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

PRELIMINARY. PLEASE DO NOT CITE.

Dalila Figueiredo*

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Abstract

In this paper I study the effect of a conditional cash transfer program in Latin America on the probability of marriage for children under 18 years old. I estimate the impact of Progresa/ Oportunidades leveraging the staggered implementation of the program. I find that the monetary transfer, conditional on school attendance, *increased* the probability of female beneficiaries being married. After five years of exposure to the program, beneficiary girls are, on average, almost 7 p.p more likely to be married than the control group. I find no effect for boys. These findings contrast with the previously documented positive effects of the program in education, which is usually associated with decreases in child marriage. To disentangle the effect of the monetary transfer from the education channel, I exploit the variation in household composition and find that non-eligible children in beneficiary households - who were only exposed to the increase in household income - were between 10 and 18p.p more likely to be married than their counterparts in non-treated villages. I reconcile the findings in a conceptual framework that helps rationalize how both education and marriage are increasing in response to the program.

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*European University Institute.

1 Introduction

Child marriage has long been recognized as an violation of human rights, mostly prevalent in developing countries. It has been pointed both as a consequence and a cause of poverty [1], mainly because of its association with education abandonment and lower participation in formal labor markets. Child marriage disproportionately affects girls, which makes it an important issue to consider when thinking about gender inequality in developing countries. In 2021, according to UNICEF, around 20% of world's women aged 20 to 24 years old, were first married or in a union before the age of 18. It is estimated that the prevalence of child marriage among boys is one sixth that among girls. The consequences of child marriage are particularly striking for girls due to the social and biological differences between the two genders. Girls who marry before turning 18 are more likely to suffer violence, abuse and exploitation after marriage, and to have their autonomy restricted. Child brides are also more prone to early childbearing, which has been documented to have adverse effects for both mothers and children. [2]

In this paper I study whether one of the largest conditional cash transfer programs in the world, Progresa/Oportunidades had an effect on young beneficiaries' marriage decisions. This program was implemented in Mexico, with the primary purpose of reducing poverty and its inter-generational cycle in rural Mexico. This conditional cash transfer program provides a monetary transfer to families and basic healthcare, provided that children attend regularly school and health centers. Initially transfers were given to poor households who had newborns or children who could attend primary or secondary school. In 2001, the program was extended to households with children who could enroll in high school.

To estimate the causal effect of the program on child marriage, I exploit the random allocation of the program across municipalities and the variation in the timing of implementation. Progresa was implemented in 1998 and, out of 506 villages, 320 were randomly selected to receive the program (T1998) and 186 to be the control group. Households in the later group started receiving benefits in 2000 (T2000) and the program was renamed Oportunidades. In 2003 a new control group of localities was created by the implementation authorities using propensity score matching (C2000). Its staggered implementation and the rich panel structure of the data allows me to obtain dynamic causal treatment effects, by comparing the three groups for 6 years through a staggered differences in differences strategy. Given that the third group is created by propensity score matching, using a doubly robust estimator improves the comparability between the groups, as it corrects for potential

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

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

differences pre-treatment.

I find that exposure to the program increases the probability of marrying before 18 years old. One year after the start of the program, beneficiaries are 0.8 p.p (CI=[0.001, 0.015]) more likely to be married and 3.3p.p (CI=[0.005, 0.061]) after five years, which almost doubles the probability of being married for the treatment group relative to the control group. These effects are largely driven by treated girls, who, after 5 years of program exposure, were almost 7 p.p (CI=[0.013, 0.123]) more likely to be married than non treated girls. This effect is indistinguishable from zero for boys. The effects start to be large and statistically significant after 2000, which coincides with the extension of the benefit to high school students.

For girls, I also investigate whether the treatment effects vary by age group by looking separately at three groups: (i) girls between 6 and 7 years old in 1997; (ii) girls aged 9 to 11 in 1997; and (iii) girls between 12 and 14 at baseline. The same pattern of treatment effects emerges. The point estimate, despite noisier, is positive from 2000 onward across all age groups and it is larger for older girls. Given the similar magnitudes of the effect across both treatment groups, the results seems to suggest that length of exposure to the program does not seem to matter, and the important determinant is the age at which girls are receiving the program. For example, girls who are 16 or 17 years old in 2001 are around 7p.p more likely to be married in both treated groups compared to similar girls in C2000, in spite of those in T1998 having been exposed to the program for twice as long as those in T2000.

Finally, I find that, conditional on getting married, beneficiaries of the program marry almost 1 year earlier and they are more likely to marry before turning 18 years old than similar individuals in the control group. Girls and boys in treated villages marry approximately 0.8 years earlier than those in the control group and girls are, on average, 8p.p more likely to have married before 18.

The positive effects of the program on schooling, documented by Behrman et al. (2005b, 2009) and Dubois et al. (2012), among others, would suggest that the program would lead to a decrease of children's marriage. Theoretically, conditional on school attendance or performance, cash transfer programs are believed to create a deep systemic change on child marriage (Kalamar et al., 2016). If more schooling years increase labor market opportunities, the opportunity cost of marriage also increases(Becker, 1974; Brien and Lillard, 1994). There is also evidence that education increases autonomy and knowledge, thereby decreasing girls' need to rely on marriage, empowering them concerning partner's choice, increasing their bargaining power in the relationship and changing their fertility preferences (Ferré, 2009).

However, in this setting, increases in schooling cannot be isolated from the effect of the monetary transfer itself, which might also be influencing children's decisions. It could act as twofold: either leading families and children to rely less on marriage as a safety net due to a loosed budget constraint (Amin et al., 2016); or increasing the market value of beneficiary families, changing their network, and

allowing or facilitating the formation of a new household by making marriage expenditures more affordable.

Empirically, the design of the program does not allow isolating perfectly education and income, since all beneficiaries received both simultaneously. However, the positive effect the program had on marriage suggests that the monetary transfer is enabling marriages more than the increase in education is preventing them.

To test this hypothesis, I restrict the analysis to children who are no longer eligible for the benefits themselves, but who live with an eligible member. I find that, from 2001 onward, the effect of the program on marriage is positive and economically significant. Note that these individuals are exposed to an income effect only, and not to the conditionality. Nevertheless, the program could still be incentivizing these individuals to pursue more education. If this was the case, then I wouldn't be able to disentangle both mechanisms. However, I observe that they do not get more education than their counterparts in the control group. Therefore, I interpret this result as the effect of an income transfer on marriage, which is positive and economically significant.

To help rationalizing these findings I build a conceptual framework where individuals derive utility from consumption and marriage. Individuals live for two periods and they start by either being (i) single and out of school, (ii) single and in school, (iii) married and in school, and (iv) married and out of school. In each state individuals consume a state-specific endowment and if individuals are in school they add to their consumption the equivalent to a monetary benefit. which mimics the CCT program. If they are married they receive utility from marriage. If they are in school and married, their marriage utility is penalized to account for having to split time and energy between school and marriage. At the end of the first period they must decide their state in the second period. Endowments might change in the second period but individuals have perfect information on them. If not yet married, individuals draw a quality of their potential match and decide on whether to marry or not. Once individuals are married, they cannot divorce, but all the other state transitions are allowed.

This framework gives me four testable predictions: an increase in the school benefit leads to (i) an increase in the mass of people who choose being single in school over married and out of school; (ii) an increase in the mass of people who chose to be married in school rather than married and out of school; (iii) an increase in the mass of people who choose being school and married over single and out of school; and (iv) inconclusive effects on what happens to those individuals choosing between staying in school single and staying in school but married. If the consumption associated to the state in school and married in the second period is larger than the one associated to the school and single state, then, by concavity, an increase in the benefit will yield a larger utility increase if choosing to stay in school and single than in the other state. This leads to an increase in the mass of people who prefer

the first to the later. If, on the other hand, consumption in the second period when in school and married is expected to be smaller than in school and single, an increase in the monetary benefit will increase the mass of people who get married but stay in school. If, for instance, there is a large cost associated to marriage (e.g. a wedding celebration, extra consumption in the household, or setting up a new household) that decreases consumption, then the extra benefit will be used to compensate for that loss. So in this case, an increase in the program will lead to an increase in the mass of people who get married but still choose to stay in school. A simple comparison between transition matrices across the treatment and control groups suggest that the predictions of the model are compatible with the data.

This paper contributes to the broader literature on child marriage, its causes and potential solutions. It does so in a context that has been understudied, a country in Central and Latin America, which differs importantly from the most studied countries by the literature. In Mexico arranged marriages, dowries and bride prices are not institutionalized and it is believed that those getting married are the decision makers, rather than their parents[3]. It is a country in which it is estimated that 1 in every 4 women between 20 to 24 years old married before turning 18, and this rate has not changed in the last 40 years. These contextual differences might be important to determine the mechanisms through which programs impact marriage decisions and so adding evidence on these countries is important to broaden our understanding of child marriage around the world.

Even within a country, important contextual differences and similar policies might lead to heterogeneous end results. Gulemetova-Swan (2009), for example, studies the effect of Oportunidades on marriage and fertility choices of girls in urban Mexico between 2002 and 2004. Using a multistate hazard model, she finds a small, but negative effect on the timing to premarital sexual activity and age at marriage, as a result of increasing incentives to attend school and information on family planning. Using the estimated hazard function in a structural model to obtain interpretable coefficients, she finds a delay in the age at first marriage of 1 to 4 months. Besides the empirical strategy and modeling choices, there are two important differences between our studies. First, the two populations studied are considerably distinct: I study the rural population in Mexico that is known to be more prone to child marriage, where marriage has a higher social value and where there are less returns from education in the labor market.These might be important mitigators of the effect of the program on marriage decisions. Second, Oportunidades in 2002 added mandatory attendance to sessions about sexual and reproductive education, family planning, gender and health and domestic violence for girls in high school. Increasing education on these topics might as well have directly influenced marriage and fertility decisions.

This paper also adds to the literature of income increases on marriage. For example, Bobonis

[3]There are reports of cases of parents "selling" their children for marriage, but these are both illegal and rare.

(2011), who also studies the effect of Progresa on adults' marriage decisions, finds results that are aligned with the ones of this paper. As the conditionality was towards their children's school enrollment and medical visits, the author interprets his results and a consequence of an increase in income. He finds that the share of women in a union did not change but there was a small increase in dissolution). However, when focusing on eligible-mothers with no relationship at baseline, this study finds that Progresa increased marriage and cohabitation as a result of an income effect. Similarly, in a completely different context, Zou (2021) shows that marriage is a normal good and male NBA players who had an exogenous and positive income shock are more likely to marry. She also suggests that lower income men are more responsive to income shocks in what concerns marriage decisions.

This paper also contributes for the literature on the effectiveness of cash transfer programs in delaying marriage. Angrist et al. (2002) and Hallfors et al. (2015) study programs that decrease the cost of education in Colombia and Zimbabwe, respectively, and their effects on education and marriage outcomes. Both find that the respective programs led to an increase in years of education and, consequently, to a decrease in the probability of marriage or cohabitation. However, in Colombia, the consequential effect was small and short-lasting. Ashraf et al. (2016) compare Zambia and Indonesia and find that an increase in education only affects probability of marriage for the community who has bride price as a tradition, which is increasing in education. For the other groups, there was no effect. One study that is able to disentangle the effects of the conditionality and the monetary effects from just the monetary effects is Baird et al. (2011). The authors compare unconditional versus conditional cash transfers in Malawi and find that the first successfully reduces the likelihood of being married but the second did not. The delays in marriage found in this setting are driven by girls who dropped out of school after the start of the program. They explain the difference in results between the conditional and unconditional by arguing that the conditionality is denying transfers to noncompliers (school dropouts) who are particularly at risk of early marriage and teenage pregnancy. These results suggest that a positive income transfer delays marriage but only for the girls who drop out of school. Since in my setting I cannot distinguish compliers from noncompliers, nor who in the household is the direct beneficiary of the program, a simple comparison between the two set of results would be inappropriate. However, further analysis would be interesting to understand what are the differences both in the programs and in the populations that might be driving the difference in the results.

The rest of this article is organized as follows. Section 2 presents the institutional setting where the program is explained in detail. Section 3 introduces the data used in this project and Section 4 explains the empirical strategy used to estimate the effect of the program on child marriage probability. In Section 5 I present the results and discuss the mechanisms in Section 6. Section 7 concludes this paper.

2 Institutional Setting

Contrary to trends observed in the rest of the developing world, in Central and Latin American Countries child marriage rates are around 25% and they have been steady for the past 20 to 30 years with no expected change (UNICEF, 2019). The puzzle is that most Latin America and Caribbean (LAC) countries witnessed rapid and prosperous socioeconomic changes. Between 1970 and 2000, the percentage of women with secondary education increased from 3 to 38% and the participation on the labor force from 21 to 44%. Yet, average age at marriage remained constant, 24 for men and 21 for women, as well as the proportion of girls and boys marrying before 18 years old.

In most LAC countries there are no practices of dowries and price brides, thus families have no direct economic incentive to marry their children. In fact children are seen as the agents deciding on whether or not to get married. Given gender inequality and discriminatory social norms, the role of women in the society does not focus on their occupation, but on their ability to create and sustain a family. By becoming wives and mothers, they are better accepted in the community and gain respect from others. Gender disparity and conservative norms also play a role through constraints in girl's sexual lives (Brides, 2017; Taylor et al., 2019). With marriage, girls are not subject to their family rules and restrictions on their sexuality and avoid the social stigma associated with out of the wedding pregnancy. It is also a mechanism to escape violent households and to protect themselves from exploitative groups in areas with extreme violence. Finally, marriage might also seem the better option for these girls' future in terms of economic stability, both due to the lack of opportunities in the labor market, and as an insurance mechanism against economic instability (UNICEF, 2019; Parrado and Zenteno, 2002).

In Latin America and the Caribbean, most early marriages occur as informal unions (UNICEF, 2019). In Mexico, for instance, around 75% of the girls between 15 and 17 years old who were ever married or in a union report being in an informal union. Since law enforcement is harder to implement, tackling this problem through legislative changes might not be efficient. In fact, a change in the state laws between 2014 and 2018 forbidding completely legal marriages under 18 years old, led to a decrease in legal marriages which was offset with an increase in informal unions (Bellés-Obrero and Lombardi, 2020).

In contrast to many other countries where child marriages are defined by minor females only, in LAC countries males also marry often underage. They are among the countries with the highest levels of groom children. In Belize and Nicaragua, for instance, one in five boys were married or in a union before completing 18 years old.

To deepen our understanding on potential mechanisms and policies to delay or prevent early marriage, I evaluate one of the most praised conditional cash transfer programs in the developing

world, Progresa/Oportunidades, implemented in Mexico in 1998. As mentioned before, conditional cash transfers might affect child marriage through several channels as health, education and poverty alleviation, depending on its design.

Progresa/Oportunidades aimed at reducing poverty and its inter-generational cycle in rural Mexico. It does so through three sets of action: (i) offering basic health care to all family member; (ii) providing a fixed monetary transfer to be spent in 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 scheme of benefits for 1998 is shown in Table 1. They are increasing in grade and slightly higher for girls than boys in secondary school. Note that the transfer consists, on average, of approximately 14% of eligible households' income (1400 pesos, equivalent to 173 USD in 1998). In 2001 the program suffered some changes and was renamed Oportunidades. Important changes for this analysis are the extension of benefits to high school (*preparatoria*) students, and the provision of bonuses in case students passed grade and if participated in other programs, such as *Jovenes con Oportunidades*[4].

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

In order to receive these transfers, families have to comply with a set of conditions. They need to attend scheduled medical visits and at least 85% of classes/school activities. Primary and secondary school education was mandatory since 1992, and although primary education had enrollment rates close to 90% in 1997, in secondary school these were about 65%.

The program was implemented in 1998 in 320 rural localities. These were randomly chosen to be beneficiaries, as were other 186 to be part of the control group. All these localities fulfill a set of geographic and socioeconomic criteria: they had to be highly deprived but with access to elementary school, middle school and a health clinic (Abúndez et al., 2006). The control group joined the beneficiary group in December 1999[5].

[4] *Jovenes con Oportunidades* is a component of the program Oportunidades that awards a monetary prize to those students who conclude high school in less than 4 years and before completing 22 years old.

[5] The last survey this group answered to as a control group was set in November 2019. Therefore, for simplicity, I will

After the expansion of the program, in order to evaluate its long-term effects, the implementation authorities and evaluation team created a new control group of localities, via propensity score matching. These localities are from the same states as the original 506 communities (except for one, for which the neighbor state was used). The matching was performed on aggregated locality aspects using individual data from the Census 1995 and 2000. These consist in housing and demographic characteristics, poverty level, labor force participation and ownership of durable goods. Besides, localities had to fulfill the eligibility criteria of the program with respect to distance to schools and health clinics.

Inside each locality, eligible households were identified through socioeconomic data collected in 1997 which assessed their degree of poverty. On average, 78% of the households in the treatment group were eligible to the program and 97% accepted the treatment ((Dubois et al., 2012)).

The design of the program allows for comparisons across three groups. T1998 is the group of households who were eligible for the program in treated villages in 1998; T2000, the eligible households in the control villages between 1998 and 2000, who were then beneficiaries in 2000; and C2000, the control group formed after the expansion (using propensity score matching), which had not received the program by 2003.

3 Data

The data used in this paper consists of a sample of households in both control and treatment villages of Progresa/Oportunidades. These households were surveyed in November 1997 (ENCASEH97) and March 1998 (before the introduction of the program), in October 1998, twice in 1999 and 2000 (ENCELs). In 2003 there was a new survey (ENCEL2003) that included all the households that could be found in the original 320 localities and the new control group, whom were asked questions referring to 1997 and the three years prior.[6]

Of the full sample of eligible and ineligible households in treated and control localities, I consider only eligible (poor) households.

The outcome variables of interest are *marital status* from 1997 to 2003. I assume that an individual married if she reports being legally married, living in an informal union, cohabiting, divorced or widowed. I choose to do so since I am interested in first marriages, thus not accounting for separations. A child is single if she reported her status to be single. Married rates in 1997 were balanced in treatment and control groups. [7]

name this group T2000.

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

[7] In ENCEL2007, individuals were asked age at first marriage or union. This allows me to retrieve age at marriage for

My population of interest are all children in eligible (poor) household who were between 6 and 16 years old in 1997, the baseline year.[8] Keeping all those whose relevant information is non-missing, my sample consists of 25,304 children, 48% of which are women. By the year of 2003, 9.8% of all boys and 21.5% of all girls declared to be married, totaling 4464 reports of marriage. I observe age at marriage for 5058 children, of which 4325 married before 2003. Since I'm interested in marriages that occurred before 18 years old, individuals leave my sample after completing 18 years old.

Table 2 presents the proportion of married individuals by group and year in order to contextualize the magnitude of the effects. Individuals of the treatment groups T1998 and T2000 receive weight 1 and individuals in the control group C2000* receive a weight of $\frac{p(x)}{1-p(x)}$, where $p(x)$ is the probability of being in either T1998 or T2000.[9] This way I am presenting the summary statistics of the same weighted sample that I am using in the empirical analysis. The last row corresponding to C2000 presents the unweighted proportion of married children. Across all years, there are more married children in the treatment groups than in the control groups. However, in the first years of analysis the proportions are close across groups, starting to diverge after 1999.

Table 2: Proportion of Married by Group and Year

	1997	**1998**	**1999**	**2000**	**2001**	**2002**	**2003**
T1998	0.70%	1.28%	2.28%	3.21%	4.41%	5.15%	4.57%
T2000	0.89%	1.54%	2.36%	3.21%	4.93%	5.68%	5.19%
C2000*	0.75%	1.30%	1.10%	1.44%	1.71%	2.11%	1.83%
C2000	1.49%	2.03%	1.66%	1.88%	2.19%	2.90%	2.55%

Note: This table presents the proportion of married individuals by group and year in order to contextualize the magnitude of the effects. Individuals of the treatment groups T1998 and T2000 receive weight 1 and individuals in the control group C2000* receive a weight that is defines as $\frac{p(x)}{1-p(x)}$, where $p(x)$ is the probability of being in either T1998 or T2000. The last row corresponding to C2000 presents the unweighted proportion of married children.

Of those who report age at marriage and married before 18 years old, 3.6% married before turning 12, 36.8% married between 12 and 15 and almost 60% married while 16 or 17 years old.

individuals who married after 2003. I use this information only for the descriptive statistics and to complete marriage status in case of missing information from the other surveys.

[8] Of the entire sample of children, only 1.84% of those who married declare doing it when younger than 12 years old, therefore I assume that a child becomes at risk of marriage only at that age. Thus I exclude from the sample all children who do not turn 12 years old until 2003. I also do not consider children over 16 years old in 1997, given that they were exposed to the program close to turning 18 years old.

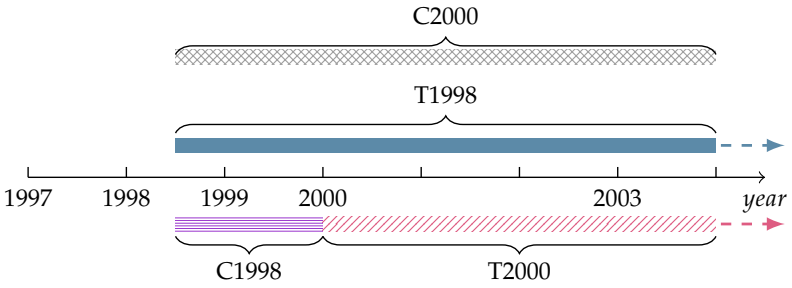
[9] This weighting scheme is the one used by the estimator I use to estimate the causal average treatment effect on the treated. More details in section 5.

4 Empirical Strategy

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

In 1998 treatment was randomly allocated to eligible villages in rural Mexico, T1998 (the treatment group) and T2000 (the control group) . In 2000, the program was extended to the first control group, T2000. In order to allow for long-run evaluations, in 2003, the program included a new group in the sample that never received benefits, C2000. These 152 communities were selected by matching observed community-level characteristics to those villages in T1998 and T2000. Figure 1 is an illustration of the program allocation across groups and the years of the analysis and the role they represent in the empirical strategy.

Figure 1: Treatment and Control Groups Across Years of Analysis



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

Its staggered implementation and the rich panel structure of the data allows me to obtain dynamic causal treatment effects, by comparing the three groups over a period of 6 years. I use the doubly-robust estimator proposed by Callaway and Sant'Anna (2020) for three reasons. First, it has been shown that in staggered designs two-way fixed effects models with staggered treatment cannot be interpreted causally when treatment effects are heterogeneous. The intuition behind this is that the estimate for the causal effect at a certain time period might be contaminated by the treatment effects from other periods, even if the parallel trends and no anticipation assumptions hold.[10] Second, since the randomization was done at village-level and the analysis is at individual-level, using individual pre-treatment characteristics allows me to have a closer comparison between individuals. Callaway

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

and Sant'Anna (2020), from now on the CS estimator, allows to do this through the inverse probability weighting approach, in which the propensity score accounts for the probability of each individual being in a given group. This is fundamental when using the third group (C2000) in the analysis, since its selection was not at random. With this I am minimizing the risk of bias on the estimated effect, by selecting similar individuals within the groups of villages. Lastly, this doubly robust estimator identifies the average treatment effect for each group at a given point in time even if either the propensity score model or the outcome regression models are misspecified.

The CS estimator identifies a group-time causal effect if the following assumptions hold. The first requires that the treatment is irreversible, meaning that if a group is treated at $t-1$, it is treated at t, which this design satisfies. The second assumption requires limited treatment anticipation. Attanasio et al., 2012 find no evidence of anticipatory behavior by any of the cohorts. The third assumption is the conditional parallel trends assumption: in the absence of treatment, the average conditional outcome of the treated and not yet treated groups would have evolved in parallel. A common test used as supporting evidence that this assumption holds is to test whether there are different pre-treatment trends for treated and control groups. Finally, I need to assume that the overlapping condition is satisfied, which means that at least a small fraction of the population is treated at each "starting" period (when treatment starts for each group) and that for all periods the propensity score is uniformly bounded away from one[11].

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

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

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

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

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

The variables used for both estimating the probability of treatment and the outcome evolution are gender, age at baseline, housing characteristics, household composition, characteristics of the head and spouse of the household, the state where the household lives, and the poverty index score for program eligibility in 1997 of the municipality of residence.[12]

The authors propose a bootstrap procedure for inference that can account for clustering. Since treatment was allocated across villages, this will be the level of clustering.

5 Results

5.1 Probability of Marriage

This section provides the estimated results on the effect of receiving a conditional cash transfer on the probability of being married before turning 18 years old. To do that, I leverage on the staggered implementation of the program in two sets of villages and compare the outcome of interest in the treated villages (T1998 and T2000) with the outcome in the villages that never received the program (C2000). As detailed in Section 4, I use the estimator proposed by Callaway and Sant'Anna (2020).

First, I start by analyzing if, overall, the program impacted the probability of marriage. On Table **??**, I observe that the program increased, on average, the probability of early marriage by 1.7 percentage points (p.p) (with a confidence interval ranging from 0.0086 to 0.0259, hereafter CI=[0.0086, 0.0259]), significant at 1%.

I can then explore how this effect varies with the length of exposure to Progresa/Oportunidades. Figure 2 shows the effect of the program on the probability of being married after a certain number of years of being in a beneficiary village (these results are also in Table **??** in the appendix). Time -1 represents one period to treatment, so for group T1998, $t = -1$ corresponds to 1997 and for T2000 to 1999. Time 2 represents two years after treatment. Note that the effects in Time 4 and 5 are only estimated using T1998, given that the last outcome to be observed is in 2003. Similarly for period -2, which is only observed for T2000. In the periods pre-treatment, I do not reject the null hypothesis of no effect of the program at any conventional significance level, which is supporting evidence that the

[12] **Housing characteristics**: dummy variables for dirt floor, inferior quality wall, inferior quality roof, number of bedrooms, piped water, electricity, ownership of animals, land, blender, refrigerator, gas-stove, gas-heater, radio, tv, dish washer, car and truck; **Household composition**: the number of members in the household and dummy variables for having at least one child between 0 and 5, at least one teenager between 16 and 19, at least one woman between 20 and 30, 40 and 59 and 60+, respectively, and at least one man between 20 and 30, 40 and 59 and 60+, respectively; **Head and Spouse characteristics**: if any of them had ever gone to school, if any of them worked the week before, if anyone in the household speaks an indigenous language, if the spouse of the household head is a housewife, if the household head is a woman and the age of the household head. Given the large number of missing data on education levels, working status and indigenous language of either the head or the spouse of the household, I decided to use variables at couple level (e.g. either chief or spouse worked the week before), instead of the two separately. For the same reason, instead of using the education level of both, I use if any of them had ever gone to school. Finally, a household with indigenous background is one where at least one person spoke an indigenous language.

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

	All		
	All	T1998	T2000
ATT	0.0172	0.0166	0.0183)
	(0.004)	(0.0044)	(0.0058)
	[0.0094 , 0.025]	[0.0068 , 0.0264]	[0.0054 , 0.0313]
N		25304	

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

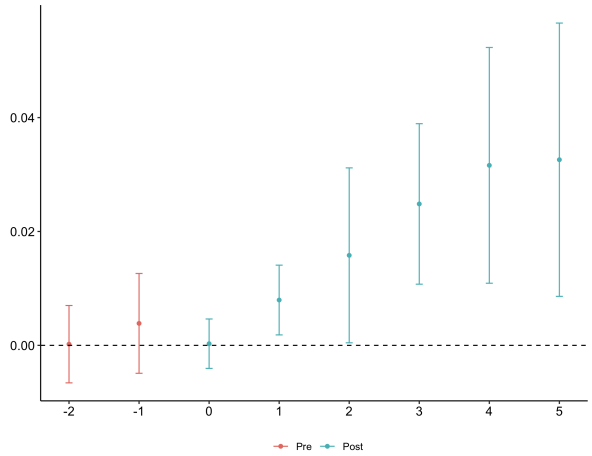
parallel trends assumption is likely to hold.

Then, I observe that in its first year of implementation the program has no effect on child marriage. However, it starts having a positive effect after one year of exposure. One year after receiving the benefit, treatment groups 0.8 p.p (CI=[0.001, 0.015]) more likely to be married than the control group, but this effect increases to 2.5 pp (CI=[0.011, 0.038]) in the third year and around 3.3 p.p (CI=[0.005,0.061]) after five years, statistically significant at 1%.

Figure 3 shows how these effects differ across treatment groups (these results can be found in Table **??** in the appendix). For group T1998, I observe a positive trend in the estimated coefficients one year after the program started. These are not statistically different from zero until 2002, where beneficiaries are 3.2 p.p (CI=[0.008,0.055]) more likely to marry before turning 18 years old than non-beneficiaries. In 2000 and 2001, the point estimated are already economically significant, 1.2 and 2.3 p.p, respectively, but the estimates are noisy. After 5 years of exposure, beneficiaries of T1998 are 3.3p.p (CI=[0.009,0.057]) more likely to marry, almost 3 times more likely than the control group (C2000).

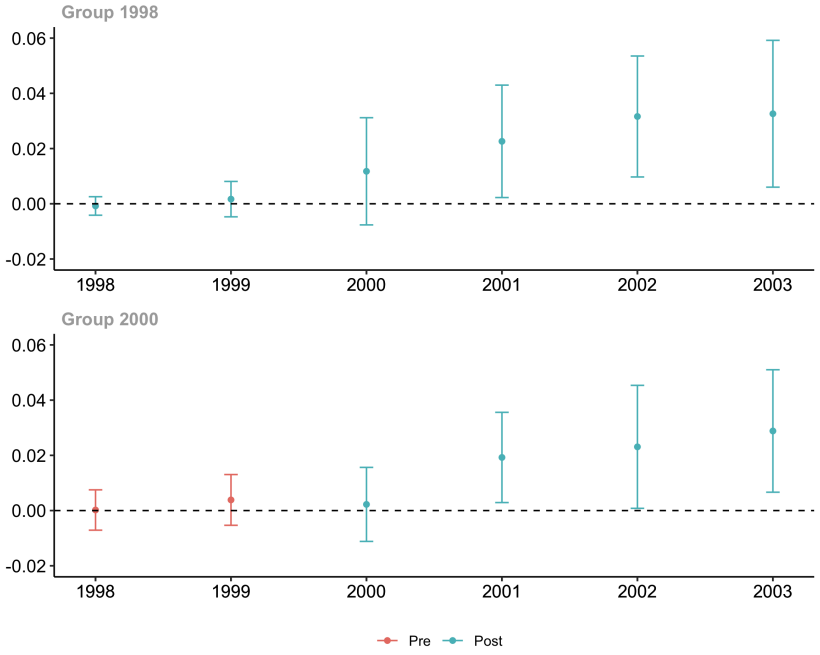
For the second treatment group, T2000, the program has a positive effect after the first year of implementation. In 2001, the effect is close to 2 p.p (CI=[0.004,0.04]) , 2.3 p,p (CI=[0.001,0.04]) in 2003 and almost 3 p.p (CI=[0.009,0.05]) in 2003, significant at 1%. These results suggest that the changes made in the program around 2001 (from Progresa to Oportunidades) are important to explain the positive effect of the program on marriage. I will discuss them in detail in the next section.

Figure 2: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure



Note: This figure presents the average treatment effect on the treated by length of exposure to treatment. Time -1 represents one periods prior treatment. For T1998 corresponds to 1997 and for T2000 to 1999. Period 2, on the other hand, represents two years after treatment. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustered bootstrap, at the randomization level: locality. The p-value for the pre-test of parallel trends assumption is 0.565.

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



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

5.2 Results by Gender

Looking at the results by gender, Table 4, I observe that the overall effects are driven by the large effects on girls. On average, the program increased the probability of child marriage for girls by 3 p.p (CI=[0.0123, 0.0496]). For boys this effect is neither economically not statistically different from zero.

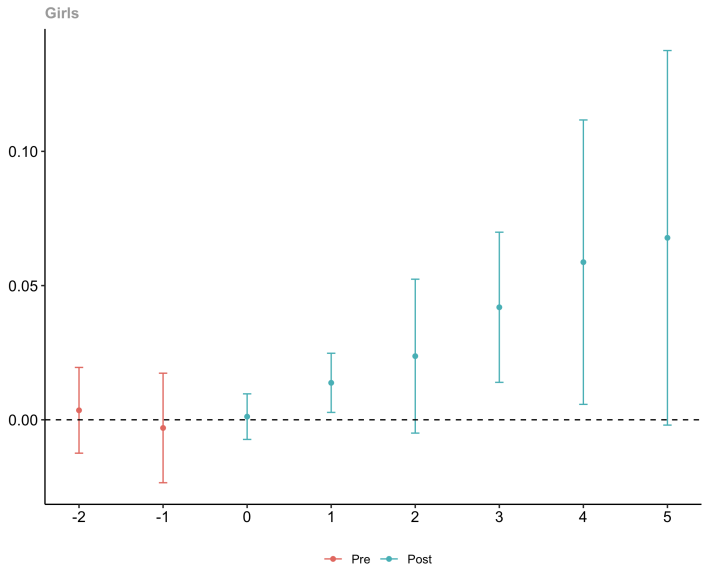
Table 4: Average Treatment Effect of Progresa/Oportunidades on Marriage, by Gender

		Girls	
	All	T1998	T2000
ATT	0.0309	0.0295	0.0334)
	(0.008)	(0.009)	(0.0105)
	[0.0153 , 0.0466]	[0.0097 , 0.0493]	[0.0104 , 0.0565]
N		12208	
		Boys	
	All	T1998	T2000
ATT	0.0059	0.0049	0.0076)
	(0.0036)	(0.0034)	(0.0063)
	[-0.0013 , 0.013]	[-0.0023 , 0.0121]	[-0.0056 , 0.0208]
N		13095	

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

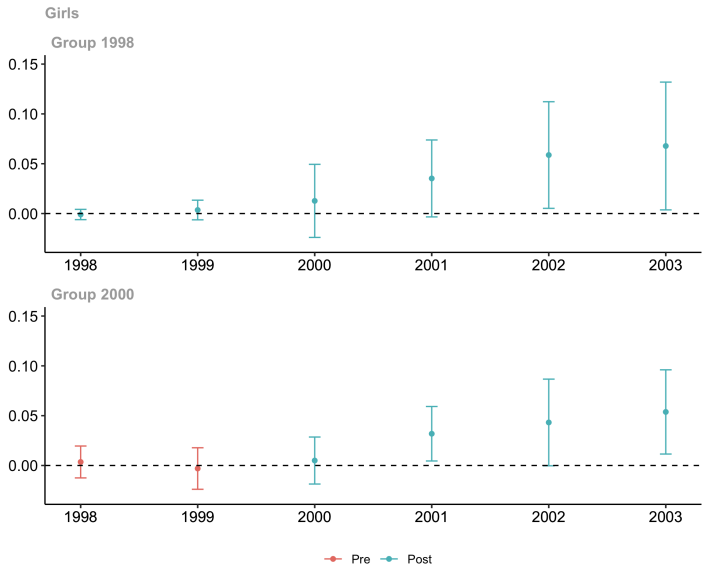
After 1 year of exposure to Progresa/Oportunidades girls are, on average, 1.4 p.p (CI=[0.0029, 0.0247]) more likely to be married if living in a beneficiary village, significant at 1%, and after 5 years the probability increases almost 7 p.p (CI=[0.0130, 0.1226]), significant at 5% (see Figure 4 and Table **??**). The point estimates are positive and increasing for both treatment groups across the years, but it is again after 2001 that they start being meaningful economically (see Figure 5 and Table A6, that show the estimate for each treatment cohort separately).

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



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

Figure 5: Effect of Progresa/Oportunidades on the Probability of Marriage by Group: Girls



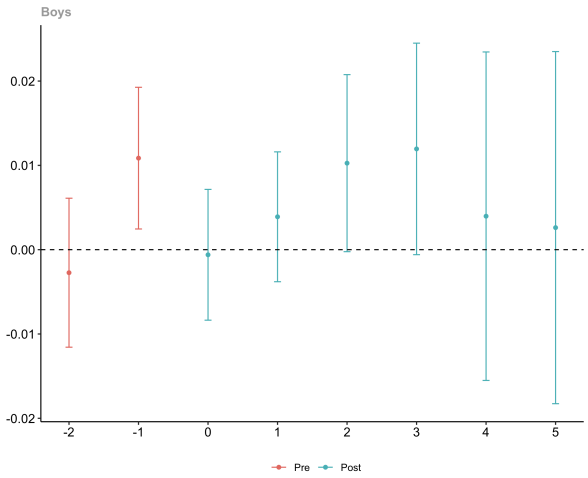
Note: This figure presents the average treatment effect on the treated by treatment group and time period. Group 1998, or T1998, is the group that first received treatment in 1998 and Group 2000, or T2000, is the group that first received treatment in 2000. In red are the estimates before treatment started,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

Results for boys, presented in Figures 6 and 7, are to be interpreted cautiously, as I cannot reject

the hypothesis of pre-trends. There seems to be a positive trend before the program started for boys in T2000, so it is plausible that there boys were already behaving differently pre-treatment and thus the post-treatment results might not be due to the program, but a product of those pre-existing differences. Despite most point estimates being positive, most are not statistically different from zero and the magnitudes are quite low. For the disaggregated results by treatment group, see Tables A7 and A8.

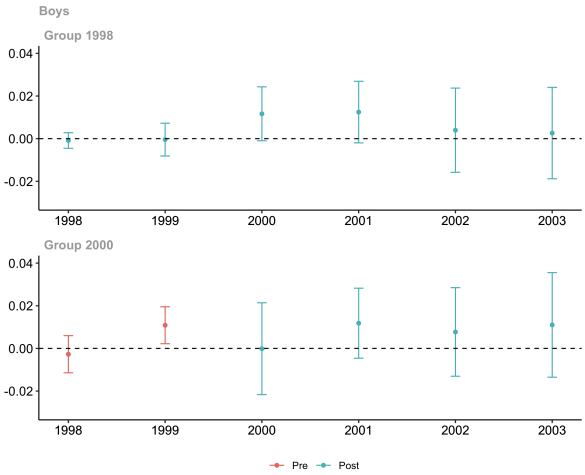
In summary, after Progresa/Oportunidades was introduced, eligible girls in beneficiary villages were more likely to be married before 18 years old when compared with eligible girls in villages that did not receive the conditional cash transfer program. The program seems to start having larger effects when benefits were extended to secondary high school. I will discuss these findings in Section 8.

Figure 6: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure: Boys



Note: This figure presents the average treatment effect on the treated by length of exposure to treatment for boys. Time -1 represents one periods prior treatment. For T1998 corresponds to 1997 and for T2000 to 1999. Period 2, on the other hand, represents two years after treatment. In red are the estimates before treatment started, and in blue after. Standard errors were obtained through clustered bootstrap, at the randomization level: locality.

Figure 7: Effect of Progresa/Oportunidades on the Probability of Marriage by Group: Boys



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

5.3 Results by Age

In this section, I look deeper at if specific age cohorts are more affected by the program than others. Given the results in the previous section suggest that the program had no impact on boy's marriage probability, I will restrict this analysis for girls.[13] I split the sample in three age groups, defined at baseline: (i) girls aged between 6 and 8 years old at baseline, (ii) girls from 9 to 11 years old, and (iii) girls from 12 to 14 years old. Recall that once an individual turns 18 she leaves my sample, therefore the last year I will observe for the oldest group is 2002, since in 2003 all of these children would have turned 18 years old. For the same reason I do not consider girls who were 15 and 16 years old at baseline since I would not have post-treatment periods for them in T2000.

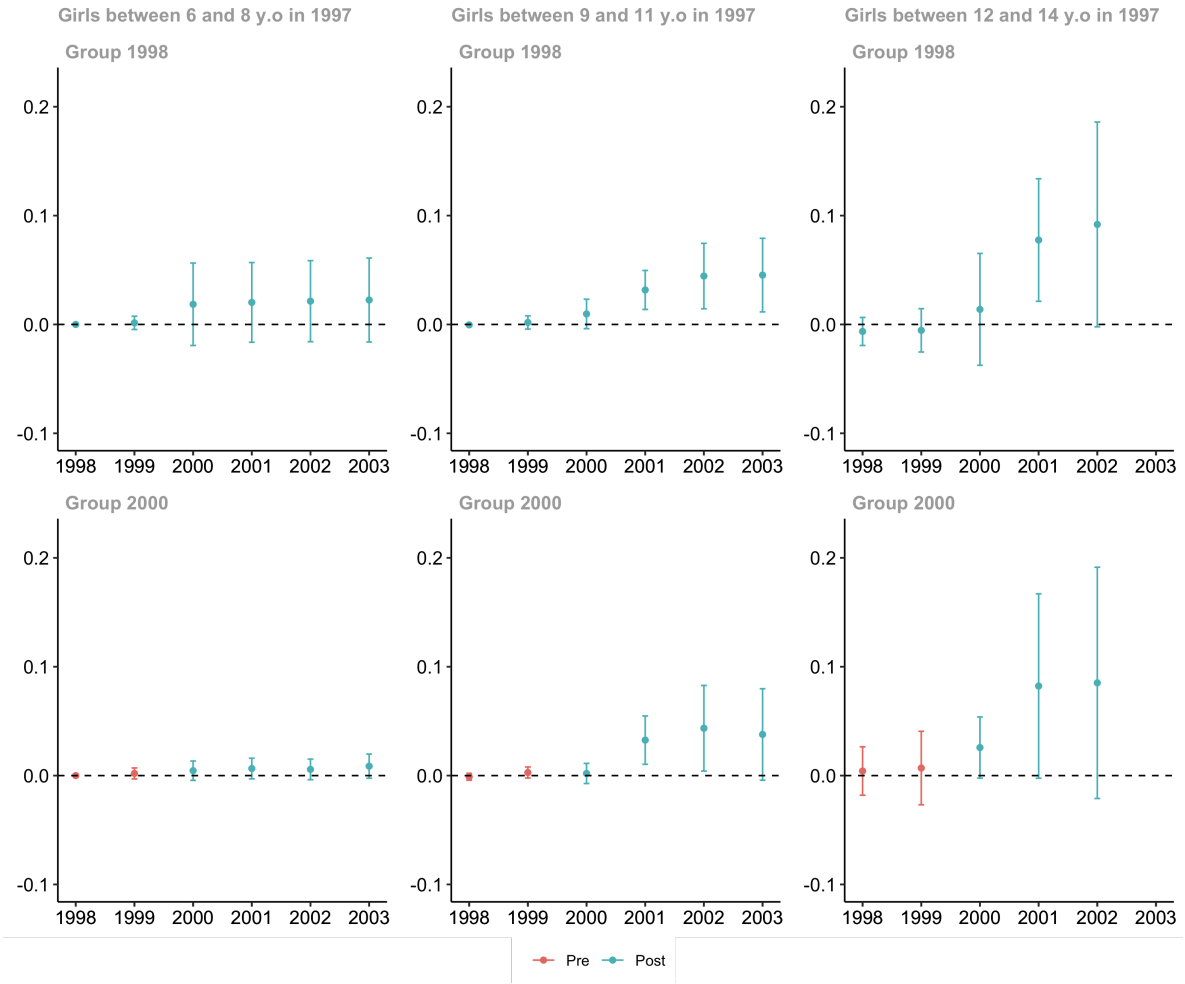
Figures 8 and **??** show the effect of Progresa/Oportunidades on early marriage separately for girls in T1998 and T2000. Note that girls in T1998 are being compared to those in T2000 until 1999 (including) and to those in the pure control group. Those in T2000 are being compared exclusively to the pure control group.

Across the three age groups and the two treatment groups we observe the same pattern as in the aggregated results. The lower number of observations in each age group make the estimates noisier, but the point estimates are consistent with what we would predict. I estimate a positive effect of the program on marriage probability across all ages and both groups, particularly after 2001, and these are increasing with age.

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

Those girls who were first exposed to the program in 1998 are all more likely to be married 5 years after the program started than their non-beneficiary counterparts. I estimate that those girls who were between 11 and 13 years old in 2002 were 2.1p.p (CI=[-0.018,0.06]) more likely to be married at that time, although not statistically significant. Those who, in 2002, were between 14 and 16, however, were 4.5p.p (CI=[0.012,0.077]) more likely to be married at that time. The effect increases to 8.5p.p (CI=[-0.026,0.197]) for those girls who were 17 years old after 5 years old program exposure. The magnitudes for T2000 are very similar for each year. This suggests that the length of exposure to the program does not seem to affect marriage decision. What seems to be relevant is having been exposed to the program and the age of the girl at that given year.

Figure 8: Effect of Progresa/Oportunidades on the Probability of Marriage: Girls, by Age



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

5.4 Age at Marriage and Marriage before 18

Another question I can answer in this context is, conditional on getting married, are people in treated villages marrying earlier than their counterparts in control villages?

In this analysis, my sample consists of all individuals who are between 6 and 16 years old in 1997, who got married. Note that these could have married after turning 18 years old and after 2003.[14]

I use a doubly robust estimator by combining both outcome regression and a model for the exposure to treatment. I calculate the probability of each individual to be in one of the treatment groups (T1998 or T2000) and weight the observations of the control group with the inverse probability of being in one of these treatment groups. The specification of the propensity score and the outcome coincide with the one detailed in the empirical strategy. Then, I estimate the effect of being in each group on the outcome of interest: age at marriage.

Results are presented on Panel A of Table 5, also split by gender. I find that the treated villages have seen a decrease of age at marriage between 0.8 (CI=[-1.184, -0.482]) and 0.9 (CI=[-1.298, -0.570])years, both statistically different from zero. The same holds when separating the analysis for girls and boys.

Table 5: Age at Marriage and Marriage Below 18

	A. Age at Marriage			B. Married before 18 y.o		
	All	**Girls**	**Boys**	**All**	**Girls**	**Boys**
T1998	-0.833	-0.829	-0.704	0.069	0.085	0.006
	(0.178)	(0.195)	(0.224)	(0.031)	(0.035)	(0.042)
T2000	-0.934	-0.787	-0.979	0.094	0.095	0.054
	(0.1284)	(0.125)	(0.258)	(0.035)	(0.039)	(0.048)
N	4232	2616	1616	4232	2616	1616

Since the interest of this paper is on child marriage, I can also investigate whether, conditional on being married, beneficiaries are more likely to marry before turning 18 years old. I keep the same strategy, changing the outcome to a dummy variably equal to 1 if married before 18 years old and 0 otherwise. Panel B of Table 5, shows that individuals in treated villages are between 7 (CI=[0.007,0 .130]) and 9 (CI=[0.025, 0.164]) p.p more likely to marry under 18 than individuals in control villages. This results is led, once again, by girls. For boys the estimates are not statistically different from zero. Conditional on being married, girls in T1998 and T2000 are 8.5 and 9.5 p.p, respectively, more likely to marry below 18 years old.

[14] As explained in the Data section, I retrieve age at marriage from a survey conducted in 2007.

6 Discussion of Results: Mechanisms

In the previous analysis, I find that the conditional cash transfer program Progresa/ Oportunidades leads to an increase in the probability of marriage for those under 18 years old. This result might be seen as surprising for one main reason. The program has been praised for its positive effects on education, which has been shown to be an important mechanism to decrease child marriage. The results of this paper suggest that despite the program having a positive effect on educational outcomes, the mechanisms through which education affects marriage decisions are not operating as theoretically predicted. In fact, Attanasio et al. (2012) states that in rural villages, in Mexico, the relationship between wages and education is flat within villages, thus showing no to low returns to education in these localities. As there are limited types of jobs, returns to education are only obtained by individuals migrating to urban areas. Even if this is the case, and education is not delaying marriage decisions, this does not explain the positive effect found in this paper.

The other component of the program that could be affecting children's marriage decision is the monetary transfer to the families. On one hand, the monetary transfer could lead children and families to rely less on marriage as a safety net since their budget constraint is comparatively more relaxed. On the other hand, the extra income in the household might be increasing the market value of beneficiary families, changing their network, and/or facilitating the formation of a new household by making marriage expenditures more affordable.

In this session I provide empirical evidence that the monetary transfer by itself leads to increases in marriage and I discuss this issue theoretically through a conceptual framework that helps rationalize these findings.

6.1 Empirical Test of the Income Effect

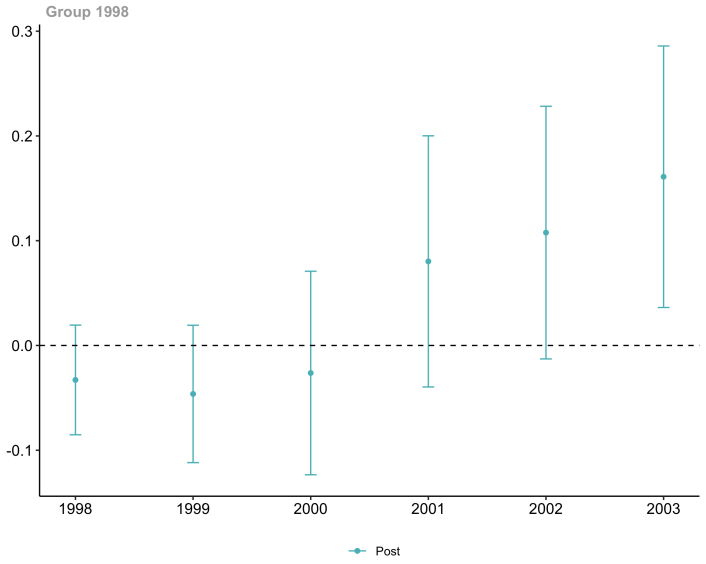
Empirically, the design of the program and the data collected do not allow us to disentangle the effect of education and income, for the entire population of interest. Data limitations on who was the beneficiary in the household, on who was receiving the benefit, or on compliance, do not allow me to investigate heterogeneous effects by compliance group, nor the intensive margin of the effect of the monetary benefit on individuals' behavior. Furthermore, because all beneficiary households who received the transfer had to comply with the conditionality, I cannot perfectly disentangle the effect of the full package from the effect of the monetary transfer itself, like Baird et al. (2011). However, I can exploit variations in household compositions to separate the two channels and show that there is an income effect on marriage.

I start by selecting a sub-sample of individuals between 6 and 16 years old at baseline that might have been exposed to the income effect only. I restrict my sample to those individuals who are not

eligible for the benefit themselves since they have completed, in 1997, the last grade of middle school or higher, but live with an eligible child. [15] My sample is made of 453 individuals, 47% of them female and 15.51 years old on average. As an example, that I will carry for the rest of the explanation, these could be older siblings who have completed middle school whose younger sibling(s) is(are) eligible for the program. Note that given the sample size, if I restrict the sample to marriages below 18 years old I do get enough variation to study this mechanism. However, I believe that for the purpose of this exercise it is enough to understand if a positive income shock leads to an increase in marriage.

Figure 9 shows the effect of a positive income shock on the probability of marriage. In the first years, I observe a negative effect of the benefit on the probability of marriage, although not statistically significant. However, from 2001 onward, I observe positive and large effects, between 10 and 18p.p increase in marriage probability, statistically significant in 2002 and 2003.

Figure 9: Effect of an Income Shock on the Probability of Marriage



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

If it was the case that the program incentivized older siblings to pursue more years of education, then I could not disentangle the two effects. However, the amount of benefit is likely not enough to compensate both the wage of the beneficiary child and its older sibling. In fact, each benefit was calculated so to compensate around two thirds of a child's wage in rural Mexico. Thus, it is unlikely that this would be a high incentive enough to compensate also for the wage of the older sibling. In fact, empirically I do not observe different levels of education between treated and control groups in

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

1997, 2000 and 2003, which is suggestive evidence of no "spillover" effects of the program to non-eligible members of the household. Therefore, we can proceed to interpret these results as the effect of a positive income transfer on the marriage.

6.2 Formalizing the Problem (work in progress)

A vast literature has shown that the introduction of Progresa/Oportunidades led to an increase in education years. In this paper, I show that it also led to an increase in child marriage, explained by the monetary transfer households received.

I show that both findings are compatible through a theoretical framework where individuals live for two periods, $t = 1, 2$ and derive utility from consumption and, if married. In period 1, individuals are in one of four states: (i) single and out of school ($\varnothing$); (ii) single and in school (S); (iii) married and in school (SM); and (iv) married and out of school (M). In each state and period individuals consume a state and time-specific endowment. If the agent is married in the first period, she cannot divorce and can only choose between being in school (SM) or not (M). If the agent is not married, she draws a potential marriage match, with quality q that follows a distribution $F(q)$ with $q \geq 0$. This quality will determine how much utility from marriage she will get. After this draw, the individual has to choose whether to get married or not and whether to go to school or not. If she is in school, independently of her marriage status, she receives a transfer $p \geq 0$ - a benefit from going to school. If she is married and also going to school, her utility from marriage is penalized by $\alpha \in (0, 1)$. One can think of this as a penalty for the effort to multitask between school and marriage.

Endowments depend on the agent's state and period, but are not path dependent. Meaning, being in a given state in period one does not necessarily affect the income of period two. In the first period all individuals who are single and out of school receive $y_1^{\varnothing}$; those who are single and in school receive y_1^{S}; those married and in school receive y_1^{SM}; and those married and out of school receive y_1^{M}. In the second period, the same logic applies. All individuals who are single and out of school receive $y_2^{\varnothing}$; those who are single and in school receive y_2^{S}; those married and in school receive y_2^{SM}; and those married and out of school receive y_2^{M}. I do not impose any constraint on the endowments, although we could hypothesize different relations between them. Consumption each period is bounded by each period's endowment and the transfer from going to school, if that is the case.

I assume that utility from consumption and marriage are additive. I also assume that utility from consumption has the standard properties: it is increasing ($u' > 0$) and concave ($u'' < 0$) and satisfies the Inada conditions; and that the utility from marriage is linear in q.

In summary, at time t:

Utility if single out of school:	$U_t^{\varnothing} = u_t(c_t^{\varnothing})$	$s.t.$	$c_t^{\varnothing} \leq y_t^{\varnothing}$
Utility if single in school:	$U_t^{S} = u(c_t^{S})$	$s.t.$	$c_t^{S} \leq y_t^{S} + p$
Utility if in school and married:	$U_t^{SM} = u(c_t^{SM}) + \alpha q$	$s.t.$	$c_t^{SM} \leq y_t^{SM} + p$
Utility if married:	$U_t^{M} = u(c_t^{M}) + q$	$s.t.$	$c_t^{M} \leq y_t^{M}$

Assuming time separability and no discounting, for generic states $x, x' \in \{\varnothing, S, SM, M\}$, the value of the problem is given by $V(x, x') = U_1^x + U_2^{x'}$.[16]

For tractability, assume that $u(.) = ln(.)$. An individual in state x in period 1 chooses the state in period 2 that yields more utility. For example, SM is chosen over S if $U_2^S < U_2^{SM} \iff ln(c_2^S) < ln(c_2^{SM}) + \alpha q$. Let $q^{S=SM}$ be the quality match that makes the agent indifferent between S and SM. If $q > q^{S=SM}$, then SM is chosen over S. Define similarly $q^{S=M}, q^{M=SM}, q^{\varnothing=M}$ and $q^{\varnothing=SM}$.

Note that given the exogenous distribution of q, this endogenous cutoff rules characterize the transition probabilities between states and the masses in each state. Then, I ask how these change as the generosity of the program p increases. Starting with the decision between marriage out of school (M) and married in school SM, it is easy to see that both $q^{M=SM}$ and $q^{S=M}$ are strictly increasing in p. An increase in the benefit from going to school is successful in increasing schooling for both single and married people. On the other hand, $q^{\varnothing=SM}$ is strictly decreasing in p, which leads to an increase in the mass of individuals that chose SM over $\varnothing$. The effect of the program on $q^{S=SM}$ is ambiguous. If $y_2^{SM} > y_2^S$, an increase in the program leads to people choosing S over SM. Albeit counter intuitive, this is a result that comes from the concavity of the utility function with respect to consumption. For large values of y_2^{SM}, an incremental increase in consumption by p will increase utility by less than in the state S. If, on the other hand, being married in school is associated with a loss of income with respect to just being in school, then the introduction of the program increases the probability of choosing SM over S. This could be the case if we think about large costs associated to marriage, such as a wedding celebration, higher expenses in the household, or setting up a new home. In this case the program would be compensating for that loss and marriage while in school would yield higher utility than being single in school.

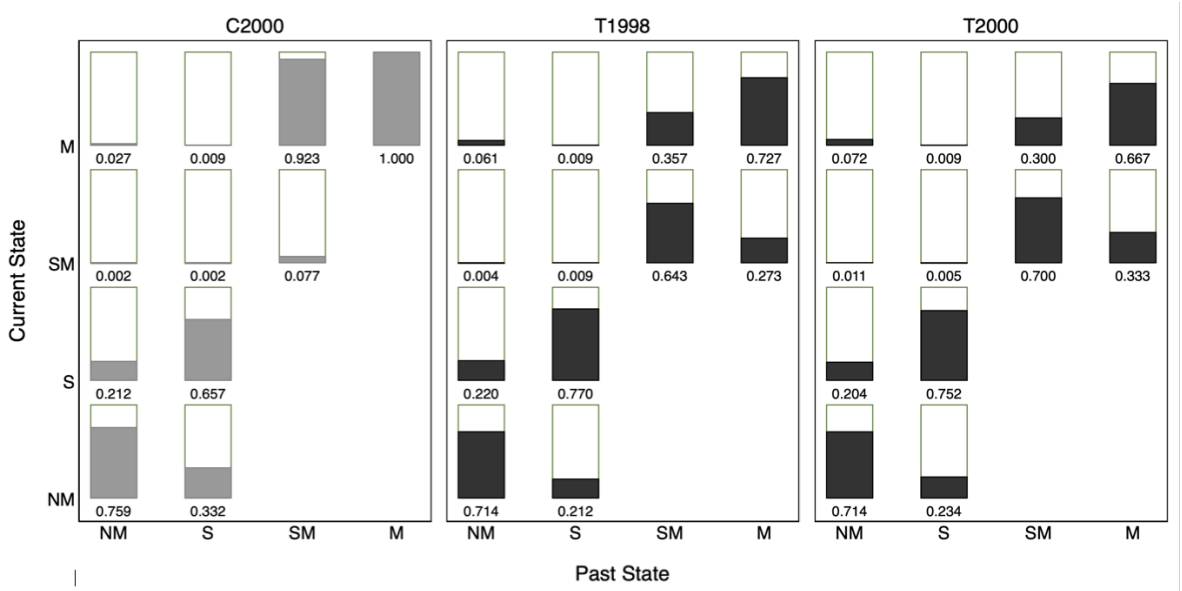
For individuals who start married in school, the program increased the probability of staying in the same state rather than moving to married and out of school. Trivially, if an individual is married and out of school in the first period, the program incentivizes children to go back to school in the second. If the girl starts single and out of school, the program increases the transition rates to single in school and married in school. It does not affect the decision of marriage out of school. Finally, if the

[16] Note that if $x = \{SM, M\}$, then $x' = \{SM, S\}$

agent starts the first period single in school, then it is less likely that she chooses to leave school single in the second period after the introduction of the program. If $y_2^{SM} > y_2^S$, then we always see more people staying in school, either married or single. Additionally, if $(1-\alpha) > \frac{c_2^S}{c_2^{SM}}$, then we see more people choosing S over SM than over M. In this case, the program is more successful at retaining children who were going to stay in school anyway rather than those who were leaving school to get married. However, if $y_2^{SM} < y_2^S$, then the program is still successful at convincing people to stay single in school rather than dropping out, but more people are getting married while staying in school.

We can start assessing the predictions of the model by looking at the transition between states in the data. Due to data limitations, I only have each individual's state for the years of 1997, 2000 and 2003. Once again I focus the analysis on girls, since the program only had effect on them and I analyze the treatment groups separately due to the difference in treatment timings. Figure **??** shows the proportion of people in a given state at t, given their past state, $t-1$, separately by treatment and control groups. NM in the graph corresponds to $\varnothing$ in the model and the other states keep the name notation. Apart from two transitions, all the others are aligned with the predictions of the model. The exceptions are the transition from $\varnothing$ to S, that the model would predict to increase and in the data we do not see virtually any difference; and the lower transition from S to M that the program should create with respect to the control group, that seems to be similar across groups. It also seems to be the case that a larger fraction of people are marrying and staying out of school under the program if they were single and out of school, compared to the control group - for which the model had no prediction. Finally, there more people in treated groups transitioned from S to SM than in the control group. The model would predict this if and only if the expected endowment from married in school in the second period was smaller than the expected endowment.

Figure 10: State Transitions by Group



Note: This figure presents the descriptive statistics of the state transitions using data in 1997, 2000 and 2003. On the horizontal axes is the past state and the vertical axes the current state. The first panel displays the transition between states for the control group, the middle one for T1998 and the right panel for T2000. NM in the graph corresponds to ∅.

7 Conclusion

In this paper I study the effect of a conditional cash transfer program in Mexico, Progresa/Oportunidades, on the probability of marriage for children under 18 years old. Beneficiary households receive a monetary transfer conditional on the school-aged children to enroll and attend school.

Leveraging the random assignment of the program at the locality level and its subsequent expansion, I study the effects of the program on marriage decisions by comparing two treatment groups that received the treatment in different times and a quasi-experimental control group. I estimate the average treatment effect on the treated using the doubly-robust estimator in a staggered differences-in-differences design proposed by Callaway and Sant'Anna (2020).

I find that the program leads to an increase in child marriage. The longer the exposure to the benefits, the higher the probability of being married under 18 years old.

Since the program was considered a success in terms of improving educational outcomes, and that education is negatively correlated with child marriage, this result might sound surprising. Theoretically, with an increase in education, the opportunity cost of marriage also increases, which leads to decreasing marriage rates and delayed marriages. However, besides the education component, the program provides a monetary transfer to beneficiary households. The relaxation of the financial constraint of the household might also affect marriage decisions, and the direction of the effect is not clear

ex-ante. It could be either leading families and children to rely less on marriage since their budget constraint is more relaxed thus decreasing its probability; or increasing the market value of beneficiary families and children and allowing or facilitating the formation of a new household. I discuss these mechanisms with a simple theoretical exercise that helps rationalizing both sets of empirical evidence and provide suggestive evidence that support the predictions of mentioned theory.

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A Appendix

Table A1: Proportion of Children Attending School Conditional on Being Married

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

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

Table A2: Proportion of Children Attending School Married by Cohort and Year

	1997	**1998**	**1999**	**2000**	**2003**
T1998	0.0018	0.0035	0.0072	0.0104	0.0061
T2000	0.0016	0.0029	0.0056	0.0097	0.0047
C2000*	0.0066	-	-	0.0002	0.0009
	P-value H0: Equal coefficients				
T1998 VS T2000	0.673	0.44	0.1772	0.6446	0.2546
T1998 VS C2000*	0.0002	-	-	0	0
T2000 VS C2000*	0.0002	-	-	0	0.0002

Note: This table shows the proportion of children who attend school and are married by cohort and year. Individuals of the treatment groups T1998 and T2000 receive weight 1 and individuals in the control group C2000* receive a weight that is defines as $\frac{p(x)}{1-p(x)}$, where $p(x)$ is the probability of being in either T1998 or T2000.

Table A3: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure

Event-Time	ATT(t)	Std. Error	Conf. Interval
-2	2e-04	0.0027	[-0.0066 , 0.007]
-1	0.0039	0.0035	[-0.0049 , 0.0126]
0	3e-04	0.0017	[-0.004 , 0.0046]
1	0.008	0.0025	[0.0018 , 0.0141]
2	0.0158	0.0062	[4e-04 , 0.0312]
3	0.0248	0.0057	[0.0107 , 0.0389]
4	0.0316	0.0083	[0.0109 , 0.0523]
5	0.0326	0.0096	[0.0086 , 0.0566]
N		25304	

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

Table A4: Effect of Progresa/Oportunidades on the Probability of Marriage by Group and Year

	Time	ATT(g,t)	Std. Error	Conf. Interval
T 1998	1998	-8e-04	0.0013	[-0.0041 , 0.0026]
T 1998	1999	0.0017	0.0024	[-0.0047 , 0.0081]
T 1998	2000	0.0118	0.0073	[-0.0076 , 0.0312]
T 1998	2001	0.0226	0.0077	[0.0023 , 0.043]
T 1998	2002	0.0316	0.0082	[0.0097 , 0.0535]
T 1998	2003	0.0326	0.01	[0.006 , 0.0592]
T 2000	1998	2e-04	0.0028	[-0.0071 , 0.0075]
T 2000	1999	0.0039	0.0035	[-0.0053 , 0.013]
T 2000	2000	0.0022	0.005	[-0.0112 , 0.0156]
T 2000	2001	0.0192	0.0062	[0.0029 , 0.0356]
T 2000	2002	0.0231	0.0084	[8e-04 , 0.0454]
T 2000	2003	0.0288	0.0083	[0.0066 , 0.051]
N			25304	

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

Table A5: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure: Girls

Event-Time	ATT(t)	Std. Error	Conf. Interval
-2	0.0035	0.0061	[-0.0124 , 0.0195]
-1	-0.003	0.0079	[-0.0234 , 0.0174]
0	0.0012	0.0033	[-0.0073 , 0.0097]
1	0.0138	0.0042	[0.0028 , 0.0248]
2	0.0237	0.011	[-0.005 , 0.0524]
3	0.0419	0.0108	[0.0139 , 0.0699]
4	0.0587	0.0204	[0.0057 , 0.1117]
5	0.0678	0.0268	[-0.002 , 0.1376]
N		12208	

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

Table A6: Effect of Progresa/Oportunidades on the Probability of Marriage by Group and Year: Girls

	Time	ATT(g,t)	Std. Error	Conf. Interval
T 1998	1998	-0.001	0.002	[-0.0061 , 0.0042]
T 1998	1999	0.0035	0.0037	[-0.0063 , 0.0134]
T 1998	2000	0.0127	0.0139	[-0.024 , 0.0494]
T 1998	2001	0.0352	0.0146	[-0.0034 , 0.0738]
T 1998	2002	0.0587	0.0203	[0.0052 , 0.1122]
T 1998	2003	0.0678	0.0243	[0.0037 , 0.1319]
T 2000	1998	0.0035	0.0061	[-0.0125 , 0.0195]
T 2000	1999	-0.003	0.0079	[-0.0239 , 0.0178]
T 2000	2000	0.005	0.0089	[-0.0186 , 0.0286]
T 2000	2001	0.0319	0.0104	[0.0045 , 0.0592]
T 2000	2002	0.0432	0.0165	[-3e-04 , 0.0867]
T 2000	2003	0.0538	0.016	[0.0115 , 0.096]
N			12208	

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

Table A7: Effect of Progresa/Oportunidades on the Probability of Marriage by Length of Exposure: Boys

Event-Time	ATT(t)	Std. Error	Conf. Interval
-2	-0.0027	0.0034	[-0.0116 , 0.0061]
-1	0.0109	0.0032	[0.0025 , 0.0193]
0	-6e-04	0.003	[-0.0084 , 0.0072]
1	0.0039	0.003	[-0.0038 , 0.0116]
2	0.0103	0.0041	[-2e-04 , 0.0208]
3	0.012	0.0048	[-6e-04 , 0.0245]
4	0.004	0.0075	[-0.0155 , 0.0235]
5	0.0026	0.0081	[-0.0183 , 0.0235]
N		13095	

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

Table A8: Effect of Progresa/Oportunidades on the Probability of Marriage by Group and Year: Boys

	Time	ATT(g,t)	Std. Error	Conf. Interval
T 1998	1998	-9e-04	0.0014	[-0.0045 , 0.0028]
T 1998	1999	-4e-04	0.003	[-0.0081 , 0.0073]
T 1998	2000	0.0117	0.005	[-0.001 , 0.0243]
T 1998	2001	0.0125	0.0057	[-0.002 , 0.0269]
T 1998	2002	0.004	0.0077	[-0.0158 , 0.0237]
T 1998	2003	0.0026	0.0084	[-0.0188 , 0.024]
T 2000	1998	-0.0027	0.0034	[-0.0114 , 0.006]
T 2000	1999	0.0109	0.0034	[0.0022 , 0.0195]
T 2000	2000	-1e-04	0.0085	[-0.0217 , 0.0214]
T 2000	2001	0.0118	0.0064	[-0.0046 , 0.0282]
T 2000	2002	0.0077	0.0082	[-0.0131 , 0.0285]
T 2000	2003	0.011	0.0096	[-0.0135 , 0.0355]
N			13095	

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