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THE CONSEQUENCES OF TEENAGE CHILDBEARING BEFORE ROE V WADE

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ABSTRACT

Using data from three cycles of the National Survey of Family Growth, we investigate whether there were adverse consequences of teenage childbearing in the 1950s and 1960s, when most abortions were illegal, and access to the pill was limited. We find negative effects of teen motherhood on the likelihood of obtaining at least 12 years of education and on the number of marriages. We find positive effects of teen motherhood on family income, and, unsurprisingly, on the number of children. These effects are heterogeneous by predicted education. For those with high levels of predicted education, giving birth does not affect educational attainment but increases the probability of being divorced. For those predicted to be on the margin of high school completion, giving birth has strong negative effects on 12th grade completion and age at first marriage, while increasing the probability of never having married. In general, for less advantaged teens, motherhood appears to have increased expected family income but also the risk of not graduating from high school and never marrying. We find surprisingly little evidence that births affected teens conceiving pre- and post-marriage differently.

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1 Introduction

There is a growing consensus among economists that while women who give birth as teens have worse adult outcomes than those who do not, the causal effect of teen motherhood on the mothers is small and possibly zero. But the supporting evidence comes almost entirely from a period when abortion was legal and effective contraception readily available. Teens who anticipate that motherhood would have significant adverse consequences can avoid pregnancy even if they engage in sex, and, if they become pregnant, can terminate the pregnancy. It would not be surprising if the teens who give birth are those who anticipate that motherhood will not adversely affect their lives.

Little is known about the consequences of teen motherhood in periods when abortion and contraception are less available, for example before Roe v Wade in the United States or similar periods elsewhere.[1] In these periods, it is possible that some of the teens giving birth would make, or be forced to make, different investment decisions as a result of motherhood. The popular view is that an out-of-wedlock birth was a social and economic disaster, and the alternative for a pregnant teen was a premature marriage. In their discussion of the decline of shotgun marriages, Akerlof, Yellen and Katz (1996) suggest that the consequences for the mother and the effect on the reputation of the father were sufficiently severe that the shotgun was rarely necessary.

Even if conception occurs after marriage, currently we generally view early marriages as undesirable, especially if they give rise to an early pregnancy and motherhood. We believe that it is important that girls finish high school before becoming mothers, and most girls marrying and conceiving before turning 18 will not have done so. Moreover, today we are inclined to believe that sixteen and seventeen year olds are too young to decide whether to marry. But did today's perspective apply in the 1950s and 60s? While rare today, it was not all that uncommon in the 1950s and 1960s for a girl to marry before her 18th birthday even though she was not pregnant. This suggests that many people viewed such early marriages as reasonable, and perhaps they were right. On the other hand, those marrying in the 1950s and, to a lesser extent, the early 1960s would face significant social and legal barriers to divorce. Women marrying when very young might find themselves stuck in bad matches. Even when divorce became more readily available, their remarriage prospects may have been reduced by the presence of one or more children. And we now know that within one or two decades, social and economic change would greatly increase the labor force participation of married women. It is plausible that women who had married and had children early and

[1] Bailey (2013) examines changes in individuals' legal and financial access to contraception (though not abortion), and finds suggestive evidence of long-run consequences for the individuals' children.

therefore not completed high school would face serious disadvantages.

In this paper we draw on three waves of the National Survey of Family Growth (1982, 1988 and 1995) to study adult outcomes among women who conceived as teenagers (less than 18) between 1951 and 1969. In this period, neither legal abortion nor highly effective contraception were available to teens. These are the earliest data of which we are aware on pregnancy outcomes for a representative sample of teens.

In 1972, the Supreme Court decision Eisenstadt v. Baird guaranteed unmarried adults legal access to contraception. Its 1977 Carey v. Population Services International decision struck down a New York State law prohibiting the sale or distribution of contraceptives to individuals less than sixteen years old and permitting the sale of contraceptives only by pharmacists. A detailed review of parental notification laws is beyond the scope of this paper,[2] but it is safe to say that teenagers have had legal access to effective contraception either over the counter or by prescription since the 1970s. For those who did not use effective contraception or for whom it failed, the 1973 Roe v. Wade decision guaranteed legal access to abortion, although admittedly funding restrictions, parental notification laws and other restrictions especially since the 1990s have reduced access.

We focus on conceptions before 1969. While other forms of contraception, notably condoms, were widely available, before 1969 only 10 states allowed unmarried, childless women under the age of 21 to obtain the pill legally without parental consent. Such laws were followed closely because the pill required a prescription from a licensed physician and sale by a licensed pharmacist, and violation of the laws was subject to significant penalties (Bailey, 2006). In addition, before 1969 legal abortions were extremely rare in all states.

We follow a standard approach in the recent literature, limiting our sample to women who first conceived as teens and compare those whose first conception ended in a live birth with those for whom it ended in a miscarriage.[3] This approach relies on the assumption that in the absence of abortions, miscarriage is random, an assumption supported by our data. We provide evidence that abortion was rare for our sample and show that the bias from abortions is therefore small. Consequently, comparing women who gave birth and miscarried as teens provides us with (nearly) unbiased estimates of the effects of teen motherhood on adult outcomes.

Our results are strikingly different from those obtained using more recent data (e.g. Ashcraft, Fernandez-Val and Lang, 2013). We find a very large adverse effect on high school completion, particularly among those whom we predict would otherwise complete twelve

[2]See Maradiegue (2003) for more detail.

[3]We use nontechnical language. We refer to spontaneous abortions as miscarriages and induced abortions as abortions.

years of schooling. This group marries earlier and is also more likely never to have married. Among those with higher levels of predicted schooling, those giving birth are more likely to be divorced or separated. The effects of motherhood for the most disadvantaged teens are less clear. For this group, we find large adverse effects of motherhood on high school completion, and a higher probability both of entering into early marriage and never having married in one specification but much more modest effects that are not statistically significant in a second.

These findings suggest that, except for a higher rate of divorce, the more advantaged were largely able to offset the adverse effects of teen motherhood. But a broad middle group (and perhaps also those least advantaged) did enter into early marriage or never married and failed to complete high school. However, this group also tends to have higher family incomes, suggesting that while there were risks associated with teen motherhood, there was also a positive expected payoff. We find surprisingly little evidence that births affected teens conceiving pre- and post-marriage differently. The adverse effect on high school graduation is clearer among those conceiving out-of-wedlock and the beneficial effect on family income is clearer for those conceiving after marriage but in neither case is the difference between the two groups statistically significant.

Finally, we note that the proportion of pregnancies that were out-of-wedlock conceptions trended upward during our period and the proportion of those ending in marriage prior to the birth trended downward. Nevertheless, we find little or no evidence of trends in the teens' outcomes.

2 Miscarriage and Teen Births

There have been a variety of approaches to measuring the effect of teen motherhood on the mothers. Early work largely ignored the potential endogeneity of teen motherhood and compared outcomes controlling for only a modest number of possible confounding factors. However, there is an important strand of the literature, beginning with Geronimus and Korenman (1992), that relies on matching estimators including comparisons of sisters. Holmlund (2005) compares sisters and controls for educational performance before pregnancy. She finds a modest adverse effect on educational attainment. Sanders, Smith and Zhang (2007) use propensity score matching and also find adverse effects on educational attainment while Levine and Painter (2003) match women within schools using the National Education Longitudinal Survey and find adverse effects on both education and earnings.

In contrast, instrumental variables approaches frequently find little or no effect, but here too, we find variation in the results. One common approach has been to use age at menarche as an instrument (often in combination with other instruments). Ribar (1994) estimates insignificant or beneficial effects of early childbearing on educational attainment while Chevalier and Viitanen (2003) and Klepinger, Lundberg and Plotnick (1999) document more adverse effects. Walker and Zhu (2009) who use exogenous variation in school-leaving age find no effects on employment.

Our focus is on miscarriage. Relying on the fact that miscarriage is largely medically exogenous, Hotz, McElroy and Sanders (1998, 2005) use miscarriage as an instrument. They find that teen motherhood has only a small effect on completed fertility and modest adverse impacts on education and labor market outcomes. Goodman, Kaplan and Walker (2004) and Ermisch and Pevalin (2003) also use miscarriage as an instrument and find only small, generally insignificant results.

However, Ashcraft and Lang (2006), while accepting the argument that miscarriage is largely medically random, point out that it is not socially random. Potential miscarriages are often preempted by an abortion. In the standard approach, miscarriage is used as an instrument for not giving birth. Implicitly (or explicitly in the reduced form), women who miscarried as teens are compared with those who either gave birth or had an abortion. But teenagers who would otherwise abort their pregnancy are underrepresented among those who miscarry because early abortions preclude a miscarriage. Building on this insight, Fletcher and Wolfe (2009) show that miscarriage is nonrandom and can be predicted by the frequency of abortion in the individual's neighborhood. Ashcraft and Lang show pregnant teenagers who have abortions tend to be drawn from more favoured backgrounds than those who do not. Consequently, girls who miscarry are not a random sample of pregnant teenagers, and are drawn disproportionately from disadvantaged backgrounds. The IV estimator therefore underestimates the true costs of teenage childbearing. Using miscarriage as an instrument biased the findings towards a benign view of teenage childbearing.

Ashcraft and Lang analyze a second approach: ordinary least squares (OLS) on miscarriages and births only. Since some miscarriages preempt abortions, the set of teens who miscarry is more favorably selected than those who give birth. Consequently OLS is biased toward a more adverse view of teen motherhood. Ashcraft, Fernandez-Val and Lang (2013) present two approaches to consistent estimation. The first is to derive a consistent estimate as a weighted average of the OLS and IV estimates. The second involves estimating the number of abortion types included in the miscarriages and purging the miscarriage group of their expected outcomes. This yields a consistent estimate of the miscarriage outcome among those who would otherwise have given birth. Both approaches show modest, generally statistically insignificant effects on adult outcomes.

From our perspective, the important point is that the bias from estimating the effect of teen motherhood by comparing those who gave birth and those who miscarried comes from the fact that a nontrivial fraction of the teens who miscarry would otherwise have had abortions. We argue below that for the period we study, this fraction is likely to be small.

3 Induced Abortion

While Roe v. Wade legalized abortion throughout the United States in 1973, even before this decision states had begun liberalizing their abortion laws. Before 1967, abortion was a crime in all states except to save the life (in some states also to preserve the health) of the mother.[4] In 1967 and 1968, two states made abortion legal if it was performed by a licensed physician, and the physical or mental health of the mother or child was endangered, or if the pregnancy was the result of rape or incest. Seven states passed similar laws in 1969 and three more did so in 1970. One state adopted similar legislation in 1969, but added that in addition to the mother's physical or mental health, account could be taken of her total environment, actual or reasonably foreseeable. Four states completely repealed their legislation banning abortion in 1970, but required that the abortion be done early in the pregnancy and by a licensed physician. In the remaining 33 states abortion remained generally illegal with the exceptions noted above. Thus, before 1969, legal abortion was only available in two states and then only under fairly restrictive conditions. It started to become more widely available in 1969, and by 1970 was plausibly sufficiently available as to be a significant factor in understanding teen births. According to data compiled by Johnston (2013), reported abortions grew from 1,028 in 1966 to 2,061 in 1967, to 6,211 in 1968 to 27,512 in 1969 and 193,491 in 1970.[5]

While legal abortion was not widely available pre-1969, some women did have abortions through illegal means. By its nature, the incidence of illegal abortion is difficult to estimate. In 1959, it was estimated that somewhere between 200,000 and 1.2 million illegal abortions were obtained per year in the United States (Calderone 1960).[6] Estimates at the upper end of this range imply that legal abortions simply replaced illegal ones. The number of legal abortions in the United States did not reach 1.2 million until 1976 (Johnston 2013).

[4] This history comes from Roemer (1971).

[5] Before Roe v. Wade, "therapeutic abortions" were generally performed if psychiatrists believed that the mother would commit suicide if the abortion was not performed. Interpretation of this law was inconsistently applied, with the legal therapeutic abortion rate higher on private services than on ward services (Calderone 1960). Calderone quotes a participant at a conference of the American Public Health Association as saying that the difference between an illegal abortion and a legal therapeutic abortion was "$300 and knowing the right person" (Calderone 1960).

[6] Some women, likely only a small fraction, went abroad to have abortions in countries where it was legal, for example Japan, Mexico, England, Scandinavia, and Puerto Rico (Greenhouse and Siegel 2012).

Our data imply that roughly 2 percent of first teen pregnancies before 1969 ended in an induced abortion. Ashcraft, Fernandez-Val and Lang (2013) estimate that roughly 75% of legal abortions are reported in a closely related data set. Even an abortion rate of 3 percent would imply noticeably fewer illegal abortions than even the lower range of the expert guestimates (In 1959 there were approximately 4.2 million live births in the United States (US Department of Health, Education, and Welfare 1959)). Either the illegal abortion rate was lower for teens than for adults, higher for subsequent pregnancies, the experts guessed too high or the underreporting of abortions is much more serious for this group. It is impossible to establish the relative importance of these factors conclusively. However, there is little reason to expect that underreporting of abortions would be particularly problematic for this sample. We use recall data from 1982, 1988 and 1995 when the laws making those abortions illegal had long since been voided. Moreover, the frequency with which pre-1970 pregnancies are reported as having ended in an abortion is unrelated to the survey year.

In our data, described below, the miscarriage rate is about 7 percent. This is lower than currently estimated, but as Lang and Nuevo (2012) show, miscarriage rates have been rising, probably because home pregnancy tests have increased awareness of pregnancy. Let us be conservative and assume that 10 percent of teens miscarry sufficiently late in pregnancy to be symptomatic and that 3 percent of teens would have abortions if they were aware of being pregnant. Given that miscarriage is medically random, this means that 0.3 percent of teens are both miscarriage and abortion types. Roughly speaking, from Ashcraft, Fernandez-Val and Lang, half of these will miscarry and half will have abortions. So the fraction of abortion types among teens who miscarry is about 1.5 percent. The bias from including these abortion types is approximately .015 times the difference in outcomes between women who have abortions and those who would choose to give birth but miscarry. Any bias is very small and is mitigated by our ability to control for differences in observables.

4 Data

We use cycles III (1982), IV (1988) and V (1995) of the National Survey of Family Growth (NSFG), a survey of non-institutionalized women aged 15-44, administered by the National Center for Health Statistics, an agency of the United States Department of Health and Human Services. Personal interviews were conducted with the respondents to collect data on pregnancy history, family planning, as well as many social, economic, and demographic characteristics. The 1982 survey contains a sample of 7,969 women, the 1988 survey 8,450, and the 1995 survey 10,847 women. Following Ashcraft, Fernandez-Val and Lang (2013), we define teen pregnancies as first conceptions that occur before age 18.

As we include pregnancies in later years, we risk both more bias due to a growing number of abortions and including years where the growing availability of abortion and the pill changed who gave birth as a teen. This must be weighed against the loss of power as we set an earlier cutoff. In order to enlarge the sample size, we had originally planned to present primary results restricting the sample to women who first conceived before 1970 and to show results excluding those who conceived in 1969 as a robustness check. However, we have reversed the priority for two reasons. Firstly, because the results are stronger when excluding conceptions in 1969. Secondly, and more importantly, because of the dramatic increase in the availability of abortion in 1969, as seen in our analysis of state abortion laws and abortion rates in the previous section. The reader should be aware that there is some element of "data-mining" in this decision.

Note that by 1995, only the oldest women in the sample could have become pregnant as a teen before 1970. Therefore, most of our sample comes from cycles III and IV. We define all pregnancies reported to have resulted in a miscarriage or a stillbirth within the first 22 weeks or five months of pregnancy as "miscarriages." Thus we exclude outcomes reported as miscarriages if the pregnancy lasted more than five months and include the one pregnancy reported to end in a stillbirth at 5 months.[7] Twenty weeks is a more standard cutoff for distinguishing between miscarriages and other forms of fetal death, but the duration of pregnancy is often reported in months in our data. Moreover, as the Supreme Court noted in Roe v. Wade, at that time viability before 28 weeks was rare and the Court felt it was safe to declare that a fetus was not viable before 24 weeks. Extending the definition of miscarriages to the four stillbirths reported at six months might be problematic since a live birth is also reported at six months.[8]

Our principal sample of those conceiving as teens before 1969 consists of 1058 first pregnancies that result in birth, 79 in miscarriage, and 20 that end in an abortion.[9] When we

[7]Following Lang and Nuevo (2012), we exclude ectopic pregnancies since respondents were only explicitly asked about this outcome in 1995. Including ectopic pregnancies may introduce bias from the changing treatment of this outcome over the cycles. Women who do not remember the number of weeks at which the miscarriage or stillbirth occurred are asked for the month or trimester in which this outcome occurred. We treat miscarriages or stillbirths reported to have occurred in the first and second trimesters as "miscarriages." None of the women with teen pregnancies before 1969 report that their first pregnancy ended in a miscarriage after 22 weeks. Thus, assuming that reported second trimester miscarriages occur in the first 22 weeks is reasonable.

[8]In contrast with "miscarriage," late fetal death is predicted in our data by socioeconomic factors which makes it important to restrict the sample in this way.

[9]We note that there are eight observations who report a live birth after less than 4 weeks of pregnancy, when asked for pregnancy duration in weeks. However, when asked for the duration in months they report either 8 or 9 months. We thus continue to include these observations. There is one observation who reports a live birth, but does not report the duration. Since we are interested in the effect of motherhood, the duration of the pregnancy for a live birth is not important, and so we include this observation.

include conceptions in 1969, the sample consists of 1,231 first pregnancies that resulted in birth and 99 in miscarriage. There are also 34 respondents (2.5%) who report a pregnancy ending in an abortion. This reflects a much higher abortion rate in 1969 than in the previous years.

There is some concern that miscarriages are underreported. Many miscarriages occur very early in pregnancy and are often asymptomatic (Pandya et al. 1996). Moreover, since this paper uses recall data, it is possible that women forget miscarriages that happened long ago. Lang and Nuevo (2012) find no evidence that the reported miscarriage rate in a given year is higher in more recent cycles of the NSFG, for either all miscarriages or just for early miscarriages. We confirm that in our data the miscarriage rate for a given year is independent of the survey year. This mitigates our concern that recall bias is an issue for our analysis.

Nonreporting of miscarriages, because the woman was never aware of being pregnant, has forgotten the miscarriage or simply chooses not to report it, will be problematic only if the tendency to recognize and report a miscarriage is related to future outcomes. Below we will confirm that reported miscarriage is unrelated to measured background characteristics in our data.

We focus on 9 outcome variables. The first two are measures of education: years of educational attainment, and an indicator for whether the individual obtained at least twelve years of education. The next variables are related to marital history: age at first marriage, total number of marriages, whether currently married, whether currently divorced or separated and whether the respondent was never married. We also consider family income.[10] However, the income data are somewhat problematic because they are reported in 17 intervals in each year, and we impute income using these intervals.[11] Finally, we look at the number of live births. Observations are weighted by the sampling weights of the survey, normalized so that the weighted sample size for each survey equals the actual sample size for that survey.

Table 1 presents weighted means and standard deviations for the variables in the em-

[10] Early work also examined whether the woman was working at the time of the survey, but the standard errors were too large to exclude either very large positive or very large negative effects.

[11] In 1982 and 1988, there are 17 intervals which we impute as follows: less than $2,500 ($1,250), $2,500-$4,999 ($3,750), $5,000-$5,999 ($5,500), $6,000-$6,999 ($6,500), $7,000-$7,999 ($7,500), $8,000-$8,999 ($8,500), $9,000-$9,999 ($9,500), $10,000-$10,999 ($10,500), $11,000-$11,999 ($11,500), $12,000-$12,999 ($12,500), $13,000-$14,999 ($13,999), $15,000-$16,999 ($15,999), $17,000-$19,999 ($17,999), $20,000-$24,999 ($22,499), $25,000-$34,999 ($29,999), $35,000-$49,999 ($42,499), $50,000 and more ($74,499).

In 1995, there are 18 intervals which we impute as follows: less than $7,000 ($3,500), $7,000-$8,499 ($7,750), $8,500-$9,999 ($9,250), $10,000-$11,999 ($10,999), $12,000-$13,999 ($12,999), $14,000-$15,999 ($14,999), $16,000-$17,999 ($16,999), $18,000-$19,999 ($18,999), $20,000-$24,999 ($22,499), $25,000-$29,999 ($27,499), $30,000-$39,999 ($34,499), $40,000-$49,999 ($44,499), $50,000-$59,999 ($54,499), $60,000-$69,999 ($64,499), $70,000-$79,999 ($74,499), $80,000-$89,999 ($84,499), $90,000-$99,999 ($94,499), $100,000 and up ($124,499).

pirical specification, by birth outcome. The outcome variables are listed first. With a few exceptions, there are only minor, and statistically insignificant differences in these outcome variables between teens who miscarry and those who give birth. However, teens who give birth are 10 percentage points less likely to obtain at least twelve years of education, although this falls short of statistical significance at conventional levels. They have had almost .5 fewer marriages and about .6 more live births (statistically significant at the .01 level). Their family income is higher by about $5000, though this difference is not statistically significant. Teens who give birth are approximately eight percentage points more likely to be black, significant at the .05 level. Those giving birth are 11 percentage points less likely to be Protestant, and 10 percentage points more likely to be Catholic, both significant at the .05 level. It is not surprising that a few of the 14 explanatory variables would be significantly different at the .05 level. Using the Bonferroni adjustment for 14 tests, and adjusting for the average correlation between the 14 variables, none of the differences in the explanatory variables are significant at the .1 level. The similarity in these explanatory variables across pregnancy outcome provides evidence that miscarriages in this period are biologically and socially random. We examine this in greater detail below.

The third column of Table 1 shows that the few individuals who have abortions are positively selected. Relative to those giving birth, they are less likely to have conceived before the age of 15 and less likely to be Hispanic; their mothers have more education, and their own predicted education levels are higher.[12] They also have better outcomes: they obtain more education, their family income is higher, and they are less likely to be currently divorced. The table additionally shows that they are older at their first marriage. As discussed in the previous section, because so few teens have abortions during this period, the positive selection into abortion will result in only very minimal bias.

5 Empirical Strategy

If reported miscarriages are random, there are no abortions, and miscarriage does not directly affect adult outcomes, then simply comparing mean outcomes for women who gave birth with those who miscarried provides an unbiased estimate of the causal effect of teen motherhood on adult outcomes, for women like those who become pregnant as teens. We have argued that the frequency of abortion during the period we study is sufficiently low to be inconsequential. We will provide additional evidence that miscarriage is random. Thus, we need go no further than table 1. And if contrary to our implicit assumption, miscarriage affects adult outcomes

[12]These statistics mitigate the concern that Hispanics may have had more access to abortion services if they regularly traveled to countries where abortion was legal (for example Mexico and Puerto Rico).

directly, at least we measure the effect of a pregnancy ending in a birth relative to a pregnancy ending in miscarriage.

However, the standard errors of the estimates in that table are frequently quite high. Even though our point estimate is that teen motherhood reduces the probability of completing at least twelve years of schooling by 10 percentage points, we cannot reject the null hypothesis of no effect. If we can control for other factors that affect adult outcomes, we can increase the precision of our estimates. Moreover, if reported miscarriages are not random, these additional controls can reduce bias. Therefore, we use a straightforward ordinary least squares regression:

$$y = \alpha + X\beta + \gamma_1 Birth + \varepsilon \tag{1}$$

to measure the effect of teen motherhood for 9 separate dependent variables, y.

The variables in X include the respondent's current age, age at first conception and a dummy if this age was less than 15, respondent's mother's education, two indicators for the cycle of the survey, Hispanic, black, white, Protestant, Catholic, whether during most of the respondent's childhood her mother worked (either full- or part-time), and whether the respondent lived with both parents at age 14.[13] *Birth* is an indicator equal to one if the first pregnancy resulted in a birth, and equal to zero if it resulted in a miscarriage. We limit the sample to women whose first teen pregnancy ended in either a birth or a miscarriage.

One explanation for the absence of adverse effects of teen motherhood is that more advantaged teens can readily avoid motherhood and choose to do so because of its large cost. More advantaged teens in the 1950s and 1960s could reduce or eliminate the risk of pregnancy by using less effective methods of birth control or practicing abstinence. However, it is plausible that during this period teens for whom giving birth would be costly were more likely to give birth than they are now. Alternatively, disadvantaged teens might have had fewer options for mitigating the effects of a birth than they have currently. For example, it is now more common for high schools to permit and even support attendance by teens with babies. This suggests that we should interact *Birth* with measures of socioeconomic advantage.

Because our sample of miscarriages is relatively small, it is not feasible to interact *Birth* with each of the background measures. Instead, we use the individuals in the three cycles of the NSFG who were born before 1951 and who first conceived at age 18 or older. For

[13] The indicator for whether the respondent lived with both parents at the age of 14 is not available in the 1995 survey. We set the variable equal to zero in 1995, and include indicators for the year of the survey in the regressions. Since the education of the respondent's mother, and whether the respondent's mother is working, are missing for some of the respondents, we include indicators for whether these variables are missing and set the variables to zero for those for whom it is missing.

these individuals, we regress education on the explanatory variables in X above, excluding age at first conception and the indicator for age at conception less than 15. We use the coefficients from this regression to create a predicted education index for each individual in our teen pregnancy sample of pre-1969 conceptions.[14] Predicted education for our sample ranges from about eight to seventeen years. The 90/10 range is roughly 11 to 14. For ease of interpretation, we have rescaled predicted education to (predicted education -12). In the regressions below, the coefficient on $Birth$ should be interpreted as the effect of teen motherhood for someone with 12 years of predicted education.

Using these predicted education levels, we then estimate:

$$y = \alpha + X\beta + \gamma_1 Birth + \gamma_2 Birth * \widehat{Ed} + \varepsilon \tag{2}$$

where $\widehat{Ed}$ is the predicted education level of the respondent minus 12. We are not able to control for $\widehat{Ed}$ in the regressions above, since we include all of the variables that are used to predict $\widehat{Ed}$.

To allow for nonlinearities in the effect of predicted education, we also estimate the following:

$$y = \alpha + X\beta + \gamma_1 \widehat{Ed}^2 + \gamma_2 Birth + \gamma_3 Birth * \widehat{Ed} + \gamma_4 Birth * \widehat{Ed}^2 + \varepsilon. \tag{3}$$

6 Results

6.1 Are reported miscarriages random?

Ashcraft, Fernandez-Val and Lang (2013) review the medical literature on miscarriage and conclude that the medical evidence for large behavioral effects on miscarriage is relatively weak. Ashcraft and Lang (2006), however, point out that since the decision to have an abortion is nonrandom, miscarriages are as well. And Lang and Nuevo (2012) show that reported miscarriages are drawn from a more advantaged population, presumably because of greater awareness of pregnancy.

In table 1, we report the relation between birth outcome and predicted education, a summary measure of socioeconomic advantage. The difference in predicted education between

[14] We note that if teens who expect to get less education are more likely to become pregnant, this is not a consistent estimate of each teen's expected education in the absence of a teen birth. Indeed, we find that teens who miscarry get almost two years less education than "predicted." However, their actual education is strongly increasing in their predicted education. We cannot reject at the .05 level that the slope is one, but this reflects small sample size rather than a point estimate close to one.

those giving birth and those miscarrying is .01 years, suggesting that the two groups come from similar backgrounds.

In table 2 we report the result of a linear probability model in which we regress the pregnancy outcome on our controls. Only two of the t-statistics exceed 1 and neither of these approaches statistical significance at conventional levels. The F-statistic is 1 and the R-squared is .02. In sum, there is no evidence that reported miscarriages are nonrandom. Because the probability of miscarriage is only about 7% in this sample, there is a risk that different distributional assumptions would give different results. The second and third columns of table 2 show probit and logit estimates. The individual estimated coefficients remain far from statistically significant. The coefficients are not jointly significant; however, in the logit estimation they approach statistical significance, with $p = .12$.

We note that if either nonreporting of miscarriages were nonrandom or the abortion rate were much higher than we report, miscarriages would be predictable. Our inability to predict miscarriage supports the view that unreported, previously illegal, abortions are not a major factor.

6.2 Adult outcomes

Table 3 presents the results from specification (1). Each column represents a separate regression, with the dependent variables listed across the first row. Our point estimate (-0.20) of the effect of teen motherhood on average education is small and statistically insignificant although we cannot rule out modest adverse effects. However, we find that those who give birth are 14 percentage points less likely to obtain at least 12 years of education than those who miscarry, significant at the .05 level.

The point estimates suggest that those giving birth as teens get married about a half year earlier, are slightly more likely to be currently married, slightly more likely to be currently divorced or separated, and slightly more likely never to have married, but these effects fall well short of statistical significance at conventional levels. Interestingly, they have had .3 fewer marriages (significant at the .05 level). This is driven by their much greater tendency to be married only once. They are 20 percentage points more likely to have been married exactly once with no controls and 17 percentage points more likely if we control for other factors (neither result shown). Teen mothers appear to be both substantially less likely to divorce following their first marriage and less likely to remarry if they do.

Unsurprisingly, those giving birth as teens have more children. We note that this need not be causal. Women with a history of miscarriage are more likely to miscarry during subsequent pregnancies.

Family income is substantially higher for those giving birth than for those who miscarry and is significant at the .05 level, although we feel the need for caution since this is the one variable with substantial missing data and has been converted from a categorical variable. Nevertheless, assuming the result is real, it may reflect that those giving birth are somewhat more likely to be married, and also perhaps more likely to be receiving alimony since they are more likely to be divorced with children.

We do not discuss the remaining explanatory variables except to note that they generally enter in the expected way. Table 3 suggests that teen motherhood has an effect on education and marital outcomes; these effects will be further explored in the following tables.

6.2.1 Heterogeneity by Predicted Education

Table 3 imposes that a teen birth had similar effects regardless of the teen's family background and future prospects. Studies based on more recent data find little effect of teen motherhood, largely because teen mothers are drawn from groups whose education and job prospects are already poor. We might therefore expect that in the earlier period we study, adverse effects would be more prominent among the more advantaged teens who give birth. On the other hand, very advantaged teens may be more able to overcome any adverse effects.

Therefore, in table 4 we present the results from specifications (2) and (3). Each column represents a separate regression. The explanatory variables for each of these regressions are the same as in table 3, but in the top panel we include predicted education, rescaled to predicted education minus 12, interacted with birth. Predicted education is not included by itself because all of the variables that enter the predicted education calculation are included independently in the specification. This specification allows the effect of a teen birth to increase or decrease with this summary measure of background. In the lower panel we add predicted education squared and its interaction with a teen birth to allow for a somewhat more complex relation between family background and the effect of a teen birth.

In both specifications, the coefficient on $Birth$ can be interpreted as the effect of teen motherhood for someone with 12 years of predicted education. In both panels the top rows present the coefficients on the birth variables. Below these coefficients, we show the p-value for the hypothesis that the two or three birth coefficients are jointly 0. The last two rows show the estimated effects of a birth for individuals with predicted education of 10 and 14 years (rescaled to -2 and +2). It will be apparent that in no case does the interaction with the squared term approach statistical significance. However, the Bayesian Information Criterion is larger in the quadratic specification for each outcome variable. The Akaike Information Criterion is larger in the quadratic specification for 7 out of the 9 outcome variables. Therefore, since in some cases its inclusion changes the interpretation of the results, we use the lower panel to temper some of our conclusions.

We continue to find no strong evidence of an effect on average education. The increase in total parity is independent of predicted education. However, the remaining outcomes show some interesting patterns.

In table 3, we found a large and marginally significant adverse effect of teen motherhood on the probability of completing 12th grade. When we include the linear interaction term, the effect on those whose predicted education was 12 years becomes very large (20 percentage points) and significant at the .05 level. The estimated effect on those with low predicted education is even larger and statistically significant at the .1 level. However, when we allow for a quadratic interaction term, the effect on the low predicted education group declines and becomes statistically insignificant although it remains numerically large. In contrast, the effect on those predicted to get 12 years of education becomes even larger (22 percentage points) and remains statistically significant at the .05 level.

We see a similar pattern when we examine age at first marriage. In table 3, the estimated effect of a teen birth was to lower the age at first marriage by .6 years, but the estimate fell short of statistical significance. When we allow a linear interaction term, the effect on those predicted to have 12 years of education rises in absolute value to .9 years and reaches significance at the .1 level, and the estimated effect on those predicted to have low education is even larger and also marginally statistically significant. Finally, when we allow for a quadratic interaction, the effect at 12 predicted years rises in absolute value to -1.09 and remains marginally significant. In contrast the effect at 10 predicted years becomes less negative and is no longer statistically significant.

The estimated effects of teen motherhood on current marital status are quite different once we allow for the predicted education interactions. With a linear interaction, teen motherhood increases the probability of having never married for those with 12 years of predicted education, and for those with low levels of predicted education, but this effect disappears at higher levels of predicted education. The results are similar with a quadratic interaction but lose statistical significance for the low predicted education group. In contrast, once we allow for a linear interaction between a teen birth and predicted education, we see that advantaged teens who give birth are much more likely to be divorced or separated, a result that is replicated with the quadratic interaction.

In none of the specifications is there a significant effect on being currently married. However, both specifications with interactions show that a teen birth reduces the number of marriages among more advantaged teens.

Finally, and with a reminder about the caveat regarding the family income data, we continue to find a positive effect of teen births on family income but one that is concentrated among the more disadvantaged teens. At high levels of predicted education, there is no effect on family income. In contrast at 12 years of predicted education, and at lower levels the effect is large, and statistically significant.

6.3 Robustness Checks

Conceptions in our sample range from 1951 to 1968, a period of dramatic social change in the United States. We consider whether the effect of teen motherhood was different for conceptions earlier in the period relative to later. In the earlier part of the period, abortion was not legal in any state and the pill, even after being approved by the Food and Drug Administration, was only available in four states to unmarried, childless women under the age of 21 without parental consent (Bailey, 2006). Therefore, we experimented with interacting birth with a time trend. We also experimented with an interaction between a dummy for 1964 or later and birth.[15]

Although some of the results are suggestive, none of the interactions between birth and the time trend and almost none of the differences between the estimated effects pre-1964 and later were statistically significant. The positive effect on family income is noticeably larger pre-1964 and is statistically significant only for this period. In contrast, the adverse effect on education is statistically significant only in the later period, but the difference in point estimates is small. The main conclusion is that the sample is too small for this exercise. We do not consider interactions with predicted education since this would produce results driven by very small numbers of observations. There is one exception to the absence of differences. In the pre-1964 sample, women who gave birth were more likely than those who miscarried to report being married. This is driven by a high rate of widowhood among those who miscarried pre-1964. We have no explanation for this finding.

The suggestive differences between the earlier and later period suggest that adding 1969 to our sample is likely to reduce the magnitude of the results. Since it noticeably increases our sample size, as a robustness check we examine the effect of teen motherhood including these conceptions albeit, as noted above, at the risk of increased bias from latent abortions among those who miscarried. The top panel of table 5 contains the results when we do not allow for heterogeneity by predicted education. These results are very similar to those in table 3. Those giving birth are less likely to complete high school, they have fewer marriages, more live births, and larger family income. However, the magnitudes of these effects are more modest than in table 3 (with the exception of number of live births).

[15] We estimate the following regressions: $y = \alpha + X\beta + \gamma_1 Birth + \gamma_2 Birth * pre1964 + \gamma_3 pre1964 + \varepsilon$. and $y = \alpha + X\beta + \gamma_1 Birth + \gamma_2 Birth * trend + \gamma_3 trend + \varepsilon$.

The lower panels of table 5 contain the results when we allow for heterogeneity by predicted education. The point estimates in panel B, in which predicted education only enters linearly in the interaction term, generally tell a similar story to those in table 4, although again the point estimates are more modest and their statistical significance falls. In addition, the birth variables are no longer jointly significant when the outcome is high school completion or never married. The results in panel C also tell a similar story to those in table 4; however, as in panel B, the point estimates are smaller in magnitude and the statistical significance falls. The smaller effects on high school completion are expected if fewer teens for whom motherhood would be costly give birth when abortion and contraception are more available. If availability of abortion and contraception reduces the prevalence of shot-gun marriages, this would explain the smaller effects on age at first marriage and family income. The smaller magnitude of the effect on never having married perhaps suggests that for the teens who get pregnant when abortion and contraception are more available, teen motherhood was less of an obstacle to future marriage.

Finally, as a further check that the results are not biased by immigrants who had access to abortion in their teens, we perform the estimation but excluding Hispanics. Interpretation of the results is generally unchanged (not shown), although there are more adverse consequences of teen birth on educational attainment, for those predicted to obtain low levels of education. The negative effect of teen birth on age at first marriage is also stronger for those less advantaged when Hispanics are excluded.

6.4 Shotgun Weddings

In 2007 93% of births to 15-17 year old mothers and 82% of births to 18 and 19 year old mothers were out-of wedlock (Ventura, 2009) as were, by extension, the overwhelming majority of conceptions among these age groups. In contrast, 32% of the (weighted) conceptions in our sample occurred after marriage. Of those pre-marital conceptions ending in a birth, 55% resulted in a pre-birth marriage. We cannot tell whether these marriages were to the putative fathers or how frequently the father married the mother after she gave birth. We do know, however, that only 22% of the women whose first birth had been out-of-wedlock had never been married by the time they were interviewed.

There is reason to be cautious about these estimates. Holding only the date of conception constant, compared with those surveyed in 1995, women were about 10 percentage points more likely to report being married prior to conception if they were surveyed in 1982 and 5 percentage more likely if they were surveyed in 1988. The former difference is significant at the .1 level. The results are similar if we control for other differences. This suggests that premarital conceptions may be underestimated.

There are clear trends in these patterns. If we regress whether the teen was married when she became pregnant along with our usual controls,[16] the probability of being married prior to conception declines by 1.4 percentage points per year (significant at the .01 level). Conditional on a premarital conception, the probability of a shotgun marriage declines by 1.0 percentage point per year (significant at the .1 level). And conditional on having given birth out-of-wedlock, the probability of never having married increases by 2.3 percentage points per year as we move from conceptions in the early 1950s to the late 1960s.

We further look to see if the effect of giving birth was different for teens conceiving pre-marriage relative to post-marriage. With one exception, the interaction term fell short of statistical significance at conventional levels although this was related as much to the imprecision of the estimates rather than to a clear absence of differences. The point estimates suggest that the adverse effect on high school completion is driven by out-of-wedlock conceptions while the beneficial effect on family income is primarily a result of births following post-marital conceptions.

It is reassuring that the one exception to the absence of difference is age at first marriage. If miscarriage is truly random, then it cannot affect the age at first marriage among those who are already married. While giving birth lowers the age at first marriage by about a year among those conceiving premaritally, the estimated effect on those who were already married is a relatively precise zero.

Were shotgun marriages good or bad for the pregnant teens? We cannot answer this question quasi-experimentally since we have no arguably exogenous source of variation in whether the mother marries between conception and birth. Instead we limit the sample to women who report that their first conception was premarital and ended in a birth and compare outcomes for those who report that they married prior to the birth and those who do not. The first column of table 6 shows the coefficient on shotgun marriage when we control only for the survey year and the date of conception. The second column adds the rest of our standard controls except that as noted in footnote 16 we cannot control for current age due to multicollinearity.

The results suggest that shotgun marriage had a positive effect on the teen's educational attainment although the 6-7 percentage point effect on high school completion falls well short of statistical significance at conventional levels. If they married, teens who gave birth out-of-wedlock married close to four years later than those who had a shotgun marriage. Not

[16] We cannot control for age because survey year - year of conception + age at conception = age. We are able to control for these variables when we are not focused on the effect of year of conception because minor differences make these variables less than perfectly collinear.

surprisingly, shotgun marriages lead to a greater likelihood both of being married and being divorced, at least when we use controls, but only the latter achieves statistical significance. While their are large and statistically significant differences in the number of marriages (shotgun .4 higher), parity (shotgun .5 lower) and family income (shotgun 31% higher), these differences are largely eliminated by the controls.

In short, while it appears to have been somewhat better for unwed pregnant teens to marry prior to giving birth, the effects do not seem to have been large. These effects do not change over the sample period, with the exception that the positive effect of a shot-gun marriage on high school completion dissipates over time (significant with $p = .057$).

7 Conclusion

Recent literature has found only modest adverse consequences of teen motherhood. One explanation for this finding is that teens who wish to be sexually active now have access to effective contraception and if they fail to use contraception effectively, can often gain access to abortion services. Therefore, the argument continues, teens who would suffer significant adverse consequences if they gave birth avoid doing so. The teens who give birth are the ones for whom the consequences are at most modest.

This paper investigates whether the adverse consequences of teen motherhood were stronger before Roe v Wade, when access to abortion and contraception were much more limited. During this period, teenage girls who wished to be sexually active had to choose between abstinence and risking pregnancy. As long as some of those for whom a teen birth would be costly chose the latter option, it is possible that the cost of teen motherhood was higher in the 1950s and 1960s than it is currently. Of course, it is also possible that the threat of pregnancy was sufficient to deter girls for whom teen motherhood would be costly from engaging in sexual intercourse or that early marriage either before or after conception alleviated the potential costs. Therefore even in this period it is possible that teen motherhood was not costly for those who became pregnant.

Taken together the results suggest a pattern whereby disadvantaged teens who gave birth were more likely never to get married but if they did marry were less likely to divorce. On the contrary, more advantaged teens who gave birth were not less likely to get married but were more likely to divorce and not to remarry. We can only speculate both because our estimates are imprecise and because we do not know how many of the teens married the putative or actual father of their child. However, it appears that for disadvantaged teens, teen motherhood was a significant obstacle to marriage to someone other than the father but if they did marry, the marriages were more likely to persist which yielded higher family incomes. In contrast, advantaged teens who became pregnant were sufficiently likely to marry that, if anything, their marriage rate increased, but such marriages were much less likely to persist.

For those who, in the absence of a child, were predicted to just graduate high school, teen motherhood made it substantially less likely that they would graduate. But this should be viewed not through the prism of 2013 when women who have not graduated high school are unlikely to marry and have poor employment prospects. Rather it appears that in the 1950s and 1960s, there were significant risks associated with teen motherhood, but there was a positive expected payoff for all but the most advantaged. And the risk/return trade-off may have been better for the least advantaged. They increased the probability that in the 1980s they would find themselves with little education and never having been married. But they also increased the probability that they would be on their first marriage and have a higher family income.

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Table 1: Summary Statistics by Birth Outcome

	Birth	Miscarriage	Abortion
Education	10.78	10.81	13.83***
	[2.51]	[2.31]	[2.2]
Education>=12	0.45	0.55	0.97***
	[.5]	[.5]	[.16]
Age at First Marriage	17.68	17.93	22.07***
	[3.11]	[3.46]	[4.81]
Number of Marriages	1.47	1.91***	1.42
	[.99]	[1.09]	[.67]
Currently Married	0.6	0.63	0.69
	[.49]	[.48]	[.48]
Currently Divorced/Separated	0.3	0.23	0.14**
	[.46]	[.42]	[.35]
Never Married	0.07	0.05	0.04
	[.25]	[.21]	[.2]
Number of Live Births	3.22	2.59***	2.09***
	[1.58]	[1.49]	[1.06]
Currently Working	0.63	0.63	0.72
	[.48]	[.48]	[.46]
Family Income	28,286	23,613	48,393.12**
	[22,376]	[18,551]	[27,538]
Current Age	38.53	38.47	38.64
	[4.2]	[4.06]	[4.01]
Age at First Conception	16.43	16.16	16.79
	[1.14]	[1.45]	[1.05]
Age at First Conception<15	0.13	0.16	0.02***
	[.33]	[.37]	[.15]
Hispanic	0.11	0.07	0.02***
	[.31]	[.26]	[.16]
Black	0.26	0.18**	0.33
	[.44]	[.38]	[.48]
White	0.71	0.78	0.67
	[.46]	[.42]	[.48]
Protestant	0.71	0.82**	0.54
	[.45]	[.39]	[.51]
Catholic	0.21	0.11**	0.2
	[.41]	[.32]	[.41]
Working Mother	0.53	0.53	0.58
	[.50]	[.50]	[.51]
Mother's Education	9.54	9.44	12.31***
	[3.52]	[3.45]	[3.32]
Lived with both parents at 14	0.67	0.64	0.73
	[.47]	[.48]	[.46]
Predicted Education	12.79	12.8	14.01***
	[1.37]	[1.23]	[1.32]
Survey1982	0.54	0.46	0.52
	[.5]	[.5]	[.51]
Survey1988	0.35	0.4	0.32
	[.48]	[.49]	[.48]
Number of Observations	1058	79	20

Note: Observations are weighted using the sampling weights of the survey. Standard Deviations are in parentheses. The means for "Lived with both parents at 14" are just calculated over the 1982 and 1988 surveys, as the variable is not available in the 1995 survey. The means for working mother and mother's education are calculated over the non-missing values. Stars in the second column denote statistically significant differences between columns 1 and 2. Stars in the third column denote statistically significant differences between columns 1 and 3.

Table 2: Predictors of Miscarriage

Dependent Variable: Birth	OLS	Probit	Logit
Age	0.002	0.017	0.034
	(0.003)	(0.020)	(0.042)
Age at First Conception	0.017	0.117	0.237
	(0.016)	(0.099)	(0.219)
Age at First Conception<15	0.029	0.210	0.411
	(0.048)	(0.302)	(0.633)
Hispanic	0.025	0.199	0.379
	(0.045)	(0.367)	(0.866)
Black	0.061	0.429	0.941
	(0.069)	(0.398)	(0.804)
White	0.007	0.034	0.098
	(0.066)	(0.378)	(0.748)
Protestant	-0.021	-0.160	-0.333
	(0.033)	(0.259)	(0.554)
Catholic	0.020	0.185	0.406
	(0.036)	(0.313)	(0.706)
Working Mother	-0.001	0.009	0.00007
	(0.022)	(0.152)	(0.331)
Mother's Education	0.001	0.006	0.017
	(0.003)	(0.021)	(0.045)
Lived with Both parents at 14	0.009	0.088	0.149
	(0.024)	(0.164)	(0.351)
Survey1982	0.041	0.297	0.638
	(0.040)	(0.276)	(0.592)
Survey1988	0.010	0.037	0.132
	(0.039)	(0.251)	(0.516)
Observations	1137	1137	1137

Notes: Robust standard errors in brackets. Probit and Logit coefficients, not marginal effects, are reported. Dependent variable is an indicator for first pregnancy resulted in a birth, and is zero if first pregnancy resulted in a miscarriage. Working Mother refers to whether the respondent's mother worked either full- or part-time while the respondent was growing up. Mother's education refers to the education of the respondent's mother. Indicators for whether each of these two variables is not missing are also included in the regressions.

Table 3: Effects of Teenage Motherhood, Conceptions before 1969

	Education	Education ≥12	Age at First Marriage	Married	Divorced/ Separated	Never Married	# of Marriage	# Live Births	Ln(Family Income)
Birth	-0.20	-0.14**	-0.58	0.02	0.03	0.003	-0.34**	0.56***	0.28**
	[0.30]	[0.07]	[0.39]	[0.07]	[0.06]	[0.02]	[0.15]	[0.16]	[0.14]
Age	0.001	0.001	-0.01	-0.0008	0.01	-0.01***	0.02*	0.12***	-0.002
	[0.02]	[0.01]	[0.02]	[0.01]	[0.01]	[0.002]	[0.01]	[0.02]	[0.01]
Age at First Conception	0.34***	0.07***	0.65***	0.002	0.02	-0.01	-0.06	-0.12*	-0.04
	[0.11]	[0.02]	[0.13]	[0.02]	[0.02]	[0.01]	[0.05]	[0.06]	[0.04]
Age at First Conception<15	-0.49	0.06	-0.14	-0.04	0.13*	-0.06*	-0.10	0.38	-0.26*
	[0.36]	[0.08]	[0.40]	[0.08]	[0.07]	[0.03]	[0.17]	[0.23]	[0.14]
Hispanic	-1.08**	-0.09	0.52	-0.08	0.01	0.06	-0.14	0.61**	-0.23
	[0.43]	[0.08]	[0.38]	[0.09]	[0.08]	[0.04]	[0.20]	[0.27]	[0.15]
Black	1.63**	0.20*	2.89***	-0.58***	0.36***	0.20***	-0.56***	0.31	-0.64***
	[0.77]	[0.11]	[0.37]	[0.08]	[0.07]	[0.04]	[0.16]	[0.26]	[0.16]
White	0.86	0.11	-0.07	-0.21***	0.22***	-0.0008	0.19	-0.26	-0.19
	[0.77]	[0.11]	[0.27]	[0.08]	[0.06]	[0.03]	[0.16]	[0.25]	[0.16]
Protestant	0.28	0.11	-0.20	0.09	-0.04	-0.04	-0.19	-0.02	0.12
	[0.34]	[0.08]	[0.26]	[0.07]	[0.07]	[0.03]	[0.21]	[0.21]	[0.14]
Catholic	0.35	0.19**	-0.23	-0.11	0.10	-0.01	-0.38	-0.08	0.08
	[0.42]	[0.09]	[0.30]	[0.08]	[0.08]	[0.03]	[0.25]	[0.24]	[0.17]
Working Mother	0.14	-0.01	0.04	-0.04	0.04	-0.004	0.14	0.0003	0.03
	[0.18]	[0.04]	[0.18]	[0.04]	[0.04]	[0.01]	[0.09]	[0.11]	[0.07]
Mother's Education	0.20***	0.04***	0.06**	-0.01	0.01**	-0.003	0.01	-0.06***	0.04***
	[0.03]	[0.01]	[0.03]	[0.01]	[0.01]	[0.003]	[0.01]	[0.02]	[0.01]
Lived with Both Parents at 14	0.24	0.07	-0.15	0.06	-0.04	-0.02	-0.06	-0.20	0.17**
	[0.18]	[0.04]	[0.20]	[0.04]	[0.04]	[0.02]	[0.10]	[0.13]	[0.08]
Survey1982	-0.35	-0.02	-0.28	-0.01	0.06	-0.05*	-0.12	1.38***	-0.68***
	[0.33]	[0.07]	[0.41]	[0.07]	[0.07]	[0.03]	[0.15]	[0.23]	[0.13]
Survey1988	-0.16	-0.001	0.24	0.01	0.001	-0.01	-0.04	0.89***	-0.40***
	[0.30]	[0.06]	[0.40]	[0.06]	[0.06]	[0.03]	[0.13]	[0.19]	[0.12]
Observations	1,137	1,137	1,021	1,137	1,137	1,137	1,137	1,137	986

Notes: Robust standard errors in brackets. Birth is an indicator for first pregnancy resulted in a birth, and is zero if first pregnancy resulted in a miscarriage. Working Mother refers to whether the respondent's mother worked either full- or part-time while the respondent was growing up. Mother's education refers to the education of the respondent's mother. Indicators for whether each of these two variables is not missing are also included in the regressions.

Table 4: Effects of Teenage Motherhood, Allowing for Heterogeneity by Predicted Education

	Education	Education ≥12	Age at First Marriage	Married	Divorced/ Separated	Never Married	# of Marriages	# Live Births	Ln(Family Income)
Panel A: Linear Interaction Only									
Birth	-0.40	-0.20**	-0.87*	0.10	-0.06	0.04*	-0.24	0.56***	0.43**
	[0.43]	[0.09]	[0.47]	[0.10]	[0.09]	[0.02]	[0.16]	[0.22]	[0.18]
Birth*PredEd	0.24	0.07	0.38	-0.10	0.11**	-0.04*	-0.12	0.0005	-0.17
	[0.25]	[0.06]	[0.26]	[0.06]	[0.06]	[0.02]	[0.11]	[0.14]	[0.11]
p-value for joint significance of Birth variables									
	0.60	0.10	0.15	0.26	0.06	0.08	0.09	0.004	0.05
Effect by Predicted Education									
10 years	-0.87	-0.34*	-1.64*	0.29	-0.28	0.13**	-0.003	0.56	0.78**
	[.86]	[.19]	[.87]	[.21]	[.19]	[.06]	[.31]	[.45]	[.37]
14 years	0.08	-0.05	-0.1	-0.1	0.17**	-0.05	-0.49**	0.56**	0.09
	[.37]	[.09]	[.47]	[.08]	[.07]	[.05]	[.23]	[.22]	[.17]
Panel B: Quadratic Interaction									
Birth	-0.47	-0.22**	-1.09*	0.15	-0.07	0.04*	-0.31	0.56**	0.47**
	[0.51]	[0.11]	[0.60]	[0.11]	[0.10]	[0.03]	[0.20]	[0.25]	[0.20]
Birth*PredEd	0.16	0.05	0.24	-0.07	0.11	-0.04**	-0.19	0.04	-0.16
	[0.35]	[0.08]	[0.40]	[0.11]	[0.10]	[0.02]	[0.14]	[0.25]	[0.13]
Birth*PredEd^2	0.07	0.02	0.16	-0.03	0.01	-0.01	0.06	-0.02	-0.01
	[0.19]	[0.04]	[0.21]	[0.05]	[0.05]	[0.02]	[0.08]	[0.11]	[0.08]
p-value for joint significance of Birth variables									
	0.79	0.22	0.26	0.29	0.16	0.12	0.15	0.01	0.08
Effect by Predicted Education									
10 years	-0.51	-0.23	-0.93	0.16	-0.26	0.1	0.3	0.38	0.74
	[1.3]	[.29]	[1.42]	[.42]	[.39]	[.07]	[.49]	[.95]	[.51]
14 years	0.11	-0.04	0.03	-0.12	0.17**	-0.05	-0.45*	0.56***	0.08
	[.37]	[.09]	[.4]	[.09]	[.08]	[.05]	[.23]	[.22]	[.18]
Observations	1,137	1,137	1,021	1,137	1,137	1,137	1,137	1,137	986

Notes: Bootstrapped standard errors in brackets. Birth is an indicator for first pregnancy resulted in a birth, and is zero if first pregnancy resulted in a miscarriage. The sample is limited to conceptions before 1969. PredEd denotes the predicted education of the respondent, calculation explained in the paper. This variable is rescaled to be PredEd-12. The values of PredEd for 10 and 14 years are thus actually -2 and +2. Explanatory variables include predicted education squared (only in panel B), current age, age at first conception, respondent's mother's education, and an indicator for this variable non-missing, and indicators for age at first conception less than 15, Hispanic, Black, White, Protestant, Catholic, respondent's mother worked while growing up, an indicator for this variable non-missing, respondent lived with both parents at the age of 14, and indicators for the survey year.

Table 5: Effects of Teenage Motherhood, Conceptions Before 1970

	Education	Education ≥12	Age at First Marriage	Married	Divorced/ Separated	Never Married	# of Marriages	# Live Births	Ln(Family Income)
Panel A: Without Heterogeneity by Predicted Education									
Birth	-0.16	-0.11*	-0.50	0.04	0.03	-0.02	-0.28**	0.59***	0.27**
	[0.28]	[0.06]	[0.35]	[0.06]	[0.05]	[0.03]	[0.14]	[0.15]	[0.12]
Panel B: Heterogeneity by Predicted Education, Linear Interaction Only									
Birth	-0.28	-0.15*	-0.74	0.11	-0.05	0.01	-0.15	0.56***	0.42**
	[0.37]	[0.08]	[0.46]	[0.09]	[0.08]	[0.04]	[0.15]	[0.22]	[0.17]
Birth*PredEd	0.14	0.05	0.29	-0.08	0.09*	-0.04	-0.15	0.04	-0.15
	[0.23]	[0.05]	[0.26]	[0.06]	[0.05]	[0.02]	[0.10]	[0.14]	[0.11]
p-value for joint significance of Birth variables									
	0.75	0.18	0.27	0.35	0.09	0.26	0.07	0.0002	0.04
Effect by Predicted Education									
10 years	-0.56	-0.25	-1.32	0.28	-0.24	0.08	0.16	0.48	0.73*
	[.77]	[.17]	[.89]	[.2]	[.17]	[.07]	[.29]	[.47]	[.37]
14 years	0.01	-0.06	-0.16	-0.06	0.13**	-0.06	-0.45**	0.64***	0.12
	[.35]	[.08]	[.44]	[.07]	[.06]	[.04]	[.2]	[.2]	[.16]
Panel C: Heterogeneity by Predicted Education, Quadratic Interaction									
Birth	-0.29	-0.16	-0.77	0.17*	-0.07	0.01	-0.18	0.56**	0.45**
	[0.46]	[0.10]	[0.56]	[0.10]	[0.09]	[0.05]	[0.20]	[0.23]	[0.19]
Birth*PredEd	0.11	0.03	0.26	-0.05	0.08	-0.03	-0.19	0.09	-0.15
	[0.31]	[0.07]	[0.43]	[0.10]	[0.08]	[0.02]	[0.12]	[0.28]	[0.14]
Birth*PredEd^2	0.02	0.01	0.03	-0.04	0.01	-0.004	0.04	-0.03	-0.01
	[0.17]	[0.04]	[0.21]	[0.05]	[0.04]	[0.02]	[0.07]	[0.12]	[0.08]
p-value for joint significance of Birth variables									
	0.92	0.38	0.53	0.25	0.20	0.32	0.13	0.001	0.09
Effect by Predicted Education									
10 years	-0.44	-0.18	-1.16	0.13	-0.19	0.06	0.36	0.25	0.7
	[1.15]	[.25]	[1.58]	[.38]	[.31]	[.08]	[.44]	[1.06]	[.55]
14 years	0.02	-0.05	-0.11	-0.09	0.14**	-0.07	-0.42**	0.62***	0.11
	[.35]	[.08]	[.37]	[.08]	[.07]	[.05]	[.21]	[.19]	[.17]
Observations	1,330	1,330	1,187	1,330	1,330	1,330	1,330	1,330	1,150

Notes: Robust standard errors in brackets in panel A. Bootstrapped standard errors in brackets in panels B and C. Birth is an indicator for first pregnancy resulted in a birth, and is zero if first pregnancy resulted in a miscarriage. PredEd denotes the predicted education of the respondent, calculation explained in the paper. This variable is rescaled to be PredEd-12. The values of PredEd for 10 and 14 years of education are thus actually -2 and +2. Explanatory variables include predicted education squared (only in panel B), current age, age at first conception, respondent's mother's education, and an indicator for this variable non-missing, and indicators for age at first conception less than 15, Hispanic, Black, White, Protestant, Catholic, respondent's mother worked while growing up, an indicator for this variable non-missing, respondent lived with both parents at the age of 14, and indicators for the survey year.

Table 6: Effects of Teenage Motherhood: Shot-Gun Marriages Relative to Out-of-Wedlock Births

Outcome	(1)	(2)
Education	0.45**	0.54**
	[0.23]	[0.25]
Education ≥12	0.07	0.06
	[0.05]	[0.05]
Age at First Marriage	-3.76***	-3.63***
	[0.28]	[0.34]
Married	0.24***	0.07
	[0.04]	[0.05]
Divorced/Separated	0.01	0.09**
	[0.04]	[0.04]
# of Marriages	0.37***	0.05
	[0.11]	[0.11]
# Live Births	-0.51***	-0.07
	[0.13]	[0.13]
Ln(Family Income)	0.31***	0.08
	[0.09]	[0.10]
Controls for Survey Year, Date of Conception	Yes	Yes
Standard Controls	No	Yes

Notes: Each row presents the coefficient, from separate regressions, on an indicator for shot-gun marriage, on the sample of teens giving birth who conceived before marriage. Robust standard errors in brackets. The explanatory variables in column 1 are the date of conception, and indicators for survey year. Column 2 additionally includes the explanatory variables listed in table 5, excluding Age because of multicollinearity. Observations are weighted by the sampling weights of the survey. Sample Size is 825 for all outcome variables, except for Age at First Marriage (715) and Family Income (723).