Teenage Pregnancy in Mexico: Evolution and Consequences

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Teenage Pregnancy in Mexico: Evolution and Consequences

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Abstract

We analyze the consequences of a teenage pregnancy event in the short- and long-run in Mexico. Using longitudinal and cross-section data, we match females who got pregnant and those that did not based on a propensity score. In the short-run, we find that a teenage pregnancy causes a decrease of 0.6-0.8 years of schooling, lower attendance to school, less hours of work and a higher marriage rate. In the long-run, we find a loss in years of education of 1-1.2, which implies a permanent effect on education, and that household income per capita is lower.

JEL: I00; J10; J11; O54.

Keywords: Teenage pregnancy; Schooling; Labor outcomes; Propensity score; Matching.

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1 Introduction

According to Geronimus and Korenman (1992), "teenage childbearing has been described as a cause of persistent poverty, and poverty that is transmitted intergenerationally" (p. 1187). As the event of teenage pregnancy may lead to an intergenerational cycle of poverty, the causes and consequences of teenage childbearing have been widely studied among social scientists (see or example the book edited by Hoffman and Maynard, 2008, for an analysis in the United States and the book by Stern, 2012, for a sociological analysis in the Mexican case). However, most of the literature in the topic estimates associations or correlations of teenage pregnancy and socioeconomic outcomes and most of the international literature focuses in developed countries.

In this paper, we try to fill this void in the literature by analyzing the Mexican case. This is important because teenage mothers are far more common in Mexico than in the United States or other developed countries. According to World Bank data, in Mexico 69 out of every 1000 adolescents between 15 and 19 years old have children, whereas in the United States only 36 per thousand do. As compared to other countries in Latin America with similar development, Mexico's teenage childbearing rates are just above average: Brazil has a rate of 76 per 1,000 women, but Argentina and Chile have rates of 56 and 57 per thousand, respectively. Pantelides (2004) reviews the evolution of the phenomenon in Latin America. She points out that these rates have not shown any significant decreases in the last decades.

In order to disentangle the causal effect of teenage childbearing on several socioeconomic outcomes, we match females who got pregnant during their adolescence to those who did not based on a propensity score. In other words, using several observable characteristics we are able to compare very similar individuals with the only difference being the pregnancy event. We find substantial evidence that there is balance and common support between treatment and control groups after matching. Our analysis focuses on both short- and long-run outcomes. We find that the single most important effect of teenage childbearing is to lower the educational attainment of females by 0.6 to 0.8 years in the short-run. Most importantly, we present evidence that this effect is permanent: our long-run estimates suggest a loss of between 1 to 1.2 years of schooling. There does not seem to be any short-run effect on the household labor supply or household income per capita. However, and most likely due to their lower educational attainment, we find that in the long-run teenage mothers live in households with lower income per capita as compared to females with a child-free adolescence.

The estimation of the causal effects of teenage childbearing has proven to be very elusive. The main empirical challenge in the estimation of that causal effect is that teen mothers are systematically different than child-free adolescents. This selection bias suggests that even in the absence of a child, those females who ended up raising a child during their teenage years would have had a lower socioeconomic status than those females who lived a child-free adolescence. The literature presents several approaches to identify the causal effect of teenage childbearing in the case of the United States. For instance, Bronars and Grogger (1994) analyze the effect out-of-wedlock motherhood by comparing twin first births to single first births using a couple of censuses. Despite that teenage mothers tend to be unwed, this identification strategy seems to answer a different empirical question: it estimates the effect of having an additional child in the first birth of single women rather than the effect of the first birth of single women (independently of whether it was a multiple birth or not).

Other more successful approaches have been used. Geronimus and Korenman (1992) compare teen mothers to their childless sisters using several longitudinal surveys, and as a result, they are able to remove the unobserved heterogeneity coming from family background. Hotz, McElroy and Sander (2005), and Ashcraft and Lang (2006) used miscarriages as an instrumental variable of birth delays. In this way, they estimate the causal effect of age at first birth on several socioeconomic outcomes. Hotz, McElroy and Sander (2005) find statistically significant positive effects on the probability of getting a General Educational Development degree (GED), hours of work and wage. In contrast, Ashcraft and Lang (2006) find adverse, but modest effects. Finally, Levine and Painter (2003) implement propensity score matching within school attended by treatment and control teenagers in the United States. They find that teenage mothers are 20 percent less likely to graduate from high school. Similarly, Chevalier and Viitanen (2003) estimate a propensity score matching using data from Great Britain. They also find adverse effects of teenage childbearing on schooling attainment, labor market experience, and wages in adulthood.

In our view, the evidence on the consequences of teenage pregnancy is more limited for developing countries than for developed countries.[1] Ferre, Gerstenblüth, Rossi, and Triunfo (2009) estimate the impact of childbearing only on educational outcomes using matching methods. These authors work with a cross-section with no retrospective data. As a consequence, they are only able to match females on a very limited number of observable characteristics. Kruger, Berthelon, and Navia (2009) study the effect of teenage pregnancies on high-school completion using an instrumental variable strategy. The instruments they use reflect the society's and household tolerance for teenage births. In order to measure social acceptance they estimated the proportion of teenagers in the county who gave birth and the average county rate of unwed births; and to measure household tolerance they use a dummy of whether the mother also had a teenage pregnancy. As for the first set of instruments, we doubt that they meet the exclusion restriction because social acceptance of teenage births may reflect preferences for gender roles, which in turn affect educational attainment. The same is also true for the measure of household tolerance: if having a teen birth reduces the probability of high school completion, the same is true for the teen mother's mother; hence, high school completion of the teen today is affected through the intergenerational transmission of educational attainment.[2] The paper more similar to ours is Ranchhod, Lam, Leibbrandt, and Marteleto (2011) who use the Cape Area Panel Study to estimate propensity score weighted regression. They find a negative effect of a teenage birth on educational

[1] Another strand of the literature focuses on the determinants of a teenage pregnancy and other risky behaviors. For literature on developing countries, see for instance Blunch (2011) on Ghana; Cardoso and Verner (2007) on Brazil; Marteleto, Lam, and Ranchhod (2008) on South Africa.

[2] For instance, Navarro Paniagua and Walker (2010) find that children of teenage mothers have lower educational attainment and are more likely to be teenage mothers themselves in Europe.

attainment, but the effect tends to diminish over time suggesting that teenage moms catch up with childless teenagers. Unlike us, Ranchhod, Lam, Leibbrandt, and Marteleto (2011) do not exploit the longitudinal nature of their data by estimating a difference-in-difference estimator. None of the studies cited above contrast the short- and long-run effects of teenage births as we do in this paper.

In the case of Mexico, most of the studies analyze the association of pregnancy with outcomes, but lack a clear control group to measure the impact of teenage pregnancy in later outcomes. For example, Stern (2012) makes an excellent sociological review of the evolution of teenage pregnancy in Mexico. Using qualitative work, Stern (2007) finds that teenage pregnancy occurs in stable couples, and it is not due to random encounters. Echarri Cánovas and Pérez Amador (2007) construct event histories of teenagers. They find that events like dropping out of school, first consensual union, and leaving the parental home occur before the childbearing event. Menkes and Suárez (2003) find that a low schooling level is associated with lower contraceptive knowledge and with a lower age in the first sexual encounter. These two factors in turn lead to a higher propensity of lower educated women to become pregnant during adolescence. Menkes and Serrano (2010) find that women in poor families have higher rates of teenage pregnancy. Although those studies are relevant and important to increase our understanding of the teenage pregnancy phenomenon, they only estimate associations of the pregnancy event with different outcomes. Those studies also point out that teenage women with a pregnancy event are very different to women without the event. Hence, in order to estimate the causal impact of teenage pregnancy on outcomes like education, income and work, we apply a novel strategy to the Mexican case in order to compare similar women in terms of observable characteristics.

Our identification strategy follows Levine and Painter (2003), and Chevalier and Viitanen (2003) in the sense that we match females who became mothers during adolescence to females who lived a child-free adolescence based on a propensity score. Due to data limitations, we are not able to match females within schools or families. However, we exploit two different databases to be able to estimate short- and long-run causal effects. For the short-run effects we use the Mexican Family Life Survey (MxFLS), which is a longitudinal survey with currently two waves publicly available (2002 and 2005). For the long-run effects, we use the 2011 Social Mobility Survey (EMOVI for its acronym in Spanish), which is a cross-section with socioeconomic information of the individuals when they were 14 years old.

Our results show that the most important effect of teenage childbearing is a permanent lower educational attainment of the teenage mother. As a result, we find that in the long-run the households of those females who had their first child as teenagers tend to have lower income per capita. We also find that in the short-run, teenage mothers reduce their college attendance (hence the lower educational attainment), and their labor supply. Finally, and in contrast with the literature in the United States, we find that having a child during adolescence has a positive effect on the probability of being married. This difference is most likely a result of cultural differences between Mexico and the United States.

The remainder of the paper is organized as follows. Section 2 shows the aggregate trends in teenage childbearing in Mexico. Section 3 describes the sources of data used in this paper and presents some descriptive statistics. Section 4 explains the empirical strategy that we implement. Then Section, 5 presents the estimates of the short- and long-run causal effects, and Section 6 concludes and discusses some policy implications.

2 Aggregate Trends

In this section we discuss the aggregate trends on teenage births. The data of this section comes from The World Bank, the Mexican Census of Population (1990, 2000 and 2010), and administrative birth records.[3] Figure 1 Panel A shows the number of births per 1,000 women among teenagers aged 15-19 in 2009 for a sample of Latin American countries. The unweighted average number of births per 1,000 women for this sample of countries is 75.8,

[3] Census data provides information on the stock of childbearing women. Our results are very similar to those presented in Menkes and Serrano (2010), even though they use a different survey.

whereas Mexico has a rate equal to 68.6. Among those 18 countries, Mexico has the 5th lowest rate in the number of births per 1,000 women after Argentina, Chile, Costa Rica, Peru and Uruguay. However, using the same data source for all available countries results in an unweighted world average of 50 births per 1,000 women. Hence, although Mexico shows a slightly lower teenage pregnancy rate as compared to other Latin American countries, its rate is still larger than that of the rest of the world. Panel B shows the evolution of number of births per 1,000 women among teenagers using administrative records.[4] The number of births per 1,000 women shows a decline from 1990 to 1997, then presents a relatively stable path from 1998-2006 at around 65 births per 1,000 women, and finally it increases for the period 2007-2008 to almost 70 births per 1,000 women.

Panel A in Figure 2 exhibits the fraction of births from teenage mothers out of total births. The percent of births among teenage mothers is stable at around 16 percent. In contrast, the percent of births from single mothers among all births from teenage mothers has increased in the period. As a result, the proportion of births to married women or women cohabitating has decreased. These findings could be a result of a lower marriage rate triggered by teen pregnancies or a higher age at first marriage that results in less married teen mothers. Also, Panel B shows that while in 1985 a teenage mother was more likely to have a primary degree or less (less or equal to 6 years of schooling); by 2002 that changed, and a teenage mother became more likely to have a secondary degree (9 to 11 years of schooling). This last finding could be a result of higher educational achievement, and not necessarily to a decrease in the teen childbearing rate for those with primary schooling or less.

Table 1 shows statistics for females aged 15-19 years old in Mexico for the period 1990-

[4] The administrative birth records are published by the Statistical Institute in Mexico (INEGI) and the Ministry of Health. The data includes all births registered in order to get a birth certificate. The administrative records include age of mother at birth, education, marital status and location of birth (county and state). We use these records in order to give a broad picture on the evolution of teenage pregnancies. Data can be downloaded from INEGI, http://www.inegi.org.mx/ and Ministry of Health, http://www.sinais.salud.gob.mx/basesdedatos/index.html. We use information of the year of birth not year of birth registry. To calculate a series without the problem of right-censoring (births that ocurred in the past can be registered at any time in the future), we restrict to births registered only in the same and the following year to birth ocurrence. This represents approximately to 93 percent of births.

2010 using Census data.[5] The first three columns show the proportion of each group in the population and the last three columns show the percent of women in that age group with at least one child born alive. The table shows that the percent living in rural areas (less than 2,500 inhabitants) has been relatively constant at 25 percent. On the other hand, education and attendance to school has improved for the period of study. An interesting fact is that the proportion of single females is stable at 82 percent and the proportion either married or cohabitating is stable at 16-17 percent. However, the percent of females that are married has decreased substantially over time from 10.8 percent in 1990 to 4.7 percent in 2010. At the same time, the percent of females that are cohabitating has increased from 5.8 percent in 1990 to 11.7 percent in 2010.

When one looks at childbearing teenagers only (Columns 4 to 6 in the table), we find that the percent of females with at least one child born alive has increased from 12.3 percent in 1990 to 13 percent in 2010. The increase in childbearing rates is mostly within the urban sector, as females in the rural sector have become less likely to be teenage mothers. Within education groups, the highest childbearing rate is among women with primary or less (less than 8 years of schooling). Hence, the trends shown in Figure 2 are a result of a higher school attainment over time. However, the rate is slightly decreasing for the group of women with primary education and increasing for women with more education like secondary (9-11 years of schooling) or more than secondary (more than 12 years of schooling). In terms of school attendance, if a woman is attending school the probability that the woman is childbearing is small. When we distinguish by marital status we find that the childbearing rate is very small (1.3-2.5 percent) among single women, although it has doubled for the period 1990-2010. In Mexico, childbearing is associated with marriage or cohabitation.[6] Moreover, the childbearing rate among this group has been stable over time, which points out that the increase in childbearing has been borne by single women.

[5] Census data is available at the Statistical Institute in Mexico (INEGI) http://www.inegi.org.mx.

[6] Census data cannot disentangle the timing of the events. We cannot know whether pregnancy occurred before either marriage or cohabitation took place.

3 Data and Descriptive Statistics

We are interested in the effects of teenage pregnancy on individual outcomes of the teenage mother and also on family outcomes. Most of the previous literature has focused on short-run outcomes given data availability. In this paper, we attempt to measure the consequences of teenage pregnancy both in the short- and long-run. For the short-run analysis, we use the Mexican Family Life Survey (MxFLS) for the period 2002-2005.[7] The MxFLS is a nationally representative longitudinal survey. In the baseline year, the MxFLS interviewed to 8,440 households and approximately 35,000 individuals. The follow-up survey was collected in years between 2005 and 2006 with an attrition rate of approximately 10 percent at the household level. The survey includes information on demographics, work, and health.

In the short run analysis, we restrict the MxFLS data to females aged 14 to 18 in 2002 who are childless and not pregnant. Moreover, we restrict the sample to females who are not married or cohabitating in 2002. Then, we follow those females into the 2005 survey. Hence, we are interested in females who got pregnant between 2002-2005 while being a teenager, which represents the treatment variable. Under these restrictions, the final dataset includes 1,003 females with 131 observations in the treatment group. The teenage pregnancy rate is around 13 percent in our sample, which is similar to our findings in the previous section. Due to the small sample size, we do not focus on teenage out-of-wedlock childbearing specifically, but we do present some results in the extensions section.[8] The variables in the analysis include age, years of schooling, indicators of school attendance, work status, indigenous language, knowledge of contraceptives, previous sex life, born in rural areas (less than 2,500 inhabitants), and father absent in the household. We also use information about the head of the household: age, years of schooling, indicators of female and work status. Finally, we use variables at the household level: household size; number of members between ages 0 to 5, 6 to 18, and more than 65; average hours of work for members older than 18; average age;

[7] Data available at http://www.ennvih-mxfls.org.

[8] We include cohabitation in the definition of marriage.

income per capita; number of rooms; and characteristics of the dwelling (assets ownership).

In order to measure long-run impacts, we use the 2011 Social Mobility Survey (EMOVI, for its acronym in Spanish).[9] This survey is representative at the national level for both males and females in the ages between 25 and 64 years old. The main goal of the survey is to estimate intergenerational mobility. The survey not only records current characteristics, but also asks characteristics of the household of origin when the individual was 14 years old. For example, the survey asks education of both parents and characteristics of the dwelling. The survey includes a question on the age of the individual when he or she had his or her first child. Hence, we define the treatment variable as females who had their first child when they were 15-19 years old. We do not include teenagers who got pregnant when they were 14 years old in order to have pre-treatment characteristics of the household of origin. In this way, we can capture long-run effects because, for example, we can analyze outcomes of females from 6 to 45 years after the teenage pregnancy.

Table 2 presents some descriptive statistics for both samples. The MxFLS sample is restricted to the baseline year. Age is relatively similar across samples. In the MxFLS, females who got pregnant between 2002 and 2005 had lower education than other females, however the difference is not statistically significant at the 5 percent level. On the other hand, women in the treatment group had lower school attendance levels and were more likely to work before the pregnancy event. In the case of EMOVI, schooling and proportion working refer to current outcomes. They show that women after a teenage pregnancy have lower schooling and lower probability of be employed than women without a teenage pregnancy. The following rows show that women who got pregnant come from a more disadvantaged background measured by years of schooling of the head of the household (MxFLS) or parents (EMOVI). Also, in the case of the MxFLS, women who got pregnant were already more sexually active than women in the control group. These results show the importance of controlling for selection bias.

[9] For more information visit http://www.ceey.org.mx.

4 Empirical Strategy

Our goal in the paper is to estimate the causal effect of teenage pregnancy on outcome variables like years of schooling, school attendance, working status, and marriage status. The ideal experiment would be to randomly assign pregnancies to teenagers (treatment), and then compare the outcomes. Obviously, that experiment is unethical and unfeasible. Define Y_{1i} as the potential outcome in the treatment state and Y_{0i} as the potential outcome in the control state for individual i. Define treatment as $D_i = 1$. The parameter of interest is the Average Treatment on the Treated (ATT) defined as the mean difference in outcome variables given treatment, $ATT = \mathbb{E}[Y_{1i} - Y_{0i}|D_i = 1]$. However, the term cannot be estimated given that it is not possible to observe the same individual in the treatment and the control group at the same time. This is the "fundamental problem of causal inference" (Holland, 1986). The problem is that the term $\mathbb{E}[Y_{0i}|D_i = 1]$ is not observed and has to be estimated (from this point forward we will omit the subscript i for notational simplicity).

We rely on the assumption of selection on observables in order to construct a valid counterfactual. In particular, we assume that conditioning on observable characteristics before the treatment occurs removes differences in the untreated state between teenagers who got pregnant and those who did not. In other words, we assume that $(Y_0 \perp D)|X$, which is commonly referred in the literature as the Conditional Independence Assumption (CIA) or the unconfoundedness assumption. This assumption means that the outcome for teenagers who did not get pregnant (untreated state), for example years of schooling, is independent of treatment conditional on observable characteristics.

In order to identify the ATT, the common support also needs to hold, $\Pr(D = 1|X) < 1$. This assumption means that for every X there are individuals who do not get the treatment. Ideally, we would like to match individuals in the treatment and control groups within cells of observable characteristics. However, this is not possible due to the "multidimensionality problem". In order to solve for this issue, Rosenbaum and Rubin (1983) propose to estimate propensity scores. This is easily estimated using a logit or probit of the probability of treatment on observable characteristics, $\Pr(D = 1|X) = P(X)$. Rosenbaum and Rubin (1983) show that under the CIA:

$$(Y_0 \perp D)|X \Longrightarrow (Y_0 \perp D)|P(X) \tag{1}$$

Instead of comparing treatment and control groups within the same set of X, we compare individuals based on an index that summarizes the observable characteristics information. If the assumptions of the model are satisfied, the ATT using a propensity score is estimated as:

$$\theta_{ATT}^{PSM} = \mathbb{E}_{P(X)|D=1}\left\{\mathbb{E}\left[Y_1|D=1, P(X)\right] - \mathbb{E}\left[Y_0|D=0, P(X)\right]\right\} \tag{2}$$

The ATT is just the difference in mean outcomes for treated individuals and mean outcomes of individuals in the control group but reweighted or readjusted by the propensity score, $P(X)$, such that they are as similar as possible to the treatment group in the common support region. In other words, we estimate the impact of a pregnancy event comparing very similar individuals in terms of observable characteristics.

We estimate the causal impact of teenage pregnancy in the short- and long-run. For the long-run estimates, we apply equation (2). For the short-run estimates, we can improve our estimates by taking advantage of the panel structure of the data. If there is unobserved heterogeneity that is fixed over time for individuals in the sample, then we can eliminate this bias by estimating difference-in-difference effects:

$$\theta_{ATT}^{PSM} = \mathbb{E}_{P(X)|D=1}\left(\begin{array}{c}\left\{\mathbb{E}\left[Y_{t1}|D=1\right] - \mathbb{E}\left[Y_{t0}|D=1\right]\right\} - \\ \left\{\mathbb{E}\left[Y_{t1}|D=0, P(X)\right] - \mathbb{E}\left[Y_{t0}|D=0, P(X)\right]\right\}\end{array}\right) \tag{3}$$

Before estimating the ATT, three key aspects need to be considered. First, it is important to question the conditional independence assumption. Of course, the assumption is untestable, but we do have possible checks to investigate whether the assumption is likely to hold. Second, there are no strict rules as to what variables should be included in the propensity score estimation (Caliendo and Kopeinig, 2008). Third, it is possible that the ATT is sensitive to the matching method (Smith and Todd, 2005).

The main assumption of matching on the propensity score is that observable characteristics are balanced between the treatment and control groups. In other words, within some specified values of the propensity score there should be no differences in observable characteristics between the treatment and control group. If there are differences in observable characteristics, then it is likely that there are differences in unobservable characteristics making the estimation of the ATT unfeasible. Below, we present different tests in order to provide evidence of balance in the propensity score.

One of the main advantages of the propensity score is that the information of all observable characteristics is summarized in a single index. There is a trade-off of bias versus efficiency on the number of explanatory variables. On the one hand, Caliendo and Kopeinig (2008), Dehejia and Wahba (1999, 2002) and Heckman, Ichimura, and Todd (1997) mention that omitting important variables that determine treatment could bias the ATT estimate. On the other hand, Bryson, Dorsett, and Purdon (2002) mention that including irrelevant variables increases the variance of the ATT estimate. Moreover, the assumption of balance needs to hold not only for linear terms but also for non-linear terms. This implies that the propensity score may include interactions and higher order terms (Dehejia and Wahba, 1999, 2002). This could potentially increase the variance in the ATT estimate. Instead of relying on statistical significance of observable characteristics on the propensity score, we include variables in order to achieve balance. Nonetheless, in the robustness checks section we compare models with variations in the set of observable characteristics included in the propensity score estimation in order to compare the ATT and its standard errors.

Smith and Todd (2005) show that the ATT estimate may be sensitive to the matching method. Also, Heckman, Ichimura, and Todd (1997) suggest that the matching may be done on the log odds ratio ($\log \frac{P(X)}{1-P(X)}$) instead of on the propensity score $P(X)$. This is especially recommended when there is choice-based sampling in the survey. We include both recommendations in our analysis.

5 Results[10]

As previous literature has pointed out that the ATT may differ depending on the matching method, we present our results for three different matching methods: (1) matching with a kernel Epanechnikov and a bandwidth of 0.01; (2) matching to the three nearest neighbors within a radius of 0.01; and (3) in order to restrict even further the comparison group, we match treatment and control individuals within urban/rural, age and school attendance status (in the case of the long-run estimates we only restrict to urban/rural and age). We also present the results using other matching methods as a robustness check.

The main results are presented using a propensity score that includes linear, squares and interaction terms. The model using the MxFLS uses 86 variables and the model using EMOVI employs 57 variables.[11] The robustness section includes results for different specifications of the propensity score. Also, we present robustness checks using the log odds ratio as the matching score instead of the propensity score. In general, our results are stable across specifications and matching methods. We calculate standard errors of the ATT using

[10] All our matching results use the ado-file psmatch2 in Stata provided by Leuven and Sianesi (2003). We employ a logistic regression to estimate the propensity score.

[11] MxFLS 2002: age, years of schooling, school attendance, work status, indigenous language, knowledge of contraceptives, previous sex life, rural status, and father absent in the household. The variables included about the head of the household are: years of education, age, female, and work status. We also include household size; and members 0-5, 6-18, and more than 65; average hours worked in the household; mean age and income per capita of the household; number of rooms in the household; and several dummies for assets in the household such as indicator variables for without vehicle, without stove, without public water and without sewage. We also include 51 interactions terms between individual variables (age, schooling, work, indigenous, knowledge of contraceptives and previous sex life) and household variables and squares of age and years of schooling. We include 57 variables in the estimation of the propensity score for EMOVI: age and age squared, born in rural areas, and information about both parents when individual was 14 years old, namely: education, work status, formal sector job, indigenous language, and what parent the individual was living with. The variables included about the household are: number of siblings, household size, number of rooms and cars, assets in the household like without stove, without washing machine, without refrigerator, without television, without public water, without sewage, and without electricity. Finally, we include interactions of individual variables with household characteristics as well as squares and interactions of years of education of both parents, and work status of both parents.

500 bootstrap replications.

5.1 Balance of the propensity score

We estimate different tests to corroborate balance in the propensity score. First, we provide graphical evidence based on results by Dehejia and Wahba (1999, 2002) before and after matching to corroborate the balancing and the common support assumptions. We also include the stratification test before and after matching proposed by Dehejia and Wahba (1999, 2002).[12] Second, we include the standardized bias measure proposed by Rosenbaum and Rubin (1985) before and after matching.[13] We report only the median standardized bias. According to Caliendo and Kopeinig (2008), a median standardized bias less than 5% is "sufficient". Third, as proposed by Sianesi (2004), we report the p-value of the joint significance test of the propensity score model before and after matching.[14] Fourth, we report the percent of variables that fail to reject the null hypothesis of equal means before and after matching. Finally, we report the number of observations in the treatment and control for each matching method. With all these tests, we aim to provide evidence in favor of the balancing and common support assumptions.

Figure 3 shows box plots and histograms before and after matching. To present the results, we use 3 nearest neighbors within a radius of 0.01. The figure includes the results both for the MxFLS and EMOVI. The figure shows that even before matching, the treatment and control groups are not substantially different. Before matching in the MxFLS (Panel A), the mean value of the propensity score for the control group is approximately 0.10 and

[12] However, they only present the stratification test before matching. We believe is also informative to present the result of the test after matching. The stratification test relies on dividing observations in the treatment and control in quintiles or deciles. Then, within each quintile or decile employ t-tests for difference in means between treatment and control groups. If we have 10 variables and 5 quintiles, we have 50 tests. We report the percent of tests out of total tests that fail to reject the null of equal means. Dehejia and Wahba (1999, 2002) point out that this test can be used to select the variables included in the propensity score.

[13] The Standardized Bias (SB) is defined as $100 \times \frac{\overline{X}_1 - \overline{X}_0}{\sqrt{0.5(V(X_1)+V(X_0))}}$, where the subscript refers to treatment (1) and control (0).

[14] In other words, we estimate $P(X) = \boldsymbol{\beta}\mathbf{X}$ and test the joint hypothesis that $H_0 : \boldsymbol{\beta} = \mathbf{0}$ before and after matching. The procedure after matching includes the weights for each control.

for the treated group is approximately 0.25. For the EMOVI (Panel B), the mean values are even closer. Panels E and F show the box plots after matching. The box plots do not show differences in the range of the propensity score between treatment and control. Panels C and D show the number of observations in the treatment and control by deciles of the propensity score. The histograms show there is enough number of observations in the control group to match for the treated group. The after matching histograms show that for each decile we have more observations in the control than in the treated group, with the exception of the top decile in the MxFLS.

Figure 4 shows the estimated propensity score for each treated observation and the average propensity score for the matched controls. The figure shows that the matching method succeeds in finding very similar observations between the treatment and control groups. In general, Figures 3 and 4 show that the common support condition for the estimation of ATT holds.

Table 3 shows the balance tests of stratification (Dehejia and Wahba 1999, 2002), standardized bias (Rosenbaum and Rubin 1985), likelihood ratio (Sianesi, 2004), the difference in means and the number of observations after matching. We include only three matching methods for each survey. A full set of results can be found in the appendix Table A1. The matching method is successful in balancing treatment and control groups. After matching, there are no significant differences in observable characteristics between treatment and control. However, balance is relatively more difficult to achieve with MxFLS than with EMOVI as measured by the standardized median bias and the difference in means. Nonetheless, the values are very small and fall within the region of "sufficient" balance mentioned by Caliendo and Kopeinig (2008). In the appendix, we show that balance is more successfully achieved in a model in which the propensity score excludes interaction terms and only incudes linear terms. But since excluding important variables may bias the ATT estimates, we present the main results using the estimated propensity score with interactions and squares, and as a robustness exercise we show the ATT results using the model with linear terms.

5.2 Short-run impacts

Table 4 shows the main results using MxFLS with a difference-in-difference ATT. The table includes the individual outcomes of years of schooling, school attendance, marriage, working, hours of work and whether the individual left the household by 2005. The table also includes outcomes at the household level restricting to females who did not leave the household during the period of study. There is evidence that a teenage pregnancy reduces school attainment. Females who had a child between 2002 and 2005 or 2006 have 0.6-0.8 years of schooling less than a female who did not have a child. The estimate is statistically significant, although with relatively large standard errors. The ATT in years of schooling is roughly equivalent to the pregnancy period. So it seems that females dropout of school once they are pregnant. If they drop permanently, we should expect the gap to grow, if they drop temporarily we should observe a reduction in the gap in the long-run, or that the gap remains constant if women target the age to drop out of school. We also find that school attendance decreases. However, it is important to point out that not all teenagers who got pregnant drop out of school by 2005-2006. The estimate implies that between 27 to 33 percent of teenagers who got pregnant are not attending school after pregnancy compared to similar teenagers in the control group.

A key difference to results in the literature in the United States is that teenage pregnancy does not reduce the likelihood of marriage. In fact, a larger share of childbearing teenagers are married as compared to similar childless teenagers. These results are very possibly due to cultural differences between Mexico and the United States. Mexican females tend to marry more in general and teenage pregnancies are severely stigmatized by Mexican society. In the extension section, we analyze outcomes for teenage pregnancy out-of-wedlock.

Additionally, there is some evidence that teenage pregnancy reduces the probability of working by 13-15 percentage points. However, the standard errors are large and in the case of exact matching the results are not statistically significant. But there is statistical evidence that teenagers who got pregnant reduce the hours of work by 8.8-9.9 on average.

Also, teenagers who got pregnant are 41-43 percent more likely to leave their household than teenagers who did not get pregnant. This latter finding is a result of marriage.

It is important to analyze not only the consequences of childbearing of teenagers themselves, but the consequences to the family of origin. This is interesting but hard to measure. As we analyze longitudinal data, we observe households in two periods. But if the teenager left the household, we would only observe the information of the newly formed family. We could link the information to the family of origin, but in this case the interpretation of the treatment effect would not be clear given that the treatment on the family of origin is somewhat lost. For these reasons, we focus on teenagers who did not leave the household of origin during the period of study. In this way, we are comparing how the family reacts in the short-run when a teenager gets pregnant.

The bottom panel in Table 4 includes the results at the household level. For females that did not leave the household of origin, we observe little changes at the household level. There is no evidence that the family reacts with more hours of work (this variable excludes the labor supply of the childbearing adolescent). The results are close to zero and not statistically significant. Maybe this effect is due to a higher hours of work of parents and fewer hours of work of siblings. In order to test for this possibility, we estimate the effect on parents' labor supply (as shown in the next column). However, the estimates are not statistically significant for hours of work of parents. There seems to be no adjustment in the labor supply of other household members. This could be due to the timing of data recollection. We observe teenagers after birth, and it is possible that the household already adjusted to previous levels of hours of work. We also do not find any significant effect on income per capita, but there is a clear increase in household size. The reason that the effect on the household size is greater than one is that some teenagers got pregnant and their husband or partner moved in with her and her family. In sum, we find little evidence that a pregnancy for a teenager that stays in the household of origin has large consequences for the family of origin itself. It is important to stress that we do not measure immediate effects of pregnancy but on average 1 to 2 years after pregnancy.

5.3 Long-run impacts

Table 5 presents the estimates using EMOVI. Women who got pregnant when they were teenagers attain less schooling than females who did not get pregnant. We find that the difference is close to 1 year of education. Although the estimate is larger than the short-run results, it is not possible to reject the null hypothesis of equal effects. However, the results do not support the hypothesis that the gap in education is reduced in the long-run. On the contrary, once a teenage pregnancy occurs, the difference in years of education will be maintained.

Females who got pregnant while being adolescents are more likely to be married, and in turn less likely to be single in the long-run than their counterparts. At the same time, they are more likely to go through a divorce or separation. Hence, we do not find any evidence in the short- or long-run that a teenage pregnancy reduces the likelihood of marriage. Also, it seems that a teenage pregnancy is considered as an "extra child", otherwise they would have had the same total number of children as the control females. Moreover, the increase in the number of children results in a larger household size . As for the effects in the labor supply, although the effect of teenage pregnancy on work is negative, it is not statistical significant. Hence, there is no evidence that having children as an adolescent reduces the likelihood of working in the long-run. However, there is some evidence to a lower income per capita in the household, which is most likely a consequence of a lower educational attainment.

5.4 Extensions and Robustness checks

In the previous sections, we did not analyze outcome for pregnancies out-of-wedlock. It is possible that pregnancies out-of-wedlock are more costly to teenagers. The MxFLS identifies the year of pregnancy and the year of marriage. We restrict the sample of treatment to females that are not married in 2005 or to females that had a birth before marriage. The sample in the treatment is reduced to 76 observations instead of 131.[15] Table 6 shows the estimates for this sample.

There are no large differences between the estimates using the full sample and restricting to out-of-wedlock pregnancies. Both the loss in years of education and the reduction in the percent working are similar to the full sample. As we dropped from the sample pregnancies after marriage, the effect on marriage decreases but it is still high and close to 35 percent. Hence, there is no evidence that pregnancies out-of-wedlock are different to teenage pregnancies in a marriage. Table 6 also includes results for the EMOVI restricting the sample to females between 25 and 39 years old. There is no evidence that the loss in years of education or the probability to work is different than for the full sample. However, the percent that is married is relatively higher than for the full sample, although we cannot reject the hypothesis of equal coefficients.

In Table 7 we provide robustness results using more matching methods and results that use a different estimated propensity score. Panel A includes the main propensity score that includes interactions and squares of many variables. Results are robust to changes in the matching method. Panel B modifies the estimated propensity score by including only linear terms. In total, we include only 25 and 26 variables for the MxFLS and EMOVI respectively. The ATT are on average similar to previous estimates, but the standard error is lower as suggested by Bryson, Dorsett, and Purdon (2002). Panel C matches on the log of odds ratio of the main estimated propensity score as suggested by Heckman, Ichimura, and Todd (1997). The impact in years of schooling in the short-run varies from -0.58 to -0.93 and in the long-run from -1.09 to -1.16. Both are within the standard errors obtained for the main estimates. The impact on income per capita is consistently negative and varies from -279 to -346. In sum, the main estimates are robust to the matching method and to the estimated propensity score.

[15] From the 76 observations, 41 are unmarried in 2005 and 35 are married in 2005.

6 Conclusions

In this paper we estimated the causal effect of teenage childbearing on several outcomes of the teenage mother and her family of origin in the short-run, and also the long-run effects on the mother. The identification of the causal effect of teenage childbearing has proven to be very elusive due to selection bias: those adolescents who give birth to a child are systematically different from child-free adolescents. For instance, we found that in the case of Mexico, treated teenagers tend to be more sexually active before pregnancy and come from more disadvantaged backgrounds.

In order to solve this selection problem and to be able to identify the treatment effect on the treated, we implemented a propensity score matching using two different data sources: a longitudinal survey, the Mexican Family Life Survey (MxFLS), and a cross-section survey designed to measure mobility in Mexico, the Social Mobility Survey (EMOVI), so that we have information on the individual and her household when she was 14 years old. The MxFLS allows us to estimate the short-run effects on the teenage mother and her family of origin. On its part, the EMOVI enables us to estimate the long-run effects on the mother. We provide significant evidence that the identification assumptions of propensity score matching are satisfied in our empirical problem.

Our results show that the single most important effect of teenage childbearing is a lower educational attainment of the teenage mother, both in the short- and long-run. As a result, we find that in the long-run the households of those females who had their first child as teenagers tend to have a lower income per capita. We also find that in the short-run, teenage mothers reduce their college attendance (hence the lower educational attainment), and reduce their labor supply. We do not find any significant effects on labor supply of other household members in the short-run, nor in the labor supply of the teenage mothers themselves in the long-run. Finally, and in contrast with the literature in the United States, we found that having a child during adolescence has a positive effect on the probability of being married. This difference is most likely a result of cultural differences between Mexico and the United States.

Although still highly debated, there is evidence that teenage childbearing is associated with higher levels of poverty and welfare dependence in the United States. In Mexico, most of the scientific literature on teenage pregnancy focuses in associations and/or qualitative studies. To our knowledge, there is no previous literature on the causal effects of teenage childbearing in Mexico. This paper fills this gap in the literature by using a novel identification strategy. Our findings provide evidence that teenage childbearing has adverse effects in the Mexican context. The fact that teenage childbearing prevents teenage mothers from continuing their human capital investments shows that teenage childbearing may have an increasing effect on the probability of living in a poor household. Moreover, given that there is little social mobility in Mexico (Torche, 2010), teenage childbearing may be a gateway into an intergenerational poverty trap. As such, our work has two important policy implications. First, there should be more programs aimed at preventing teenage pregnancies such as sexual education during primary and secondary education, and access to contraceptives through public health systems. And second, once the teenage has become pregnant, the state could provide support in the form of childcare and possibly merit scholarships, so that the teenage mother does not drop out of school.

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Table 1: Aggregate Statistics. Females 15-19 years old: 1990-2010

	Proportions			% Childbearing		
	1990	2000	2010	1990	2000	2010
National	100.0	100.0	100.0	12.3	12.5	13.0
Rural	25.8	25.7	26.0	17.4	16.0	14.9
Urban	74.2	74.3	74.0	10.5	11.3	12.3
By Education						
Primary or less	50.1	38.9	28.7	18.3	19.5	17.7
Secondary	45.0	49.1	55.4	6.4	8.3	12.0
More than Secondary	5.0	12.0	15.9	4.1	4.9	7.4
By Marital Status						
Single	82.5	82.3	82.1	1.3	1.7	2.5
Married	10.8	8.5	4.7	65.3	64.6	63.2
Cohabitating	5.8	8.2	11.7	60.4	60.1	60.0
Other	0.9	1.1	1.5	70.2	71.5	65.7
By School Attendance						
Not attending	59.4	54.6	42.9	19.9	22.1	28.0
Attending	40.6	45.4	57.1	1.1	1.1	1.8

Notes: Calculations by the authors using census data. Sample is restricted to females aged 15-19 years old with a valid answer in the number of own children. The last three columns indicate the percent of women with at least one children born alive given the condition in the first column.

Table 2: Descriptive Statistics. MxFLS and EMOVI

	MxFLS: Baseline 2002			EMOVI		
	Control	Treatment	Diff	Control	Treatment	Diff
N	872	131		3378	1030	
Age	15.69	15.92	0.23	39.11	39.61	0.49
	[0.047]	[0.112]	[0.122]	[0.210]	[0.359]	[0.416]
Yrs School	8.29	8.00	-0.29	8.63	6.83	-1.81*
	[0.074]	[0.210]	[0.223]	[0.072]	[0.113]	[0.134]
Working	0.12	0.20	0.08*	0.48	0.44	-0.04*
	[0.011]	[0.035]	[0.037]	[0.009]	[0.015]	[0.018]
Attendance	0.72	0.49	-0.23*			
	[0.015]	[0.044]	[0.046]			
HH Size	5.79	5.42	-0.37	5.81	6.29	0.49*
	[0.067]	[0.179]	[0.192]	[0.042]	[0.079]	[0.089]
Yrs School	5.85	5.12	-0.73			
(Head Household)	[0.146]	[0.359]	[0.388]			
Father: Yrs School				3.99	2.83	-1.15*
				[0.075]	[0.106]	[0.130]
Mother: Yrs School				3.70	2.65	-1.05*
				[0.070]	[0.103]	[0.125]
Knowledge of	0.90	0.91	0.00			
Contraceptives	[0.009]	[0.025]	[0.027]			
Previous Sexual	0.02	0.08	0.06*			
Experience	[0.005]	[0.023]	[0.024]			

Notes: Calculations by the authors using MxFLS and EMOVI. Sample is restricted to females aged 15-19 years old with a valid answer in the number of own children for the case of MxFLS. In MxFLS: Treatment is defined as women with a pregnancy event (only 3 women report a pregnancy but no child alive). In EMOVI, treatment is defined as child was born when woman was a teenager. HH Size in EMOVI refers to household size when female was 14 years old. Standard errors in brackets. * denotes significance at 5 percent.

Table 3: Balance in the propensity score

	DW Test		Median Bias		LR Test		Diff Means		# Treat	# Control
	Before	After	Before	After	Before	After	Before	After		
A. MxFLS										
Kernel Epanechnikov, bw=0.01	0.03	0.00	13.97	4.69	0.00	0.99	0.32	0.01	118	865
NN-3, radius 0.01	0.03	0.00	13.97	5.65	0.00	0.99	0.32	0.02	118	224
Exact match + NN-3, radius 0.01	0.03	0.00	13.97	6.85	0.00	0.98	0.32	0.02	99	195
B. EMOVI										
Kernel Epanechnikov, bw=0.01	0.04	0.00	12.16	0.87	0.00	0.99	0.68	0.00	1024	3376
NN-3, radius 0.01	0.04	0.00	12.16	0.91	0.00	0.99	0.68	0.00	1024	1691
Exact match + NN-3, radius 0.01	0.04	0.00	12.16	1.38	0.00	0.99	0.68	0.00	956	1637

Notes: Calculations by the authors. The first column indicates the matching method. NN refers to nearest neighbor matching. The exact matching method restricts individuals within rural or urban areas and exact age for EMOVI, and for ENNVIH also restricts to individuals with the same school attendance status. "DW test" refers to the Dehejia and Wahba (1999) stratification test using quintiles of the estimated propensity score, The column "Median Bias" shows the median standardized bias, The column "LR test" shows the p-value of the likelihood ratio test that all coefficients in the regression are equal to zero. The column "Diff Means" shows the percent of tests out of total possible tests in which the null hypothesis of equal means between treatment and control is rejected. The last two columns indicate the number of observations in treatment and control after matching. We include 86 variables in 2002 for the estimation of the propensity score for MxFLS: age, years of schooling, school attendance, work status, indigenous language, knowledge of contraceptives, previous sex life, rural status, and father absent in the household. The included variables about the head of the household are: years of education, age, female, and work status. We also include household size, and members 0-5, 6-18, more than 65, average hours worked in the household, mean age and income per capita of the household, number of rooms in the household, and dummy variables of assets in the household, such as: without vehicle, without stove, without public water and without sewage. We also include 51 interactions terms between individual variables (age, schooling, work, indigenous, knowledge of contraceptives and previous sex life) and household variables and squares of age and years of schooling. We include 57 variables in the estimation of the propensity score for EMOVI: age and age squared, born in rural areas, and information about both parents when individual was 14 years old, such as: education, work status, formal sector job, indigenous language, what parent the individual was living with. We also include information about the household: number of siblings, household size, number of rooms and cars, and dummies of assets in the household like: without stove, without washing machine, without refrigerator, without television, without public water, without sewage, and without electricity. Finally we include interactions of individual variables with household characteristics as well as squares and interactions of years of education of both parents, and work status of both parents.

Table 4: Short-Run results. MxFLS

Individual Outcomes	Yrs School	Attendance	Married	Working	Hrs Work	Left HH
Kernel Epanechnikov,	-0.645	-0.327	0.608	-0.130	-8.816	0.437
bw=0.01	[0.210]	[0.060]	[0.059]	[0.068]	[3.182]	[0.057]
NN3, Radius 0.01	-0.602	-0.325	0.599	-0.136	-8.493	0.427
	[0.263]	[0.073]	[0.062]	[0.077]	[3.599]	[0.059]
Exact match + NN-3,	-0.827	-0.274	0.547	-0.155	-9.899	0.416
radius 0.01	[0.291]	[0.069]	[0.072]	[0.094]	[4.481]	[0.071]
Household Outcomes	Total Hours of Work	Parents Hours of Work	Income per Capita	HH Size		
Kernel Epanechnikov,	1.01	2.44	-199.00	1.12		
bw=0.01	[11.62]	[7.35]	[237.22]	[0.24]		
NN3, Radius 0.01	0.64	2.94	-187.70	1.29		
	[12.20]	[7.89]	[259.99]	[0.24]		
Exact match + NN-3,	5.03	3.20	12.32	1.30		
radius 0.01	[15.00]	[9.86]	[370.40]	[0.29]		

Notes: Calculations by the authors. The model includes linear and interaction terms, in total the estimated propensity score includes 86 variables. Exact macthing restricts to individuals within the same rural/urban, age and school attendance cells. The first panel includes outcomes at the individual level. The second panel restricts to females that did not leave the household between 2002-2005 and analyzes outcomes at the household level. Standard errors were estimated using 500 bootstrap replications.

Table 5: Long-Run results. EMOVI

	Yrs School	Married	Single	Separated	# Children	Works	HH Size	Income per Capita
Kernel Epanechnikov,	-1.065	0.050	-0.124	0.051	1.085	-0.043	0.555	-322.322
bw=0.01	[0.106]	[0.017]	[0.012]	[0.013]	[0.049]	[0.018]	[0.065]	[72.89]
NN3, Radius 0.01	-1.122	0.059	-0.132	0.048	1.120	-0.054	0.560	-338.132
	[0.169]	[0.022]	[0.017]	[0.016]	[0.066]	[0.024]	[0.086]	[140.32]
Exact match + NN-3,	-1.159	0.045	-0.108	0.042	1.062	-0.043	0.528	-401.429
radius 0.01	[0.168]	[0.024]	[0.018]	[0.020]	[0.067]	[0.026]	[0.089]	[128.12]

Notes: Calculations by the authors. The model includes linear and interaction terms, in total the estimated propensity score includes 57 variables. Exact macthing restricts to individuals within the same rural/urban, age and school attendance cells. Standard errors were estimated using 500 bootstrap replications.

Table 6: Extensions: Short Run effects of teenage pregnancy out-of-wedlock and teenage pregnancy effects at 25-39 years old

	MxFLS: Unmarried teenagers			EMOVI: 25-39 years old		
	Yrs School	Works	Marriage	Yrs School	Works	Marriage
Kernel Epanechnikov,	-0.611	-0.146	0.358	-1.177	-0.065	0.111
bw=0.01	[0.302]	[0.088]	[0.084]	[0.152]	[0.025]	[0.026]
NN3, Radius 0.01	-0.614	-0.153	0.349	-1.061	-0.061	0.115
	[0.352]	[0.101]	[0.088]	[0.204]	[0.032]	[0.032]
Exact match + NN3,	-0.522	-0.144	0.349	-1.226	-0.045	0.100
radius 0.01	[0.402]	[0.132]	[0.100]	[0.215]	[0.034]	[0.035]

Notes: Calculations by the authors. The model includes linear and interaction terms, in total the estimated propensity score includes 86 variables for MxFLS and 57 variables for EMOVI. Exact macthing restricts to individuals within the same rural/urban, age and school attendance (for MXFLS) cells. Standard errors were estimated using 500 bootstrap replications.

Table 7: Robustness Tests

	MxFLS			EMOVI			
	Yrs School	Works	Marriage	Yrs School	Works	Marriage	Income per capita
A. Propensity score with linear, squares and interaction terms							
Kernel Epanechnikov,	-0.828	-0.146	0.597	-1.099	-0.047	0.051	-345.757
bw=0.0025	[0.254]	[0.078]	[0.065]	[0.118]	[0.020]	[0.017]	[82.12]
Kernel Gaussian,	-0.563	-0.117	0.617	-1.084	-0.044	0.053	-305.646
bw=0.01	[0.335]	[0.097]	[0.072]	[0.103]	[0.018]	[0.017]	[71.94]
NN3, radius 0.025	-0.641	-0.142	0.590	-1.129	-0.054	0.060	-334.045
	[0.263]	[0.073]	[0.059]	[0.168]	[0.024]	[0.022]	[140.25]
B. Propensity score with linear terms							
Kernel Epanechnikov,	-0.812	-0.151	0.619	-1.158	-0.043	0.059	-318.719
bw=0.01	[0.179]	[0.050]	[0.043]	[0.145]	[0.017]	[0.018]	[75.10]
NN3, Radius 0.01	-0.917	-0.157	0.621	-1.163	-0.039	0.069	-279.820
	[0.232]	[0.063]	[0.048]	[0.303]	[0.051]	[0.044]	[165.54]
Kernel Epanechnikov,	-0.891	-0.149	0.603	-1.137	-0.043	0.065	-313.687
bw=0.0025	[0.208]	[0.063]	[0.048]	[0.156]	[0.019]	[0.019]	[165.57]
NN3, radius 0.025	-0.934	-0.134	0.619	-1.157	-0.040	0.070	-279.689
	[0.222]	[0.064]	[0.048]	[0.302]	[0.051]	[0.044]	[80.72]
C. (A) + matching in the log odds ratio							
Kernel Epanechnikov,	-0.694	-0.114	0.597	-1.090	-0.045	0.054	-306.752
bw=0.01	[0.209]	[0.061]	[0.058]	[0.110]	[0.019]	[0.017]	[76.69]
NN3, Radius 0.01	-0.633	-0.142	0.593	-1.130	-0.055	0.060	-336.757
	[0.260]	[0.072]	[0.062]	[0.171]	[0.024]	[0.022]	[141.56]
Kernel Epanechnikov,	-0.701	-0.122	0.587	-1.068	-0.042	0.050	-341.993
bw=0.0025	[0.236]	[0.076]	[0.062]	[0.128]	[0.020]	[0.019]	[104.20]
NN3, radius 0.025	-0.598	-0.142	0.591	-1.132	-0.055	0.060	-334.589
	[0.263]	[0.074]	[0.060]	[0.171]	[0.024]	[0.022]	[140.64]

Notes: Calculations by the authors. Panel A includes linear and interaction terms, in total the estimated propensity score includes 86 variables for MxFLS and 57 variables for EMOVI. Panel B includes only linear terms in the estimation of the propensity score. We include 25 and 26 variables in the MxFLS and EMOVI respectively. Standard errors were estimated using 500 bootstrap replications.

Figure 1: Teenage pregnancy in Latin America and Mexico. Females 15-19.

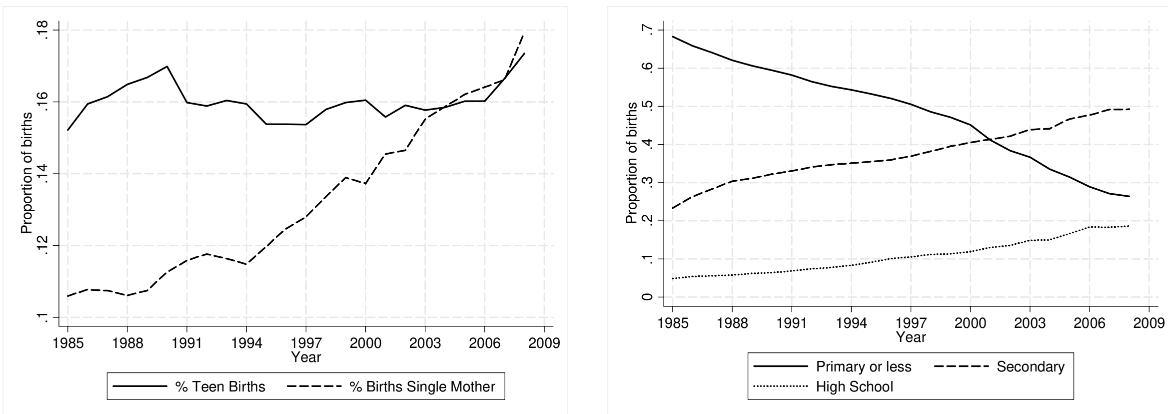
A. Latin America   B. Mexico

Notes: Calculations by the authors. Panel A uses World Bank data for 2009. Data available at http://data.worldbank.org. ARG=Argentina, BLZ=Belize, BOL=Bolivia, BRA=Brazil, CHL=Chile, COL=Colombia, CRI=Costa Rica, ECU=Ecuador, GTM=Guatemala, HND=Honduras, MEX=Mexico, NIC=Nicaragua, PAN=Panama, PER=Peru, SLV= El Salvador, URY=Uruguay, VEN=Venezuela. Panel B uses information from the Statistical Institute (INEGI). To construct teenage births per 1,000 people, we interpolate population rates using Census Data 1990, 2000 and 2010. We use year of pregnancy and not year of registry of birth. Due to right-censoring of the data, we limit the calculation to births registered in the same or next year to ocurrence (93 percent of the cases on average).

Figure 2: Teenage pregnancy in Mexico. Females 15-19. 1985-2008.

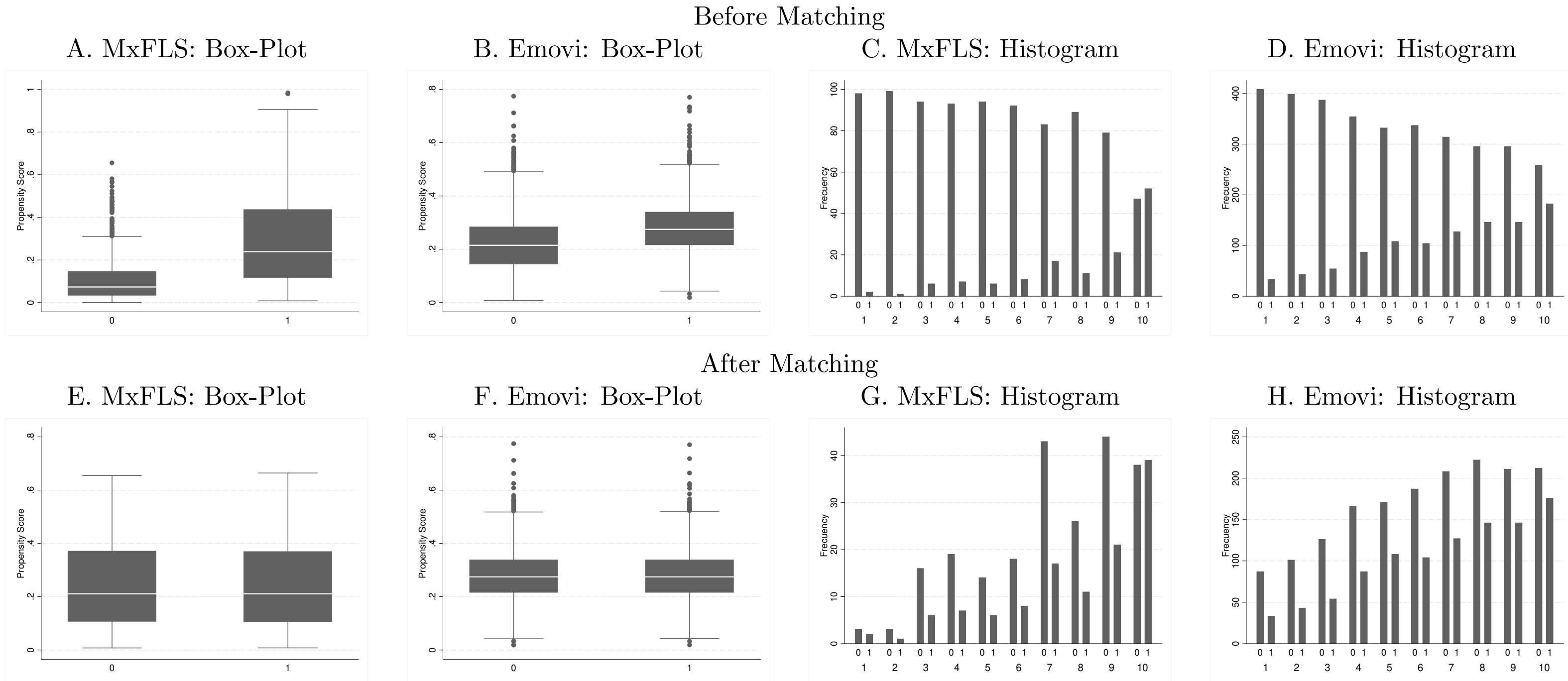
A. Percent of births by teenagers and single mothers

B. Percent of births by education of teenage mothers



Notes: Calculations by the authors. Panels use information from the Statistical Institute (INEGI). To construct teenage births per 1,000 people, we interpolate population rates using Census Data 1990, 2000 and 2010. We use year of pregnancy and not year of registry of birth. Due to right-censoring of the data, we limit the calculation to births registered in the same or next year to ocurrence (93 percent of the cases on average). In panel A, the percent of births reported by single women excludes the percent of women with invalid information on civil status. % Teen births refers to the percent of teen births among total births. % Births Single Mother refers to the percent of teen births with a single mother (excludes cohabitation). In panel B, there is around 3-5 percent of females with invalid education information.

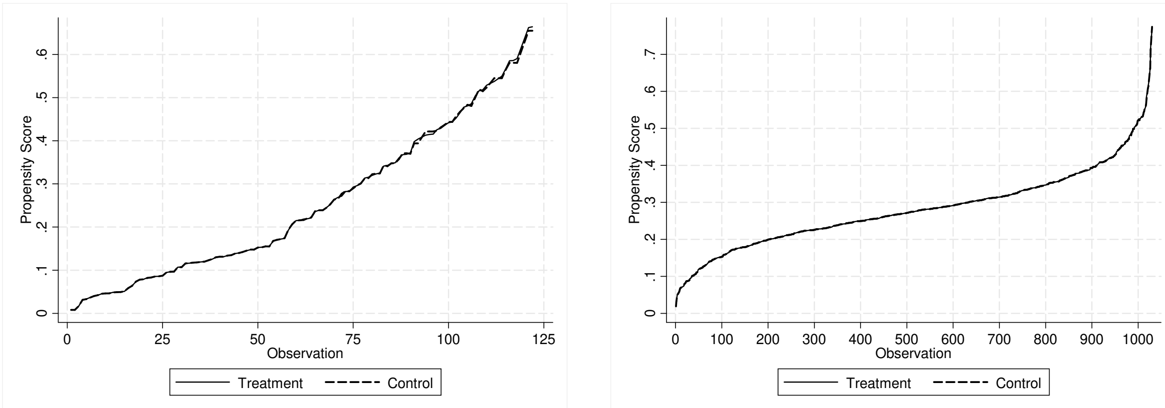
Figure 3: Balance in the Propensity Score. MxFLS and EMOVI



Notes: Calculations by the authors. After matching figures use the method of 3 Nearest Neighbors within a radius of 0.01. In the box plots, the number 0 refers to controls and 1 to treatment observations. In the histograms, the x-axis has two rows, the first row refers to control and treatment, and the second row to deciles of the estimated propensity score. We include 86 variables in 2002 for the estimation of the propensity score for MxFLS: age, years of schooling, school attendance, work status, indigenous language, knowledge of contraceptives, previous sex life, rural status, and father absent in the household. The included variables about the head of the household are: years of education, age, female, and work status. We also include household size, and members 0-5, 6-18, more than 65, average hours worked in the household, mean age and income per capita of the household, number of rooms in the household, and dummy variables of assets in the household, such as: without vehicle, without stove, without public water and without sewage. We also include 51 interactions terms between individual variables (age, schooling, work, indigenous, knowledge of contraceptives and previous sex life) and household variables and squares of age and years of schooling. We include 57 variables in the estimation of the propensity score for EMOVI: age and age squared, born in rural areas, and information about both parents when individual was 14 years old, such as: education, work status, formal sector job, indigenous language, what parent the individual was living with. We also include information about the household: number of siblings, household size, number of rooms and cars, and dummies of assets in the household like: without stove, without washing machine, without refrigerator, without television, without public water, without sewage, and without electricity. Finally we include interactions of individual variables with household characteristics as well as squares and interactions of years of education of both parents, and work status of both parents.

Figure 4: Average propensity score in treatment and control. MxFLS and EMOVI.

A. MxFLS B. EMOVI



Notes: Calculations by the authors. Matching uses the method of 3 Nearest Neighbors within a radius of 0.01. We sort the treated observations by the propensity score (solid line), and then take the average of the propensity score for the matched controls of each treated observation (dash line).

Table A1: Balance in the propensity score. Robustness.

	DW Test		Median Bias		LR Test		Diff Means		# Treat	# Control
	Before	After	Before	After	Before	After	Before	After		
A. MxFLS										
Kernel Epanechnikov, bw=0.0025	0.03	0.00	13.97	4.34	0.00	0.99	0.32	0.05	97	597
NN-1, radius 0.01	0.03	0.00	13.97	8.95	0.00	0.06	0.32	0.03	118	99
Kernel Gaussian, bw=0.01	0.03	0.00	13.97	10.06	0.00	0.99	0.32	0.11	131	868
NN-3, radius 0.025	0.03	0.00	13.97	4.64	0.00	0.99	0.32	0.02	122	226
Propensity score with linear terms										
Kernel Epanechnikov, bw=0.01	0.06	0.00	10.43	1.63	0.00	0.99	0.24	0.00	125	862
NN-3, radius 0.01	0.06	0.00	10.43	3.65	0.00	0.99	0.24	0.00	125	269
Exact match + NN-3, radius 0.01	0.06	0.00	10.43	6.71	0.00	0.99	0.24	0.00	114	236
Kernel Epanechnikov, bw=0.0025	0.06	0.00	10.43	3.48	0.00	0.99	0.24	0.00	120	674
B. EMOVI										
Kernel Epanechnikov, bw=0.0025	0.04	0.00	12.16	0.45	0.00	0.99	0.68	0.00	1012	3302
NN-1, radius 0.01	0.04	0.00	12.16	1.43	0.00	0.99	0.68	0.00	1024	754
Kernel Gaussian, bw=0.01	0.04	0.00	12.16	0.59	0.00	0.99	0.68	0.00	1030	3378
NN-3, radius	0.04	0.00	12.16	0.98	0.00	0.99	0.68	0.00	1030	1691
Propensity score with linear terms										
Kernel Epanechnikov, bw=0.01	0.08	0.00	10.71	0.33	0.00	0.99	0.62	0.00	1025	3377
NN-3, radius 0.01	0.08	0.00	10.71	1.45	0.00	0.99	0.62	0.00	1025	1781
Exact match + NN-3, radius 0.01	0.08	0.00	10.71	0.93	0.00	0.99	0.62	0.00	972	1687
Kernel Epanechnikov, bw=0.0025	0.08	0.00	10.71	0.75	0.00	0.99	0.62	0.00	1019	3327

Notes: Calculations by the authors. The first column indicates the matching method. NN refers to nearest neighbor matching. The exact matching method restricts individuals within rural or urban areas and exact age for EMOVI, and for ENNVIH also restricts to individuals with the same school attendance status. "DW test" refers to the Dehejia and Wahba (1999) stratification test using quintiles of the estimated propensity score, The column "Median Bias" shows the median standardized bias, The column "LR test" shows the p-value of the likelihood ratio test that all coefficients in the regression are equal to zero. The column "Diff Means" shows the percent of tests out of total possible tests in which the null hypothesis of equal means between treatment and control is rejected. The last two columns indicate the number of observations in treatment and control after matching. We include 86 variables in 2002 for the estimation of the propensity score for MxFLS including interactions.We include 57 variables in the estimation of the propensity score for EMOVI. The models with the "Propensity score with linear terms" do not include interaction terms. In this case, MxFLS includes 25 variables and EMOVI 26 variables.