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

Dalila Figueiredo*

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Abstract

I study the effect of a conditional cash transfer program on child marriage in rural Mexico and show that it led to a significant increase in early marriage. The program doubled the marriage rates for girls without impacting boys. I show that this result can be attributed to the program's income effect. Contrary to conventional wisdom that such transfers lower early marriage by increasing education, my findings align with a model of schooling and marriage choices accounting for heterogeneous educational preferences. Further analysis supports the model's predictions, showing more significant marriage rates among those with a strong schooling taste.

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1 Introduction

Child and early marriage are widely recognized as a violation of human rights, particularly prevalent in developing countries. They are both a consequence and a cause of poverty, linked to educational abandonment and reduced participation in formal labor markets.[1] This practice disproportionately affects girls and exposes them to increased risks of early childbearing, violence, abuse, and limited autonomy.[2] According to UNICEF, around 20% of women worldwide aged 20 to 24 in 2021 married or entered in a union before turning 18. Early marriage is more prevalent in societies characterized by gender inequality, conservative social norms, conflict, insecurity, and acute poverty. Given the belief that education and poverty alleviation can attenuate child marriage, cash transfers conditional on school attendance or payment of school fees have been identified as one of the most promising strategies to decrease early marriage (Kalamar et al., 2016). However, little is known about the effects of these programs on early marriage decisions in marriage markets with no arranged marriages or marriage payments.

In this paper, I address this question and study one of the world's largest conditional cash transfer programs, Progresa/Oportunidades, implemented in rural Mexico. I show its unintended consequences as it *increased* early marriage and provide evidence of a mechanism behind this unexpected result. The program gives monetary transfers to poor households, conditional primarily on children's school attendance. Therefore, the program can affect marriage through two main channels: (i) education and (ii) income. I discuss these channels in turn.

First, education has been shown to affect marriage decisions. Empirically, most evidence points to a negative effect of education on early marriage (Angrist et al., 2002; Skirbekk et al., 2004; Ferré, 2009; Hallfors et al., 2015; Ashraf et al., 2020; Kırdar et al., 2018; Giacobino et al., forthcoming). In the presence of returns to education in the labor market, increasing schooling increases the opportunity cost of marriage, thus leading to decreases or delays in marriage. Since Progresa/Oportunidades increased years of education, we could expect it to decrease

[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).

early marriage (Behrman et al., 2005, 2009; Dubois et al., 2012). However, Attanasio et al. (2012) show that wages do not respond to education in rural Mexican villages. In the absence of returns to education, increases in education might not actually affect marriage decisions.

Second, Progresa/Oportunidades might affect marriage decisions through an income effect. This effect is ambiguous ex-ante. On the one hand, increased income may reduce households' reliance on marriage as an insurance mechanism (Amin et al., 2016). On the other hand, it could increase the marriage market value of beneficiaries or make marriage-related expenses more affordable, facilitating the formation of new households.

I study the overall effect of Progresa/Oportunidades on early marriage and isolate the effect of income. Initially introduced in 1998 in randomly selected villages, Progresa was renamed Oportunidades in 2000 when the control group villages were also incorporated into the program. In 2003, a new set of villages was selected through propensity score matching to serve as the pure control group. The staggered implementation of the program and the comprehensive panel data available allow for dynamic analysis of the program's causal effect by comparing the three groups of villages over six years using a staggered differences-in-differences estimator.

I find that exposure to the program increased the probability of marrying before the age of 18 years old. One year after the start of the program, the effect was small, of 1.1p.p., not statistically different from zero. After five years of exposure to the program, beneficiaries were 3.4p.p. more likely to be married than the control group. The program more than doubles marriage probability for treated individuals, from 2.5% in the absence of the program to 5.9%. These effects were driven by girls, whose marriage probability increased by 6 p.p. after five years of program exposure. For reference, in 2003, the unweighted proportion of married girls under 18 in the control group was 4%. The program's effect on boys' marriage probability is indistinguishable from zero.

The program had larger effects on older girls. However, being exposed longer to Progresa/Oportunidades did not change the magnitude of the program's impact on marriage. I also observe that the program's effect on marriage is larger in magnitude from 2001 onward for both treated groups. This observation suggests that the changes in the program around that time, which included expanding the benefit to high school years, might be relevant to explain the overall effect. I provide supporting evidence for this hypothesis, as I show that receiving the benefit at an age with high marriage risk, which coincides with high school, facilitates marriage for girls in treated villages.

To understand the mechanisms behind this unexpected result, I estimate the causal effect of income on marriage. To isolate the income effect from the education effect of the program, I restrict the analysis to all children (female and male) who are no longer eligible for program benefits but live in the same household as an eligible member. The program led to substantial increases in marriage for this population.[3] Because these individuals are exposed to an increase in income only and not to the program's conditions, this is evidence that, in this population, positive income transfers facilitate individuals to marry. Marriage might increase with income due to different reasons. If there are financial costs to marriage, positive income transfers allow new households to form or welcome new members in already-formed ones. Alternatively, the program might have increased girls' bargaining power in the household, leading to more independence in decision-making, including marriage. Finally, marriage markets might have changed due to the program: beneficiary girls might be more valuable in the marriage market since they are now relatively wealthier due to the program.

This increase in marriage rates may appear counterintuitive given the common finding that the program increased education. I show that a simple conceptual framework can rationalize both the increase in education and marriage. Agents are heterogeneous in schooling taste and draw a potential partner each period. They simultaneously make marriage and schooling decisions in the presence of a marriage cost. The model provides three key insights. First, in line with the evidence, a school subsidy unambiguously increases school attendance. Second, the effect of the program on marriage is ambiguous. On the one hand, the program increases the attractiveness of being in school, thereby increasing girls' reservation partner quality, which, all else equal, reduces marriage. On the other hand, since marriage is a normal good, the program's income effect increases marriage rates. Depending on the relative magnitude of these effects, a school subsidy like Progresa/Oportunidades can lead to both increases in education

[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.

and marriage. Third, the model provides testable predictions on the program's effect on education and marriage along a schooling taste dimension. I test these predictions by proxying schooling taste with predicted dropout probability. I find that the model correctly predicts the behavior of rural Mexican girls. Namely, girls with low school taste (or high dropout probability) respond more in terms of schooling, while they do not change marriage decisions due to the program. On the other hand, the program does not change schooling decisions for girls whose schooling taste is very high (i.e., the predicted dropout probability is low) since they would have stayed in school with or without the program. However, they marry at higher rates as they are subject to a pure income effect.

The findings in this paper provide novel insights into our understanding of the causes and determinants of early marriage. I look at one of the largest education-conditional cash transfers in the world and show that the program increased early marriage for girls. I provide evidence that income effects can explain the increases in marriage due to the program. Additionally, I study this topic in an understudied context regarding early marriage. In this setting, there are no widespread marriage arrangements or payments, such as dowries or bride prices, and children are the decision-makers. The results in this paper challenge our conventional wisdom on the relationship between conditional cash transfers and early and child marriage. These results are important for the design of large-scale programs. In particular, they highlight how context-specific marriage-market features may determine the intensities of potentially opposing mechanisms, such as income and education effects, and how these can generate unintended consequences if not accounted for.

This paper adds to the research on how marriage markets and income fluctuations interact. Unlike Baird et al. (2011) who found that higher disposable income delays marriage in Malawi, this study presents the opposite results for rural Mexico. It also contrasts with Handa et al. (2015), who show no impact of unconditional cash transfers on child marriage in Kenya. Understanding marriage market responses to income fluctuations is crucial to understand the impact of cash transfers. Corno et al. (2017) highlight how income shocks affect early marriage differently across dowry and bride-price systems, suggesting transfer programs reduce early marriages in bride-price contexts like Malawi and Kenya. This paper's findings offer new insights into income effects on early marriage in settings without marriage payments. These are consistent with Bobonis et al. (2011), who looked at the effect of 1.5 more years of Progresa benefits on mothers of beneficiary children and found an income effect on marriage for young adult mothers of beneficiary.

This paper contributes to the literature on the effect of educational programs on early marriage. My findings are consistent with Behrman et al. (2005) and Araujo and Macours (2021). They find no significant effects on marriage outcomes, looking at the difference between the two treatment groups. By adding a pure control group to the analysis, I show that this null result masks the fact that both groups increased their marriage rates with respect to the control group. I also show that the age at which a girl is exposed to the program affects marriage decisions and that the program's effect is heterogeneous across predicted dropout probability. Surprisingly, the results show that the program, which was praised for its success in educational outcomes, actually led to an increase in early marriage. One possible explanation for this is that the education channel is unable to offset the positive income effect due to a lack of educational returns. In contrast, Gulemetova-Swan (2009) shows Oportunidades in urban Mexico, where returns from education are positive, led to a delay in age at marriage. However, this delay was small (1-4 months) and households self-selected into the program, raising endogeneity concerns. In addition to the causal estimate of the program on early marriage, I contribute with a theoretical framework that can jointly rationalize the increase in early marriage and the findings on the educational effect of Progresa.

The rest of this article is organized as follows. Section 2 presents the Mexican context, and Section 3 introduces Progresa/Oportunidades, the data used in this project, and some relevant summary statistics. Section 4 explains and motivates the empirical strategy, and in Section 5, I present the primary analysis results. In Section 6, I provide evidence of the mechanisms in place and a theoretical model with testable predictions. Finally, Section 7 discusses the implications of these findings, and Section 8 concludes this paper.

2 Context

In the past decades, Mexico has witnessed rapid and prosperous socioeconomic change, and the witnessed economic development was significant 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 labor-market participation from 13 to 47% during the same period (Bhalotra and Fernández, 2021). Usually, age at marriage correlates positively with economic progress, and early marriage is more prevalent in poorer societies. In Mexico, however, the average marriage age has only increased slightly, from 21.2 to 23 years, despite the large economic growth in the past decades. For 1990 and 2010, one would predict a lower per-capita GDP for Mexico, given the country's average age at first marriage (see Figure A1). The percentage of women aged 20-29 in consensual unions has decreased from 60 to 55% (World Bank), but early marriage rates have remained relatively constant, around 23% (UN Women). Fertility, however, has been consistently decreasing.

In Mexico, there are no widespread practices of dowries, and price brides and arranged marriages are rare. Children are the decision-makers. The main reasons offered to explain the high rates of early marriage in Mexico are: first, women in rural areas are mostly valued by their ability to create and sustain a family rather than their occupation; second, girls marry to initiate their sexual lives without the risk of the stigma attached to out-of-wedlock pregnancy; and third, to escape violent households and protect themselves from exploitative groups in areas with extreme violence (Brides, 2017; Taylor et al., 2019). Finally, marriage also offers economic stability, as formal insurance and labor-market opportunities are limited (UNICEF, 2019; Parrado and Zenteno, 2002).[4]

Marriage markets in Mexico are relatively local. According to 'Estadistica de matrimonios' (marriage statistics) from the Mexican Statistical Institute INEG, in 1997 and 1999, 83% of formal marriages occurred between spouses from the same municipality.

[4]According to the survey 'Lo que dicen los pobres', run 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, almost 60% seek family help first to improve housing conditions, and around 65% count on family in case of an accident and 43% when they need a job. Family is a social institution in Mexico; the wider it is, the better insurance it provides.

Most early marriages occur as informal unions. Around 75% of the girls between 15 and 17 years of age who were ever married or in a union report being in an informal union (Girls Not Brides). Given this informality, tackling this problem through legislative changes might be inefficient. A change in the state laws between 2014 and 2018, forbidding completely legal marriages under 18 years of age, led to a decrease in legal marriages offset by an increase in informal unions (Bellés-Obrero and Lombardi, 2020). At the time of the implementation of Progresa/Oportunidades, the minimum legal age at marriage varied by state.

Finally, in Mexico, schooling and marriage are not exclusive. According to Rivero and Palma (2017), in 2015, 17.10% and 8.15% of formally and informally married girls were enrolled in school. School attendance during marriage rarely happens in countries often covered by the literature on early marriage.

3 Progresa/Oportunidades and Data

In 1998, a conditional cash transfer program, Progresa/Oportunidades, was implemented to reduce poverty and its inter-generational cycle in rural Mexican areas through increased education. There were three sets of actions: (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. Benefits were increasing in grade and were slightly higher for girls than boys in middle and secondary school. Transfers consisted, on average, of approximately 14% of eligible households' income (1400 pesos, equivalent to 173 USD in 1998). To receive these transfers, eligible children had to attend scheduled medical visits and at least 85% of classes/school activities.

Eligible households were identified inside each locality through socioeconomic data collected in 1997, assessing their poverty status. On average, 78% of the households in the treatment group were eligible for the program, and 97% of these accepted being beneficiaries (Dubois et

Table 1: 1998 Monthly Benefit (pesos)

Primary School			Secondary School		
	Boys	Girls		Boys	Girls
3rd Year	60		1st Year	175	185
4th Year	70		2nd Year	185	205
5th Year	90		3rd Year	195	225
6th Year	120				

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.

al., 2012). In the analysis of this paper, I consider only those households within the surveyed villages that were eligible for the benefit (poor households).

The program was first implemented in 320 randomly chosen rural localities (now referred to as T1998). A further 186 localities were randomly assigned to the control group. All these localities were highly deprived, with access to elementary school, middle school, and a health clinic (Abúndez et al., 2006). In December 1999, all villages in the control group started receiving the program (T2000).[5] Households in T1998 and T2000 villages 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).

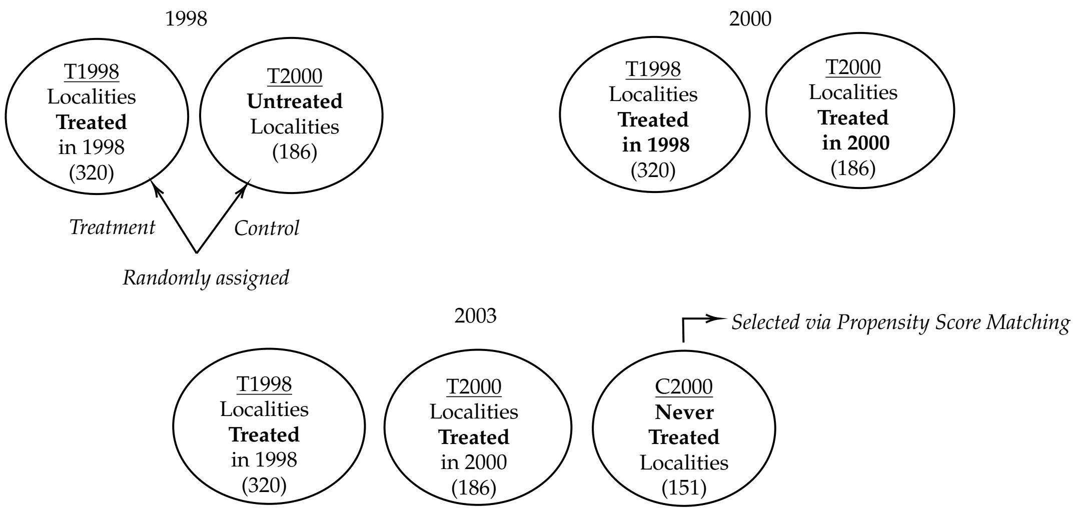
In 2000 and 2001, the program underwent some changes, including its geographic expansion to T2000, and was renamed Oportunidades. Other significant changes for this analysis are extending benefits to high school (*preparatoria*) students and providing bonuses in case students pass grades.[6] After the 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 151 villages are from the same states as the original 506 communities (except for one, for which the neighboring state was used). The matching was performed on aggregated locality aspects using individual data from the Census in 1995 and 2000. These include housing and

[5]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.

[6]Oportunidades introduced *Jóvenes 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.

demographic characteristics, poverty level, labor-force participation, and ownership of durable goods. Besides, localities had to fulfill the program's eligibility criteria concerning distance to schools and health clinics. I refer to this set of localities as C2000, the pure control group. Figure 1 shows a diagram summarizing the program allocation across villages.

Figure 1: Treatment and Control Villages



Note: This figure presents the three groups I will be comparing: T1998, 320 villages that were randomly selected to start treatment in 1998; T2000, 186 localities that were randomly selected to be the control group in 1998, who were then included in the program in 2000; C2000, 151 localities that were selected in 2003, via propensity score matching, to be the pure control group.

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.[7]

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 pure control, or the group of villages that did not receive the program until 2003. I use the data collected through the ENCAESH and ENCEL to evaluate the effect of the program from its start until 2003.

The main outcome of interest for this analysis is *marital status* from 1997 to 2003. I consider an individual married if they report being legally married, living in an informal union, cohabit-

[7] I use information from the survey conducted in 2007 to complete missing information on year and age at marriage.

ing, being 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 reports her status to be single. Marriage rates in 1997 were balanced in treatment and control groups.[8]

My population of interest is all children between 6 and 16 years old in 1997, the baseline year.[9] Keeping all those whose relevant information is not 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.

Summary Statistics Table 2 presents the proportion of married individuals by group and year for the whole sample (Panel (a)) and those younger than 18 (Panel (b)). In the first three rows of the descriptive tables, I present the unweighted average of married individuals in each group, T1998, T2000 and C2000. Due to the non-randomness of the pure control group, C2000, I also present in the fourth and fifth rows the weighted average of married individuals for the pure control group, weighted by the probability of being first treated in 1998 and 2000, respectively, versus being in the control group.

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, 60% married at 16 and 17 (see Figure A2 in the Appendix). The average age at marriage is 1.1 years lower for girls, at 17, than for boys, at 18.1.

[8]In ENCEL2007, individuals were asked about age at first marriage or union. This information allows me to retrieve the year and age at marriage for individuals for whom I did not have that information in previous surveys and for those 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.

[9]Only 1.5% of children declared marrying younger than 12; therefore, I assume that a child becomes at risk of marriage only at that age. I excluded from the sample all children who had not turned 12 until 2003. I do not consider children who were over the age of 16 in 1997 either, given that they were exposed to the program close to their 18th birthday.

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

(a) All

	1997	1998	1999	2000	2001	2002	2003
T1998	0.74	1.45	2.95	5.46	10.01	13.23	15.12
T2000	0.88	1.55	3.14	5.77	10.76	14.07	16.14
C2000	1.48	2.01	3.00	4.39	6.65	9.72	11.34
C2000(IPW1998)	1.33	2.11	3.40	4.85	7.18	9.88	11.31
C2000(IPW2000)	1.39	2.25	3.32	4.81	7.07	9.80	11.27

(b) Under 18 years old

	1997	1998	1999	2000	2001	2002	2003
T1998	0.74	1.37	2.31	3.24	4.44	5.22	4.62
T2000	0.88	1.49	2.34	3.15	4.89	5.64	5.32
C2000	1.48	2.01	1.66	1.87	2.22	2.90	2.53
C2000(IPW1998)	1.33	2.11	1.55	1.67	2.10	2.39	1.88
C2000(IPW2000)	1.39	2.25	1.69	1.74	2.08	2.42	1.94

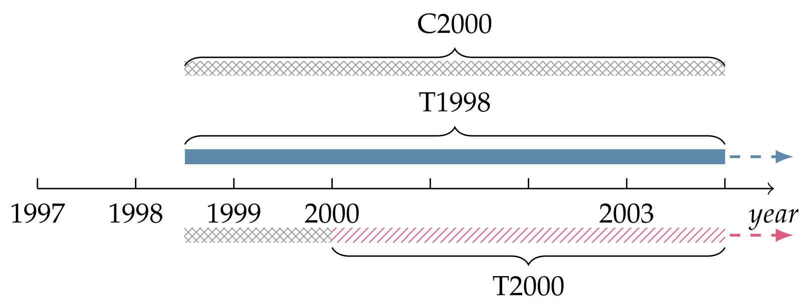
Note: This table presents the proportion of married individuals by group and year. Panel (a) refers to all individuals between 6 and 16 years old in 1997, and Panel (b) refers to the same individuals until they turn 18. T1998 are those individuals who started receiving the program in 1998. T2000 is the 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.

4 Empirical Strategy

To estimate the program's causal effect on early marriage, I exploit the random and quasi-random allocation of the program across municipalities and the variation in the timing of implementation. Recall that I have information on three groups: (i) T1998, the group of villages (320 villages) receiving the treatment in 1998 and beyond; (ii) T2000, the group that first received treatment in 2000 (186 villages); and (iii) a pure control group, C2000, which was never treated in the analysis period until 2003 (151 villages). I observe these groups from 1997 until 2003.

As described before, the program was randomly allocated at the start of the implementation. Villages in T1998 were randomly selected to receive the treatment in 1998, and villages in T2000 were randomly selected as the control group. In 2000, the control group was incorporated into the program and started receiving the benefits. C2000 is the group of villages selected in 2003, through propensity score matching, to be the pure comparison 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 crosshatched gray and pink slide stripes, to emphasize that the same group of villages is a control group until 2000 (crosshatched gray) and joins the treated group from that year onward (pink and slide stripes); and C2000, the control group selected through propensity score matching which was never treated, crosshatched and gray.

The staggered implementation of the program and the rich panel structure of the data allow the estimation of 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-effect 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 period might be contaminated by the treatment effects from other periods, even if the parallel trends and no anticipation assumptions hold.[10]

Second, this estimator allows us to use individual pre-treatment characteristics for more credible parallel trend assumptions. Controlling for these characteristics allows us to compare more similar individuals across the groups of localities. Improving this comparison is particularly important due to the non-randomness of 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. How-

[10]See, for example, Goodman-Bacon (2018), Athey and Imbens (2022),Borusyak and Jaravel (2018), de Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2021), Sun and Abraham (2021).

ever, the selection of villages in C2000 assumes that, given the observed characteristics, the treatment allocation was as good as random. Including individual attributes strengthens the plausibility of the assumption since the comparison is then across similar individuals in similar municipalities. The estimator proposed by Callaway and Sant'Anna (2021), from now on the CS estimator, allows for the use of pre-treatment characteristics 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 the 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.[11] 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 that they would be beneficiaries of the program prior to its implementation. Attanasio et al. (2012) find no evidence of anticipatory behavior 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 a given year and the groups not yet treated would have evolved in parallel.

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

A common practice used to provide evidence on the plausibility of 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

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

where $g \in \mathcal{G}$ is the first treatment year for a given cohort, or group, $p_{g,t}(X)$ 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', ($(1 - D_s)(1 - G_g) = 1$). 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 into the sample analog of the ATT to obtain its estimate. I cluster the standard errors at the locality level, since this was the unit of treatment randomization. Standard errors are estimated using 10.000 bootstrap iterations.

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. However, we cannot guarantee that, on average, these groups are equal in observed and unobserved characteristics. Since we cannot control for potential unobserved differences across individuals in the treated and control villages, it is important to ensure that we compare 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 labor market occupation. Besides, I add wall quality and asset/durable-goods possession, which are good proxies for wealth, and household composition.[12]

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 (Escobar Latapí and González De La Rocha (2009)).[13]

[12]**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 or 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;

[13]**Head and Spouse characteristics**: if any of them has ever attended 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 house-

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.[14] 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 the reduction in the bias.[15]

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 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 (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.

Another caveat I need to address is that, for the pure control group (C2000), the pre-treatment information on the used covariates is recall data collected in 2003 regarding 1997. Therefore, there could be recall bias regarding the baseline characteristics, which could then lead to biased estimates. Since the recall data was only collected for C2000 and not T1998 and T2000, it is hard

wife, 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 consider whether any of them has even attended school. Finally, a household with an indigenous background is one where at least one person speaks an indigenous language.

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

[15]My specification is also similar to the one used by Behrman et al. (2011), which estimates the program's effect on education. The most significant 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.

to judge the accuracy of this data. One way of sensing how problematic this might be is to remove from the propensity score and outcome regressions the variables that are more likely to be subject to recall bias, such as asset possession. I kept those unlikely to have that issue, like household composition and parental education. The results are robust to this specification.

An in-depth analysis of attrition and missing data 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 a robustness check using Lee bounds with inverse probability weights and tight bounds. Treating the data as repeated crossection, I estimate a lower bound for the aggregate effect for girls of 2p.p, statistically different from zero at 1%, CI=[0.016, 0.025]. Besides attrition, the age of some individuals does not progress as expected, or their gender changes. 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 exclude those observations with missing education in the primary analysis. If I instead exclude the variable from both the propensity score estimation and the outcome regression, thus still keeping those observations, I obtain qualitatively the same results but with a smaller magnitude.

5 Results

5.1 Probability of Early Marriage

I start by analyzing whether the program has affected the probability of early 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 2.2 percentage points (p.p) (with the lower bound of the 95% confidence interval being 0.009, and the upper bound 0.035, hereafter CI=[0.009, 0.035]), 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 1.8%, and 1.9% for C2000(IPW2000)).

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

	All		
	All	T1998	T2000
ATT	0.0222	0.0198	0.0266
	(0.0066)	(0.0074)	(0.0087)
	[0.0093 , 0.0351]	[0.0037 , 0.0358]	[0.0079 , 0.0452]
Control Mean		0.018	0.019
Effect in %		107.90	139.43
N		25643	

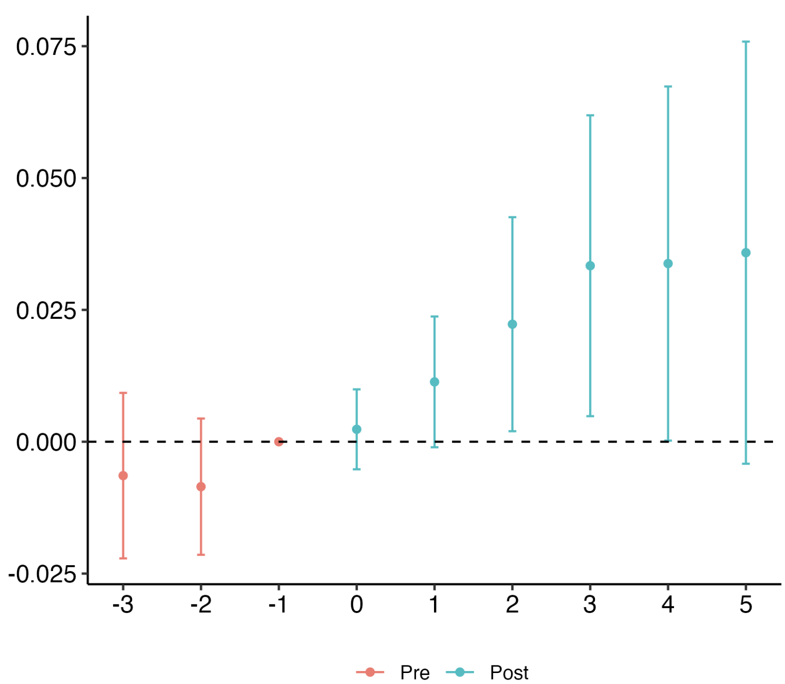
Note: This table presents the aggregated average treatment effect on the treated. 'All' represents the estimate using treatment groups T1998 and T2000. The second and third columns separately present the average treatment effect over time for treatment groups T1998 (who first received the treatment in 1998) and T2000 (who first received the treatment in 2000). Standard errors were obtained through clustering at the randomization level: locality. The average marriage rate for C2000(IPW1998) is 1.8% and for C2000(IPW1998) 1.9%.

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 to 1999 for group T2000. Similarly, time 2 represents two years after treatment. Note that the effects in times 4 and 5 are only estimated using T1998, the only group treated for more than

3 years in the studied period. It is similar 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 the plausibility of the parallel trends assumption.

Then, I observe that the program did not affect early marriage in its first year of implementation ($t = 0$). However, it started leading to increases in marriage after one year of exposure. One year after receiving the benefit ($t = 1$), treatment groups were 1.1 p.p (CI=[-0.001, 0.024]) more likely to be married than the control group, not statistically different from zero. This effect increased to 3.3 pp (CI=[0.005, 0.062]) in the third year and 3.6 p.p (CI=[-0.004,0.076]) after five years. For reference, the unconditional and unweighted proportion of married individuals in the control group was 2.5% in 2003, so the effect corresponds to more than doubling marriage incidence.

Figure 3: Progresa/Oportunidades' Effect on Early 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 (the group that first received the treatment in 1998) time -1 corresponds to 1997, and for T2000 (the 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 clustering at the randomization level: locality.

Across treatment groups, I observed a positive trend in the estimated coefficients one year after the program started for T1998. Despite noisy, the point estaimtes for T1998 are relevant from 2000 onward. In 2000, beneficiaries were 1.7p.p (CI=[-0.01,0.045]) more likely to be married before 18 than non-beneficiaries. In 2001, the estimate increases to 3.4p.p (CI=[-0.0007, 0.068]) and in 2003 3.6p.p (CI=[-0.007, 0.079]), 3 times more likely than the control group (C2000(IPW1998)). Figure A9 and Table A13 in the Appendix show these results.

For the second treatment group, T2000, the program increased marriage after the first year of implementation. In 2001, the effect is 2.6p.p (CI=[-0.008,0.06]) , 3.2p.p (CI=[0.002,0.062]) in 2002 and 4.1 p.p (CI=[0.006,0.075]) in 2003. These results hint that the changes made in the program around 2001 (from Progresa to Oportunidades) were important in explaining the program's positive effect on marriage.

5.2 Heterogeneous Effects

5.2.1 Gender

Around the world, early and child marriage are a more prevalent phenomenon among girls than boys. Also, the socioeconomic consequences associated with early marriage are known to be more damaging for females than males due to early childbearing, exposure to violence, and a higher likelihood of formal labor-market exclusion. 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 early marriage for girls by 3.6 p.p (CI=[0.017, 0.055]). However, this effect was not significant for boys, 0.8p.p (CI=[-0.008, 0.024]).

After 1 year of exposure to Progresa/Oportunidades, girls were, on average, 1.7 p.p (CI=[-0.001, 0.035]) more likely to be married if living in a beneficiary village (see Figure 4, upper panel). After 5 years, early marriage probability increased by 6.4 p.p (CI=[0.02, 0.11]) due to the program. In 2003, in the weighted control group C2000(IPW1998), 2.3% of the girls were married; thus, the program more than tripled the likelihood of marriage for girls in T1998. The point estimates are positive and increasing for girls in both treatment groups across the years.

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

	Girls		
	All	T1998	T2000
ATT	0.0356	0.0318	0.0423
	(0.0097)	(0.0107)	(0.014)
	[0.0166 , 0.0545]	[0.0084 , 0.0551]	[0.0117 , 0.0729]
Control Mean		0.027	0.029
Effect in %		115.82	144.13
N		12356	

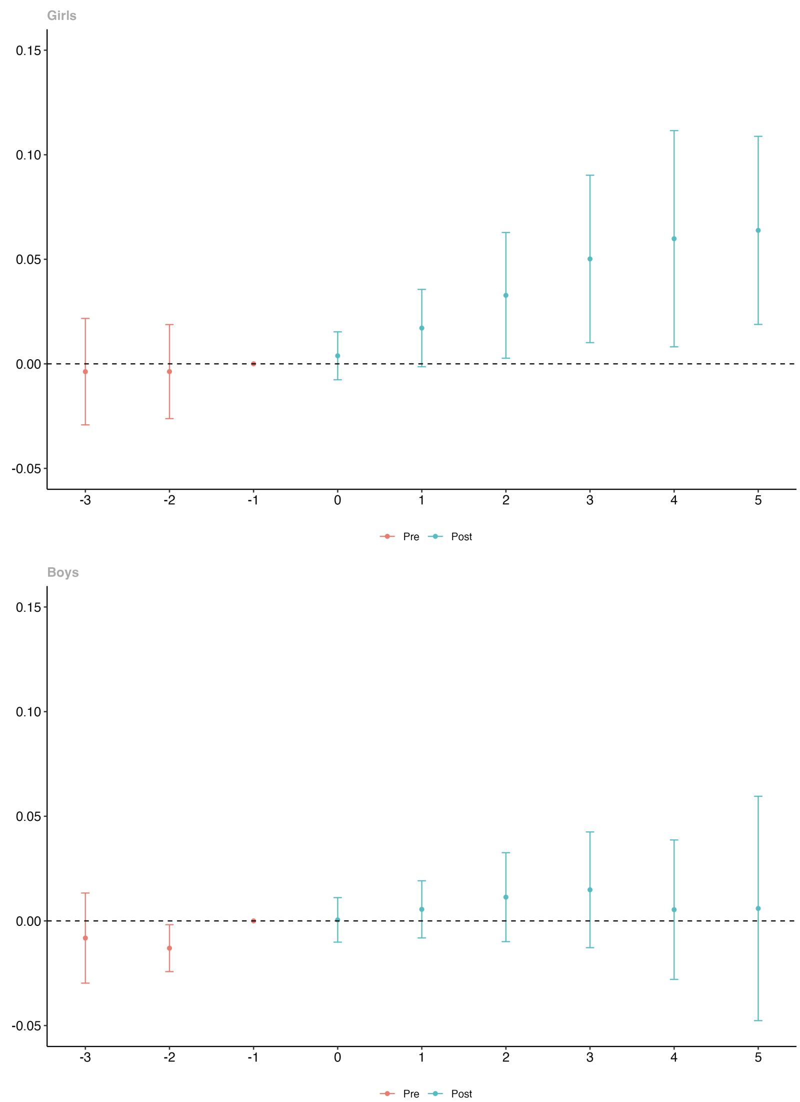
	Boys		
	All	T1998	T2000
ATT	0.0078	0.0065	0.0101
	(0.0081)	(0.0076)	(0.0144)
	[-0.0082 , 0.0237]	[-0.0083 , 0.0214]	[-0.0181 , 0.0383]
Control Mean		0.009	0.01
Effect in %		69.89	105.17
N		13287	

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, respectively. Standard errors were obtained through clustering at the randomization level: locality.

However, it is after 2001 that they start being meaningful (see Figure A10 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%, larger than the weighted averages — between 2.3 and 2.6% (see Table A7).

Results for boys, presented in the bottom panel of Figure 4 (and A11 in the Appendix) are to be interpreted cautiously, as I reject the null hypothesis of no pre-trends. Before the program started for boys in T2000, there was a negative trend, which hints at a different pre-treatment behavior. Thus, the post-treatment results may not be due to the program, but a product of those pre-existing differences. Despite overall positive point estimates, most are not statistically different from zero and have low magnitudes. For the disaggregated results by treatment

Figure 4: Progresa/Oportunidades' Effect on Early Marriage, by Year and Gender



Note: This figure presents the average treatment effect on the treated by length of exposure for girls and boys separately. In red are the estimates before treatment started, and in blue after. The top panel restricts the analysis to girls, and the bottom panel to boys. Standard errors were obtained through clustering at the randomization level: locality.

group, see Tables A16 and A17, in the Appendix.

Those girls for whom I have information on their partners' ages 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 find that older men in eligible and non-eligible households in treated villages were likelier to marry than those in control villages.[16]

In summary, after Progresa/Oportunidades was introduced, girls in households eligible to receive the program in beneficiary villages were more likely to be married before the age of 18, 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, but I observe an increase in marriage probability for older men.

5.2.2 Age

Since marriage is positively associated with age, I investigate whether the program had heterogeneous effects across this dimension. Given the results in the previous section, I restrict this analysis to girls.[17] 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 of age, 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. 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.

Figure A12 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 make the estimates noisier, but the point estimates are consistent with the aggregate results. I find

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

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

positive point estimates across all ages and treatment groups, large for the two older groups, despite under-powered for the oldest group. Those girls who are between 13 and 17 between 2001 and 2003 are the ones for whom the program has the largest, and statistically significant, effect. The magnitude of the effect increases with age, but conditional on age, there is no difference in the effect across treatment groups. This suggests 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 age at which that happens.

Another important observation is that the program started having more substantial effects in 2001 when it extended benefits to secondary school. For example, girls who were 13 or 14 years old in 1997 and turned 16 or 17 in 2000 had 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 program's changes might be relevant to explain its overall effect.

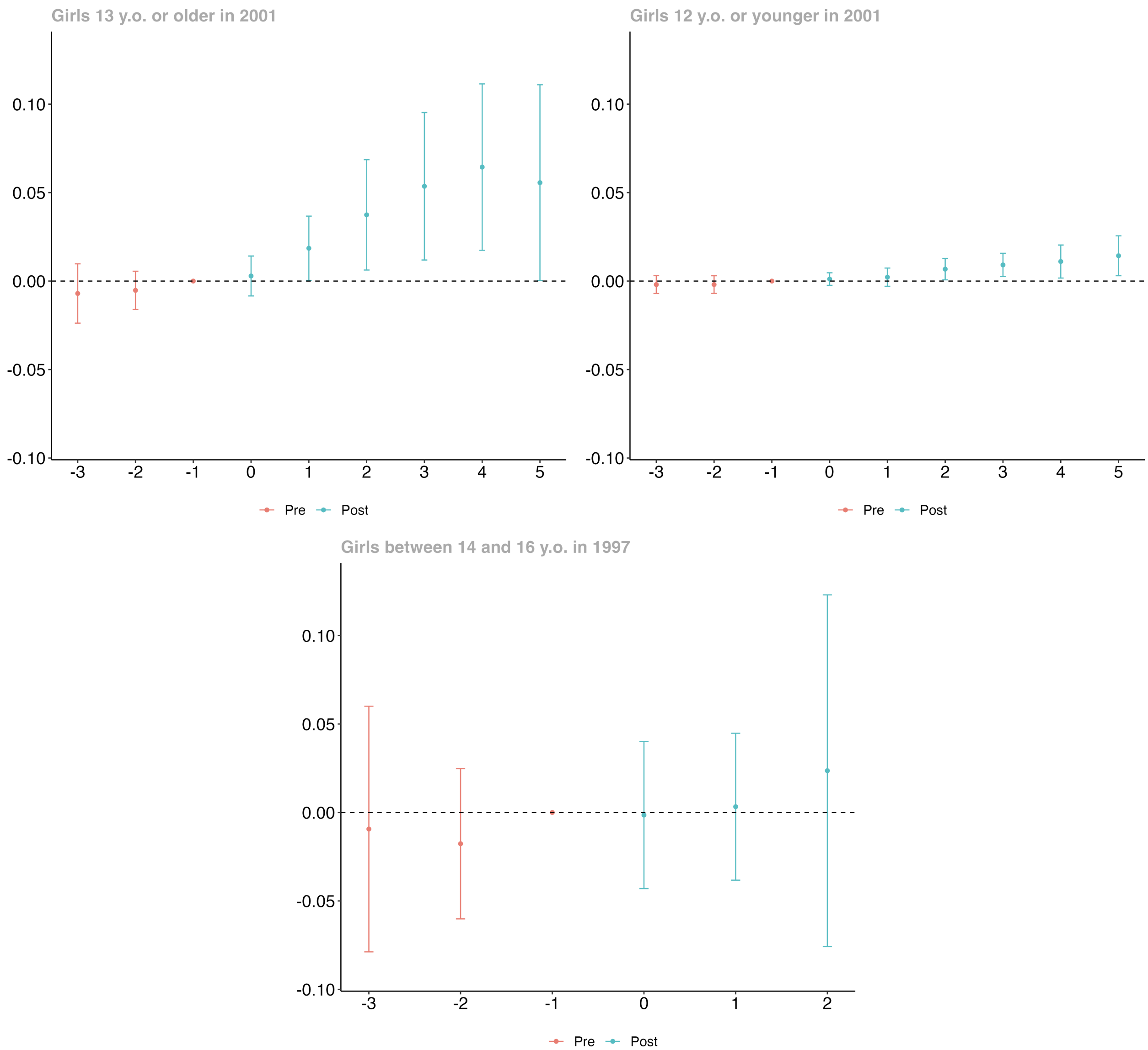
I test this hypothesis by comparing the program's effect across three groups. The first group is all the girls older than 13 in 2001, who were likely to be in high school between then and 2003. The second group is girls younger than 12 years old in 2001, who were unlikely to be in high school during this period. To compare girls of similar ages, the third group is girls between 14 and 17 in the years before the benefits extension. If these were in high school until 2001, they received no benefit.[18]

Figure 5 shows evidence supporting 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 did not behave differently before the program's implementation, but the effect is significantly larger for the older cohort than the younger one (see Figure A13 in the Appendix). Comparing treatment and control individuals who 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 A14 in the Appendix shows that we can reject the null of equal effects for girls

[18]Girls with 13 years of age in 1997 are likely to still be in high school in 2001, thus my choice of restricting the third group to 14-17.

older than 13 in 2001 and girls between 14 and 16 in 1997.

Figure 5: Progresa/Oportunidades' Effect on Early Marriage: High School Benefits



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

Most early marriages in Mexico happen between 15 and 17 years old; thus, the evidence suggests that receiving the benefit at this age facilitates marriage decisions for girls in treated villages. In the next section I study whether the program's income effect explains the overall observed effect.

6 Mechanism: Income Effect

In the previous analysis, I found that the conditional cash transfer program Progresa/ Oportunidades led to an increase in the marriage probability for girls under 18. Although the program was not targeted at reducing early marriage, this result might be surprising since the program led to increases in education, which is often an important mechanism for decreasing child and early marriage.

Given the nature of Progresa/Oportunidades, we cannot disentangle the effect of education from the overall program's effect. This is because if beneficiary children are exposed to more education due to the program, then their household should be receiving the benefit. However, we can hypothesize if and how education is changing marriage decisions in rural Mexico. If there are labor-market returns to 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) find no evidence that the program led to better grades, and Dubois et al. (2012) find that the program harmed grade progression for secondary school students. Second, Attanasio et al. (2012) show that the relationship between wages and education is flat in rural Mexican villages. If this is the case, education may not directly affect marriage decisions. To test the plausibility of this argument, I compare Progresa/Oportunidades' effect on early marriage between villages with returns to education above the median and villages below the median.[19] I find that, on average, the program's effect is larger in villages where returns to education are below the median compared to villages above the median (see Figure A15). This suggests that there might be a negative education effect on early marriage in Mexico, but it is not strong enough due to no returns from education in the labor market.

However, we cannot disregard the hypothesis that education actually increases early mar-

[19] I estimate a Mincerian regression using municipal-level data from the 1995 census. Out of 658 localities of the Progresa/Oportunidades sample, I could match returns to education to 261. The availability of this information is not statistically different between treatment and control villages, and early marriage rates do not statistically differ between villages for which there is information on returns to education and those for which the information is unavailable. The overall effect of the program on villages for which I observe returns to education is similar to the effect in the entire sample.

riage. Agarwal et al. (2023), for example, show that in India — where dowries are common practice — education and youth are valuable in the marriage market, leading to young educated girls marrying earlier than less-educated ones. These findings are consistent with Andrew and Adams (2022), who show that parents believe education is valuable in the marriage market, but age is not. Thus, early school abandonment might push parents to marry off their daughters earlier, leading to the positive effect of education on marriage. In the Mexican case, despite the absence of arranged marriages, it can also be that education is valuable in the labor market, or a way for girls to meet their partners. Further investigation is necessary to determine where the Mexican case falls.

An alternative channel through which the program might affect marriage decisions is the income effect of the program. The monetary transfer received by eligible households that complied with the conditionality might lead to increases in marriage rates. Ex-ante, however, the direction of the income effect is not clear. 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.[20]

6.1 Income Effect on Marriage

To test whether positive income transfers causally change marriage decisions, I exploit household composition variations to separate the program's income effect from its overall impact. I focus on the sub-sample of individuals between 6 and 16 years of age at baseline who were exposed to the income effect only. I restrict the analysis to those individuals who are not eligible for the benefit themselves since they completed, in 1997, the final grade of middle school or higher but live in the same household as an eligible child. For example, these could be older siblings who have completed middle school and whose younger sibling(s) is(are) eligible for the program.[21] The sample consists of 3,115 individuals, 46% of them female and, on average,

[20]It could also be the case that girls in treatment villages marry to have children themselves and continue receiving the program through their children. However, this is unlikely to be the case, as Parker and Ryu (2023) find no evidence suggesting that any age group of women increased fertility in response to the Progresa program.

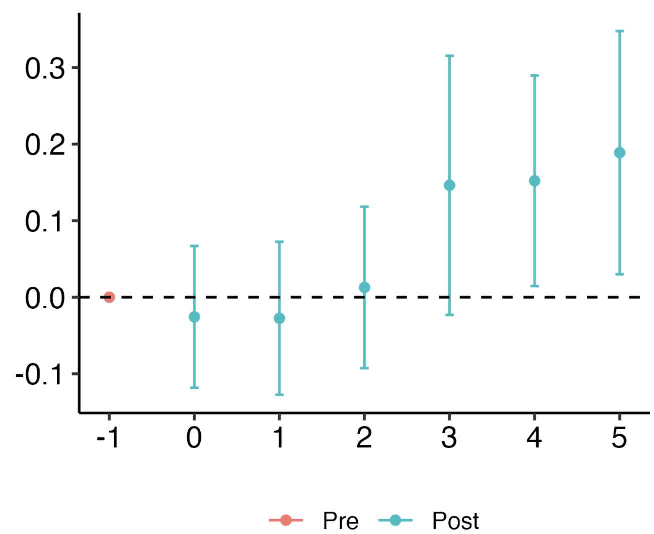
[21]Given changes in eligibility rules in 2000, I compare T1998 and C2000 to avoid eligibility misclassification.

15.51 years old at baseline. I consider marriage after 18 years old.[22] I believe that it is enough to understand if a positive income shock leads to an increase in marriage in rural Mexico without focusing on the age at which the union occurred.

If the program incentivized older siblings to pursue more years of education, then I could not disentangle the two effects. Since the benefit was calculated to compensate for around two-thirds of a child's wage, it is likely not enough to compensate both the wage of the beneficiary child and the older sibling. 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 education 'spillover' effects on non-eligible members within the household.

Figure 6 shows the effect of a positive income shock on marriage probability. Initially, the program had no significant effect on marriage. However, from 2000 onward, I observe positive and substantial effects: the income component of the program led to an increase in marriage probability of between 14 (CI=[-0.02,0.32]) and 19p.p (CI=[0.03,0.35]).

Figure 6: Causal Effect of an Income Shock on the Probability of Marriage



Note: This figure presents the average treatment effect on the treated, by lengthy of exposure, for 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 clustering at the randomization level: locality.

These results suggest that, in this population, receiving a positive income transfer enables

[22]I find consistent, but noisier, estimates if I analyze marriages below 18 due to a small sample size.

individuals to marry. This might be the case for several reasons. First, income availability increases marriage affordability, such as allowing for paying the costs of a new in-law in the household or helping support the creation of new households. Mier y Terán (2004) argue that higher earnings and the opportunity for better jobs in rural Mexico allow young people to create independent households and marry earlier. Rubalcava and Teruel (2005), who studies the effect of Progresa (T1998 vs C2000) on living arrangements, finds that the transfer led to both young adults (children of the head of the household) leaving the household and constitute their own family and to an inflow of new members, some of those being sons-in-law and daughters-in-law.

A second possibility that might explain this income effect is that girls' bargaining power in the household increases with the program. Since the benefit is only received if girls regularly attend school, girls might use it to improve their household situation. Higher bargaining power could entail reducing their homemaking labor supply, thus decreasing the need to live in the same household or through more independent decision-making, such as marrying and exiting the household, conditional on continuing to attend school. Hence, the family continues receiving the benefit. Evidence that supports this is that married girls in treatment villages attend school at a higher rate than married girls in control villages. In 2003, beneficiary-married girls were 6p.p. more likely to attend school than non-beneficiary-married girls. Girls might stay in school, even after marriage, to continue receiving the benefits. A caveat of this theory is that it can be the case that all married girls want to go to school (despite their arrangement within the household), but only beneficiaries can afford it, which also explains the observed pattern.

Finally, the program might also be changing marriage markets. Beneficiary girls' value in the marriage market might increase due to better economic conditions after the program. Mier y Terán (2004), for example, argues that early marriage is more common in land-holding households, suggesting resources are an asset in the marriage market. Indeed, the program also increased the marriage probability of non-eligible young male adults in treatment villages. Within these villages, these men are relatively more affluent (thus the non-eligibility), which hints that some beneficiary girls marry into higher economic status households.[23]

[23]Unrelated to income but due to schooling, girls might also have a broader marriage market. They might be

These are all plausible explanations that support the presented evidence on the causal effect of income on marriage. Despite being unable to pinpoint which of the previously discussed channels is prevailing, these are important results for future research and policy design. Given that marriage is a normal good in Mexico and there are no counteracting forces in the society and the economy, giving monetary transfers to young people leads them to marry more. In the next section, I rationalize this result in a two-period schooling and marriage decisions model, where a school subsidy is introduced.

6.2 Theoretical Framework

In this section, I theoretically discuss how introducing a school subsidy as Progresa/Oportunidades can change schooling and marriage decisions.

Agents live for two periods and discount the future at rate $\beta < 1$. They derive utility from consumption, $u(c)$, and marriage, $f(q)$, which depends on marriage quality q. I assume $u' > 0$, $u'' < 0$ and $f' > 0$. Individuals are heterogeneous in their preference for school $\xi \sim J(\xi)$. Agents can be either in school or in the labor market. Whenever they are in school, they receive an endowment ω and, in the first period, they receive payments from the program p. When they participate in the labor market, they obtain a market wage $w(h)$, increasing in human capital h. School attendance in the first period implies that the human capital level in the second period is $h' > h$.

Agents also make marriage market decisions. At the beginning of both periods, if single, each agent draws a match quality $q \sim G(q)$. If they decide to marry, they pay a cost c. If, in the first period, they choose to marry a partner of quality $\tilde{q}$, they will receive a marriage utility $f(\tilde{q})$ in both periods. Denote single agents not in school as in state N. Single agents in school as in state E. Married and in school agents as EM and married not in school as in state M. Then,

meeting new potential partners at a higher rate, which increases the likelihood of marriage. However, girls are unlikely to meet their partners in school since, on average, their partners are three years older than them. Schooling could also act as a deterrent, thus decreasing child and early marriage.

formally, the per-period payoffs are given by

$$\begin{aligned}
U^N(\xi, h, q) &= u(w(h)) \\
U^E(\xi, h, q) &= u(\omega + p) + \xi \\
U^{EM}(\xi, h, q) &= u(\omega + p - c) + \xi + f(q) \\
U^M(\xi, h, q) &= u(w(h) - c) + f(q)
\end{aligned}$$

Define ξ^{**} as the level of taste for schooling such that the agent is indifferent today between marrying and going to school (EM) and marrying and leaving school (M). If an individual's taste for schooling ξ is larger than ξ^{**}, then this individual prefers to be married in school rather than married out of school. ξ^{**} is decreasing in p, which means that the program decreases the minimum taste necessary for individuals to choose schooling over the labor market, conditional on marriage. Therefore, the program increases the mass of people who choose EM over M.

Furthermore, denote ξ^* the preference for schooling such that agents are indifferent between being single in school and being single in the labor market. If agents' taste for schooling is larger than ξ^*, they will prefer to be single than in the labor market. The program decreases ξ^*, increasing the mass of people choosing E over N.

I assume that $\xi^{**} > \xi^*$, meaning the reservation school taste to choose between going to school married and married is higher than the reservation school taste to choose between going to school single and leaving school single. The idea behind this assumption is that for an agent to continue going to school while married, they need to enjoy school more than when they are single.[24]

Depending on their preference for schooling, agents will have different reservation partner qualities. Denote q^{**} the partner quality that makes agents indifferent between being in school and being in school and married. Denote q^* the partner quality that makes agents indifferent between being in school and married out of school. And, finally, denote $\tilde{q}$, the reservation partner that makes agents indifferent between out-of-school married and out-of-school single.

[24]It is immediate to rationalize this assumption by introducing a time endowment that agents have to use for schooling and marriage activities.

It is immediate to show that $\partial q^{**}/\partial p < 0$.[25] Namely, agents in school are more likely to marry as the program induces an income effect on marriage. Further, $\partial q^{*}/\partial p > 0$: in the choice between marriage out of school and single in school, the program increases the payoff associated with schooling, thus agents are less likely to marry. Finally, $\partial \tilde{q}/\partial p = 0$ since the program does not change the trade-off between being out of school single or out of school married.

Note that, if $\omega > w(h') > w(h)$, all agents would choose schooling in both periods. Since we observe that the program induces higher schooling but that older individuals work, a natural assumption is that $\omega < w(h) < \omega + p < w(h')$. If this is the case, then $q^{**} < q^{*}$ and $\tilde{q} < q^{*}$. Partner's reservation quality is the highest when choosing between being single in school and being married in the labor market.

To understand what a school subsidy does to schooling and marriage, define the mass of individuals in school, $\mathcal{E}$, as the sum of those who choose to be in school single (E) and being in school married (EM), and the mass of married individuals, $\mathcal{M}$, as the sum of those who choose to be in school married (EM) and married in the labor market (M):

$$\begin{aligned}
&\mathcal{E} = 1 - J(\xi^{**}) + (J(\xi^{**}) - J(\xi^{*}))G(q^{*}) \\
&\mathcal{M} = (1 - J(\xi^{**}))(1 - G(q^{**})) + J(\xi^{*})(1 - G(\tilde{q})) + J((\xi^{**}) - J(\xi^{*}))(1 - G(q^{*}))
\end{aligned}$$

The first key result is that an expansion in the program changes the mass of agents in school

$$\frac{\partial \mathcal{E}}{\partial p} = j(\xi^{**})\frac{\partial \xi^{**}}{\partial p}(G(q^{*}) - 1) - j(\xi^{*})\frac{\partial \xi^{*}}{\partial p}G(q^{*}) + (J(\xi^{**}) - J(\xi^{*}))g(q^{*})\frac{\partial q^{*}}{\partial p} > 0$$

The program unequivocally increases schooling, consistent with the empirical findings. In the model, it does so in two distinct ways. First, it reduces the threshold preference ξ^{**} above which agents never consider dropping out of school. Next, it increases the reservation partner quality q^{*} above which agents are happy to get married and leave school.

The second insight from the model is that an increase in the program generosity affects the

[25] See Appendix C for detailed derivations

mass of married agents, according to

$$\frac{\partial \mathcal{M}}{\partial p} = \overbrace{j(\xi^{**})\frac{\partial \xi^{**}}{\partial p}[G(q^{**}) - G(q^{*})]}^{\text{composition effect }>0} + \overbrace{j(\xi^{*})\frac{\partial \xi^{*}}{\partial p}[G(q^{*}) - G(\tilde{q})]}^{\text{composition effect }<0} -$$
$$\underbrace{-g(q^{**})\frac{\partial q^{**}}{\partial p}[1 - J(\xi^{**})]}_{\text{income effect }>0} \underbrace{-g(q^{*})\frac{\partial q^{*}}{\partial p}[J(\xi^{**}) - J(\xi^{*})]}_{\text{composition effect }<0}$$

The effect of the program on overall marriage is, however, ambiguous. It can be decomposed in three composition effects and one income effect.

The first term, $j(\xi^{**})\partial\xi^{**}/\partial p[G(q^{**}) - G(q^{*})] > 0$ contributes to increases in marriage. The program lowers ξ^{**}, the threshold of the school preference above which individuals go to school, independently of their marriage status. These individuals have a lower reservation partner than individuals choosing between school and marriage: $q^{**} < q^{*}$. As a consequence, the average reservation partner in the population decreases, and marriage increases.

A second composition effect $j(\xi^{*})\partial\xi^{*}/\partial p[G(q^{*}) - G(\tilde{q})] < 0$ decreases the prevalence of marriage. The presence of the program moves more mass in the group choosing between school and marriage by decreasing ξ^{*}. These agents have the highest reservation partner quality. As a consequence the average q increases and marriage declines.

The third term, $-g(q^{**})\partial q^{**}/\partial p[1 - J(\xi^{**})] > 0$, denotes the income effect. Agents choosing between schooling and schooling while married have a reservation partner q^{**}. The higher income associated with the program reduces the reservation quality and, therefore, increases marriage.

Finally, the fourth term, $-g(q^{*})\partial q^{*}/\partial p[J(\xi^{**}) - J(\xi^{*})] < 0$ reduces marriage. The presence of the program implies that agents choosing between marriage and schooling are more likely, all else equal, to choose schooling. As a consequence, their reservation partner q^{*} increases, making them less likely to marry.

Adding to the increases in education, the model shows that, depending on the intensity of the income effect, a school subsidy program also increases marriage rates, consistent with the findings in this paper.

The third insight from this exercise are the predictions on the program's effect on marriage and education along the school taste (ξ) dimension. Namely, for low levels of schooling taste, we would observe negative or zero effects of the program on marriage and zero or positive effects on education. For middle levels of schooling taste, marriage could either decrease or increase due to the program, and education increases. Finally, marriage would increase for large levels of schooling taste, and schooling would not be affected. I can test these predictions by testing the program's effect on marriage and education across different levels of schooling taste.

Effect on Education and Early Marriage, by school taste I use their predicted dropout probability to proxy girls' taste for schooling. I start by predicting the dropout probability for every girl in the sample. I define someone as likely to have dropped out from school if, by 2003, their education level was lower than their supposed education level minus one. If a girl were 12 years old in 2003, she would be expected to have 6 years of education. If, in 2003, she had 4 years of education or lower, she is assumed to be likely to have dropped out of school. If their 2003 education level is 5 or higher, or if she is attending school, then she assumed to to be unlikely to be a dropout. I predict girls' probability of dropout by age at baseline using the control group's baseline characteristics available and described in Section 4.[26] Then, I divide the sample by predicted dropout decile and estimate the program's effect on education and marriage by dropout decile. To estimate the effect on marriage, I use the same empirical strategy as in the primary analysis and show the program's aggregate effect across time and treatment groups. To estimate the effect of the program on education (due to lack of information on education for roughly half of the control group for 1998 and 1999), I look at the effect of being in either treatment group on the education level in 2003, using inverse probability weighting to minimize concerns of endogeneity coming from the non-experimental control group.[27] Note

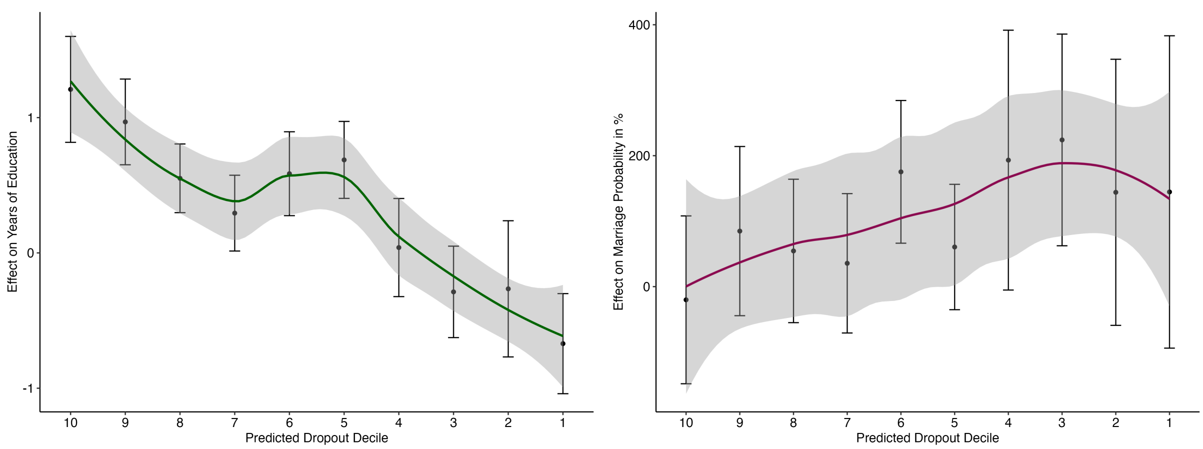
[26]Since dropout probability varied substantially with age, which is highly correlated with marriage and educational attainment, the prediction and decile classification were made separately by baseline age.

[27]The empirical strategy used to estimate the program's effect in education presents some caveats regarding the plausibility of the causality claims. However, the results are similar to the ones obtained using the CS estimator. I present the OLS results instead of the CS results because of the lack of information for the control group in 1998 and 1999. Using OLS, I only need to have the information in 2003.

that decile 1 corresponds to those girls whose dropout prediction is very low; thus, their taste for school is very high. Similarly, girls in decile 10 are the ones whose dropout prediction was very high; thus, their taste for school is very low.

Figure 7 shows that the empirical results are consistent with the model predictions. Girls with low levels of school taste (or high dropout probability) are the ones most affected by the program regarding schooling. However, their marriage decisions do not respond to the benefit. As the probability of dropout decreases, the program has less impact on schooling and more impact on marriage. The program does not change their schooling decisions for girls whose schooling taste is very high (i.e., the predicted dropout probability is low) since they would stay in school with or without the program.[28] Therefore, for these girls, we can interpret the program's effect as purely its income effect and confirm the previous finding that it increased marriage rates.[29]

Figure 7: Effect of Progresa/Oportunidades on the Probability of Marriage by School Taste



Note: This figure presents the average treatment effect on the treated on marriage and education by predicted dropout probability (in deciles).

[28]Estimates are overall noisy due to low number of observations in each decile.

[29]Results are robust to looking into 8 or 12 quantiles of the dropout probability, instead of deciles, as shown in Figures A16 and A17.

7 Discussion of Results

This paper studies child and early marriage in a setting where children are the decision-makers. It is reasonable to question whether this practice in rural Mexico is as harmful as in contexts with arranged marriages and marriage payments. If children decide to marry, they must receive some utility from it. Is it, then, prejudicial for their future? Due to self-selection into marriage and age at marriage, it is extremely challenging to understand the causal effect of early marriage on girls' education, well-being, and labor-market outcomes. However, we can analyze the association between early marriage and female well-being, which are the indicators governments and institutions use to call for the end of this practice.

To do this, I use two datasets. First is the Progresa/Oportunidades data to understand whether child brides differ from adult and single children regarding education and labor-market outcomes. I focus on girls between 6 and 16 years old in 1997. Second, to address the differences in well-being, and partnership quality between child and adult brides, I use the 'Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares - ENDIREH, 2003', a Mexican national household survey on household dynamic and relationships, collected in 2003. Using ENDIREH, I look at females aged between 14 and 24 in 2003. This sample consists of approximately 2400 individuals.[30]

Looking at the analyzed girls in the Progresa/Oportunidades data, married girls in this sample are 0.15 years less educated than single girls. Married girls are 30p.p more likely to work at the house without pay (i.e., homemakers), but conditional on working outside the house, they are more likely to work for money. Of these, 10% of married and single girls work in agriculture (Table A18). Comparing those girls who married under 18 with those who married at 18 or later, I observe that child brides are, on average, less educated but equally likely to be homemakers and to work for money and, conditional on working, more likely to work in agriculture (Table A19).

[30] I do not fully analyze the Progresa/Oportunidades data, given the poor quality of the available data regarding partners and fertility. There are about 14,000 girls in this sample, of whom 2,543 were married by 2003. Out of these, I am able to identify 367 partners. Fertility questions were only asked in 2003 to those girls above 15 years old (a total of 9,589), but I only have information on pregnancy for 1,625 girls, out of which 264 had been previously pregnant.

Among all married girls, those living in treated villages are more educated, less likely to be homemakers, but equally likely to work for money and in agriculture (Table A20). Finally, comparing child brides across treatment and control villages, I observe that treated brides are 0.6 years more educated. They are also less likely to be homemakers, equally likely to work for money, and less likely to work in the agricultural sector (Table A21).

Using ENDIREH 2003, I compare child brides (married between 12 and 17) and adult brides (married between 18 to 24) regarding their education, labor market, well-being, and relationship quality (see A22).[31] Approximately 40% of the girls were child brides, and their average marriage age was 15.8. On average, adult brides married at 19.6. Similarly to what Progresa/Oportunidades data suggests, in the population covered by ENDIREH 2003, child brides have fewer years of education, are less likely to work, and, conditional on working, have lower monthly wages. They are also less likely to have money to spend on themselves and are more likely to be financially dependent and to receive social benefits. I find no difference in reported decision-making power between child and adult brides but child brides are more likely to live in their in-laws' houses. I also find no difference regarding their reported socialization, but child brides are more likely to have suicidal thoughts and live in more violent houses. They are more likely to harm their children physically and verbally and are themselves more likely to be victims of sexual and physical violence from their partners. They are also more likely to have conservative gender views and less likely to have a pre-nuptial agreement. Child brides' partners are, on average, older and less educated. Conditional on working, the partners of child brides earn lower wages.

To conclude, as in other regions in the world, early marriage in rural Mexico is associated with several adverse outcomes: girls who marry before turning 18 years old are, on average, less educated, participate less in the labor market, have more children, and are subject to more violence. Descriptive statistics also suggest that, even though the program has increased early marriage, it might have attenuated the negative consequences of this practice, given that those girls in treated villages are more educated and have better labor-market outcomes, independently of their marital status.

[31] All correlations will be conditional on the girl's age and her partner's age, as well as on housing conditions.

8 Conclusion

I study the effect of a conditional cash transfer program implemented in rural Mexico, Progresa/Oportunidades, on child and early marriage. Leveraging the random assignment of the program at the locality level and its subsequent expansion, I show that the program led to an increase in early marriage rates for girls. I empirically test the program's income channel and show that positive income inflows lead to higher marriage rates in rural Mexico. Therefore, the unintended consequences of the program on early marriage can be explained through this income mechanism.

It is important to highlight that, despite the negative effect I present in this paper, Progresa/Oportunidades was a successful program regarding many other social and economic outcomes.[32] The program improved beneficiary children's physical development, increased their schooling years, reduced child labor, and increased the probability of working and working for a wage while adults. It also increased the likelihood of beneficiaries having a micro-enterprise. In the long run, Araujo and Macours (2021) and Parker and Vogl (2023) documented positive effects of the program on education, labor-market outcomes, housing, and ownership of durable goods, particularly for women. The program has also reduced household poverty and increased consumption and investment in children and livestock. An important caveat, as mentioned before, is that there is no evidence of improvement in cognitive development or achievement tests due to the program. These might be explained by the low investment in the supply side of education, whether within the household or the educational system.

Further research should assess early marriage's economic and social consequences and do a cost-benefit analysis, accounting for the program's documented positive economic consequences, to determine the overall program's effect. Future research should also focus on understanding the causal consequences of early marriage in this context and how education and social norms motivate marriage decisions.

[32]See Parker et al. (2007) for a comprehensive summary and discussion of research on Progresa/ Oportunidades.

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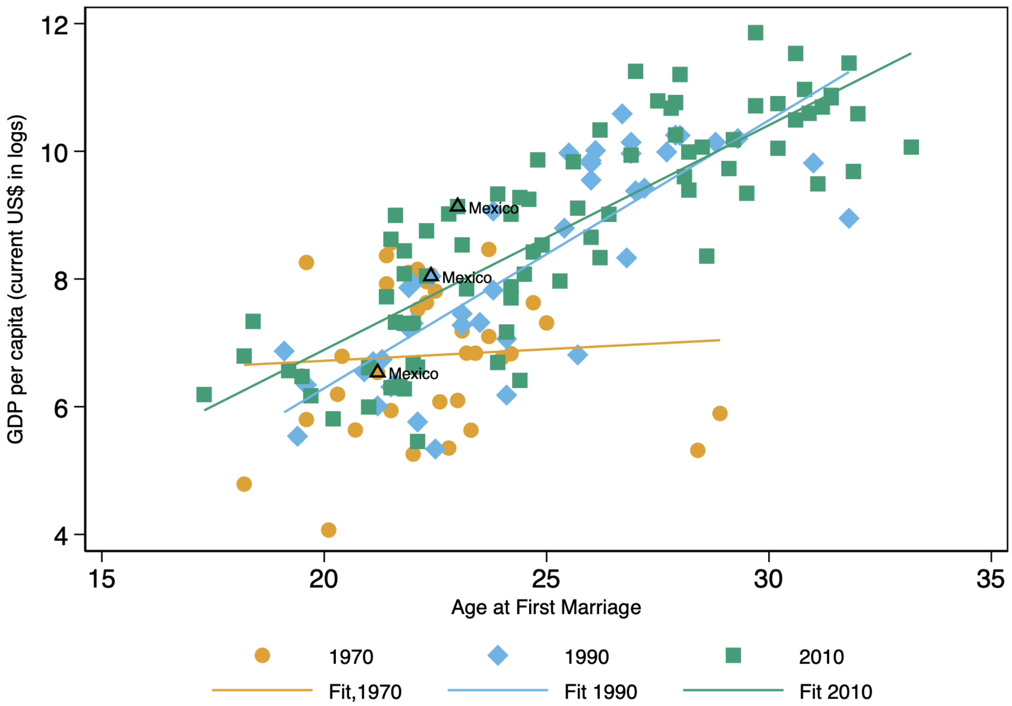
A Appendix

Figure A1: 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 corresponds 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, blue diamonds to 1990, and green squares to 2010.

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

	T1998	T2000	T1998(IPW)	T2000(IPW)	Control
Married	-0.007	-0.01	-0.006	-0.01	0.01
	(-3.344)	(-2.53)	(-2.343)	(-1.93)	
Education Level	-0.067	-0.15	-0.023	0.05	3.44
	(-0.931)	(-1.88)	(-0.265)	(0.61)	
Age in 97	0.165	0.12	-0.067	-0.04	10.64
	(2.650)	(1.77)	(-0.702)	(-0.44)	
Dirt Floor	-0.007	0.02	-0.022	0.01	0.72
	(-0.193)	(0.60)	(-0.657)	(0.24)	
Inferior quality wall	0.002	0.06	0.017	0.04	0.23
	(0.056)	(1.71)	(0.590)	(1.01)	
Inferior quality roof	-0.072	-0.05	-0.005	-0.01	0.21
	(-1.987)	(-1.19)	(-0.158)	(-0.26)	
No. of bedrooms	0.083	0.03	-0.042	-0.00	1.71
	(1.676)	(0.61)	(-0.575)	(-0.02)	
Piped water	-0.067	-0.15	0.011	-0.00	0.28
	(-1.329)	(-2.81)	(0.223)	(-0.04)	
Electricity	-0.018	-0.00	0.008	0.01	0.70
	(-0.421)	(-0.07)	(0.170)	(0.24)	
Animals	0.139	0.11	-0.001	-0.02	0.40
	(4.785)	(3.59)	(-0.019)	(-0.49)	
Land	0.194	0.16	0.002	-0.01	0.64
	(4.696)	(3.59)	(0.057)	(-0.18)	
Blender	-0.008	0.02	0.014	0.02	0.25
	(-0.321)	(0.62)	(0.532)	(0.67)	
Refrigerator	-0.021	-0.04	0.013	0.00	0.05
	(-1.320)	(-2.68)	(1.027)	(0.35)	
Gas Stove	-0.089	-0.09	-0.004	-0.01	0.19
	(-2.098)	(-2.02)	(-0.136)	(-0.17)	
Gas heater	-0.004	-0.01	-0.002	-0.00	0.02
	(-0.760)	(-1.73)	(-0.326)	(-0.58)	
Radio	0.055	0.06	-0.016	-0.00	0.62
	(2.194)	(2.22)	(-0.592)	(-0.10)	
TV	0.025	0.05	0.000	0.02	0.42
	(0.667)	(1.33)	(0.007)	(0.43)	
Video player	0.006	-0.00	0.004	0.00	0.01
	(1.436)	(-0.40)	(0.738)	(0.20)	
Dish Washer	0.006	-0.00	-0.001	0.00	0.02
	(1.031)	(-0.12)	(-0.181)	(0.42)	
Car	-0.011	-0.01	-0.000	-0.00	0.00
	(-2.699)	(-4.02)	(-0.014)	(-0.32)	
Truck	-0.011	-0.01	0.001	-0.00	0.03
	(-1.822)	(-1.00)	(0.176)	(-0.41)	
Anyone in the HH speaks an indigenous language	0.131	0.15	-0.016	-0.01	0.43
	(2.179)	(2.17)	(-0.264)	(-0.12)	

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 it 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	0.079	0.07	0.007	-0.01	0.71
	(2.351)	(2.10)	(0.219)	(-0.31)	
HH Chief or Spouse worked the week before	-0.003	-0.02	-0.012	-0.02	0.91
	(-0.393)	(-1.95)	(-1.252)	(-1.55)	
Housewife	0.248	-0.05	-0.004	-0.01	0.07
	(3.591)	(-5.42)	(-0.546)	(-0.81)	
Number of individuals in the HH	-0.045	0.00	0.066	0.08	7.51
	(-5.893)	(0.02)	(0.479)	(0.44)	
HH head age	-0.076	1.16	-0.597	0.12	43.22
	(-0.591)	(2.88)	(-1.393)	(0.25)	
HH head is female	1.082	-0.05	0.007	0.01	0.06
	(3.373)	(-4.99)	(0.901)	(0.94)	
Anyone in the HH speaks an indigenous language	-0.051	0.15	-0.016	-0.01	0.43
	(-4.910)	(2.17)	(-0.264)	(-0.12)	
HH Chief or Spouse have gone to school	0.131	0.07	0.007	-0.01	0.71
	(2.179)	(2.10)	(0.219)	(-0.31)	
HH Chief or Spouse worked the week before	0.079	-0.02	-0.012	-0.02	0.91
	(2.351)	(-1.95)	(-1.252)	(-1.55)	
At least one child between 0 and 5 y.o	-0.003	0.02	-0.003	0.01	0.69
	(-0.393)	(1.14)	(-0.125)	(0.27)	
At least one teen between 16 and 19 y.o	0.006	0.04	0.003	0.01	0.42
	(0.364)	(2.24)	(0.140)	(0.46)	
At least one woman between 20 and 39 y.o	0.059	0.05	0.006	0.00	0.74
	(3.691)	(3.32)	(0.379)	(0.10)	
At least one woman between 40 and 59 y.o	0.023	-0.03	-0.015	-0.00	0.36
	(1.839)	(-1.73)	(-0.733)	(-0.02)	
At least one woman over 60 y.o	-0.010	-0.03	0.012	0.01	0.10
	(-0.712)	(-2.29)	(1.266)	(0.94)	
At least one man between 20 and 39 y.o	-0.041	0.04	0.014	0.01	0.57
	(-3.012)	(2.15)	(0.661)	(0.25)	
At least one man between 40 and 59 y.o	0.025	-0.02	0.006	0.00	0.46
	(1.574)	(-1.05)	(0.276)	(0.10)	
At least one man over 60 y.o	0.001	-0.04	-0.001	0.01	0.10
	(0.084)	(-2.85)	(-0.099)	(0.81)	
Guerrero	-0.041	-0.00	0.039	0.02	0.06
	(-2.843)	(-0.10)	(1.033)	(0.84)	
Hidalgo	0.043	0.01	-0.007	0.00	0.12
	(1.137)	(0.33)	(-0.127)	(0.05)	
Michoacan	0.077	0.01	-0.021	-0.00	0.13
	(1.748)	(0.16)	(-0.423)	(-0.03)	
Puebla	0.012	0.07	-0.033	-0.04	0.16
	(0.289)	(1.47)	(-0.549)	(-0.58)	
Queretaro	0.057	-0.07	0.003	-0.00	0.04
	(1.411)	(-1.04)	(0.098)	(-0.01)	
San Luis Potosi	-0.064	0.01	0.018	0.02	0.13
	(-1.024)	(0.14)	(0.416)	(0.38)	
San Luis Potosi	0.025				
	(0.467)				

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	-0.010	-0.01	0.01
	(-3.472)	(-3.09)	(-2.182)	(-2.14)	
Education Level	-0.111	-0.17	0.111	0.09	3.47
	(-1.168)	(-1.67)	(0.931)	(0.82)	
Age in 97	0.071	0.01	0.064	0.01	10.59
	(0.831)	(0.15)	(0.483)	(0.08)	
Dirt Floor	0.004	0.03	-0.016	0.01	0.71
	(0.099)	(0.64)	(-0.475)	(0.31)	
Inferior quality wall	0.000	0.06	0.021	0.04	0.22
	(0.002)	(1.64)	(0.715)	(0.92)	
Inferior quality roof	-0.077	-0.05	-0.003	-0.01	0.21
	(-2.158)	(-1.26)	(-0.095)	(-0.23)	
No. of bedrooms	0.090	0.06	-0.073	-0.02	1.72
	(1.829)	(1.01)	(-0.865)	(-0.22)	
Piped water	-0.075	-0.15	0.009	-0.00	0.29
	(-1.483)	(-2.76)	(0.176)	(-0.07)	
Electricity	-0.033	-0.02	0.005	0.01	0.70
	(-0.768)	(-0.44)	(0.100)	(0.24)	
Animals	0.137	0.11	0.001	-0.01	0.40
	(4.791)	(3.36)	(0.028)	(-0.31)	
Land	0.186	0.14	0.005	-0.01	0.63
	(4.404)	(3.13)	(0.136)	(-0.20)	
Blender	-0.013	0.02	0.014	0.01	0.26
	(-0.514)	(0.77)	(0.464)	(0.40)	
Refrigerator	-0.026	-0.05	0.012	0.00	0.05
	(-1.476)	(-2.61)	(0.885)	(0.13)	
Gas Stove	-0.090	-0.09	-0.001	-0.00	0.20
	(-2.066)	(-1.95)	(-0.030)	(-0.10)	
Gas heater	-0.001	-0.01	-0.003	-0.01	0.01
	(-0.114)	(-1.89)	(-0.368)	(-0.67)	
Radio	0.053	0.06	-0.019	0.00	0.62
	(1.902)	(2.00)	(-0.602)	(0.05)	
TV	0.021	0.05	0.003	0.01	0.42
	(0.521)	(1.08)	(0.073)	(0.30)	
Video player	0.005	-0.00	0.005	0.00	0.01
	(1.004)	(-0.60)	(0.893)	(0.26)	
Dish Washer	0.004	-0.00	-0.002	0.00	0.02
	(0.509)	(-0.58)	(-0.176)	(0.32)	
Car	-0.010	-0.01	-0.001	-0.00	0.00
	(-1.923)	(-3.38)	(-0.192)	(-0.40)	
Truck	-0.008	-0.01	-0.002	-0.00	0.03
	(-1.264)	(-0.58)	(-0.224)	(-0.33)	
Anyone in the HH speaks an indigenous language	0.129	0.14	-0.012	0.00	0.42
	(2.100)	(1.99)	(-0.194)	(0.02)	

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 (2)

	T1998	T2000	T1998(IPW)	T2000(IPW)	Control
HH Chief or Spouse have gone to school	0.078	0.06	0.012	-0.01	0.71
	(2.183)	(1.54)	(0.333)	(-0.31)	
HH Chief or Spouse worked the week before	0.001	-0.02	-0.011	-0.02	0.91
	(0.059)	(-2.30)	(-1.161)	(-1.52)	
Housewife	-0.081	-0.08	-0.007	-0.01	0.14
	(-5.364)	(-5.02)	(-0.511)	(-0.77)	
Number of individuals in the HH	-0.036	-0.00	0.047	0.09	7.52
	(-0.270)	(-0.02)	(0.314)	(0.52)	
HH head age	0.652	1.22	-0.584	0.41	43.39
	(1.664)	(2.44)	(-1.147)	(0.74)	
HH head is female	-0.056	-0.05	0.006	0.01	0.07
	(-4.856)	(-4.07)	(0.751)	(0.57)	
Anyone in the HH speaks an indigenous language	0.129	0.14	-0.012	0.00	0.42
	(2.100)	(1.99)	(-0.194)	(0.02)	
HH Chief or Spouse have gone to school	0.078	0.06	0.012	-0.01	0.71
	(2.183)	(1.54)	(0.333)	(-0.31)	
HH Chief or Spouse worked the week before	0.001	-0.02	-0.011	-0.02	0.91
	(0.059)	(-2.30)	(-1.161)	(-1.52)	
At least one child between 0 and 5 y.o	0.017	0.02	0.006	0.01	0.70
	(0.905)	(0.90)	(0.249)	(0.43)	
At least one teen between 16 and 19 y.o	0.038	0.03	0.001	0.01	0.43
	(2.196)	(1.49)	(0.047)	(0.43)	
At least one woman between 20 and 39 y.o	0.028	0.05	0.009	0.00	0.74
	(1.985)	(2.68)	(0.471)	(0.02)	
At least one woman between 40 and 59 y.o	-0.016	-0.02	-0.016	0.01	0.36
	(-0.930)	(-1.07)	(-0.662)	(0.32)	
At least one woman over 60 y.o	-0.056	-0.05	0.006	0.01	0.09
	(-3.506)	(-2.93)	(0.446)	(0.49)	
At least one man between 20 and 39 y.o	0.032	0.05	0.014	0.00	0.58
	(1.890)	(2.44)	(0.598)	(0.19)	
At least one man between 40 and 59 y.o	-0.003	-0.03	0.005	0.00	0.45
	(-0.176)	(-1.45)	(0.190)	(0.07)	
At least one man over 60 y.o	-0.053	-0.05	0.004	0.02	0.10
	(-3.192)	(-2.70)	(0.309)	(1.32)	
Guerrero	0.033	-0.01	0.034	0.02	0.06
	(0.838)	(-0.24)	(0.922)	(0.76)	
Hidalgo	0.074	0.01	-0.006	0.00	0.11
	(1.671)	(0.26)	(-0.106)	(0.07)	
Michoacan	0.008	0.01	-0.027	-0.01	0.14
	(0.166)	(0.11)	(-0.511)	(-0.19)	
Puebla	0.055	0.07	-0.036	-0.03	0.17
	(1.306)	(1.37)	(-0.588)	(-0.50)	
Queretaro	-0.058	-0.06	0.004	-0.00	0.04
	(-0.939)	(-0.99)	(0.120)	(-0.06)	
San Luis Potosi	0.034	0.02	0.024	0.02	0.12
	(0.679)	(0.32)	(0.560)	(0.45)	

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	-0.001	-0.00	0.00
	(-0.463)	(0.77)	(-0.660)	(-0.10)	
Education Level	-0.026	-0.13	-0.120	0.00	3.41
	(-0.334)	(-1.50)	(-1.079)	(0.05)	
Age in 97	0.251	0.21	-0.165	-0.07	10.68
	(3.091)	(2.48)	(-1.354)	(-0.71)	
Dirt Floor	-0.017	0.02	-0.024	0.01	0.72
	(-0.457)	(0.55)	(-0.658)	(0.21)	
Inferior quality wall	0.003	0.06	0.015	0.04	0.23
	(0.101)	(1.71)	(0.480)	(1.08)	
Inferior quality roof	-0.067	-0.05	-0.006	-0.01	0.21
	(-1.782)	(-1.10)	(-0.186)	(-0.29)	
No. of bedrooms	0.076	0.01	-0.020	-0.00	1.69
	(1.405)	(0.19)	(-0.288)	(-0.01)	
Piped water	-0.059	-0.15	0.012	-0.00	0.26
	(-1.162)	(-2.81)	(0.245)	(-0.08)	
Electricity	-0.004	0.01	0.012	0.01	0.70
	(-0.089)	(0.27)	(0.261)	(0.24)	
Animals	0.140	0.12	0.000	-0.02	0.41
	(4.482)	(3.53)	(0.013)	(-0.61)	
Land	0.202	0.18	0.005	-0.00	0.66
	(4.839)	(3.92)	(0.121)	(-0.03)	
Blender	-0.003	0.01	0.013	0.03	0.24
	(-0.122)	(0.43)	(0.466)	(0.82)	
Refrigerator	-0.017	-0.04	0.013	0.00	0.05
	(-1.060)	(-2.54)	(1.016)	(0.43)	
Gas Stove	-0.089	-0.09	-0.008	-0.01	0.18
	(-2.084)	(-2.04)	(-0.283)	(-0.32)	
Gas heater	-0.007	-0.01	-0.002	-0.00	0.02
	(-1.243)	(-1.24)	(-0.297)	(-0.42)	
Radio	0.058	0.06	-0.011	-0.01	0.61
	(2.216)	(2.17)	(-0.397)	(-0.31)	
TV	0.029	0.06	-0.000	0.02	0.42
	(0.788)	(1.52)	(-0.008)	(0.46)	
Video player	0.007	-0.00	0.003	0.00	0.02
	(1.629)	(-0.12)	(0.459)	(0.27)	
Dish Washer	0.009	0.00	-0.001	0.00	0.02
	(1.466)	(0.36)	(-0.061)	(0.42)	
Car	-0.013	-0.01	0.000	0.00	0.00
	(-3.010)	(-3.72)	(0.206)	(0.09)	
Truck	-0.014	-0.01	0.003	-0.00	0.03
	(-2.051)	(-1.30)	(0.517)	(-0.42)	
Anyone in the HH speaks an indigenous language	0.133	0.16	-0.017	-0.01	0.44
	(2.218)	(2.32)	(-0.293)	(-0.20)	

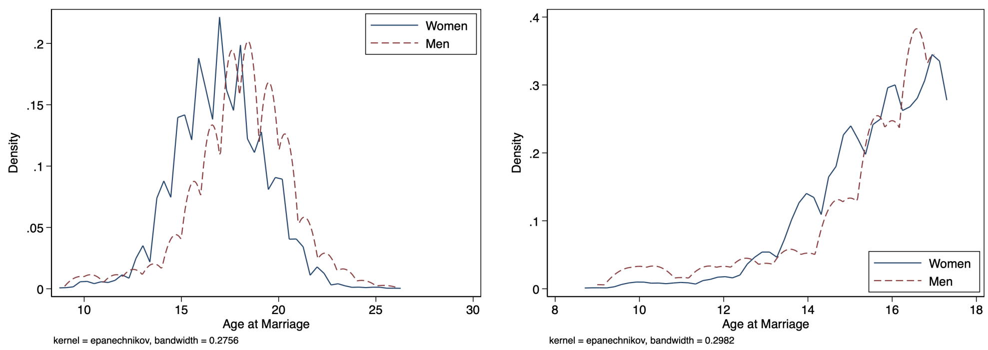
Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator for boys. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns 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	0.081	0.09	0.001	-0.01	0.72
	(2.413)	(2.56)	(0.031)	(-0.27)	
HH Chief or Spouse worked the week before	-0.006	-0.01	-0.014	-0.02	0.91
	(-0.705)	(-1.23)	(-1.260)	(-1.25)	
Housewife	-0.010	-0.01	0.001	-0.00	0.00
	(-3.606)	(-4.38)	(0.709)	(-0.09)	
Number of individuals in the HH	-0.113	0.01	0.071	0.07	7.50
	(-0.833)	(0.06)	(0.506)	(0.38)	
HH head age	1.479	1.10	-0.539	-0.18	43.06
	(4.301)	(2.75)	(-1.095)	(-0.36)	
HH head is female	-0.047	-0.06	0.008	0.01	0.06
	(-4.119)	(-4.90)	(0.944)	(1.21)	
Anyone in the HH speaks an indigenous language	0.133	0.16	-0.017	-0.01	0.44
	(2.218)	(2.32)	(-0.293)	(-0.20)	
HH Chief or Spouse have gone to school	0.081	0.09	0.001	-0.01	0.72
	(2.413)	(2.56)	(0.031)	(-0.27)	
HH Chief or Spouse worked the week before	-0.006	-0.01	-0.014	-0.02	0.91
	(-0.705)	(-1.23)	(-1.260)	(-1.25)	
At least one child between 0 and 5 y.o	-0.004	0.02	-0.011	0.00	0.69
	(-0.256)	(1.19)	(-0.481)	(0.18)	
At least one teen between 16 and 19 y.o	0.078	0.05	0.003	0.01	0.42
	(4.431)	(2.52)	(0.114)	(0.37)	
At least one woman between 20 and 39 y.o	0.019	0.05	0.001	0.00	0.74
	(1.262)	(3.22)	(0.050)	(0.13)	
At least one woman between 40 and 59 y.o	-0.005	-0.04	-0.011	-0.01	0.35
	(-0.345)	(-2.07)	(-0.500)	(-0.37)	
At least one woman over 60 y.o	-0.027	-0.02	0.019	0.02	0.11
	(-2.057)	(-1.32)	(1.870)	(1.47)	
At least one man between 20 and 39 y.o	0.020	0.04	0.019	0.01	0.57
	(1.048)	(1.60)	(0.717)	(0.42)	
At least one man between 40 and 59 y.o	0.005	-0.01	0.004	-0.00	0.47
	(0.321)	(-0.43)	(0.179)	(-0.11)	
At least one man over 60 y.o	-0.031	-0.04	-0.004	0.00	0.09
	(-2.149)	(-2.67)	(-0.295)	(0.14)	
Guerrero	0.053	0.00	0.045	0.02	0.06
	(1.406)	(0.05)	(1.150)	(0.91)	
Hidalgo	0.080	0.02	-0.010	-0.00	0.12
	(1.794)	(0.40)	(-0.169)	(-0.03)	
Michoacan	0.017	0.01	-0.015	0.01	0.13
	(0.409)	(0.21)	(-0.306)	(0.13)	
Puebla	0.060	0.07	-0.030	-0.05	0.16
	(1.502)	(1.55)	(-0.507)	(-0.65)	
Queretaro	-0.069	-0.07	0.002	0.00	0.04
	(-1.101)	(-1.08)	(0.075)	(0.04)	
San Luis Potosi	0.017	0.00	0.012	0.01	0.13
	(0.294)	(0.00)	(0.270)	(0.29)	

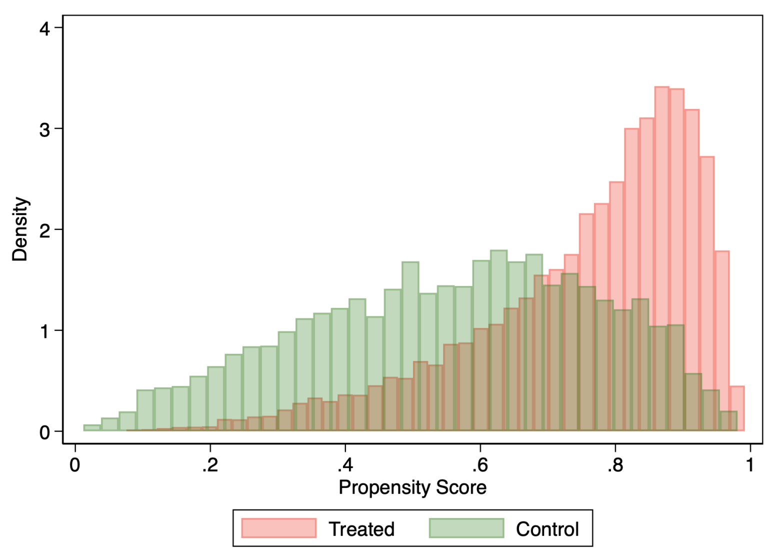
Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator for boys. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns 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 A2: Distribution of Age at Marriage



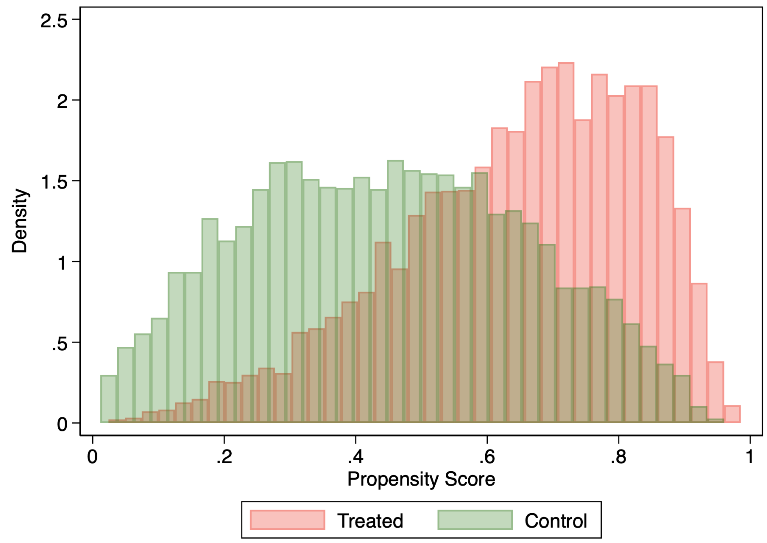
Note: The left panel presents the age distribution at marriage for the entire sample, separately for men and women. The right panel presents the same distribution but only considers individuals who married before 18 years old.

Figure A3: Distribution of the Propensity Score by Group: T1998 VS C2000



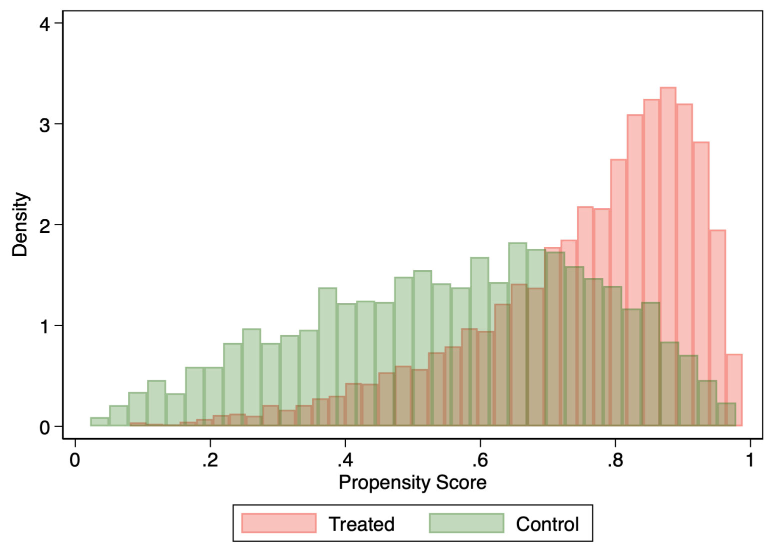
Note: This figure separately presents the histogram of the propensity score for treated (T1998) and control (C2000) groups.

Figure A4: Distribution of the Propensity Score by Group: T2000 VS C2000



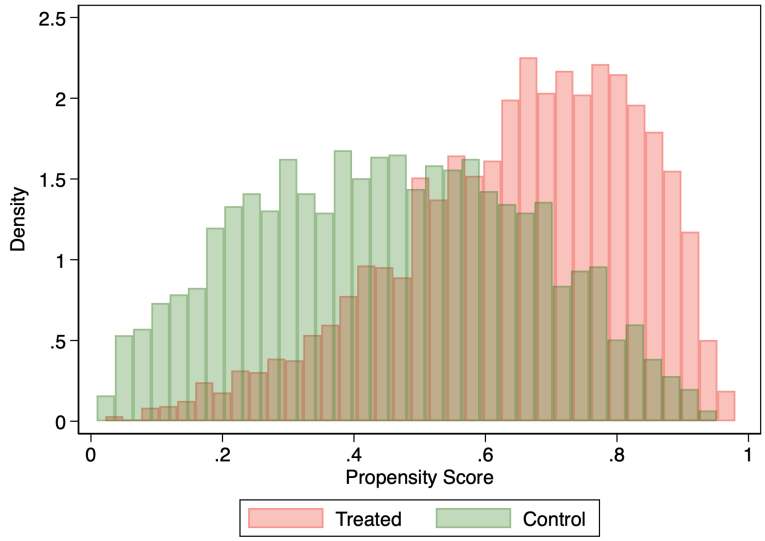
Note: This figure separately presents the histogram of the propensity score for treated (T2000) and control (C2000) groups.

Figure A5: Distribution of the Propensity Score by Group: Girls T1998 VS C2000



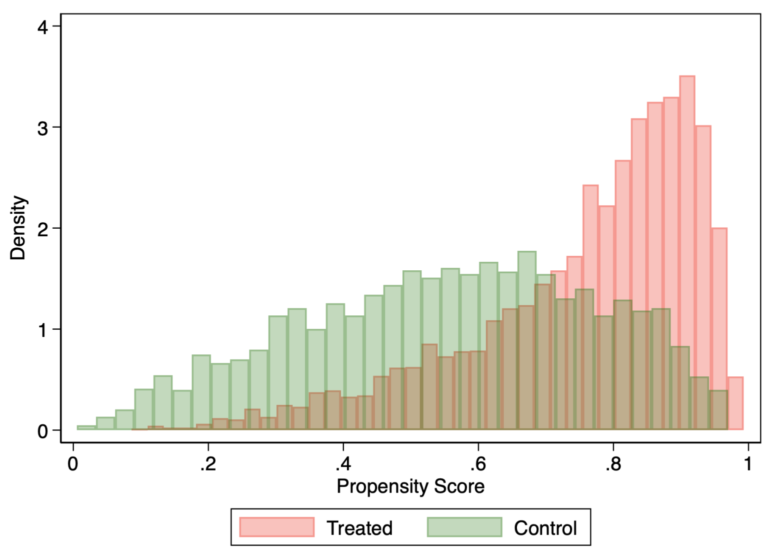
Note: This figure separately presents the histogram of the propensity score for girls in treated (T1998) and control (C2000) groups.

Figure A6: Distribution of the Propensity Score by Group: Girls T2000 VS C2000



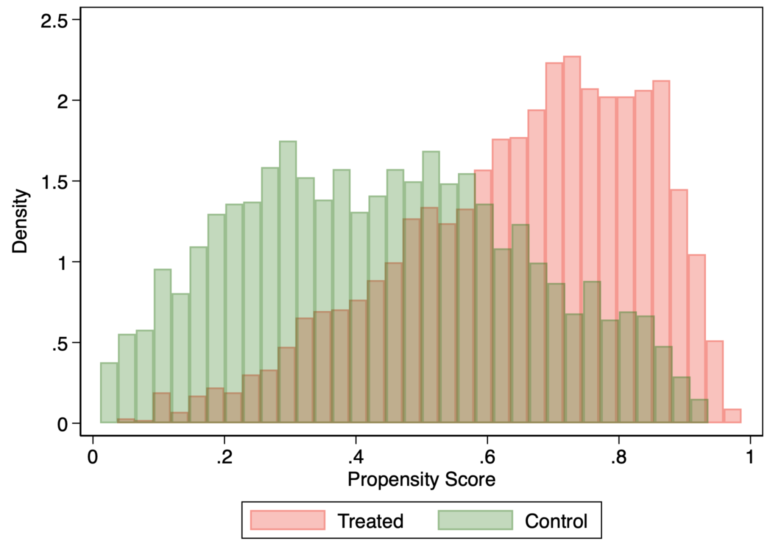
Note: This figure separately presents the histogram of the propensity score for girls in treated (T2000) and control (C2000) groups.

Figure A7: Distribution of the Propensity Score by Group: Boys T1998 VS C2000



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

Figure A8: Distribution of the Propensity Score by Group: Boys T2000 VS C2000



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

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

(a) All

	1997	1998	1999	2000	2001	2002	2003
T1998	1.30	2.22	4.22	7.53	14.03	18.64	20.88
T2000	1.41	2.32	4.30	7.92	15.06	19.43	21.78
C2000	2.77	3.25	4.73	6.76	10.01	13.78	15.63
C2000(IPW1998)	2.33	2.93	4.33	6.38	9.68	12.50	14.27
C2000(IPW2000)	2.37	3.21	4.45	6.49	9.93	12.89	14.64

(b) Under 18 years old

	1997	1998	1999	2000	2001	2002	2003
T1998	1.30	2.12	3.29	4.51	6.69	8.07	7.00
T2000	1.41	2.22	3.09	4.31	7.21	8.41	7.50
C2000	2.77	3.25	2.83	3.13	3.66	4.60	4.01
C2000(IPW1998)	2.33	2.93	2.41	2.68	3.36	3.31	2.27
C2000(IPW2000)	2.37	3.21	2.65	2.70	3.51	3.67	2.62

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. 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	0.75	1.79	3.56	6.31	8.26	9.82
T2000	0.38	0.82	2.02	3.72	6.67	8.96	10.77
C2000	0.27	0.86	1.40	2.19	3.53	5.96	7.37
C2000(IPW1998)	0.33	1.31	2.50	3.29	4.70	7.27	8.32
C2000(IPW2000)	0.40	1.35	2.29	3.25	4.51	7.07	8.08

(b) Under 18 years old

	1997	1998	1999	2000	2001	2002	2003
T1998	0.22	0.68	1.41	2.06	2.29	2.51	2.38
T2000	0.38	0.80	1.62	2.03	2.65	2.96	3.18
C2000	0.27	0.86	0.59	0.71	0.91	1.35	1.18
C2000(IPW1998)	0.33	1.31	0.71	0.69	0.86	1.48	1.50
C2000(IPW2000)	0.40	1.35	0.81	0.92	0.79	1.36	1.35

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

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

Note: This table shows the proportion of children attending school in the year of or after declaring marriage.

Table A10: Proportion of Individuals in the State 'Married and in School' VS All Other States (in %)

	1997	1998	1999	2000	2003
T1998	0.17	0.30	0.74	1.12	0.91
T2000	0.16	0.22	0.57	1.02	0.70
C2000	1.48	.	.	0.08	0.31
C2000(IPW1998)	1.34	.	.	0.08	0.26
C2000(IPW2000)	1.40	.	.	0.08	0.31
T-stat	.	.	.	.	.
T1998vsT2000	0.27	0.88	1.33	0.57	1.09
T1998vsC2000(IPW1998)	-4.79	.	.	8.85	4.81
T2000vsC2000(IPW2000)	-5.14	.	.	6.44	2.01

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 the individual was in group T1998 and 0 if in T2000. In row T1998vsC2000(IPW) the treatment indicator was equal to 1 if the 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	30.43	15.68
T2000	18.33	26.42	31.40	26.14	11.73
C2000	100.00	.	.	3.15	4.48
C2000(IPW1998)	100.00	.	.	3.46	4.30
C2000(IPW2000)	100.00	.	.	3.30	5.18
T-stat	.	.	.	.	.
T1998vsT2000	0.84	2.20	1.52	1.09	1.27
T1998vsC2000(IPW1998)	-15.61	.	.	8.24	4.93
T2000vsC2000(IPW2000)	-16.50	.	.	6.47	2.07

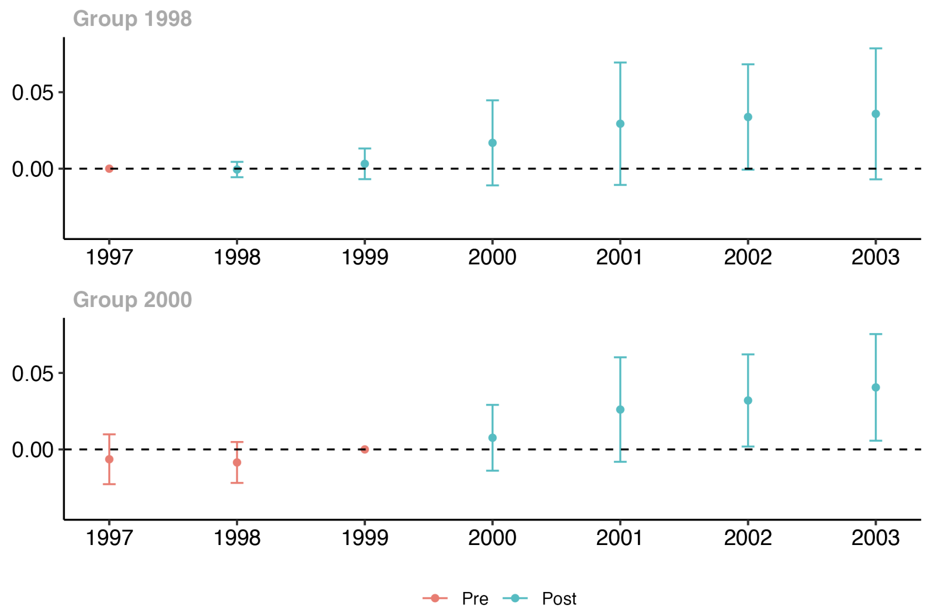
Note: This table reports the proportion of individuals in the state 'in school', versus all other states (married out of school, married in school and single out of school). The first five rows present this statistic for each group. T1998 are those individuals who started receiving the program in 1998. 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 the individual was in group T1998 and 0 if in T2000. In row T1998vsC2000(IPW) the treatment indicator was equal to 1 if the 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
-3	-0.0064	0.0062	[-0.0221 , 0.0093]
-2	-0.0085	0.0051	[-0.0214 , 0.0044]
-1	0	NA	[NA , NA]
0	0.0024	0.003	[-0.0052 , 0.0099]
1	0.0113	0.0049	[-0.0011 , 0.0237]
2	0.0223	0.008	[0.002 , 0.0426]
3	0.0334	0.0112	[0.0048 , 0.0619]
4	0.0338	0.0132	[2e-04 , 0.0674]
5	0.0358	0.0158	[-0.0042 , 0.0759]
N		25643	

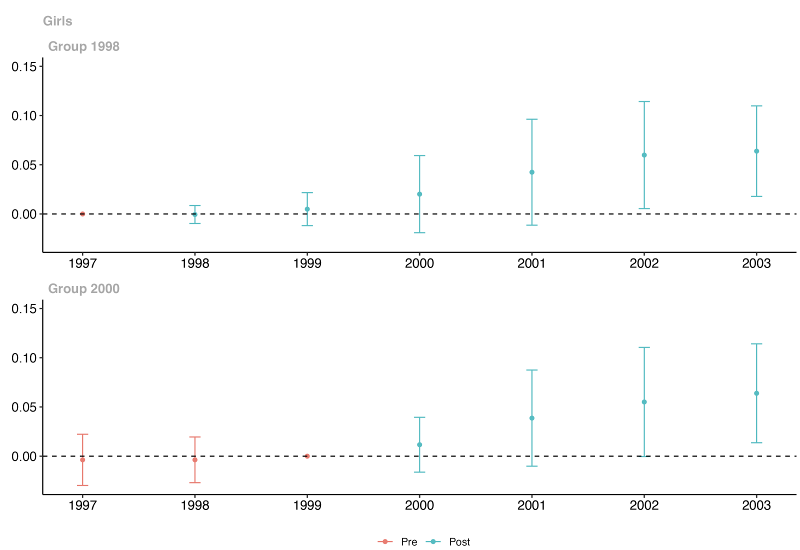
Note: This table shows the average treatment effects by length of exposure and the respective standard errors and confidence intervals. N is the number of observations. Event-Time refers to the period relative to the treatment year.

Figure A9: 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 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 clustering, at the randomization level: locality.

Figure A10: 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 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 clustering, at the randomization level: locality.

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	1997	0	NA	[NA , NA]
T 1998	1998	-6e-04	0.0019	[-0.0056 , 0.0045]
T 1998	1999	0.0032	0.0038	[-0.0068 , 0.0132]
T 1998	2000	0.0169	0.0105	[-0.0109 , 0.0447]
T 1998	2001	0.0294	0.015	[-0.0106 , 0.0694]
T 1998	2002	0.0338	0.013	[-7e-04 , 0.0682]
T 1998	2003	0.0358	0.0161	[-0.007 , 0.0787]
T 2000	1997	-0.0064	0.0061	[-0.0227 , 0.0099]
T 2000	1998	-0.0085	0.005	[-0.0219 , 0.0049]
T 2000	1999	0	NA	[NA , NA]
T 2000	2000	0.0076	0.0081	[-0.0139 , 0.0291]
T 2000	2001	0.0261	0.0128	[-0.0081 , 0.0602]
T 2000	2002	0.032	0.0113	[0.0019 , 0.0621]
T 2000	2003	0.0405	0.0131	[0.0057 , 0.0754]
N			25643	

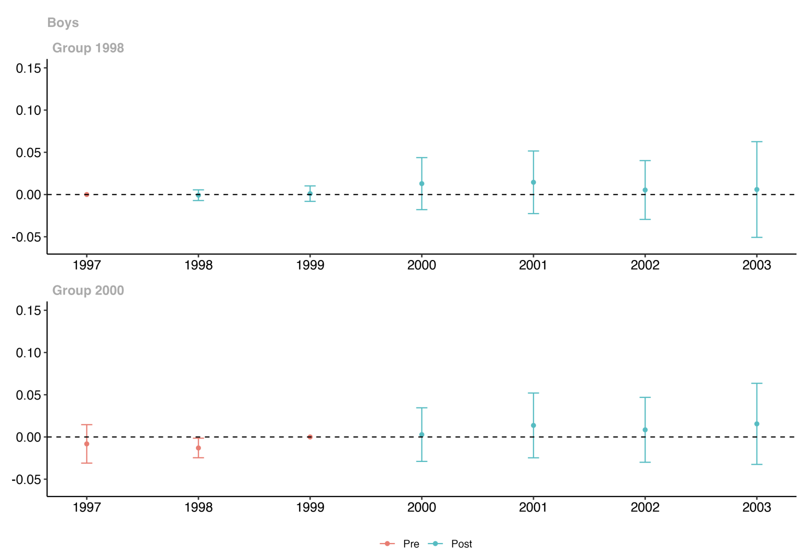
Note: This table shows the average treatment effects by group and length of exposure and the respective standard errors and confidence intervals. N is the number of observations.

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

Event-Time	ATT(t)	Std. Error	Conf. Interval
-3	-0.0037	0.0099	[-0.0292 , 0.0217]
-2	-0.0037	0.0088	[-0.0262 , 0.0188]
-1	0	NA	[NA , NA]
0	0.0038	0.0045	[-0.0076 , 0.0153]
1	0.0171	0.0072	[-0.0014 , 0.0356]
2	0.0328	0.0117	[0.0027 , 0.0628]
3	0.0502	0.0156	[0.0101 , 0.0902]
4	0.0598	0.0202	[0.0081 , 0.1115]
5	0.0638	0.0176	[0.0189 , 0.1088]
N		12356	

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 A11: 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 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 clustering at the randomization level: locality.

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	1997	0	NA	[NA , NA]
T 1998	1998	-6e-04	0.0034	[-0.0097 , 0.0086]
T 1998	1999	0.0049	0.0062	[-0.0118 , 0.0217]
T 1998	2000	0.0201	0.0146	[-0.019 , 0.0593]
T 1998	2001	0.0424	0.02	[-0.0114 , 0.0962]
T 1998	2002	0.0598	0.0203	[0.0055 , 0.1142]
T 1998	2003	0.0638	0.0171	[0.0178 , 0.1098]
T 2000	1997	-0.0037	0.0097	[-0.0297 , 0.0223]
T 2000	1998	-0.0037	0.0086	[-0.0269 , 0.0195]
T 2000	1999	0	NA	[NA , NA]
T 2000	2000	0.0116	0.0104	[-0.0162 , 0.0395]
T 2000	2001	0.0387	0.0182	[-0.0102 , 0.0875]
T 2000	2002	0.0551	0.0206	[-4e-04 , 0.1105]
T 2000	2003	0.0639	0.0187	[0.0136 , 0.1141]
N			12356	

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.

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

Event-Time	ATT(t)	Std. Error	Conf. Interval
-3	-0.0082	0.0094	[-0.0297 , 0.0133]
-2	-0.013	0.0049	[-0.0242 , -0.0018]
-1	0	NA	[NA , NA]
0	5e-04	0.0046	[-0.0101 , 0.0111]
1	0.0055	0.0059	[-0.0081 , 0.0192]
2	0.0114	0.0092	[-0.0099 , 0.0326]
3	0.0149	0.012	[-0.0128 , 0.0425]
4	0.0054	0.0145	[-0.0279 , 0.0387]
5	0.006	0.0233	[-0.0477 , 0.0596]
N		13287	

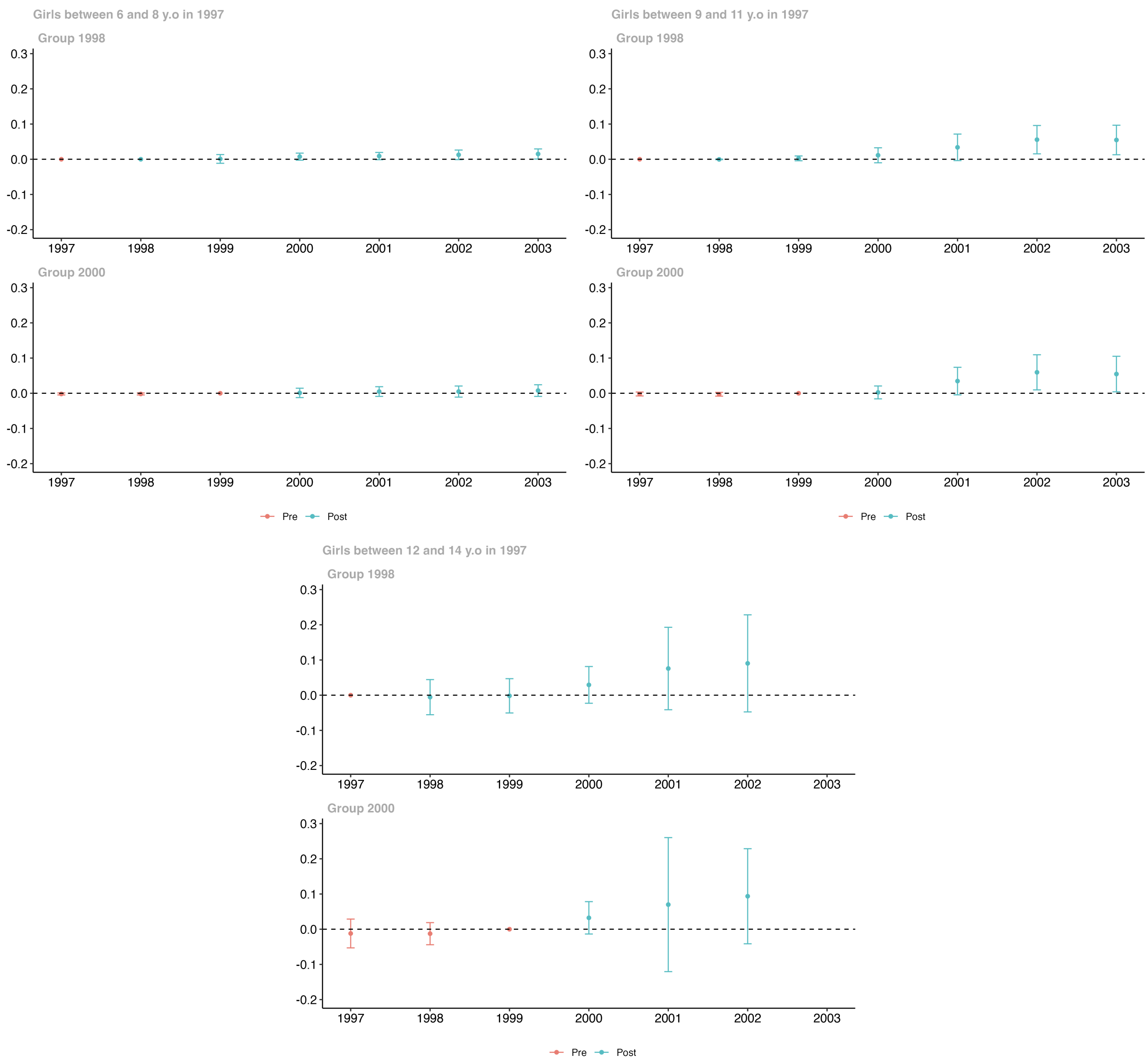
Note: This table shows the average treatment effects by length of exposure and the respective standard errors and confidence intervals. N is the number of observations. Event-Time refers to the time period relative to the treatment year.

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	1997	0	NA	[NA , NA]
T 1998	1998	-8e-04	0.0027	[-0.0071 , 0.0056]
T 1998	1999	0.0011	0.0039	[-0.0081 , 0.0102]
T 1998	2000	0.0129	0.0131	[-0.0179 , 0.0437]
T 1998	2001	0.0145	0.0157	[-0.0226 , 0.0516]
T 1998	2002	0.0054	0.0148	[-0.0295 , 0.0402]
T 1998	2003	0.006	0.024	[-0.0507 , 0.0626]
T 2000	1997	-0.0082	0.0097	[-0.031 , 0.0146]
T 2000	1998	-0.013	0.0049	[-0.0246 , -0.0014]
T 2000	1999	0	NA	[NA , NA]
T 2000	2000	0.0028	0.0135	[-0.0289 , 0.0345]
T 2000	2001	0.0137	0.0163	[-0.0247 , 0.052]
T 2000	2002	0.0085	0.0163	[-0.03 , 0.0469]
T 2000	2003	0.0155	0.0204	[-0.0325 , 0.0635]
N			13287	

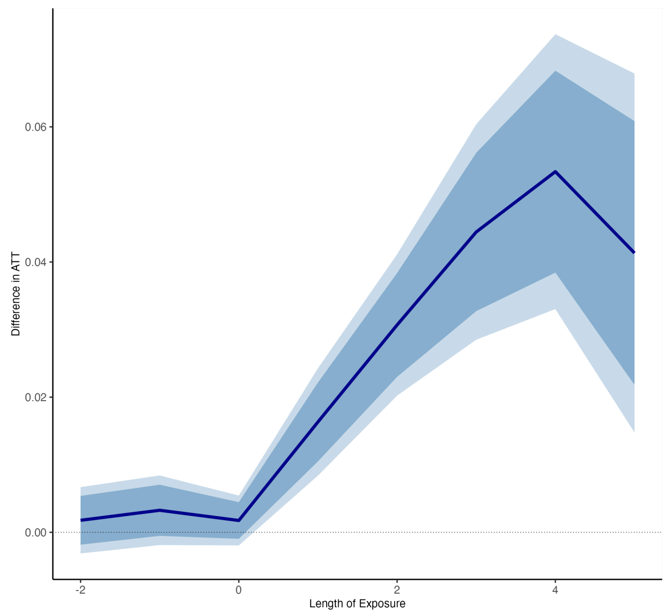
Note: This table shows the average treatment effects by group and length of exposure and the respective standard errors and confidence intervals. N is the number of observations.

Figure A12: Effect of Progresa/Oportunidades on the Probability of Marriage: Girls, by Age



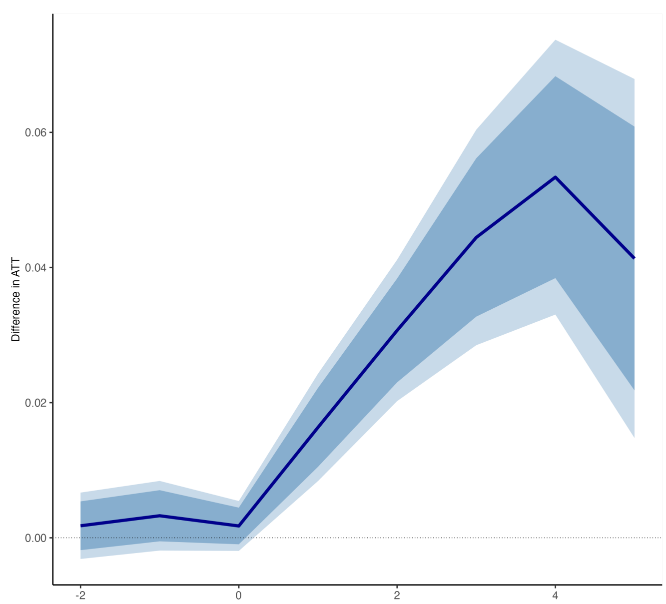
Note: This figure presents the average treatment effect on the treated girls in T1998 and T2000 by year and age group. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustering at the randomization level: locality.

Figure A13: Difference Between the ATT for the Cohort of Girls 13 y.o or older in 2001 and the Cohort of Girls 12 y.o or younger in 2001



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

Figure A14: 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 older than 13 in 2001 and girls younger than 12 in 2001. Standard errors were obtained through clustering at the randomization level: locality.

Table A18: Summary Statistics of Girls by Marriage Status: Married (M) VS Single (S)

	M	S	M(IPW)	S(IPW)	Difference(IPW)
Education Level in 2003	6.69	6.78	6.41	6.56	-0.15
	(2,326.00)	(8,595.00)			(-1.14)
Occupation: Unpaid housekeeper (week before survey, 2003)	0.44	0.19	0.45	0.15	0.30
	(833.00)	(8,658.00)			(9.53)
Worked for money (week before survey, 2003)	0.70	0.55	0.71	0.56	0.15
	(284.00)	(2,463.00)			(3.49)
Occupation: agriculture (week before survey, 2003)	0.09	0.08	0.10	0.10	0.00
	(202.00)	(1,689.00)			(0.01)

Note: This table shows the summary statistics by marriage status: married or single by 2003. The first two columns report the mean for each characteristic for married (M) and single (S), respectively, and the number of observations in parentheses. The third and fourth column report the re-weighted means, re-weighting each observation in the control group 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 marriage status indicator, re-weighting the control group as described.

Table A19: Summary Statistics of Married Girls by Age at Marriage: Child Brides (CM) VS Adult Brides (AM)

	CM	AM	CM(IPW)	AM(IPW)	Difference(IPW)
Education Level in 2003	6.44	7.05	6.24	6.62	-0.38
	(1,473.00)	(1,020.00)			(-1.71)
Occupation: Unpaid housekeeper (week before survey, 2003)	0.40	0.43	0.39	0.38	0.01
	(618.00)	(353.00)			(0.18)
Worked for money (week before survey, 2003)	0.69	0.68	0.70	0.70	-0.00
	(190.00)	(148.00)			(-0.03)
Occupation: agriculture (week before survey, 2003)	0.10	0.12	0.22	0.05	0.17
	(127.00)	(75.00)			(1.65)

Note: This table shows the summary statistics by age at marriage: child brides who married below 18 (CM) or adult brides who married at 18 or above (AM). The first two columns report the mean for each characteristic for child brides (CM) and adult brides (AM), respectively, and the number of observations in parentheses. The third and fourth column report the re-weighted means, re-weighting each observation in the control group 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 marriage status indicator, re-weighting the control group as described.

Table A20: Summary Statistics of Girls in 2003: By Marriage and Treatment Status

(a) Single

	T	C	C(IPW)	Difference(IPW)
Education Level in 2003	6.88	6.48	6.31	0.56
	(6,597.00)	(1,998.00)		(3.84)
Occupation: Unpaid housekeeper (week before survey, 2003)	0.18	0.22	0.13	0.05
	(6,499.00)	(2,159.00)		(3.99)
Worked for money (week before survey, 2003)	0.55	0.56	0.56	-0.01
	(1,705.00)	(758.00)		(-0.28)
Occupation: agriculture (week before survey, 2003)	0.07	0.09	0.11	-0.04
	(1,166.00)	(523.00)		(-1.15)

(b) Married

	T	C	C(IPW)	Difference(IPW)
Education Level in 2003	6.73	6.49	6.02	0.71
	(1,918.00)	(408.00)		(2.88)
Occupation: Unpaid housekeeper (week before survey, 2003)	0.36	0.65	0.54	-0.17
	(617.00)	(216.00)		(-2.78)
Worked for money (week before survey, 2003)	0.71	0.66	0.70	0.00
	(226.00)	(58.00)		(0.07)
Occupation: agriculture (week before survey, 2003)	0.08	0.14	0.12	-0.04
	(166.00)	(36.00)		(-0.52)

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. The first two columns report the mean for each characteristic for treated and control groups, respectively, and the number of observations in parentheses. 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.

Table A21: Summary Statistics of Married Girls by Age at Marriage and Treatment Status

(a) Married under 18y.o

	T	C	C(IPW)	Difference(IPW)
Education Level in 2003	6.51	6.11	5.91	0.61
	(1,192.00)	(281.00)		(2.56)
Occupation: Unpaid housekeeper (week before survey, 2003)	0.33	0.58	0.45	-0.12
	(445.00)	(173.00)		(-1.92)
Worked for money (week before survey, 2003)	0.71	0.62	0.70	0.01
	(151.00)	(39.00)		(0.12)
Occupation: agriculture (week before survey, 2003)	0.10	0.12	0.35	-0.25
	(103.00)	(24.00)		(-1.41)

(b) Married at 18 or older

	T	C	C(IPW)	Difference(IPW)
Education Level in 2003	7.04	7.12	6.20	0.84
	(833.00)	(187.00)		(2.29)
Occupation: Unpaid housekeeper (week before survey, 2003)	0.41	0.47	0.36	0.05
	(249.00)	(104.00)		(0.68)
Worked for money (week before survey, 2003)	0.69	0.66	0.71	-0.02
	(104.00)	(44.00)		(-0.19)
Occupation: agriculture (week before survey, 2003)	0.12	0.11	0.02	0.10
	(48.00)	(27.00)		(2.06)

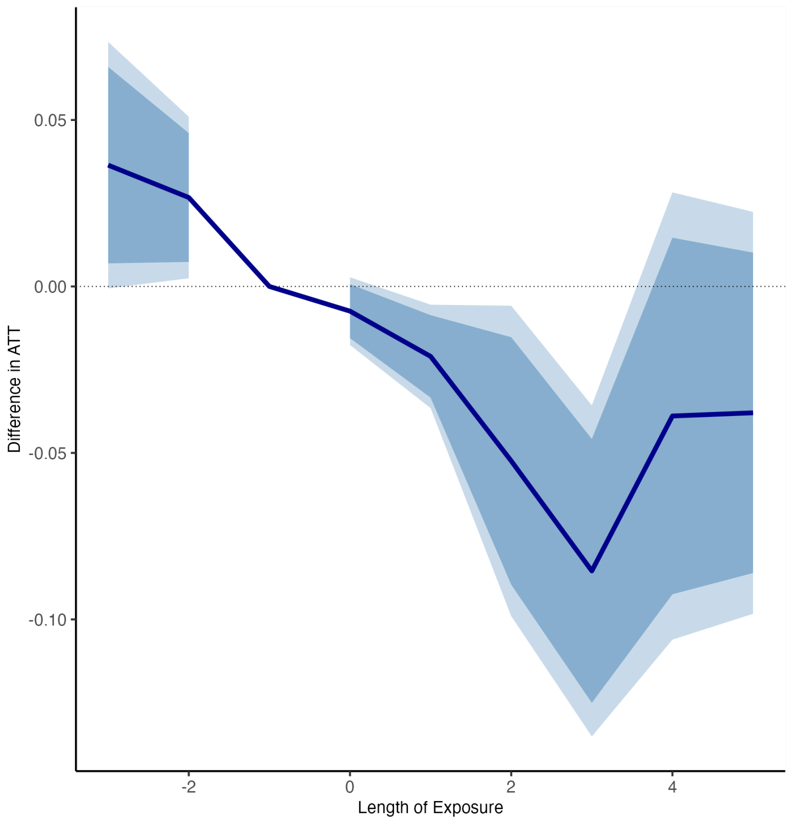
Note: This table shows the summary statistics by early marriage status and treatment group. Panel (a) restricts the analysis to girls who married under 18. Panel (b) to girls who married at 18 or older. The first two columns report the mean for each characteristic for treated and control groups, respectively, and the number of observations in parentheses. 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.

Table A22: Correlation Between Outcomes and Age at Marriage: Child Brides VS Adult Brides - ENDIREH 2003

	ChildBride	T-Stat
Education level	-0.84	-5.41
Monthly wage	-945.76	-3.48
Worked last week	-0.06	-2.77
Money to spend on herself	-0.04	-1.89
Financial dependence index	0.20	4.00
Socialization	0.05	0.86
Decision making power	0.27	0.95
Sexism index	0.31	4.01
Social benefits	0.14	8.79
Suicidal thought	0.05	2.40
Number of children	0.51	7.19
Couple lives in own house	-0.13	-5.41
Couple lives with husband's parents	0.15	6.19
Prenuptional Agreement	-0.04	-1.60
Partner's education level	-0.82	-4.92
Partner Works	-0.00	-0.08
Partner's monthly wage	-251.75	-2.37
Physical violence from the partner	0.14	3.25
Sexual violence from the partner	0.04	1.70
Physical violence in the household	0.02	1.22
Verbal violence in the household	0.02	0.89
Mother thinks about harming children	0.18	7.12
Mother harms children	0.21	8.14
Mother insults children	0.07	3.88
Partner's age	0.66	1.75

Note: This table shows the characteristics of child brides (married below 18) compared with adult brides (married at 18 or above). The column Child Bride presents the coefficient of a regression of each characteristics on a dummy variable that is equal to one if the woman married before turning 18 and 0 if married after 18, controlling by woman's age, asset possession and partner's age(except when partner's age is the outcome of interest). The column T-Stat present the t-statistic of the coefficient.

Figure A15: Difference Between the ATT for Villages with Returns to Education Above and Below the Median



Note: This figure presents the difference in the program's average treatment effect for villages with returns to education above the median and villages with returns to education below the median. Standard errors were obtained through clustering at the randomization level: locality.

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 affected 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 who are not in October 1998 reappeared in March 1999. Roughly, between 50 and 70% of those missing in a specific survey reappear in the consecutive one; therefore, I can often retrieve marriage information for each year. 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 cross-section, I estimate a lower bound for the aggregate effect for girls of 2p.p, statistically different from zero at 1%, CI=[0.016, 0.025].

Besides attrition, there are other inconsistencies across surveys. Namely, the age of individuals does not progress as expected, or their gender changes from female to male or vice-versa, which might indicate a mismatch in the IDs or misreporting 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 drop 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

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: This table presents the proportion of individuals, by treatment group, surveyed in 1997 and missing in subsequent surveys.

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 proportion of individuals, by treatment group, with missing outcome information in each survey.

values on marriage in those years in which the observation has an inconsistency, the estimator would only consider some of those observations that 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 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 proportion of individuals for whom age is not consistent across surveys, by treatment group.

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 in 1997. I did not recur to imputation of these missing values: 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 excluded those observations with education missing. I ran the main analysis with those individuals for whom I observe education information for baseline. When instead I exclude the variable from the econometrics models, and keep the observations, I obtain qualitatively the same results, but with a lower magnitude for the effect of the program on girls across the years: in 2003, girls were 3p.p more

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: This table presents the proportion of individuals for whom gender is not consistent across surveys, by treatment group.

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 if head or spouse is a housewife	0	0	0
Missing gender of household head	0	.000067	.0079

Note: This table presents the proportion of individuals for whom the main control variables are missing at baseline, by treatment group.

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 proportion of individuals for whom the main control variables are missing at baseline, by treatment group.

C Appendix: Model's Detailed Derivations

The value functions of this problem are the following:

$$\begin{aligned} V^N(\xi,h,q) &= U^N(\xi,h,q) + \beta \max\{U^N(\xi,h,q'), U^E(\xi,h,q'), U^{EM}(\xi,h,q'), U^M(\xi,h,q')\} \\ &= u(w(h)) + \beta \max\{u(w(h)), u(\omega)+\xi, u(\omega-c)+\xi+f(q'), u(w(h)-c)+f(q')\} \end{aligned}$$

$$\begin{aligned} V^E(\xi,h,q)) &= U^E(\xi,h,q) + \beta \max\{U^N(\xi,h',q'), U^E(\xi,h',q'), U^{EM}(\xi,h',q'), U^M(\xi,h',q')\} \\ &= u(\omega+p)+\xi + \beta \max\{u(w(h')), u(\omega)+\xi, u(\omega-c)+\xi+f(q'), u(w(h')-c)+f(q')\} \end{aligned}$$

$$\begin{aligned} V^{EM}(\xi,h,q) &= U^{EM}(\xi,h,q) + \beta \max\{U^{EM}(\xi,h',q), U^M(\xi,h',q)\} \\ &= u(\omega+p-c)+\xi+f(q) + \beta \max\{u(\omega-c)+\xi+f(q), u(w(h')-c)+f(q)\} \end{aligned}$$

$$\begin{aligned} V^M(\xi,h,q) &= U^M(\xi,h,q) + \beta \max\{U^{EM}(\xi,h,q), U^M(\xi,h,q)\} \\ &= u(w(h)-c)+f(q) + \beta \max\{u(\omega-c)+\xi+f(q), u(w(h)-c)+f(q)\} \end{aligned}$$

Define ξ^{**} as the school taste that, in the first period, makes agents indifferent between marrying and going to school (EM) and marrying and leaving school (M). Then,

$$\begin{aligned} &\xi^{**} : u(\omega+p-c)+\xi^{**}+f(q)+\beta\max\{u(w(h'))+f(q), u(\omega)+\xi^{**}+f(q)\} \\ &= u(w(h)-c)+f(q)+\beta\max\{u(w(h))+f(q), u(\omega)+\xi^{**}+f(q)\} \\ &\iff \xi^{**} = u(w(h)-c)-u(\omega+p-c)+\beta\max\{u(w(h))+f(q), u(\omega)+\xi^{**}+f(q)\} - \\ &\beta\max\{u(w(h'))+f(q), u(\omega)+\xi^{**}+f(q)\} \end{aligned}$$

$$\Rightarrow \frac{\partial \xi^{**}}{\partial p} = -u'(\omega+p-c) < 0$$

Let ξ^* be such that agents in the first period are indifferent between going to school single and working in the labor market single, $E = N$. Then,

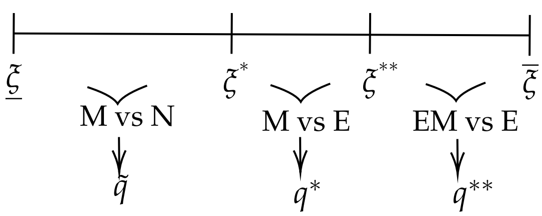
$$\xi^* : u(\omega + p) + \xi^* + \beta \mathbf{E}_{q'} \max\{u(\omega) + \xi^*, u(\omega - c) + \xi^* + f(q'), u(w(h')), u(w(h') - c) + f(q')\}$$
$$= u(w(h)) + \beta \mathbf{E}_{q'} \max\{u(\omega) + \xi^*, u(\omega - c) + \xi^* + f(q'), u(w(h)), u(w(h) - c) + f(q')\}$$

$$\begin{aligned} \iff \xi^* = {} & u(w(h)) - u(\omega + p) + \\ & \beta \mathbf{E}_{q'} \max\{u(\omega) + \xi^*, u(\omega - c) + \xi^* + f(q'), u(w(h)), u(w(h) - c) + f(q')\} - \\ & \beta \mathbf{E}_{q'} \max\{u(\omega) + \xi^*, u(\omega - c) + \xi^* + f(q'), u(w(h')), u(w(h') - c) + f(q')\} \end{aligned}$$

$$\Rightarrow \frac{\partial \xi^*}{\partial p} = -u'(w + p) < 0$$

Assuming $\xi^{**} > \xi^*$, then:

1. If $\xi > \xi^{**} > \xi^* \Rightarrow$ Agents choose between EM or E
2. If $\xi < \xi^* < \xi^{**} \Rightarrow$ Agents choose between M or N
3. If $\xi^* < \xi > \xi^{**} \Rightarrow$ Agents choose between M or E



Those individuals for whom $\xi > \xi^{**} > \xi^* \Rightarrow$ choose between EM and E. That decision will depend on the potential partner's quality. Namely, define q^{**} as the reservation partner quality to make individuals indifferent between EM and E. Note that q^{**} exists unique since V^{EM} is strictly increasing in q while V^E is a constant with respect to q.

$$\exists! q^{**} : V^{EM} = V^E \iff$$

$$u(\omega + p - c) + \xi + f(q^{**}) + \beta \max\{u(w(h)) + f(q^{**}), u(\omega) + \xi + f(q^{**})\}$$

$$= u(\omega + p) + \xi + \beta \mathbf{E}_{q'} \max\{u(\omega) + \xi, u(\omega - c) + \xi + f(q'), u(w(h)), u(w(h) - c) + f(q')\}$$

$$\iff f(q^{**}) = \frac{1}{1+\beta}[u(\omega + p) - u(\omega + p - c) + \beta \mathbf{E}_{q'}(\max\{N(h'), E(h'), EM(h', q'), M(h', q')\}$$

$$- \max\{N(h'), E(h'))\})]$$

Since $\xi > \xi^{**} > \xi^*$, we know than E is preferred to N, thus:

$$\iff f(q^{**}) = \frac{1}{1+\beta}[u(\omega + p) - u(\omega + p - c) - \beta(u(\omega) + \xi)$$
$$+ \beta \mathbf{E}_{q'} \max\{EM(h', q'), M(h', q'), N(h'), E(h')\}]$$

Note that $\dfrac{\partial q^{**}}{\partial p}$ is driven by the income effect induced by the program on marriage.

$$\frac{\partial f(q^{**})}{\partial p} = \frac{1}{1+\beta}[u'(\omega + p) - u'(\omega + p - c)] < 0 \quad \text{,since} \quad u'' < 0$$

$$\text{Because } f \text{ is strictly increasing in } q \Rightarrow \quad \frac{\partial q^{**}}{\partial p} < 0$$

As the program's generosity increases, the minimum partner quality to be indifferent between being single in school and married in school decreases.

If agents' taste for school is such that $\xi < \xi^* < \xi^{**}$, then they choose between M and N.

Define $\tilde{q}$ as the reservation quality to make individuals indifferent between M and N:

$$
\begin{aligned}
&\exists! \tilde{q} : V^M = V^N \iff \\
&u(w(h) - c) + f(\tilde{q}) + \beta \max\{u(w(h)) + f(\tilde{q}), u(\omega) + \xi + f(\tilde{q})\} \\
&= u(w(h)) + \beta \mathbf{E}_{q'} \max\{u(w(h) - c) + f(q'), u(\omega - c) + \xi + f(q'), u(\omega) + \xi, u(w(h))\} \\
&\iff f(\tilde{q}) = \frac{1}{1+\beta}[u(w(h)) - u(w(h) - c) - \beta \max\{N(h), E(h)\} \\
&+ \beta \mathbf{E}_{q'} \max\{N(h), M(h, q'), E(h), EM(h, q')\}]
\end{aligned}
$$

Since $\xi < \xi^* < \xi^{**}$, we know than N is preferred to E, thus:

$$
\iff f(\tilde{q}) = \frac{1}{1+\beta}[(1-\beta)u(w(h)) - u(w(h) - c) + \beta \mathbf{E}_{q'} \max\{N(h), E(h), EM(h, q'), M(h, q')\}]
$$

The program does not change the reservation partner quality for those individuals who are not going to school and are choosing between marrying or staying single.

$$
\Rightarrow \frac{\partial f(\tilde{q})}{\partial p} = 0 \Rightarrow \frac{\partial \tilde{q}}{\partial p} = 0
$$

Finally, if an individual's school taste is such that $\xi^* < \xi < \xi^{**}$, then they will choose between M and E. I show that there exists a unique q^* that makes individuals indifferent between marrying and leaving school and staying in school single.

$$
\begin{aligned}
&\exists! q^* : V^M = V^E \\
&\iff u(w(h) - c) + f(q^*) + \beta \max\{u(w(h)) + f(q^*), u(\omega) + \xi + f(q^*)\} \\
&= u(\omega + p) + \xi + \beta \mathbf{E}_{q'} \max\{u(\omega) + \xi, u(\omega - c) + \xi + f(q'), u(w(h) - c) + f(q'), u(w(h))\} \\
&\iff f(q^*) = \frac{1}{1+\beta}[u(\omega + p) - u(w(h) - c) - \beta \max\{N(h)), E(h)\} \\
&+ \beta \mathbf{E}_{q'} \max\{E(h'), M(h', q'), EM(h', q'), N(h')\}]
\end{aligned}
$$

Since $\xi^* < \xi < \xi^{**}$, then E is preferred to N, thus:

$$\iff f(q^*) = \frac{1}{1+\beta}[u(\omega+p) - u(w(h)-c) - \beta(u(\omega)+\xi) + \xi$$
$$+ \beta \mathbf{E}_{q'} \max\{E(h'), M(h',q'), EM(h',q'), N(h')\}]$$

Increasing the program's generosity for those individuals who have $\xi^* < \xi < \xi^{**}$ and are undecided between being single in school and getting married and drop out of school, leads to increases in the reservation partner's quality:

$$\Rightarrow \frac{\partial f(q^*)}{\partial p} = \frac{1}{1+\beta}[u'(\omega+p)] > 0 \Rightarrow \frac{\partial q^*}{\partial p} > 0$$

I can also show that q^* is the largest reservation partner quality:. Assuming $\omega < w(h) < \omega + p$ and consequently $\omega - c < w(h) - c < \omega + p - c$. Recall

$$f(q^{**}) = \frac{1}{1+\beta}[u(\omega+p) - u(\omega+p-c) - \beta(u(\omega)+\xi) + \beta \mathbf{E}_{q'} \max\{N(h'), E(h'), EM(h',q'), M(h',q')\}$$
$$f(\tilde{q}) = \frac{1}{1+\beta}[(1-\beta)u(w(h)) - u(w(h)-c) + \beta \mathbf{E}_{q'} \max\{N(h), E(h), EM(h,q'), M(h,q')\}]$$
$$f(q^*) = \frac{1}{1+\beta}[u(\omega+p) - u(w(h)-c) - \beta(u(\omega)+\xi) + \xi + \beta \mathbf{E}_{q'} \max\{N(h'), E(h'), EM(h',q'), M(h',q')\}]$$

Due to the previous assumption, $u(w(h)-c) - u(\omega+p-c) < 0$ and $u(\omega+p) - u(w(h)) > 0$. Trivially, $\xi > 0$ and $(1-\beta)\xi > 0$. Finally, because all payoff are strictly increasing in human capital h, then $max\{N(h'), E(h'), EM(h',q'), M(h',q')\} > \max\{N(h'), E(h'), EM(h,q'), M(h,q')\}$. It follows that:

$$f(q^{**}) - f(q^*) = u(w(h)-c) - u(\omega+p-c) - \xi < 0 \Rightarrow f(q^* *) < f(q^*) \Rightarrow q^{**} < q^* \quad \text{and}$$
$$f(q^*) - f(\tilde{q}) = u(\omega+p) - u(w(h)) + (1-\beta)\xi + \beta \max\{N(h'), E(h'), EM(h',q'), M(h',q')\}$$
$$- \beta \max\{N(h'), E(h'), EM(h,q'), M(h,q')\} > 0 \Rightarrow f(q^*) - f(\tilde{q}) > 0 \Rightarrow q^* > \tilde{q}$$

Therefore, $q^{**} < q^*$ and $\tilde{q} < q^*$.

To conclude, define $\mathcal{E}$ and $\mathcal{M}$ as the mass of individuals who are in school and married, respectively.

$$\mathcal{E} = 1 - J(\xi^{**}) + (J(\xi^{**}) - J(\xi^{*}))G(q^{*})$$
$$\mathcal{M} = (1 - J(\xi^{**}))(1 - G(q^{**})) + J(\xi^{*})(1 - G(\tilde{q})) + J((\xi^{**}) - J(\xi^{*}))(1 - G(q^{*}))$$

We are interested in showing how the program changes these two masses:

$$\frac{\partial \mathcal{E}}{\partial p} = j(\xi^{**})\frac{\partial \xi^{**}}{\partial p}(G(q^{*}) - 1) - j(\xi^{*})\frac{\partial \xi^{*}}{\partial p}G(q^{*}) + (J(\xi^{**}) - J(\xi^{*}))g(q^{*})\frac{\partial q^{*}}{\partial p} > 0$$

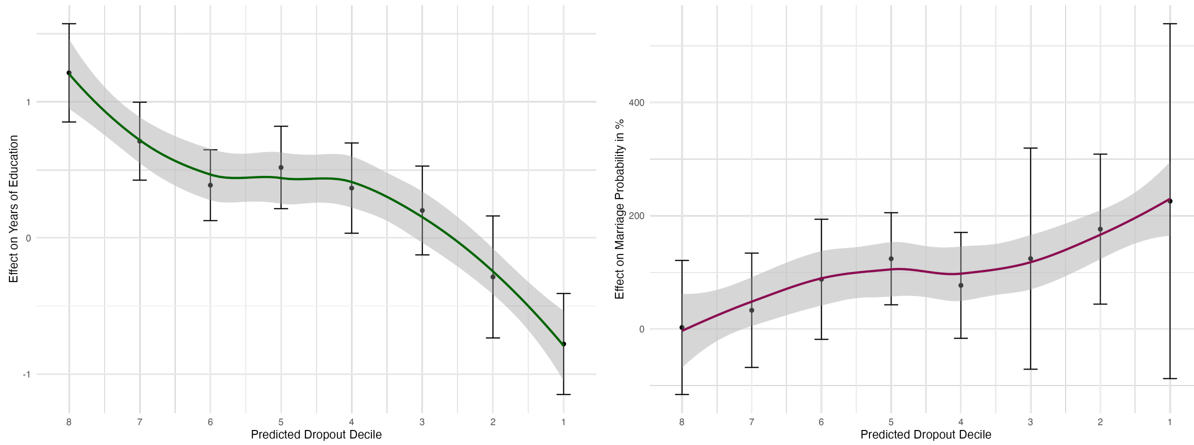
The first term is positive because: $j(\xi)$ is trivially positive since it is the p.d.f of the ξ's distribution, $\partial \xi^{**}/\partial p < 0$, as shown before, and $(G(q^{*}) - 1) < 0$ because $G(q)$ is the c.d.f of q's distribution. The second term is also positive because $j(\xi^{*})$ and $G(q^{*})$ are positive and $\partial \xi^{*}/\partial p < 0$ as shown before. Finally, the last term is also positive since I assumed $\xi^{**} > \xi^{*}$, thus $J(\xi^{**}) - J(\xi^{*}) > 0$, $g(q^{*}) > 0$ and as shown before $\partial q^{*}/\partial p > 0$.

Regarding the effect of the program on marriage, we have three composition effects and an income effect:

$$\frac{\partial \mathcal{M}}{\partial p} = \overbrace{j(\xi^{**})\frac{\partial \xi^{**}}{\partial p}[G(q^{**}) - G(q^{*})]}^{\text{composition effect (a)}>0} + \overbrace{j(\xi^{*})\frac{\partial \xi^{*}}{\partial p}[G(q^{*}) - G(\tilde{q})]}^{\text{composition effect (b)}<0} -$$
$$\underbrace{-g(q^{**})\frac{\partial q^{**}}{\partial p}[1 - J(\xi^{**})]}_{\text{income effect }>0}\underbrace{-g(q^{*})\frac{\partial q^{*}}{\partial p}[J(\xi^{**}) - J(\xi^{*})]}_{\text{composition effect (c)}<0}$$

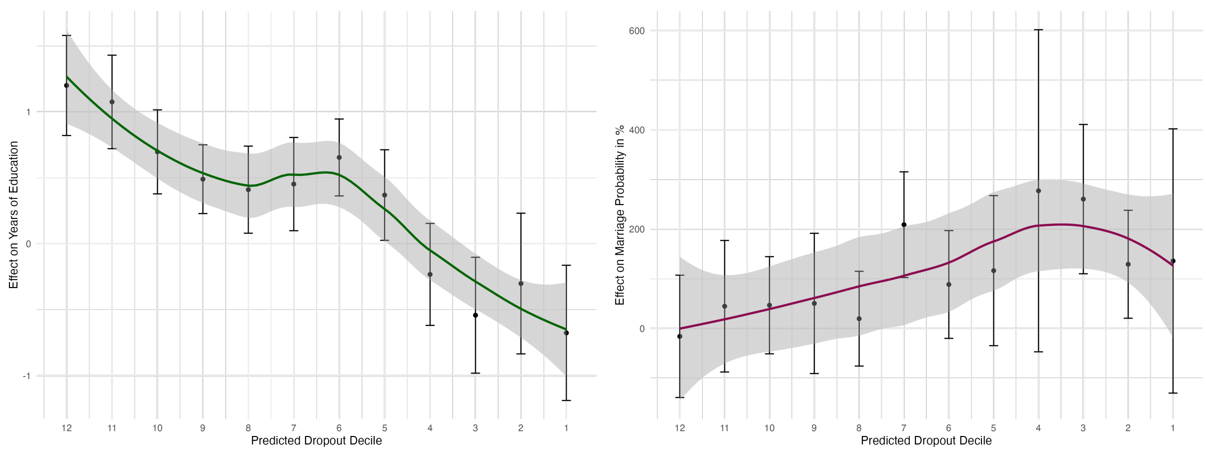
First, composition effect (a) is positive because $\partial \xi^{**}/\partial p < 0$, as shown before, and $G(q^{**}) - G(q^{*}) < 0$ since $q^{*} > q^{**}$. Second, composition effect (b) is negative. This is due to $\partial \xi^{*}/\partial p < 0$, as shown before, and $G(q^{*}) - G(\tilde{q}) > 0$ since $q^{*} > \tilde{q}$. Third, the income effect is positive, since $g(q^{**}) > 0$, $[1 - J(\xi^{**})] > 0$ and $\partial q^{**}/\partial p < 0$, as shown before. Finally, composition effect (c) is negative. Once again, $g(q^{*}) > 0$, $[J(\xi^{**}) - J(\xi^{*})] > 0$ and $\partial q^{*}/\partial p > 0$.

Figure A16: Effect of Progresa/Oportunidades on the Probability of Marriage by School Taste



Note: This figure presents the average treatment effect on the treated on marriage and education by predicted dropout probability (in 8 quantiles).

Figure A17: Effect of Progresa/Oportunidades on the Probability of Marriage by School Taste



Note: This figure presents the average treatment effect on the treated on marriage and education by predicted dropout probability (in 12 quantiles).