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**Teenage Childbearing and Its Life Cycle Consequences: Exploiting a Natural Experiment***

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**Teenage Childbearing and Its Life Cycle Consequences:**
**Exploiting a Natural Experiment**


## Abstract

We exploit a "natural experiment" associated with human reproduction to identify the causal effect of teen childbearing on the socioeconomic attainment of teen mothers. We exploit the fact that some women who become pregnant experience a miscarriage and do not have a live birth. Using miscarriages an instrumental variable, we estimate the effect of teen mothers not delaying their childbearing on their subsequent attainment. We find that many of the negative consequences of teenage childbearing are much smaller than those found in previous studies. For most outcomes, the adverse consequences of early childbearing are short-lived. Finally, for annual hours of work and earnings, we find that a teen mother would have lower levels of each at older ages if they had delayed their childbearing.



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## I. Introduction

Over the past several decades, social scientists have documented a strong statistical association between the age at which a woman has her first child and economic and social indicators of her subsequent well-being. Most of these studies find that women who bear children as teenagers are subsequently less likely to complete high school, less likely to participate in the labor force, more likely to have low earnings, and less likely to marry than are women who do not have children as teenagers. Furthermore, adolescent mothers, and their children, are likely to spend a substantial fraction of their lifetimes in poverty and are more likely to rely on government support. (See Upchurch and McCarthy 1990 and Card 1981).

The important question is whether these statistical associations reflect a causal effect of early childbearing on the subsequent economic and demographic outcomes of teen mothers. It is possible that these associations simply reflect differences between the type of women who bear children as teens and those who avoid it. For example, teenage mothers typically were raised in families that were especially disadvantaged based on a number of measurable indicators. Because teenage mothers tend to be from families of lower socioeconomic status, and socioeconomic status of parents and children appear to be themselves correlated, it is difficult to assess whether a teen birth is responsible for the poorer outcomes of teenage mothers or whether a teen mother's subsequent outcomes are attributable to the socioeconomic conditions in which she was reared.

The key issue in attempts to estimate the causal effect is how to estimate reliably the counterfactual state to the observed outcomes of teen mothers: namely, what would have been the adolescent mother's outcomes if she had not had a child as a teen? Varieties of econometric strategies have been used to estimate this counterfactual outcome. The most common approach is to control for observable factors, typically using regression methods, that account for the lower economic status of teenage mothers when they were growing up and to attribute any differences in outcomes between teenage mothers and other women, net of these observables, to the causal effects of teenage childbearing. (See Waite and Moore 1978, Card and Wise 1978, Hofferth and Moore 1979, Upchurch and McCarthy 1990, Marini 1984, and McElroy 1996a, 1996b as examples of this strategy.) The validity of this approach requires that, conditional on these observable factors or covariates, a woman's status (teen mother or not a teen mother) be uncorrelated with all remaining and unobservable factors that might influence the outcomes under consideration. Clearly, this condition is strong and, as we show below, its validity is dubious for a variety of reasons.

A second econometric methodology uses the outcomes of an adolescent mother's sisters who did not have a child as a teenage to construct counterfactual outcomes for teen mothers. (See Geronimus and Korenman 1992, 1993 and Hoffman, Foster, and Furstenberg 1993). Comparing the outcomes of a teenage mother to her sister who did not have a child as a teen has the intuitive appeal of controlling for a variety of pre-teen characteristics and factors, both observed and unobserved, that were common to the environments—family, socioeconomic, and otherwise—in which these two women were reared. The challenge to the validity of this sibling-differences or sister-differences approach is that the socioeconomic conditions facing sisters and the parental inputs received by sisters may differ if family circumstances change over time and with the childrearing experiences of their parents.[1]

A third econometric approach attempts to model explicitly the joint process determining the woman's decision to bear a child as a teenager as well as the maternal outcome of interest,

[1] Hao, Hotz, and Jin (2004) develop a game-theoretic model of parental-daughter interactions over teenage childbearing decisions in which parents differentially treat older versus younger daughters to reduce the incidence of teen births. Their empirical tests reject the results that births are random across daughters within the same family.

such as, education, labor supply, or poverty status. (See Ribar 1992, 1994, and Lundberg and Plotnick 1989 as examples of this strategy.) Such studies typically rely on rational choice models that hypothesize that women with lower returns to work and education are the ones that have children as teens. Such models maintain an equally strong, albeit different, set of assumptions in order to identify the effects of early childbearing: namely, that the model of these behavioral decision processes adequately characterizes both the teenage childbearing and outcomes decisions and how they interact.

Finally, the work of Grogger and Bronars (1993) provides a fourth approach that makes use of a "naturally-occurring" experiment to estimate causal effects of early childbearing.[2] In particular, Grogger and Bronars make use of the fact that some teenage mothers have twins at their first birth rather than a single child. Since the occurrence of twins from a typical conception can be viewed, by and large, as random, it as if the "extra" child was randomly assigned. These authors compare the outcomes of teen mothers whose first birth is twins with those whose first birth is a single child to estimate a causal effect of this extra child. While an innovative approach, the Grogger-Bronars "twins" method estimates a causal effect that is different from the one that is the subject of much of the literature and, as will be made clear below, is also different from the one considered in this article. Most of the previous literature on the (causal) effects of teenage childbearing seeks to estimate the effect of having at least one child as a teenager relative to having no children as a teenager. In contrast, the Grogger and Bronars study identifies the marginal effect of having two children as a teen compared to having one child. Grogger and Bronars recognize this potentially important difference in their work. They argue that if having one more child lowers the outcomes of teen mothers, the effect of having only one child as a teen

[2] Bronars and Grogger (1995) use this same twins strategy to identify the causal effect of women having an extra out-of-wedlock birth on the socioeconomic attainment of such mothers.

is likely to be at least as large.

In this article, we exploit an alternative "natural experiment" associated with human reproduction to measure counterfactual outcomes: namely, what would have happened to a teen mother if she had not had her first birth as a teen? In particular, we exploit the fact that some women who become pregnant as teenagers experience a miscarriage (spontaneous abortion) and thus do not have a birth.[3] The physiology of human reproduction implies that some miscarriages occur at random resulting from the formation of abnormal fetal chromosomes at the time of conception, which causes fetal expulsion early in a pregnancy.[4] Since miscarriages are close to random, we argue for using miscarriages as an instrument in order to obtain unbiased estimates of the causal effects of teenage childbearing on indicators of women's subsequent socioeconomic attainment and maternal outcomes.

In the next section, we describe the data from the National Longitudinal Survey of Youth, 1979 (NLSY79) that we use in this study. In Section III, we lay out our use of miscarriages as a natural experiment and show how miscarriages can be used to form an instrumental variables (IV) estimator for the effect of teen births on maternal outcomes. Therein, we also consider the threats to our inferences due to the possibility that some miscarriages are not random and that fertility events, especially miscarriages and induced abortions, are likely to be underreported in survey data. We discuss how we address these complications and report relevant findings from our previous work. In Section IV, we present our basic findings of the effect of teenage childbearing on a wide variety of subsequent economic and demographic outcomes, including educational attainment, subsequent fertility and marriage rates, labor market success, personal and

[3] Using a testing strategy for assessing the validity of instruments, developed in Hotz, Mullin and Sanders 1997, we show that one cannot reject the validity of miscarriages as an instrument.

[4] Kline, Stein and Susser (1989).

spousal income, the incidence of living in poverty, the likelihood of receiving various forms of public assistance, and the dollar amounts of the benefits from these programs.

Our major finding is that many of the apparent negative consequences of teenage childbearing on the subsequent socioeconomic attainment of teen mothers are much smaller than those found in studies that use alternative methodologies to identify the causal effects of teenage childbearing. We also find evidence that teenage mothers earn more in the labor market at older ages than they would have earned if they had delayed their births. Comparing our IV estimates with estimates based on ordinary least squares (OLS) regression methods that control for observable characteristics, we find that the apparent negative consequences previously attributed to teenage childbearing appear to be the result of the failure to account for other, unobservable factors. In Section V, we offer some concluding comments on our analysis.

## II. Data and Samples Used

In this study, we use data from the National Longitudinal Survey of Youth (NLSY79) to estimate the causal effects of teenage childbearing in the United States. The NLSY79 is a nationally representative sample of young men and women who were between the ages of 14 and 21 years old as of January 1, 1979. Thus, the women in our study were teenagers (ages 13 to17) during the years 1971 and 1982.

Respondents have been interviewed annually in the years 1979 through 1992, the last year used in our analysis. The female respondents were asked a range of questions about all of their pregnancies and births, as well as about their marital arrangements, educational attainment, labor force experiences, family income, and participation in various welfare programs.

The NLSY79 contains a cross-sectional sample designed to be representative of women in the non-institutionalized civilian population in the U.S. for the above-noted birth cohorts, as well as supplemental samples of blacks, Hispanics, disadvantaged whites, and women who were enlisted in the military in 1979. As is common in many analyses using the NLSY79, we eliminate the economically disadvantaged white supplementary and military samples from our analysis.[5] We refer to this sample, those in the random sample and the black and Hispanic supplemental samples, as the *All Women* sample. This sample contains 4,926 women, of which 3,108 were from the random sample, and 1,067 and 751 women, were in the black and Hispanic supplemental samples, respectively.

In Table 1, we provide summary statistics on background characteristics—most of which are measured when these women were age 14[6]—for the All Women sample. These statistics are calculated using weighted data—as are all of the estimates presented in the remainder of this article—where we use base-year (1979) weights to account for the original design of the sample drawn in this study and the differential probabilities of completing the base-year interviews.[7] We divide the sample into teen mothers and women who did not have births as a teenager. Teen mothers came from much more disadvantaged backgrounds than did women who did not have

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[5] We eliminated the military sample from our analysis because the vast majority of the women in this subsample were not interviewed after 1984, which means that we are not able to measure the outcomes of these women at older ages. We eliminated the economically disadvantaged white women because of serious concerns that the criteria used to select women into this supplemental sample—namely whether their household income (which was not necessarily their parent's income) in 1978 fell below the poverty level—is not a very reliable way of identifying a representative sample of disadvantaged (white) women. Furthermore, the women in this supplementary sample were not interviewed after 1990.

[6] The two exceptions to this in Table 1 are the annual income of the household in which a woman resided, which was taken in 1978, and the woman's score on the Armed Forces Qualifying Test (AFQT), which was administered to all women in the NLSY79 in 1981.

[7] We wish to thank Jay Zagorsky of the Center for Human Resource Research at the Ohio State University for providing us with the appropriate base-year weights for our particular combination of the cross-sectional and supplemental samples.(The appropriate set of weights for this combination of subsamples are not available in the public-release versions of the NLSY79.) We note that the NLSY79 also provides yearly updated weights to take account of non-response at each interview using a set of post-stratification adjustment procedures described in Frankel, McWilliams and Spencer (1983). In an extensive evaluation of the NLSY79 data, MaCurdy, Mroz and Gritz (1998) find differences in estimating the distributions of labor market earnings and hours of work when using weighted versus unweighted data. However, they also find that it does not matter whether one weights the data with the 1979 base weights or year-by-year versions of these weights that adjust for attrition over the course of the study.

births as teens. For example, teenage mothers grew up in homes that were poorer. The average annual income of the households in which teenage mothers lived in 1978 was $30,532 versus $50,717 (in 1994 dollars) for their non-teen mother counterparts. Teen mothers had parents who were less educated. The fathers of women who later became teenage mothers completed an average of 9.9 years of schooling versus 11.9 years of schooling for the fathers of other women. Teen mothers were more likely to grow up in single-parent families (31 percent versus 16 percent). In addition, they were more likely to have been in a family living on welfare when growing up (19 percent versus 11 percent) than women who did not have a child as a teen. Clearly, teenage mothers were markedly different from women who delayed childbearing into adulthood in many ways we can observe.

We next consider the subsample of women who experienced at least one pregnancy while they were teens, that is, prior to their eighteenth birthday. We refer to this subsample as the *Teen Pregnancy* sample. The Teen Pregnancy sample consists of 1,042 women, of whom 74.7 percent (778) had a pregnancy that ended in a birth[8] and 25.3 percent (264) had a pregnancy that did not end in a birth. Women whose pregnancies did not end in a birth can be further divided into the 192 (18.4 percent) who had an induced abortion and the 72 (6.9 percent) whose pregnancies ended in a miscarriage.[9] Table 2 presents statistics on the same background characteristics displayed in Table 1 for the Teen Pregnancy sample by how their first pregnancy was resolved. Note that while the subsample of women whose first pregnancy before age 18 did *not* end in a birth had background characteristics that were more similar to those of teen mothers than to

[8] The difference between the All Women and Teen Pregnancy samples with respect to the occurrence of births is due to differences in the dating conventions used for birth events. Both samples contain women who had a child prior to their 18$^{th}$ birthday. The Teen Pregnancy sample also includes as teenage mothers women who became pregnant just prior to their 18$^{th}$ birthday who carried the pregnancy to term but the birth occurred after their 18$^{th}$ birthday.

[9] The details of how we constructed the pregnancy and pregnancy resolution variables from the information available in the NLSY79 data are available in a detailed Data Appendix that can be found at www.econ.ucla.edu/~hotz/teen_data.pdf.

characteristics of women who were not teen mothers (see Table 1). As shown in Table 1, these two subgroups (teen mothers and women with first pregnancies prior to age 18 that did not end in a birth) have quite different characteristics. As revealed in columns (3) and (4) in Table 2, (women whose first pregnancies ended in an abortion and miscarriage, respectively), this dissimilarity in background characteristics is due primarily to the women whose first pregnancies were resolved with an abortion. In particular, the abortion group has characteristics that are much more similar to those of women who did not have teen births (see Table 1) than they are to those of teen mothers.[10]

In contrast, the women whose (first) teen pregnancies were resolved via a miscarriage are much more similar to teen mothers than they are to any of the other potential comparison groups displayed in Tables 1 and 2. This similarity in observable characteristics for the two groups is indicative of why the estimates of causal effects of teenage childbearing derived from our natural experiment presented below differ from estimates found in the previous literature. In the next section, we provide a more formal justification for the appropriateness of using the data on women who experience miscarriages as teens when estimating the counterfactual outcomes for teen mothers to identify the causal effect of teenage childbearing on maternal outcomes.

## III. The Use of Miscarriages as a Natural Experiment (and as an Instrumental Variable)

Consider the population of women who first become pregnant as adolescents and, thus, are at risk to become a teen mother. A pregnancy can be resolved in one of three ways: the occurrence of a birth, an induced abortion, or a miscarriage. Let $D$ be the indicator of how the pregnancy is resolved, where $D = B$ (birth), $A$ (abortion), or $M$ (miscarriage). For now, assume that

[10] Cooksey (1990) also documents that teens who abort their pregnancies tend to come from higher socioeconomic backgrounds and/or have higher socioeconomic attainment (e.g., more educated) than are teenage women who carry their pregnancies to term.

miscarriages are beyond the control of women, while the births and abortions represent choices by those who did not experience a miscarriage.[11] Among women who experience miscarriages, we define a woman's *latent type* as the way a woman would *choose* to resolve a pregnancy if she did not experience the miscarriage. Let $D^* = B^*$ if a woman's latent type is to have a birth and $D^* = A^*$ if her latent type is to have an abortion. Finally, let $Y$ denote outcomes women experience as an adult age, that is, at ages greater than 18, and $Y_k$ ($k = B$, $A$, or $M$) denote the outcome conditional on the way in which the pregnancy was resolved, and $Y_{k^*}$ ($k^* = B^*$ or $A^*$) denote the outcome that would occur if a woman had a particular latent pregnancy type.[12] The outcomes associated with a woman's latent type are hypothetical in that the econometrician can not observe a woman's latent type.

We define the causal effect of interest in this article as the average effect of a woman having a birth as a teen versus delaying it—either to an adult age or permanently—on adult outcomes for the population of women whose first birth is as a teen. More precisely, we are interested in identifying and estimating

$$\beta = E(Y_B - Y_{B^*} \mid D = B) \tag{1}$$

Angrist and Imbens (1991) refer to this type of causal effect as the *selected average treatment effect* (SATE), where "selected" refers to the fact that the causal effect applies to a selected population.[13] In our context, the selected population is women who have their first births as a teenager. Because of this selectivity in the population and because we do not presume that pregnancies are random events, we cannot make inferences about the causal effects of early childbearing

[11] We note that most miscarriages occur very early in a pregnancy so that they usually occur before women could choose to have an induced abortion.

[12] To simplify notation, we forego indexing outcomes by particular adult age at which they are measured.

[13] In the evaluation literature (see Heckman, 1992), this effect also is referred to as the effect of the "treatment on the treated."

for a randomly chosen teenage woman in the United States.[14] Nonetheless, identifying the causal effect defined in (1) is of interest for at least two reasons. First, as we will argue below, $\beta$ is more readily identified from available data than is the speculative causal effect of the consequences of a teen birth among a randomly selected woman from the population of all women, regardless of teen childbearing status. Second, identifying $\beta$ enables one to assess the potential consequences of completely eliminating teenage childbearing in the U.S. Assessing such effects provides a benchmark against which to judge the potential benefits that could be derived from any particular policy mechanism directed at reducing the incidence of teenage childbearing.

It is apparent from (1) that the problem of estimating $\beta$ centers on the identification of $E(Y_{B^*} \mid D{=}B)$, since $E(Y_B \mid D{=}B)$ is readily obtained from data on women who had their first births as teenagers. Ideally, one would like to use data on $Y$ for women who have miscarriages as teens but for which $D^* = B^*$. Unfortunately, we cannot identify the members of this group. However, we do observe the outcomes for women who miscarry, denoted by $Y_M$, which provides some information about the women in the $Y_{B^*}$ group. In particular, $E(Y_M)$ is equal to

$$E(Y_M) = P^* E(Y_{B^*}) + (1 - P^*)E(Y_{A^*}), \qquad (2)$$

where the weighting factor, $P^*$, is the proportion of pregnant women who would have had a birth if they not miscarried. Solving (2) for the average outcome for latent-birth women, $E(Y_{B^*})$, we obtain

$$E(Y_{B^*}) = \frac{E(Y_M) - (1 - P^*)E(Y_{A^*})}{P^*}. \qquad (3)$$

While $E(Y_M)$ can be identified (and consistently estimated) from observable data on women who

[14] By analogy to the program evaluation literature, the causal effects we focus on are analogous to making inferences about the effect of a program on those who choose to participate and need not apply to a randomly selected individual being required to participate in program. See Heckman (1992) for a discussion of the distinctions between and usefulness of various treatment effect definitions.

have a miscarriage as a teen, we cannot identify (or readily estimate) either $E(Y_{A*})$ or $P*$ since doing so would also require knowing each woman's latent pregnancy type when she was a teen.

If (*i*) all miscarriages are random and (*ii*) all fertility events are correctly reported, then the fraction of women who would have carried the pregnancy to term among women who miscarried ($P*$) must equal the fraction of women who did carry the pregnancy to term among women who do not miscarry ($P$). That is, $P* = P$.[15] Furthermore, if (*iii*) having a miscarriage or an abortion has the same direct effect on $Y$, then, on average, the outcomes for women who have abortions will be equal to those of women in the latent-abortion group.[16] That is, $E(Y_{A*}) = E(Y_A)$.[17] Under these conditions, $E(Y_{B*})$ is equal to:

$$E(Y_{B*}) = \frac{E(Y_M) - (1-P)E(Y_A)}{P}. \tag{4}$$

It follows that $\beta$ can be written as a function of statistics that are identified (and, thus, readily estimated) from observable data. In particular,

$$\begin{aligned}
\beta* &= E(Y_B - Y_{B*} \mid D = B) \\
&= \frac{PE(Y_B) + (1-P)E(Y_A) - E(Y_M)}{P} \\
&= \frac{E(Y_{\sim M} - Y_M)}{P} \\
&= \frac{Cov(Y, \tilde{M})}{Cov(\tilde{B}, \tilde{M})}.
\end{aligned} \tag{5}$$

where $E(Y_{\sim M})$ is the average outcome for women who did not miscarry—since $E(Y_{\sim M}) \equiv PE(Y_B)$

[15] The expression $P* = P$ would be true if the fate of the fetus were determined at the time of conception. In reality, miscarriages and abortions occur throughout the nine months of pregnancy. We have used an adjustment to account for the longer exposure time to miscarriages for fetuses being carried to term relative to aborted fetuses with little effect on the results.

[16] On average, the outcomes for women who have abortions will be equal to those of all women in the latent-abortion group if an abortion and a miscarriage affected $Y$ only through the absence of a child. Alternatively, a miscarriage and an abortion could have a direct effect on $Y$ as long as the effects were equal.

[17] In the program evaluation literature, this assumption is referred to as the "No Hawthorne Effect" assumption, namely, that the random assignment affects outcomes only through the treatment provided. In our context, this assumption implies that only the presence (or absence) of a child affects maternal outcomes.

+ (1-$P$)$E(Y_A)$—and $\tilde{B}$ and $\tilde{M}$ denote indicator variables equal to 1 if a women $D = B$ and $M$, respectively, and 0 otherwise.[18]

Given the definition in (5), it follows that a simple Instrumental Variables (IV) estimator can be formed for $\beta$. Let $\bar{Y}_{\sim M}$ denote the sample mean of $Y$ for those women (observations in the data set) who do not experience a miscarriage, $\bar{Y}_M$ denote the sample mean for those women who do experience miscarriages, and $\hat{P}$ denote the sample proportion of women who do not experience a miscarriage. Then it follows that an IV estimator for $\beta$ is

$$\begin{aligned}\hat{\beta}_1^{IV} &= \frac{\bar{Y}_{\sim M} - \bar{Y}_M}{\hat{P}} \\ &= \frac{\widehat{Cov}(Y, \tilde{M})}{\widehat{Cov}(\tilde{B}, \tilde{M})},\end{aligned} \tag{6}$$

where $\widehat{Cov}(w_1, w_2)$ denotes the sample covariance between variables $w_1$ and $w_2$. It follows from (6) that the miscarriages variable ($\tilde{M}$) serves as an instrument for teen births ($\tilde{B}$) in estimating the causal effect on outcomes ($Y$).

As noted above, the validity of the estimator in (6) hinges on maintaining the three conditions noted above (*i* through *iii*). The validity of each is subject to debate. For example, epidemiological studies have found that smoking and drinking during pregnancy significantly increase the incidence of miscarriages.[19] Furthermore, such behaviors are likely to be correlated with such subsequent outcomes for women as labor market earnings. Thus, some miscarriages may fail the exclusion restrictions required of a proper instrumental variable estimator applied to some or all

[18] Note that $\tilde{B}$ = 0 for all women who have a miscarriage.

[19] See Kline, Stein, and Susser (1989) for a review of these findings. We note that epidemiologists have not found evidence of statistical associations between other behaviors, such as a woman's socioeconomic status, her nutrition, or her drug use, and the incidence of miscarriage, although these factors are found to affect birth weight. The latter findings also are summarized in Kline, Stein and Susser (1989).

maternal outcomes. Other challenges to the validity of these conditions are examined in Hotz, Mullin and Sanders (1997), who systematically assess the consequences of violating each of these conditions for the estimation of the causal effect of teenage childbearing ($\beta$). They show that in the presence of violations to (*i*) - (*iii*) one cannot point identify the casual effect in (1)—and, thus, ensure the consistency of the IV estimates in (6)—without knowledge of a woman's latent type among women who experience miscarriages.

However, Hotz, Mullin, and Sanders (1997) also demonstrate that one can form non-parametric bounds on $\beta$, even when none of these conditions holds. Furthermore, these bounds are *tight* as defined by Horowitz and Manski (1995) and can be derived and non-parametrically estimated, using auxiliary information on the proportion of miscarriages which are random and on the incidence of underreporting in surveys of abortion and miscarriage events. In their empirical investigation on a more limited set of outcomes than considered in this article, specifically educational attainment, annual hours of work, and earnings—Hotz, Mullin, and Sanders (1997) find that the estimated bounds are sufficiently tight to reject the null hypotheses on the signs of the causal effects. For example, the lower bound on the estimated effect of teenage childbearing on earnings is found not to be less than zero.

Using their estimated bounds on causal effects, Hotz, Mullin, and Sanders (1997) also are able to provide a direct assessment of the validity of the simple IV estimator in (6). Since the bounds constructed in Hotz, Mullin, and Sanders (1997) collapse to the simple IV point estimates when assumptions (*i*) – (*iii*) hold, a clear indication that they are violated would be if the IV estimates lie outside of the non-parametric bounds. In 832 tests at 13 different ages at which outcomes were measured for the same sample of women used in this article, the simple IV estimator is rejected only 21 times at the 0.05 level.[20]

Based on the findings from the above study, we maintain the assumptions necessary for miscarriages to be a valid instrument for teenage births on a more complete set of maternal outcomes than considered in that study. A further piece of supporting evidence for using miscarriages as an instrument is provided by a comparison of the background characteristics for the samples of women whose first pregnancy occurred prior to age 18 and those women who reported that their pregnancy ended in a miscarriage. Recall that summary statistics for a set of background characteristics for each of these two groups are presented in Table 2. If miscarriages are random, there should be no difference, on average, in the characteristics of women who miscarry and those women who become pregnant as teens. In the last column of Table 2, we present the p-values for tests of differences in the means of the background variables of these two groups. With the exception of the income of the woman's family in 1978, there are no statistically significant differences in the mean values of pre-pregnancy background characteristics of women who became pregnant before age 18 and did not miscarry and those who became pregnant before age 18 and miscarried. The striking similarity of these two groups of women in terms of their pre-pregnancy background characteristics provides substantive evidence that women who became pregnant as teens and miscarried constitute an appropriate control group to women who were pregnant as but did not miscarry.

In the next section, we present two sets of IV estimates of the causal effects of teenage childbearing. The first set estimates the causal effects on maternal outcomes measured at a particular adult age *a*, based on the following linear regression function,

$$Y_{ia} = \alpha_a + \beta_a \tilde{B}_i + \theta_a X_i + \varepsilon_{ia}, \tag{7}$$

[20] We test on four outcomes—receiving a GED, receiving a high school diploma, annual hours worked and annual earnings—and for multiple demographic groups under alternative weighting methods.

where $Y_{ia}$ denotes the $i^{\text{th}}$ woman outcome as of age $a$, $\varepsilon_{ia}$ is a disturbance term, $\alpha_a$ is the intercept, and $\beta_a$ is the age-specific causal effect of teenage childbearing and $X_i$ denotes a vector of covariates, measured when the woman is a teen. We estimate three variants of (7). One variant includes no covariates. A second controls for such a set of behavioral factors that the epidemiological literature has documented to be correlated with the incidence of miscarriages. These include, for example, whether the woman reported that she smoked or drank alcohol prior to her first pregnancy or age 18. A third variant of (7) also includes the personal and family background characteristics displayed in Tables 1 and 2, in an attempt to improve the precision of our estimates of $\beta_a$. For each of these specifications, we use $\tilde{M}_i$ to instrument for $\tilde{B}_i$ in order to produce IV estimates of $\beta_a$. For comparison, we also present estimates of $\beta_a$ using ordinary least squares (OLS) methods.

As noted in the Introduction, we are particularly interested in how the effect of teenage childbearing varies over a teen mother's life cycle. To produce these age-specific estimates, we use the following modified version of the regression specification in (7),

$$Y_{ia} = (\alpha_{18} + \beta_{18}\tilde{B}_i)I_{ia}^{18} + (\alpha_{19} + \beta_{19}\tilde{B}_i)I_{ia}^{19} + \ldots + (\alpha_{35} + \beta_{35}\tilde{B}_i)I_{ia}^{35} + \theta X_i + \varepsilon_{ia}, \tag{8}$$

where $I_{ia}^{a'}$ is an indicator variable equal to 1 if $a = a'$ and equal to zero otherwise and, to reduce the number of parameters estimated, we constrain $\theta$ to not vary with age.[21] We use the method of Huber (1967) to correct the estimated standard errors of parameter estimates for the temporal de-

[21] We note that we also estimated a variant of the specification in (8) in which in place of the age-specific intercepts and causal effects, we instead used lower-order polynomials in age and interactions of these age-polynomials with $\tilde{B}_i$ to produce a "smoothed" version of the causal effects of teenage childbearing of a teen mother's life cycle. While not presented here, we note that for almost all of the outcomes considered below, we could not reject the joint hypothesis that the life cycle variation in the $\beta_i$'s in (8) can be characterized by a quadratic version of such an age-polynomial specification.

pendence of age-specific outcomes for the same woman.

## IV. Estimates of the Causal Effects of Delaying Childbearing on Adult Outcomes among Teen Mothers

In this section, we present the estimated effects of teenage childbearing on measures of women's subsequent outcomes: (a) educational attainment; (b) fertility and marriage outcomes, (c) hours of work and market wages; (d) labor market earnings and earnings from spouses; and (e) receipt of various forms of public assistance. These maternal outcomes have been the focus of previous studies of the effects of teenage childbearing on mothers.

We first compare OLS and IV estimates of the causal effects teenage childbearing for various maternal outcomes measured when women are age 28, which is approximately 10 years after teen births would have occurred.[22] We then present a detailed examination of how our IV estimates of the effects of teenage childbearing vary across a woman's life cycle, from ages 18 through 35.

### *A. Comparison of Alternative Estimates of Effects of Teenage Childbearing*

Table 3 presents estimates of the impact of teenage childbearing at age 28 using alternative samples and methods. Column (1) contains estimates of the effects of not delaying childbearing until adulthood using OLS methods, controlling for background characteristics, on the All Women sample; Column (2) contains OLS estimates with the same controls as Column 1, using the Teen Pregnancy sample; Column (3) contains IV estimates with no controls; Column (4) are IV estimates with controls for the covariates correlated with non-random miscarriages; and Column (5) shows the IV estimates that include a full set of control variables, i.e., background characteristics and correlates with non-random miscarriages. The final three columns are estimated with the Teen Pregnancy sample. We also include a column with the sample means of

[22] Age 28 is the oldest age for which we have year-by-year data on women from all birth cohorts in the NLSY79.

each of these outcomes for teen mothers when they were age 28.

Consider first the estimates in Column (1) of Table 3. These estimates mirror the methodology used in a number of the previous studies of the effects of teenage childbearing. (See the Introduction for a partial list of these studies). Consistent with those findings, the estimates in Column (1) typically indicate "adverse" consequences of women not delaying their childbearing until adulthood on a range of subsequent maternal outcomes. Compared to women who delayed childbearing until after age 18, teen mothers (at age 28): were 46 percent less likely to have received a high school diploma; had 1.16 more children on average; were 16 percent more likely to be a single mother; had worked 170 fewer hours per year; had lower wages ($.88 per hour lower);[23] and earned $3,780 less in the paid labor market per year. In addition, teen mothers (at age 28): were in households with, on average, $2,231 less in spousal income; were 15 percent more likely to reside in a household with total income below the poverty level; were more likely to receive public assistance; and received $1,159 more from these programs. Furthermore, most of these estimated effects are sizeable relative to the average outcomes for teen mothers in our data. For example, these estimates imply that fraction of teen mothers receiving high school diplomas would have been 1.5 times higher and would have had 50 percent higher earnings at age 28 if they had delayed their childbearing.[24] If such estimates accurately characterize the causal effects of teenage childbearing, they imply that the failure of teen mothers to delay their childbearing has dire consequences for the socioeconomic attainment of these women that are size-

[23] As explained below, the estimated effects of teenage childbearing on hourly wage rates are not comparable to the other estimates, given that they are estimated only for women who work at a particular age and our methods do not account for this potential source of selectivity.

[24] More generally, these estimates imply that the likelihood that teen mothers would have received a high school diploma or a GED would have been .51 times higher, would have had 46 percent fewer children, would have been 45 times less likely to be a single mother, would have had a 21 percent higher wage rate, would have been .43 times less likely to be on AFDC and would have received 42 percent less in public assistance benefits at age 28 if they had delayed their childbearing to adulthood, where these comparisons are made to the average outcomes of teen mothers at that age.

able and apparently persistent.

As one moves across the columns in Table 3, changing the sample used and exploiting the “miscarriages-as-a-natural-experiment” in the estimation of the effects of teenage childbearing, we find that the adverse effects found in Column (1) are progressively weakened and, for some outcomes, are even reversed. Furthermore, the estimates in the subsequent columns suggest a very different set of conclusions about the consequences of early childbearing for teenage mothers. Simply changing the sample from all women to those who were pregnant as teens, Column (1) vs. Column (2), we find that the adverse effects of teenage childbearing appear to be reduced, after controlling for a comparable set of covariates. For some outcomes, such as single motherhood (Row 7), all of the work-related outcomes (Rows 8-10), spousal income (Row 12), and the public assistance measures (Rows 14-16), the estimated effects are no longer statistically significant and the effects on being married actually reverse sign and are statistically significant compared to the OLS estimates in Column (1). Thus, it does appear that using a more comparable comparison group, namely women who become pregnant as teens but do not have a teen birth, to estimate the counterfactual outcomes for teen mothers does alter one’s inferences about the effects of early childbearing.

A comparison of either Column (2) or (1) with the IV estimates in Columns (3) through (5) of Table 3 clearly demonstrates that using miscarriages as an instrument has even more dramatic consequences for the estimation of the causal effects of teenage childbearing. All of the IV estimates, except for the effects on obtaining a GED, are either statistically insignificant or are significant and have the opposite sign of the OLS estimates. Note that for most of the outcomes in Columns (3) through (5) in Table 3 the IV estimates—and the inferences they imply about the effects of teenage childbearing—are quire similar. Among the statistically significant IV estimates, we find that a teen mother's annual hours of work are between 317 and 420 *higher* and her annual earnings are between $4,218 and $5,075 *higher* at age 28 than if she had delayed her childbearing. Furthermore, these effects are sizeable. If teen mothers had delayed their childbearing, their annual hours of work and annual earnings would have been, respectively, 30 to 40 percent and 56 to 62 percent *lower* at this age relative to the average values of these outcomes for teen mothers. In short, which samples and, more importantly, what statistical methods one uses has a profound impact on the inferences one draws about the consequences of teenage childbearing for the socioeconomic attainment of this group of women.

### *B. IV Estimates on Maternal Outcomes over Life Cycle*

We now turn to a more detailed consideration of our estimated effects for particular outcomes and examine how these effects vary over the life cycle. IV estimates—with and without covariates and organized by types of outcomes at each age, 18 through 35—are presented in Tables 4 through 7.[25] We focus on whether and how the effects of teenage childbearing vary over the life cycle. Having children when women are teenagers, rather than delaying them, is a decision about the *timing* of fertility. Much of the previous literature has suggested that this timing decision has permanent (and adverse) consequences, e.g., higher completed fertility or less success in both labor and marriage markets. Alternatively, the timing decision may have rather tran-

[25] We note an important feature of the structure of our data that has consequences for the reliability of our estimated effects at older ages. As noted in Section 2, the NLSY79 consists of longitudinal data for women (and men) that were ages 14 through 21 as of 1979 and we use data on these women from the 1979 through 1992 interviews. It follows that we will have fewer observations on women at ages beyond 28, the oldest age that the women in the earliest birth cohort of the NLSY79 attain by 1992. In particular, we have observations for 1,042 18 year-old women in our sample, 1,041 19 year-olds, …, and 1,014 28 year-olds. (The loss of observations between ages 18 and 28 is due to sample attrition, which is minimal in the NLSY79.) But we have observations on only 909 29 year-olds, 764 on 30 year-olds, …, 232 on 34 year-olds and only 116 35 year-olds, with most of the lower numbers of observations at ages past 28 attributable to the cohort structure of our data and a small fraction of the loss due to sample attrition. Thus our estimates of effects of teenage childbearing for ages 29 through 35 are based on progressively fewer observations *and* from progressively earlier birth cohorts. While we present estimates for the effects of teenage childbearing through age 35 in the results that follow, one should keep in mind this decline in observations and increase in the proportion of observations from earlier birth cohorts with age when interpreting our results.

sitory consequences for some or all of a teen mother's subsequent life. We present evidence below on which of these alternatives better characterizes the consequences of teenage childbearing.

*Educational Attainment*

We begin with the effects of teenage childbearing on a women's subsequent educational attainment. Since there is little change in the educational attainment of the women in either of our samples after the early twenties, we restrict our attention to attainment by age 28, as displayed in the first three rows of Table 3. While previous studies and our OLS estimates suggest that teenage childbearing has negative effects on educational attainment, we find little evidence of this in our IV estimates. While negative, our IV estimates of the effect of teenage childbearing on having attained a regular high school diploma by age 28 in Columns (3) through (5) are always small, varying between a 5 to 12 percentage-point decrease in the likelihood of having attained a high school diploma and are never statistically significant. In contrast, teenage childbearing appears to *increase* the rate of completion of the General Educational Development (GED) certificate,[26] by between 11 to 13 percentage points, and the IV estimates that control for covariates are statistically significant at modest levels. Thus, it appears that teen mothers compensate for their lower rates of attaining a high school diploma by receiving a GED. However, this substitution is only partial. We find no statistically significant effect of early childbearing on the probability that teen mothers obtain a high school level education—either in the form of a regular high school diploma or GED—relative to what would have happened to these women if they had delayed their childbearing.

It is unclear what the implications are of this apparent substitution by teen mothers of attaining a GED rather than a regular high school diploma. Cameron and Heckman (1993) find that

[26] The GED is granted upon the successful completion of an examination that tests competency in a basic high school curriculum. The GED does not require a fixed class schedule and may offer teenage mothers substantial flexibility in study time.

the value of the GED in the labor market is minimal, with its recipients earning no more than high school dropouts. If their findings apply to teen mothers, the consequences of this substitution of a diploma for a GED are not innocuous, at least not for labor market earnings. However, the Cameron and Heckman analysis is only for *men*. In a subsequent study of the GED for *women* Cao, Stromsdorfer and Weeks (1996) find that the labor market returns (in terms of earnings) to a GED are no different from the returns to a high school diploma. What remains to be determined is whether this result also holds for the subset of women who become teen mothers.

*Fertility and Marriage*

In Table 4, we present age-specific IV estimates of the causal effects of teenage childbearing on women's subsequent fertility and marriage outcomes. We consider the effects of teenage motherhood on the number of children to whom a woman gives birth and on the probability of having any children at each age. Since according to our definition of teen mothers, all teen mothers have children by age 18, it is important to recognize that the estimated effects of a woman not delaying her childbearing until adulthood (that is, age 18 and older) on her subsequent fertility are measured relative to what would have happened if she had delayed her fertility. That is, the counterfactual outcome here is the life cycle childbearing pattern of fertility for "latent-birth" women characterized in Section III, where we use miscarriages to estimate their outcomes. While the latent-birth (hypothetical) women, by definition, delay their childbearing, at issue is how long they delay their childbearing. The estimates in the first two sets of columns in Table 4 indicate that, on average, they do not delay childbearing much past their early twenties and, in many cases, for even less time than that.

According to the estimates in the first column of Table 4, only 56 percent of latent-birth type women have not had a child by age 18.[27] Regardless of the IV estimates used, by age 24, only between 17 and 19 percent of latent-birth type women have not had a child, with these rates of "no birth" diminishing further with age. The estimated effects actually start to rise when women reach age 30 and older, although, as noted above, these estimates are based on much smaller numbers of observations. Because of the generally high fertility among latent-birth women, the effect of teenage childbearing on the number of children to whom a woman gives birth is never more then 0.84 and declines to 0.30 in her late 20s. A similar age pattern holds for the estimated effects of teenage childbearing on the number of children born by the time the woman reaches various ages (see Table 4). Thus, while teenage childbearing does clearly increase the incidence of births and family size in the short run relative to the counterfactual state of postponing childbearing until they are older, this effect fades rather quickly with age. Thus, as seen in Table 3, the effect of not postponing childbearing by teen mothers is negligible and insignificant by age 28.

Table 4 also presents IV estimates of the effects of teenage childbearing on whether a woman is married and is a single mother at age *a*, which combines information on a woman's marital and fertility statuses. As is clear from Table 4, the effects of early childbearing on the likelihood of being married (regardless of a woman's fertility status) are small and always statistically insignificant. We find that at ages 18 and 19, being a teenage mother increases the chances of being a single mother by 28 percentage points and 19 percentage points, respectively but no significant effect is found at older ages. In fact, the estimated effects at older ages tend to be negative, although none of them is statistically significant. Thus, the effects of teenage childbearing on single motherhood are even more transitory than the effects on fertility alone, with the

[27] While the average delay of childbearing after a miscarriage is 4.4 years, approximately 11 percent delay childbearing more than 10 years.

early effects driven almost entirely by the higher rates of motherhood at early ages.

*Hours of Work and Wages*

Table 5 displays IV estimates of the effects of early childbearing on annual hours of work, cumulative number of hours worked, and hourly wage rates at different ages over a woman's life cycle. Consider the effects on annual hours of work in the first set of columns in this table. While women who have teen births work slightly fewer hours at ages 18 and 19 than they would have if they had delayed their childbearing, from age 20 on, they actually work more hours in a year than if they had postponed their childbearing. By age 28, for example, women who have their first births early work between 342 and 405 more hours than if they had delayed their childbearing until adulthood. Some of these differences are statistically significant when teen mothers are in their mid to late twenties, although the age-specific estimates that control for covariates are less precisely estimated. Finally, we note that these estimated effects are not trivial in magnitude. Over the ages of 21 through 35, teen mothers worked an average of 1,045 hours per year. Based on the "All Covariates" estimates, teen mothers would have worked an average of 21 percent fewer hours per year over this age range if they had delayed their first birth until adulthood. Moreover, the negative effects of not delaying childbearing as a percentage of hours of work become larger with age, rising to 29 percent fewer hours when teen mothers are in their early thirties.

Our estimates show similar effects of teen mothers not delaying their childbearing on *cumulative* number of hours worked, although the effects for this variable are less precisely estimated. By age 28, teen mothers have worked between 1,999 to 2,600 more hours—a year or more of full-time work—than if they had delayed childbearing.

We next consider the effect of teen mothers not delaying their childbearing on hourly wage rates. Before doing so, it is important to distinguish these estimates from those for the other socioeconomic outcomes we consider in this article. Note that wage rates are measured only at ages when women work and our estimation strategy does not account for this source of selectivity. Without further assuming that our instrumental variable (IV) is uncorrelated with whether a woman works—an assumption that is not readily justifiable—our IV estimator of the effects of early childbearing on wages need not be consistent. Thus, the estimates in Table 5 for wages—as well as the corresponding estimates in Table 3—do not have the same credibility as to those for the other outcomes we consider and should be interpreted with much greater caution. With this important caveat, we briefly consider the estimated effects of teenage childbearing on a woman's hourly wages. Unlike annual hours of work, there is little evidence of a strong life cycle pattern with respect to the effects of early childbearing on hourly wage rates based on the age-specific estimates presented in Table 5. Furthermore, with the exception of age 30, the age-specific estimates of these effects are not precisely estimated. Thus, we tentatively conclude that early childbearing does not result in permanently lower wage rates relative to what teen mothers would have received if they had delayed their childbearing.

*Earnings and the Incidence of Living in Poverty*

In Table 6, we examine the effect for teen mothers of not delaying their childbearing on their labor market earnings, spousal earnings, and the likelihood that they live in poverty. As shown in Table 5, not delaying their births actually resulted in increased hours of work when these women reached their mid- to late twenties and did not result in any significant declines in the wages they received. Thus, our finding that not delaying childbearing led to an increase in the labor market earnings of these women starting at ages 24 or 25 is plausible rather than surprising (refer to the first two columns of Table 6.) These effects on a woman's earnings persist into the thirties, although the latter effects for the thirties are not as precisely estimated, possibly because of the reduced numbers of observations at these ages. Furthermore, the magnitudes of these estimated effects of teenage childbearing on subsequent labor market earnings are sizeable. Over the ages of 21 through 35, teen mothers earned an average $7,917 per year (in 1994 dollars). Based on the “All Covariates” estimates in Table 6, teen mothers would have earned an average of 31 percent *less* per year if they had delayed their childbearing, where the largest reductions are estimated to be 24 percent during the early twenties, 43 percent during the late twenties and 27 percent during the early thirties.

Our findings for the life cycle consequences of teen mothers not delaying their childbearing for their success in the labor market, especially earnings, are in sharp contrast to all previous studies of the effects of teenage childbearing and to the view that teenage childbearing severely restricts the ability of these women to be successful in the labor market. While a coherent explanation of why teen mothers appear to benefit from not delaying their childbearing when it comes to the labor market awaits further research, we offer the following speculation.

Our evidence, and that of others, documents that women who begin motherhood as teens come from less advantaged backgrounds, are less likely to be successful in school, and, as such, are less likely to end up in occupations that require higher education compared with women who postpone motherhood. Our evidence further suggests that these women are more likely to acquire their skills on the job (rather than in school) and work in jobs where educational credentials are less important than continuity and job-specific experience. For such women, concentrating their childbearing at early ages may prove to be more compatible with their labor market career options than postponing their childbearing to older ages would be. If this characterization is accurate, forcing teen mothers to postpone their childbearing, as miscarriages do, may “explain” why they both appear to acquire no more formal education and actually end up doing less well in the labor market than if they had been able to follow their preferred life cycle plan.

Estimates of the effects of teen mothers not delaying their childbearing on the financial support they receive in the form of spousal income also are displayed in Table 6. Based on these estimates, there is no evidence that teenage mothers draw substantially less support from spouses over their early to mid-adult years. In fact, starting at around age 30, both of the IV estimates presented in the second and third columns of Table 6 indicate that teenage mothers actually derive substantially more support in the form of spousal income than they would have received if they had delayed childbearing.[28] We have no ready explanation for this finding. We speculate that it may be related to our finding that teenage mothers are more successful in the labor force, that is, they generate higher earnings, than if they had postponed their childbearing and that this success affects their success in the marriage market, at least with respect to the earnings capacity of a spouse.

Finally, we consider what effects delaying early childbearing would have for the likelihood that teen mothers are in households with such low levels of earnings—from both their own earnings and the earnings of spouses—that they fall below the federal poverty line. IV estimates of the effects of teenage childbearing on the incidence of living in poverty are presented in the last two columns of Table 6. Given the results for own and spousal earnings presented in this table, it is not surprising that teenage childbearing does not seem to be associated with an increase in the incidence of living in poverty among teen mothers, in either the short run or the long run. In fact, our estimates suggest that not delaying childbearing actually *reduces* the incidence of poverty, although these effects are statistically significant at only a few older ages. At the same

[28] Recall that the chances of being married at any age appears to be unaffected by teenage childbearing.

time, a sizeable fraction of women who begin their childbearing as teens (38 percent) do live in poverty during their twenties and early thirties. Our estimates indicate that the incidence of poverty for women who began their childbearing as teens would have been .58 times *more* likely to live in poverty over this period of their lives if they delayed their childbearing until adulthood.

<u>*Receipt of Public Assistance*</u>

In Table 7, we present estimates of the effects of teenage childbearing on the use of and financial support from several forms of public assistance for ages 18 through 35. In particular, we examine the effects on the following outcomes: (1) whether a woman was receiving public assistance through the Aid to Families with Dependent Children (AFDC) program at a particular age *a*,[29] (2) whether she received Food Stamps at that age, and (3) the annual dollar amount (in 1994 dollars) of benefits a woman received from the AFDC, Food Stamps programs and the Medicaid program.[30] Both sets of IV estimates of the effects of not delaying births among teen mothers have similar age patterns for all three of these measures of public assistance. In particular, all three outcomes show that at young adult ages, from age 18 until around 22, the estimated effects of teenage childbearing are positive, indicating that teen mothers were more likely to be

[29] The Aid to Families with Dependent Children (AFDC) program, a federal program, was replaced in 1996 with the Temporary Assistance for Needy Families (TANF) program.

[30] The annual benefits received from AFDC and Food Stamps were derived from women's responses to annual questions about number of months in a (calendar) year that they were on each of these programs and the average monthly payment/benefit they received. To estimate the implied Medicaid benefits she received, we used the following strategy to estimate the relationship between AFDC and Food Stamps and Medicaid expenditures. In particular, we regressed aggregate state Medicaid expenditures on the sum of aggregate AFDC and Food Stamp expenditures by state, yielding the following regression:

Medicaid Expend. = 250 + .193(AFDC Expend. + Food Stamp Expend.)

using 1993 monthly data taken from the *Green Book* (U.S. House of Representatives, Ways and Means Committee, 1994). We interpreted the intercept as being the fixed costs of running a state's Medicaid program *plus* the expenditures going to non-AFDC/Food Stamp recipients (i.e., the Elderly) and calculated the "marginal" Medicaid expenditures for a woman in our sample as:

Medicaid Payments = .193(AFDC Expend. + Food Stamp Expend.)

We the measure of annual benefits used as the dependent variable in our regressions was *WelfBen* = AFDC Payments + Food Stamp Payments + (Est.) Medicaid Payments). These amounts were all converted to 1994 dollars.

on public assistance and receive larger amounts of transfers from these programs than if they had delayed their childbearing. For these younger ages, the estimated effects are statistically significant only for the annual amount of benefits received in the form of public assistance. However, from around age 22, the estimated effects reverse in sign, implying that teen mothers actually reduced their participation in and amount of benefits received from these public assistance programs compared to what they would have done if they had delayed their childbearing. Most of these estimated effects at older ages are not statistically significant, although we do find significant effects for receiving Food stamps at age 25 and 26. Once again, our IV estimates indicate that the causal effect of teen motherhood does not appear to increase the utilization of various forms of public assistance as suggested by earlier studies.

## V. Conclusion

In this study, we have used an alternative and innovative strategy to estimate the causal effects associated with teenage childbearing in the U.S. In particular, we have focused on women who first become pregnant as teenagers and employ a natural experiment to obtain a more comparable, and plausible, comparison group with which to derive estimates of counterfactual outcomes for teen mothers. Our results suggest that much of the "concern" that has been registered regarding teenage childbearing is misplaced, at least based on its consequences for the subsequent educational and economic attainment of teen mothers. In particular, our estimates imply that the "poor" outcomes attained by such women cannot be attributed, in a causal sense, primarily to their decision to begin their childbearing at an early age. Rather, it appears that these outcomes are more the result of social and economic circumstances than they are the result of the early childbearing of these women. Furthermore, our estimates suggest that simply delaying their childbearing would not greatly enhance their educational attainment or subsequent earnings or affect their family structure.

Our estimates suggest that teenage mothers are much more adaptable over their life cycle than previous discussions of the consequences of teenage childbearing have suggested. For example, teen mothers do appear to be less likely to receive a high school diploma than if they had delayed their childbearing. However, they appear to offset this shortcoming by being more likely to obtain a GED and, more importantly, by working much more over their early adulthood than if they had delayed childbearing. Moreover, we find that teen mothers may actually achieve higher levels of earnings over their adult lives than if they had postponed motherhood. Finally, we find evidence that while teenage childbearing does seem to increase public aid expenditures immediately after a teen birth, this “negative” consequence of teenage childbearing is not a permanent one, in that teen mothers use less public aid in their late 20s as their earnings rise and their children age.

Taken together, the results presented in this article call into question the view that teenage childbearing is one of the nation’s most serious social problems, at least when one measures its severity in terms of the potential financial gains to these women and to taxpayers of having all teen mothers delay their childbearing until they are older. At the same time, we caution the reader not to generalize from these findings. We have considered only selected potential consequences of teenage childbearing. Furthermore, the findings from one study cannot be considered as conclusive. However, for many of the socioeconomic outcomes considered in this article, our findings are consistent with the estimated effects of teenage childbearing found in the work of Geronimus and Korenman (1992) and Grogger and Bronars (1993). Our work, along with these earlier studies, raises serious doubts about the extent and nature of teenage childbearing as a “social problem” in the U.S. More importantly, our research casts doubt on the view that postponing childbearing will improve the socioeconomic attainment of teen mothers in any substantial way.

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**Table 1**
*Background Characteristics of Teenage Mothers and Women Who Delayed Childbearing until after Age 18*

| | Teenage Mothers | | Not Teenage Mothers | |
|---|---|---|---|---|
| Characteristic | Mean | Std. Dev. | Mean | Std. Dev. |
| Black | 0.33 | 0.47 | 0.12 | 0.33 |
| White | 0.58 | 0.49 | 0.82 | 0.39 |
| Hispanic | 0.09 | 0.29 | 0.06 | 0.24 |
| Family on welfare in 1978[1] | 0.19 | 0.39 | 0.11 | 0.31 |
| Family income in 1978[2] | $30,532 | $22,401 | $50,717 | $31,841 |
| In female-head household at age 14 | 0.20 | 0.40 | 0.12 | 0.33 |
| In intact household at age 14 | 0.69 | 0.46 | 0.84 | 0.37 |
| Mother's education | 9.88 | 2.86 | 11.67 | 2.76 |
| Father's education | 9.94 | 3.37 | 11.91 | 3.56 |
| AFQT score[1] | 25.81 | 21.39 | 49.58 | 27.49 |
| Number of Observations | 603 | | 4,323 | |

Data Source: NLSY79, All Women sample; weighted estimates.
[1]Estimates are expressed in 1994 dollars.
[2]Armed Forces Qualifying Test Score.

**Table 2**

*Background Characteristics of Women Pregnant Prior to Age 18 by Pregnancy Outcomes*

| | All Women Pregnant before 18 (1) | | First Pregnancy before 18 ended in Birth (2) | | First Pregnancy before 18 did not end in Birth (3) | | First Pregnancy before 18 ended in Abortion (4) | | First Pregnancy before 18 ended in Miscarriage (5) | | P-Value, Difference in Means[1] |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. | |
| Black | 0.27 | 0.44 | 0.30 | 0.46 | 0.18 | 0.38 | 0.16 | 0.36 | 0.26 | 0.44 | 0.838 |
| White | 0.65 | 0.48 | 0.61 | 0.49 | 0.76 | 0.43 | 0.79 | 0.41 | 0.63 | 0.49 | 0.773 |
| Hispanic | 0.08 | 0.28 | 0.09 | 0.29 | 0.07 | 0.25 | 0.05 | 0.22 | 0.11 | 0.32 | 0.353 |
| Family on welfare in 1978[2] | 0.16 | 0.37 | 0.19 | 0.39 | 0.09 | 0.29 | 0.09 | 0.28 | 0.11 | 0.32 | 0.127 |
| Family income in 1978[2] | $37,551 | $28,201 | $32,267 | $23,217 | $47,975 | $33,809 | $52,774 | $34,999 | $27,441 | $16,919 | 0.003 |
| In female-headed family at age 14 | 0.18 | 0.39 | 0.19 | 0.39 | 0.16 | 0.36 | 0.14 | 0.34 | 0.23 | 0.42 | 0.361 |
| In intact household at age 14 | 0.72 | 0.45 | 0.71 | 0.45 | 0.75 | 0.43 | 0.78 | 0.42 | 0.64 | 0.48 | 0.189 |
| Mother’s education | 10.41 | 2.74 | 10.00 | 2.84 | 11.36 | 2.23 | 11.70 | 2.15 | 10.15 | 2.07 | 0.401 |
| Father’s education | 10.47 | 3.33 | 9.93 | 3.33 | 11.67 | 3.02 | 11.89 | 2.93 | 10.70 | 3.23 | 0.620 |
| AFQT score | 31.55 | 23.65 | 27.30 | 21.92 | 41.63 | 24.59 | 44.38 | 24.52 | 31.59 | 22.30 | 0.990 |
| Number of Observations | 1,042 | | 778 | | 264 | | 192 | | 72 | | |
| % of those Pregnant before Age 18 | | | 74.7% | | 25.3% | | 18.4% | | 6.9% | | |

Data Source: NLSY79, Teen Pregnancy sample; weighted estimates.

[1]Test of difference in means across categories (1) through (5).

[2]Estimates are expressed in 1994 dollars.

**Table 3**
*Change in Outcomes Due to Not Delaying Childbearing Measured at Age 28*

| | OLS | | IV (on Teen Pregnancy Sample) | | | |
|---|---|---|---|---|---|---|
| | All Covariates,[2] All Women Sample | All Covariates,[2] Teen Pregnancy Sample | No Covariates | Covariates Correlated with Miscarriages[1] | All Covariates[1,2] | Sample Mean for Teen Mothers at Age 28 |
| | (1) | (2) | (3) | (4) | (5) | |
| *Education Outcomes:* | | | | | | |
| 1. High School Diploma (HSD) by Age 28 | -0.46*** | -0.19*** | -0.05 | -0.07 | -0.11 | 0.31 |
| | (18.66) | (4.45) | (0.51) | (0.70) | (1.31) | |
| 2. General Educational Development (GED) by Age 28 | 0.17*** | 0.09** | 0.11 | 0.12* | 0.13** | 0.25 |
| | (7.15) | (2.50) | (1.61) | (1.75) | (1.99) | |
| 3. HSD or GED by Age 28 | -0.28*** | -0.10*** | 0.05 | 0.05 | 0.01 | 0.55 |
| | (10.77) | (2.79) | (0.54) | (0.47) | (0.14) | |
| *Fertility and Marriage Outcomes:* | | | | | | |
| 4. Had Some Children by Age 28 | 0.30*** | 0.25*** | 0.11 | 0.11 | 0.12 | 1.00 |
| | (25.20) | (7.49) | (1.49) | (1.39) | (1.45) | |
| 5. Number of Kids Born by Age 28 | 1.16*** | 0.84*** | 0.20 | 0.18 | 0.23 | 2.54 |
| | (18.64) | (7.96) | (0.83) | (0.72) | (0.89) | |
| 6. Woman Married at Age 28 | 0.03 | 0.12*** | 0.01 | 0.01 | -0.02 | 0.63 |
| | (1.07) | (2.85) | (0.14) | (0.11) | (0.21) | |
| 7. Single Mother at Age 28 | 0.16*** | 0.01 | -0.04 | -0.04 | -0.02 | 0.37 |
| | (6.18) | (0.01) | (0.47) | (0.46) | (0.18) | |
| *Work Outcomes:* | | | | | | |
| 8. Annual Hours Worked at Age 28 | -170*** | -21 | 405** | 420** | 317* | 1,039 |
| | (2.96) | (0.24) | (2.26) | (2.24) | (1.67) | |
| 9. Cumulative Number of Hours Worked by Age 28 | -2,009*** | -969 | 2,600** | 2,790** | 2,031 | 7,759 |
| | (5.19) | (1.56) | (2.24) | (2.36) | (1.49) | |
| 10. Hourly Wage Rate at Age 28 (in 1994$)[3] | -0.88** | -0.91 | 1.82 | 2.07* | 2.72** | 7.90 |
| | (2.03) | (1.42) | (1.53) | (1.65) | (2.07) | |
| *Earnings-Related Outcomes:* | | | | | | |
| 11. Woman's Annual Earnings at Age 28 (in 1994$) | -3,780*** | -2,599*** | 4,677*** | 5,075*** | 4,218** | $7,500 |
| | (3.50) | (2.68) | (2.93) | (2.95) | (2.47) | |
| 12. Annual Earnings of Spouse at Age 28 (in 1994$) | -2,213** | 115 | 1,177 | 1,029 | 1,668 | $10,742 |
| | (2.07) | (0.06) | (0.31) | (0.28) | (0.45) | |
| 13. Fraction Living in Poverty at Age 28 | 0.15*** | 0.06 | -0.14 | -0.14 | -0.13 | 0.47 |
| | (5.36) | (1.42) | (1.40) | (1.43) | (1.41) | |
| *Public Assistance Outcomes:* | | | | | | |
| 14. On AFDC while Age 28 | 0.11*** | 0.02 | -0.05 | -0.06 | -0.02 | 0.27 |
| | (4.85) | (0.57) | (0.62) | (0.65) | (0.21) | |
| 15. Received Food Stamps while Age 28 | 0.14*** | 0.04 | -0.07 | -0.07 | -0.03 | 0.36 |
| | (5.76) | (1.07) | (0.81) | (0.82) | (0.33) | |
| 16. Ann. Pub. Asst. Benefits at Age 28 (in 1994$) | 1,159*** | 230 | -510 | -455 | 53 | $2,787 |
| | (4.84) | (0.69) | (0.57) | (0.53) | (0.07) | |

Notes: Dollar figures in 1994 dollars; t-statistics in parentheses; based on weighted regressions

[1] Includes dummy variables for smoked prior to pregnancy, drank prior to pregnancy and whether had pregnancy prior to age 16.

[2] Includes dummy variables for the woman's age as of 1979, ethnicity (black and Hispanic), living in a female-headed family at age 14, living in an intact family at age 14, and whether woman's AFQT score fell in 1st, 2nd, or 3rd quartiles of distribution, as well as measures of woman's family income in 1978 (in 1994$), her mother's and father's educational attainment and missing value indicators for the last three variables.

[3] Estimated for women who worked at that age.

**Table 4**
*Estimates of the Effect of Teenage Childbearing on Fertility and Marriage Outcomes*

| | Had Some Children by Age *a* | | Number of Kids Born as of Age *a* | | Woman Married at Age *a* | | Single Mother at Age *a* | |
|---|---|---|---|---|---|---|---|---|
| Age of Mother | No Covariates | All Covariates | No Covariates | All Covariates | No Covariates | All Covariates | No Covariates | All Covariates |
| 18 | 0.56*** | 0.61*** | 0.63*** | 0.71*** | 0.09 | 0.09 | 0.28*** | 0.33*** |
| | (6.12) | (7.03) | (4.64) | (5.98) | (0.71) | (0.69) | (3.22) | (4.25) |
| 19 | 0.43*** | 0.48*** | 0.66*** | 0.73*** | 0.02 | -0.01 | 0.19** | 0.24*** |
| | (4.52) | (5.06) | (4.77) | (6.01) | (0.22) | (0.05) | (2.23) | (2.99) |
| 20 | 0.28*** | 0.31*** | 0.47*** | 0.60*** | 0.05 | 0.03 | 0.06 | 0.10 |
| | (3.04) | (3.35) | (2.66) | (4.12) | (0.55) | (0.34) | (0.75) | (1.16) |
| 21 | 0.19** | 0.21** | 0.42** | 0.57*** | 0.06 | 0.04 | 0.00 | 0.03 |
| | (2.11) | (2.28) | (2.11) | (3.61) | (0.67) | (0.41) | (0.02) | (0.31) |
| 22 | 0.21** | 0.23** | 0.42** | 0.55*** | 0.03 | 0.01 | -0.01 | 0.01 |
| | (2.36) | (2.53) | (1.99) | (3.13) | (0.31) | (0.06) | (0.14) | (0.12) |
| 23 | 0.15* | 0.17* | 0.36* | 0.46** | 0.06 | 0.04 | -0.07 | -0.06 |
| | (1.82) | (1.93) | (1.67) | (2.49) | (0.57) | (0.41) | (0.75) | (0.62) |
| 24 | 0.17** | 0.19** | 0.33 | 0.43** | 0.04 | 0.02 | -0.04 | -0.02 |
| | (2.04) | (2.11) | (1.48) | (2.09) | (0.46) | (0.21) | (0.38) | (0.25) |
| 25 | 0.16* | 0.17** | 0.32 | 0.40* | -0.02 | -0.05 | 0.02 | 0.04 |
| | (1.91) | (1.97) | (1.36) | (1.79) | (0.17) | (0.56) | (0.22) | (0.51) |
| 26 | 0.14* | 0.15* | 0.25 | 0.32 | 0.05 | 0.02 | -0.03 | -0.02 |
| | (1.76) | (1.79) | (1.07) | (1.38) | (0.54) | (0.24) | (0.37) | (0.18) |
| 27 | 0.14* | 0.15* | 0.19 | 0.24 | -0.04 | -0.07 | 0.04 | 0.06 |
| | (1.76) | (1.80) | (0.75) | (0.95) | (0.43) | (0.83) | (0.47) | (0.81) |
| 28 | 0.11 | 0.12 | 0.20 | 0.25 | 0.01 | -0.01 | -0.04 | -0.03 |
| | (1.49) | (1.51) | (0.83) | (0.99) | (0.14) | (0.13) | (0.47) | (0.29) |
| 29 | 0.11 | 0.13 | 0.21 | 0.30 | 0.02 | 0.01 | -0.08 | -0.06 |
| | (1.35) | (1.55) | (0.81) | (1.17) | (0.23) | (0.12) | (0.77) | (0.61) |
| 30 | 0.10 | 0.12 | 0.22 | 0.29 | 0.10 | 0.08 | -0.14 | -0.13 |
| | (1.27) | (1.39) | (0.77) | (0.99) | (0.88) | (0.80) | (1.32) | (1.29) |
| 31 | 0.10 | 0.12 | 0.29 | 0.32 | 0.12 | 0.09 | -0.16 | -0.13 |
| | (1.21) | (1.31) | (0.98) | (1.06) | (1.01) | (0.87) | (1.36) | (1.26) |
| 32 | 0.13 | 0.14 | 0.62** | 0.53 | 0.07 | 0.09 | -0.10 | -0.13 |
| | (1.34) | (1.36) | (2.00) | (1.56) | (0.57) | (0.85) | (0.82) | (1.16) |
| 33 | 0.16 | 0.18 | 0.55 | 0.48 | -0.08 | -0.03 | 0.05 | 0.00 |
| | (1.20) | (1.32) | (1.39) | (1.11) | (0.61) | (0.31) | (0.36) | (0.04) |
| 34 | 0.22 | 0.23 | 0.60 | 0.62 | 0.00 | -0.05 | -0.02 | 0.02 |
| | (1.32) | (1.43) | (1.15) | (1.22) | (0.02) | (0.37) | (0.15) | (0.17) |
| 35 | 0.50* | 0.51** | 1.71** | 1.71*** | 0.11 | 0.09 | -0.11 | -0.08 |
| | (1.88) | (2.09) | (2.42) | (2.60) | (0.41) | (0.39) | (0.41) | (0.31) |
| No. of Person-Ages | 14,839 | 14,096 | 14,839 | 14,096 | 14,428 | 13,716 | 14,428 | 13,716 |

Notes: Robust t-statistics in parentheses. Estimates based on weighted regressions. See notes in Table 3 for covariates used in "All Covariates" regressions.
* significant at 10%; ** significant at 5%; *** significant at 1%.

**Table 5**
*Estimates of the Effect of Teenage Childbearing on Work and Wage Outcomes*

| | Woman's Ann Hours Worked at Age *a* | | Cumulative Number of Hours Worked at Age *a* | | Hourly Wage Rate at Age *a* | |
|---|---|---|---|---|---|---|
| Age of Mother | No Covariates | All Covariates | No Covariates | All Covariates | No Covariates | All Covariates |
| 18 | -68 | -140 | 114 | -214 | -$2.93 | -$4.45 |
| | (0.47) | (0.76) | (0.53) | (0.33) | (1.03) | (1.44) |
| 19 | -145 | -239 | -66 | -349 | $1.26 | $0.82 |
| | (0.96) | (1.62) | (0.22) | (0.62) | (1.61) | (0.86) |
| 20 | 251* | 193 | 313 | 181 | $0.45 | $0.17 |
| | (1.74) | (1.30) | (0.96) | (0.38) | (0.40) | (0.14) |
| 21 | 186 | 145 | 352 | 231 | $1.28 | $0.81 |
| | (1.17) | (0.80) | (0.99) | (0.45) | (0.95) | (0.58) |
| 22 | 147 | 37 | 639 | 181 | -$0.40 | -$0.84 |
| | (0.96) | (0.22) | (1.42) | (0.31) | (0.34) | (0.71) |
| 23 | 76 | -7 | 883* | 444 | -$1.44 | -$1.87 |
| | (0.45) | (0.04) | (1.66) | (0.69) | (0.93) | (1.19) |
| 24 | 215 | 118 | 954 | 360 | $1.34 | $0.93 |
| | (1.18) | (0.60) | (1.43) | (0.48) | (1.38) | (0.92) |
| 25 | 331* | 254 | 1,160 | 701 | $0.75 | $0.26 |
| | (1.91) | (1.33) | (1.43) | (0.76) | (0.51) | (0.19) |
| 26 | 374** | 306 | 1,424 | 949 | -$1.49 | -$2.06 |
| | (2.20) | (1.60) | (1.54) | (0.92) | (0.49) | (0.69) |
| 27 | 344* | 285 | 1,963* | 1,186 | $2.19 | $1.93 |
| | (1.94) | (1.58) | (1.86) | (1.06) | (1.58) | (1.33) |
| 28 | 405** | 342* | 2,600** | 1,999 | $1.82 | $1.80 |
| | (2.26) | (1.86) | (2.24) | (1.61) | (1.53) | (1.52) |
| 29 | 70 | -47 | 1,747 | 1,120 | $1.08 | $2.29 |
| | (0.30) | (0.18) | (1.28) | (0.79) | (0.48) | (1.45) |
| 30 | 106 | 96 | 2,097 | 1,559 | $2.73** | $2.11* |
| | (0.47) | (0.40) | (1.31) | (0.90) | (2.10) | (1.76) |
| 31 | 230 | 329 | 994 | 368 | -$7.86 | -$9.07 |
| | (0.94) | (1.39) | (0.52) | (0.18) | (1.14) | (1.26) |
| 32 | 479** | 511** | 355 | -31 | $1.00 | $0.11 |
| | (1.97) | (1.99) | (0.16) | (0.01) | (0.58) | (0.07) |
| 33 | 470 | 556 | 2,398 | 1,848 | $2.44 | $0.86 |
| | (1.51) | (1.48) | (0.83) | (0.60) | (1.09) | (0.50) |
| 34 | 494 | 542 | 1,662 | 2,251 | $0.55 | $0.56 |
| | (1.55) | (1.51) | (0.54) | (0.65) | (0.25) | (0.26) |
| 35 | -131 | -129 | -3,567 | -3,338 | $2.59 | $2.46 |
| | (0.41) | (0.38) | (0.76) | (0.67) | (0.77) | (0.82) |
| No. of Person-Ages | 13,508 | 12,907 | 13,508 | 12,907 | 8,045 | 7,785 |

Notes: Robust t-statistics in parentheses. Estimates based on weighted regressions. See notes in Table 3 for covariates used in "All Covariates" regressions.
* significant at 10%; ** significant at 5%; *** significant at 1%.

**Table 6**
*Estimates of the Effect of Teenage Childbearing on Earnings-Related Outcomes*

| Age of Mother | Woman's Annual Earnings at Age *a* | | Annual Earnings of Spouse at Age *a* | | Living in Poverty at Age *a* | |
|---|---|---|---|---|---|---|
| | No Covariates | All Covariates | No Covariates | All Covariates | No Covariates | All Covariates |
| 18 | -$360 | -$1,240 | $3,744*** | $2,054 | -0.06 | -0.03 |
| | (0.30) | (0.70) | (2.76) | (0.89) | (0.67) | (0.31) |
| 19 | -$759 | -$1,706 | $2,956* | $878 | 0.04 | 0.06 |
| | (0.58) | (1.03) | (1.94) | (0.43) | (0.40) | (0.59) |
| 20 | $1,665 | $1,312 | $3,681** | $3,337* | -0.12 | -0.13 |
| | (1.61) | (1.05) | (2.39) | (1.87) | (1.27) | (1.30) |
| 21 | $2,435** | $2,108 | $1,624 | -$177 | -0.04 | -0.04 |
| | (2.22) | (1.49) | (0.65) | (0.07) | (0.43) | (0.45) |
| 22 | $1,092 | $191 | $3,082 | $2,297 | -0.03 | -0.03 |
| | (0.89) | (0.15) | (1.29) | (0.97) | (0.34) | (0.37) |
| 23 | $806 | $42 | $1,207 | $746 | 0.02 | 0.03 |
| | (0.51) | (0.02) | (0.44) | (0.26) | (0.18) | (0.27) |
| 24 | $2,792* | $2,072 | $4,692* | $3,780 | -0.05 | -0.04 |
| | (1.93) | (1.27) | (1.77) | (1.43) | (0.47) | (0.48) |
| 25 | $3,174** | $2,542 | $4,683 | $3,962 | -0.11 | -0.12 |
| | (2.16) | (1.59) | (1.64) | (1.38) | (1.12) | (1.21) |
| 26 | $3,812** | $3,213* | $2,713 | $2,523 | -0.07 | -0.08 |
| | (2.44) | (1.87) | (0.89) | (0.86) | (0.71) | (0.83) |
| 27 | $4,530*** | $4,210** | $2,361 | $2,126 | -0.09 | -0.10 |
| | (2.59) | (2.34) | (0.74) | (0.68) | (0.87) | (1.07) |
| 28 | $4,677*** | $4,198*** | $1,177 | $1,048 | -0.14 | -0.13 |
| | (2.93) | (2.59) | (0.31) | (0.28) | (1.40) | (1.43) |
| 29 | $3,075 | $2,327 | $8,198*** | $7,349*** | -0.07 | -0.06 |
| | (1.61) | (1.21) | (3.28) | (2.92) | (0.73) | (0.67) |
| 30 | $3,634* | $2,959 | $5,434* | $5,922** | -0.08 | -0.09 |
| | (1.76) | (1.46) | (1.88) | (2.18) | (0.76) | (0.89) |
| 31 | $1,439 | $1,021 | $8,923** | $11,029*** | -0.16 | -0.19* |
| | (0.65) | (0.45) | (2.39) | (4.12) | (1.37) | (1.71) |
| 32 | $2,875 | $2,938 | $5,055 | $7,939*** | 0.00 | -0.07 |
| | (1.02) | (1.00) | (1.23) | (2.65) | (0.03) | (0.54) |
| 33 | $4,496 | $4,848 | $4,719 | $7,932** | -0.12 | -0.23* |
| | (1.37) | (1.40) | (1.06) | (2.29) | (0.74) | (1.71) |
| 34 | $3,632 | $4,511 | $15,923*** | $15,545*** | -0.28* | -0.27* |
| | (0.96) | (1.13) | (3.89) | (3.66) | (1.78) | (1.81) |
| 35 | $435 | $861 | $8,093 | $8,516 | -0.21 | -0.21 |
| | (0.07) | (0.16) | (1.55) | (1.58) | (0.85) | (0.90) |
| No. of Person-Ages | 13,396 | 12,804 | 13,272 | 12,665 | 14,839 | 14,096 |

Notes: Robust t-statistics in parentheses. Estimates based on weighted regressions. See notes in Table 3 for covariates used in "All Covariates" regressions.
* significant at 10%; ** significant at 5%; *** significant at 1%.

**Table 7**
*Estimates of the Effect of Teenage Childbearing on Public Assistance Outcomes*

| | On AFDC while Age *a* | | On Food Stamps while Age *a* | | Annual Public Assistance Benefits at Age *a* | |
|---|---|---|---|---|---|---|
| Age of Mother | No Covariates | All Covariates | No Covariates | All Covariates | No Covariates | All Covariates |
| 18 | 0.001 | 0.001 | 0.06 | 0.02 | $1,048 | $1,430* |
| | (0.01) | (0.02) | (0.57) | (0.17) | (1.43) | (1.80) |
| 19 | 0.02 | 0.05 | 0.11 | 0.10 | $1,101 | $1,755*** |
| | (0.19) | (0.54) | (1.16) | (1.08) | (1.41) | (2.66) |
| 20 | 0.03 | 0.08 | 0.04 | 0.05 | $879 | $1,441*** |
| | (0.31) | (0.98) | (0.48) | (0.51) | (1.24) | (2.69) |
| 21 | -0.04 | 0.01 | 0.05 | 0.06 | $603 | $1,246** |
| | (0.46) | (0.12) | (0.57) | (0.67) | (0.77) | (2.24) |
| 22 | -0.04 | 0.01 | 0.07 | 0.08 | -$94 | $600 |
| | (0.45) | (0.07) | (0.89) | (0.91) | (0.10) | (0.81) |
| 23 | -0.11 | -0.07 | -0.12 | -0.09 | -$293 | $276 |
| | (1.23) | (0.81) | (1.29) | (0.96) | (0.33) | (0.42) |
| 24 | -0.07 | -0.02 | -0.11 | -0.08 | -$715 | -$258 |
| | (0.73) | (0.27) | (1.19) | (0.85) | (0.61) | (0.25) |
| 25 | -0.09 | -0.05 | -0.21** | -0.18* | -$548 | -$73 |
| | (1.01) | (0.57) | (2.13) | (1.84) | (0.56) | (0.08) |
| 26 | 0.03 | 0.06 | -0.22** | -0.20** | -$83 | $123 |
| | (0.31) | (0.76) | (2.18) | (2.13) | (0.09) | (0.14) |
| 27 | 0.06 | 0.09 | -0.14 | -0.12 | -$394 | $111 |
| | (0.73) | (1.24) | (1.46) | (1.36) | (0.42) | (0.14) |
| 28 | -0.05 | -0.03 | -0.07 | -0.03 | -$510 | -$51 |
| | (0.62) | (0.32) | (0.81) | (0.34) | (0.57) | (0.07) |
| 29 | -0.09 | -0.07 | -0.11 | -0.10 | -$1,174 | -$579 |
| | (1.02) | (0.81) | (1.17) | (1.08) | (1.05) | (0.67) |
| 30 | -0.07 | -0.05 | -0.11 | -0.11 | -$892 | -$576 |
| | (0.76) | (0.65) | (1.06) | (1.11) | (0.73) | (0.58) |
| 31 | -0.14 | -0.13 | -0.16 | -0.16 | -$2,595* | -$2,298 |
| | (1.37) | (1.24) | (1.38) | (1.35) | (1.74) | (1.60) |
| 32 | -0.06 | -0.09 | -0.10 | -0.16 | -$2,684 | -$3,323 |
| | (0.55) | (0.90) | (0.81) | (1.18) | (1.33) | (1.63) |
| 33 | -0.05 | -0.09 | -0.02 | -0.07 | $88 | -$461 |
| | (0.45) | (0.79) | (0.15) | (0.53) | (0.09) | (0.45) |
| 34 | -0.05 | -0.03 | -0.07 | -0.05 | -$598 | -$357 |
| | (0.46) | (0.33) | (0.46) | (0.41) | (0.45) | (0.30) |
| 35 | -0.04 | -0.05 | 0.09 | 0.08 | -$2,028 | -$2,079 |
| | (0.27) | (0.40) | (0.60) | (0.55) | (0.65) | (0.75) |
| No. of Person-Ages | 14,839 | 14,096 | 14,839 | 14,096 | 14,235 | 13,563 |

Notes: Robust t-statistics in parentheses. Estimates based on weighted regressions. See notes in Table 3 for covariates used in “All Covariates” regressions.
* significant at 10%; ** significant at 5%; *** significant at 1%.