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MARRIAGE, MOTHERHOOD, AND WAGES

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ABSTRACT

We explore several problems in drawing causal inferences from cross-sectional relationships between marriage, motherhood, and wages. We find that heterogeneity leads to biased estimates of the "direct" effects of marriage and motherhood on wages (i.e., effects net of experience and tenure); first-difference estimates reveal no direct effect of marriage or motherhood on women's wages. We also find statistical evidence that experience and tenure may be endogenous variables in wage equations; IV estimates suggest that both OLS cross-sectional and first-difference estimates understate the direct (negative) effect of children on wages.

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I. Introduction

Cross-sectional studies find little association between a woman's marital status and her wage rate, but often find a negative relationship between children and wages. The negative relationship between children and wages is reduced and sometimes eliminated by inclusion in wage equations of detailed controls for "labor force attachment" such as experience and tenure.

There are, however, a number of reasons to be cautious in drawing causal inferences from these cross-sectional relationships. First, labor market experience and tenure may be endogenous if labor supply is responsive to wages. Because the estimated effect of children on wages is sensitive to the inclusion of controls for experience and tenure, it is important to explore whether experience and tenure are in fact exogenous variables in wage equations. Second, economic theories of fertility and marriage (*e.g.*, Becker 1981; Butz and Ward, 1979; Easterlin, 1980), suggest that marital status and number of children may also be endogenous. Third, estimated wage effects of marriage or children may be biased by unmeasured heterogeneity: women may be selected or may self-select into different marital or fertility states on the basis of unmeasured characteristics that are correlated with wages (*e.g.*, "career orientation"). Finally, bias could result if, among married women or women with children, those with high wages tend to select into employment (*i.e.*, the standard problem of sample selection bias).

Previous researchers have recognized some of these potential problems in interpreting cross-sectional relationships between wages, marriage and children, but they have not attempted to evaluate their empirical importance. This paper presents evidence on the magnitudes of these biases, assesses the sensitivity of the estimated effects to alternative approaches to eliminating bias, and attempts to arrive at unbiased estimates of the effects of marriage

and children on wages.

The effects of marriage and motherhood on wages are of particular interest due to their relation to male-female wage differentials. For example, Becker (1985) has hypothesized that a portion of male-female wage differentials is attributable to gender-role specialization by married women and men. In particular, he has argued that the "hourly earnings of single women [should] exceed those of married women even when both work the same number of hours and have the same market capital because child care and other household responsibilities induce married women to seek more convenient and less energy intensive jobs" (p. S54).[1] Therefore, the empirical analysis of wage differentials between single and married women, and women with and without children, can shed light on the wage effects of gender-role specialization.

II. Empirical Studies of Marriage, Children, and Women's Wages

Using the 1976 Panel Study of Income Dynamics, in multiple regression analyses with a standard set of human capital controls, Hill (1979) finds that married, white women with a spouse present earn more than their never married counterparts, but less than divorced, separated or widowed women. However, these differences are small and not statistically significant. Controlling for marital status, the number of children present is significantly negatively related to earnings. But when Hill adds detailed measures of labor force attachment and interruptions, the negative association between children and wages becomes small and insignificant (less than one percent per child).[2,3]

Focusing exclusively on the association between children and wages, Moore and Wilson (1982) examine a cross-section of married women aged 35-49 in 1972 who were full-time workers, from the NLS Mature Women file. Controlling for a

wide variety of worker characteristics, they find that married women with three or more children earn about eleven percent less per hour than married women without children, but that there are small and statistically insignificant differences among women with zero, one, or two children.

Goldin and Polachek (1987) examine the 1/1000 Public Use Sample of the 1980 U.S. Census of Population and find substantial and significant annual earnings differentials favoring never married women. However, substituting a variable called the "Expected Human Capital Stock" (which varies across individuals with differences in expected lifetime labor force participation) for education and experience variables reduces the marriage differential to about 38 percent of its original size.[4] Goldin and Polachek argue, à la Becker (1985), that much of the remaining differential "probably owes to the problem of controlling for intensity of work among individuals with greater home responsibilities" (p. 149).

Three English studies provide further evidence on the relationship between marital status and wages. Greenhalgh (1980), analyzing repeated cross-sections of British women from the General Household Surveys of 1971 and 1975 finds mean hourly earnings differentials favoring single women of 45 and 42 percent in 1971 and 1975, respectively. But estimates of separate wage equations by marital status leave only three to twelve percent of these differentials "unexplained" by differences in the characteristics of workers or jobs.

Siebert and Sloane (1981), using wage surveys of four English establishments, find roughly 10 to 25 percent unadjusted (i.e., mean) annual earnings differentials favoring single women in three establishments, and an 11 percent differential favoring married women in the fourth. Controlling for worker attributes (typically experience, tenure, and education) lowers the

differentials substantially in one establishment, but raises or leaves unchanged the others. Siebert and Sloane also report that the presence of children under age 12 is unrelated to wages for married women who worked in the one establishment that collected information on children.

Dolton and Makepeace (1987) estimate the association between marriage, children, and wages using the (English) 1970 Survey of Graduates. The coefficient of a marriage variable from an OLS log wage equation is -0.02 (with a standard error of 0.02) for women with no children, and -0.03 for those with children.[5] Correcting for selectivity into employment leads to a negligible change in the coefficient estimates.

In summary, these findings suggest that marriage has little or no association with women's wages,[6] while children appear to reduce wages primarily "indirectly," by reducing labor force participation and the accumulation of human capital, rather than "directly," by lowering the productivity of otherwise similar women. However, all of these studies take children and marital status, as well as experience and tenure, to be exogenous determinants of wage rates, and only one study (Dolton and Makepeace) attempts to account for employment selectivity.

III. Data and Empirical Findings

a. Data

The data analyzed are from the National Longitudinal Survey of Young Women. Most of our cross-sectional specifications are estimated using the 1982 wave of the survey, when the respondents were aged 28-38. This period covers an age range that captures both post-schooling labor market experience and marital status and fertility transitions that are needed for longitudinal estimation. From the original sample of 5,159 in 1968, attrition, non-missing

data requirements, and the restriction of the sample to white women reduce the sample size to 1,207 women working for a wage as of the 1982 survey. To perform longitudinal analyses, we need repeated wage observations; much of our longitudinal analysis focuses on the 911 women who worked for a wage in 1980 and 1982. We will address the influence of selection into the sample of women working in 1982, as well as the smaller sample working in both 1980 and 1982.

In order to estimate a direct effect of marriage and children on wages--apart from an indirect effect that may operate through reduced human capital accumulation--good controls for labor market experience and tenure are needed. Experience and tenure are constructed from job history questions that were asked over the entire range (1968-1982) of the data set. Experience is measured as year-equivalents of actual weeks worked, while tenure simply counts the number of years for which a respondent reports working for the same employer.

b. A First Look at Wages Levels and Changes

The upper panel of Table 1 reports mean log hourly wages for women with different marital statuses, and different numbers of children, as well as the distribution of women across these categories. Row (1) covers the sample of women working for a wage in 1982. On average, never married women earn wages 15 percent higher than divorced or separated women who, in turn, earn slightly more than women who are married with a spouse present.[7] More pronounced differences appear between women with different numbers of children. The greatest wage differential (about 27 percent) is found between childless women and those with two or more children.

Although such differentials suggest that marriage and children lower a woman's wage, they may also reflect other differences (observable or

unobservable) among women. One approach to this heterogeneity problem is to examine how wages change as a woman changes marital status or has children. Rows (3) and (4) provide a first pass at this first-difference analysis. Row (4) reports mean changes in log wages between 1980 and 1982 for women who changed marital or fertility states. These changes can be compared to the figures in row (3), which are the mean changes in log hourly earnings for all women.

The mean change in the log wage is lower for women who marry between 1980 and 1982 (0.17 in row 4) than for women who remain single in 1982 (0.22, row 3). However, the mean change is only slightly lower for women who have a second child (0.18, row 4) compared to women with one child as of 1982 (0.19, row 3), and is actually relatively high for women who have a first child between 1980 and 1982 (0.25, row 4). Thus, wage growth appears unaffected by changes in marital status or number of children, raising the possibility that the cross-sectional differences in wages by marital status or number of children may be mostly due to heterogeneity, observable or not.

However, the first-difference approach may be flawed because recent changers (*e.g.*, women who had a first birth between 1980 and 1982) who worked in 1982 may be a select group. In addition, because they are aged 28-38 in 1982, these recent changers are also relatively "late" child bearers, who tend to be relatively high earners (Bloom, 1987; Blackburn, *et al.*, 1990). Finally, as mentioned, the marital status and fertility transitions may to some extent reflect responses to wages. These potential problems underscore the need for multivariate analyses that explicitly account for heterogeneity, employment selectivity, and endogeneity of marital status and number of children.

c. OLS Regression Estimates

Table 2 reports estimates from OLS cross-sectional log wage regressions. Never married women and childless women are the reference categories. Columns (1) and (2) report regressions of log wages on the marital status and number of children variables. As column (2) shows, when dummy variables for number of children are included in the equation, the negative associations of marriage or divorce with wages disappear, while children (especially two or more) are associated with significantly lower wages. When years of education completed and dummy variables for living in the South and in an SMSA are added in columns (3) and (4), the coefficients of the number of children dummy variables are reduced by about one-half; only the coefficient of the two or more children dummy variable remains statistically significant. In column (5) experience and tenure are included, reducing the magnitude of the coefficients of the number of children variables even further.[8] These results are consistent with the findings of many of the studies reviewed in Section II: after controlling for experience and tenure, marriage and children have relatively little association with wages.[9,10]

d. Bias in OLS Regression Estimates

As discussed in the Introduction, there are many potential sources of bias in the cross-sectional estimates presented in Table 2. The previous subsection touched on biases that could be addressed by the inclusion of better measures of observed characteristics. This section considers sources of bias associated with unobservables: endogeneity; heterogeneity arising from selection into different categories of marital status and number of children on the basis of unmeasured characteristics correlated with wages; and selection into employment.

d.1. Endogeneity Bias and IV Estimates

Table 3 reports estimated coefficients and standard errors from equations in which marital status and number of children, as well as experience and tenure, are treated as potentially correlated with the wage equation error. The table also reports test statistics for the exogeneity of these variables with respect to the wage equation error (Hausman, 1978). The instrumental variables, described in detail in the footnotes to Table 3, fall into two categories: family background variables and measures of attitudes and expectations. Exclusion restrictions to identify coefficients necessarily involve untestable assumptions. In this section, the maintained assumption is that family background measures are valid instruments. Researchers using sibling pairs to identify wage equation parameters have shown that, once ability and schooling are taken into account, family background does not have an independent effect on earnings or wages (Griliches, 1979). These findings suggest using family background measures as instruments. Conditional upon the maintained assumption that family background variables are valid instruments, the exclusion of measures of attitudes and expectations from the wage equation can be tested as an overidentifying restriction.

Instrumental variables estimates of the specification from Table 2, column (5)--using family background variables as instruments for experience, tenure, marital status and number of children--are reported in column (1) of Table 3. There is no statistical evidence that experience, tenure, marital status, or number of children are correlated with the wage equation error; in the bottom panel of the table, the p-values for the exogeneity tests are all greater than 0.05.[11] As the last entry in column (1) indicates, when the equation was reestimated including measures of attitudes and expectations,

the exclusion of these measures from the wage equation could not be rejected. Hence, column (2) adds attitudes and expectations to the instrument list. In this specification there is also no evidence that marital status or children are correlated with the wage equation error; the p-values for the exogeneity tests are 0.79 and 0.27, respectively. But the exogeneity of experience and tenure is rejected.

To examine the sensitivity of these conclusions to alternative specifications, in columns (3) and (4) we specify a simpler wage equation that includes a dummy variable for married, spouse present only (so that never married or divorced or separated is the reference category), and a dummy variable for whether the woman has any children. Repeating the analysis of columns (1) and (2) with this simpler specification, our conclusions are unchanged (using a five percent significance level). However, the correlation between children and the wage equation error is stronger; the p-value for the exogeneity test in column (4) is 0.07.

The instrumental variables methods do not lead to a rejection of the exogeneity of marital status and children, and yield imprecise estimates of their effects. Consequently, in columns (5) and (6) we treat experience and tenure (only) as potentially correlated with the wage equation error. We continue to reject the exogeneity of experience and tenure, and their coefficients are not significantly different from zero. More importantly, the coefficients of the marital status and children variables are very similar to the OLS coefficients in column (4) of Table 2, in which experience and tenure were omitted. Of course, this finding stands in contrast to OLS estimates that include experience and tenure, in which the negative association between children and wages is attenuated or eliminated.

As long as the error is uncorrelated with the instruments, the

instrumental variables methods used in Table 3 are sensitive to any source of correlation between the right-hand-side variables and the wage equation error. If the source of correlation is a fixed effect, then first-difference methods provide an alternative estimation strategy that may be preferable to IV methods for two reasons. First of all, first-difference methods do not rely on untestable exclusion restrictions. Although they do involve other assumptions, these assumptions are fully testable. Second, if there are fixed effects that, for example, influence both wages and fertility, IV methods require not only an exclusion restriction, but an additional assumption that the excluded variable(s) are orthogonal to the fixed effect.

d.2. Unobserved Heterogeneity Bias and Fixed-Effects Estimates

We use first-differences estimated for 1980 to 1982 to eliminate potential biases from fixed, unmeasured characteristics on the basis of which women select into different marital or fertility states. Although we exclude experience and tenure from these specifications, this exclusion has virtually no effect on estimates of the marital status and children coefficients. This is because a short first difference implicitly controls for experience and tenure by using a sample of women employed in two consecutive survey years (*e.g.*, 1980 and 1982.) For women employed in two consecutive years, having a child, for example, does not lead to much reduction in experience and tenure, relative to childless women.[12]

For comparison with what follows, the first row of Table 4 reports OLS coefficient estimates for the subset of the sample for which 1980 and 1982 data are available. The second row of the table reports estimates from the 1980 to 1982 change equation.[13] None of the marital status or children coefficients is significantly different from zero, nor are these variables

jointly significant (the p-value from the F-test is 0.76). The coefficients of marital status fall considerably and become negative, while the coefficients of the number of children variables actually become positive. The fixed-effects estimates suggest that cross-sectional estimates are biased by unmeasured heterogeneity, *i.e.*, women with wage-enhancing characteristics (net of observables) appear less likely to have (two or more) children.

Heckman and Hotz (1989) have proposed an "overidentification" test of the fixed-effect assumption. This test asks whether early wage levels are associated with later wage growth, indicating selection into different marital or fertility states based on wage growth, in contrast to the fixed-effects assumption of selection on wage levels. To perform this test, we retain an earlier (1978) wage, and estimate

$$(1) \quad (w_{82} - w_{80}) = (X_{82} - X_{80})\beta_{82} + w_{78}\delta + (\epsilon_{82} - \epsilon_{80}).$$

An estimated δ significantly different from zero indicates a violation of the fixed-effects assumption. The results reported in row (3) indicate that $\hat{\delta}$ is not significantly different from zero, so we do not reject the fixed-effects specification.[14]

Despite the results of the specification test, two problems may arise from using 1980-1982 first-differences, because the effects of marital status and number of children are identified from women who changed states between 1980 and 1982. First, because they are recent changers (*i.e.*, they had a birth between 1980 and 1982), those who chose to work in 1982 might be changers with particularly high wages. This is the standard problem of selection into employment, but it may be particularly severe for recent changers (Solon, 1988). We examine the problem of selectivity of recent changers in the following subsection.

Second, the women in our sample were aged 26-36 in 1980, which suggests

that the changers are relatively late marriers or child bearers who may have higher than average wages following first births or marriage (perhaps because they are more able to afford child care), leading to upward-biased estimates in the first-difference specifications. To address the problem of "late changer" bias, we estimated an early first-difference (1971 to 1973), when the women were 17 to 29 years old.[15] The results, also reported in Table 4, differ little from the 1980-1982 first difference estimates, indicating that there is no serious bias from using late marriers and child bearers.

One approach to the problem of "recent changer" bias would be to use a longer first difference, in which effects of children and marriage are identified from women who change states, even if they are not employed in the years immediately following the change. However, equality of the wage equation coefficients for 1973 and 1982 (the long difference we examined) was rejected ($p = 0.02$). Moreover, the fixed-effects specification was rejected according to the overidentification test described above. Thus, we could not use long differences to correct for selectivity among recent changers. In the following section we use alternative methods to test and correct for employment selectivity.

d. 3. Employment Selectivity Bias and Sample Selection Corrections

We first selectivity-correct the 1980-1982 first-difference estimates, applying Heckman's (1979) sample-selection correction techniques to first differences, assuming jointly normally distributed errors. Before turning to these estimates, OLS and selectivity-corrected estimates of the 1982 cross-sectional wage equation are reported in the first two columns of Table 5.[16] Since recent changers influence the cross-sectional estimates, some selectivity bias in the cross-sectional estimates is expected. Indeed, the

estimates of the number of children coefficients become more negative when account is taken of selectivity, although the changes are small. The coefficient of the inverse Mills' ratio (lambda) is marginally significant, with a t-statistic of 1.86.

In columns (3) and (4) we apply a similar technique to the 1980-1982 first difference, using a bivariate selectivity criterion for employment in 1980 as well as 1982.[17] The coefficient estimates are unaffected by the selectivity correction, and the estimated coefficients of the lambda terms (one for each year) do not indicate the presence of sample selectivity.

As an alternative test for selectivity bias from recent changers--one that does not require the specification of employment equations--we estimate a wage regression for all women who worked in 1980 (whether or not they worked in 1982). In this wage regression for 1980, we include dummy variables indicating whether a woman changed into one of the marital or fertility states between 1980 and 1982, as well as interactions between these variables and a variable indicating whether the woman was employed in 1982. The coefficients of the interaction terms measure, for example, the wage in 1980 of a woman who had her first child between 1980 and 1982 and worked in 1982, relative to the wage of a similar woman who had her first child between 1980 and 1982, but was not employed in 1982. If high earners among recent changers select into employment, then a woman who had a child between 1980 and 1982 and who worked in 1982, should earn relatively higher wages in 1980 than a woman who had a child between 1980 and 1982 but did not work in 1982.

Table 6 reports results for these specifications for both 1971 and 1980, to examine the importance of selectivity bias in the 1971-1973 and 1980-1982 first differences. We find no evidence of selectivity bias from recent changers. None of the coefficients of the recent changer interactions is

statistically significant. These conclusions do not differ from the tests based on the more standard selectivity-correction methods.

IV. Conclusion

We have explored the consequences of a number of potential problems with drawing causal inferences from cross-sectional relationships between marriage, motherhood, and wages. These problems include: endogeneity of marriage and motherhood, and experience and tenure; heterogeneity; and selectivity into employment.

We have three main findings to report. First, introducing experience and tenure into wage equations estimated by OLS attenuates but does not eliminate the large negative relationship between children and wages. Instrumental variables techniques and the accompanying tests suggest that marital status and number of children are exogenous variables in wage equations. However, IV results also suggest that experience and tenure are not exogenous. This finding is important because the size and statistical significance of the wage effects of children in cross-sections are sensitive to the exclusion of experience and tenure controls, and to their estimated coefficients. Instrumenting for experience and tenure yields estimated effects of marital status and number of children that are similar to OLS estimates when experience and tenure are excluded.

Second, first-difference specifications suggest that fixed unobservables bias cross-sectional estimates of the effects of children on wages. Short first-differences (estimated over a two-year period) indicate no negative effects of motherhood on wages.

Finally, standard sample selection corrections, as well as selectivity tests that do not depend upon on specifying an employment equation, provide no

evidence of selectivity bias from using a sample of recent changers (women who are employed despite recent changes of marital or fertility states).

Like many previous cross-sectional estimates, the first-difference estimates do not support the contention that marriage or motherhood lower women's wages. However, the IV results call into question specifications that include controls for experience and tenure, either explicitly (as in cross-sections) or implicitly (as in first differences). Estimates from such specifications are likely to understate the direct effects of children on wages, because the lower experience and tenure associated with marriage and motherhood may arise as an endogenous response to lower wages. The sensitivity of the principal conclusions to the treatment of experience and tenure highlights the need for continued research to determine whether experience and tenure are exogenous variables in women's wage equations.

Endnotes

[1]Fuchs (1989) makes a similar argument.

[2]Although we focus on white women, we note that Hill finds positive associations between both marriage and children and the earnings of black women.

[3]Suter and Miller (1973) discuss unreported regression estimates from a cross-sectional sample of women aged 30-44 drawn from the 1967 NLS Mature Women's file that lead them to essentially the same conclusion as Hill: "It appears that once a woman's occupational status and work experience are known, learning that she is married and has children does not significantly improve our ability to predict her income" (p.192).

[4]Goldin and Polachek also note (p.149) that their results confirm those of of an earlier study by Polachek (1975) using 1960 Census data.

[5]This last coefficient comes from an interactive specification; its standard error cannot be computed from the information given in the paper.

[6]For a review of the evidence for men see Korenman and Neumark (1990).

[7]The category "divorced or separated" includes a few widows as well as a few women who are married with no spouse present.

[8]When potential experience (defined as age - schooling - six) is used in place of actual experience, the marital status coefficients are unchanged, while the children coefficients become more negative (relative to a specification with no control for experience). This is expected because potential experience overstates actual labor market experience for women with the most labor force interruptions, who are likely to be those with children.

[9]We explored the sensitivity of the coefficients reported in column (5) of Table 2 to the inclusion of controls for, in turn, years married and years divorced or separated; the number of preschool-age children; and age of the mother at first birth. The results do not differ qualitatively from those in Table 2: adding years married and years divorced or separated reduces the coefficients of the dummy variables for marital status, although only the coefficient of the years divorced or separated variable is significant, and there is no negative effect of marital status in any year of marriage; the effect of young children is slightly positive, but insignificant; finally, the effect of children on wages varies with maternal age at first birth, but the marital status and number of children coefficients are essentially unchanged. (These results are available upon request.)

[10]Perhaps the most direct interpretation of Becker's hypothesis is that the joint effect of marriage (or divorce) and children is to lower wages. The p-values for the F-statistics for the joint significance of the marital status and number of children variables in the regressions in Table 2 indicate that these variables are jointly significant. However, in most of the specifications the coefficient of the married variable is positive. Indeed, in column (5) the point estimate of the summed effect of marriage and children is close to zero: -0.02 for women with two or more children; and 0.01 for women with one child.

[11]This test involves: i) regressing the potentially correlated variables on the set of instrumental variables and exogenous variables; ii) including the residuals from these regressions in the wage equation estimated by ordinary least squares; and iii) testing the joint significance of the constructed residuals.

[12]An implication of the fact that a short difference implicitly controls for experience and tenure is that a short difference cannot be used to obtain an estimate of the direct plus the indirect effect of marriage and motherhood on wages (i.e., effects that do not control for experience and tenure).

[13]The p-value for the F-test of equality of coefficients across the 1980 and 1982 cross-sections was 0.93.

[14]A similar specification test of the fixed-effects assumption (Heckman and Hotz, 1989), which uses information for two years prior to 1980 and prior to marriage and childbearing, also did not lead to rejection of the fixed-effects specification.

[15]The p-value for the test of equality of coefficients in the 1971 and 1973 cross sections was 0.40. In addition, the 1971-1973 first difference specification was also not rejected according to the overidentification test described in the text.

[16]Variables included in the employment probit, but excluded from the wage equation, include: husband's income; income from alimony or child support; and weeks the husband spent unemployed in the year preceding the 1982 survey. The sample is reduced slightly because these additional variables used in the employment probit were occasionally unavailable.

While these variables should affect the reservation wage, and not the offer wage, they are correlated with marital status, and may therefore provide little independent information. However, because under the normality assumption the selection model is identified without these exclusion restrictions (Olsen, 1980), the restrictions were tested by including these variables in the wage equation and testing their significance; the p-value for the F-test of the joint significance of these three variables in the wage equation was 0.59.

[17]Details are given in the footnotes to Table 5. The exclusion restriction of husband's income, income from alimony or child support, and weeks unemployed (of the husband) was not rejected ($p = 0.78$).

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Table 1

Mean Log Wages and Changes in Log Wages,

for Young White Women Classified by Marital Status,

Number of Children, and Changes in Marital Status and Number of Children[1]

Status as of 1982

	Married, Spouse Present	Divorced or Separated	Never Married	No Children	One Child	Two + Children
1982 sample:						
(1) Log Wage (1982)	6.39 (.01)	6.45 (.03)	6.60 (.04)	6.59 (.02)	6.48 (.03)	6.32 (.02)
N	787	262	158	371	234	602
1980-1982 sample:						
(2) Log Wage (1982)	6.46 (.02)	6.47 (.03)	6.59 (.04)	6.60 (.02)	6.53 (.03)	6.37 (.02)
(3) Δ Log Wage (1980 to 1982)	.20 (.01)	.21 (.02)	.22 (.02)	.22 (.02)	.19 (.02)	.19 (.02)
N	576	194	141	316	190	405

Status Entered 1980-1982

	Married, Spouse Present	Divorced or Separated	One Child	Two + Children
1980-1982 sample:				
(4) Δ Log Wage (1980 to 1982)	.17 (.06)	.21 (.05)	.25 (.05)	.18 (.04)
Number of Changers	46	53	33	26

1. Standard errors of means are reported in parentheses. Sample weights were not used in computing estimates. 1980 data are reported only for observations with wages and other variables used in wage regressions available for 1982. Wages are nominal values, with levels coded in cents.

Table 2

Wage Equation Estimates for White Working Women, 1982
Ordinary Least Squares
(Dependent Variable: Natural Logarithm of Hourly Earnings)[1]

	(1)	(2)	(3)	(4)	(5)
Married, Spouse Present	-.21	-.02	.01	.02	.05
	(.04)	(.05)	(.04)	(.04)	(.04)
Divorced or Separated	-.16	-.00	.05	.05	.10
	(.05)	(.05)	(.05)	(.05)	(.04)
One Child	...	-.13	-.05	-.05	-.04
		(.04)	(.04)	(.04)	(.04)
Two + Children	...	-.30	-.18	-.18	-.07
		(.03)	(.03)	(.03)	(.03)
Education	...	...	.06	.06	.07
			(.01)	(.01)	(.01)
South	...	...	...	-.05	-.05
				(.02)	(.02)
Urban	...	...	...	.15	.15
				(.03)	(.02)
Experience	...	...	...	...	.02
					(.004)
Tenure	...	...	...	...	.03
					(.003)
F-test[2]	.00	.00	.00	.00	.03
R^2	.04	.10	.19	.23	.33

1. There are 1,207 observations. Standard errors are reported in parentheses. Sample weights were not used in computing estimates. Observations are included only if the wage reported is for a job at which the respondent is currently working. Never married and no children are the reference categories. Single-year age dummy variables are included in all specifications.
2. P-value for joint test of significance of marital status and fertility variables, in columns (2)-(5), and marital status variables, in column (1).

Table 3

Wage Equation Estimates for White Working Women, 1982
Two Stage Least Squares
(Dependent Variable: Natural Logarithm of Hourly Earnings)[1]

	Experience, Tenure, Marital Status, and Fertility Endogenous				Experience and Tenure Endogenous	
Coefficients:	(1)	(2)	(3)	(4)	(5)	(6)
Married, Spouse Present	.40	-.02	-.07	-.14	.04	.04
	(.51)	(.30)	(.31)	(.19)	(.05)	(.05)
Divorced or Separated	.75	.16	...	...	.07	.08
	(.59)	(.30)			(.07)	(.06)
One Child	-.59	-.44	...	...	-.04	-.04
	(.69)	(.41)			(.04)	(.04)
Two + Children	-.38	-.40	...	...	-.20	-.19
	(.43)	(.24)			(.08)	(.06)
Children	...	...	-.27	-.33	...	...
			(.36)	(.20)		
Experience	-.03	-.03	-.02	-.03	-.03	-.03
	(.05)	(.02)	(.04)	(.02)	(.03)	(.02)
Tenure	.03	.03	.01	.03	.03	.03
	(.06)	(.03)	(.04)	(.03)	(.04)	(.02)
F-test[2]	.59	.07	.49	.01	.02	.00
Family Background variables used as IV's[3]	Yes	Yes	Yes	Yes	Yes	Yes
Expectational/ Attitudinal variables used as IV's[4]	No	Yes	No	Yes	No	Yes
Specification tests (p-values):						
Experience and Tenure exogenous[5]	.23	.00	.09	.00	.10	.00
Marital status exogenous[5]	.38	.79	.80	.49	...	...
Fertility status exogenous[5]	.59	.27	.54	.07	...	...
Expectational/ Attitudinal variables excluded[5]	.74	...	.74	...	.36	...

Table 3 (continued)

[1]There are 1,207 observations. Standard errors are reported in parentheses. Sample weights were not used in computing estimates. Observations are included only if the wage reported is for a job at which the respondent is currently working. Single-year age dummy variables were included in all specifications. Never married and no children are the reference categories.

[2]P-value for joint test of significance of marital and fertility status variables.

[3]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; and dummy variables corresponding to each of these variables, equal to one when the variable was missing (in which case the variables were set equal to zero).

[4]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; a dummy variable set equal to one if respondent agreed or strongly agreed with this statement, in 1971; 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; and dummy variables corresponding to each of these variables, equal to one when the variable was missing (in which case the variables were set equal to zero).

[5]P-value for joint significance of coefficients of the residuals from least squares regression of these variables on instruments and exogenous variables, when these residuals are added to wage equation in Table 2, column (5), estimated with OLS.

Table 4

Wage Equation Estimates for White Working Women, First-Difference Specifications (Dependent Variable: Levels and Changes of Natural Logarithm of Hourly Earnings)[1]

	Married, Spouse Present	Divorced or Separated	One Child	Two + Children	Early Wage[2]	F-test[3]	N
1980-1982 data:							
(1) 1982	.05	.05	-.01	-.15	...	.00	911
	(.04)	(.05)	(.04)	(.04)			
(2) 1982-1980[4] Change	-.05	-.04	.05	.02	...	.76	911
	(.08)	(.08)	(.05)	(.07)			
Specification test:							
(3) 1982-1980 Change	-.05	-.01	-.00	-.03	.04	.62	728
	(.07)	(.08)	(.05)	(.07)	(.03)		
Early difference:							
(4) 1973	.05	.06	.01	-.09	...	.11	543
	(.03)	(.06)	(.04)	(.05)			
(5) 1973-1971[4] Change	.03	.07	-.02	-.02	...	.70	543
	(.03)	(.06)	(.06)	(.11)			

1. Standard errors are reported in parentheses. Sample weights were not used in computing estimates. Observations are included only if the wage reported is for a job at which the respondent is currently working. Single-year age dummy variables were included in all specifications for wage levels. Other independent variables are the same as those in Table 2, column (4). Right-hand-side variables are levels in rows (1) and (4), and first differences in all other rows. In the cross-sectional specifications, never married and no children are the reference categories.
2. 1978 log wage.
3. P-value for joint test of significance of marital status and fertility variables.
4. Standard fixed-effects estimator.

Table 5

Wage Equation Estimates for White Working Women,
Sample Selectivity-Corrected Maximum Likelihood Estimates

(Dependent Variable: Natural Logarithm of Hourly Earnings)[1]

	1982 OLS (1)	1982 SSC[2] (2)	1982-1980 OLS[3] (3)	1982-1980 SSC[4] (4)
Status in 1982:				
Married, Spouse Present	.02	.01	-.05	-.07
	(.04)	(.04)	(.08)	(.08)
Divorced or Separated	.05	.07	-.04	-.05
	(.05)	(.05)	(.08)	(.08)
One Child	-.05	-.07	.04	.05
	(.04)	(.04)	(.05)	(.06)
Two or More Children	-.18	-.22	.04	.05
	(.03)	(.04)	(.07)	(.09)
Lambda	...	.13	...	...
		(.07)		
Lambda-80	...	...	...	-.04
				(.05)
Lambda-82	...	...	...	-.06
				(.07)
F-test[5]	.00	.00	.83	.75
N	1181	1181	888	888

1. Standard errors are reported in parentheses. Sample weights were not used in computing estimates. Observations are included only if the wage reported is for a job at which the respondent is currently working. Single-year age dummy variables were included in all specifications for wage levels. Other independent variables are the same as those in Table 2, column (4). In the cross-sectional specifications, never married and no children are the reference categories.

2. Variables in the employment probit include 1982 values of all variables included in wage equation specification in Table 2, column (4), as well as dummy variables indicating whether a respondent changed marital or fertility states between 1980 and 1982 (a different dummy variable is used for each of the four categories). Finally, measures of husband's income and weeks husband spent employed in 1982 (both set to zero for unmarried women), and the sum of income from alimony and child support (set to zero for never married women), are included.

3. Standard fixed-effects estimator.

4. A bivariate probit model is used for the selectivity correction. Variables included in probits for 1982 and 1980 are values for corresponding year of all variables included in wage equation specification in column (4) of Table 2, values of husband's income and weeks husband spent employed (both set to zero for unmarried women), the sum of income from alimony and child support (set to zero for never married women), and--for 1982--dummy variables indicating whether a respondent changed marital or fertility states between 1980 and 1982 (a different dummy variable is used for each of the four categories).

5. P-value for joint test of significance of marital status and fertility variables.

Table 6

Wage Equation Estimates for White Working Women,
Survey Year Prior to Change in Marital Status of Number or Children
(Dependent Variable: Natural Logarithm of Hourly Earnings)[1]

1980 Wage Regression

	Became Married, Spouse Present	Became Divorced or Separated	Had First Child	Had Second Child
(Change in marital or fertility status) × (worked in 1982)[2]	.02 (.15)	.24 (.27)	.05 (.12)	-.03 (.15)
Number worked 1980 and 1982[3]	50	57	38	30
Number worked 1980 only[4]	7	2	15	8

1971 Wage Regression

	Became Married, Spouse Present	Became Divorced or Separated	Had First Child	Had Second Child
(Change in marital or fertility status) × (worked in 1973)[2]	.02 (.05)	-.17 (.11)	.14 (.08)	-.11 (.11)
Number worked 1971 and 1973[3]	111	20	21	9
Number worked 1971 only[4]	65	9	51	21

1. There are 833 observations for the 1971 regression, and 1,091 observations for the 1980 regression. Standard errors are reported in parentheses. Sample weights were not used in computing estimates. For each year, observations are included only if the wage reported is for a job at which the respondent is currently working. Single-year age dummy variables were included in all specifications for wage levels. All independent variables listed in Table 2, column (4) are included. In addition, dummy variable indicators of changes in marital status and numbers of children are included.
2. Estimated coefficients of variables defined as the 1982 (1973) marital status or number of children variable, times a dummy variable equal to one if the woman changed into the category between 1980 and 1982 (1971 and 1973), times a dummy variable equal to one if the woman remained in the workforce in 1982 (1973).
3. Number of women who changed into the indicated marital status or number of children category between 1980 and 1982 (1971 and 1973), and worked for a wage in 1982 (1973).
4. Number of women who changed into the indicated marital status or number of children category between 1971 and 1973 (1980 and 1982), and did not work for a wage in 1973 (1982).