UNOBSERVABLE INDIVIDUAL EFFECTS, MARRIAGE AND THE EARNINGS OF YOUNG MEN

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While there is compelling evidence that married men earn more than unmarried men, the source of this premium remains unsettled. Using panel data from the National Longitudinal Survey of Young Men, we show that much of the premium normally attributed to marriage is associated with unobservable individual effects that are correlated with marital status and wages. To the extent there is a gain, it is purely an intercept shift and no more than 5% to 7%. Our findings cast doubt on the interpretation that marriage enhances productivity through specialization. (*JEL* J0)

I. INTRODUCTION

Married men earn more than unmarried men. This fact is unassailable and is robust across data sets and over time. Two common explanations of the married-male wage premium are (i) the division of labor in a married household which allocates more of the man's time to the market and (ii) lower cost human capital acquisition for married men. Both of these stories view marriage as *productivity-enhancing*. Kenny [1983] provides some evidence that marriage facilitates human capital acquisition, however the potential endogeneity of marital status is ignored. Furthermore, the argument that marriage makes it cheaper to accumulate human capital is difficult to reconcile with the fact that individuals who acquire more formal education tend to marry later than those who acquire less (Bergstrom and Schoeni [1992]). An alternative explanation for the marriage premium is that marriage *signals* certain unobservable individual characteristics, such as ability, honesty, loyalty, dependability and determination that are valued in both the labor and marriage markets.[1]

Although (i) and (ii) are quite different in terms of their behavioral content, they do share a common thread. Whether marriage *per se* increases productivity or high-productivity men are more likely to be married, marital status should not be treated as an exogenous determinant of the wage rate.

Nevertheless, in most wage regressions that control for marital status, it is assumed to be exogenous, resulting in an estimated marriage premium that is biased upward. Two exceptions are Nakosteen and Zimmer [1987] and Korenman and Neumark [1991]. Although both papers base their empirics on samples of young men, they come to very different conclusions regarding the size and source of the marriage premium. Nakosteen and Zimmer find the marriage premium to be insignificantly different from zero and claim that marriage does not make men more productive. Korenman and Neumark (KN) present evidence that the marriage premium is at

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1. Reed and Harford [1989] provide another alternative: the marriage premium represents a compensating differential required to induce married men to accept undesirable working conditions. In their view, marriage is related to the purchase of costly "family goods" such as children. One other explanation (we thank Eleanor Brown for this point), is that men "settle down" and spend more effort in the market.

ABBREVIATIONS

FGLS: Feasible generalized least squares
KN: Korenman and Neumark
NLSYM: National Longitudinal Survey of Young Men
OLS: Ordinary least squares

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least 15% and rises with years married, indicating that marriage may be productivity enhancing.

In this paper we reexamine the empirical relationship between marriage and wages using a sample drawn from the 1971, 1976, 1978 and 1980 waves of the National Longitudinal Survey of Young Men (NLSYM). Our data set is an extension of the one used by KN, whose empirical work is based on the 1976, 1978 and 1980 waves of the NLSYM.[2]

Our results may be summarized as follows. Estimates of the marriage premium derived from fixed effects models (which control for unobserved heterogeneity) are uniformly smaller than those obtained from random effects models (and other cross-section approaches that do not). Specification tests reject the latter and suggest that the individual effects are positively correlated with both marriage and wages. Conditional on labor market experience, job tenure and year effects, time spent in marriage has no significant effect on wages. This stands as evidence against any dynamic productivity enhancing effect of marriage. We conclude that whatever the source, the marriage premium is a pure intercept shift and no more than 5% to 7%.

Further support for this conclusion comes from the relationship between prospective marriage and wages. Using our longer panel, we construct a "to-be-married" variable which identifies men who marry sometime during the sample period; men who are not married in 1971, but who will be, earn at least as much as those who are already married. Finally, an interesting related result is the robust and significant effect of children on wages; conditional on marital status and years married, the presence of children raises wages about 5% regardless of the estimator employed. We interpret the effects of marriage and children on wages similarly—as structural breaks in the wage-generating process involving adjustments in market work and homework (among other things). In any case, the findings regarding prospective marriage and children are anomalous in a specialization story of marriage and wages.

2. We are grateful to Sanders Korenman for providing their data and regression programs to us.

II. MARRIAGE AND WAGES

Virtually all cross-section wage regressions that control for marital status report a large, statistically significant wage premium for married men. Some of the more prominent examples are discussed by Reed and Harford [1989] and KN. Our view is that the true marriage premium is much smaller than that commonly reported in cross-section wage regressions. We find that the wage premium can be explained largely in terms of unobservable individual characteristics which are positively correlated with marriage and wages. In other words, attributes leading to "good" (long and stable) marriages are also important in obtaining "good" (long and stable) jobs and higher wages.

This view is supported by some of the work on the returns to tenure. Abraham and Farber [1987] propose that workers in long-tenure jobs earn more *in every year* on the job and that most of the cross-section return to tenure is due to unobserved individual and job match effects. They test their proposition by estimating wage equations conditional on predicted job duration. Interestingly, results from the estimation of their job duration model indicate that marriage has a large, positive and statistically significant effect.

Further evidence exists in the quit behavior of married men. Consistent with the positive relationship between marriage and job duration is the depressing effect marriage has on quits. Shaw [1987] reports that the quit rate for married men aged 25 to 54 is less than half that of unmarried men, and marriage has its strongest deterrent effect on younger men.

Only Nakosteen and Zimmer [1987] argue explicitly that marriage does not significantly influence the wages of young men. However, the empirical support (which is based on a cross-section of young men from the 1977 wave of the Panel Study of Income Dynamics) for their argument is weak. When they restrict marital status to be exogenous and apply ordinary least squares (OLS) to their wage equation, Nakosteen and Zimmer obtain a statistically significant marital status coefficient estimate of 0.370. Relaxing the exogeneity restriction actually causes the marital status coefficient estimate to *rise*, although it is no longer statistically significant. Furthermore, specification tests of exogeneity are inconclusive. Nevertheless, Nakosteen and Zimmer conclude that the true marriage premium is insignificantly different from zero.

Inference based primarily on an increase in a parameter estimate's standard error, when the parameter estimate remains large and positive, is not very powerful. The imprecision of Nakosteen and Zimmer's results may reflect the difficulty of specifying the process generating marital status. KN suggest that the problem may be collinearity of the selection term with marital status.

KN offer a different view, claiming that the gains to marriage are large for young (white) men, even in fixed effects regressions. From their preferred specification, which includes years married and its square in the wage equation along with a marital status dummy, KN report a fixed effects marriage premium estimate of 15%. Eighty percent of the premium is attributable to time spent in marriage; only 3 percentage points are due to an (insignificant) intercept shift.

We are skeptical about the Nakosteen and Zimmer and KN results. The weaknesses of the former study are outlined above, but are not addressed here, owing to our belief that marriage premium estimates derived from panel data techniques are more credible.[3] Our skepticism regarding the latter is based on two characteristics of their sample. First, marital status changes represent a relatively small proportion—less than 25%—of their sample. More importantly, the vast majority—over 80%—of status changes are divorces and second marriages. The proportion and nature of the changes is important since identification in fixed effects models is obtained through "within-group" variation (variation over time in the regressors). Thus, the main source of the KN marriage premium is individuals who divorce or marry for the second time during the sample period. As discussed below, the over-representation of divorces and remarriages among the status changes increases the likelihood of an insignificant intercept shift, along with a significant years-married coefficient.

III. MODEL AND ESTIMATION

Our empirical analysis of the differences in wages between married and single men is carried out on models of the form:

$$(1) \qquad y_{it} = \delta + X'_{it}\beta + M'_{it}\gamma + \alpha_i + \varepsilon_{it},$$

where i indexes individuals ($i = 1,\ldots,N$), t indexes time ($t = 1,\ldots,T$), y_{it} is the natural logarithm of the real wage, X_{it} is a vector of explanatory variables and M_{it} is a vector of marriage variables (which in the simplest form of the model contains only a status dummy). The α_i are intended to capture unobserved, individual-specific attributes that are valued in both the labor and marriage markets and therefore may be positively correlated with both wages and marriage, while the ε_{it} reflect aspects of the wage-determining process which may be represented as statistical noise. Also, the α_i and ε_{it} are taken to be i.i.d. random variables, uncorrelated with each other, with zero means and constant variances σ^2_α and σ^2_ε, respectively.

Estimation of (1) depends on whether or not inference is to be performed conditional on the unobserved individual attributes α_i. If not, then (1) is estimated by feasible generalized least squares (FGLS). As long as the set of explanatory variables (X'_{it}, M'_{it}) is uncorrelated with α_i, FGLS will be consistent, as would be OLS. But given the composed disturbance, FGLS will be efficient relative to OLS.[4]

If the explanatory variables (X'_{it}, M'_{it}) are correlated with the individual-specific attributes α_i, then the individual effects should be controlled for in estimation; that is, inference should be conditional on the effects. The simplest means of accomplishing this is by treating the α_i as fixed parameters and estimating (1) by applying OLS to data that has been transformed into deviations from individual means. The fixed effects, or "within," estima-

3. Elsewhere, in Cornwell and Rupert [1993], we have pursued the Nakosteen and Zimmer modelling strategy using the NLSYM, with similarly unconvincing results. The main obstacle is the lack of good instruments for marriage.

4. Note that KN estimate a slightly different version of (1). The individual-specific component, α_i, is ignored in their cross-section estimation, but the T-vector ε_i is assumed to have an arbitrary intertemporal covariance. Estimating (1) under both assumptions, we obtained essentially the same estimated coefficients and standard errors.

tor is consistent regardless of the relationship between explanatory variables and the α_i. The issue of which set of estimates to use in constructing the marriage premium can be resolved by performing a Wu-Hausman test of the difference in the within and FGLS estimates.

IV. RESULTS FROM ESTIMATION

Data

Our marriage premium estimates are derived from a sample of young men drawn from the 1971, 1976, 1978 and 1980 waves of the NLSYM. The young men who comprise the NLSYM were first surveyed in 1966, at which time they were all between the ages of 14 and 24. Thus, in 1971 we begin with a cohort that is aged 19 to 29. As noted above, our sample is an extension of KN's, which uses only the 1976, 1978, and 1980 waves of the NLSYM. Since their sample does not start until 1976, the youngest men are already 24 years old, restricting the number and type of their status changes. Fewer than 25% of their young men change status, and over 80% of the changes are divorces and second marriages.

The details of the construction of the data set are given in KN, so we will not repeat them here. However, we will briefly review the variables contained in the data set and indicate how our sample differs from theirs. After attrition and imposing sample selection criteria (for example, restricting the analysis to white men who have completed their schooling and report a positive wage), they end up with 1228 individuals. For each young man in each time period, KN were able to observe their wage rate, marital status (whether married with spouse present, divorced or never married),[5] years married and divorced, experience (which is actual post-schooling work experience, not the usual age – schooling – 6), residence (whether he lives in the south or in an urban setting), union status (whether the wage is set by collective bargaining), non-spouse dependents (whether there are any), age, education (in terms of highest degree obtained) and one-digit occupational and industry affiliation.

To the KN sample, we add observations on each of these variables from the 1971 wave of the NLSYM. In addition, we include the variable job tenure.[6] Variable means by year and marital state are presented in Table I. The addition of the 1971 data allows us to observe these young men over a period (between 1971 and 1976) when marriage market activity is relatively greater. The result is a higher percentage (34) of the sample changing status and a lower percentage (52) of the status changes coming from divorces and second marriages. At the same time, the ratio of never marrieds to always marrieds is roughly equivalent in both samples.

Extending the KN sample to 1971, while maintaining a balanced panel, forces a reduction in the cross-section size (N) to 666 young men.[7] Despite the smaller cross section, in terms of non-wage characteristics, the average young man in our sample looks very much like the average young man in theirs. The only notable cross-sectional difference is that the average man in our sample is about one year older, due in large part to the elimination of some young men who are still in school in 1971.

Random Effects (FGLS) Results

First we estimate (1) by FGLS, restricting the effect of marriage on wages to an intercept shift. These results, which are consistent only if the explanatory variables are uncorrelated with the individual effects, are reported in column (1) of Table II. As indicated in Table II, the FGLS marriage premium is estimated conditional on labor market experience and its square, five education dummies identifying highest grade completed, dummy variables for residence in the south and in an urban area, a dummy variable for union status of the job and a dummy variable for the presence of non-spouse dependents. Although the coefficient estimates are not presented, we also included eight occupation, eleven industry and ten age dummies, as well as dummy variables to cap-

5. The few men who are married but whose spouse is absent, or are widowed, are included in the divorced category.

6. KN used tenure as a control in regressions that they did not report (see p. 296 of their paper), but the variable was not provided in the sample they sent us.

7. Missing wages reduces N to 966. Another 48 men are lost due to missing years married. Finally, restricting the sample to only those who have completed their schooling leaves us with $N = 666$.

TABLE I
Summary Statistics by Marital Status and Year ($N = 666$)

	1971			1976		
	Married	Divorced	Never Married	Married	Divorced	Never Married
Percent of Sample	68	4	28	85	4	11
Wage	6.00	5.77	5.74	6.46	6.39	6.28
Years Married	5.14	4.61		8.61	5.47	
Years Divorced	0.09	1.33		0.23	3.45	
Experience	6.18	7.65	3.21	8.24	7.17	6.07
Tenure	4.19	3.77	2.28	6.87	5.79	6.14
South	0.32	0.35	0.25	0.31	0.31	0.28
Urban	0.66	0.73	0.78	0.67	0.83	0.78
Union	0.37	0.35	0.28	0.35	0.41	0.28
Dependents	0.73	0.62	0.03	0.85	0.48	0.06
Age	25.1	25.3	22.3	29.5	29.4	28.1
No High School	0.04	0.04	0.02	0.04	0.03	0.01
Some High School	0.12	0.31	0.09	0.13	0.07	0.07
Some College	0.15	0.11	0.31	0.18	0.24	0.27
College Grad	0.15	0.04	0.11	0.14	0.07	0.14
Post-College	0.10	0.04	0.10	0.10	0.07	0.13
	1978			1980		
Percent of Sample	88	3	9	86	6	8
Wage	6.65	6.33	6.47	6.84	6.67	6.67
Years Married	10.5	4.10		12.4	7.78	
Years Divorced	0.30	5.10		0.36	3.71	
Experience	10.0	8.36	8.13	11.8	10.5	10.3
Tenure	7.76	5.15	7.43	8.92	7.58	8.79
South	0.32	0.30	0.28	0.32	0.32	0.31
Urban	0.68	0.65	0.75	0.67	0.71	0.77
Union	0.38	0.25	0.31	0.37	0.37	0.29
Dependents	0.89	0.40	0.11	0.92	0.58	0.09
Age	31.5	31.2	30.2	33.5	32.7	32.3
No High School	0.04		0.02	0.03	0.05	0.02
Some High School	0.12	0.20	0.07	0.12	0.16	0.07
Some College	0.18	0.35	0.26	0.18	0.26	0.23
College Grad	0.14		0.16	0.14		0.16
Post-College	0.10	0.05	0.11	0.10	0.05	0.12

ture year effects. The list of right-hand-side variables in our FGLS regression corresponds exactly to the KN specification (1) (see their Table II), from which they obtain an estimated cross-section marriage premium of 10.6%.

FGLS applied to the dummy variable specification of (1) yields a marital status coefficient estimate of 0.083 with a standard error of only 0.022, which implies significance at any reasonable test size. This suggests a marriage premium of 8.7%, which is about 2 percentage points smaller than the cross-section premium reported by KN. We also find a statistically significant wage premium of about 6.6% for divorced men.

Like KN, our results indicate a positive and statistically significant cross-section effect of non-spouse dependents on wages. When the marriage premium is computed conditionally on the presence of non-spouse dependents, it rises to 14.4%, which is essentially the result obtained by KN for married men with non-spouse dependents. Thus married men with children earn, on average, 5.3% more than married men without children. As we will see below, in our sample the dependents effect is positive, statistically significant and typically about 5%, whether the individual effects are treated as random or fixed, and even when we control for marriage duration. KN's depen-

TABLE II
Estimated Wage Regressions
(Standard Errors in Parentheses)

Variable	(1) FGLS	(2) Within	(3) Within	(4) Within
Married	0.083	0.056	0.051	0.033
	(0.022)	(0.026)	(0.026)	(0.028)
Divorced	0.064	0.062	0.057	0.040
	(0.033)	(0.036)	(0.036)	(0.038)
Years Married				–0.005
				(0.006)
Years Married^2				–0.0003
				(0.0003)
Years Divorced				–0.014
				(0.008)
Experience	0.027	0.027	0.024	0.021
	(0.004)	(0.004)	(0.004)	(0.005)
Experience^2	–0.001	–0.001	–0.001	–0.001
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Tenure			0.013	0.011
			(0.004)	(0.004)
Tenure^2			–0.0006	–0.0005
			(0.0002)	(0.0002)
South	–0.091	–0.121	–0.117	–0.118
	(0.019)	(0.034)	(0.034)	(0.034)
Urban	0.137	0.057	0.059	0.059
	(0.017)	(0.024)	(0.024)	(0.024)
Union	0.109	0.106	0.102	0.103
	(0.015)	(0.018)	(0.018)	(0.018)
Dependents	0.052	0.052	0.048	0.047
	(0.017)	(0.019)	(0.019)	(0.020)
No High School	–0.325			
	(0.057)			
Some High School	–0.148			
	(0.032)			
Some College	0.091			
	(0.028)			
College Grad	0.278			
	(0.034)			
Post-College	0.322			
	(0.041)			
Standard error	0.215	0.212	0.212	0.211
χ^2_{27}	111.9			

dents effect is much less robust, becoming smaller and insignificant in their fixed effects regressions.

With regard to the other coefficient estimates, they are generally typical of those found in the literature and very similar to those obtained by KN in their sample. In general, despite the smaller cross-section dimension of our sample, the basic qualitative and quantitative results obtained from the larger, but shorter KN panel have been preserved. We emphasize that the *only* significant difference between our results and those of KN lies in the returns to marriage.

Fixed Effects (Within) Results

Next, we treat the individual effects in (1) as fixed and re-estimate the model with the same set of explanatory variables as in the FGLS regression. These fixed effects, or within, estimates are given in column (2) of Table II. Note that all time-invariant variables (the education and age dummies) are eliminated along with the effects by the data transformation implied by the within estimator.

The fixed effects regression that produced the estimates in column (2) corresponds to the specification (1′) of KN. Based on our sample, the within estimate of the marital status coefficient is 0.056, and it is statistically significant at the 5% level. This translates into a marriage premium of 5.8%, which is close to the KN result from their model (1′) of 6.1%. The sharp diminution of the marriage premium in our fixed effects regression suggests there are important omitted individual-specific characteristics which are positively correlated with both marriage and the wage rate.

A question not addressed by KN is whether the difference between the cross-section and fixed effects results are statistically significant. The question is important because a statistically significant difference between the two regressions would cast doubt on the cross-section results. Since the within estimates are consistent even if the explanatory variables are correlated with the individual effects, while cross-section based estimates (like those produced by OLS and FGLS) are not, the null hypothesis of no correlation between the regressors and the effects is easily tested through a Wu-Hausman test of the estimates in the first two columns of Table II. The test statistic for this contrast is asymptotically distributed as χ^2_{27} and has a value of 111.9, which is almost three times greater than the critical value. Hence, the null hypothesis is rejected and cross-section based marriage premia along with it. Interestingly, aside from the coefficient of the urban dummy, it is the marital status coefficient estimate that is the most affected by controlling for unobserved heterogeneity.

The regressions in the first two columns do not control for job tenure, which is a misspecification to the extent firms make specific investments in workers. If wages rise with tenure, and tenure is positively correlated with marital status, then excluding it would likely bias upward the marital status coefficient. The results in column (3), where we have added tenure and its square to the baseline fixed effects regression, suggest that this is the case. First, both tenure and tenure-squared enter statistically significantly, and their estimated coefficients imply roughly a 1.3% return per year in the early years on the job. Second, including tenure further depresses the marital status coefficient estimate to 0.051, which translates into a marriage premium of 5.2%.

This result might be expected given the evidence cited from Abraham and Farber [1987] on the relationship between job duration (completed tenure) and marital status. We find the Abraham and Farber result in our sample as well. In an estimated Weibull hazard (not reported) of job duration of the 1971 job, correcting for right-censoring, conditional on union status, education, occupational and industry affiliation and pre-job experience, marriage has the largest statistically significant positive impact.

If the effect of marriage on wages varies over time, the dummy variable specification is incorrect. Furthermore, evidence of a time-varying wage premium would tend to support the view that marriage is productivity enhancing; for example, via ongoing specialization. Thus, we re-estimate our fixed effects regression, adding years married, its square and years divorced.[8] The results from this specification, which corresponds to KN's model (2′) with the addition of tenure and tenure-squared, are presented in column (4).

First, notice that the estimated coefficients of the years-married variables are insignificantly different from zero. In neither case do the coefficient estimates even exceed their standard errors. In contrast, KN report statistically significant years married and years married-squared coefficients of 0.022 and –0.00096. However, as noted above, divorces and remarriages are over-represented among the status changes in the KN sample. Second, including the years-married and years-divorced variables reduces the status coefficient estimate from 0.051 to 0.033 and its associ-

8. Years married and years divorced are measured as the total time spent in each state.

ated t statistic from 1.96 to 1.17. Finally, the estimated coefficients and standard errors of the tenure variables are essentially unaffected by the addition of years married and divorced.

In sum, when we control for unobserved heterogeneity and job tenure, our sample does not provide evidence of a statistically significant time-varying marriage premium. If marriage influences wages at all, the premium is a pure intercept shift and only about 5%, which is much smaller than standard cross-section results suggest. To the extent it exists, such a shift in the wage-generating process might be regarded as the effect of "settling down"—a kind of structural break involving adjustments in market work and homework in the move from single to married life. It is possible that years married captures the same thing in the KN sample. Since the marriage premium in their study is identified largely by men who divorce and remarry (not by men who marry for the first time), years married (rather than the status variable) may indicate whether a man ever "settled down." It is reasonable to suppose that such adjustments are more likely to have taken place in marriages that lasted longer.

That marriage also signals certain individual attributes that are valued in the labor market is further supported by the relationship between prospective marriage and current wages. Using the 1971 cross section, we define the variable, "to-be-married," as a dummy equal to one if a (single) young man marries sometime during the sample period (after 1971). Then, in a separate regression (not reported), we estimated the effects of to-be-married on the 1971 wages of the young men in our sample. Conditional on the 1971 values of the explanatory variables corresponding to column (1) in Table II, the wages of those who are not yet but will be married are about 4% higher than those who are married in 1971. The to-be-married coefficient estimate is 0.148 and the marital status coefficient estimate is 0.106; the former is statistically significant at the 5% level, while the latter is significant at only the 10% level.[9] Apparently, those young men who are not married, but for whom marriage is likely—i.e., they have characteristics that lead to success in the marriage market—are at least as productive as men who are already married.

The Effect of Children

As noted above, we find the positive effect of children on wages to be robust across a wide variety of specifications. Although conditioning on the individual effects reduces the overall marriage premium, the positive and significant effect of non-spouse dependents is unchanged. The results in column (2) of Table II still suggest that young men with non-spouse dependents earn 5.3% more those without. Thus, the premium for married men with children is over 11%. Controlling for tenure diminishes the "dependents premium" slightly, but it remains nearly 5% and statistically significant.

With regards to the marriage premium, the robust effect of children on the wages of young men raises several questions. Since married couples with children usually have been married longer than childless couples, could the dependents premium be masking the effect of years married on wages? If not, how are the wages of young men affected by the presence of children in the household? Does the effect of marriage on wages depend on the presence of children?

First, it does not appear to be the case that the dependents variable and years married are measuring the same thing. When we omit the dependents variable from the specification given in column (4), the estimated coefficients (and standard errors) of years married and its square are, respectively, 0.00008 (0.006) and –0.0005 (0.00027). Thus, the years-married coefficient estimate is clearly insignificant, while the estimated coefficient of the quadratic term has a marginal significance level of only 0.06. Dropping dependents does affect the marital status coefficient estimate; it rises to 0.41, although it remains insignificant even at the 10% level.

Second, the robust effect of children on wages seems to be like that of marriage depicted in column (3)—a change in the process generating wages associated with the reallocation of market, housework and leisure time to finance the added costs of children. At the simplest level, young men with children will allocate more time to market work and less to leisure; for example, they may arrive to work

9. The large return to marriage is due to the fact that it is obtained from a cross section regression.

TABLE III
Dependents and the Returns to Marriage
(Standard Errors in Parentheses)

Variable	(1) Within	(2) Within
Married	0.071 (0.026)	0.027 (0.038)
Divorced	0.008 (0.049)	− 0.033 (0.058)
Years Married		0.011 (0.012)
Years Married^2		−0.001 (0.0007)
Years Divorced		−0.013 (0.011)
Experience	0.024 (0.004)	0.021 (0.005)
Experience^2	−0.001 (0.002)	−0.001 (0.0002)
Tenure	0.013 (0.004)	0.011 (0.004)
Tenure^2	−0.0006 (0.0002)	−0.0005 (0.0002)
South	−0.113 (0.034)	−0.109 (0.034)
Urban	0.061 (0.023)	0.061 (0.024)
Union	0.101 (0.018)	0.110 (0.018)
Dependents	0.292 (0.076)	0.281 (0.076)
Married × Dependents	−0.266 (0.077)	−0.232 (0.087)
Divorced × Dependents	−0.156 (0.095)	−0.124 (0.103)
Years Married × Dependents		−0.013 (0.012)
Years Married^2 × Dependents		0.001 (0.0008)
Years Divorced × Dependents		−0.001 (0.012)
Standard error	0.213	0.211

more promptly. Or, as indicated by Reed and Harford [1989], young men with children also may accept jobs that offer fewer amenities and benefits, but greater wage compensation.

Finally, as indicated in Table III, the dependents (marital status) effect varies significantly with the marital status (dependents) effect. Column (1) reports results from a regression which omits years married and divorced (as in Table II, column (3)), but includes an interaction of marital and divorced status and dependents. Column (2) adds the years married variables (as in Table II, column (4)) and interactions thereof with dependents to test whether the effect of children (marriage) varies with years married (children in the household).

In the first case, the interaction between marital status and dependents is large, negative and statistically significant. With the interaction, wages of married men with children are still about 10% higher than childless single men (which is similar to the premium implied in column (3) of Table II), but the estimated premium for married men without children rises from 5.2% to 7.3%. In the second case, the only term involving a marriage variable that enters statistically significantly is the interaction between status dummy and dependents. Like in column (1), its coefficient estimate is large and negative. Although they are not statistically significant, the estimated coefficients of the years-married variables and their interactions with dependents suggest that the marriage premium for young men with children declines during the early years of marriage.

V. CONCLUSIONS

While there is little or no debate about the claim that married men earn more than unmarried men, the source of the wage premium remains unsettled. Using a sample of young men from the NLSYM, observed between 1971 and 1980, we show that much of the return is associated with unobservable individual effects that are correlated with marital status and wages. Fixed effect regressions, which condition on the unobservables, substantially attenuate the estimated wage premium normally attributable to marriage. Moreover, to the extent that there is a gain to marriage, it is purely an intercept shift (does not vary with years married) and is no greater than 5% to 7%. We regard this as evidence against marriage enhancing productivity through specialization. In contrast, KN [1991] report a large marriage premium that increases with years married; however, their panel (also drawn from the NLSYM) is shorter and the status changes contained therein are dominated by divorces and remarriages. As an alternative to the specialization story, perhaps marriage induces men to "settle down" and substitute leisure for wages.

Additional support for this view is provided by two findings which are anomalous in the specialization story of marriage. First, men who are not married in 1971, but who eventually will be, earn at least as much as those who are already married. Second, conditional on marital status and years married, the presence of children raises wages about 5%, regardless of the estimator employed. Parenthood seems to affect wages similarly to marriage—as a shift in the wage-generating process involving adjustments in market work and homework.

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