

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

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This Version: 11th August 2025

## 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. Using the program's random assignment and staggered implementation, I find that the program doubled the marriage rates for girls without impacting boys. I find evidence that the program's income effect was a driver for the increase in child marriage.

JEL: I25, J12, J13, I21, I38

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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.<sup>1</sup> This practice disproportionately affects girls and exposes them to increased risks of early childbearing, violence, abuse, and limited autonomy.<sup>2</sup> 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 how these programs affect early marriage decisions in marriage markets with no arranged marriages or marriage payments. Parental involvement in spouse selection is a key determinant of marriage outcomes, especially when parental and child preferences diverge (Vogl, 2013; Banerjee et al., 2013). Additionally, the presence and type of marriage transfers affect how income shocks influence families' marriage decisions for girls (Corno et al., 2020). Given the important role of these institutional features, it is crucial to understand how income transfers impact child marriage in settings where parental interference and marriage payments are absent.

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.

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<sup>1</sup>See Thomson (2003) and Sperling and Winthrop (2015).

<sup>2</sup>On education and labor market, see Adebawale 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).

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. There is vast evidence showing that Progresa/Oportunidades increased significantly children's years of education (Behrman et al., 2005, 2009; Dubois et al., 2012), and, most recently, that its rollback led to decreases in schooling enrollment (Marquez-Padilla et al., 2025). Therefore, we could expect Progresa to decrease early marriage through the documented increases of education. A particularity of rural Mexico, however, is the co-existence between schooling and marriage. Rivero and Palma (2017) reports that 17.1% of formally married girls attend school. In this paper's sample, I observe that, across all years, 20% of married girls attend school. Additionally, Attanasio et al. (2012) find little to no returns from education in rural Mexican villages' labor markets. It might be the case, then, that if returns to education are low in the labor market, there is no change in the opportunity cost of marriage. These two observations suggest that increases in education might have a limited effect on 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 program's income effect. Initially introduced in 1998 in randomly selected villages, Progresa was renamed Oportunidades in 2000 when the initially 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 available comprehensive panel data allow for a 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. Five years after the program started, beneficiaries were 3.4p.p. more likely to be married than the control group. The program more than doubles the 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 6p.p. in 2023. For reference, in that year, 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 Progres/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.

The program's design does not allow for perfectly disentangling income and education effects. However, to understand the mechanisms behind this unexpected result, I conduct two exercises to isolate the causal effect of income on marriage. I examine two distinct populations that were likely influenced only by the program's income component. First, I analyze children and young adults who were not direct beneficiaries but lived in households eligible for the program. Second, I explore the program's heterogeneous effects on children based on their likelihood of dropping out, focusing on girls whose schooling decisions were unlikely to be influenced by the program - the 'always takers', or those who would have gone to school even in the absence of the program - thereby isolating its income effect. In both cases, the program increased the probability of marriage, providing evidence that the program's income effect contributes to the increase in child marriage observed in the paper's main results.

The findings in this paper contribute to a vast literature on Progres/Oportunidades' impact on their beneficiaries. Most of the literature documents positive effects of the program

on education, labor market participation, network formation, consumption and poverty, both in the short and medium-run (summarized by Parker and Todd, 2017) and in the long-run (Araujo and Macours, 2021; Parker and Vogl, 2023). I contribute to this literature by providing causal evidence of the program's effect on child marriage, showing that it led to increased early marriage among girls. This finding is particularly important for any comprehensive cost-benefit analysis of the program, as it highlights an unintended and previously undocumented consequence alongside its well-established positive impacts.

The findings in this paper provide novel insights into our understanding of the causes and determinants of early marriage. 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. 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. 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 understanding the impact of cash transfers. Corno et al. (2020) 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 the effects of income on early marriage in settings without marriage payments. These are consistent with Bobonis (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 children.

This paper also contributes to the literature on the effect of educational programs on early marriage. My findings are consistent with and complement those of Behrman et al. (2005) and Araujo and Macours (2021). The authors compare the two treatment groups (that received the program in 1998 and 2000, respectively) and document no significant effect of the program on marriage. By adding a pure control group to the analysis (which did not receive the program until the end of my analysis), I show that this null result conceals the fact that both groups increased their marriage rates compared to the control group. My results are also consistent with Vogl (2013), who use administrative data to study the long-run impact of the program. They find that the marriage rates of individuals aged between 22 and 31 do not differ due to exposure to the program during childhood. Although a direct comparison between the sets of results should be done cautiously - due to the differences in samples and inclusion of marriages after 18 - my results do not conflict with theirs, if we believe that the program changed the intensive margin (when to marry) rather than the extensive margin (marry or not). An income effect can explain this. Therefore, I show evidence that a program like Progres/Oportunidades can increase marriage if the income effect offsets the education effect. This can be the case, for example, if increases in education do not increase the opportunity cost of marriage. Consistent with this prediction, Gulemetova-Swan (2009) shows that Oportunidades' urban beneficiaries, where returns from education are positive, married 1-4 months later than non-beneficiaries. However, a caveat of this analysis is that, contrary to rural areas, where enrollment was close to universal within the randomly selected localities, in urban areas, there was a large extent of household self-selection into the program, hindering the ability to estimate a causal effect.

The rest of this article is organized as follows. Section 2 presents the Mexican context, and Section 3 introduces Progres/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, including a discussion on the relationship between these results and the literature. In Section 6, I provide evidence of the mechanisms in place. Finally, Section 7 discusses the implications of these findings in terms of welfare and places them in the broad literature on Progres.

## 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 21% (UN Women). In 1993, 20.6% of women aged 20-24 had married before 18, 21.2% in 1998, 22.9% in 2008 and 22.1% in 2013. The latest data, from 2018, points to a child marriage incidence of 20.7%, 17.7% in urban areas and 31.2% in rural areas.<sup>3</sup> Fertility, in Mexico, has been consistently decreasing over the past decades.

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).<sup>4</sup>

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<sup>3</sup>Data for child marriage among boys is rare. But in Central and Latin America child marriage rates for boys are estimated to be around 8%. Low rates in Mexico can be expected given two observations: (i) women in general marry older men; and (ii) 'Most girls age 12-17 who are in a union are at least six years younger than their partner(...)', according to Rivero and Palma (2017).

<sup>4</sup>According to the survey 'Lo que dicen los pobres', run by the Secretary for Social Development in Mexico

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

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 Progreso/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 Progreso/Oportunidades and Data**

In 1998, a conditional cash transfer program, Progreso/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

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(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.



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.

**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 Progres a 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.

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 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).

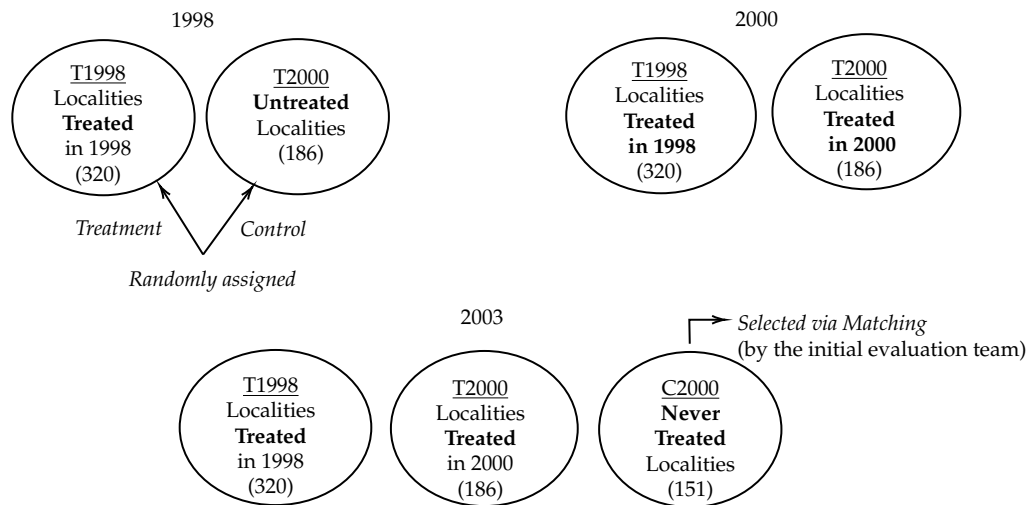
The program was first implemented in 320 randomly chosen rural localities (now referred to as T1998). A further 186 localities were randomly assigned to the control group. All these localities were highly deprived, with access to elementary school, middle school, and a health clinic (Abúndez et al.,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

<sup>5</sup>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.

pass grades.<sup>6</sup> After the expansion of the program, in order to evaluate its long-term effects, in 2003 the evaluation team also surveyed a new set of villages that had not received the program until then. These villages were selected matching the treatment villages on aggregated locality characteristics obtained from the 1990 and 1995 census. These include housing and 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. These 151 villages are also from the same states as the original 506 communities (except for one, for which the neighboring state was used). 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 T1998, T2000 and C2000. The survey asked the control group, C2000, current and retrospective questions, referring to 1997, 2000, 2001, and 2002.<sup>7</sup>

<sup>6</sup>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.

<sup>7</sup>I use information from the survey conducted in 2007 to complete missing information on year and age at marriage.

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, cohabiting, 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.<sup>8</sup>

My population of interest is all children between 6 and 16 years old in 1997, the baseline year.<sup>9</sup> 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 - albeit

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<sup>8</sup>In ENCEL2003 and 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. This data in 2003 allows me to check consistencies in the civil status reporting. The data in 2007 I use only for the descriptive statistics and to complete marriage status in case of missing information from the other surveys.

<sup>9</sup>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.

statistically different -, starting to diverge after 1999. Note that for the empirical strategy used in this paper, and described in the next section, child marriage rates do not have to be the same at baseline, since I will rely on the parallel trends assumption. 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.

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

<b>(a) All</b>							
	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>(b) Under 18 years old</b>							
	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

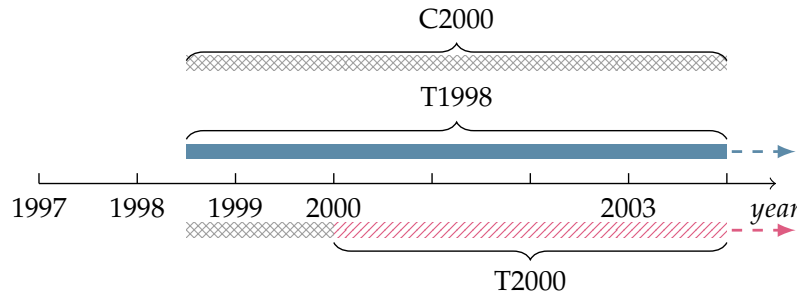
Note: This table presents the proportion of married individuals by group and year. Panel (a) refers to all individuals between 6 and 16 years old in 1997, and Panel (b) refers to the same individuals until they turn 18. T1998 are those individuals who started receiving the program in 1998. T2000 is the 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 non-randomly selected in 2003, 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. Therefore, I will use a differences-in-differences estimator that accounts for multiple periods and staggered treatment allocation. In particular, 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.<sup>10</sup>

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 in this setting. The treatment allocation was done at locality level and the empirical analysis is at the individual level, thus the randomization does not guarantee balancing. Additionally, the pure control group (C2000) is not experimental. On average, unobserved characteristics of villages in T1998 and T2000 are uncorrelated with treatment allocation due to the random assignment to the program across these localities. However, the selection of villages in C2000 assumes that, given the observed characteristics, the treatment allocation was as good 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

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<sup>10</sup>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).

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.<sup>11</sup> 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.

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

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

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 con-

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<sup>11</sup>In practice, I exclude from my total sample 15 observations that have an estimated propensity score higher than 0.999. See Figures A3 to A8 for the distribution of the propensity scores. The main results of this paper are robust to restricting the analysis to observations with a propensity score less than 0.8 and between 0.2 and 0.8.

ditional 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’,  $m_{g,t}^{ny}(X) = \mathbb{E} [Y_t - Y_{g-1} \mid X, D_t = 0, G_g = 0]$ .

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** The pure control group, C2000, was selected by the program’s evaluation team using a matching model to select 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. Using a differences-in-differences design allows us to relax the assumption that the groups were identical at baseline. It requires only to assume parallel trends, as explained before. Nevertheless, to increase the likelihood that that is the case, I use a doubly robust estimator, which requires specifying the propensity score model and the outcome regression models. I use two sets of characteristics: (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 final specification of both outcome regression and propensity score model 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.<sup>12</sup> 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. This is evidence that the propensity score model is well specified, which supports the assumption that the used doubly-robust differences-in-differences estimator is identifying the ATT.<sup>13</sup>

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 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.<sup>14</sup>

One might worry that marriage status may be subject to misreporting in the ENCAESH and ENCEL data, particularly among adolescents, due to social stigma or misremembering civil

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<sup>12</sup>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. My specification includes: **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; **Marriage predictors:** gender, age, education level at baseline, indigenous background, and household head and spouse characteristics.

<sup>13</sup>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.2). Despite these differences, **I can replicate the results from the paper mentioned above regarding the program's effect on educational achievement.**

<sup>14</sup>Looking at control municipalities in the 1990 and 1995 census, I observe that the summary statistics of the baseline characteristics are very close to C2000, for similar populations (for example, children between 6 and 16 - as used in this paper - and children between 0 and 9 in 1990 - those who would be between 6 and 16 in 1997). That the mean of the variables of interest are the same as in the census is reassuring regarding the absence of recall bias. A caveat of this analysis is, however, that the program was implemented at the locality level and the census data is only at the municipal level. Therefore, the municipalities that are defined as the control group for this exercise are those in which no locality was treated, which reduces the sample size and the representativeness of the analysis.

status. This misreporting can also differ by treatment status. To assess the reliability of marriage reporting, I conducted a series of empirical exercises: comparing the self-reported data I use in the analysis of this paper, the Census data, and administrative records, examining potential inconsistencies in reports of marriage status, and comparing rates of child marriage between the three datasets. The results, presented in detail in Section B.1, suggest that marriage reporting is reliable, with similar proportions of individuals reporting marriage status across datasets and no significant correlation between treatment intensity and the probability of mis-reporting child marriage. Overall, I conclude that marriage misreporting is unlikely to be a significant problem in my analysis. Note that early marriage was a common and widely accepted practice in the late 1990s and early 2000s, particularly in rural areas, and therefore unlikely to be stigmatized. Marriage, either it in the form of formal or informal union, is also an extremely important decision girls make, thus unlikely to forget.

An in-depth analysis of attrition and missing data is available in Appendix B.2. 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 ( 0.0066 ) [ 0.0093 , 0.0351 ]	0.0198 ( 0.0074 ) [ 0.0037 , 0.0358 ]	0.0266 ( 0.0087 ) [ 0.0079 , 0.0452 ]
Control Mean		0.018	0.019
Effect in %		107.90	139.43
N		25643	

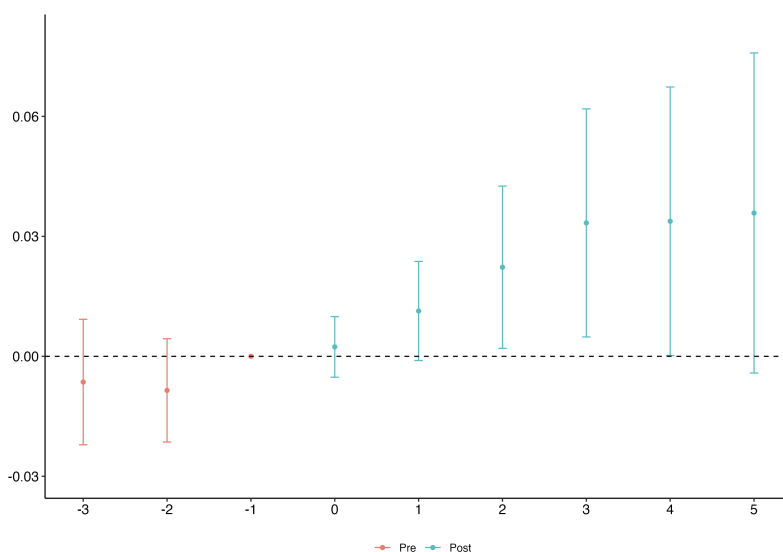
Note: This table presents the aggregated average treatment effect on the treated. 'All' represents the estimate using treatment groups T1998 and T2000. The second and third columns separately present the average treatment effect over time for treatment groups T1998 (who first received the treatment in 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 A15 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: Progres/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 observe a positive trend in the estimated coefficients one year after the program started for T1998. Despite noisy, the point estimates 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 A10 and Table A16 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 Progres/Oportunidades) were important in explaining the program's positive effect on marriage.

## 5.2 Heterogeneous Effects

### 5.2.1 Gender

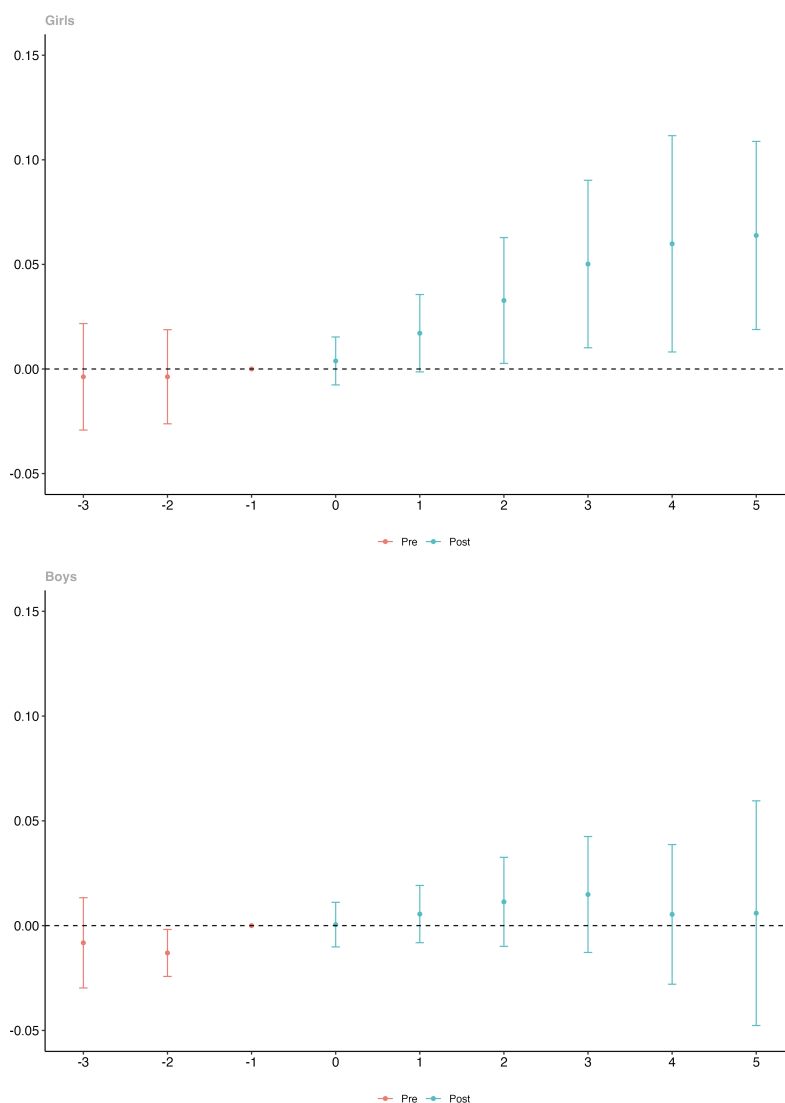
Around the world, early and child marriage are a more prevalent phenomenon among girls than boys. Also, the socioeconomic consequences associated with early marriage are known to be more damaging for females than males due to early childbearing, exposure to violence, and a higher likelihood of formal 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]). This effect is 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. However, it is after 2001 that they start being meaningful (see Figure A11 and Table A18 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 A12 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 group, see Tables A19 and A20, in the Appendix.

Those girls for whom I have information on their partners' ages were, on average, 3.5 years

**Figure 4: Progresas/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.

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

Girls			
	All	T1998	T2000
ATT	0.0356 ( 0.0097 ) [ 0.0166 , 0.0545 ]	0.0318 ( 0.0107 ) [ 0.0084 , 0.0551 ]	0.0423 ( 0.014 ) [ 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 ( 0.0081 ) [ -0.0082 , 0.0237 ]	0.0065 ( 0.0076 ) [ -0.0083 , 0.0214 ]	0.0101 ( 0.0144 ) [ -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.

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.<sup>15</sup>

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.

<sup>15</sup>Results available upon request. Just like Bobonis (2011), I also find a positive effect on older women who were single at baseline.



### 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.<sup>16</sup> 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 A13 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 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.

## 5.3 Discussion

When I test the effect of Progresa using only the experimental component of the program I do not find any impact on child marriage. As presented in Tables A12, A13, and A14 and Figure A9, having received 1.5 more years of program did not change marriage probability for either

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<sup>16</sup>Analyzing just boys, results suggest positive but small, effects at younger ages and no significant effect for the last age group.

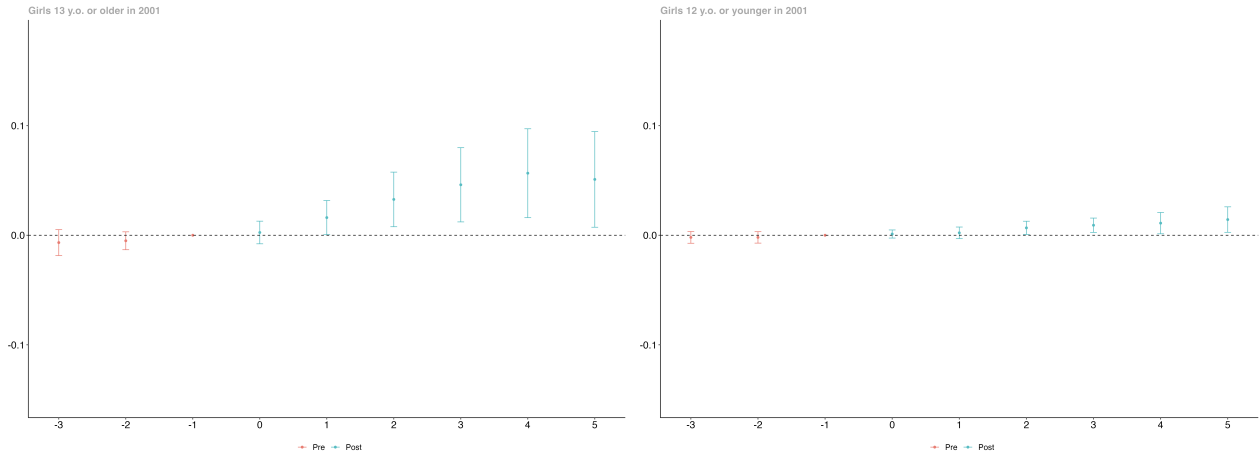
girls or boys. The point estimates for the effect of the program in the 3 first years of (1998-2000) are positive, but economically meaningless and not statistically significant from zero. In the subsequent years, the point estimates are negative, but also small and not statistically significant. These findings are consistent and complement the ones by Behrman et al. (2005) and Araujo and Macours (2021). The authors use the experimental component of the program and estimate the effect of receiving 1.5 additional years of the program by comparing the outcomes of the two treatment groups. Behrman et al. (2005) look at marriage rates in 2003 of individuals aged between 9 and 15 at baseline (1997). They find no statistically significant difference between marriage rates of T1998 and T2000. Their results are not directly comparable to the ones presented in this paper since they do not focus on child marriage (they include marriages after 18). Araujo and Macours (2021) also compares T1998 and T2000 in 2017, 20 years after the start of the program. Similarly, they find no significant effect of the program on marriage probability. They find a half-a-year delay in marriage age, but the delay is not statistically significant for women. Like Behrman et al. (2005), they do not restrict this analysis to marriages below 18. Finally, Parker and Vogl (2023) use 10% of census data and a differences-in-differences strategy to compare individuals who were fully exposed to the program (younger than 14 in 1997) to those who were 15 and older and for whom the program did not affect schooling. They interact the age cohort dummies with a measure of the program's incidence during the first roll-out phase. They find that the program did not affect the likelihood of marriage. By 2010 - when individuals are between 22 and 31 - treated individuals are as likely to be married as control individuals.

The caveat with Progresas's experimental analysis is that we can only test the effect of the program for 1.5 years. From 2000 onward, the comparison is always regarding the length of exposure to the program. This does not allow us to test the effect of receiving versus not receiving the program after this year, nor take into account the changes that went through in that same year. The differences-in-differences analysis overcomes these drawbacks since it allows us to analyze the effect of the program for up to 6 years, by calendar year and exposure length. Using this strategy I find both treatment groups increased their marriage rates from 2000 onward, whereas the pure control group did not (A11). I also find that the program started signific-

antly affecting marriage decisions after the year 2000, which might be explained by the changes implemented by the program, particularly the extension of benefits to secondary school.

Students are supposed to reach secondary school at around 14-15 years old if they do not repeat any year. Since most early marriages in Mexico happen between 15 and 17 years old, it might be the case that receiving the benefit at this age facilitates marriage decisions for girls in treated villages. In an exercise similar to the one executed by Parker and Vogl (2023), I compare the program's effect across two groups: (i) all the girls older than 13 in 2001, who were likely to be in high school between then and 2003 - highly exposed, and (ii) all girls younger than 12 years old in 2001, who were unlikely to be in high school during this period - not exposed. Figure 5 shows that if we look at all girls who were likely to be in high school in the analysis period, those who could receive the benefit during high school (treatment groups) were more likely to marry than those who could have not received the benefit (control group). If, in turn, we look at the effect of the program on younger girls, we observe positive and statistically significant effects, but in a much smaller magnitude. The effect is significantly larger for the older cohort than the younger one (see Figure A14 in the Appendix). Note that these results do not conflict with Parker and Vogl (2023) if we believe the program did not change the extensive margin on whether to marry, but it affected the intensive margin and anticipated the decision for those individuals who would marry in the following years. Meaning, that those individuals who were already likely to marry had their decision facilitated and anticipated by the program. An income effect could explain this, as I discuss in the next section of the paper.

**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.

## 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 Progreso/Oportunidades' effect on early marriage between villages with returns to education above the median and villages below the median.<sup>17</sup> 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 marriage. 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 marriage market, or a way for girls to meet their partners. In fact, Attanasio and Kaufmann (2017) find that girls believe college has positive returns in both labor and marriage markets. Further investigation is necessary to determine whether the returns materialize and whether this replicates for lower levels of education.

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

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<sup>17</sup>I estimate a Mincerian regression using municipal-level data from the 1995 census. Out of 658 localities of the Progreso/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.

increase boys' and girls' desirability on the marriage market, change their network, and/or it may facilitate marriage by making marriage-related expenditures - such as setting up a new household, having more people to support within a household, or losing workforce - more affordable.<sup>18</sup>

To investigate further the hypothesis that the income effect of the program is driving increases in marriage I perform two empirical exercises. In both exercises, the aim is to restrict the analysis to a population that is only affected by income and not schooling. I exploit two different populations for this purpose.

## 6.1 Income Effect on Marriage (I)

In this first exercise, I test whether positive income transfers causally change marriage decisions by exploiting 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.<sup>19</sup> The sample consists of 3,115 individuals, 46% of them female and, on average, 15.51 years old at baseline. In this exercise, I consider marriage both before and after 18 years old, otherwise I would not have observations to run the exercise. Since the goal is to test whether marriage is a normal good in Mexico, I believe it is enough to understand if a positive income shock leads to an increase in marriage, in general, 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

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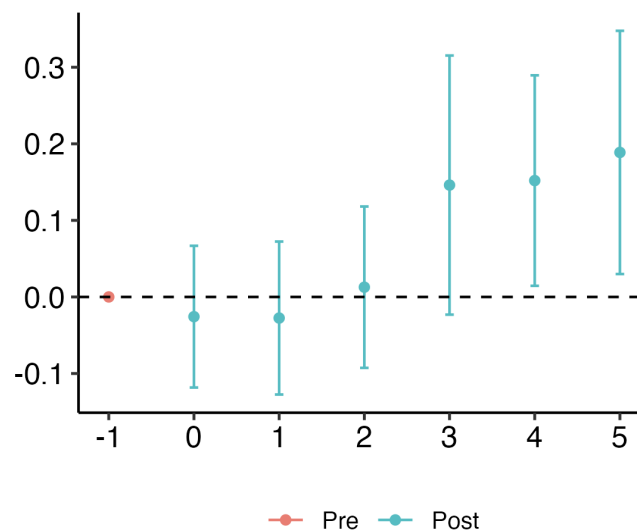
<sup>18</sup>In a Working Paper, Berge et al. (2022) show that an entrepreneurship program increased young women's fertility, in Tanzania. They explain this through an income effect, for which they provide suggestive evidence looking at the association between income and the probability of being a teen mom. Interestingly, they find this positive correlation only for self-employed women. They argue, in line with the literature, that self-employment is more compatible with motherhood due to more flexible labor arrangements.

<sup>19</sup>Given changes in eligibility rules in 2000, I compare T1998 and C2000 to avoid eligibility misclassification.

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 length 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 individuals to marry.

## 6.2 Income Effect on Marriage (II)

In this second exercise, I exploit a different population. The goal is to estimate the effect of the program on girls for whom the program did not change school choices. These would be the equivalent to the always-takers - girls who would reach the level of education they reached independently of the program. The assumption is that the program only has an impact on their decisions through the income effect since their education choices were not affected.

To do this, 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 is assumed 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.<sup>20</sup> Then, I divide the sample by predicted dropout decile and estimate the program's effect on child marriage by dropout decile, using the same empirical strategy as in the primary analysis. Note that decile 1 corresponds to those girls whose dropout prediction is very low - the always-takers. These would be the ones affected only by the income effect. Similarly, girls in decile 10 are the ones whose dropout prediction was very high - so the program has scope to affect their decisions both regarding schooling and marriage. Therefore, in this exercise, a decreasing effect by dropout decile is suggestive evidence of an income effect.

Figure 7 shows the empirical results. Girls with high dropout probability are not affected by the program, regarding their marriage decisions. This could be the case because the schooling effect on these girls is very strong, counteracting the income effect. As the probability of dropout decreases, the program has a larger impact on marriage. In particular, for girls in the first decile, for whom we can interpret the program's effect as purely its income effect, results are consistent with the previous finding of an income effect on marriage. These results are consistent with marriage market matching problems, where education delays marriage, but income

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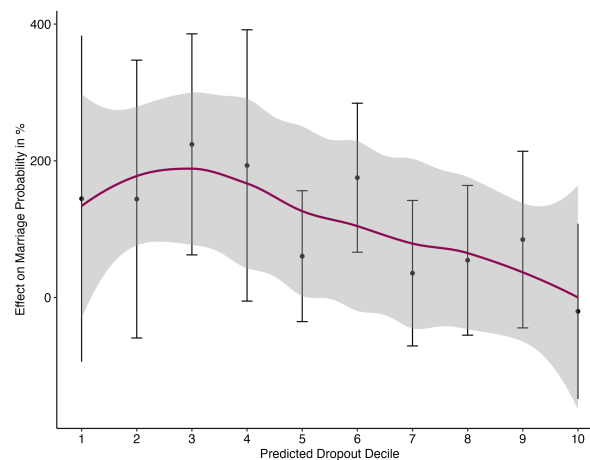
<sup>20</sup>Since dropout probability varied substantially with age, which is highly correlated with marriage and educational attainment, the prediction and decile classification were made separately by baseline age.



accelerates it.

The estimates are overall noisy due to the low number of observations in each decile. However, the negative slope we observe in the exercise is supporting evidence of the program's income effect explaining child marriage increases.<sup>21</sup>

**Figure 7:** Effect of Progresa/Oportunidades on the Probability of Marriage by Predicted Dropout Decile



Note: This figure presents the average treatment effect on the treated on marriage by predicted dropout probability (in deciles). Only marriages under 18 are included here.

**Discussion** By looking at two populations that were likely exposed to the program's income effect alone, I find supporting evidence that the increases in child marriage driven by the program were a result of income increases.

Income impacts marriage decisions through several channels. The first is through the costs of marriage. Even if there is no formal wedding, as most unions at this age are informal, there are still expenses associated with marriage. 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

<sup>21</sup>Results are robust to looking into 8 or 12 quantiles of the dropout probability, instead of deciles, as shown in Figure A16.

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.<sup>22</sup>

Second, the program might 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.<sup>23</sup>

Third, the income effect might change girls' bargaining power in the parents' household. 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 home production, thus decreasing the cost of leaving the household. Further, higher bargaining power might also lead to more independent decision-making, such as marrying and exiting the household. Usually, higher bargaining power is associated with decreases in marriage. This is true in settings where marriages are arranged and girls are not the decision-makers. In Mexico, more decision making power might comprise earlier marriages, if the girl chooses so. If the girl wants to leave the household and marry, she can do it as long as she continues going to school and the family continues receiving the benefit.<sup>24</sup> 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 (des-

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<sup>22</sup>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 Progreso program.

<sup>23</sup>Unrelated to income but due to schooling, girls might also have a broader marriage market. They might be 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 marriage.

<sup>24</sup>Note that, according to the regulation of the program, girls older than 16 can receive the benefit themselves providing the mother's approval. Unfortunately, there is no data available to determine who benefited from this rule.

pite their arrangement within the household), but only beneficiaries can afford it, which also explains the observed pattern.

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.

## 7 Conclusion

I study the effect of a conditional cash transfer program implemented in rural Mexico, Progresas/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.

Given that Mexico is a context 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. 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.

It is important to highlight that, despite the negative effect I present in this paper, Pro-

progres/Oportunidades was a successful program regarding many other social and economic outcomes.<sup>25</sup> 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. 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. 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. It also had positive externalities on non-beneficiaries, as documented by Lalive and Cattaneo (2009); Bobonis and Finan (2009) and Angelucci et al. (2010). 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.

It is worth noting that the data used in this paper does not allow for a serious and in-depth analysis of the effect of the program on marriage quality or on the consequences of child marriage. Therefore, further research should assess early marriage's economic and social consequences and do a cost-benefit analysis, accounting for this or similar 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.

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<sup>25</sup>See Parker et al. (2007) for a comprehensive summary and discussion of research on Progres/Oportunidades.

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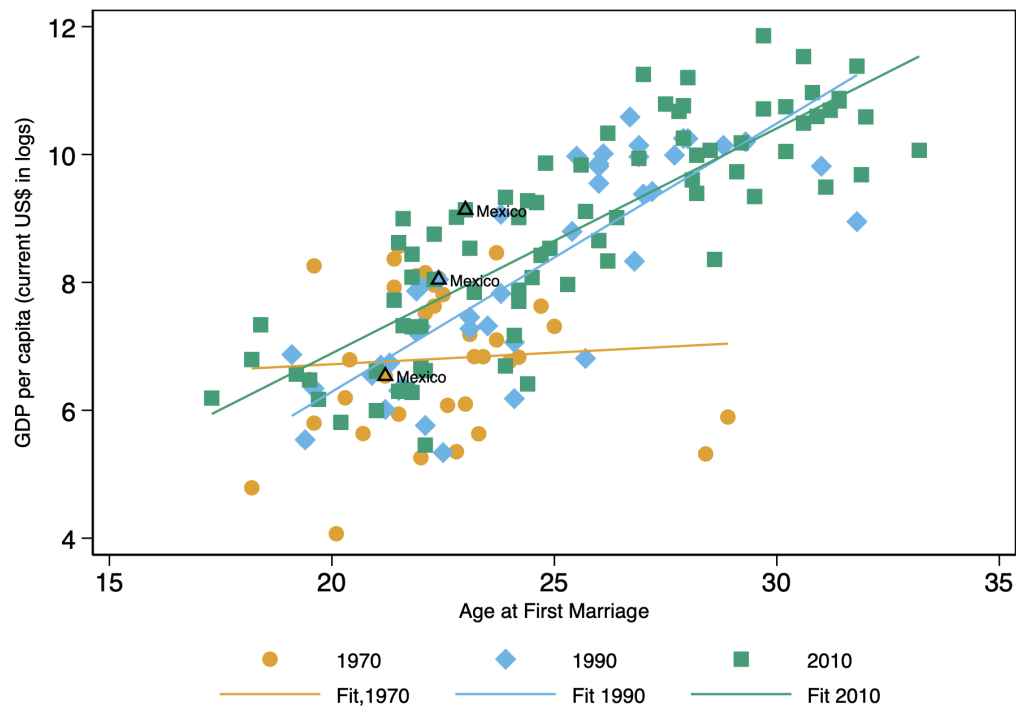


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## A Appendix

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

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

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

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

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

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-weighting 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.

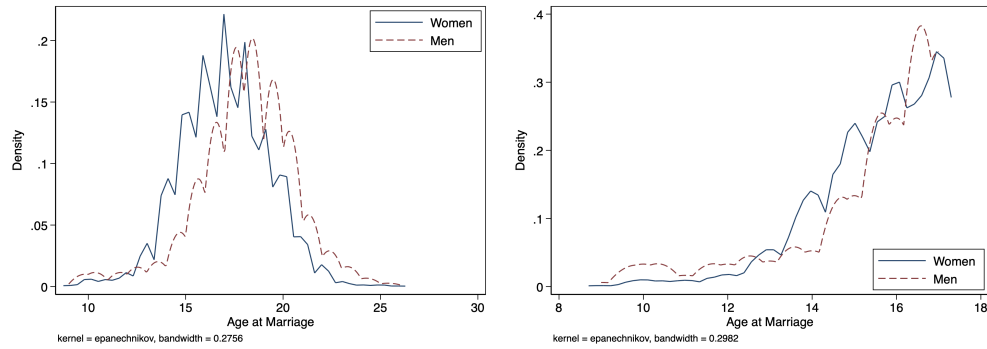


**Table A6: Balance Test on Baseline Characteristics: Boys (2)**

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

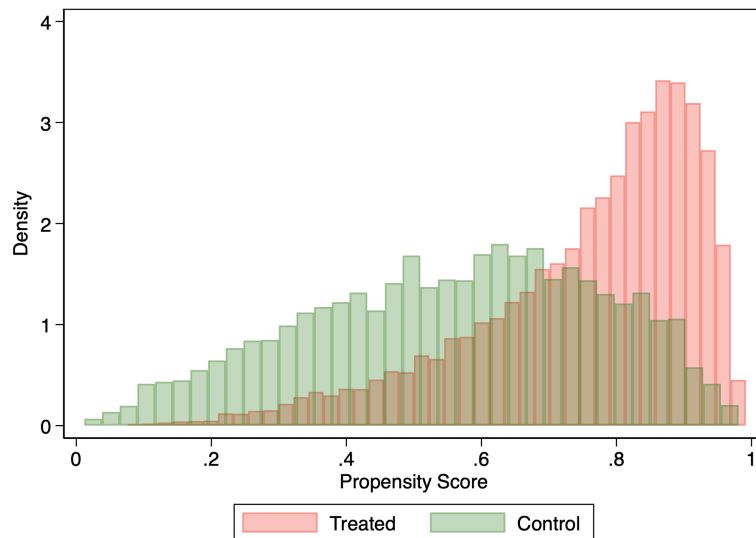
Note: This table reports parameter estimates and t-statistics (in parentheses) for regressions of baseline characteristics on a treatment indicator for boys. T1998 (T2000) equals 1 if the individual belongs to the set of villages treated in 1998 (2000) and 0 if belongs to C2000. The first two columns report the parameters without any re-weighting. The third and fourth columns report the estimates re-weighting 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**



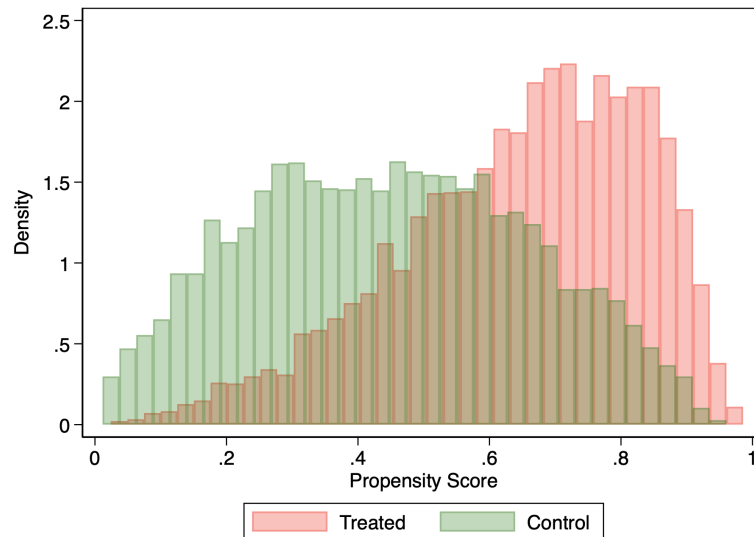
Note: The left panel presents the age distribution at marriage for the entire sample, separately for men and women. The right panel presents the same distribution but only considers individuals who married before 18 years old.

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



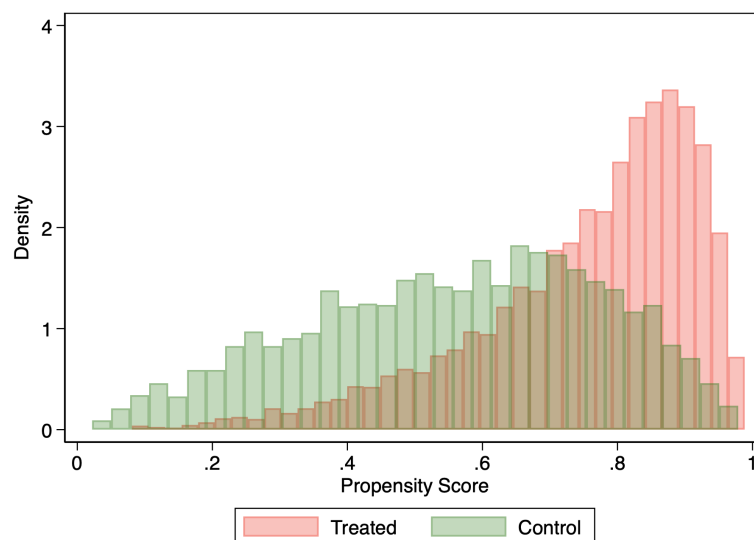
Note: This figure separately presents the histogram of the propensity score for treated (T1998) and control (C2000) groups.

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



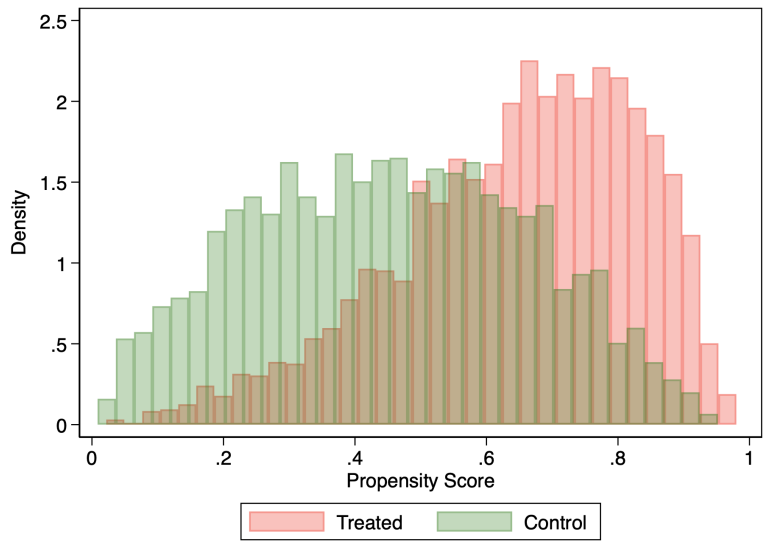
Note: This figure separately presents the histogram of the propensity score for treated (T2000) and control (C2000) groups.

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



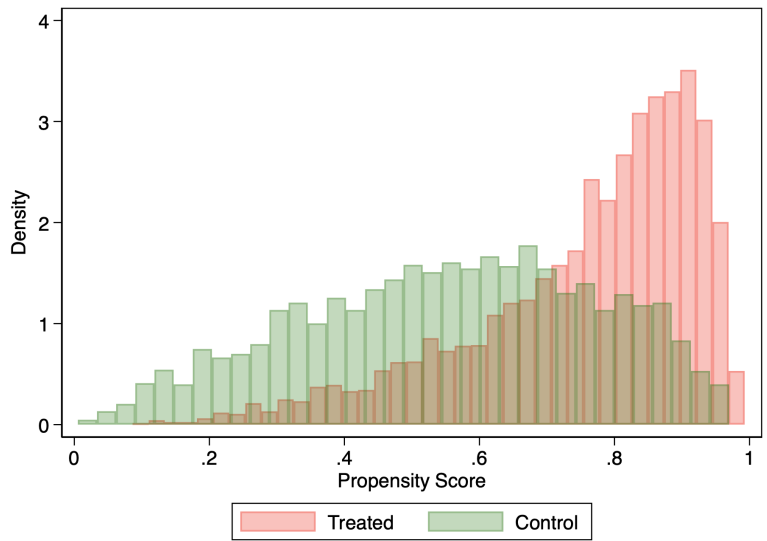
Note: This figure separately presents the histogram of the propensity score for girls in treated (T1998) and control (C2000) groups.

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



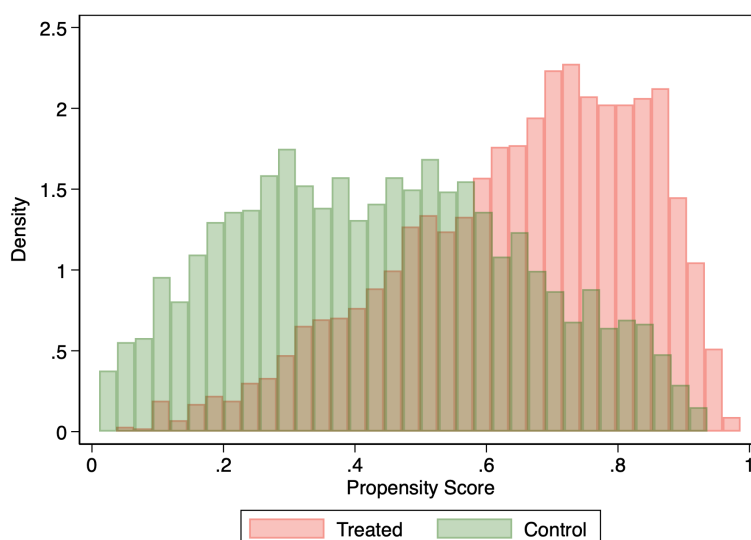
Note: This figure separately presents the histogram of the propensity score for girls in treated (T2000) and control (C2000) groups.

**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**

<b>(a) All</b>							
	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>(b) Under 18 years old</b>							
	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
Year of Marriage	1997	51.32	38.03	26.67	7.74	3.2
	1998		51.67	33.77	21.74	13.04
	1999			50	40.99	31.91
	2000				46.45	34.96
	2001					8.43
	2002					8
	2003					20.24

Note: This table shows the proportion of children 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: Experiment (T1998 VS T2000)

Event-Time	ATT(t)	Std. Error	Conf. Interval
-1	0	NA	[ NA , NA ]
0	2e-04	0.0019	[ -0.0046 , 0.005 ]
1	0.0012	0.003	[ -0.0063 , 0.0087 ]
2	0.0023	0.0039	[ -0.0075 , 0.0122 ]
3	-0.0031	0.005	[ -0.0157 , 0.0096 ]
4	-0.0028	0.0055	[ -0.0169 , 0.0113 ]
5	-0.0056	0.006	[ -0.0207 , 0.0096 ]
N	20017		

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.



**Table A13:** Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure: Experiment (T1998 VS T2000) - Girls

Event-Time	ATT(t)	Std. Error	Conf. Interval
-1	0	NA	[ NA , NA ]
0	1e-04	0.0032	[ -0.008 , 0.0081 ]
1	0.0031	0.0045	[ -0.0082 , 0.0144 ]
2	0.003	0.006	[ -0.0122 , 0.0182 ]
3	-0.0041	0.0083	[ -0.0252 , 0.0171 ]
4	-0.0023	0.0099	[ -0.0274 , 0.0229 ]
5	-0.0039	0.0098	[ -0.0287 , 0.021 ]
N	9649		

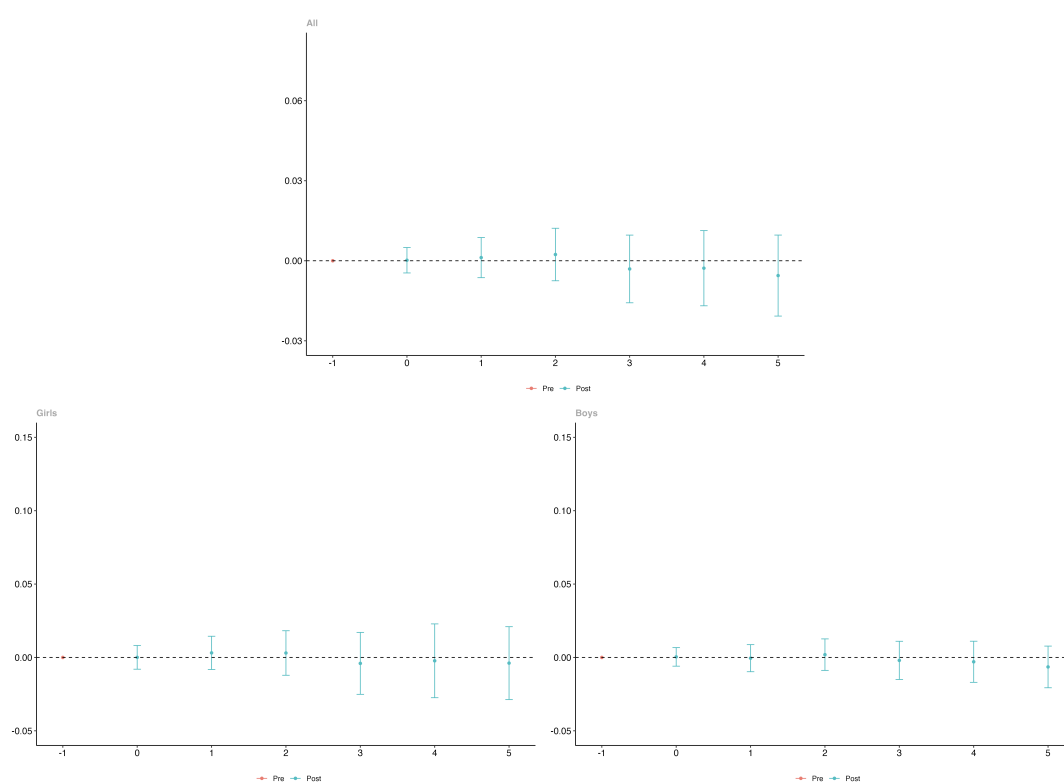
Note: This table shows the average treatment effects by length of exposure and the respective standard errors and confidence intervals. N is the number of observations. Event-Time refers to the period relative to the treatment year.

**Table A14:** Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure: Experiment (T1998 VS T2000) - Boys

Event-Time	ATT(t)	Std. Error	Conf. Interval
-1	0	NA	[ NA , NA ]
0	4e-04	0.0026	[ -0.006 , 0.0067 ]
1	-5e-04	0.0038	[ -0.0097 , 0.0088 ]
2	0.0019	0.0044	[ -0.0089 , 0.0126 ]
3	-0.002	0.0054	[ -0.015 , 0.011 ]
4	-0.003	0.0058	[ -0.017 , 0.011 ]
5	-0.0065	0.0059	[ -0.0207 , 0.0077 ]
N	10368		

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: Experiment (T1998 VS T2000)



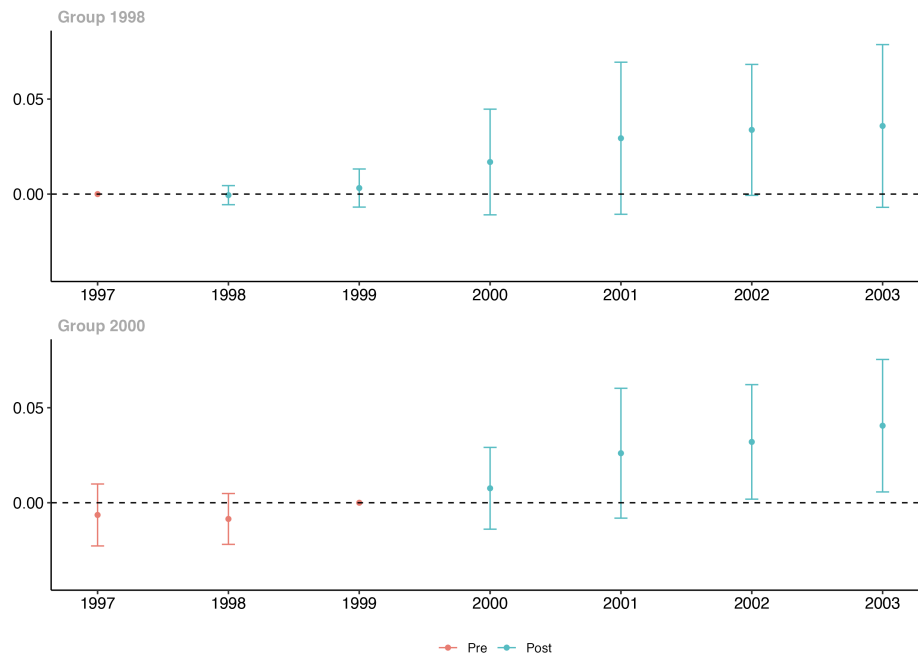
Note: This figure presents the average treatment effect on the treated, comparing T1998 and T2000. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustering, at the randomization level: locality.

**Table A15:** Effect of Progresa/Oportunidades on the Probability of Marriage by 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 A10:** 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.

**Table A16:** Effect of Progres/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	

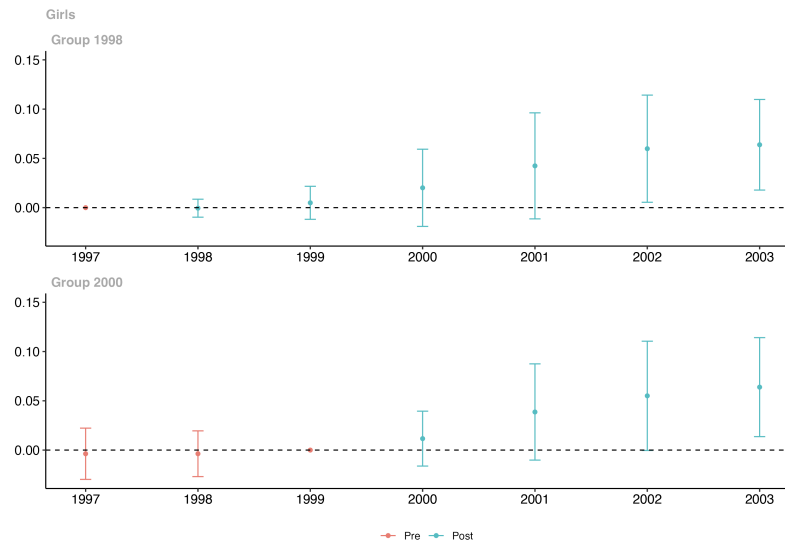
Note: This table shows the average treatment effects by group and length of exposure and the respective standard errors and confidence intervals. N is the number of observations.

**Table A17:** 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: 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 A18:** 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	

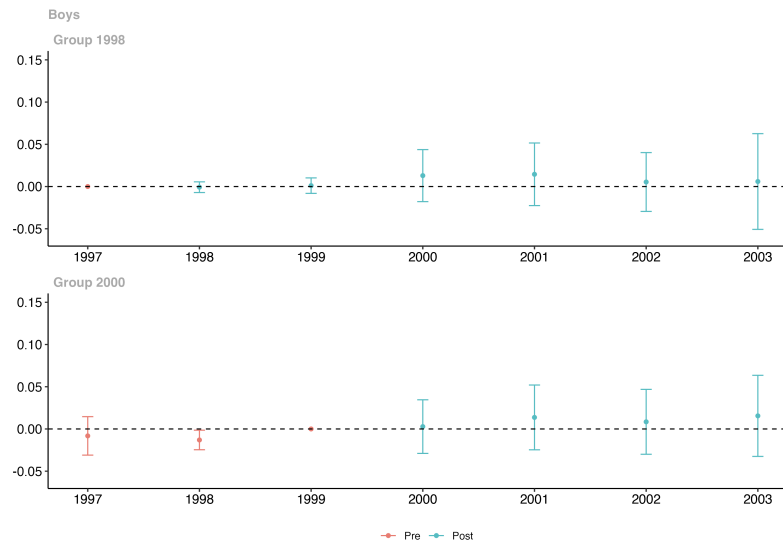
Note: This table shows the average treatment effects by group and length of exposure and the respective standard errors and confidence intervals. N is the number of observations.

**Table A19:** 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.

**Figure A12:** 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.

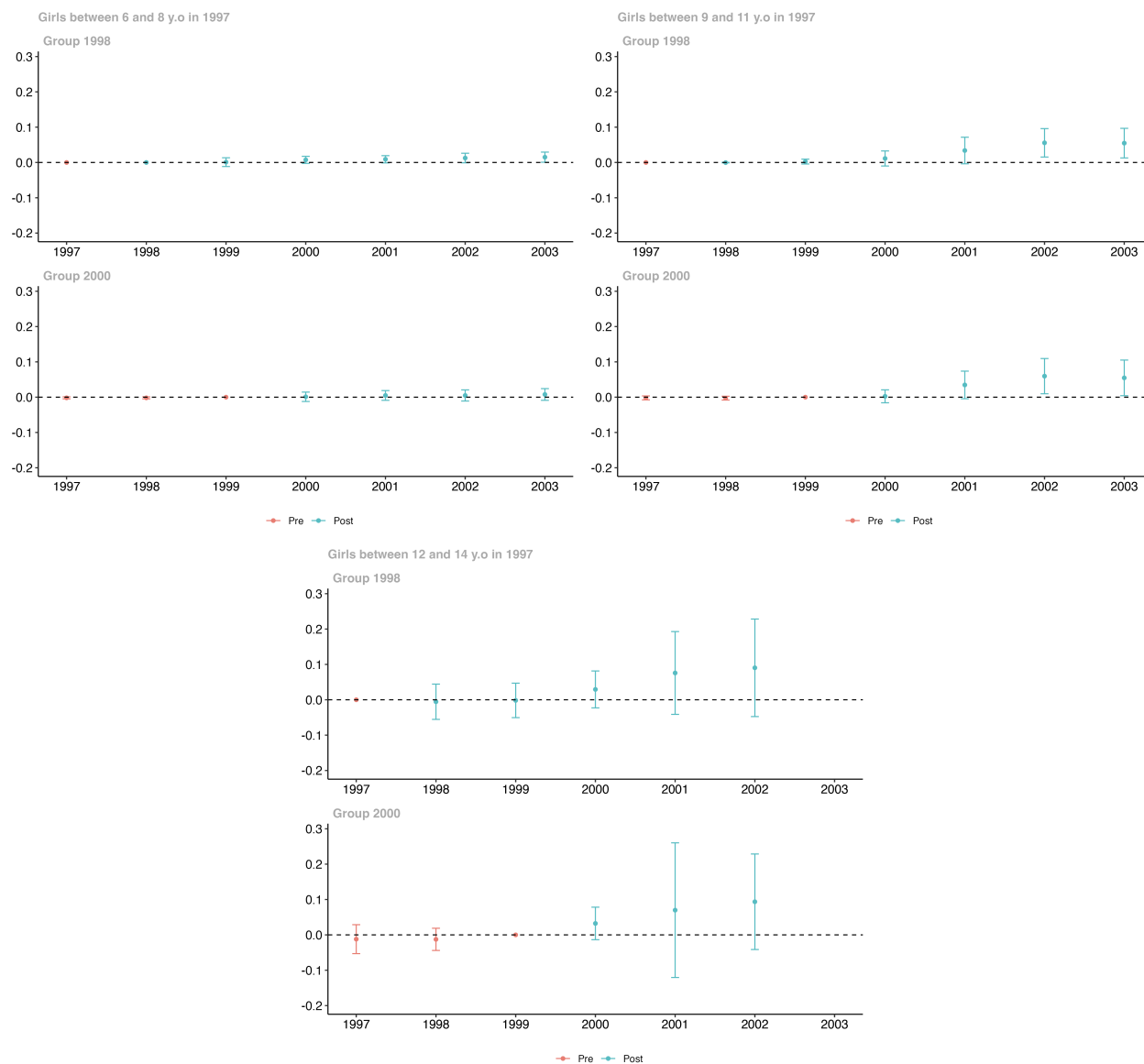
**Table A20:** Effect of Progresa/Oportunidades on the Probability of Marriage by Group and Year: Boys

	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.

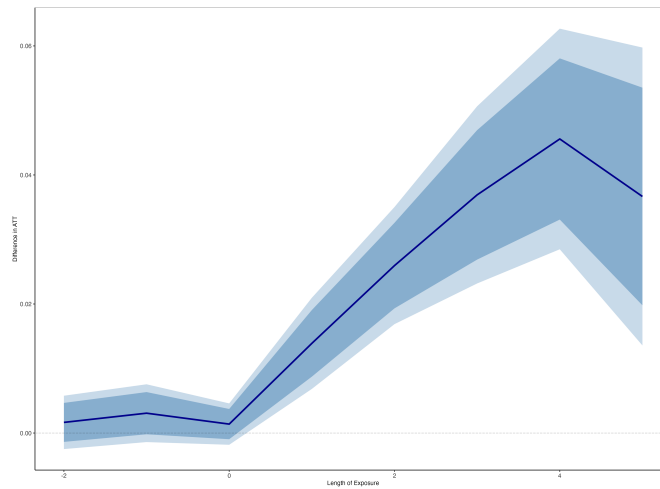


**Figure A13: Effect of Progresa/Oportunidades on the Probability of Marriage: Girls, by Age**



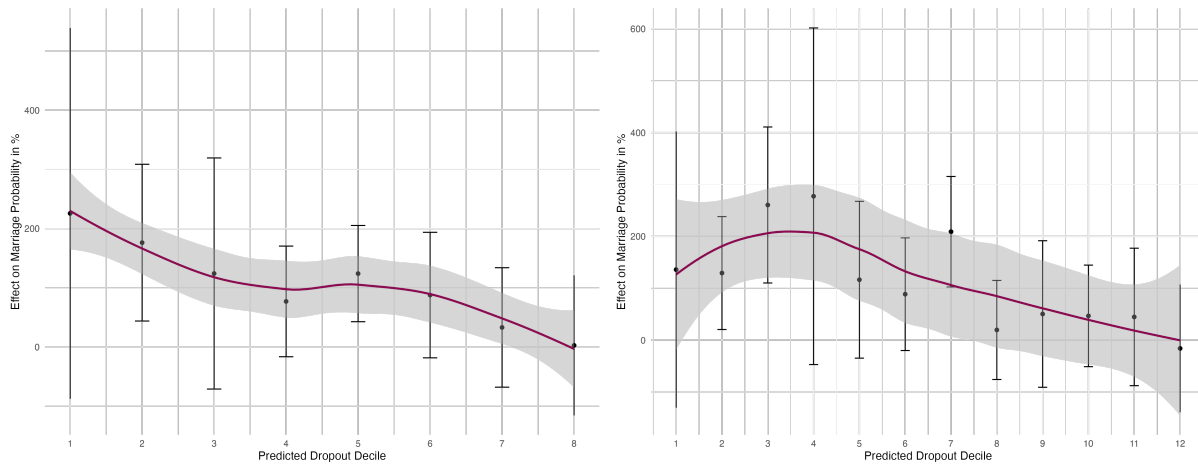
Note: This figure presents the average treatment effect on the treated girls in T1998 and T2000 by year and age group. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustering at the randomization level: locality.

**Figure A14:** 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



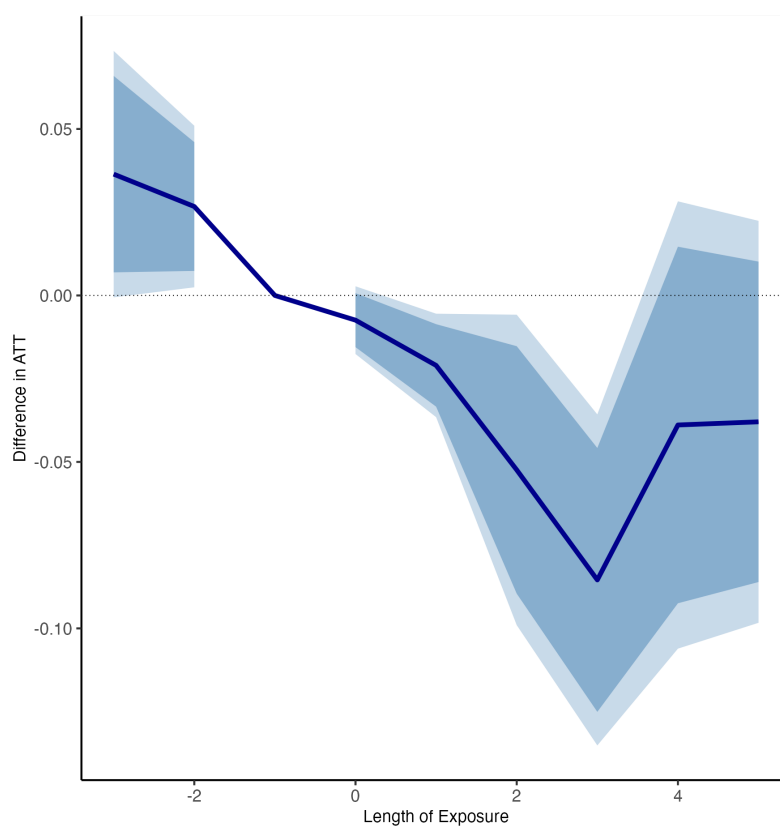
Note: This figure presents the difference in the program's average treatment effect for girls older than 13 in 2001 and girls younger than 12 in 2001. Standard errors were obtained through clustering at the randomization level: locality.

**Figure A16:** Effect of Progres/Oportunidades on the Probability of Marriage by Dropout Probability



Note: This figure presents the average treatment effect on the treated on marriage by predicted dropout probability (in 8 quantiles). Only marriages under 18 are considered.

**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: Data Limitations

### B.1 Marriage Reporting

The outcome of interest, marriage status, may be subject to misreporting. Three concerns arise regarding marriage reporting. First, the control group's marriage status and age at first union were asked in 2003, whereas the treatment group was asked about marriage status in every wave. Second, existing evidence suggests that adolescents under-report marriage (Neal and Hosegood, 2015), which may be due to social desirability bias. Third, misreporting might differ by treatment status.

To address these concerns, I conduct a series of empirical exercises to assess the likelihood of misreporting. I start by comparing the proportion of individuals legally married in the Census and the Mexican administrative marriage data (La Estadística de Matrimonios) in 1995, for the villages covered by the Census.<sup>26</sup> I find very close proportions in the census data (1.77 marriages per 1000 inhabitants) and the administrative marriage records (1.42 marriages per 1000 inhabitants) for both children (under 18) and adults (6.2 in the Census and 5.9 in the registry data). This result is reassuring, as it suggests that self-reported data is close to administrative records.

Then, I compare the rates of child marriage in the Census 2000 data with the ENCEL 2000 data. I use the year 2000 because it is the only year where both Census and ENCEL data were collected simultaneously. Restricting the census data to villages covered by ENCEL and aggregating the ENCEL data to the village level to match the coarser level of the Census data, I find similar proportions of girls married in 2000 in both datasets (6.14% in ENCEL and 6.24% in the Census).

Finally, I examine all girls in the ENCEL data between 15 and 17 years old in 1997 who reported being single in 1997. Of these, only 2% declared an age at marriage in 2003 that was inconsistent with the report in 1997. This suggests that the majority of girls reported their

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<sup>26</sup>In the administrative data we have the universe of legal marriages that happen at a given municipality and year. To have the same data in the Census, I consider those individuals who report an age at marriage equal to the current age. This way, I ensure that I am only considering marriages that happen that same year and these are comparable with the marriage records.

marriage status accurately. These three exercises provide evidence of the reliability of marriage reporting in the ENCEL data.

However, these exercises do not address the role of treatment in the issue of misreporting. To examine this, I use the proportion of individuals in each municipality treated by the program as a measure of treatment intensity.<sup>27</sup> I then regress this variable on the probability of over-reporting child marriage in the ENCEL data, compared to the census data. I find a non-statistically significant correlation between the intensity of treatment and the probability of over-reporting child marriage (the point estimate is 0.53, and the CI=[-1.16, 2.22]). If I divide the villages into two groups (below and above the median treatment intensity), I find that being in a 'high' treatment intensity village is also not significantly correlated with the probability of over-reporting (p.e 0.09 and CI=[-0.099, 0.34]).

Furthermore, I examine the extent of misreporting by comparing the rates of child marriage between the two datasets (ENCEL minus Census). I find an average difference of -0.004. I also find no statistically significant correlation between this difference and treatment intensity (p.e=-0.12, CI=[-0.45, 0.21] using the continuous variable, and p.e=-0.003, CI=[-0.03,0.02] using the dummy variable). In conclusion, although I cannot rule out the possibility entirely, these exercises provide evidence that marriage misreporting is unlikely to be a significant problem in my analysis.

## B.2 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

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<sup>27</sup>Since the smaller geographical stratum in the Census data is the municipality, and given that the program was implemented at the locality level, I use the proportion of individuals in each municipality treated by the program as a measure of treatment intensity.

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%,  $CI=[0.016, 0.025]$ .

**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.

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

**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.

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 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.

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

**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.

**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.



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 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.