Demography (2019) 56:345–365
https://doi.org/10.1007/s13524-018-0748-6

The Timing of Teenage Births: Estimating the Effect on High School Graduation and Later-Life Outcomes

Lisa Schulkind[1] · **Danielle H. Sandler**[2]

Published online: 3 January 2019

Abstract
We examine the long-term outcomes for a population of teenage mothers who give birth to their children around the end of high school. We compare the mothers whose high school education was interrupted by childbirth (because the child was born before her expected graduation date) with mothers who did not experience the same disruption to their education. We find that mothers who gave birth during the school year are 5.4 percentage points less likely to complete their high school education, are less likely to be married, and have more children than their counterparts who gave birth just a few months later. The wages for these two sets of teenage mothers are not statistically different, but with a lower likelihood of marriage and more children, the households of the treated mothers are more likely to fall below the poverty threshold. Although differences in educational attainment have narrowed over time, the differences in labor market outcomes and family structure have remained stable.

Keywords Teenage childbearing · Signaling value · Education · Family structure

Electronic supplementary material The online version of this article (https://doi.org/10.1007/s13524-018-0748-6) contains supplementary material, which is available to authorized users.

✉ Lisa Schulkind
lschulki@uncc.edu

Danielle H. Sandler
danielle.h.sandler@census.gov

[1] Department of Economics, University of North Carolina at Charlotte, 9201 University City Boulevard, Charlotte, NC 28223-0001, USA

[2] Center for Economic Studies, U.S. Census Bureau, 4600 Silver Hill Road, Washington, DC 20233, USA

Introduction

An extensive body of literature has established a strong correlation between teenage childbearing and poor economic outcomes later in life. Teenage mothers are less likely to complete high school, are less likely to be working, have lower earnings, and are less likely to be married than those women who did not become teenage mothers (Card and Wise 1978; Ellwood 1989; Trussell 1988; Upchurch and McCarthy 1990). However, evidence has shown that the young women who have children as teenagers come from less advantaged backgrounds than those who delay childbearing until later in life (Abrahamse et al. 1988; Hayes 1987), making it very difficult to determine whether having a child as a teenager causes the poor economic outcomes or whether they are symptoms of the less advantaged upbringing that is also correlated with these outcomes. In fact, the few studies that have used more sophisticated identification methods than comparing teenage mothers with those who delayed childbearing have found that the effect of teenage childbearing on economic outcomes is small (Ashcraft et al. 2013; Geronimus and Korenman 1992; Hotz et al. 2005; Olsen and Farkas 1989).

One clear difference between women who have their first child as teenagers and women who do not is that a teenage mother's high school experience is more likely to be interrupted by the arrival of her child. A common policy prescription for improving the economic well-being of teenage mothers and their families is to make sure that the mothers have the necessary support to graduate from high school, and we aim to shed light on whether this type of policy is likely to have the intended effect. We look at the effect of having a child during high school versus becoming a young mother after already having finished high school. Specifically, we compare the outcomes of women who had a child near the end of their senior year of high school (treatment group) with those who have a child just after the (expected) end of high school (control group). In classifying women into groups, we assume normal school progress. We define "near the end of their senior year" as occurring in the six months before the end of women's expected senior year of high school, regardless of whether they have already dropped out or have been held back along the way; we define "just after the end of high school" as occurring in the six months after the graduation date, regardless of whether they graduated. In the next section, we discuss this assumption as well as other issues related to sample construction.

These two groups are similar in many respects, even when it comes to the interruption caused by childbearing. Both groups of mothers discover they are pregnant while they are still of high school age, so any changes in expectations for the future as well as any resulting changes in investment in education could begin for both groups before expected graduation. Changes in investment could include the decision to drop out of high school or the decision to return to school after having dropped out, which Upchurch and McCarthy (1990) found to be an important reason for the difference in high school graduation rates for young mothers. In addition, both groups of mothers will begin to experience physical changes related to pregnancy, including morning sickness, before graduation. To the extent that these changes make remaining in school difficult, both groups will experience at least the first trimester of pregnancy before graduation. The experiences of the young women in the treatment and control groups differ in two primary ways. First, women in the treatment group learn of their pregnancy and experience pregnancy symptoms earlier in their high school career.

These women need to remain in school longer after becoming pregnant to make it to graduation, and any challenges or changes in investment will begin earlier, relative to their expected high school graduation. Second, only women in the treatment group experience the acute change in demands on their time from needing to care for an infant and health effects from delivery prior to graduation.

This natural experiment does not allow us to identify the effect of *becoming* a teenage mother, given that both the treatment and control groups comprise teenage mothers. Instead, the differences that we find identify the effect of this relatively small difference in timing for the population of teenage mothers. We find that women whose high school education was interrupted by the birth of their child are 5.4 percentage points less likely to complete their high school education than those who had their first child a few months later. We also find that the interruption of schooling affects later-life outcomes of the teenage mothers. Young women who have a child prior to their expected graduation date are slightly less likely to be working or married and have a higher number of children, on average. We find no statistically significant difference in earnings for the women whose education was interrupted. As a result of having more children and a lower likelihood of being married, they and their families are more likely to fall below the federal poverty line (FPL).

Our finding that the sizable difference in educational attainment has no estimated effect on wage income differs from the majority of studies that estimate the signaling value of a high school degree and find (sometimes large) positive effects (see, e.g., Jaeger and Page 1996; Tyler et al. 2000), but it is consistent with findings of Clark and Martorell (2014), who found little evidence of a signaling effect when comparing high school seniors who score just above the passing threshold on their high school exit exam with those who score just below. Knowing the difference in the value of a degree for this population is important for policy-makers who have worked hard to increase the graduation rate for teenage mothers. For example, one of the benefits of Title IX of the 1972 Educational Amendments to the Civil Rights Act, which prohibits discrimination on the basis of gender, is that it has increased the high school graduation rates for teenage mothers (Guldi 2016). We also see teen fertility responding to incentives produced by immigration policy (Kuka et al. 2018). Understanding the potential long-term effects of these policies is important, both to know where the young women affected will be helped as well as where they may need additional support.

Data

Our study takes advantage of the fact that the timing of birth in the teenage years might matter for future outcomes. In particular, we look at a tight band of time around expected high school graduation. The main comparison examines the differences in outcomes for women who had their first child in January through June of their (expected) senior year of high school and women who had their first child shortly after the (expected) end of their senior year, between July and December. In a robustness check, we also consider a smaller window, where women had their first child in April–June for the treatment group and in July–September for the control group. The main identifying assumption is that the two groups of teenage mothers, who differ in the timing of their births by only a few months, do not differ prior to childbearing. In the upcoming section, Validity of the Identification Assumption, we provide evidence that this is the case.

The main analysis uses the population of 20- to 35-year-old mothers from the 1980 and 2000 censuses (U.S. Census Bureau 1980, 2000) as well as the American Community Survey (ACS) (U.S. Census Bureau 2005–2014). One benefit of using census and ACS data is a large sample size. Our main treatment and control groups include more than 450,000 teenage mothers. The census data come from the long-form of the decennial census, a one-in-six sample of U.S. households that were asked a longer series of questions, including questions on education, labor force status, and income. The ACS is an ongoing survey of approximately 3 million households annually. The data we use come from the 2005–2014 ACS surveys. The sample is limited to women aged 20–35 at survey. For a woman to be observed as a mother, her child must be living in the same household when surveyed. The youngest mothers in the treatment group were 17 years old when they gave birth, meaning that their children turn 18 when the mothers are 35 years old. Figure A1 in the online appendix shows the relationship between the timing of the birth of the child and the date of the survey. We limit the sample to mothers who were at least 20 years old by the survey date to abstract from the end of high school, thus excluding those that became teenage mothers within two to three years of the survey date.

We use information on the relationship of the household members to the household head to identify mothers and children in each survey. The 2000 census data and the ACS include exact birthdate for both the mother and child. Using this information for the mother and for the oldest "own child" living with her at survey date,[1] we calculate each mother's age when she gave birth to her first child. However, the 1980 census includes only the age (on the survey date: April 1, 1980) and quarter of birth for both the mother and child. We use this information, together with the assumption that if both mother and child are born in the same quarter, the mother's birthday is first, to impute the age of the mother when she gave birth.[2]

Figure A2 in the online appendix shows how women are assigned to groups. To assign women to the treatment and control groups, we assume that the cutoff for starting school is October 1 (the first day of the third quarter of the calendar year) and that everyone makes normal school progress. We assume normal school progress out of necessity (retrospective information on school progress at the time of giving birth is not available in the data) but also to abstract from any endogenous differences in school progress. By imposing normal school progress on all mothers, we introduce some measurement error but do not introduce any additional bias stemming from endogenous differences resulting from the pace of school progress. We also do not know, for the women who do not complete high school, when they dropped out of high school. Some may have dropped out before becoming

[1] We only include children who are 18 or younger at survey date.

[2] Because year of birth is not available in the 1980 census data, survey date is important for calculating each mother's age when she gave birth. For example, imagine a mother who was born on April 15, 1950 (Q2) and has a child who was born on May 1, 1965 (Q3). On survey day, the mother is 29, and her child is 14 (neither one has had their birthday yet). Here, it is easy to see that the mother was 15 years old when her child was born. Now, imagine that the mother was born on March 1, 1950 (Q1). In this example, the mother has already turned 30 by April 1, but the child is still 14. Knowing that April 1 falls between their birthdays allows us to accurately calculate that the mother was 15 years old when her child was born, not 16. This is used only in 1980 because the 2000 census and ACS have exact date of birth, so approximating the quarter of birth is not necessary.

pregnant. The shaded rows in the first two columns represent the treated group, and the shaded rows in the third and fourth columns of Fig. A2 represent the control group. Each cell displays the school year at ages 17, 18, and 19 for someone who has made normal progress in school. For example, if a woman was born in Q1, had her first child in Q1, and was 18 years old when she had the child, she became a mother during January–March of her senior year of high school. She is assigned to the treatment group. However, if a woman was born in Q1, had her first child in Q3, and was 18 when she had the child, she became a mother during the summer after her senior year of high school and is assigned to the control group.

Table 1 displays the summary statistics for the treatment and control groups as well as for a sample of women who delayed childbearing until they were between 23 and 25 years old.[3] Research has found evidence of seasonality in the types of women giving birth over the year (Buckles and Hungerman 2013), so these women will be used to difference out any seasonality. Women who had their first child between January and June are the treated group, and women who had their first child between July and December are the control group. Among the teenage mothers, the treatment group is slightly younger at survey date, and a higher percentage of the group is black or Hispanic. These differences highlight the importance of including controls for age and race/ethnicity in the main specification. In addition, we stratify the results by race, looking at white, black, and Hispanic mothers separately both because of this difference in the treatment and control groups and because we believe these young women may have substantially different experiences due to unobserved differences in their environments.

Table 1 also gives a first look at the main outcome variables of interest. The asterisks in the first column indicate a statistical difference between means for the treatment and control groups in the teenage cohort.[4] The treated group has fewer years of completed education and is less likely to have completed high school.[5] They are also less likely to be married, have more children, have slightly lower family income levels, and are more likely to fall below the FPL.[6]

Figure 1 gives another initial picture of our main outcome variables of interest. The figure shows the average value of the education variables for women who had their first children in the months surrounding their expected graduation date. Although these figures do not include any of the control variables described in the next section and include only the women surveyed in the 2000 census and the ACS, they help us understand the nature of the first-stage variation (the effect of timing of birth on education). The probability of obtaining a high school degree increases leading up to and following expected high school graduation. However, we see do not see a break in trend or discontinuity, suggesting that the treatment is more continuous than one might expect. The timing of births is important for the probability of completing high school, but we do not see evidence that one particular time during this period around high school graduation matters most.

Although important for interpreting our results, the lack of an observed break in trend is perhaps unsurprising for a few reasons. First, all women observed in the 2000 census and ACS samples completed high school after passage of Title IX, which requires schools to

[3] We pick ages 23–25 because most women will have completed their education by this age. In a robustness check, we also consider a younger control group.

[4] These are simple differences without controls or clustered standard errors.

[5] “High School Education” is defined as having completed at least 12 years of education or having obtained a General Educational Development (GED) certification.

[6] Summary statistics that have been compiled separately for each survey are available in Tables A1–A3 in the online appendix.

Table 1 Summary statistics, full sample

	Teen		Older	
	Control	Treatment	Control	Treatment
Age	27.76***	27.64	29.76***	29.74
Black	0.159***	0.174	0.0734***	0.0706
Hispanic	0.155***	0.160	0.108***	0.105
Education (years)	11.82***	11.68	13.40	13.40
High School Education	0.757***	0.705	0.937***	0.938
College Graduate	0.0279*	0.0268	0.208	0.207
Married	0.653***	0.634	0.825**	0.828
Total Children	2.263***	2.327	1.780**	1.785
Working	0.548**	0.544	0.565	0.566
Wage	10,340.4	10,367.4	12,901.0*	12,968.9
Total Family Income	39,907.8***	39,274.5	55,149.9**	55,388.6
Log Wage	9.200	9.204	9.442	9.442
Log Total Income	10.28***	10.26	10.68***	10.68
<100 % Federal Poverty Line	0.229***	0.243	0.0919*	0.0908
<200 % Federal Poverty Line	0.515***	0.535	0.266**	0.264
N	248,400	198,100	614,200	587,900

Notes: Data compiled from the 1980 census, the 2000 census, and the 2005–2014 ACS samples. Women who had their first child between January and June are Treated, and women who had their first child between July and December are the Control group. The first two columns show the averages for the treatment and control groups of a cohort of teenage mothers. The last two columns show the averages for a cohort of women who had their first child between ages 23 and 25. The asterisks represent statistically significant differences between the treatment and control groups, within cohort.

*$p < .05$; **$p < .01$; ***$p < .001$

provide equal access to education for girls, even if they are pregnant or have children (Guldi 2016). So, it is very unlikely that schools would force girls to drop out or purposefully make attending more difficult because doing so would put their federal funding at risk. Second, giving birth is likely to disrupt schooling, and having an infant at home continues the potential disruption. So, the earlier the birth, the longer the mother must balance school and motherhood in order to complete her high school education. Similarly, the initial disruption might occur when the mother discovers she is pregnant. Even for our control group, this realization occurs during the school year, which might affect her likelihood of graduating. Finally, our measure of high school graduation includes those who received their General Education Development (GED) certification. Even if a mother drops out of high school, her likelihood of successfully completing a GED is expected to increase with the length of time she remained in school.

However, there *is* a break in the probability of having some college education or a college degree. For those giving birth after expected graduation, the slopes become steeper. Furthermore, it is interesting to note that although they are part of our control group, those giving birth during the summer between high school and college are even less likely to begin or complete college than those who give birth at the end of their final year of high school. This finding suggests that the summer between high school and college is a

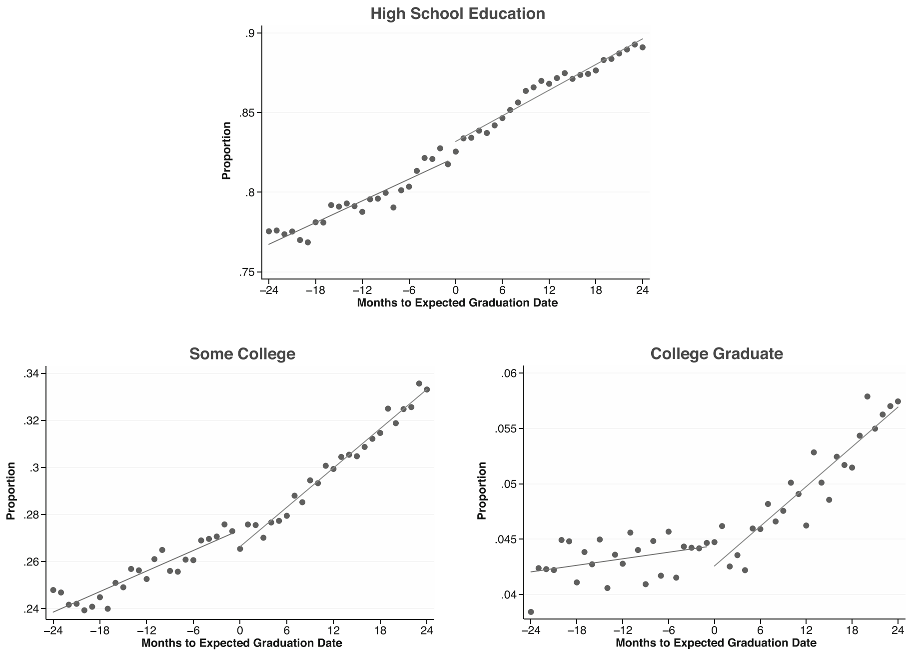

Fig. 1 Birth and graduation timing. Each data point is the average value of the variable for women who had their first children *t* months from their expected graduation date, where *t* is given on the *x*-axis. The sample includes only those women who were at least 23 years old when surveyed and were surveyed in the 2000 census and 2005–2014 ACS samples.

crucial period for college attendance decisions. This is also consistent with Upchurch and McCarthy's (1990) finding that reentry is important for differences in educational attainment. If the period after high school is a natural time to take a break from school, those who give birth exactly in that period might be least likely to return to school (start college) even compared with those who had to return in order to finish high school.

Again, these graphs do not include any control variables, and the next section describes our estimation strategy that uses this timing to better identify the effect of the timing of teenage pregnancy on educational and other outcomes.

Estimation Strategy

To estimate the effect of an interruption in high school education on our outcomes of interest, we estimate the following equation, first for the full sample and then separately for white, black, and Hispanic mothers:

$$\begin{aligned} Outcome_{isarc} = \alpha + \beta_1 Treat_{isarc} \times Teen_{isarc} + \beta_2 Treat_{isarc} + \beta_3 Teen_{isarc} \\ + \Phi_s + \Phi_a + \Phi_r + \Phi_c + \varepsilon_{isarc}, \end{aligned} \quad (1)$$

where $Treat_{isarc}$ is an indicator variable equal to 1 if individual *i*'s child was born January–June, and $Teen_{isarc}$ is an indicator variable equal to 1 if the mother had her first child as a teenager. The coefficient of interest is β_1, which measures the difference-in-differences for the outcome variable. It gives the effect of giving birth just before the end of high school rather than just after the end of high school, with seasonality differenced out by the sample of older mothers. Additionally, all regressions include a full set of fixed effects for state (Φ_s), mother's age at survey (Φ_a), mother's race (Φ_r), and survey year (Φ_c). Standard errors are clustered by state.

We examine a number of outcome variables. First, we look at whether there is a difference in years of education or in the probability that the mother has completed at least 12 years of school.[7] Next, we estimate whether the interruption in education has detrimental effects on income or on the probability that her family falls below 100 % or 200 % of the FPL. Then, we look at whether there are any differences in family structure at survey date. Does she have more children, is the age gap between the first and second child larger or smaller, and is she more or less likely to be married?

Validity of the Identification Assumption

To validate our identification assumptions, we show that prior to their pregnancy, the young women in the two groups of teenage mothers, who differ in the timing of their births by only a few months, do not differ otherwise. In addition, within this small window, we argue that the timing of the birth is exogenous given that most teenage pregnancies are unplanned. However, it is possible that the young women in the control group were planning to have a child just after graduation. Then, we would be concerned that much of our variation is driven by wanting/not wanting to have a child. The following analysis attempts to rule out any pre-birth differences and any differences in planning.

Natality Data

We use Natality Data from the National Vital Statistics System of the National Center for Health Statistics from the years 1969–2012 to look at seasonality in the demographics of women giving birth over the year. The files contain a mix of either a 50 % or 100 % sample of births (depending on state and year). The records contain data on the mother's age, race, and number of prenatal visits.

In addition to overall birth rate, Fig. 2 shows changes in the racial composition and average number of prenatal visits over the year. Despite seasonality in birth rates and in the racial composition of mothers, the pattern is very similar for the two cohorts. One concern was that some teenage mothers *plan* to give birth just after the end of the school year, and that this differential selection into the control group might bias our results. If this were the case, we would expect to see a larger spike in teenage birth rates in the summer relative to birth rates for older mothers. The similarity in summer patterns helps alleviate the concern that this type of selection is problematic. Although the older cohort has more prenatal visits, on average, when compared with the teenage cohort, there does not appear to be strong seasonality in this measure for either group.

For all outcomes in Fig. 2, we also run a simple difference-in-differences model, without any additional control variables and without clustering standard errors, to test whether any of these outcomes were statistically different. For the first two outcomes, the regressions

[7] This also includes women who have obtained a GED.

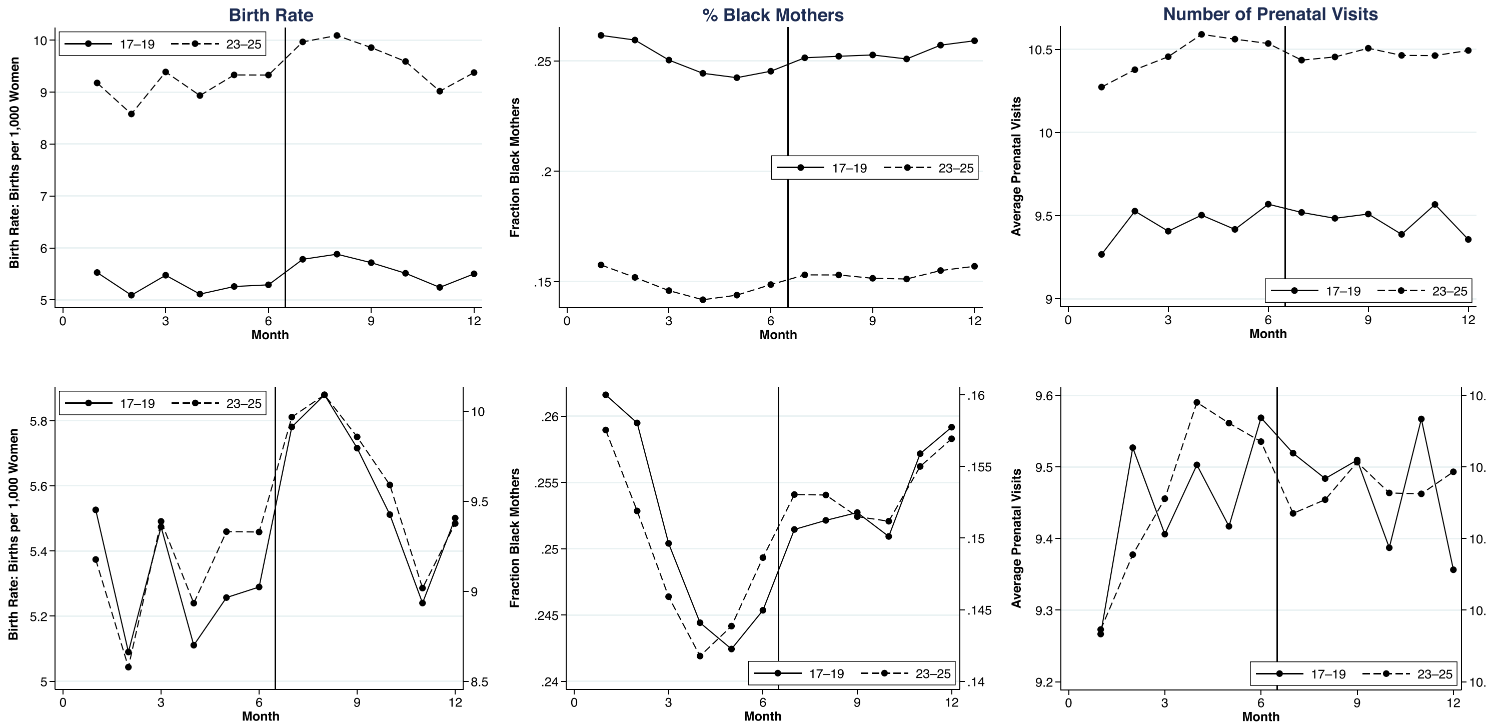

Fig. 2 Seasonality of births. Figures are compiled using the 1970–2012 Natality Data from the National Vital Statistics System of the National Center for Health Statistics. The solid connected line shows the average, by month of (child's) birth, for 17- to 19-year-old mothers. The dashed line shows the average for 23- to 25-year-old mothers. Graphs in the first row show both age groups on the same scale; graphs in the second row use different scales to allow for a better comparison of the seasonal patterns (left axis for 17- to 19-year-olds and right axis for 23- to 25-year-olds). Observations to the left of the vertical line are equivalent to the treated group (January–June births), and those to the right are equivalent to the control group (July–December births).

are (necessarily) run after the data are collapsed into rates. We confirm no statistical differences in birth rates or the percentage of mothers who are black, although the sample size—after the data are collapsed—is small. For the number of prenatal care visits, the regression is run on the full sample of 36 million individuals. After differencing out seasonal differences for the older mothers, we find a statistically significant difference-in-differences, indicating that treated teenage mothers have 0.05 fewer visits, on average, than the control group of teenagers. The direction of this difference indicates that the treated mothers might be less advantaged, or at least that they are less likely to see a doctor as often. However, this difference is very small, at roughly 0.01 of a standard deviation.[8]

National Survey of Family Growth

We use data from the National Survey of Family Growth (NSFG) to examine whether the treatment and control groups are similar on a number of additional measures, many of them determined pre-pregnancy. Although this data set is not ideal for the main analysis because of its small size and imprecise income data, it offers a number of interesting survey questions. The survey includes questions regarding sexual activity, sexual education, and family background. The variables we use are defined in Table A4 in the online appendix. For each variable, we estimate the difference-in-differences, using a sample of 23- to 25-year-olds to difference out seasonality, as we do in the main results. These results are displayed in Table A5 in the online appendix. The difference-in-differences is not statistically significant for any of the control variables tested. These results lend credibility to the assumption that these are comparable groups. That said, the signs of the coefficients point toward the treatment group being slightly less advantaged than the control group. If this is truly the case, we will be biased toward finding a negative effect of having an interruption during high school. It will bias both the education *and* the income coefficients, which are both expected to be negative, to be larger in magnitude.

National Longitudinal Survey of Youth 1979

Finally, we use data from the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 offers a second look at some of the family background variables that are also available in the NSFG. These data also allow us to look at additional family background measures and measures of cognitive skills. Perhaps most importantly, for the subset of women who were surveyed before entering (expected) 11th grade, we can examine educational and family structure aspirations, measured before their pregnancies began. The variables we use are defined in Table A6 in the online appendix. As with the NSFG, for each variable, we estimate the difference-in-differences, using a sample of 23- to 25-year-olds to difference out seasonality. These results are displayed in Table A7 in the online appendix. First, we look at what will be two of our main outcome variables to check whether the pattern matches what we find in the main estimates. We see that, as expected, the treatment group completes fewer years of education and is less likely to graduate from high school, although only years of education is statistically different from 0. Next, we test a number of pretreatment variables. The difference-in-differences is not statistically significant for any of the

[8] The coefficient is –0.046, and the standard deviation of the number of visits is 4.13. More details are available upon request.

Table 2 Effect of teen fertility on education

	All	White	Black	Hispanic
High School Education	–0.0536***	–0.0631***	–0.0404***	–0.0362***
	(0.00236)	(0.00328)	(0.00335)	(0.00344)
	0.734	0.744	0.787	0.629
Some College	–0.00919***	–0.00723***	–0.0191***	–0.0129**
	(0.00148)	(0.00169)	(0.00496)	(0.00458)
	0.194	0.18	0.264	0.168
College Graduate	0.00122	0.00242*	0.000721	–0.00698***
	(0.000913)	(0.00107)	(0.00242)	(0.00197)
	0.0274	0.0261	0.0341	0.0218
Years of Education	–0.136***	–0.130***	–0.126***	–0.199***
	(0.00824)	(0.00977)	(0.0216)	(0.0226)
	11.75	11.85	12.17	10.83
Number of Observations	1,648,600	1,217,700	160,600	198,100

Notes: Each cell contains the coefficient β_1 from a separate estimation of the following equation: $Outcome_{isarc} = \alpha + \beta_1 Treat_{isarc} \times Teen_{isarc} + \beta_2 Treat_{isarc} + \beta_3 Teen_{isarc} + \Phi_s + \Phi_a + \Phi_r + \Phi_c + \varepsilon_{isarc}$ on data from the 1980 census, 2000 census, and 2005–2014 ACS. The variable listed in the first column is the outcome variable. Mean values of each outcome variable are listed below the coefficient and standard errors. Standard errors are shown in parentheses and are clustered by state.

$^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

pretreatment variables tested. In contrast to the NSFG analysis, examining the signs of the coefficients does not consistently suggest that the treated group is less advantaged. For example, the treated group's mothers are less likely to have completed 12th grade, but their fathers are more likely. In addition, although the treated group scored lower on the Armed Forces Qualification Test (AFQT) when surveyed in 10th grade or earlier, they said that they expected to obtain more years of education relative to the control group.

Results

Education

The outcome variable that is most likely to be affected by the disruption in high school from a teenage pregnancy is education itself. This is the first-order effect. If there is no estimated effect on education, we would not expect to see an effect on other outcomes. Table 2 displays the results from estimating Eq. 1 for four measures of education. As expected, there are fairly large and statistically significant differences in both the probability of receiving a high school diploma, the probability of having some college education, and the total amount of completed education between the treatment and control groups. The results for a college degree are mixed.

Row 1 shows the effect on an indicator variable equal to one if the mother has a high school degree. The teenage mothers who have their child prior to their expected graduation date are 5.4 percentage points less likely to have finished high school than the control group, a 7 % decrease relative to the mean for the teenage mothers in our sample. The mean is displayed below the standard error for each coefficient. The effect is largest for white mothers and smallest for Hispanic mothers. Treated white mothers are 6.3 percentage points less likely to complete high school, while black mothers are 4.0 percentage points less likely and Hispanic mothers are 3.6 percentage points less likely.

The second row shows the probability of completing some college education. Some college education is 0.9 percentage points lower for treated mothers, relative to the mean. Even though the timing of our variation is based on high school graduation date, the effect on college attendance is consistent with the expected effect of educational disruption. Most colleges require a high school diploma or GED for enrollment. Even if the teenage moms that miss their graduation date get a GED at a later date, they are less likely to have it in time for college enrollment than their counterparts that were able to finish the last several months of high school. The inconsistent results on college graduation imply that even those teenage moms who have their children after their expected high school graduation date struggle to complete college, which is consistent with Stange's (2011) findings and the pattern observed in Fig. 1. For college completion, those who give birth during the following summer are at least as affected as those who give birth during high school because both have a child throughout their college career.

The last row of Table 2 displays the results for the mothers' total years of education. Column 1 shows that, on average, the treated teenagers completed 0.136 fewer years of education than the control group. Columns 2–4 show the results stratified by race. The magnitude, when measured as a percentage of the group-specific mean, is largest for Hispanic mothers and smallest for black mothers for this measure of completed education.

Family and Labor Market Outcomes

We show that an interruption in high school due to childbirth decreases the probability of graduating from high school by 5.4 percentage points, decreases the probability of attending college by 0.9 percentage points, and decreases total years of education by 0.137 years in Table 2. Tables 3, 4, and 5 show the effects of this interruption on later-life outcomes.

Table 3 Effect of teen fertility on family structure

	All	White	Black	Hispanic
Married	–0.0105***	–0.00705***	–0.0174**	–0.0118***
	(0.00170)	(0.00196)	(0.00542)	(0.00238)
	0.644	0.734	0.325	0.625
Number of Children	0.0624***	0.0449***	0.0788***	0.0914***
	(0.00383)	(0.00427)	(0.0101)	(0.00972)
	2.398	2.302	2.517	2.617
Age Gap	0.0949***	0.128***	0.00273	0.0270
	(0.0101)	(0.0121)	(0.0283)	(0.0206)
	3.361	3.362	3.334	3.388
Number of Observations	1,648,600	1,217,700	160,600	198,100

Notes: Each cell contains the coefficient β_1 from a separate estimation of the following equation: $Outcome_{isarc} = \alpha + \beta_1 Treat_{isarc} \times Teen_{isarc} + \beta_2 Treat_{isarc} + \beta_3 Teen_{isarc} + \Phi_s + \Phi_a + \Phi_r + \Phi_c + \varepsilon_{isarc}$ on data from the 1980 census, 2000 census, and 2005–2014 ACS. The variable listed in the first column is the outcome variable. Mean values of each outcome variable for the treated group are listed below the coefficient and standard errors. Standard errors are shown in parentheses and are clustered by state.

$^{**}p < .01$; $^{***}p < .001$

Table 4 Effect of teen fertility on labor market outcomes

	All	White	Black	Hispanic
Working	–0.00676***	–0.00479*	–0.00481	–0.00747
	(0.00178)	(0.00217)	(0.00506)	(0.00411)
	0.545	0.548	0.588	0.489
Log Wage	–0.000526	0.00173	–0.00468	0.00178
	(0.00400)	(0.00497)	(0.0102)	(0.0134)
	9.2	9.16	9.31	9.26
Log Total Family Income	–0.0158***	–0.00622	–0.0492***	–0.0212**
	(0.00284)	(0.00325)	(0.0108)	(0.00779)
	10.27	10.4	9.872	10.15
Number of Observations	1,648,600	1,217,700	160,600	198,100

Notes: Each cell contains the coefficient β_1 from a separate estimation of the following equation: $\text{Outcome}_{isarc} = \alpha + \beta_1 Treat_{isarc} \times Teen_{isarc} + \beta_2 Treat_{isarc} + \beta_3 Teen_{isarc} + \Phi_s + \Phi_a + \Phi_r + \Phi_c + \varepsilon_{isarc}$ on data from the 1980 census, 2000 census, and 2005–2014 ACS. The variable listed in the first column is the outcome variable. Mean values of each outcome variable for the treated group are listed below the coefficient and standard errors. Standard errors are shown in parentheses and are clustered by state.

*$p < .05$; **$p < .01$; ***$p < .001$

The biggest long-term effect of high school disruption due to teen fertility is on family structure. Row 1 of Table 3 shows that the treated teenagers are less likely to be married, and row 2 shows that they have more children, on average, than the control group. These effects show up for all groups. White women in the treated group also wait longer to have their second child than the control group, although this effect is smaller and not statistically significant for black and Hispanic women. It is hard to explain why the birth of a child during high school—rather than just after high school graduation—would result in a lower likelihood of marriage and more children, given that this is just a small change in first-birth timing. However, Lang and Weinstein (2015) found that teenagers who are unmarried at the time of conception get married younger, on average, or not at all. If teenagers who give birth during high school are less likely to get married right away than those who give birth just after high school, it might push more of them into the "not at all" category.

Next, we look at whether these large differences in educational attainment translate into differences in the labor market. Row 1 of Table 4 shows that the treated mothers are less likely to be working, but the magnitude is small, and the coefficients are not statistically significant for black or Hispanic mothers. Despite strong estimated effects of the timing of birth on completed education, row 2 shows no measured difference in mother's wage income between treatment and control groups.[9] The estimated coefficient is statistically indistinguishable from 0, and we can reject a wage loss of more than 0.8 % at a 95 % confidence level. Although the difference in educational attainment could be partly attributable to the fact that the treated group appears to be slightly less-advantaged according to the NSFG analysis, we would expect this to also bias the wage coefficients. The fact that even despite this potential downward bias, the coefficients are precisely estimated 0s,

[9] The dependent variable in these regressions is log(wage), so women with zero earnings are excluded from the regression. When wage in dollars is used instead, and women with zero earnings are included, the story remains the same. The coefficients are small and statistically indistinguishable from 0. Given the cross-sectional nature of the wage estimates, we cannot use a longer-term measure of earnings. The short-term measure we use might measure permanent income with more error, possibly attenuating the estimates.

Table 5 Effect of teen fertility on poverty

	All	White	Black	Hispanic
Below 100 % of Federal Poverty Line	0.00989***	0.00508**	0.0203***	0.0130***
	(0.00119)	(0.00162)	(0.00337)	(0.00283)
	0.236	0.173	0.397	0.316
Below 200 % of Federal Poverty Line	0.0151***	0.0125***	0.0253***	0.0144**
	(0.00151)	(0.00196)	(0.00487)	(0.00469)
	0.5246	0.4533	0.6877	0.6374
Number of Observations	1,648,600	1,217,700	160,600	198,100

Notes: Each cell contains the coefficient β_1 from a separate estimation of the following equation: $Outcome_{isarc} = \alpha + \beta_1 Treat_{isarc} \times Teen_{isarc} + \beta_2 Treat_{isarc} + \beta_3 Teen_{isarc} + \Phi_s + \Phi_a + \Phi_r + \Phi_c + \varepsilon_{isarc}$ on data from the 1980 census, 2000 census, and 2005–2014 ACS. The variable listed in the first column is the outcome variable. Mean values of each outcome variable for the treated group are listed below the coefficient and standard errors. Standard errors are shown in parentheses and are clustered by state.

*$p < .05$; **$p < .01$; ***$p < .001$

suggests that receiving a high school degree does not have any positive effect on wage income for this population. Row 3 shows that despite the lack of difference in mother's wage income, total family income is lower for treated mothers, and this difference is large and statistically significant for black and Hispanic mothers. Total family income combines the income of all family members—in particular, the spouse—which implies either lower spousal earnings for the treated group or reflects the differences in the probability of marriage observed in Table 3. The combination of lower family income levels, but a higher number of children, means that the treated group is less able to meet their family's needs.

Table 5 shows that the treated teenagers are significantly more likely to fall below 100 % and 200 % of the FPL. On average, they are 1 percentage point more likely to fall below 100 % of the FPL and 1.5 percentage points more likely to fall below 200 % of the FPL.

Differences Over Time and Over the Life Cycle

High school graduation rates have been increasing over the last several decades. The women surveyed in the 1980 census had their children between 1963 and 1978. The most recent sample, from the ACS, includes mothers surveyed in 2014, who could have had their children as recently as 2012. These young women faced very different environments and opportunities. We include fixed effects for the age of the women and the survey year to capture this heterogeneity in the main results. In Table 6, we explore some of that heterogeneity. We find that both high school graduation and years of education effects are strongest in the 1980 census. For example, for those surveyed in the 1980 census, the treatment group is 9.6 percentage points less likely to have earned a high school degree, and that number falls to 2.6 percentage points for those surveyed in the 2000 census. The magnitude of the coefficient for high school graduation falls even further between the 2000 census and the ACS, but years of education remains stable over the more recent surveys.

Of note, though, is that Fig. 1 includes only the 2000 census and ACS samples, where the education effects are the smallest. If we were able to produce the same graphs for the 1980 census, where the effect is larger, we may have observed a discontinuous change in high school graduation. The labor market and family structure outcomes look fairly similar across survey years.

Table 6 Effect of teen fertility by survey

	All	1980 Census	2000 Census	ACS
High School Education	–0.0536***	–0.0953***	–0.0261***	–0.0139***
	(0.00236)	(0.00388)	(0.00196)	(0.00227)
	0.734	0.632	0.78	0.86
GED				0.00475**
				(0.00152)
				0.0523
Some College	–0.00919***	–0.0145***	–0.0115**	–0.00292
	(0.00148)	(0.00158)	(0.00381)	(0.00226)
	0.194	0.111	0.299	0.249
College Graduate	0.00122	0.0122***	–0.00775***	–0.00745***
	(0.000913)	(0.00139)	(0.00215)	(0.00175)
	0.0274	0.0172	0.0371	0.0362
Years of Education	–0.136***	–0.166***	–0.0995***	–0.105***
	(0.00824)	(0.0108)	(0.0114)	(0.0242)
	11.75	11.42	11.91	12.38
Working	–0.00676***	–0.00935***	–0.00268	–0.00473
	(0.00178)	(0.00237)	(0.00332)	(0.00327)
	0.545	0.484	0.608	0.597
Log Wage	–0.000526	0.00668	–0.00567	–0.00541
	(0.00400)	(0.00663)	(0.00744)	(0.00753)
	9.2	9.01	9.27	9.41
Log Total Family Income	–0.0158***	–0.00793	–0.0228**	–0.0167**
	(0.00284)	(0.00426)	(0.00690)	(0.00527)
	10.27	10.32	10.14	10.28
Married	–0.0105***	–0.00827***	–0.0150***	–0.0107**
	(0.00170)	(0.00175)	(0.00360)	(0.00350)
	0.644	0.754	0.595	0.509
Number of Children	0.0624***	0.0686***	0.0500***	0.0624***
	(0.00383)	(0.00357)	(0.00962)	(0.00701)
	2.398	2.391	2.331	2.456
Age Gap	0.0949***	0.0858***	0.121***	0.0633**
	(0.0101)	(0.0130)	(0.0206)	(0.0183)
	3.361	3.1	3.45	3.713
Below 100 % of Federal Poverty Line	0.00989***	0.00957***	0.0116***	0.00747**
	(0.00119)	(0.00205)	(0.00269)	(0.00265)
	0.236	0.201	0.273	0.26
Below 200 % of Federal Poverty Line	0.0151***	0.0151***	0.0147***	0.0133***
	(0.00151)	(0.00244)	(0.00362)	(0.00343)
	0.5246	0.4893	0.5939	0.5269
Number of Observations	1,648,600	729,500	398,900	520,200

Notes: Each cell contains the coefficient β_1 from a separate estimation of the following equation: $Outcome_{isar} = \alpha + \beta_1 Treat_{isar}\, Teen_{isar} + \beta_2 Treat_{isar} + \beta_3 Teen_{isar} + \Phi_s + \Phi_a + \Phi_r + \varepsilon_{isar}$ on data from the 1980 census, 2000 census, and 2005–2014 ACS. The variable listed in the first column is the outcome variable. Mean values of each outcome variable are listed below the coefficient and standard errors. Standard errors are shown in parentheses and are clustered by state.

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

The high school graduation rate here includes recipients of a GED certification. Some discussion in the literature (Jaeger and Page 1996) debates whether having a GED is equivalent to a high school diploma. The 1980 and 2000 census data do not distinguish the two paths to completing the high school credential, but the ACS separates them. Table 6 shows that the treated mothers were more likely to earn a GED, rather than a high school diploma, as expected. This implies that if we were able to separate GED recipients from high school graduates in the main results, the effect of the disruption from having a child prior to high school graduation would be higher than the estimated 5.4 percentage points.

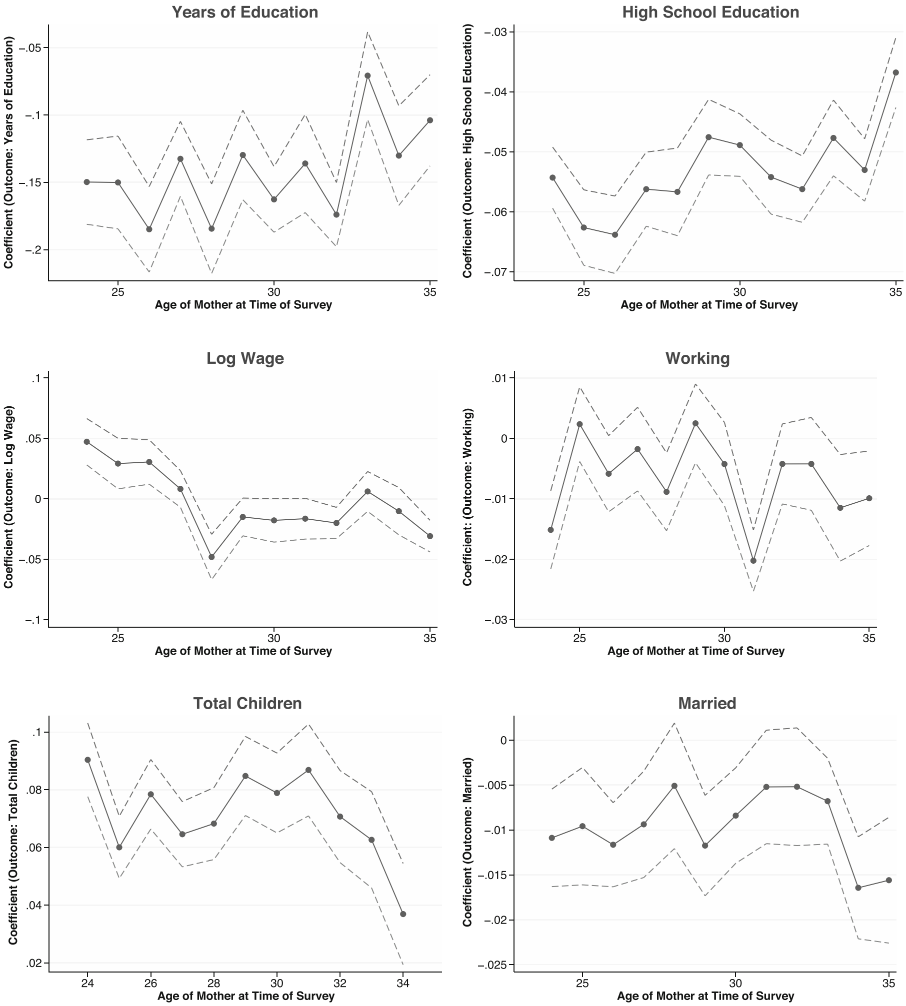

Fig. 3 Changes over the life cycle. Figures compiled from the 1980 census, 2000 census, and 2005–2014 ACS. Each data point represents the coefficient β_1 from a separate estimation of the following equation: $Outcome_{is} = \alpha + \beta_1 Treat_{is} \times Teen_{is} + \beta_2 Treat_{is} + \beta_3 Teen_{is} + \Phi_s + \Phi_r + \varepsilon_{is}$. The regression is estimated separately for each year of mothers' age. The dotted lines give the 95 % confidence interval. It is important to note that these graphs also show differences between cohorts. For example, mothers who are aged 35 at survey were (mostly) born in 1945 (1980 – 35), 1965 (2000 – 35) and 1970–1979 (ACS year – 35).

Figure 3 shows the coefficients estimated separately by the age of the mother at survey date. These figures are meant to show the effect of teen fertility over the life cycle, although they could also partly reflect differences over time in the penalty for giving birth during high school. A 35-year-old mother, for example, was 35 in 1980, 2000, or 2005–2014; a 25-year-old mother was 25 in those same years. Thus, those who are age 35 as of the survey date were born, on average, 10 years earlier than those who were age 25 at the survey date. Nevertheless, the graphs provide insight on potential life cycle patterns. Most notably, the negative effect on high school graduation becomes smaller in magnitude over time (the coefficient trends toward 0), perhaps reflecting the fact that there is likely some catching-up in the form of GED receipt. In addition, we see a small but positive effect on log wage in the early to mid-20s, which declines and then levels out at 0 around age 29. This would be consistent with a motherhood earnings penalty (Chung et al. 2017; Lu et al. 2017; Staff and Mortimer 2012) affecting the treated group less than the control group that dissipates as the children reach preschool/kindergarten age. The family structure variables do not appear to have any strong patterns.

Robustness Checks and Discussion

In the earlier section, Validity of the Identification Assumption, we argue that women in the treatment and control groups are very similar before becoming mothers, particularly after we control for seasonality with the older cohort. However, even though we have chosen a fairly tight band around the end of high school—approximately six months on either side—the data allow us to test an even tighter band, which should help to alleviate concerns that the control group is not a good comparison. Column 1 of Table 7 shows the results of running the same regressions as the main specification but for the new treatment and control groups. The treatment group includes only those mothers who gave birth during March–June of their senior years, and the control group includes only those mothers who gave birth during July–September following their senior years. For the older cohort, mothers who gave birth during the second quarter are classified as treated, and those who gave birth during the third quarter are classified as control. The coefficients in this column are sometimes smaller, especially for the educational outcomes, as would be expected from Fig. 1, but maintain the same patterns as the main results.

We also check the importance of the decision to use women who gave birth at ages 23–25 as our older control sample rather than another age group. We choose ages 23–25 in an attempt to use relatively young mothers but ones who are far less likely to experience an interruption in school (including college) due to their pregnancy. Column 3, Table 7 shows the results using a sample of women who became mothers at ages 20–22. This group is much closer in age to the teenage mothers. The coefficients in these columns look very similar to the main results.

The analysis in the earlier section, Validity of the Identification Assumption, suggests that the treatment and control groups are largely comparable. For example, the NLSY analysis, which allows us to test for differences in a wide range of confounding variables, reveals that the treatment group looks more advantaged along some dimensions and looks less advantaged along others; and none of the coefficients are statistically significant. On the other hand, even though the NSFG analysis also results in a set of statistically insignificant coefficients, the signs of the coefficients suggest that the

Table 7 Robustness checks

	Main	Small Window	Young Control	Propensity Score Matching
High School Graduate	–0.0536***	–0.0338***	–0.0456***	–0.0555***
	(0.00236)	(0.00251)	(0.00233)	(0.00228)
	0.734	0.7267	0.7335	0.712
Years of Education	–0.136***	–0.0695***	–0.118***	–0.163***
	(0.00824)	(0.0103)	(0.00757)	(0.00847)
	11.75	11.7	11.8	11.7
College Graduate	0.00122	0.00517***	–0.000817	–0.00234**
	(0.000913)	(0.00124)	(0.000631)	(0.000874)
	0.0274	0.0275	0.0274	0.0263
Working	–0.00676***	–0.00662**	–0.00236	–0.00295
	(0.00178)	(0.00203)	(0.00184)	(0.00181)
	0.545	0.547	0.545	0.541
Log Wage	–0.000526	0.00241	0.00294	0.0102*
	(0.004)	(0.00662)	(0.00341)	(0.00448)
	9.2	9.21	9.2	9.19
Log Total Family Income	–0.0158***	–0.0121**	–0.0120***	–0.0131***
	(0.00284)	(0.00411)	(0.00324)	(0.00333)
	10.27	10.26	10.27	10.2
Married	–0.0105***	–0.00748***	–0.00755***	–0.00865***
	(0.0017)	(0.00208)	(0.00138)	(0.00184)
	0.644	0.638	0.644	0.631
Number of Children	0.0624***	0.0254***	0.0451***	0.0609***
	(0.00383)	(0.00553)	(0.00407)	(0.00371)
	2.398	2.413	2.398	2.29
Age Gap	0.0949***	0.0420**	0.109***	0.0812***
	(0.0101)	(0.0126)	(0.0102)	(0.00962)
	3.361	3.217	3.361	3.35
Below 100 % of FPL	0.00989***	0.00604**	0.00696***	0.00627***
	(0.00119)	(0.00184)	(0.00125)	(0.00127)
	0.236	0.239	0.236	0.245
Below 200 % of FPL	0.0151***	0.00886**	0.0122***	0.0133***
	(0.00151)	(0.00256)	(0.00173)	(0.00160)
	0.5246	0.5295	0.5246	0.537
Number of Observations	1,648,600	836,000	2,170,700	1,648,600

Notes: Each cell contains the coefficient β_1 from a separate estimation of the following equation: $Outcome_{isa} = \alpha + \beta_1 Treat_{isa}\, Teen_{isa} + \beta_2 Treat_{isa} + \beta_3 Teen_{isa} + \Phi_s + \Phi_a + \Phi_r + \varepsilon_{isa}$ on data from the 1980 census, 2000 census, and 2005–2014 ACS. The first column contains the main results from Tables 2–5. The second column shows the results for a subsample of births within a three-month window of the expected graduation date. The third column uses an age 20–22 control group. The forth column shows results using propensity score match weights.

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

treatment group might be less advantaged. To address potential observed and unobserved differences in our treatment and control group, we estimate a propensity score match between the two groups. The propensity score is estimated on age, black, Hispanic, state fixed effects, and survey year. We match using a normal distribution kernel density, and then we use the weights from that match to estimate a weighted least squares regression, which is otherwise identical to our main estimating equation. The results of this propensity score estimate are reported in column 4 of Table 7. The results from the propensity score match are consistent with our main estimates. Although the results are not displayed in Table 7, we also use a logistic regression, instead of linear probability model, for the outcome variables that are categorical variables. The coefficients remain consistent in sign and significance.[10]

Finally, we check the pattern of the estimates around the change in abortion availability with the *Roe v. Wade* decision in 1973. Figure A3 in the online appendix shows the life cycle graphs from the earlier section, Differences Over Time and Over the Life Cycle, with only the observations from the 1980 census. This cohort overlapped with the *Roe v. Wade* decision, which is indicated on the graphs with the vertical line. Women who were younger when they were surveyed in 1980 turned 18 after the *Roe v. Wade* decision, and the women who were older in 1980 turned 18 before the decision. If selective abortion were driving our results, we would expect to see strong effects to the left of the vertical line and no effects to the right. Although the coefficients bounce around, our results do not appear to be driven strongly by the availability of abortions. If anything, the results are stronger in the earlier period, when there were more teenage pregnancies, and we would expect less positive selection into the control group.

Conclusions

Lowering teenage pregnancy rates is one of the top priorities for public health officials in the United States. Its correlation with poor economic outcomes for both teenage mothers and their children makes it an easy target. However, given the disadvantaged backgrounds of teenage mothers when compared with women who delay childbearing, it is difficult to establish causality. The most convincing previous literature has found that the true causal effect is much lower than correlations would suggest, even when those correlations control for observable measures of family background.

Our study finds that having a child during high school significantly affects some later-life outcomes of young mothers. We find that having a child during the last six months of high school causes a 5.4 percentage point decrease in the probability of obtaining a high school degree compared with women who had a child just after the end of high school. It also decreases the probability of marriage and increases the average number of children. However, the disruption to education does not have any measurable effect on earnings. This suggests that the signaling value of a high school degree is not important for this group of young mothers and that simply helping a teenage mother finish high school will not help improve her earnings potential. Because we find that the timing of births matters for family structure, the families of women

[10] Results are available upon request.

who have a child during the last six months of high school are significantly more likely to be living in poverty.

This builds on a larger national discussion on poverty and inequality. How do we improve the outcomes of individuals from poor socioeconomic backgrounds? How do we reduce the cycle of poverty? Fortunately, teen pregnancy has been declining for the last several decades (Kearney and Levine 2015), making the disruption of schooling due to teen fertility much less common. Also, the results show that the treatment effects have declined during the more recent samples compared with the earlier sample, which may be a result of policies such as Title IX, which requires schools to provide equal access to education for girls, even if they are pregnant or have children (Guldi 2016). Our study suggests that reducing teenage pregnancy and increasing high school completion may not be enough to increase earnings of teenage mothers but may have positive effects on other measures of later-life outcomes, such as total family income, poverty, and marriage rates.

Acknowledgments We thank Marianne Page, Hilary Hoynes, Scott Carrell, Jeremy Moulton, Jason Lindo, Martha Stinson, Marie Hull, Julie Cullen, Amanda Gaulke, four anonymous referees, and seminar participants at UC Davis, The Vienna Institute of Demography Colloquium, The University of Kentucky, the Southern Economic Association, South Carolina Applied Micro Day, and the Society of Labor Economists annual meetings for their helpful comments and suggestions. Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed.

References

Abrahamse, A. F., Morrison, P. A., & Waite, L. J. (1988). *Beyond stereotypes: Who becomes a single teenage mother?* (Report). Santa Monica, CA: The Rand Corporation.

Ashcraft, A., Fernandez-Val, I., & Lang, K. (2013). The consequences of teenage childbearing: Consistent estimates when abortion makes miscarriage non-random. *Economic Journal, 123,* 875–905.

Buckles, K. S., & Hungerman, D. M. (2013). Season of birth and later outcomes: Old questions, new answers. *Review of Economics and Statistics, 95,* 711–724.

Card, J. J., & Wise, L. L. (1978). Teenage mothers and teenage fathers: The impact of early childbearing on the parents' personal and professional lives. *Family Planning Perspectives, 10,* 199–205.

Chung, Y., Downs, B., Sandler, D. H., & Sienkiewicz, R. (2017). *The parental gender earnings gap in the United States* (CES Working Paper No. 17-68). Washington, DC: U.S. Census Bureau, Center for Economic Studies.

Clark, D., & Martorell, P. (2014). The signaling value of a high school diploma. *Journal of Political Economy, 122,* 282–318.

Ellwood, D. (1989). *Poor support*. New York, NY: Basic Books.

Geronimus, A. T., & Korenman, S. (1992). The socioeconomic consequences of teen childbearing reconsidered. *Quarterly Journal of Economics, 107,* 1187–1214.

Guldi, M. (2016). Title IX and the education of teen mothers. *Economics of Education Review, 55,* 103–116.

Hayes, C. D. (Ed.). (1987). *Risking the future: Adolescent sexuality, pregnancy, and childbearing* (Vol. I). Washington, DC: National Academy Press.

Hotz, V. J., McElroy, S. W., & Sanders, S. G. (2005). Teenage childbearing and its life cycle consequences: Exploiting a natural experiment. *Journal of Human Resources, 40,* 683–715.

Jaeger, D. A., & Page, M. E. (1996). Degrees matter: New evidence on sheepskin effects in the returns to education. *Review of Economics and Statistics, 78,* 733–740.

Kearney, M. S., & Levine, P. B. (2015). Investigating recent trends in the U.S. teen birth rate. *Journal of Health Economics, 41,* 15–29.

Kuka, E., Shenhav, N., & Shih K. (2018). *Do human capital decisions respond to the returns to education? Evidence from DACA* (NBER Working Paper No. 24315). Cambridge, MA: National Bureau of Economic Research.

Lang, K., & Weinstein, R. (2015). The consequences of teenage childbearing before Roe v. Wade. *American Economic Journal: Applied Economics, 7*(4), 169–197.

Lu, Y., Wang, J. S.-H., & Han, W.-J. (2017). Women's short-term employment trajectories following birth: Patterns, determinants, and variations by race/ethnicity and nativity. *Demography, 54,* 93–118.

Olsen, R. J., & Farkas, G. (1989). Endogenous covariates in duration models and the effect of adolescent childbirth on schooling. *Journal of Human Resources, 24,* 39–53.

Staff, J., & Mortimer, J. T. (2012). Explaining the motherhood wage penalty during the early occupational career. *Demography, 49,* 1–21.

Stange, K. (2011). A longitudinal analysis of the relationship between fertility timing and schooling. *Demography, 48,* 931–956.

Trussell, J. (1988). Teenage pregnancy in the United States. *Family Planning Perspectives, 20,* 262–272.

Tyler, J. H., Murnane, R. J., & Willett, J. B. (2000). Estimating the labor market signaling value of the GED. *Quarterly Journal of Economics, 115,* 431–468.

Upchurch, D. M., & McCarthy, J. (1990). The timing of a first birth and high school completion. *American Sociological Review, 55,* 224–234.

U.S. Census Bureau. (1980). *Decennial census 1980* [Data file]. Retrieved from https://www.census.gov/fsrdc

U.S. Census Bureau. (2000). *Decennial census 2000* [Data file]. Retrieved from https://www.census.gov/fsrdc

U.S. Census Bureau. (2005–2014). *American Community Survey* 2005–2014 [Data file]. Retrieved from https://www.census.gov/fsrdc