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Is the Baby to Blame?

An Inquiry into the Consequences of Early Childbearing

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Abstract

Teenage pregnancy has been a cause of concern for policy makers because it is associated with a complex and often adverse social context for women. It is seen as the cause of lower social and economic achievement for mothers and their children, and as the potential determinant of inter-generational poverty traps. However, the question of whether pregnancy—and the subsequent rearing of a child—is actually *the* trigger of poverty, higher dependence on social welfare and/ or other undesirable social and economic consequences has not been studied in developing countries with enough rigor to establish a causal relation. This paper follows a methodology previously applied in the United States, using Mexican data from the National Survey of Demographic Dynamics, to exploit information about miscarriages as an instrument to identify the long-term consequences of early child bearing. Thus, the paper takes the advantage of a natural experiment: it compares the outcomes of women who became pregnant in adolescence, and gave birth, to outcomes of women who became pregnant in adolescence and miscarried. This approach only allows for estimating the costs of adolescent childbearing for teenagers in a risk group, that is, teenagers who are likely to experience a pregnancy. The results are consistent with findings in the United States, suggesting that, contrary to popular thinking, adolescent childbearing does not hamper significantly the lifelong opportunities of the young mothers. Actually, women who gave birth during their adolescence have on average 0.34 more years of education, and are 21 percentage points more likely to be employed, compared with their counterparts who miscarried. The results also suggest, however, greater dependence on social welfare among women who gave birth during adolescence: their social assistance income is 36 percent higher, and they are more likely to participate in social programs, especially the conditional cash transfer program *Oportunidades.*

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Is the baby to blame?

An inquiry into the consequences of early childbearing[1]

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1. Introduction

Information on the adverse effects associated with teenage pregnancy is widely spread. Indeed, governmental and non-governmental organizations often remind their audiences that teenage mothers are more likely to depend on social welfare and remain poor, less likely to graduate from high school, or less likely to ever attain post-secondary education. Nevertheless, the question of whether teen pregnancy is a cause or a consequence of the heralded social issues is rarely addressed. Does pregnancy – and the subsequent rearing of a child – trigger poverty, a higher dependence on social welfare and/ or other undesirable social and economic consequences? Or are women who are poor, highly dependent on welfare, and unlikely to complete secondary education simply more likely to become pregnant as teenagers? These are complex questions; yet, without a clear answer, policymakers may run the risk of focusing on correlates as opposed to addressing the cause.

Given that women who experience pregnancy as adolescents frequently differ from the rest of the population of women – in terms of their socioeconomic status, parental characteristics and other dimensions – it is easy to confound the effects of bringing up a child in adolescence with the determinants of this decision. Unfortunately, few discussions go beyond noting the strong correlation between teenage pregnancy and negative socioeconomic conditions, failing to establish causality between the two. The failure to establish a causal link is not surprising given the Gordian knot of interdependencies between teenage pregnancy and various social and economic phenomena.

This paper will attempt to shed light on the extent to which teenage pregnancy is the cause or the consequence of the socioeconomic issues regularly associated with it. To achieve this goal we will focus on one of the aspects of the multi-faceted issue of teenage pregnancy and provide a rigorous, unbiased analysis of it. Specifically, we will estimate the impact of early childbearing on women who became pregnant as teenagers – in Angrist and Imbens' (1991) terminology we will provide an estimate of the selected average treatment effect. Our research thus aims to answer the following questions: Conditional on being pregnant as a teenager, what is the cost of early child bearing? What would the change in welfare be if potential teenage mothers delayed child bearing?

By focusing on adolescent childbearing, rather than a broader issue of adolescent pregnancy, we will be able to provide causal estimates, as opposed to merely register correlations. Identifying the impact of childbearing on a randomly chosen teenage woman presents serious difficulties; nonetheless it is

possible to identify the impact of teenage childbearing among teenagers who become pregnant using miscarriages as a natural experiment. Not uncommon during pregnancy, miscarriages present a methodologically-advantageous characteristic: they are neither related to the mother's demographic condition, nor appear to be substantially connected with the characteristics that impact adult outcomes such as education, income, and so forth (Lang, 2007). This paper takes advantage of the 2006 round of the Mexican *Encuesta Nacional de la Dinamica Demografica* (ENADID), which records not only the birth histories of its respondents, but also their miscarriage and abortion histories. By focusing on the population of women who became pregnant as teenagers, this paper circumvents the issue of selection bias inherent to any inquiry into the consequences of teen pregnancy. Using miscarriages as a natural experiment, however, requires caution, and results must be interpreted carefully.[5]

Although teenage pregnancy has received much attention in social science and popular media alike, rigorous evidence on the consequences of early childbearing is scarce. Hotz et al. (2005) find that, contrary to popular perception, adolescent childbearing in the US has negligible if not positive consequences on the overall welfare and economic opportunities of teenage mothers. The authors look at the consequences of early childbearing on a sample of women who became pregnant as teenagers, providing the estimate of the effects of early childbearing on that selected population only. Geronimus and Korenman (1992) estimate the effect of adolescent childbearing by comparing outcomes of sisters. They find little difference between the sister who gave birth in adolescence and the sister who did not. Using age at menarche as an exogenous source of variation in the age at first birth, Ribar (1994) does not find significant effects of teenage childbearing on high school completion. Ashcraft and Lang (2006) provide narrow bounds of the effect of adolescent childbearing combining IV estimates as in Hotz et al. (2005) with OLS estimates. They find generally adverse, but modest effects. To our best knowledge, the impact of adolescent childbearing in developing countries has yet to be rigorously evaluated.

[5] The literature points out that the use of miscarriages as a natural experiment to estimate the effects of giving birth may be subject to under or overestimation problems when accounting for abortion, as described by Lang (2007). Results may also be estimating the cost of a miscarriage (i.e. the emotional toll on the mother), vis-à-vis the cost of child bearing. We address each of these issues in the paper.

This paper is organized as follows. Section 2 describes the data. Section 3 discusses the identification strategy, its potential shortcomings due to the data problems and the proposed solutions to address these shortcomings. Section 4 presents the results. Section 5 discusses robustness checks and rules out an alternative explanation. Section 6 concludes.

2. The data

The success of rigorously assessing the impact of adolescent childbearing hinges on the availability of a dataset that reports not only birth histories, but miscarriages and abortions as well. ENADID is a nationally representative survey, containing a wide variety of demographic, social and health information. Of particular interest to this work is the detailed questionnaire administered to women aged 15 to 54, including questions on birth histories, medical care during pregnancy and delivery, as well as knowledge of and use of contraception. The module also records all pregnancies that resulted in live birth, stillbirth and abortion. Fortuitously for this study, the survey administered in 2006 distinguishes between spontaneous and induced abortions for the last reported abortion. Although it is not possible to determine whether any abortion prior to the last reported one was spontaneous or induced, the data show that very few women in the sample had more than one abortion.[6]

ENADID data contain a number of socio-demographic characteristics, such as marital status, age at the first union, participation in social programs, number of hours worked in the last week, social assistance income, income from labor and others. Unfortunately, ENADID does not include a consumption module, and consequently we are unable to determine the welfare status of the respondents. To address this problem, we construct an asset index based on principal component analysis. Following Filmer and Pritchett (2001), we consider the asset index to be a proxy of the household's long-run wealth; assuming that the long-run wealth of the household explains the maximum variance and covariance in the assets used for the construction of the index (summarized in Table A1).

[6] Of all women who reported ever experiencing a pregnancy, 1.56 percent had more than 1 abortion (spontaneous or induced). Of them, 1.35 percent reported having had two abortions, 0.2 percent and 0.01 percent reported having had three and four abortions, respectively.

This paper uses two samples. The first sample includes all women aged 19 to 30 who have ever experienced a pregnancy. This sample is used to demonstrate the differences in background characteristics and outcomes of interest between women who had a child during their adolescence and those who delayed childbearing until a later age. The second sample is limited to women aged 19 to 30, who experienced a pregnancy before the age of 19. This sample is used to establish the causal effect of adolescent childbearing by comparing women whose teenage pregnancy resulted in a live birth to women who miscarried. Although ENADID survey was administered to women aged 15 to 54, women under 19 are not included into the analysis. Even if they have not experienced a pregnancy by the time of the interview, they still may do so before turning 19. Thus, keeping women aged 15 to 18 in the sample may result in false inclusion of potential teenage mothers into the control group. The sample also exclude women who experienced more than one miscarriage, as repeat miscarriages may not be random and are likely to be associate with poor health status and other characteristics which may affect the outcomes (Wang et al., 2003).

The large sample includes 7,805 women aged 19 to 30. The teen pregnancy sample includes only 2,803 of these women. Among women who experienced pregnancy as adolescents, 2,543 had living children, the children of 26 women who had a live birth died; 142 women were pregnant at the time of the interview; for 85 women the pregnancy resulted in a miscarriage or a stillbirth. Only 7 women reported having induced abortion (we discuss the implications of this likely underreporting bias later in the paper).

Census data collected in 2000 is used to control for municipal-level characteristics. The respondents in the first sample live in 787 municipalities (with 10 observations per municipality on average), while respondents in the second sample come from 617 municipalities (with 5 observations per municipality on average). The number of municipalities provides sufficient variation to investigate the heterogeneity in the impact of early childbearing depending on various community characteristics. The census allows for the calculation of such community-level variables as the share of indigenous population and of the Catholic population, average access to medical services (captured in the fraction of people who have some form of medical insurance), average school attendance, educational level, the quality of employment (captured in the share of the employed working less than 32 hours a week and the share of employed earning less than minimal salary), and other characteristics. The complete list of municipal-level controls with their averages and standard

deviations is presented in Table A2. Table A2 clearly shows that the averages of selected characteristics are virtually indistinguishable in the two samples, and are very similar in magnitude to the national averages (when all 2,443 municipalities in Mexico are used).

3. Empirical strategy

3.1. Using miscarriages to identify the impact of adolescent childbearing

Identification of the causal effect of early childbearing on mother's welfare, during adolescence as well as later in life, presents considerable problems. Unquestionably, women who become pregnant as teenagers differ along a number of important dimensions from women who delay childbearing. Table 1 illustrates this point. In Mexico, women who become pregnant as teenagers (before turning 19 years old) are significantly more likely to be indigenous, more likely to be members of less educated and less wealthy families. They are less likely to live with their fathers. They begin sexual activity and form unions at significantly earlier ages. Indisputably, there is a selection on observable (and most likely unobservable) characteristics in the group of women who experience pregnancy before age 19.

To address this selection bias, the present paper uses miscarriages as a natural experiment. It estimates the difference in outcomes among women whose teen pregnancy resulted in a live birth, and women who became pregnant as teenagers but whose pregnancy did not result in a live birth due to a miscarriage. Let Y be an outcome of interest, B a dummy equal to one if a pregnancy resulted in a live birth, and $\boldsymbol{X}$ a vector of individual characteristics. To capture the effect of early childbearing on Y, we run the following regression on a sample of women who became pregnant as teenagers and whose pregnancy resulted in a live birth or a miscarriage:

$$Y = \beta_0 + \beta_1 B + \beta_2 X + \varepsilon, \qquad (1)$$

This identification strategy poses several concerns. We assume that pregnancy results in three possible outcomes: live birth, abortion, and miscarriage.[7] Following Hotz et al. (2005), we can also

[7] ENADID survey distinguishes between stillbirth and miscarriage: if a pregnancy loss occurs in the first 5 months of pregnancy, it is recorded as miscarriage (*aborto*); if the loss of pregnancy occurs after 5 months of

divide adolescent pregnant women in two groups, depending on their latent status: those who, after becoming pregnant, choose to give birth (latent status B^*), and those who would choose to abort (latent status A^*). Equation (1) will identify the effect of adolescent childbearing only if:

(i) The sample is limited to women of latent status B^*, who after becoming pregnant choose to give birth. Consequently, they would have only two outcomes: live birth or miscarriage.

(ii) Miscarriages are exogenous, or there is nothing in the error term ε that is simultaneously correlated with the identifier for live birth X and the outcome of interest Y.

In order to ensure (i), it is necessary to exclude from the sample of women who experienced pregnancy as adolescents those who either aborted or would have aborted, had they not miscarried. Do ENADID data allow for this? At first sight the data present two problems that may hinder the seemingly simple process.

First, abortions are grossly underreported in ENADID. Table 2 shows that the number of abortions per thousand women reported in ENADID is 82 times lower than a low estimate based on hospitalization data and surveys of health professionals: 0.34 vs. 28 (Juarez et al., 2006). This is not surprising, considering that in 2006 abortion was illegal throughout the country (it became decriminalized in Mexico City in 2007). However, underreporting of abortions will not bias estimates in (1), as long as women *do not report* pregnancies that were terminated in abortions (as opposed to reporting them as miscarriages). Provided that women do not report abortions as miscarriages, the coefficient β in equation (1) will be unbiased regardless of the extent of underreporting. A data analysis in the subsequent section will show that reporting abortions as miscarriages is highly unlikely.

The second issue is that some women who reported miscarriages may have latent status A^*, but miscarried before aborting. Hotz et al. (2005) use data on the actual proportion of abortions to address this problem.[8] Given the quality of abortion data in ENADID, this option is unavailable here. However, ENADID contains data on the length of pregnancy at the time of the miscarriage.

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pregnancy, it is recorded as a stillbirth (*mortinato*). In this paper we refer to both: aborto and mortinato as miscarriages.

[8] Hotz et al. (2005) demonstrate that they can identify the effect of having a baby on a woman of latent status A^* if they use indicator for abortion as an instrument for not giving birth.

This allows us to verify the robustness of our findings to the variations in the sample. Namely, we can limit the sample to miscarriages late in the pregnancy, when the option of abortion is likely to have already been exercised by those who intended to do so.

3.2. How do women underreport abortions?

As mentioned earlier, one of the major threats to the identification strategy is the possibility of abortions being reported as miscarriages. The present section looks at this issue in more depth. Substantial underreporting of abortions *is* present in ENADID data. Juarez et al. (2008) construct an estimate of the number of induced abortions in Mexico in 2006, based on data on hospitalization for complications due to induced abortion, and a survey of medical personnel. The results show a low and a high estimate of the abortion rate: 28 and 39 abortions per 1000 women aged 15 to 44 (Table 2).

According to ENADID data, there were 8,438 induced abortions in Mexico in 2005[9] – slightly more than 1 percent of the low estimate of the number of induced abortions estimated in Juarez et al., 2008 (Table 2). Nevertheless, underreporting of abortions will impede identification only if women report abortions as miscarriages (as opposed to just not reporting pregnancies that terminated in abortion). Using epidemiological data on the likelihood of a miscarriage, we demonstrate that this scenario is highly unlikely.

Epidemiological studies show that the probability of miscarriage varies depending on the time elapsed since conception. The probability of very early miscarriage (within six weeks after the last menstrual period [LMP]) is quite high – 25 percent (Wilcox et al., 1999; Wang et al., 2003). This statistic however, is based on studies that use very sensitive pregnancy tests. Within normal settings, women are likely to mistake such early miscarriages for delayed menstrual periods, without realizing they were pregnant. Six weeks after the LMP, the probability of miscarriage decreases dramatically – clinical miscarriages are estimated to occur in 7.9 percent of pregnancies (Wang et al., 2003). At this

[9] We use miscarriages reported to occur in 2005 for two reasons: (i) the survey was administered in January-May of 2006, so we would not be able to capture only abortions which occurred in the first 5 months of 2006; (ii) the respondents are only asked whether their last abortion was spontaneous or induced. Of 266 abortions reported in 2005, 260 (or 98 percent) were the last abortions. In previous years this proportion is lower.

stage of pregnancy, women are also more likely to be aware that they are pregnant and can recognize miscarriage as such.

ENADID data virtually replicates the epidemiologically predicted proportion of naturally occurring clinical miscarriages. Of all pregnancies reported in 2005, 7.05 percent ended in clinical miscarriages (Table 3). This number just about accounts for biologically inevitable miscarriages – being too low to also include abortions reported as miscarriages. We conclude that women who resort to clandestine abortion do not report a pregnancy at all, as opposed to reporting such a pregnancy as having terminated in miscarriage.

3.3. How to account for women who chose to abort, but had a miscarriage prior to executing this option?

Another challenge to the identification strategy is that, among women who miscarried, there is a fraction who would have aborted had miscarriage not occurred before they acted on their decision. These women may be different from women who chose to carry the pregnancy to term, and therefore will not constitute a valid comparison group. To derive an unbiased estimate of the effect of early childbearing, we need to compare the outcomes of women who had children to the outcomes of women who miscarried, but who intended to carry the pregnancy to term, or (using earlier terminology) whose latent status is *B**. Hotz et al. (2005) use data on reported abortions to correct their estimates for the possibility that among women who miscarried, some had latent status *A**. However, as shown above, ENADID 2006 does not contain reliable information on abortions.

To account for the possibility that women who miscarried may include women with latent status *A**, we will take advantage of the fact that ENADID includes the number of months of pregnancy when a miscarriage occurred. If we define latent status as a permanent characteristic that does not vary with time, women with latent status *A** know that they want to abort soon after they discover that they are pregnant. They have many incentives to act on their decision as early as possible: abortions are safer and emotionally easier when carried out early (Bartlett et al., 2004); furthermore, it is also easier to conceal a pregnancy during the first months.

Taking these facts into consideration, it is reasonable to assume that if a woman has not carried out the option of aborting a pregnancy during the first trimester, it is most likely that her latent status is *B**. Based on this reasonable assumption, we can check the robustness of our findings by limiting the sample of women who miscarried to those who miscarried after the third month of pregnancy.

4. Estimation results

4.1. Average impacts

The estimations based on ENADID data confirm that women who become pregnant as teenagers indeed differ from their peers along a number of characteristics (Table 1). They are more likely to be indigenous and to come from less wealthy families, their parents tend to be younger; they are less likely to live in the same household as their fathers, and they begin sexual activity and forming unions at a significantly earlier ages. Most of these characteristics are "pre-treatment" in the sense that although they are measured after the event of interest occurred, they are unlikely to change as a result of the event (such as ethnicity or parental age). Table 1 clearly illustrates the inevitability of a selection bias in a simple comparison of outcomes among women who experienced a pregnancy as teenagers and those who did not. On the other hand, to validate our empirical strategy we need to demonstrate that among women who experienced pregnancy as teenagers those who miscarried are not significantly different from those who had a live birth. Table 4 presents the corresponding analysis. We fail to reject the hypothesis of equality of the means for all cases except parental ethnicity.

Table 5 presents the results of the analysis of the impact of early childbearing on the mother's outcomes. To illustrate the extent of the selection bias we present two types of estimates. First, we regress the outcomes of interest on an indicator for adolescent childbearing as in equation (1) on the sample of all ENADID respondents aged 19 to 30. The results are shown in columns (I) through (III). Second, we limit the sample to women who have experienced pregnancy as teenagers. Columns (IV) through (VI) present these results. In the former case, the group of controls comprises all women who were not rearing a child in their adolescence; in the latter one, the control group includes only women who experienced a pregnancy as adolescents, but did not bring up a child because their pregnancies terminated in a miscarriage.

Both regressions include state fixed effects[10], and an extensive set of controls. Standard errors are clustered at the state level. At the individual level, we control for current age with a set of dummies, indicators for being a household head and a spouse of the household head, the asset index and its square, an indicator for being indigenous, the number of children and its square, the number of children aged 8 to 14 (eligible for the *Oportunidades* program) and its square. Additionally, the regression of log of labor income also includes the number of work hours and dummies for the units in which income was reported (days, weeks, months or year). In order to account for selection bias in the regressions with the log of labor income and the number of hours worked as dependent variables, Heckman correction is used. In the first stage of Heckman estimator selection into employment is modeled as a function of age, ethnicity, the number of children, relationship to the household head, the asset index and its square.

The municipal-level controls include the fraction of: indicator equal to one if the municipality is rural, average grade level, fraction of people aged 15 to 24 attending school at the time of the interview, fractions of population with secondary education completed, with primary education completed, and with no education at all. We also attempt to capture local labor market characteristics by including a fraction of: the employed among the economically active; the employed who work less than 32 hours per week; and the employed who receive less than the minimum wage. We proxy accessibility to health services with a share of residents who have some insurance. Finally, we control for the fractions of residents who are indigenous, for the proportion of Catholics, and for a share of female-headed households.

The comparison of results based on different samples highlights the extent of the selection problem in estimating the consequences of teenage pregnancy and adolescent childbearing. Limiting the sample to women who are truly comparable – i.e. to those who became pregnant as teenagers – frequently reverses the sign of the estimated coefficients, or renders significance to previously insignificant ones. The estimates presented in columns (I) through (III) indeed confirm that adolescent pregnancy is associated with several negative outcomes. For example, women who gave birth as adolescents have on average 0.5 less years of education compared to peers who delayed

[10] The results are similar in magnitude and direction of the effect if municipal fixed effects are included, and are available upon request from the authors.

childbearing until a later age. They are also seven percentage points less likely to be married; and more likely to live in an informal union. They are more dependent on social welfare, which is evidenced in higher income from social assistance and greater probability of participation in various social programs (except for *Seguro Popular*).

Interpreting the impact of adolescent childbearing on employment is less straightforward. While an 11 percentage points higher likelihood to be employed does not qualify as a "negative" consequence of adolescent childbearing, it may be indicative of greater economic need, compared to women who delayed childbearing. Once employed, women who started bringing up children in adolescence tend to work less hours a week.

It is startling, however, to observe the drastic changes in some of the outcome indicators once the sample is limited to women who experienced a pregnancy as adolescents. Once the sample is reduced to truly comparable women, the impact of raising a child on the mother's education becomes positive and significant. Column (IV) shows that women whose pregnancies resulted in a live birth have on average 0.34 more years of education, compared to their counterparts, whose pregnancies ended in miscarriage. Moreover, coefficients that were statistically significant in the previous estimation are no longer distinguishable from 0. For example, there are no significant differences in the likelihood of being married, single or having a partner between women who gave birth and women who miscarried. Similarly, women who were bringing up children since their adolescence do not seem to work less hours. The coefficients on employment status, the likelihood to participate in *Oportunidades*, and social assistance income remain significant and increase in magnitude.

Similar to Hotz et al. (2005), these results suggest that the consequences of adolescent childbearing can hardly qualify as adverse from *individual* standpoint. However, unlike Hotz et al. (2005), our results suggest that the costs of early childbearing are at least partially shouldered by the state: teenage mothers receive 36 percent higher social assistance income and are either 10 (if living with parents) or 14 percentage points more likely to participate in *Oportunidades*.

Thus, it would appear that bearing a child under the age of 19 does not irreparably hurt the prospects of adolescent mothers. It is important to remember, however, that these results estimate the cost of early childbearing, *conditional on being pregnant as a teenager*. As shown in Table 1, women

who become pregnant as teenagers are more vulnerable than their peers along a number of dimensions. This should be taken into consideration when interpreting the findings, which overall seem to confirm the conclusion by Lang (2007) about teenage pregnancy in the US: "What the research does tell us is that girls with poor educational and economic prospects are more likely to become teenage mothers and that, on average, they and their children suffer little, if any, adverse effect from their doing so."

4.2. Heterogeneity in impacts

It is quite plausible that the impacts of early childbearing analyzed here will vary depending on the characteristics of the mothers, as well as on the settings where they give birth. This section explores heterogeneity in the effects of adolescent childbearing by estimating the following regression:

$$Y = \gamma_0 + \gamma_1 B + \lambda_2 BC + \lambda_3 C + \lambda_4 \boldsymbol{X} + \varepsilon, \qquad (2)$$

where, as in equation (1) above, the sample is limited to women who experienced a pregnancy as adolescents; Y is the outcome of interest; B is equal to 1 if the pregnancy resulted in a live birth, and 0 otherwise; C captures a dimension along which we expect some heterogeneity in impacts; and $\boldsymbol{X}$ captures individual and municipal-level controls. Given that miscarriages are random (or at least are not correlated with the error term) the coefficient on the interaction between B and C will indicate whether the impact of raising a child significantly varies along C.

It is important to note that the observed effects should be interpreted with much caution. Our identification strategy allows for rigorously estimating heterogeneous effects of raising a child depending on various individual, household and community level characteristics; however, it is beyond the scope of this study to pinpoint the reasons behind this heterogeneity. Although we propose some potential explanations, they should be treated only as hypotheses suggested for further qualitative and quantitative work.

4.2.1. Heterogeneity in impacts depending on individual and household level characteristics

Wealth and ethnicity are among the top candidates for the characteristics that can either mitigate or exacerbate the consequences of adolescent childbearing. To capture the interplay of adolescent childbearing and welfare, we explore heterogeneity in the impact of giving birth as a teenager

depending on an asset index (a proxy for household long-run wealth). Another dimension along which the impacts may vary is age at birth. Although we define teen pregnancy as a pregnancy experienced by a woman younger than 19, the phenomenon and its consequences are likely to be different for an 18 year-old versus for a 12 year old. To explore this possibility, we compare the effects of bringing up a child on girls who gave birth before the age of 17, to the effects of adolescent childbearing on girls who gave birth after the age of 17. Our analysis suggests that labor market and family formation outcomes indeed vary along these dimensions.

Labor market outcomes

Table 6 shows that the probability of being employed increases with asset index. An increase in the asset index by one standard deviation raises the probability of being employed by 16 percentage points. Higher chances to have a job among teenage mothers from wealthier families may reflect greater availability of support, which is likely to make childbearing more compatible with a job. Alternatively, this effect may be due to the better networks of wealthier families. Notably, the likelihood of being employed is lower among indigenous adolescent mothers, compared to non-indigenous women who gave birth in adolescence (Table 7). Given that in Mexico ethnicity is strongly associated with poverty status, this result is consistent with an increase in probability of employment in asset index.

The impact of adolescent childbearing on labor income also varies along two dimensions. Earnings of adolescent mothers are lower among women with higher asset indices, and higher among women who gave birth at the age of 16 and younger. A plausible explanation of the latter finding is that the extended family (parents and other relatives) help to shoulder the burden of raising a child more when the pregnancy occurs earlier. Younger women may also be more likely to live with their parents, potentially enjoying free childcare. They may feel less pressured to give up studies or to seek part-time jobs. Alternatively, we may be observing a "recovery" effect. For any given age, a woman who gave birth earlier will have had a longer "recovery" time – period in which her child is old enough to require less care – and consequently will have had a chance to participate in the labor market for longer. The greater length of labor market participation may account for greater labor income.

Family formation outcomes

The impacts of adolescent childbearing vary along the asset index dimension for family formation outcomes. Adolescent mothers from wealthier families are less likely to be single, and more likely to have a partner. The likelihood of being married does not seem to be affected differentially depending on the wealth of the household. This heterogeneity in the effect of adolescent childbearing suggests that the welfare of women plays a non-trivial role in the matching decisions – however, the ENADID data do not provide sufficient information to pinpoint the mechanism behind the observed effect.

4.2.2. Heterogeneity in impacts depending on community level characteristics

Individual and household characteristics are not the only elements that can lessen or magnify the consequences of bringing up a child in adolescence. Certain features of the community may add yet another degree of difficulty to parenting a child as a teenager. Alternatively, the community may shoulder part of the burden – for example by providing educational opportunities or childcare. Following, we explore heterogeneity in the effects of adolescent childbearing depending on the municipal characteristics which provide imperfect proxies for educational and labor market opportunities in the municipality. Specifically, we analyze how labor market, education and welfare dependence outcomes differ depending on the fraction of residents aged 15 to 24 who are currently attending an educational institution and the fraction of employed who work 32 hours or less. These two characteristics are likely to capture educational and labor market opportunities in the municipality.

Labor market outcomes

Raising a child differentially affects the labor market income, the probability of employment and the number of hours worked of teenage mothers depending on municipal-level characteristics. Although we do not observe an average effect of giving birth on labor market income (Table 5), we detect significant effects in specific labor markets, namely those characterized by high fractions of: individuals attending school (among people aged 15 to 24) and individuals who work less than 32 hours a week (among all employed).

Raising a child positively affects the earnings of women who became pregnant as teenagers in both types of markets considered. Coefficients on the corresponding interactions in Tables 9 and 10 are positive and significant. For women who gave birth, a one percentage point increase in the fraction of school attendees and in the underemployed respectively trigger 4 and 11 percent increases in labor incomes.

What could be the mechanism behind the observed effect of giving birth on earnings? It is not improbable that the likelihood to become pregnant as an adolescent is associated with some degree of recklessness, lack of ambition or carelessness. The need to take care of a baby may at least partially mitigate these qualities[11]. Thus, a plausible mechanism behind the observed positive effect of bringing up a child on labor income may be due to the fact that young mothers become more responsible (and hence, more employable) after they give birth. This effect is more pronounced in the municipalities characterized by better educational opportunities; as well as in municipalities where the share of part-time employment is higher – and teenage mothers are probably more competitive.

Another labor market outcome of potential teenage mothers that is differentially affected by giving birth is the likelihood of employment. The probability of being employed varies significantly depending on the fraction of adolescents (aged 15 to 24) who attend school. On average, raising a child substantially increases the likelihood that a teenage woman is employed (Table 5). However, this probability is lower for women who live in municipalities with more students. A 1 percentage point increase in the fraction of students among 15 to 24 year-olds lowers the probability that a teenage mother is employed by nearly 1 percentage point. There is an optimistic and a pessimistic explanation for this effect. On the one hand, a higher prevalence of students may be indicative of better educational opportunities, and teenage mothers may be taking advantage of them. Alternatively, greater numbers of 15 to 24 year-olds may choose to pursue more education because of tighter competition for jobs, and teenage mothers may not be sufficiently competitive. However, considering the positive effect of higher prevalence of students on the labor earnings of teenage mothers, the former explanation seems more plausible.

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[11] To some extent, numerous appearances of teenage mothers in press claiming that "their babies saved them" serve as an anecdotal evidence of such mechanism.

Dependence on welfare

Prior analysis has shown that adolescent childbearing imposes additional costs on the state: teenage mothers are more likely to participate in social programs and receive higher social assistance income. The magnitude of these effects varies depending on the municipal characteristics.

Table 10 shows that the social assistance income of teenage mothers increases with the fraction of underemployed in their municipality. Indeed, a 1 percentage point increase in the fraction of people working less than 32 hours a week triggers a 7 percent increase in the social assistance income of teenage mothers. A higher fraction of underemployed is also associated with an increased likelihood of participation in some social program (for women who live with their parents only) and *ProCampo* (for all teenage mothers: those who live independently and those who live with parents). Only the likelihood of participation in *Linconsa* decreases with the fraction of underemployed. Overall, the data indicate that dependence on social welfare among teenage mothers is higher in the settings characterized by worse labor market and economic opportunities (captured in an indicator of underemployment).

It is important to emphasize that while our identification strategy allows for rigorous estimation of heterogeneity in the effect of adolescent childbearing depending on individual, household and municipal characteristics, it is beyond the scope of this analysis to pinpoint the mechanisms that drive such heterogeneity. Hence our findings should be considered with much caution; for example, as indications of promising areas for further qualitative and quantitative work.

5. Alternative explanations and robustness checks

As discussed in the previous sections, validity of the proposed identification strategy hinges on the assumption that women, who upon becoming pregnant decide to abort, are not mistakenly included into the control group. This may happen if a woman who would choose to abort suffers a miscarriage before she is able to actually act on her decision.

Inclusion of these women into the control group is likely to result in overestimating negative effects of bearing a child as a teenager. Women who choose to have an abortion are likely to come from more favored backgrounds. Ashcraft and Lang (2006) show it with the US data; the same is likely to hold for Mexico – although due to underreporting of abortions we cannot demonstrate it with the

ENADID data. Consequently, if women who would have chosen to abort are included in the sample of women who miscarried, this sample is more favorably selected than the sample of women who gave birth. The results from estimation carried out on these samples would be biased towards finding adverse effects of teenage childbearing.

To address the issue of the potential bias, we can take advantage of the fact that ENADID reports the amount of time elapsed between the start of the pregnancy and the date when miscarriage occurred. It is reasonable to assume that those women, who would choose to abort, would prefer to do so during the early months of pregnancy. Medical literature confirms that the likelihood of complications due to abortion increases with every month of pregnancy (Bartlett et al., 2004). Many pregnancies are associated with physical discomfort. Additionally, it is easier to conceal a pregnancy during earlier months. Overall, it appears that a pregnant woman who does not want to give birth does not have any incentive to extend the length of her pregnancy. Therefore excluding women who miscarried during the first two months of pregnancy from the control group seems to be a reasonable safeguard against contaminating the sample with women who are not comparable to the "treated" in their latent status. Intuitively, we include in the control group only women whose pregnancies lasted long enough to conclude that they were not planning to have an abortion. However, it is important to keep in mind that such modification reduces the sample size, which may increase the standard errors and result in lower significance of the estimated coefficients.

Table 11 shows that our results are highly robust to such modification in the sample. Raising a child has a significant and positive effect on the level of social assistance income, the likelihood of employment and on participation in *Oportunidades*. Indeed, nearly all of the outcomes that appear to be significantly impacted by bearing a child in the full sample of potential teenage mothers remain significant in the reduced sample. Their magnitude is nearly identical. The only exception is education – the coefficient is positive, but no longer significant. Overall, the results in Table 12 suggest that our findings are not driven by erroneous inclusion of not truly comparable women into the control group.

Another consideration that may cast doubts on the interpretation of our results is that miscarriage may not be without costs. Therefore, an alternative interpretation of our findings is that rather than reflecting the consequences of bearing a child, these results reflect the costs of having suffered a miscarriage. To rule out this explanation, we test whether having had a miscarriage has an effect on

women who did not become pregnant during adolescence. More specifically, we compare two groups of women who did not experience an adolescent pregnancy: women who had one child and one miscarriage, and women who had one child and no miscarriages. We restrict the sample to these two groups for the following reasons. First, it is important to exclude repeat miscarriages. Re-occurring miscarriages are not random and are likely to be linked to some important unobservable characteristics (Wang et al., 2003). Moreover, re-occurring miscarriages are likely to result in lasting stress for a woman, which may impair other outcomes. Women in our control group of potential teenage mothers suffered from no more than one miscarriage. Second, when comparing women who had a miscarriage to women who did not, it is easy to capture the effect of having children as opposed to the effect of having a miscarriage if women who only had a miscarriage, and no other pregnancies, dominate the sample. Therefore we attempt to make our control and treated groups more comparable by limiting the sample to women who had one child and zero or one miscarriage only.

To capture the effect of having a miscarriage, we run a regression as in equation (1) on the sample of women with one child and zero or one miscarriage, where *B* takes the value of one if a woman had one child and zero miscarriages. *B* is equal to zero if a woman had one child and one miscarriage. The coefficient on this indicator variable should capture the effect of not having a miscarriage in adulthood. It is very similar in spirit to the specification we used to capture the effect of adolescent childbearing. Therefore, finding a significant effect of not having a miscarriage would cast doubts on interpreting our earlier results as consequences of adolescent childbearing. However, results shown in Table 12 prove that this is unlikely.

There are no significant effects on any outcome, with only three exceptions: women who did not miscarry are less likely to be single; more likely to have a partner and to work less hours. However, it is important to take into consideration an important caveat; although we attempted to limit the sample to comparable women, they differ in that one group experienced only one pregnancy, while the other group had two pregnancies. An intention to have a second baby, shared by at least some women[12] in the group who miscarried, may be indicative of stronger attachment within the couple, stronger confidence in the partner or simply greater ability "to afford" the second child. These

[12] It is plausible that some of the pregnancies were not intended.

differences may account for the significant effect of not having a miscarriage on the probability of being single and having a partner, and the number of hours worked. However, the absence of any impact on education, level of social assistance, likelihood to be employed and to participate in social programs confirms that the observed impacts found in the earlier analysis should indeed be attributed to adolescent childbearing.

6. Conclusions

Our paper aims at answering the following question: Conditional on being pregnant as a teenager, what is the cost of early child bearing? What would the change in welfare be if women who are likely to become pregnant during adolescence delayed child bearing? To answer this question, we take advantage of a natural experiment: namely, we compare the outcomes of women who became pregnant in adolescence and gave birth to outcomes of women who became pregnant in adolescence and miscarried. This approach only allows for estimating the costs of adolescent childbearing for teenagers in a risk group, i.e. teenagers who are likely to experience a pregnancy. It does not provide information on how the life trajectory of a woman with a low probability of becoming pregnant in her teens would change, had she been randomly assigned the task of raising a child during her adolescence.

However despite this limitation, our approach offers certain advantages. First, it allows us to circumvent the issue of selection bias. The data confirm that women who become pregnant as teenagers differ from women who delay childbearing along a number of important dimensions. Our results suggest that they are likely to be indigenous and to come from less wealthy families. Their parents are significantly younger; they are less likely to live in the same household as their fathers, and tend to begin sexual activity and to form unions at a significantly earlier ages. Consequently, by focusing on women who became pregnant in their adolescent years, we limit our analysis to truly comparable samples. Second, the group we focus on – teenagers who are at risk of becoming pregnant - is the relevant population from a policy standpoint. Most of the policy instruments designed to tackle the problem of teenage pregnancy will be targeted at this group.

Although using miscarriages as a natural experiment makes it possible to circumvent the problem of selection bias, it is not without caveats. We demonstrate that among women who became pregnant as adolescents, women who miscarried do not significantly differ in observable characteristics from

those who carried the pregnancy to term. However, we cannot completely rule out the possibility that they may differ along some unobservable dimensions. Therefore it is important to interpret the results with caution.

Once the selection bias is accounted for, and the sample is limited to women who experienced a pregnancy during adolescence, we find a counter-intuitive effect: results suggest that, contrary to popular thinking, adolescent childbearing does not hamper the lifelong opportunities of the young mothers. More specifically, women who gave birth during their adolescence have on average 0.34 more years of education, and are 21 percentage points more likely to be employed, compared to their counterparts who miscarried. Our results also suggest greater dependence on social welfare among women who gave birth during adolescence: their social assistance income is 36 percent higher, and they are more likely to participate in social programs, especially *Oportunidades*. Our results suggest that from individual perspective there are barely any negative consequences of raising a child.

We also find that the effect of adolescent childbearing varies depending on individual, household and community characteristics. For example, the impact of adolescent childbearing on employment is higher among wealthier women. Labor income is affected differentially, depending on the community characteristics where the teenage mother lives. However, it is beyond the scope of this paper to pinpoint the channels through which the observed effects occur. Therefore, these findings should be taken only as an indication of the need for further quantitative and qualitative research.

It is important to keep in mind that our paper estimates the cost of early childbearing, *conditional on being pregnant as a teenager*. Women who become pregnant as teenagers differ from their peers along a number of dimensions. Jointly, these dimensions suggest that the opportunities of these women to receive education and progress toward rewarding and fulfilling jobs – the pursuits believed to be hampered due to raising a child – are limited regardless of their fertility decisions. Therefore their opportunity cost of having a child may be much lower than for women who choose to delay childbearing until later age. Given the constraints they are facing, for this vulnerable group the decision to have a baby early may not be as irrational as it is popularly perceived. Hence the relevant policy recommendation would be to focus more on improving the lifelong opportunities of young women who are at risk of adolescent pregnancy. Our results fully support Lang's (2007) conclusion:

"To prevent teenage pregnancy, then, we must change reality. We must improve the educational and economic prospects of potential teenage mothers".

Although our results indicate that the individual costs of adolescent childbearing are low, they by no means suggest that adolescent childbearing is costless. Teenage mothers are more likely to depend on social welfare: their social assistance incomes are 27 percent higher compared to women who miscarried during adolescence, and they are more likely to participate in social programs. Moreover, our data do not allow us to estimate the impact on other potential cost bearers apart from the state – parents of the adolescent mother and her siblings, who may help to shoulder the burden. Finally, our analysis does not include all possible outcomes among potential teenage mothers that can be affected by raising a child. Some outcomes that are potentially negatively affected, such as quality and stressfulness of work, are absent from this study due to the lack of data.

Our results by no means suggest that having children during adolescence is a desirable outcome, and should not be interpreted as such. However, they do suggest that for a group of young women who are likely to get pregnant during adolescence, giving birth and raising a child may not be an irrational decision, given the poor educational and economic opportunities they are facing. This conclusion is especially important for the choice of policies that could effectively tackle the problem of adolescent childbearing. Our findings suggest that policies aimed at improving young women's opportunities may be more successful and effective than policies targeted at the decision to have a child per se.

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Is the baby to blame? Tables.

Table 1: Background characteristics

	mean among women who did not get pregnant before 19	difference with those who got pregnant before 19	number of observations
indigenous	0.16***	0.06***	7,803
	(0.01)	(0.02)	
mother lives in the same house	0.26***	-0.02	7,803
	(0.01)	(0.02)	
father lives in the same house	0.19***	-0.05***	7,803
	(0.01)	(0.01)	
asset index	-0.00	-0.66***	7,312
	(0.04)	(0.07)	
already had sexual relations?	0.99***	-0.01*	7,805
	(0.00)	(0.00)	
age when started having sex	19.23***	-3.63***	7,504
	(0.05)	(0.06)	
age at which had first union	20.18***	-4.03***	6,329
	(0.06)	(0.09)	
equal to 1 if first union was a marriage	0.02***	0.01**	6,952
	(0.00)	(0.00)	
number of children	1.57***	0.64***	7,805
	(0.02)	(0.04)	
number of children aged between 8 and 14	0.27***	0.55***	7,805
	(0.01)	(0.03)	
mother's education	6.06***	-0.21	7787
	(0.35)	(0.34)	
mother's age	49.05***	-2.57***	7742
	(0.74)	(0.57)	
mother is indigenous	0.18***	0.04	7756
	(0.04)	(0.05)	
father's education	6.58***	-0.16	7800
	(0.55)	(0.38)	
father's age	48.15***	-1.86**	7756
	(2.14)	(0.72)	
father is indigenous	0.17***	-0.00	7800
	(0.04)	(0.03)	

The table presents coefficients from a regression on a constant and a dummy for women who had at least one pregnancy before 19

Table 2: Abortion underreporting in ENADID 2006

ENADID estimate	
number of induced abortions in 2005	8,438
Estimates based on hospitalization data and health professionals surveys for 2006*	
Low	725,070
Medium	874,747
High	1,024,424

* presented in Juarez et al. (2006)

Table 3: ENADID 2006 pregnancy statistics

		number	percent of all live births
1	miscarriages	176,701	8.18%
2	miscarriages after 1 month of pregnancy	152,233	7.05%
3	induced abortions	8,438	0.39%
4	infant deaths	44,621	2.07%
5	stillbirths	15,352	0.71%
6	live births	1,914,058	88.65%
7	all pregnancies (1+3+4+5+6)	2,159,170	100.00%
8	number of women aged 15 to 44	24,779,239	

Table 4: Background characteristics. Miscarriages vs. live births among teenagers

	mean among women who gave birth before 19	difference with women who got pregnant before 19 and miscarried	number of observations
indigenous	0.22***	0.01	2,628
	0.03	0.05	
mother lives in the same house	0.21***	0.08	2,628
	0.02	0.08	
father lives in the same house	0.13***	0.00	2,628
	0.01	0.03	
mother's education	5.01***	-0.72	
	0.99	1.29	2625
mother's age	45.83***	-2.57	2,607.00
	2.98	1.57	
mother is indigenous	0.35*	-0.15**	2,619.00
	0.19	0.06	
father's education	6.60***	-2.21	2625
	1.42	1.40	
father's age	31.95***	0.44	2613
	3.58	1.64	
	0.09	-0.07	2625
father is indigenous	0.15	0.06	

The table presents coefficients from a regression on a constant and a dummy for women who had at least one pregnancy before 19

Table 5: Effects of not delaying childbearing - comparison of estimates

	Sample of all women aged 19 to 30			Sample limited to women who got pregnant as teenagers		
	coefficient and standard error	number of observations	R2	coefficient and standard error	number of observations	R2
household participates in Oportunidades	0.03** (0.02)	7,137	0.299	0.10*** (0.04)	2,410	0.356
household participates in Seguro Popular	-0.02** (0.01)	7,130	0.122	-0.01 (0.05)	2,405	0.146
household participates in ProCampo	0.00 (0.01)	7,132	0.100	-0.04 (0.03)	2,406	0.166
household participates in Liconsa	0.01 (0.01)	7,128	0.094	0.00 (0.03)	2,406	0.112
household participates in some social program	0.02 (0.02)	7,139	0.198	0.07 (0.06)	2,410	0.243
years of education	-0.50*** (0.13)	7,165	0.333	0.34* (0.20)	2,416	0.347
is currently employed	0.07*** (0.02)	7,165	0.151	0.21*** (0.05)	2,416	0.189
log of social assistance income	0.21** (0.09)	7,151	0.218	0.36*** (0.10)	2,410	0.294
is married	-0.07** (0.03)	7,165	0.195	-0.02 (0.08)	2,416	0.211
has a partner	0.04** (0.02)	7,165	0.241	0.05 (0.05)	2,416	0.253
is single	-0.07*** (0.01)	7,165	0.356	-0.08 (0.06)	2,416	0.391
log of labor income	0.00 (0.03)	7,028		0.11 (0.20)	2,453	
number of hours worked	-2.37** (1.05)	7,165		-11.76 (8.93)	2,495	
For the following variables, the sample is limited to women who are either heads of hhs or spouses of hh heads						
participation in Oportunidades	0.06*** (0.02)	4,633	0.305	0.14*** (0.03)	1,660	0.380
household participates in Seguro Popular	-0.00 (0.01)	4,629	0.123	0.05 (0.04)	1,656	0.157
household participates in	0.01*	4,629	0.110	-0.06	1,656	0.204

ProCampo	(0.01)			(0.05)		
household participates in Liconsa	0.01 (0.01)	4,628	0.096	-0.00 (0.03)	1,656	0.132
household participates in some social program	0.04* (0.02)	4,635	0.220	0.10 (0.06)	1,660	0.274

All regressions include state fixed effects. Standard errors clustered at state level. Individual level control variables include: dummies for current age, asset index, square of asset index, indicator for being indigenous, indicator for being the household head and indicator, indicator for being the spouse of the household head, number of children and its square, number of children aged 8 to 14 (eligible for Oportunidades) and its square. Municipal level controls include: percent of insured, of children who attend school among 15 to 24 y.o.; percent speaking indigenous language, catholic, employed, working less than 32 hours, receiving less than minimal wage, average grade level, percent with secondary education completed, with primary education completed, and with no education; percent of female headed households. Regressions with log of labor income as dependent variables also include number of work hours and dummies for units in which income was reported (days, weeks, months or year). Heckman correction used for regressions with log of labor income and number of hours worked as dependent variables.

Table 6: the consequences of adolescent childbearing - heterogeneous effects

	Without interaction		With interaction				
	indicator for live birth	asset index	indicator for live birth	asset index	interaction between live birth indicator and asset index	number of observations	R2
household participates in Oportunidades	0.10*** (0.04)	-0.06*** (0.01)	0.10*** (0.03)	-0.05** (0.02)	-0.00 (0.02)	2,410	0.356
household participates in Seguro Popular	-0.01 (0.05)	-0.03*** (0.00)	-0.01 (0.05)	-0.06** (0.03)	0.03 (0.03)	2,405	0.147
household participates in ProCampo	-0.04 (0.03)	-0.00 (0.00)	-0.03 (0.02)	-0.05** (0.02)	0.05** (0.02)	2,406	0.171
household participates in Liconsa	0.00 (0.03)	0.01 (0.01)	0.01 (0.03)	-0.01 (0.03)	0.03 (0.03)	2,406	0.112

household participates in some social program	0.07	-0.05***	0.07	-0.07**	0.02	2,410	0.244
	(0.06)	(0.01)	(0.06)	(0.03)	(0.03)		
years of education	0.34*	0.72***	0.28	0.97***	-0.26	2,416	0.347
	(0.20)	(0.08)	(0.22)	(0.27)	(0.27)		
is currently employed	0.21***	0.02*	0.23***	-0.07**	0.08***	2,416	0.192
	(0.05)	(0.01)	(0.05)	(0.03)	(0.03)		
log of social assistance income	0.36***	-0.18***	0.36***	-0.18**	-0.01	2,410	0.294
	(0.10)	(0.03)	(0.10)	(0.08)	(0.07)		
is married	-0.02	0.04***	-0.03	0.06	-0.02	2,416	0.211
	(0.08)	(0.01)	(0.08)	(0.05)	(0.05)		
has a partner	0.05	-0.01*	0.06	-0.07***	0.07***	2,416	0.257
	(0.05)	(0.01)	(0.05)	(0.02)	(0.02)		
is single	-0.08	0.01	-0.09	0.07**	-0.06*	2,416	0.393
	(0.06)	(0.01)	(0.06)	(0.03)	(0.04)		
log of labor income	0.11	0.18***	-0.08	0.34***	-0.17*	2,346	
	(0.19)	(0.03)	(0.17)	(0.10)	(0.09)		
number of hours worked	-12.07	-0.66	-7.69	-6.32*	5.53	2,346	
	(9.12)	(0.78)	(5.38)	(3.75)	(3.66)		

For the following variables, the sample is limited to women who are either heads of hhs or spouses of hh heads

	Without interaction		With interaction				
participation in Oportunidades	0.14***	-0.07***	0.14***	-0.08***	0.01	1,660	0.380
	(0.03)	(0.01)	(0.03)	(0.02)	(0.02)		
household participates in Seguro Popular	0.05	-0.04***	0.06**	-0.06**	0.02	1,656	0.157
	(0.04)	(0.01)	(0.03)	(0.03)	(0.03)		
household participates in ProCampo	-0.06	-0.00	-0.03	-0.04	0.04	1,656	0.207
	(0.05)	(0.00)	(0.03)	(0.03)	(0.03)		
household participates in Liconsa	-0.00	0.01	-0.02	0.04*	-0.03	1,656	0.133
	(0.03)	(0.01)	(0.04)	(0.02)	(0.02)		
household participates in some social program	0.10	-0.06***	0.11*	-0.08***	0.02	1,660	0.274
	(0.06)	(0.01)	(0.06)	(0.03)	(0.03)		

All regressions include state fixed effects. Standard errors clustered at state level. For detailed description of control variables, see note to Table 5.

Table 7: the consequences of adolescent childbearing - heterogeneous effects

	indicator for live birth	indigenous	indicator for live birth	indigenous	interaction between live birth indicator and indigenous	number of observations	R2
	Without interaction		With interaction				
household participates in Oportunidades	0.10***	0.04*	0.09**	0.01	0.03	2,410	0.356
	(0.04)	(0.02)	(0.04)	(0.09)	(0.08)		
household participates in Seguro Popular	-0.01	0.00	-0.04	0.13*	-0.12**	2,405	0.147
	(0.05)	(0.03)	(0.05)	(0.07)	(0.06)		
household participates in ProCampo	-0.04	-0.01	-0.00	-0.18	0.17	2,406	0.171
	(0.03)	(0.01)	(0.02)	(0.13)	(0.12)		
household participates in Liconsa	0.00	-0.01	-0.01	0.06	-0.06	2,406	0.112
	(0.03)	(0.01)	(0.03)	(0.04)	(0.04)		
household participates in some social program	0.07	0.03	0.07	-0.03	0.06	2,410	0.243
	(0.06)	(0.02)	(0.07)	(0.12)	(0.13)		
years of education	0.34*	-0.63***	0.38	-0.42	-0.22	2,416	0.347
	(0.20)	(0.22)	(0.27)	(1.15)	(1.11)		
is currently employed	0.21***	-0.08**	0.28***	0.27*	-0.36**	2,416	0.192
	(0.05)	(0.04)	(0.05)	(0.15)	(0.15)		
log of social assistance income	0.36***	0.16*	0.38***	0.27	-0.11	2,410	0.294
	(0.10)	(0.09)	(0.13)	(0.40)	(0.37)		
is married	-0.02	0.01	-0.02	0.03	-0.02	2,416	0.211
	(0.08)	(0.04)	(0.09)	(0.15)	(0.15)		
	(0.05)	(0.03)	(0.06)	(0.07)	(0.07)		
is single	-0.08	-0.02	-0.08	-0.03	0.02	2,416	0.391
	(0.06)	(0.03)	(0.07)	(0.11)	(0.10)		
log of labor income	0.11	-0.19*	-0.01	-0.42	0.24	2,346	
	(0.19)	(0.12)	(0.22)	(0.38)	(0.38)		
number of hours worked	-12.07	2.72	-5.38	18.78	-15.64	2,346	
	(9.12)	(2.98)	(5.00)	(18.15)	(18.75)		

For the following variables, the sample is limited to women who are either heads of hhs or spouses of hh heads							
	Without interaction		With interaction				
participation in Oportunidades	0.14***	0.03	0.16***	-0.09	0.12	1,660	0.381
	(0.03)	(0.03)	(0.04)	(0.10)	(0.10)		
household participates in Seguro Popular	0.05	-0.00	0.02	0.07	-0.07	1,656	0.157
	(0.04)	(0.03)	(0.04)	(0.06)	(0.06)		
household participates in ProCampo	-0.06	-0.00	-0.01	-0.16	0.15	1,656	0.208
	(0.05)	(0.02)	(0.02)	(0.14)	(0.13)		
household participates in Liconsa	-0.00	-0.00	-0.02	0.05	-0.05	1,656	0.132
	(0.03)	(0.02)	(0.04)	(0.06)	(0.05)		
household participates in some social program	0.10	0.03	0.14**	-0.13	0.15	1,660	0.275
	(0.06)	(0.04)	(0.06)	(0.14)	(0.14)		

All regressions include state fixed effects. Standard errors clustered at state level. For detailed description of control variables, see note to Table 5.

Table 8: the consequences of adolescent childbearing - heterogeneous effects

	indicator for live birth	gave birth under 17	indicator for live birth	gave birth under 17	interaction between live birth indicator and giving birth under 17	number of observations	R2
	Without interaction		With interaction				
household participates in Oportunidades	0.10***	-0.01	0.08**	-0.07	0.09	2,410	0.355
	(0.04)	(0.03)	(0.03)	(0.05)	(0.07)		
household participates in Seguro Popular	-0.01	0.00	0.01	0.13	-0.14	2,405	0.147
	(0.05)	(0.02)	(0.03)	(0.18)	(0.19)		
household participates in ProCampo	-0.04	0.00	-0.02	0.14	-0.14	2,406	0.168
	(0.03)	(0.01)	(0.03)	(0.13)	(0.13)		
household participates in Liconsa	0.00	-0.04*	-0.00	-0.05	0.02	2,406	0.111
	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)		
household participates in some	0.07	-0.03	0.09**	0.20	-0.21	2,410	0.243

social program	(0.06)	(0.05)	(0.04)	(0.18)	(0.20)		
years of education	0.34*	0.12	0.34*	-1.26	0.71	2,416	0.338
	(0.20)	(0.24)	(0.20)	(0.80)	(0.84)		
is currently employed	0.21***	0.06*	0.21***	0.15	-0.04	2,416	0.187
	(0.05)	(0.03)	(0.06)	(0.19)	(0.19)		
log of social assistance income	0.36***	-0.16	0.30***	-0.24	0.24	2,410	0.293
	(0.10)	(0.14)	(0.10)	(0.30)	(0.35)		
is married	-0.02	-0.03	-0.03	-0.14	0.08	2,416	0.211
	(0.08)	(0.02)	(0.09)	(0.15)	(0.14)		
	(0.05)	(0.02)	(0.06)	(0.09)	(0.09)		
is single	-0.08	0.03	-0.05	0.12	-0.12	2,416	0.390
	(0.06)	(0.03)	(0.06)	(0.10)	(0.09)		
log of labor income	0.11	0.18**	-0.10	-0.64**	0.83***	2,346	
	(0.19)	(0.09)	(0.16)	(0.32)	(0.32)		
number of hours worked	-12.07	2.35	-6.10	29.51	-28.44	2,453	
	(9.12)	(2.87)	(5.89)	(19.92)	(20.08)		

For the following variables, the sample is limited to women who are either heads of hhs or spouses of hh heads

	Without interaction		With interaction				
participation in Oportunidades	0.14***	-0.03	0.12***	-0.07	0.06	1,660	0.380
	0.03	0.04	0.03	0.08	0.10		
household participates in Seguro Popular	0.05	-0.01	0.04	-0.09	0.07	1,656	0.157
	0.04	0.02	0.03	0.07	0.07		
household participates in ProCampo	-0.06	0.00	-0.02	0.21	-0.21	1,656	0.209
	0.05	0.01	0.04	0.18	0.18		
household participates in Liconsa	-0.00	-0.05	-0.00	-0.02	-0.01	1,656	0.131
	0.03	0.03	0.04	0.04	0.05		
household participates in some social program	0.10	-0.05	0.10**	0.05	-0.07	1,660	0.273
	0.06	0.05	0.04	0.17	0.19		

All regressions include state fixed effects. Standard errors clustered at state level. For detailed description of control variables, see note to Table 5.

Table 9: the consequences of adolescent childbearing - heterogeneous effects

	indicator for live birth	fraction of population aged 15 to 24 who attend school	indicator for live birth	fraction of population aged 15 to 24 who attend school	interaction between live birth indicator and fraction who attend school	number of observations	R2
	Without interaction		With interaction				
household participates in Oportunidades	0.10***	-0.01	0.06*	0.01	0.07	2,410	0.303
	(0.04)	(0.34)	(0.04)	(0.48)	(0.43)		
household participates in Seguro Popular	-0.01	0.49	-0.01	0.55	-0.38	2,405	0.168
	(0.05)	(0.32)	(0.04)	(0.45)	(0.42)		
household participates in ProCampo	-0.04	-0.19	-0.03	-0.02	-0.05	2,406	0.120
	(0.03)	(0.13)	(0.03)	(0.29)	(0.28)		
household participates in Liconsa	0.00	0.38	-0.02	0.21	-0.08	2,406	0.063
	(0.03)	(0.28)	(0.02)	(0.40)	(0.40)		
household participates in some social program	0.07	0.48*	0.04	0.10	-0.03	2,410	0.240
	(0.06)	(0.27)	(0.04)	(0.51)	(0.47)		
years of education	0.34*	1.67	0.39	2.56	-1.07	2,416	0.300
	(0.20)	(1.75)	(0.30)	(3.92)	(3.70)		
is currently employed	0.21***	0.03	0.18***	0.54	-0.98**	2,416	0.168
	(0.05)	(0.32)	(0.04)	(0.46)	(0.42)		
log of social assistance income	0.36***	-0.44	0.37**	0.32	0.07	2,410	0.258
	(0.10)	(1.06)	(0.15)	(1.77)	(1.40)		
is married	-0.02	0.13	-0.04	-0.21	0.31	2,416	0.205
	(0.08)	(0.43)	(0.06)	(0.61)	(0.55)		
	(0.05)	(0.24)	(0.03)	(0.37)	(0.36)		
is single	-0.08	0.12	-0.04	-0.25	0.42	2,416	0.367
	(0.06)	(0.24)	(0.04)	(0.42)	(0.39)		
log of labor income	0.11	-0.91	-0.32	-3.52**	3.85**	2,346	
	(0.19)	(0.76)	(0.27)	(1.67)	(1.87)		
number of hours worked	-12.07	7.44	-15.75	-160.09**	149.27*	2,388	
	(9.12)	(28.63)	(10.26)	(79.77)	(77.42)		

For the following variables, the sample is limited to women who are either heads of hhs or spouses of hh heads

	Without interaction		With interaction				
participation in Oportunidades	0.14***	0.19	0.11***	-0.05	0.12	1,660	0.332
	(0.03)	(0.27)	(0.04)	(0.43)	(0.37)		
household participates in Seguro Popular	0.05	0.64	0.03	0.51	-0.26	1,656	0.162
	(0.04)	(0.43)	(0.04)	(0.44)	(0.38)		

household participates in ProCampo	-0.06 (0.05)	-0.10 (0.17)	-0.03 (0.03)	0.33 (0.30)	-0.12 (0.28)	1,656	0.117
household participates in Liconsa	-0.00 (0.03)	0.34 (0.26)	0.01 (0.02)	0.32 (0.24)	-0.13 (0.22)	1,656	0.084
household participates in some social program	0.10 (0.06)	0.66** (0.31)	0.09* (0.05)	0.14 (0.56)	0.06 (0.50)	1,660	0.273

All regressions include state fixed effects. Standard errors clustered at state level. For detailed description of control variables, see note to Table 5.

Table 10: the consequences of adolescent childbearing - heterogeneous effects

	indicator for live birth	fraction of employed who work less than 32 hours a week	indicator for live birth	fraction of employed who work less than 32 hours a week	interaction between live birth indicator and fraction working less than 32 hours	number of observations	R2
	Without interaction		With interaction				
household participates in Oportunidades	0.10***	0.54	0.11***	-0.16	0.71	2,410	0.356
	(0.04)	(0.35)	(0.03)	(0.58)	(0.55)		
household participates in Seguro Popular	-0.01	0.59**	-0.02	0.99	-0.41	2,405	0.146
	(0.05)	(0.25)	(0.05)	(0.63)	(0.63)		
household participates in ProCampo	-0.04	0.40**	-0.03	-0.19	0.60	2,406	0.166
	(0.03)	(0.16)	(0.03)	(0.65)	(0.59)		
household participates in Liconsa	0.00	-0.47***	-0.01	0.33	-0.82*	2,406	0.112
	(0.03)	(0.18)	(0.03)	(0.46)	(0.45)		
household participates in some social program	0.07	0.36	0.06	0.74	-0.39	2,410	0.244
	(0.06)	(0.32)	(0.07)	(0.83)	(0.80)		
years of education	0.34*	5.29***	0.51**	-4.68	10.11*	2,416	0.347
	(0.20)	(1.33)	(0.26)	(5.58)	(5.22)		
is currently employed	0.21***	-0.23	0.19***	1.12	-1.37	2,416	0.190
	(0.05)	(0.32)	(0.06)	(1.11)	(1.05)		
log of social assistance income	0.36***	2.59**	0.49***	-4.89**	7.58***	2,410	0.294
	(0.10)	(1.29)	(0.09)	(2.28)	(1.96)		
is married	-0.02	0.30	-0.01	-0.41	0.72	2,416	0.211
	(0.08)	(0.22)	(0.07)	(1.64)	(1.70)		
	(0.05)	(0.11)	(0.05)	(0.67)	(0.71)		
is single	-0.08	0.22	-0.07	0.01	0.21	2,416	0.391
	(0.06)	(0.15)	(0.05)	(0.95)	(0.97)		
log of labor income	0.11	1.40	0.09	-9.41*	10.73**	2,346	
	(0.19)	(1.39)	(0.18)	(5.29)	(4.66)		
number of hours worked	-12.07	21.62	-12.11	92.20	-70.09	2,388	
	(9.12)	(41.40)	(9.21)	(226.80)	(222.46)		

For the following variables, the sample is limited to women who are either heads of hhs or spouses of hh heads

	Without interaction		With interaction				
participation in Oportunidades	0.14***	0.72**	0.16***	-0.62	1.34	1,660	0.381
	(0.03)	(0.33)	(0.04)	(0.93)	(0.84)		
household participates in Seguro	0.05	0.69***	0.05	0.50	0.20	1,656	0.157

Popular	(0.04)	(0.24)	(0.04)	(0.72)	(0.69)		
household participates in ProCampo	-0.06	0.32**	-0.03	-1.36	1.69**	1,656	0.208
	(0.05)	(0.13)	(0.04)	(0.90)	(0.84)		
household participates in Liconsa	-0.00	-0.50**	-0.00	-0.33	-0.17	1,656	0.132
	(0.03)	(0.22)	(0.03)	(0.48)	(0.41)		
household participates in some social program	0.10	0.40*	0.12	-0.62	1.03	1,660	0.275
	(0.06)	(0.24)	(0.07)	(1.18)	(1.13)		

All regressions include state fixed effects. Standard errors clustered at state level. For detailed description of control variables, see note to Table 5.

Table 11: Effects of not delaying childbearing - early miscarriages excluded

Sample limited to women who got pregnant as teenagers; early miscarriages excluded

	coefficient and standard error	number of observations	R2
household participates in Oportunidades	0.11***	2,385	0.357
	(0.03)		
household participates in Seguro Popular	0.02	2,380	0.143
	(0.06)		
household participates in ProCampo	-0.06	2,381	0.167
	(0.05)		
household participates in Liconsa	0.03*	2,381	0.117
	(0.02)		
household participates in some social program	0.10	2,385	0.245
	(0.07)		
years of education	0.32	2,391	0.347
	(0.35)		
is currently employed	0.16**	2,391	0.190
	(0.06)		
log of social assistance income	0.31***	2,385	0.293
	(0.12)		
is married	0.02	2,391	0.211
	(0.10)		
is single	-0.09	2,391	0.392
	(0.06)		
log of labor income	0.10	2,427	
	(0.23)		
number of hours worked	-12.15	2,469	

	(11.27)		
For the following variables, the sample is limited to women who are either heads of hhs or spouses of hh heads			
participation in Oportunidades	0.13***	1,646	0.380
	(0.04)		
household participates in Seguro Popular	0.06	1,642	0.156
	(0.04)		
household participates in ProCampo	-0.07	1,642	0.205
	(0.06)		
household participates in Liconsa	0.03	1,642	0.134
	(0.02)		
household participates in some social program	0.12*	1,646	0.274
	(0.07)		

All regressions include state fixed effects. Standard errors clustered at state level. For detailed description of control variables, see note to Table 5.

Table 12: the effects of having a miscarriage

Sample of women aged 15 to 30, who had one child and zero or one miscarriage

	coefficient and standard error	number of observations	R2
household participates in Oportunidades	0.01	2,217	0.259
	(0.03)		
household participates in Seguro Popular	-0.01	2,216	0.124
	(0.03)		
household participates in ProCampo	0.01	2,217	0.122
	(0.01)		
household participates in Liconsa	0.02	2,215	0.105
	(0.02)		
household participates in some social program	0.01	2,218	0.178
	(0.05)		
years of education	-0.09	2,231	0.275
	(0.25)		
is currently employed	0.02	2,231	0.173
	(0.04)		
log of social assistance income	-0.03	2,228	0.090
	(0.14)		

is married	0.01 (0.04)	2,231	0.231
has a partner	0.10*** (0.03)	2,231	0.233
is single	-0.15*** (0.04)	2,231	0.359
log of labor income	-0.04 (0.11)	2,128	
number of hours worked	-5.50** (2.51)	2,181	
For the following variables, the sample is limited to women who are either heads of hhs or spouses of hh heads			
participation in Oportunidades	-0.01 (0.04)	1,167	0.167
household participates in Seguro Popular	-0.01 (0.04)	1,167	0.138
household participates in ProCampo	0.00 (0.00)	1,167	0.075
household participates in Liconsa	0.01 (0.02)	1,167	0.120
household participates in some social program	-0.02 (0.06)	1,168	0.172

All regressions include state fixed effects. Standard errors clustered at state level. For detailed description of control variables, see note to Table 5.

Appendix

Table A1: Scoring factors and summary statistics for variables entering into the computation of the first principal component

	Scoring factor	Mean	Standard Deviation
Roof made of solid materials	0.27	0.69	0.46
Dirt floor	-0.26	0.09	0.29
Access to water	0.22	0.88	0.33
Flush toilet	0.34	0.65	0.48
Access to sewage	0.25	0.86	0.34
Access to electricity	0.15	0.99	0.12
Own phone	0.33	0.63	0.48
Own tv	0.25	0.93	0.25
Own a car	0.26	0.41	0.49
Own a radio	0.17	0.86	0.34
Own a stove	0.30	0.87	0.34
Own a fridge	0.32	0.81	0.40
Own a washing machine	0.31	0.65	0.48
Own a PC	0.23	0.19	0.40

Scoring factor is the "weight" assigned to each variable (normalized by its mean and standard deviation) in the linear combination of the variables that constitute the first principal component. The percentage of covariance explained by the first principal component is 30%. The first eigenvalue is 4.23; the second eigenvalue is 1.32.

Table A2: Municipal characteristics

	All Mexico		Sample of all women aged 15 to 30		Sample of women who got pregnant as teenagers	
Number of municipalities	2442		787		617	
Characteristic	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Fraction with some insurance	0.76	0.19	0.67	0.19	0.66	0.19
Fraction of illiterate (among aged 15 and older)	0.18	0.12	0.13	0.10	0.13	0.10
Fraction without any education (among aged 15 and older)	0.18	0.11	0.14	0.09	0.14	0.09
Fractin speaking indigenous language (among aged 5 and older)	0.20	0.32	0.11	0.23	0.11	0.22
Fraction Catholic (among aged 5 and older)	0.88	0.12	0.88	0.10	0.87	0.10
Fraction of workers earning less than minimal salary (among economically active)	0.73	0.17	0.63	0.17	0.63	0.17
Fraction of employed who work less than 32 hours	0.23	0.08	0.20	0.06	0.20	0.07
Fraction of female headed households	0.19	0.05	0.19	0.04	0.19	0.04
Fraction working (among economically active)	0.99	0.01	0.99	0.01	0.99	0.01
Average grade level	5.39	1.56	6.30	1.62	6.34	1.66
Fraction with primary education completed (among aged 15 and older)	0.22	0.06	0.21	0.05	0.21	0.05
Fraction with secondary education completed (among aged 15 and older)	0.14	0.06	0.17	0.05	0.17	0.05
Fraction of aged 15 to 24 attending school	0.24	0.09	0.27	0.10	0.27	0.10
Fraction rural	0.55	0.50	0.20	0.40	0.17	0.38
Number of observations per municipality			9.90	17.26	4.54	6.40

Table A3: household level controls

	Women aged 15 to 30			Women aged 15 to 30 who became pregnant as teenagers		
	mean	standard deviation	number of observations	mean	standard deviation	number of observations
indigenous	0.19	0.39	7,803	0.21	0.41	2,803
number of children	1.80	1.07	7,805	2.21	1.21	2,803
number of children aged between 8 and 14	0.47	0.78	7,805	0.82	0.94	2,803
asset index	-0.26	2.00	7,312	-0.70	2.11	2,585
gave birth under 17	0.33	0.47	2,661	0.33	0.47	2,661
head of household	0.06	0.24	7,803	0.07	0.26	2,803
wife of household head	0.59	0.49	7,803	0.60	0.49	2,803
other relation to household head	0.35	0.48	7,803	0.32	0.47	2,803