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ABSTRACT

In this paper, we exploit a "natural experiment" associated with human reproduction to identify the effect of teen childbearing on subsequent educational attainment, family structure, labor market outcomes and financial self-sufficiency. In particular, we exploit the fact that a substantial fraction of women who become pregnant experience a miscarriage (spontaneous abortion) and thus do not have a birth. If miscarriages were purely random and if miscarriages were the only way, other than by live births, that a pregnancy ended, then women, who had a miscarriage as a teen, would constitute an ideal control group with which to contrast teenage mothers. Exploiting this natural experiment, we devise an Instrumental Variables (IV) estimators for the consequences of teen mothers not delaying their childbearing, using data from the National Longitudinal Survey of Youth, 1979 (NLSY79). Our major finding is that many of the negative consequences of not delaying childbearing until adulthood are much smaller than has been estimated in previous studies. While we do find adverse consequences of teenage childbearing immediately following a teen mother's first birth, these negative consequences appear short-lived. By the time a teen mother reachers her late twenties, she appears to have only slightly more children, is only slightly more likely to be single mother, and has no lower levels of educational attainment than if she had delayed her childbearing to adulthood. In fact, by this age teen mothers appear to be better off in some aspects of their lives. Teenage childbearing appears to *raise* levels of labor supply, accumulated work experience and labor market earnings and appears to *reduce* the chances of living in poverty and participating in the associated social welfare programs. These estimated effects imply that the cost of teenage childbearing to U.S. taxpayers is negligible. In particular, our estimates imply that the widely held view that teenage childbearing imposes a substantial cost on government is an artifact of the failure to appropriately account for pre-existing socioeconomic differences between teen mothers and other women when estimating the causal effects of early childbearing. While teen mothers are very likely to live in poverty and experience other forms of adversity, our results imply that little of this would be changed just by getting teen mothers to delay their childbearing into adulthood.

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1. Introduction

There is growing concern in the United States about the number of children born to teenage mothers and the proportion of these births that occur out of wedlock. In reaction to these trends, a report by the National Research Council concluded, "adolescent pregnancy and childbearing are matters of substantial national concern" (Hayes, 1987, p. ii). Part of the concern over teenage childbearing in the United States stems from its apparent adverse impacts on teenage mothers. Over the past several decades, social scientists have documented a strong association between the age at which a woman has her first child and the 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. The apparent adverse consequences of teenage childbearing for the educational attainment and skill development of the mother are particularly problematic in light of mounting evidence that the earnings prospects of unskilled workers have declined substantially over the last 20 years (see Bound and Johnson 1992; Juhn, Murphy, and Pierce 1993). As a result, adolescent mothers, and their children, are likely to spend a substantial fraction of their lifetimes in poverty (see Upchurch and McCarthy, 1990, and Card, 1981).

As has been highlighted in the current debate over reforming the U.S. welfare system, teenage mothers and their children constitute a growing proportion of those receiving various forms of public assistance. Government programs, such as the Aid to Families with Dependent Children (AFDC) program, have become a major source of support for the children of teenage mothers. Thus, the apparent costs of teenage childbearing are not borne solely by the teenage mothers, but also by U.S. taxpayers. Moreover, because teenage mothers work less over their lifetimes, these women will end up contributing little to the tax revenues needed to finance the governmental assistance they receive.

Unfortunately, estimating the causal effects of teenage childbearing is not straightforward. The evidence typically cited is based on the observed statistical association between early childbearing and the subsequent socioeconomic attainment of teenage versus non-teenage mothers. This evidence may reflect a causal influence of early childbearing on the outcomes of teenage mothers. Alternatively, it may simply reflect differences between the type of women who bear children as teens and those that avoid this. Presumably if the two groups of women are substantially different in other aspects that effect socioeconomic outcomes, then it is difficult to assess whether the birth or these other factors are responsible for the poorer outcomes of teenage mothers relative to other women.

At issue is how to obtain reliable estimates of the *causal* effects of teenage childbearing, i.e., the answer to the following question: What would have been the adolescent mother's (behavioral) outcome if she had not had a child as a teen? For obvious reasons, the use of experimental methods is simply not an option for conducting this evaluation. One cannot conduct trials in which teenage women are assigned or denied children via a randomizing mechanism. Consequently, the design for studies of the effects of teenage childbearing must be based on nonexperimental (or quasi-experimental) methods. Such methods are subject to the challenge of *selection bias*, i.e., that selective differences between women who have children as teenagers and those who do not bias estimate of causal impacts.

A number of econometric strategies have been used in the literature on the effects of teenage childbearing to deal with selection bias. The most common approach used is to control for observable characteristics (race, family structure, etc.) in order to account for the differences between teen mothers and the women to which they are compared. (See Waite and Moore (1978), Card and Wise (1978), Hofferth and Moore (1979), Upchurch and McCarthy (1990), Marini (1984), and McElroy (1996) as examples of this strategy.) This approach requires that, conditional on observed differences between teen mothers and the comparison groups used to measure counterfactual outcomes, births are random. Clearly, this is a strong assumption and, as we show below, it's validity is dubious. A second econometric approach models the joint process determining the woman's decision to bear a child as a teenager as well as the maternal outcome of interest (education, labor supply, poverty status, etc.). (See Ribar, 1992, Ribar, 1994, and Lundberg and Plotnick, 1989, as examples of this strategy.) Such studies typically rely on a rational choice framework that suggests that it is women with lower returns to work and education that have children as teens. Such models maintain a different, but equally strong, set of assumptions in order to identify the effects of early childbearing. A third approach employed in the past 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 Geronimous and Korenman, 1992, and Hoffman, Foster, and Furstenberg, 1993.) The validity of this method requires that, within a family, births are random to sisters. In the presence of changing circumstances within a family, such as the wealth of parents or what they learn about their children over time, this assumption will not be met.[1]

In an innovative paper, Grogger and Bronars (1993) make use of a "naturally-occurring" experiment to estimate causal effects of early childbearing.[2] In particular, they make use of the

[1] Hao, Hotz, and Jin (1999) develop a game-theoretic model of parental-daughter interactions over teenage childbearing decisions in which parents differentially treat older versus younger daughters so as to reduce the incidence of teen births. Their empirical tests reject the results that births are random across daughters within the same family.

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

fact that some teenage mothers have twins at their first birth rather than a single child. Since twins are beyond the control of the mother, twins can be viewed as randomly assigning some teenage mothers an "extra" child. By comparing teen mothers whose first birth is twins to those whose first birth is a single child, Grogger and Bronars estimate the effect of an extra child on a range of socioeconomic outcomes of teen mothers. As long as twins are random, the differences in outcomes between teen mothers with twins and those with a single birth measure the causal effect of an *extra child* without the intrusion of selection bias.

The causal effect identified in the work of Grogger and Bronars is somewhat different than in the research of others. Typically studies have focused on the effect on maternal outcomes of having at least one child as a teenager relative to having no children as a teenager. Grogger and Bronars argue that if an extra child lowers the outcomes of teen mothers that the effect of having any child as a teen is likely to be at least as large. Grogger and Bronars (1993) also make an important contribution when they distinguish the immediate consequences of an extra birth (0 to 3 years after the birth) from the longer-term consequences (10 to 13 years after the birth). An important finding from their work is that many of the negative consequences measured just after the birth of the extra child diminish over time.

In this paper, we exploit a different "natural experiment" associated with human reproduction to identify the causal effect of teenage mothers not delaying their childbearing on their subsequent educational attainment, family formation, labor market success, and poverty status. In particular, we exploit the fact that a substantial fraction of women who become pregnant experience a miscarriage (spontaneous abortion) and thus do not have a birth. If miscarriages were purely random and if miscarriages were the only way, other than by live births, that a pregnancy ended, then women, who had a miscarriage as a teen, would constitute an ideal control group with which to contrast teenage mothers. These women would randomly end their pregnancy with no child while women who had no miscarriage would randomly end their pregnancy with a child.[3] In reality, these ideal conditions do not hold. For example, pregnancies can be terminated by induced abortions, a choice that is elected by as many as 25% of teenage women who become pregnant (see Alan Guttmacher Institute, 1994). But, as argued below, we can still exploit the occurrence of spontaneous abortions (i.e., miscarriages) as an Instrumental Variable (IV) to identify the effect of teenage childbearing on the same socioeconomic outcomes considered by Grogger and Bronars (1993) and the other studies in this literature. Moreover, elsewhere[4] we show that this IV estimator is robust to a range of other sources of "contamination" of the "miscarriage-as-a-natural-experiment" used in this paper.

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 3, we lay out our use of miscarriages as a natural experiment and show how it can be used to form an Instrumental Variable 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 under reported in survey data. We discuss how we deal with these complications in this paper and report on findings from our previous work.

In Section 4, we present our basic findings of the effect of teenage childbearing on human capital development, family structure, poverty, and self-sufficiency. Our major finding is that

[3] Notice the contrast between the miscarriage and non-miscarriage groups more closely measures the contrasts between having a birth as a teenager versus not doing so than does the contrast of an "extra" child generated by the twins "natural experiment". However, both our work and Grogger and Bronars measure only the effect of teenage childbearing among women willing to give birth while the literature as a whole has concentrated on the effect in the population of all women. In Section 6 we investigate whether our results differ from the literature as a whole because of this sample selection.

many of the negative consequences are smaller than estimated elsewhere in the literature and are short lived. In fact, over the life course, teenage childbearing may actually aid human capital development. In Section 5, we compare our IV estimates with the most common statistical methodology employed in previous studies, namely, regression methods that control for differences in observable characteristics of early childbearers and women who did not become mothers as adolescents. These comparisons strongly suggest that the negative consequences previously attributed to teenage childbearing are, in fact, the result of the many unobserved disadvantages not accounted for by observable background characteristics. Finally, in Section 6, we use our estimates of the causal effects of teenage childbearing to estimate the cost to taxpayers of teenage childbearing. In Section 7, we offer some concluding comments on our analysis.

2. 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 14–21 years old in 1979. Thus, the women in our study were teenagers (i.e., from 13-17) during the years 1971 and 1982. Respondents have been interviewed each year since 1979. We make use of data through the 1993 interview 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 random sample of women in the U.S. population who were between the ages of 14-21 in 1979 along with supplemental samples of blacks, Hispanics, disad-

[4] See Hotz, Mullin and Sanders (1997).

vatnaged whites and women who were enlisted in the military in 1979. Some of our analysis is based on data from the random sample and the supplemental samples for blacks and Hispanics.[5] This sample—which we refer to as the *All Women Sample*—contains a total of 4,728 women.[6] We summarize the data for the All Women Sample in Table 1. Therein, we divide the sample into teen mothers and women who did not have births as a teenager and then compare estimates of a number of background characteristics taken for the most part when these women were age 14.[7] As is clear from this table, teen mothers come from much more disadvantaged backgrounds than do women who delay childbearing. 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 $9,512 versus $17,669 for their counterparts. Teen mothers had parents who were less educated. The fathers of women who later became teenage mothers completed an average of only 9 years of school versus 11 years of schooling for the fathers of other women. They were more likely to grow up in single-parent families (41 percent versus 22 percent). In addition they were more likely to have been in a family living on welfare when growing up (26 percent versus 15 percent) than women who did not have a child as a teen. Clearly, teenage mothers are different than women who delay childbearing into adulthood in many ways we can observe.

As noted in the Introduction, we exploit the random occurrence of spontaneous abortions (miscarriages) in order to devise a natural experiment for identifying causal effects of teenage childbearing on the subsequent socioeconomic attainment of teen mothers. To implement this

[5] Where appropriate, we use sampling weights provided with the NLSY79 to account for the use of the minority oversamples.

[6] The random sample and black and Hispanic supplementary samples from the NLSY79 contained 3,108, 1,067, and 751 women, respectively, for a total of 4,926 women. Of this total, we excluded data on 198 women for whom there was insufficient information to determine the timing of their pregnancies or the resolutions of their first pregnancies.

[7] 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.

natural experiment, we will focus on a subset of the women in the All Women Sample who reported that they experienced their *first* pregnancy prior to their eighteenth birthday. (We elaborate on the reasons for and implications of using this subsample below.) We refer to this subsample as the *Teen Pregnancy Sample*. This sample contains data on 980 women, of which 727 (74.2%) had a pregnancy that ended in a birth, 185 (18.9%) that ended in an induced abortion, and 68 (6.9%) that ended in a miscarriage. The characteristics of the latter sample are presented in Table 2. We briefly compare the characteristics of the All Women and Teen Pregnancy Samples drawn from the NLSY79. Note that the characteristics of teen mothers from the All Women Sample (see Table 1) are extremely similar to teenage mothers in the *Teen Pregnancy Sample* (see in Table 2).[8] However, the subsample of women who miscarry, which will form the basis for a comparison group for teen mothers in our natural experiment, are quite different from the women who are not teen mothers in our All Women Sample. (Other studies typically use the latter women as their comparison group for teen mothers.) Clearly the socioeconomic status of women who become pregnant as teens and miscarry (Table 2) is much lower than that of women who are not teen mothers (Table 1). Finally, it is worth noting that women who become pregnant as teens and who abort are very similar to women who do not become pregnant prior to age 18 in terms of pre-pregnancy socioeconomic status (Table 2). It is equally true that they are very similar in pre-pregnancy socioeconomic status to women who do not have a birth prior to age 18 (Table 1). This similarity is important for understanding why the estimates of causal effects of teenage childbearing derived from our natural experiment tend to differ from the previous literature. While previous studies have attempted to "adjust" for these *observable* differences when

[8] For teenage mothers, the difference between these two samples is only from the time dating convention used. Both samples contain women who had a child prior to their 18th birthday. The *Teen Pregnancy Sample* also includes as teenage mothers women who became pregnant just prior to their 18th birthday who carried the pregnancy to term after their 18th birthday.

estimating the causal effects of teenage childbearing with regression-based methods, such methods need not account for differences in other characteristics that we cannot measure with existing data. Therefore, we now turn to how our natural experiment based on the existence of randomly occurring miscarriages can be used to obtain an appropriate comparison group to measure the counterfactual outcomes for teen mothers.

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

The use of experimental designs to estimate causal effect, via random assignment of treatment status, eliminates the problem of selection bias by ensuring that treatment and comparison groups have, on average, the same outcomes in the absence of "treatment." In our context, a perfect control group would consist of women who are like teenage mothers in all ways except that they do not have a child. In this section, we discuss how we can exploit a *natural experiment* in which women who are pregnant as teenagers but experience spontaneous abortions, or miscarriages. We outline the assumptions required for this natural experiment to identify the causal effect of teenage childbearing and the consequences for our analysis if they are violated.

Consider the population of women who first become pregnant as adolescents and, thus, are at risk to become a teen mother. (We shall assume that adolescence is ages between 12 and 18.) Among this set of women, 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 miscarriages are beyond the control of women, while the births and abortions represent choices by those who did not experience a miscarriage.[9] Among women who experience miscarriages, we define a woman's *latent status* as how a woman would *choose* to resolve a pregnancy if she did

[9] We note that most miscarriages occur very early in a pregnancy so that they almost always occur before women

not experience the miscarriage. Let $D^* = B^*$ if a woman's latent-status is to have a birth and $D^* = A^*$ if her latent status is to have an abortion. Finally, let Y denote outcomes women experience as an adult age, i.e., 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 status.[10] The outcomes associated with latent statuses are hypothetical in that the econometrician can not observe a woman's latent type.

We define the causal effect of interest in this paper 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.[11] (In our context, the selected population is women who have their first births as a teenager.) Because of this selectivity in the population and the fact that we do not presume that pregnancies are random events, we can not make inferences about the causal effects of early childbearing for a randomly chosen teenage woman in the United States.[12] Nonetheless, identifying the causal effect defined in (1) is of interest for at least two reasons. First, as we will argue

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could choose to have an induced abortion.

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

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

[12] By analogy to the program evaluation literature, the causal effects we focus on are analogous to making inferences of 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 use-

below, β is more readily identified from available data than the more speculative causal effect of the consequences of a teen birth to a randomly selected woman from the population of all women, regardless of teen childbearing status. Second, identifying β enables one to assess the potential consequences of completely eliminating teenage childbearing in the U.S. Determining such effects provide 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. In Section 6, we use our estimates to provide a quantitative assessment of such potential benefits to U.S. taxpayers.

It is apparent from (1) that the problem of estimating β 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 outcome for women who miscarry, denoted by Y_M, which provides some information about the Y_{B^*}'s. 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 have a miscarriage as a teen, we cannot identify (or readily estimate) either $E(Y_{A^*})$ or P^* since

fulness of various treatment effect definitions.

they, too, require knowing the latent status of women who miscarry as teens.

However, under additional behavioral assumptions, we can achieve identification of β by exploiting the occurrence of miscarriages as a natural experiment. In particular, suppose that the following conditions hold:

(i) all miscarriages are random,
(ii) all fertility events are correctly reported,
(iii) having a miscarriage or abortion has the same effect on Y, since neither event results in a birth as a teenager,

If all miscarriages are random and all fertility events are correctly reported—i.e., assumptions (i) and (ii) hold—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, $P^* = P$.[13] Furthermore, if only the existence of a child affects outcomes—i.e., assumption (iii) holds—then, on average, the outcomes for women who have abortions will be equal to all women in the latent-abortion group. That is, $E(Y_{A^*}) = E(Y_A)$.[14] Under these conditions, $E(Y_{B^*})$ is equal to:

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

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

[13] This would be true if the fate of the fetus is 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.

[14] In the program evaluation literature this 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.

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

where $E(Y_{\sim M})$ is the average outcome for women who did not miscarry—since $E(Y_{\sim M}) \equiv PE(Y_B) + (1\text{-}P)E(Y_A)$—and $\tilde{B}$ and $\tilde{M}$ denote indicator variables equal to 1 if a women $D = B$ and M, respectively, and 0 otherwise.[15]

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

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

where $\widehat{Cov}(w_1, w_2)$ denotes the sample covariance between variables w_1 and w_2. Miscarriages ($\tilde{M}$) serve as an instrument for births ($\tilde{B}$). Thus, our use of the natural experiment of the occurrence of miscarriages to obtain the differences in outcomes by miscarriage status is equivalent to using miscarriages as an instrumental variable for births.

As noted above, the validity of the identification of β via (5), and of the associated IV es-

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

timator in (6), require that conditions (i)-(iii) hold. Epidemiological evidence suggests that (i) and (ii) do not hold and condition (iii) is a strong assumption about the influence of miscarriages on a woman's subsequent behavior. While the physiology of miscarriages implies that some miscarriages are truly random events,[16] some may be induced by pre- and within-pregnancy behaviors of women. In particular, epidemiological studies find that smoking and drinking during pregnancy, the use of an IUD at conception and conceiving very young, at less than 16 years of age, all increase the likelihood that a woman miscarries.[17] Such behaviors may be correlated with subsequent outcomes, *Y*, thereby compromising using miscarriages as a natural experiment (or an instrumental variable) for analyzing the causal effect of early childbearing births. Other factors, such as a woman's socioeconomic status, her nutrition, or her drug use, have not been shown to increase rates of miscarriage.[18]

There also is evidence that abortions and possibly miscarriages are underreported in survey data. Jones and Forrest (1992) find that induced abortions are underreported in the NLSY79—the data source used in this study—by comparing the incidence of abortions reported by NLSY79 respondents with data on numbers of abortions performed in the U.S. The latter data is gathered annually by the Alan Guttmacher Institute in a survey of abortion providers in the U.S. and is thought to provide reliable estimates of the incidence of abortions in the U.S. While the accuracy of reporting on miscarriages in survey data is more difficult to verify, data from

[16] Some miscarriages (spontaneous abortions) are the result of the abnormal formation of the chromosomes of a human fetus and occur at random. As a result of such abnormalities, the fetus is not viable and is expelled from the womb early in a pregnancy. See Klein, Stein and Susser (1989) for more on the physiology of spontaneous abortions.

[17] See Klein, Stein and Susser (1989) for a summary of this evidence.

[18] It should be noted that the effect of these factors on birth weight is well documented. See Klein, Stein and Susser (1989).

U.S. Vital Statistics suggests that this fertility event also is underreported in surveys.[19] As demonstrated in Hotz, Mullin and Sanders (1997), underreporting of such events can lead to bias when using the IV estimator proposed in (6).

A systematic assessment of the consequences of violations of conditions (i) through (iii) for estimating the causal effects of teenage childbearing is undertaken in Hotz, Mullin and Sanders (1997). Therein, these authors show that one cannot point identify the casual effect in (1)—and, thus, ensure the consistency of the IV in (6)—without knowledge of the latent-statuses of women who experience miscarriages. However, they do show that one can form non-parametric bounds on β, even when none of these conditions hold. Furthermore, these bounds are *tight* as defined by Horowitz and Manski (1995) and can be derived and non-parametrically estimated using auxiliary information to form lower bounds on the proportion of miscarriages which are random and upper bounds on the incidence of underreporting in surveys of abortion and miscarriage events. In their empirical investigation on a subset of outcomes examined here (educational attainment, annual hours of work and labor market experience. More importantly, the estimated bounds for the latter two outcomes turn out to be informative in that they are sufficiently tight to enable rejection of null hypotheses on the signs of the causal effects, such as the lower bound on the estimated effect of teenage childbearing on earnings is not less than zero. Thus, while the conditions needed to ensure that using miscarriages as an instrumental variable for teen births may not hold, any violations do not appear to bias the qualitative inferences drawn from estimates of causal effects of teenage childbearing on the adult outcomes of teen mothers.[20]

[19] U.S. Vital Statistics reports suggest that approximately 45% of pregnancies of teenage women end in births and 41% end in abortions. This implies that 14% of pregnancies to women in this age group end in miscarriages. As one can see from Table 2, the proportion of women in the NLSY79 who reported that their teenage pregnancy ended in a miscarriage is only 6.9%.

[20] We note that it is unclear whether it is empirically important that condition (i) is violated, i.e., that some miscarriages are not random. If miscarriages were random then characteristics unaffected by the miscarriage (race, etc.)

In the next section, we present three sets of IV estimates of the causal effects of teen births on a woman's adult outcomes. One set consists of the simple IV estimator defined in (6). Note that this estimator is equivalent to estimating the following regression equation,

$$Y_{ia} = \alpha_a + \beta_{1,a}\tilde{B}_i + \varepsilon_{ia}\,, \tag{7}$$

where Y_{ia} denotes the outcome for the i^{th} woman when she is age "a", ε_{ia} is a disturbance term and $\beta_{1,a}$ is the age-specific causal effect defined (1). Using $\tilde{M}_i$ to instrument $\tilde{B}_i$, we produce IV estimates, $\hat{\beta}_{1,a}^{IV}$, given in (6). We also present IV estimates that control for various sets of covariates, X_{ia}, via the regression function

$$Y_{ia} = \alpha_a + \beta_{2,a}\tilde{B}_i + \theta' X_{ia} + \varepsilon_{ia}\,, \tag{8}$$

and again using $\tilde{M}_i$ to instrument for $\tilde{B}_i$. We report IV estimates of $\beta_{2,a}$ for two sets of covariates: one that includes a set of behavioral factors known to be related to miscarriages and another set that includes these behavioral factors plus personal and family background characteristics of the i^{th} women measured prior to a woman's first teenage pregnancy. The first set includes variables, such as smoking, drinking and use of an IUD prior to the pregnancy,[21] that epidemiological studies have found to influence the incidence of miscarriages. We control for them to minimize potential bias in our IV estimators of causal effects that might arise because some miscar-

should in expected value be equal between women who miscarry and women who do not. Table 3 presents the same background characteristics as presented in Tables 1 and 2 except we now divide the Teen Pregnancy Sample into two groups—women who miscarried their teen pregnancy and women who did not. Examining the mean of the 9 pre-pregnancy background characteristics, in no case is the mean for women who miscarry statistically different than for women who do not miscarry.

[21] The NLSY79 records smoking and drinking behavior of teenagers. Unfortunately the data on contraceptive use is recorded only for pregnancies that end in births. Therefore we can only condition on smoking and drinking behavior but not on IUD use. This is unlikely to bias our results as the use of an IUD among teenagers is extremely rare. In 1988, of women less than age 18 who were sexually active on an on going basis 38% used the pill, 29% used condoms, 28% used no contraception and only 5% used any other form of birth control including the IUD (See AGI report, 1994). Given the relative risk of pregnancy from these contraceptive practices the proportion of teenage pregnancies that occurred while using an IUD is very small. Further it is not clear that IUD use would be correlated with adult outcomes such as educational attainment.

riages may have been behaviorally-induced. In the second set of covariates, we include the background characteristics listed in Tables 1 and 2 in order to improve the efficiency of the IV estimators of causal effects.

Because we have a small number of women who have miscarriages in our data, we also present a third set of IV estimates in Section 5 based on the following, more parsimonious, specification of the regression function in (8)

$$Y_{ia} = \alpha + \eta_1 a_i + \eta_2 a_i^2 + \ldots + \eta_K a_i^K + \beta_0 \tilde{B}_i + \beta_1 \tilde{B}_i \cdot a_i + \beta_2 \tilde{B}_i \cdot a_i^2 + \ldots + \beta_J \tilde{B}_i \cdot a_i^J + \theta' X_{ia} + \varepsilon_{ia}, \qquad (9)$$

where a_i denotes the adult age at which the outcome, Y, is measured and K and J are the orders of the age polynomials used for this particular outcome. In this specification, $\tilde{M}_i$ and its interactions with age polynomials are used to instrument for $\tilde{B}_i$ and its interactions with the woman's age. While the use of these interactions with age polynomial potentially distort the causal effect of teenage childbearing for outcomes at particular ages, we employ them in an attempt to increase the precision of our estimates of causal effects and the power of statistical tests on their significance.

Finally, for all of the regressions in which we pool data across different ages for the same woman, we correct for temporal dependence in the estimated standard errors of parameter estimates using the method of Huber (1967).

4. 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: (a) indicators of human capital accumulation; (b) sources of financial support, (c) measures of poverty; and (d) indicators of family structure. These are the outcomes that have been examined in previous studies of the effects of teenage childbearing on mothers. We distinguish between two types of effects of early childbearing on a teenage mother's subsequent socioeconomic status: permanent effects and those that represent temporary substitution of behaviors over the life course. Even if teenage childbearing raises the cost of some activities in the short-run, there is scope for substitution between activities at some point in time and over time. A key question we address is whether teenage mothers substitute activities in order to minimize the impact of a teenage birth on their lives.

Table 4 presents estimates of the long-term impact of teenage childbearing. The table presents the effect of teenage childbearing on maternal outcomes measured when the women is 28 years old, more than 10 years after the birth of her first child.[22] Column 1 of Table 4 presents the non-parametric IV estimates defined in (6). Column 2 presents estimates of effects that control for smoking prior to pregnancy, drinking prior to pregnancy and age at conception.[23] Column 3 contains IV estimates that also are for a number of background characteristics that may be correlated with maternal outcomes.[24] Finally, column 4 provides estimates based on the specification in (9) where age is entered as a polynomial. In general, the estimates are extremely similar across specifications.

While previous studies have concluded that teenage childbearing has negative effects on human capital development, we find little evidence to support this. We do find modest evidence that having a child as a teenager lowers the rate of high school completion. Our non-parametric IV estimate in Column 1 shows that teen childbearing lowers the probability of completing high

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[22] This is the oldest age at which all cohorts of women in the NLSY79 are observed.

[23] Ideally we would like to know the number of cigarettes and the amount of alcohol consumed during pregnancy since there is evidence that there is a dose-response in miscarriages to both factors. Unfortunately, the NLSY79 asks these specific questions only of women whose pregnancies ended in live births. Therefore we rely on information collected elsewhere in the survey as to whether the women smoked or drank alcohol at all as this information is collected on all women.

[24] The means of background characteristics used in the regression analysis are given in Table 2. Again these are used only to increase the precision of the estimates.

school by 11 percentage points, but this effect is not statistically significant. However, once we condition on background characteristics, we find that teen childbearing lowers the probability of completing high school by 15 or 16 percentage points, a result that is statistically significant at the 0.10 level.[25] In contrast, teenage childbearing *increases* the rate of completion of the General Equivalence Diploma (GED) substantially. Our non-parametric estimate suggests that teen childbearing *raises* the probability of completing a GED by 18 percentage points an effect that is strongly significant and one that suggests that teenagers do not receive less education but instead substitute the GED for a high school diploma. In fact, taking high school diplomas and GEDs together, we find that there is *no* significant causal effect of early childbearing on the probability that teen mothers obtain a high school level education. (Our point estimate is small and positive but statistically insignificant.) That is, teen mothers do not seem to receive less education; rather, they appear to substitute one form of high school completion for another.[26]

The differences in the estimated effects of teenage childbearing on these two alternative forms of high school completion are somewhat surprising in light of recent findings on the effects of the GED. Cameron and Heckman (1993) find that the value of the GED in the labor market is minimal, with its recipients earning no more than high school dropouts. But their analysis is only for *men*. More recent evidence, for *women*, find that the labor market returns for those with a GED are no different than those a high school diploma (see Cao, Stromsdorfer and Weeks, 1996). What remains to be determined is whether this result also hold for the subset of women who become teen mothers. While this paper does not address this issue, our findings for

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[25] As discussed, the rate of high school completion does not change after age 20. The point estimate of the impact of teenage childbearing on high school completion does not change after age 21 but since nearly all NLSY79 respondents are observed at age 21 the precision of the estimated impacts is greater at this age than at latter ages. Since other outcomes are likely to vary over the life cycle we estimate other outcomes at age 28.

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

the effects of early childbearing on the labor market outcomes of teen mothers, to which we turn below, are not inconsistent with the equivalence of GED and high school graduation effects found in Cao, Stromsdorfer and Weeks (1996).

Row 4 of Table 4 displays the estimated effects of early childbearing on the number of hours worked at age 28. We estimate that teenage mothers work 369 *more* hours at age 28 than they would have if they delayed childbearing. Row 5 reports that between the birth of the first child and age 28 teen mothers accumulate more than 2,600 hours *more* of labor market experience than they would if they delayed childbearing until adulthood. The effect of teenage childbearing on both hours worked at age 28 and the cumulative number of hours-worked by that age are statistically different than zero at the 0.05 level. Far from causing a life of idleness, teenage mothers work much more than they otherwise would have if they delayed their childbearing into adulthood. These estimated effects are not trivial in magnitude. On average, teenage mothers work approximately 1,000 hours per year at age 28 and 820 hours per year over their 20s. Our estimates suggest that delaying childbearing would lead to more than a 35% *decline* in hours worked at age 28 and *cumulatively would lead to almost three less typical work years* of labor market experience by age 28.[27] This additional work experience appears to have payoffs in terms of subsequent wage rates. Our non-parametric estimates indicate that teenage childbearing *raises* wages (of working women) by $4.32 per hour. This result is essentially unchanged after controlling for a large set of background characteristics. However, the impacts estimated from our regression that models the effects on wages as a polynomial in age suggests a more modest (and

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flexibility in study time.

[27] The specifications with age polynomials suggest that teenage childbearing raises hours worked at age 28 by 303 hours and cumulatively raises hours worked up to age 28 by 1732 hours. These estimates are somewhat lower than the non-parametric IV estimates but are still qualitatively large and still indicate that teenage childbearing does not lower but *raises* labor supply.

statistically insignificant) increase in wages of $1.63 per hour. In any case, there is no evidence that teenage childbearing harms the long-term employment or earnings potential of women although there is some ambiguity of whether it helps.

Recall that the estimated effects on these labor market outcomes are the result of comparing the behavior of women who have their first births as teens to those who are forced to delay their childbearing because they experienced a miscarriage. Studies of women's labor supply patterns typically show that women work less when their children are very young and more when the children get older. Consequently, the estimated effects of early childbearing on hours worked at age 28 reflect, in part, differences in the *timing* of the childbearing between teen mothers and the comparison group of women who had miscarriages. It is likely that women who began childbearing later in life will rejoin the labor force after completing their delayed childbearing and narrow the gap in hours worked at older ages. However, it is unlikely that any advantage of teen mothers in accumulated labor market experience will completely disappear. Many studies have shown that the presence of young children and wages are paramount to female labor force participation. For most women, especially those from disadvantaged backgrounds, do not have young children present age 30. Given that we find that the wages of teen mothers are higher at age 28, these wage gains are likely to persist to older ages because of the additional previous labor market experience teen mothers have acquired relative to what would have happened if they had delayed their childbearing.[28]

Row 7 of Table 4 provides estimates of the effect of teenage childbearing on the financial support available to teen mothers at age 28. At that age, teen mothers earned $9,269 a year more than they would have if they had delayed childbearing. Much of this earnings advantage is due to

[28] If the return to a high school degree is substantially higher than a GED then the wages of women who delay childbearing may be greater than those of teen mothers. This question remains unresolved.

the 369 more hours they worked at age 28. But as Row 6 reports there is also a substantial gap in the average wage paid to teen mothers. This is consistent with a human capital interpretation in which the 2,605 more cumulative hours worked is more valuable than the amount of human capital potentially lost by receiving a GED rather than a regular high school diploma. Estimates of the effect of teenage childbearing on two other major sources of support for poor families, earnings from a husband (Row 8) and public assistance (Row 9), are not sizeable or statistically different from zero at age 28. In fact, while not statistically significant, all point estimates indicate that at age 28 teenage mothers use less public aid than if they had delayed childbearing. Since welfare participation tends to fall with the age of a woman's children and the children of teenage mothers are, on average, older than they would be if these mothers delayed childbearing, it is not surprising that teenage mothers receive no more (or even less) public aid at older ages. The fact that support from a husband is unaffected by teenage childbearing is in large part due to the lack of an effect of teenage childbearing on the probability of being married at age 28 (Row 11).[29]

While we show below that there are important effects of teenage childbearing at early adult ages, it is unclear whether teenage childbearing has any permanent, or long run, effects on family size or family structure. Ten years after the first child of a teen is born, our point estimate suggests that a teen mother is likely to have 0.30 more children than if she delayed childbearing (Row 13) and this estimate is not statistically significant.[30] Our point estimates suggest that having a child as a teenager raises the probability of having any children by age 28 by 13 percentage

[29] In general, the regressions using age polynomials show similar results. The one exception is that while all nonparametric models indicate no effect of teenage childbearing on spousal income, the regressions using age polynomials suggest that teenage childbearing *raises* support from a spouse. This result is difficult to reconcile with the finding in all specifications that there is no effect of teenage childbearing on marriage at age 28.

[30] We investigate below whether this reflects that total family fertility of women who delayed childbearing is incomplete while teenage mothers have completed their families.

points (Row 10) relative to avoiding childbearing as a teen, but again this is statistically insignificant. In other words, our point estimate suggests only 13% of women who delayed childbearing would not have had at least one child by age 28 and we can not reject that the fraction of women with children is unaffected by teenage childbearing. This limits the scope for teenage childbearing to affect the incidence of single motherhood at age 28. In fact, while our point estimate suggests that teenage childbearing raises the probability of being a single mother by 5 percentage points (Row 12) at age 28, this effect is statistically insignificant. We can not reject that teenage childbearing does not effect the probability of being a single mother in the long run.

We next examine the effects of teenage childbearing on several indicators of the financial well-being of teen and their families. We examine three separate measures of financial well-being: whether the family's income falls below the federal poverty threshold (Row 14), whether they participate in the Federal food stamps program (Row 15), and whether they participate in the AFDC program (Row 16) when the mother is 28 years old. Based on our non-parametric estimator, not delaying childbearing until adulthood does not significantly affect the incidence of poverty or of the likelihood of receiving either form of public assistance. Based on the causal effects estimates derived from the age-polynomial interaction specification in (9), we find that teenage childbearing *lowers* AFDC participation by 4 percentage points, *lowers* food stamp participation by 15 percentage points and *lowers* the poverty rate by 14 percentage points when the mother is age 28. Since the poverty line is a function of family size and family earnings, that teenage childbearing lowers poverty is consistent with teenage childbearing increasing family size little while it raises earnings a great deal.

Tables 5 through 8 explore the effects of teenage childbearing over a woman's life course. These tables allow us to compare the effects of teenage childbearing in the short and long runs. In each table, we present our least restrictive model, the non-parametric model conditioning on no covariates and then the most restrictive model, the model with all covariates that allows the effect of teenage childbearing to vary as a polynomial function of age. In general, the point estimates, at most ages, are similar for the two specifications. However, the additional restrictions of the latter specification tend to yield smaller standard errors on the estimated coefficients.

Table 5 examines the effect of teenage childbearing on family structure. Columns 2 and 3 report the effect of teenage motherhood on the number of children in the women's family and Columns 4 and 5 contain estimates of the effect of teenage childbearing on the probability of having any children. Since all teenage mothers have children, Columns 4 and 5 can also be interpreted as the fraction of latent-birth women who, at each age, have had a child. What is clear is that the delay in fertility is quite short for the majority of latent-birth women. The non-parametric estimates suggest that at age 18 (by her 19th birthday), typically 1 to 1.5 years after a miscarriage, only 59% of latent-birth women remain without a child. At age 19, only 47% of latent-birth women remain without a child. However, a small minority of women—approximately 16 percent—delays childbearing more than 10 years.[31] Because of the generally high fertility among latent-birth women, the effect of teenage childbearing on the number of children in a woman's family is never more then 0.70 and declines to 0.30 in her late 20's. Teenage childbearing does seem to increase family size in the short-run, but, to a large degree, having births early in life is offset by having fewer births later in life.

In Table 4, we found little evidence of a long-run effect of teenage childbearing on single motherhood. Columns 8 and 9 of Table 5 provide estimates of the effect of teenage childbearing on single motherhood at all ages between 18 and 28. Our non-parametric results suggest that at

[31] This implies a lower bound of 4.4 years for the average delay in childbearing.

ages 18 and 19 being a teenage mother increases the chances of being a single mother by 32 percentage points and 30 percentage points respectively. However, there is no evidence that teenage motherhood effects the rate of marriage at any age (Columns 6 and 7). The higher rate of single motherhood among teen mothers is caused simply by their higher rate of motherhood. As latent-birth women who delayed childbearing begin having their children, they are as likely to have children outside of marriage as teen mothers are to be raising their children outside of marriage. The effect on single motherhood is strong in the late teen years but quickly dissipates. Further, it is entirely driven by the effect of teenage childbearing on the timing of fertility and not at all by the timing of marriage.

Table 6 presents estimates of the effect of teenage childbearing on hours worked. We argued earlier that, by age 28, teenage mothers are working substantially more hours than if they had delayed childbearing. In fact this pattern is detected much earlier in the life course. By their mid 20s, teenage mothers are working substantially more hours than their counterparts who delayed childbearing and the additional number of hours they work grows over their twenties and into their thirties. Our non-parametric (parametric) estimates suggest that at age 22 teenage mothers work 284 (175) more hours than women who delay childbearing. In their thirties, teenage mothers work closer to 400 (300) hours more per year than women who delay childbearing. By age 30 teenage mothers have accumulated nearly 3,000 (1,900) more hours of work experience. This is the result of teen mothers consistently working several hundred more hours per year than they would have if they had delayed their childbearing. There is little evidence that teenage childbearing ever lowers a teen mother’s wages. In the short run, there is no significant effect of teenage childbearing on wages and in the long run there is some weak evidence that teenage childbearing raises wages. The non-parametric estimates (Column 6) suggest that by her late 20’s teenage childbearing raises a teenage mother's wages by $3 to $5 per hour while the parametric estimates (Column 7) suggest a more modest rise of $1 to $2 per hour. The non-parametric estimates are usually statistically significant for ages between 26 and 30.

Teenage childbearing appears to lead to an increase in labor supply and labor market experience, which leads to a modest increase in wages. Together these factors raise the earnings of a teenage mother a great deal. Table 7 presents estimates of how teenage childbearing changes the sources of support for these mothers at different ages. At ages older than 19, both our non-parametric (Column 2) and parametric (Column 3) point estimates suggest that teen mothers earn substantially more than they would have if they had delayed childbearing. After age 24, our non-parametric (parametric) estimates suggest that teen mothers typically earn $6,000 to $9,000 ($5,000 to $7,000) more annually than if they had delayed childbearing. Usually these estimated effects are statistically significant.

There is no evidence that teenage mothers draw substantially less support from spouses. As discussed earlier, the chance of being married at any age is unaffected by teenage childbearing. Hence the husbands of teenage mothers would have to earn less than the husbands of women who delay childbearing if teenage childbearing is to lower the financial support from a husband. In fact, at older ages there is some evidence that teenage mothers derive substantially more support from their husbands than if they had delayed childbearing. We have no ready explanation for this finding, but it is consistent with teenage mothers being more attractive mates because of their apparently higher levels of human capital acquired through work experience.

Finally, it is worth noting that there is little support, at any age, that teenage childbearing increases the amount of financial support received in the form of public assistance. Our point estimates suggest that in their late teens and early twenties teenage mothers receive somewhat more public assistance than they would have if they delayed childbearing. However, in their late twenties and early thirties they receive substantially less public support than they otherwise would have if they had delayed childbearing.[32] In particular, the parametric estimates recorded in Column 7 indicate that, on average, teen mothers receive $800 to $1,000 more per year when they are ages 18 to 21 but then years later, at ages 28 to 30, receive $1,000 to $2,000 less per year than comparable women who delay childbearing to adulthood.

Finally, in Table 8, we present estimates of the effects of teenage childbearing on three measures of the poverty status of teen mothers, namely, the incidence of family earnings below the federal poverty threshold (Columns 2 and 3), participation in AFDC (Columns 4 and 5) and participation in Food Stamps (Columns 6 and 7). Given very small changes in family structure and positive changes in earnings it is not surprising that teenage childbearing does not seem to be associated with increases in poverty in either the short-run or long-run. In fact, in general our point estimates suggest that teenage childbearing somewhat reduces poverty and the use of public assistance at older ages. In interpreting these findings it is important to realize that at age 18, 45 percent of *women who are not teenage mothers* (latent-birth women) live in poverty and by age 20 over sixty percent of these women live in poverty. The poverty rate among these women are astoundingly high even in the absence of a teenage birth. Since many women who delay childbearing do so for only a few years, it is very likely that when these women have children they will be in circumstances very similar to those of the teenage mothers.

Our results for the effects of early childbearing on the use of public aid are similar to those on the incidence of family poverty. The parametric estimates on Food Stamp use (Column

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[32] When we restrict the age-polynomial teen-birth interaction in (9) to be cubic in age when estimating the effects of teenage childbearing on AFDC and food stamp payments, each term in the age polynomial is statistically significant. This suggests that public assistance payments to teenage mothers are larger than women who delay when teenage mothers are in their early 20’s but become smaller than women who delay when teenage mothers are in their late

7) suggest that while teenage childbearing may raise the use of Food Stamps at age 18 by 8 percentage points, the increased use of Food Stamps dissipates quickly. Ten years after the birth of her first child a teen mother's rate of Food Stamp use is actually 15 percentage points lower than comparable women who delay childbearing until adulthood. Interestingly, there does not seem to be an effect on AFDC participation (Column 5) at any age. This suggests that lower reliance on public aid for teen mothers later in life is largely a result of less Food Stamp use not more use of AFDC, the program targeted to single mothers. An Examination of Estimated Effects from IV Estimation and OLS Estimation

In this section we explore the differences between the empirical strategy we have employed to generate the above results with those used in previous analyses. One way of characterizing the differences across the methods is in how each forms a comparison group with which to compare the outcomes of teen mothers. The natural experiment we have exploited above makes use of data on women who became pregnant as a teen and uses those who had a miscarriage to compare with women who had a teen birth. The most prevalent strategy used in previous studies is to "regression adjust" for differences between teen and non-teen mothers so that the adjusted latter group is more comparable to the former, net of teenage childbearing for teen mothers. In this section, we analyze how these two groups differ and the consequences of these differences for estimates of the impact of teenage childbearing on the adult outcomes of mothers.[33]

Using data on women who experienced a miscarriage as a teen to estimate the causal effects of teenage childbearing differs from previous studies that employ the "control for observable differences" strategy differ in two important ways: the samples used and the instrumenting

20's.

[33] In analyses not reported here, we have also compared our natural experiment results using sister-pairs data in which sisters who did not have a teen birth are used as a comparison group for the outcomes of sisters that did have a teen birth. Such results are available from the authors upon request.

of teen births. With respect to samples, our natural experiment restricts the analysis to data on women who become pregnant as teens; studies that rely on the regression-adjustment approach typically use for all women, including both those who do and do not become pregnant as teens. Note that it is possible that a comparison group consisting of women who became pregnant as teens but did not have a teen birth is more comparable to teen mothers than all women, even without using miscarriages to instrument for births.[34] In Table 9, we present estimates of the effects of teenage childbearing for the same set of outcomes and the same age (28) as was presented in Table 4. Columns 1 and 2 of Table 9 give estimates of the teen birth effect based on ordinary least squares (OLS) estimation of the specification in (8). Recall that this specification includes a set of pre-pregnancy characteristics to control for pre-existing differences across the samples used in the estimation. The estimates in Column 1 are for the All Women Sample, i.e., they include teen mothers *and* women who did not have a birth (and possibly a pregnancy) as a teen. Column 2 displays the corresponding OLS estimates based on using the Teen Pregnancy Sample, i.e., women who did become pregnant as teens. Finally, Column 3 of Table 9 reproduces the IV estimates that controlled for the same set of covariates as the OLS regressions. (This third column is reproduced from Column 3 of Table 4.)

Comparing Columns 1 and 3, we see that restricting the sample to women who became pregnant prior to age 18, even without using an instrument for teen births, does result in estimates of the effects of teenage childbearing that are closer to the corresponding IV estimates.

[34] While not discussed in the text, we also examined whether the estimates and inferences about the effects of teenage childbearing were sensitive to the particular age criteria for inclusion in our sample of women who became pregnant as a teen. Recall that women were included in our analysis sample if they became pregnant prior to her 18th birthday. However, some women who first became pregnant prior to their eighteenth birthday gave birth just after their eighteenth birthday (within nine months). One small difference between our sample of teenage mothers and that in other studies is that we include these women as teenage mothers while other studies include only women whose birth occurred prior to their 18th birthday. We experimented with using different ages for this criteria, such as including women whose first pregnancy occurred before they reached their 19th birthday. With the exception of our estimates for receipt of a high school diploma, varying this age criteria had a negligible effect on our IV estimates or

That is, eliminating women who were never pregnant as teens appears to yield a comparison group that is more comparable to teenage mothers than can be obtained by using all women and trying to use regression adjustments to eliminate all of the pre-existing differences between the latter group of women and teen mothers. However, this sample restriction alone is not enough to make the OLS and IV estimates comparable.[35] For example, the OLS estimates (on a sample restricted to women who became pregnant prior to age 18) still imply that teenage childbearing *lowers* hours worked by 102 hours, *lowers* accumulated hours worked by 2,379 hours, *lowers* earnings by \$6,441 dollars, *lowers* wages by \$2.28 per hour and *raises* the poverty rate by 13 percent. For these particular outcomes, our IV estimates in Column 3 imply conclusions about the effects of teenage childbearing that are dramatically different. The IV estimates imply that, at age 28, teenage childbearing *raises* hours worked by 333 hours, *raises* accumulated hours worked by 2,029 hours, *raises* earnings by \$8,488 dollars and *lowers* the poverty rate by 12 percent.[36] For many of the other outcomes, the IV estimates imply that that teenage childbearing has either a much smaller negative impact or even a positive impact of teenage childbearing relative to the OLS estimates when the samples used are the same (i.e., consist of women who became pregnant prior to age 18).

To illustrate the differences between the IV and OLS estimates over the life cycle, we present, in Figures 1 through 5, predicted values for various outcomes of teen mothers implied by these two alternative estimation strategies. The predicted values denoted by diamonds trace

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the inferences derived form them.

[35] One exception is that the effect of teenage childbearing on the rate of high school completion is similar for the OLS and IV models on the sample of women who become pregnant prior to age 18 (-0.16) but the effect in the sample of all women is far more negative (-0.41). This occurs largely because the high school completion rate among teen mothers in the sample of women who were pregnant prior to their eighteenth birthday is higher than the rate among teen mothers in the all women sample.

[36] Regressions in which age is restricted to enter the model as a polynomial (columns 5 and 6) suggest the same difference between the OLS and IV estimates

out the actual outcomes for teenage mothers. The predicted counterfactual outcomes, based on OLS estimates derived from the All Women sample, are denoted triangles and the counterfactual predicted outcomes, based on IV estimates, are depicted with squares in the various figures.[37] Looking across these figures, one sees the dramatic differences in the inferences one would draw about the consequences of teenage childbearing, depending on the method used. While these differences, by themselves, do not definitely *prove* that using miscarriages to instrument for teen births is preferred to the methodology most commonly used in previous studies, such differences do prove two things: the method one uses matters greatly and that selection bias is a real issue when estimating the causal effects of teenage childbearing. Based on what we consider to be the compelling rationale for our miscarriages-as-a-natural-experiment provided in Section 3 above, we think there is a strong case for concluding that much of the previous literature has drawn seriously flawed inferences about the consequences of teenage childbearing in the U.S.

5. The Cost to Government of Teenage Childbearing

One of the concerns that has been expressed about the teenage childbearing “problem” in the U.S. is the cost it imposes on society through the heightened use of public assistance by teen mothers. Given the estimates of the effects of teen childbearing, and the fact that they are qualitatively different than those found in most previous studies, we re-examine, in this Section, these costs. Regardless of what method is used for estimating the effects of teenage childbearing, it is important to distinguish between what society spends on teen mothers in the form of public assistance, and the portion of these expenditures that can be *attributed* to the failure of these women to delay their childbearing. To make this distinction clear, we use our estimates of causal effects to estimate what the government spends each year on public assistance for women who

[37] The counterfactual outcomes are constructed by adding the treatment effect at each age implied by the OLS and

became mothers as teens and what they would save if these women delayed childbearing. We estimate both the direct costs that government incurs for teen mothers on public assistance programs as well as these costs *net* of the taxes that women who were teen mothers will pay at various stages of their lifetimes. Obviously, the taxes paid by teen mothers must finance more than just what they incur in expenditures on public assistance programs. But given our finding that the effect of early childbearing is to raise the labor market earnings of teen mothers at older ages, it is worthwhile to determine the extent to which the taxes these women pay "cover" the cost to government in public assistance over their lifetimes.

We attempt to calculate as comprehensive an estimate of the public assistance costs that are incurred by teen mothers as possible. From the NLSY79, we have data on annual benefits received from AFDC and Food Stamps, as well as the benefits from other social programs, including the Supplemental Security Income (SSI) and General Assistance (GA) programs. Another important form of public assistance for teen mothers is the medical care they and their children often receive under the Medicaid program.[38] While information is not available in the NLSY79, we also attempted to estimate the dollar value of the public assistance teen mothers receive in the form of medical care through this program.[39] We added these derived estimates for

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IV models in which age is entered as a polynomial.

[38] While the NLSY79 contains information on whether its respondents receive rental subsidies or subsidized housing, it does not contain information on the dollar value of these forms of assistance. Because we were unable to obtain reliable data with which to estimate the costs of housing subsidies, they are not included in our cost calculations.

[39] Using state aggregate data on the maximum monthly AFDC and food stamp benefits for a family of four (consisting of one adult and three children) and average monthly Medicaid expenditures for a family of this size in the years between 1984 and 1989, we regressed the Medicaid benefits against the sum of food stamp and AFDC benefits, controlling for a linear time trend to account for the rising level of expenditures on Medicaid relative to those for the other programs over this period. The results produce an estimate that in each month a typical family on public assistance receives $250 plus 0.193 times the benefits received (in 1993 dollars) from the AFDC and food stamp programs combined. Based on this formula, the average monthly Medicaid expenditure on a family of four, receiving the maximum allowable AFDC and food stamp benefits, would be $404. In 1993, the median state's maximum monthly AFDC benefit for a family of four was $435 and the maximum monthly food stamp allotment for such a family was $375 (see U.S. House of Representatives, 1994). Since the average monthly expenditure in 1993 for a family of four receiving assistance under the Medicaid program was $386 (U.S. House of Representatives, 1994),

Medicaid received to those for the other benefits teen mothers received at various ages and used this measure to estimate the causal effects of early childbearing for this measure of public assistance, using the instrumental variables regression methods described earlier.[40]

While not directly measured from NLSY79 data, we used the following strategy to estimate what teen mothers pay in federal, state and local sales and income taxes at each age of their early adulthood. We assumed that the average teenage mother faces a federal marginal tax rate of 15 percent and that she would pay an additional 8 percent of her income in state and local income and sales taxes. Using the resulting tax rate of 23 percent, we multiplied the annual earnings of teen mothers, as well as our estimates of the causal effects of early childbearing on annual earnings, to produce annual estimates of taxes paid by teen mothers and the additional taxes they would pay if they had delayed their childbearing.

To obtain estimates of the total annual costs and savings to government associated with teen motherhood, we also must estimate the total number of women in the U.S. population in 1996 who first became mothers as teens (i.e., had their first birth prior to age 18). We assumed that the number of early childbearers at each age, from 17 through 34, was equal to the number of women who became teen mothers for the first time in 1993. Based on the Current Population Survey, there were 175,259 new teen mothers in 1993.

Using these estimates, we simulated the total amount that all levels of government would have spent in 1996 on public assistance for women who became mothers as teens, what these costs would be net of the taxes government collects from these women, and what the gross and net *savings* would be in these costs if all of these women had delayed their childbearing. Our estimates are presented in Figure 6. We estimate that each year government spends $11.3 billion

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the above estimation procedure appears to yield reasonable estimates.

(in 1996 dollars) on AFDC, Food Stamps, Medicaid and other forms of public assistance for women, ages 17 through 34, who began motherhood as teens. To put this in perspective, this expenditure represents 6 percent of total expenditures on the AFDC, Food Stamps and Medicaid programs in the U.S. in 1993[41] and amounts to an annual expenditure of $3,596 per woman. While these costs to government are substantial, we find that they are offset, in part, by the taxes that women who bore their first child as a teenager would be expected to pay. In fact, the total annual public assistance costs of early childbearers, net of the taxes they pay, is $2.1 billion, which amounts to a net cost, per woman, of $665. Nonetheless, this is a substantial net governmental outlay to women who chose not to delay their childbearing.

While substantial, most of these costs that U.S. taxpayers incur for women who began motherhood at early ages is not attributable to the failure to delay their childbearing. Taxpayers would save virtually nothing if these women had delayed their first births by 4 years on average. In fact, we estimate that the total annual expenditures on public assistance would increase slightly, rising by $1.3 billion, if all of these women had delayed their childbearing. Moreover, the *net* (of taxes) annual outlays by government for cash-assistance and in-kind transfers to these women would actually *increase* by 35 percent, or $3.9 billion. This increase in annual net expenditures associated with delaying childbearing would amount to over $1,250 per teen mother (a $400 annual increase in public aid and the remainder from forgone tax revenue). The fact that getting teen mothers to delay their childbearing results in additional costs to government, rather than savings, is a direct consequence of the finding that teenage childbearing raises rather than lowers labor market earnings in the long-run. We found that teen mothers would earn less over

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[40] The estimates for this regression are available from the authors.

[41] In 1993, the total expenditures, in terms of 1996 dollars, for the AFDC, Food Stamp and Medicaid programs were $27.3 billion, $28.4 billion and $197.1 billion, respectively (U.S. House of Representatives, 1994).

their lifetimes if they were forced to delay their first births. This loss in earnings translates to a reduction in taxes paid by these women and an increase, rather than decrease, in the net costs to government associated with the postponement of motherhood.[42]

6. Conclusion

In this study, we have used an alternative strategy to estimate the causal effects associated with teenage childbearing in the U.S. In particular, we have employed a natural experiment to obtain a more comparable, and plausible, comparison group with which to derive estimates of counterfactual outcomes for teen mothers, i.e., what would have happened to these women if they had postponed their childbearing. Our results suggest that much of the "concern" that has been registered concerning teenage childbearing, at least based on its consequences for the subsequent attainment of teen mothers, is misplaced. 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 the poor circumstances than the early childbearing of these women. Furthermore, our estimates suggest that simply delaying their childbearing would not greatly enhance their educational attainment, subsequent earnings, or 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

[42] To assess the robustness of our findings concerning the various cost estimates displayed in Figure 6, we recalculated all of our cost estimates, varying both the ranges of ages of teen mothers used to calculate these costs (estimates also were derived using women, aged 17 through 40 and aged 17 through 30) and the tax rate used to derive tax revenue estimates (we also used a rate of 15 percent). While the magnitudes of the various costs vary across these alternative assumptions, the general conclusions did not. Government spending on public assistance would not have declined and net spending would have increased, even if society had been able to get all the women who had teen births to postpone their childbearing until they were adults.

delayed their childbearing. But, 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 the birth of their first child, this "negative" consequence of teenage childbearing is not a permanent one, in that teen mothers use less public aid in their late 20's as their earnings rise and their children age.

Taken together, the results presented in this paper 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 the findings. We have considered only a limited range of potential consequences, and costs, of teenage childbearing. Furthermore, the findings from one study cannot be considered as conclusive. However, we note that our findings, at least for many of the socioeconomic outcomes considered in this paper, are consistent with the estimated effects of teenage childbearing found in the work of Geronimus and Korenman (1992) and Grogger and Bronars (1933). Taken together with our work, this evidence raises serious doubts about the extent and nature of teenage childbearing as a "social problem" in the U.S. and, more importantly, on the view that getting these women to postpone their childbearing will improve the socioeconomic attainment of teen mothers in any substantial way.

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Figure 1: Estimates of the Effect of Teenage Childbearing on Annual Hours of Work

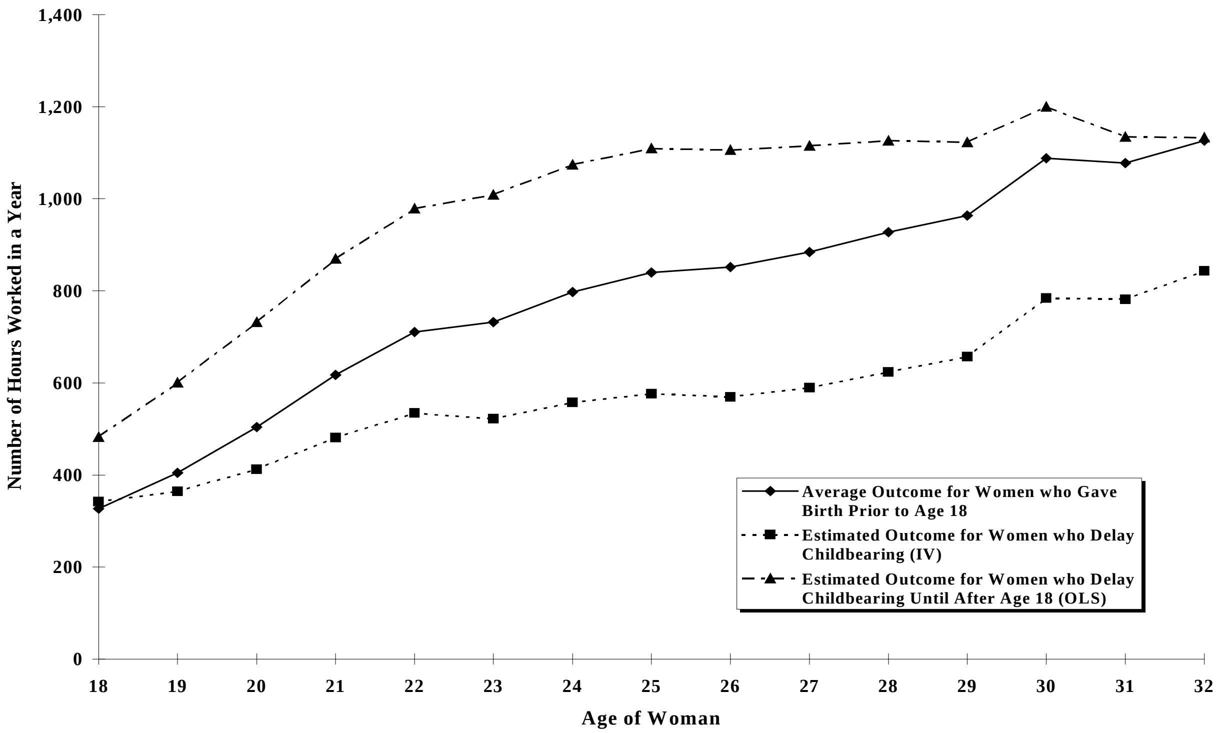

Figure 2: Estimates of the Effect of Teenage Childbearing on Annual Earnings

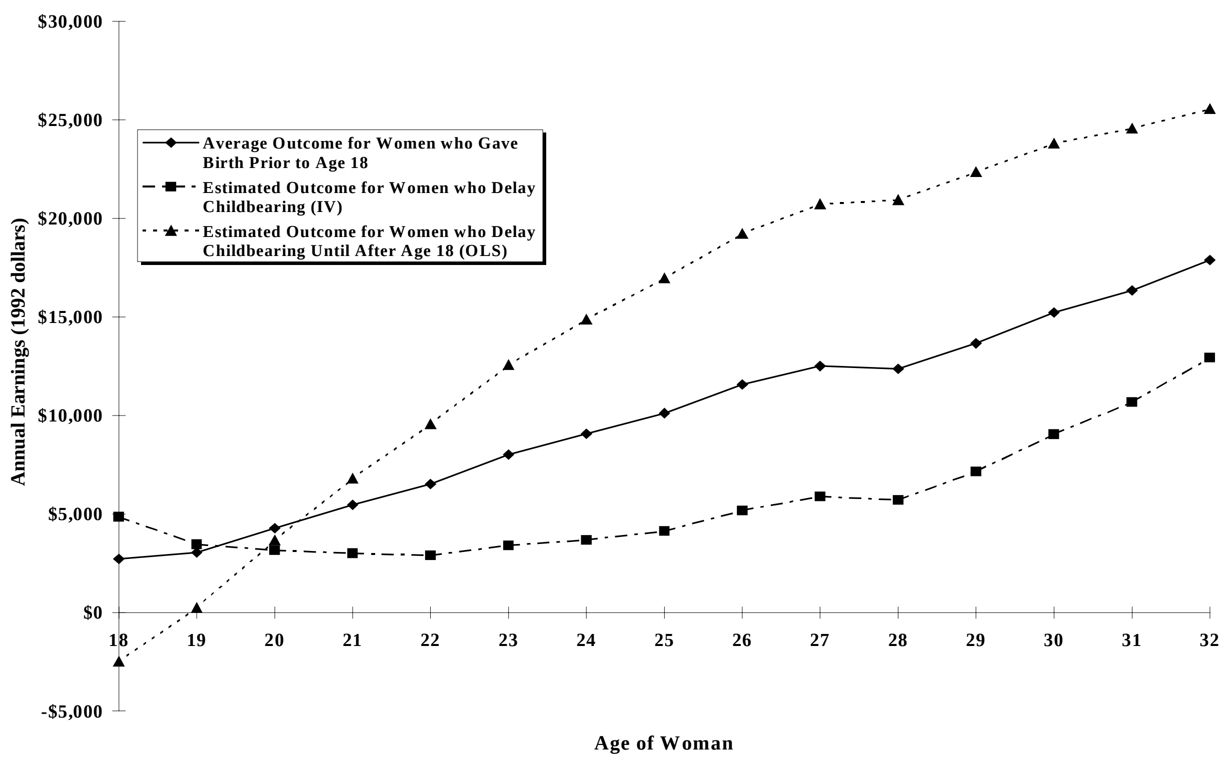

Figure 3: Estimates of the Effect of Teenage Childbearing on the Probability of Being Married at a Given Age

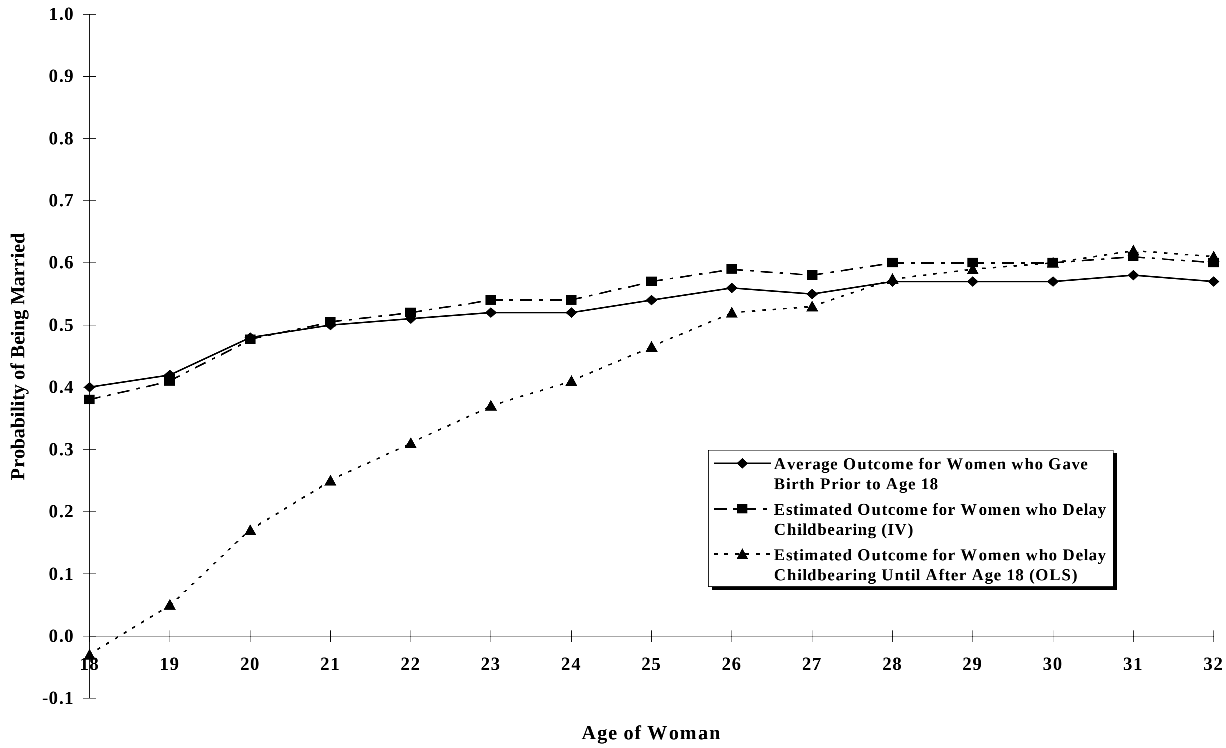

Figure 4: Estimates of the Effects of Teenage Childbearing on the Probability of Being a Single Mother at a Given Age

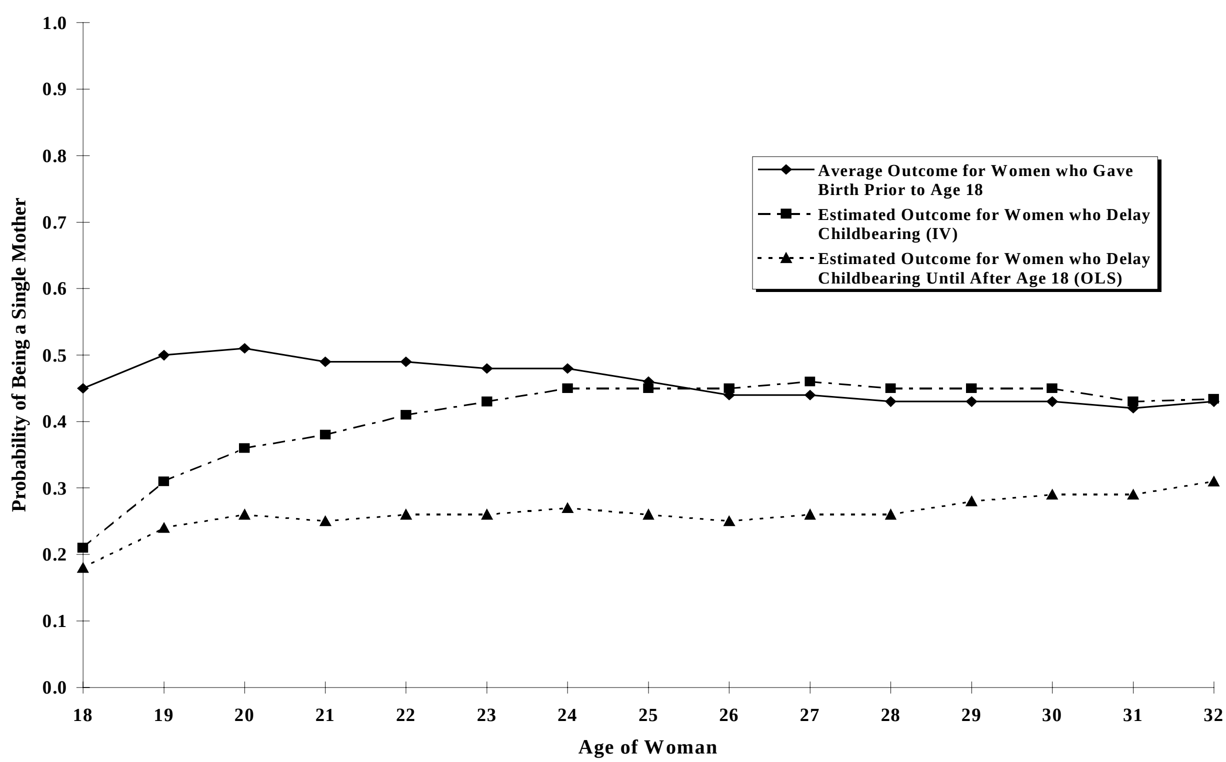

Figure 5: Estimates of the Effect of Teenage Childbearing on the Annual Benefits Received from AFDC and Food Stamps

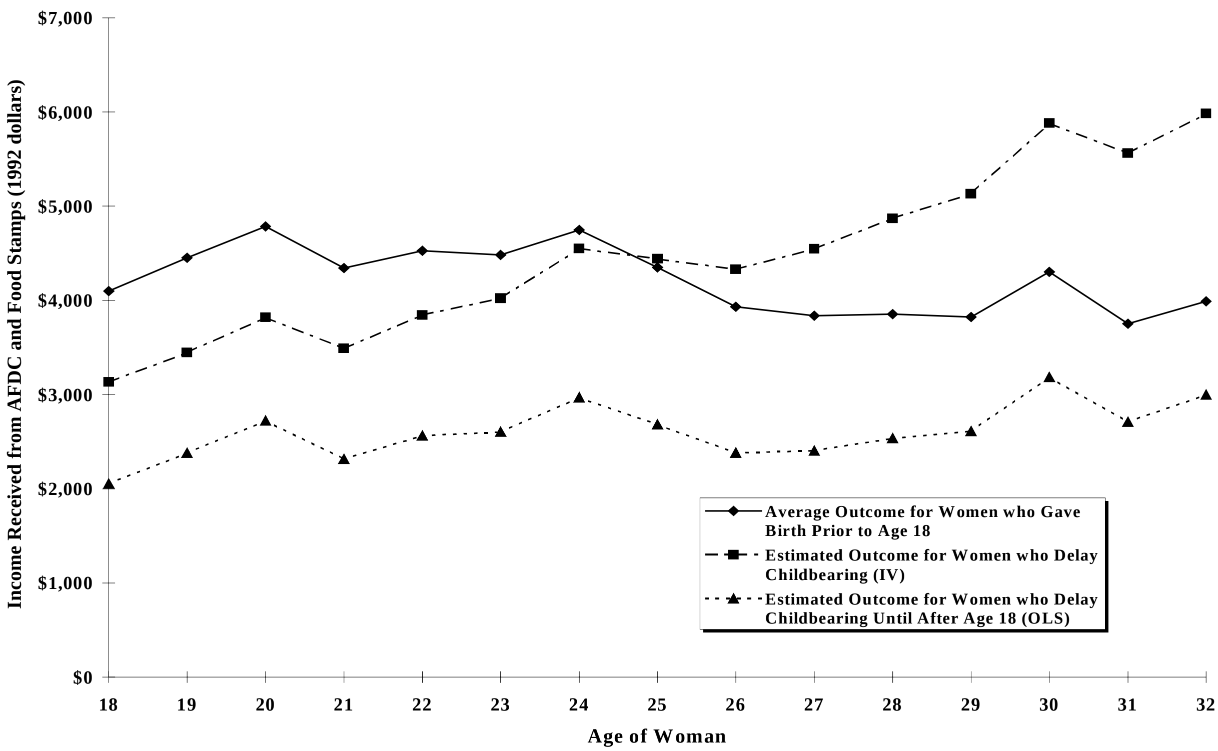

Figure 6: Total Annual Government Expenditures on Teen Mothers for AFDC, Food Stamps and Medicaid and Savings to U.S. Taxpayers if Teen Mothers Delayed Their Childbearing
(Data Source: NLSY79 and U.S. Vital Statistics)

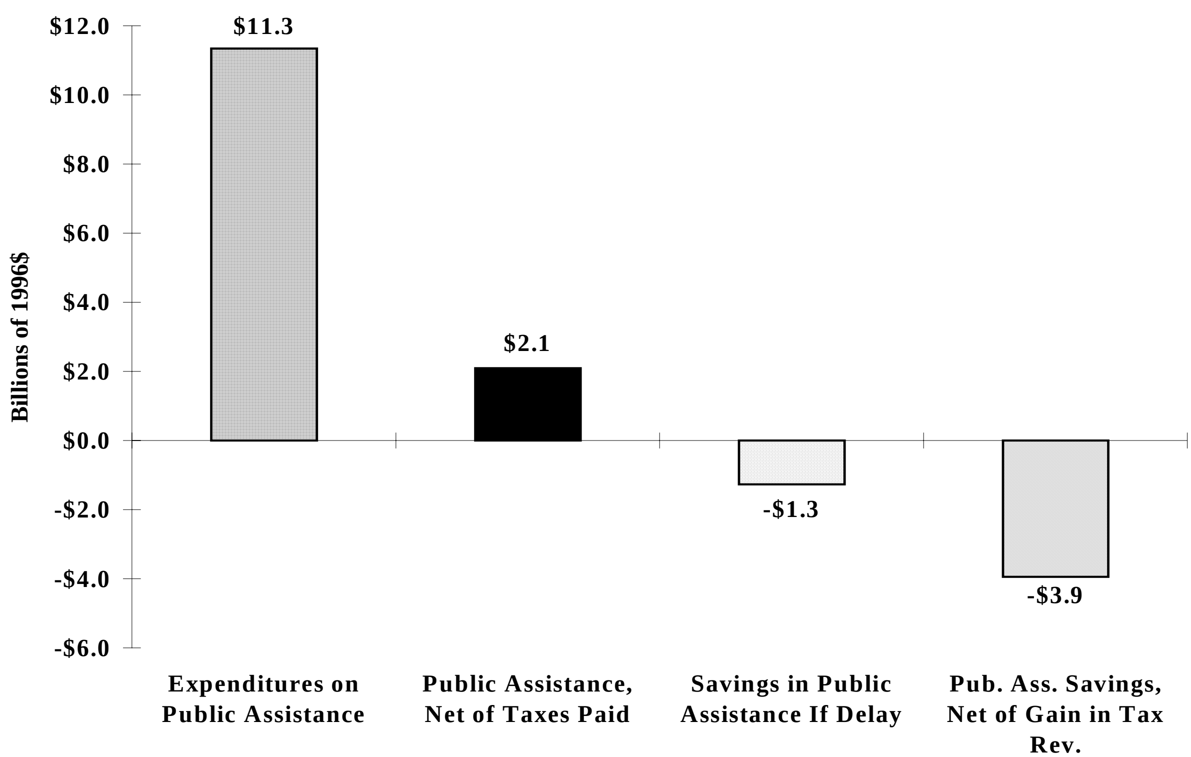

Table 1: Background Characteristics of Teenage Mothers and Women Who Delayed Childbearing until after Age 18
(Data Source: NLSY79, *All Women Sample*)

Characteristic	*Teenage Mothers* Mean	*Teenage Mothers* Standard Deviation	*Not Teenage Mothers* Mean	*Not Teenage Mothers* Standard Deviation
Black	0.553	0.497	0.279	0.448
White	0.223	0.417	0.517	0.500
Hispanic	0.223	0.417	0.204	0.403
Family on welfare in 1978	0.268	0.443	0.153	0.360
Family income in 1978	$12,146	$18,621	$29,200	$32,115
In female-head household at age 14	0.275	0.447	0.160	0.366
In intact household at age 14	0.593	0.491	0.782	0.413
Mother's education	8.335	4.103	10.391	3.958
Father's education	6.592	5.212	9. 596	5.290
AFQT score[a]	20.262	18.718	41.187	27.615
Number of Observations	564		4,164	

[a]Armed Forces Qualifying Test Score.

Table 2: Background Characteristics of Teenage Women by Pregnancy Outcomes Prior to Age 18
(Data Sources: NLSY79, *All Women and Teen Pregnancy Sample*)

Characteristic	*Not Pregnant Before 18*[1]		*Pregnant Before 18*[2]		*First Pregnancy Before 18 ended in Birth*[2]		*First Pregnancy Before 18 ended in Abortion*[2]		*First Pregnancy Before 18 ended in Miscarriage*[2]	
	Mean	*Standard Deviation*	*Mean*	*Standard Deviation*	*Mean*	*Standard Deviation*	*Mean*	*Standard Deviation*	*Mean*	*Standard Deviation*
Black	0.267	0.442	0.483	0.500	0.527	0.499	0.324	0.468	0.441	0.497
White	0.530	0.499	0.298	0.457	0.246	0.431	0.503	0.500	0.294	0.456
Hispanic	0.203	0.402	0.219	0.414	0.227	0.419	0.173	0.378	0.265	0.441
Family on welfare in 1978	0.150	0.357	0.229	0.420	0.261	0.439	0.103	0.304	0.221	0.415
Family income in 1978	$30,060	$32,396	$16,098	$23,681	$13,472	$20,319	$27,134	$32,742	$14,149	$18,537
In female-headed family at age 14	0.153	0.360	0.251	0.433	0.259	0.438	0.195	0.396	0.309	0.462
In intact household at age 14	0.791	0.407	0.637	0.481	0.622	0.485	0.714	0.452	0.588	0.492
Mother's education	10.464	3.956	8.930	4.083	8.450	4.082	10.676	3.862	9.309	3.261
Father's education	9.745	5.273	7.294	5.290	6.766	5.136	9.735	5.021	6.294	5.711
AFQT score[a]	42.343	27.849	24.786	21.307	21.374	19.404	37.489	23.627	26.385	20.658
Number of Observations	3,748		980		727		185		68	
% of those Pregnant before Age 18					74.2%		18.9%		6.9%	

[1]Data from *All Women Sample*.
[2]Data from *Teen Pregnancy Sample*.
[a]Armed Forces Qualifying Test Score.

Table 3: Background Characteristics of Women Pregnant Prior to Age 18 and Women Whose Pregnancy Ended in a Miscarriage
(Data Source: NLSY79, *Teen Pregnancy Sample*)

Characteristic	***Pregnant Before 18***		***Pregnant Before 18 ended in Miscarriage***	
	Mean (Uweighted)	***Standard Error***	***Mean (Uweighted)***	***Standard Error***
Black	0.48	0.015	0.44	0.060
White	0.29	0.012	0.29	0.055
Family on Welfare in 1978	0.22	0.013	0.22	0.050
Family Income in 1978	\$30,165	\$1,095	\$26,003	\$2,994
In Female Headed Family at age 14	0.25	0.013	0.30	0.055
In Intact Family at Age 14	0.63	0.015	0.59	0.059
Mothers Education	9.78	0.105	10.04	0.255
Fathers Education	9.69	0.137	9.72	0.628
AFQT Score	24.7	0.69	26.3	2.57

Table 4: Change in Outcomes Due to Not Delaying Childbearing Measured at Age 28
(T-Statistics in Parentheses)

		Estimates Conditioning on:		
Outcomes	*No Covariates*	*Correlates of Miscarriage*[1]	*All Covariates*[2]	*All Covariates with Polynomials in Age*
Human Capital				
1. High School Diploma (HSD) (Age 21)	-0.11	-0.11	-0.16	-0.15
	(1.03)	(1.10)	(1.73)	(1.67)
2. General Equivalence Degree (GED)	0.18	0.19	0.18	0.19
	(3.34)	(3.46)	(3.42)	(4.02)
3. HSD or GED	0.08	0.08	0.03	0.05
	(0.77)	(0.76)	(0.38)	(0.48)
4. Hours Worked	368.90	390.85	330.67	303.64
	(1.94)	(2.02)	(1.78)	(1.71)
5. Cumulative Hours Worked	2605.41	2774.30	2029.62	1732.10
	(2.26)	(2.40)	(1.72)	(1.36)
6. Wages	4.34	4.34	4.22	1.63
	(2.15)	(2.11)	(2.16)	(1.30)
Sources of Support				
7. Dollars from Earnings	9269.65	9667.12	8488.79	6660.43
	(3.08)	(3.10)	(2.92)	(2.47)
8. Dollars from Spouse	1269.85	1314.03	2505.17	7511.76
	(0.19)	(0.20)	(0.38)	(1.71)
9. Dollars from AFDC and Food Stamps	-372.27	-444.64	-515.60	-1017.61
	(0.44)	(0.54)	(0.62)	(1.27)
Family Structure				
10. Proportion with Children	0.13	0.13	0.13	0.12
	(1.53)	(1.46)	(1.51)	(1.27)
11. Proportion Married	-0.08	-0.07	-0.07	-0.03
	(0.90)	(0.83)	(0.85)	(0.38)
12. Proportion Unmarried with Children	0.05	0.04	0.03	0.02
	(0.56)	(0.44)	(0.38)	(0.30)
13. Number of Children	0.30	0.28	0.27	0.35
	(1.20)	(1.11)	(1.03)	(1.34)
Measures of Poverty				
14. Proportion in Poverty	-0.11	-0.11	-0.12	-0.14
	(1.00)	(1.01)	(1.22)	(2.06)
15. Proportion on Food Stamps	-0.10	-0.11	-0.09	-0.15
	(1.01)	(1.12)	(0.89)	(1.95)
16. Proportion on AFDC	-0.04	-0.05	-0.06	-0.04
	(0.49)	(0.62)	(0.63)	(0.55)

1. These include dummy variables for smoked prior to pregnancy, drank prior to pregnancy and pregnancy prior to age 16.
2. These include dummy variables for ethnicity (black and Hispanic), living in a female headed family at age 14, living in an intact family at age 14, and controls for missing values. The woman's family income in 1978, her mother's education and her fathers education were entered linearly.

Table 5: Estimates of the Effect of Teenage Childbearing on Family Structure
(Dollar figures in 1994 Dollars, T-Statistics in Parentheses)

	Number of Children		***Probability of Having a Child***		***Probability of being Married***		***Probability of being Unmarried with a Child***	
Age of Mother	*No Covariates*	*All Covariates with Poly-nomials in Age*	*No Covariates*	*All Covariates with Poly-nomials in Age*	*No Covariates*	*All Covariates with Poly-nomials in Age*	*No Covariates*	*All Covariates with Poly-nomials in Age*
18	0.66	0.83	0.59	0.56	0.02	0.02	0.32	0.24
	(5.07)	(5.63)	(6.31)	(6.20)	(0.14)	(0.23)	(4.16)	(3.87)
19	0.70	0.71	0.47	0.47	-0.08	0.01	0.30	0.19
	(5.09)	(5.42)	(4.73)	(5.67)	(0.71)	(0.15)	(5.26)	(3.33)
20	0.53	0.61	0.33	0.40	0.02	0.00	0.12	0.15
	(3.04)	(4.58)	(3.29)	(4.97)	(0.19)	(0.04)	(1.43)	(2.54)
21	0.50	0.53	0.23	0.33	0.02	0.00	0.06	0.11
	(2.76)	(3.58)	(2.29)	(4.18)	(0.18)	(0.06)	(0.68)	(1.76)
22	0.56	0.46	0.25	0.27	-0.02	-0.01	0.04	0.08
	(2.86)	(2.77)	(2.54)	(3.42)	(0.23)	(0.15)	(0.48)	(1.15)
23	0.46	0.40	0.18	0.22	0.01	-0.02	-0.03	0.05
	(2.25)	(2.19)	(1.94)	(2.75)	(0.10)	(0.21)	(0.27)	(0.69)
24	0.42	0.36	0.20	0.18	-0.01	-0.02	0.02	0.03
	(1.87)	(1.81)	(2.14)	(2.22)	(0.05)	(0.26)	(0.19)	(0.35)
25	0.48	0.34	0.19	0.15	-0.08	-0.03	0.08	0.01
	(2.09)	(1.56)	(2.01)	(1.81)	(0.82)	(0.30)	(0.95)	(0.10)
26	0.38	0.33	0.16	0.13	0.00	-0.03	0.02	-0.01
	(1.60)	(1.42)	(1.82)	(1.52)	(0.03)	(0.33)	(0.24)	(0.09)
27	0.29	0.33	0.16	0.12	-0.10	-0.03	0.10	-0.02
	(1.13)	(1.35)	(1.82)	(1.35)	(1.09)	(0.35)	(1.23)	(0.22)
28	0.30	0.35	0.13	0.12	-0.08	-0.03	0.05	-0.02
	(1.20)	(1.34)	(1.53)	(1.27)	(0.90)	(0.38)	(0.56)	(0.30)
29	0.32	0.38	0.12	0.12	-0.07	-0.03	0.01	-0.03
	(1.16)	(1.37)	(1.37)	(1.29)	(0.74)	(0.39)	(0.09)	(0.33)
30	0.34	0.43	0.12	0.14	-0.01	-0.03	-0.05	-0.02
	(1.13)	(1.43)	(1.30)	(1.37)	(0.08)	(0.40)	(0.44)	(0.30)
31	0.42	0.49	0.13	0.16	-0.02	-0.03	-0.03	-0.02
	(1.34)	(1.49)	(1.26)	(1.49)	(0.19)	(0.40)	(0.24)	(0.21)
32	0.67	0.57	0.15	0.20	-0.02	-0.03	-0.02	0.00
	(2.00)	(1.54)	(1.32)	(1.64)	(0.15)	(0.36)	(0.15)	(0.05)

Table 6: Estimates of the Effect of Teenage Childbearing on Hours Worked and Wages
(Dollar figures in 1994 Dollars, T-Statistics in Parentheses)

	Number of Hours Worked		***Cumulative Number of Hours Worked***		***Hourly Wage Rate (1994 Dollars)***	
Age of Mother	*No Covariates*	*All Covariates with Polynomials in Age*	*No Covariates*	*All Covariates with Polynomials in Age*	*No Covariates*	*All Covariates with Polynomials in Age*
18	-36.75	-15.17	-229.46	-909.16	-0.36	-1.66
	(0.24)	(0.10)	(0.70)	(1.19)	(0.20)	(1.11)
19	47.42	40.54	-149.68	-516.62	2.43	-1.07
	(0.30)	(0.32)	(0.44)	(0.84)	(2.39)	(0.88)
20	236.13	90.95	105.25	-152.61	0.47	-0.53
	(1.56)	(0.81)	(0.28)	(0.28)	(0.31)	(0.52)
21	223.60	136.07	386.06	182.87	1.15	-0.06
	(1.34)	(1.22)	(1.01)	(0.33)	(0.48)	(0.06)
22	284.19	175.89	670.25	489.80	-0.60	0.36
	(1.86)	(1.48)	(1.46)	(0.81)	(0.28)	(0.39)
23	130.58	210.42	834.93	768.20	-0.91	0.72
	(0.72)	(1.63)	(1.48)	(1.13)	(0.33)	(0.76)
24	363.68	239.66	1204.18	1018.05	2.34	1.02
	(2.06)	(1.70)	(1.76)	(1.33)	(1.36)	(1.03)
25	411.94	263.59	1559.21	1239.37	1.73	1.26
	(2.30)	(1.74)	(1.93)	(1.44)	(0.60)	(1.21)
26	338.83	282.24	1900.05	1432.15	2.68	1.45
	(1.90)	(1.75)	(2.00)	(1.47)	(1.61)	(1.32)
27	396.88	295.59	2287.49	1596.40	4.78	1.57
	(2.12)	(1.74)	(2.16)	(1.44)	(2.15)	(1.35)
28	368.90	303.64	2605.41	1732.10	4.34	1.64
	(1.94)	(1.71)	(2.26)	(1.36)	(2.15)	(1.30)
29	52.39	306.40	2263.10	1839.27	3.57	1.64
	(0.21)	(1.65)	(1.63)	(1.25)	(1.21)	(1.18)
30	146.21	303.86	3046.87	1917.89	4.89	1.59
	(0.58)	(1.55)	(1.86)	(1.12)	(1.95)	(1.00)
31	407.72	296.03	2584.32	1967.98	-8.08	1.48
	(1.75)	(1.42)	(1.27)	(0.98)	(1.29)	(0.79)
32	469.40	282.90	2210.81	1989.54	3.96	1.31
	(1.89)	(1.25)	(0.96)	(0.85)	(1.13)	(0.59)

Table 7: Estimates of the Effect of Teenage Childbearing on Sources of Support
(Dollar figures in 1994 Dollars, T-Statistics in Parentheses)

	Earned Income		***Income from Husband***		***Income From AFDC and Food Stamps***	
Age of Mother	*No Covariates*	*All Covariates with Polynomials in Age*	*No Covariates*	*All Covariates with Polynomials in Age*	*No Covariates*	*All Covariates with Polynomials in Age*
18	-110.17	-2132.60	5062.27	1937.36	995.23	965.42
	(0.07)	(0.93)	(2.59)	(0.48)	(1.30)	(1.26)
19	-90.39	-413.37	3044.89	1641.26	1300.56	1006.23
	(0.05)	(0.22)	(1.18)	(0.53)	(2.19)	(1.84)
20	2561.08	1119.21	5572.48	1534.83	1139.49	965.22
	(1.76)	(0.68)	(2.58)	(0.56)	(1.87)	(1.91)
21	4170.56	2465.14	690.36	1618.08	889.13	853.13
	(2.77)	(1.47)	(0.16)	(0.56)	(1.38)	(1.57)
22	3172.03	3624.42	2568.54	1891.00	160.29	680.74
	(1.86)	(1.99)	(0.63)	(0.57)	(0.20)	(1.15)
23	3769.43	4597.05	1320.92	2353.61	90.88	458.79
	(1.69)	(2.28)	(0.29)	(0.63)	(0.12)	(0.73)
24	6530.27	5383.03	7218.38	3005.88	-565.27	198.05
	(2.73)	(2.46)	(1.44)	(0.73)	(0.47)	(0.30)
25	6896.04	5982.36	6417.08	3847.84	229.96	-90.72
	(2.61)	(2.56)	(1.14)	(0.88)	(0.26)	(0.13)
26	7600.31	6395.03	2839.67	4879.47	408.60	-396.76
	(2.65)	(2.60)	(0.46)	(1.08)	(0.47)	(0.55)
27	9297.22	6621.06	1774.07	6100.78	-199.68	-709.31
	(2.92)	(2.58)	(0.26)	(1.35)	(0.23)	(0.93)
28	9269.65	6660.43	1269.85	7511.77	-372.27	-1017.61
	(3.08)	(2.47)	(0.19)	(1.71)	(0.44)	(1.27)
29	6861.12	6513.16	14168.80	9112.43	-727.29	-1310.91
	(1.87)	(2.27)	(2.73)	(2.16)	(0.72)	(1.57)
30	6941.48	6179.23	9333.75	10902.77	-565.23	-1578.44
	(1.75)	(1.97)	(1.54)	(2.71)	(0.48)	(1.82)
31	838.23	5658.66	11459.36	12882.79	-2755.89	-1809.44
	(0.19)	(1.60)	(1.36)	(3.28)	(1.76)	(1.99)
32	6948.71	4951.43	8973.83	15052.48	-3596.84	-1993.15
	(1.25)	(1.22)	(0.98)	(3.68)	(1.61)	(1.96)

Table 8: Estimates of the Effect of Teenage Childbearing on the Probability of Living in Poverty
(Dollar figures in 1994 Dollars, T-Statistics in Parentheses)

	Probability of Living in Poverty		***Probability of Receiving AFDC***		***Probability of Receiving Food Stamps***	
Age of Mother	*No Covariates*	*All Covariates with Polynomials in Age*	*No Covariates*	*All Covariates with Polynomials in Age*	*No Covariates*	*All Covariates with Polynomials in Age*
18	0.01	0.05	-0.01	-0.01	0.03	0.08
	(0.05)	(0.57)	(0.14)	(0.07)	(0.31)	(1.01)
19	0.14	0.02	0.02	0.00	0.06	0.04
	(1.36)	(0.29)	(0.20)	(0.06)	(0.57)	(0.65)
20	-0.13	-0.01	0.07	0.00	0.02	0.01
	(1.35)	(0.10)	(0.92)	(0.06)	(0.21)	(0.10)
21	-0.05	-0.03	-0.04	0.00	-0.01	-0.03
	(0.46)	(0.53)	(0.44)	(0.08)	(0.07)	(0.56)
22	0.02	-0.05	-0.03	-0.01	0.02	-0.06
	(0.20)	(0.91)	(0.34)	(0.11)	(0.27)	(1.11)
23	-0.08	-0.07	-0.11	-0.01	-0.14	-0.08
	(0.75)	(1.19)	(1.18)	(0.15)	(1.33)	(1.47)
24	-0.11	-0.09	-0.02	-0.01	-0.13	-0.10
	(1.05)	(1.41)	(0.26)	(0.20)	(1.30)	(1.68)
25	-0.05	-0.11	-0.04	-0.02	-0.19	-0.12
	(0.45)	(1.59)	(0.39)	(0.26)	(1.80)	(1.81)
26	-0.10	-0.12	0.05	-0.03	-0.19	-0.14
	(0.91)	(1.76)	(0.63)	(0.34)	(1.77)	(1.89)
27	-0.23	-0.13	0.06	-0.04	-0.14	-0.15
	(2.18)	(1.92)	(0.81)	(0.43)	(1.38)	(1.94)
28	-0.11	-0.14	-0.04	-0.04	-0.10	-0.15
	(1.00)	(2.06)	(0.49)	(0.55)	(1.01)	(1.95)
29	-0.05	-0.15	-0.06	-0.06	-0.11	-0.15
	(0.49)	(2.15)	(0.65)	(0.70)	(1.04)	(1.91)
30	-0.01	-0.15	-0.03	-0.07	-0.05	-0.15
	(0.10)	(2.13)	(0.33)	(0.87)	(0.43)	(1.80)
31	-0.07	-0.15	-0.15	-0.08	-0.18	-0.15
	(0.56)	(1.98)	(1.38)	(1.08)	(1.48)	(1.62)
32	-0.16	-0.15	-0.12	-0.10	-0.17	-0.13
	(1.14)	(1.72)	(1.05)	(1.28)	(1.30)	(1.38)

Table 9: Comparison of Estimates of Effects of Not Delaying Childbearing Measured at Age 28 Using Alternative Estimators and Samples[1]

(T-Statistics in Parentheses.)

Outcomes	*OLS Estimate on Full Sample*	*OLS Estimates on Women Pregnant Prior to Age 18*	*IV Estimates on Women Pregnant Prior to Age 18*
Human Capital			
1. High School Diploma (HSD) (Age 21)	-0.41	-0.16	-0.16
	(15.79)	(3.63)	(1.73)
2. General Equivalence Degree (GED)	0.20	0.12	0.18
	(7.74)	(3.05)	(3.42)
3. HSD or GED	-0.19	-0.03	0.03
	(7.54)	(0.91)	(0.38)
4. Hours Worked	-222.81	-102.52	330.67
	(3.99)	(1.15)	(1.78)
5. Cumulative Hours Worked	-3411.11	-2379.90	2029.62
	(8.39)	(4.20)	(1.72)
6. Wages	-2.98	-2.28	4.22
	(3.97)	(2.05)	(2.16)
Sources of Support			
7. Dollars from Earnings	-9779.98	-6441.65	8488.79
	(5.51)	(3.41)	(2.92)
8. Dollars from Spouse	-4148.29	39.24	2505.17
	(3.00)	(0.01)	(0.38)
9. Dollars from AFDC and Food Stamps	2136.10	45.64	-515.60
	(5.91)	(0.13)	(0.62)
Family Structure			
10. Proportion with Children	0.31	0.26	0.13
	(26.70)	(7.43)	(1.51)
11. Proportion Married	-0.01	0.09	-0.07
	(0.32)	(2.21)	(0.85)
12. Proportion Unmarried with Children	0.16	0.16	0.03
	(6.44)	(0.42)	(0.38)
13. Number of Children	1.26	0.84	0.27
	(21.16)	(8.04)	(1.03)
Measures of Poverty			
14. Proportion in Poverty	0.19	0.13	-0.12
	(7.24)	(3.06)	(1.22)
15. Proportion on Food Stamps	0.14	0.01	-0.09
	(5.87)	(0.40)	(0.89)
16. Proportion on AFDC	0.11	0.01	-0.06
	(5.37)	(0.20)	(0.63)

1. All estimates control for covariates. For the OLS estimates these include dummy variables for ethnicity (black and Hispanic), living in a female headed family at age 14, living in an intact family at age 14, and controls for missing values. The woman's family income in 1978, her mother's education and her fathers education were entered linearly. In addition dummy variables for smoked prior to pregnancy, drank prior to pregnancy and pregnancy prior to age 16 are included for the IV estimates.

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