Married with children: What remains when observable biases are removed from the reported marriage wage premium

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Abstract

There is a substantial research literature that discusses and documents a wage premium for married men. Our meta-analysis of 59 studies and 661 finds a marriage premium for US men of between 9% and 13% after misspecification and selection biases are filtered. Results from this meta-regression analysis cast doubt upon both the 'selection' and the 'specialization' explanation for the marriage-wage premium but are consistent with the notion that married men are or may be perceived to be more stable and committed workers.

Keywords: Marriage premium, wages, productivity, meta-regression analysis, omitted-variable bias

JEL classification: J12, J31

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Introduction

Several dozen studies in economics have found that married men earn between 10% and 50% higher wages than their single counterparts. Hill (1979) was among the first to investigate this wage premium thoroughly. She uses the Panel Study of Income Dynamics (PSID) and examines the wage effects of marital status for white men after carefully controlling for work experience, training, and labour force attachment. The marriage premium persists even after these controls for worker qualifications are introduced. Since that time, the primary explanations that have been offered for this phenomenon are: employer discrimination towards married men, selection of high-ability men into marriage, and increased productivity as a result of greater specialization of labour for married men. Although many attempts have been made to determine the actual cause of the male marriage wage premium, the existing literature provides mixed results about which factors are responsible for the observed wage premium.

Is it is possible that the marriage wage premium has not been fully explained or adequately estimated in spite of the volume of research conducted? Key variables are: productivity, household specialization of labour, and ability. Unfortunately, they are very difficult to measure empirically. Many previous studies (Korenman and Neumark 1991; Cornwell and Rupert 1997; Gray 1997; Hersch and Stratton 2000) control for unobservable individual factors such as ability by using fixed-effects panel models. Most, but not all, of these studies find a significant marriage premium even after controlling for individual-specific fixed effects. Thus, it remains unclear how important selection and other unobserved factors are in explaining this phenomenon.

Specialization of labour within the home is arguably even more difficult to measure than ability using readily available survey data. Loh (1996), for example, uses

the wife's labour force participation as a proxy for specialization within the home and finds that the marriage premium does not diminish when her labour force participation is included. Yet even theoretically, the relationship between a wife's market work and her husband's household production is ambiguous. Married men with working wives may spend less time on household production because household income is greater. On the other hand, the working wife's time is more valuable, so her husband may spend more time on housework than a man whose wife does not work. One important issue is whether the benefits from specialization occur as a one-time (or instantaneous) increase in wages at marriage (an intercept shift) or if the premium increases over time as a couple is more able to specialize effectively. The variable "years of marriage" is often included to address this issue, for example: Akerlof (1998), Cornwell and Rupert (1997), Korenmann and Neumark(1991), Krashinsky (2004), and Loughran and Zissimopoulous (2009). Our meta-regression analysis documents how omitted-variable biases, reporting estimates based on a restricted age range, and publication selection bias still dominate this well-developed empirical literature.

In this paper, we employ meta-regression analysis (MRA) to examine the size of the male marriage wage premium, whether selection or productivity differences are primarily responsible, and whether the wage premium is changing over time with changes in gender norms and family structure. MRA is the statistical analysis of previously reported research results (Stanley and Jarrell 1989). In hundreds of applications, MRA has explained much of the disparate empirical findings routinely found in empirical economics (Stanley 2001). In labour economics, MRA has been profitably employed to understand: the union-wage gap (Jarrell and Stanley 1990); the

employment effect of the minimum wage (Card and Krueger 1995; Doucouliagos and Stanley 2009, de Linde Leonard, Stanley and Doucouliagos 2013), participation and productivity (Doucouliagos 1995), the gender wage gap (Stanley and Jarrell 1998; Jarrell and Stanley 2004; Weichselbaumer and Winter-Ebmer, 2005), returns to schooling (Ashenfelter et al., 1999), unions and productivity (Doucouliagos and Laroche 2003), the wage curve (Nijkamp and Poot 2005), the effect of immigration on wages (Longhi, Nijkamp and Poot 2005) and efficiency wages (Krassoi-Peach and Stanley 2009), to cite a few. In particular, we focus on omitted-variable and publication biases, whether fixed-effects methods were used, the time period of the data and whether or not "years of marriage" were included in the researchers' wage equation as well as many other variables previously found to be important in wage determination (see Table 2).

Meta-Regression of Labour Economics

Labour economists employ a wide variety of approaches, models, methods and datasets, which unsurprisingly produce a corresponding large variation among the reported estimates. For example, researchers report estimates of the marriage-wage premium that range from 100% to a negative 39% of average wage, and the coefficient of variation is 120%. Thus, there is more research variation than there is central tendency. With such large variation, it is highly risky to make any general statement about what this area of research tells us about the marriage-wage premium. New research is being produced all of the time. In fact over the past four years, we found 258 estimates, nine percent of which are statistically negative, 40% are significantly positive, and the majority of recent research is not significantly different than zero. So, what is the research consensus?

Nor is marriage-wage research more diverse than other areas of labour research.

Among 1,492 reported US minimum-wage employment elasticities, reported estimates range from -19 to +5 with a coefficient of variation of 577% (Doucouliagos and Stanley, 2009). Efficiency-wage productivity elasticities vary from -0.06 to +5.4 with a coefficient of variation of 138% (Krassoi Peach and Stanley, 2009). Even worse, reported productivity effects from union membership have a coefficient of variation equal to 955% (Doucouliagos and Laroche, 2003). When research variation overwhelms an underlying trend or tendency, how can researchers sensibly summarize an area of research without relying on their preconceived preferences?

> What we need is some objective and critical methodology to integrate conflicting research findings and to reveal the nuggets of "truth" that have settled to the bottom. . . . Meta-analysis is the statistical analysis of previously published, or reported, research findings on a given hypothesis, empirical effect, phenomenon, or policy intervention. It is a systematic review of all the relevant scientific knowledge on a specific subject and is an essential part of the "evidence-based practice" movement—Stanley and Doucouliagos (2012, p. 2)

Since Jarrell and Stanley (1990), labour researchers have used meta-regression analysis to summarize research findings statistically and analyze their wide variation objectively. To summarize and explain the variation among research findings, all comparable empirical estimates are collected and included in a multiple regression:

$$\hat{\delta}_i = \beta_0 + \sum \beta_k Z_{ki} + \eta_i \quad . \qquad (1)$$

Where $\hat{\delta}_i$ is the estimated marriage premium from study i, and Z_{ki} are research dimensions that might potentially affect the reported estimate (Stanley and Jarrell, 1989). Over the last quarter century, hundreds of such meta-regressions of economics research have been conducted (Stanley and Doucouliagos, 2012).

Worse than the large variation of reported findings is the wide variety of data, research approaches and methods used by labour economists. Although conventional econometric methods allow researchers to investigate some of the variation in their choice of controls and methods, available data often limits a systematic and comprehensive investigation of the drivers of research findings. For example, our meta-analysis reveals that the marriage-wage premium is largely a US phenomenon. As long as researchers are confined to US data, such an 'aggregate' research finding is invisible to conventional research. We also find that restricting the age range of workers, omitting the years of marriage, and omitting the union status of workers are the primary drivers (along with publication selection) of the observed differences among estimates of the marriage-wage premium. Together these and other factors explain 97% of the large variation of reported estimates of the marriage-wage premium.

Conventional labour research cannot address these questions, if, for no other reason, than that available datasets do not allow the researcher to vary systematically all of these important dimensions: country, years married, union status, etc. Furthermore, most areas of economics research selectively report statistically significant results or suppress insignificant ones (Doucouliagos and Stanley, 2013). When even a minority of researchers selectivity report their findings, publication selection bias can make it appear that there is, for example, an adverse employment of raising the US minimum wage when there is in fact none (Doucouliagos and Stanley, 2009; Elmendorf, 2014). Unfortunately, it only takes a small minority of studies to engage in selective reporting to distort the overall summary of any area of research (Doucouliagos and Stanley, 2009; Moreno et al., 2009; Stanley and Doucouliagos, 2012). Only the research-wide, aggregate perspective

that meta-regression offers can statistically filter out these publication selection biases (Stanley, 2008; Stanley and Doucouliagos, 2014).

If estimates are selected for their statistical significance, selection will be more intense and the resulting publication bias will be larger for those studies with larger standard errors. In this case, the reported estimated marriage premium will depend on its own standard error:

$$\hat{\delta}_i = \alpha_1 + \alpha_0 Se_i + u_i \quad (2)$$

where $\hat{\delta}_i$ is the estimated marriage premium, and Se_i is the associated standard error of $\hat{\delta}_i$. Meta-regression analysis (MRA) equation (2) will clearly contain heteroskedasticity, because Se_i differs greatly from one study, or estimate, to the next. Weighted least squares (WLS) is the conventional remedy for heteroskedasticity, which can be implemented either by using a WLS routine with $1/Se_i^2$ as the weight or by dividing MRA equation (2) by Se_i (Stanley and Jarrell, 1989; Stanley, 2008).

$$t_i = \beta_0 + \beta_1 (1/Se_i) + v_i \quad (3)$$

where t_i is the t-value for the estimated marriage premium. If β_0 is significantly different from zero, this is evidence of publication selection (Egger et al. 1997). This test is known as the funnel asymmetry test (FAT). Testing whether $\beta_1 = 0$ is the precision-effect test (PET) (Stanley 2005; Stanley 2008), and finding $\beta_1 > 0$ is evidence that there is a positive marriage premium after correcting for publication selection.

The estimate of the marriage premium, $\hat{\beta}_1$, given by the FAT-PET-MRA equation (3) is known to be biased downward when there is a genuine empirical effect (Stanley

2008), and Stanley and Doucouliagos (2014) propose a nonlinear version of (3) to provide a less biased corrected estimate of empirical effect. The resulting MRA approximation replaces Se_i in equation (2) with Se_i^2, and its WLS version is:

$$t_i = \gamma_0 Se_i + \gamma_1 (1/Se_i) + e_i \quad (4)$$

$\hat{\gamma}_1$ is the estimated marriage premium corrected for publication selection. Stanley and Doucouliagos (2014) call this corrected estimate 'precision-effect estimate with standard error' (PEESE). Lastly, Se_i can be added to multiple meta-regression (1) to allow simultaneously for publication selection and any number misspecification biases along with other factors that might influence the reported estimate of the marriage-wage premium.

The Male Marriage Wage Premium

Estimates of the marriage wage premium come from a standard log-wage regression that includes, among other considerations, a control for marital status.

$$\ln W_i = \beta X_i + \delta M_i + \varepsilon_i \quad (5)$$

where W is the worker's wage, X is a vector of worker characteristics thought to affect his earnings, and M is his marital status. The coefficient on the dummy variable for marital status is the estimate of interest in this analysis. When multiplied by 100, the coefficient on the marital status dummy variable can be read as an approximate percentage wage premium that married men enjoy. The exact percent premium is given by $(e^{\delta} - 1)*100$.

If an unobserved factor influences both wages and marital status, the estimates from equation 1 will be biased. For example, it is possible that some desirable

personality characteristic, like 'charisma,' could affect both wages and marital status. If 'charisma' positively affects both wages and the likelihood of being married, then δ will be upwardly biased. If sample selection is the main channel for the marriage-wage premium, then the observed marriage premium is simply the artifact of some unobserved individual characteristic. To address this possibility, individual effects are often explicitly incorporated into the wage equation:

$$\ln W_{it} = \beta X_{it} + \delta M_{it} + \alpha_i + \varepsilon_{it} \qquad (6)$$

where W_{it} is the wage of individual i in year t, and α_i captures the time-invariant characteristics of individual i (e.g., his 'charisma') and its potential to affect wages.

Using panel data with fixed effects renders these individual time-invariant individual effects (α_i) harmless. If the estimate of the marriage premium, δ, falls significantly when individual fixed effects are included in the wage regression, this is evidence that selection of more desirable men into marriage is one important cause of the wage premium.

If, on the other hand, marriage is causally related to wages, it then becomes important to ask how the benefits of marriage accrue. If specialization of labour within the household is the causal mechanism, one might expect the benefits of marriage to increase over time, as couples adjust to their comparative advantages. Kenny (1983) contends that most of the wage differential between married and unmarried men is the product of additional human capital accumulation during marriage. Since human capital accumulation takes time, there is reason to expect the marriage premium to grow with years married rather than as a lump-sum increase on the wedding day.

If the marriage premium is due to specialization of labour within the home, it stands to reason that having a wife who devotes more of her time to home production allows the husband to concentrate on market work, resulting in a positive wage premium. Over recent decades, women have entered the labour force in larger numbers, and time spent in home production has steadily decreased. It is of interest to find the effects of these changes in family structure on the wage premium. A meta-regression analysis allows for a comprehensive view of the changes in the marriage premium over time across the entire research literature.

Methods

To identify all the empirical estimates of the marriage wage premium, we searched the EconLit database and the RePEc (Research Papers in Economics) database, which contains over 300,000 working papers and 500,000 journal articles. After having identified a dozen early influential papers, we also used the Social Sciences citation index to find papers that cited these seminal works. Our search stopped June 2014. This process uncovered over 150 papers. We reviewed each paper individually to determine whether it contained at least one comparable empirical estimate of the effect of marriage on male wages. Eliminating those that did not leaves 59 relevant papers containing empirical estimates of this wage premium. Furthermore, a few studies were excluded because regressions included married men only or also included women, resulting in incomparable marriage wage premium measures. These remaining 59 studies contain 661 estimates of the marriage wage premium. Because marriage is a common control variable in wage regressions, estimates of the marriage premium may also be found in papers about compensating wage differentials (Brown 1980; Duncan and Holmlund

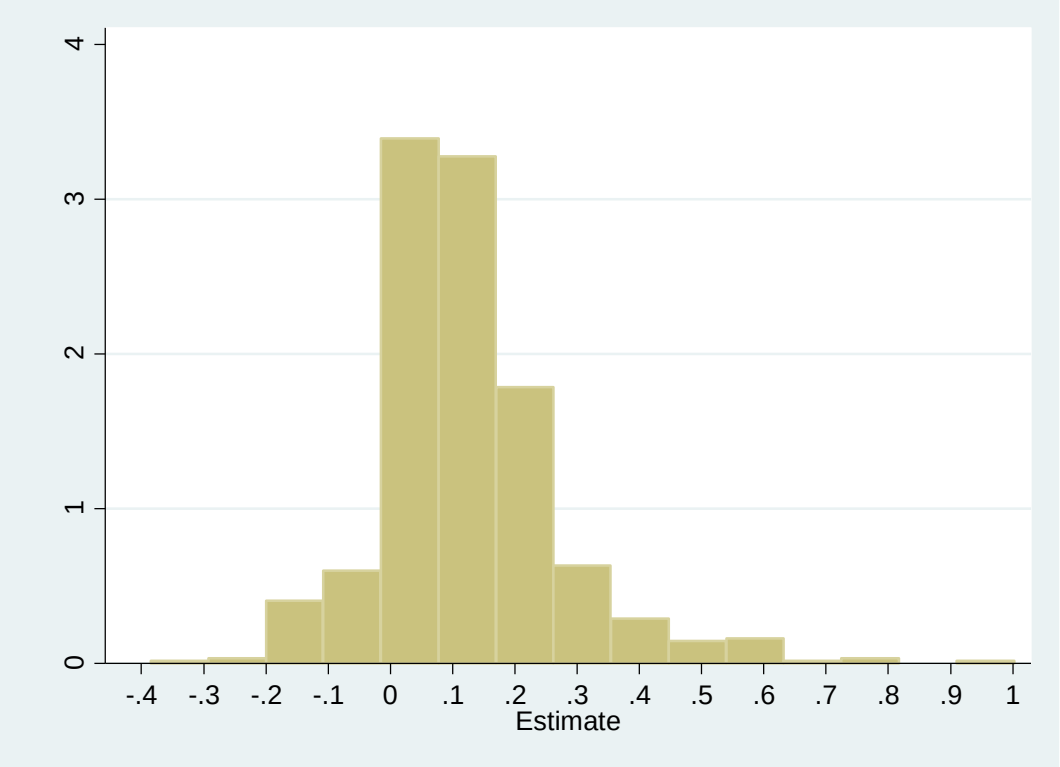
1983), the effect of self-employment (Hundley 2000), life-cycle earnings (Rosen and Taubman 1982), earnings differentials (Blau and Beller 1988; Kalacheck and Raines 1976; Malkiel and Malkiel 1973), and other topics in labour economics. About 21% of the estimates come from studies that are not primarily concerned about the marriage-wage premium.

Results

On average, these studies report that married men earn 12% more than their single counterparts. The smallest wage premium reported is -0.385, and the maximum is 1.002. Approximately 50% of the estimates are between 0.05 and 0.2. See Figure 1.

A positive coefficient on a marriage dummy variable in a wage regression has become the norm in labour economics. Whenever there is an established research expectation, there is also a threat that researchers will change their research methods until they arrive at the expected results or, alternatively, that journal editors and referees will discount papers that do not find statistical significance in the expected direction.

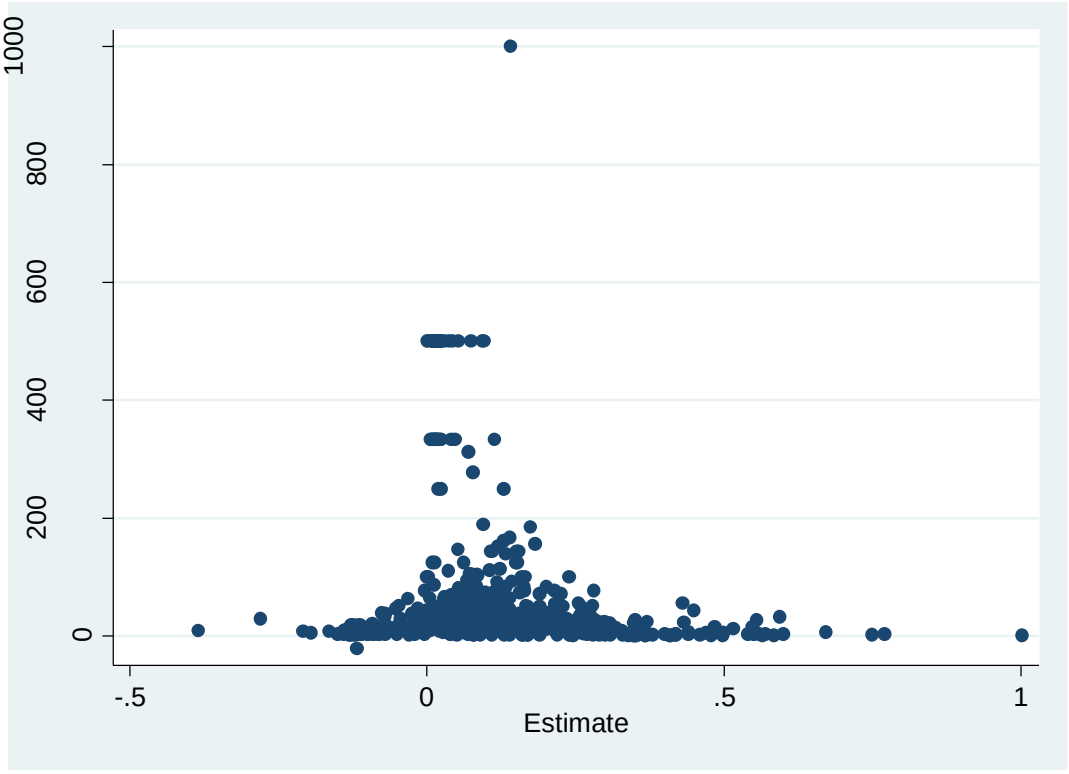
Figure 1: Distribution of the Reported Marriage-Wage Premiums



A visual examination of a funnel graph (Figure 2), a scatter diagram of precision (1/standard error) against the estimated empirical effect, is often a rough indicator of publication selection (Stanley and Doucouliagos, 2010). In the absence of publication selection, the estimates should vary symmetrically around the 'true' effect. Selective reporting can cause estimates to be biased and to exaggerate empirical effects. Publication selection bias is suggested when a funnel graph is asymmetric or skewed to one side or the other.

Visual inspection of the funnel graph seems to indicate approximate symmetry, but looks can be deceiving. Perhaps there is some slight leaning towards the right? Visual inspection is never sufficient, and a formal statistical test is always necessary. Fortunately there is a simple test for publication selection (Egger et al., 1997; Stanley 2005; Stanley 2008)—recall the FAT-PET-MRA, equation (2).

Figure 2: Funnel Graph of Reported Marriage-Wage Premiums



The results of this FAT-PET-MRA for male marriage premiums are found in Table 1—recall equation (2). The funnel asymmetry test reveals clear evidence of publication selection (t=6.99; p<<.001). Nonetheless, there is a sharp signal of a genuinely positive marriage premium beyond any distortion that publication selection might give—PET (t=17.21; p<<.001). Thus, our meta-analysis confirms the presence of a positive male marriage wage premium, at least from the perspective of the overall research literature.

TABLE 1
Tests for Publication Selection
(Dependent Variable: t)

FAT-PET-MRA		**PEESE**	
Intercept	2.02	Se	2.536
	(6.99)		(1.68)
(1/Se)	0.034	(1/Se)	0.044
	(17.21)		(22.48)

Notes: (t-values in parentheses)

Having established the existence of a marriage wage premium beyond selection for statistical significance, the magnitude of this premium becomes of central interest. The fixed-and random-effects weighted averages are the conventional summary statistics in meta-analysis (0.044, 0.102; respectively); their 95% confidence intervals are: (0.043, 0.045) and (0.097, 0.107). When there is publication bias, the random-effects weighted average is widely known to be biased upward, which is consistent with what we see in this area of research. In such cases, an unrestricted weight least squares approach is the best, giving a wider confidence interval than the conventional fixed-effect estimate (0.040, 0.048)—see Stanley and Doucouliagos (2013).

Yet, none of these simple averages fully corrects for publication selection bias. The best corrected estimator for publication selection is PEESE, equation (4) above (Stanley and Doucouliagos, 2014). This PEESE corrected estimate is found in column 2 of Table 1, and it estimates the true marriage premium to be 4.4%, exactly the same as the weighted least squares weighted average but only 36% as large as the simple mean (12%) of the reported estimates of the marriage-wage premium.

Thus far, we found that marriage-wage research suffers from publication selection bias, and the true effect is likely to be much smaller than the average reported effect. However, any average, corrected for publication bias or not, cannot take into account how the premium is affected by omitted-variable biases, the number of years married or other factors that are likely to influence its magnitude or bias its estimation. If the typical study contains some net bias (for example, by omitting variable(s) that are positively related to both wages and marriage) and if these omissions are correlated with *Se*, then our corrected estimate will also be biased even after correcting for potential publication

selection. Next, we investigate which factors exert a detectable influence on the reported magnitude of the marriage wage premium or perhaps on their biases.

Multiple Meta-Regression Analysis

Like every other meta-analysis in economics, the conventional Cochran 'Q-test' for heterogeneity shows that there is excess heterogeneity (Q= 572083; df=660; p<.001). The Cochran Q-test is essentially a test of whether the error variance from MRA (3) is statistically larger than one (Stanley and Doucouliagos, 2012). What factors affect the marriage wage premium? Is the premium caused by selection or productivity differences? Is it is changing over time as gender roles have evolved? Can obvious misspecification biases be identified, and their potential effects moderated?

Twenty-eight explanatory variables are coded based on what the literature regards as important and experience derived from past labour economics meta-analysis. Decade dummy variables are of interest because changing social norms could affect the marriage premium, especially if the selection hypothesis is dominant. Thirty seven percent of the estimates are from data collected in the 1960s and 1970s, 64% from the 1980s and 1990s, and 34% from the 2000s. The numbers do not add to 100% because approximately 50% of the studies use panel data which spanned more than one decade. A dummy variable for fixed effects is coded because fixed effects are the primary method for dealing with sample selection. Thirty one percent of the wage equations employ fixed effects panels to control for time-invariant, unobserved, individual effects. Controls are included for both panel data and fixed effects because studies often use ordinary least squares on panel data as a benchmark. Studies that do not control for the number of years a man is married

restrict any wage increase to be a one time intercept shift at the time of marriage; 85% of included studies restrict marriage's effect on wages in this way. Controls for time spent in home production and the labour force participation of the wife are included to shed light on the specialization of labour hypothesis. Controls for self-employment are included because a study may use self-employment as a test of the employer discrimination hypothesis. Divorce is also of interest because the specialization of labour hypothesis and the employer discrimination hypothesis suggest that the premium should disappear once a man becomes divorced. If divorce is a signal of unobserved undesirability, the selection hypothesis would suggest that divorced men earn lower wages than single men. We include a dummy variable for whether the study includes divorce as a reference category; 42% of our studies do so. The measure of wages is potentially important. Married men work more hours on average, so studies that use annual or weekly earnings will capture the effect of these longer work hours even if hourly wages are equal between married and single men. Fifty three percent of studies used the natural log of hourly wages as the dependent variable in the wage regression.

Wage structure and the social value of marriage are likely to vary across country, so we control for whether the estimates were from the United States. Seventy four percent were estimated from U.S. data. The remaining 26% are from Europe (16%), Africa (5%), Asia (3%) and Australia (2%). We also include controls for whether a study omits standard variables in wage regressions. 77% of studies omit whether the individual worked for the government, 80% omitted the worker's union status, 39% omit the workers age, 28% his region, and 65% his tenure. Thirty seven percent omit his experience and 13% his educational attainment. Because of potential publication

selection, we include a control for whether the study was specifically estimating the marriage premium. Finally, because many panel data sets such as the National Longitudinal Surveys collect data on very specific age groups and lifecycle concerns are important in wage regressions, we include a control variable for whether the age range represented was smaller than the conventional 25-64 years. Table 2 defines these 28 variables, and Table 3 reports their summary statistics.

Table 4 is the correlation matrix for the meta-independent variables (excluding the year dummies). Most correlations were small, with a few exceptions. Unsurprisingly, the correlation between fixed effects and panel data is high. The correlation is not equal to one because many studies use ordinary least squares on panel data as a benchmark. Studies that were restricted to white men only were unlikely to omit union status. If worker's industry was included in the study, his occupation was very likely to be included as well, with the correlation between *omit industry* and *omit occupation* of 0.86.

Because the National Longitudinal Surveys have restricted age ranges and include data on job tenure, there is a large negative correlation between *restricted age* and *omit tenure*. Even though the way that experience is often calculated might make it highly correlated with age; *omit age* and *omit experience* are not highly correlated (0.33).

TABLE 2

Meta-Independent Variable Definitions

sixtiesseventies = 1	if the data was from the time period 1960-1979
eightiesnineties = 1	if the data was from the time period 1980-1999
Nineties = 1	if the data was from the time period 1990-1999
Two thousands = 1	if the data was from the time period 2000-2009
fixed effects=1	if the study used fixed effects estimation
panel = 1	if the estimates were obtained using panel data
include yearsmarried = 1	if the study included the number of years that the respondent had been married
include wife participation=1	if the study included a variable for whether the wife participated in the labor force
self employed=1	if the study included a control for self-employment
include housework=1	if the study included a control for time spent in home production
white only = 1	if the sample was restricted to white men only
include divorce = 1	if the study included a dummy variable for whether the worker was divorced
lnHourlywage = 1	if the dependant variable in the regression was the natural log of the hourly wage
usdata = 1	if the data were collected in the United States
omit age = 1	if the study omitted the worker's age
omit experience = 1	if the study omitted the worker's years of job experience
omit kids = 1	if the study omitted whether or not the worker has children
omit occupation = 1	if the study omitted the worker's occupation
omit industry = 1	if the study omitted the worker's industry of employment
omit government = 1	if the study omitted a government/private employment distinction
omit union = 1	if the study omitted the union/nonunion status of the worker
omit region = 1	if the study omitted the worker's geographical region of employment
omit education = 1	if the study omitted the worker's years of education
omit veteran = 1	if the study omitted whether the worker was a Veteran
omit urban = 1	if the study omitted whether or not the worker was employed in an SMSA
omit tenure = 1	if the study omitted the worker's tenure with his current employer
not about marriage = 1	if the study was not specifically about the marriage wage premium
restricted age = 1	if the ages studied in the paper encompassed a smaller range than the standard 25-64

TABLE 3
Summary Statistics for Coded Variables

Variable	Obs	Mean	Std. Dev.	Min	Max
estimate	661	0.121	0.145	-0.385	1.002
Se	661	0.093	0.148	0.001	1.089
sixtiesseventies	661	0.369	0.483	0	1
eightiesnineties	661	0.635	0.482	0	1
twothousands	661	0.337	0.473	0	1
fixed effects	661	0.310	0.463	0	1
panel	661	0.502	0.500	0	1
include yearsmarried	661	0.154	0.362	0	1
include wife participation	661	0.082	0.274	0	1
self employed	661	0.020	0.139	0	1
include housework	661	0.023	0.149	0	1
white only	661	0.301	0.459	0	1
include divorce	661	0.424	0.495	0	1
lnHourlywage	661	0.526	0.500	0	1
usdata	661	0.741	0.438	0	1
omit age	661	0.390	0.488	0	1
omit experience	661	0.368	0.483	0	1
omit kids	661	0.558	0.497	0	1
omit occupation	661	0.549	0.498	0	1
omit industry	661	0.587	0.493	0	1
omit government	661	0.766	0.424	0	1
omit union	661	0.802	0.399	0	1
omit region	661	0.275	0.447	0	1
omit education	661	0.132	0.338	0	1
omit veteran	661	0.896	0.306	0	1
omit urban	661	0.436	0.496	0	1
omit tenure	661	0.654	0.476	0	1
not about marriage	661	0.215	0.411	0	1
restricted age	661	0.442	0.497	0	1

TABLE 4

Correlation Matrix for Meta-Independent Variables

	fe	panel	yrs	wwrk	self	hswrk	white	div	wge	us	age	exp	kids	occ	ind	gov	union	reg	educ	vet	urb	ten	notmar	resage
fe	1																							
panel	0.64	1																						
incl_yearsmarried	0.19	0.29	1																					
incl_wifeparticipation	0.03	0.03	0.04	1																				
selfemployed	-0.07	-0.14	-0.06	-0.04	1																			
incl_housework	0.01	-0.01	0.02	0.10	0.27	1																		
whiteonly	0.00	0.03	0.27	0.23	-0.02	0.06	1																	
incldivorce	0.02	0.05	0.27	0.01	-0.12	0.01	0.26	1																
lnhrlywge	0.12	0.13	0.27	0.02	0.00	0.06	0.33	0.46	1															
usdata	0.02	0.04	0.02	0.08	0.06	0.00	0.33	-0.16	0.08	1														
omitage	-0.09	-0.09	0.15	-0.20	0.11	0.11	0.01	-0.04	0.21	-0.15	1													
omitexp	-0.12	-0.22	0.04	0.14	0.01	-0.12	-0.01	-0.10	-0.24	0.03	-0.33	1												
omitkids	-0.11	-0.04	-0.15	-0.18	-0.09	-0.17	-0.03	-0.23	-0.11	0.14	-0.07	0.03	1											
omitocc	0.02	-0.19	-0.03	0.06	0.04	0.00	-0.15	-0.13	-0.07	0.08	-0.05	0.52	0.04	1										
omitindustry	-0.07	-0.29	-0.08	0.04	0.01	-0.02	-0.20	-0.16	-0.05	0.01	-0.02	0.49	-0.01	0.86	1									
omitgovt	0.05	-0.12	0.12	0.06	0.08	-0.01	0.10	0.08	0.29	-0.05	0.09	0.34	-0.06	0.59	0.49	1								
omitunion	0.00	-0.08	-0.18	-0.12	-0.01	-0.03	-0.55	-0.17	-0.31	-0.22	-0.15	0.21	0.15	0.43	0.44	0.20	1							
omitregion	-0.07	-0.06	0.03	-0.18	-0.09	-0.09	-0.26	-0.03	-0.03	-0.47	0.26	-0.06	-0.02	0.14	0.16	0.10	0.23	1						
omiteduc	0.11	0.31	-0.04	-0.12	-0.06	-0.06	-0.24	-0.21	-0.31	0.17	-0.19	-0.18	0.29	-0.26	-0.29	-0.50	0.19	-0.11	1					
omitvet	0.16	0.22	0.06	0.10	0.05	0.05	-0.16	0.17	-0.07	-0.18	-0.24	0.18	-0.16	0.11	0.08	0.02	0.10	-0.02	0.10	1				
omiturban	0.03	0.17	-0.04	-0.17	-0.12	-0.05	-0.38	-0.19	-0.10	-0.43	0.13	-0.16	0.27	-0.15	-0.13	-0.16	0.27	0.40	0.44	-0.11	1			
omittenure	-0.22	-0.36	-0.01	0.09	-0.08	-0.17	-0.05	0.13	-0.05	-0.28	0.02	0.40	-0.06	0.35	0.45	0.36	0.30	0.23	-0.36	-0.10	0.04	1		
notaboutmarriage	-0.27	-0.43	-0.22	-0.16	0.19	0.12	-0.05	-0.11	0.03	0.04	0.34	0.05	0.17	0.15	0.26	0.05	0.02	0.02	-0.20	-0.24	-0.13	0.15	1	
restrictedage	0.24	0.50	0.18	-0.05	0.05	-0.14	0.13	-0.02	0.17	0.31	-0.03	-0.27	0.13	-0.33	-0.43	-0.16	-0.17	-0.13	0.33	0.00	0.02	-0.52	-0.25	1

To diminish the possibility of omitted variable bias in our MRA, all 28 explanatory variables were included in the initial regression. In order to minimize specification searching and its own potential bias, we systematically drop the variable with the least explanatory power in a given regression until all variables are statistically significant. Thus, not all variable listed in Table 2 appear in Table 5. This 'general-to-specific' process yields a multiple MRA that explains over 77% of the weighted variation and 97% of the raw variation among the reported estimates of the male marriage wage premium (Table 5).

The WLS-MRA that is reported in Table 5 column 1 provides very clear evidence that variables included (or omitted) in a researcher's wage equation and the methods and data she chooses can have a large effect on the reported marriage premium. In particular, using US data ($t=9.9$; $p< .001$), using a restricted age range ($t=-8.8$; $p< .001$) or including years of marriage ($t=-6.5$; $p< .001$) has a substantial effect on the reported estimates. In general, omitted-variable bias is an important dimension in this area of research. Together, these omitted variables (*include years married, omit occupation, omit union, omit region, omit education, omit veteran* and *omit urban)* are responsible for a substantial proportion of our MRA's explanatory power ($F_{(7,646)}= 50.29$; $p<.001$).

However, studies typically report more than one estimate in this research literature. As a consequence, estimates within a study cannot be assumed to be independent of one another. To accommodate potential within study dependence, we report cluster-robust standard errors in column 2 of Table 5, random-effects panel estimates in Column 3, and fixed-effects panel estimates in Column 4. The Hausman test

Table 5. Multiple Meta-Regression Results
(Dependent variable is the estimated marriage wage premium)

Variables	WLS	Cluster-Robust	RE Panel	FE Panel
	(1)	(2)	(3)	(4)
Constant	0.045*	0.045*	0.037*	0.064*
	(3.82)	(2.62)	(2.75)	(2.75)
Se	0.937*	0.937*	0.936*	-0.411
	(5.33)	(2.58)	(3.31)	(-1.47)
two thousands	0.034*	0.034*	0.033*	0.040*
	(6.55)	(4.84)	(5.24)	(4.49)
fixed effects	-0.021*	-0.021*	-0.019*	-0.016*
	(-6.49)	(-5.42)	(-6.17)	(-4.95)
restricted age range	-0.044*	-0.044*	-0.036*	-0.030*
	(-8.45)	(-3.43)	(-6.78)	(-4.47)
panel	0.013*	0.013*	0.013*	0.002
	(3.11)	(2.02)	(3.05)	(0.41)
include years married	-0.015*	-0.015	-0.014	-0.012
	(-6.47)	(-1.48)	(-6.38)	(-5.78)
usdata	0.072*	0.072*	0.079*	0.086*
	(9.90)	(5.13)	(9.49)	(7.37)
omit occupation	-0.016*	-0.016*	-0.009	0.035*
	(-3.06)	(-2.20)	(-1.42)	(3.75)
omit union	0.055*	0.055*	0.050*	0.011
	(6.55)	(3.69)	(4.97)	(0.81)
omit region	0.016*	0.016	0.019	0.035*
	(3.57)	(1.49)	(3.23)	(3.68)
omit education	-0.062*	-0.062*	-0.054*	0.009
	(-5.29)	(-3.46)	(-3.69)	(0.36)
omit veteran	-0.032*	-0.032*	-0.039*	-0.080*
	(-3.90)	(-2.49)	(-4.04)	(-5.79)
omit urban	-0.033*	-0.033*	-0.021*	0.016
	(-3.97)	(-2.16)	(-2.17)	(1.13)
not about marriage	0.032*	0.032*	0.032*	0.053*
	(3.94)	(2.35)	(3.19)	(3.107
Adjusted R^2	0.772	0.772	0.831	0.649

Notes: Cell entries in parentheses report t-statistics. *denotes statistically significant at least at the 5% level. Adjusted R^2 are in term of weighted sums of squares or variations among the reported t-values. The number of observations is 661. All estimates use weighted least squares (WLS), with the inverse variance as weights. Columns (2)-(4) test the robustness of the basic WLS findings and accommodate within-study dependence using: cluster-robust standard errors (2), random-effects panel (3), and fixed-effects panel methods (4).

is rejected in favor of the fixed-effects panel model, column 4 ($\chi^2_{(14)}$ =116.8; p<<.001). We concentrate our discussion on those factors that are robust across estimation approaches.

Including fixed effects lowers the wage premium by approximately 2%, suggesting that individual specific characteristics and selection into marriage do play a role in the wage premium, but are not the primary cause of the reported magnitudes. That is, although fixed-effects panels do cause a statistically significant reduction to the male marriage wage premium, it is small relative to the overall average of approximately 12%. The magnitude the coefficient on *usdata,* 7.2%- 8.6%, is also worthy of note. It suggests that the marriage wage premium may be largely a US phenomenon.

However, the single most influential factor is publication selection bias. We estimate that researchers' selection of statistically positive marriage wage premiums inflates the magnitude of the overall marriage wage premiums by 8.7%. When combined with intercept of the multiple MRA, the overall reported mean (12%) is fully explained. In seeming contradiction to this interpretation, the publication bias term, *Se*, is not statistically significant in the fixed-effects panel model, column 4. However, the interpretation of *Se* is different for panel models. In all cases, we use WLS. As a result, the study-level effects in both panel models represent differences in *Se*'s coefficients or, equivalently, differential publication selection bias. We believe that this is the appropriate way to characterize labour research because personal experience indicates that not all studies select which empirical results to report by suppressing some findings. Individual estimated study effects from the fixed-effects panel model suggests that 61% of the studies engage in significantly notable positive selection. With the fixed-effects panel, we still find that there is considerable publication selection bias, but its magnitude varies by study. Further note that random-effects panel estimate of the average amount publication selection is the same as the WLS estimate discussed above.

The meta-regression findings reported in Table 5 allows us to estimate the magnitude of the marriage wage premium were none of the relevant covariates omitted. We define our benchmark study as one which uses U.S. data, a fixed-effects panel model to control for individual characteristics, does not omit any potentially relevant variable, engage in selection for statistical significance, or rely upon a restricted age range. The WLS-MRA model reported in Table 5 column 1 implies that the one-time intercept shift from marriage is approximately 9.4% {CI= (7.3%; 11.4%)} for this benchmark study. That is to say that US married men get approximately a 9% wage bonus for being married. Thus, after correcting for potential misspecification and selection biases, the corrected marriage wage premium for US men is only a few percent lower than the reported overall average marriage wage premium, 12.1%. Married men in other countries do not seem to be so lucky. Their wage premium is only 2.2%, and the confidence interval encompasses zero {CI= (-0.2%; 4.6%)}. When we use the fixed-effects panel estimates, column 4, the corrected marriage wage premium for US men returns to the overall average, 12.2% {CI= (9.1%; 15.4%)}, but the premium for men from other countries, 3.6% , is again not significantly different than zero—{CI= (-0.1%; 7.4%)}.

Any overall estimate of the marriage wage premium depends on how the benchmark research study is defined. If we include the most recent data from the twenty-first century as part of our definition of the benchmark, then the overall estimate of the marriage premium increases by either 3.4% or 4%, giving either 12.8% or 16.2% for the US men and 5.6% or 7.6% for nonUS married men. Even after correcting for ubiquitous misspecification and selection biases, there remains a notable marriage wage premium.

Furthermore, the marriage wage premium does not seem to be disappearing, because it is significantly higher in the most recent data.

Discussion

What explains our findings? To address this question, let us review the three main explanations found in this literature for the existence of a positive male marriage-wage premium. The first is 'selection.' This hypothesis suggests that certain men are more desirable as both as mates and as employees due to some factor unobservable to the researcher but not to employers or women. Fixed-effects estimation is the traditional method to control for such unobservable characteristics. Our MRA shows that selection does play a role, because studies that included individual fixed effects found estimates of the wage premium that were approximately 2% lower than those that did not. However, this does not completely explain the reported wage premium, because it is typically much larger than 2%.

The second major hypothesis, 'specialization,' is that married men are more productive than single men; that is, marriage has a causal effect on both productivity and wages. This enhanced productivity could be the result of many factors. Married men might be better at work because their wives specialize in home production, freeing husbands to specialize in market work. The importance of years married is compatible with this explanation. We expect that couples would perfect their household roles over time. Thus the specialization hypothesis is consistent with the direct evidence from our meta-analysis. However, the MRA coefficient is small, just over 1%, which implies that perfecting these gender roles has only a small effect on the overall marriage wage premium.

Furthermore, the observed persistence and increase of the marriage premium over time provides some indirect evidence that the specialization hypothesis may not provide a full explanation. If specialization of labour within the home is the main cause of the wage premium, it stands to reason that the premium would decrease as more women enter the labour force and generally spend less time in home production. In addition, as divorce rates rise, it becomes more costly for a woman to sacrifice her own career so that her husband can better specialize in his. When controls are added for relevant study characteristics, there is a significantly higher marriage wage premium in the 2000s (recall Table 5), in seeming contradiction to obvious social trends. This calls into question whether specialization of labour within the home is the primary cause of the marriage-wage premium. To maintain the marriage specialization hypothesis in the face of these clear demographic trends requires a plausible intervening force such as a technological change in home production which continues to support the gradual acquisition of a male marriage wage premium.

Other direct tests of the specialization hypothesis have also found it to be an incomplete explanation of the wage premium. Loh (1996) uses the wife's labour force participation as a proxy for specialization within the home and finds that the marriage premium does not diminish when this control is added. Likewise, our meta-analysis finds that the inclusion of the wife's labour force participation across the literature does not affect the reported marriage-wage premium. Hersch and Stratton (2000) include self-reported information on time spent by men in nine different household production activities as a measure of household specialization. They find very little difference in the amount of time spent on home production by married and single men, and the inclusion

of these variables do not affect estimates of the marriage premium. Our meta-analysis confirms that controlling for time spent in home production has little effect on the marriage wage premium estimate.

In a related hypothesis, married men may also be more productive because they invest more in human capital than their single counterparts in part because of the financial investment of their wives (Kenny, 1983). As divorce rates have increased over time, investments in one's spouse's human capital become less appealing. If this were the primary cause of the premium, we would also expect it to be declining over time.

Increased productivity may also be the result of the stronger labour force attachment of married men. If marriage causes men to 'settle down,' be more stable, and focused on work and career, this additional commitment may be the root cause of higher productivity and wages. It is reasonable for these factors to increase with years of marriage as well, because marriage duration will roughly correspond to an increased likelihood of addition of children. This 'married with children' explanation is also consistent with a stable marriage premium over time because society's changing gender roles within and outside the home need not lessen a man's commitment to his family.

Even if married men are not significantly more productive than their single male counterparts, employers might use marriage as a signal of stability. Employers may believe that married men are more stable and more likely to remain with the firm long term, thereby saving the employer future training and hiring costs. If so, discrimination towards married men might be the source of this premium, whether or not there are, in fact, actual productivity differences. If employers perceive married men to be more

'stable,' a positive male marriage wage premium that increases with years married might result, which is what this meta-regression analysis reveals.

Conclusion

Our meta-regression analysis (MRA) finds a sizable and rather stable male marriage wage premium for US workers once misspecification and selection biases are filtered. Overall estimates depend on one's definition of best-practice research, which define the benchmark. However, a reasonably general benchmark finds that there is a sizeable marriage wage premium in the US (9.4% or 12.8% for the twenty-first century); however, it is much smaller or nonexistent in nonUS labour markets. Nonetheless, this simple summary of the research on the male wage premium is easily undermined by the complex interplay of effects found in this area of research. Our meta-regression analysis identifies that differential omitted-variable biases explain a substantial portion of the variation found in this research literature. Yet, publication selection bias is even more responsible for the relatively large value values of the marriage premium reported in this literature. When our MRA model is used to filter out these potential omitted-variable and selection biases, a notable male marriage wage premium remains for US workers. Perhaps, the marriage wage premium is an American institutional/cultural phenomenon? In any case, it varies significantly across countries, time and the choices that individual researchers make.

Furthermore, we find no evidence that the marriage-wage premium is declining over time as would be expected in the 'specialization' hypothesis when there are no compensating shifts in technology. This is the view that married men are more productive due to the more efficient specialization within the home. If this explanation

were true, the well-documented changes in gender roles and divorce rates over recent decades would be expected to gradually lessen a 'specialization' marriage-wage premium. Yet, after likely biases are controlled for, the marriage-wage premium appears to have *increased* in the twenty-first century (note the coefficients on *two thousands* in Table 5). Thus, on balance, indirect evidence from our meta-analysis also casts doubt on the 'specialization' hypotheses while supporting the 'married with children' view. No doubt, further detailed analysis is still required to uncover the more nuanced complexities that might underlie the associated socio-economic trends in marriage.

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