Testing Explanations of the Male Marital Wage Differential:

Ability and Training

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ABSTRACT

Wage analyses indicate that white men in the U.S. who are married earn between 8 and 15 percent more than white men who are not married, even after controlling for education, experience, and occupation. The precise nature of this wage differential has not been fully explained, although considerable effort has been expended to determine how much of the differential is attributable to a selection effect and how wages change following marriage.

We expand upon this literature in several dimensions using data from the National Longitudinal Survey of Youth. First, we extend the analysis to nonwhite men, finding that the marital wage effect is indeed not race specific. Second, we introduce a control for cognitive skills to determine if the selection effect is brought about because more able men are more likely to marry. While we find that our measure of cognitive skills does increase wages and that married men have higher skills than unmarried men, our measure has no significant impact on the marital wage differential. Finally we test the hypothesis that the marital wage differential reflects a differential in formal training between married and unmarried men. There exists some evidence, both theoretical and empirical, that married men are more likely to receive formal training than unmarried men. Our formal training measures are significant determinants of wages, but again have no impact on the estimated marital wage differential.

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Wage analyses indicate that white men who are married earn between 8 and 15 percent more than white men who are not married, even after controlling for education, experience, and occupation. Some of this differential has been attributed to a selection effect, to the fact that men who marry earn more throughout their career than men who do not marry. Some has been attributed to a productivity differential, whereby marriage somehow makes men more productive. This productivity effect has been modeled both as a faster rate of wage growth and as a discrete jump in wages that occurs around the time of marriage.

In this paper, we use data from the National Longitudinal Survey of Youth to further examine the male marital wage differential. First, we extend the analysis to nonwhite men to determine if the marital wage effect is race specific. Second, we introduce a control for cognitive skills that should help determine if the selection effect is brought about because more able men are more likely to marry. Finally we test the hypothesis that the marital wage differential reflects a formal training differential between married and unmarried men. There exists some evidence, both theoretical and empirical, that married men are more likely to receive formal training than unmarried men. By controlling directly for cognitive skills and formal job training we can further isolate the sources of the marital wage differential.

Literature Review

Evidence of the marital wage differential for white men is extensive and beyond the scope of this article to document. The literature seeking to explain the differential is primarily focused on distinguishing between that portion that is attributable to selection effects and that

portion that is attributable to changes occurring around the time marital status changes. There is a concern that men who marry may be more productive and hence receive higher wages at all points in their career, than men who do not marry. Those higher earnings attract marriage partners so that marriage and wages are endogenously determined. This is the selection effect. Alternatively it is possible that wages or the wage function literally changes at the time of marriage. Such wage changes could be due to changes in productivity or to discrimination in favor of married men.

There exists evidence to support both the selection and the wage change hypotheses. Most attempts to control for selection use panel data to estimate individual-specific fixed effects wage equations.[1] The estimated impact of marriage on wages universally declines with the introduction of such individual-specific fixed effects. Korenman and Neumark (1991) report finding a marital differential of 12% when controlling for education, age, industry, occupation, and a quadratic in experience, but not individual-specific effects. This differential declines to 9% but remains statistically significant when individual-specific fixed effects are included. Bartlett and Callahan (1984), Cornwell and Rupert (1997), Gray (1997), Daniel (1991), Hersch and Stratton (2000), and Stratton (2002) also report smaller marital wage differentials in fixed effects specifications.

Most researchers, however, conclude that selection does not explain the entire differential.[2] The remaining marital effect may be due to either a discrete jump in wages that occurs around the time of marriage or to higher wage growth following marriage. In order to

[1] In an alternative approach to modeling the selection effect, Nakosteen and Zimmer (1987) simultaneously model the decision to marry and wages. In doing so, they obtain large but imprecisely estimated marital wage effects that are difficult to interpret.

[2] Chun and Lee (2001) reach a similar conclusion using a switching regression with endogenous marital selection rather than fixed effects estimation.

distinguish between these hypotheses, researchers have incorporated controls for both marital status and years married in their analysis of wages. Korenman and Neumark (1991) find that after incorporating a quadratic in years married in a fixed effects specification, the coefficient to marriage alone, while still positive, becomes statistically insignificant. Kenny (1983), Daniel (1991), and Stratton (2002) obtain similar results showing particularly rapid wage changes in the first years after marriage. Daniel finds some evidence that wages begin rising somewhat before the date of marriage. Others (Gray 1997 using the National Longitudinal Survey of Men; Hersch and Stratton 2000 using the National Survey of Families and Households) find evidence both of faster growth and of a discrete wage change following marriage. Both effects are even more pronounced in cross-section analyses (Loh 1996; Blackburn and Korenman 1994) where the discrete effect may be partially attributable to selection. In general, the literature suggests that wages rise faster following marriage, but one can not rule out the possibility that wages also jump upon marriage.[3]

The theory most frequently invoked to explain how marriage might enhance men's productivity is Becker's theory of the family (1991). Becker notes that when two single-person households are joined to form a single two-person household there are economies to be gained from specialization. Women have traditionally specialized in home-based activities, and men in market based activities. This specialization allows married men more time and energy to devote to job related activities. Hence men's productivity and wages will rise following marriage. The rapid increase in married women's labor force participation over the last thirty years has provided an opportunity to test this theory. If dual earner households are less specialized, the gains from specialization should on average have decreased.

[3] An exception is Cornwell and Rupert (1997), who contest the evidence of faster wage growth

Indeed, there is some evidence that the marital wage differential for white men has been declining. Using Current Population Survey data, Blackburn and Korenman (1994) report that the estimated marital wage differential fell from 28.7% in 1968 to 18.5% in 1989. Other researchers have contested the magnitude of the decline (Wetzell 2002 and Cohen 2002), but not the decline itself. Gray (1997) also finds evidence of changes over time as he uses samples of white men aged 24-31 from different cohorts to examine the marital wage differential. Evidence from the National Longitudinal Survey (NLS) of Men (1976-80 data), reveals a marital wage differential that persists after using fixed effects to control for selection. Using a later cohort of data (1989-93) from the NLS of Youth, Gray finds both a smaller differential and one that appears to be entirely attributable to selection.

Attempts to explain the differential at the micro level have been of two types. First, there are numerous articles comparing the wage differential for dual versus single earner households. Daniel (1991), Gray (1997), and Chun and Lee (2001) find evidence that married men whose wives work fewer hours receive higher wages than married men whose wives work longer hours. These results are robust to fixed effects and/or instrumental variables estimation to control for the possible endogeneity of the wife's employment decision with the husband's wage. Loh (1996), however, finds that men married to working wives earn more than men married to women who do not work. In general, wives working status may be related to wages, but the evidence is inconclusive.

The second line of work uses information on reported housework time to more directly control for the relation between housework time and wages. A key premise of the literature looking at the employment decision of wives is that when a man's wife works for pay there is

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following marriage.

less specialization within the household and so fewer gains to be made. Married men with employed wives are presumed to spend more time and energy on household tasks than married men whose wives are not employed. Thus, those with employed wives have less time and energy to spend on the job. While there does exist evidence that more time spent on housework reduces wages (Hersch and Stratton 1997, 2000), the impact of spousal employment on husband's reported housework time is minimal. Neither Hersch and Stratton (2000) nor South and Spitze (1994) find that reported housework time for married men varies significantly with the employment status of their spouse. In a fixed effects regression analysis, Hersch and Stratton (2000) find that housework time has a marginally significant negative effect on men's wages but that its inclusion on the wage equation has no impact upon the magnitude or significance of the marital wage effect. Further, reported housework time varies little with marital status for men. Thus, the traditional explanation for how marriage might influence the wage function has not fared well under scrutiny.

Methodology

This paper expands upon the current literature in several ways. First, the analysis is extended to nonwhites (African American and Hispanic men) as well as whites. In the past, the analysis has typically been restricted to white men on the grounds that "patterns of family formation and marriage differ greatly between black and white Americans" (Korenman and Neumark 1991). Blackburn and Korenman (1994) and Cohen (2002) are the only studies to report the marital wage differential for black men, finding it to be slightly smaller than that for whites. Comprehensive work including controls for individual-specific fixed effects is

uncommon in nonwhite samples. We perform comparable, detailed analysis for both white and nonwhite men.

Second, we explore the selection hypothesis. If men who marry earn more throughout their careers than men who do not because they are more able, then incorporating a measure of ability should significantly reduce the estimated impact of marriage upon wages within a pooled cross-section. We incorporate an often-used measure of cognitive skills to test this hypothesis.

Third, we explore the wage growth hypothesis by proposing an alternative explanation for the faster wage growth observed following marriage. Specifically, we examine the possibility that men receive more productivity-enhancing job training following marriage and that it is this training not marriage per-se that results in higher wage growth. In a wage equation that includes marital dummies, these dummies may serve as a proxy for unobserved job training activities. Correctly controlling for training should reduce or eliminate the estimated marital wage differential if it is job training not marital status that increases productivity.

This link between training and marital status deserves further discussion. Why would married men receive more job training? There are several possible explanations. First, marriage does often entail significantly greater financial responsibilities and may make men more receptive to job training opportunities. There may be no difference in the availability of job training by marital status, only in the take-up rate. Thus, married men may receive both more general and more firm-specific training. Alternatively, marriage to a working spouse may actually provide men the financial support to pursue additional general training. If capital markets are not perfect and individuals are not always able to borrow against future earnings in

order to finance current training costs, then the presence of a working spouse can provide an alternative source of support. In this case, however, one would expect greater gains from a working spouse. Finally, firms may offer firm-specific training to married men more often than to not married men. Optimally, both the firm and the recipient share the cost of firm-specific training. This up front training cost is recouped after the training is completed. The worker then receives a higher wage than he/she could expect to receive at any other firm, while the firm receives a marginal value product of labor that exceeds the wage. Both parties benefit so long as the post-training employment spell endures long enough for each to recoup their portion of the training costs. The greater is expected turnover, the less investment there will be in firm specific training.

The standard human capital model does not, however, dictate the precise division of the firm specific training costs. Firms that are viewed as particularly risky from the perspective of employees will have to bear a greater share of firm specific training costs in order to induce workers to accept any share of such costs. Theoretically firms can also ask workers whom they perceive as having a higher probability of quitting to pay a higher share of their training costs. In practice, however, it would be difficult for a firm to pay different workers different amounts for the same type of work. Such a pay scheme could run afoul of legislative provisions demanding equal pay.

Instead of offering different pay for the same opportunities, firms may offer different opportunities to different individuals. Assuming that the firm bears some of the training cost, the firm will have an incentive to provide more training to those individuals who are less likely to quit. Similarly, assuming that the individual bears some of the training cost, the individual will have an incentive to seek more firm specific training if he/she believes his/her probability

of quitting is low. Overall, more training will be offered and accepted as the extraneous quit probability falls.

The critical issue here is whether firms perceive a difference between married and not married men. While perceptions are difficult to measure, there is evidence that married men have higher tenure, lower turnover, and lower quit-to-fire ratios than not married men. These data are summarized in Table 1.

Data on tenure by marital status are obtained from the 2000 Tenure supplement to the Current Population Survey (CPS). These data indicate that white, non-Hispanic men between the ages of 30 and 39 who are married have an average tenure of 6.4 years while similar men who have never married (are divorced or separated) have an average tenure of 5.0 (5.4) years. These mean differences are statistically significant. These differences are not due to outliers as median tenure is also higher for married men than for never married or divorced/separated men. Similar differences are evident for blacks and Hispanics, though only the difference between married and never married men is statistically significant for nonwhites.

Data on turnover and quit-to-fire ratios were obtained from the 1989, 1991, and 1993 CPSs, a time period spanning that of the data used in this analysis. These data indicate that only 3.0% of married, white, non-Hispanic men age 23-37 changed jobs while the comparable turnover rate was significantly higher for never married men (6.1%) and for separated/divorced men (6.9%). Employers will not want to invest in training individuals who are likely to leave their employ in short order. Employers have some control over hiring and firing, but are less able to control voluntary quits. Thus, we also report the quit-to-fire ratio by marital status. For every 100 married men who are fired or layed off, approximately 15 quit. This ratio is similar for separated and divorced men (14), but significantly higher for never married men (24).

Furthermore, Shaw (1987) presents evidence that the quit rate for married men is higher the higher the earnings share of the wife. Thus, both the marital wage differential and the evidence that the marital wage differential is lower for men whose wives work longer hours may be attributable to differences in job training that arise from differences in quit rates. If employers perceive married men as more stable employees, they may provide married men with more firm specific training opportunities than not married men. If married men are more stable employees, married men may be more likely to seek out and accept firm specific training opportunities. This increased training will cause the wages of married men to rise more rapidly than the wages of not married men, just as the empirical evidence indicates.

The paper proceeds with a description of the data, the empirical results, and a brief conclusion. The analysis is conducted on both white and nonwhite men. If the marital wage differential is attributable to ability or to training, then including more direct measures of these factors should reduce the magnitude of the estimated marital wage differential.

Data

The data used in this analysis are the NLS of Youth (NLSY). The NLSY is a longitudinal data set of 10,000 civilian young adults who have been interviewed annually since 1979.[4] At the survey's beginning, the youth were 14 to 22 years of age. The data used for this analysis are from 1988 through 1994. Thus, respondents are between the ages of 23 and 37. The final sample is constructed subject to the following restrictions. Respondents are required

[4] We exclude the NLSY's original military sample. Doing this reduces the sample by 1,280. Along with a stratified random sample, the National Longitudinal Survey of Youth over samples low-income whites, blacks and Hispanics. In 1990, the low-income whites were dropped from the sample. Respondents used in this study are from the stratified random sample or the low-income black and Hispanic samples.

to have completed their formal schooling as of the 1988 interview date, and to have provided full-information on wages and all other variables.[5] Respondents whose constructed hourly earnings are less than $1.00 or greater than $100 in 1982-84 dollars (using the CPI-U to convert to real dollars) are treated as outliers and are excluded from the sample. After applying these restrictions, our sample contains observations on 2497 white, non-Hispanic men (henceforth referred to as white men) and 1138 African American and Hispanic men (henceforth referred to as nonwhite men). Not all respondents provide complete information every year from 1988 to 1994. Some experience unemployment; others leave the labor force. Pooling the observations across time generates unbalanced panels of 13,091 white male-year observations and 6,016 nonwhite male-year observations.

The data contain information on race, marital status, number of children, education, occupation, industry, union status, and area of residence. Years married is inferred based on observed annual marital status. Data on the unemployment rate in the county of residence are gathered on the expectation that local labor market conditions will influence earnings. Firm size is reported in recognition of the positive link established between wages and firm size. In addition, each year respondents are asked how many weeks they worked during the previous calendar year. These responses are used to construct a measure of actual work experience. Respondents are also asked how many weeks they have been at their current job, thus enabling the construction of a measure of job tenure. The two variables upon which we focus that are unique to this study of marital wage differentials are the cognitive skills and formal training measures.

[5] A small number of men who were widowed are also excluded from the sample on the grounds that such a change in marital status is, at this age, typically unforeseen.

The cognitive skills measure we employ is a function of the respondent's composite score on the Armed Force Qualification Test (AFQT). Several researchers have used the AFQT score to represent skills.[6] This score is assumed to be a direct measure of cognitive skills obtained via family and school environments. Researchers have found this measure to be a significant determinant of wages, however, it is also influenced by age and education level. Since the AFQT score was administered to every respondent in 1980 when they were between the ages of 15 and 23, some of the respondents were older and had more education than others. In order to control for possible tainting of the AFQT score by pre-testing education level, we included not the AFQT score itself but the residual from a regression of the AFQT score against the age of the respondent at the time the test was administered. In addition, we conduct analysis using a sample of individuals who obtained no more than a high school degree in order to see how sensitive the results might be to the timing of the AFQT test. Most individuals with this level of education had completed school prior to taking the test.

Detailed firm-provided job training information has been available from the NLSY since 1988. At each subsequent interview, respondents have been asked to describe whether they received any type of training since the last interview.[7] If the respondents answered "yes", then they were asked to describe whether the program was an on-the-job program such as company or apprenticeship training, or an off-the-job program at a business school, vocational center, correspondence school, or seminar outside of work. Apprenticeship programs are formal programs in which an individual agrees to work in return for wages and training in a skilled trade or art for a predetermined amount of time. Business programs refer to classes that

[6] See, for example, Rodgers and Spriggs (1996, 2002), Neal and Johnson (1996), and Ferguson (1995).

are not taken for college credit at either the undergraduate or graduate level. Vocational or technical training programs refer to beauty, auto mechanic, and welder school programs. Correspondence training refers to programs offered through the mail; vocational rehabilitation programs offer specialized training to prepare disabled persons to enter or re-enter the work force. Seminar training covers a wide variety of programs. These range from the provision of new information on the latest developments in one's field (e.g., changes in accounting law, advances in computer technology, or new medical techniques), to the development of general skills such as management, leadership or public speaking. Attendance at professional meetings, seminars on personal finance and lifestyle improvements are also examples of this type of training. We test several alternative measures of training, reporting the result of a specification that controls for both the incidence of training and the log duration of training.

Sample statistics by race and marital status for the primary variables of interest are presented in Table 2. See Appendix A for information on the remaining variables: dummy variables to identify the interview year, the region and city size of residence, the industry and occupation of employment, union status, and public sector employment; continuous variables reflecting the number of children in the household, firm size, and the local county unemployment rate. These data are from the pooled cross-section sample. They confirm that wages are higher for married men than for men who have never married. Amongst white men the difference is approximately 17%. Amongst nonwhite men it is approximately 15%.[8] Men who are divorced or separated have wages roughly similar to those who never married. This wage differential is not readily attributable to differences in educational attainment, as married

[7] The interval in the 1988 interview covers 1986 to 1988. The intervals for the 1989-1994 interviews cover the preceding year.

and never married men report having approximately the same amount of education. However, married men clearly have more job experience or more tenure on the job than either never married or separated/divorced men. These differences will explain some fraction of the raw marital wage differential.

The variables of particular interest here, however, are the residual AFQT score that serves as a measure of ability and the measure of job training. Higher AFQT scores denote higher ability. What matters for this analysis is the value for married as opposed to not married men. These data suggest that married men are more able than their never married counterparts. The difference between married and separated/divorced men is even more substantial. Thus, some of the marital wage differential might be directly attributable to individual specific ability. The measure of job training reported in Table 2 is the log of time spent in job training since 1988. As hypothesized, married men received more training than men who were not married – about 30% more training. If this training raises wages, then some of the observed marital wage differential may be attributable to differences in job training.

Regression analysis allows us to control for all these factors simultaneously. If differences in job experience, job tenure, ability, or job training explain the marital wage differential, then the effect of marital status in a log wage regression that includes controls for all these factors will not be statistically significant.

Wage Analysis

The wage analysis begins with pooled cross-section estimation of the simple marital wage differential with controls for years married. We then add progressively more and more

[8] Blackburn and Korenman (1994) also report finding a smaller marital wage differential for

explanatory variables and examine the impact of these controls on the estimated magnitude of the marital wage differential. We close by estimating a fixed effects specification in order to control for all individual specific and time invariant unobservables. We perform this analysis first for the white sample and then for the nonwhite sample.

In particular, the initial specification estimates the gross or unadjusted marital wage differential:

(1) $\text{Ln Wage}_{it} = \beta_0 + \text{Mar}_{it}\,\beta_1 + \varepsilon_{it}$

where Mar is a vector of marital status variables for individual i at time t. This vector has two components: dummy variables identifying those who are married and those who are not currently married but have been separated or divorced, and continuous variables that capture a quadratic in years married. Never married men constitute the base case. The next specification, labeled the Basic specification, adds a vector X of individual, job, and time specific characteristics such as are typically incorporated in wage analyses. The third specification, labeled AFQT+, adds the firm size, county unemployment rate, and, most importantly, the residual AFQT score variables to the specification. The Training specification incorporates a set of year specific indicator variables to identify if the respondent is currently in training and a measure of log time spent in training since 1988. We tested alternative specifications with linear and quadratic training measures, but the log specification provided the best fit. Finally, we estimate a Fixed Effects specification that incorporates an individual specific, time invariant dummy variable to capture all individual specific, time invariant factors – including the impact of the AFQT score. To the extent that the important time invariant factor is ability and that the residual AFQT score captures ability, the coefficient to marital

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nonwhites.

status should not be significantly different between the Training and Fixed Effects specifications.

Selected coefficient results for the white sample are reported in Table 3.[9] These indicate that married men earn about 9% more than never married men and that wages rise with years married by approximately 2.8% annually. The coefficient to the quadratic in years married is negative but statistically insignificant, perhaps because this sample is still relatively young and the rate of wage growth with years married is imprecisely measured. Separated /divorced men earn about 4% less than never married men, while retaining the benefit of faster wage growth that occurred during their marriage. As do others in the literature, we assume that wage growth reverts back to pre-marriage levels when a couple separates.

Including the basic controls for education, quadratics in experience and tenure, a measure of the number of children, 1-digit industry and occupation, region of residence, city size, interview year, union status, and government employment reduces the observed marital wage differential by 40% to 5.5% and the observed higher growth rate by 30% to 2.0% annually. These results are comparable to those reported elsewhere in the literature. Introducing these controls also increases the magnitude of the wage differential between separated/divorced and never married men from –4% to +3%, a differential comparable to that reported in the literature.

Results from the AFQT+ specification confirm that cognitive skills, as measured by higher residual AFQT scores, have a statistically significant impact on wages with more able individuals earning higher wages than less able individuals. This variable is significant at the 1% level. Yet controlling for this variable has only a small impact on the estimated marital

[9] Full table results are available from the authors upon request.

wage differential. The coefficient to Married declines from 0.055 to 0.050, marking a less than 10% decrease in the estimated marital wage effect. The coefficient to Years Married also declines on the order of 10% from 0.020 to 0.018. Further, marital status remains a significant and substantial determinant of wages. Even though married men have higher AFQT scores, cognitive skills as measured by the residual AFQT score do not explain the marital wage differential.

One explanation for this finding may be that the residual AFQT score is a poor measure of cognitive skills because individuals took this test at different ages and at different points in their education. As discussed above, in order to explore this possibility we reestimated the model restricting the sample to only those individuals who completed no more than a high school education. While the estimated effect of marital status per se was somewhat larger (and the estimated effect of years married somewhat smaller) for this sample, the impact of the residual AFQT score on wages was virtually identical (0.0032 with a standard error of 0.0002) and inclusion of the AFQT score reduced the marital wage effect by less than 10.0%. None of the results reported in Table 3 are particularly sensitive to this sample selection criterion.

Adding indicators of job training and a logarithmic measure of the time spent in job training programs to the specification has a similarly small impact and one that primarily effects the return to years married. While the impact of job training on wages is statistically significant at the 1.0% level and married men receive more training than not married men, the coefficient to Married increases marginally with controls for training while the rate of wage growth declines almost 8.0% from 1.8% to 1.7% annually. That the training measure has a greater impact on wage growth than on wage level is not surprising as training is expected to influence wages primarily through its impact on wage growth. We estimated specifications

with linear and quadratic measures of job training with similar results. Training is an important determinant of earnings, but controls for training do not eliminate the observed marital wage differential, which remains highly statistically significant at the 1.0% level.

Results for nonwhite men are reported in Table 4. This sample consists of both blacks and Hispanics. A dummy variable is included in each specification to allow wage differences between these populations. Blacks are uniformly found to receive lower wages than Hispanics. As discussed earlier, the raw marital wage differential is slightly smaller for nonwhite as opposed to white men (15% versus 17%). While adding controls for years married reduced the magnitude of the coefficient to Married for white men, however, the same is not true for nonwhite men. In fact, in all but this Gross specification, the quadratic in years married is not statistically significant at even the 10% level for the nonwhite sample. The coefficient to Married bears the primary responsibility for capturing the marital wage differential and is larger in magnitude than that observed in the white sample, typically about twice as large.[10] The observation that wages are higher for married versus never married nonwhite men, but do not rise at a more rapid rate during marriage may indicate a greater role for selection effects within the nonwhite sample as opposed to the white sample.

Before performing this selection analysis, we repeat the analysis already conducted for the white sample. Adding the standard variables included in cross-section marital wage studies (the Basic specification) reduces the magnitude of the coefficient to Married by about 41% from 0.170 to 0.102. Results from the AFQT+ specification again confirm that the residual AFQT score is highly correlated with earnings, but the magnitude of the estimated marital wage

[10] We also estimated models allowing racial differences in the marital wage effect. These results tend to suggest a higher wage for married black men and higher wage growth for

effect actually increases somewhat with inclusion of this ability measure for nonwhite men. The results are similar when the sample is further restricted to those with no more than a high school education. Likewise it is clear from the next to the last column that while training activities significantly increase earnings for nonwhites, adding controls for training has virtually no impact on the estimated marital wage effect.

In general, our cross-section estimates indicate there is a substantial marital wage differential for both white and nonwhite men. For white but not nonwhite men, about half of this differential appears to be in the form of increased wage growth during marriage. For neither whites nor nonwhites is the observed differential readily explained by cognitive skills (as measured by the residual AFQT score) or by formal job training.

The marital wage differential is, however, very sensitive to the inclusion of individual specific, time invariant controls. Fixed effects estimation drives the magnitude and the significance of the marital wage differential to zero for both the white and nonwhite samples. While the coefficient to the quadratic in years married is statistically significant at the 10% level for white men, a joint test of all four marital wage parameters yields a p-value of 0.291. In the case of nonwhite men, the coefficient to Married becomes negative and the joint test yields a p-value of 0.433.[11] These results could be in part due to measurement error in years married. Estimates including only marital status dummies also fail to find a significant marital wage effect, but this could be due to specification error. Since the introduction of the residual AFQT score into the pooled cross-section analysis does not have a similar impact on the marital

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married Hispanic men, but the interaction terms are only marginally significant (p-values in the 0.05 to 0.12 range).

[11] Gray (1997) also finds no significant marital wage effect using simple panel data techniques on the NLSY data.

wage differential, these results suggest that some factor other than cognitive skills (at least as measured by the AFQT score) is driving the marital wage differential.

Conclusion

In this analysis, we explore the possibility that differences in ability and training by marital status explain the observed male marital wage differential. We find evidence that men who marry have greater cognitive skills than men who do not marry and that married men receive more training than men who are not married. Our measures of skills and formal training are positively linked to wages. However, the differences observed by marital status explain little of the observed marital wage differential. Two other results are worthy of further attention.

First, while we observed a significant marital wage differential for both white and nonwhite men, that differential appears to take a somewhat different form by race. Married white men earn more **and** experience higher wage growth during marriage than otherwise similar unmarried white men. Married black and Hispanic men earn more than their unmarried counterparts, but do not experience significantly higher wage growth. Researchers have traditionally restricted their analysis of the marital wage differential to whites on the grounds that there are distinct racial differences in marriage rates. Further analysis of the marriage decision separately by race may provide some insight into our findings of racial differences in the marital wage differential.

Second, while we find that the marital wage differential is still highly significant after including controls for cognitive skills, fixed effects estimates that control for all individual specific, time invariant factors (such as cognitive skill) yield no evidence of a marital wage

differential within our sample. There are several possible explanations for these counterintuitive results. For example, our measure of cognitive skills may be too imperfect to accurately account for individual differences in ability. The solution here would be to obtain better measures of ability. Alternatively, it is well known that fixed effects analysis magnifies measurement errors within individuals. Our measure of years married is particularly imprecise, and the noise from this variable in fixed effects estimation may be contaminating our results. The measure of marital status may also be imperfect. Estimation of the model on data with more detailed marital information could resolve this issue. Finally, it could be that there is some other individual specific, time invariant (at least within the observed time frame) variable that is driving the observed cross-section marital wage differential. This possibility, too, deserves further consideration.

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Table 1

Data on Tenure, Turnover Rates, and Quit-to-Fire Ratios
By Marital Status

White, non-Hispanic Men	Marital Status			
	Married	Never Married	Separated, Divorced	Statistical Significance
Mean Tenure (1)	6.37	5.03	5.44	a,b
Median Tenure	5	4	4	a,b
Turnover Rate (2)	2.98%	6.07%	6.89%	a,b,c
Quit-to-Fire Ratio (2)	15.3	23.6	14.3	a,c

(1) Calculated using CPS 2000 Tenure Supplement for White, Non-Hispanic Men age 30-39. Measured in Years.

(2) Calculated using the data from the January, May, and September 1989, 1991, and 1993 CPSs for White, Non-Hispanic Men age 23-37 who were not enrolled in school; married, spouse absent; widowed; or in the military. Sample consists only of employed and unemployed persons, excluding those between temporary jobs. Turnover Rate is calculated as % who lost, quit or were laid off from their job. Quit-to-Fire Ratio is calculated as 100*#quit job/(# lost + # laid off).

(3) a indicates that Married and Never Married means are significantly different at the 5% level.
b indicates that Married and Separated/Divorced means are significantly different at the 5% level.
c indicates that Never Married and Separated/Divorced means are significantly different at the 5% level.

Table 2

Sample Means
by Race and Marital Status

	White Men			NonWhite Men		
Variable	Married	Never Married	Separated/ Divorced	Married	Never Married	Separated/ Divorced
Log Hourly Wage	2.11	1.92	1.91	1.95	1,70	1.76
Wage (1982-84$)	9.44	8.09	7.74	7.88	6.30	6.66
Black	0.00	0.00	0.00	0.22	0.10	0.15
Hispanic	0.00	0.00	0.00	0.78	0.90	0.85
Education	12.79	12.92	11.86	12.40	12.15	11.82
Experience (in weeks)	1530.11	1011.36	1491.41	1228.50	871.24	1245.82
Tenure (in weeks)	243.41	159.85	170.33	216.41	131,19	135.39
Years Married	3.44	0.00	1.02	3.39	0.00	1.01
Residual AFQT Score	8.97	6.86	0,75	-15.06	-18.86	-21.37
Log of Training Time	1.32	1.02	1.12	1.30	0.92	1.17
Number of Observations	7892	3598	1601	2344	2877	795

Sample Source: 1988-94 pooled cross-sections of employed men from the NLSY, who have completed school and are not in the military.

Table 3

Estimates of the Marital Wage Differential
White Men

Variable	Gross Specification		Basic (a) Specification		AFQT+ (b) Specification		Training (c) Specification		Fixed Effects (d) Specification	
Married	0.0914	***	0.0548	***	0.0501	***	0.0502	***	0.0306	
	(0.0195)		(0.0184)		(0.0181)		(0.0180)		(0.0191)	
Separated/Divorced	-0.0400	**	0.0308	**	0.0244	*	0.0243	*	0.0072	
	(0.0169)		(0.0144)		(0.0141)		(0.0141)		(0.0225)	
Years Married	0.0283	***	0.0203	**	0.0182	*	0.0169	*	-0.0150	
	(0.0107)		(0.0102)		(0.0100)		(0.0100)		(0.0094)	
Years Married Squared	-0.0003		-0.0012		-0.0011		-0.0010		0.0020	*
	(0.0014)		(0.0014)		(0.0013)		(0.0013)		(0.0012)	
Residual AFQT Score					0.0031	***	0.0030	***		
					(0.0002)		(0.0002)			
Log of Training Time							0.0088	***	0.0114	***
							(0.0020)		(0.0031)	
F-Test on Marital Status	125.25	***	26.27	***	22.72	***	21.40	***	1.24	
Number of Observations	13091		13091		13091		13091		13091	
R-Squared	0.0369		0.3689		0.3912		0.3945		0.7670	

*** indicates statistical significance at the 1% level
** indicates statistical significance at the 5% level
* indicates statistical significance at the 10% level

a This specification includes controls for years of education; a quadratic in experience and tenure; dummy variables to identify 11 industries, 10 occupations, 3 regions, 3 city sizes, 5 interview years; a measure of the number of children; and dummy variables to identify union members and government employees.

b This specification includes all the controls listed in note (a) as well as a measure of the local unemployment rate and firm size, neither of which has traditionally been included in marital wage studies.
c This specification includes all the controls listed in note (b) as well as dummy variables to indicate receipt of training in the particular interview year and the log of accumulated training time.
d This specification includes all the controls listed in note (c) except education and residual AFQT score which are invariant within an individual.

Table 4

Estimates of the Marital Wage Differential
NonWhite Men

Variable	Gross Specification		Basic (a) Specification		AFQT+ (b) Specification		Training (c) Specification		Fixed Effects (d) Specification
Married	0.1730	***	0.1016	***	0.1070	***	0.1049	***	-0.0056
	(0.0275)		(0.025)		(0.0246)		(0.0245)		(0.0310)
Separated/Divorced	0.0361	*	0.0373	**	0.0440	**	0.0414	**	-0.0195
	(0.0213)		(0.0178)		(0.0176)		(0.0175)		(0.0351)
Years Married	0.0194		0.0024		-0.0053		-0.0048		0.0197
	(0.0158)		(0.0139)		(0.0137)		(0.0137)		(0.0151)
Years Married Squared	-0.0001		0.0009		0.0017		0.0016		-0.0031
	(0.0022)		(0.0019)		(0.0019)		(0.0019)		(0.0019)
Residual AFQT Score					0.0032	***	0.0030	***	
					(0.0003)		(0.0003)		
Log of Training Time							0.0079	***	0.0039
							(0.0027)		(0.0049)
F-Test on Marital Status	85.34	***	19.82	***	17.74	***	16.89	***	0.95
Number of Observations	6016		6016		6016		6016		6016
R-Squared	0.0730		0.3883		0.4093		0.4127		0.7053

*** indicates statistical significance at the 1% level
** indicates statistical significance at the 5% level
* indicates statistical significance at the 10% level

a This specification includes controls for years of education; a quadratic in experience and tenure; dummy variables to identify 11 industries, 10 occupations, 3 regions, 3 city sizes, 5 interview years; a measure of the number of children; and dummy variables to identify union members, government employees, and blacks (base case = Hispanic).

b This specification includes all the controls listed in note (a) as well as a measure of the local unemployment rate and firm size, neither of which has traditionally been included in marital wage studies.
c This specification includes all the controls listed in note (b) as well as dummy variables to indicate receipt of training in the particular interview year and the log of accumulated training time.
d This specification includes all the controls listed in note (c) except education and residual AFQT score which are invariant within an individual.