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ORIGINAL PAPER

Cohabitation Premium in Men's Earnings: Testing the Joint Human Capital Hypothesis

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Abstract This paper provides new evidence on the increase in wage earnings for men due to marriage and cohabitation (in the literature, commonly referred to as marital and cohabitation wage premiums for men). Using data for a sample of white men from the National Longitudinal Survey of Youth 1979, the paper shows that even after accounting for potential selection bias there is a cohabitation wage premium for men, albeit smaller than the marriage premium. Our analysis shows that a joint human capital hypothesis (*a la* Benham in J Polit Econ 82(2, Part 2):S57–71, 1974) with intra-household spillover effects of partner's education can explain the existence of the wage premiums. Our estimates provide some empirical support for the joint human capital hypothesis.

Keywords Cohabitation · Marriage · Wage premium

Introduction

It is now empirically well recognized among researchers that marriage is associated with higher earnings for men. However, a perfunctory look at the data would reveal that over the last three decades men in the U.S. have not only been postponing marriage, but further, a growing percentage of the population is never getting married. A plausible explanation for such delay in reaping the benefits of marriage might lie in the fact that men can acquire similar benefits in another comparable form of family union, viz., cohabitation.[1] The present study brings together new evidence on the earnings benefits of forming a family as we try to identify whether or not wage benefits accrue to cohabiting men as they do to married men.

Considering the dramatic rise in cohabitation over the last four decades, the significance of understanding the implications of cohabitation for men's wage earnings cannot be overemphasized. Evidently, men obtain an increase in wage earnings when they are married even after accounting for selection effects (in the literature, this is commonly referred to as "marital wage premium" for men). It is then imperative to ask whether men obtain any increase in wage earnings when they are cohabiting (i.e., whether there is any "cohabitation premium" in men's wages). While researchers in recent years have started to examine this question using data from the United States, very few have attempted to address the potential selection problem in estimating cohabitation wage premium. This paper provides critical empirical evidence in this regard. The evidence would also indicate whether the underlying intra-household choice decisions in cohabitation are similar to those in marriage.

The focus of this paper is to understand the nature of the marital and cohabitation wage premiums for men by estimating wage equations using longitudinal data that allow for both differential wage growth and selection effects. Our empirical estimates provide new evidence in this regard. In order to explain the existence of marital as well as cohabitation wage premium, we utilize the joint human capital hypothesis put forward by Benham (1974). According to this hypothesis, partner's human capital contributes positively to

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[1] Benefits of marriage potentially include improved health, decreased mortality, and improved well-being for children, along with increased earnings for men. A recent review of the literature on benefits of marriage is provided by Ali and Ajilore (2011), Wood et al. (2007), Mamun (2005), and Ribar (2004).

a married or cohabiting man's effective stock of human capital and thereby increases his productivity, which translates into increased wage earnings for him. In other words, the marriage and cohabitation wage premiums in men's earnings are reflections of intra-household spillover effects of partner's human capital. Thus, the hypothesis offers a causal mechanism linking men's productivity and their family status. This hypothesis has not received adequate attention in the relevant empirical literature heretofore, and applying this hypothesis is a key contribution of the present study.

The relationship between cohabitation and men's earnings also has important policy implications given the recent upsurge in interest among policy makers for a direct intervention to promote "healthy marriages". Ignoring cohabitation as a family form in the analysis of the effects of marriage on men's earnings can potentially lead to ineffective policy intervention aimed at improving well-being in the family. The empirical evidence presented in this paper provides a broader understanding of the effects of marriage vis-à-vis that of cohabitation for men's earnings, which may provide important input to policymaking.

Some Conceptual Discussions

Wage Differential for Cohabiting Men

Over the past several decades there has been a considerable growth in the number of people making their first family union in cohabiting relations in the U.S. The number of households formed by cohabiting couples increased from 1.1 million (1.5% of all households in the U.S.) in 1970 to about 6.3 million (5.6% of all households) in 2007 (Kreider and Elliott 2009; Casper and Cohen 2000). Moreover, the percentage of marriages preceded by cohabitation rose from about 10% for those marrying during the period 1965–1974 to well over 50% for those marrying during 1990–1994 (Bumpass and Sweet 1989; Bumpass and Lu 2000). Goodwin et al. (2010) using data from the National Survey of Family Growth 2002 also found that about 28% of men and women cohabited before their first marriage. Altogether these studies indicate that over the years cohabitation is evolving to be a major mode of living arrangement—either as a transitional phase prior to a more permanent arrangement in marriage, or as an alternative family arrangement.

Considering the rise in the prevalence of cohabitation, it is important to understand how cohabitation might influence men's earnings. To that end, let us briefly discuss how cohabitation compares with marriage as a family arrangement. This discussion would also point out why one may expect the effect of cohabitation on men's wages to be similar to that of marriage.

The functional aspects of these two forms of co-residential relationships are very similar. For instance, the type of intra-household specialization that occurs in marriage can also be hypothesized in a cohabiting union. Also, intra-household spillover effects of a partner's human capital endowment can be similar in married and cohabiting households. As a result, we might expect cohabiting men to demonstrate labor market experiences that are similar to those of a married man and significantly different from the experiences of non-cohabiting single men. The theoretical explanations that are offered for the existence of men's marital wage premium can all be equally applied for cohabiting men. Hence, a priori there are sufficient grounds to expect that cohabiting men might earn a wage premium in the labor market.

Despite these broad similarities, there are certain differences between marriage and cohabitation. The primary difference is legal. In the U.S., the majority of jurisdictions follow the general rule that unmarried cohabitating couples do not achieve legal rights or obligations analogous to those possessed by a married couple (Seff 1995). Also, given the relatively higher cost of dissolving a marital union than a cohabiting union, the degree of commitment to the relationship is expected to be higher in marriage. Because marriage involves higher level of commitment and legal responsibilities than cohabitation, married men may choose to be more dedicated toward employment, increasing their effort, and hence productivity at work, and consequently, earn a higher wage premium than cohabiting men.

In the present study, we consider cohabitation as a co-residential union since the National Longitudinal Survey of Youth 1979 (NLSY79) identifies cohabiters by the presence of an unmarried partner of the opposite sex in the household (further details on data and construction of variables are provided later in the paper). However, cohabitation is recognized to be a particularly diverse arrangement that ranges from a short-term co-residential relationship considered to be an alternative to dating, to an alternative family arrangement in itself which is economically identical to marriage (Manning and Smock 2005; Seltzer 2000). The heterogeneous nature of cohabitation could cause the interpretation of the effect of cohabitation on men's wages to be somewhat ambiguous. We conduct appropriate sensitivity analysis to understand the robustness of our key estimates, and address any potential ambiguity about the findings.

Explaining Family Union Premium in Men's Wage: A Theoretical Framework

Practically all cross-sectional estimates of human capital wage equations find that married men earn more than otherwise comparable single men. To explain the marital wage premium, a number of hypotheses have been put forward in the literature, and these may be broadly identified into two categories: selection hypothesis and causal explanations. The selection hypothesis indicates that men with higher unobservable skills that are valued in the labor market select into marriage (Becker 1991; Korenman and Neumark 1991; Nakosteen and Zimmer 1987). Causal explanations for marital wage premium include the following. First, the specialization hypothesis that argues that marriage per se makes a man more productive by allowing him to specialize in non-household work, and pay differentials ensue from the differentials in productivity (Becker 1985, 1991). Second, employers discriminate against single men, and the marital wage premium reflects employer favoritism (Bartlett and Callahan 1984; Hill 1979). Third, the marriage premium is a reflection of the differences in the workers' taste and compensating wage differentials (Reed and Harford 1989). And fourth, the joint human capital hypothesis, that suggests that the wife's human capital contributes to the husband's productivity and consequently married men have higher earnings (Benham 1974).

Substantial research has been conducted to identify the selection bias in the estimated marital wage premium. The existing literature has also extensively dealt with the first three causal explanations, although the empirical tests do not provide compelling evidence for accepting or rejecting any of these three hypotheses. The literature, however, does not adequately explore the potential for the joint human capital hypothesis, which this paper considers as an explanation for marital and cohabitation wage premiums.

We follow Benham (1974) in considering the effective stock of human capital for a man in a family union (H_t^*) to be a positive function of his own stock of human capital as well as his partner's stock of human capital:

$$H_t^* = H(H_t^O, H_t^P),$$
$$\text{where } \partial H_t^*/\partial H_t^O > 0 \text{ and } \partial H_t^*/\partial H_t^P > 0.$$

Since a man's market (and non-market) productivity is a function of his effective stock of human capital, increments to the capital stock of the partner will be reflected in his productivity in the market, which would translate into increased wages for him. The presence of a better educated spouse or partner can contribute to men's work performance directly or indirectly, e.g., by assisting in the central task related to his job, by influencing decisions regarding job changes and transfers (e.g., see Mano-Negrin and Kirschenbaum 2000), by investing in his human capital, by assisting in his peripheral tasks such as social relations and networks at workplace (Grossbard-Shechtman 1993). Any of such contributions can substantially affect his earnings. Single men, on the other hand, will not be in a position to extract such benefits from their dating partners or friends presumably because of the absence of household production activities that integrate the preferences and opportunities of the individuals involved. Thus, married or cohabiting men would benefit from the presence of a better-educated spouse or partner, and obtain a wage premium compared to their unmarried, non-cohabiting counterparts.

However, a positive relationship between a partner's human capital and men's earnings can arguably reflect selective-mating. The hypothesis of selective-mating asserts that the more productive men marry or cohabit with more highly educated women. In other words, men with higher observable and unobservable skills that are valued in the labor market might be more likely to partner with better-educated women. Benham (1974) as well as Jepsen (2005) identified a positive association between wife's schooling and the husband's earnings, but due to the cross-sectional nature of their data, they were unable to conclusively remove the possibility that such positive association might reflect selective mating. Using longitudinal data we are able to address the selective-mating issue, at least to the extent that such self-selection occurs over men's time-invariant unobservable characteristics.

Family Union Status and Wage Differentials: A Brief Review of Empirical Literature

We present a brief review of the studies that directly address the selection issue in men's marital wage premium, and also consider research that looks into the effect of cohabitation on men's earnings.[2] It is evident from the review that the size as well as the source of marital wage premium is still a debated issue, and that there is very limited evidence on the effect of cohabitation on men's earnings in the U.S. The present study not only provides additional evidence in this regard, but also delineates the joint human capital hypothesis as a causal mechanism explaining the existence of wage premiums for married and cohabiting men.

Studies of Wage Differential for Married Men

A number of empirical studies on men's earnings and marriage have tried to directly address the issue of selection bias, which denotes the notion that a positive association between marriage and earnings is observed because men who are likely to earn more self-select into marriage.

[2] Partial summaries of cross-section studies on men's marital wage premium, which are commonly criticized for their inability to address selection, are available in Ribar (2004), Korenman and Neumark (1991), Nakosteen and Zimmer (1987), and Kenny (1983).

The findings from these studies provide mixed evidence on whether married men earn a wage premium over and above what a selection hypothesis would suggest. Some studies show that men earn a wage premium even after accounting for selection. For example, Korenman and Neumark (1991) using data for white men from the 1976–1980 National Longitudinal Survey of Young Men (NLSYM), Stratton (2002) using data from the National Survey of Families and Households 1987–1988 and 1992–1994, Ginther and Zavodny (2001) using 'shotgun weddings' as a natural experiment, and Antonovic and Town (2004) using data from the Minnesota Twin Registry—all find that selection can explain only a fraction of the marital wage premium observed in their data. Some studies, however, conclude that selection underlies much of the observed marriage premium in men's earnings (Cornwell and Rupert 1997). Some other studies, on the other hand, report that for an earlier cohort of men in the U.S., the marriage premium resulted from the productivity enhancing effects of marriage, but for later cohorts, the marriage premium reflects only selection (Blackburn and Korenman 1994; Gray 1997).

Among the studies that find evidence of marital wage premium even after accounting for possible selection effects, intra-household specialization is a prominent candidate as an explanation of marital wage premium. However, Hersch and Stratton (2000) using data for white men from the National Survey of Families and Households show that marital wage premium is not substantially affected by controls for home production activities. Conversely, Loh (1996) argue that men whose wives are active in the labor market must have relatively less scope for intra-household specialization, and hence, should receive a lower marriage premium. Empirical estimates in Loh (1996) show that the size of the marriage wage premium did not vary with how long the wife's had been engaged in the labor market. Moreover, results in Astone et al. (2010) suggests that men's work effort may depend on whether their fatherhood status, more so that than their marital status. Taken together, these studies suggest that the marriage premium is not explained by specialization within the household.

To summarize, the empirical literature indicates that for men a marital wage premium exists even after accounting for selection, but there is mixed evidence on intra-household specialization as the mechanism underlying any increase in married men's productivity which can explain the marital wage premium for men. Further research is clearly called for. The current study goes beyond the specialization hypothesis, and offers evidence on an alternative mechanism that helps explain the increase in productivity and the resultant marital wage premium for men.

Studies of Wage Differential for Cohabiting Men

Research on wage differentials for cohabiting men is considerably more limited. Cross-sectional evidence suggests the existence of a cohabitation premium in men's earnings, that is, currently cohabiting men receive significantly higher wages than non-cohabiting never married men (Cohen 1999, 2002; Daniel 1992; and Loh 1996). A handful of studies that have taken into account the possibility of selection do not provide clear evidence on whether cohabitation premium simply reflects selection. Among these, only one study uses data from the U.S.—Stratton (2002) uses a sample of white men in the National Survey of Families and Households—and finds that controlling for individual specific selection effects, the marital wage premium persists but the cohabitation premium disappears. The other studies that examine cohabitation premium in men's earnings are mostly focused on non-U.S. populations: Bardasi and Taylor (2008) uses data from the British Household Panel Survey, Gupta et al. (2007) uses panel data for young men in Denmark, and Richardson (2000) uses longitudinal data from the Swedish Level of Living Survey. Similar to Stratton (2002), Bardasi and Taylor (2008) find that after accounting for selection, while there is substantial marriage premium in men's earnings, cohabitation has no significant effect on men's earnings. However, both Gupta et al. (2007) and Richardson (2000) report that even after accounting for selection, men earned significant marriage and cohabitation premiums.

Given the limited and mixed empirical evidence, further research is needed to identify the effects of cohabitation on men's wage rate. The need for further research seems all the more relevant as cohabitation has emerged as a key family status in the last few decades for men and women in the U.S. Moreover, in the context of increased investment in women's human capital and rising female labor force participation in the U.S., the joint human capital hypothesis pursued in this paper is expected to provide better understanding of the underlying mechanism that generates wage differentials by men's family status.

Empirical Methodology

A standard cross-section log wage regression augmented by controls for family status is the starting point for analyzing the effects of family status on wage. However, given that unobservable characteristics that enhance labor market achievements may also augment a man's prospects of finding a partner, either in marriage or in cohabitation, an improved model would be of the following form:

$$\ln(W_{it}) = \alpha + \beta \cdot X_{it} + \gamma \cdot FST_{it} + A_i + \varepsilon_{it} \quad (1)$$

where W_{it} is the wage of individual i in year t, X_{it} is a vector of observable characteristics, FST_{it} is the family status of individual i in year t, and A_i is an unobserved characteristic of individual i, which is assumed to be time-invariant. The selection of men with wage-enhancing attributes into family union suggests that $Cov(FST_{it}, A_i) > 0$. Since A_i is unobservable, for ordinary least squares estimation, A_i would be part of the error term, and hence the estimated $\hat{\gamma}$ would be biased upwards. Using the panel structure of the data, we can employ a "within" or fixed effects estimation technique to remove the selection bias. The estimation model in this case would be:

$$\ln(W_{it}) - \ln(\overline{W_i}) = \alpha + \beta \cdot (X_{it} - \overline{X_i}) + \gamma \cdot (FST_{it} - \overline{FST_i}) + v_{it} \quad (2)$$

where for any variable Z, $\bar{Z}_i$ denotes the mean of Z for individual i across the years t.[3] We use the fixed-effects estimator available in Stata to estimate the model.

As pointed out in Korenman and Neumark (1991), the benefits of a co-residential (marital or cohabiting) union may not accrue quickly and may depend upon the duration of the union. Consequently, we include measures of duration in different family status to examine whether the effect of marriage and cohabitation changes over time, and whether such duration effects differ across married and cohabiting men. These duration measures not only include duration in current status, but also duration in previous marriage and duration in cohabitation with current wife prior to marriage. The latter two measures of duration are intended to control for the effect of a man's life-course experience in co-residential unions on his earnings. Also, considering the plausible non-linear effects of relationship duration, we incorporate a quadratic term of each duration measure included in the specification.

In order to test the joint human capital hypothesis as a mechanism that can explain the family union premium in men's wages, one specification includes measures of partner or wife's level of schooling, considering schooling to be a measure of her stock of human capital. The schooling measures are included in the specification as interaction terms with marriage and cohabitation indicators. Thus the coefficients on these interaction terms would indicate whether the marital premium varies by the level of partner's schooling. However, we recognize that due to the possibility of selective-mating, partner's education is potentially endogenous in our specification. With the identifying assumption that such selective mating occurs over men's time-invariant characteristics, fixed effects estimates of the associated coefficients will be consistent.

All the specifications presented in this study include quadratic controls for age, dummy variable for residence in Standard Metropolitan Statistical Area (SMSA), unemployment rate for labor market of current residence, three dummy variables for region of residence, and 7 year-dummy variables.[4] Three dummy variables for own education as measured by the highest grade completed (high school graduate, some college education, college graduate or higher, with less than high school graduate as the comparison category) are also included in the least square specifications. Since these measures of own education are time-invariant, they drop out of the fixed-effects model. Our specifications consciously exclude a number of variables such as tenure in current job, coverage by collective bargaining, occupation and industry, and presence of children in the household. Although these are commonly included in the related literature as explanatory variables, we feel that all of them are potentially endogenous to family status and wage.[5] Our estimates (reported in Table 5 in the Appendix) from specifications that include tenure, tenure squared, dummy for union coverage, industry and occupation dummy variables, and number of children in the household show that the results remain qualitatively similar to those presented here.

NLSY79 Data and Summary Statistics

The National Longitudinal Survey of Youth 1979 (NLSY79) is a nationally representative sample of young men and women who were 14–22 years old when they were first interviewed (Center for Human Resource Research 2001). The respondents were interviewed annually until 1994, and biennially since then. In this paper, we use data from the 12th through the 19th round (1990–2000) of the survey to construct a panel of repeated observations on individuals. Data from the later years of the survey are considered for two reasons: first, data on the duration of cohabiting relationships is available only for the survey rounds implemented since 1990; second, and more

[3] The intercept α in Eq. 2 reflects the average value of the individual fixed effects A_i. Stata parameterizes the fixed-effects estimator under the constraint that average fixed-effects is zero $[E(A_i) = 0]$, and reports an estimate for α in the output. We keep α in Eq. 2 essentially to remain consistent with Stata's output. This has no implication for the coefficient estimates. For further discussion, see Gould (1997).

[4] Although an 11-year period is covered by the data, since we have data from eight rounds of the survey, we could only include 7 year-dummies. These year-dummy variables are included to capture the year-specific inflation in the individual's nominal wages. The quadratic controls for age are included as proxies for potential experience.

[5] For example, Bratsberg and Terrell (1998) provide a discussion of the endogeneity of experience and tenure in a wage equation.

importantly, we would like to conduct our analyses based on the post-schooling labor market experience of the men in the sample and data from the later rounds can be considered to be generally more appropriate in this regard (the youngest men were 25 years old in 1990).[6] In fact, we exclude all individuals who were ever enrolled in school during 1990–2000, so that our analyses would not be confounded by the individual's decision to work for a temporary period only to return to school later on. Thus, the sample is restricted to men who completed schooling by 1990, and for whom all the required variables are available.

The analyses in this paper employ a sample of non-black non-Hispanic (identified by the survey screener) men from the NLSY who self-identified themselves as ethnically European. Thus, our sample of white men excludes the Native Americans, Asians and others from the Non-black/Non-Hispanic sample.[7] Out of 3,790 non-black non-Hispanic men in the NLSY79 sample, 892 were not interviewed during the 1990–2000 period, leaving us with 2,898 men in the sample. 645 of these men were dropped since they identified themselves as ethnically Native American, Asian or other. Conditioning on ever-enrolled in school during the period under consideration reduced the sample by another 279. We also dropped the cases where the respondent indicated having a same-sex partner, reducing the sample by 85 men. In the remaining sample, 425 men had information for a single year only, making them inappropriate for panel data analysis, and consequently dropped from the sample. After meeting all data requirements, we have 9,924 observations on 1,464 men in the sample.

Marital status data was compiled using the yearly created "marital status" variables in NLSY79. Since 1987, the survey also asked all respondents not living with a spouse about opposite-sex partners. We generated the respondent's cohabitation status from this partner variable. From 1990 round of the survey, data is also available on (1) the month and year the respondent and his/her opposite-sex partner began living together; (2) whether the respondent lived with his/her spouse before marriage; (3) the month and year the respondent and his/her spouse began living together; and (4) whether the respondent and his/her spouse lived together continuously until marriage. We used this information to measure relationship duration in partner specific spells.

The baseline (1990) summary statistics for the men in our sample are presented in Table 1 by their family union status. The first row of Table 1 presents mean hourly wages in dollars for men in different family contexts. The dependent variable in the regressions that follow is the natural logarithm of the hourly wage. In NLSY79 wages are available as hourly wages for hourly workers, and are constructed (in NLSY) from weekly or annual earnings divided by the appropriate hours for those who report non-hourly earnings. Married men have higher hourly wages than men in the three other family status groups, viz., never married, currently cohabiting, and divorced or separated.[8]

The figures in Table 1 also indicate that non-wage characteristics differ according to a man's family status. For example, never married and cohabiting men in the sample are younger than the married men. Married men worked about 3 hours more per week than never married men, and about 3 weeks more per year than never married or cohabiting men. Also, compared to the other groups, married men have enhanced job stability as indicated by the substantially higher mean tenure at current job. Between cohabiting and married men, the fraction of the sample having at least a high school graduate education is larger in the married group. On average married men are more likely to have wives who completed at least high school than the partners of cohabiting men. The figures in Table 1 also indicate that married men in this sample have spent, on average, about 2 years living together with their spouse prior to marriage, while the average length of current marriage for these men is little less than 6 years. The average length of cohabiting relationships for currently cohabiting men is about 2 years, which is substantially less than the average years in marriage for married men. The last two rows in Table 1 reflect that while a significant proportion of men who were never married in 1990 were married by 2000, the proportion of men in cohabiting unions remained unexpectedly stable over this period.

[6] One can potentially collate data on cohabitation status and cohabitation duration since 1979 for the NLSY79 sample by using the partner identifier variable. This would allow for creating a longitudinal sample where each individual will be included after they have reached age 25 (the oldest respondents reached this age in 1982) and have completed their schooling. While such a "uniform age" sample would broaden the size of the sample, there are a few disadvantages of using such a sample: first, the risk of error in inferring cohabitation status and duration from partner identifier, and second, changes in the broader economic and social environment over the two decades that would be covered by such a sample can potentially confound the results.

[7] A Chow test rejects, at 1% significance level, pooling of the data on all non-black non-Hispanics for the specifications reported.

[8] Discussions in this section comparing characteristics of married men and men in other family status as of 1990 are based on appropriate analysis of statistical difference using a two-tailed *t*-test. The differences mentioned in the text are all statistically significant at least at the 10% level. The results of the t-tests are not portrayed in Table 1.

Table 1 Summary characteristics of sample by family union status (NLSY white men, 1990)

Variables	Never married	Cohabiting[a]	Married	Divorced or separated
Hourly wage (in dollars)[b]	11.71	10.33	12.40	9.99
Hours worked per week	43.60	45.62	46.49	45.09
Weeks worked per calendar year	46.53	47.09	49.64	46.58
Age (in years)	28.30	28.46	29.35	29.46
Years in marriage, total	–	2.02	6.16	5.17
Years in current marriage	–	–	5.76	–
Years div. or sep.	–	1.01	–	2.32
Years cohabiting	0.32	2.17	–	0.32
Years cohabited with wife before marriage	–	–	2.04	–
Has schooling level				
Less than high-school (<12)	0.10	0.18	0.10	0.16
High-school grad (=12)	0.41	0.53	0.51	0.63
Some college (>12 & <16)	0.18	0.17	0.17	0.15
College grad (≥16)	0.32	0.13	0.23	0.06
Partner's schooling level				
Less than high-school (<12)	–	0.15	0.08	–
High-school grad (=12)	–	0.54	0.48	–
Some college (>12 & <16)	–	0.18	0.22	–
College grad (≥16)	–	0.10	0.22	–
Missing	–	0.03	0.01	–
Lives in the				
North east	0.24	0.22	0.20	0.16
North central	0.36	0.32	0.37	0.34
South	0.21	0.22	0.27	0.31
West	0.19	0.24	0.16	0.18
Lives in SMSA	0.79	0.83	0.71	0.76
Local unemployment rate	2.34	2.28	2.39	2.33
Years in current job	2.88	2.93	4.39	3.02
Is covered by union	0.14	0.15	0.18	0.16
Sample size (1990)	394	78	732	137
Proportion of sample in different status				
1990	0.31	0.06	0.58	0.11
2000	0.14	0.05	0.69	0.17

[a] About 63% of currently cohabiting white men are never married

[b] For our sample we dropped the cases where hourly wages fell outside the \$1–\$100 range

Empirical Findings

Results on Marriage and Cohabitation Premium

Since selection has been the center of debate regarding wage differentials by marital status, in presenting our empirical findings we focus mostly on the fixed effects estimates of the different wage equation specifications. Parallel cross-sectional estimates are also produced to provide convenient comparison. The empirical findings are reported in Table 2 (The full set of estimates is reported in the Appendix Table 5).

The first column in Table 2 presents the fixed effects estimates of a wage equation specification which contains two marital status dummy variables (viz., married, and divorced or separated) and the common set of covariates indicated above, but does not control for cohabitation status. These results, therefore, correspond to the longitudinal estimates of the marriage premium available in the literature. The estimates show that controlling for the other covariates, married men earn about 5% more than the unmarried men. A comparison with the cross-section estimates of the same specification in column 6 shows that a large proportion of the cross-section marital wage premium

Table 2 NLSY wage regression: white men 1990–2000 dependent variable: Ln (hourly wage)

	Longitudinal					Cross-sectional				
	Dummy var. spec (w/o cohabit) (1)	Dummy var. spec (2)	Duration spec (3)	Partner's edu spec (4)	Partner's edu spec X (partner employed) (5)	Dummy var. spec (w/o cohabit) (6)	Dummy var. spec (7)	Duration spec (8)	Partner's edu spec (9)	Partner's edu spec X (partner employed) (10)
Married	0.052 (2.69)**	0.061 (3.08)**	0.033 (1.50)	−0.021 (0.35)	0.042 (0.77)	0.213 (8.55)**	0.222 (8.57)**	0.124 (3.77)**	−0.046 (0.57)	0.150 (2.00)*
Divorced/separated[c]	−0.007 (0.27)	−0.002 (0.09)	0.012 (0.42)	0.062 (1.01)	0.068 (1.11)	0.049 (1.55)	0.049 (1.54)	0.068 (1.45)	0.06 (0.70)	0.138 (1.55)
Cohabit		0.036 (1.90)†	0.014 (0.54)	−0.02 (0.32)	0.072 (1.36)		0.065 (2.23)*	0.05 (1.29)	0.049 (0.59)	0.058 (0.74)
Years in current marriage			0.016 (3.76)**	0.016 (3.81)**	0.015 (3.48)**			0.02 (3.37)**	0.019 (3.21)**	0.001 (0.11)
Years div/sep			0.003 (0.36)	0.002 (0.33)	0.003 (0.36)			0.008 (0.68)	0.01 (0.82)	0.005 (0.39)
Years cohabit			0.017 (1.44)	0.017 (1.40)	0.013 (1.08)			0.007 (0.40)	0.005 (0.29)	0.018 (0.87)
Years in previous marriage			0.004 (0.63)	0.007 (1.01)	0.005 (0.69)			−0.01 (1.08)	−0.009 (1.05)	−0.011 (1.17)
Years cohab w/wife			0.001 (0.08)	0.002 (0.24)	0.002 (0.22)			0.009 (0.87)	0.01 (0.97)	0.009 (0.88)
Married * wife HS grad				0.07 (1.97)*	−0.032 (2.01)*				0.147 (3.11)**	−0.071 (2.70)**
Married * wife somecoll				0.023 (0.59)	−0.047 (2.40)*				0.145 (2.79)**	−0.072 (2.32)*
Married * wife collgrad				0.00 (0.00)	−0.052 (2.46)*				0.184 (3.43)**	−0.045 (1.34)
Coh * partner HS grad				0.117 (2.23)*	−0.007 (0.17)				0.103 (1.42)	0.069 (1.17)
Coh * partner somecoll				0.03 (0.46)	−0.105 (1.83)†				0.11 (1.22)	0.092 (1.13)
Coh * partner collgrad				0.013 (0.17)	−0.109 (1.64)				0.206 (1.96)*	0.178 (1.76)†
Observations	9924	9924	9924	9924	9924	9924	9924	9924	9924	9924
Number of id	1464	1464	1464	1464	1464					
R^2	0.25	0.25	0.25	0.25	0.25	0.29	0.29	0.29	0.30	0.30

[a] Also included in the longitudinal specifications are: age, age-squared, squared terms of the various relationship duration measures, dummy for SMSA, local unemployment rate, regional dummy (3), and year dummy variables (7). In addition, specifications in columns 4, 5, 9, and 10 include own education interacted with family status. The pooled cross-section regressions include 3 schooling dummy variables along with the controls listed above for the longitudinal specifications. Being time-invariant, the schooling dummy variables drop out of the fixed effects model. The standard errors for the cross-sectional estimates are robust. The full set of estimates is reported in the Appendix Table 5

[b] Divorce and separated category includes a small number of widowers

[c] Absolute value of t-statistic in parentheses; † $p < 0.10$, * $p < .05$, ** $p < .01$, in a two-tailed test

is associated with individual specific selection effects, the estimated cross-section premium being more than 21%. The estimated coefficient for the divorced or separated dummy is not statistically significant. These fixed effects estimates are very similar to those in Korenman and Neumark (1991) who report a 6% marriage premium and no significant effect of divorce/separation, when any control for duration of relationships is not included. Longitudinal estimates in Stratton (2002) with a similar specification, however, indicate that there is no significant effect of marriage on men's earnings.

Columns 2 and 7 of Table 2 present the estimates for the specification which directly includes an indicator variable for cohabitation as an additional family union status. We find that both cross-section and longitudinal estimates of the marital wage premium are a little larger than the corresponding estimates with cohabiting men in the reference group. If we consider cohabitation to be somewhat closer to marriage as a household structure than non-cohabiting singlehood, we might expect the marital wage premium to rise as we change the reference group from never married men to non-cohabiting never married men. These estimates suggest that inclusion of cohabitation as a control might diminish the dramatic reduction in men's marital wage premium reported in Gray (1997) who uses longitudinal estimation techniques on NLSY79 data for the period 1989–1993, and shows that there is no statistically significant marital wage premium.[9] This is also consistent with the results in Cohen (2002).

More interestingly, cross-section estimates show that cohabiting men earn 6.5% more than the non-cohabiting never married men. The size of the cohabitation wage premium is about a third of the marital wage premium identified in the same equation. These cross-section estimates are very similar to those in Cohen (1999). The fixed effects estimate of the cohabitation premium is smaller (3.6%) than the cross-section estimate, but it is still statistically significant. This essentially indicates that for white men about half of the cross-sectional cohabitation premium reflects selection into cohabitation. Our fixed effects estimate of the cohabitation premium contradicts Stratton's (2002) findings that show the entire cohabitation premium to reflect individual selection effects.

We recognize that fixed effects estimation is only able to account for selection through time-invariant unobservable characteristics. If selection into marriage or cohabitation depends on wage growth (i.e., men with high wage growth are more likely to be married or cohabiting), then changes in wages and family status will be interdependent, and even the fixed effects coefficients will be biased upwards. To identify whether such selection through wage-growth are important for our estimated marriage and cohabitation premiums, we examine the pre-marriage (pre-cohabitation) wage growth for men who married (cohabited) during the sample period versus other non-married (non-cohabiting) men. More specifically, we estimate two separate equations of the following form for ever-married and ever-cohabited men during their pre-union period:

$$\ln(W_{it+1}) - \ln(W_{it}) = \alpha + \beta \cdot (X_{it+1} - X_{it}) + \delta F_i + (\eta_{it+1} - \eta_{it}) \quad (3)$$

where W_{it} is the wage of individual i in year t, X_{it} is a vector of observable characteristics (age, tenure, local unemployment rate, a binary indicator for SMSA, and three regional indicators), η_{it} is the error term. F_i is 1 if the man married (cohabited) over the sample period, zero otherwise. The sample is restricted to pre-marriage (pre-cohabitation) period of men who were single at period t. The estimated coefficient $(\hat{\delta})$ is −0.007 (with a t-statistic of 0.56) for white men who married later on, and −0.014 (with a t-statistic of 0.89) for white men who cohabited later during the sample period.[10] These results suggest that white men who married (cohabited) during the sample period did not exhibit higher pre-marriage (pre-cohabitation) wage growth than otherwise similar white men. Therefore, the selection into marriage and cohabitation can safely be considered to be independent of wage growth.

In the next specification in Table 2, measures of duration in different family status are included. The coefficients on the various duration measures indicate the speed of wage growth in different types of family status as compared to non-cohabiting never-married men. Fixed effects and least squares estimates for this specification are produced, respectively, in columns 3 and 8 of Table 2. Comparison of the fixed effects estimates with and without the duration controls reveal, not too surprisingly, that there is no statistically significant marital *intercept* shift, and the effect of marriage is enhanced only gradually, through the steepening of the wage profile over the length of the marriage as wages grow 1.6% for each additional year of marriage. The marriage duration effect on wage declines over the years, as reflected in the statistically significant coefficient on marital duration squared (not reported in Table 2), but only at a very minimal rate. Cross-sectional estimates reveal a similar result, although the intercept shift is statistically significant and quite large (potentially reflecting a selection bias). While the sign of the estimated coefficients (not

[9] Gray (1997) uses specifications that included actual experience, union, child, occupation and industry dummy variables. As has already been indicated, even with a very comparable specification, our results do not change in any important way (see Table 6 in the Appendix).

[10] We get similar results even with a changed definition of the family status indicator in Eq. 3 where the dummy variable F_i is 1 if the man married (cohabited) in the next year, zero otherwise.

reported) on the two other quadratic duration terms measuring years cohabited with current wife prior to marriage and years in earlier marriages are in the expected direction, none of them are statistically significant.

The cross-section estimates of the coefficients on the cohabitation dummy as well as the linear and quadratic measures of duration in cohabitation have the expected signs, but are not statistically significant. The parallel fixed effects point estimates are somewhat smaller than the cross-section estimates, indicating a positive selection bias, but these again are not statistically significant. These results suggest that there is not a significant wage growth associated with duration of cohabitation, perhaps due to the relatively transient nature of such relationships.

It would be appropriate to note that even though we have some evidence of a cohabitation premium in men's earnings, we have recognized earlier that cohabitation is heterogeneous in nature. In an attempt to distinguish between different types of cohabiting relationships, we identify three categories of cohabitation in our data: cohabitation ending in marriage, cohabitation ending in separation, and continued cohabitation when the respondent was last interviewed. We replace the cohabitation dummy variable in our specifications with the indicators for these three types of cohabitation. The results, shown in column 1 of Table 3, indicate that the cohabitation premium accrues only to those who get married later on, but not to men in the other two categories of cohabitation. These results do not change significantly even when the specification includes years in different family status as shown in column 2 of Table 3. The impact of cohabitation that ends in marriage remains constant over time, since we find no statistically significant effect of the duration in cohabitation on white men's earnings. Although these results are quite intriguing, they need to be considered with caution. The three cohabitation indicators in this specification contain information about how a current spell of cohabitation ends in the future. These regressors may not be strictly exogenous in the sense that future values of the dependent variable (i.e., future wages) can influence the current value of these indicator variables. In other words, how a cohabitation spell ends in the future might be endogenous to the individual's future wages. Consequently, the dummy variables indicating the three types of cohabitation are potentially endogenous to wages, and therefore, the estimated coefficients may fail to be consistent.

Table 3 Heterogeneous nature of cohabitation: white men 1990–2000

	Dummy var. spec (1)	Duration spec (2)
Married	0.076 (4.09)**	0.048 (2.28)*
Divorced or separated[a]	0.03 (1.16)	0.041 (1.47)
Cohabit (ends in marriage)[b]	0.093 (3.02)**	0.088 (2.56)*
Cohabit (ends in separation)[b]	0.006 (0.18)	−0.005 (0.13)
Cohabit (continued)[b]	0.056 (1.17)	0.069 (1.40)
Years in current marriage		0.017 (4.03)**
Years div. or sep		0.01 (0.89)
Years cohabit		0.002 (0.32)
Years in previous marriage		0.004 (0.60)
Years cohabited w/wife		0.002 (0.27)
Observations	9886	9886
Number of id	1464	1464
R^2	0.25	0.25

Fixed Effects Estimates of NLSY Wage Regression

[a] Divorce and separated category includes a small number of widowers

[b] There were 479 spells of cohabitation that ended in marriage, 1,285 spells of cohabitation that ended in separation, and 644 spells of cohabitation that continued at the time of the last interview

[c] Also included in the longitudinal specifications are: age, age-squared, squared terms of the various relationship duration measures, dummy for SMSA, local unemployment rate, regional dummy (3), and year dummy variables (7). The full set of estimates is available upon request

[d] Absolute value of t-statistic in parentheses; † $p < 0.10$, * $p < .05$, ** $p < .01$ in a two-tailed test

Results on Intra-Household Spillover Effects of Wife's/Partner's Human Capital

The results presented so far indicate that both marriage and cohabitation have a positive effect on men's wage earnings. These findings generally support the hypothesis that family union raises men's labor market productivity. In order to identify a causal mechanism that connects a man's family status and his productivity, we proposed a joint human capital hypothesis. Hence, our next specification includes a set of dummy variables indicating the educational attainment of the married or cohabiting partner as a measure of her human capital stock. The relevant fixed effects and least squares results are presented, respectively, in columns 4 and 9 in Table 2. Note that the married and cohabitation dummies now measure the returns to men whose (marital or cohabiting) partners are not high school graduates.

Least squares estimates (in column 9) show that the higher the level of education of the wife, the better-off married men are. Fixed effects estimates are much smaller than least squares estimates, reflecting selection and positive sorting in the marriage market. However, even after addressing the selection prospects, men who are married to high school graduate wives have a statistically significant wage premium. A similar effect of partner's education is identified among cohabiting men. After controlling for selection and sorting into cohabitation, men with a partner who completed high school earn about 10% more than non-cohabiting never-married men. However, we should note that the spillover effects are not sustained for men with a wife/partner who has more than a high school degree. While the estimated effects of a partner having some college, and college degree or higher education are not significantly different from zero, the longitudinal point estimates of the spillover effects decline with more education of the wife/partner.

This declining wage effect of partner's education can be explained if we consider the estimated effects in column 4 to have captured the actual spillover effects of partner's education along with the effects of intra-household specialization on men's earnings. Given that more educated women are more likely to be involved in the labor market,[11] men with more educated partner's face a lower prospect of intra-household specialization, which may have a negative influence on their wage compared to men with not-employed partners. Notwithstanding the endogeneity of the partner's employment with respect to men's wages, this line of argument suggests a negative association between men's wages and the partner's employment, and that the magnitude of such negative association to be stronger for higher levels of partner's education. In order to test whether this argument is supported by the data, we try to separate the spillover effects for men with employed partner versus those with not-employed partner. We achieve this by interacting the partner's employment status indicator with the interaction of family status and partner's education. Data on partner's employment is collected from the "household record" files in NLSY79. The variable indicates whether the spouse or partner was employed in the past calendar year. The results reported in column 5 in Table 2 suggest that for each level of partner's education, men with an employed partner are likely to have lower wages than those with not-employed partner. Moreover, such negative association between men's wages and partner's employment get stronger for higher levels of partner-education. Once again, given that partner's employment or labor force participation is endogenous to men's wages, the evidence is only suggestive. But these results reveal a plausible reason for the declining spillover effects of partner's education on men's wages.

Thus, accounting for selection and assortative mating as well as duration in current family status, we have some evidence of intra-household spillover effects of a female partner's education. Basu et al. (2002) find similar spillover benefits of literacy in the household in Bangladesh, while Neuman and Ziderman (1992) identify cross-productivity effects of women's education in higher status occupations in Israel. Tiefenthaler (1997) also finds some evidence of spillover effects of spousal education in Brazil. We should note that the specifications in columns 4 and 5 of Table 2 also included a man's own education interacted with their family status. Clearly, controlling for the effect of man's own education on the likelihood of being married or cohabiting does not remove the household spillover effects of a female partner's education.

As has been noted earlier, the identifying assumption for consistently estimating the fixed-effects coefficients on partner's education is that any selective mating occurs over men's time-invariant characteristics. However, partner's education can potentially be associated with men's time-varying characteristics which are not included in the specification. More specifically, men with a higher educated partner may be more likely to increase work hours, or to receive on-the-job-training, or are less likely to have quick job-turnovers. Any of this set of changes can have a positive effect on men's wages. Therefore, if such association exists, the estimated positive spillover effects of partner's education may reflect the potential positive influence of these time-varying factors. In order to identify whether the estimated effects of partner's education represents the underlying association with hours worked, incidence of training, and tenure, we look into their relationship by estimating three separate fixed-effects equations for men's weekly hours worked, on-the-job-training incidence, and tenure with current employer. Estimates in Table 4 suggest that the nature of the association between partner's education and any of these three measures could not explain the positive spillover effect on men's wages. For cohabiting men, partner's education does not significantly influence their tenure in current job, or weekly hours worked, and the negative effect a college graduate partner has on a man's propensity to receive on the job training only indicates that the positive spillover effect of partner's education on men's wage is not a reflection of training. For married men, wife's education has no significant influence on tenure and propensity to receive training. Men who have a wife with more than high school education work fewer hours per week, so that the effect of a better-educated wife on their earnings may not originate from increased working hours. Thus, these auxiliary estimates help us to maintain that the spillover effect of partner's education on men's wages does not embody the relationship of partner's education with these time-varying characteristics.

Conclusion

We recognize that with the nature of family arrangements in the U.S. changing rapidly, men's choices in the family

[11] For instance, in our sample for the year 1990, 42% of less than high school educated partners were employed in the past calendar year. This percentage among partners with high school, some college, and college graduate or higher education is 65, 75, and 80%, respectively. The pattern holds for the other sample years as well.

Table 4 Tenure, weekly hours worked, and training: white men 1990–2000

	Tenure (1)	Weekly hours worked (2)	Training (3)
Wife less HS	−0.195 (0.40)	1.291 (0.67)	−0.022 (0.41)
Wife HS grad	0.319 (1.09)	−1.663 (1.46)	0.026 (0.84)
Wife somecoll	0.516 (1.64)	−3.165 (2.56)**	0.041 (1.21)
Wife collgrad	0.26 (0.81)	−3.609 (2.87)***	0.022 (0.62)
Partner less HS	0.536 (1.13)	−2.761 (1.49)	0.045 (0.88)
Partner HS grad	−0.049 (0.11)	−0.31 (0.18)	−0.064 (1.38)
Partner somecoll	−0.537 (1.00)	0.08 (0.04)	−0.064 (1.11)
Partner collgrad	−0.477 (0.79)	−0.306 (0.13)	−0.169 (2.61)***
Observations	9980	9885	9980
Number of id	1464	1463	1464
R^2	0.19	0.02	0.07

[a] Each of the estimated equation also included age, age-squared, dummy for SMSA, local unemployment rate, 3 regional dummy variables, and 7 year-dummy variables

[b] Absolute value of t statistics in parentheses; † $p < .10$, * $p < .05$, ** $p < .01$, in a two-tailed test

sphere and the labor market are ever increasingly intertwined. The challenge of untangling this simultaneity is not easy to overcome. Such challenges notwithstanding, the present study is expected to further our understanding of the implications of men's choices regarding cohabitation and marriage. The paper provides new evidence on the male wage premiums in relation to marriage and cohabitation, and has three principal findings to report.

First, hourly wage premium paid to married men are large in cross-sectional estimates. However, once we address the selection issue through fixed effects estimation, the marital premium persist. Also, the marital wage premium arises slowly and is apparently the outcome of wage growth over the length of the marriage.

Second, cross-section estimates indicate the presence of a cohabitation premium, albeit smaller than the marital premium, for men. Fixed effects estimates show that about one-half of the cross-sectional cohabitation premium is associated with the individual's fixed unobservable characteristics that are positively correlated with both cohabitation and wages. More precisely, even after accounting for individual specific unobservable characteristics, men earn a cohabitation wage premium of 3.6%. Comparison of estimates from specifications with and without measures of cohabitation-duration indicates that while cohabitation is associated with improved wage earnings, such improvements are not manifested in terms of wage growth over the length of cohabitation. We also have some weak evidence indicating that the cohabitation premium appears to accrue to men who eventually marry their partner.

Finally, the most intriguing finding in this paper is the positive contribution of wife or partner's education to men's hourly wage. Cross-sectional estimates show that men with a better-educated wife/partner earn significantly more than non-cohabiting never married men. Even fixed effects estimates show a positive effect of marital or cohabiting partner's education on men's wage rate.

Our findings are consistent with the hypothesis that family union enhances men's labor market productivity. More importantly, we argue that in a joint human capital framework, intra-household spillover effects of partner's education would provide a causal mechanism linking family union status and men's productivity. With rising female labor force participation in the recent decades, the joint human capital hypothesis appears to be more appealing than a more traditional specialization hypothesis to explain wage premiums for married or cohabiting men. Our results provide some empirical support for the joint human capital hypothesis. These results also underscore the importance of recognizing the value of the individual's family union choice—in marriage as well as in cohabitation. Ignoring cohabitation in the analysis of the effects of marriage on men's earnings might lead to ineffective policy measures, and restrict efficient use of public funds in this regard.

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Appendix

See Tables 5 and 6.

Table 5 NLSY wage regression: white men 1990–2000

	Longitudinal					Cross-sectional				
	Dummy var. spec (w/o cohabit)	Dummy var. spec	Duration spec	Partner's edu spec	Partner's edu spec X (partner employed)	Dummy var. spec (w/o cohabit)	Dummy var. spec	Duration spec	Partner's edu spec	Partner's edu spec X (partner employed)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Married	0.052 (2.69)**	0.061 (3.08)**	0.033 (1.50)	−0.021 (0.35)	0.042 (0.77)	0.213 (8.55)**	0.222 (8.57)**	0.124 (3.77)**	−0.046 (0.57)	0.150 (2.00)*
Divorced/separated[c]	−0.007 (0.27)	−0.002 (0.09)	0.012 (0.42)	0.062 (1.01)	0.068 (1.11)	0.049 (1.55)	0.049 (1.54)	0.068 (1.45)	0.06 (0.70)	0.138 (1.55)
Cohabit		0.036 (1.90)†	0.014 (0.54)	−0.02 (0.32)	0.072 (1.36)		0.065 (2.23)*	0.05 (1.29)	0.049 (0.59)	0.058 (0.74)
Years in current marriage			0.016 (3.76)**	0.016 (3.81)**	0.015 (3.48)**			0.02 (3.37)**	0.019 (3.21)**	0.001 (0.11)
Years in current marriage^2			−0.001 (3.50)**	−0.001 (3.58)**	−0.001 (3.28)**			−0.001 (2.06)*	−0.001 (1.81)†	−0.001 (1.77)†
Years div/sep			0.003 (0.36)	0.002 (0.33)	0.003 (0.36)			0.008 (0.68)	0.01 (0.82)	0.005 (0.39)
Years div/sep^2			0.000 (0.35)	0.000 (0.28)	0.000 (0.30)			−0.001 (1.03)	−0.001 (1.20)	−0.001 (1.12)
Years cohabit			0.017 (1.44)	0.017 (1.40)	0.013 (1.08)			0.007 (0.40)	0.005 (0.29)	0.018 (0.87)
Years cohabit^2			−0.001 (1.46)	−0.002 (1.58)	−0.001 (1.30)			0.000 (0.11)	0.000 (0.03)	0.000 (0.10)
Years in previous marriage			0.004 (0.63)	0.007 (1.01)	0.005 (0.69)			−0.01 (1.08)	−0.009 (1.05)	−0.011 (1.17)
Years in previous marriage^2			0.000 (0.48)	0.000 (0.93)	0.000 (0.64)			0.001 (1.51)	0.001 (1.52)	0.001 (1.57)
Years cohab w/wife			0.001 (0.08)	0.002 (0.24)	0.002 (0.22)			0.009 (0.87)	0.01 (0.97)	0.009 (0.88)
Years cohab w/wife^2			0.000 (0.38)	0.000 (0.27)	0.000 (0.28)			−0.001 (0.63)	−0.001 (0.60)	−0.001 (0.65)
Married * wife HS grad				0.07 (1.97)*	−0.032 (2.01)*				0.147 (3.11)**	−0.071 (2.70)**
Married * wife somecoll				0.023 (0.59)	−0.047 (2.40)*				0.145 (2.79)**	−0.072 (2.32)*
Married * wife collgrad				0.000 (0.000)	−0.052 (2.46)*				0.184 (3.43)**	−0.045 (1.34)
Married * wife edu missing				0.045 (1.05)	−0.037 (0.19)				0.013 (0.22)	−0.117 (0.76)
Coh * partner HSgrad				0.117 (2.23)*	−0.007 (0.17)				0.103 (1.42)	0.069 (1.17)
Coh * partner somecoll				0.03 (0.46)	−0.105 (1.83)†				0.11 (1.22)	0.092 (1.13)
Coh * partner collgrad				0.013 (0.17)	−0.109 (1.64)				0.206 (1.96)*	0.178 (1.76)†
Coh * partner edu missing				0.193 (1.62)	0.065 (0.33)				0.281 (1.86)†	0.409 (1.60)
Married * HS grad				−0.036 (0.68)	−0.025 (0.48)				0.032 (0.42)	0.087 (1.12)
Married * somecoll				0.074 (1.10)	0.085 (1.28)				0.093 (1.00)	0.156 (1.67)†
Married * collgrad				0.06 (0.95)	0.056 (0.90)				−0.034 (0.40)	0.036 (0.43)
Coh * HSgrad				−0.082 (1.60)	−0.063 (1.23)				−0.102 (1.39)	−0.116 (1.55)
Coh * somecoll				0.052 (0.71)	0.081 (1.10)				−0.057 (0.48)	−0.075 (0.62)
Coh * collgrad				−0.004 (0.06)	0.031 (0.40)				−0.362 (3.12)**	−0.367 (3.10)**
Div/sep * HS grad				−0.081 (1.38)	−0.087 (1.49)				−0.01 (0.12)	−0.008 (0.10)

Table 5 continued

	Longitudinal					Cross-sectional				
	Dummy var. spec (w/o cohabit)	Dummy var. spec	Duration spec	Partner's edu spec	Partner's edu spec X (partner employed)	Dummy var. spec (w/o cohabit)	Dummy var. spec	Duration spec	Partner's edu spec	Partner's edu spec X (partner employed)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Div/sep * somecoll				−0.051 (0.65)	−0.049 (0.63)				0.066 (0.61)	0.067 (0.61)
Div/sep * collgrad				−0.083 (1.10)	−0.085 (1.14)				−0.003 (0.03)	−0.007 (0.06)
Age	0.075 (3.76)**	0.074 (3.73)**	0.06 (2.92)**	0.06 (2.90)**	0.059 (2.86)**	0.057 (2.30)*	0.057 (2.30)*	0.051 (1.98)*	0.051 (2.01)*	0.049 (1.89)†
Age^2	−0.001 (4.57)**	−0.001 (4.53)**	−0.001 (3.29)**	−0.001 (3.23)**	−0.001 (3.18)**	−0.001 (1.73)†	−0.001 (1.72)†	−0.001 (1.55)	−0.001 (1.61)	−0.001 (1.46)
Own educ: HS grad						0.173 (5.53)**	0.175 (5.64)**	0.181 (5.75)**	0.156 (2.29)*	0.154 (2.26)*
Own educ: some college						0.339 (8.97)**	0.343 (9.12)**	0.35 (9.17)**	0.267 (3.21)**	0.265 (3.19)**
Own educ: college grad						0.604 (16.84)**	0.609 (16.96)**	0.62 (16.76)**	0.623 (8.59)**	0.622 (8.57)**
SMSA	0.01 (0.68)	0.01 (0.68)	0.01 (0.68)	0.009 (0.64)	0.01 (0.69)	0.186 (8.57)**	0.186 (8.57)**	0.189 (8.68)**	0.189 (8.75)**	0.187 (8.65)**
Local unemployment rate	−0.01 (1.55)	−0.01 (1.54)	−0.01 (1.57)	−0.01 (1.46)	−0.01 (1.52)	−0.002 (0.23)	−0.002 (0.22)	−0.004 (0.35)	−0.003 (0.30)	−0.005 (0.44)
Region: north central	−0.08 (1.64)	−0.081 (1.66)†	−0.089 (1.82)†	−0.084 (1.71)†	−0.086 (1.76)†	−0.144 (5.32)**	−0.144 (5.35)**	−0.147 (5.43)**	−0.143 (5.32)**	−0.138 (5.19)**
Region: south	−0.133 (3.08)**	−0.133 (3.09)**	−0.138 (3.20)**	−0.137 (3.17)**	−0.138 (3.21)**	−0.168 (5.79)**	−0.167 (5.78)**	−0.168 (5.77)**	−0.159 (5.50)**	−0.158 (5.48)**
Region: west	−0.016 (0.34)	−0.017 (0.36)	−0.021 (0.44)	−0.018 (0.38)	−0.021 (0.44)	−0.061 (1.79)†	−0.063 (1.83)†	−0.06 (1.75)†	−0.057 (1.65)†	−0.058 (1.67)†
Dummy for year 2	0.017 (0.94)	0.017 (0.93)	0.018 (0.97)	0.018 (0.96)	0.019 (1.01)	0.013 (0.97)	0.013 (0.95)	0.02 (1.32)	0.02 (1.36)	0.021 (1.44)
Dummy for year 3	0.062 (2.12)*	0.062 (2.12)*	0.06 (2.01)*	0.059 (1.98)*	0.06 (2.03)*	0.046 (2.69)**	0.046 (2.68)**	0.054 (2.89)**	0.055 (2.96)**	0.056 (3.02)**
Dummy for year 4	0.082 (2.01)*	0.082 (2.01)*	0.077 (1.85)†	0.075 (1.81)†	0.076 (1.84)†	0.073 (3.80)**	0.073 (3.76)**	0.079 (3.88)**	0.08 (3.92)**	0.081 (3.97)**
Dummy for year 5	0.151 (2.77)**	0.152 (2.77)**	0.142 (2.57)*	0.141 (2.56)*	0.142 (2.58)**	0.13 (5.62)**	0.13 (5.60)**	0.136 (5.64)**	0.137 (5.71)**	0.139 (5.79)**
Dummy for year 6	0.245 (3.11)**	0.245 (3.11)**	0.228 (2.87)**	0.227 (2.86)**	0.229 (2.88)**	0.194 (6.42)**	0.193 (6.39)**	0.198 (6.41)**	0.2 (6.50)**	0.205 (6.63)**
Dummy for year 7	0.326 (3.12)**	0.327 (3.13)**	0.303 (2.88)**	0.302 (2.88)**	0.304 (2.89)**	0.253 (6.66)**	0.252 (6.66)**	0.261 (6.76)**	0.262 (6.86)**	0.266 (6.94)**
Dummy for year 8	0.43 (3.24)**	0.432 (3.25)**	0.401 (3.00)**	0.4 (2.99)**	0.4 (2.99)**	0.285 (5.95)**	0.284 (5.93)**	0.294 (6.08)**	0.297 (6.18)**	0.301 (6.27)**
Constant	1.103 (2.41)*	1.109 (2.42)*	1.304 (2.81)**	1.302 (2.80)**	1.318 (2.83)**	0.8 (1.93)†	0.788 (1.90)†	0.938 (2.21)*	0.964 (2.26)*	1.001 (2.33)*
Observations	9924	9924	9924	9924	9924	9924	9924	9924	9924	9924
Number of id	1464	1464	1464	1464	1464					
R^2	0.25	0.25	0.25	0.25	0.25	0.29	0.29	0.29	0.30	0.30

Full set of covariates for results reported in Table 2. Dependent Variable: Ln (Hourly Wage)

[a] The pooled cross-section regressions include 3 schooling dummy variables along with the controls listed above for the longitudinal specifications. Being time-invariant, the schooling dummy variables drop out of the fixed effects model. The standard errors for the cross-sectional estimates are robust

[b] Divorce and separated category includes a small number of widowers

[c] Absolute value of t-statistic in parentheses; † $p < 0.10$, * $p < .05$, ** $p < .01$, in a two-tailed test

* significant at 10%; ** significant at 5%; *** significant at 1% level, in a two-tailed test

Table 6 Including tenure, occupation, industry, collective bargaining coverage and number of children in the specification

	Longitudinal			Cross-sectional		
	Dummy var. spec (w/o cohabit) (1)[a]	Dummy var. spec (2)	Duration spec (3)	Dummy var. spec (w/o cohabit) (6)	Dummy var. spec (7)	Duration spec (8)
Married	0.044 (2.28)*	0.052 (2.62)**	0.037 (1.70)†	0.128 (5.42)**	0.129 (5.16)**	0.16 (5.20)**
Divorced/separated[c]	0.00 (0.01)	0.003 (0.13)	0.013 (0.44)	−0.005 (0.20)	−0.005 (0.20)	0.105 (2.47)*
Cohabit		0.03 (1.65)†	0.012 (0.46)		0.004 (0.16)	0.002 (0.07)
Years in current marriage			0.014 (3.02)**			−0.001 (0.13)
Years div/sep			0.003 (0.38)			0.00 (0.04)
Years cohabit			0.017 (1.45)			0.005 (0.35)
Years in previous marriage			0.006 (0.82)			−0.024 (2.96)**
Years cohab w/wife			0.001 (0.08)			−0.004 (0.37)

NLSY Wage Regression: White Men 1990–2000. Dependent Variable: Ln (Hourly Wage)

[a] To facilitate easy reference, the column numbers correspond to those in Table 2. The longitudinal specification also includes: age, age-squared, squared terms of the various relationship duration measures, dummy for SMSA, local unemployment rate, regional dummy variables (3), year dummy variables (7), tenure, tenure squared, dummy for union coverage, industry dummy variables (12), occupation dummy variables (11), and number of children in the household. The pooled cross-section regressions include 3 schooling dummy variables along with the controls listed above for the longitudinal specifications. Being time-invariant, the schooling dummy variables drop out of the fixed effects model. The standard errors for the cross-sectional estimates are robust. The full set of estimates is available upon request

[b] Divorce and separated category includes a small number of widowers

[c] Absolute value of *t*-statistic in parentheses; † $p < 0.10$, * $p < .05$, ** $p < .01$, in a two-tailed test

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