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SOURCES OF BIAS IN WOMEN'S WAGE EQUATIONS: RESULTS USING SIBLING DATA

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ABSTRACT

We use data on sisters to jointly address heterogeneity bias and endogeneity bias in estimates of wage equations for women. This analysis yields evidence of biases in OLS estimates of wage equations for white and black women, some of which are detected only when these two sources of bias are addressed simultaneously. For both white and black women there is evidence of upward bias in the estimated returns to schooling. Bias-corrected estimates of the effect of marriage on wages, for white women, suggest a positive marriage premium. We also use the sibling data to identify our models, and test a number of other commonly used identifying assumptions as overidentifying restrictions.

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I. Introduction

Wage equations estimated for samples of women are used frequently in labor economics. Often the wage equation estimates are the direct objects of interest. Examples include: estimating wage discrimination via "decomposition" techniques; documenting and explaining the rise in women's earnings relative to men's; testing theories explaining wage differentials between men and women; and estimating effects of demographic decisions or outcomes on wages.[1] In addition, estimates of women's wage equations are often used as initial inputs in studying other questions. Examples include: constructing instrumental variables for wages to estimate labor supply parameters (*e.g.*, Mroz, 1987; Nakamura and Nakamura, 1985a); computation of wage values in microanalytic simulation models (*e.g.*, Orcutt and Glazer, 1980); and legal applications regarding pay discrimination or computations of potential earnings (*e.g.*, Bloom and Killingsworth, 1982).

Much of this research uses standard OLS estimates of variants of the human capital earnings function. But much of this research also recognizes the potential for biases from endogeneity of the regressors, and from unobserved heterogeneity associated with the regressors. Different researchers address alternative sources of bias, using varying identifying assumptions. At the same time, some recent research papers in this area continue to use OLS estimates of wage equations for women (*e.g.*, O'Neill and Polachek, 1991). In general, our reading of the literature on women's wage equations, summarized in Section III, suggests that there is no consensus regarding the empirical importance of these various sources of bias, nor the validity of the assumptions used to attempt to correct for them; as a consequence, there is no consensus regarding the treatment of these alternative

[1]Representative examples of this research are listed in Table 1, discussed in Section III.

sources of bias.

In this paper, we utilize data on sisters in an attempt to provide a more unified and compelling analysis of sources of bias in women's wage equations. Data on sisters offer advantages for estimation of wage equations correcting for endogeneity and heterogeneity bias; these advantages are spelled out in Section IV. The paper shows how the estimated effects of marital status, number of children, labor market experience, and schooling differ depending on the source of bias considered, and the identifying assumptions used to correct for the bias. It also exploits the sibling data to test a variety of overidentifying assumptions. Our goal is to build more of a consensus in the statistical approaches to estimating women's wage equations.

II. Sources of Bias in Women's Wage Equations

Past research on women's wage equations has considered three principal sources of potential bias: heterogeneity bias, endogeneity bias, and bias from selection into employment. In this paper, we focus on the first two sources of bias, based on our past research (Korenman and Neumark, 1992) showing that heterogeneity and endogeneity bias are important in estimates of women's wage equations.[2] The wage equation we are interested in estimating takes the general form

$$(1) \quad w_i = X_i\beta + Y_i\gamma + \epsilon_i \ ,$$

where w is the log wage, X is a vector of exogenous control variables, and ϵ is a stochastic normally-distributed error uncorrelated with X, assumed to be i.i.d. Y is the vector of

[2]Another reason we do not analyze sample-selection bias and heterogeneity bias jointly is because with a small number of observations per family, estimates of family fixed effects (in contrast to the other parameters of the model) in a probit (or logit) model are inconsistent (Maddala, 1987).

variables on which attention is focused. For the purposes of exposition, we will assume that Y contains a single variable; for concreteness in providing examples, we will suppose this variable is the number of children.

Heterogeneity bias may arise if the true wage equation also includes an unmeasured variable, denoted A, which is correlated with Y.[3] For example, A may represent unobserved characteristics that raise market productivity relative to home productivity, and therefore increase wages, and (via substitution effects) reduce childbearing. A standard approach to this problem is to make the identifying assumption that A is fixed over time, with a constant coefficient. Then for repeated observations on individuals, the true wage equation is

(2) $w_{it} = X_{it}\beta + Y_{it}\gamma + A_i + \epsilon_{it}$,

where t indexes time.[4] Because there are repeated observations on each unit i, individual-specific dummy variables can be included to control for the unobservable A, yielding consistent estimates of γ; this is the fixed-effects estimator.

Endogeneity bias may arise if childbearing is simultaneously determined along with wages. For example, a draw of a high wage residual (*i.e.*, a high wage net of observables) may lead, via substitution effects, to lower childbearing. In this case, though, the bias comes from a contemporaneous correlation between ϵ and Y, so that a fixed-effects estimator does not solve the problem. The only potential correction for this type of bias is to assume that there is an instrumental variable Z, which is correlated with Y, but does not itself enter the

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[3]Because this is a study of bias in women's wage equations, we do not explore random effects estimation.

[4]Another implicit assumption, which can be relaxed, is that the other coefficients (β and γ) are constant across time.

wage equation. In principle, then, there is an equation for Y of the form

$$(3) \qquad Y_i = Z_i\delta + X_i\beta' + \eta_i \ ,$$

where η also satisfies the standard assumptions. Two-stage least squares can then be used to obtain consistent estimates of γ by estimating equation (3) by OLS, substituting the fitted values from (3) for Y_i in equation (1), and estimating equation (1) by least squares. (The standard errors must be estimated by using the actual, not the fitted values of Y_i.)

III. Past Research: An Overview

As we pointed out in the Introduction, previous researchers have considered these sources of bias. Table 1 provides a chronological survey of this literature. The summary of this research provided in the table supports the claims made in the Introduction. Different researchers address alternative sources of bias, most frequently addressing a single source of bias (such as endogeneity bias), in a limited set of variables (such as experience). Second, different researchers invoke alternative identifying assumptions to address the same source of bias (compare, *e.g.*, Mincer and Polachek, 1974, to Wright and Ermisch, 1991). Finally, some recent research papers continue to use OLS estimates of wage equations for women. The lack of consensus is apparent.

In Korenman and Neumark (1992), we conducted an analysis of the sensitivity of wage equation estimates for women to heterogeneity and endogeneity bias (as well as selection bias), considered individually. Our principal concern was with wage differences associated with marital status and number of children, which turn out to be closely related to returns to labor force attachment (experience and tenure); together, these coefficients are the focus of much of the research cited above. Our results offer one reason why no consensus has emerged regarding the treatment of these biases. Specifically, the resulting coefficient estimates are sensitive to the source of bias that is addressed; details are summarized in Section V.b. below. The other reason that no consensus in the treatment of these sources of bias has emerged may be skepticism regarding the identifying assumptions needed to correct for each of these sources of bias. In Korenman and Neumark (1992) we provide tests of the overidentifying assumptions whenever possible, but *a priori* identifying assumptions are always required, the validity of which must necessarily remain untested.[5] The sibling data set that we use in this paper offer numerous advantages relative to earlier research by ourselves and others.

IV. Advantages of Using Sibling Data

IV.a. Estimation

We can best highlight the potential advantages of using sibling data to study sources of bias in women's wage equations by referring to our earlier work (Korenman and Neumark, 1992), which used more standard cross-sectional and longitudinal panel data. In our earlier paper, we attempted to correct for heterogeneity bias in the estimated effects of marriage and children by differencing data on individuals over time (specifically, over a two-year interval). We recognized the danger that such an estimator may implicitly control for experience and tenure if there is little variation in labor force attachment (*i.e.*, experience and tenure) among women who work in both years.[6] Consequently, it may have

[5]In estimates correcting for heterogeneity, the test is based on Heckman and Hotz (1989). For the instrumental variables estimates, we report tests of overidentifying restrictions, given *a priori* assumptions regarding exclusion restrictions that secure identification.

[6]Although a first-difference estimator computed over a long interval ought to mitigate this problem, specification tests (from Heckman and Hotz, 1989) indicated that only a first-difference estimator computed over a short interval could be used.

been difficult to untangle the independent roles of labor force attachment and heterogeneity bias in generating wage differentials associated with children. In contrast, sibling data allow us to difference across sisters rather than across time; differencing across sisters should not implicitly control for labor force attachment. Of course, sister differences remove heterogeneity bias only under the assumption that such bias results from unmeasured attributes that are common to sisters, such as, for example, unmeasured (and equal) parental investment in their daughters' human capital.[7]

Second, we assume that family background and attitudes/expectations variables that are fixed over time (in contrast to contemporaneous or lagged variables that change over time), can be excluded from the wage equation, and hence provide the identifying information for instrumental variables estimation.[8] As a consequence of this assumption, repeated observations on individuals do not permit estimation of wage equations that simultaneously account for heterogeneity and endogeneity. Sibling data, however, allow us to address jointly heterogeneity bias and endogeneity bias, using these fixed family background and attitudes/expectations variables as instruments.

Econometrically, it is straightforward to study heterogeneity bias and endogeneity bias simultaneously. Let the wage equation be given by equation (2), where Y is now a potentially endogenous variable, and A is an unmeasured, time-invariant variable potentially correlated with Y. Assume that there is a set of instrumental variables Z that identify the

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[7]Previous research suggests sister-sister correlations in wage equation residuals on the order of 0.2 (*e.g.*, Shackett, 1981). We give more direct evidence on family vs. individual fixed effects below.

[8]In particular, we used a set of instruments describing sample respondents' family background, and measures of gender-role attitudes and fertility, marriage and educational expectations that were asked generally more than ten years prior to the period to which wages and labor market characteristics pertain.

parameters of equation (2), such that

(4) $$Y_{it} = Z_{it}\delta + X_{it}\beta' + A_i\phi + \eta_{it} ,$$

where η also satisfies the standard assumptions. This parellels equation (3), but also includes the fixed effect A, to reflect the fact that there can be endogeneity bias and heterogeneity bias at the same time. Nonetheless, the same two-stage least squares procedure can be used to recover consistent estimates of the wage equation. The only change is that individual fixed effects are included in both the first- and second-stage regressions; these fixed effects in equation (4) must be estimated to construct the fitted values of Y.

In principle, instrumental variables estimation is sufficient to solve the endogeneity and the heterogeneity problem without repeated observations (and hence without the estimation of individual-specific intercepts). This requires instruments that are uncorrelated with A as well as ϵ (see, *e.g.*, Heckman and Robb, 1986). Except in instances of the occurrence of "natural experiments" (*e.g.*, Angrist and Krueger, 1991), this seems an overly strong requirement of instruments. For example, family background variables such as parents' education may be correlated with the individual's schooling and uncorrelated with the nonsystematic part of the error structure ϵ, but correlated with the individual-specific component, A (see Griliches, 1979).

Assuming that repeated observations are required to account for heterogeneity bias, the difficulty with using fixed family background or attitudes/expectations variables as instruments in a standard longitudinal data set is that they are fixed over time for an individual. This is not a matter of data availability, but instead reflects our belief that it is unlikely that there exist contemporaneous or lagged (changing) variables that provide valid instruments in wage equations. The problem this poses can be seen easily from equation

(4); if Z is time-invariant for an individual, it is perfectly collinear with the individual fixed effect A, and hence provides no identifying information. (Equivalently, if we difference equation (2) and equation (4), the instrument set Z drops out.) Thus, given the restriction to time-invariant instrumental variables, endogeneity cannot be addressed in the context of the longitudinal within specification.

An alternative approach is to use data on sisters, attempting to eliminate the heterogeneity bias by computing within-family estimates. The assumption here is that the source of heterogeneity bias is something common to sisters. In this case, A_i indicates a family-specific fixed effect, the "i" in the subscripts indexes families, and the "t" indexes sisters within families. Other than this, the same assumptions are made regarding the regressors, A_i, and ϵ_{it}; in particular, only the family-specific component of the unobservable determinants of wages are correlated with the regressors. The advantage of panel data of this type, conditional on the validity of the assumptions, is that the instrumental variables are not necessarily constant across the repeated observations (*i.e.*, sisters in the same family). Consequently, with sibling data the two-stage least squares procedure accounting for heterogeneity, described above, can be carried out. What information identifies the model in the presence of fixed family effects? Because there are fixed family effects in the wage equation, these fixed effects also appear in the first-stage reduced-form regressions for the potentially endogenous variables.[9] The implication is that within-family differences in the family background and attitudes/expectations variables provide the identifying information.

[9]Because these first-stage regressions are reduced forms, we do not know whether the fixed family effects represent common family influences on wages, which in turn affect the potentially endogenous variables, or instead represent direct common influences on the endogenous variables.

There are at least two alternative approaches to jointly addressing heterogeneity and endogeneity biases, by introducing alternative identifying assumptions that could be used with either longitudinal or sibling data. The first, in the context of either longitudinal or sibling data, is to use a woman's own lagged characteristics (experience, number of children, etc.) as instruments for contemporaneous values. However, it seems plausible that, for example, the pattern of accumulation of experience, or the pattern of past child bearing, may have an effect on wages independently of the contemporaneous value of experience or number of children; this is precisely the point of the intermittent labor force participation literature developed by Polachek and others.

Second, as Table 1 indicates, spouse's income and spouse's unemployment (or employment) have sometimes been used as instruments; these instruments could in principle also be used with longitudinal or sibling data. But models of family specialization with respect to labor supply, human capital investment, and the allocation of effort (Becker, 1985; Killingsworth, 1990) suggest that a wife's wage may be influenced by her husband's wages, labor supply, etc.[10]

More generally, however, the sibling approach offers a potentially better means of

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[10]Yet another possibility is that the wage equation (2) could be differenced across time, with levels of the family background and attitudes/expectations variables used as instruments for the changes in the potentially endogenous variables. In order for this approach to be valid the following equation must hold:

$$\Delta Y_{it} = Z_i\delta + \Delta X_{it}\beta' + \Delta\eta_{it},$$

that is, Z_i must remain in the differenced equation for Y. The implication is that the variable $Z_i\delta \cdot t$ must appear in the (level) equation for Y. This specification may not be unreasonable for, say, experience, since it implies that the growth rate of Y is related to Z, but it does seem an inappropriate specification for marital and fertility status. Furthermore, we want to consider the potential endogeneity of schooling, which drops out in this approach. Finally, there may be considerably less scope for endogeneity of experience in a short first difference than in a cross-section, so that this set-up may not be fruitful for studying this question.

controlling for heterogeneity--differencing across sisters rather than across a short interval--which does not implicitly control for labor force attachment, and does not therefore remove a potentially important source of covariation between number of children and wages.

IV.b. Identification

In addition to the identifying assumptions mentioned above, a sister's fertility, experience, *etc.*, would seem to be valid instruments for a respondent's own values of these variables, since the sister's value may reflect other unobservable characteristics of the family that affect the respondent's values of the variables that are not captured in the observable instrumental variables, yet have no independent effect on the respondent's wage.[11,12] Unfortunately, in the fixed family effects specifications, the sibling's value of the endogenous variable cannot be used as an instrument, since controlling for the fixed family effect, the sibling value provides a near-perfect prediction of the endogenous variable.[13] However, we do carry out an alternative heterogeneity experiment in which we can use the

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[11]Of course no argument justifying an identifying assumption is fool-proof. It could be argued that a respondent may choose a low-wage job because her sister has high schooling and experience, and therefore a high actual or potential wage, some of which may be transferred to the respondent. However, in the 1985 wave of the NLS Young Women's survey, only 16 out of 3708 women reported transfers from siblings (brothers or sisters), so this seems an unlikely problem.

[12]If we had repeated observations on sibling values of the endogenous variables, we could in principle use these as instruments while correcting for heterogeneity in longitudinal data for individuals, since these sibling values can change over time. But to the extent that the sibling values reflect omitted fixed characteristics, variation over time in the sibling values does not provide any identifying information (in contrast, say, to the mean of the sibling values over many periods).

[13]In a sample containing pairs of siblings only, the family-specific dummy variable and the sibling's value of the endogenous variable provide a perfect fit for the respondent's value of the endogenous variable. This is nearly, but not exactly so, in a sample with triplets of siblings as well as pairs, as we have in our sample.

sibling values as instruments. We can then make the *a priori* identifying assumption that sibling values of the potentially endogenous variables are excluded from the wage equation, and then test the exclusion of the family background and attitudes/expectations variables from the wage equation.[14] We also extend the analysis to consider the validity of identifying assumptions used by other researchers to correct for endogeneity, as described in Table 1. In particular, with the sibling data in hand, we can test, as overidentifying restrictions, some typical identifying assumptions invoked by other researchers.

V. Empirical Results for Sibling Data Analyses

V.a. The Data

The Young Women's cohort of the National Longitudinal Survey contains over one thousand women who have at least one sister in the survey. A sample of sisters was constructed in two steps. First, data on wages, labor market characteristics, and the instrumental variables were extracted, looking first in 1982, but if data were missing (perhaps because of non-employment), then taking data from 1980, 1978, 1977, 1975, or 1973, if available, but always drawing data from the latest year possible. All combinations of sisters in the resulting extract were matched. This yielded a final sample of 766 observations. Of these, there are 518 whites and 248 blacks;[15] it turns out, as reported below, that there is statistical evidence against pooling these two samples. Descriptive statistics for the white and black samples used for the panel data analysis are reported in

[14]This analysis has some parallels in earlier research for men by Chamberlain and Griliches (1977), using a sibling's ability test score to instrument for test scores in wage equations.

[15]For expository ease, we refer to "non-blacks" as "whites," The Young Women's NLS did not distinguish Hispanics.

Appendix Table A1.[16]

V.b. Heterogeneity Bias

In our earlier paper, OLS estimates of women's wage equations excluding experience and tenure suggested large negative effects of children, but controlling for actual experience and tenure lowered the estimated effects considerably, consistent with the findings of, *e.g.*, Hill (1979). These estimates suggested that the effects of children were primarily "indirect," reducing wages by reducing mothers' labor force attachment. But these estimates were quite sensitive to corrections for heterogeneity or endogeneity bias. Longitudinal within estimates excluding experience and tenure revealed no effect of children on wages, in contrast to the negative association found in the cross-sectional OLS estimates. On the other hand, when we treated labor market experience and tenure as endogenous variables, instrumenting with family background and attitudes/expectations variables, we found that the overall return to time in the labor force was near zero, and that children were associated with substantial negative effects on wages. Thus, the longitudinal estimates suggested that unobserved heterogeneity generates the cross-sectional negative association between children and wages (women who would earn low wages tend to have children), while the instrumental variables estimates suggested a "true" negative effect of children, whether because of discrimination or productivity, which was understated by OLS

[16]For sibships of more than two members, "sibling value" refers to the average over the other siblings. We maintain this definition throughout the paper.

The relatively smaller numbers of observations from particular years (1975, 1977, and 1980) reflect the need to construct the number of children variable from retrospective information for these years.

estimates.[17,18]

Table 2, columns (1)-(4), reports OLS and fixed family effects estimates of a log wage equation specification similar to that used in Korenman and Neumark (1992). We have simplified the specification by using a single "married, spouse present" category, a linear specification for the number of children variable, and including only experience (omitting tenure).[19] The OLS and fixed-effects estimates for this specification are reported in columns (1) and (2) of Table 3. The estimates are reported for specifications first excluding, and then including experience, for white women. The OLS estimates are similar to our earlier estimates. With experience excluded, in column (1), there is a large and statistically significant negative coefficient on the number of children variable. On the other hand, there is no statistically significant effect of marriage. When experience is added to the equation, in column (2), the coefficient on the number of children variable falls by about one fifth, but remains statistically significant.

Columns (3) and (4) report estimates from specifications that allow a fixed family effect. In the previous paper we were cautious in interpreting our longitudinal (or fixed

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[17]This difference in the instrumental variables and first difference results may also reflect our concerns, noted in Section IV, regarding using instrumental variables estimation to account for heterogeneity.

[18]In contrast to these findings, there was no evidence of bias from selective employment in either the cross-sectional or longitudinal estimates.

[19]OLS and fixed-effects results reported for this specification in the remaining tables were qualitatively unchanged by using the specification in Korenman and Neumark (1992), including both "married, spouse present" and "divorced or separated" dummy variables, dummy variables for one, and for two or more children, and including both experience and tenure. Experiments with this richer specification revealed that serious multicollinearity plagued estimates instrumenting for both experience and tenure (especially when fixed family effects were included). Nonetheless, the instrumental variables estimates indicated no change in the estimated effect of children on wages, just as in the results reported in the paper using experience only.

individual-effects) results because we used a short (two-year) first difference that may have implicitly controlled for labor force attachment. Among women working in both 1980 and 1982, there was relatively little variation in accumulated experience. As a result, the first-difference estimates of the effects of children that excluded experience (and tenure) controls may have been biased towards zero.

The within-family estimates reported in columns (3) and (4) are consistent with this suspicion. When experience is omitted from the model, the negative association between wages and children persists in the fixed-effects estimates (column (3)), whereas in our previous paper it fell to zero. These results suggest two differences with respect to our earlier work. First, the heterogeneity bias detected using longitudinal data, even in specifications excluding labor force attachment controls, was overstated, and at least partially reflected the effect of implicitly controlling for this attachment.[20] Compared to our earlier results, these estimates suggest that differences in labor force attachment are a more important determinant of wage differentials associated with children, even after accounting for heterogeneity; when experience is included in the model, in column (4), the negative association between wages and children falls by about one-third. Second, a statistically significant negative association between children and wages persists in the fixed-effects estimates. However, as in our previous work, in either the OLS or fixed-effects estimates there is no evidence to support a negative effect of marriage that a simple interpretation of Becker's (1985) model of specialization within marriage might lead one to

[20]Consistent with this, the Hausman specification test does not reject the exclusion of fixed effects (and hence the use of OLS) from the specification either including or excluding experience. Of course, this result does not necessarily imply an absence of heterogeneity bias in estimates accounting for endogeneity, so we retain the family fixed effects in the instrumental variables estimation that follows.

predict.

An alternative reason for the differences we have found between individual and family fixed-effects estimates of the effects of children on wages is that the two estimators may remove different fixed effects; in particular, the family fixed effect may not capture all of the individual heterogeneity that potentially biases the estimates. While there is no way to address this question definitively, in columns (5) and (6) of Table 2 we offer some evidence. We reestimated the OLS and family fixed-effect wage equation adding the individual's lagged wage. One interpretation of a significant coefficient on the individual's lagged wage in the family fixed-effects estimates is that the family fixed effect does not sufficiently capture the heterogeneity.[21] The significant, positive coefficient of the lagged wage is consistent with this interpretation. However, this evidence is only suggestive, since the relevant question is whether the family fixed effect controls for the heterogeneity that is correlated with the included variables, not whether the lagged individual's wage has explanatory power once the family effect is included. To shed light on this question, we can ask whether the inclusion of the family fixed effect or the lagged individual wage appears to have more impact on the coefficient estimates, relative to the OLS estimates. With respect to the number of children coefficient, the inclusion of the family fixed effect has a relatively larger impact, reducing the coefficient by .024 (from -.074 to -.050), whereas the impact of adding the lagged wage is smaller. While this suggests that the family fixed effect may remove much of the bias, we cannot rule out the possibility that the family fixed effect does not adequately remove heterogeneity bias.

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[21]This parallels the test of the fixed-effect assumption in longitudinal data on individuals proposed by Heckman and Hotz (1989), entailing including earlier values of the dependent variable in first-difference specifications.

V.c. Endogeneity Bias in the Estimated Effects of Experience, Marital Status, Number of Children, and Schooling

In Table 3 we present results from specifications in which we address endogeneity of experience, marital status, number of children, and schooling, along with heterogeneity bias. Our earlier work indicated that potential endogeneity bias in the estimated returns to labor force attachment had serious consequences for the estimated effects of children on wages. A Hausman test led us to reject the joint hypothesis of the exogeneity of experience and tenure, and IV estimates led to overall returns to labor force attachment that were near zero. Because women with children have lower experience and tenure than women without children, we found that the reduction in the return to time in the labor force in turn led to a much stronger negative association between children and wages than suggested by the OLS estimates.

In column (1) of Table 3 we report specification tests and final estimates of the wage equation with family fixed effects, and endogenous experience. We use the data on sisters to account for heterogeneity while also instrumenting for experience. We began by estimating an unrestricted specification in which the attitudes/expectations variables were included in the wage equation. The identifying assumption is that the family background variables are excluded from the wage equation.[22] (In Section V.e., below, we use the sibling data coupled with an alternative heterogeneity experiment to explore the validity of excluding the family background variables from the wage equation.) We then tested the exclusion of the attitudes/expectations variables from the wage equation; this exclusion is rejected at the five percent significance level, as indicated by the p-value in the last row of

[22]These assumptions regarding the instruments are identical to those used in our earlier paper.

the table.[23,24] Consequently, for endogenous experience we report estimates of the less restricted model. The estimates reveal little change relative to the fixed-effects estimates in Table 2; the return to experience declines slightly, and the negative effect of children grows slightly (from -.050 to -.056). But the exogeneity of experience is not rejected; the p-value is .759.[25]

We also estimated models to examine the possible endogeneity of other determinants of women's wages, in particular, marital status, number of children, and schooling.[26] The results are reported in Table 3, columns (2)-(4), where again we first estimated the unrestricted model, including the attitudes/expectations variables in the wage equation, and then tested the exclusion of these variables from the wage equation. We report coefficient estimates for the model that survives this procedure. The benchmark for comparison is the

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[23]In all cases, we report the results from the joint test of significance of the instruments themselves, not including in the test the coefficients of the dummy variables indicating missing data for the potential instruments.

[24]This is not the test of the validity of instruments as suggested in Hausman and Taylor (1981), since the question of the correlation of the attitudes/expectations variables with the wage equation error does not arise. Rather, it is a simple Wald test of the joint significance of the coefficients of these variables in the wage equation.

[25]The absence of any change in the return to labor force attachment contrasts with the results from our earlier paper (Korenman and Neumark, 1992). However, results not reported in the tables reveal that the difference in results in not attributable to the omission of tenure, nor is it attributable to the inclusion of family fixed effects. The remaining candidate explanation is changes in the sample, which is necessarily much smaller in the present paper. Given the large standard errors of the instrumental-variables estimates of the returns to experience (or tenure), it is not surprising that the point estimates are fragile.

[26]Korenman and Neumark (1992) considered jointly instrumenting for experience and tenure as well as number of children and marital status. The resulting estimates were imprecise; in particular, there was no statistically significant evidence against the exogeneity of marital status and number of children.

fixed-effects specification reported in column (4) of Table 2.[27]

In the specification treating marital status as endogenous, in column (2) of Table 3, the exclusion of the attitudes/expectations variables from the wage equation is not rejected at the five-percent significance level, so the restricted model is reported. Instrumenting for marital status leads to a sharp increase in the coefficient on the marital status dummy, resulting in a statistically significant positive effect of marriage on women's wages. This may reflect downward bias in the estimated coefficient of marital status in the pure fixed-effects specification; while a point estimate of .463 seems dubiously high, it is plausible that there is downward bias in the estimate treating marital status as exogenous, if women with low contemporaneous wage draws are more likely to become married. (Alternatively, the bias may be generated by individual heterogeneity that is unrelated to the fixed family effect, such that low wage women, net of observables and the fixed family effect, tend to marry.) Furthermore, this result was replicated in alternative specifications. We computed IV/FE estimates adding a second dummy variable for divorced or separated, and alternatively did and did not treat the divorced or separated dummy variable as endogenous; the point estimate of the married, spouse present variable remained positive and statistically significant, and the p-values for tests of exogeneity were .01. Also, this result is not attributable to the instability of the estimated effects of marriage and children; in a specification excluding the number of children, the coefficient (standard error) of the married, spouse present dummy variable was .55 (.26), and the p-value for the exogeneity

[27]Strictly speaking, depending on the results of the tests of the exclusion restrictions, the IV/FE estimates should perhaps be compared with fixed-effects estimates including the attitudes/expectations variables. But the coefficients reported in Table 2 were little changed by including these variables.

test was .01.[28] On the other hand, pure instrumental variables estimates (without fixed effects) led to no change in the marital status coefficient, and no evidence against its exogeneity, implying that heterogeneity is important.[29,30]

In contrast to the results for marital status, instrumenting for the number of children has no significant effect on the estimated effect of the number of children, and exogeneity is not rejected. The results for the schooling coefficient are suggestive of upward endogeneity bias; the p-value for the Hausman specification test (.096) indicates that exogeneity is rejected at the ten-percent level. The point estimate of the schooling coefficient, .020, has a high standard error, since it comes from the unrestricted wage equation; we report this specification in the table since there was evidence, at the ten-percent significance level, against the exclusion of the attitudes/expectations variables from the wage equation. Nonetheless, the results were qualitatively similar in the restricted model; the coefficient (standard error) of schooling was .049 (.019), and the p-value for the exogeneity test was .12.[31]

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[28]The pure fixed-effects estimate was .02 (.05).

[29]However, there is not a straightforward Hausman-type test of the IV vs. the IV/FE specification, since the inclusion of the fixed family effects in the first-stage regressions of the IV/FE estimation can (and does) make the IV/FE estimates more efficient than the IV estimates.

[30]In all cases, a linear probability model was used for marital status, given that fixed effects cannot be consistently estimated in dichotomous choice models.

[31]Appendix Table A2 reports the coefficients of the family background and attitudes/expectations variables from the first-stage regressions corresponding to Table 3, columns (1)-(4).

Nakamura and Nakamura (1985b) show that the power of Hausman exogeneity tests falls as the R-square values from these first-stage regressions fall. In fact, the adjusted R-squares for the "ever married" and tenure regressions, reported in Table A2, are relatively low, while the evidence against exogeneity is strongest for these variables. This has two implications: first, the likelihood that the power of the exogeneity test for marital status was

Finally, in column (5) these three variables as well as experience are allowed to be endogenous. Overall, the specification test nearly rejects exogeneity of the entire set of variables at the ten-percent level. Not surprisingly, the estimated standard errors are large relative to the fixed-effects estimates or the estimates instrumenting for one variable at a time. A significant positive effect of marriage persists, whereas the estimated schooling coefficient returns to a value nearer to the pure fixed-effects estimate, as does the return to experience.

V.d. Wage Equation Estimates for Black Women

Table 4 summarizes the key specifications estimated for black women. For purposes of comparison, in columns (1) and (2) of Table 4 we report OLS and fixed-effects estimates of the wage equation. A comparison with columns (1) and (2) of Table 3 reveals lower returns to schooling (in fixed-effects estimates), and to experience for black women, and no discernible negative effect of children.[32] These differences are statistically significant. In OLS and fixed-effects estimates in which the coefficients of the five variables reported in the table (and the intercept for OLS) differed, the equality of coefficients across black and white women was rejected at the five-percent level.

The fixed-effects estimation leads to a sizable reduction in the estimated return to

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low gives added credibility to the rejection of exogeneity of marital status; and second, the fact that we can reject exogeneity, in this sample, even for variables for which the instruments have relatively little explanatory power, means that our results from this sample are informative.

[32]Estimates of the effects of children were also near zero in specifications excluding experience. The OLS and fixed-effects estimates (standard errors) were .001 (.017), and -.005 (.024).

schooling (from .070 to .036).[33] The overall evidence of heterogeneity bias is statistically significant, as indicated by the p-value from the Hausman specification test (.014). Columns (3)-(7) report IV/FE estimates corresponding to those estimated for white women in Table 3. In all cases we report results for the restricted specification in which the attitudes/expectations variables (as well as the family background variables) are excluded from the wage equation; the exclusion of this set of variables from the wage equation could not be rejected in any of the columns. In contrast to the results for white women, there is no statistically significant evidence of endogeneity bias in the estimated coefficients of marital status, number of children, schooling, or experience and tenure.

V.e. Testing the Exclusion of the Family Background Variables from the Wage Equation

To this point, the maintained identifying assumption in the instrumental variables fixed-effects estimation has been that the family background variables can be excluded from the wage equation. While the sibling values of the endogenous variables may provide better *a priori* valid instruments, for reasons explained in Section IV we cannot use these values as instruments in specifications with fixed family effects. In Table 5 we report results from an alternative statistical experiment in which we attempt to account for heterogeneity by including the sibling's wage in the wage equation.[34] This experiment may provide a less adequate means of controlling for heterogeneity, since it does not control for differences

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[33]Results for the test of whether the family fixed effect adequately removes the heterogeneity were similar to the results for white women.

[34]This is the residual from an OLS regression of the sibling's wage on the year dummy variables. Extracting the residual from an OLS regression on all of the control variables would ignore potential biases in the OLS estimates.

across siblings in the right-hand-side variables (in contrast to fixed-effects estimates).[35] On the other hand, it does allow us to use the sibling values of the potentially endogenous variables as instruments, and to test the exclusion of the family background variables from the wage equation.

The estimates reported in column (1) show that the sibling's wage is significantly related to the respondent's wage. But the point estimates of the other coefficients are closer to the OLS estimates in column (2) of Table 2 than to the fixed-effects estimates in column (4) of that table, implying that this is not an equivalent means of controlling for heterogeneity. Nonetheless, it is the best we can do with respect to testing the identifying assumptions. The last two rows of the table report the tests of the exclusion restrictions. In none of the specifications is there evidence against the exclusion of the family background variables from the wage equations. In addition, in contrast to Table 3, there was no evidence against the exclusion of the attitudes/expectations variables, conditional on excluding the family background variables.

V.f. Testing Identifying Assumptions Used in Previous Research

One of the points we raised with respect to the review of the literature summarized in Table 1 is that researchers have used a variety of identifying assumptions to attempt to correct for endogeneity. Earlier, we presented *a priori* arguments for the identifying assumptions we used (and against those we chose not to use). In this subsection, we continue to use these identifying assumptions, and based on them, test as overidentifying

[35] Furthermore, this approach does not increase the attractiveness of panel data on individuals across time (rather than across families) because the same problem of reducing or eliminating variation in experience or tenure if we condition on the availability of two wage observations arises as with fixed-effects estimation.

restrictions some of the identifying assumptions made in the literature. While Table 1 reveals a host of such assumptions, we focus on a limited subset, which appears in numerous papers, to correct for endogeneity of experience and of fertility. These overidentification tests may prove useful to researchers who have available as instruments only the variables that, in our data set (and with our assumptions), provide overidentification.

In columns (1) and (2) of Table 6, we report tests of exclusion restrictions of three variables commonly used to instrument for experience and fertility, based on our preferred specifications from the IV/FE analysis in Table 3. Three common choices of instruments for experience are husband's weeks unemployed (in a year), husband's income, and number of children. (Of the papers cited in Table 1, see Mincer and Polachek, 1974; Sandell and Shapiro, 1978; and Wright and Ermisch, 1991.) As reported in column (1), we do not reject the exclusion of husband's unemployment from the wage equation, but do reject the exclusion of either husband's income or number of children. In column (2) we report results treating the number of children as endogenous (see Lundberg and Plotnick, 1989). Again, we do not reject the exclusion of husband's unemployment, but do reject the exclusion of husband's income. In columns (3) and (4) we repeat the analysis for the specifications considered in Table 5, in which we include the sibling's wage residual instead the family fixed effect. The conclusions with respect to the exclusion restrictions are virtually unchanged, although strictly speaking the exclusion of husband's income is rejected at significance levels slightly higher than five percent.

Finally, the results reported in columns (1)-(4) are based on specifications with some attempt made to control for heterogeneity, whereas other researchers may have available only cross-sectional data. Since the test results may differ without the heterogeneity controls, we also use the sibling data to test the validity of these same identifying assumptions used in other research on a cross-sectional sample constructed from our data set. including sibling values of the potentially endogenous variables. To construct the data set for this analysis, we randomly sampled one individual from each family, to eliminate any error components structure to the data.[36] Results are reported in columns (5) and (6) of Table 6; the conclusions are largely unchanged.

VI. Conclusions

In this paper we report results from the analysis of heterogeneity and endogeneity bias in women's wage equations, using data on siblings both to estimate models with fixed family effects, and to provide instrumental variables. The small samples of sisters available for the statistical experiments carried out in this paper may seem to preclude rejecting any restrictions. But indeed numerous restrictions were rejected.

To summarize our results, we find evidence of statistically significant biases (at the ten-percent level or less) in OLS estimates of wage equations for white and black women, in the coefficients that have attracted attention in the literature on women's wages (marital status, number of children, experience, and schooling). There is evidence of significant downward bias in the estimated effect of marital status on wages of white women. Estimates of the parameter are insignificantly different from zero in OLS estimates in our sample as well as others (Korenman and Neumark, 1992), but strongly positive after accounting for heterogeneity and endogeneity. There is also evidence, for white women, of

[36]While random effects could be used to account for such a structure, the random-effects estimator can be interpreted as a weighted average of the OLS and fixed-effects estimator, in which case the experiment would not replicate that faced by a researcher using a single cross section of data.

upward endogeneity bias in the estimated return to schooling.[37] Finally, there is evidence of significant upward heterogeneity bias in the estimated return to schooling for black women.

A question of equal importance is whether researchers can correct for biases in such estimates with the data typically available. We do find that some of the exclusion restrictions used to identify parameters in instrumental variables estimation of women's wage equations in cross-sectional data are not rejected; generally speaking, this is true for family background variables but not for attitudes/expectations variables, although these results may be specific to our data set. We also find that some of the exclusion restrictions used by other researchers to identify parameters in instrumental variables estimation are rejected in overidentifying tests using sibling data to obtain identification. Furthermore, while there appear to be valid instruments available in more typical data sets, the evidence of heterogeneity bias for white women (in estimates in which we simultaneously instrument) and for black women (more generally) suggests that panel data may be required to obtain unbiased estimates. Unfortunately, in this particular context longitudinal data sets on individuals may be problematic with respect to controlling for heterogeneity.

We close by pointing out some of the implications of the biases we find for the uses or applications of women's wage equations discussed in the Introduction. In constructing predicted wages for studies of other labor market variables with which wages may be endogenously determined, the incorrect treatment of wage equation regressors as exogenous can lead to misleading results. Mroz (1987) provides a striking example, showing that estimates of women's labor supply elasticities are considerably larger (positive) when

[37]This contrasts with results for men in which there is some evidence of downward endogeneity bias in the return to schooling (Griliches, 1977).

experience is included in the reduced form wage equation; in contrast. when experience is treated as endogenous, the estimated labor supply elasticity is near zero (see his Table VI).

Our findings are also relevant to estimates of discrimination based on decomposing sex-differences in wages into discriminatory and non-discriminatory components. First, in most samples, OLS estimates indicate higher returns to schooling for women than for men (*e.g.*, Oaxaca, 1973 (Table 3); Corcoran and Duncan, 1979; Neumark, 1988). Upward bias in the estimated return to schooling for women would lead to underestimates of gender-based wage discrimination. Second, the same decomposition techniques treat the positive wage premium paid to married men, coupled with the absence of a premium for women, as a wage differential attributable to discrimination.[38] But our endogeneity-corrected estimates of the effect of marital status on women's wages suggest that the premium for women may be as large as that paid to married men. Estimates in Neumark (1988) suggest that wage equation decompositions based on equal wage premiums for married men and women would reduce the estimate of discrimination by about one-third, compared to decompositions based on OLS estimates. Finally, the lower estimates of the return to schooling for black women would contribute to a larger estimate of the discriminatory component of within-sex race differences in wages.

A third use made of women's wage equations is in testing theories of wage differences between men and women. For example, one explanation of the positive effect of marriage on men's wages (Korenman and Neumark, 1991) is that married men specialize in market production and human capital investment, while women specialize in the home (Becker, 1985). A positive effect of marriage on women's wages, especially in specifications

[38]Our findings in Korenman and Neumark (1991), however, suggest that the wage differential paid to married men may represent higher productivity.

that do not control for children, would undermine this hypothesis. Finally, estimates of the effects of demographic decisions on wages are interesting for numerous reasons, including estimating the opportunity costs of these decisions (*e.g.*, Lundberg and Plotnick, 1990; Geronimus and Korenman, 1991). With respect to the effects of children on wages, for white women the results in this paper provide a more conclusive answer regarding the effects of children on wages than did our previous research; while the effect is not significant in all specifications, it is unambiguously negative. On the other hand, our estimates for black women, which are generally as precise as those for white women, provide no compelling evidence that wages are reduced by having children. It remains to be seen whether additional (and alternative) approaches to untangling causal effects of demographic decisions on wages and earnings will provide explanations for variation in demographic behavior across individuals and time. In our view, this is likely to be an area in which research on women's wage equations will yield high returns.

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Table 1

Estimators and Identifying Restrictions in Existing Research on Women's Wages (A Partial Summary)[1]

Paper	Source of Bias[2]	Estimator[3]	Dataset	Identifying Restrictions
Oaxaca (1973)	...	OLS	1967 SEO	...
Mincer and Polachek (1974)	Endogenous experience	2SLS	NLS Mature Women	Husband's education and number of children excl. from wage eqn.
Sandell and Shapiro (1978)	Endogenous experience and hometime	2SLS	NLS Mature Women	Husband's education, number of children, and potential experience excluded from wage eqn.
Corcoran and Duncan (1979)	...	OLS	PSID	...
Hill (1979)	...	OLS	PSID	...
Corcoran, et al. (1983)	Heterogeneity, selection	First difference, SSC first difference	PSID	Education, experience, non-labor income, number of children, number of young children in household, and marital status (at time of first wage observation) excluded from wage change eqn.
Cox (1984)	...	OLS	1973 CPS-SSA Match File	...
O'Neill (1985)	...	OLS	NLS Young Women and Mature Women	...
Dolton and Makepeace (1987)	Selection, heterogeneity	SSC longitudinal	Early Careers of 1970 Graduates (U.K.)	Marital status, number of children, age, occupational status of job, and interactions of these excluded from wage eqn. is some specifications.
Goldin and Polachek (1987)	...	OLS	1980 Census	...
Neumark (1988)	...	OLS	NLS Young Men and Young Women	...
Lundberg and Plotnick (1989)	Endogeneity of fertility/marriage states	SSC earnings equations for different fertility/marriage groups	NLSY	Husband's unemployment, fraction of year woman married, spouse's income, local labor market conditions, number of children, number of children under age 6 excluded from initial wage equation, included in employment eqn. Age at menarche, religion, and attendance at religious services excluded from potential earnings eqn., included in fertility/marriage eqn.
Blackburn, et al. (1990)	Heterogeneity, selection	Longitudinal, SSC	NLS Young Women	Husband's income, husband's weeks unemployed, and income from alimony and child support excluded from wage eqn.
Geronimus and Korenman (1990)	Heterogeneity	First difference	NLS Young Women	...
Kim and Polachek (1991)	Heterogeneity, endogeneity of experience and schooling	2SLS Within	PSID	Race, age, father's education, mother's education, and occupation dummy variables excluded from wage eqn.

Table 1 (continued)

O'Neill and Polachek (1991)	...	OLS	CPS, PSID	...
Wright and Ermisch (1991)	Endogeneity of experience and tenure, selection	2SLS, SSC	1980 Women and Employment Survey (U.K.)	2SLS and SSC: Wife's age, housing tenure, number and age of children, local unemployment rate, husband's employment, non-labor income, husband's age, husband's education, husband's social class, and wife's age at marriage excluded from wage equation.[4]
Korenman and Neumark (1992)	Heterogeneity, endogeneity, selection	First difference, 2SLS, SSC, SSC first difference	NLS Young Women	2SLS: Father's education, mother's education, parents' educational goal, number of siblings, mother worked, and lived with father and mother excluded from wage eqn.[5] SSC: Husband's income, husband's weeks employed, income from alimony and child support, and changes in marital or fertility status excluded from employment eqn. for each year.

1. When OLS analysis of cross-sectional data was included along with an analysis of a source of bias, only the latter approach is described in the table.
2. This refers to the three sources of bias considered in this paper: heterogeneity bias, endogeneity bias, and sample selection bias. It does not refer to the numerous attempts made to reduce omitted variable bias or measurement error bias by introducing new or improved variables into wage equations. Many of these latter types of studies were omitted from the table.
3. "OLS" implies that cross-sectional data were used. All fixed-effects estimators difference across individuals over time, except for Geronimus and Korenman (1990). "Longitudinal" refers to including an early wage or early wage residual.
4. Wright and Ermisch also consider the endogeneity of experience and home time in the employment probit.
5. These are the a *priori* identifying restrictions; other overidentifying restrictions are tested.

Table 2

OLS and Fixed-Effects Wage Equation Estimates for Sibling Sample, White Women
(Dependent Variable: Natural Logarithm of Hourly Earnings)[1]

	OLS		Family Fixed Effect (FE)		OLS	FE
Coefficients:	(1)	(2)	(3)	(4)	(5)	(6)
Married, spouse present	.062 (.039)	.039 (.038)	.069 (.054)	.058 (.053)	.029 (.034)	.052 (.049)
Number of children	-.089 (.018)	-.074 (.017)	-.071 (.024)	-.050 (.024)	-.052 (.016)	-.039 (.022)
Experience	...	.030 (.006)	...	.035 (.008)	.020 (.005)	.021 (.008)
Schooling	.054 (.008)	.066 (.008)	.058 (.013)	.070 (.013)	.051 (.007)	.056 (.012)
Lagged own wage[2]	...	...	...	...	.518 (.046)	.436 (.062)
$\bar{R}^2$	.494	.521	.546	.573	.617	.641
Heterogeneity bias: p-value[3]	...	...	.610	.464	...	...

1. There are 518 observations. Standard errors are reported in parentheses. Observations are included only if the wage reported is for a job at which the respondent is currently working. Observations were drawn from 1982 if possible, and otherwise from 1980, 1978, 1977, 1975, or 1973; observations were always selected from the latest year possible. Other variables included were: years of schooling; dummy variables for residence in the south and in an SMSA, and dummy variables for the year from which the observation was drawn.
2. Wage from previous survey. A dummy variable was included for a missing lagged wage.
3. p-value from Hausman specification test of statistical significance of the difference between the OLS and FE estimates of the coefficients reported in the table.

Table 3

Two-Stage Least Squares, Fixed Family Effects Estimates of Wage Equation for Sibling Sample, Alternative Endogenous Variables, White Women
(Dependent Variable: Natural Logarithm of Hourly Earnings)[1]

Coefficients:	(1)	(2)	(3)	(4)	(5)
Married, spouse present	.049	.463	.117	.061	.534
	(.053)	(.217)	(.078)	(.055)	(.254)
Number of children	-.056	-.104	-.149	-.075	-.039
	(.027)	(.038)	(.087)	(.031)	(.085)
Experience	.028	.032	.027	.028	.038
	(.022)	(.010)	(.011)	(.010)	(.024)
Schooling	.081	.063	.066	.020	.055
	(.019)	(.015)	(.023)	(.059)	(.023)
Variables used as instruments:[2]					
Family background	Yes	Yes	Yes	Yes	Yes
Attitudes/expectations	No	Yes	No	No	Yes
Endogeneity tests:					
Experience residual[3]	.007	...	...	...	-.003
	(.024)				(.022)
Married, spouse present residual[3]	...	-.437	...	...	-.508
		(.202)			(.223)
Number of children residual[3]	...	...	.105	...	-.005
			(.088)		(.076)
Schooling residual[3]	...	...	...	.071	.035
				(.060)	(.027)
p-value[4]	.759	.031	.232	.096	.102
Exclusion restrictions in wage equation:[5]					
Attitudes/expectations	.050	.206	.055	.099	.482

1. See Table 2 for details. Family background variables include: father's education; mother's education; parents' educational goal for respondent at age 14; number of siblings; a dummy variable equal to one if the respondent's mother worked when respondent was age 14; a dummy variable equal to one if the respondent lived with both a father and mother at age 14. Attitudes/expectations variables include: a dummy variable set equal to one if respondent disagreed or strongly disagreed with statement that it is alright for a woman to work even if her husband disagrees, asked in 1971 (non-traditional sex-role attitude); a dummy variable set equal to one if respondent agreed or strongly agreed with this statement, in 1971 (traditional sex-role attitude); ideal age at marriage reported by respondent at age 14 (set equal to zero, with a dummy variable set equal to one if response was never to marry); expected number of children, in 1970; educational expectations, in 1970; educational goal, in 1970. For both sets of instruments, dummy variables corresponding to each of these variables were also included, equal to one when the variable was missing (in which case variables were set equal to zero).

2. When attitudes/expectations variables are not used as instruments, they are included in wage equation.

3. Coefficient of residual from regression of potentially endogenous variable on instruments and exogenous variables, included in log wage equation least squares.

4. p-value from (joint) test of significance of residual coefficient(s).

5. p-value from Wald test of joint significance of set of instruments in wage equation, in unrestricted model.

Table 4

Alternative Estimates of Wage Equation for Sibling Sample, Black Women
(Dependent Variable: Natural Logarithm of Hourly Earnings)[1]

	OLS	Family Fixed Effect (FE)	IV/FE				
Coefficients:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Married, spouse present	.013	.017	.023	-.077	.004	.012	-.055
	(.044)	(.053)	(.056)	(.101)	(.059)	(.054)	(.111)
Number of children	.008	-.003	-.010	.009	.019	-.006	.033
	(.017)	(.024)	(.026)	(.027)	(.049)	(.025)	(.061)
Experience	.016	.011	-.023	.011	.011	.010	-.028
	(.007)	(.010)	(.024)	(.010)	(.010)	(.010)	(.027)
Schooling	.070	.036	.031	.033	.039	.027	.035
	(.011)	(.015)	(.016)	(.015)	(.016)	(.025)	(.032)
$\bar{R}^2$	.544	.657	...	...	...	...	...
Heterogeneity bias:							
p-value	...	.014	...	...	...	...	...
Variables used as instruments:							
Family background	...	...	Yes	Yes	Yes	Yes	Yes
Attitudes/expectations	...	...	Yes	Yes	Yes	Yes	Yes
Endogeneity tests:							
Experience residual	...	...	.042	...	...	...	.046
			(.026)				(.028)
Married, spouse present residual	...	...	...	.132	...	...	.109
				(.118)			(.121)
Number of children residual	...	...	...	...	-.029	...	-.043
					(.057)		(.064)
Schooling residual	...	...	...	...	...	.015	.007
						(.031)	(.035)
p-value	...	...	.108	.264	.611	.622	.379
Exclusion restrictions in wage equation:							
Attitudes/expectations	...	...	.996	.968	.958	.978	.972

1. There are 248 observations. See Table 2 for details regarding sample and variables. See Table 3 for details regarding specifications and test statistics.

Table 5

Two-Stage Least Squares Estimates of Wage Equation for Sibling Sample, Using Sibling's Wage Residual to Control for Heterogeneity, White Women (Dependent Variable: Natural Logarithm of Hourly Earnings)[1]

	OLS	2SLS				
Coefficients:	(1)	(2)	(3)	(4)	(5)	(6)
Married, spouse present	.039	.042	-.007	.052	.041	-.014
	(.038)	(.038)	(.175)	(.052)	(.037)	(.170)
Number of children	-.072	-.074	-.065	-.090	-.076	-.085
	(.017)	(.018)	(.029)	(.058)	(.018)	(.060)
Experience	.030	.027	.031	.030	.030	.027
	(.005)	(.014)	(.006)	(.006)	(.006)	(.015)
Schooling	.063	.062	.064	.060	.059	.056
	(.008)	(.009)	(.008)	(.012)	(.010)	(.012)
Sibling's wage[2]	.088	.088	.088	.086	.091	.088
	(.039)	(.039)	(.039)	(.040)	(.039)	(.041)
$\bar{R}^2$	.525	...	...	...	...	...
Variables used as instruments:						
Family background	...	Yes	Yes	Yes	Yes	Yes
Attitudes/expectations	...	Yes	Yes	Yes	Yes	Yes
Endogeneity tests:						
Experience residual	...	.004	...	...	...	.004
		(.015)				(.016)
Married, spouse present residual	...	...	.049	...	...	.055
			(.181)			(.176)
Number of children residual	...	...	...	.020	...	.055
				(.062)		(.176)
Schooling residual	...	...	...	...	.012	.015
					(.016)	(.018)
p-value	...	.777	.786	.741	.447	.932
Exclusion restrictions in wage equation:						
Family background	...	.574	.643	.946	.639	.992
Attitudes/expectations[3]	...	.659	.763	.790	.823	.648

1. There are 518 observations. See Table 2 for details regarding sample and variables. See Table 3 for details regarding specifications and test statistics.
2. Residual from regression on year dummy variables.
3. These come from specifications excluding the family background variables from the wage equation.

Table 6

Results from Overidentification Tests for Alternative Instruments[1]

	Endogenous Variables					
	Table 3 Specifications[2] (IV/FE)		Table 5 Specifications[3] (IV with sibling residual)		"Cross-Section"[4]	
	Experience	Number of Children	Experience	Number of Children	Experience	Number of Children
	(1)	(2)	(3)	(4)	(5)	(6)
Weeks husband unemployed[5]	.478	.286	.478	.464	.175	.176
Husband's income	.036	.022	.060	.057	.027	.041
Number of children[6]	.034	...	.000	...	.001	...

1. p-values from tests of exclusion restrictions of indicated variables are reported. Each of the two alternative instruments is added to the model specification individually. Dummy variables for missing weeks husband unemployed or husband's income are included, in which case the variables were set to zero. See Tables 2, 3, and 5 for further details.
2. Family background variables are excluded from wage equation, and used as instruments, while attitudes/expectations variables are included in wage equation, based on tests of exclusion restrictions in Table 3.
3. Family background and attitudes/expectations variables are excluded from wage equation, and used as instruments, based on tests of exclusion restrictions in Table 5.
4. Based on subsample of one woman randomly sampled from each sibship. Family background and attitudes/expectations variables are excluded from wage equation, and used as instruments, based on overidentification tests identical to those carried out in Table 5.
5. For 1973, this is restricted to weeks husband collected unemployment compensation.
6. This parallels earlier work (see Table 1) excluding number of children from the wage equation, and using it as an instrument for experience. For columns (1) and (3), these are just the p-values for t-test of the significance of the coefficients of the number of children variable in Table 3, column (3), and Table 5, column (4).

Appendix Table A1

Descriptive Statistics for Panel Data Sample

	White Women	Black Women
Distribution of sibships:		
Two sisters	223	96
Three sisters	24	17
Five sisters	0	1
Means (Standard Deviations):		
Log wage	6.177	5.992
	(.537)	(.485)
Married, spouse present	.680	.536
Number of children	1.002	1.641
	(1.117)	(1.430)
Experience	5.806	5.695
	(3.556)	(3.465)
Schooling	13.463	12.359
	(2.323)	(2.233)
South	.266	.633
Urban	.685	.746
1982	.527	.516
1980	.098	.077
1978	.154	.165
1977	.029	.032
1975	.058	.040
1973	.133	.169
Correlation with sibling values:[1]		
Ever married	.154	.003
Number of children	.140	.286
Experience	.181	.317
Schooling	.447	.397
Sample size	518	248

1. For sibships with more than two members, this is the average over the other siblings.

Appendix Table A2

First-Stage Regressions for Instrumental Variables/Fixed Effects Estimation[1]

Coefficients of Instruments in First-Stage Regressions[2]

Coefficients:	Experience (1)	Married, Spouse Present (2)	Number of Children (3)	Schooling (4)
Father's education	-.324	.001	-.067	-.022
	(.124)	(.022)	(.046)	(.064)
Mother's education	-.174	-.019	-.018	.089
	(.194)	(.033)	(.072)	(.100)
Parents' educational goal	.085	.001	.027	.062
	(.051)	(.009)	(.019)	(.026)
Number of siblings	.293	-.019	.040	.005
	(.132)	(.023)	(.050)	(.069)
Mother worked	-2.358	.170	-.691	.133
	(.587)	(.104)	(.221)	(.312)
Lived with father and mother	-1.412	.017	-.464	-.461
	(1.010)	(.174)	(.377)	(.521)
Nontraditional sex-role attitude	...	.068	...	...
		(.084)		
Ideal age at marriage	...	.001	...	...
		(.012)		
Expected number of children	...	-.002	...	...
		(.019)		
Educational expectations	...	.025	...	...
		(.029)		
Educational goal	...	-.031	...	...
		(.026)		
R^2	.545	.221	.359	.717
Joint significance[3]	.000	.815	.017	.288

1. See Tables 2 and 3 for details regarding sample and variables.
2. These correspond to the specifications in Table 3, columns (1)-(4). Coefficients are reported for variables excluded from wage equation and used as instruments; coefficients of other variables included in wage equation (including family dummy variables), and dummy variables for missing instruments, are not reported.
3. p-value from F-test of joint significance of coefficients reported in table.