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# Teen Childbearing and Human Capital: Does Timing Matter?

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## Abstract

In this paper, we model and estimate the relationship between teenage childbearing at different ages and human capital investment. Taking advantage of a large set of potential instruments for fertility--principally state and county-level indicators of the costs of fertility and fertility control--we use instrumental variables procedures to generate unbiased estimates of the effects of early fertility at different ages on education and work. Using data from the NLSY, we find that teenage childbearing at any age substantially reduces years of formal education and early adult work experience for both black and white women. The effects of early and later teen births are similar for both education and early adult work experience. There are no important racial differences in the effects. In contrast, we find no significant impact of a first birth during ages 20-24 on education or work experience. An early teen birth fails have stronger detrimental effects because younger teen mothers are as likely to graduate from high school as older teen mothers, and are equally unlikely to attend college. Our results suggest that "a teen birth is a teen birth", and that public policies that reduce teenage childbearing are likely to have positive effects on the economic well being of many young mothers and their families.

Does teenage childbearing adversely affect women's social and economic outcomes? This question has given rise to a large, contentious research literature. If the answer is "yes," a second question naturally follows: do the effects vary by the mother's age at birth? Knowing whether the timing of a teenage birth matters can help us better understand the relationship between teenage childbearing and later outcomes and may provide useful information in designing policy interventions. This paper builds on our earlier evidence that teenage childbearing has negative effects on young mothers' human capital accumulation (Klepinger et. al. 1995a, 1999), and considers whether early and later teenage childbearing have different effects on this important outcome. It also examines the impact of having a first birth when a woman is in her early twenties.

The presence of young children, with their need for care, will tend to conflict with the human capital investment activities typical of adolescence and early adulthood -- completing high school, attending college or obtaining other post-secondary education and training, and obtaining early work experience -- by raising the costs of and possibly reducing the returns to time spent in human capital investment. Any human capital reductions that follow are likely to reduce the mother's long-run earnings capacity.

If the presence of children does lead to reductions in early human capital investments, a young woman's age at the time of her first birth may influence the extent to which early investments are affected by the presence of children. For instance, older teen mothers may be more likely to complete high school than younger teen moms because they are closer to graduation. Thus, a teen birth may cause a greater disruption in educational attainment if it occurs at an earlier age. Similarly, having children at a younger age may have a more detrimental effect on work experience because younger teen mothers will have had to devote time to caring for their children for a longer period of time. On the other hand, parents and other kin may provide greater support to younger teen moms because they are especially ill prepared to support themselves and their children. If support is inversely related to age at birth, then younger teen moms may be as likely as older moms to complete high school and participate in the labor force. Alternatively, age at first birth may only have a timing effect. Early teen mothers may leave school and the labor force at an earlier age than older teen mothers, but return to school and the labor force at a sufficient rate later to cancel these early effects.

While there is a sizable literature on the effects of teen childbearing on educational attainment, the question of the timing of teen childbearing has received little attention. To examine this issue, we present a life-cycle model of adolescent and young adult choices about fertility and human capital acquisition that underlies the empirical analysis. The model recognizes that the early childbearing decision is endogenous in a model of human capital investment. We then specify instrumental variables models of the effects of early fertility at different ages on human capital accumulation as measured by years of schooling, work experience as a teenager, and work experience as a young adult. State and county level indicators of abortion and family planning facilities and policies are appended to a sample of young women from the

National Longitudinal Survey of Youth (NLSY) to provide a rich set of potential instruments for fertility. We implement a conservative procedure for choosing an acceptable instrument set in the presence of a large set of potential instruments. The instrumental variables estimates indicate that early and later teenage childbearing have large and similar effects on longer run human capital accumulation, and that a first birth during ages 20-24 has no significant impact on human capital accumulation.

**Research on the Human Capital Effects of Adolescent Fertility**

Most investigators have found that early fertility has a negative effect on educational attainment, though there is considerable disagreement as to the magnitude of this effect. Early research on the effects of teenage childbearing on educational attainment provided evidence for a strong negative effect (Waite and Moore 1978; Upchurch and McCarthy 1990; Forste and Tienda 1992). Much of this research treated fertility as exogenous to educational and employment decisions. This approach is now widely recognized as likely to lead to biased estimates, since differences in such outcomes may be due to pre-existing unobserved differences between women who parent early and those who delay childbearing, rather than a causal relationship between adolescent childbearing and adverse adult outcomes (Hofferth and Hayes 1987; Geronimus and Korenman 1992).

More recent studies follow one of three improved methodological paths. One approach uses an instrumental variables approach. Some studies employing instrumental variables find no significant effect (Rindfuss, Bumpass, and St. John 1980; Olsen and Farkas 1989; Ribar 1994a, 1994b), while others find a significant impact that is much smaller than that reported in the earlier studies (Marini 1984; Moore et al. 1993). The most recent instrumental variables studies (Klepinger, Lundberg and Plotnick 1995a, 1999; Angrist and Evans 1996) find that early childbearing substantially reduces schooling. In our previous studies we use a large set of instrumental variables that predict fertility well, suggesting that weak identification of fertility may be responsible for the failure to find statistically significant effects in previous studies.

A second set of studies use family fixed-effect models to account for unobserved heterogeneity. In general, these studies find smaller effects than did the early studies that treated fertility as exogenous (Ahn 1994; Geronimus and Korenman 1992; Hoffman, Foster and Furstenberg 1993; Ribar 1994b). Concern that unobserved family heterogeneity biases the estimated effects of early childbearing upward appear warranted, yet significant negative effects persist in most samples.

Although family fixed-effects models are an improvement over OLS, they do have some limitations. Estimates derived from family fixed-effects models are unbiased only if family heterogeneity fully captures any association between the unobservables in the risk of having a teen birth and in the relevant adult outcomes. If, however, there is unobserved individual heterogeneity that also influences both teen childbearing and adult outcomes, or endogenous relationships between fertility and other choices, family fixed-effects models are likely to yield biased estimates. Furthermore, family fixed-effects models restrict the sample to women who had a teen birth and also have a sister who was not a teen mother. This restriction severely limits the sample size, reducing the efficiency of the estimates, and may yield an unrepresentative sample (Hoffman 1998).

The third approach relies on natural experiments to provide reduced form estimates of the impacts of adolescent fertility. Studies of this type typically find either non-significant effects or small effects (Grogger and Bronars 1993; Hotz, McElroy and Sanders 1997). Grogger and Bronars (1993) use the occurrence of a twin birth as an exogenous event, and compare the outcomes of women who have twin births to women who have a single birth.[1] However, their estimate of the impact of a teen birth assumes that the effect of a twin birth is exactly equal to twice that of a single birth. Hotz et al. (1997) use spontaneous abortions as exogenous events, and compare teens that had a spontaneous abortion to teens who gave birth. However, they must impose assumptions that are inherently difficult to test in an attempt to control for certain non-random aspects of spontaneous abortions. The underreporting of teenage abortions in the NLSY (Jones and Forrest 1992) and the misreporting of miscarriages also raise concerns about the unbiasedness of their estimates. Moreover, about a third of the women in the teen miscarriage sample had a second pregnancy that led to a teen birth. Thus, the comparison group in this study includes teen mothers (Hoffman 1998).

Little research specifically addresses how adolescent childbearing affects labor supply and work experience and what there is produces no consensus. Geronimus and Korenman (1992) find no effect on current employment. Ribar (1994b) generally finds negative effects on both labor force participation and hours of work. Grogger and Bronars (1993) find no effect on participation for whites but a large negative effect for blacks, while Trussell and Abowd (1980) find a positive effect for whites but no effect for blacks. However, because early childbearing is likely to affect work choices over many years, and because the positive effect of experience on wages is well established, studies that examine only current employment may well miss an important long run impact of adolescent childbearing.

Among papers that examine impacts on work experience, Moore et al. (1993) find that a teen birth has no impact on work experience for whites, blacks or Hispanics, and affects education only for Hispanics. Blackburn, et al. (1993) report that early childbearing reduces schooling, experience and tenure for white women. Hotz et al. (1997) report that becoming a teen mother is associated with short term declines in the likelihood of working and hours of work, but that these effects dwindle over time and eventually reverse direction. These studies focus on labor force activity when the respondents are in their mid-twenties or older, rather than teenage employment. Klepinger et al. (1999) examine both teenage and

[1] Angrist and Evans (1998) discuss other reasons why twin studies produce lower estimates of the impact of fertility. They also point out that the use of twins as instruments may be problematic because twinning probabilities are correlated with characteristics of the mother that may also be correlated with human capital formation and wages.

young adult work experience, and find that a teen birth is associated with substantial reductions in both teenage and adult work experience.

The few studies that examine fertility timing and human capital investments yield conflicting results. Upchurch and McCarthy (1990) find that earlier childbearing reduces the likelihood of completing high school, at least among dropouts. Ahn's (1994) results suggest that much of the age difference is due to unmeasured heterogeneity. Using an instrumental variables approach, Moore et al. (1993) find little effect of age at first birth on either completed education or work experience, except for Hispanics. Most of these studies examine the timing issue by considering births at all ages, not just teen births. Consequently, findings of timing effects in these studies could be due to large teen birth effects, rather than evidence that there is a smooth monotonic effect of age at first birth. Hotz et al. (1997) examine the timing of all births using dummy variables for age at first birth, and find that earlier teen births have a greater detrimental impact on high school completion than later teen births, but find that the timing of a teen birth has little impact on hours of work.

Our study contributes to this literature in several ways. We link the empirical model to a behavioral model of adolescent childbearing and its impact on both adolescent and adult human capital. We consider the effect of early childbearing on a set of human capital measures that includes teenage work experience as well as education and later experience. We employ a large set of theoretically plausible instruments to identify the model, and implement a systematic method for selecting acceptable instruments from this set. This paper extends our earlier work by examining how the timing of early fertility affects human capital accumulation. Specifically, we examine the impact of early teen fertility, later teen fertility and early adult fertility on educational attainment and work experience.

## A Model of Adolescent Fertility and Human Capital Investment

In this section, we outline a simple model of a young woman's fertility and human capital investment decisions that guides the specification and identification of our empirical models.[2] Though the model is designed to contrast the optimal human capital investment decisions of a young mother with those of a childless young woman, it illustrates how the effects of early childbearing might vary with the age of the mother. A lifetime consists of two periods – adolescence and early adulthood and later adulthood -- with investment in human capital occurring in only the first period. Each young woman maximizes a utility function of the form:

*(1)* $$U = U_1(C_1, L_1, KQ; K) + r\,U_2(C_2, L_2; K),$$

[2] This model is developed in more detail in Klepinger, Lundberg, and Plotnick (1999).

where period 1 is adolescence and early adulthood and period 2, later adulthood. Future utility is discounted at rate $r$. Utility in each period depends upon consumption of goods and services, $C_i$, and leisure, $L_i$. Early childbearing is represented by a dummy variable, $K$, equal to one if the youth bears and keeps a child, and equal to zero otherwise. If $K = 1$, the utility of the young mother will also be a function of child quality, $Q$, which depends upon inputs of time and goods to childrearing. Later adult utility is also conditional upon earlier fertility, since the child is likely to remain in the household.

Consumption and leisure are constrained by limits on time and resources in each period. Each youth has a fixed amount of time, which can be devoted to leisure, market work, school attendance, or childcare, although childcare can be provided or paid for by others. Consumption in period 1 depends upon the youth's own earnings, financial or in-kind support from relatives or a spouse, and the presence of a child with whom resources must be shared. The availability of support will depend upon the youth's decisions regarding marriage and fertility, as well as exogenous factors such as parental resources. In general, actual support received is endogenous, and choices of fertility, marital status and living arrangements by young mothers will depend upon the availability of such support, and the perceived costs of receiving it. Young mothers can be expected to receive more kin support, both because the willingness to provide support for those closer to dependent childhood will be greater, and because alternative support regimes, such as marriage or self-support, will be less attractive.

Our measure of early fertility, $K$, requires that a pregnancy occurs and is carried to term, and depends upon the young woman's decisions regarding sexual activity, contraception, and abortion. Young women face a two-stage decision process. In the first stage, a young woman makes decisions regarding sexual activity, contraception, and abortion that determine whether she becomes a mother or remains childless. An individual makes these decisions cognizant of their second-stage implications. In the second stage, she decides how to allocate her time and resources, conditional on the presence or absence of a child. The second stage of the young woman's utility maximization problem yields her demands for education and work experience conditional on bearing and keeping a child or on remaining childless during adolescence and early adulthood.

For each young woman, the probability of becoming pregnant, $p$, will be influenced by her choice of costly pregnancy-avoidance measures, $c$, including contraceptive use and delay of sexual activity. The cost vector, $m(c)$, will depend upon the availability of contraceptive information and services, as well as individual characteristics. Conditional on a pregnancy occurring, she may choose to terminate it via abortion, incurring costs which will vary over individuals (psychic costs) and location (time and money costs, and possibly socially induced personal costs). We assume that the utility of a young woman who decides to have an abortion is equal to maximum no-child utility minus $a$, which represents the disutility of abortion itself. Abortion disutility (or abortion cost) will depend on personal characteristics, the social context within which fertility decisions are made, and variables measuring the availability of abortion services. The parameters affecting the fertility decision will change as the adolescent matures, with greater exposure to sexual opportunities increasing the probability of pregnancy, while the availability of contraceptives is also likely to increase.

The first stage decision consists of choosing $c$ so as to maximize expected utility, where:

$$(2) \qquad E(U) = p(c)[\max(U^0 - a, U^1) - m(c)] + (1 - p(c))[U^0 - m(c)].$$

The fertility outcome we observe, $K$, will be a function of abortion costs, $a$, and of the pregnancy-avoidance cost vector $m$, as well as all variables entering the young woman's budget constraint, either with or without children. These costs, however, do not affect schooling and work experience except through their effect on observed fertility, and hence provide a way to statistically identify the effects of fertility on human capital investment decisions.

Maximization of lifetime utility, conditional on early fertility, yields a set of demands for schooling and work experience that depend upon the opportunity set facing the adolescent, including the expected rates of return to these investments. To introduce some empirical content, we recognize that the arguments of these investment functions vary over individuals. Family background variables, $x_B$, affect adolescent market wages, the cost of schooling, and possibly the rate of time preference, as well as available parental and other kin support. Age may affect the rates of return to schooling and experience: the return to high school completion is higher than the return to college, and the work experience of young teenagers tends to have less effect on future wages than later experience.[3] Community variables, $x_C$, include measures of local educational services, local social characteristics and housing market conditions. Variations in wages and employment opportunities are reflected in local labor market variables, $x_{L1}$. The reduced form investment equations for schooling and work experience are of the form:

$$(3a) \qquad S = s(x_B, x_C, x_{L1}, K)$$

$$(3b) \qquad H_1 = h(x_B, x_C, x_{L1}, K)$$

where the remaining endogenous variable is early fertility. Childbearing necessarily depends upon all determinants of human capital investment and also upon the vector of contraception and abortion costs, $z = (a, m)$, so

$$(4) \qquad K = k(x_B, x_C, x_{L1}, z)$$

We use this relationship to identify the schooling and experience models in (3a) and (3b).

[3] For a recent survey of research on the effects of high school employment on future school performance, work, and earnings, see Ruhm (1997).

This model of early human capital investment leads to reduced form empirical functions for early fertility, schooling and early work experience. Since work experience is an alternative to formal schooling for teenagers and young adults, the model implies that the same variables should be included in both functions. The effect of early fertility on the investment functions is identified by the exclusion of contraceptive and abortion costs, which should affect adolescent time allocation only through realized fertility.

The dependence of early fertility on abortion and contraception costs, and the likelihood that these parameters vary with age, suggests that the determinants of early childbearing at different ages be estimated separately. The model also suggests that the effect of early childbearing on schooling and work experience will depend upon age, but does not generate a firm prediction as to the direction of that effect. For instance, an earlier teen birth may more severely interrupt schooling and have greater long-term effects on earnings, but greater kin support for younger adolescent mothers may mitigate the effect of fertility on schooling.

**Estimation Methods**

To test whether teenage childbearing affects educational attainment and work experience, we include dummy variables for adolescent fertility before age 18, at ages 18-19, and at ages 20-24 in regression models of these outcomes. The primary issue raised by this procedure is the potential endogeneity of fertility. Through abstinence and the use of contraception, young women can control the likelihood that they will become pregnant, and through abortion determine whether to carry a pregnancy to term. Consequently, if young women perceive that childbearing will affect their schooling and work opportunities, fertility will be determined jointly with those outcomes. To control for this potential source of bias, we estimate the effects of early fertility at different ages using an instrumental variables (IV) approach.[4] We report Hausman endogeneity tests and, for comparison purposes, results from OLS models.

We identify the effects of early childbearing on education and work experience by excluding from the education and experience equations a set of variables included in the childbearing equation. As suggested by the theoretical framework, external influences on fertility control costs, such as state policy variables that influence contraception and abortion costs, provide instruments for early childbearing. Age of menarche, an individual characteristic that affects fertility but is likely to affect other outcomes only via its effect on fertility, and indicators of the social context within which childbearing decisions occur provide further potential instruments.

Proper implementation of IV methods requires acceptable instruments. Acceptable instruments must be valid: they should be uncorrelated with the error term in the estimating equation. They must also be

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[4] We use a linear probability model to estimate (4). The 2SLS estimator is consistent when the stochastic regressor is dichotomous (see Heckman, 1978 for a discussion).

relevant: they should explain a significant amount of the variance of the endogenous regressor. Otherwise, the IV estimator has poor small-sample properties and is likely to be inconsistent (Nelson and Startz 1990a, 1990b; Bound, Jaeger, and Baker 1995; Shea 1993; Staiger and Stock 1997).

The data file we have developed appends many measures of community characteristics, local economic conditions and the policy environment to individual records. These measures provide a rich set of theoretically plausible potential instruments that far exceed the minimum number needed to exactly identify the education and experience equations. We would expect the inclusion of additional instruments to generate more efficient estimates and increase the power of tests of the substantive hypothesis. However, though the *a priori* arguments for the acceptability of the available instruments are good, they are not so compelling as to preclude testing for validity and relevance.[5] We face the problem of choosing sets of instruments when the universe of potential instruments is large.

The current econometrics literature offers little guidance in designing an optimal method of selecting instruments. Our object, then, is more modest. We wish to devise an instrument choice methodology which is conservative (i.e. unlikely to include unacceptable instruments), and which is sufficiently mechanical to avoid unintended investigator bias.

To this end, we first choose a set of valid instruments from the full set using a test of over-identifying restrictions (OIR).[6] The initial IV estimate of the schooling equation (3a) includes all the potential instruments in the first stage regression. If the $\chi^2$ based on the full set of theoretically plausible instruments fails the over-identifying restrictions test, we exclude each instrument that achieves a 10 percent significance level in the OIR regression.

We then use a goodness-of-fit test to determine whether a set of potentially valid instruments is relevant to the endogenous regressor and significantly improves model fit in the first-stage estimation. Since we have a large number of potential instruments, we cannot test all possible combinations. We adopt a mechanical testing procedure that allows systematic consideration of a large number of possible predictive models and eliminates unintended investigator bias in selecting the instruments for the final model. Backward stepwise regression is used until each instrument remaining in the model achieves a 10 percent level of significance in the first-stage equation. The OIR test is then rerun on the remaining instruments and any that now achieve a 10 percent significance level are dropped from the model. Thus, each instrument that is ultimately retained is insignificant at the 10 percent level in the OIR regression and significant at the

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[5] We would argue, in fact, that a priori arguments are unlikely to be sufficiently compelling in the absence of a true experiment.

[6] We use Godfrey's (1988) test since it is straightforward, but other tests to determine the validity of potential instruments are also available (Hausman 1978; Hausman and Taylor 1981; MacKinnon 1992; Ruud 1984; White 1982). For a full discussion of the approach, see Klepinger, Lundberg and Plotnick (1995b).

10 percent level in a regression predicting fertility. We follow analogous procedures to instrument each endogenous variable in experience equation (3b).[7]

**Data, Samples, and Variables**

The data are from the National Longitudinal Survey of Youth (NLSY), the Alan Guttmacher Institute (AGI), and other public sources. In 1979 the NLSY interviewed 12,686 male and female youths that were between ages 14 and 21 on January 1, 1979. Blacks, Hispanics and economically disadvantaged whites were oversampled. Re-interviews were conducted in succeeding years through 1991 in the file available at the start of this study. The sample for this analysis includes all women aged 14 to 20 in 1979, excluding those in the military subsample and the oversample of economically disadvantaged whites. All analyses are conducted separately for non-Hispanic whites, and non-Hispanic blacks (hereafter "whites" or "blacks"). Sample sizes after exclusion for missing values depend on the dependent variable being analyzed.[8]

Early fertility is represented by three dummy variables that indicate whether the respondent had a first birth prior to her 18th birthday, a first birth during ages 18-19, or a first birth during ages 20-24.[9] Among whites, 6.5 percent were mothers before age 18, 9.3 percent had a first birth during ages 18-19, and 23.8 percent had a first birth during ages 20-24; among blacks, 21.2 percent had a birth prior to age 18, 17.1 percent had a first birth during ages 18-19, and 27.0 percent had a first birth during ages 20-24.

We measure educational attainment as completed years of schooling at the time of interview in the year the respondent turned 25. Reductions in human capital investments during the teenage and early adult years associated with the demands of parenting may be partially replaced by later investments. By examining education levels at age 25, when most people will have completed their formal schooling, or at least will have begun college if they intend to do so, we capture most delayed (as opposed to permanently

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[7] To examine the robustness of the estimates, we experimented with other significance levels to select instruments and with selectively eliminating a few of the final instruments from the set of initial potential instruments and repeated the entire process to see whether the results were being driven by the significance levels we chose or by specific instruments. Point estimates were robust with respect to the significance level and the set of potential and final instruments, but differ substantially from the results of an exactly identified model or a model that uses the full set of instruments.

[8] We also examined the Hispanics subsample, but the relatively small sample led to unstable results that we do not report.

[9] We also estimated models with additional dummies for first births after age 19. For whites, none of these are statistically significant at even the .10 level, and adding them has little effect on the qualitative results for teen births. For blacks, the additional dummies are rarely statistically significant. The inclusion of additional dummies for non-teen births yields much larger standard errors for the teen birth dummies, suggesting that their inclusion creates multicolinearity among the instrumented variables. Consequently, we only report results for the models that include one dummy variable for first births to women in their early twenties.

foregone) investment in schooling. If the measure is missing for the interview year in which a respondent turned age 25, we substitute the value recorded at the time of interview in the year she turned 26.

There is some evidence to suggest that much teen work experience may have little career relevance and a correspondingly low payoff (Klepinger et. al. 1999; Ruhm 1997). Consequently, the returns to teen and early 20s experience as well as their empirical determinants may differ. For this reason, we estimate separate equations for teenage and early adult experience. We measure full-time equivalent years of work experience during ages 16 through 19, and adult experience is measured for ages 20 through 24.[10] Table 1 lists the dependent variables and their means.

[Table 1 about here]

The education and experience equations include the same exogenous variables (also listed with means in Table 1). Personal and family background variables include highest grade completed by mother and father, a set of variables for different living arrangements experienced as a child, number of siblings and of older siblings, whether there was an adult female working for pay in the household when the respondent was age 14, whether the respondent or her parents were born outside the US, whether the respondent was born in the South, whether the respondent lived in the South or an urban area at age 14, whether a non-English language was spoken at home when the respondent was age 14, whether her household subscribed to magazines or newspapers, whether anyone in her household had a library card, the respondent's religious affiliation, and frequency of attendance at religious services.[11] We measure employment opportunities open to adolescents by the percentage of workers employed in services and in wholesale and retail trade for the state where the respondent lived at age 14. We also include county level variables that measure aspects of the distribution of income, local economy, religious and social environment, and educational climate and school enrollment in the county in which the respondent resided in 1979. These additional controls capture potential geographical variation in the costs and returns to education and early employment.

The bottom panel of Table 1 lists the full set of potential instruments for early fertility used in the analysis. State policy variables likely to affect childbearing include the maximum AFDC payment for a family of two, the presence of restrictive abortion provisions, the ages at which parental consent is no longer needed for a young woman to have an abortion or use contraception, and similar variables indicative of state policies on abortion and family planning funding and services. We measure the state-level instruments for the state in which the respondent resided at age 14, when residential location can be regarded as exogenous. We also include indicators of the availability of abortion and family planning

[10] Full-time, full-year equivalent years of work experience are calculated by dividing total hours worked by 2,000 per year. If a respondent has missing data for one or two years, we substitute the mean observed yearly experience for the missing value(s) and add it to the observed values to obtain the relevant measure of experience. If three or more years are missing, we drop the respondent from the analysis.

[11] Early regressions also included the ratio of family income to the poverty line. Since it was insignificant for all groups and since many cases lack income data, we exclude it in results reported here.

services and of the social context within which fertility decisions are made. A substantial body of research (e.g. Billy and Moore 1992; DeGraff, Bilsborrow and Guilkey 1990; Grady, Klepinger and Billy 1993; Klepinger et al. 1999; Lundberg and Plotnick 1995; Rosenzweig and Schultz 1985; Tsui 1985) shows that such variables exert important influences on fertility. We measure these instruments for the county in which the respondent was living at the time of interview in 1979 (or in 1980 if data are not available for 1979).[12] Potential county-level instruments are the abortion rate, whether there is an abortion clinic performing more than 400 abortions, whether there are any Planned Parenthood clinics, the proportion of women aged 15-19 using family planning services, marital and non-marital fertility rates for women aged 15-19, and similar variables listed in the table. The final sets of acceptable instruments for schooling and the two measures of work experience were selected using the procedures described in preceding section.[13]

## Results

Table 2 displays the mean values of schooling, work experience, and hourly wages among women who became mothers before age 18, those who became mothers while they were age 18-19, those who became mothers while they were age 20-24, and those who avoided early parenthood. The simple differences are large, and show that human capital investment is positively related to age at first birth. For instance, compared to women who were not mothers by age 25, white teenage mothers who had their first birth before they turned 18 complete 3.3 years (24%) less schooling, while whites who became mothers while they were age 18 or 19 complete 2.5 fewer years (18%) of schooling, and whites who had first births while they were age 20-24 complete 1.8 years (13%) less schooling. The comparable figures for adult

[12] We would prefer to measure these variables at uniform early age, as we did for the state-level ones, but county of residence prior to 1979 is not available in the NLSY.

[13] Our empirical procedure does not require that the sets of acceptable instruments for the education and two experience models be identical and, while there is overlap, they do differ. For whites, the final instrument set for a birth before age 18 in the education and the two experience equations includes age of menarche, family planning clinics per 1000 women aged 15-19, and the variables indicating ages of consent for abortion and contraceptive use. The instrument set for a first birth at ages 18-19 in the education and the two experience equations includes age at menarche, hospital expenditures per capita, and the sex ratio. In addition, the early work experience model includes the fertility rate of unmarried white women age 15-19. The instrument set for a first birth at ages 20-24 in the education and two experience equations includes the abortion rate, whether there is an abortion provider in the county, and whether there are restrictive laws on the sale/advertisement of contraception. In addition, the education equation includes whether there is a maximum percent of state median income for eligibility under title XX for family planning services, and the later work experience model includes whether there are any restrictions on Medicaid funding for abortion. For blacks, the instrument set for a birth prior to age 18 in the education and two experience equations includes age of menarche, the variables indicating ages of consent for abortion and contraceptive use, the abortion rate, and family planning clinics per 1000 women. For a first birth during ages 18-19, the instruments are the same as those described above for a birth prior to age 18, but also include the sex ratio and nurses per capita. In addition, the adult work experience model includes whether there is a restriction for eligibility under Title XX. For a first birth during ages 20-24, the instruments for the education and both experience equations include family planning clinics per 1000 women, whether there is an age of consent restriction for abortion, nurses per capita, and doctors per capita.

work experience for whites are somewhat larger in proportional terms; 1.5 years (39%), 1.7 years (45%), and 1.1 years (29%). The figures for blacks follow the same general pattern. The only exceptions are that for both blacks and whites, women who had a first birth during ages 20-24 have somewhat more early work experience than women who did not have a birth by age 25, and among whites, women with a first birth prior to age 18 have somewhat more adult work experience than women who had a first birth during ages 18-19, but these differences are small.

[Table 2 about here]

Multivariate regression results in Table 3 show that the direct effects of teenage childbearing on human capital development are both statistically and substantively significant.[14] For whites, the 2SLS point estimate for a birth before age 18 on completed years of schooling is -2.1 years, while a birth at ages 18-19 is associated with 2.7 fewer completed years of schooling. These estimates are relative to not having a birth by age 25. In contrast, the point estimate for a first birth during ages 20-24 is small and non-significant. For a birth prior to age 18, the estimated effect is about a year less than the unconditional mean difference shown in Table 2. For a birth during ages 18-19, the estimated effect is almost equal to the unconditional mean difference. For a birth during ages 20-24, the estimated effect is much smaller than the unconditional mean difference.

For whites, a birth before age 18 is estimated to lower early work experience by 1.7 years, while the point estimate for adult work experience is –1.9 years. A birth during ages 18-19 is estimated to have no effect on early work experience and to lower adult work experience by over 2.5 years. In contrast, a first birth during ages 20-24 is associated with a slight increase in early work experience and a decrease in adult work experience, but neither effect is statistically significant. Although the estimate for a birth prior to age 18 is not statistically significant at conventional levels for either schooling or adult work experience for whites or blacks, the estimates are close to statistical significance (all four coefficients have p-values of .20 or less), and the point estimates are very similar to that observed for a first birth at ages 18-19 which are significant at the .05 level. Moreover, formal tests show that the effects of an early teen birth are not significantly different from the effects of a first birth during ages 18-19 for these outcomes. The lack of statistical significance for births prior to age 18 is, in part, due to the rarity of early teen births.

For black women, the point estimate for a birth before age 18 on schooling is -1.8 years, while the negative effect for a birth during ages 18-19 is 2.1 years; similar to the estimates found for whites. A first birth during ages 20-24 for black women is associated with a positive but non-significant effect on completed schooling. As was the case for whites, an early teen birth is associated with a reduction in early work experience of over 2 years for blacks, but a later teen birth and a birth during ages 20-24 have only small effects that are not statistically significant. The point estimates for an early teen birth and for a birth

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[14] For brevity Table 3 only presents coefficients on the key explanatory variables. Complete results for the first and second stage regressions are available from the first author.

during ages 18-19 on adult work experience are -1.6 and -1.4 years, respectively.[15] A birth during ages 20-24 is associated with nearly a year more adult work experience, although the effect is not statistically significant.

These results indicate that early and later teen births have large and nearly identical effects on completed schooling and adult work experience, that only early teen births affect early work experience, and that a birth during ages 20-24 has little impact on human capital accumulation. Formal t-tests (not shown) confirm this pattern in the results.[16] For both whites and blacks, the t-test results fail to reject the hypothesis that a birth before age 18 and a birth during ages 18-19 have the same effect on completed schooling and adult work experience. The t-test results do, however, reject the hypothesis that the effects of a birth during ages 20-24 on these outcomes is equal those for a birth during ages 18-19. For early work experience, t-test results indicate that the effects of a early teen birth are greater than those of a later teen birth, but that the effects of a later teen birth do not exceed those of a birth during ages 20-24. The F-tests for the significance of the instruments in explaining fertility all exceed a value of three, with many exceeding four, indicating that the set of final instruments significantly improve the fit of the first-stage fertility regressions.[17]

[Table 3 about here]

The OLS results, shown on the right-hand side of Table 3, also show significant effects of early childbearing on human capital development. The OLS estimates are frequently smaller than the IV estimates. This finding is unexpected. The usual story is that the OLS estimates should overstate the effect of early childbearing because early childbearing and low educational attainment are the result of a joint optimizing process or are influenced by common unobservable characteristics.[18]

Despite the apparent differences in the IV and OLS estimates, interpretation of these differences depends on the results of the Hausman exogeneity tests. The Hausman exogeneity test formally tests the difference between IV and OLS estimates. The Hausman test probabilities displayed in Table 3 suggest

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[15] Note that the p-value for births during ages 18-19 for the black for adult work experience is also about .20.

[16] Because earlier births are anticipated to have greater impacts, one-tailed tests are applied. All of the results for whites are significant at the .10 level or better, and all of the results for blacks are significant at the .01 level or better.

[17] The reported F-values greatly exceed the minimum F-values recommended by Bound et. al. (1995).

[18] Angrist and Evans (1996), also find IV estimates of the effect of fertility on schooling that are greater than OLS estimates. They argue that although IV estimates avoid the endogeneity bias of the OLS estimates, they reflect the marginal impact of early childbearing for that portion of the sample whose fertility has been affected by variation in the instruments (see Imbens and Angrist, 1994, for more details). Since many of the acceptable instruments in our models measure access to abortion and family planning services, one explanation for the relatively large IV estimates is that young mothers facing high costs of fertility control who would have avoided early childbearing had these costs been lower experience larger human capital losses. Note also that this reasoning applies to the natural experiment studies, which compare teenage mothers with a narrowly defined comparison group (such as teenagers who experienced miscarriages) that may be an atypical subsample of the relevant population.

that any differences between the OLS and IV estimates should be interpreted with considerable caution. For example, in the black sample, only two of the Hausman test probabilities indicate that we can reject at the .10 level or better the hypothesis that fertility is exogenous, and in one additional case the test probability is less than .13. For the white sample, none of the Hausman test probabilities are less than .05 and only two are less than .15.

These findings suggest that the IV and OLS estimates are not as different as they may at first appear, and that it would be a mistake to overemphasize the differences between the two sets of estimates. Nonetheless, in instances where the exogeneity hypothesis is not rejected at conventional levels of statistical significance, it may be still imprudent to accept the OLS estimates because of the risk of Type II errors.[19] Rather, for p-values that do not decisively reject or fail to reject (e.g., p-values that are not very low or very high) the assumption of exogeneity, the IV estimates are preferred to OLS because, unlike the OLS estimates, they are unbiased whether or not the exogeneity assumption is true.

The results in Table 3 do show very clearly that there are significant adverse impacts of teenage childbearing on human capital investment in both formal schooling and work experience, and that these impacts do not disappear when the endogeneity of fertility is taken into account using IV methods. For both completed schooling and adult work experience, the effects of a birth prior to age 18 are nearly identical to the effects of a first birth during ages 18-19 (though the standard errors are high for the younger group). This pattern is the same for both blacks and whites. Moreover, the quantitative estimates are nearly the same for both blacks and whites; about 2 years for completed schooling and 1.5-2 years for adult work experience. For both whites and blacks, we find that an early teen birth has a substantial negative impact on early work experience, but that an older teen birth has little impact. The results also consistently show little impact of a first birth during ages 20-24. Using estimated wage equations from Klepinger et al. (1999), the point estimates in Table 3 imply that, relative to not having a birth before age 25, an early teen birth lowers hourly wages at age 25 by 48% for whites and 42% for blacks, that a later teen birth lowers adult wages by 66% for whites and 31% for blacks, and that a first birth during ages 20-24 lowers wages by 14% for whites and raises wages by 19% for blacks.

The finding that early and later teen births have nearly equivalent effects on human capital accumulation is somewhat surprising, in that an earlier birth affects investments in human capital for a longer period, and may interrupt investments at earlier more critical periods (i.e., disrupting high school completion). As mentioned earlier, one possible explanation is that parents may be more likely to assist

[19] Endogeneity tests consider the null hypothesis that the potentially endogenous regressor is exogenous. As noted by Nakamura and Walker (1994), failure to reject this null hypothesis is subject to Type II errors. That is, failure to reject the null hypothesis does not necessarily imply that acceptance of the null hypothesis is appropriate. For instance, while a .05 significance level implies that the risk of rejecting the null hypothesis when it is true (Type I error) is five percent, it does not imply that the risk of accepting a false null hypothesis (Type II error) is also five percent. On the contrary, the risk of a Type II error is inversely related to the risk of a Type I error.

younger teen mothers than older teen mothers because younger teen mothers are especially ill prepared to support themselves and their children. One way to examine this hypothesis is to compare the high school graduation rates of younger and older teen mothers. Since a first birth during ages 18-19 is less likely to interfere with high school graduation than having a first birth prior to age 18, we would expect those with earlier teen births to have lower graduation rates.

In Table 4, we present information on levels of educational attainment.[20] The results show that younger teen mothers have somewhat lower high school graduation rates than older teen mothers, and that teen mothers in general have much lower graduation rates than women who did not have a teen birth. The pattern is the same for black teen mothers, but the graduation rate is somewhat higher. There is little difference in graduation rates among women who had a first birth during ages 20-24 and women who did not have a birth by age 25.

Very few teen mothers attend college. Women who did not have a teen birth, particularly those who did not have a birth prior to age 25, have much higher college attendance rates. Less than 10 percent of white teen mothers attended college, while about 15-20 percent of black teen mothers attended college. College attendance rates more than double for women who had a first birth during ages 20-24, and nearly 60 percent of women who did not have a birth before age 25 attended college. These results suggest that the timing of the first birth may be more strongly related to post-secondary educational attainment than to high school completion.

While the unconditional means in Table 4 support our interpretation of the IV results in Table 3 for years of schooling, they may provide biased estimates of the marginal effects of the timing of first birth if early motherhood is endogenous with respect to educational attainment or if there are other important conditioning factors. To further explore the effects of the timing of early motherhood on educational attainment, we estimate multivariate single stage and two-stage logistic models of high school completion and college attendance.[21] These results are in Table 5.

The IV logit estimates for whites parallel the pattern observed in the unconditional means presented in Table 4.[22] For whites, timing of first birth is not significantly related to the likelihood of graduating from high school. In contrast, a teen birth is associated with a much lower likelihood of attending college for whites. While the point estimate for an early teen birth does not quite reach standard significance levels (p-

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[20] Categories of school attainment are derived from reported years of schooling. Although the NLSY contains self-reported information on degree attainment, that data is at great variance with the information on completed years of schooling. Because we find many more cases of degrees without enough years of schooling than the reverse, we rely on the years of completed schooling.

[21] Too few teen mothers completed college to permit IV estimation of models of completing college.

[22] Standard errors for the IV logit results are corrected following Murphy and Topel (1985).

value of .14), it is similar in size to the point estimate for a later teen birth. Moreover, the two estimates are not significantly different (not shown).

To aid in interpreting these results, we calculated the predicted probabilities of high school completion and of attending college using the IV logit parameter estimates. These figures are presented in the lower panel of Table 5. The predicted probabilities of high school completion for whites are all similar, regardless of a woman's age at first birth or whether she had a birth by age 25. For college attendance, however, there are large differences in the predicted probabilities. For whites, the predicted probability of college attendance for teen mothers is less than ten percent, while a first birth during ages 20-24 is associated with a predicted probability of 32 percent. For women who remained childless to age 25, the comparable figure is 66 percent.

The general pattern of the results for blacks is similar to that of whites: teen births have no significant impact on high school graduation, but are associated with lower college attendance. For blacks, however, we find that the point estimates for high school completion are large and imprecisely estimated, resulting in implausibly small predicted probabilities. We also find a large and significant positive effect of a birth during ages 20-24 on high school completion, and a small and statistically non-significant positive effect for college attendance. The estimated effects of teen births on college attendance are, however, similar in size to those observed for whites, and predicted probabilities of college attendance for early and later teen birth are also similar.

On the basis of these results, we conclude that "a teen birth is a teen birth." Having a teen birth is detrimental to human capital accumulation. A teen birth at any age is associated with significantly less formal schooling and less adult work experience, and the effects are similar for whites and blacks. We also find that having a teen birth does not adversely affect high school completion, and the effects of an early and later teen birth are not significantly different. These results suggest that the parents of very young teen mothers assist their children in completing high school. Further, we find that both early and later teen births are associated with a dramatic reduction in the likelihood of attending college, but that a birth during ages 20-24 has little impact. The loss of human capital associated with a teen birth has a large effect on adult wages. Although we also find that an early teen birth, but not a later teen birth, is associated with significantly less teenage work experience, prior work suggests that teenage work experience generally has little impact on adult wages.

**Conclusion**

The results reported here support the main findings of early work on the consequences of teen childbearing, and are consistent with the conventional wisdom that adolescent childbearing has major adverse socio-economic consequences. These results conflict with much of the recent research, which has found modest or no significant consequences of adolescent childbearing. More precise estimates resulting from a much larger set of potential instruments may explain the differences between our results and those of prior IV studies. The contrast between our results, based on conventional IV methods, and those of recent family fixed-effect models and the natural experiment studies suggest that the identification of a control group in these studies may be crucial, and that possible variability in the causal effects of teenage childbearing requires further examination.

Does early adolescent fertility affect the human capital and adult wages of women to a greater extent than later adolescent fertility? Our 2SLS results indicate that adolescent fertility substantially reduces the human capital investments of young women, regardless of their age at the time of the birth. A first birth during ages 20-24 has no significant impact on education or work experience. The apparent lack of a larger detrimental effect for very early teen births is due to the similar high school completion rates of younger and older teen mothers, suggesting that younger teen mothers receive sufficient private assistance to permit them to complete high school. Barriers to the transition to college are shared by early and later teen mothers, and distinguish them from women who had a first birth during their early twenties. Moreover, the findings show that the effects of teen births are similar for blacks and whites. The public policy implications of these results are straightforward: measures that reduce teenage childbearing at any age will have positive effects on the economic prospects of young women and their families.

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**Table 1**

**Means and Sources for Variables**

| *Variables* | *White Mean* | *Black Mean* | *Source* |
|---|---|---|---|
| **1. Endogenous** | | | |
| Birth before age 18 | .07 | .21 | NLSY |
| First birth during ages 18-19 | .09 | .17 | NLSY |
| First birth during ages 20-24 | .24 | .27 | |
| Years of schooling at age 25 | 13.2 | 12.7 | NLSY |
| Teenage work experience | 1.4 | 0.7 | NLSY |
| Early adult work experience | 3.3 | 2.5 | NLSY |
| **2. Exogenous - Fertility, education and experience models** | | | |
| Mother's education | 12.0 | 10.7 | NLSY |
| Mothers education missing | .04 | .07 | |
| Father's education | 12.2 | 9.6 | NLSY |
| Father's education missing | .07 | .26 | |
| Living arrangements at age 14 | | | NLSY |
| Mother only | .08 | .32 | |
| Mother and step-father | .07 | .07 | |
| Other | .06 | .13 | |
| Both parents | .79 | .48 | |
| Years with mother only | .69 | 3.42 | NLSY |
| Years with mother and step-father | .53 | .73 | NLSY |
| Years in other living arrangements | .32 | .82 | NLSY |
| Ever experienced divorce | .12 | .17 | NLSY |
| Number of siblings | 3.1 | 4.8 | NLSY |
| Number of older siblings | 1.9 | 2.8 | NLSY |
| Number of older siblings missing | .06 | .06 | |
| Mother worked | .53 | .58 | NLSY |
| Foreign born | .03 | .02 | NLSY |
| Mother foreign born | .05 | .02 | NLSY |
| Father foreign born | .04 | .02 | NLSY |
| Foreign language at home | .08 | .04 | NLSY |
| Born in South | .25 | .61 | NLSY |
| South residence at age 14 | .26 | .59 | NLSY |
| Urban residence at age 14 | .75 | ..92 | NLSY |
| Magazines in home at age 14 | .74 | .40 | NLSY |
| Newspapers in home at age 14 | .88 | .64 | NLSY |
| Library card at age 14 | .80 | .64 | NLSY |
| Employment in state of residence at age 14 | | | NLSY |
| Percent in services | .18 | .17 | |
| Percent in wholesale/retail trade | .22 | .22 | |

| | | | |
|---|---|---|---|
| Percent in other | .60 | .61 | |
| Religion | | | NLSY |
| Baptist | .16 | .61 | |
| Catholic | .31 | .06 | |
| Other Protestant | .29 | .12 | |
| Jewish/Other | .14 | .12 | |
| None | .10 | .09 | |
| Attendance at religious services | | | |
| Never | .17 | .09 | NLSY |
| Rare | .27 | .21 | |
| Occasional | .19 | .29 | |
| Often | .37 | .41 | |
| *County level variables* | | | |
| Educational spending per 1000 students | 1651 | 1582 | CCDB |
| Median household income in 1979 | 17377 | 15691 | CCDB |
| Median gross rent in 1980 | 235 | 224 | CCDB |
| Percent of population moved into county | 10.0 | 7.8 | CCDB |
| Proportion of county population | | | CCM |
| Catholic | .22 | .17 | |
| Conservative Protestant | .21 | .31 | |
| Jewish and other | .004 | .004 | |
| Percent of county population | | | CCDB |
| Education 12 or more years | 67 | 61 | |
| Education 16 or more years | 16 | 15 | CCDB |
| Percent of families female-headed | 13 | 18 | CCDB |
| Percent of labor force female | 42 | 44 | CCDB |
| Percent of children in poverty families | 15 | 22 | CCDB |
| Unemployment rate in 1980 | 6.8 | 7.2 | CCDB |
| School enrollment rate: 5-17 year olds | .78 | .78 | CCDB |
| Proportion of 16-17 year olds in school – state | .90 | .88 | CENS |
| Proportion of 18-19 year olds in school – state | .52 | .52 | CENS |

**3. Potential instruments for teenage fertility**

| | | | |
|---|---|---|---|
| *Individual* | | | |
| Age at menarche | 12.9 | 12.8 | NLSY |
| *State level* | | | |
| Maximum AFDC payment to 2 person family | $211 | $163 | HEW1 |
| Restrictive abortion provisions | .08 | .14 | HEW2 |
| Restrictive laws on the sale/advertisement of contraception | .40 | .27 | HEW2 |
| Restrictions on Medicaid funding of abortion | .19 | .14 | HEW2 |
| Maximum percent of state median income for eligibility under Title XX family planning services | .75 | 1.71 | HEW2 |
| No maximum | .02 | .13 | |

| | | | |
|---|---|---|---|
| Age of consent for abortion | 16.7 | 16.5 | HEW2 |
| No age of consent | .64 | .49 | |
| Age of consent for contraception | 16.6 | 16.1 | HEW2 |
| No age of consent | .68 | .62 | |
| | | | |
| *County level* | | | |
| Abortion rate per 1,000 women | 26.0 | 46.5 | AGI |
| Abortion provider providing more than 400 abortions | .50 | .65 | AGI |
| Presence of abortion provider | .71 | .76 | AGI |
| Proportion of women 15-19 using family planning services | .13 | .16 | AGI |
| Proportion of family planning patients aged 15-19 | .35 | .32 | AGI |
| Family planning clinics per 1000 women aged 15-19 | .43 | .68 | AGI |
| Number of patients per family planning clinic | 1344 | 1361 | AGI |
| Hospital expenditures per 1000 population | 49 | 71 | CCDB |
| Number of doctors per 1,000,000 population | 1639 | 1937 | CCDB |
| Number of nurses per 1,000,000 population | 4790 | 4477 | CCDB |
| | | | |
| *County level fertility rates and sex ratio* * | | | |
| Marital fertility rate women aged 15-19 | 368 | 588 | AGI |
| Nonmarital fertility rate women aged 15-19 | 16 | 89 | AGI |
| Sex Ratio (# of men 15-19 / # women 15-19) | .946 | .929 | AGI |
| | | | |
| Number of observations | 2014 | 1280 | |

NLSY - Data were obtained from the National Longitudinal Survey - Youth Cohort.
AGI - Data were obtained from the Alan Guttmacher Institute.
HEW1 - Data were obtained from the United States Department of Health, Education and Welfare.
HEW2 - Data were prepared for the United States Department of Health, Education and Welfare by the Alan Guttmacher Institute.
CCDB - Data were obtained from the City-County Data Book.
CCM - Data were obtained from B. Quinn et al., Church and Church Membership in the U.S., 1982
CENS - Data were obtained from the 1980 Census of the United States.
* These are race-specific measures.

**Table 2**

**Mean Schooling and Experience by Age at First Birth, White and Black Women**

| | **White Women** | | | |
|---|---|---|---|---|
| | Age at First Birth | | | |
| | Before Age 18 | Ages 18-19 | Ages 20-24 | Not Before 25 |
| Years of Schooling | 10.7 | 11.5 | 12.4 | 14.0 |
| Years of Early Work Experience | 0.7 | 1.0 | 1.6 | 1.4 |
| Years of Adult Work Experience | 2.3 | 2.1 | 2.7 | 3.8 |
| Hourly Wage ($1990) | $6.42 | $6.64 | $7.19 | $9.14 |

| | **Black Women** | | | |
|---|---|---|---|---|
| | Age at First Birth | | | |
| | Before Age 18 | Ages 18-19 | Ages 20-24 | Not Before 25 |
| Years of Schooling | 11.4 | 12.0 | 12.8 | 13.7 |
| Years of Early Work Experience | 0.5 | 0.6 | 1.0 | 0.8 |
| Years of Adult Work Experience | 1.7 | 2.1 | 2.3 | 3.1 |
| Hourly Wage ($1990) | $5.66 | $6.40 | $6.76 | $7.60 |

Source: Tabulations from the NLSY.

**Table 3**

**Impact of Teenage and Young Adult Childbearing on Human Capital Accumulation, for White and Black Women (Standard errors in parentheses)**

**White Women**

| | *Two Stage Least Squares* | | | *Ordinary Least Squares* | | |
|---|---|---|---|---|---|---|
| | Age at First Birth | | | Age at First Birth | | |
| | Before age 18 | Ages 18-19 | Ages 20-24 | Before age 18 | Ages 18-19 | Ages 20-24 |
| 1. Years of Schooling | -2.08<br>(1.68) | -2.70**<br>(1.33) | -0.17<br>(1.04) | -1.63**<br>(0.14) | -1.30**<br>(0.15) | -0.71**<br>(0.13) |
| Hausman p[a] | .99 | .37 | .45 | | | |
| F-statistic[b] | 3.05 | 5.34 | 3.58 | | | |
| 2. Early Work Experience | -1.66*<br>(0.94) | 0.26<br>(0.75) | 0.61<br>(0.69) | -0.17**<br>(.07) | -0.10<br>(.07) | 0.16**<br>(.06) |
| Hausman p[a] | .12 | .13 | .39 | | | |
| F-statistic[b] | 3.05 | 5.82 | 3.65 | | | |
| 3. Adult Work Experience | -1.91<br>(1.52) | -2.56**<br>(1.23) | -0.82<br>(0.88) | -0.88**<br>(.15) | -0.62**<br>(.16) | -0.69**<br>(.14) |
| Hausman p[a] | .66 | .54 | .73 | | | |
| F-statistic[b] | 3.05 | 5.34 | 4.10 | | | |

**Black Women**

| | *Two Stage Least Squares* | | | *Ordinary Least Squares* | | |
|---|---|---|---|---|---|---|
| | Age at First Birth | | | Age at First Birth | | |
| | Before age 18 | Ages 18-19 | Ages 20-24 | Before age 18 | Ages 18-19 | Ages 20-24 |
| 1. Years of Schooling | -1.82<br>(1.47) | -2.14**<br>(1.26) | 1.59<br>(1.28) | -2.14**<br>(.16) | -1.54**<br>(.14) | -1.05**<br>(.10) |
| Hausman p[a] | .98 | .51 | .03 | | | |
| F-statistic[b] | 4.28 | 3.93 | 4.77 | | | |
| 2. Early Work Experience | -2.14**<br>(0.86) | -0.58<br>(0.72) | -0.02<br>(0.81) | -0.70**<br>(.09) | -0.58**<br>(.08) | 0.11**<br>(.05) |
| Hausman p[a] | .00 | .54 | .92 | | | |
| F-statistic[b] | 4.28 | 3.93 | 4.77 | | | |
| 3. Adult Work Experience | -1.57<br>(1.24) | -1.44<br>(1.19) | 0.95<br>(1.18) | -1.28**<br>(.15) | -1.76**<br>(.13) | -1.11**<br>(.09) |
| Hausman p[a] | .53 | .43 | .13 | | | |
| F-statistic[b] | 4.28 | 3.95 | 4.77 | | | |

[a] The Hausman p shows the confidence level for rejecting the null hypothesis that teen childbearing is exogenous.
[b] F-statistic for improvement in fit of the first stage equations due to the instruments. All are significant at the .01 level.
* = significant at 10% level, ** = significant at 5% level

**Table 4**

**Educational Attainment by Age at First Birth, White and Black Women**

| | **White Women** | | | |
|---|---|---|---|---|
| | Age at First Birth | | | |
| Proportion who completed stated level: | Before age 18 | Ages 18-19 | Ages 20-24 | Not Before 25 |
| High school graduation | 53.1 | 68.7 | 88.5 | 92.1 |
| Some college | 6.2 | 9.2 | 24.8 | 58.5 |
| Bachelor's degree (16+) | 0 | 0 | 5.7 | 34.8 |

| | **Black Women** | | | |
|---|---|---|---|---|
| | Age at First Birth | | | |
| Proportion who completed stated level | Before age 18 | Ages 18-19 | Ages 20-24 | Not Before 25 |
| High school graduation | 63.3 | 77.4 | 88.9 | 90.1 |
| Some college | 14.2 | 18.9 | 39.8 | 59.3 |
| Bachelor's degree (16+) | 1.9 | 1.8 | 7.3 | 24.2 |

Source: Tabulations from the NLSY.

**Table 5**

**Impact of Teenage and Young Adult Childbearing on Educational Attainment, for White and Black Women**
**(Standard errors in parentheses)**

**White Women**

| | *Two Stage Logit* | | | *Ordinary Logit* | | |
|---|---|---|---|---|---|---|
| | Age at First Birth | | | Age at First Birth | | |
| | Before age 18 | Ages 18-19 | Ages 20-24 | Before age 18 | Ages 18-19 | Ages 20-24 |
| High School Graduation | -0.24<br>(3.56) | -1.79<br>(3.06) | -2.58<br>(2.36) | -2.94**<br>(0.31) | -2.27**<br>(0.30) | -0.75**<br>(0.30) |
| College Attendance | -3.67<br>(2.52) | -4.89**<br>(2.02) | -1.80<br>(1.52) | -2.47**<br>(0.41) | -2.45**<br>(0.32) | -1.19**<br>(0.15) |

**Black Women**

| | *Two Stage Logit* | | | *Ordinary Logit* | | |
|---|---|---|---|---|---|---|
| | Age at First Birth | | | Age at First Birth | | |
| | Before age 18 | Ages 18-19 | Ages 20-24 | Before age 18 | Ages 18-19 | Ages 20-24 |
| High School Graduation | -4.17<br>(2.65) | -2.94<br>(2.26) | 3.66*<br>(2.18) | -1.81**<br>(0.32) | -1.25**<br>(0.34) | -0.19<br>(0.17) |
| College Attendance | -1.65<br>(1.85) | -3.24**<br>(1.54) | 1.25<br>(1.60) | -1.97**<br>(0.25) | -1.87**<br>(0.25) | -0.84**<br>(0.20) |

* = significant at 10% level, ** = significant at 5% level .

**Predicted Probabilities From the IV Logit Results**

| | Age at First Birth | | | |
|---|---|---|---|---|
| Proportion predicted to complete stated level | Before age 18 | Ages 18-19 | Ages 20-24 | Not Before 25 |
| *White Women* | | | | |
| High school graduation | .95 | .84 | .74 | .96 |
| College attendance | .09 | .03 | .32 | .66 |
| *Black Women* | | | | |
| High school graduation | .24 | .47 | .99 | .91 |
| College attendance | .21 | .06 | .73 | .50 |