and treatment villages of Progresa/Oportunidades. Households were surveyed in November 1997 (ENCASEH97) and March 1998 (before the introduction of the program), in October 1998, and twice in 1999 and 2000 (ENCELs). In addition, in 2003, a new survey (ENCEL2003) included all the households found in the original 320 treated localities and the new control group (C2000). The survey asked the control group current and retrospective questions, referring to 1997, 2000, 2001 and 2002.[8]

An in-depth attrition and missing data analysis is available in Appendix B. In summary, attrition increases with years, and it is higher for T1998 than T2000 (this difference is statistically significant from November 1999 onward. Since individuals in the treatment group are more likely to have missing information regarding marriage, I perform one robustness check using Lee bounds with inverse probability weights and tight bounds. Treating the data as if it was repeated crossection, I estimate a lower bound for the aggregate effect for girls of 2p.p, statistically different from zero at 1%, CI=[0.0176, 0.0293]. Besides attrition, some individuals' age does not progress as expected, or their gender swaps. These might indicate a mismatch in the IDs or misreporting gender or age. For the main analysis, I exclude all those observations in which gender is inconsistent, and age decreases. If I am stricter and drop those observations that show any inconsistency in age (either decreasing or unreasonably increasing), I obtain qualitatively similar results with larger magnitudes. A third problem concerns missing data regarding baseline characteristics, mainly in the control group. I exclude all observations for which I do not have complete information on these characteristics. Imputing missing values would introduce bias in the propensity score estimates due to the non-zero covariance across the predictors. Finally, 34% of the sample does not have information on education at baseline. Since the literature suggests that education is a good predictor of marriage decisions, I opt for excluding those observations with missing education in the main analysis. If I instead exclude the variable

[8] Although for the analysis I will only use outcome variables referring to 1999 and 2003, and baseline characteristics, I use the information of all surveys collected (including in 2007) in order to complete missing information.

from both the propensity score estimation and the outcome regression, thus still keeping those observations, I obtain qualitatively the same results, but with a lower magnitude.

The main outcome of interest is *marital status* from 1997 to 2003. I consider an individual married if they report being legally married, living in an informal union, cohabiting, divorced or widowed. I choose to do so since I am interested in first marriages, thus not accounting for separations. A child is single if she reported her status to be single. Marriage rates in 1997 were balanced in treatment and control groups. [9]

My population of interest are all children who were between 6 and 16 years old in 1997, the baseline year.[10] Keeping all those whose relevant information is non-missing, I have 25 thousand observations, roughly half of which are females. Recall that I am considering only poor households within each locality, meaning those eligible for the program.

3.1 Summary Statistics

Due to the non-randomness of the pure control group, I use a doubly robust estimator to assess the causal impact of the program on marriage. The estimator requires specifying two models: one for the treatment probability of the control group and one for the outcome regression (more details in Section 4). In order to present the summary statistics that more truthfully represent the sample used for the empirical analysis, I provide them using similar weights as the estimator. Individuals in treatment groups T1998 and T2000 receive weight 1. Individuals in the control group receive one of two weights, depending on which treatment group they are compared to. If the comparison is between C2000 and T1998, individuals in the control group receive a weight of $\frac{p(x)}{1-p(x)}$, where $p(x)$ is the probability of being in T1998 versus in the control group. To these weighted individuals, I will call C2000(IPW1998). Similarly, if they are compared to individuals in T2000, they receive the same weight, but $p(x)$ is instead the probability

[9] In ENCEL2007, individuals were asked age at first marriage or union. This information allows me to retrieve the age at marriage for individuals who married after 2003. I use this information only for the descriptive statistics and to complete marriage status in case of missing information from the other surveys.

[10] Of the entire sample of children, only 1.5% of those who married declare doing it when younger than 12 years old, therefore I assume that a child becomes at risk of marriage only at that age. I exclude from the sample all children who do not turn 12 years old until 2003. I also do not consider children over 16 years old in 1997, given that they were exposed to the program close to turning 18 years old.

of being in T2000 versus being in C2000. To these, I call C2000(IPW2000). Tables A1 and A2, in the Appendix, report parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns report the parameters without any re-weighting. The third and fourth columns report the estimates re-weighing the control group as described. In the four regressions, standard errors were clustered at the locality level. The last column presents each characteristic's unconditional and unweighted mean for the control group C2000. Note that after appropriately reweighing the control group, there are still statistically different variables across groups (10 out of 46), although significantly less than when compared with C2000 (30 out of 46).

Table 2 presents the proportion of married individuals by group and year for the whole sample (Panel (a)) and those who have not turned 18 until that year (Panel (b)). Across all years, there are more married children in the treatment groups than in the control group. However, in the first years of analysis, the proportions are close across groups, starting to diverge after 1999. The proportion of girls who are married is systematically larger than the proportion of boys who are married, across all groups (see Tables A7 to A8 in Appendix A.)

Of those reporting age at marriage, 1.5% married before turning 12, 18.6% married between 12 and 15, 30% married while 16 or 17 years old and almost 50% at 18 or later. From those who married before turning 18 years old, 60% married at 16 and 17, (see Figure A1 in the Appendix). The average age at marriage is 1.1 years lower for girls, 17, than for boys, 18.1.

4 Empirical Strategy

To estimate the program's causal effect on child marriage, I exploit the random and quasi-random allocation of the program across municipalities and the variation in the timing of implementation. I have information on three groups: (i) T1998, the first group receiving the treatment in 1998 and beyond; (ii) T2000, a group that has first received treatment in 2000; and (iii) a pure control group, C2000, that was never treated until 2003. I observe these groups from 1997 until 2003.

Table 2: Proportion of Married by Group and Year (in %)

(a) All

	1997	1998	1999	2000	2001	2002	2003
T1998	0.74	1.46	2.97	5.49	10.03	13.25	15.17
T2000	0.88	1.58	3.17	5.81	10.83	14.13	16.26
C2000	1.49	2.03	3.02	4.42	6.62	9.71	11.33
C2000(IPW1998)	0.65	1.18	2.11	3.42	5.02	6.91	8.13
C2000(IPW2000)	0.78	1.35	2.23	3.76	5.46	7.74	9.11

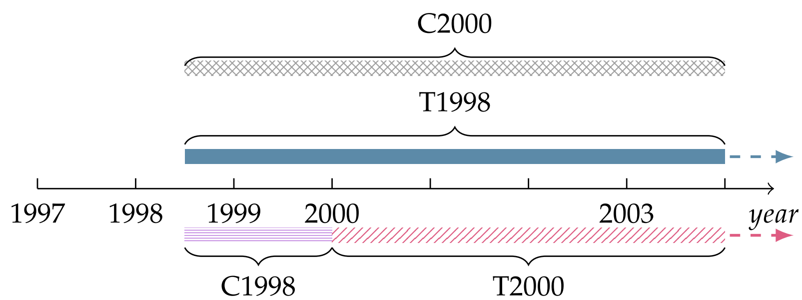
(b) Under 18 years old

	1997	1998	1999	2000	2001	2002	2003
T1998	0.03	0.04	0.55	1.34	2.30	3.70	4.42
T2000	0.00	0.02	0.37	1.16	2.62	4.18	5.02
C2000	1.49	2.03	1.66	1.88	2.19	2.90	2.55
C2000(IPW1998)	0.00	0.01	0.01	0.10	0.41	1.26	1.70
C2000(IPW2000)	0.00	0.01	0.01	0.12	0.48	1.49	2.01

Note: This table presents the proportion of married individuals by group and year. Panel (a) refers to all individuals between 6 and 16 years old in 1997, and Panel (b) refers to the same individuals until they turn 18 years old. T1998 are those individuals who started receiving the program in 1998. T2000 is the set of individuals who first received the program in 2000. C2000 is the control group. C2000(IPW1998) and C2000(IPW2000) are the control group weighted by the probability of being first treated in 1998 and 2000, respectively, versus being in the control group.

Figure 2 illustrates the program allocation across groups, the years of the analysis, and the role they represent in the empirical strategy.

Figure 2: Treatment and Control Groups Across Years of Analysis



Note: This figure presents the three groups I will be comparing: T1998, in full and blue, the first treated group; T2000, in both purple and horizontal stripes and pink slide stripes, to emphasize that the same group of villages is a control group until 2000 (purple and horizontal stripes) and joins the treated group from that year onward (pink and slide stripes); and C2000 the control group created by propensity score matching which was never treated, crosshatched and grey.

The staggered implementation of the program and the rich panel structure of the data allow me to obtain dynamic causal treatment effects by comparing the three groups over 6 years. I use the doubly-robust estimator proposed by Callaway and Sant'Anna (2021) for three reasons. First, it has been shown that in staggering designs, two-way fixed effects models with staggered treatment cannot be interpreted causally when treatment effects are heterogeneous. The intuition behind this is that the estimate for the causal effect at a certain time period might be contaminated by the treatment effects from other periods, even if the parallel trends and no anticipation assumptions hold.[11]

Second, this estimator allows using individual pre-treatment characteristics for more credible parallel trends assumptions. Controlling for these characteristics allows comparing more simiar individuals across the groups of localities. Improving this comparison is particularly important when using the pure control group (C2000). On average, unobserved characteristics of villages in T1998 and T2000 should be uncorrelated with treatment allocation due to the random assignment to the program across these localities. However, the selection of villages in C2000 assumes that, given the observed characteristics, the treatment allocation was as good

[11]See for example Goodman-Bacon (2018) Athey and Imbens (2018), Borusyak and Jaravel (2017), de Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2021) and Abraham and Sun (2020).

at random. Including individual attributes strengthens the plausibility of the assumption since the comparison is, then, across similar individuals in similar municipalities. Callaway and Sant'Anna (2021), from now on, the CS estimator allows doing this through the combination of outcome regression and inverse probability weighting approaches. Outcome regression adjustment allows for covariate-specific trends in potential outcomes across groups. For example, if the potential outcome (marriage) evolution in case of non-treatment depends on covariates (e.g. gender and age), conditional parallel trends are less restrictive. The causal treatment effect is identified as long as the remaining unobserved characteristics affecting the outcome are time-invariant. Inverse probability weighting allows re-weighting the observations by the estimated treatment assignment probability to improve comparability across groups. The identifying assumption is that, conditional on these characteristics, the treatment assignment was as good as random. Third, this doubly robust estimator identifies the average treatment effect for each group at a given point in time even if either the propensity score model *or* the outcome regression models are misspecified, but not both.

The CS estimator identifies a group-time causal effect if the following assumptions hold. First, I need to assume that the overlapping condition is satisfied. Meaning that at least a small fraction of the population is treated at each "starting" period (when treatment starts for each group) and that for all periods, the propensity score is uniformly bounded away from one[12]. Second, treatment must be irreversible, meaning that if a group is treated at time t, then it is treated at $t+1$ for any t, which this design satisfies. The third assumption requires limited treatment anticipation: individuals could not anticipate they would be beneficiaries of the program prior to its implementation. Attanasio et al. (2012) find no evidence of anticipatory behaviour by any of the cohorts. The fourth and final assumption is the conditional parallel trends assumption: in the absence of treatment, the average conditional outcome of the group first treated at time g ($G_g = 1$) and the not yet treated groups by the time $t + \delta$ ($D_s = 0, G_g = 0$), $\delta \geq 0$, would have evolved in parallel. Based on the cited evidence, assume there was no anticipation, $\delta = 0$. So I assume conditional parallel trends between t and $t-1$ between

[12]In practice, I exclude from my total sample 15 observations that have an estimated propensity score higher than 0.999.

group $g \leq t$ and groups that are 'not-yet-treated' by time $s \geq t$. Formally, the parallel trends assumption requires that for any periods $(s,t) \in \{2,\ldots,\mathcal{T}\} \times \{2,\ldots,\mathcal{T}\}$, and cohort $g \in \mathcal{G}$, such that $t \geq g, s \geq t$,

$$\mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, G_g = 1\right] = \mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, D_s = 0, G_g = 0\right], \tag{1}$$

where G_g is a binary variable equal to one if the unit belongs to the group that was first treated in period g, and D_s is a binary variable equal to one if the unit is treated at s and zero otherwise.

A common practice used to support the parallel trends assumption is to test whether there are different pre-treatment trends for treated and control groups. The idea is that conditional on observed characteristics, the change in the outcome that the treated group would have if they had not participated in the treatment is the same as the change observed for the untreated group. Conditional on the observed characteristics, the groups' evolution only differs due to their treatment status.

The estimand of interest is the average treatment effect at time t for the group that was first treated in period g, using the groups that were not yet treated for comparison. It is defined as

$$ATT_{dr}^{ny}(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}\left[G_g\right]} - \frac{\frac{p_{g,t}(X)(1-D_t)\left(1-G_g\right)}{1-p_{g,t}(X)}}{\mathbb{E}\left[\frac{p_{g,t}(X)(1-D_t)\left(1-G_g\right)}{1-p_{g,t}(X)}\right]}\right)\left(Y_t - Y_{g-1} - m_{g,t}^{ny}(X)\right)\right], \tag{2}$$

where $p_{g,t}(X)$ is the propensity score, or the probability of being first treated in period g conditional on covariates X and conditional on either being treated the first time at g, ($G_g = 1$), or "not-yet-treated" by time t, ($(1-D_s)(1-G_g) = 1$). Y_t is the outcome of interest at time t, and Y_{g-1} is the outcome at baseline before the unit is treated. Finally, $m_{g,t}^{ny}(X)$ is the expected outcome evolution from baseline to time t, conditional on covariates X for the not yet treated, $m_{g,t}^{ny}(X) = \mathbb{E}\left[Y_t - Y_{g-1} \mid X, D_t = 0, G_g = 0\right]$.

The estimation follows a two-step strategy. The first step estimates the propensity score and outcome regression, $p_{g,t}(X)$ and $m_{g,t}^{ny}(X)$. In the second step, the fitted values of these estimands are plugged-in the sample analogue of the ATT to obtain its estimate.

Callaway and Sant'Anna (2021) propose a bootstrap procedure for inference that can account for clustering. I cluster the standard errors at the locality level since this was the unit of treatment randomization.

4.1 Threats to Identification

Progresa/Oportunidades was first implemented in the poorest Mexican villages, and the set of villages included in C2000 by the program was determined by a matching model to select those localities that were the closest possible to the treatment groups.

Due to potential differences across individuals in the treated and control villages, it is important to ensure the comparison between individuals for whom, conditional on a set of characteristics, treatment was equally likely and/or for whom outcomes would have evolved similarly.

I use two sets of characteristics for the propensity score and the outcome regression models: (i) those that are important to determine outcome progression - motivated by the literature on the determinants of marriage; (ii) and those that are determinants of treatment status - stated and used by the program authorities. Despite the different motivations for including the different variables (either them being relevant for the outcome evolution or the treatment status), both models include all variables.

The propensity score model is misspecified if its functional form is not the true one and/or if it does not include all relevant characteristics that predict treatment status. The functional form chosen is the logistic function. Regarding treatment status, the program's documentation lists the characteristics used to calculate the marginality index of the village, which determined treatment eligibility. I use the same variables for determining the eligibility of individuals: adult literacy, the existence of water in the dwelling, drainage system and electricity, floor quality, number of occupants for room and labour market occupation. Besides, I add wall quality and asset/durable goods possession, which are good proxies for wealth, household composition, and a poverty index score calculated by the program. [13]

I also include gender, age, education level at baseline, indigenous background, and household head and spouse characteristics. The marriage literature has identified these characteristics as important determinants of marriage decisions besides wealth, as mentioned earlier and household composition. Furthermore, qualitative evaluations of Progresa/Oportunidades suggest heterogeneous effects over these dimensions (Latapí and Gonzales de la Rocha (2009)). [14]

The final specification is very close to the one used by Diaz and Handa (2006), who show that propensity score matching performs well in the evaluation of Progresa, replicating the RCT results.[15] They show that for outcomes that are measured comparably across survey instruments, which is the case of marriage, matching estimates on a non-experimental sample are not statistically different from the experimental estimates. They also show that the larger the set of (relevant) covariates, the larger is the reduction in the bias.[16] Additionally, a common practice to assess propensity score misspecification is to compare the density of the propensity score between treatment and control groups. I show that despite the low proportion of individuals

[13]**Housing characteristics**: dummy variables for dirt floor, inferior quality wall, inferior quality roof, number of bedrooms, piped water, electricity, ownership of animals, land, blender, refrigerator, gas-stove, gas-heater, radio, tv, dishwasher, car and truck; **Household composition**: the number of members in the household and dummy variables for having at least one child between 0 and 5, at least one teenager between 16 and 19, at least one woman between 20 and 30, 40 and 59 and 60+, respectively, and at least one man between 20 and 30, 40 and 59 and 60+, respectively;

[14]**Head and Spouse characteristics**: if any of them had ever gone to school, if any of them worked the week before, if anyone in the household speaks an indigenous language, if the spouse of the household head is a housewife, if the household head is a woman and the age of the household head. Given the large number of missing data on education levels, working status and indigenous language of either the head or the spouse of the household, I decided to use variables at the couple level (e.g. either chief or spouse worked the week before), instead of the two separately. For the same reason, instead of using the education level of both, I use if any of them had ever gone to school. Finally, a household with indigenous background is one where at least one person speaks an indigenous language.

[15]My specification includes the same variables than the ones used in Diaz and Handa (2006), except for access to social security. I add more variables that I believe are important determinants of wealth, treatment heterogeneity and marriage.

[16]My specification is also similar to the one used by Behrman et al. (2011), that estimate the effect of the program on education. The biggest difference is that I am not using missing variable flags; instead, I am losing the observations for which there is no information on baseline characteristics (see Appendix B). Despite these differences, I can replicate the results from the paper mentioned above regarding the program's effect on educational achievement.

in the treated group with low propensity score values, there is overlap across the entire distribution. I also show evidence of balance in the baseline characteristics across treated groups and the re-weighted control group, using the probability of being in one of the two treatment groups as weights, as explained in section 3.1 and shown in Tables A1 to A6 in the Appendix, for all individuals and separate by gender. Given that some means are statistically different across groups, I run the main analysis of the paper using the improved doubly robust DiD estimator for the ATT based on the inverse probability of tilting and weighted least squares, after which there is, by construction, perfect mean balance, and the results are robust.

5 Results

5.1 Probability of Marriage

I start by analysing if, overall, the program impacted the probability of child marriage. In this set of results, and when not stated otherwise, I consider only the individuals until they turn 18. Table 3 shows that the program increased, on average, the probability of early marriage by 1.9 percentage points (p.p) (with the lower bound of the 95% confidence interval being 0.012, and the upper bound 0.026, hereafter CI=[0.012, 0.026]), significant at 1%. This effect corresponds to more than doubling the marriage rate compared to the control group (the average marriage rate for C2000(IPW1998) is 1.3% and for C2000(IPW1998) 1.5%).

Then, I explore how this effect varied with the length of exposure to Progresa/Oportunidades. Figure 3 shows the effect of the program on the probability of being married by the number of years exposed to the benefit (these results are also in Table A12 in the Appendix). For instance, time -1 represents one period before treatment, so for group T1998, $t = -1$ corresponds to 1997 and for T2000 to 1999. Similarly, time 2 represents two years after treatment. Note that the effects in times 4 and 5 are only estimated using T1998, which was the only group to be treated for more than 3 years. Similarly, for period -2, which is only observed for T2000. In the pre-treatment periods, I do not reject the null hypothesis of no effect of the program at any conventional significance level, supporting evidence that the parallel trends assumption is likely

Table 3: Average Treatment Effect of Progresa/Oportunidades on Marriage

	All		
	All	T1998	T2000
ATT	0.019	0.0181	0.0206)
	(0.0037)	(0.0044)	(0.0057)
	[0.0117 , 0.0262]	[0.0086 , 0.0275]	[0.0082 , 0.0329]
Control Mean		0.013	0.015
N		25623	

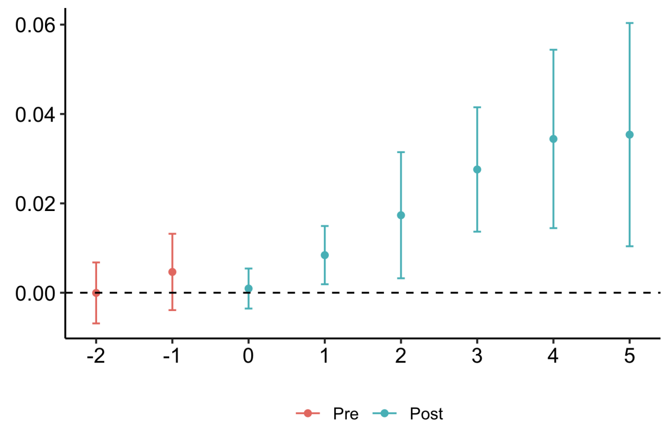
Note: This table presents the aggregated average treatment effect on the treated. "All" represents the estimate using as treatment groups both T1998 and T2000. The second and third columns separately present the average treatment effect over time for treatment groups T1998 (who first received the treatment in 1998) and T2000 (who first received the treatment in 2000). Standard errors were obtained through clustered bootstrap at the randomization level: locality. The average marriage rate for C2000(IPW1998) is 1.3% and for C2000(IPW1998) 1.5%.

to hold.

Then, I observe that in its first year of implementation, the program ($t = 0$) did not affect child marriage. However, it started leading to increases in marriage after one year of exposure. One year after receiving the benefit ($t = 1$), treatment groups were 0.8 p.p (CI=[0.002, 0.015]) more likely to be married than the control group. This effect increased to 2.8 pp (CI=[0.014, 0.042]) in the third year and around 3.5 p.p (CI=[0.01,0.06]) after five years, statistically significant at 1%. For reference, the unconditional and unweighted proportion of married individuals in the control group was 2.55% in 2003, so the effect corresponds to more than doubling marriage incidence.

Across treatment groups, I observed a positive trend in the estimated coefficients one year after the program started for T1998. However, these were not statistically different from zero until 2001, when beneficiaries were 2.4 p.p (CI=[0.005,0.044]) more likely to marry before turning 18 than non-beneficiaries. In 2000, the point estimate was already substantively significant, 1.3p.p., but the estimates are noisy, with a 95% confidence interval ranging from -0.006 to 0.03p.p. After 5 years of exposure, beneficiaries of T1998 were 3.5p.p (CI=[0.01,0.06]) more likely to marry, 3 times more likely than the control group (C2000(IPW1998)). Figure A8 and Table A13 in the Appendix show these results.

Figure 3: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure



Note: This figure presents the average treatment effect on the treated by the length of exposure to treatment. Time -1 represents one period prior to treatment. For T1998 (group that first received the treatment in 1998) corresponds to 1997 and for T2000 (group that first received the treatment in 2000) to 1999. Period 2 represents two years after treatment, and so on. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustered bootstrap at the randomization level: locality. The p-value for the pre-test of parallel trends assumption is 0.38.

For the second treatment group, T2000, the program increased marriage after the first year of implementation. In 2001, the effect is 2 p.p (CI=[0.005,0.04]) , 2.6 p,p (CI=[0.005,0.046]) in 2002 and 3.4 p.p (CI=[0.013,0.05]) in 2003, significant at 1%. These results hint that the changes made in the program around 2001 (from Progresa to Oportunidades) were important to explain the program's positive effect on marriage.

5.2 Heterogeneous Effects

5.2.1 Gender

Around the world, child marriage is a more prevalent phenomenon among girls than boys. Also, the socio-economic consequences associated with child marriage are known to be more damaging for females than males due to early childbearing, higher likelihood of formal labour market exclusion and violence. Hence, in this section, I look at the heterogeneous effect of the program by gender. Table 4 shows that the large treatment effects on girls drive the overall effects. On average, the program increased the probability of child marriage for girls by 3 p.p

(CI=[0.0183, 0.0472]). However, this effect was not substantively significant for boys, 0.7p.p (CI=[0.0003, 0.01]).

Table 4: Average Treatment Effect of Progresa/Oportunidades on Marriage, by Gender

	Girls		
	All	T1998	T2000
ATT	0.0327 (0.0074) [0.0183 , 0.0472]	0.0312 (0.0092) [0.0118 , 0.0506]	0.0355 (0.0094) [0.0156 , 0.0554]
Control Mean		0.019	0.024
N		12350	

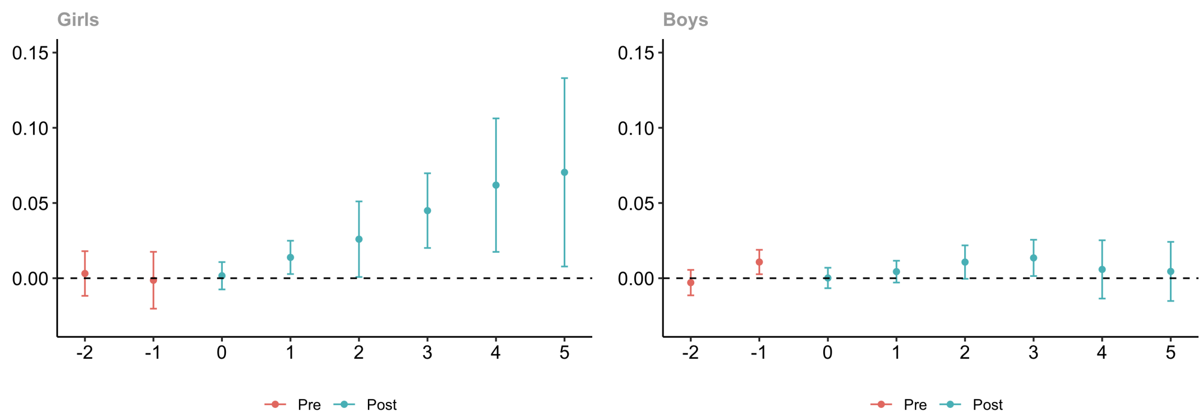
	Boys		
	All	T1998	T2000
ATT	0.007 (0.0034) [3e-04 , 0.0137]	0.0058 (0.0033) [-0.0013 , 0.0129]	0.0092 (0.0061) [-0.0037 , 0.0221]
Control Mean		0.007	0.007
N		13273	

Note: This table presents the aggregated average treatment effect on the treated by gender. In the first column of each gender, "All" represents the estimate using as treatment groups both T1998 and T2000. The second and third columns separately present the average treatment effect over time for treatment groups T1998 and T2000. Standard errors were obtained through clustered bootstrap at the randomization level: locality.

After 1 year of exposure to Progresa/Oportunidades, girls were, on average, 1.4 p.p (CI=[0.0028, 0.0249]) more likely to be married if living in a beneficiary village, significant at 1%, and after 5 years the probability increased 7 p.p (CI=[0.008, 0.133]). In 2003, in the weighted control group C2000(IPW1998) 2.21% of the girls were married, thus the program quadrupled the likelihood of marriage for girls in T1998. The point estimates are positive and increasing for both treatment groups across the years, but it is after 2001 that they start being meaningful (see Figure A9 and Table A15 in the Appendix for the estimates for each treatment cohort separately). For reference, in 2003, the unweighted proportion of married girls under 18 in the control group was 4%, and this proportion is lower appropriately re-weighted - between 2 and 2.9% (see Table

A7).

Figure 4: Effect of Progresa/Oportunidades on the Probability of Marriage by Year and Gender



Note: This figure presents the average treatment effect on the treated by treatment group and time for girls and boys separately. Group 1998, or T1998, is the group that first received treatment in 1998 and Group 2000, or T2000, is the group that first received treatment in 2000. In red are the estimates before treatment started, and in blue after. The left panel restricts the analysis for girls and the right panel for boys. Standard errors were obtained through clustered bootstrap at the randomization level: locality.

Results for boys, presented in Figure 4 (and A10 in the Appendix) are to be interpreted cautiously, as I reject the null hypothesis of no pre-trends. There seems to be a positive trend before the program started for boys in T2000, so it is plausible that they were already behaving differently pre-treatment, and thus the post-treatment results might not be due to the program but a product of those pre-existing differences. Despite overall positive point estimates, most were not statistically different from zero, with low magnitudes. For the disaggregated results by treatment group, see Tables A16 and A17, in the Appendix.

Those girls for whom I have information on their partner's age were, on average, 3.5 years younger than their partners. 60% of these girls married older men, so I look at the program's effect on young men up to 30 years old at baseline. For this population, I found that older men in eligible and non-eligible households in treated villages were likelier to marry than those in control villages.[17]

In summary, after Progresa/Oportunidades was introduced, girls in households who were eligible to receive the program in beneficiary villages were more likely to be married before 18

[17] Results available upon request. Just like Bobonis (2011), I also found a positive effect on older women who were single at baseline.

years old when compared with similar girls in villages that did not receive the conditional cash transfer program. The same does not happen for boys under 18 years old, but I observe an increase in marriage probability for older men.

5.2.2 Age

Since marriage is positively associated with age, it is also interesting to investigate whether the program had heterogeneous effects across this dimension. Given the results in the previous section, I restrict this analysis to girls.[18] Therefore, I split the sample into three age groups, defined at baseline: (i) girls aged between 6 and 8 in 1997, (ii) girls from 9 to 11 years old, and (iii) girls from 12 to 14 years old. Recall that I stop considering individuals once they turn 18. Therefore, the last year I observe the oldest group is 2002, since in 2003 all of these children would have turned 18 years old. For the same reason, I do not consider girls 15 and 16 years old at baseline since I would not have post-treatment periods for those in T2000.

Across the three age groups and the two treatment groups I observed the same pattern as in the aggregated results. Figure A11 shows the effect of Progresa/Oportunidades on early marriage separately for girls in T1998 and T2000. Note that girls in T1998 are being compared to those in T2000 until 1999 (including) and those in the pure control group, C2000. Those in T2000 are being compared exclusively to the pure control group. The fewer observations in each age group made the estimates noisier, but the point estimates were consistent with the aggregate results. The program increased marriage across all ages and treatment groups, particularly after 2001. The magnitude of the effect increased with age.

Those girls first exposed to the program in 1998 were more likely to be married 5 years after the program started than their non-beneficiary counterparts. I find that those girls between 12 and 14 years old in 2002 were 2.1p.p (CI=[-0.018,0.06]) more likely to be married then. Although not statistically significant, the upper bound of the confidence interval corresponds to an effect of 6p.p. Those who were between 15 and 17 in 2002 were 4.5p.p (CI=[0.012,0.077]) more likely to be married then. The effect increases to 8.5p.p (CI=[-0.026,0.197]) for those girls who were

[18] Analyzing just boys, results suggest positive but small effects at younger ages and no significant effect for the last age group.

17 years old after 5 years of program exposure. The magnitudes for T2000 are very similar for each year. The similar effects across T1998 and T2000 suggest that the length of exposure to the program does not affect marriage decisions. What appears relevant is having been exposed to the program and the girl's age at that given year.

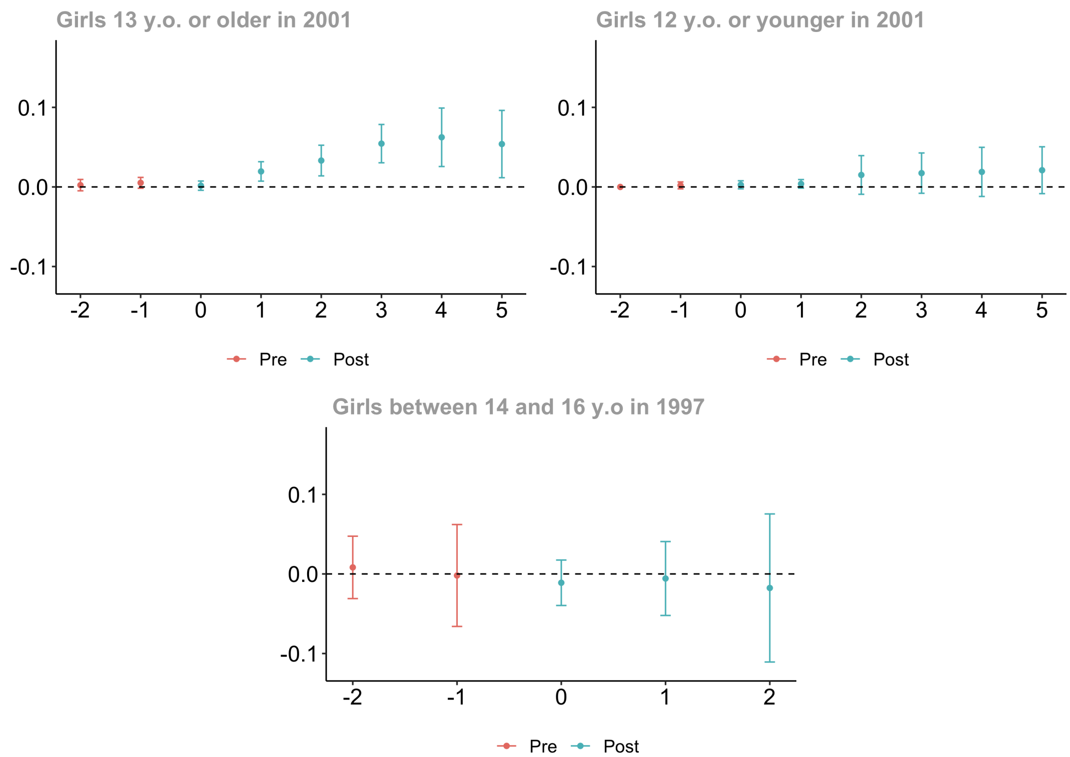
The program started having more substantial effects in 2001 when it extended benefits to secondary high school. For example, the girls who were 13 or 14 years old in 1997 and turned 16 or 17 in 2000 have a lower marriage probability than those girls who turned 16 or 17 in 2003. Students are supposed to reach secondary school at around 15 years old if they do not repeat any year. This observation suggests that the changes might be relevant to explain the effect.

I test this hypothesis by comparing the program's effect across three groups. The first group are all the girls older than 13 in 2001, who were likely be in high school between then and 2003. The second group are girls younger than 12 years old in 2001, who were unlikely to be in high school during this period. To compare girls of similar ages, the third group are girls between 14 and 17 in the years prior to the benefits extension. If these were in high school until 2001, they received no benefit.

Figure 5 shows evidence in favour of the previous hypothesis. The first graph shows that the program had large and statistically significant effects on the group likely to receive benefits during high school. For the younger cohort, the point estimates are positive but not statistically significant. The two age groups do not behave differently before the implementation of the program, but the effect is significantly larger for the older cohort than the younger (see Figure A12 in the Appendix). Comparing treatment and control individuals that were similar in age to the first group but did not receive benefits during high school age, I found no effect of the program. Figure A13 in the Appendix shows that we can reject the null of equal effects for girls older than 13 in 2001 and girls between 14 and 16 in 1997.

Most early marriages in Mexico happen between 15 and 17 years old; thus, receiving the benefit at this age facilitates that decision for girls in treated villages.

Figure 5: Effect of Progresa/Oportunidades on the Probability of Marriage by Group Receiving Secondary High School Benefits



Note: This figure presents the average treatment effect on the treated by treatment group and time for three different groups: (i) girls 12 y.o. or younger in 2001, (ii) girls 13 y.o. or older in 2001, and (iii) girls between 14 and 16 y.o. in 1997. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustered bootstrap at the randomization level: locality.

5.3 Descriptive Characteristics of Married and Single Girls

In light of the negative association between child marriage and female well-being, it is important to understand whether married girls are worse off than their single counterparts in this context. In this section, I present the differences between single and married girls in 2003 on important characteristics: education, fertility, labour market outcomes and their partner.

Importantly, these statistics must be interpreted cautiously due to the small sample sizes for each characteristic of interest. Furthermore, these are descriptive statistics and should not be given a causal interpretation due to self-selection into marriage. In this sample, there are about 14,500 girls, of which 3,387 were married by 2003. Out of these, I was able to identify close to 770 partners. Fertility questions were only asked in 2003 to those girls above 15 years old (a total of 10,250), but I only have information on pregnancy for 2,174 girls, out of which 515 had been previously pregnant. Table 5 shows the summary statistics by marriage status and treatment group. The first two columns report the mean for each characteristics for treated and control groups, respectively, and in parentheses the number of observations. The third column reports the mean of the re-weighted control group, re-weighting each observation by $\frac{p(x)}{1-p(x)}$, where $p(x)$ is the probability of ever being treated. The last column reports the parameter estimated and the t-statistic (in parentheses) for a regression of the characteristic on a treatment indicator, re-weighting the control group as described.

On average, girls in treatment villages were more educated than those in control villages, even if they were married. The difference for married individuals is, however, not statistically significant. Within the treatment group, single girls in 2003 were, on average, more educated than married girls living in the same group of villages. The average level of education for single girls in the weighted control group C(IPW) was also higher than for married girls.

Single girls in the treatment and control group had similar age at first sexual relationship. Married girls in the treatment group, however, started having sexual relationships 2.27 years younger than girls in the weighted control group.

Unsurprisingly, pregnancy was more common among married girls, and around 80% in the treatment group had been pregnant by 2003, versus 86% in C(IPW), this difference is not

Table 5: Summary Statistics of Girls and Partners in 2003

(a) Single

	T	C	C(IPW)	Difference(IPW)
Education Level in 2003	6.88	6.48	6.34	0.54
	(6,600)	(1,985)		(5.61)
Age at first sexual relationship	16.87	16.85	17.03	-0.16
	(60)	(61)		(-0.25)
Ever Pregnant	0.04	0.03	0.02	0.03
	(720)	(642)		(3.21)
Age at first child	17.37	17.47	16.69	0.67
	(30)	(17)		(1.66)
Total no. of children in 2003	1.82	1.61	1.47	0.36
	(17)	(18)		(0.87)
Occupation: Unpaid housekeeper (week before survey, 2003)	0.18	0.22	0.11	0.07
	(6,500)	(2,143)		(4.30)
Worked for money (week before survey, 2003)	0.55	0.56	0.58	-0.03
	(1,706)	(751)		(-0.47)
Occupation: agriculture (week before survey, 2003)	0.07	0.09	0.09	-0.02
	(1,167)	(520)		(-0.55)

(b) Married

	T	C	C(IPW)	Difference(IPW)
Education Level in 2003	6.73	6.48	6.08	0.65
	(1,920)	(403)		(1.47)
Age at first sexual relationship	16.66	16.50	18.94	-2.27
	(74)	(72)		(-2.11)
Ever Pregnant	0.81	0.82	0.86	-0.05
	(174)	(79)		(-0.68)
Age at first child	16.89	17.97	19.69	-2.80
	(132)	(61)		(-3.61)
Total no. of children in 2003	1.88	1.76	1.31	0.57
	(59)	(63)		(2.18)
Occupation: Unpaid housekeeper (week before survey, 2003)	0.36	0.65	0.40	-0.03
	(617)	(215)		(-0.34)
Worked for money (week before survey, 2003)	0.71	0.65	0.70	0.01
	(226)	(57)		(0.14)
Occupation: agriculture (week before survey, 2003)	0.08	0.14	0.04	0.04
	(166)	(36)		(0.86)

(c) Partners

	T	C	C(IPW)	Difference(IPW)
Age difference between wife and husband	-3.76	-3.17	-3.73	-0.03
	(82)	(115)		(-0.04)
Partner's education level by 2003	4.81	5.88	5.41	-0.60
	(83)	(108)		(-1.40)
Partner worked for money (week before survey, 2003)	0.96	0.97	0.98	-0.02
	(54)	(102)		(-0.63)
Partner's occupation: agriculture (week before survey, 2003)	0.37	0.41	0.32	0.05
	(81)	(101)		(0.60)

Note: This table shows the summary statistics by marriage status and treatment group. Panel (a) restricts the analysis to girls who were single in 2003. Panel (b) to girls who were married in 2003. And Panel (c) to the partners of the married girls. The first two columns report the mean for each characteristics for treated and control groups, respectively, and in parentheses the number of observations. The third column reports the mean of the re-weighted control group, re-weighting each observation by $\frac{p(x)}{1-p(x)}$, where $p(x)$ is the probability of ever being treated. The last column reports the parameter estimated and the t-statistic (in parentheses) for a regression of the characteristic on a treatment indicator, re-weighting the control group as described.

statistically different from zero. This rate was at most 4% for single girls, larger for treated than control groups.

Married girls in treated groups had their first child earlier than married girls in the control group. They also had more children than in the control group, both if they were single or married.

As expected, single girls were less likely to work as housekeepers across all treatment groups and less likely to have worked for money. On the other hand, married and single girls were equally likely to work in agriculture as their main occupation, but these rates did not go above 10%.

Finally, across treatment groups, girls married boys who were 3.7 years older than them and less educated than them. Almost all partners have had a remunerated job the week before they were surveyed and around 34% of them worked in agriculture. Women in treatment and control groups appear to be marrying similar partners, regarding these characteristics.

6 Discussion of Results: Mechanisms

In the previous analysis, I find that the conditional cash transfer program Progresa/ Oportunidades led to an increase in the marriage probability for girls under 18 years old. Although the program was not targeted at reducing child marriage, this result might be surprising. The program led to increases in education, which is an important mechanism for decreasing child marriage (Angrist et al., 2002; Hallfors et al., 2015; Ashraf et al., 2016; Kırdar et al., 2018; Skirbekk et al., 2004; Ferré, 2009). If there are labour market returns from education, increases in education should lead to an increase in the opportunity cost of marriage. In Mexico, however, this might not be the case. First, there is evidence that education may be an imperfect measure of human capital accumulation. Behrman et al. (2005) found no evidence that the program led to better grades, and Dubois et al. (2012) found that the program harmed the probability of passing grades for secondary school students. Second, Attanasio et al. (2012) showed that the relationship between wages and education is flat in rural Mexican villages. If this is the case, then education may not affect directly marriage decisions. These considerations still do not predict the increase in early marriage that I observe. The increase may have been caused by the monetary transfer received by eligible households that complied with the conditionality.

The transfers may reduce reliance on marriage as a safety net by relaxing budget constraints. On the other hand, the household's extra income may increase boys and girls' desirability on the marriage market, change their network, and/or it may facilitate marriage by making wedding expenditures more affordable. Mier y Terán (2004) argue that higher earnings and the opportunity for better jobs in rural Mexico allow young people to create an independent household and marry earlier. Similarly, early marriage is more common in land-holding households suggesting that wealth is an asset in the marriage market.

In this section, I provide a conceptual framework that describes the effect of a cash transfer, conditioned on school attendance, marriage and schooling decisions. Then I provide empirical evidence that, in this setting, marriage responds positively to income.

6.1 Formalising the Problem

Consider a framework in which individuals choose their education level today and marriage status tomorrow. Individuals derive utility from consumption $u(c)$ and the quality of the marriage match $v(q)$ if married. They discount the future at a rate $\beta < 1$. Both $u(c)$ and $v(q)$ are assumed to be increasing, concave and satisfy Inada conditions. Individual's preferences over consumption c and marriage match quality q are assumed to be additively separable, i.e. $U(c, q) = u(c) + v(q)$.

In each period, single individuals draw the quality of their potential match q. Today they decide whether to marry partner q tomorrow, and whether to go to school today. Individuals choose their education level e from the bounded set $[\underline{e}, \bar{e}]$.

If the individual is single today, they choose their education level e for today and they draw the quality of their potential match, q. Agents make decisions taking into account the level of education achieved today, the drawn match quality and the expected match quality for tomorrow (e, q and $\mathbb{E}_{q'}$, respectively). They can choose between (i) leaving school today and staying single tomorrow (N); (ii) going to school today and staying single tomorrow (E); (iii) going to school today and marrying tomorrow (EM); or (iv) leaving school today and marrying tomorrow (M). In this model marriage is an absorbing state, thus individuals are not allowed to divorce and seek for a better match in the future. They can exit and enter school at will.

If individuals are in school their income is the sum of an endowment ω and a subsidy p. If they are out of school, their income is their human capital e. Note that this assumption implies positive and linear returns to schooling in the labour market. Despite the evidence of little to no return from education in rural Mexico, I make this assumption so that education can endogenously delay marriage. If they decide to get married tomorrow, they have to incur in a cost τ paid today. Their consumption today is equal to their income less of marriage cost, if deciding to marry tomorrow.

Denote the value of a single individual V^N. Then, a single individual's joint choice of education and marriage can be summarised as

$$\begin{aligned} V^N(e,q) = \max\{ & u(e) + \beta \mathbb{E}_{q'|e} V^N(e,q'), \quad \text{(N)} \\ & u(\omega + p) + \beta \mathbb{E}_{q'|e} V^N(e+1,q'), \quad \text{(E)} \\ & u(\omega + p - \tau) + \beta V^M(e+1,q), \quad \text{(EM)} \\ & u(e - \tau) + \beta V^M(e,q), \quad \text{(M)}\} \end{aligned}$$

where 'x' is a variable today and 'x' the same variable tomorrow.

Married individuals decide whether to go to school or drop out today, given their current level of education and their match's quality, $\tilde{q}$. Their income depends on whether they are in school ($\omega + p$) or working e. Denote the value of a single individual V^M. Then,

$$\begin{aligned} V^M(e,\tilde{q}) = \max\{ & u(\omega + p) + v(\tilde{q}) + \beta V^M(e+1,\tilde{q}), \quad \text{(EM)} \\ & u(e) + v(\tilde{q}) + \beta V^M(e,\tilde{q}). \quad \text{(M)}\} \end{aligned}$$

Parametrization I solve the model numerically calibrating it to moments of the data before the introduction of the program.

First, I assume that $u(c) = ln(c)$ and $v(q) = ln(q)$.

I also allow, on top of labour market return from education, for marriage market returns. In particular, I do so by assuming highly educated agents draw from a better pool of partners. Formally, every period, single individuals draw a match with quality q from a distribution $\Pi_e(q)$ over the support $[\underline{q}, \bar{q}]$. The distribution of match quality depends on the agent's level of education as follows

$$\Pi_e(q) = \xi(e)\Pi_H + (1 - \xi(e))\Pi_L,$$

where, $\xi(e)$ satisfies $\xi' > 0, \lim_{e \to \bar{e}} \xi' = 0, \xi(\underline{e}) = 0, \xi(\bar{e}) = 1$ and Π_H dominates Π_L by the monotone likelihood ratio property, such that $\forall q_1 > q_0$

$$\frac{\Pi_H(q_1)}{\Pi_L(q_1)} > \frac{\Pi_H(q_0)}{\Pi_L(q_0)}.$$

Individuals with more education are more likely to draw from the high quality distribution, Π_H, a triangular distribution with lower limit $\underline{q}$ and mode and upper limit $\bar{q}$. Individuals with less education are more likely to draw from the low quality distribution Π_L, a uniform distribution over the support $[\underline{q}, \bar{q}]$. Match's quality ranges from 0 to 1 and there are 100 types of matches.

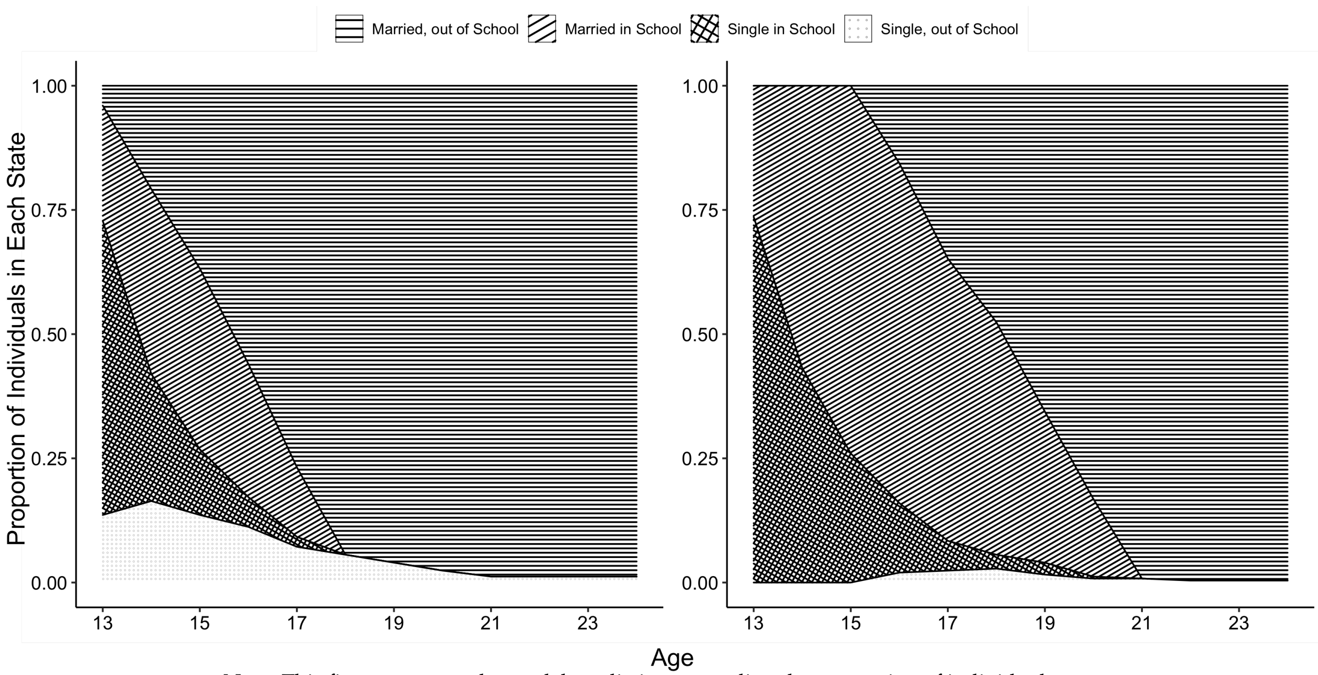
Given these functional form assumptions, I am left with five parameters to calibrate: $\bar{e}, \beta, \omega, p, \tau$. I assume all agents in the models start at age 12 with a level of education between 1 and 6, randomly allocated. They can obtain at most $\bar{e} = 12$ which corresponds to completing high school. I use β to match the median education attainment in the control group. I set $\beta = 0.9$ to match the median education attainment in the data, for the control group, equal to 6 years. I use the relative size of the program to household income to pick p. On average, households in my sample have 7 individuals of which 4 are children. I assume there are two adults working, thus total household income is 2×6, 6 being the median education. The program is, on average, 14% of the total household income, which implies $p = 1.68$. I use data on household expenditure to choose ω. Households, in Mexico, spend approximately 80% of their income in food, drinks, housing, health, transportation, services and personal care. Then, I calibrate ω to match total household expenditure per capita $\omega = 1.37$.[19] Finally, given the lack of information on marriage costs, I assume the cost of marriage to be per capita household expenditure, $\tau = 1.37$, with the idea that the married individual will have to, at least support their own consumption. According to the 2003 National Survey on the Dynamics of Household Relationships (ENDIREH), 52% of the women who marry under 18 years old start living with their spouse in the parents in law's house and 30% of these live for at most 1 year. 35% of these women, however, reported they started living alone with their spouse. Given the different costs that these choices entitle, as robustness, I provide results for different levels of τ in the Appendix (A14 and A15), and the main conclusions of the model remain. The larger the marriage cost, the stronger is the effect of the program.

Results Figure 6 shows the proportion of individuals in each state for each age. Recall that individuals can get in and out of education, thus age does not represent years of education. The left hand side shows the distribution of individuals in a world with no conditional cash transfer, so $p = 0$. The right hand side shows what happens to the proportion of individuals in each state after the introduction of the program. The dotted pattern area represents the proportion of individuals single and out of school (N), the checked pattern area the single individuals who are in school (E), the diagonal stripes area the ones who are married and in school (EM), and the horizontal stripes area the ones who are married and out of school (M). The model predicts that the introduction of the program leads to an increase in both marriage and schooling. The total proportion of married people increases. In the first years, this increase comes from people married in school, and in the later years from married out of school. Schooling increases at every age until 21, and this increase comes both from single and married individuals.

This model predicts that an increase in disposable income, conditional on school attendance, leads individuals to go to school more and to marry more, consistent with the empirical findings.

[19]Data from the Household Income and Expenditure Survey, 1996, from INEGI, translated from 'Encuesta Nacional de Ingresos y Gastos de los Hogares', 1996.

Figure 6: Effect of an Education-Conditional Cash Transfer on Marriage and Schooling Choices



Note: This figure presents the model predictions regarding the proportion of individuals in each state at every year of education. On the left panel it's the prediction if there is no CCT, and on the right panel if there is. The area with: (i) the dotted pattern corresponds to individuals single and out of school, (ii) the checked pattern to individuals who are single in school, (iii) the diagonal stripes to married in school, and (iv) the horizontal stripes to married and out of school.

Looking at the data on the transition from the four states, I compare individuals in 1997, 2000 and 2003, between treated and control groups.[20] Out of almost 80 thousand observations, I observe school attendance and marriage for 89%, 3% of which are married, and 20% of those married are also still in school.[21] Keeping in mind the data limitations, I do observe a larger transition to married states (EM and M) in the treated group than in the control group, both from 1997 to 2000 and from 2000 to 2003. In the treated group, 10.8% of single individuals in 1997 got married in 2000, 1.6% going to school (EM). In the control group, only 0.5% of the observed individuals married in those 3 years. When looking at the second transition, from 2000 to 2003, I observe that 12% of the single treated individuals married, compared to

[20]I do not observe school attendance for the control group in 1998 and 1999. So to look at equally distant periods, I opt to analyze the data every three years.

[21]I not observe school attendance for 11% of individuals, 11.6 in the control and 9.9% in the treated groups. Out of these individuals, 66% report to be single.

5.6% in the control group. Another interesting observation is that in the treatment group I observe individuals who are previously married and out of school going back to school, which is consistent with Latapí and Gonzales de la Rocha (2009) findings in the qualitative evaluation of the program.

6.2 Empirical Test of the Income Effect

The theoretical framework presented above assumes that marriage is a normal good in Mexico and it replicates the empirical result of a positive effect of Progresa/Oportunidades on education and marriage. To test the hypothesis that marriage is a normal good empirically, I exploit variations in household composition to separate the program's income effect from the its overall impact.

In Table 6, I start by showing that there is a positive correlation between the yearly benefit amount the household received, per capita, and girls' marriage probability. The benefit amount might vary across households due to household composition, i.e, number of children, their school level and gender. Compared to households in the lowest quarter of the benefit distribution, those in the 2nd and 3rd are 2 and 1.5p.p more likely to marry, respectively. However, this correlation is not linear since it becomes negative for those in the highest quarter of the per capita benefits distribution. When looking exclusively at child marriage, I observe the same pattern.

Since the amount of the benefit is endogenous, I proceed with a different strategy to estimate the *causal* effect of an increase in income on marriage decisions. I start by selecting a sub-sample of individuals between 6 and 16 years old at baseline that were exposed to the income effect only. I restrict the sample to those individuals who are not eligible for the benefit themselves since they have completed, in 1997, the final grade of middle school or higher but live in the same household as an eligible child.[22] The sample consists of 453 individuals, 47% of them female and 15.51 years old on average. For example, these could be older siblings who have completed middle school and whose younger sibling(s) is(are) eligible for the program. Note

[22] Since the rules of eligibility change in 2000, I use the comparison between T1998 and C2000 to avoid misclassification of eligibility.

Table 6: Correlation between Yearly Benefit and Marriage: Girls

	(1) Married (all)	(2) Married (CM)
2nd quartile of per capita yearly benefit	0.0208***	0.0101**
	(0.00584)	(0.00427)
3rd quartile of per capita yearly benefit	0.0146**	0.00948**
	(0.00596)	(0.00419)
4th quartile of per capita yearly benefit	-0.0308***	-0.0213***
	(0.00538)	(0.00353)
N	46232	38411

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

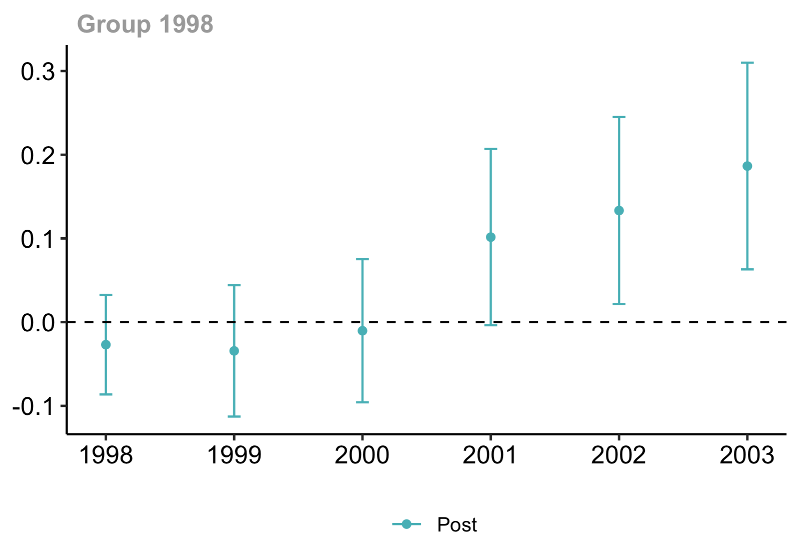
Note: This table presents the correlation between yearly monetary benefit received by each household, divided into quartiles, and the probability of girls being married. Observations were weighted by the inverse of the propensity score, and all regressions have the control variables described in the Data section, including age and household composition. Standard errors were obtained through clustered bootstrap at the randomization level: locality.

that given the sample size, if I restrict the sample to marriages below 18 years old, I do not get enough variation to study this mechanism. However, I believe that for this exercise, it is enough to understand if a positive income shock leads to an increase in marriage.

If it was the case that the program incentivised older siblings to pursue more years of education, then I could not disentangle the two effects. However, the benefit amount is likely not enough to compensate both the wage of the beneficiary child and the older sibling. In fact, each benefit was calculated so to compensate for around two-thirds of a child's wage in rural Mexico. Thus, it is unlikely that this would be a high enough incentive to compensate for the older sibling's wage. Furthermore, empirically I do not observe different levels of education between treated and control groups in 1997, 2000 and 2003, which is suggestive evidence of no 'spillover' effects of the program on non-eligible members within the household.

Figure 7 shows the effect of a positive income shock on the probability of marriage. In the first years, I observed a negative effect of the benefit on the probability of marriage, although not statistically significant. However, from 2001 onward, I observed positive and substiantial effects, between 10 and 18p.p increase in marriage probability, statistically significant in 2003.

Figure 7: Causal Effect of an Income Shock on the Probability of Marriage



Note: This figure presents the average treatment effect on the treated by year on the sample of individuals who would not be eligible for the program but share the household with an eligible individual. Standard errors were obtained through clustered bootstrap, at the randomization level: locality.

These results suggest that, in this population, when households receive a positive income transfer, this relaxes their budget constraint and enables individuals to marry.

This result is particularly important for policy design. If marriage is a normal good in Mexico and there are no other changes in the society and the economy, giving monetary transfers to young people leads them to marry more. However, this decision might not be the best in the long run for these children due to the negative associations between early marriage and economic outcomes.

7 Conclusion

In this paper, I study the effect of a conditional cash transfer program in Mexico, Progresa/Oportunidades, on the probability of marriage for children under 18. Ending child marriage is one of the UN's Sustainable Goals due to its association with several adverse outcomes, particularly for young brides. Around the world, there have been initiatives to delay marriage. From financial incentives, law changes on the minimum age to marriage, to programs aiming at changing social norms, their success varies depending on their design and the context where they operate. Education is an important determinant of marriage decisions, and programs targeting schooling have been evaluated in terms of their efficacy in decreasing child marriage. In this paper, I look at a conditional cash transfer program implemented in Mexico, and I show that there might be unintended and unexpected effects that are important to consider.

I study Progresa/Oportunidades, a program where beneficiary households receive a monetary transfer conditional on the school-aged children enrolling and attending school. Leveraging the random assignment of the program at the locality level and its subsequent expansion, I study the program's effects on marriage decisions by comparing two treatment groups that received the treatment at different times and a quasi-experimental control group. I estimate the average treatment effect on the treated using the doubly-robust estimator in a staggered differences-in-differences design proposed by Callaway and Sant'Anna (2021).

I find that the program leads to an increase in child marriage. I also provide evidence that when children receive the benefits, it is important since girls at higher risk of marriage - those in high school - marry at higher rates in the treatment group than in the control group when they receive school subsidies for those schooling years.

Since the program was considered a success in improving educational outcomes, and education is negatively correlated with child marriage, this result might sound surprising. Theoretically, with an increase in education, the opportunity cost of marriage also increases, which leads to decreasing marriage rates and delayed marriages. However, besides the education component, the program provides a monetary transfer to beneficiary households. The relaxation of the financial constraint of the household might also affect marriage decisions, and the direction of the effect is not clear ex-ante. It could be either leading families and children to rely less on marriage since their budget constraint is more relaxed, thus decreasing its probability, or increasing the market value of beneficiary families and children and allowing or facilitating the formation of a new household. I discuss these mechanisms with a simple theoretical exercise that helps rationalise both sets of empirical evidence and provides empirical evidence that supports the hypothesis that marriage is a normal good in Mexico and that positive income transfers lead to more marriage.

This result is important to inform policymakers aiming at decreasing child marriage in contexts similar to the one study in this paper. Even if these increase education, providing monetary transfers can backlash and enable children to marry earlier than they would had they not received the money.

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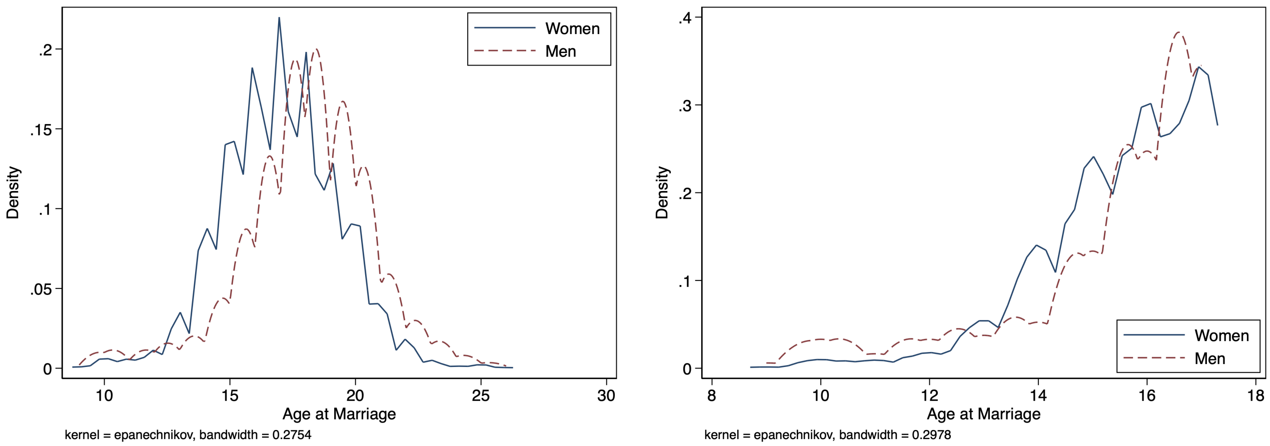
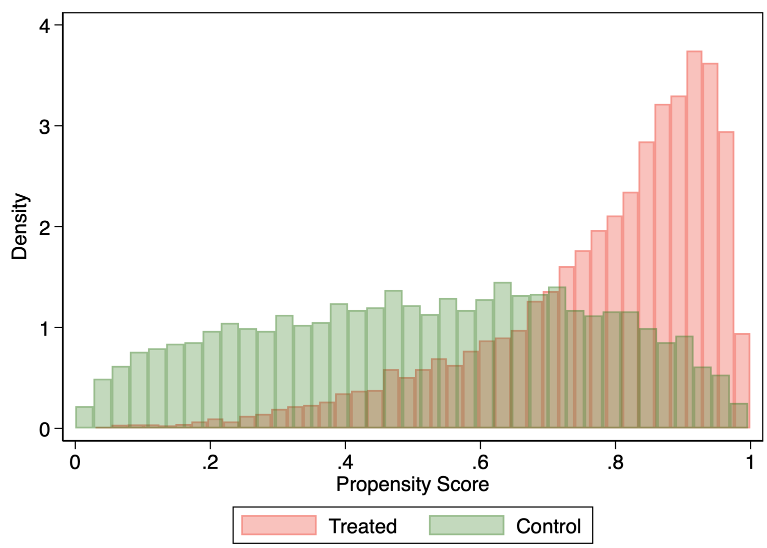
Figure A1: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure



Note: The left panel presents the distribution of age at marriage for the entire sample, separately for men and women. The right one present the same distribution but considering only those individuals who married before 18 years old.

A Appendix

Figure A2: Distribution of the Propensity Score by Group: T1998 VS C2000



Note: This figure presents the histogram of the propensity score for treated (T1998) and control (C2000) groups separately.

Table A1: Balance Test on Baseline Characteristics: All (1)

	T1998	T2000	T1998(IPW)	T2000(IPW)	Control
Married	-0.008	-0.01	0.001	0.00	0.01
	(-3.472)	(-2.49)	(0.518)	(0.57)	
Education Level	-0.057	-0.14	-0.159	-0.01	3.44
	(-0.799)	(-1.80)	(-2.110)	(-0.13)	
Age in 97	0.177	0.13	-0.208	-0.12	10.64
	(2.917)	(2.05)	(-1.810)	(-1.28)	
Dirt Floor	-0.009	0.02	0.029	0.02	0.72
	(-0.256)	(0.57)	(0.470)	(0.36)	
Inferior quality wall	0.002	0.06	-0.001	0.02	0.23
	(0.054)	(1.50)	(-0.022)	(0.44)	
Inferior quality roof	-0.073	-0.05	-0.025	-0.03	0.21
	(-1.995)	(-1.25)	(-0.671)	(-0.72)	
No. of bedrooms	0.081	0.03	-1.424	-0.39	1.71
	(1.722)	(0.59)	(-2.145)	(-1.59)	
Piped water	-0.065	-0.15	-0.038	-0.02	0.28
	(-1.260)	(-2.60)	(-0.593)	(-0.36)	
Electricity	-0.018	-0.00	0.038	0.03	0.69
	(-0.472)	(-0.09)	(0.749)	(0.55)	
Animals	0.140	0.12	0.062	0.01	0.40
	(4.951)	(3.74)	(1.604)	(0.30)	
Land	0.196	0.16	0.024	0.00	0.64
	(5.434)	(4.14)	(0.460)	(0.10)	
Blender	-0.009	0.02	-0.013	0.02	0.25
	(-0.406)	(0.58)	(-0.260)	(0.80)	
Refrigerator	-0.022	-0.04	0.020	0.01	0.05
	(-1.314)	(-2.69)	(1.608)	(0.68)	
Gas Stove	-0.089	-0.09	0.015	-0.00	0.19
	(-2.179)	(-2.04)	(0.491)	(-0.07)	
Gas heater	-0.004	-0.01	-0.025	-0.02	0.02
	(-0.850)	(-1.71)	(-1.102)	(-1.21)	
Radio	0.056	0.06	0.035	0.01	0.61
	(2.242)	(2.20)	(0.765)	(0.20)	
TV	0.025	0.05	-0.023	0.03	0.42
	(0.689)	(1.29)	(-0.381)	(0.53)	
Video player	0.006	-0.00	0.003	0.00	0.01
	(1.350)	(-0.43)	(0.448)	(0.17)	
Dish Washer	0.007	-0.00	-0.006	0.00	0.02
	(0.981)	(-0.04)	(-0.549)	(0.46)	
Car	-0.011	-0.02	-0.000	-0.00	0.00
	(-2.672)	(-3.89)	(-0.095)	(-0.49)	
Truck	-0.012	-0.01	0.004	0.00	0.03
	(-1.863)	(-1.05)	(0.627)	(0.07)	
Anyone in the HH speaks an indigenous language	0.130	0.15	0.014	-0.00	0.43
	(2.299)	(2.27)	(0.201)	(-0.01)	

Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns report the parameters without any re-weighting. The third and fourth columns report the estimates re-weighing the control group as described. In the four regressions, standard errors were clustered at the locality level. The last column presents each characteristic's unconditional and unweighted mean for the control group C2000.

Table A2: Balance Test on Baseline Characteristics: All (2)

	T1998	T2000	T1998(IPW)	T2000(IPW)	Control
HH Chief or Spouse have gone to school	0.077	0.07	0.009	-0.00	0.71
	(2.806)	(2.41)	(0.201)	(-0.03)	
HH Chief or Spouse worked the week before	-0.003	-0.02	-0.025	-0.02	0.91
	(-0.415)	(-2.10)	(-3.291)	(-2.23)	
Progresa/Oportunidades'poverty index	0.248	0.31	0.061	0.02	0.59
	(3.779)	(4.64)	(0.841)	(0.24)	
Housewife	-0.045	-0.05	0.007	-0.00	0.07
	(-5.801)	(-5.34)	(0.960)	(-0.14)	
Number of individuals in the HH	-0.063	0.01	-0.048	0.02	7.51
	(-0.501)	(0.10)	(-0.285)	(0.12)	
HH head age	1.104	1.17	-1.517	-0.18	43.23
	(3.428)	(2.80)	(-2.173)	(-0.35)	
HH head is female	-0.051	-0.05	0.013	0.01	0.06
	(-5.217)	(-5.04)	(1.598)	(1.04)	
Anyone in the HH speaks an indigenous language	0.130	0.15	0.014	-0.00	0.43
	(2.299)	(2.27)	(0.201)	(-0.01)	
HH Chief or Spouse have gone to school	0.077	0.07	0.009	-0.00	0.71
	(2.806)	(2.41)	(0.201)	(-0.03)	
HH Chief or Spouse worked the week before	-0.003	-0.02	-0.025	-0.02	0.91
	(-0.415)	(-2.10)	(-3.291)	(-2.23)	
At least one child between 0 and 5 y.o	0.004	0.02	-0.015	0.01	0.69
	(0.230)	(0.92)	(-0.404)	(0.50)	
At least one teen between 16 and 19 y.o	0.063	0.04	-0.008	-0.01	0.42
	(3.810)	(2.42)	(-0.175)	(-0.20)	
At least one woman between 20 and 39 y.o	0.023	0.05	0.099	0.04	0.74
	(1.767)	(3.26)	(2.216)	(1.49)	
At least one woman between 40 and 59 y.o	-0.010	-0.03	-0.113	-0.04	0.36
	(-0.675)	(-1.77)	(-2.689)	(-1.44)	
At least one woman over 60 y.o	-0.042	-0.04	0.006	0.01	0.10
	(-3.140)	(-2.44)	(0.352)	(0.78)	
At least one man between 20 and 39 y.o	0.026	0.04	0.068	0.03	0.57
	(1.672)	(2.19)	(1.707)	(1.01)	
At least one man between 40 and 59 y.o	0.002	-0.02	-0.072	-0.03	0.46
	(0.103)	(-0.96)	(-1.867)	(-1.13)	
At least one man over 60 y.o	-0.042	-0.04	0.010	0.01	0.10
	(-2.739)	(-2.77)	(0.767)	(1.32)	
Guerrero	0.043	-0.00	0.052	0.03	0.06
	(1.116)	(-0.07)	(1.307)	(1.07)	
Hidalgo	0.077	0.01	0.008	0.01	0.12
	(2.062)	(0.40)	(0.146)	(0.20)	
Michoacan	0.011	0.01	-0.012	0.00	0.13
	(0.252)	(0.12)	(-0.225)	(0.00)	
Puebla	0.058	0.07	-0.012	-0.02	0.16
	(1.442)	(1.30)	(-0.216)	(-0.35)	
Queretaro	-0.061	-0.06	-0.013	-0.01	0.04
	(-1.019)	(-1.02)	(-0.379)	(-0.22)	
San Luis Potosi	0.026	0.01	-0.037	0.00	0.13
	(0.543)	(0.17)	(-0.508)	(0.01)	

Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns report the parameters without any re-weighting. The third and fourth columns report the estimates re-weighing the control group as described. In the four regressions, standard errors were clustered at the locality level. The last column presents each characteristic's unconditional and unweighted mean for the control group C2000.

Table A3: Balance Test on Baseline Characteristics: Girls (1)

	T1998	T2000	T1998(IPW)	T2000(IPW)	Control
Married	-0.015	-0.01	0.003	0.00	0.01
	(-3.636)	(-3.07)	(1.011)	(0.58)	
Education Level	-0.102	-0.16	-0.001	0.02	3.47
	(-1.105)	(-1.66)	(-0.005)	(0.14)	
Age in 97	0.082	0.03	-0.062	-0.07	10.60
	(0.967)	(0.31)	(-0.248)	(-0.43)	
Dirt Floor	0.001	0.02	0.031	0.03	0.71
	(0.036)	(0.59)	(0.463)	(0.63)	
Inferior quality wall	-0.000	0.06	0.016	0.03	0.22
	(-0.003)	(1.44)	(0.404)	(0.51)	
Inferior quality roof	-0.079	-0.05	0.006	-0.01	0.21
	(-2.191)	(-1.32)	(0.169)	(-0.31)	
No. of bedrooms	0.091	0.06	-1.723	-0.66	1.72
	(1.913)	(1.04)	(-2.273)	(-1.77)	
Piped water	-0.072	-0.15	-0.048	-0.03	0.29
	(-1.401)	(-2.51)	(-0.693)	(-0.49)	
Electricity	-0.033	-0.02	0.007	0.02	0.70
	(-0.822)	(-0.48)	(0.146)	(0.46)	
Animals	0.140	0.11	0.058	0.02	0.40
	(4.893)	(3.52)	(1.295)	(0.42)	
Land	0.188	0.15	0.030	-0.00	0.63
	(4.965)	(3.61)	(0.499)	(-0.07)	
Blender	-0.014	0.02	-0.010	0.01	0.26
	(-0.581)	(0.72)	(-0.193)	(0.27)	
Refrigerator	-0.027	-0.05	0.021	0.01	0.05
	(-1.456)	(-2.66)	(1.500)	(0.55)	
Gas Stove	-0.090	-0.09	0.020	-0.00	0.20
	(-2.153)	(-1.99)	(0.612)	(-0.04)	
Gas heater	-0.001	-0.01	-0.030	-0.02	0.01
	(-0.162)	(-1.83)	(-1.201)	(-1.34)	
Radio	0.054	0.06	0.071	0.03	0.62
	(1.946)	(1.99)	(1.359)	(0.84)	
TV	0.022	0.05	-0.013	0.02	0.42
	(0.559)	(1.07)	(-0.182)	(0.37)	
Video player	0.005	-0.00	-0.000	0.00	0.01
	(0.918)	(-0.63)	(-0.019)	(0.14)	
Dish Washer	0.004	-0.00	-0.008	0.00	0.02
	(0.539)	(-0.50)	(-0.623)	(0.17)	
Car	-0.010	-0.02	-0.003	-0.00	0.00
	(-1.947)	(-3.36)	(-0.559)	(-0.77)	
Truck	-0.008	-0.01	-0.000	-0.00	0.03
	(-1.267)	(-0.59)	(-0.048)	(-0.05)	
Anyone in the HH speaks an indigenous language	0.128	0.14	0.044	0.03	0.42
	(2.180)	(2.05)	(0.635)	(0.34)	

Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator for girls. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns report the parameters without any re-weighting. The third and fourth columns report the estimates re-weighing the control group as described. In the four regressions, standard errors were clustered at the locality level. The last column presents each characteristic's unconditional and unweighted mean for the control group C2000.

Table A4: Balance Test on Baseline Characteristics: Girls (1)

	T1998	T2000	T1998(IPW)	T2000(IPW)	Control
HH Chief or Spouse have gone to school	0.077	0.06	0.004	-0.01	0.71
	(2.561)	(1.72)	(0.078)	(-0.34)	
HH Chief or Spouse worked the week before	0.000	-0.02	-0.026	-0.03	0.91
	(0.036)	(-2.26)	(-2.737)	(-2.72)	
Progresa/Oportunidades'poverty index	0.275	0.32	0.127	0.07	0.58
	(4.158)	(4.68)	(1.719)	(0.85)	
Housewife	-0.081	-0.08	0.018	0.00	0.14
	(-5.466)	(-5.03)	(1.203)	(0.16)	
Number of individuals in the HH	-0.023	0.01	-0.058	0.04	7.52
	(-0.179)	(0.07)	(-0.266)	(0.19)	
HH head age	0.697	1.24	-2.257	-0.27	43.39
	(1.718)	(2.35)	(-2.338)	(-0.40)	
HH head is female	-0.056	-0.05	0.013	0.01	0.07
	(-4.881)	(-4.11)	(1.486)	(1.13)	
Anyone in the HH speaks an indigenous language	0.128	0.14	0.044	0.03	0.42
	(2.180)	(2.05)	(0.635)	(0.34)	
HH Chief or Spouse have gone to school	0.077	0.06	0.004	-0.01	0.71
	(2.561)	(1.72)	(0.078)	(-0.34)	
HH Chief or Spouse worked the week before	0.000	-0.02	-0.026	-0.03	0.91
	(0.036)	(-2.26)	(-2.737)	(-2.72)	
At least one child between 0 and 5 y.o	0.014	0.02	-0.007	0.01	0.69
	(0.714)	(0.69)	(-0.146)	(0.34)	
At least one teen between 16 and 19 y.o	0.042	0.03	-0.047	-0.02	0.43
	(2.324)	(1.59)	(-0.873)	(-0.59)	
At least one woman between 20 and 39 y.o	0.029	0.05	0.098	0.04	0.74
	(2.040)	(2.71)	(1.895)	(1.34)	
At least one woman between 40 and 59 y.o	-0.015	-0.02	-0.128	-0.04	0.36
	(-0.874)	(-1.04)	(-2.567)	(-1.05)	
At least one woman over 60 y.o	-0.056	-0.05	0.016	0.01	0.09
	(-3.649)	(-3.20)	(1.635)	(1.20)	
At least one man between 20 and 39 y.o	0.032	0.05	0.070	0.04	0.57
	(2.027)	(2.37)	(1.454)	(1.22)	
At least one man between 40 and 59 y.o	-0.002	-0.03	-0.102	-0.05	0.45
	(-0.125)	(-1.31)	(-2.590)	(-1.69)	
At least one man over 60 y.o	-0.053	-0.05	0.017	0.02	0.10
	(-3.228)	(-2.75)	(1.282)	(1.95)	
Guerrero	0.032	-0.01	0.035	0.02	0.06
	(0.806)	(-0.21)	(0.749)	(0.76)	
Hidalgo	0.074	0.01	-0.012	0.00	0.11
	(1.939)	(0.32)	(-0.187)	(0.06)	
Michoacan	0.006	0.00	-0.012	-0.00	0.14
	(0.137)	(0.08)	(-0.198)	(-0.08)	
Puebla	0.055	0.07	-0.012	-0.02	0.17
	(1.308)	(1.19)	(-0.216)	(-0.27)	
Queretaro	-0.057	-0.06	-0.012	-0.01	0.04
	(-0.939)	(-0.98)	(-0.310)	(-0.30)	
San Luis Potosi	0.036	0.02	-0.015	0.00	0.13
	(0.758)	(0.36)	(-0.198)	(0.05)	

Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator for girls. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns report the parameters without any re-weighting. The third and fourth columns report the estimates re-weighing the control group as described. In the four regressions, standard errors were clustered at the locality level. The last column presents each characteristic's unconditional and unweighted mean for the control group C2000.

Table A5: Balance Test on Baseline Characteristics: Boys (1)

	T1998	T2000	T1998(IPW)	T2000(IPW)	Control
Married	-0.001	0.00	-0.000	0.00	0.00
	(-0.504)	(0.68)	(-0.283)	(0.33)	
Education Level	-0.015	-0.12	-0.226	-0.02	3.41
	(-0.196)	(-1.40)	(-1.966)	(-0.23)	
Age in 97	0.266	0.23	-0.260	-0.13	10.68
	(3.151)	(2.75)	(-1.942)	(-1.03)	
Dirt Floor	-0.019	0.02	0.029	0.01	0.72
	(-0.522)	(0.52)	(0.484)	(0.15)	
Inferior quality wall	0.004	0.06	-0.004	0.03	0.23
	(0.101)	(1.51)	(-0.088)	(0.60)	
Inferior quality roof	-0.067	-0.05	-0.039	-0.03	0.21
	(-1.765)	(-1.15)	(-1.004)	(-0.88)	
No. of bedrooms	0.072	0.01	-1.163	-0.23	1.69
	(1.397)	(0.13)	(-1.910)	(-1.28)	
Piped water	-0.059	-0.15	-0.034	-0.02	0.26
	(-1.104)	(-2.64)	(-0.558)	(-0.43)	
Electricity	-0.005	0.01	0.048	0.02	0.69
	(-0.119)	(0.26)	(0.956)	(0.45)	
Animals	0.141	0.12	0.064	0.01	0.41
	(4.698)	(3.66)	(1.618)	(0.14)	
Land	0.203	0.18	0.029	0.02	0.66
	(5.667)	(4.49)	(0.565)	(0.49)	
Blender	-0.005	0.01	-0.013	0.04	0.24
	(-0.201)	(0.39)	(-0.289)	(1.27)	
Refrigerator	-0.018	-0.04	0.019	0.01	0.05
	(-1.069)	(-2.52)	(1.406)	(0.59)	
Gas Stove	-0.088	-0.09	0.005	-0.01	0.18
	(-2.154)	(-2.05)	(0.153)	(-0.25)	
Gas heater	-0.007	-0.01	-0.022	-0.01	0.02
	(-1.363)	(-1.29)	(-1.014)	(-0.97)	
Radio	0.059	0.06	0.016	-0.01	0.61
	(2.250)	(2.13)	(0.356)	(-0.36)	
TV	0.029	0.06	-0.030	0.02	0.42
	(0.789)	(1.47)	(-0.530)	(0.48)	
Video player	0.007	-0.00	0.005	0.00	0.02
	(1.582)	(-0.16)	(0.615)	(0.28)	
Dish Washer	0.009	0.00	0.000	0.01	0.02
	(1.343)	(0.42)	(0.026)	(0.80)	
Car	-0.013	-0.02	0.001	0.00	0.00
	(-2.935)	(-3.55)	(0.304)	(0.05)	
Truck	-0.015	-0.01	0.006	0.00	0.03
	(-2.166)	(-1.40)	(1.030)	(0.15)	
Anyone in the HH speaks an indigenous language	0.132	0.16	-0.001	-0.01	0.44
	(2.387)	(2.44)	(-0.008)	(-0.13)	

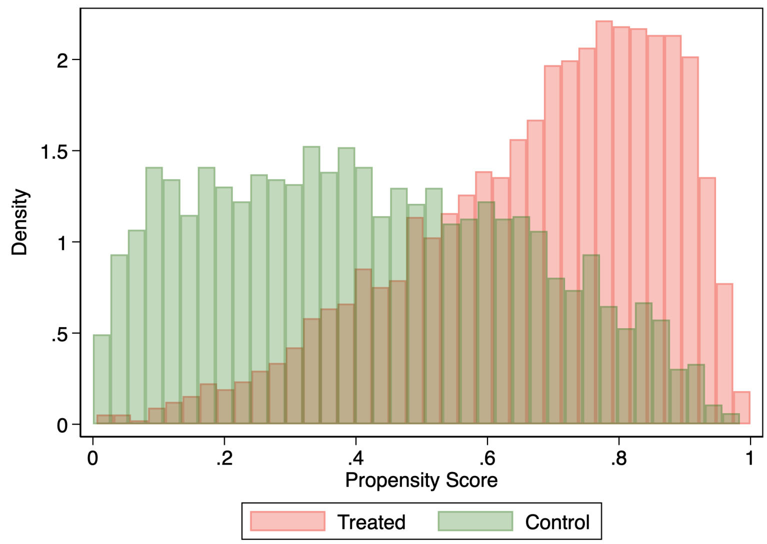
Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator for boys. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns report the parameters without any re-weighting. The third and fourth columns report the estimates re-weighing the control group as described. In the four regressions, standard errors were clustered at the locality level. The last column presents each characteristic's unconditional and unweighted mean for the control group C2000.

Table A6: Balance Test on Baseline Characteristics: Boys (2)

	T1998	T2000	T1998(IPW)	T2000(IPW)	Control
HH Chief or Spouse have gone to school	0.077	0.08	0.008	0.01	0.72
	(2.889)	(2.95)	(0.173)	(0.23)	
HH Chief or Spouse worked the week before	-0.007	-0.01	-0.026	-0.02	0.91
	(-0.740)	(-1.40)	(-3.124)	(-1.30)	
Progresa/Oportunidades'poverty index	0.223	0.31	0.038	0.01	0.60
	(3.271)	(4.34)	(0.505)	(0.13)	
Housewife	-0.011	-0.01	0.002	-0.00	0.00
	(-3.404)	(-4.20)	(1.355)	(-0.02)	
Number of individuals in the HH	-0.099	0.02	-0.036	0.01	7.50
	(-0.764)	(0.12)	(-0.238)	(0.06)	
HH head age	1.478	1.10	-0.827	-0.18	43.08
	(4.469)	(2.71)	(-1.300)	(-0.33)	
HH head is female	-0.047	-0.06	0.012	0.01	0.06
	(-4.458)	(-4.92)	(1.283)	(0.75)	
Anyone in the HH speaks an indigenous language	0.132	0.16	-0.001	-0.01	0.44
	(2.387)	(2.44)	(-0.008)	(-0.13)	
HH Chief or Spouse have gone to school	0.077	0.08	0.008	0.01	0.72
	(2.889)	(2.95)	(0.173)	(0.23)	
HH Chief or Spouse worked the week before	-0.007	-0.01	-0.026	-0.02	0.91
	(-0.740)	(-1.40)	(-3.124)	(-1.30)	
At least one child between 0 and 5 y.o	-0.006	0.02	-0.020	0.02	0.69
	(-0.355)	(1.05)	(-0.567)	(0.65)	
At least one teen between 16 and 19 y.o	0.083	0.05	0.004	-0.01	0.42
	(4.377)	(2.64)	(0.092)	(-0.16)	
At least one woman between 20 and 39 y.o	0.018	0.05	0.087	0.03	0.74
	(1.173)	(3.14)	(2.087)	(1.26)	
At least one woman between 40 and 59 y.o	-0.005	-0.04	-0.096	-0.04	0.35
	(-0.326)	(-2.15)	(-2.396)	(-1.75)	
At least one woman over 60 y.o	-0.029	-0.02	0.010	0.02	0.11
	(-2.152)	(-1.39)	(0.459)	(1.21)	
At least one man between 20 and 39 y.o	0.021	0.04	0.059	0.01	0.57
	(1.122)	(1.66)	(1.550)	(0.44)	
At least one man between 40 and 59 y.o	0.005	-0.01	-0.048	-0.01	0.47
	(0.308)	(-0.40)	(-1.228)	(-0.49)	
At least one man over 60 y.o	-0.031	-0.04	0.009	0.01	0.09
	(-1.984)	(-2.50)	(0.608)	(0.62)	
Guerrero	0.053	0.00	0.062	0.03	0.06
	(1.403)	(0.07)	(1.632)	(1.18)	
Hidalgo	0.080	0.02	0.017	0.01	0.12
	(2.128)	(0.47)	(0.317)	(0.14)	
Michoacan	0.015	0.01	-0.009	0.01	0.13
	(0.370)	(0.16)	(-0.183)	(0.10)	
Puebla	0.060	0.07	-0.014	-0.03	0.16
	(1.564)	(1.39)	(-0.249)	(-0.42)	
Queretaro	-0.066	-0.07	-0.016	-0.01	0.04
	(-1.091)	(-1.06)	(-0.460)	(-0.17)	
San Luis Potosi	0.017	0.00	-0.042	0.00	0.13
	(0.319)	(0.01)	(-0.608)	(0.06)	

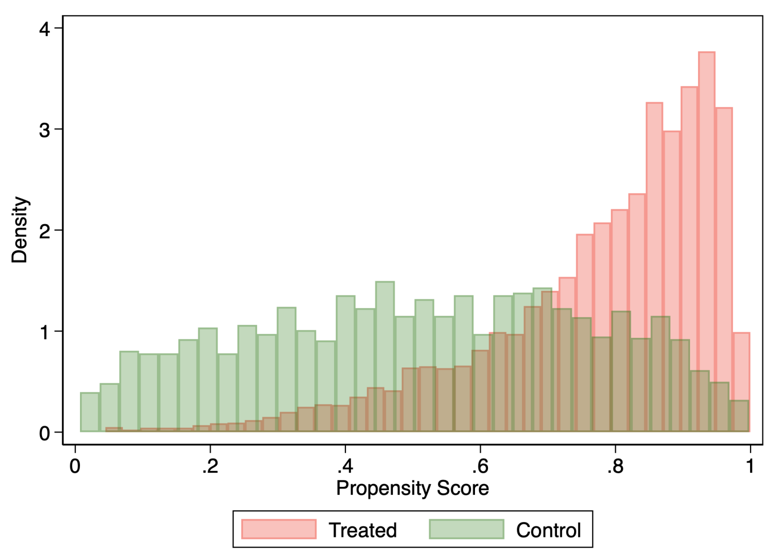
Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator for boys. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns report the parameters without any re-weighting. The third and fourth columns report the estimates re-weighing the control group as described. In the four regressions, standard errors were clustered at the locality level. The last column presents each characteristic's unconditional and unweighted mean for the control group C2000.

Figure A3: Distribution of the Propensity Score by Group: T2000 VS C2000



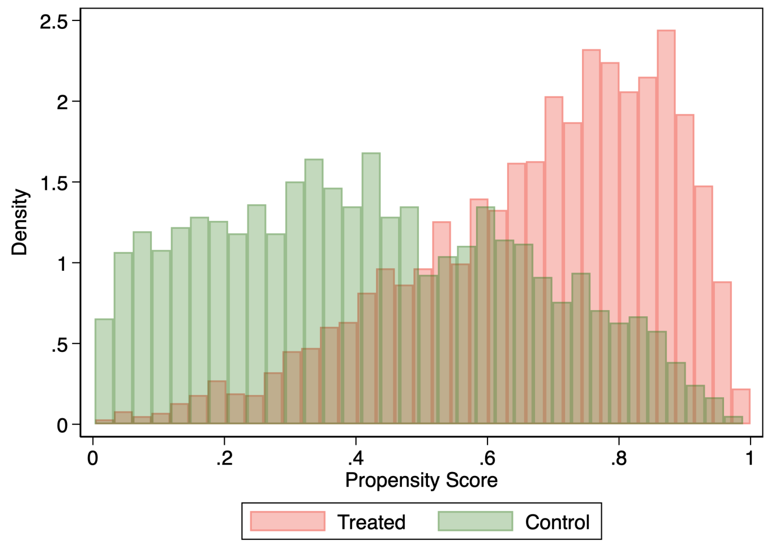
Note: This figure presents the histogram of the propensity score for treated (T2000) and control (C2000) groups separately.

Figure A4: Distribution of the Propensity Score by Group: Girls T1998 VS C2000



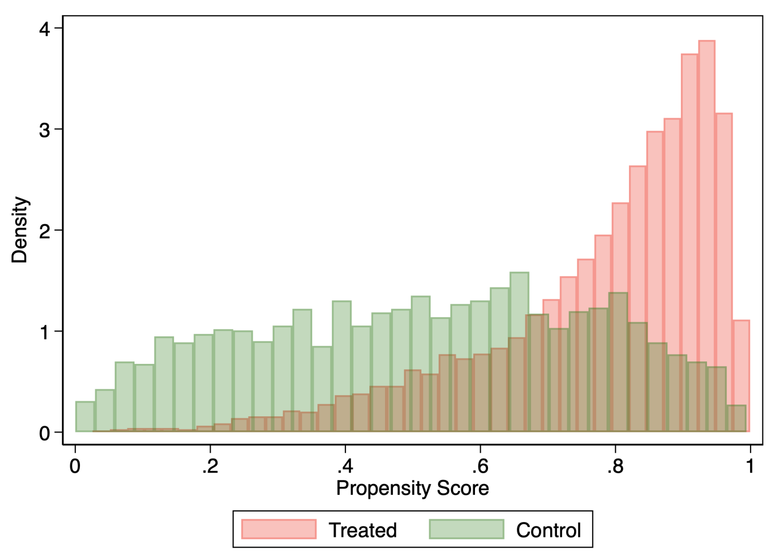
Note: This figure presents the histogram of the propensity score for girls in treated (T1998) and control (C2000) groups separately.

Figure A5: Distribution of the Propensity Score by Group: Girls T2000 VS C2000



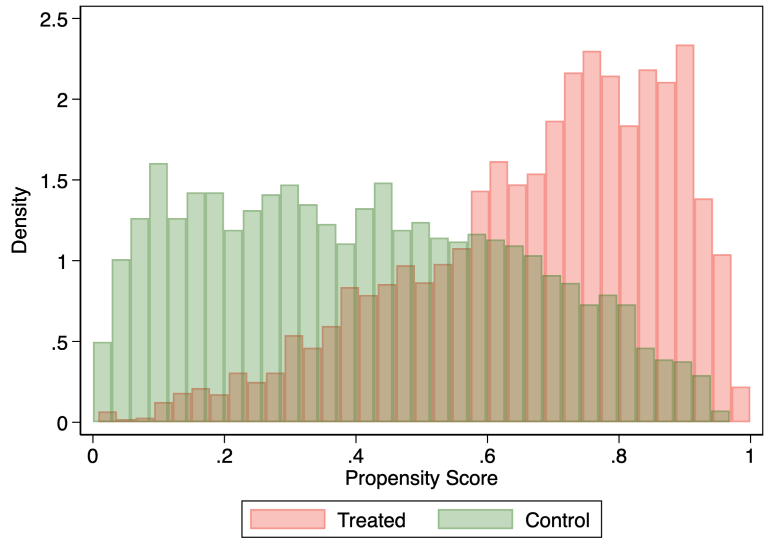
Note: This figure presents the histogram of the propensity score for girls in treated (T2000) and control (C2000) groups separately.

Figure A6: Distribution of the Propensity Score by Group: Boys T1998 VS C2000



Note: This figure presents the histogram of the propensity score for boys in treated (T1998) and control (C2000) groups separately.

Figure A7: Distribution of the Propensity Score by Group: Boys T2000 VS C2000



Note: This figure presents the histogram of the propensity score for boys in treated (T2000) and control (C2000) groups separately.

Table A9: Proportion of Children Attending School Conditional on Being Married

		Attends School				
		1997	1998	1999	2000	2003
Year of Marriage	1997	51.32	38.03	26.67	7.74	3.2
	1998		51.67	33.77	21.74	13.04
	1999			50	40.99	31.91
	2000				46.45	34.96
	2001					8.43
	2002					8
	2003					20.24

Note: This table shows the proportion of children who attend school in the year of or after declared being married.

Table A7: Proportion of Married by Group and Year (in %): Girls

(a) All

	1997	1998	1999	2000	2001	2002	2003
T1998	1.29	2.25	4.26	7.58	14.08	18.70	20.95
T2000	1.40	2.35	4.35	7.96	15.14	19.49	21.89
C2000	2.79	3.28	4.73	6.78	9.95	13.71	15.57
C2000(IPW1998)	1.00	1.37	2.68	4.72	7.20	9.32	10.94
C2000(IPW2000)	1.23	1.77	3.04	5.44	8.22	11.02	12.74

(b) Under 18 years old

	1997	1998	1999	2000	2001	2002	2003
T1998	0.05	0.05	0.59	1.37	3.08	5.57	6.75
T2000	0.00	0.00	0.19	0.94	3.56	6.23	7.07
C2000	2.79	3.28	2.81	3.16	3.60	4.58	4.04
C2000(IPW1998)	0.00	0.03	0.03	0.20	0.69	1.76	2.21
C2000(IPW2000)	0.00	0.02	0.02	0.25	0.90	2.37	2.98

Note: This table presents the proportion of married individuals by group and year, restricting the analysis to girls. Panel (a) refers to all girls between 6 and 16 years old in 1997, and Panel (b) refers to the same girls until they turn 18 years old. T1998 are those individuals who started receiving the program in 1998. T2000 is the set of individuals who first received the program in 2000. C2000 is the control group. C2000(IPW1998) and C2000(IPW2000) are the control group weighted by the probability of being first treated in 1998 and 2000, respectively, versus being in the control group.

Table A8: Proportion of Married by Group and Year (in %): Boys

(a) All

	1997	1998	1999	2000	2001	2002	2003
T1998	0.22	0.74	1.78	3.57	6.30	8.25	9.87
T2000	0.38	0.84	2.04	3.76	6.73	9.02	10.90
C2000	0.28	0.87	1.42	2.22	3.53	5.99	7.38
C2000(IPW1998)	0.26	0.97	1.57	2.05	2.79	4.42	5.21
C2000(IPW2000)	0.32	0.96	1.50	2.22	2.99	4.74	5.62

(b) Under 18 years old

	1997	1998	1999	2000	2001	2002	2003
T1998	0.00	0.03	0.51	1.31	1.57	1.95	2.22
T2000	0.00	0.05	0.55	1.38	1.70	2.16	2.99
C2000	0.28	0.87	0.59	0.71	0.92	1.36	1.19
C2000(IPW1998)	0.00	0.00	0.00	0.00	0.13	0.75	1.20
C2000(IPW2000)	0.00	0.00	0.00	0.00	0.08	0.64	1.11

Note: This table presents the proportion of married individuals by group and year, restricting the analysis to boys. Panel (a) refers to all boys between 6 and 16 years old in 1997, and Panel (b) refers to the same boys until they turn 18 years old. T1998 are those individuals who started receiving the program in 1998. T2000 is the set of individuals who first received the program in 2000. C2000 is the control group. C2000(IPW1998) and C2000(IPW2000) are the control group weighted by the probability of being first treated in 1998 and 2000, respectively, versus being in the control group.

Table A10: Proportion of Individuals in the State Married and School VS All other states (in %)

	1997	1998	1999	2000	2003
T1998	0.17	0.29	0.74	1.12	0.91
T2000	0.16	0.22	0.57	1.02	0.71
C2000	1.50	.	.	0.09	0.31
C2000(IPW1998)	0.65	.	.	0.06	0.19
C2000(IPW2000)	0.78	.	.	0.07	0.27
	T-stat H0: Equal coefficients				
T1998vsT2000	0.29	0.89	1.29	0.58	1.09
T1998vsC2000(IPW1998)	-3.09	.	.	8.75	6.52
T2000vsC2000(IPW2000)	-4.41	.	.	6.61	2.26

Note: Note: This table reports the proportion of individuals in the state Married and in School, versus all other states (married out of school, single in school and single out of school). The first five rows present this statistic for each group. T1998 are those individuals who started receiving the program in 1998. T2000 is the set of individuals who first received the program in 2000. C2000 is the control group. C2000(IPW1998) and C2000(IPW2000) are the control group weighted by the probability of being first treated in 1998 and 2000, respectively, versus being in the control group. The last three rows present the t-statistic of a regression of the probability of being Married and in School on a treatment indicator, with clustered standard errors at the locality level. In row T1998vsT2000, the treatment indicator was equal to 1 if individual was in group T1998 and 0 if in T2000. In row T1998vsC2000(IPW) the treatment indicator was equal to 1 if individual was in group T1998 and 0 if in C2000, and the control units were re-weighted based on the probability of being in either group. Similarly for T2000vsC2000(IPW2000).

Table A11: Proportion of Married Individuals in School (in %)

	1997	1998	1999	2000	2003
T1998	24.18	45.95	39.91	30.29	15.68
T2000	18.33	26.42	31.40	25.94	11.73
C2000	100.00	.	.	3.15	4.49
C2000(IPW1998)	100.00	.	.	5.81	5.37
C2000(IPW2000)	100.00	.	.	6.11	6.73
	T-stat H0: Equal coefficients				
T1998vsT2000	0.94	2.27	1.54	1.08	1.31
T1998vsC2000(IPW1998)	-16.33	.	.	5.29	4.52
T2000vsC2000(IPW2000)	-17.48	.	.	3.88	1.43

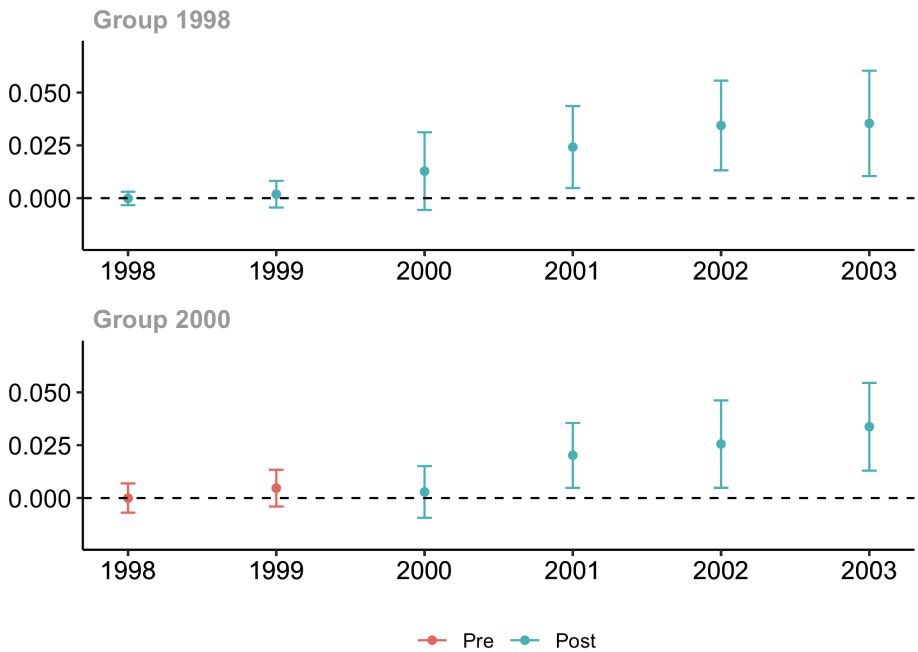
Note: Note: This table reports the proportion of individuals in the state Married and in School, versus all other states (married out of school, single in school and single out of school). The first five rows present this statistic for each group. T1998 are those individuals who started receiving the program in 1998. T2000 is the set of individuals who first received the program in 2000. C2000 is the control group. C2000(IPW1998) and C2000(IPW2000) are the control group weighted by the probability of being first treated in 1998 and 2000, respectively, versus being in the control group. The last three rows present the t-statistic of a regression of the probability of being Married and in School on a treatment indicator, with clustered standard errors at the locality level. In row T1998vsT2000, the treatment indicator was equal to 1 if individual was in group T1998 and 0 if in T2000. In row T1998vsC2000(IPW) the treatment indicator was equal to 1 if individual was in group T1998 and 0 if in C2000, and the control units were re-weighted based on the probability of being in either group. Similarly for T2000vsC2000(IPW2000).

Table A12: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure

Event-Time	ATT(t)	Std. Error	Conf. Interval
-2	0	0.0027	[-0.0069 , 0.0068]
-1	0.0047	0.0033	[-0.0039 , 0.0132]
0	9e-04	0.0018	[-0.0035 , 0.0054]
1	0.0084	0.0025	[0.0019 , 0.0149]
2	0.0173	0.0055	[0.0032 , 0.0315]
3	0.0276	0.0054	[0.0137 , 0.0415]
4	0.0344	0.0078	[0.0145 , 0.0544]
5	0.0354	0.0098	[0.0104 , 0.0604]
N		25623	

Note: This table shows the average treatment effects by length of exposure and the respective standard errors and confidence intervals. N is the number of observations. Event-Time refers to the time period relative to the treatment year.

Figure A8: Effect of Progresa/Oportunidades on the Probability of Marriage by Group



Note: This figure presents the average treatment effect on the treated by treatment group and time period. Group 1998, or T1998, is the group that first received treatment in 1998 and Group 2000, or T2000, is the group that first received treatment in 2000. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustered bootstrap, at the randomization level: locality. The p-value for the pre-test of parallel trends assumption is 0.565.

Table A13: Effect of Progresa/Oportunidades on the Probability of Marriage by Group and Year

	Time	ATT(g,t)	Std. Error	Conf. Interval
T 1998	1998	-1e-04	0.0012	[-0.0033 , 0.0031]
T 1998	1999	0.0019	0.0024	[-0.0044 , 0.0082]
T 1998	2000	0.0128	0.007	[-0.0056 , 0.0312]
T 1998	2001	0.0242	0.0074	[0.0048 , 0.0436]
T 1998	2002	0.0344	0.0081	[0.0132 , 0.0557]
T 1998	2003	0.0354	0.0095	[0.0104 , 0.0603]
T 2000	1998	0	0.0026	[-0.007 , 0.0069]
T 2000	1999	0.0047	0.0033	[-0.0041 , 0.0134]
T 2000	2000	0.0028	0.0047	[-0.0094 , 0.0151]
T 2000	2001	0.0202	0.0059	[0.0048 , 0.0355]
T 2000	2002	0.0255	0.0079	[0.0049 , 0.0462]
T 2000	2003	0.0337	0.0079	[0.0129 , 0.0545]
N			25623	

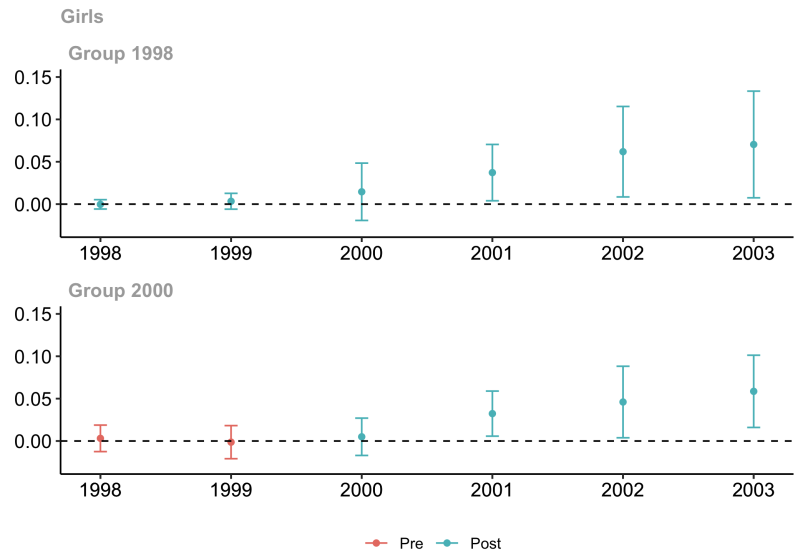
Note: This table shows the average treatment effects by group and length of exposure and the respective standard errors and confidence intervals. N is the number of observations. P-value for pre-test of parallel trends assumption is 0.565.

Table A14: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure: Girls

Event-Time	ATT(t)	Std. Error	Conf. Interval
-2	0.0031	0.0056	[-0.0117 , 0.018]
-1	-0.0014	0.0071	[-0.0203 , 0.0175]
0	0.0016	0.0034	[-0.0075 , 0.0107]
1	0.0138	0.0042	[0.0028 , 0.0249]
2	0.0259	0.0095	[8e-04 , 0.0511]
3	0.0449	0.0094	[0.0201 , 0.0698]
4	0.0619	0.0167	[0.0175 , 0.1063]
5	0.0704	0.0236	[0.0078 , 0.133]
N		12350	

Note: This table shows the average treatment effects by length of exposure and the respective standard errors and confidence intervals. N is the number of observations. Event-Time refers to the time period relative to the treatment year.

Figure A9: Effect of Progresa/Oportunidades on the Probability of Marriage by Group: Girls



Note: This figure presents the average treatment effect on the treated by treatment group and time period. Group 1998, or T1998, is the group that first received treatment in 1998 and Group 2000, or T2000, is the group that first received treatment in 2000. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustered bootstrap, at the randomization level: locality. The p-value for the pre-test of parallel trends assumption is 0.588.

Table A15: Effect of Progresa/Oportunidades on the Probability of Marriage by Group and Year: Girls

	Time	ATT(g,t)	Std. Error	Conf. Interval
T 1998	1998	-3e-04	0.0021	[-0.0058 , 0.0053]
T 1998	1999	0.0034	0.0035	[-0.006 , 0.0127]
T 1998	2000	0.0146	0.0127	[-0.0193 , 0.0484]
T 1998	2001	0.0372	0.0125	[0.004 , 0.0704]
T 1998	2002	0.0619	0.0201	[0.0085 , 0.1153]
T 1998	2003	0.0704	0.0236	[0.0075 , 0.1333]
T 2000	1998	0.0031	0.0059	[-0.0125 , 0.0188]
T 2000	1999	-0.0014	0.0073	[-0.0209 , 0.0182]
T 2000	2000	0.005	0.0083	[-0.017 , 0.027]
T 2000	2001	0.0323	0.01	[0.0057 , 0.0589]
T 2000	2002	0.046	0.0159	[0.0038 , 0.0882]
T 2000	2003	0.0586	0.016	[0.016 , 0.1012]
N			12350	

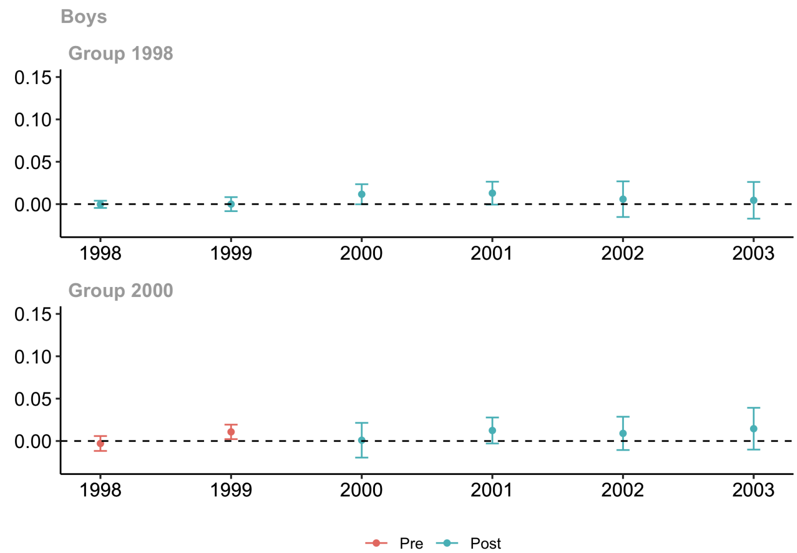
Note: This table shows the average treatment effects by group and length of exposure and the respective standard errors and confidence intervals. N is the number of observations. P-value for pre-test of parallel trends assumption is 0.565.

Table A16: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure: Boys

Event-Time	ATT(t)	Std. Error	Conf. Interval
-2	-0.0029	0.0032	[-0.0114 , 0.0055]
-1	0.0108	0.0031	[0.0027 , 0.0189]
0	2e-04	0.0026	[-0.0066 , 0.007]
1	0.0044	0.0028	[-0.0029 , 0.0116]
2	0.0107	0.0043	[-4e-04 , 0.0218]
3	0.0135	0.0046	[0.0015 , 0.0256]
4	0.0058	0.0074	[-0.0135 , 0.0252]
5	0.0045	0.0075	[-0.0151 , 0.0242]
N		13273	

Note: This table shows the average treatment effects by length of exposure and the respective standard errors and confidence intervals. N is the number of observations. Event-Time refers to the time period relative to the treatment year.

Figure A10: Effect of Progresa/Oportunidades on the Probability of Marriage by Group: Boys



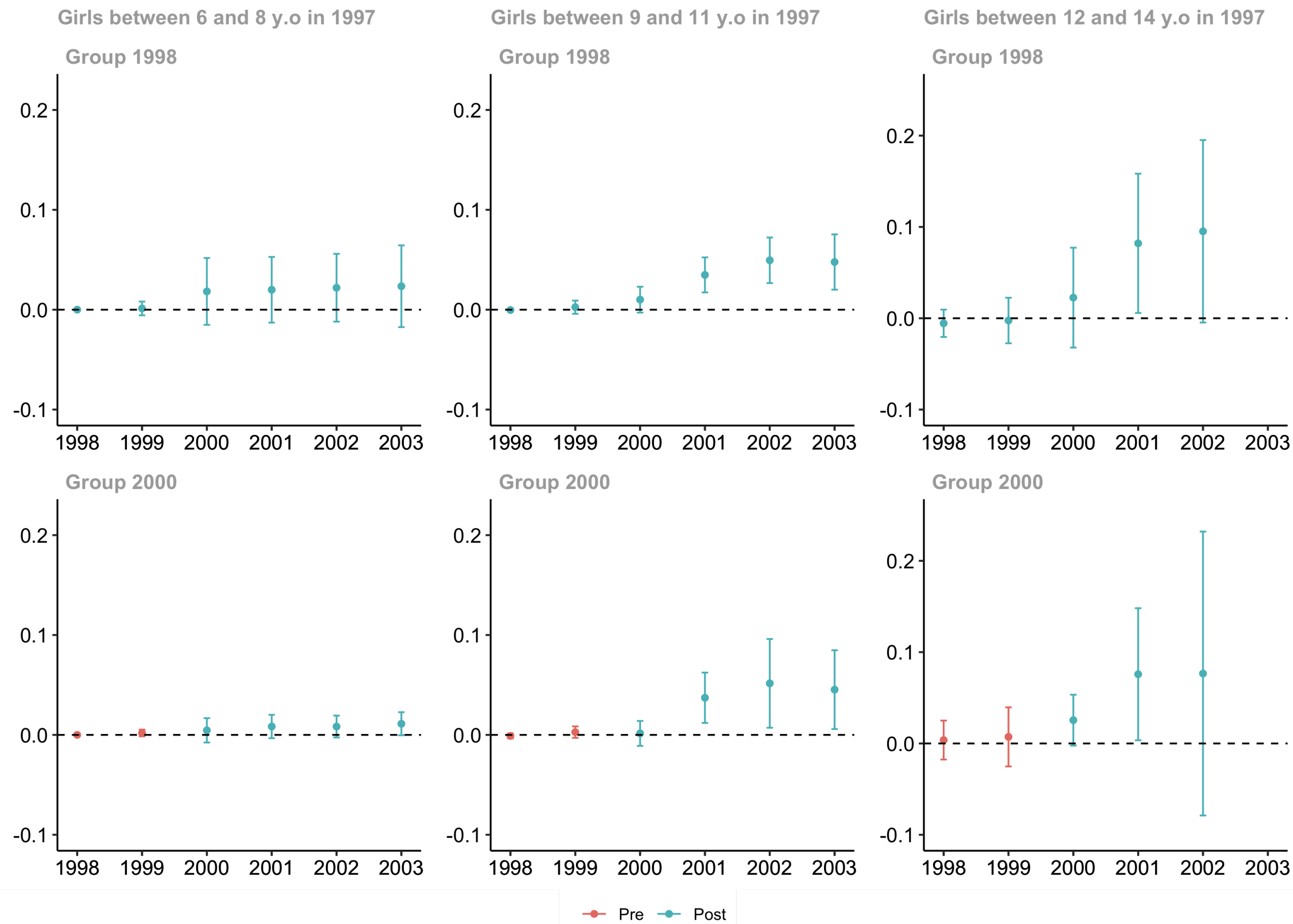
Note: This figure presents the average treatment effect on the treated by treatment group and time period. Group 1998, or T1998, is the group that first received treatment in 1998 and Group 2000, or T2000, is the group that first received treatment in 2000. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustered bootstrap, at the randomization level: locality. The p-value for the pre-test of parallel trends assumption is 0.0109.

Table A17: Effect of Progresa/Oportunidades on the Probability of Marriage by Group and Year: Boys

	Time	ATT(g,t)	Std. Error	Conf. Interval
T 1998	1998	-3e-04	0.0016	[-0.0045 , 0.004]
T 1998	1999	0	0.0031	[-0.0083 , 0.0083]
T 1998	2000	0.0117	0.0044	[-2e-04 , 0.0235]
T 1998	2001	0.013	0.005	[-5e-04 , 0.0265]
T 1998	2002	0.0058	0.0078	[-0.0152 , 0.0269]
T 1998	2003	0.0045	0.008	[-0.0171 , 0.0262]
T 2000	1998	-0.0029	0.0033	[-0.0117 , 0.0059]
T 2000	1999	0.0108	0.0032	[0.0022 , 0.0193]
T 2000	2000	9e-04	0.0076	[-0.0196 , 0.0214]
T 2000	2001	0.0124	0.0057	[-0.0029 , 0.0277]
T 2000	2002	0.009	0.0073	[-0.0107 , 0.0286]
T 2000	2003	0.0145	0.0091	[-0.0101 , 0.0392]
N			13273	

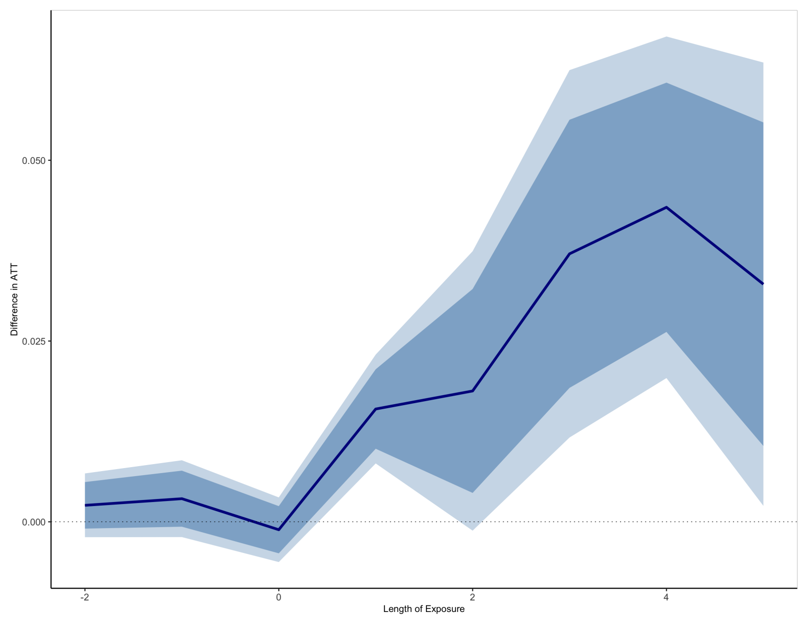
Note: This table shows the average treatment effects by group and length of exposure and the respective standard errors and confidence intervals. N is the number of observations. P-value for pre-test of parallel trends assumption is 0.565.

Figure A11: Effect of Progresa/Oportunidades on the Probability of Marriage: Girls, by Age



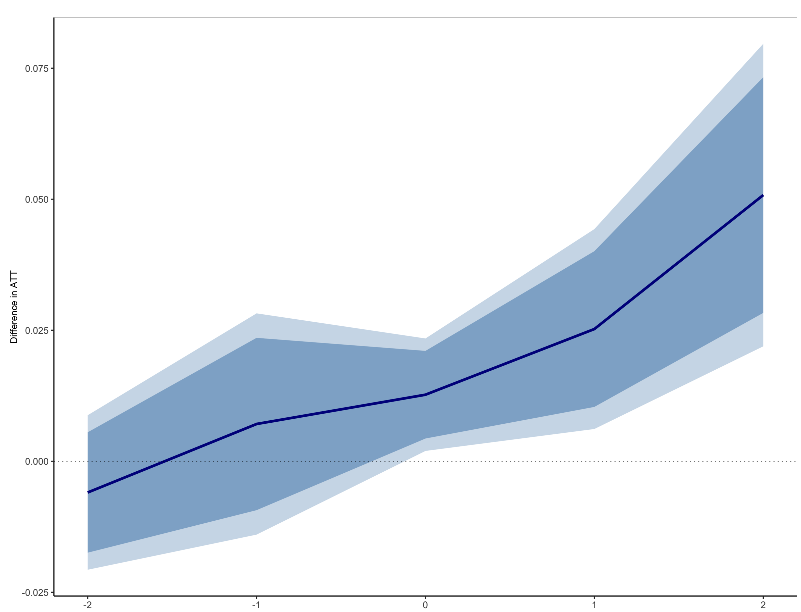
Note: This figure presents the average treatment effect on the treated girls in T1998 and T2000 by year and age at baseline. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustered bootstrap, at the randomization level: locality.

Figure A12: Difference Between the ATT for the Cohort of Girls 13 y.o or older in 2001 and the Cohort of Girls 12 y.o or younger in 2001



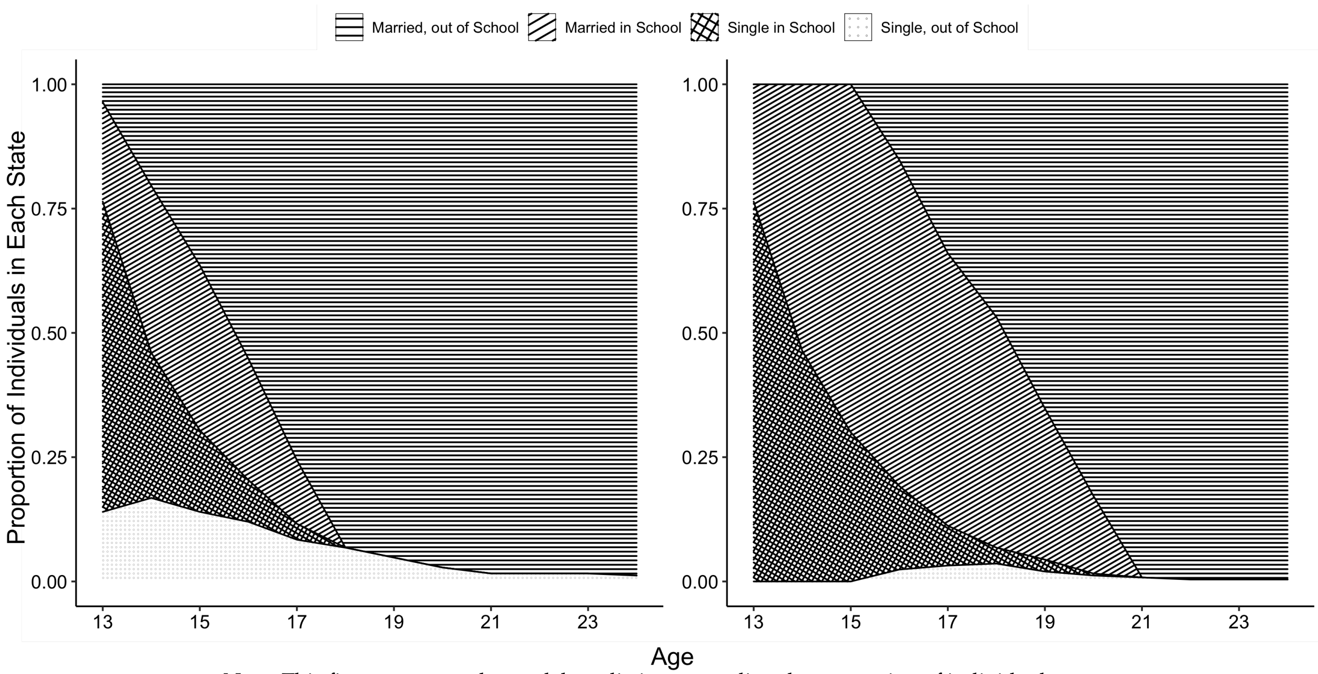
Note: This figure presents the difference in the program's average treatment effect for girls who were older than 13 in 2001 and for girls who were younger than 12 in 2001. Standard errors were obtained through clustered bootstrap, at the randomization level: locality.

Figure A13: Difference Between the ATT for the Cohort of Girls 13 y.o or older in 2001 and the Cohort of Girls 14 and 16 y.o in 1997



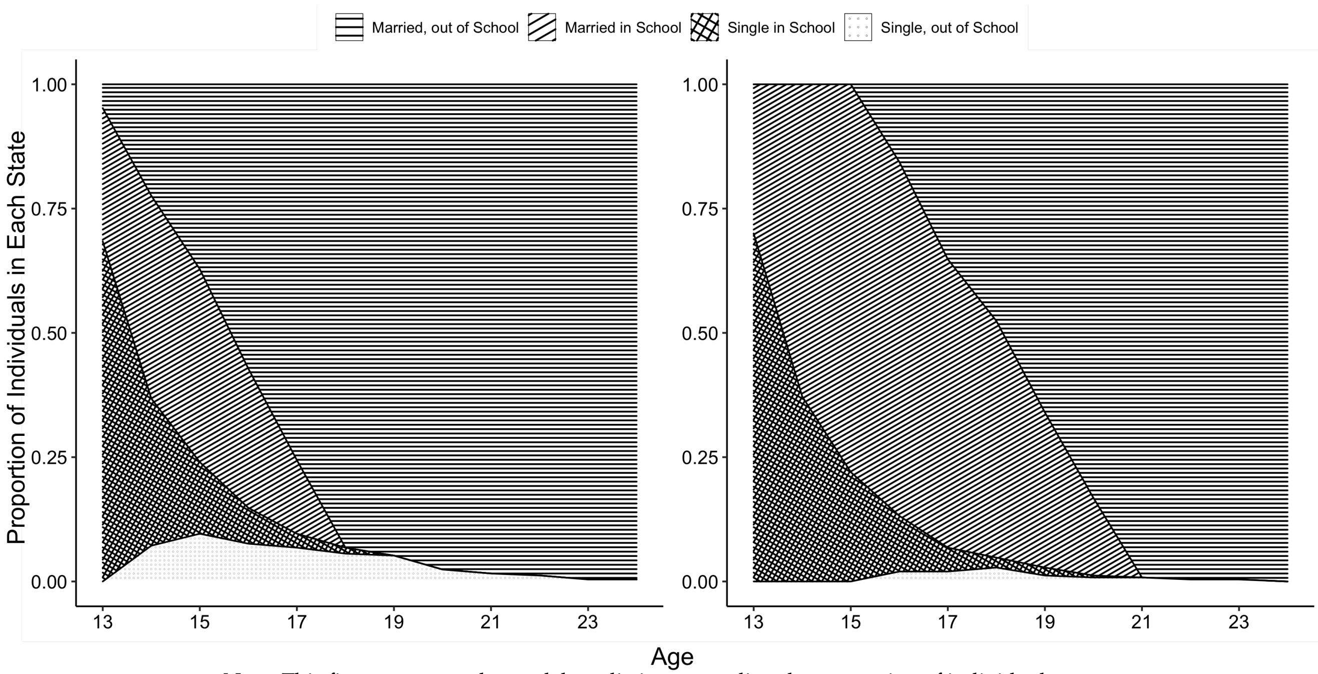
Note: This figure presents the difference in the program's average treatment effect for girls who were older than 13 in 2001 and for girls who were younger than 12 in 2001. Standard errors were obtained through clustered bootstrap, at the randomization level: locality.

Figure A14: Effect of an Education-Conditional Cash Transfer on Marriage and Schooling Choices: no marriage cost, $\tau = 0$



Note: This figure presents the model predictions regarding the proportion of individuals in each state at every year of education. On the left panel it's the prediction if there is no CCT, and on the right panel if there is. The area with: (i) the dotted pattern corresponds to individuals single and out of school, (ii) the checked pattern to individuals who are single in school, (iii) the diagonal stripes to married in school, and (iv) the horizontal stripes to married and out of school.

Figure A15: Effect of an Education-Conditional Cash Transfer on Marriage and Schooling Choices: high marriage cost, $\tau = 3$



Note: This figure presents the model predictions regarding the proportion of individuals in each state at every year of education. On the left panel it's the prediction if there is no CCT, and on the right panel if there is. The area with: (i) the dotted pattern corresponds to individuals single and out of school, (ii) the checked pattern to individuals who are single in school, (iii) the diagonal stripes to married in school, and (iv) the horizontal stripes to married and out of school.

B Appendix: Attrition and Missing Data

In this analysis, there are three important groups of villages: T1998, the group that first received the program in 1998; T2000, the villages that received the program in 2000; and C2000, villages that did not receive the program until 2003. C2000 was included in 2003 and asked retroactive questions regarding 1997, and from 2000 to 2003. So by construction, attrition from the sample only regards the two treated groups. Although the analysis stopped in 2003, I added the attrition information in 2007, since some missing information from age at marriage was recovered from the survey in 2007. Table B1 shows the attrition rate measured by missing individual identifiers from baseline to follow-up surveys. Attrition increases with the years and is higher for T1998 than T2000 (this difference is statistically significant from November 1999 onward). The program positively impacted migration, which might be a potential cause for attrition. However, it is important to note that some individuals not in a year's survey appear in the following years. For example, half of those missing in October 1998 reappear in March 1999. Roughly, between 50 and 70% of those missing in a specific survey reappear in the consecutive one; therefore, I'm often able to retrieve marriage information for each year. Even if the individual is missing all year, in 1999, I can obtain marriage information on 37% of the cases, 30% in 2000 and only 4% in 2003, usually using the information on different surveys, like age at marriage or year of marriage. Since individuals in the treatment groups are more likely to have missing information regarding the outcome of interest, I perform a robustness check using Lee bounds with inverse probability weights and tight bounds. Then, treating the data as if it was repeated crossection, I estimate a lower bound for the aggregate effect for girls of 2p.p, statistically different from zero at 1%, CI=[0.0176, 0.0293].

Table B1: Attrition - Missing ID

	Means	
	T1998	T2000
Individual ID lost from 1997 to 1998 (march)	0	0
Individual ID lost from 1997 to 1998 (october)	.043	.044
Individual ID lost from 1997 to 1999 (march)	.11	.1
Individual ID lost from 1997 to 1999 (november)	.11	.077
Individual ID lost from 1997 to 2000 (march)	.13	.097
Individual ID lost from 1997 to 2000 (november)	.13	.11
Individual ID lost from 1997 to 2003	.15	.13
Individual ID lost from 1997 to 2007	.28	.24

Note: Attrition is higher for T1998 than T2000, from november 1999 this is statistically significant.

Table B2: Missing in Outcome

	Means		
	T1998	T2000	C2000
Missing marriage status in 1997	.023	.021	.02
Missing marriage status in 1998	.059	.057	.02
Missing marriage status in 1999	.072	.059	.02
Missing marriage status in 2000	.094	.083	.021
Missing marriage status in 2001	0	0	0
Missing marriage status in 2002	0	0	0
Missing marriage status in 2003	.17	.16	.018
Missing Age at Marriage	.045	.042	0

Note: This table presents the aggregated average treatment effect on the treated by gender. In the first column of each gender ”All” represents the estimate using as treatment groups both T1998 and T2000. The second and third columns present the average treatment effect over time for treatment groups T1998 and T2000, separately. Standard errors were obtained through clustered bootstrap, at the randomization level: locality.

Besides attrition, there are other inconsistencies across surveys. Namely, individuals' age does not progress as expected, or their gender swaps, which might indicate a mismatch in the IDs or misreport of gender or age (see Tables B3 and B4). These inconsistencies are not statistically different across T1998 and T2000. For the main analysis, I exclude all those observations in which gender is inconsistent, and age decreases. If I am stricter and also drop those observations that show any inconsistency in age (either decreasing or unreasonably increasing), I obtain qualitatively similar results with larger magnitudes. Therefore, if anything, I am being conservative in the main specification. Note that if I forced missing values on marriage in those years in which the observation has an inconsistency, the estimator would not consider all those observations that do not have information on two consecutive years.

Table B3: Attrition - Age inconsistency

	Means	
	T1998	T2000
Age in 1998 (march) not consistent with age in 1997	.039	.037
Age in 1998 (october) not consistent with age in 1997	.036	.037
Age in 1999 (march) not consistent with age in 1997	.16	.15
Age in 1999 (november) not consistent with age in 1997	.052	.051
Age in 2000 (march) not consistent with age in 1997	.16	.16
Age in 2000 (november) not consistent with age in 1997	.11	.11
Age in 2003 not consistent with age in 1997	.09	.083
Age in 2007 not consistent with age in 1997	1	1
Age is inconsistent in at least one year	.32	.33
Age is decreasing in at least one year	.065	.064
Age is inconsistent with 1997 in 2000 and 2003	.23	.22

Note: This table presents the aggregated average treatment effect on the treated by gender. In the first column of each gender "All" represents the estimate using as treatment groups both T1998 and T2000. The second and third columns present the average treatment effect over time for treatment groups T1998 and T2000, separately. Standard errors were obtained through clustered bootstrap, at the randomization level: locality.

Table B4: Attrition - Gender inconsistency

	Means	
	T1998	T2000
Gender changes from 1997 to 1998 (march)	.035	.033
Gender changes from 1997 to 1998 (october)	0	0
Gender changes from 1997 to 1999 (march)	0	0
Gender changes from 1997 to 1999 (november)	0	0
Gender changes from 1997 to 2000 (march)	.035	.034
Gender changes from 1997 to 2000 (november)		
Gender changes from 1997 to 2003	.025	.021
Gender changes from 1997 to 2007	.035	.034
Gender changes from 1997 in at least one year	.06	.058
Gender is missing	0	0

Note: Differences are not statistically different.

The data lacks information for some individuals regarding baseline characteristics used to estimate the propensity score and the outcome regression. Missing rates are extremely low for both treatment groups, but between 5 and 7% of the control group did not have information on asset holdings and household head information in 1997. I did not recur to imputation of these missing values because since I estimated the probability of treatment with these variables, imputation would have introduced bias in the estimates due to the non-zero covariance across the predictors. Therefore, I opted to exclude those observations from the sample.

Furthermore, 34% did not have information on education at baseline. Since the literature suggests that education is a good predictor of marriage decisions, I opted to exclude those observations with education missing and run the main analysis with those with that information for baseline. If I instead excluded the variable from the econometrics models and instead kept the observations, I obtained qualitatively the same results, but with a lower magnitude for the effect of the program on girls across the years: in 2003, girls are 3p.p more likely to be married if they were beneficiaries of the program.

Table B5: Missing in main controls (1)

	Means		
	T1998	T2000	C2000
Missing education in 1997	.026	.026	.34
Missing age in 1997	0	0	0
Missing indigenous background information	.000042	.000067	.00087
Missing if head or spouse went to school	.002	.0021	.067
Missing if head or spouse worked recently	.00013	.00013	.069
Missing standardized poverty index	0	0	.067
Missing if head or spouse is a housewife	0	0	0
Missing gender of household head	0	.000067	.0079

Note: Differences are not statistically different.

Table B6: Missing in main controls (2)

	Means		
	T1998	T2000	C2000
Missing floor quality information	.0033	.0021	.057
Missing wall quality information	.0027	.0013	.057
Missing roof quality information	.0011	.0012	.056
Missing no. bedrooms information	.002	.0011	.064
Missing water provision information	.0012	.0014	.057
Missing electricity provision information	.00059	.00054	.058
Missing animals ownership information	.0015	.0016	.059
Missing land ownership information	.0026	.00087	.057
Missing blender ownership information	.0011	.0004	.056
Missing refrigerator ownership information	.001	.00081	.057
Missing stove ownership information	.0008	.00034	.057
Missing heater ownership information	.004	.0025	.057
Missing radio ownership information	.0015	.00067	.057
Missing TV ownership information	.0011	.00094	.057
Missing video player ownership information	.0014	.0002	.057
Missing washing machine ownership information	.0015	.00027	.057
Missing car ownership information	.0022	.0012	.057
Missing truck ownership information	.0017	.00074	.057

Note: This table presents the aggregated average treatment effect on the treated by gender. In the first column of each gender ”All” represents the estimate using as treatment groups both T1998 and T2000. The second and third columns present the average treatment effect over time for treatment groups T1998 and T2000, separately. Standard errors were obtained through clustered bootstrap, at the randomization level: locality.