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How Does Adolescent Fertility Affect the Human Capital and Wages of Young Women?

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Abstract

The consequences of teen childbearing for the future well-being of young women remain controversial. In this paper, we model and estimate the relationship between early childbearing and human capital investment, and its effect on wages in early adulthood. 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 on education and work experience, and the effects of these outcomes on adult wages. For both black and white women, adolescent fertility substantially reduces years of formal education and teenage work experience. White teenage mothers also obtain less early adult work experience than young women who delay childbearing. We also find that, through these human capital effects, teenage childbearing has a significant effect on a young woman's market wage at age 25. Our results, unlike those of recent "revisionist" studies, suggest 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.

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INTRODUCTION

The human capital young women bring to the market is a major determinant of their earnings capacity. It seems reasonable that the presence of young children, with their need for care, will 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 investment.

If reductions in these early investments occur, they are likely to have adverse long-term consequences for the wages, earnings, and employability of the mother. Reduced earnings will have substantial negative effects on the total income and, hence, economic well-being of young mothers and their families, both because the contribution of young married women's earnings to total family income is substantial and increasing (Dechter and Smock, 1994) and because a young mother is likely to be single for several years when her children are young. In 1994, more than three-quarters of teen births were nonmarital (Child Trends, 1996), and divorce rates for very young married couples are high. Lower earnings and the need for child care also make long-term dependence on government aid a more likely outcome for adolescent mothers.

Despite a sizable literature on how teenage childbearing affects educational attainment (see Klepinger, Lundberg and Plotnick, 1995a, and the references therein), research on how it affects experience and wages is relatively meager. Since these matters are central to the scholarly and public policy debates about adolescent childbearing, this study estimates the relationships between teenage childbearing and human capital accumulation as measured by years of schooling, work experience as a teenager, and work experience as a young adult. It then considers the implications of these relationships for the wages that young women can expect to earn. Teenage fertility is allowed to affect wages in two ways: by reducing human capital accumulation and by affecting the rate of return to these investments.

We develop a life-cycle model of adolescent choices about fertility and human capital acquisition that underlies the empirical analysis. The model recognizes that the adolescent childbearing decision is endogenous in models of human capital investment and wage determination and suggests an identification strategy, in that factors affecting the costs of fertility control should affect human capital decisions only through realized fertility.

We then specify instrumental variables models of the effects of early fertility on education and work experience, and of the effects of these outcomes on adult wages. State- and county-level indicators of abortion and family planning facilities and policies are appended to our sample of young women from the National Longitudinal Survey of Youth (NLSY) to provide a rich set of potential instruments for fertility. A conservative policy for choosing an instrument set in the presence of a large set of potential instruments is suggested, and we follow a mechanical, stepwise procedure to exclude instruments that are uncorrelated with the endogenous regressor variables, or that cause the model to fail a test for overidentifying restrictions. With this new application of instrumental variables, we fail to reject the conventional wisdom that teenage childbearing has substantial effects on future labor market opportunities. These results are different from, and usefully supplement, the largely negative results of other recent studies based on the comparison of selected subsamples.

RESEARCH ON THE HUMAN CAPITAL AND WAGE EFFECTS OF ADOLESCENT FERTILITY

Education

Early research provided strong evidence for the expected negative effects of teenage childbearing on educational attainment. Waite and Moore’s (1978) pathbreaking paper reports large negative effects of early childbearing on educational attainment after controlling for a variety of individual and family background factors. That work, as well as some more recent studies (Upchurch and McCarthy, 1990, Forste and Tienda, 1992), treats fertility as exogenous to educational decisions. Such an approach is now widely recognized as likely to lead to biased estimates, since differences in outcomes such as educational attainment may be due to pre-existing differences between women who parent early and those who delay childbearing, rather than to any causal relationship between adolescent childbearing and adverse adult outcomes. For instance, compared with women who delay their first births, women who have early births may have low educational and earnings aspirations, more disadvantaged backgrounds, or other unobserved characteristics that lead to poorer outcomes later in life (Hofferth and Hayes, 1987; Geronimus and Korenman, 1992).

More recent studies follow one of three improved methodological paths. Some estimate the relationship between fertility and schooling using an instrumental variables approach. Rindfuss, Bumpass, and St. John (1980) find no significant effect of age at first birth on educational attainment. Marini (1984) reports a significant impact much smaller than that reported in the earlier literature. Olsen and Farkas (1989) find no effect of pregnancy on the drop-out behavior of poor black female high school students. Using a pooled sample of whites, blacks, and Hispanics from the NLSY, Ribar (1994a) reports that teenage fertility does not affect the likelihood of dropping out of high school by age 20. His later NLSY study (Ribar 1994b) reports similar results and also finds no effect on completed years of schooling.[1] In another instrumental variables study using the NLSY, Moore et al. (1993) report no effect of age at first birth on highest grade completed for whites and blacks, but a significant positive relationship for Hispanics.

[1] His disaggregated estimates show negative effects for whites and Hispanics, but positive effects for blacks.

In contrast, the most recent instrumental variables study (Klepinger, Lundberg, and Plotnick, 1995a) finds that early childbearing reduces schooling by nearly 3 years for white, black, and Hispanic women. The authors use a large set of instrumental variables that predict fertility well, whereas many of the studies that report insignificant results use a small number of instrumental variables (1 or 2). Weak identification of fertility in those studies may be responsible for the failure to find statistically significant effects.

A second set of studies use family fixed-effect models to account for unobserved heterogeneity. Geronimus and Korenman (1992) use three major data sets to compare the experiences of sisters who timed their births at different ages. Hoffman, Foster, and Furstenberg (1993) and Ribar (1994b) replicate this study on different samples. With all five samples, cross-section regressions show that early childbearing reduces the probability both of completing high school and of obtaining post-secondary schooling. The fixed-effect approach finds an insignificant relationship in three of these ten cases and substantially reduces the magnitude of the significant effect in several others. Ribar (1994b) finds a similar pattern with years of schooling as the dependent variable. Concern that unobserved family heterogeneity biases upward the estimated effects of early childbearing appears warranted, yet significant negative effects persist in most samples.[2]

Despite their appeal, family fixed-effect models have limitations. Estimates derived from such models are unbiased only if unobserved family heterogeneity is the only factor that affects both the risk of having a teen birth and 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, then family fixed-effect models are likely to yield

[2]Because of the small sample sizes typically obtained in sibling analyses of qualitative outcomes, no study disaggregates by race when analyzing the two probabilities. With years of schooling as the dependent variable, Ribar (1994b) can disaggregate by race and finds similar patterns. In a similar vein, Ahn (1994) finds that controlling for individual-specific heterogeneity reduces the estimated impact of a teen birth on high school completion, but the effect of a birth is still negative and statistically significant.

biased estimates. Family fixed-effect models restrict the sample to women who had a teen birth and also had a sister who was a nonteen mother. This restriction severely limits the sample size, reduces the efficiency of the estimates, and may introduce sample selection bias.

The third approach taken in more recent studies relies on natural experiments to provide reduced form estimates of the impacts of adolescent fertility. Grogger and Bronars (1993) use U.S. census data to compare outcomes of teenage women experiencing twin first births to those of teenage women experiencing single first births. They report insignificant effects of teenage childbearing on years of schooling and the likelihood of high school graduation for whites, but significant negative effects for blacks.[3] Although the birth of twins can be viewed as a random event, use of twin births as a natural experiment will not yield unbiased estimates of the effects of a teen birth unless the effect of a twin birth is exactly twice that of a single birth.

Hotz, McElroy, and Sanders (1995) suggest an interesting "control group" with which to compare teenage mothers. They argue that since miscarriages are largely random events, women who miscarry as teenagers are a random sample of women who become pregnant as teenagers and, thus, comparing outcomes of teenagers who miscarry to those who have births is an appropriate natural experiment.[4] Their analysis of the NLSY finds that teen mothers are less likely to complete high school and more likely to earn a GED than teens who miscarry. Although Hotz, McElroy, and Sanders (1995) attempt to control for certain nonrandom aspects of spontaneous abortions, the assumptions they impose are inherently difficult to test. The underreporting of teenage abortions in the NLSY (Jones and Forrest,

[3]Bronars and Grogger (1994) report similar results in an analysis of the consequences of unwed motherhood. In this study unwed mothers can be of any age.

[4]Although Hotz, McElroy, and Sanders (1995) actually use spontaneous abortions to identify an IV estimation method, they do so to account for certain nonrandom aspects of spontaneous abortions. That is, they use the IV approach to create a "better" comparison group for their natural experiment.

1992) and the possible misreporting of miscarriages also raise concerns about the randomness of this control group.

To date, most investigators have found that early fertility has a negative effect on educational attainment, although there is considerable disagreement about the magnitude of this effect. Many recent “revisionist” studies that employ family fixed-effect, natural experiment, or instrumental variables methods have reported small or insignificant effects of teenage childbearing on the probability of high school graduation.

Work Experience, Wages, and Earnings

The literature on determinants of women’s labor supply and wages is enormous.[5] Many estimates of the effect of fertility on wages have been made, and some recent studies have accounted for the endogeneity of fertility, education, and experience (Korenman and Neumark, 1992; Neumark and Korenman, 1994; Blackburn and Neumark, 1995). Little research, though, specifically addresses how *adolescent* childbearing affects work experience or later wages. We review the most relevant studies.

No consensus emerges from recent estimates of the effects of adolescent childbearing on current employment or labor force participation. Geronimus and Korenman (1992) find no effect on current employment. Ribar (1994b) generally finds negative effects on both participation and hours of work. Grogger and Bronars (1993) find no effect on participation of whites but a large negative effect for blacks, while Trussell and Abowd (1980) find a positive effect for whites but no effect for blacks. These studies focus on labor force activity when the respondents are in their mid-twenties or older, rather than on teenage employment.

[5]For a recent set of papers on women’s labor supply and wages, see the spring 1994 issue of the *Journal of Human Resources*.

Moore et al. (1993) and Blackburn, Bloom, and Neumark (1993) provide the two most comprehensive studies of the labor market effects of early childbearing. Moore et al. (1993) examine accumulated work experience by age 26 and real earnings at age 27 and address the possibility that wages may be jointly determined with fertility, schooling, and experience. Age at first birth has no impact on work experience for whites, blacks, or Hispanics, and affects education only for Hispanics. Since the model allows age at first birth to affect earnings only indirectly through its effects on education or experience, we infer it has an indirect effect only for Hispanics.

Blackburn, Bloom, and Neumark (1993) report that early childbearing reduces schooling, experience, tenure, and wages for white women. Lower investments in schooling, experience, and job tenure due to early childbearing account for most of the wage effect. Fertility timing appears to have a small direct effect on wages even after controlling for its impact on human capital accumulation. The analysis assumes education is exogenous with respect to wages. It does test for whether fertility and experience are endogenous to wages and concludes that the latter is but the former is not.

Hotz, McElroy, and Sanders (1995) report that becoming a teen mother is associated with short-term declines in the likelihood of working, hours of work, and earnings, but that these effects dwindle over time and eventually reverse direction. The study does not examine whether the effects of teen motherhood on work and schooling account for the effect on earnings.

The existing literature suggests that the effect of early childbearing on wages or earnings is mostly indirect.[6] Women who become young mothers earn less because they obtain less formal education and work experience. Our study contributes to this literature in several ways. Unlike most researchers

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[6]Two important early studies which do not consider possible endogeneity of fertility also support this conclusion. Hofferth and Moore (1979) show that delaying the first birth raises earnings at age 27. This effect arises largely because delaying a birth reduces family size, which in turn increases earnings. Among women who had a first birth at 18 or younger, the earnings impact is largely due to another indirect effect: delaying a birth increases education. Trussell and Abowd (1980) find that after controlling for education and experience, age at first birth has no effect on wages of married mothers aged 25–44.

(except Blackburn, Bloom, and Neumark (1993)), we link the empirical estimates to an explicit behavioral model of adolescent childbearing and its impact on both adolescent and adult human capital and labor market outcomes. We also consider the effect of early childbearing on teenage work experience as well as education and later experience. Since early childbearing is likely to affect work choices over many years, and 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. We employ a large set of theoretically plausible instruments, most of them indicators of local variations in the cost of fertility control, to identify the model. Finally, in pursuing the instrumental variables estimation approach, we implement a systematic method for selecting acceptable instruments from a large set of conceptually plausible potential instruments.

A MODEL OF ADOLESCENT FERTILITY AND HUMAN CAPITAL INVESTMENT

We present a simple model of a young woman's decisions to become a mother and to invest in human capital through formal education and work experience. The model does not provide a fully general description of adolescent behavior as it relates to childbearing and investment in human capital, but instead is designed to contrast the optimal human capital investment decisions of a teen mother with those of a childless teenager. We use the theoretical results to help specify and identify empirical models of the determinants of education, work experience, and adult wages in a manner consistent with a theory of individual decision-making.

For simplicity, we represent a lifetime as two periods—adolescence and adulthood—with investment in human capital occurring in the first period only. Each young woman maximizes a utility function of the form:

$$U = U_1(C_1, L_1, KQ; K) + \rho U_2 \; (C_2, L_2; K), \qquad \textbf{(1)}$$

where period 1 is adolescence and period 2, adulthood. Future utility is discounted at rate ρ. Utility in each period depends on consumption of goods and services, C_i , and leisure, L_i. Early childbearing is represented by a dummy variable, *K*, equal to 1 if the adolescent bears and keeps a child, and equal to 0 otherwise. If $K = 1$, the utility of the adolescent mother will also be a function of child quality, *Q*, which depends on inputs of time and goods to child rearing. Adult utility is also conditional on adolescent fertility, since the child is likely to remain in the household, but we do not consider explicitly the determinants of adult childbearing or its effects on adult time allocation.

Consumption and leisure are constrained by limits on time and resources in each period. Each adolescent has a fixed amount of time, $\overline{T}_1$, which can be devoted to leisure (L_1), market work (H_1), school attendance (*S*), or child care (D_1), so that

$$\overline{T}_1 = L_1 + H_1 + S + D_1 . \tag{2}$$

The budget constraint is assumed to be binding in each period, so that adolescents are not permitted to borrow against their adult earnings. Consumption in period 1 depends on the teenager'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, so that

$$C_1 = \left\{ w_1 H_1 + Y_1 \right\} / N_1 , \tag{3}$$

where w_1 is the market wage of a teenager, $N_1 = N(K)$ is a consumption deflator, and Y_1 is the value of support received from parents, spouse, or other kin. The availability of support will depend on the adolescent's decisions regarding marriage and fertility, as well as on exogenous factors such as parental resources. In general, actual support received is endogenous, and choices of fertility, marital status, and living arrangements by adolescent mothers will depend on the availability of such support and the perceived costs of receiving it.

The adolescent mother chooses child quality through endogenous inputs of time and money to child raising. If household public goods, such as shelter, are important determinants of child quality, it seems reasonable to tie money inputs to children to the mother's own consumption level. Child care time can be provided by the mother (D_1), or donated by others (DO_1), so that $Q = Q(C_1, D_1, DO_1)$.

Time and budget constraints in the second period are:

$$\overline{T}_2 = L_2 + H_2, \text{ and} \tag{4a}$$

$$C_2 = \{w_2 H_2 + Y_2\} / N_2, \tag{4b}$$

where the variables are analogous to those defined above. The market wage in adulthood depends on work experience and schooling undertaken in adolescence, so that $\ln w_2 = \ln w_1 + r_S S + r_H H_1$, where r_S and r_H are the rates of return to schooling and work experience, respectively. In general, the parameters of the adult budget constraint, specifically the wage rate, potential husband's income, and the consumption deflator, will be functions of the fertility decisions made in period 1.

Our measure of adolescent fertility, K, requires that a pregnancy occur and be carried to term, and depends on the young woman's decisions regarding sexual activity, contraception, and abortion. Adolescent 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 teenage mother or remains childless. These decisions are made by an individual 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.

To examine the effects of fertility on human capital investments, given that fertility is endogenous, we consider the young woman's decision process in reverse order. First, we maximize her utility conditional on $K = 0$ (U^0) and derive the conditional demands for schooling and work experience by nonchildbearers. Then, we maximize utility conditional on $K = 1$ (U^1) and derive the corresponding conditional demands for human capital investment for an adolescent mother. Finally, the maximal levels of utility conditional on K enter into the young woman's decision to employ costly pregnancy-avoidance and pregnancy-resolution strategies. Combining the costs of avoiding or terminating a pregnancy with the utility consequences of fertility enables us to derive an equation for observed fertility.

For each young woman, the probability of becoming pregnant, p, will be influenced by her choice of costly pregnancy-avoidance measures, c, including use of contraceptives and delay of sexual activity. The cost vector, $\mu(c)$, will depend on the availability of contraceptive information and services, as well as on individual characteristics. If a pregnancy occurs, 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 first-stage decision consists of choosing c so as to maximize expected utility, where:

$$E(U) = p(c)[\max(U^0 - a, U^1) - \mu(c)] + (1 - p(c))[U^0 - \mu(c)] \,. \qquad \textbf{(5)}$$

The fertility outcome we observe, K , will be a function of abortion costs, a, and of the pregnancy-avoidance cost vector μ, 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 they provide a way to statistically identify the effects of fertility on human capital investment decisions.

Maximization of lifetime utility, conditional on $K = i$, will yield a set of demands for adolescent human capital investment of the form:

$$S_1^i = s^i(w_1, r_S, r_H, \rho, N_1^i, Y_1^i, DO_1^i, N_2^i, Y_2^i) \qquad \textbf{(6a)}$$

$$H_1^i = h^i(w_1, r_S, r_H, \rho, N_1^i, Y_1^i, DO_1^i, N_2^i, Y_2^i) \,, \qquad \textbf{(6b)}$$

where Y_1^i, DO_1^i , and Y_2^i are the endogenous amounts of support received, given the young woman's optimal choice of a support regime when $K = i$.

In general, this human capital model predicts that completed years of schooling will be a positive function of income and child care support received during adolescence, which reduces the marginal cost of time spent in school, and a negative function of the income support expected during adulthood, which reduces the marginal benefit to school by encouraging a fall in future labor supply. An increase in the rate of return to formal education will increase schooling, as will an increase in the relative value of adult versus adolescent consumption (ρ).

Adolescent fertility has both positive and negative effects on schooling, although the net effect is expected to be negative. The direct negative effect of early childbearing will act through the effect of child care time on the marginal cost of school time. However, the effect on adult labor supply, and thus the return to schooling, is uncertain, since the presence of a child will increase both consumption demands and available financial support in adulthood.

Work experience is usually analyzed with a standard labor supply model, in which the level of schooling and fertility decisions are taken as given. This model shows that early work experience must be recognized as an alternative to formal education in terms of sacrificed leisure and of the opportunity to transfer resources into the future by investing in skills, and so implies that adolescent work experience is a function of the same variables determining formal schooling.

To introduce some empirical content, we recognize that the arguments of the 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. Community variables, x_C, include measures of local educational services, local social characteristics, and housing market conditions. Variations in adolescent wages and employment opportunities are reflected in local labor market variables, x_{L1}. Substitution into the above equations gives us reduced form investment equations of the form:

$$S = s(x_B, x_C, x_{L1}, K) \tag{7a}$$

$$H_1 = h(x_B, x_C, x_{L1}, K) \tag{7b}$$

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

$$K = k(x_B, x_C, x_{L1}, z)\,. \tag{8}$$

We use this relationship to identify the schooling and experience models in (7a) and (7b).

Adult wages will be affected by adolescent fertility indirectly through the influence of child-rearing responsibilities on realized education and early work experience, and possibly directly if such responsibilities affect the rates of return to human capital investments. We can therefore write the adult wage equation as:

$$w_2 = w(S, H_1, K, x_{L2})\,, \tag{9}$$

where x_{L2} is a vector of variables affecting adult labor markets. The wage equation omits x_B, x_C, and x_{L1}, as discussed below, to identify the wage model.

This model of adolescent human capital investment leads to reduced form empirical functions for adolescent fertility and demands for schooling and early work experience. Since work experience is an alternative to formal schooling for teenagers, the model implies that the same variables should be included in both functions. The effect of adolescent 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. Finally, this model yields an equation for adult wages in which the effect of adolescent fertility can be disaggregated into changes in levels of schooling and work experience, and changes in the rates of return to these investments.

ESTIMATION METHODS

To test whether teenage childbearing affects educational attainment, work experience, and wages, we include dummy variables for early fertility in a regression model of each of these outcomes. The primary estimation issue raised by this procedure is the potential endogeneity of fertility. Through abstinence and the use of contraception, adolescents can control the likelihood that they will become pregnant, and through abortion determine whether they carry a pregnancy to term. Consequently, if adolescents 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 impacts of teenage childbearing in (7a) and (7b) using an instrumental variables (IV) approach.[7] Fertility is also endogenous in a model of wage determination because it is likely to be related to the expected costs of and returns to investing in education, work experience as a teenager, and work experience as an adult. Education and experience, moreover, are likely to be correlated with the error in the wage equation because, in a life-cycle decision-making context, adolescent investments in human capital will be related to expected future market returns.[8] We estimate the effects of these variables and teenage fertility on wages in (9), also using an IV approach. We report Hausman endogeneity tests and, for comparison purposes, results from ordinary least squares (OLS) models.

We identify the effect of teenage 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 (z), such as state policy variables that influence contraception and abortion costs, provide instruments for teenage

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[7]We use a linear probability model to estimate (5). The two-stage least squares estimator is consistent when the stochastic regressor is dichotomous (see Heckman, 1978 for a discussion).

[8]Heckman (1980) and Mroz (1987) present evidence that labor market experience is not exogenous with respect to market wages, although Mroz finds no evidence to suggest that schooling is not exogenous.

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 instruments. To identify the wage equation, we allow family background characteristics (x_B) to enter the schooling and experience equations, but not to directly affect wages.[9] In addition we allow local social conditions, local educational services, and labor, housing, and marriage market conditions during adolescence (x_C) to influence schooling and work experience, but not to directly affect wages, conditional on labor market conditions during adulthood.

Proper implementation of IV methods requires acceptable instruments. Acceptable instruments must meet two criteria. First, they must be valid, i.e., uncorrelated with the error term in the estimating equation. Second, they must be relevant, i.e., able to explain a significant amount of the variance of the endogenous regressor (Nelson and Startz, 1990a, 1990b; Bound, Jaeger, and Baker, 1995; Shea, 1993; Staiger and Stock, 1994). Otherwise, the IV estimator may be severely biased.

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, experience, and wage 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.[10] We face the problem of choosing sets of instruments when the universe of potential instruments is large, and

[9]Exclusion of family background characteristics (x_B) from wage equations is a standard approach for identifying wage equations with endogenous schooling and labor market experience (Griliches, 1977, 1979). Recently, Neumark and Korenman (1994) tested whether this approach is econometrically appropriate and concluded that it is.

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

the current econometrics literature offers little guidance in designing an optimal method of doing so. Our object, then, is more modest. We wish to devise an instrument choice methodology which is conservative (i.e., unlikely to include invalid 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) discussed in Godfrey (1988).[11] To exactly identify the model, we maintain age of menarche, an individual characteristic likely to affect childbearing but not educational attainment or work experience, as an acceptable instrument throughout the analysis. For example, we initially estimate (7a) using IV with all the potential instruments included in the first-stage regression. If the χ^2 based on the full set of theoretically plausible instruments fails the OIR test, we exclude each instrument that achieves a 10 percent significance level in the OIR regression.

Second, we use a goodness-of-fit test to determine whether a set of potentially valid instruments is relevant to the endogenous regressor (e.g., adolescent fertility in (7a)) and significantly improves model fit in the first-stage estimation. Since we have a large number of instruments, we can not 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. We apply backward stepwise regression until each instrument remaining in the model achieves a 10 percent level of significance in the first-stage equation. We then rerun the OIR test on the remaining instruments and drop any that now achieve a 10 percent significance level. Thus, each instrument we ultimately use is insignificant at the 10 percent level in the OIR regression and significant at the 10 percent level in a regression predicting fertility. We follow analogous procedures to instrument each endogenous variable in (7b). In another paper (Klepinger, Lundberg, and Plotnick,

[11]We use Godfrey's 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).

1995b) we compare estimates of the education equation derived using this and alternative instrument-choice algorithms. We find that the substantive results are relatively insensitive to the choice of instruments from the full set, but differ substantially from the results of a just-identified model.[12]

The wage equation contains several endogenous regressors: teenage fertility, schooling, and work experience. To select acceptable instruments in this situation, we alter the procedure slightly. The potential instruments for each endogenous regressor are a large set of family background characteristics and measures of local social conditions, local educational services, abortion and contraception costs, and local labor, housing, and marriage market conditions during adolescence. We use identical sets of characteristics and measures for all endogenous regressors. We first estimate (9) with the full set of instruments. We conduct the OIR test and delete instruments that achieve a 10 percent significance level in the initial regression. Using the remaining set, we run separate goodness-of-fit tests to determine which subset of potentially valid instruments is relevant to each endogenous regressor. We again require relevant instruments to be significant at the 10 percent level in the first-stage regression. We take the union of the subsets as the tentative set of acceptable instruments and rerun the OIR test. If necessary, we delete any instruments that now achieve a 10 percent significance level. The instruments that survive these screens are all used in the first-stage regressions for fertility, education, and experience.[13]

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[12]As a sensitivity check, we experimented with using 20 percent significance levels to select instruments and found it made virtually no difference in the results. We also selectively eliminated a few of the final instruments and repeated the entire process to see whether the results were being driven by specific instruments. Point estimates were robust to varying the set of potential and final instruments.

[13]The wage equation also includes a standard selectivity correction. Standard errors are corrected for inclusion of the selection term.

DATA, SAMPLES, AND VARIABLES

The data for this study are from the NLSY, the Alan Guttmacher Institute, and other public sources. In 1979 the NLSY obtained interviews from 12,686 male and female youths who were between the ages of 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") because results are likely to vary substantially by race. Sample sizes after exclusion for missing values depend on the dependent variable being analyzed and range between 1,378 and 1,768 for whites, and 714 and 1,035 for blacks, with the smaller number applying to the wage equations.[14]

Adolescent fertility is represented by a dummy variable that indicates whether the respondent had a birth prior to her 20th birthday. Among whites, 16 percent were teenage mothers; among blacks, 38 percent. 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 years due to 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 foregone) investment in schooling. Given the sample, schooling at 25 is measured during the 1984–1990 period. If the measure is missing for the interview year in which a respondent turned 25, we substitute the value recorded at the time of interview in the year she turned 26.

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[14]We also examined Hispanics. The relatively small sample led to unstable results which we do not report.

Because we analyze wages at age 25, the measure of work experience includes work time during teenage years and during ages 20–24. Because much teen experience may have little career relevance and a correspondingly low payoff, the returns to teen and early twenties experience, as well as their empirical determinants, may differ. Hence, we estimate separate equations for teenage and adult experience. We measure full-time, full-year equivalent years of work experience during ages 16 through 19 by dividing total hours worked during those years by 2,000. Adult experience is similarly measured during the 5 years covering ages 20 through 24. 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 treat hours of work as missing.

Our measure of the wage is the natural logarithm of hourly wages (in 1990 dollars) at age 25. If wages are not available for the interview year in which a respondent turned 25, we substitute her wage in the year she turned 26. Table 1 lists the dependent variables and their means.

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 aged 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 aged 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. We measure employment opportunities open to adolescents by the percentage of workers employed in services and in

TABLE 1
Means and Sources for Variables

Variables	White Mean	Black Mean	Source
1. Endogenous			
Birth before age 20	.16	.38	NLSY
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
Hourly wage (in 1990 dollars)	$8.38	$6.80	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 stepfather	.07	.07	
Other	.06	.13	
Both parents	.79	.48	
Years with mother only	.69	3.42	NLSY
Years with mother and stepfather	.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	

(table continues)

TABLE 1, continued

Variables	White Mean	Black Mean	Source
Religion			NLSY
Baptist	.16	.61	
Catholic	.31	.06	
Other Protestant	.29	.12	
Jewish/Other	.14	.12	
None	.10	.09	
Attendance at religious services			NLSY
Never	.17	.09	
Rare	.27	.21	
Occasional	.19	.29	
Often	.37	.41	
County-Level			
Educational spending per 1,000 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. Exogenous — Wage Model			
Local unemployment rate at age 25	8.0	7.6	NLSY
Presence of health limitations (%)	4.6	5.5	NLSY
Urban residence (%)	77.5	83.2	NLSY
4. Potential Instruments for Teenage Fertility			
Individual			
Age at menarche	12.9	12.8	NLSY

(table continues)

TABLE 1, continued

Variables	White Mean	Black Mean	Source
State-Level			
Maximum AFDC payment to two-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 1,000 women aged 15–19	.43	.68	AGI
Number of patients per family planning clinic	1344	1361	AGI
Hospital expenditures per 1,000 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	

Notes: 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.

wholesale and retail trade for the state where the respondent lived at age 14.[15] We also include county-level variables which measure aspects of the distribution of income, local economy, religious and social environment, educational climate, and school enrollment in the county in which the respondent resided in 1979.

The bottom panel of Table 1 lists the full set of potential instruments for teenage fertility used in the analysis. As noted earlier, age at menarche is maintained to be an acceptable instrument throughout 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 to 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 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; 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).[16] 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

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[15]In early regressions we included the ratio of family income to the poverty line among the family background variables. Since it was insignificant for all groups and since many cases lack income data, we exclude it in results reported here.

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

women aged 15–19 using family planning services, marital and nonmarital fertility rates for women aged 15–19, and similar variables listed in the table.

The wage equation contains instrumented values of fertility, schooling, early work experience, and adult work experience, as well as the local unemployment rate for the year the wage is measured as an indicator of local labor market conditions in adulthood, a dummy variable for residing in an urban area, a dummy variable for the presence of health limitations, and year and region dummies. Panel 3 of Table 1 lists the means of the wage equation's exogenous variables.

The final sets of acceptable instruments for schooling and the two types of work experience were selected from the measures of family background and local labor market, economic, and social conditions during adolescence, shown in panel 2 of Table 1, and the indicators of the policy environment, availability of abortion and family planning services, and the social context within which fertility decisions are made, shown in panel 4.[17]

RESULTS

Table 2 displays observed means of schooling, experience, and adult wages among women who became mothers before age 20 and those who avoided teenage parenthood. The simple differences are large. On average, white teenage mothers complete 2.4 years less schooling, and have 0.6 years (40

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[17]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, each instrument set includes age of menarche and the variables indicating ages of consent for abortion and contraceptive use. The education model also includes county measures of the abortion rate, the presence of a large abortion provider, the sex ratio, and family planning clinics per 1,000 women. The early work experience model also includes county measures of the abortion rate, the presence of a large abortion provider and the fertility rate of unmarried white women aged 15–19. The later work experience model also includes county measures of the abortion rate, the presence of a large abortion provider, and the sex ratio.

For blacks, each instrument set includes age of menarche. The education model adds age of consent for abortion and county measures of the unmarried teenage fertility rate and the sex ratio. The early work experience model includes age of consent for abortion and county measures of the sex ratio. The later work experience model includes county measures of the unmarried teenage fertility rate and the sex ratio.

TABLE 2

Mean Schooling, Work Experience, and Wages by Teenage Fertility Experience for White and Black Women

	White Women		Black Women	
	Mother Before Age 20	Not a Teenage Mother	Mother Before Age 20	Not a Teenage Mother
Years of schooling	11.2	13.6	11.7	13.3
Years of early work experience	0.9	1.5	0.6	0.9
Years of adult work experience	2.2	3.5	2.0	2.8
Hourly wage (in 1990 dollars)	\$6.58	\$8.67	\$6.02	\$7.21

Source: Tabulations from the NLSY.

percent) less early work experience and 1.3 years (37 percent) less adult experience. Their mean hourly wage is $2.09 (24 percent) less than for women who avoid teenage motherhood. Differences for blacks are smaller but still sizable—1.6 years less schooling, 0.3 years (33 percent) less early experience, 0.8 years (29 percent) less early adult experience, and wages $1.19 (17 percent) lower.

Multivariate regression results in Table 3 show that the direct effects of teenage childbearing on human capital development are both statistically and substantively significant.[18] The two-stage least squares (2SLS) estimates for whites indicate that a birth before age 20 lowers completed years of schooling by 2.6 years, which is slightly more than the unconditional mean difference shown in Table 2. A birth before age 20 is estimated to lower early work experience by 1.2 years and adult work experience by 2.2 years—also very large numbers. For black women the negative effect on schooling of a birth before age 20 is 2.5 years, nearly identical to the white estimate. Significant negative effects on early work experience again appear, but the effect of teen childbearing on adult experience is not significant, and the large standard error suggests that the fertility effect is poorly identified in this model.

The OLS results in columns 3 and 4 of Table 3 also show statistically and substantively significant effects of teenage childbearing on human capital development, although they are smaller than the IV estimates. This is not the expected pattern; the usual story is that early childbearing and low educational attainment are the result of a joint optimizing process or influenced by common unobservable characteristics, and that the OLS estimates should overstate the effect of early childbearing on education. A recent paper by Angrist and Evans (1996), however, also finds that IV estimates of the effect of fertility on schooling outcomes are greater than OLS estimates, and the authors offer an explanation based on variability over the sample in the causal effects of fertility. In our case, the IV estimates, though they avoid the endogeneity bias of the OLS estimates, reflect the marginal impact of

[18]For brevity, Tables 3 and 4 only present coefficients on the key explanatory variables. Complete results for the first and second stage regressions are available from the first author.

TABLE 3

Impact of Teenage Childbearing on Human Capital Accumulation for White and Black Women

	Two-Stage Least Squares		Ordinary Least Squares	
Impact of teenage childbearing on	Whites	Blacks	Whites	Blacks
1. Years of schooling	-2.59**	-2.54**	-1.42**	-1.13**
	(.86)	(.94)	(.11)	(.11)
Hausman p	.15	.10		
Adj. R^2	.366	.273	.411	.366
2. Early work experience	-1.20**	-1.79**	-.67**	-.22**
	(.42)	(.66)	(.06)	(.05)
Hausman p	.20	.00		
Adj. R^2	.070	.054	.122	.138
3. Adult work experience	-2.24**	.27	-1.14**	-.62**
	(.80)	(1.50)	(.11)	(.15)
Hausman p	.15	.54		
Adj. R^2	.080	.148	.133	.166

Notes: Standard errors are in parentheses. The Hausman p shows the confidence level for rejecting the null hypothesis that teen childbearing is exogenous.

* = significant at .10 level.
** = significant at .05 level.

teenage childbearing on schooling and work experience for that portion of the sample whose fertility has been affected by variation in the instruments. Since many of the acceptable instruments in these models measure access to abortion and family planning services, one explanation for the relatively large IV estimates is that teenage mothers facing high costs of fertility control, and who would have avoided early childbearing had these costs been lower, experience larger human capital losses than the average teenage mother.[19] The natural experiment studies, which compare teenage mothers with a narrowly defined comparison group (such as teenagers who experienced miscarriages) suffer, more obviously, from the same limitation—they estimate causal impacts of fertility for a (possibly atypical) subsample of the relevant population.

Though the OLS and IV estimates differ, the Hausman tests of the exogeneity of fertility should lead us to be cautious in interpreting these differences. For two of the three black estimates, Hausman tests indicate we can reject at the .10 level or better the hypothesis that fertility is exogenous. For the white sample, the probabilities lie between .15 and .20. Even in such cases, it may be imprudent to accept the OLS estimates. 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.[20] For p values that do not decisively reject or fail to reject the assumption of exogeneity, the IV estimates may be preferred to OLS, since they are unbiased whether or not the exogeneity assumption is true. With this ambiguity, it would be a mistake to

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[19]Angrist and Evans (1996) focus on the effects of the 1970 state abortion reforms on teen marriage and fertility, out-of-wedlock childbearing, and the schooling and labor market consequences for mothers, and changes in abortion availability are the basis of their IV estimates of the effects of fertility on schooling.

[20]For instance, while a .05 significance level implies that the risk of rejecting the null hypothesis when it is true (Type I error) is 5 percent, it does not imply that the risk of accepting a false null hypothesis (Type II error) is also 5 percent. On the contrary, the risk of a Type II error is inversely related to the risk of a Type I error. Type II errors are typically not bounded in the way Type I errors are, although Type II errors are typically somewhat lower than $(1-\alpha)$.

overemphasize the differences between the two sets of estimates. However, the results in Table 3 show clearly that teenage childbearing has significant adverse impacts 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.

Table 4 reports the estimates from the wage equations. The 2SLS wage equations in columns 1 and 2 show significant, strong effects of schooling on wages. The coefficients on early work experience are insignificant. Thus, the significant loss of early experience for teenage mothers shown in Table 3 does not appear to carry a wage penalty in the long run. For whites, the coefficient on adult experience is significant and large, but for black women the coefficient is not significant.

Table 4 indicates that years of schooling and, for white women only, adult work experience have significant positive effects on adult wages. Combining the 2SLS estimates with those in Tables 3, we can show that teenage childbearing has large indirect effects on wages which operate via its impact on human capital accumulation. The first column in Table 4 shows that a 1-year change in schooling is associated with a change of 0.11 in the logarithm of wages for white women. Since, from row 1, column 1 of Table 3, a teen birth lowers schooling by 2.59 years, a teen birth is predicted to change the logarithm of wages by -2.59 x 0.11 = -0.285 through its schooling effect.

Carrying out such calculations using all the 2SLS point estimates in Tables 3 and 4, whether significant or not, suggests that the indirect effects of a teenage birth lower wages 44 percent for whites and 33 percent for blacks. Calculations based on the OLS results yield smaller respective wage losses of 23 and 13 percent. In contrast, the unadjusted differences between the wages of teen mothers and those who delayed childbearing are 24 percent for whites and 16.5 percent for blacks.

Hausman tests indicate that we cannot, in general, decisively reject the hypothesis of exogeneity of the human capital variables in the wage equation. For whites, two of the three tests fail to reject the hypothesis that an explanatory variable is exogenous at even the .30 level. For blacks, none of the tests

TABLE 4

Impact of Education and Work Experience on the Logarithm of Adult Wages for White and Black Women

	Two-Stage Least Squares		Ordinary Least Squares	
Impact on log wages of	Whites	Blacks	Whites	Blacks
1. Years of schooling	.11**	.10**	.09**	.07**
	(.02)	(.02)	(.01)	(.01)
Hausman p	.10	.19		
2. Early work experience	-.01	.09	.03*	.03
	(.07)	(.07)	(.02)	(.02)
Hausman p	.50	.38		
3. Adult work experience	.14**	.03	.10**	.09**
	(.05)	(.05)	(.01)	(.01)
Hausman p	.30	.11		
Selection correction	.26**	.25**	.14	.22**
	(.13)	(.10)	(.11)	(.09)
R^2			.24	.27

Notes: Standard errors are in parentheses. The Hausman p shows the confidence level for rejecting the null hypothesis that the relevant variable is exogenous.

* = significant at .10 level.
** = significant at .05 level.

reject the exogeneity hypothesis at better than the .10 level. The OLS and 2SLS results are very similar and, regardless of which estimates one uses, the main implication of Table 4 is clear: Teenage childbearing leads to substantively important wage losses through reductions in formal education and young adult work experience.

The models reported in Table 5 allow for the possibility that the rates of return to human capital investments by teenage mothers differ from those experienced by the rest of the sample. The household responsibilities associated with early childbearing might reduce the effort that young women put into schooling and their jobs, and reduce the observed rate of return. On the other hand, the presence of a child to support might increase a young woman's motivation, effort, and rates of return. Table 5 shows that, for white women, the rates of return to schooling and work experience for young mothers are not significantly different from those of the rest of the sample, and the results of Table 4 are essentially unchanged. For black women, however, we estimate a rate of return to schooling that is significantly higher for teenage mothers than for nonmothers, and the rate of return to early work experience is positive and significant, but only for young women who delay childbearing to age 20 or later. The average effects of teenage childbearing on wages change very little for young black women with the inclusion of the interaction terms, although they fall somewhat for white women.

CONCLUSION

How does adolescent fertility affect the human capital and adult wages of women? Our 2SLS results indicate that adolescent fertility substantially reduces the human capital investments of young women. Young white mothers earn less because they obtain less formal education and work experience, but early childbearing does not change the rates of return to these investments. For black women, early childbearing has adverse effects on schooling and thus on wages. A model that includes interaction terms

TABLE 5

Impact of Education and Work Experience on the Logarithm of Adult Wages, with Differential Rates of Return for Teenage Mothers

Impact on log wages of	Two-Stage Least Squares Whites	Two-Stage Least Squares Blacks	Ordinary Least Squares Whites	Ordinary Least Squares Blacks
1. Years of schooling	.09**	.13**	.10**	.08**
	(.03)	(.03)	(.01)	(.01)
Hausman p	.78	.02		
2. Schooling * Teen Birth	-.00	.04*	.00	.00
	(.04)	(.02)	(.01)	(.01)
Hausman p	.85	.05		
3. Early work experience	-.05	.16*	.03*	.02
	(.09)	(.09)	(.02)	(.03)
Hausman p	.33	.08		
4. Early * Teen Birth	-.08	-.19	.01	.05
	(.28)	(.22)	(.04)	(.05)
Hausman p	.77	.20		
5. Adult work experience	.14**	.07	.10**	.10**
	(.05)	(.05)	(.01)	(.01)
Hausman p	.42	.44		
6. Adult * Teen Birth	-.06	-.03	.01	-.02
	(.12)	(.08)	(.02)	(.02)
Hausman p	.51	.95		
Selection correction	.25*	.27**	.13	.22**
	(.13)	(.11)	(.11)	(.08)
R^2			.24	.27

Notes: Standard errors are in parentheses. The Hausman p shows the confidence level for rejecting the null hypothesis that the relevant variable is exogenous.

* = significant at .10 level.
** = significant at .05 level.

shows that the schooling effect is partially offset by a significantly higher rate of return for black teenage mothers, but teen mothers do not receive a positive return to early work experience. The negative wage effects of lower education and work experience are substantively important. The smallest point estimates indicate that teenage childbearing reduces white women's wages by 23 percent and black women's wages by 13 percent; estimates that control for the endogeneity of fertility and human capital investment are somewhat larger. We would expect these wage decreases to have serious negative impacts on the economic well-being of many young mothers and their children.

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 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 suggests 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. The public policy issues that depend on the causal effects of early fertility are substantive ones; our results suggest that measures that reduce teenage childbearing will have positive effects on the economic prospects of young women and their families.

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