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

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ABSTRACT

This analysis uses several identification strategies and data sources to control for individual ability and determine the causal effect of marital status and computer usage on wages. Although data from the CPS, NLSY and a data set of identical twins show that there are large cross-sectional effects of these variables, new econometric specifications are applied to these data which 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. 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. Both marital status and computer usage are typically found to increase earnings by as much as 20 percent or more for males, implying that marriage alone, or learning to use a computer, will raise earnings more than two or three additional years of schooling. If correct, this suggests that factors other than long-term human capital investments are important determinants of earnings. Perhaps because of the magnitude of the wage premium for using a computer or being a married male, many researchers have questioned whether factors such as

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1. Early work by Mincer (1958) and others outlining the importance of human capital as a determinant of earnings has been expanded upon by many authors in recent years. For surveys of this work, see Card (1999).

computer usage and marital status are really causal determinants of earnings. Alternative hypotheses have attributed the computer usage and marital wage premia to omitted variables in wage regressions, such as intelligence, appearance, or family background, or unobserved components of ability that happen to be correlated with earnings.

In this paper, I use new identification strategies on different data sources to ascertain the extent to which marital status and computer use are causal determinants of earnings. First, using the Current Population Survey (CPS) and the National Longitudinal Survey of Youth (NLSY), I confirm that there is a substantial effect for both variables on wages in simple, cross-sectional regressions. But using new approaches to identify the causal effect of marital status and computer usage (such as changes in marital status and a fixed-effect with computer usage) dramatically reduces their effect on wages. 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 use within-twin contrasts in marital status and computer usage to control for the effect of nongenetic 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, and the results persist even after making adjustments for measurement error and within-twin differences in unobserved ability. In addition, the estimated causal effects of marital status and computer usage in all three data sets are remarkably similar, suggesting that the findings presented in this paper are quite robust.

II. Literature Review

There is a remarkable similarity in the debates over the effect of marital status and computer usage on wages for men. In both cases, it is widely recognized that being a married male or using a computer at work is, at the very least, partially correlated with ability. However, there remains some disagreement about whether the positive effects of marital status and computer usage on wages are in any way causal, or if they are just a statistical artifact of their positive correlation with the unobserved component of an individual's ability. For example, in the large literature on the marital premium for men, 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 the marital premium for men was posited by Becker (1973) 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. Also, more productive men have a greater comparative advantage to labor market specialization, so we should observe more-able men becoming married. Some authors have provided support for the theory that even after controlling for ability, marriage itself causes higher wages (Greenhalgh 1980; Kenny 1983; Korenman and Neumark 1991), but others believe the significance of the married male indicator variable is nothing more than an identification of his prior productivity (Keeley 1977, Nakosteen and Zimmer 1987). Korenman and Neumark (1991) proposed an interesting extension to the theory that marriage improves a man's productivity; they demonstrated that the marital premium gradually accrues to men over time, perhaps because after a man is married, his ability to specialize in the labor market allows for a lower cost of accumulating human capital. As more human capital is gained, the marital wage premium is fully developed. It has also been argued that the evidence is consistent with a third, 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 benefiting from their specialized roles. For instance, positive discrimination in favor of a married male may arise if he is believed to have a lower probability of shirking for fear of losing his job (and being unable to support his family).

The debate over the effect of computers on wages is quite similar to the argument over the causal nature of the marital premium for men. There is no dispute over whether or not computer usage is positively correlated with ability, but rather, over the causal impact of computers on productivity once proper controls for ability are included in the analysis. 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, even after controlling for unobserved ability. But although the effect of computers on the overall labor market are undeniable, others have argued (DiNardo and Pischke 1997) that selection effects are the dominant reason for the wage premium associated with using a computer at work. Since the use of computers is more likely at high-paying jobs, the significance of computer usage in a wage regression may only capture the fact that more productive workers are employed at higher paying jobs.

III. 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, since such data are amenable to a fixed-effect framework. 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 and the National Longitudinal Survey of Youth. The matched CPS data are advantageous because the two-year panels provide information on individuals who do and do not become married—specifically, the pre- and post-marital wages received by men who become married—thus providing insight about immediate impact of marriage on wages. Although the CPS is not able to follow respondents who change houses (which is a weakness of the data for this analysis), it will present some general results that will be shown to be consistent with findings from other data sets. The NLSY is useful because it is a long panel data set that tracks youths aged 14–22 in 1979. This data set is ideal for analyzing 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,[2] and the length of the NLSY panel will allow for an analysis of respondents well before and after this critical age range. Summary statistics are displayed in Table 1, and both the CPS and NLSY data indicate that married males are typically older, more educated and receive higher wages than those men who are single. In addition, the NLSY includes scores from the Armed Forces Qualifying Test (AFQT), which respondents

2. U.S. Bureau of Census, Marital Status and Living Arrangements, 1991–95.

Table 1
Sample Means for Male CPS Respondents from 1990–95 and Male NLSY Respondents in 1993 and 1994

	CPS Sample				NLSY Sample			
	Married	Single	Use Computer	Don't Use Computer	Married	Single	Use Computer	Don't Use Computer
Age	42.53	33.82	38.19	39.76	33.47	32.93	32.62	32.18
	(10.04)	(11.08)	(10.08)	(11.04)	(2.21)	(2.14)	(2.22)	(2.18)
Education	13.43	13.13	14.92	13.69	13.34	12.65	15.56	12.77
	(2.70)	(2.45)	(2.06)	(2.37)	(2.68)	(2.52)	(2.34)	(2.45)
White	0.899	0.850	0.907	0.877	0.87	0.73	0.89	0.81
	(0.30)	(0.36)	(0.29)	(0.33)	(0.34)	(0.44)	(0.31)	(0.39)
Real log weekly	6.34	5.92	6.21	6.01	5.89	5.53	6.19	5.68
earnings	(0.55)	(0.67)	(0.79)	(0.79)	(0.55)	(0.62)	(0.51)	(0.63)
AFQT	—	—	—	—	73.8	64.4	87.01	68.76
					(20.98)	(22.34)	(12.20)	(21.61)
N	75,360	31,908	2,736	1,306	1,542	1,102	280	2,038

Standard deviations are listed in parentheses. The CPS sample consists of respondents between the ages of 18 and 65, who work full-time and earn real hourly wages of at least \$2 per hour and no more than \$100 per hour. The Married and Single samples in Columns 1 and 2 are drawn from the outgoing rotations groups of the CPS from 1990–95. The computer usage samples in Columns 3 and 4 were drawn from the October 1993 supplement of the CPS. The NLSY samples for marital status (which are displayed in Columns 5 and 6) were drawn from the 1994 wave of the survey, and only contain men who earn at least \$2 per hour and no more than \$100 per hour. Similarly, the sample for computer usage refers to usage at home—the only information on computer usage collected by the NLSY, which was only colleted in 1993—was drawn from the 1993 wave of the survey from male respondents, who earn at least \$2 per hour and no more than \$100 per hour. For a proper comparison, all earnings in the CPS and NLSY are in real terms, adjusted to 1993 dollars.

Table 2
Cross-Sectional and Wage Growth Regressions for Males, Using Matched Outgoing Rotation Groups from CPS Supplements, 1990–95

	Cross-Sectional Wage Regressions		Wage-Growth Regressions
	(1)	(2)	(3)
Married	0.114	—	—
	(0.003)		
Will become married next year	—	0.088	0.004
		(0.011)	(0.020)
R^2	0.393	0.395	0.021

White standard errors are listed in parentheses beneath the coefficient estimates. The dependent variable in Columns 1 and 2 is the log of real hourly wages, deflated to 1993 dollars, while the dependent variable is the change in log wages. The variable "Will Become Married Next Year" is equal to one if the respondent is currently single and becomes married in the following period, and zero if he single in the current period and also next period. All three regressions include a full set of control variates, which are: education, experience (defined as age-education-5) and experience squared, six industry indicator variables, four occupation indicator variables and five year indicator variables. The wage-change regression in Column 6 also includes change of industry and change of occupation dummy variables.

completed in 1979 and 1980. The scores have been viewed as 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. Table 1 demonstrates that AFQT scores are significantly higher for married males than for single males (this is also confirmed within a regression context), suggesting that more able men become married.[3]

To consider how ability bias may affect the marital premium, three different regressions are estimated in Table 2 using CPS data. Column 1 of Table 2 uses a standard cross-sectional regression with a full set of control variables to compare married males to single males, demonstrating that married men receive wages 11 percent higher than single men. Using data from the first year of the two-year CPS panel data set, Column 2 compares two sorts of single men: men who will become married in the following year and men who will remain single. This regression specification (which has not been used in other studies with CPS data) demonstrates that there is a 9 percent wage premium associated with being a male who will become married, and this is very close to the 11 percent cross-sectional wage premium for being a married male. (Similar results are found when estimating the same regressions using NLSY

3. 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 by regressing AFQT scores was regressed on 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, and the analysis was conducted again, but the overall results remained the same.

data: Married males earn roughly 14 percent more than their unmarried counterparts, but in the period before he becomes married, a male respondent exhibits roughly 13 percent higher wages than a male who will not become married.) This also suggests that males who become married are significantly more productive than males who remain single, and that the preexisting productivity differences between married and single men are the primary cause for the cross-sectional wage premium for married males, because most of the marital premium is accounted for by pre-existing cross-sectional wage differences between single men who will or will not become married. To test the hypothesis that the act of becoming married instantly boosts a man's wages (because he can immediately specialize in labor market work) Column 3 of Table 2 compares the wage growth of males who become married to men who remain single. The results demonstrate that both groups of men do not exhibit significantly different wage growth, which suggests that there is no immediate impact on wage growth resulting from marriage. Overall, the evidence in Table 2 shows that the higher wages received by married men do not result from any immediate advantages of specialization (or preferential treatment) that accrues to them because of their marital status—instead, it their higher ability that causes them to receive higher wages.

One advantage that the NLSY has over the CPS is that it has information on a respondent's AFQT score, which can be used in a regression context to assess the extent to which ability bias affects the marital premium. When a standard wage regression is estimated using data from the NLSY without including AFQT scores, the coefficient for the marital indicator is roughly 0.14 with a standard error of approximately 0.006. However, when AFQT scores are included in the regression along with an interaction between the AFQT score and the marital indicator, the coefficient is reduced to 0.042 with a standard error of 0.018, suggesting that ability bias plays a significant role in the analysis of the marital premium.

Another disadvantage of using matched CPS data is that it only permits the analysis of married and single males over a two-year period. If it is the case that there are effects of marriage on wages outside of a two-year period, then a longer time frame is needed for the analysis. This was the hypothesis advanced by Korenman and Neumark (1991), who suggested that the marriage premium accrues to men slowly over the course of the marriage (perhaps because of gradually increasing human capital accumulation that occurs due to labor market specialization). Using the National Longitudinal Survey of Young Men (NLS—a precursor to the NLSY), 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 the first three columns of Table 3 with NLSY data, which shows that the marital indicator becomes insignificant in a fixed-effect specification. However, the variable accounting for the total number of years married remains significant in the fixed-effect framework, which leads Korenman and Neumark to conclude that the marital premium for males is something that accrues 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.[4] This

4. 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 square, 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 affected by ability bias.

Table 3
The Effect of "Years of Marriage" in the Wage Regressions for Males in the NLSY

	Cross-Sectional Regressions			Wage-Growth Regressions			
	GLS	GLS	Fixed-Effects	GLS	GLS	GLS (Full Sample)	GLS (Small Sample)
Married	0.163	0.065	–0.025	0.017		–0.002	0.012
	(0.012)	(0.027)	(0.026)	(0.007)		(0.006)	(0.027)
Ever married					0.019	0.020	0.006
					(0.006)	(0.007)	(0.020)
Years married		0.020	0.021				
		(0.007)	(0.010)				
Years married^2/100		–0.058	–0.048				
		(0.051)	(0.062)				
R^2	0.308	0.319	0.201	0.015	0.021	0.021	0.021
N	5,168	5,168	5,168	26,443	26,443	26,443	2,973

Standard errors are listed in parentheses. The first three columns display results from cross-sectional wage regressions which use the log of hourly wages as the dependent variable (deflated to 1993 dollars). The last three columns display the wage-growth regression results, which use the change in log hourly wages as the dependent variable. Other covariates included in the regressions are: 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. The wage growth regressions also include change in industry and occupation indicator variables.

would lead to a significant effect for the "total years married" variable in the fixed effect framework (because 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:

$$(1) \quad \Delta wage_{it} = \beta_1 married_{it} + \beta_2 (Ever\ Married_i) + \gamma X_{it} + \varepsilon_{it}$$

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 marital status independently leads to higher wage growth, then β_1 should be significantly positive. Column 4 of Table 3 shows that married males do exhibit higher wage growth than unmarried males, but the fifth column demonstrates that this is true even in the periods when both groups of men are unmarried. Furthermore, the estimation of the full model in Column 6 demonstrates that β_1 is insignificant and β_2 is significant, illustrating that males who will become married already have higher wage growth than their counterparts, and becoming married does little to improve future wage growth. In contrast to the findings of Korenman and Neumark, the results in Table 3 suggest that increased human capital accumulation is not the cause of the marital premium for men—it is due to the fact that they already had higher wage growth before they were married. One fact that led Korenman and Neumark to their conclusions about the gradual accrual of the marital premium was that they found that high-wage-growth males were not more likely to become married. However, they arrived at this conclusion after using wage growth data from only one year of the NLS. Loh (1996) finds a similar result with only two years of wage growth data from the NLSY (1984–86), (which is generally consistent with Gray's 1997 finding that there are not significant changes over this time period in the type of men who become married). But limited amounts of wage growth data is highly prone to the attenuating effects of measurement error, and the job-shopping behavior of young workers (who tend to move between many jobs early in their careers) may lead to atypical wage growth patterns over short periods of time. For illustration, the seventh column of Table 3 presents estimates of the model using only a shortened sample (from 1986 to 1989), which would imply that there is no difference in wage growth for men who become married. However, because the analysis in Column 6 of Table 3 uses longitudinal data from 1979 to 1993, it reflects the long-term wage growth of men who become married (which is not the case with the results of Loh or Korenman and Neumark). This leads to a more accurate comparison of pre- and post-marital wage growth. Table 3 shows these to be quite similar. Overall, the results from the CPS and NLSY suggest that both the immediate and long-term effects of marital status on wages are negligible for men, once proper controls for ability are included in the empirical framework.

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 same question is included in the 1993 October supplement to the CPS. Table 1 shows that workers who use a computer at home to perform job-related work have more education and higher earnings than individuals who do not use a computer, and the NLSY shows that AFQT scores differ by computer usage, suggesting that there may be inherent differences in the types of workers who do and do not use computers. The regression results in Columns 2 and 4 of Table 4 show that the premium associated with using a computer at home to do job-related work is roughly the same for respondents in the CPS and NLSY—roughly 14 percent and 16 percent, respectively. Because the NLSY is a panel data set, person-specific effects can be incorporated into the wage equation to control for the effects of ability, which is done in Column 5. The inclusion of a fixed effect causes a large decrease in the wage premium associated with using a computer at home to complete job-related work, and suggests that computer usage only moderately raises an individual's wage, even after controlling for his or her ability. In Column 6, a wage regression that includes a variable for the individual's AFQT score, and an interaction of the AFQT score with the computer usage variable. Although the standard error for the computer variable is larger in Column 6 than in Column 5, the coefficient magnitudes in Columns 5 and 6 are generally similar, and together suggest that the premium for using a computer at home for work appears to be prone to serious ability biases.[5]

5. This is not inconsistent with Krueger's (1993) argument, which also asserts that computer usage at home is correlated with ability. However, the results in Table 4 are important in comparison with the results from

Table 4
Computer Usage Wage Regressions for Males in the NLSY and CPS, in 1993

	CPS Data		NLSY Data			
	(1)	(2)	(3)	(4)	(5)	(6)
Use computer at home for work	0.331	0.136	0.411	0.162	0.066	0.071
	(0.024)	(0.022)	(0.032)	(0.032)	(0.021)	(0.089)
Education		0.089		0.084	0.082	0.052
		(0.004)		(0.007)	(0.003)	(0.028)
Experience		0.050		0.081	0.070	0.054
		(0.003)		(0.018)	(0.080)	(0.052)
$Exp^2/100$		–0.121		–0.2120	–0.088	–0.002
		(0.016)		(0.065)	(0.123)	(0.002)
Industry dummies	No	Yes	No	Yes	Yes	Yes
Occupation dummies	No	Yes	No	Yes	Yes	Yes
Person-specific effect	No	No	No	No	Yes	No
AFQT scores	No	No	No	No	No	Yes
R^2	0.028	0.313	0.063	0.292	0.600	0.319

White Standard Errors are listed in parentheses. 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 the survey, also using men who received wages of at least $2 per hour. The variable "Use a Computer at Home for Work" is equal to one if the respondent uses a computer at home for work-related tasks, and zero otherwise. All regressions use the log of hourly wages as the dependent variable. Other covariates used in the regressions listed in Columns 2, 4, 5, and 6 include a marital dummy variable and a dummy variable equal to one if the respondent is white. To compute the fixed effect used in Column 5, wage regressions were calculated for the years prior to 1992 in which the respondent worked full time and earned at least $2 per hour. The person-specific fixed effect from this regression was included in the 1993 cross-sectional regression for the computer premium. The results for AFQT controls are reported for a regression with a variable for the individual's AFQT score, and the interaction of the AFQT score with the computer use variable.

IV. Twins Analysis

Another approach for 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. This data was collected during the summers of 1991 to 1993 and 1995 at the Twinsburg Twins Festival in Twinsburg, Ohio, using interview questionnaires modeled after the Census and CPS instruments.[6] The data are drawn from the subsample

Table 6, which show virtually the same effect of ability controls on the premium for computer usage at work, implying that computer usage at work premium is subject to ability bias like the premium for computer usage at home.

6. Ashenfelter and Krueger (1994) and Ashenfelter and Rouse (1998) provide a discussion of the procedures used to collect this data. Some additional questions were specifically designed for interviewing twins, such as 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.

Table 5

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	13.99	13.86
	(2.07)	(2.55)	(2.18)
Hourly wage	14.39	12.77	13.06
	(12.21)	(9.95)	(9.64)
Age	37.56	37.99	37.74
	(10.92)	(12.37)	(11.13)
Female	0.59	0.58	0.58
	(0.49)	(0.56)	(0.49)
Covered by union	0.21	0.22	0.23
	(0.40)	(0.49)	(0.42)
Job tenure (years)	8.36	9.00	
	(8.49)	(9.33)	
Married	0.49	0.49	0.53
	(0.50)	(0.57)	(0.50)
Computer used at work[a]	0.59		0.59
	(0.48)		(0.49)
Sample size	778	17,132	11,384

Standard deviations are listed in parentheses. The twins data was collected in 1991–93 and 1995. 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 all data sets only include 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.

of identical white twins, both of whom have worked within two years prior to the interview and are living within the United States. Table 5 displays the characteristics of the twins sample, and compares them to white workers from reweighted CPS supplements. The data set composed of identical twins is generally similar to the reweighted CPS samples, with some small differences evident in characteristics like wages or education.[7]

To determine the causal effect of marital status and computer usage on wages, it will be assumed that the unobserved component of ability is equal for both twins. This implies that the difference in earnings between a married twin and his unmarried sibling—or a twin that uses a computer and one who does not—will be attributed to the effect of marital status and computer on earnings that is not biased by the unobserved component of ability. Assuming that ability has a linear effect on earnings, the earnings equations for each twin can be expressed as follows:

(2) $Y_{ij} = \beta_{ij} X_{1j} + \alpha Z_j + A_j + \varepsilon_{ij}$

7. I find (as do Ashenfelter and Rouse (1998)) that these differences have no large effect on the results in this paper—wage regressions using CPS and twin data yield very similar coefficients on all the variables in my regressions.

Table 6
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	0.008	0.008	0.251	– 0.001	–0.001
	(0.061)	(0.084)	(0.084)	(0.065)	(0.095)	(0.095)
Average marital status		0.329			0.354	
		(0.121)			(0.130)	
Married female	–0.233	0.006	0.007	–0.235	0.014	0.014
	(0.078)	(0.114)	(0.114)	(0.082)	(0.129)	(0.131)
Average married female		–0.321			–0.339	
		(0.157)			(0.168)	
Computer at work	0.205	0.075	0.075	0.214	0.067	0.067
	(0.040)	(0.048)	(0.048)	(0.046)	(0.059)	(0.059)
Average computer		0.205			0.229	
		(0.078)			(0.089)	
Own education	0.105	0.078	0.078	0.114	0.137	0.134
	(0.010)	(0.020)	(0.020)	(0.011)	(0.043)	(0.037)
Average education		0.027			–0.029	
		(0.024)			(0.046)	

Age	0.073	0.068		0.071	0.067	
	(0.012)	(0.014)		(0.012)	(0.014)	
Age squared/100	–0.084	–0.079		–0.081	–0.078	
	(0.014)	(0.018)		(0.015)	(0.018)	
Female	–0.191	–0.165		–0.184	–0.160	
	(0.054)	(0.070)		(0.056)	(0.071)	
Covered by a union	0.100	0.098	0.098	0.114	0.097	0.097
	(0.047)	(0.058)	(0.058)	(0.053)	(0.067)	(0.070)
Average union		0.011			0.026	
		(0.093)			(0.101)	
Tenure	0.019	0.016	0.016	0.019	0.017	0.017
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Average tenure		0.003			0.002	
		(0.005)			(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.

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 expressed as: $A_j = \gamma(X_{1j} + X_{2j})/2 + v_j$. These assumptions lead to the reduced-form correlated random-effects model (Chamberlain 1982):

$$(3)\quad \begin{aligned} Y_{1j} &= \beta X_{1j} + \alpha Z_j + \gamma(X_{1j} + X_{2j})/2 + v_j + \varepsilon_{1j} \\ Y_{2j} &= \beta X_{2j} + \alpha Z_j + \gamma(X_{1j} + X_{2j})/2 + v_j + \varepsilon_{2j} \end{aligned}$$

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 familial ability 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: $(Y_{1j} - Y_{2j}) = \beta(X_{1j} - X_{2j}) + (\varepsilon_{1j} - \varepsilon_{2j})$, which yields unbiased estimates of β that are not correlated with ability, but it does not provide a direct estimate of γ.

The results in Table 6 demonstrate the effect of controlling for familial ability in an earnings equation, using a specification which separates the effect of marriage on wages for men and women (which has not been done in prior studies using this data set of twins). If familial ability had no effect on earnings, then results from the generalized least-squares estimation procedure displayed in the first column of Table 6 would provide an unbiased estimate of the effect of the exogenous regressors. Also, the GLS and correlated random effects estimates (in Column 2) would differ only because of sampling error. However, the results in Columns 2 and 3 show that coefficients for marital status and computer usage differ dramatically depending on the estimation procedure. Without controls for ability, the premium for being a married male or using a computer at work is approximately 20 percent. However, Column 2's results demonstrate that accounting for familial ability greatly reduces the significance and the magnitude of both coefficients—the marital premium is basically reduced to zero—with correspondingly small *t*-values. In addition, the correlation between ability and both the married male indicator and computer usage indicator is positive and significant, reinforcing the conclusion that the significance of being a married male or using a computer at work is primarily subject to ability bias, and not from separate causal effects. It should also be noted that the magnitude of the coefficient for the usage of a computer at work is quite close to the estimated effect which was calculated using NLSY data for computer usage at home. The fact that the inclusion of ability controls yields a 6 percent return for computer usage in both data sets speaks to the robustness of this finding. The similarity of the results between the NLSY and twins data is interesting for another reason: the use of a computer at home to do work-related tasks is clearly correlated with ability (as Krueger (1993) suggests), so it is not surprising that its magnitude is decreased by controls for unobserved ability. But, the fact that computer usage at work is decreased in a similar way is suggestive of the large bias that unobserved ability contributes to the computer premium. The results in Column 2 are corroborated by the within-twin estimates displayed in Column 3, and the estimated marital premium in this framework stands in contrast to evidence presented by Loh (1996), who found that familial fixed effects don't impact the marital premium for men.[8] Also, the results in Table 6 do not change year-by-year, or if the sample is limited to only men.

The results in Table 6 provide a direct test of the competing theories for the significance of the marital premium for male workers and computer usage at work. If becoming married improves a male's productivity (or provides him with positive discrimination from his employer), then controlling for ability should still result in a significant estimate of the coefficient for marital status. Similarly, if computer use raises a worker's productivity (independent of his or her ability), then controlling for familial ability shouldn't affect the estimate of the computer premium. However, the marked decrease in the magnitude and significance of the computer usage and marital premia implies that married males are better-paid than unmarried males because, in all likelihood, married males were more productive before they became married. Likewise, the computer usage premium is also due to the higher ability of workers who use a computer. Overall, the conclusions drawn about the causal effect of marital status for men and computer usage are consistent across all three data sets used in this paper.

V. Measurement Error

The particular importance of accounting for measurement error in the econometric framework for twins has been discussed by many authors (Ashenfelter and Krueger 1994; Griliches 1979). In general, measurement error has an attenuating effect on coefficient estimates, so it is possible that the results in the first three columns of Table 6 (especially Columns 2 and 3) are affected by measurement error. If all of the explanatory variables are measured with error,[9] then the measurement-error-corrected GLS estimator b_{GLS} and correlated random effects (CRE) estimator b_{CRE} are: $b_{GLS} = b_{CRE} = [X'X - n\Sigma]^{-1} X'y$, 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 errors associated with each explanatory variable. The measurement-error-corrected fixed-effects estimator uses a similar formula: $b_{FE} = [\Delta X'\Delta X - 2n\Sigma]^{-1}\Delta X'\Delta y$, where ΔX and Δy are the respective within-twin-differenced analogs of X and y. The presence of the matrix Σ could seriously affect the parameters of interest from the data, so it is important to account for measurement error in the analysis to provide accurate parameter estimates.

To account for the possibility of measurement error in the twins data, I constructed the matrix of measurement error variances and covariances, Σ. Some variances had to culled from preexisting studies,[10] since only the education variable in the twins data

8. Loh finds that the within-sibling estimate of the marital premium is insignificant, but the cross-sectional estimate is negatively significant. This difference may be due to the fact that Loh's regressions include variables about spousal labor supply and education, which may be endogenous and bias his estimates of the marital premium.
9. 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.
10. A data appendix (available upon request from the author) provides a more detailed discussion of the data contained in these studies and how they were used to compute measurement error variances.

set has validating variable that can be used to assess its measurement error. To derive the measurement error for the indicator variables in the regressions (marital and union status), I relied upon Card's (1996) symmetric measurement error approach using misclassifications detected in reinterviews of survey respondents.[11] To obtain estimates of the reporting error for years of tenure, I used Duncan's and Hill's (1985) cross-tabulations of employee survey data and employer records collected from a large manufacturing firm.[12] Accounting for the measurement error in years of education can be accomplished by following Ashenfelter and Krueger's (1994) well-established method of using the own-reported and sibling-reported measures of education in the data set of identical twins.[13] Lastly, 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, because none exist to the best of my knowledge. As such, different measurement error variances for computer use were considered, ranging from 0.02 to 0.04. Though arbitrarily defined, these two bounds seemed reasonable because the measurement error variance for 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 that is above the measurement error variance of any other indicator variables) allows for a conservative estimate of the effect of measurement error on the computer-use variable.[14]

Columns four through six of Table 6 present regression results that are corrected for measurement error, and it is clear that all of the results in Columns 1–3 are still evident after the measurement-error correction is performed—specifically, the wage premium associated with being a married male or using a computer at work still disappears after familial controls are included in the regression. Also, the positive and significant correlation between familial ability and both marital status for males and using a computer at work is still evident within the measurement-error-corrected framework,[15] demonstrating that the results from the data set of twins are robust to the potential effects of measurement error.

11. Specifically, an estimate of the reporting error for marital status is taken from a reinterview of Census respondents in the CPS in 1970. This approach yielded a measurement error variance of 0.01 for both marital status and female marital status. A similar analysis for union status used data 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.

12. 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 in the twins data 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.

13. Using the sibling report of one's own education as a type of validating instrument, a measurement error variance of 0.31 is derived for this variable. For more details about this calculation, see Ashenfelter and Krueger (1994).

14. This approach gave me a variance-covariance measurement-error matrix Σ, with the variances along the diagonal and (by assumption) zero covariance between the measurement errors for all variables except for marital status and being a married female (which must covary by construction) which I compute to be 0.0059.

15. 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 Table 6. 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.

a. 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

VI. Within-Twin Differences in Ability

Another criticism that must be accounted for in the twins analysis is the possibility of within-twin differences in ability. Both Neumark (1999) and Bound and Solon (1999) outlined the potential biases that can affect within-twin estimates of the return to education, and the same biases can affect within-twin estimates of any other variable in the wage equation. If twin *i*'s individual-specific component of ability is denoted by the variable $\hat{A}_{ij}$, then the wage equation for twin *i* in family *j* can be written as:

$$(4)\quad y_{ij} = \beta X_{ij} + \alpha Z_j + \theta A_j + \varphi \hat{A}_{ij} + \varepsilon_{ij}$$

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:

$$(5)\quad \Delta y_j = \beta \Delta X_j + \varphi \Delta \hat{A}_j + \Delta \varepsilon_j$$

and the resulting estimates of β are biased by the correlation of $\Delta \hat{A}_j$ and ΔX_j:

$$(6)\quad b_{FE} = \beta + \varphi (\Delta X_j{}' \Delta X_j)^{-1} \Delta X_j{}' \Delta \hat{A}_j$$

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, $\Delta X_j' \Delta \hat{A}_j$, would be expected to contain exclusively positive entries. The more able twin would also receive a higher wage than his or her counterpart, suggesting that $\varphi > 0$, causing an upward bias in the estimation results. This lead 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 the existence of within-twin differences in ability may weaken conclusions drawn about estimates of the return to education from the data set of twins, it has strong implications for the married-male and computer-usage-at-work wage premia. Since 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 the marital and computer use wage premia are insignificant, both with and without a correction for measurement error, then this suggests that the unbiased coefficients also are insignificant (and possibly negative). Thus, the presence of any within-twin differences in ability would actually strengthen the conclusions drawn from the results in Table 6 about the causal effects of marital status for men and computer usage on wages.

VII. Conclusion

Many authors have disagreed over whether or not the large wage premia for being a married male or using a computer at work are due to truly causal effects. The purpose of this paper was to use several identification strategies as well as different sources of data to examine the causal effect of marital status for men and computer usage on wages. Using the CPS and NLSY, it was found (using conventional estimation procedures) that 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 married male and computer use wage premia. An analysis of data from the NLSY showed that AFQT scores or a simple person-specific fixed-effect greatly reduced the wage premium associated with using a computer at home for work or being a married male. Both data sets also showed that most of the marital premium for males is already evident in the period before they become married, and that the act of becoming married does little for wage growth in the first year after marriage. The length of the NLSY panel also permitted a longer-term analysis of the marital premium, which showed that males who get married already have high wage-growth, which can account for Korenman and Neumark's (1991) finding about the significance of the number of years a male is married.

A data set of identical twins was used in this paper's second major identification strategy, which involved a simple econometric model to control for familial ability in typical earnings equations. The results showed that the incorporation familial controls greatly reduced the premia for both being a married male and using a computer at work, which were similar to the findings from the NLSY and CPS. These results could be criticized, however, because they could be biased by measurement error or within-twin differences in ability. I have dealt with both criticisms to determine that they do not alter my main findings. First, the results are robust to a measurement-error correction, and, second, the presence of within-twin differences in ability (if they exist) actually reinforce my findings. Overall, the findings from the three data sets used in this paper suggest that ability bias has a strong effect on the wage premia for married men and using a computer at work. In contrast to theories that rely upon labor market specialization or preferential treatment given to married men by employers to account for the significance of the marital status variable in an earnings equation, the results suggest that married males are always more productive than their unmarried counterparts. And, as with the married-male indicator, it was also shown that the significance of the return to using a computer at work is not derived from the increased productivity caused by the computer itself; instead, this suggests that workers who use computers at work were already more productive than their counterparts.

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