Do Marital Status and Computer Usage Really Change the Wage Structure?

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January 31, 2002

Abstract

Both marital status and computer usage on the job have been found to increase earnings by as much as two additional years of schooling. If correct, these findings suggest that factors other than long-term human capital are key determinants of earnings. This analysis uses several identification strategies and data sources to determine the causal effect of marital status and computer usage on wages. First, analyzing commonly used data sets such as the Current Population Survey and the National Longitudinal Survey of Youth, I find that there are large cross-sectional effects of marital status and computer usage on wages. However, identifying these effects using changes in marital status and including a fixed-effect with computer usage, it is found that these variables have only a small effect on wages. Second, I identify the causal effect of marital status and computer usage on wages using within-twin contrasts in these variables to control for the effect of non-genetic or family background factors on earnings differences. The results also indicate that marital status and computer usage are not important causal determinants of earnings, even after adjustments are made for measurement error and within-twin differences in ability.

*I would like to thank Orley Ashenfelter and Cecilia Rouse for providing me with both data for this analysis and helpful comments and suggestions. I would also like to thank David Card and Henry Farber for their advice, and I thank participants of the Princeton Labor seminar series for their comments. I gratefully acknowledge financial support from the Alfred P. Sloan Foundation. All errors are my own.

1. Introduction

Although schooling and experience are now well established as key determinants of worker earnings,[1] considerable statistical evidence has begun to suggest that other factors are equally influential determinants of earnings. Both marital status[2] and computer usage[3] are typically found to increase earnings by as much as 20 percent or more for males. Taken at face value, these estimates imply that marriage alone, or learning to use a computer, will raise earnings more than two additional years of schooling. If correct, these findings suggest that factors other than long term human capital investments are key determinants of earnings. Perhaps because of their size, some researchers have begun to question whether factors such as computer usage and marital status are really causal determinants of earnings, or whether they instead reflect omitted variables, such as intelligence, appearance, or family background, that happen to be correlated with earnings.

In this paper, I use several identification strategies and data sources to ascertain the extent to which marital status and computer use are causal determinants of earnings. I first show that in commonly used data sets (the Current Population Survey (CPS) and the National Longitudinal Survey of Youth (NLSY)) there is a substantial effect of computer use and marital status on wages. Identifying the causal effect of these variables using changes in marital status and a fixed-effect with computer usage dramatically reduces the effects of these variables. The results confirm that ability bias greatly contributes to the wage premium associated with being a married male or using a computer at work. In a second approach, I

[1] Early work by Mincer (1958), Ben-Porath (1967) and Griliches (1970) outlines the importance of human capital as a determinant of earnings has been expanded upon by many authors using both within-sibling comparisons and instrumental variables methods in recent years. For surveys of this work, see Ashenfelter, Harmon, and Oosterbeek (1999), Card (1999), and Angrist and Krueger (1999).

[2] Studies by Korenman and Neumark (1991) and Loh (1996) consider the strong significance of this variable and also contain surveys of prior work on this subject.

[3] See especially Krueger (1993), but also: Boozer, Krueger and Wolkon (1992); Autor, Katz and Krueger (1998).

identify the causal effect using within-twin contrasts in these variables to control for the effect of non-genetic or family background factors on earnings differences. The empirical results also indicate that marital status and computer usage are not important causal determinants of earnings, even after adjustments for measurement error, and within-twin differences in unobserved ability.

The remainder of the paper is structured as follows: The next section presents a brief literature review of prior work on marital status and computer usage, outlining the debate over ability bias and its effects on the marital and computer use premia. The third section presents results from both a cross-sectional and longitudinal analysis of data from the CPS and NLSY. The fourth section discusses the data set of twins and its limitations, provides an explanation of the estimation procedure and reports the main empirical results from the twins data set. The fifth section highlights the importance of accounting for measurement error in the estimation, provides estimates of reliability for schooling, job tenure, marital status, computer usage, and union status, presents the measurement-error corrected results and considers the effects of within-twin differences in ability on the results. The sixth section concludes the paper.

2. Literature Review

There is a remarkable similarity in the debates on the effect of marital status and computer usage on wages for men. In both cases, positive results have been challenged by arguments suggesting that the variable in question might be correlated with ability, so that the measured result might be due to this correlation, and not to any true causation. For example, many studies have established the strong positive significance of marital indicator for males, yet its interpretation is still contested. One of the first theories accounting for this result was posited by Becker (1973, 1981), who argued that married life naturally leads to the specialization of tasks performed by both partners, which, in turn, results in increased productivity for the married partner working in the labor market. While some authors have provided support for this theory (Greenhalgh (1980), Kenny (1983), Korenman and Neumark (1991), Loh(1996)), it has also been argued that the evidence is consistent with an alternative explanation. Specifically, other authors (Hill (1979), Bartlett and Callahan (1984)) have argued that married males receive preferred treatment from their employers. This preferential status can manifest itself as a wage premium, even though married males may not be benefitting from their specialized roles. It can also be argued that the significance of the married male indicator variable represents a selection effect. It may be the case that married men were, on average, already more productive before they became married, and because of that were more likely to become married. As such, the significance of the married male indicator variable is nothing more than an identification of this prior productivity (Keeley (1977), Nakosteen and Zimmer (1987, 1997)).

Recently, some studies have sought to determine the effect of computers on productivity. Some studies (Boozer, Krueger and Wolkon (1992), Krueger (1993), Autor, Katz and Krueger (1998)) have found that computers have had a highly significant effect on wages. But while the effect of computers on the wage structure and overall labor market are undeniable, others have argued (DiNardo and Pischke (1997)) that a selection effect is evident here, as well, contending that the use of computers is more likely at high paying jobs. As such, the significance of this variable may capture the fact that more productive workers are employed at higher paying jobs.

The debate surrounding the "true" effect of these variables has persisted because different studies which have used a fixed-effect to control for ability have arrived at very different conclusions over the causal nature of these regressors. Some (Loh (1996) Korenman and Neumark (1991)) have found a significant marital effect in this framework while others (Nakosteen and Zimmer (1987, 1997) have not. A similar debate has occurred over the effect of computer usage – a fixed effect approach has yielded significant (Boozer, Krueger and Wolkon (1992)) and insignificant results (DiNardo and Pischke (1997)). To determine whether or not unobserved individual ability figures into the analysis of either of these variables, I will use multiple data sets to account for this bias and provide new estimates of the effect of all these standard explanatory variables. It will be shown that the results are remarkably consistent (despite the use of different data sources and econometric approaches), and suggest a strong ability bias for both of these regressors.

3. Longitudinal Analysis

Relying upon longitudinal data is a natural approach to determining the effect of ability bias on the wage premia accruing from being a married male or using a computer at work. Such data may be used to account for ability bias using different estimation techniques (such as a fixed-effect framework). To that end, I will consider findings from alternative surveys to examine the causal relationship between marital status and computer usage on wages; specifically, I will use matched outgoing rotation groups from the Current Population Survey (CPS) and the National Longitudinal Survey of Youth (NLSY). These data sets will demonstrate that there is strong evidence suggesting that ability bias effects the wage premium for married males and for computer use at work.

3.1. Results from the CPS

To consider the effect of marriage on wages received by males, a data set was composed of matched outgoing rotation groups from the Current Population Survey.[4] These data are advantageous in many ways for considering the effect of marital status for males. First, the two-year panels provide information on those individuals who become married and those who do not. Specifically, this data contains information on the wages received by those who become married before the marriage takes place, thus providing purchase into the question of what kind of males elect to become married. The evidence will demonstrate that males with significantly higher wages choose to become married, and the wage growth occurring as a result of marriage is not different than that exhibited by males who remain single.

Summary statistics of these data are displayed in Table 1, which compare married and single males, and those for whom marital status changes over time. The first two columns of the table illustrate the pronounced differences between married and single men. Married males are typically older, more educated and receive higher wages[5] than those men who are single. Columns three and four of Table 1 demonstrate that males who become married are more educated and have higher wages in the period before they become married. Combined with the fact that wage growth for men who become married is similar to that of men who remain single, this evidence suggests that the potential returns to the specialization of labor that result after entering into a marriage is not the main factor responsible for the cross-sectional wage premium associated with being a married male. Instead, this suggests

[4] The Current Population Survey (CPS) interviews households over a sixteen month period to obtain data. The timing of the interviews occurs as follows: a household will be interviewed for the first four consecutive months, then it will be left out of the sample for the next eight months, and for the final four months, the household will be interviewed again. The interviews obtained in the months before the household leaves the sample (the fourth and sixteenth months) are considered to be the "outgoing rotation groups".

[5] The measure of wages used is "usual weekly earnings", and these earnings are adjusted to 1992 levels using the CPI.

that more able men are more likely to become married, and this higher ability is the cause of the marriage premium.

This hypothesis is tested in a regression context, and the results are displayed in Tables 2 and 3. Table 2 shows that the marital premium for males is evident in the matched CPS data, even with various controls for human capital. But, Table 3 demonstrates that there is a 9% wage premium associated with being a male who will become married – this is very close to the 11% cross-sectional wage premium for being a married male. To test the hypothesis that the act of becoming married boosts a male's wages because he can greater specialize in labor market work, Table 4 compares the wage growth of males who become married to those who remain single. If the act of becoming married results in greater specialization, then it should be the case that the wage growth of males who become married should be higher than that of males who remain single. The results in Table 4 demonstrate that there are no differences in the wage growth.

These results suggest that males who become married are significantly different from those who remain single, and that these differences are the primary cause for the cross-sectional wage premium for married males. The fact that most of the marital premium is accounted for by a pre-existing cross-sectional wage differences (as seen in Table 3) suggests that more able males select into marriage, and the higher wages received by these men results not from the advantages of specialization that accrues to them from the marital status. Instead, it their higher ability that causes them to receive higher wages.

3.2. Results from the NLSY

The National Longitudinal Survey of Youth (NLSY) is a panel data set that tracks youths aged 14-22 in 1979. This data set is ideal for tracking males who eventually become married; the modal age of first marriage for males in the United States is between 25 and 27 years of age[6], so NLSY respondents will be closely examined during this time frame. Table 5 shows the characteristics of males who are married as of 1994[7], and they tend to be higher-educated and exhibit greater wage growth than males who do not become married. Further evidence on ability bias and the marriage premium for males can be derived from test scores of respondents in this data set. In 1979 and 1980, respondents in the data set were attributed Armed Forces Qualifying Test (AFQT) scores after completing a battery of tests.[8] These scores are a proxy for an individual's level of ability, given his or her age and level of education at the time of writing the test, so a natural question to consider is whether or not respondents with higher AFQT scores ultimately become married. Table 5 shows that the AFQT scores for married males are nearly ten points higher than those of single males, suggesting that more "able" men become married. This is confirmed within a regression context, as seen in the first two columns of Table 6, which show that men with higher AFQT scores are significantly more likely to be married in 1994.

One complication of this analysis is that the AFQT scores were assembled from tests given to all respondents, regardless of their age or education. It may thus be the case that the difference in AFQT scores between married and single men is due to the different levels of education they had at the time of writing the test. To account for this potential problem, "adjusted" AFQT scores were computed – to calculate these adjusted

[6] U.S. Bureau of Census, *Marital Status and Living Arrangements*, 1991-1995.

[7] Although the panel continues collecting data in 1996, 1998, and 2000, 1994 is the last year in which the panel collects data every year. Both this fact and the fact that it coincides with the period when the data set of twins was collected makes it a useful year to consider these sample means.

[8] The AFQT was compiled from scores on the Armed Service Vocational Aptitude Battery test. This test examines respondents in ten areas: general science, arithmetic reasoning, word knowledge, paragraph comprehension, numerical operations, coding speed, auto and shop information, mathematics knowledge, mechanical comprehension, and electronics information. The ASVAB was administered to over 94% of all respondents.

AFQT scores were compiled from ASVAB scores. Specifically, AFQT is the sum of the number of correct scores in the sections for arithmetic reasoning, word knowledge, and paragraph comprehension, plus half of the correct scores from the section on numerical operations.

scores, a respondent's AFQT scores was regressed on his educational attainment and age in the year in which the test was administered. The residual of this regression was then used as the "adjusted" score (the part of the score not accounted for by education or age), and the analysis was conducted again. The results are displayed in columns three and four of Table 6, which demonstrate that higher adjusted AFQT scores make a male more likely to be married as of 1994. To consider whether or not more able males are more likely to become married, regressions similar to those run with matched CPS data are re-run with NLSY data. Table 7 demonstrates that the results are remarkably similar in both data sets. Specifically, the first column of Table 7 shows that married males earn roughly 14% more than their unmarried counterparts, but column 2 of this table shows that in the period before he becomes married, a male respondent exhibits roughly 13% higher wages than those who will not become married.

These results are useful for considering a hypothesis advanced by Korenman and Neumark (1991), who suggest that the marriage premium accrues to men slowly over the course of the marriage. Using the National Longitudinal Survey of Young Men, they find that the years a male has been married makes a significant contribution to the wage he receives. Their regressions have been replicated in Table 8, which shows that the marital indicator is not robust to a fixed-effect specification, but the variable accounting for the total number of years married remains significant within this framework. They conclude from this finding that the marital premium for males is something that accrues to them slowly over time. But it could also be the case that males who become married are already on steeper earnings paths before they become married.[9] This would lead to a significant effect for total

[9] It could also be the case that more able men are more likely to be married for a longer period of time. A regression of the total number of years a respondent has been married on his AFQT score yields a coefficient of 0.032 with a t-statistic of 6.6. Including controls for education, experience and its sqaure, and a race indicator yields a coefficient of 0.028 with a t-value of 4.4. This suggests that this variable could also be

years married variable in the fixed effect framework (because this higher wage growth would not be captured by the fixed-effect specification) that was due to ability bias. To test this possibility, the following wage growth regression was estimated:

$$\Delta wage_{it} = \beta_1 * married_{it} + \beta_2 * Ever\ Married_i + \gamma X_{it} + \varepsilon_{it}$$

where $married_{it}$ is a marital indicator variable for respondent i in period t, $Ever\ Married_i$ is equal to one if the respondent is married at any point in the panel (and zero otherwise), X_{it} are "typical" observable variables such as education or experience, and ε_{it} is a residual. If it is the case that marital status independently leads to higher wage growth, then β_1 should be significantly positive in this specification. The first column of Table 9 shows that married males do exhibit higher wage growth than unmarried males, but the second column demonstrates that this is true even in the periods when these men are not married. Furthermore, the estimation of the full model demonstrates that β_1 insignificant and β_2 is significant, illustrating that males who become married already have higher wage growth than their counterparts, and the institution of marriage does little to contribute to the future wage growth.

The findings from the CPS and the NLSY suggest that there are strong ability biases which affect the marital premium for men. Results from these data sets have established that men who become married have higher wages and higher wage growth than their unmarried counterparts. Overall, these results provide further evidence of the fact that the marriage premium for males is highly influenced by ability bias; that is, more able men seem more likely to marry than those who do not, so the marriage "premium" these married men exhibit is a function of their ability, not the benefits accruing from their specialization of

affected by ability bias.

labor market production.

To consider the significance of the computer use variable, the NLSY provides information in 1993 on the use of a computer at home to complete work related to a respondent's job. Although this is not the same as using a computer at work, the CPS asks the same question of its respondents in the 1993 October supplement to the survey. The results in Table 10 show the strong correlation between both AFQT[10] and education with the use of a computer at home to perform job-related work, suggesting that there may be inherent differences in the types of workers who do and do not use computers. To consider this issue in a regression context, Table 11 shows a comparison of the premium associated with using a computer at home to do job-related work for respondents in the CPS and NLSY. The similarity of these coefficients suggests that findings from the NLSY are not specific only to that data set, and the magnitude of the coefficients are quite close to those calculated for the use of a computer at work. Furthermore, because the NLSY is a panel data set, person-specific effects[11] can be incorporated into the wage equation to control for the effects of ability. The inclusion of these controls causes a strong decrease in the wage premium associated with using a computer at home to complete job-related work, and the new wage premium is extremely similar to that calculated with the fixed-effect approach for the data set of twins. This result suggests that the ability bias identified by the data set of twins is not specific to that data set – this result is corroborated by the NLSY.

[10] This finding is also evident for adjusted AFQT scores.

[11] To compute these person-specific effects, wage regressions were calculated for the years prior to 1992 in which the respondent worked full-time and received at least $2 per hour at his or her job. Using these data, a person-specific fixed effect was calculated, and then included in the 1993 cross-sectional regression which calculated the wage premium associated with using a computer at home to complete job-related work.

4. Twins Analysis

Another approach to considering the effect of ability bias on the marital and computer usage wage premium relies upon the use of a data set of identical twins. The advantage provided by such data is that specific assumptions can be about the unobserved component of ability for each twin. In particular, if it is assumed that this unobserved component is equal for both twins, then the difference in earnings between a married twin and his unmarried sibling will be attributed to the causal effect of marital status on earnings. Similarly, the difference in earnings between a twin who uses a computer at work and his or her sibling who does not use a computer at work will yield the causal effect of computer usage on earnings.

4.1. The Twins Data

The first data set used in this analysis is a data set of identical twins which was collected during the summers of 1991, 1992, 1993 and 1995 at the Twinsburg Twins Festival in Twinsburg, Ohio, and the interview questions were modeled after those in the Census and CPS instruments. Some of the data from the first three waves of this survey were used by Ashenfelter and Krueger (1994) and Ashenfelter and Rouse (1998), who provide a discussion of the procedures used to collect this data. Some additional questions were specifically designed for interviewing twins.[12] The data used in this study is drawn from the sub-sample of identical white twins[13], both of who have worked within two years prior to the interview and are living within the United States. For those twins interviewed more than once, their

[12] One such question was the twin's report of his or her sibling's educational attainment. This report will be used as an instrumental variable to account for the effect of measurement error on the return to education.

[13] The sample of white twins was selected to avoid convoluting the analysis with the anomolous sub-sample of black twins. Ashenfelter and Rouse (1998) document the fact that the coefficient on a indicator variable for black twins is positive in a regression on the pooled sample of twins, suggesting that the black sub-sample of twins may not be representative of the general population.

responses were averaged over the years in which they were interviewed.

Table 12 displays the characteristics of the twins sample, and compares it to other salient aspects of white workers from reweighted[14] CPS supplements. The data set composed of identical twins is generally similar to those of the reweighted CPS samples, with some differences evident in characteristics like wages or education. To determine what effects these differences may have on a statistical analysis of the twins data set, Table 13 reports the estimates of earnings equations using the identical twins data set and the CPS. The results in this table demonstrate that the coefficient estimates for the identical twins sample are reasonably close to those from the CPS – every coefficient in the twins sample is of the same sign as those from the CPS samples, and all but the union status coefficient are of a larger magnitude. This suggests the possibility that the CPS data contain more measurement error than the twins data, but generally, the results from Table 13 indicate that the two data sets provide similar basic information.

4.2. Estimation Procedure

One useful procedure for providing evidence on the magnitude of ability bias contrasts twins who share identical genes and also have very similar family backgrounds. Assuming that ability has a linear effect on earnings, the earnings equations for each twin can be expressed as follows:

$$y_{1j} = \beta'_{1j} X_{1j} + \alpha' Z_j + A_j + \varepsilon_{1j} \tag{4.1}$$

$$y_{2j} = \beta'_{2j} X_{2j} + \alpha' Z_j + A_j + \varepsilon_{2j}$$

[14] The CPS data was re-weighted to become more comparable to the twins sample. Specifically, the CPS supplements were re-weighted on the basis of where the respondent lived, their marital status, age, education and wage.

where X_{ij} represents a vector of individual characteristics for twin i from family j, Z_j represents common characteristics for family j, A_j is a family-specific ability term and ε_{ij} is an individual-specific error term. The identifying assumption of the model assumes that the returns to individual characteristics X_{ij} are the same for both twins, and that ability is correlated between twins. Specifically, A_j is allowed to be correlated with X_{ij} as follows:

$$A_j = \gamma \left(\frac{X_{1j} + X_{2j}}{2} \right) + v_j \tag{4.2}$$

These assumptions lead to the reduced-form correlated random-effects model[15]:

$$\begin{aligned} y_{1j} &= \beta X_{1j} + \alpha Z_j + \gamma \left(\frac{X_{1j} + X_{2j}}{2} \right) + v_j + \varepsilon_{1j} \\ y_{2j} &= \beta X_{2j} + \alpha Z_j + \gamma \left(\frac{X_{1j} + X_{2j}}{2} \right) + v_j + \varepsilon_{2j} \end{aligned} \tag{4.3}$$

where γ represents the correlation between a family's ability level and each twin's individual characteristics. An attractive component of this model is that it provides estimates of both γ, the effect of family-related characteristics on wages, and β, the effect of individual-specific variables on earnings.

An alternative estimation procedure that accounts for familial ability bias is the fixed-effects model, which differences the two regressions used in the correlated random effects model. The resulting equation is:

$$(y_{1j} - y_{2j}) = \beta(X_{1j} - X_{2j}) + (\varepsilon_{1j} - \varepsilon_{2j}) \tag{4.4}$$

[15]See Chamberlain (1982). Due to inter-twin error correlation, a stacked GLS estimation procedure is used in this framework.

Although this model yields unbiased estimates that are not correlated with ability[16], it does not provide a direct estimate of the correlation between ability and individual variables, γ.

4.3. Empirical Results

The results in Table 14 demonstrate the effect of controlling for familial ability in an earnings equation. If familial ability had no effect on earnings, then results from the generalized least-squares estimation procedure displayed in the first column of this table would provide an unbiased estimator of the effect of the exogenous regressors. Further, the generalized least squares and correlated random effects estimator (in column two) would differ only because of sampling error. As is apparent from Table 14, however, the coefficients of marital status differ dramatically with the estimation procedure.[17] Without controls for ability, marital status raises wages by 20% for males. This estimate of the marital premium is consistent with the previous estimates reported in other studies, which typically range between 10% and 40%.[18] However, accounting for familial ability greatly reduces the significance and the magnitude of the marital-status indicator variable – the coefficient estimate is basically reduced to zero, with correspondingly small t-values[19]. Furthermore, the correlation between ability and the married male indicator across families is positive and significant, reinforcing the conclusion that the significance of the marital-status indica-

[16] Assuming that there are no within-differences in "ability" that are correlated with both earnings and the factors in question.

[17] The Hausman test statistics listed in Table 14 reject the hypothesis that similar estimates are generated by the GLS and Correlated Random Effects estimation procedures. Most of the difference in estimation results is due to a change in the marital status coefficent. There is also a strong change in the coefficient for the married female indicator, but this change is due to the reduced significance of being a married male. Regressions similar to those in Table 14 for a female sample show no significant changes in the marriage premium resulting from controlling for familial ability.

[18] See Korenman and Neumark (1991) or Loh (1996) for a survey of this literature.

[19] These results differ from those of Loh (1996), who does not find that familial controls significantly change the marital status coefficient for males.

tor variable is subject to ability bias. These results are corroborated by the within-twin estimates displayed in column 3.

These results provide a direct test of the competing theories for the significance of the marital premium for male workers: if the marriage institution itself improves the productivity of a married male, then controlling for ability should have no effect on this coefficient's estimate. Similarly, if it is the case that married males are the recipients of employer favoritism or if employers discriminate against non-married males, then the addition of ability controls also should not affect the significance of the married-male indicator variable. The marked decrease in the coefficient's point estimate and significance is evidence against both of these theories, suggesting that a selection effect can account for this significance. Specifically, these results imply that married males are more productive than unmarried males for reasons that are independent of an institutional explanation; in all likelihood, these males were more productive before they became married.

Another result that is apparent from Table 14 is that little of the effect of educational attainment, tenure and union coverage variables can be attributed to correlation with familial ability. The coefficient estimates for these variables remain virtually unchanged after the inclusion of familial ability controls, and all of estimates of the correlation between these variables and ability are statistically insignificant. However, some caveats must accompany these results. Some studies[20] have established the importance of match quality between a worker and a firm to the return to tenure. To account for this dimension of the analysis, it can be assumed that each twin pair have roughly the same match quality at their jobs. The limitations to the analysis of the union variable are due to the large standard errors associated with this variable's coefficient. Greater precision would be necessary before a

[20] See especially Abraham and Farber (1987).

proper assessment could be made of the effects of familial controls on this variable.

The results in Table 15 provide estimates of an earnings equation that includes an indicator variable for whether or not a worker uses a computer at his or her job. The introduction of this variable into the regression does not significantly change any of the results found in Table 14. And like the change in the marital-status indicator variable seen in Table 14, the strong significance of the computer-at-work variable under the GLS estimation procedure in the first column of Table 15 is not evident once inter-twin ability controls are introduced into the analysis. Both the fixed-effect and correlated random effects estimation procedures yield estimates of this variable that are not only significantly different from those calculated without controls for ability, but also results that are not themselves statistically significant. These results, combined with the positive and statistically significant correlation of familial ability with computer usage at work, lend support to the argument that computers in the workplace do not themselves create a wage premium for a given worker; instead, the results indicate that more able workers tend to work at jobs which require the use of a computer.

5. Measurement Error

The particular importance of accounting for measurement error in this econometric framework has been discussed by many authors[21]. In principle, measurement error can have substantial effects on all the regressors in a model, and careful inference requires that the model be modified to allow for measurement error in all of the explanatory variables. Specifically, if all of the observed explanatory variables, X, are measured with error, U, then

[21] For example, Ashenfelter and Krueger (1994) and Griliches (1979) both discuss the importance of measurement error in the school regressor. See also Deaton (1985) and Fuller (1975).

the regressors of the model may be expressed as follows:

$$X_{1j} = X_{1j}^* + U_{1j} \text{ and } X_{2j} = X_{2j}^* + U_{2j}$$

where X_{ij}^* is the unobserved true value of X_{ij} that is unaffected by measurement error. Furthermore, the within-twin differenced variables can be expressed in a similar manner:

$$\begin{aligned} \Delta X_j &= X_{1j} - X_{2j} \\ &= \Delta X_j^* + \Delta U_j \end{aligned}$$

Assuming that the measurement error for the one twin is uncorrelated with the true value of X for the other twin, and assuming that any measurement error in the earnings variable is uncorrelated with both the independent variables and the measurement error associated with these variables, then the measurement-error-corrected GLS estimator ($\widetilde{b}_{GLS}$) and correlated random effects (CRE) estimator ($\widetilde{b}_{CRE}$) are:

$$\widetilde{b}_{GLS} = \widetilde{b}_{CRE} = \left[X'X - n\Sigma\right]^{-1} X'y$$

where: X and y are the matrices formed by the components X_{ij} and y_{ij}, respectively; n is the sample size; and Σ is the variance-covariance matrix of the measurement error matrix, U. The measurement-error-corrected fixed-effects estimator uses a similar formula:

$$\widetilde{b}_{FE} = \left[\Delta X'\Delta X - 2n\Sigma\right]^{-1} \Delta X'\Delta y_j$$

where ΔX and Δy are the respective within-twin-differenced analogs of X and y. Accounting for the matrix Σ is thus important for providing unbiased coefficient estimates, and it is also necessary to properly compute the standard errors of the regression coefficients.[22]

5.1. Reliability Estimates

To account for the possibility of measurement error in the twins data, I constructed the matrix of measurement error variances and covariances, Σ, from various studies that have examined the level of measurement error in particular variables. The matrix is displayed in Table 16. To derive the measurement error for the indicator variables in this matrix (marital and union status), I relied upon misclassifications detected in reinterviews of survey respondents. For example, an estimate of the reporting error for marital status is taken from a reinterview of Census respondents in the CPS in 1970. The cross-tabulation with an individual's reported Census status and their reported CPS status is replicated in Appendix Tables 1a and 1b. Unfortunately, it is impossible to know how the mismatches in records are generated – the both the Census and CPS records could be subject to reporting error. In general, the misclassification error, $u_{married}$, could take on three possible values:

$$\begin{aligned} &= 1 \text{ if the respondent is improperly classified as being married} \\ u_{married} &= 0 \text{ if there is no misclassification error} \\ &= -1 \text{ if the respondent is improperly classified as being non-married} \end{aligned}$$

Generally, it is not possible to know whether or not a discrepancy between the CPS and Census records is caused by an error of value 1 or -1. As such, I estimate the measurement

[22] Appendix 1 contains measurement-error corrected standard error formulas.

error variance in the case where both records are measured with error, and that the misclassification rates for both records are equal.[23] This approach yielded a measurement error variance of 0.01 for both marital status and female marital status. This approach is equally valid with the union status variable, and using results in Appendix Table 1c from Freeman's (1984) work with the matched May 1979 "Dual Job" and "Pension" supplements of the CPS (both of which asked respondents if their jobs were covered by a collective bargaining agreement) a measurement error variance of 0.013 was calculated for union status.

To obtain estimates of the reporting error for years of tenure, Duncan's and Hill's (1985) cross-tabulations of employee survey data and employer records collected from a large manufacturing firm are used. They report that the ratio of the measurement error variance to the true variance for years of tenure is 0.011, and the variance of years of tenure is 78.76. Thus, if the true years of tenure are denoted as T^*, where $T = T^* + u_{tenure}$, then the variance of the response error on years of tenure, u_{tenure}, is 0.857.

Accounting for the measurement error in years of education can be accomplished by following Ashenfelter and Krueger's (1994) method of using the own-reported and sibling-reported measures of education in the data set of identical twins. Let the "true" level of education for twin i in family j be denoted as E^*_{ij}, and let both "own-reported" education for twin i in family j, E_{ij}, and the sibling-reported level of education for twin i in family j, $\widetilde{E}_{ij}$, be measured with error, so that:

$$E_{ij} = E^*_{ij} + \eta_{ij}$$

$$\widetilde{E}_{ij} = E^*_{ij} + \widetilde{\eta}_{ij}$$

[23]This method is patterned after Card's (1996) approach. Please refer to Appendix 2 for a detailed discussion of the model used to estimate the measurement error variances.

where it is assumed that: $E(\eta_{ij}) = E(\widetilde{\eta}_{ij}) = 0$ and $cov(\eta_{ij}, \widetilde{\eta}_{ij}) = 0$. This implies that:

$$cov(E_{ij}, \widetilde{E}_{ij}) = cov(E^*_{ij} + \eta_{ij}, E^*_{ij} + \widetilde{\eta}_{ij}) = V(E^*_{ij})$$

Then an estimate of the measurement error in years of education can be obtained from the following equation:

$$\begin{aligned} V(\eta_{ij}) = V(\widetilde{\eta}_{ij}) &= V(E_{ij}) - V(E^*_{ij}) \\ &= V(E_{ij}) - cov(E_{ij}, \widetilde{E}_{ij}) \\ &= 4.29 - 3.98 = 0.31 \end{aligned}$$

A similar approach was used to calculate the measurement error variance of the within-twin differences in education, ΔE.[24]

To accommodate measurement error in the dummy variable accounting for the use of computers at the respondent's job, it was not possible to consult prior validation studies,

[24] To calculate the measurement error variance for ΔE_{ij}, a procedure suggested by Ashenfelter and Krueger (1994) is employed. Specifically, because of the potential for correlation in the measurement error of a twin's own reported education and the error for the report of the other twin's education. That is, twins who tend to over-report their own education will also over-report their sibling's education. To demonstrate this point, let E^1_{1j} represent twin 1's report of his own educational attainment, and let E^2_{1j} denote his report of his sibling's educational attainment. Similarly, E^2_{2j} will represent twin 2's own-report of education, and E^1_{2j} will represent his report of his sibling's level of education. In this case, each variable E^k_{ij} is measured with error:

$$E^k_{ij} = E^{k*} + \eta^k_{ij}$$

Because of the potential for the correlation between η^1_{1j} and η^2_{1j}, the difference of one twin's reported levels of education for both siblings can understate the measurement error variance. Consider:

$$\begin{aligned} V(E^1_{1j} - E^2_{1j}) &= V(E^1_{1j}) + V(E^2_{1j}) - 2cov(E^1_{1j}, E^2_{1j}) \\ &= V(E^{1*} + \eta^1_{1j}) + V(E^{2*} + \eta^2_{1j}) - 2cov(E^{1*} + \eta^1_{1j}, E^{2*} + \eta^2_{1j}) \\ &= [V(E^{1*}) + V(E^{2*}) - 2(cov(E^{1*}, E^{2*})] + V(\eta^1_{1j}) + V(\eta^2_{1j}) - 2cov(\eta^1_{1j}, \eta^2_{1j}) \\ &= V(\Delta E^*) + V(\eta^1_{1j}) + V(\eta^2_{2j}) - 2cov(\eta^1_{1j}, \eta^2_{1j}) \end{aligned}$$

Assuming that $V(\eta^1_{1j}) = V(\eta^2_{2j})$, then using $2V(\eta^1_{1j})$ as an estimate for the measurement error will overstate the effect of the measurement error in this case. As such, the expression $E^1_{1j} - E^2_{2j}$ is used as the measure of the inter-twin differences in educational attainment.

because none exist to the best of my knowledge. As such, an array of measurement error variance in this variable was considered, ranging from 0.02 to 0.04. Though arbitrarily defined, these two bounds seem to represent a reasonable scope of the possible reporting error in this variable, because the measurement error variance of other indicator variables (such as marital status or union status) is in the low end of this range. Selecting an upper bound of 0.04 (an estimate of reporting error variance that is above that of any other indicator variables) allows for a conservative estimate of the effect of measurement error on the computer-use variable.

5.2. Measurement-error Corrected Results

Columns four through six of Tables 14 and 15 present regressions results that are corrected and not corrected for measurement error. Table 14 demonstrates that all of the results derived without a correction for measurement error are still evident after this correction is performed. The significance of the tenure and education variables are not diminished in the correlated random effect or fixed-effect framework, and the wage premium associated with being a married male disappears after familial controls are introduced into the regression, even after a measurement error correction is performed. Similarly, Table 15 demonstrates that these results are evident after the inclusion of a dummy variable for the usage of a computer at work, and the measurement-error-corrected estimates of the premium associated with using a computer are insignificant in the fixed-effect and correlated random effect framework. Furthermore, the positive and significant correlation between familial ability and both marital status for males and using a computer at work is still evident within then measurement-error-corrected framework.[25] These results demonstrate that the initial

[25] These results were calculated using a variance of measurement error of 0.02 for the computer use at work variable. When higher values are used, the results are not substantively different than those listed in

findings of this paper are robust to the possible effects of measurement error.

5.3. Within-Twin Differences in Ability

Another criticism that must be accounted for in this analysis is the possibility of within-twin differences in ability. Both Neumark (1999) and Solon and Bound (1999) outlined the potential biases that can affect within-twin estimates of the return to education, and these criticisms are equally valid for within-twin estimates of any other variable in the wage equation. If the individual-specific component of ability is denoted by the variable $\widehat{A}_{ij}$, then the wage equations for each twin can be written as:

$$y_{1j} = \beta X_{1j} + \alpha Z_j + \theta A_j + \phi \widehat{A}_{1j} + \varepsilon_{1j}$$

$$y_{2j} = \beta X_{2j} + \alpha Z_j + \theta A_j + \phi \widehat{A}_{2j} + \varepsilon_{2j}$$

In this case, the within-twin estimates of β derived from a regression of Δy_j on ΔX_j are not unbiased, because a within-twin estimator will not a fully remove the effects of ability:

$$(y_{1j} - y_{2j}) = \beta(X_{1j} - X_{2j}) + \phi(\widehat{A}_{1j} - \widehat{A}_{2j}) + (\varepsilon_{1j} - \varepsilon_{2j})$$

$$\Delta y_j = \beta \Delta X_j + \phi \Delta \widehat{A}_j + \Delta \varepsilon_j$$

Estimates from the differenced wage equation are biased by the correlation of $\Delta A'_j$ and ΔX_j:

$$b_{FE} = (\Delta X'_j \Delta X_j)^{-1} \Delta X'_j \Delta y_j$$

$$= \beta + \phi(\Delta X'_j \Delta X_j)^{-1} \Delta X'_j \Delta \widehat{A}_j$$

Table 15. Specifically, when a measurement error variance of 0.04 is used, the GLS coefficient is 0.223, with a standard error of 0.044, but the fixed-effect estimate of this coefficient is 0.073 with a standard error of 0.060. The Hausman statistic for the entire regression is 35.46, which has a p-value of less than 0.0001.

It has been suggested that there exists a positive correlation with A'_{ij} and education, tenure, computer use at work, and marital status for males. Thus, the row vector, $\Delta X'_j \Delta \widehat{A}_j$, would be expected to contain exclusively positive entries. It would also be expected that the more able twin would obtain higher average wages than his or her counterpart, suggesting that $\phi > 0$. Lastly, it is observed that the matrix $(\Delta X'_j \Delta X_j)^{-1}$ tends to have both large positive diagonal terms and generally few negative terms,[26] suggesting that the omission of $\Delta \widehat{A}_j$ from the regression would result in a possible upward bias in the estimation results, which leads both Bound and Solon and Neumark to suggest that the within-twin estimates are upper-bounds of the unbiased return to education – and this is equally valid for any other variable analyzed in the within-twin framework. Although this criticism may weaken conclusions drawn about the effect of ability controls on such variables as tenure and education, it also has strong implications for the married-male and computer-usage-at-work wage premia. If any biases due to differences in inter-twin ability cause an upward bias of the within-twin fixed-effect estimator, then the fixed-effect estimate is an upper-bound on the true value of the return to marriage for men and the use of a computer at work. Because the fixed-effect estimates of these variables are insignificant, both with and without a correction for measurement error, then this suggests that the "true" coefficients are insignificant (and possibly negative).

6. Conclusion

The purpose of this paper was to use several identification strategies with different sources of data to examine the causal effect of marital status for men and computer usage on wages. Using well-known data sources such as the CPS and NLSY, it was found that

[26] The exact values of the entries in this matrix are available from the author upon request.

both marital status and computer use have a large cross-sectional effect on wages. However, different estimation strategies also demonstrated that accounting for ability bias greatly reduced the effect of the these variables. Results from the CPS demonstrated that high-wage males are more likely to become married than those men who choose to remain single. In fact, relying upon data from matched outgoing group files, the results suggested that most of the marital premium for males is already evident in the period before they become married – males who will become married in the following year exhibit a 9% higher earnings than other men who choose to remain single, and the average wage differential between married and single men is only slightly larger at 11.5%. The matched data also demonstrated that the act of becoming married does little for wage growth. Evidence from the NLSY was consistent with these findings, and also served to show that not only high-wage males are more prone to marry, but high-wage-growth males are more likely to marry, too. This result accounts for the finding presented by Korenman and Neumark (1991) which shows that the total number of years a male has been married is significant.

The NLSY was also used to examine the wage premium associated with computer usage. Although the data set does not include information about using a computer at work, it does provide information about whether or not the respondent uses a computer at home to perform job-related work. Results from the data showed how individuals who use a computer at home to for work-related reasons are much more educated, have higher AFQT scores and earn more than their counterparts. When the returns to using a computer at home were considered in a regression context, they were found to be approximately 16%, roughly the same magnitude as the return to using a computer at work. However, in a fixed-effect framework, the same return was reduced to about 6%, a rate that is quite similar to that calculated using a fixed-effect approach with the data set of twins.

Using a data set of identical twins, a simple econometric model was used to control for familial ability and to determine its effect on estimates derived from typical earnings equations. Using this framework, it was established that the incorporation familial controls had a strong effect on the premia for being a married male or using a computer at work. Not only was the coefficient of the marital-status indicator variable significantly different after controls for familial ability were introduced into the regression, it also became statistically insignificant as a result of the inclusion of these controls. The inclusion of familial controls had a similarly dramatic effect on the return to using a computer at work, notably reducing the point estimate and significance of this coefficient. These results could be criticized, however, because they could be biased by the effects of measurement error or within-twin differences in ability. I have dealt with both of these criticisms to determine that they do not alter my main findings. First, a measurement-error correction was performed and the results were robust to this correction. Second, although within-twin differences in ability could weaken conclusions drawn about the effects of education and tenure on wages, these differences actually reinforce the findings on the wage premium associated with being a married male or using a computer at work.

Overall, the findings in this paper suggest that ability bias has a strong effect on the wage premia for married men and using a computer at work. Unlike institutional explanations offered to account for the significance of the marital status variable in an earnings equation, such as a theory that relies upon the institution of marriage itself (Becker, 1973, 1981; Greenhalgh, 1980; Kenny, 1983; Korenman and Neumark, 1991) or preferential treatment given to married men by employers (Hill, 1979; Bartlett and Callahan, 1984), these findings suggest that married males are always more productive than their unmarried counterparts. And, as with the married-male indicator, these results show that the significance of the return to using a computer at work is not derived from the increased productivity caused by the computer itself (as suggested by Boozer, Krueger and Wolkon, 1992; Krueger, 1993; Autor, Katz and Krueger, 1998); instead, this suggests that workers who use computers at work were already more productive than their counterparts.

7. Appendix 1

Measurement error biases the estimated standard errors of the regression coefficients. To correct for this bias, the following formula must be used:

$$\widetilde{V}(\widetilde{b}_{OLS}) = \left(\frac{1}{n}\right) \widetilde{\Omega}_1^{-1}[(n^{-1}X_j{}'X_j) * e_j'e_j + n^{-2}X_j{}'e_je_j'X_j] \ \ \widetilde{\Omega}_1^{-1}$$

$$\widetilde{V}(\widetilde{b}_{CRE}) = \left(\frac{1}{n}\right) \widetilde{\Omega}_1^{-1}[(n^{-1}X_j{}'\Phi X_j) + \Sigma\beta\beta'\Sigma] \ \ \widetilde{\Omega}_1^{-1}$$

$$\widetilde{V}(\widetilde{b}_{FE}) = \left(\frac{1}{n}\right) \widetilde{\Omega}_2^{-1}[(n^{-1}\Delta X_j{}'\Delta X_j) * \Delta e_j'\Delta e_j + n^{-2}\Delta X_j{}'\Delta e_j\Delta e_j'\Delta X_j] \ \ \widetilde{\Omega}_2^{-1}$$

where:

$$\widetilde{\Omega}_1^{-1} = n^{-1}X_j{}'X_j - \Sigma$$

$$e_j = y_j - X_j{}'\widetilde{b}_{GLS}$$

$$\widetilde{\Omega}_2^{-1} = n^{-1}\Delta X_j{}'\Delta X_j - 2\Sigma$$

$$\Delta e_j = \Delta y_j - \Delta X_j{}'\widetilde{b}_{FE}$$

$$\Phi = E(\varepsilon\varepsilon') = E[(e - \beta U)(e - \beta U)']$$

8. Appendix 2

The effect of measurement error in a dichotomous variable such as union status can be determined by using the following notation. Let U^* represent a respondent's true unionization status, and let U represent his observed union status. In this case,

$$U = U^* + \eta_{union}$$

where η_{union} can take a value of 1, 0 or -1. Furthermore, let the following probabilities be defined:

$$P(U^* = 1) = \Pi \text{ and } P(U^* = 0) = 1 - \Pi$$

$$P(U = 1|U^* = 1) = q_1$$

$$P(U = 1|U^* = 0) = q_0 \text{ where } q_0 < q_1$$

Now consider the cross-tabulations presented in Appendix Table 1C. If we assume that both CPS records are measured with error, and that both union and non-union workers have the same probability of misreporting their status (denoted by q, so that $q = q_0 = 1 - q_1$), then the cross-tabulations in Appendix Table 1C are a function of only two parameters, q and Π. Specifically, the probability of being in either off-diagonal cell is:

$$P(\mathrm{Re}\, cords\ are\ conflicting) = \Pi q(1-q) + (1-\Pi)(1-q)q$$

$$= q(1-q)$$

The probability of being in the upper-left cell (where both records indicate that the respondent is a union member) is:

$$P(Both\ records\ show\ union\ membership) = \Pi(1-q)^2 + (1-\Pi)q^2$$

The probability of being in the lower-right cell (where both records indicate that the respondent is not a union member) is:

$$P(Neither\ records\ show\ union\ membership) = (1-\Pi)(1-q)^2 + \Pi q^2$$

Using these three different predicted probabilities, this "symmetric measurement error" model has two parameters (Π and q) and three independent data points (the three independent elements of the cross-tabulation). Intuitively, this model seems to fit the data in Appendix Tables 1A-1C quite well, since all three tables have roughly equal numbers in their off-diagonal cells. Formally, the model can be fit to the data with minimum chi-squared methods and tested with a goodness-of-fit test. The best fit to Appendix Table 1A yields $q = 0.0066$ and $\Pi = 0.676$ with an associated test statistic of 0.00000062 (this statistic has one degree of freedom and a p-value of 0.9994). The best fit to Appendix Table 1B yields $q = 0.0071$ and $\Pi = 0.6412$ with an associated test statistic of 0.00000072 (with a p-value of 0.9979). The best fit to Appendix Table 1C yields $q = 0.0151$ and $\Pi = 0.2246$ with an associated test statistic of 0.000106 (with a p-value of 0.9918). In all three cases, then, the fit of the symmetric measurement error model is quite good, and can not be rejected by the data.

To calculate the measurement error variance for these dichotomous variables, it is necessary to account for the fact that a dichotomous random variable can not have classical measurement error[27]. Instead, this error must be correlated with an individual's "true" status. To illustrate, consider the unionization example. If $q = 0.0151$ and $\Pi = 0.2246$, then the covariance of the error term with true unionization status is:

$$
\begin{aligned}
cov(U^*, \eta_{union}) &= E(U^* \eta_{union}) - E(U^*)E(\eta_{union}) \\
&= E(U^* \eta_{union}) - \Pi(0) \\
&= \Pi(1-q)(1)(0) + \Pi q(1)(-1) + (1-\Pi)(1-q)(0)(0) + (1-\Pi)q(0)(1) \\
&= -\Pi q
\end{aligned}
$$

Thus, calculating the variance of the measurement error requires that this term be taken into account:

$$
\begin{aligned}
V(U) &= V(U^*) + 2cov(U^*, \eta_{union}) + V(\eta_{union}) \\
V(\eta_{union}) &= V(U) - V(U^*) - 2cov(U^*, \eta_{union}) \\
&= p(1-p) - \Pi(1-\Pi) - 2(-\Pi q) \\
&= p(1-p) + 2(\Pi q) - \Pi(1-\Pi)
\end{aligned}
$$

where p is the probability of being reported as unionized by the variable U, observed union

[27] That is, the measurement error can not be independent of the true (unobserved) variable in question. A formal discussion of this relationship can be found in Klepper (1988).

status. As such, the measurement error variance in the union status variable is:

$$
\begin{aligned}
V(\eta_{union}) &= (0.2354)(1-0.2354) + 2(0.2246)(0.0151) - (0.2246)(1-0.2246) \\
&= 0.1800 + 0.0068 - 0.1742 \\
&= 0.013
\end{aligned}
$$

This same formula can be applied to the marital status variable and the married female indicator variable. As such., the measurement error variances for those variables are:

$$
\begin{aligned}
V(\eta_{married}) &= (0.6736)(1-0.6736) + 2(0.676)(0.0066) - (0.676)(1-0.676) \\
&= 0.2199 + 0.0089 - 0.2190 \\
&= 0.01
\end{aligned}
$$

$$
\begin{aligned}
V(\eta_{married\ female}) &= (0.6388)(1-0.6388) + 2(0.6412)(0.0071) - (0.6412)(1-0.6412) \\
&= 0.2307 + 0.0091 - 0.2300 \\
&= 0.01
\end{aligned}
$$

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Table 1: Sample Means for Men from the Pooled Matched CPS Outgoing Rotation Groups, 1990-1995

	Married	Single	Become Married	Stay Single	Remain Married	Marriage Ends
Age	42.53 (10.04)	33.82 (11.08)	34.61 (9.62)	33.78 (11.16)	42.59 (10.03)	38.97 (10.45)
Education	13.43 (2.70)	13.13 (2.45)	13.37 (2.50)	13.12 (2.44)	13.43 (2.70)	13.08 (2.56)
White	0.899 (0.30)	0.850 (0.36)	0.883 (0.32)	0.848 (0.36)	0.899 (0.30)	0.870 (0.34)
Period 1 Real Log Weekly Earnings	6.34 (0.55)	5.92 (0.67)	6.17 (0.56)	5.91 (0.67)	6.35 (0.55)	6.18 (0.55)
Period 2 Real Log Weekly Earnings	6.36 (0.55)	5.97 (0.64)	6.21 (0.55)	5.95 (0.65)	6.36 (0.55)	6.21 (0.55)
Δ Real Log Weekly Earnings	0.011 (0.41)	0.046 (0.46)	0.047 (0.44)	0.046 (0.46)	0.011 (0.41)	0.025 (0.46)
N	75,360	31,908	1,723	30,185	74,264	1,096

Standard deviations are listed in parentheses. The sample consists of men between the ages of 18 and 65, who work full-time and earn real hourly wages of at least $2 per hour.

Table 2: Cross-Sectional Wage Regressions for Males, Using Matched Outgoing Rotation Groups from CPS Supplements, 1990-1995

Married	0.301 (0.004)	0.147 (0.004)	0.114 (0.003)
Education		0.095 (0.001)	0.068 (0.001)
Experience		0.036 (0.0005)	0.033 (0.0005)
$Exp^2/100$		-0.054 (0.001)	-0.050 (0.001)
White		0.162 (0.005)	0.118 (0.004)
Industry Dummies	No	No	Yes
Occupation Dummies	No	No	Yes
Year Dummies	No	No	Yes
R^2	0.060	0.316	0.393

White Standard Errors are listed in parentheses beneath the coefficient estimates. The dependent variable is the log of real hourly wages, deflated to 1992 dollars. When used in the regression, the specification included six industry indicator variables, four occupation indicator variables and five year indicator variables.

Table 3: Cross-Sectional Wage Regressions for Single Males, Using Matched CPS Outgoing Rotation Groups, 1990-1995

Will Become Married Next Year	0.147 (0.013)	0.115 (0.011)	0.088 (0.011)
Education		0.098 (0.001)	0.071 (0.002)
Experience		0.041 (0.001)	0.037 (0.001)
$Exp^2/100$		-0.058 (0.002)	-0.054 (0.002)
White		0.131 (0.008)	0.090 (0.007)
Industry Dummies	No	No	Yes
Occupation Dummies	No	No	Yes
Year Dummies	No	No	Yes
R^2	0.004	0.308	0.395

White Standard Errors are listed in parentheses beneath the coefficient estimates. The dependent variable is the log of real hourly wages, deflated to 1992 dollars. When used in the regression, the specification included six industry indicator variables, four occupation indicator variables and five year indicator variables. The variable "Will Become Married Next Year" is equal to one if the respondent becomes married in the following period, and zero if he does not.

Table 4: Relative Wage Growth for Single Males,
Using Matched CPS Outgoing Rotation Groups, 1990-1995

Will Become Married Next Year	0.002 (0.011)	-0.0003 (0.011)	0.001 (0.011)
Education		0.002 (0.001)	0.005 (0.002)
Experience		0.0002 (0.001)	0.0004 (0.001)
$Exp^2/100$		-0.002 (0.002)	-0.002 (0.002)
White		0.018 (0.008)	0.021 (0.008)
Industry Dummies	No	No	Yes
Occupation Dummies	No	No	Yes
Year Dummies	No	No	Yes
R^2	0.000	0.001	0.002

White Standard Errors are listed in parentheses beneath the coefficient estimates. The dependent variable is the change in log of real hourly wages, deflated to 1992 dollars. When used in the regression, the specification included six industry indicator variables, four occupation indicator variables and five year indicator variables. The variable "Will Become Married Next Year " is equal to one if the respondent becomes married in the following period, and zero if he does not.

Table 5: Sample Means of Married and Single Males
In the NLSY as of 1994

	Marital Status	
Variable	Married	Single
Education	13.34 (2.68)	12.65 (2.52)
Age	33.47 (2.21)	32.93 (2.14)
AFQT Score	73.8 (20.98)	64.4 (22.34)
Not Hispanic or Black	0.87 (0.34)	0.73 (0.44)
Real Log Hourly Wage	2.11 (0.54)	1.83 (0.57)
N	1542	1102

Standard deviations are listed in parentheses. The sample was drawn from the 1994 wave of the NLSY respondents, who earn at least \$2 per hour and no more than \$100 per hour (in real 1982 dollars).

Table 6: Effect of AFQT on Marital Status in 1994, For Males in the NLSY

	Normalized Probit Coefficients			
AFQT	0.004 (7.58)	0.002 (2.90)		
Adjusted AFQT			0.003 (5.48)	0.002 (2.29)
Education		0.032 (3.84)		0.040 (6.16)
Experience		0.024 (1.11)		0.045 (2.08)
$Exp^2/100$		0.005 (0.07)		-0.058 (0.78)
Not Hispanic or Black		0.151 (4.75)		0.157 (5.19)
N	2,046	2,046	2,046	2,046

t-statistics are listed in parentheses beneath the coefficient estimates. The sample is drawn from the 1994 wave of the NLSY, and the dependent variable is the respondent's marital status in 1994 (it is equal to one if he is married, and zero otherwise).

Table 7: The Effect of Marriage and Becoming Married Next Period on Wages for Males in the NLSY, 1979-1993

	GLS	GLS
Married	0.143 (0.006)	
Will Become Married Next Year		0.127 (0.012)
Education	0.092 (0.001)	0.084 (0.002)
Experience	0.073 (0.002)	0.062 (0.004)
$Experience^2/100$	-0.267 (0.012)	-0.229 (0.022)
R^2	0.341	0.288
N	28,089	9,013

Standard Errors are listed in parentheses beneath the coefficient estimates. The first column in this table compares married to single men, whereas the second column only uses a sample of single men -- those who will become married, and those who will remain single. The variable "Married" is equal to one if the respondent is married, and zero otherwise. The variable "Will Become Married Next Year" is equal to one if the respondent becomes married in the next period, and zero if he does not. Other covariates used in the regression include seven industry indicator variables, six occupation indicator variables, and fourteen dummy variables for each year in the sample.

Table 8: The Effect of "Years of Marriage" in the Wage Regressions For Males in the NLSY, 1990-1993

	GLS	GLS	Fixed-Effects
Married	0.163 (0.012)	0.065 (0.027)	-0.025 (0.026)
Years Married		0.020 (0.007)	0.021 (0.010)
Years Married^2/100		-0.058 (0.051)	-0.048 (0.062)
Divorced or Separated	0.051 (0.020)	-0.019 (0.039)	-0.104 (0.038)
Years Divorced or Separated		-0.002 (0.007)	0.014 (0.008)
R^2	0.308	0.319	0.201
N	5,168	5,168	5,168

Standard Errors are listed in parentheses beneath the coefficient estimates. Other covariates in the regression include variables for education, experience, experience squared, seven industry dummies, six occupation dummies, three year dummies and an indicator variable equal to one if the respondent is not black or Hispanic.

Table 9: Relative Wage-Growth Regressions
For Males who Become Married, Using NLSY, 1979-1993

Married	0.017 (0.007)		-0.002 (0.006)
Ever Married		0.019 (0.006)	0.020 (0.007)
Education	0.002 (0.001)	0.004 (0.002)	0.004 (0.002)
Experience	-0.014 (0.003)	-0.015 (0.003)	-0.015 (0.003)
$Exp^2/100$	0.048 (0.015)	0.057 (0.013)	0.056 (0.013)
Not Hispanic or Black	-0.005 (0.009)	-0.002 (0.007)	-0.002 (0.007)
Industry Dummies	Yes	Yes	Yes
Occupation Dummies	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
R^2	0.015	0.021	0.021
N	26,443	26,443	26,443

White Standard Errors are listed in parentheses beneath the coefficient estimates. The dependent variable in this regression is the log real wage of the next period minus the log real wage of the current period.

Table 10: Sample Means of NLSY Males Who Do and Do Not Use A Computer At Home to Complete Job-Related Work, in 1993

Variable	Computer Usage at Home Status	
	Use Computer At Home	Don't Use Computer At Home
Education	15.56 (2.34)	12.77 (2.45)
Age	32.62 (2.22)	32.18 (2.18)
AFQT Score	87.01 (12.20)	68.76 (21.61)
Not Hispanic or Black	0.89 (0.31)	0.81 (0.39)
Real Log Hourly Wage	2.35 (0.50)	1.94 (0.52)
N	280	2038

Standard deviations are listed in parentheses beneath the estimated means. The sample is drawn from the 1993 wave of the NLSY respondents, who earn at least $2 per hour and no more than $100 per hour (in real 1982 dollars).

Table 11: Computer Usage Wage Premium Regressions
For Males in the NLSY, in 1993

	NLSY DATA			CPS DATA	
	(1)	(2)	(3)	(4)	(5)
Use Comp At home for work	0.411 (0.032)	0.162 (0.032)	0.066 (0.021)	0.331 (0.024)	0.136 (0.022)
Education		0.084 (0.007)	0.082 (0.003)		0.089 (0.004)
Experience		0.081 (0.018)	0.070 (0.080)		0.050 (0.003)
$Exp^2/100$		-0.212 (0.065)	-0.088 (0.123)		-0.121 (0.016)
Industry Dummies	No	Yes	Yes	No	Yes
Occupation Dummies	No	Yes	Yes	No	Yes
Person-Specific Effect	No	No	Yes	No	No
R^2	0.063	0.292	0.600	0.028	0.313

White Standard Errors are listed in parentheses beneath the coefficient estimates. The CPS sample was drawn from the October supplement to the 1993 CPS, using males between the ages of 18 and 65 who received wages of at least $2 per hour. The NLSY sample was drawn from the 1993 wave of that survey, also using men who received wages of at least $2 per hour. Other covariates used in the regressions listed in columns 2, 3 and 5 include a marital dummy variable and a dummy variable equal to one if the respondent is white.

Table 12: Means and Standard Deviations of the CPS and Twins Data -- Whites Only

	Identical Twins	Weighted 1993 April CPS	Weighted 1993 October CPS
Self-Reported Education	14.06 (2.07)	13.99 (2.55)	13.86 (2.18)
Hourly Wage	14.39 (12.21)	12.77 (9.95)	13.06 (9.64)
Age	37.56 (10.92)	37.99 (12.37)	37.74 (11.13)
Female	0.59 (0.49)	0.58 (0.56)	0.58 (0.49)
Covered by union	0.21 (0.40)	0.22 (0.49)	0.23 (0.42)
Job tenure (years)	8.36 (8.49)	9.00 (9.33)	
Married	0.49 (0.50)	0.49 (0.57)	0.53 (0.50)
Computer used at work[1]	0.59 (0.48)		0.59 (0.49)
Sample Size	778	17,132	11,384

Standard deviations are listed in parentheses below the estimated means. The CPS samples were reweighted on the basis of age, gender, education and region to be more comparable to the data set of twins. The samples from both data sets were drawn from respondents between the ages of 18 and 64, with earnings of at least $2 per hour (in 1993 dollars) and no more than $100 per hour.

[1] The sample mean for the "Computer Used at Work" variable was calculated from only 606 observations in the data set of identical twins. This is because the question about computer usage at work was not asked in the 1991 wave of this survey.

Table 13: Weighted OLS estimates of the Earnings Equation using the CPS and Identical Twins Data sets -- Whites Only

	Identical Twins	1993 April CPS	Identical Twins	1993 October CPS
Own education	0.114 (0.009)	0.098 (0.004)	0.095 (0.010)	0.073 (0.004)
Age	0.081 (0.011)	0.073 (0.005)	0.074 (0.012)	0.056 (0.005)
Age≅ (/100)	-0.094 (0.013)	-0.088 (0.007)	-0.075 (0.015)	-0.056 (0.006)
Tenure	0.022 (0.003)	0.018 (0.001)		
Female	-0.147 (0.048)	-0.148 (0.025)	-0.231 (0.055)	-0.164 (0.027)
Married	0.231 (0.058)	0.171 (0.024)	0.270 (0.064)	0.181 (0.025)
Married Female	-0.248 (0.072)	-0.197 (0.031)	-0.280 (0.080)	-0.189 (0.033)
Covered by a union	0.068 (0.044)	0.065 (0.009)	0.150 (0.049)	0.202 (0.020)
Computer Use at Work			0.239 (0.041)	0.210 (0.017)
Sample Size	778	15,301	606	11,384
R≅	0.408	0.395	0.412	0.387

Standard errors are reported in parentheses below the point estimate of the coefficient. The CPS samples were reweighted on the basis of age, gender, education and region to be more comparable to the data set of twins

Table 14: GLS, CRE and Fixed-Effects Earnings Equation Estimates Both With and Without a Measurement-Error Correction

	Uncorrected for Measurement Error			Corrected for Measurement Error		
	GLS (1)	CRE (2)	Fixed-Effects (3)	GLS (4)	CRE (5)	Fixed-Effects (6)
Married	0.231 (0.058)	0.061 (0.108)	0.061 (0.077)	0.241 (0.062)	0.053 (0.119)	0.051 (0.088)
Avg. Marital Status		0.236 (0.128)			0.256 (0.139)	
Married Female	-0.248 (0.072)	-0.008 (0.141)	-0.008 (0.101)	-0.249 (0.076)	0.015 (0.156)	0.014 (0.116)
Avg. Married Female		-0.322 (0.164)			-0.352 (0.179)	
Own Education	0.114 (0.009)	0.092 (0.024)	0.092 (0.017)	0.124 (0.011)	0.145 (0.040)	0.143 (0.028)
Avg. Education		0.025 (0.025)			-0.023 (0.042)	
Age	0.081 (0.011)	0.082 (0.011)		0.080 (0.011)	0.081 (0.011)	
Age Squared/100	-0.094 (0.013)	-0.096 (0.013)		-0.093 (0.013)	-0.096 (0.014)	
Female	-0.147 (0.048)	-0.108 (0.052)		-0.139 (0.050)	-0.101 (0.054)	
Covered by a Union	0.068 (0.044)	0.083 (0.072)	0.083 (0.052)	0.077 (0.049)	0.117 (0.083)	0.094 (0.062)
Avg. Union		-0.026 (0.091)			-0.046 (0.103)	
Tenure	0.022 (0.003)	0.020 (0.004)	0.020 (0.003)	0.023 (0.003)	0.021 (0.005)	0.021 (0.003)
Avg. Tenure		0.003 (0.005)			0.003 (0.006)	
R^2	0.4082	0.4128	0.1546	0.4356	0.4398	0.1913
Hausman Statistic	17.39, p-value=0.0038			11.73, p-value=0.036		

Standard Errors are listed in parentheses.

Table 15: GLS, CRE and Fixed-Effects Earnings Equation Estimates Both With and Without a Measurement-Error Correction

	Uncorrected for Measurement Error			Corrected for Measurement Error		
	GLS (1)	CRE (2)	Fixed-Effects (3)	GLS (4)	CRE (5)	Fixed-Effects (6)
Married	0.241 (0.061)	0.008 (0.084)	0.008 (0.084)	0.251 (0.065)	-0.001 (0.095)	-0.001 (0.095)
Avg. Marital Status		0.329 (0.121)			0.354 (0.130)	
Married Female	-0.233 (0.078)	0.006 (0.114)	0.007 (0.114)	-0.235 (0.082)	0.014 (0.129)	0.014 (0.131)
Avg. Married Female		-0.321 (0.157)			-0.339 (0.168)	
Computer at Work	0.205 (0.040)	0.075 (0.048)	0.075 (0.048)	0.214 (0.046)	0.067 (0.059)	0.067 (0.059)
Avg. Computer		0.205 (0.078)			0.229 (0.089)	
Own Education	0.105 (0.010)	0.078 (0.020)	0.078 (0.020)	0.114 (0.011)	0.137 (0.043)	0.134 (0.037)
Avg. Education		0.027 (0.024)			-0.029 (0.046)	
Age	0.073 (0.012)	0.068 (0.014)		0.071 (0.012)	0.067 (0.014)	
Age Squared/100	-0.084 (0.014)	-0.079 (0.018)		-0.081 (0.015)	-0.078 (0.018)	
Female	-0.191 (0.054)	-0.165 (0.070)		-0.184 (0.056)	-0.160 (0.071)	
Covered by a Union	0.100 (0.047)	0.098 (0.058)	0.098 (0.058)	0.114 (0.053)	0.097 (0.067)	0.097 (0.070)
Avg. Union		0.011 (0.093)			0.026 (0.101)	
Tenure	0.019 (0.003)	0.016 (0.003)	0.016 (0.003)	0.019 (0.003)	0.017 (0.004)	0.017 (0.004)
Avg. Tenure		0.003 (0.005)			0.002 (0.006)	
R^2	0.4537	0.4676	0.1370	0.4820	0.4966	0.1659
Hausman Statistic		47.21, p-value<0.0001			31.83, p-value<0.0001	

Standard Errors are listed in parentheses.

Table 16: The Variance-Covariance Measurement Error Matrix

	$\eta_{education}$	η_{tenure}	$\eta_{married}$	$\eta_{marr*fe}$	η_{union}	$\eta_{computer}$
$\eta_{education}$	0.31	0	0	0	0	0
η_{tenure}	0	0.86	0	0	0	0
$\eta_{married}$	0	0	0.01	0.0059	0	0
$\eta_{marr*fe}$	0	0	0.0059	0.0059	0	0
η_{union}	0	0	0	0	0.13	0
$\eta_{computer}$	0	0	0	0	0	$V(\eta_{comp})$

Appendix Table 1A: Misclassification error in Marital Status For the CPS and Census Records

CPS Classification	Census Classification		Total
	Married	Not Married	
Married	10,317	99	10,416
Not Married	102	4,945	5,047
Total	10,419	5,044	7,210

Appendix Table 1B: Misclassification error in Marital Status for Women in the CPS and Census Records

CPS Classification	Census Classification		Total
	Married	Not Married	
Married	5,214	58	5,272
Not Married	61	2,920	2,981
Total	5,275	2,978	8,253

Appendix Table 1C: Misclassification error in Unionization Status In the May 1979 CPS Records

Covered by CBA on the CPS Dual Job Supplement	Covered by Collective Bargaining Agreement on the CPS Pension Supplement		Total
	Yes	No	
Yes	3,976	272	4,248
No	321	13,688	14,009
Total	4,297	13,950	18,257