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ABSTRACT

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Married white men have higher wages and faster wage growth than unmarried white men. Using the NLSY, we examine whether racial differences in intrahousehold specialization and formal training explain married men's faster wage growth, and individual-specific data on cognitive skills, family background, and self-esteem contribute to married men's higher wages. African American households engage in less intrahousehold specialization and experience no differential wage growth – a finding consistent with an intrahousehold specialization argument. However, while married men have more training, cognitive ability, and self-esteem than unmarried men, controlling for these differences does not explain any component of the marital wage differential.

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I. INTRODUCTION

There is substantial evidence that married white men earn between 8 and 15% more than unmarried white men in the U.S., even after controlling for education and job market experience. Cross-section evidence indicates that both the level and the growth rate of wages increase during marriage. Fixed effects estimates indicate that much of the differential is attributable to individual specific attributes that lead men who marry to earn more throughout their lives than men who do not. Yet, while these empirical facts are well known, there is little hard evidence regarding the underlying reason(s) for these wage patterns. Using the National Longitudinal Survey of Youth, we employ data on job training and a variety of personal attributes to investigate the structure or source of the male marital wage differential.

One explanation for the male marital wage differential, and in particular for faster post-marriage wage growth, is that married men specialize more in the market sector than men living alone. Recent studies have used cross-country differences in norms and time use, in the institution of marriage, and in the marital wage differential to provide support for specialization's contribution to the marital wage differential. We introduce a new approach. We first present evidence that married men in U.S. African American households specialize less than married white men. This less specialization should result in a smaller marital wage differential for African American men, particularly a smaller increase in wage growth following marriage, as compared to white men. We look for racial differences in the male marital wage differential that would support the specialization hypothesis.

To the extent that intrahousehold specialization is driving the male marital wage differential, it is of some interest to identify how that specialization influences productivity on-

the-job. One possibility is that formal job training opportunities and outcomes may differ by marital status. We outline a simple story suggesting that married men may be more inclined to receive training and present some evidence suggesting why firms may be more willing to offer training to married as compared to unmarried men. We then use information on job training to see if wage changes at the time of marriage are driven by training differentials.

Finally, we explore the nature of the fixed individual specific effect that accounts for much of the male marital wage differential. If the individual specific attributes that are associated with marriage and with higher wages are included in cross-section estimates of the wage equation, the estimated marital wage differentials in cross-section runs should decline and more closely resemble those observed in fixed effects estimates. To this end, we examine several possible individual-specific characteristics including cognitive skills, parental background, personality traits such as self-esteem, and attitudes toward intrahousehold specialization and gender roles that may explain the fixed effects component of the male marital wage differential.

II. LITERATURE REVIEW

Evidence of the marital wage differential for white men in the U.S. is extensive (see Ribar 2004 for a review). Some of this differential has been attributed to a selection effect, to the fact that men who marry earn more throughout their lives than men who do not marry. Some has been attributed to changes in the wage function that occur at or around the time of marriage (henceforth called the change effect). This change 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. Most of the empirical literature focuses on identifying to what extent the differential is attributable to

the selection and to what extent it is attributable to the change effect. Relatively little effort has been expended trying to identify the underlying reasons for these wage patterns.[1]

The most cited explanation offered for the change effect derives from Becker's work on the theory of the family (1991). Becker noted that when two single-person households are joined to form a single, two-person household, there are gains to be had from specialization and the division of labor as well as economies of scale in household production. Women have historically specialized in home-based activities, and men in market-based activities. Time may be thus allocated because of social norms or expectations regarding 'acceptable' activities for men and women (see Shelton and John 1996 for a review of theoretical work including the social construction of gender) or because of gender differentials in labor market opportunities that lead households to rationally allocate more market time to the partner with the highest earnings potential – typically the man. This specialization allows married men more time and energy to devote to market activities, and thus men's productivity and wages rise following marriage.[2]

This specialization hypothesis relies on the assumption that following marriage men change their behavior. Evidence based on activity reports is mixed. Hersch and Stratton (2000) and South and Spitze (1994) found that men's total reported housework time does not differ by marital status, but that married men report more time spent on home maintenance chores than on 'routine' or 'female type' housework such as cooking and cleaning. Gupta (1999) reported that this is not just a selection effect, but that men actually reduce the time they spend on 'routine' or 'female type' housework when they marry. Hersch and Stratton (2000) estimated wage equations controlling for time spent on housework as well as selection, but found that while men who spend more time on housework earn less, controlling for housework time does not change the magnitude or significance of the male marital wage effect.

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Few data sets contain information on how time is spent off the job and an alternative approach has been to relate men's behavioral change to spousal activity – the hypothesis being that men whose wives are not employed in the market are more able to specialize than men whose wives are employed. Daniel (1991), Gray (1997), and Chun and Lee (2001) reported that married men whose wives work fewer hours receive higher wages than married men whose wives work longer hours. Their results were 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. Jacobsen and Rayack (1996), however, found a premium that was not robust to fixed effects or instrumental variables estimation, and Loh (1996) found the opposite effect, a wage penalty for men in dual earner households. In general, wives' working status may be related to men's wages, but the evidence is inconclusive. This may be because men's reported housework time is not sensitive to the employment status of their spouse (Hersch and Stratton 2000; South and Spitze 1994). It may be that dual career households purchase household services in the market.

Alternatively there exists some literature using time series data to test the specialization hypothesis. Blackburn and Korenman (1994) tried unsuccessfully to relate time series evidence of a declining gross white male marital wage differential to the increasing labor force participation rate of women in the U.S.. More promising are results by Gray and Vanderhart (2000), who presented evidence that the male marital wage differential declined in the 1970s in those states that instituted a unilateral divorce law. Such laws, by making divorce easier, should make intrahousehold specialization less attractive and so reduce the differential.

Cross-country comparisons have also been made. Evidence that married men in many developed countries receive higher wages than their unmarried counterparts (Schoeni 1995)

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suggests that the differential is not unique to the U.S.. More recent in-depth studies of the marital wage differential abroad have identified some significant cross-country differences. Given substantial cross-country differences in norms and in the institution of marriage, these studies hold some promise for helping to identify the nature of the differential. Bardasi and Taylor (2004) found evidence that married men earn more than never married men in Great Britain, but that none of the differential takes the form of faster post-marriage wage growth and only a small fraction is not attributable to selection. Richardson (2003) used Swedish data and found that wages actually appear to decline with years married in Sweden. She offered no clear explanation for this effect, but Datta Gupta, Smith, and Stratton (2005) attribute a similar effect using a recent cohort of Danish men to the substantially lower intrahousehold specialization observed in Denmark and Scandinavia more generally. Thus, the intrahousehold specialization explanation for how marriage might influence the wage function may also receive support by comparing populations that demonstrate different intrahousehold specialization.

Research examining the selection component of the male marital wage differential has been limited at best. This component is often measured by comparing fixed individual-specific effects estimates with pooled cross-section estimates of the marital wage effect. While Korenman and Neumark (1991) attributed about 20% of the total marital wage differential to selection using this technique, others (especially Gray 1997) have found selection to be far more important. Furthermore, there is evidence that selection has become increasingly important in the U.S. (Gray 1997) and is of substantial importance abroad (Bardasi and Taylor 2004 for Great Britain; Datta Gupta, Smith, and Stratton 2005 for Denmark). An alternative approach to measuring the selection component has been to use data on twins and assume the selection effect is common to twins independent of their marital status. Krashinsky (2004), using a small sample

of twins, found that the marital wage differential was entirely explained by a selection or twin effect. Isacsson (2003) used a much larger sample of Swedish twin data that also has a longitudinal component. He found that estimates of the marital wage differential declined about 25% when controlling for twin-specific effects, but that individual-specific effects explained all of the differential, suggesting that differences between even twins could be important. Both authors recognized the sensitivity of difference estimators to measurement error and took steps to control for this. Neither author, however, was able to control for both marital status and years married, so that these results will be biased if wages grow faster following marriage.

While these papers present substantial evidence of the importance of selection effects, they provide little insight into the nature of the selection effect. Somehow men who marry are different from men who do not marry in a manner that is not otherwise controlled for in the wage equations but is linked to higher productivity on the job. In a rare paper addressing the selection component, Krashinsky (2004) reported that ability differentials (and differences in the returns to ability) explain a substantial fraction of the total and the selection component of the U.S. marital wage differential. Other authors (for example, Bardasi and Taylor 2004) mention attitude, self-esteem, congeniality, loyalty, honesty, dependability, leadership, industriousness, even appearance. The literature on personality traits and how they influence earnings is a growing one. Bowles, Gintis, and Osborne (2001) provided a justification for including such measures in earnings equations and a partial review of the extant literature. Meuller and Plug (2004) is representative of more recent work in the field. It seems logical to suppose that many personality attributes that are likely to increase earnings may also attract marriage partners. This attraction may be driven by the higher earnings or by the attributes themselves. Thus, in wage equations that fail to control for personality traits but do control for marital status, the marital

status dummy may be serving as a proxy for the personality traits. Further, as most men marry, the timing of the marriage, as captured by the years married measure, may be correlated with the degree to which the individual has 'attractive' and more productive personality traits and so may also be correlated spuriously with wages.

III. METHODOLOGY

In this paper, we explore further the nature of both the change and selection effects of the male marital wage differential. The change effect is investigated in two ways.

First, if the marital wage differential and in particular the higher rate of wage growth following marriage is attributable to intrahousehold specialization, then an analysis of populations with different specialization rates should reveal different marital wage differentials. Researchers comparing the marital wage differential in households with and without working spouses were essentially making such a comparison. The finding, reported above, that white men's reported time spent on housework does not vary substantially with the employment status of their spouse suggests that intrahousehold specialization is not as sensitive to household characteristics as researchers expected and may explain the mixed results obtained from this literature. Datta Gupta, Smith, and Stratton (2005), however, attribute their finding that wages do not grow more rapidly following marriage in Denmark (while they do in the U.S.) to the lower degree of intrahousehold specialization within that country, as suggested by both employment behavior and time use studies. Thus, testing for marital wage differences as a function of differences in intrahousehold specialization may have power when applied to sufficiently different populations.

In this paper, we make use not of international differences but of racial differences in intrahousehold specialization in order to test the hypothesis that the male marital wage differential is attributable to intrahousehold specialization. There is substantial evidence that white and African American households make different time allocation decisions in both the market and the home sector.

African American women have historically had higher labor force participation rates than white women in the U.S.. This continues to be true today. July 2005 Current Population Survey (CPS) data indicate that married African American women had a labor force participation rate of 67% while married white women had a labor force participation rate of 60%. In 1989, this differential was even larger as 58% of African American and 48% of white women participated in the labor force. Sample statistics from the American Time Use Survey 2003 (ATUS) provide further evidence of racial differences in labor force participation. These data indicate that married African American men spend on average 20% less time in market employment while married African American women spend on average 30% more time in market employment than their married white counterparts. African American women thus appear to contribute 45.5% of couple market hours as compared to 33.9% for white women. See Appendix A for further details.

The contribution of African American women to couple time spent on housework and on childcare is also closer to an even 50% split than is observed within white households. The differences in this sector are more substantial when housework is defined more narrowly to include only routine tasks that every household must perform such as cooking, cleaning, laundry, and grocery shopping. These results are not based on true intrahousehold comparisons because

the ATUS only collects information from one person per household, but partner data from the National Survey of Families and Households shows a similar pattern.

These patterns persist when conditioning on the presence of children, but there are many more household characteristics that could influence intrahousehold specialization and explain the observed racial differences. Shelton and John (1996) in a review of the household labor literature contend that when controlling for other such factors the evidence of racial differences in intrahousehold specialization is mixed. A more recent paper by Kamo and Cohen (1998), however, presents evidence that African American men do contribute a significantly greater share of household housework time even after controlling for education, age, employment hours, income, cohabitation status, number of children, and gender attitudes.

If African American couples are less likely to specialize than white couples and specialization is behind the wage change effect of the male marital wage differential, then we should see a smaller wage change effect for African Americans than for whites who marry. As specialization seems more likely to affect wages cumulatively over time rather than to cause an immediate jump in earnings power, this suggests that married African American men will not experience the same increased post-marriage wage growth as white men. Previous research (Blackburn and Korenman 1994 and Cohen 2002) has found the gross male marital wage differential to be slightly smaller for African Americans than for whites, but has not controlled for years married or for selection, making it difficult to truly compare the white and African American marital wage results. We perform comparable, detailed analysis for both white and African American men to test our hypothesis that wage growth should increase more post-marriage for white than for African American men.

Second, we explore the mechanism by which selection or marriage generally may change wages. Specifically we look at the possibility that married men may acquire job-related human capital faster than men who are not married. While controls for informal training, such as job experience and/or job tenure, are standard in the marital wage literature, additional controls for formal job training are not. If married men receive more job training than unmarried men, then the observed marital wage differential may be due not to marital status but to training differentials. As training is optimally received earlier rather than later and likely influences wages permanently rather than simply during the marriage, training differentials may also explain the faster growth rate of wages following marriage.

Why might married men receive more job training? On the one hand, marriage does often entail significantly greater financial responsibilities (particularly following the arrival of children) and may make men more receptive to job training opportunities. Job training may be offered without regard to marital status, but married men may be more likely to accept such opportunities. If capital markets are not perfect and individuals are not always able to borrow against future earnings in order to finance current training, then marriage to a working spouse may actually provide men the financial support to pursue additional training. In this case, one would expect more rapid wage growth for men with working spouses, but in either case married men would receive more training than men who are not married.

On the other hand, firms may be more inclined to offer firm-specific training to married men 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 danger here is turnover. The greater is expected turnover, the less investment there will be in firm-specific training.

If married men are or are perceived as being more stable employees as compared to unmarried men, then firms may offer married men more training. While perceptions are difficult to measure, Table 1 presents evidence that married white men have higher tenure, lower turnover, and lower quit-to-fire ratios than unmarried white men.

Data on tenure by marital status were obtained from the 2000 Tenure supplement to the 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 and are not due to the presence of outliers as median tenure is also higher for married men than for never married or divorced/separated men.[3]

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 firm 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 laid 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 findings 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 expected quit rates by marital status rather than to marital status itself.

The nature of the individual-specific, time invariant component of the marital wage differential is then examined. First, we estimate fixed effects wage models in order to determine the magnitude of the individual-specific component of the marital wage differential. Then we return to our cross-section specification to see how estimates of the marital wage differential respond to added controls for cognitive skills, family background, and self-esteem/personality factors. Standard wage regressions include controls for education, but there is a large literature (see Ferguson 1995, Neal and Johnson 1996, Rodgers and Spriggs 2002) demonstrating that other measures of ability are positively correlated with earnings. If more able men (by these measures) are also more likely to marry, then marriage dummies may be proxying for ability in cross-section analyses. Family background variables such as parental education and maternal employment may influence the degree to which men are attractive marriage partners and the degree to which they are likely to specialize following marriage (Cunningham 2001). These factors may also contribute to otherwise unobserved individual specific ability. Personality factors may be linked to both earnings and marital outcomes. If we correctly identify the individual specific factors that link marriage and earnings, then our cross-section estimates of the male marital wage differential should closely resemble the panel estimates of that differential.

IV. DATA

The data used in this analysis are from the National Longitudinal Survey 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. We use data from the 1988 through the 1994 waves, when respondents are between the ages of 23 and 37. The final sample is constructed subject to the following restrictions. Respondents are required 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 2436 white, non-Hispanic men (henceforth referred to as white men) and 934 African American 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 12,802 white male-year observations and 4,957 African American 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.

Detailed firm-provided job training information has been available from the NLSY since 1988. At each interview, respondents are asked to describe whether they received any type of training since the last interview.[6] If the respondents answer "yes", then they are 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 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 in such areas 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.

Individual specific attributes, whose role it is to explain the selection component of the marital wage differential, include cognitive skills, family background, and personality traits.

The cognitive skills measures we employ are based on the Armed Force Qualification Tests (AFQT) administered to the NLSY sample in 1980. Several researchers have used the composite AFQT score to represent skills.[7] 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 (Rodgers and Spriggs 1996; Hansen, Heckman, and Mullen 2003). Since the AFQT score was administered to every respondent in the same year, some of the respondents were older and had more education than others when they took the test. In order to control for age induced effects, we include 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 consideration of the impact education may have upon the score, we conduct sensitivity analysis using the subsample of individuals who obtained no more than a high school degree. Most individuals with this level of education had completed school prior to taking the test. We focus on three components of the AFQT test: one representing word knowledge, one representing reading comprehension, and a third representing arithmetic reasoning.

The family background variables for which we control are parental education and occupation. Specifically we construct a vector of dummy variables to identify those men whose fathers completed at least high school, whose mothers completed at least high school, as well as the occupation of each parent when the respondent was age 14. This approach also controls for the employment status of the mother more generally and hence may capture some information about the degree of specialization within the parental household.

Finally, the NLSY also includes some questions regarding personality type – with a particular focus on self-esteem. Waddell (2005) reported evidence that self-esteem measures

such as those reported here have a significant impact on earnings and it seems likely that self-esteem may also be related to marriage prospects. There are a total of ten questions (see Appendix B for details) to which respondents are asked to "strongly agree", "agree", "disagree", or "strongly disagree". We include three dummy variables for each question in order to allow for all nonlinearities. These questions were asked in both 1982 and 1987. As our wage observations begin in 1988, both sets of measures precede our wage estimates and we perform analysis with both.

Pertinent sample statistics by race and marital status are presented in Table 2. See Appendix C 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; and more detailed information regarding formal training, cognitive ability, family background, and self-esteem. These statistics 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 African American men it is approximately 24%. Men who are divorced or separated have wages roughly similar to those who never married. The marital wage differential is not readily attributable to differences in educational attainment, as married and never married men report having approximately the same amount of education. However, married men clearly have more job experience and/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.

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The variables of particular interest here, however, are the measures of job training, cognitive skills (as measured by the AFQT scores), family background, and self-esteem. The measure of job training reported in Table 2 is the log of time spent in job training since 1988. As hypothesized, married men have received more training than men who have never married – about 50% more. If this training raises wages, then some of the observed marital wage differential may be attributable to differences in job training. In the empirical specification, cognitive skills are measured using three different components of the AFQT scores. Table 2 reports sample means only for the residual composite AFQT Score. What matters for this analysis is the value for married as opposed to not married men. Married and never married white men have similar residual test scores, but those for separated/divorced white men are lower and married African American men have higher residual scores than all not married African American men. Again, some of the marital wage differential may be directly attributable to these measures of individual specific cognitive skills. There is also evidence that family background and self-esteem differ by marital status. Table 2 reports some information on parental education and self-esteem. See Appendix C for more detailed data. The evidence here suggests that separated/divorced men have less educated parents than all other men and that married men have more self-esteem (lower aggregate measures) than all not married men.

Regression analysis allows us to control for all these factors simultaneously. If differences in job experience, job tenure, job training, cognitive skills, family background, or self-esteem explain the marital wage differential, then the dummy variable for marital status in a log wage regression that includes controls for all these factors will fall to zero.

V. EMPIRICAL SPECIFICATION

The wage analysis begins with pooled cross-section estimation separately for whites and African Americans of the simple marital wage differential with controls for years married. This specification will identify the gross male marital wage differential and any racial difference in this differential. We then add the usual wage controls to see what fraction of the gross differential is explained by differences in education, experience, tenure, and occupation separately by race. Next we control for job training to see if this explains any of the estimated marital wage differential. Finally, we estimate a fixed effects specification in order to determine the magnitude of the selection effect and introduce individual specific factors into the cross-section estimates in hopes of explaining the nature of this selection effect, again separately by race.

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

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

where Mar_{it} 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 years married. Never married men constitute the base case. The coefficient to the dummy variable identifying married men captures the gross wage differential associated with marriage. This differential may capture wage discrimination in favor of married men and/or reflect selectivity differentials between men who marry and men who do not. The coefficient to the measure of years married captures differential post marriage wage growth. This differential wage growth is

typically attributed to the benefits of increased specialization post-marriage that allow married

men to spend more time and effort in the market than men who are not married.

The next specification, labeled the **Basic** specification, adds a vector X_{it} of individual,

job, and time specific characteristics such as are typically incorporated in wage analyses. The

third specification, labeled **Training**, incorporates a set of year specific indicator variables to

identify periods when the respondent was 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. These purely cross-sectional analyses will tell us

whether there exist racial differences in the marital wage differential that are consistent with

differences in intrahousehold specialization and whether any of the observed cross-sectional

differences in earnings, particularly those taking the form of faster wage growth for married men,

are explained by differential job training.

Then, we estimate a **Fixed Effects** specification, where individual dummy variables are

included to capture all individual specific, time invariant factors that influence earnings ($\varepsilon_{it} = \mu_i$

$+ \eta_{it}$ where μ_i is the individual specific factor). Estimates from this specification are compared

with the cross-section estimates to determine the magnitude of the marital wage differential that

is attributable to the selection effect. Our strategy is to identify the nature of this selection effect

by adding to the cross-section wage equation various individual specific attributes that are

positively correlated with both earnings and marital status. To the extent that the important time

invariant factors are captured in this expanded specification (**Individual Specific**), the

coefficients to the marital status variables in these cross-section models should approach the

values observed in the **Fixed Effects** specifications.

VI. RESULTS

Selected coefficient estimates for the *Gross*, *Basic*, and *Training* specifications are reported in Table 3. [8] The first three columns present results for the white sample, the last three columns results for the African American sample. The *Gross* specification for whites indicates that married men earn about 9% more than never married men, perhaps 13% more than separated/divorced men, and that wages rise faster following marriage by about 2.7% annually.[9] There are striking differences by race. The *Gross* specification for African Americans indicates that the marriage premium is almost twice as large and that the wage growth differential only about half as large as that observed for white men. 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 actual experience and tenure; a measure of the number of children, the firm size, and the local unemployment rate; and dummies for 1-digit industry and occupation, region of residence, city size, interview year, union status, and government employment reduces the observed white male marital wage differential by 30% to 6.4% and the observed higher growth rate by 60% to just over 1% annually. Introducing these controls also changes the sign of the wage differential between white separated/divorced and never married men from –4% to +3%. These results are comparable to those reported elsewhere in the literature. The effect on African American men is even more pronounced as the coefficient to the marital dummy declines by 45% and the coefficient to years married declines by 75%. As observed in the sample statistics, married men have more experience and tenure. Failing to control for these differences has a significant influence on estimates of the differential, even more so for African American as compared to white men. Nevertheless, for both white and African American men there remains a significant marital wage differential.

What is of greater interest is the manner in which the results of the *Basic* specification differ by race. Recently married white men earn about 6% more than otherwise similar individuals and experience a rate of wage growth about 1.1% greater than their unmarried counterparts. Thus, there is significant evidence of faster wage growth following marriage which could be attributed to increased specialization following marriage because married men have more time to devote to market activities. By contrast, recently married African American men earn over 10% more than otherwise similar individuals but experience no wage growth differential. As argued above, there is evidence from time use studies that African American households engage in less intrahousehold specialization than white households. Our finding that married African American men do not experience the faster post-marriage wage growth that married white men do is indirect evidence that higher post-marriage wage growth could be attributed to intrahousehold specialization.[10] Nevertheless, we do continue to find significant differences in the level of wages for married and unmarried men – particularly African American men – that warrant further analysis.

While we have evidence that intrahousehold specialization may be important, however, the precise mechanism by which marriage influences wages still has not been identified. One possibility is that married men receive more job training than men who are not married. It could be that this training accumulates after marriage (a result of intrahousehold specialization) and so increases the rate of wage growth following marriage. Alternatively, it could be that men who marry receive more training than men who do not marry and so have higher wage growth or higher wages throughout their lives. We control for these possibilities in the remaining specifications by including a measure of the log of total training time as well as controls for participation in current training. Current training is expected to lower earnings as training entails

time that likely reduces productivity on the job today in return for higher productivity later. Employees are likely to bear some of the cost of this training by receiving lower wages. Our results (available upon request) indicate that this is generally the case. Accumulated training time, by contrast, should increase productivity and hence wages. Results in columns 3 and 6 confirm this. We find that a 1% increase in training time increases earnings by about 1% for both whites and African Americans. Controlling for training does not, however, substantially alter the estimated marital wage premium for either population. The parameter estimates of the marital wage differential fall, but the difference is neither significant nor substantial. Thus, while training clearly increases wages, it is not the mechanism driving the male marital wage differential.

The results of our analysis of the selection component of the male marital wage differential are presented in Table 4. Again we report the results of three specifications separately for the white and African American samples. The first of these is the *Training* specification presented in Table 3. The results are repeated here for comparison with the *Fixed Effects* specification that controls for all individual specific time invariant factors and with the *Individual Specific* specification that is itself a cross-section wage model that includes controls for such individual specific factors as cognitive skills (measured by three components from the AFQT), parental education, parental occupation, and individual responses to questions intended to measure self-esteem.

The *Fixed Effects* specification shows the tremendous importance within this sample of the selection effect. Controlling for selection in this manner reduces the magnitude of the coefficient to the marital status dummy by 80% for the white sample and by 50% for the African American sample. Neither coefficient maintains statistical significance, though the differential

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for married African American men is still substantial at around 5%. There is further, no evidence of differential wage growth following marriage in the *Fixed Effects* specification and no evidence for white separated/divorced men of any wage differential as compared with never married men. The magnitude of the training effect is unchanged for the white sample, but about 30% smaller for the African American sample. While it is true that the Fixed Effects estimates are likely to be biased towards zero due to their greater sensitivity to measurement error[11] and to have larger standard errors because of substantially fewer degrees of freedom (particularly true for the smaller African American sample), these results suggest that virtually all of the white and about half of the African American male marital wage differential is due to selection.

But what is it about men who marry that is different and leads to higher wages? We seek to identify the selection mechanism by next estimating a cross-section wage model (the *Individual Specific* specification) that controls for a number of individual specific effects. While we find that individuals with higher ability (as measured by three components of the AFQTest) do earn significantly higher wages, that parental occupation is significantly linked to own earnings (perhaps through its relation to parental intrahousehold specialization as an important component of this occupation measure is the employment status of the mother), and that personal self-esteem and own earnings are positively associated for white men, incorporating these controls has little impact on the estimated marital wage differential. The coefficients to the marital status variables fall by only about ten percent and remain statistically significant.

We performed several sensitivity tests on this specification. As discussed earlier, the AFQT scores were obtained from individuals with different levels of education. As the level of education completed may impact the score, we repeat the analysis for the sample of individuals who completed no more than high school and thus had essentially completed their education by

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the time they took the AFQT tests. Our results are unchanged. We also tested the sensitivity of

our results to alternative measures of self-esteem. The results reported include self-esteem

measures taken from the 1980 NLSY interview. These substantially pre-date the first reported

wage measures from 1988. The identical battery of questions was repeated in 1987. Given the

high degree of serial correlation in wages and the likely relation between wages and self-esteem

(with high wages increasing self-esteem), we felt it was important to use personality measures

from the earlier period, however, the results were not particularly sensitive to the measure

employed.[12] In separate specifications (available upon request), we also included controls for

individual's gender role attitudes. It could be that men who marry perceive their role in the

family differently than men who do not marry and that this is somehow correlated with earnings.

Thus, we used responses to the questions, *"A woman's place is in the home – not in the office or*

shop", *"It is much better for everyone concerned if the man is the achiever outside the home and*

the woman takes care of the home and family", and *"Men should share the work around the*

house with women, such as doing dishes, cleaning, and so forth", to control for gender role

attitudes. The same attitudinal questions were asked in both 1979 and 1987. In no case did we

find these variables significantly correlated with earnings.

VII. CONCLUSION

In this analysis, we explore the nature of the white male marital wage differential.

Evidence suggests the differential is split between a wage change component and a selection

component, with selection becoming increasingly important for more recent cohorts in the U.S..

The wage change component has often been attributed to intrahousehold specialization, but

attempts to capture this empirically have met with little success. Explanations for the selection

component have received even less empirical attention. Our purpose here is to focus further attention upon the mechanisms underlying the observed differential.

We begin by exploiting substantial racial differences in intrahousehold specialization to hypothesize that the male marital wage differential for African Americans in the U.S. is smaller, particularly the differential arising from faster post-marriage wage growth, than that observed for whites. In one of the first analyses of African American marital wage differentials to control for job experience, job tenure, and years married[13], we find significant support for our hypothesis. While marriage is associated with a large overall wage differential, there is no evidence that wages rise faster following marriage for African American men. This is in contrast to the 1% annual increase in wage growth observed for white men – an increase that is of approximately the same magnitude as the return to tenure.

This evidence in support of the intrahousehold specialization argument is somewhat indirect so we delve further to look at one particular productivity enhancing behavior that appears to be associated with marital status and is potentially attributable to intrahousehold specialization: job training. We find that married men receive more informal and formal training than men who are not married and that training does significantly increase wages, but controlling for training does not substantially alter the estimated marital wage differential for either the white or the African American sample.

Finally, we examine the selection component of the male marital wage differential. Fixed Effects estimates reveal that the vast majority of the differential for white men (as reported elsewhere in the literature) and at least half of the differential for African American men is attributable to individual specific, time invariant factors. We attempt to identify these factors by controlling for cognitive skills and personality factors (which have previously been linked to

higher productivity) as well as parental background and gender role attitudes (which may be an indicator of individual attachment to the labor force). While cognitive skills, parental occupation, and for whites' self-esteem were significantly associated with earnings, none of these variables appears to explain the selection component of the male marital wage differential. Further work looking at other personality traits in this manner may, however, provide the explanation. For example, men who marry may have a more agreeable temperament than men who do not marry. More agreeable men may be easier to work with on the job and hence be more productive and earn higher wages. They also may be better marriage partners. Work to identify such traits as well as to measure intrahousehold specialization could help isolate the source of the male marital wage differential. We hope that this analysis encourages other researchers to do more to *explain* rather than simply *empirically measure* the male marital wage differential.

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Table 1

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

	Marital Status			
White, non-Hispanic Men		Never	Separated,	Statistical
	Married	Married	Divorced	Significance (3)
Mean Tenure (1)	6.37	5.03	5.44	a,b
Median Tenure	5.0	4.0	4.0	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			African American Men		
Variable	Married	Never Married	Separated/ Divorced	Married	Never Married	Separated/ Divorced
Log Hourly Wage	2.05	1.93	1.91	1.92	1.66	1.75
Wage (1982-84$)	8.76	7.99	7.75	7.51	5.91	6.62
Education	12.70	12.97	11.97	12.69	12.35	12.36
Experience (in weeks)	949.23	749.75	959.89	755.88	543.16	846.70
Tenure (in weeks)	210.82	140.970	168.70	182.53	119.34	139.59
Years Married	3.42	0.00	1.21	3.39	0.00	1.19
Log of Training Time	1.62	1.19	1.45	1.51	0.94	1.62
Residual Composite AFQT Score	5.69	5.74	-0.83	-16.20	-19.86	-20.54
Father's Education[a]	11.24	11.97	10.73	10.16	10.23	9.51
Mother's Education[a]	11.09	11.36	10.66	10.82	10.93	10.54
Self-Esteem[b]	7.87	8.39	8.13	7.52	8.27	8.62
Number of Observations	7715	3518	1569	1789	2500	668

Sample Source: 1988-94 pooled cross-sections of employed men from the NLSY, who have completed school and are not in the military.
(a) Sample restricted to those with non-missing values. Less than 10% of any white sample reported missing parental education but almost 35% of separated/divorced African American men fail to report their father's education level. Missing values are identified with a dummy variable in estimation.
(b) The sum of 10 categorical variables, each with values running from 0 to 3, designed to measure self-esteem. Variables redefined to ensure that higher values indicate less self-esteem.

Table 3

Estimates of the Marital Wage Differential
Racial Differences and Training Effects

Variable	White Men			African American Men		
	Gross Specification	Basic (a) Specification	Training (b) Specification	Gross Specification	Basic (a) Specification	Training (b) Specification
Married	0.0914	***0.0637	***0.0628	***0.1853	***0.1030	***0.1005 ***
	(0.0205)	(0.0170)	(0.0169)	(0.0276)	(0.0244)	(0.0243)
Separated/Divorced	-0.0425	0.0349	* 0.0340	* 0.0260	0.0194	0.0157
	(0.0268)	(0.0200)	(0.0200)	(0.0368)	(0.0279)	(0.0275)
Years Married	0.0271	***0.0110	** 0.0102	** 0.0151	***0.0046	0.0041
	(0.0031)	(0.0043)	(0.0042)	(0.0057)	(0.0057)	(0.0056)
Log of Training Time			0.0114	***		0.0114 ***
			(0.0034)			(0.0043)
F-Test on Marital Status	78.23 ***	10.16 ***	9.64 ***	7.09 ***	11.17 ***	10.79 ***
Number of Observations	12802	12802	12802	4957	4957	4957
R-Squared	0.0374	0.3757	0.3802	0.0526	0.3867	0.3924

Robust standard errors corrected also for clustering by individual are reported in parentheses below coefficient estimates.
*** indicates statistical significance at the 1% level, 2-sided test
** indicates statistical significance at the 5% level, 2-sided test
* indicates statistical significance at the 10% level, 2-sided test

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, firm size, and the local unemployment rate; and dummy variables to identify union members and government employees.
b   This specification includes all the controls listed in note (a) as well as dummy variables to indicate receipt of training in the particular interview year.

Table 4

Estimates of the Marital Wage Differential
Individual Specific Factors

	White Men			African American Men		
Variable	Training Specification	Fixed Effects Specification	Individual Specific Specification	Training Specification	Fixed Effects Specification	Individual Specific Specification
Married	0.0628	***0.0119	0.0565	***0.1005 ***	0.0472	0.0964 ***
	(0.0169)	(0.0187)	(0.0166)	(0.0243)	(0.0348)	(0.0240)
Separated/Divorced	0.0340	*-0.0055	0.0310	0.0157	0.0201	0.0100
	(0.0200)	(0.0255)	(0.0196)	(0.0275)	(0.0455)	(0.0272)
Years Married	0.0102	**0.0003	0.0094	**0.0041	-0.0070	0.0024
	(0.0042)	(0.0048)	(0.0042)	(0.0056)	(0.0066)	(0.0055)
Log of Training Time	0.0114	***0.0115	***0.0088	***0.0114	***0.0079	0.0096 **
	(0.0034)	(0.0044)	(0.0033)	(0.0043)	(0.0069)	(0.0041)
Including:						
3 AFQT Residual Scores			Yes ***			Yes ***
Parents' Education			Yes			Yes
Parents' Occupation			Yes **			Yes ***
Self-Esteem			Yes ***			Yes
F-Test on Marital Status	9.64 ***	0.35	8.29 ***	10.79 ***	1.16	9.44 ***
Number of Observations	12802	12802	12802	4957	4957	4857
R-Squared	0.3802	0.7651	0.4052	0.3924	0.6860	0.4202

Robust standard errors corrected also for clustering by individual are reported in parentheses below coefficient estimates.
*** indicates statistical significance at the 1% level, 2-sided test
** indicates statistical significance at the 5% level, 2-sided test
* indicates statistical significance at the 10% level, 2-sided test
Each 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, firm size, and the local unemployment rate; dummy variables to identify union members and government employees; and dummy variables to indicate receipt of training in the particular interview year.

	White	African American	African American/ White
Appendix A Differences in Household Time Allocation for Married Persons By Race			
% of Households in which Both Partners Work Full-Time	39.85	45.61	
% of Households in which Man Works Full-Time	86.41	77.69	
- % in which Woman Works Full-Time	46.12	58.71	
- % in which Woman Works Part-Time[a]	23.36	12.90	
- % in which Woman Not Employed	30.52	28.39	
Average Time Men Work for Pay	275.83	220.52	79.95%
Average Time Women Work for Pay	141.24	184.37	130.54%
% of Total Work Time Contributed by Women	33.87%	45.54%	
Average Time Men Spend on All Housework	105.57	81.94	77.62%
Average Time Women Spend on All Housework	179.19	133.54	74.52%
% of Total Housework Time Contributed by Women	62.93%	61.97%	
Average Time Men Spend on Routine Housework	40.67	39.96	98.26%
Average Time Women Spend on Routine Housework	141.89	118.73	83.68%
% of Routine Housework Time Contributed by Women	77.72%	74.82%	
Average Time Men Spend on Childcare	48.17	40.16	83.37%
Average Time Women Spend on Childcare	86.16	67.13	77.91%
% of Total Childcare Time Contributed by Women	64.14%	62.57%	
Sample Size	6223	399	

Source: Married couple households with respondents between the ages of 20 and 50 from the 2003 ATUS. Time is measured in minutes. Housework is defined broadly to include: interior cleaning, laundry, sewing; food preparation, presentation, and cleanup; interior maintenance, repair, and decoration; exterior maintenance, repair, and decoration; lawn, garden and houseplants; animals and pets; vehicles; appliances and tools; household management; other household work; and grocery shopping. Routine Housework is defined to include: interior cleaning, laundry, sewing; food preparation, presentation, and cleanup; and shopping. Childcare is defined to include care of own household children only.

a: Includes classification "hours vary".

Appendix B	
Questions Regarding Personality Traits.	
WORTH	I feel that I'm a person of worth, at least on an equal basis with others.
GOODQUAL	I feel that I have a number of good qualities.
FAILURE	All in all, I am inclined to feel that I am a failure.
CAPABLE	I am able to do things as well as most other people.
PROUD	I feel I do not have much to be proud of.
POSATT	I take a positive attitude toward myself.
SATMYSELF	On the whole, I am satisfied with myself.
SELFRESPECT	I wish I could have more respect for myself.
USELESS	I certainly feel useless at times.
NOGOOD	At times I think I am no good at all.

Appendix C
Sample Means
by Race and Marital Status

Variable	White Men			African American Men		
	Married	**Never Married**	**Separated /Divorced**	**Married**	**Never Married**	**Separated /Divorced**
Log of Firm Size (# of employees)	3.86	3.56	3.45	4.46	3.87	4.18
County Unemployment Rate × 10	68.96	65.83	67.86	61.11	61.16	62.39
Number of Children	1.27	0.11	0.30	1.61	0.27	0.43
Regional Dummies:						
Midwest	0.32	0.31	0.24	0.11	0.18	0.14
South	0.33	0.27	0.41	0.70	0.63	0.68
West	0.21	0.24	0.22	0.08	0.05	0.07
City/Metro Dummies:						
SMSA, Not Central City	0.38	0.36	0.34	0.29	0.19	0.20
SMSA, Central City Unknown	0.27	0.31	0.32	0.37	0.31	0.35
SMSA, Central City	0.07	0.13	0.08	0.15	0.28	0.21
Job Characteristics:						
Union Member	0.14	0.10	0.10	0.21	0.14	0.15
Employed in Public Sector	0.14	0.10	0.14	0.27	0.16	0.20
Industry						
Agriculture, Forestry, & Fisheries	0.04	0.04	0.05	0.00	0.04	0.01
Mining	0.01	0.01	0.00	0.01	0.00	0.02
Construction	0.12	0.15	0.19	0.14	0.14	0.13
Manufacturing	0.28	0.22	0.24	0.23	0.20	0.19
Transportation, Communication, Public Utilities	0.10	0.06	0.07	0.15	0.07	0.14
Wholesale & Retail Trade	0.17	0.23	0.21	0.16	0.21	0.14
Finance, Insurance, & Real Estate	0.04	0.05	0.04	0.02	0.02	0.05
Business & Repair Services	0.08	0.09	0.09	0.07	0.11	0.09
Personal Services	0.01	0.03	0.02	0.01	0.03	0.04
Entertainment & Recreation Services	0.01	0.01	0.01	0.01	0.02	0.01
Professional & Related Services	0.07	0.06	0.06	0.08	0.10	0.10
Public Administration	0.06	0.04	0.04	0.12	0.06	0.09
Occupation						
Professional, Technical	0.13	0.13	0.05	0.08	0.05	0.06

Managers	0.15	0.11	0.12	0.08	0.05	0.07
Sales Workers	0.06	0.06	0.03	0.02	0.02	0.04
Clerical Workers	0.06	0.08	0.05	0.12	0.09	0.09
Craftsmen & Foremen	0.23	0.22	0.35	0.16	0.16	0.16
Operatives	0.19	0.17	0.18	0.29	0.22	0.27
Laborers, except Farm	0.08	0.11	0.09	0.12	0.19	0.15
Farmers & Farm Managers	0.01	0.00	0.00	0.00	0.00	0.00
Farm Laborers & Foremen	0.01	0.02	0.01	0.00	0.01	0.00
Service Workers	0.08	0.10	0.12	0.14	0.21	0.16
Private Household Workers	0.00	0.00	0.00	0.00	0.00	0.00
Year Dummies:						
1988	0.14	0.22	0.12	0.13	0.19	0.10
1989	0.15	0.19	0.14	0.13	0.17	0.13
1990	0.16	0.16	0.16	0.14	0.16	0.13
1991	0.14	0.13	0.13	0.15	0.14	0.15
1992	0.14	0.12	0.13	0.15	0.13	0.15
1993	0.14	0.10	0.16	0.15	0.11	0.17
1994	0.14	0.09	0.17	0.15	0.10	0.16
Training Variables:						
Received Training in 1989	0.01	0.01	0.01	0.01	0.01	0.00
Received Training in 1990	0.00	0.01	0.00	0.00	0.01	0.01
Received Training in 1991	0.02	0.02	0.02	0.01	0.01	0.03
Received Training in 1992	0.01	0.01	0.00	0.00	0.00	0.00
Received Training in 1993	0.03	0.02	0.01	0.02	0.00	0.01
Received Training in 1994	0.01	0.01	0.01	0.01	0.01	0.01
Time in Training (in weeks)	83.11	85.81	84.16	68.04	52.43	98.38
Ever Received Training	0.19	0.16	0.12	0.14	0.09	0.13
AFQT Variables:						
Word Knowledge	1.59	2.11	0.30	-5.10	-6.67	-5.64
Paragraph Comprehension	0.38	0.48	-0.15	-2.08	-2.71	-2.37
Arithmetic Reasoning	2.32	1.97	0.65	-3.33	-4.06	-4.35
Family Background Variables:						
Father's Education	10.43	10.97	9.89	7.19	7.32	6.27
Father's Education Missing	0.07	0.08	0.08	0.29	0.28	0.34
Mother's Education	10.50	10.81	10.09	9.77	10.18	9.15
Mother's Education Missing	0.05	0.05	0.05	0.10	0.07	0.13

Father's Occupation						
Missing/Not Employed	0.17	0.20	0.19	0.37	0.47	0.39
Professional, Technical	0.08	0.09	0.04	0.01	0.00	0.02
Managers	0.12	0.14	0.08	0.01	0.03	0.01
Sales Workers	0.05	0.05	0.06	0.00	0.00	0.00
Clerical Workers	0.05	0.03	0.05	0.00	0.03	0.05
Craftsmen & Foremen	0.22	0.20	0.22	0.18	0.13	0.16
Operatives	0.01	0.01	0.01	0.01	0.01	0.02
Laborers, except Farm	0.15	0.16	0.14	0.21	0.17	0.14
Farmers & Farm Managers	0.02	0.03	0.06	0.11	0.09	0.09
Farm Laborers & Foremen	0.04	0.01	0.04	0.00	0.01	0.02
Service Workers	0.03	0.02	0.05	0.02	0.01	0.02
Private Household Workers	0.05	0.05	0.08	0.05	0.05	0.08
Mother's Occupation						
Missing/Not Employed	0.43	0.46	0.42	0.46	0.51	0.46
Professional, Technical	0.05	0.07	0.04	0.04	0.05	0.04
Managers	0.03	0.02	0.04	0.00	0.00	0.01
Sales Workers	0.04	0.03	0.05	0.01	0.01	0.01
Clerical Workers	0.19	0.17	0.15	0.10	0.11	0.15
Craftsmen & Foremen	0.02	0.02	0.03	0.03	0.01	0.01
Operatives	0.00	0.00	0.00	0.00	0.00	0.00
Laborers, except Farm	0.11	0.09	0.10	0.15	0.10	0.10
Farmers & Farm Managers	0.01	0.01	0.00	0.00	0.00	0.01
Farm Laborers & Foremen	0.00	0.01	0.00	0.00	0.00	0.00
Service Workers	0.02	0.01	0.02	0.01	0.02	0.02
Private Household Workers	0.11	0.11	0.16	0.20	0.17	0.19
Self-Esteem Measures						
Worth = 0	0.47	0.37	0.45	0.48	0.47	0.44
Worth = 1	0.53	0.61	0.54	0.50	0.48	0.49
Worth = 2	0.01	0.02	0.01	0.02	0.04	0.04
Worth = 3	0.00	0.00	0.00	0.00	0.01	0.04
Goodqual = 0	0.36	0.31	0.35	0.45	0.41	0.43
Goodqual = 1	0.62	0.66	0.63	0.54	0.55	0.57
Goodqual = 2	0.01	0.02	0.02	0.01	0.03	0.00
Goodqual = 3	0.00	0.00	0.00	0.01	0.01	0.00
Failure = 0	0.00	0.00	0.00	0.01	0.02	0.02
Failure = 1	0.03	0.02	0.02	0.03	0.02	0.06

Failure = 2	0.45	0.49	0.47	0.49	0.53	0.59
Failure = 3	0.52	0.49	0.51	0.46	0.43	0.34
Capable = 0	0.37	0.30	0.38	0.40	0.38	0.26
Capable = 1	0.61	0.67	0.57	0.54	0.58	0.70
Capable = 2	0.02	0.02	0.04	0.04	0.03	0.03
Capable = 3	0.00	0.00	0.01	0.02	0.01	0.01
Proud = 0	0.00	0.01	0.00	0.03	0.04	0.03
Proud = 1	0.04	0.06	0.06	0.07	0.12	0.13
Proud = 2	0.48	0.51	0.50	0.48	0.47	0.51
Proud = 3	0.48	0.43	0.44	0.43	0.38	0.33
Posatt = 0	0.32	0.30	0.35	0.47	0.37	0.41
Posatt = 1	0.63	0.64	0.56	0.45	0.52	0.49
Posatt = 2	0.04	0.06	0.07	0.06	0.10	0.10
Posatt = 3	0.01	0.00	0.02	0.02	0.01	0.01
Satmyself = 0	0.28	0.25	0.27	0.38	0.31	0.26
Satmyself = 1	0.65	0.66	0.67	0.56	0.59	0.68
Satmyself = 2	0.06	0.07	0.05	0.03	0.09	0.04
Satmyself = 3	0.01	0.02	0.01	0.02	0.02	0.01
Selfrespect = 0	0.05	0.03	0.04	0.06	0.07	0.04
Selfrespect = 1	0.32	0.36	0.34	0.41	0.39	0.48
Selfrespect = 2	0.48	0.48	0.47	0.39	0.41	0.31
Selfrespect = 3	0.16	0.13	0.15	0.15	0.13	0.17
Useless = 0	0.02	0.03	0.03	0.01	0.04	0.02
Useless = 1	0.39	0.36	0.39	0.28	0.29	0.38
Useless = 2	0.42	0.47	0.44	0.49	0.46	0.51
Useless = 3	0.17	0.14	0.14	0.22	0.20	0.10
Nogood = 0	0.02	0.01	0.02	0.00	0.04	0.01
Nogood = 1	0.21	0.21	0.23	0.11	0.09	0.07
Nogood = 2	0.45	0.46	0.41	0.44	0.49	0.62
Nogood = 3	0.33	0.33	0.33	0.44	0.38	0.31
Sample Size	7715	3518	1569	1789	2500	668

ENDNOTES

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[1] Wilson and Oswald (2005) report that the same is true for the literature relating marital status to health.

[2] There are a number of other explanations for a male marital wage differential. For example, married men may choose different types of jobs, jobs that offer a compensating wage differential, as compared with unmarried men (Reed and Harford 1989), the wife may positively affect the husband's productivity by serving as a sounding board or other job aid (Loh 1996), or employers may simply discriminate in favor of married men (Hill 1979).

[3] Similar differences are evident for blacks and Hispanics, though only the difference between married and never married men is statistically significant for nonwhites.

[4] We exclude the NLSY's original military sample as there is an explicit marriage premium in the military. Doing this reduces the sample by 1,280. Along with a stratified random sample, the NLSY 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 samples.

[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.

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

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

[8] Full results are available from the authors upon request. All standard errors are robust and corrected for clustering at the individual level.

[9] In other specifications not reported here, we included a quadratic in years married but this variable was not typically statistically significant at even the 10% level.

[10] We also estimated models for a sample of 928 observations on 177 Hispanic men. The results of the Gross specification indicate an insignificant 7% marital wage differential with substantially and significantly higher wage growth during marriage. Jointly marital status remains a significant determinant of wages in cross-section analysis, with much of the effect coming through higher wage growth during marriage. The small sample size makes it difficult to identify the impact of particular variables.

[11] Following Krashinsky (2004), we compared the wages of men who married in a given year with the wages of men who were not married in that year. The wages of the men who married

were higher than the wages of the men who did not marry even several years before the marriage date, confirming that these results are indeed likely attributable to selection.

[12] For both white men and African American men, the estimate of the marital wage differential was a bit smaller when using the more recent self-esteem measures but not significantly different. However, self-esteem as measured in 1987 is found to be significantly correlated with earnings for African American men whereas the 1980 measure is not.

[13] Mamun (2004) controls for age rather than job experience or tenure and continues to find a faster rate of wage growth following marriage for African American men – but age is a poor proxy for experience. As shown here, unmarried men have less experience and tenure than married men of the same age. When we mimic Mamun's specification we also find a faster rate of wage growth for married African American men.