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# Is There a Male Marital Wage Premium? New Evidence from the United States

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## Abstract

This study reconsiders the phenomenon that married men earn more money than unmarried men, a key result of the research on marriage benefits. Many earlier studies have found such a “male marital wage premium.” Recent studies using panel data for the United States conclude that part of this premium is due to selection of high earners into marriage. Nevertheless, a substantial effect of marriage seems to remain. The current study investigates whether the remaining premium is really a causal effect. Using conventional fixed-effects models, previous studies statistically controlled for selection based on wage levels only. We suggest a more general fixed-effects model that allows for higher wage growth of to-be-married men. The empirical test draws on panel data from the National Longitudinal Survey of Youth (1979 to 2012). We replicate the main finding of the literature: a wage premium remains after controlling for selection on individual wage levels. However, the remaining effect is not causal. The results show that married men earn more because selection into marriage operates not only on wage levels but also on wage growth. Hence, men on a steep career track are especially likely to marry. We conclude that arguments postulating a wage premium for married men should be discarded.

## Keywords

marriage, earnings, life course, career development, fixed effects

In their review of the literature on marriage benefits, Waite and Gallagher (2000:99) single out the fact that husbands earn more than unmarried men. In their view, “the data leave little room for doubt: Marriage itself makes men more successful. In fact, when it comes to earnings, for men, getting and keeping a wife may be as important as getting an education. . . . The wage premium married men receive is one of the most well-documented phenomena in social science.” Indeed, ample evidence from cross-sectional studies documents large marital wage differentials across time and space. In the United States, in the

1890s, married men already earned about 15 percent more per week than unmarried men (Eichengreen 1984), and data from the 1970s, 1980s, and 1990s show a wage difference of 25 to 30 percent per hour worked (Cohen

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2002; Hill 1979). Schoeni (1995) reports differentials of up to 40 percent for men's annual earnings in 15 industrialized countries.<sup>1</sup>

For decades, scholars have debated whether a causal interpretation of such differentials in terms of a "marital wage premium" (MWP) is appropriate. To explore this question, researchers have conducted longitudinal studies for several countries using panel data and fixed-effects (FE) methodology to control for selection of high earners into marriage. In fact, studies for Scandinavia do not show substantial marriage premiums with standard FE models (for Sweden, see Isacson [2007]; for Denmark, see Gupta, Smith, and Stratton [2007]; for Norway, see Petersen, Penner, and Hogsnes [2011]). However, Scandinavia seems to be an exception. Studies for the United States (Ahituv and Lerman 2007; Hersch and Stratton 2000; Killewald and Gough 2013) do report a substantial MWP even with FE models, and similar results have been found for European countries such as Great Britain (Bardasi and Taylor 2008) and West Germany (Pollmann-Schult 2011).

This remaining effect (found with FE models) is usually interpreted as a causal effect and is often attributed to gender role specialization among married couples (Grossbard-Shechtman and Neuman 2003). Theoretically, the specialization hypothesis is a fundamental implication of family economics (Becker 1991). Practically, an MWP would have important consequences for individuals, families, and society at large. First, it would be a crucial factor in the wage structure. Secular changes in marital behavior (decline of marriage rates, increasing divorce rates) might thus have had a detrimental impact on men's wages and careers. Second, the MWP would, to some degree, counterbalance the wage penalty associated with motherhood (see Budig and England 2001; Gough and Noonan 2013), thereby increasing family welfare and fertility.

However, convincing direct evidence supporting the claim of a marriage premium is lacking (Killewald and Gough 2013), and

there are alternative mechanisms that might explain the finding. Moreover, results of some studies cast doubt on a causal interpretation (Dougherty 2006; Killewald and Lundberg 2017; Loughran and Zissimopoulos 2009).

In the present article, we argue that existing studies may have failed to account fully for selection. We discuss arguments for causal and spurious effects, and we derive hypotheses regarding husbands' wage trajectories before and after getting married. We argue that specialization implies an effect on the wage growth after marriage, whereas alternative mechanisms imply an effect only on the wage level. Both the level and the steepness of the wage trajectory of married and never-married men may differ already prior to marriage.

Consequently, we ask whether there is really an MWP in the United States or whether the observed premium remaining with FE is merely due to the higher wage growth of to-be-married men. Hence, we address the longstanding methodological concern that selection into marriage may be related not only to high wages but also to strong wage growth (Akerlof 1998; Cheng 2016; Dougherty 2006; Killewald and Gough 2013; Killewald and Lundberg 2017; Korenman and Neumark 1991; Krashinsky 2004; Loughran and Zissimopoulos 2009).

We first compare the wage trajectory of husbands before and after first marriage to the wage profile of never-married men. Second, we introduce a fixed-effects model with individual-specific slopes (FEIS) that allows for heterogeneity with respect to the steepness of wage trajectories. Using this model, we estimate the average MWP. Third, we extend the model to analyze the time-path of the premium for up to 15 years during first marriage. For the empirical test, we use panel data from the National Longitudinal Survey of Youth (NLSY79, waves 1979 to 2012).

The results show no evidence of a causal effect of marriage on men's wages once we control for the steepness of men's careers. Thus, our analyses do not support the argument that gender role specialization speeds up men's careers. Rather, heterogeneous wage

growth accounts for previous studies' finding of an MWP. Our results are robust to various sensitivity checks. We conclude that one of the key findings on marriage benefits is probably wrong: there seems to be no marital wage premium for men in the United States. Given that marital selection matters for other marriage benefits as well, these should also be reconsidered.

## EXPLAINING MARITAL STATUS WAGE DIFFERENTIALS

The most often cited explanation of the MWP is gender role specialization among married couples. Proponents of the premium argument generally agree that a traditional marriage is a precondition for men to reap earnings benefits (Grossbard-Shechtman and Neuman 2003; Waite and Gallagher 2000). However, direct evidence supporting the specialization argument is weak, and alternative mechanisms have been put forward to explain the wage differential. Furthermore, several arguments point to a selection process of high earners into marriage.

### *Arguments for a Causal Interpretation*

Starting from Becker's (1985, 1991) model of the division of labor within households, this literature commonly assumes that husbands specialize in breadwinning and wives mainly take care of housework and childcare. Building on this assumption, scholars have inferred that married men invest in market-specific skills at a higher rate (Kenny 1983). Any gains from increased investment come into effect only over time, so the specialization hypothesis states that husbands outearn unmarried men because their wages grow faster after entry into marriage. As Figure 1a (short-dashed line) shows, gender role specialization should increase wages gradually, not immediately after marriage, because higher investment in market skills needs time to yield

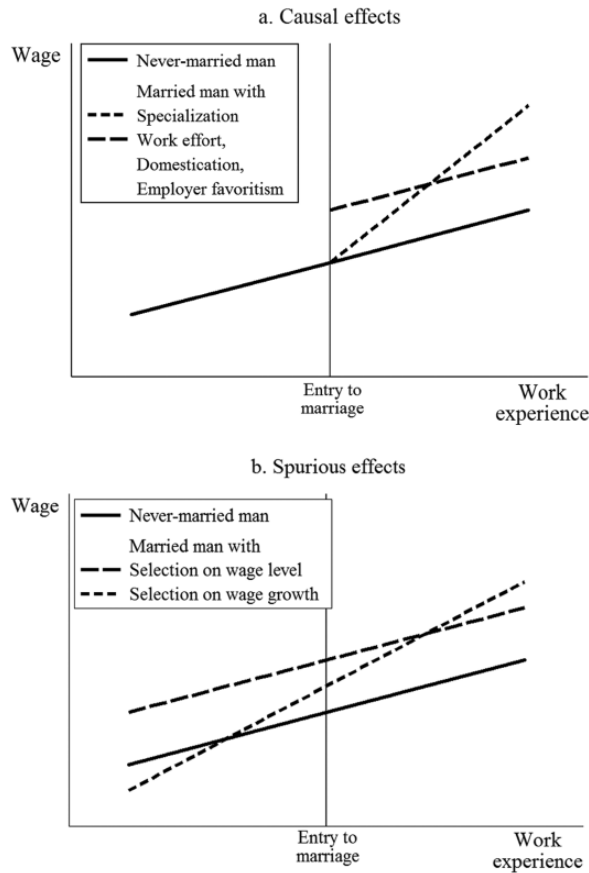
returns. Indeed, Korenman and Neumark (1991:293) argue that "there is no reason to suspect that the gains from marriage are reaped upon utterance of the words 'I do.'"

The literature generally agrees that, to some extent, gender role specialization makes husbands more productive workers, but not all proponents of the premium argument would subscribe to the view that there are no wage gains in the early years of a marriage. Notably, alternative explanations of the MWP predict a wage jump right after marriage, that is, an immediate and time-constant positive effect on the wage level (see long-dashed line in Figure 1a).

Several mechanisms could produce such an effect: the work effort hypothesis, husbands' domestication, or employer favoritism. The work effort hypothesis, derived from Becker's (1985) model of the allocation of effort, holds that husbands are relieved from strenuous housework by their wives and therefore concentrate all disposable effort on breadwinning (Hersch and Stratton 2000; Stratton 2002). Thus, even if married men do not acquire more knowledge and skills than unmarried men, they may still work harder.

According to the domestication hypothesis, husbands earn more than single men because, at the time of marriage, men "settle down" and adopt a more solid life-style. Husbands may work harder because their behavior is being watched over by a "nagging wife" (Waite 1995; Waite and Gallagher 2000), and this does not necessarily presuppose a gendered division of domestic work.

A time-constant wage increase would also be observed if married men were to benefit from employer favoritism (Grossbard-Shechtman and Neuman 2003; Waite and Gallagher 2000). According to this line of reasoning, men's wages increase after marriage because employers believe that male breadwinners deserve more money to meet the financial needs of family life (Hill 1979), or because employers erroneously believe that married men are more productive workers and discriminate against unmarried men for "statistical" (rather than "taste") considerations



**Figure 1.** Hypotheses on the Effect of Heterosexual Marriage on Men’s Wage Trajectory  
 Source: Authors’ compilation.

(Siebert and Sloane 1981). In this case, a wage premium would show up despite husbands being no more productive at the job than single men.

To sum up, we expect a positive effect of marriage on men’s wage growth if the specialization hypothesis holds true, but an effect only on the level of wages for any of the other three causal claims.

*Arguments for a Spurious Association*

The alternative to these causal claims is the selection hypothesis, that is, men with high wages are more likely to get married. It is a well-established fact that men with higher wages marry at higher rates (see

Oppenheimer 2000, 2003; Oppenheimer, Kalmijn, and Lim 1997; Sweeney 2002; Schneider [2011] shows a positive association with men’s wealth). Search models of the marriage market assume that financially successful men are especially attractive marriage candidates and that high earners may also be more willing to marry, due to anticipation of the financial needs of family life (Oppenheimer 1988). Furthermore, these men might be selected into marriage not only due to their wages, but also due to unobservable traits rewarded in the labor market, such as physical attributes (beauty, health), personality (antagonism, self-esteem, extraversion, neuroticism), or social skills (communication, conflict resolution). Hence, even if there is no causal effect of marriage, the mating process

might nevertheless produce a spurious correlation of marriage and wages. The implications of selection arguments for the time-path of the marital wage differential have not been specified precisely in the literature. Most studies implicitly assume that marriage is associated with men's earnings level (see Figure 1b, long-dashed line), and that controlling for a time-constant pay differential will therefore suffice to estimate the causal effect of marriage.

From a search-theoretic perspective, one could argue that women prefer to marry men who are on a steep career trajectory, because men's higher wage growth raises expected marital income (keeping his current wage level constant).<sup>2</sup> Alternatively, unobserved variables might influence men's wage growth and their marriage decisions. Empirical analyses in fact show that men's earning prospects are related to entry into first marriage (Xie et al. 2003). Hence, men on a steep career trajectory marry at a higher rate. In this case, depicted in Figure 1b (short-dashed line), future husbands' wages grow stronger than never-married men's wages already before marriage. Although wages might still grow stronger after marriage, marriage itself affects neither their level nor their growth. We term this the *promising men* hypothesis to indicate there might be selection into marriage based on the wage growth of potential husbands. The implication is that methods controlling for selection on wage levels will produce spurious effects. Therefore, results that have often been interpreted as demonstrating a causal effect of marriage might be explained by selection.

## PREVIOUS EMPIRICAL STUDIES

Early cross-sectional studies' finding that married men are more successful workers (see Hill 1979) was always suspected to be spurious. Researchers came up with innovative research designs and statistical models to rule out selection bias and to further discriminate between causal mechanisms. Several

studies tried to identify the causal effect using cross-sectional data and imposed auxiliary assumptions on the data. However, these assumptions were probably not met, because results often turned out very similar to those obtained by OLS (Antonovics and Town 2004; Chun and Lee 2001; Ginther and Zavodny 2001), and longitudinal studies have shown that OLS estimates are heavily biased.

A robust finding across longitudinal U.S. studies is that part of the MWP is eliminated when unobserved time-constant individual traits are controlled using panel data and FE regression. To evaluate the size of the cross-sectional bias, several studies compare the effect of marriage between pooled OLS (POLS) and standard FE regression models. Dougherty (2006) used NLSY79 data (waves 1979 to 1998) and found an average MWP of 16 percent with POLS, which reduced to 6 percent in his FE model. Also using NLSY79 data (waves 1979 to 2004), Ahituv and Lerman (2007) estimated a first-year marriage premium of 17.5 with POLS and 8 percent with FE. This result is remarkably similar to the 9 percent effect at entry into marriage that Hersch and Stratton (2000) found with a shorter two-wave panel dataset of the National Survey of Families and Households (NSFH, waves 1987–1988 and 1992–1994). According to these results, more than half of the marital wage differential in the United States is due to selection of high earners into marriage. Nonetheless, a substantial marital premium remained in all these studies even after controlling for selection on wage levels. Consequently, researchers aimed to investigate the causal mechanisms that might produce the MWP.

Some authors have tried to test the work effort hypothesis more directly by including men's housework hours in their regression models. The results, however, were negative throughout. Hersch and Stratton (2000) and Pollmann-Schult (2011) show that, although men report slightly lower housework hours after marriage, the decrease is much too small to explain the strong wage effect of marriage. As noted earlier, domestication might explain why the marital wage premium is not

eliminated in wage regressions controlling for men's hours of housework. Empirically, married men report they engage less often in risky behavior, and they exhibit less substance abuse and lower propensities for deviant behavior than do single men (Akerlof 1998; Duncan, Wilkerson, and England 2006; Sampson, Laub, and Wimer 2006). To our knowledge, however, no studies directly show that a change in life-style after marriage mediates the wage premium.

Killewald and Gough (2013) also tried to test the specialization argument directly. The authors used nested FE models to analyze how much of the MWP is explained by individual differences in the accumulation of human capital. However, their results show that less than one third of the premium is due to married men's extra work experience and tenure.

A crucial implication of the specialization argument is that the MWP increases with years married (as in Figure 1a, short-dashed line). This implication has received some empirical support from U.S. studies. Early studies relied on cross-sectional data (see Loh 1996) or on panel data spanning only a few years of young men's lives (Akerlof 1998; Korenman and Neumark 1991; Stratton 2002). It is now possible to study the time-path of the MWP more closely. Ahituv and Lerman (2007) not only find a large wage jump at entry into marriage, but also a smaller premium for the first year of marriage than for subsequent years. Dougherty (2006), Cheng (2016), and Killewald and Lundberg (2017) further elaborate on this: all three studies use NLSY79 data and estimate distributed FE models that allow for an MWP that varies over marriage duration. These studies show that married men's wages grow faster than the wages of never-married men. Furthermore, the higher wage growth was found not only within marriage (as expected by the specialization hypothesis) but also for several years before marriage (as predicted by the promising men hypothesis).

Despite the similarity of the estimated time-path of the MWP in these studies, the authors' interpretations differ markedly.

Cheng (2016) argues that the pattern should be interpreted as evidence of a causal MWP that operates to some extent already before marriage, due to anticipation. Killewald and Lundberg (2017) argue that the MWP is spurious, arising mainly because of higher wage growth during the years just before and after marriage. According to Killewald and Lundberg, this might indicate that marriage occurs simultaneously with the transition to adulthood (notably, gradual transition to steady work). Their study also provides evidence against Cheng's anticipation argument, as they find that "shotgun marriages" do not show a smaller premium compared to "non-shotgun marriages." Finally, Dougherty (2006) argues that the actual time-path indicates that both specialization and selection on wage growth are at work. In fact, his results show that the MWP arises long before marriage, as the effect of marriage is at 6 percent five years prior to marriage. This finding is at odds with both interpretations in terms of anticipation and the transition to adulthood.<sup>3</sup>

The main problem with the distributed FE model is that it does not allow estimation of the MWP after controlling for any difference in (counterfactual) wage growth between never-married and married men (that would be observed even if none of them got married). The model merely describes differential wage growth before and after marriage, but it does not estimate the causal effect of marriage net of selection on wage growth.

In summary, using panel data and standard linear FE regression models, research has demonstrated that cross-sectional results overestimate the effect of marriage. Yet these studies still found a substantial effect. Thus, most authors tried to test more closely the *causal* mechanisms that explain the MWP (some even conducted qualitative research to this end, see Ashwin and Isupova 2014). Our reading of the literature is that previous studies by and large did not succeed in distinguishing between explanations.

In fact, we argue that such an explanation may not be necessary, because the marital wage premium found with standard FE

models might be spurious. Once we allow for individual patterns of *selection* into marriage in a less restrictive way than has been done in existing quantitative studies, there will be no MWP. Hence, there is no need for a causal interpretation. One study is indicative of this: Loughran and Zissimopoulos (2009) estimated an FE model on first-differenced data in order to relax the parallel trends assumption necessary for standard FE estimation. In this study, the marital premium of 11 percent found with FE vanished completely once differences in individual wage growth were taken into account.

## ANALYTIC STRATEGY

From a methodological point of view, our objective is to rule out the potential bias of the MWP induced by marital selection that is based on men's wage growth. We aim to estimate the effect of marriage on wages that is unbiased in the presence of more general patterns of selection than are allowed by standard FE methods. In the following, we present a statistical model that serves this purpose.

### *The Standard Approach: Selection on Wage Level*

Standard FE regression has been widely used in the literature to estimate the MWP. The advantage of the FE model is that it controls statistically for stable individual unobserved characteristics that are related to the outcome and the treatment. Suppose men's wages are given by

$$\ln w_{it} = \alpha_2 \exp_{it} + \beta m_{it} + \alpha_{1i} + \varepsilon_{it}. \quad (1)$$

In this model, the natural logarithm of the wage of person  $i$  at time  $t$  ( $\ln w_{it}$ ) is a linear function of labor market experience ( $\exp_{it}$ ) and a marriage dummy ( $m_{it}$ , 0 before marriage, 1 after marriage). The model specifies that a man receives an (immediate and permanent) wage premium of approximately  $\beta$  percent after marriage. The exact percentage effect equals  $(e^\beta - 1) \times 100$ . It is further

assumed that a man's wage increases by  $\alpha_2$  percent with an additional year of experience. The wage level  $\alpha_{1i}$  is an individual-specific constant, that is, it is assumed time-constant but allowed to vary between individuals as indicated by the subscript  $i$ . The individual intercept thus subsumes all time-constant variables that affect a man's wage in the same way over time. Time-varying variables (other than marriage and experience) that influence the wage are captured by the idiosyncratic error term  $\varepsilon_{it}$ .

If selection into marriage is related to wage levels, estimation of this model by POLS yields biased estimates because  $\alpha_{1i}$  is then left to the error term. To solve this problem, estimation by FE regression relies on a within-transformation of the data (called "demeaning"), where for each variable the person-specific mean is subtracted from the actual values. Because the mean of a time-constant variable is identical to its actual values, the individual intercepts are eliminated by the transformation. As a consequence, selection based on differences in wage levels between individuals does not affect the estimate of the marriage premium. Most recent studies use this standard FE approach to reduce selection bias.

Nevertheless, the FE model requires strict exogeneity of the covariates. As is well-known, strict exogeneity of a treatment indicator does not hold if the assumption of parallel trends is violated (Allison 1990; Angrist and Pischke 2009: Chap. 5; Morgan and Winship 2007: Chap. 9). In Figure 1b, the long-dashed line runs parallel to the solid reference line. Substantively, this means the wages of to-be-married and never-married men may differ in levels, but they grow at the same rate. Thus, their wages would have developed parallel had there been no treatment. The parallel trends assumption is violated, however, in the presence of selection on wage growth (Figure 1b, short-dashed line), and thus conventional FE estimates would be biased (see Part A of the online supplement for details on the parallel trends assumption and its violation).

### *Extending the Standard Approach: Selection on Wage Level and Growth*

Consider the model

$$\ln w_{it} = \alpha_{2i} \exp_{it} + \beta m_{it} + \alpha_{1i} + \varepsilon_{it}. \quad (2)$$

This model differs from Equation 1 in that the steepness of wage trajectories  $\alpha_{2i}$  varies across individuals. What happens if we nevertheless estimate a standard FE model? Clearly, de-meaning eliminates  $\alpha_{1i}$  but not  $\alpha_{2i}$ . Hence, if  $\alpha_{2i}$  is associated with  $m_{it}$ , the effect of marriage will be biased. Specifically, the estimate of  $\beta$  will be biased upward if men with strong wage growth are the most promising marriage candidates.

Applying a modified within-transformation to the data, however, removes the bias. The intuition is as follows. The conventional FE model de-means the data. Using individual means of the wage, that is, a time-constant estimate of the unobserved individual wage potential, wipes out  $\alpha_{1i}$ . The effect is unbiased if wages are strictly exogenous,  $E(\varepsilon_{it} | \exp_{it}, m_{it}, \alpha_{1i}) = 0$ . In contrast, the FE model with individual-specific constants and slopes (FEIS) de-trends the data using a time-varying estimate of a man's wage potential. FEIS thus requires a weaker form of strict exogeneity,  $E(\varepsilon_{it} | \exp_{it}, m_{it}, \alpha_{1i}, \alpha_{2i}) = 0$ , because the model conditions on men's wage growth in addition to individual wage levels.

The procedure to estimate the FEIS model is straightforward (see Wooldridge 2010:377–81; for more details, see Brüderl and Ludwig 2015). (1) For each person  $i$ , estimate the individual wage trajectory by an OLS regression of  $\ln w_{it}$  on a constant and work experience  $\exp_{it}$ . (2) Get the residuals. These are the de-trended wages of person  $i$ . (3) Repeat steps (1) and (2) for the covariate(s). (4) Pool the resulting data and estimate an OLS regression.<sup>4</sup> Note that FEIS estimation is “data hungry” because it requires at least  $j+1$  person-years per man to estimate the individual wage profile in step (1), where  $j$  is the number of individual slope parameters plus the individual intercept. In our analysis, we

include a squared term of work experience in addition to the linear term and the individual intercepts. Estimation therefore requires at least four observations per person.

The conventional FE model is a special case of the FEIS model where the unobservables related to marriage can be subsumed in the individual intercepts (they do not interact with work experience). As a consequence, if selection operates on wage levels only, FE and FEIS produce identical point estimates. In our analyses, we will compare results of the FEIS model to results obtained by FE and POLS to assess the magnitude of the selection bias.

### *More Restrictive Versions of FEIS*

The FEIS model is a generalization of the model used by Loughran and Zissimopoulos (2009) to analyze the MWP. Loughran and Zissimopoulos estimate an FE model on first differences of the data (FEFD), which is equivalent to POLS estimated with second differences (SD), also called the “random trend” model (Wooldridge 2010). In the current research context, FEIS has two important advantages compared to that approach. First, unlike the FEFD model, it does not require ensuing person-years but allows for gaps in the individual panels. Therefore, FEIS is much more efficient in panel analyses of individual wages, which typically have many gaps due to non-employment and missing values. Loughran and Zissimopoulos (2009) resorted to interpolation to circumvent the problem. Second, the FEIS framework allows one to model the individual wage trajectory over labor market experience rather than over calendar time. It is therefore closer to labor market theory.

Morgan and Winship (2007: Chap. 9) argue that the standard FE model should be extended to allow for differences in outcome growth between treatment groups. Hence, it would not be necessary to control for individual growth curves using the FEIS model. A more parsimonious model would be the following:



$$\ln w_{it} = \alpha_{20} \exp_{it} + \alpha_{21} \text{treat}_i \times \exp_{it} + \beta m_{it} + \alpha_{1i} + \varepsilon_{it}. \quad (3)$$

In this model,  $\text{treat}_i$  is a time-constant dummy variable indicating whether a man eventually marries (an indicator of the treatment group). Hence,  $\alpha_{20}$  is the parameter estimating the steepness of the wage profile for men who do not marry, and the coefficient of the interaction term,  $\alpha_{21}$ , captures the higher wage growth of men who do marry. Rather than controlling for heterogeneous growth on the individual level, the extended FE model with group-specific slopes (FEGS) thus allows for mean differences in wage growth between treatment groups. We include FEGS results in our model comparison to investigate whether this more parsimonious model is sufficient.

The FEGS model takes an intermediate position between the FE and FEIS models. It provides unbiased estimates of the marital wage premium if  $E(\varepsilon_{it} | \exp_{it}, \text{treat}_i, m_{it}, \alpha_{1i}, \alpha_{20}, \alpha_{21}) = 0$ . The assumption is less restrictive than the strict exogeneity condition required for FE, but more restrictive than the one needed in the FEIS model. With FEGS, the assumption of parallel trends is replaced by the assumption of common trends in the two treatment groups. This means that, had there been no treatment, the slopes of the wage trajectories are allowed to differ between the two groups, as long as individual differences in slopes are adequately captured by a common treatment group-specific trend. If our argument is correct that entry to marriage is selective with regard to wage growth, we should see a smaller effect of marriage in the FEGS model than in the FE model.

However, the FEGS estimate would still be biased if the assumption of common trends is too strict. More precisely, the FEGS estimator of the MWP would be biased if individual deviations from the group-specific slopes of work experience are systematically related to marriage timing. This is shown analytically and confirmed by Monte Carlo simulations in the online supplement (Part A).

In fact, recent studies argue that higher wage growth is associated with earlier marriage (Cheng 2016; Killewald and Lundberg 2017). In this case, the FEGS estimate of the MWP is necessarily biased.

### *Descriptive Evidence of Selection on Wage Growth: Wage Profiles*

Despite its susceptibility to bias of the treatment effect, the FEGS model is useful to describe how wage trajectories differ between treatment groups. As explained earlier,  $\alpha_{20}$  gives the steepness of the wage profile for men who do not marry, and  $\alpha_{21}$  captures the higher wage growth of men who marry. Therefore, in the results section we will present these FEGS results.

However, this is only a heuristic, because in the group of unmarried men there are some men who will eventually marry after the panel's observation period ends. These misclassified cases will bias the wage growth of the unmarried group upward. With short panels, the bias could be large and we might erroneously conclude that there is no difference in wage growth between treatment groups. Notwithstanding, with long panels—such as in our estimation sample—the bias should be small enough to get valid information on wage growth for the treatment groups.

### *Crucial Test of the Specialization Hypothesis: The Time-Path of the MWP*

Finally, to shed further light on the question of whether specialization produces the MWP, we replace the marriage dummy in our FE and FEIS models with a set of dummy variables for each year of first marriage. Hence, we follow the literature in analyzing how the effect of marriage varies over marriage duration (the time-path of the effect). Unlike earlier studies (Cheng 2016; Dougherty 2006; Killewald and Lundberg 2017), however, we aim for an estimate of the causal effect net of selection on wage levels and growth. Therefore, we do not include dummy variables for

years preceding marriage. With the FEIS model, we expect to find an increasing MWP during marriage if the specialization hypothesis holds, but no effect on marital wage growth if the MWP is driven by selection.<sup>5</sup>

## DATA: VARIABLES AND ESTIMATION SAMPLE

Our study of the male marital wage premium is based on waves 1979 to 2012 of the NLSY79 (Bureau of Labor Statistics 2014). The NLSY79 is a U.S. panel survey following more than 12,000 young men and women born 1957 to 1964 over their lives. The survey was conducted annually until 1994, and every second year thereafter. Hence, we look at data spanning up to 33 years in the lives of respondents.

The NLSY79 is a long-running panel survey that collects detailed information on labor market careers and family biographies prospectively, which makes it the prime data source for studies of the MWP in the United States (see our literature review). In particular, it is possible to construct a sample of men who have been followed throughout the period when first marriage typically occurs. This property of the data is crucial in order to compare the wages of husbands and bachelors for several years before and after marriage. It also enables identification of the causal effect of marriage using the FEIS model.

### *Variables*

*Hourly wages.* Following the literature, the outcome variable of our analysis is the natural logarithm of the gross real hourly wage of respondents in their primary job. Wages are adjusted to 2006 prices using the U.S. consumer price index. In the NLSY79, the hourly wage rate is provided as a generated variable. We found that extreme outliers on wages were most likely data errors. Therefore, we decided to set real hourly wages to missing if they were lower than 50 U.S. cents or higher than 500 U.S. dollars.<sup>6</sup>

*Marital status.* We used the generated variable for respondent's marital status together with information on the date of first marriage to compute our independent variable of main interest: a time-varying dummy variable indicating whether a man is currently married for the first time. To analyze the time-path of the MWP, we measured years in first marriage as the difference of the month of the current interview and the month of first marriage (annualized and rounded up to the next integer). We model the time-varying effect of marriage by including separate dummy variables for each year of first marriage.

*Work experience.* Using respondents' weekly work histories, we calculated actual work experience in the main job by counting weeks employed up to the current survey week. In supplementary models, we used a measure of potential work experience (defined as respondent's age – years of education – 5). As explained earlier, the effect of work experience is assumed constant across persons in conventional FE models, but it is allowed to vary between individuals in the FEIS model (between groups in the FEGS model). We suspect unobserved variables are determining the steepness of individual wage profiles.<sup>7</sup>

*Control variables.* As standard measures of workers' human capital, our models control for years of education, a dummy for current enrollment in the educational system, and tenure with the current employer. Variables on educational enrollment and attainment are provided as generated variables in the NLSY79. For years of education, we applied some minor modifications. We used the last valid value reported earlier if the current information was missing. In cases in which the reported years of education decreased over time, we used the highest value that had been reported in earlier waves. To measure tenure, we counted weeks worked with the current main employer (divided by 52 weeks). Because earlier studies have reported a "daddy bonus" for men (Hodges and Budig 2010; Killewald 2013; Lundberg and Rose

2002), it is necessary to control for married men's higher fertility. Therefore, we constructed dummy variables indicating that a man is the biological father of one child, two children, or at least three children. Finally, we use indicators of survey years to capture period effects, that is, the common national wage trend.<sup>8</sup>

The set of control variables serves to estimate a basic human capital model of wages. We estimate parsimonious models to avoid controlling away the presumed causal mechanisms by adding intervening variables. In fact, to avoid problems of overcontrolling, recent studies aim to estimate the "total effect" of marriage by using even more parsimonious specifications where actual work experience and tenure are not controlled (Cheng 2016; Killewald and Gough 2013; Killewald and Lundberg 2017). We will also discuss findings using this most parsimonious specification.

### *Estimation Sample*

The NLSY79 data contain information on 6,403 men providing 120,383 interviews in total. From this pool, we extracted an estimation sample of employed male workers (see Appendix Table A1 for more details). We first selected person-years of men who were currently working (during the week of the interview or any of the six weeks preceding the interview) using labor force status from respondents' weekly work histories.<sup>9</sup> Furthermore, we dropped person-years in cases in which men reported being currently self-employed. It is standard in the literature to exclude self-employed men for analyzing the MWP, because their earnings are a bad measure of productivity (see Ahituv and Lerman 2007; Killewald and Gough 2013). Although these selection criteria for employment strongly reduced the number of available person-years (by 30.7 percent), few men were dropped altogether (3.5 percent).

The sample is further restricted to person-years of men who were never-married when they participated in the survey for the first

time. Because we are mainly interested in the transition to first marriage, we dropped all person-years observed after a first marriage ended due to separation, divorce, or death of the first wife (thus also excluding remarried men). Furthermore, we excluded person-years of men married for more than 15 years. Together, the restrictions on marital biographies reduced the number of available interviews by 20.8 percent and the number of men analyzed by 8.3 percent.

In the next step, we lost 4.5 percent of person-years (3.3 percent of respondents) due to missing values on the wage or any of the further variables needed in the analysis. After listwise deletion, persons were dropped if, due to missing values, there was no unmarried person-year left for them (1.3 percent of person-years, 4.7 percent of respondents).

Finally, we excluded men with fewer than four person-years with valid information on all variables (1.4 percent of person-years and 12.2 percent of respondents). As noted earlier, valid information on wages and covariates is needed for at least four years because our FEIS models are specified with an individual intercept as well as individual slopes for work experience and experience squared. The final estimation sample thus consists of 4,287 men providing 49,801 person-years (67 and 41.4 percent, respectively, of the total).

Although our restrictions reduce the size of the sample considerably, they are nevertheless necessary to identify well-defined causal effects. In the robustness checks section, we discuss results using a less restricted sample consisting of 4,816 men and 78,611 interviews (75 and 65 percent of the total). In this sample, we do not apply restrictions on employment, marital status, or marriage duration. However, we still require that men are observed never-married at least once.

Note that we thereby discard person-years of men who are always married, which might sound strange for researchers socialized with the cross-sectional research design. However, it is necessary when using a within research design. The FE estimate of the average MWP will be biased if we include always married

men and the true causal effect increases over the course of a marriage, as the specialization argument would expect (Sobel 2012:526).<sup>10</sup>

## RESULTS

### *Descriptive Statistics*

Table 1 describes the composition of the NLSY79 sample for the three groups of never-married, later-married, and ever-married men as classified by their (time-constant) treatment status. The table also shows the overall variation and the variation within individuals for the variables included in the analysis.

In the estimation sample, 27 percent of men remain never-married (see Table 1, lower part). More than two thirds of the men (69 percent) eventually married. These ever-married men provide information on wages before and after marriage. Finally, the sample includes 5 percent of men who are known to marry but are not observed married for any person-year contained in the estimation sample (mainly because they were not working while married or due to item non-response). We refer to these persons as later-married men. As for the never-married men, their wage is observed only before marriage. The later-married men are grouped together with ever-married men in one category (the time-constant treatment dummy equals one) when estimating the FECS model. Because they will marry in the future, they should show higher wage growth compared to never-married men. In the FEIS model, however, each man has his own wage profile. When estimating this model, we do not have to identify treatment groups explicitly to allow for heterogeneous wage trajectories.

In the sample, there is a large overall wage difference between never-married and ever-married men of .22 log points or, equivalently, 25 percent (compare Table 1, columns 1 and 3). The wage differential may be attributable, in part, to differences in observed covariates. Never-married men in the sample score higher on actual work experience (about

half a year). On the other hand, ever-married men score higher on years of education (about half a year) and they have more children. These mean differences in observables can be taken into account in a POLS model. To identify the causal effect of marriage, however, only the within-individual variation should be used.

The lower part of Table 1 reveals how much wages and covariates changed during respondents' observation window. Never-married and ever-married men were observed over a period of 18 years and 15.5 years, respectively (the mean difference of the age at the first and last person-year,  $\Delta$  age). As a result, never-married men acquired more actual work experience (about one year). However, the pay of men who got married increased more rapidly over their careers: their mean wage growth was at 5 percent per year worked, compared to only 3 percent for never-married men. There is thus descriptive evidence of higher wage growth for married men.

Looking at within-individual changes in other covariates, there are three differences that need to be controlled in multivariate models. First, ever-married men more often entered parenthood (72 percent, compared to 25 percent among the never-married men). Second, ever-married men worked for the same employer for longer periods of time (half a year). Third, they acquired slightly more years of formal education and they were more likely to complete education. Half of the ever-married men left the educational system (compared to 40 percent among the never-married). A person can expect a strong wage boost after completion of formal education, so it is crucial to account for time-varying enrollment.

### *The Wage Profiles of Ever-Married and Never-Married Men*

Figure 2 shows results for the FECS model. (Full regression results are reported in Part C, Table S3, of the online supplement.) The model estimates the effect of work experience for ever-married (including later-married) and

**Table 1.** Descriptive Statistics, NLSY79 Estimation Sample

	Never-Married	Later-Married	Ever-Married	Total
Gross hourly wage (US\$, 2006 prices)	13.40	13.04	16.68	15.70
Log gross hourly wage	2.42	2.40	2.64	2.58
Married in first marriage	0	0	.54	.38
Years in first marriage	0	0	3.08	2.18
No child	.79	.74	.58	.64
One child	.11	.16	.19	.17
Two children	.06	.07	.15	.13
Three or more children	.04	.04	.07	.06
Labor market experience (years)	8.93	5.30	8.19	8.30
Tenure with current employer (years)	3.16	1.92	3.38	3.28
Education (years)	12.49	11.90	12.96	12.81
Currently enrolled in school or training	.12	.17	.14	.14
Age (years)	29.81	25.12	27.89	28.32
$\Delta$ Log wage	.46	.45	.69	.61
$\Delta$ Log wage / $\Delta$ Experience	.03	.06	.05	.05
$\Delta$ Married	0	0	1	.69
$\Delta$ Years married	0	0	8.82	6.06
$\Delta$ No child	-.25	-.24	-.72	-.57
$\Delta$ One child	.09	.12	.22	.18
$\Delta$ Two children	.09	.05	.32	.25
$\Delta$ Three or more children	.07	.06	.19	.15
$\Delta$ Experience	14.79	7.77	13.79	13.78
$\Delta$ Tenure	4.70	2.32	5.20	4.94
$\Delta$ Education	1.61	1.46	1.88	1.79
$\Delta$ Enrolled	-.40	-.44	-.50	-.47
$\Delta$ Age	18.37	9.93	15.52	16.02
Number of persons	1,143	195	2,949	4,287
% of total	26.66	4.55	68.79	100
Number of person-years	13,143	1,470	35,188	49,801
% of total	26.39	2.95	70.66	100
Mean person-years per person	11.5	7.5	11.9	11.6

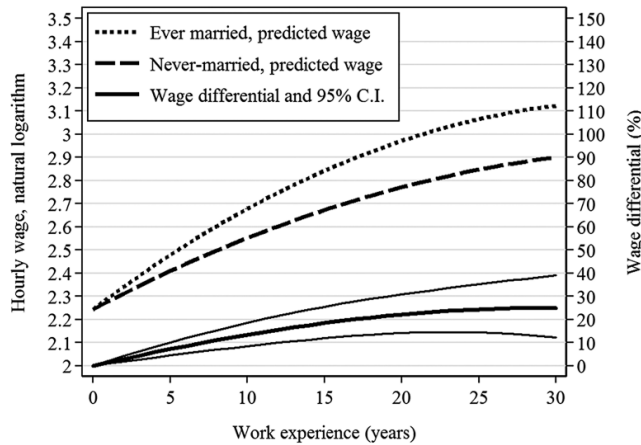
Source: NLSY79 data.

Note: Statistics shown are mean values for unweighted data.  $\Delta$  indicates within-person difference, for the respective variable, of the values observed in the last and the first person-year contained in the sample.

never-married men separately, while holding constant any time-constant difference in wage levels (along with time-varying covariates). Therefore, the wage trajectories shown in Figure 2 do not differ at career entry, where work experience equals zero. Furthermore, the model controls for a time-varying effect of marriage by including dummies for each year of marriage. Hence, the estimated wage differential captures the difference in the

wage profiles of the two treatment groups that we would have observed without anyone marrying. It is therefore a direct measure of selection on wage growth.

Looking at the predicted wage profiles of never-married and eventually married men (the upper curves in Figure 2), we see clear evidence of higher wage growth among to-be-married men. In fact, the difference in the effect of work experience between the two



**Figure 2.** Wage Profiles of Ever-Married and Never-Married Men

Source: NLSY79 data.

Note: Predicted wages and wage differentials using fixed-effects group-specific slopes (FEGS) model. Group-specific slopes for work experience are specified by interaction effects of a time-constant treatment dummy (ever-married, including later-married) and experience (linear and squared term). Regression results are shown in Part C, Table S3, in the online supplement. Regression models include as further covariates time-varying dummies for years in first marriage, number of biological children (four categories), tenure with current employer, years of education, indicator for current enrollment in education, and survey year dummies (grouped, seven categories). To predict wages, dummies for years in first marriage are set to zero. Other covariates are set to their respective sample mean.

treatment groups (the lower curves in Figure 2) alone results in an estimated wage differential as large as 24.9 percent at 30 years of work experience. On average, future husbands accumulated six years of work experience in the year preceding first-marriage. At this point of their labor market careers, they already earned 8.5 percent more than never-married men. As we can infer from the confidence interval, the wage differentials at this early career stage are significantly different from zero.<sup>11</sup>

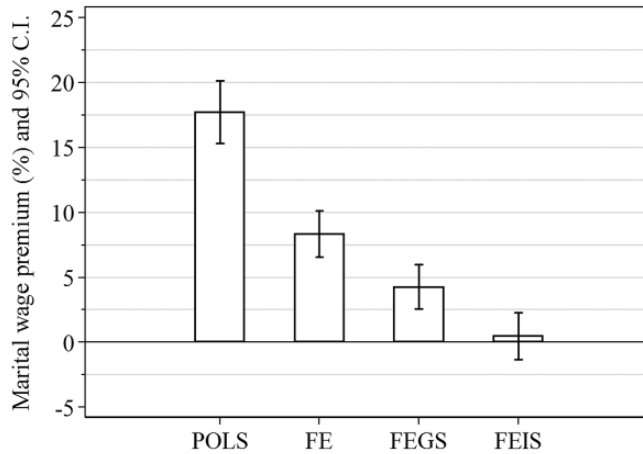
As noted earlier, the treatment group of to-be-married men includes ever-married and later-married men. There will nevertheless be misclassified cases with regard to the time-constant treatment indicator, because an unknown proportion of never-married men will get married in the future. As a consequence, the wage differential shown in Figure 2 is likely underestimated. Nevertheless, the interaction effects of the treatment indicator and work experience estimated with FEGS models are jointly significant ( $F(2, 4286) = 16.95, p < .001$ ). Overall, there is clear

evidence of heterogeneous wage profiles, pointing to marital selection that operates on men's wage growth.

### *The Average Effect of Marriage on Men's Wages*

Figure 3 compares the effect of marriage estimated by four different statistical models (see Appendix Table A2 for full regression results). The literature shows that standard FE models reduce the selection bias of the marital wage premium present in POLS results. The question we address with the FEGS and FEIS models is whether the remaining effect is still due to selection.

The POLS model estimates a large marital premium of 17.7 percent in the United States. However, this effect captures selection as well as any causal effect. The FE model that allows for selection on the wage level cuts the premium in half. Even then, the effect is still large at 8.3 percent. These results are not directly comparable with Ahituv and Lerman's (2007) findings, because we use a more



**Figure 3.** Comparison of the Average Marital Wage Premium across Models

Source: NLSY79 data.

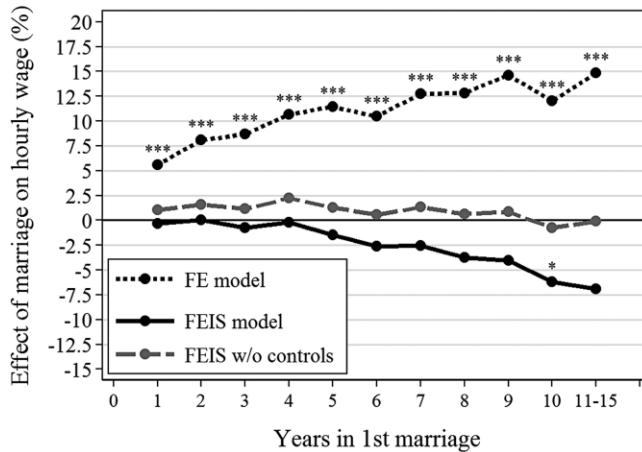
Note: Marital wage premium estimated by pooled OLS (POLS), conventional fixed-effects (FE), fixed-effects group-specific slopes (FEGS), and fixed-effects individual-specific slopes (FEIS) models. Standard errors are panel-robust. POLS, FE, FEGS, and FEIS models include as further covariates work experience and experience squared, number of biological children (four categories), tenure with current employer, years of education, indicator for persons currently enrolled in education, and survey year dummies (grouped, seven categories). Full regression results are shown in Appendix Table A2.

basic specification with regard to marital status. Nevertheless, the POLS and FE results are similar to their findings. Using POLS, Ahituv and Lerman report a coefficient of .118 for the first year of marriage, and .175 for continuing marriages. With FE, they obtain considerably smaller estimates. In their study, the coefficients are then at .076 and .119, respectively.

Killewald and Gough (2013) also estimate a similar marital premium with an FE model. In their model, they differentiate marriage from unmarried cohabitation and report a coefficient of .073 for married men without children (compared to never-married single men without children). Killewald and Gough are aware of the potential problem of marital selection based on wage growth and conduct an empirical test: they report that, among all years prior to marriage, the wages of eventually married persons grow strongest in the year preceding marriage. Therefore, they concede that their estimate of the marital premium may be biased. Nevertheless, they conclude that “marriage benefits men’s wages” (p. 496).

However, what happens to the remaining MWP if the higher wage growth of to-be-married men is taken into account using the FEIS model? Figure 3 shows that the model eliminates the premium for U.S. workers. The effect of first marriage on men’s wages now is merely .5 percent, and it is no longer significant at reasonable levels. Selection on wage growth therefore fully accounts for the premium remaining in an FE model.

Note that the FEGS model estimates a significant effect of 4.3 percent. As we argued, a model that allows for mean differences in wage growth between treatment groups is not sufficient to eliminate the selection bias. Recent studies have included interaction effects of work experience and education in FE models to at least partly control for marital selection on wage growth (see Cheng 2016; Killewald and Gough 2013; Killewald and Lundberg 2017).<sup>12</sup> A shortcoming of this approach is that individual wage growth may also be related to personality and other traits that are not contained in the NLSY79 data but nevertheless increase the probability of marriage. The FEGS model introduced here



**Figure 4.** Time-Path of the Marital Wage Premium, Estimated by FE and FEIS Models  
 Source: NLSY79 data.

Note: Comparison across models: marital wage premium estimated by fixed-effects (FE) and fixed-effects individual-specific slopes (FEIS) models. Full regression results are shown in Part C, Table S3, in the online supplement. FE model, FEIS model: include as further covariates years of education, tenure, number of children (four categories), indicator for current enrollment in education, and survey year dummies (seven categories). FEIS w/o controls: no controls for tenure and number of children; actual work experience replaced by potential experience (age – years of education – 5). Standard errors are panel-robust.  
 \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$  (two-sided test).

improves on this method because it is not necessary to specify the variables that produce different wage growth between never-married and married men. However, it does so at the price of assuming a common wage profile within treatment groups. The findings presented here indicate that this assumption is violated. Hence, even FEIS is biased and we need to control for heterogeneous wage growth on the individual level.

*The Time-Path of the Marital Wage Premium*

Figure 4 provides evidence for the test of the specialization argument that men experience higher wage growth after marriage (see Part C, Table S3, in the online supplement for full regression results). As discussed earlier, gender role specialization would predict an increasing impact over the duration of the marriage (see Figure 1a).

FE estimates a 5.6 percent wage boost in the first year of marriage. This effect further

increases over time to 11.4 percent in the fifth year of marriage and 14.8 percent after 11 to 15 years. Previous research has reported this finding of an increasing wage benefit, and it is often interpreted as evidence consistent with the specialization argument (Ahituv and Lerman 2007; Cheng 2016; Dougherty 2006; Korenman and Neumark 1991). However, given our results so far, the estimates are obviously biased. Thus, the theoretical conclusion is wrong.

To demonstrate this, we control for heterogeneous wage growth on the level of individuals using an FEIS model. The estimates from this model show that marriage does not boost men’s wage growth. The effect of marriage on husbands’ wages is virtually zero during the first years of marriage, and it turns negative after five years. This is opposite of what the specialization argument would expect.

However, we would not conclude that there is a wage penalty for later years of a marriage for two reasons. First, the negative effects in the FEIS model are non-significant,



with only one exception (the effect at 10 years). Second, the downward trend may be due to overcontrolling. Because our specification includes standard human capital variables, the estimates show only the direct effect of marriage net of any possible indirect effect through higher accumulation of human capital by men who get married (Cheng 2016; Killewald and Gough 2013). As a test of overcontrol bias, we re-estimated the FEIS model using an even more parsimonious specification, where we replaced actual work experience with potential experience and did not enter controls for tenure and number of children. With this specification, the effect of marriage is not significant and close to zero for each single year of marriage. The MWP ranges from +2 percent in the fourth year to -1 percent after 10 years. It is worth noting that overcontrolling also does not explain why we do not find an average MWP. With the parsimonious specification, the average effect of marriage remains very small (1.4 percent) and not significant (see Appendix Table A2).

In summary, the results on the time-path of the MWP show that marriage affects neither wage level nor wage growth. According to our FEIS results, there is thus no point in claiming any causal effect of marriage on men's wages. Earlier studies' findings evidently were driven by violation of the parallel trends assumption. Relaxing this assumption by allowing for heterogeneous careers of young men during courtship reveals there are no wage benefits to marriage.

### *Robustness Checks*

How much can we trust our estimates? In this section, we (briefly) discuss sensitivity analyses addressing potential problems with effect heterogeneity and sample selection (a more extensive discussion can be found in the online supplement, Part D).

*Effect heterogeneity.* The time-varying effect of marriage shown in Figure 4 is just one example of heterogeneity in the MWP.

According to our results, the MWP does not change over time. Nevertheless, interaction effects of marriage with variables other than marriage duration may exist, so our result of no average premium may be wrong for subgroups of the population studied. Notably, the literature suggests higher premiums for men who marry young (Killewald and Lundberg 2017), for men married to women working less than full-time (Budig and Lim 2016; Killewald and Gough 2013), and for white versus African American husbands (Cheng 2016). In fact, using FE models, we were able to replicate these findings. However, using FEIS models we did not find any interaction effect of first marriage with wives' employment status or with race (see Part D, Figure S1, in the online supplement). There seems to be some effect heterogeneity by marriage timing, but even for respondents who married young (before age 23), the MWP is small and not significant. We also analyzed interaction effects of first marriage with education and urbanicity. For these variables, we again found strong interaction effects in FE models that vanish if we allow for individual-specific wage trajectories in FEIS models. Taken together, there is hardly any evidence of an MWP for subgroups of U.S. men. Rather, men's heterogeneous wage growth explains previous findings of seemingly heterogeneous marital premiums. For example, men on a steep wage trajectory marry young or are more often married to women who (temporarily) quit work or work part-time.

*Sample selection.* To check whether sample selection is responsible for our results, we conducted three tests. First, as a formal test (suggested by Wooldridge 2010:833), we estimated models including a selection indicator (equals 1 if a person is not included in the estimation sample in the next year, 0 otherwise) (see Part D, Table S4, in the online supplement). The idea of the test is that a significant effect for the selection dummy points to a sample that is selective with respect to wages. The results show evidence of sample selection in POLS and FE models.

Obviously, men with high wages or high wage growth are more likely to drop out of the sample. However, we find no evidence of sample selection in the FEIS model. Given that FEIS allows for individual heterogeneity of wage growth, it is perfectly plausible that the model ameliorates selectivity problems that may persist with FE. Second, we ran a test for attrition bias in two steps. We estimated a cross-sectional logit model where we included the first person-year of each man contained in our estimation sample, using a binary attrition indicator as the dependent variable (see Table S5 in the online supplement). We then computed attrition weights and used them to correct for attrition bias in FE and FEIS models. The results show only minor changes if we take attrition weights into account, with FEIS estimates for the MWP still insignificant and close to zero (see Table S6 in the online supplement). Third, we estimated FE and FEIS models with a less restricted sample than we used in the main analyses (see Table S7 in the online supplement). For this larger sample, we still required men to be once observed never-married, but we did not apply further restrictions on employment, duration of first marriage, or marital status. Using the larger sample, the MWP for first marriage is still small (2.8 percent) but significant in the FEIS model.

However, we argue that this effect is biased upward as demonstrated by two further tests. First, the wage profile may not be approximated well by a quadratic function for work experience with a large proportion of person-years observed in later career stages. Hence, we extended the specification to include a cubic term for experience (also a squared term for tenure and education). Second, we cannot hope to get clean estimates of the MWP given that a large proportion of individual panels are short pre-treatment but observed over a long period after marriage. Because the FEIS model implicitly controls for individual wage growth that is independent of treatment, it might make sense to require more than one person-year pre-treatment (see Morgan and Winship 2007).

Therefore, we restricted the large sample to persons with four person-years pre-treatment. For both tests, the effect found with FEIS shrinks to less than 2 percent and is no longer significant (see Table S7, columns 4 and 6, in the online supplement). In summary, although our main sample is strongly restricted, sample selection does not seem to be the reason why we do not find a marriage premium.

### *Limitations*

Our analyses have several limitations due to sources of endogeneity that might bias our results. If the exogeneity assumption required for FEIS is violated, our estimates of the MWP will be biased, that is, if  $E(\varepsilon_{it} | exp_{it}, m_{it}, \alpha_{1i}, \alpha_{2i}) \neq 0$ . Potential candidates that might produce endogeneity of marriage in our models are anticipation, simultaneity, measurement errors, and time-varying confounders.

*Anticipation.* The recent literature notes that the MWP may be due to anticipation of marriage (Ashwin and Isupova 2014; Cheng 2016). In this case, future marital status would drive current wages, violating the strict exogeneity condition. Consequently, husbands' higher pre-marriage wage growth, which we interpret in terms of marital selection, may in part be causal. For example, men might make greater investments in their careers if they believe they will get married. Based on our analyses, we cannot rule out this explanation. As discussed earlier, however, we think this argument has already been disproved, because the estimated MWP is not smaller for "shotgun marriages" (Killewald and Lundberg 2017), and because there is an MWP already several years before marriage, when it seems unreasonable that marriage plans affect behavior (Dougherty 2006).

*Simultaneity.* Killewald and Lundberg (2017) found some evidence that young husbands show the highest wage increase in the years just before and after marriage. They argue that, for men who marry young, the

transition to adulthood occurs simultaneously with marriage, whereas for men who marry later, the transition occurs well before marriage. Although the concept of transition to adulthood is vague and hard to measure empirically, the literature shows that entry to stable employment is a crucial factor for young men to enter first marriage (Oppenheimer 2000, 2003; Oppenheimer et al. 1997). Thus, the small effect remaining with FEIS that we find for men who marry at a young age might be due to simultaneity: that is, they often enter stable full-time employment (and therefore get a wage raise) just before marriage.

*Measurement error.* Random measurement error on the marriage dummy could also bias our estimate, and the size of the bias is generally larger using within-estimation than in a cross-sectional model. Thus, our finding of no wage premium might be due to attenuation bias. Although we are not able to carry out a proper test with the NLSY79, Isacsson (2007) did not find any significant difference for FE estimates of the MWP using a self-reported measure of marital status compared to a measure matched from administrative data. Hence, measurement error on marital status seems unlikely to produce our findings.

*Time-varying confounders.* Because we found null effects, we think it is highly unlikely these were produced by unobserved time-varying confounders. However, there may also be problems with observed covariates. In the results section, we discussed potential bias due to overcontrolling (i.e., controlling for time-varying covariates that intervene). According to Sobel (2012), however, there may be an even more severe problem of endogenous selection bias. The problem arises if marital status affects accumulation of human capital, which in turn affects later marital status. In this case, we need to control for experience, tenure, and number of children, because these variables confound the effect of marital status, while, at the same time, we should not include them to

avoid overcontrolling. Fortunately, the problem does not seem to be large in practice. Mincy, Hill, and Sinkewicz (2009) applied Inverse Probability of Treatment Weighting (IPTW) to a U.S. sample of fathers and did not find evidence for Sobel's argument. Their analyses suggest that marital selection really is the main problem for estimation of the MWP.

## CONCLUSIONS

This study presents new evidence on the male marital wage premium. Using panel data from the National Longitudinal Survey of Youth 1979, we analyzed the careers of U.S. workers as they entered first marriage or remained unmarried. Our main aim was to estimate the average marital wage premium (MWP). We replicated the main result of recent studies for the United States using a standard fixed-effects (FE) model (Ahituv and Lerman 2007; Dougherty 2006; Killewald and Gough 2013) and found an MWP of 8.3 percent. This standard way to control for selection of successful men into marriage eliminates more than half of the gross wage difference between married and never-married men.

We argued that estimates of standard FE models are biased upward if selection into marriage not only operates via wage levels but also via wage growth. We provided descriptive evidence that men who eventually marry are in fact on a steeper wage profile. Results of a model specifying (treatment) group-specific wage profiles (FEGS) show that a wage differential develops between future husbands and never-married men. Due to their higher wage growth, to-be-married men gradually earn more already before marriage.

However, to estimate the causal effect of marriage, controlling for heterogeneous wage growth only on the group level (as proposed by Morgan and Winship 2007) is, according to our results, not sufficient. Although the effect of marriage is reduced substantially using this approach, we still find a significant

wage premium of about 4 percent. A model that allows for individual-specific wage profiles (FEIS), however, returns a non-significant effect of only .5 percent. Thus, it seems that no causal effect of marriage remains if we allow for more general patterns of selection into marriage based on wage level and wage growth.

Our results for the United States are thus very close to findings of no substantial premium obtained with standard FE models for Sweden (Isacson 2007), Denmark (Gupta et al. 2007), and Norway (Petersen et al. 2011). However, the reason why there is no marital wage premium in Scandinavia does not seem to be that gender role specialization is less common there than in the United States (Gupta et al. 2007). Rather, marriage seems to be more strongly related to the steepness of careers in the United States.

Overall, our results are consistent with a modified selection argument. Not only are financially successful men selected into marriage, but men with promising career trajectories are more likely to marry. Women may have a preference for these promising men because their higher wage growth raises expected marital income, or women may simply value other hard to observe traits that are associated with men's career progression (e.g., physical attributes, personality, social skills). These selection arguments suffice to explain the large cross-sectional marital wage premium. Future empirical research is needed to discriminate between these mechanisms of marital selection.

Our study has several implications. First, our analyses cast serious doubt on related findings. The MWP estimated with FE models for other countries might be spurious, due to the same bias we found for the United States (for Britain, see Bardasi and Taylor 2008; for West Germany, see Pollmann-Schult 2011). The finding of a premium for cohabiting men compared to never-married men (Dougherty 2006; Killewald and Gough 2013; Stratton 2002) may be an artifact as well, because existing studies do not take into account that high wage growth makes men

more attractive candidates during courtship. Similarly, recent studies may be wrong in asserting that male workers receive a daddy bonus (Hodges and Budig 2010; Killewald 2013; Lundberg and Rose 2002). In fact, we found no fatherhood premium in any of our FEIS models. Our sensitivity analyses also show hardly any evidence of heterogeneity in the marital premium with respect to wives' employment (cf. Budig and Lim 2016; Killewald and Gough 2013) or race (Cheng 2016).

Moreover, similar selection arguments may well apply to other marriage benefits, like increased health and longevity, life satisfaction, and children's well-being (Brown 2010; Diener, Lucas, and Scollon 2006; Waite and Gallagher 2000; Wilson and Oswald 2005). There are analogous methodological problems in identifying effects of marriage on these outcomes, and we do not think the literature has fully taken them into account. Marriage benefits reported by many studies might therefore be spurious. The statistical model we introduced might serve as a starting point for re-evaluating earlier findings.

Second, on theoretical grounds, we found no support for the causal mechanisms put forward to explain the MWP. Our results suggest that the average marital premium found in the data is solely due to selection. In addition, we found no evidence of an increasing MWP over the duration of first marriage. Thus, gender role specialization within marriage does not make men more productive. Household specialization may exist, but we found no evidence that this boosts men's wages after marriage. Furthermore, even though men may work harder after marriage, their higher work effort does not translate to higher wages. Moreover, according to our results, employers do not favor married men in terms of pay. In fact, we never found these theoretical arguments convincing, which is why we started to think about less restrictive models to control for selection.

Third, the FEIS model might be a useful tool for causal analysis in many other research areas. Use of the standard FE model in social research is becoming more widespread,

because researchers acknowledge that results obtained by comparing different people using between-regression models are threatened by self-selection. Therefore, a within approach as implemented by FE is preferable. However, FE results are threatened by violations of the parallel trends assumption. FEIS relaxes this assumption. In many research areas there is good reason to suspect non-parallel trends in treatment and control groups, so FEIS might be applied much more widely in the future.

Methodologically, FEIS might be seen as a model that implements a long-standing plea of life course researchers: by allowing for individual-specific trajectories, FEIS takes the life course seriously. Whereas FE presumes identical life courses that only differ by a constant, FEIS allows individual trajectories that may vary widely. Given these individual life courses, FEIS asks how these trajectories are affected by life course events. Thus, FEIS may be seen as a useful tool for life course research.

A limitation of the method is the requirement to observe enough pre-treatment information to identify the counterfactual growth trajectory (see Morgan and Winship 2007). In our main analyses, we restricted the sample to persons who were observed at least once prior to marriage and at least four times in total. In addition, we right-censored individual

life-courses at 15 years of first marriage to ensure that estimates of the treatment effect are not biased because the sample is dominated by persons mainly observed after treatment. As a consequence, our conclusions do not apply to men who marry before entering the labor market, and they may not equally apply to later years of marriage. However, similar limitations would probably apply to any method designed to handle treatment selection that is related to individual outcome trajectories.

Finally, our study is a tale in the difficulties of doing social research with observational data. After all, we have shown that an effect found in dozens of studies is most likely spurious. The wage differential found between married and unmarried men (POLS), or within men before and after marriage (FE) is, according to our results, not causal. Although scholars have speculated for decades that the marital wage premium might not be causal, it has not been shown empirically. Therefore, a large literature was probably led astray by spurious results. Unfortunately, we suspect it is not the only research area where this is the case. Thus, a lesson from our study could be that effects found with observational data should always be intensively scrutinized with respect to selection. Only if we can plausibly determine that effects are derived from truly exogenous variation should we believe in them.

## APPENDIX

**Table A1.** Selection of NLSY79 Estimation Sample

	Person-Years	Percent	Persons	Percent
All Men in the NLSY79	120,383	100.00	6,403	100.00
<b>Exclusion criteria</b>				
Not working	28,222	23.44	203	3.17
Self-employed	8,716	7.24	23	.36
Marital status invalid	1,390	1.15	18	.28
Earlier marriage or married at entry	4,932	4.10	441	6.89
Separated, divorced, widowed	13,301	11.05	68	1.06
Later than 15 years in first marriage	5,464	4.54	6	.09
Hourly wage missing	2,984	2.48	104	1.62
Wage smaller than .5 US\$ or larger than 500 US\$	152	.13	1	.02
Any covariate missing (listwise deletion)	2,268	1.88	169	2.64
Not observed never-married (after listwise deletion)	1,516	1.26	299	4.67
Fewer than four valid person-years	1,637	1.36	784	12.24
Estimation sample	49,801	41.37	4,287	66.95

*Source:* NLSY79 data.

*Note:* Estimation sample includes currently employed men (self-employed excluded), initially never-married (when first providing information on all variables), and observed up to 15 years in first marriage (person-years while separated, divorced, widowed excluded); at least four person-years per man are required.

**Table A2.** Average Male Marital Wage Premium Estimated by POLS, FE, FECS, and FEIS Models

	POLS	FE	FECS	FEIS	FEIS w/o controls
Married (ref.: never-married)	.163*** (.010)	.080*** (.008)	.042*** (.008)	.005 (.009)	.014 (.009)
Currently enrolled	-.194*** (.009)	-.199*** (.010)	-.198*** (.010)	-.123*** (.010)	-.122*** (.010)
Years of education	.079*** (.002)	.068*** (.004)	.066*** (.004)	.007 (.006)	.058*** (.004)
One child (ref.: no child)	-.001 (.011)	.020* (.008)	.014 (.008)	-.016 (.010)	
Two children	.038** (.014)	.038*** (.012)	.020 (.012)	-.028 (.015)	
Three or more children	-.026 (.020)	.004 (.018)	-.021 (.018)	-.054* (.024)	
Tenure (years)	.018*** (.001)	.011*** (.001)	.011*** (.001)	.008*** (.001)	
Work experience (years)	.051*** (.003)	.044*** (.002)	.035*** (.003)		
Experience <sup>^</sup> 2 (divided by 100)	-.069*** (.010)	-.059*** (.008)	-.045*** (.010)		
Ever-married × Exper.			.016*** (.003)		
Ever-married × Exper. <sup>^</sup> 2 (divided by 100)			-.028* (.011)		
<i>R</i> squared	.35	.34	.34	.02	.05
Number of persons	4,287	4,287	4,287	4,287	4,287
Number of person-years	49,801	49,801	49,801	49,801	49,801

Source: NLSY79 data.

Note: Regression coefficients and panel-robust standard errors (in parentheses). Estimation results from pooled OLS (POLS), fixed-effects (FE), fixed-effects group-specific slopes (FECS), and fixed-effects individual-specific slopes (FEIS) models. Models further include dummies for grouped survey years (seven categories). FEIS w/o controls: model does not control for tenure and number of children; actual experience replaced by potential experience (age – years of education – 5). FECS: F-Test for joint significance of interactions ever-married × experience and ever-married × exper. <sup>^</sup> 2;  $F(2, 4286) = 24.72$ ,  $p < .001$ . Reported *R*-squared is overall *R*-squared for POLS, and within *R*-squared for FE, FECS, and FEIS models.

\* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$  (two-sided test).

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## Notes

1. Such wage differentials could be due to married men's longer working hours or higher productivity (i.e., a higher hourly wage). Here we—like most of the literature—are interested in the productivity differential (see Killewald and Gough 2013). Furthermore, we—like most of the literature—study the wage impact of heterosexual marriages. The data we use do not allow us to identify gay men. For the period under study, however, same-sex marriages were rare in the United States, with legal restrictions still in place until 2012 in most states.
2. Gender role specialization has become less pronounced ever since the “golden age of marriage,” so one might argue there is positive assortative mating on wage growth. Following Oppenheimer (1988) and extending Sweeney's (2002) reasoning for mating on wage levels, both men and women may nowadays search for a partner with high wage growth. However, this would not invalidate the promising men argument.
3. Dougherty (2006) points out that estimation of the MWP by the distributed FE model hinges on the assumption that there is no wage differential between to-be married and never-married men for the earliest year before marriage contained in the sample. Cheng (2016) and Killewald and Lundberg (2017) limited their samples to to-be-married men observed no more than five years prior to marriage. Hence, they are not able to study differences in earlier wage growth. Furthermore, Cheng circumvents the restriction of equal wages in the treatment groups by excluding all men who do not marry while participating in the NLSY79. She thus compromises identification of the MWP because, lacking an appropriate control group of never-married men, separating the effects of potential work experience and marriage duration is not feasible.
4. In practice, we estimate the FEIS model applying the Stata command `xtfeis` (Ludwig 2015). We used Stata 14 for all computations in this article. Syntax files for replication of our data preparation and analyses can be found in the online material accompanying this article.
5. Recently, some have argued that, by allowing for unit-specific growth, FEIS “kills” any treatment effect that changes over time. Meer and West (2013) suspect that causal effects that change the slope of an outcome trajectory (as implied by specialization, see the short-dashed line in Figure 1a) are controlled away by allowing for unit-specific slopes. Therefore, FEIS might underestimate the treatment effect. Meer and West back up this assertion with simulations that seem to show exactly this. However, our own simulations using a data-generating process that mimics our research context (individual panel data with a large number of units and a binary treatment) show that a properly specified FEIS model is able to capture true causal effects that vary over time (see Part B of the online supplement). Thus, we believe our findings on the time-path of marriage are valid.
6. Inspection of the NLSY79 data show that extreme outliers on hourly wages at both tails of the distribution seem to be due to either data errors in the original variable of reported earnings or missing or wrong information on the time unit of pay in the NLSY79 data. Because NLSY data cleaners applied no corrections to the generated variable contained in the data distribution, hourly wages range from .01 to 92,564 dollars. Extremely low and high wages are therefore almost certainly data errors. Note that only a few cases are excluded from the analytic samples due to wage trimming, see Appendix Table A1.
7. We also estimated regression models using an alternative measure of actual work experience, where each year of part-time work counts as only half a year of full-time work. Using this measure did not affect our conclusions.
8. We include dummies for grouped years. We grouped years into seven categories: 1979 to 1980 (reference), 1981 to 1985, 1986 to 1990, 1991 to 1995, 1996 to 2000, 2001 to 2005, and 2006 to 2012. Coefficients of the marriage dummy differ only marginally if we use the full set of year dummies.
9. Person-years are lost mainly because respondents were currently out of the labor force (e.g., still going to school), unemployed, or serving in the armed forces. We dropped 1.4 percent of person-years because current labor force status could not be derived from the work history. Note that, among the observations excluded with the restriction on current employment, 8,079 person-years (more than one third) appear to have valid wages. We dropped these cases from our main analyses to maintain the temporal order of cause (marriage) and effect (wages), because wages recorded in the NLSY79 survey do not always refer to the current job, but to the last job if respondents are currently not working for money.
10. For respondents who are always married during the observation window, the marriage dummy does not change. Nevertheless, compared to never-married men, their wages would grow faster over time. This higher wage growth is ignored in an FE model that specifies a common effect of work experience and a time-constant effect of marital status (see Equation 1). By assuming a common wage trajectory of always-married and never-married men, the approach misrepresents the counterfactual outcomes (the wage trajectory of the married had they not married), which are used to identify the treatment effect of marriage. Hence, the FE estimate of the average marital premium would be biased.



11. As an informal test of the quadratic shape of the wage profile, we predicted wages with a more flexible specification using splines of work experience (results not shown). Although we found some deviations from the parametric specification, the estimated wage differentials are very similar, regardless of whether we use the quadratic or the spline specification. Hence, even though the quadratic function may not fit the data very well, it is sufficient to estimate (mean) differences in wage growth.
12. Estimating the FEGS model with this extended specification (interaction terms of work experience and years of education) yields a smaller MWP of 3.6 percent (results not shown).

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